

Revisiting Event Study Designs: Robust and Efficient Estimation

Kirill Borusyak
UCL and CEPR

Xavier Jaravel
LSE and CEPR

Jann Spiess
Stanford*

This version: March 2022

Abstract

We develop a framework for difference-in-differences designs with staggered treatment adoption and heterogeneous causal effects. We show that conventional regression-based estimators fail to address a series of challenges and do not provide unbiased estimates of relevant estimands absent strong assumptions on treatment effects. We then derive the efficient estimator that addresses these challenges, which has an intuitive “imputation” form when treatment-effect heterogeneity is unrestricted. We characterize the asymptotic behavior of the estimator, propose tools for inference, and develop tests for identifying assumptions. Extensions include time-varying controls, triple-differences, and certain non-binary treatments. We show the practical relevance of these insights in a simulation study and an application. Studying the consumption response to tax rebates in the United States, we find that the marginal propensity to consume is between 8 and 11 percent in the first quarter, about half as large as benchmark estimates used to calibrate macroeconomic models, and close to zero in the following quarters.

*Borusyak: k.borusyak@ucl.ac.uk; Jaravel: x.jaravel@lse.ac.uk; Spiess: jspiess@stanford.edu. This draft supersedes our 2017 manuscript, “Revisiting Event Study Designs, with an Application to the Estimation of the Marginal Propensity to Consume.” We thank Alberto Abadie, Isaiah Andrews, Raj Chetty, Itzik Fadlon, Ed Glaeser, Peter Hull, Guido Imbens, Larry Katz, Jack Liebersohn, Jonathan Roth, and Pedro Sant’Anna for thoughtful conversations and comments. We are also grateful to Jonathan Parker for his support in accessing and working with the data from Broda and Parker (2014). The results in the empirical part of this paper are calculated based on data from The Nielsen Company (U.S.) LLC and provided by the Marketing Data Center and the University of Chicago Booth School of Business. Two accompanying Stata commands are available from the SSC repository: `did_imputation` for treatment effect estimation with our imputation estimator and pre-trend testing, and `event_plot` for making dynamic event study plots.

1 Introduction

Event studies are one of the most popular tools in applied economics and policy evaluation. An event study is a difference-in-differences (DiD) design in which a set of units in the panel receive treatment at different points in time. In this paper, we investigate the robustness and efficiency of estimators of causal effects in event studies, with a focus on the role of treatment effect heterogeneity. We first develop a simple econometric framework that delineates the identification assumptions from each other and from the estimation target, defined as some average of heterogeneous causal effects. We then apply this framework in three ways. First, we analyze the conventional practice of implementing event studies via two-way fixed effect (FE) regressions and show how the implicit conflation of different assumptions leads to biases. Second, leveraging event study assumptions in an explicit and principled way allows us to derive the robust and efficient estimator, along with appropriate inference methods and tests. The estimator takes an intuitive “imputation” form when treatment effects are unrestricted. Finally, we illustrate the practical relevance of our approach in an application estimating the marginal propensity to consume (MPC) out of tax rebates; our MPC estimates are lower than in prior work, implying that fiscal stimulus is less powerful than commonly thought.

Event studies are frequently used to estimate treatment effects when treatment is not randomized, but the researcher has panel data allowing to compare outcome trajectories before and after the onset of treatment, as well as across units treated at different times. By analogy to conventional DiD designs, event studies are commonly implemented by two-way fixed effect regressions, such as

$$Y_{it} = \alpha_i + \beta_t + \tau D_{it} + \varepsilon_{it}, \quad (1)$$

where outcome Y_{it} and binary treatment D_{it} are measured in periods t and for units i , α_i are unit fixed effects that allow for different baseline outcomes across units, and β_t are period fixed effects that accommodate overall trends in the outcome. Specifications like (1) are meant to isolate a treatment effect τ from unit- and period-specific confounders. A commonly-used dynamic version of this regression includes “lags” and “leads” of the indicator for the onset of treatment, to capture the dynamics of treatment effects and test for the parallel trajectories of the outcomes before the onset of treatment.

To understand the problems with conventional two-way fixed effect estimators in event-study designs and provide a principled econometric approach to overcoming these issues, in Section 2 we develop a simple framework that makes the estimation targets and underlying assumptions explicit and clearly isolated. We suppose that the researcher chooses a particular weighted average (or weighted sum) of heterogeneous treatment effects they are interested in estimating. We make (and later test) two standard DiD identification assumptions: that potential outcomes without treatment are characterized by parallel trends and that there are no anticipatory effects. We also allow for — but do not require — an auxiliary assumption that the treatment effects themselves follow some model that restricts their heterogeneity for *a priori* specified economic reasons. This explicit

approach is in contrast to regression specifications like (1), both static and dynamic, which implicitly conflate choices of estimation target and identification assumptions. Our framework covers a broad class of empirically relevant estimands beyond the standard average treatment-on-the-treated (ATT), including group-wise ATTs and ATTs at different horizons that hold the composition of units fixed.

Through the lens of this framework, in Section 3 we uncover a set of challenges with conventional regression-based event study estimation methods and trace them back to a lack of clearly stated or separated estimation target and identification assumptions. First, we note that failing to rule out anticipation effects in “fully-dynamic” specifications (with all leads and lags of the event included) leads to an underidentification problem when there are no never-treated units, such that the dynamic path of anticipation and treatment effects over time is not point-identified. We conclude that it is important to separate out testing the assumptions about pre-trends from the estimation of dynamic treatment effects under those assumptions. Second, implicit assumptions of homogeneous treatment effects embedded in static DiD regressions like (1) may lead to estimands that put negative weights on some — typically long-run — treatment effects. With staggered rollout, regression-based estimation leverages comparisons between groups that got treated over a period of time and reference groups which had been treated *earlier*, which we label “forbidden comparisons”. Indeed, these comparisons are only valid when the homogeneity assumption is true; when it is violated, they can substantially distort the weights the estimator places on treatment effects, or even make them negative. Third, in dynamic specifications, implicit assumptions about treatment effect homogeneity across groups first treated at different times lead to the spurious identification of long-run treatment effects for which no DiD comparisons valid under heterogeneous treatment effects are available. The last two challenges highlight the danger of imposing implicit treatment effect homogeneity assumptions instead of allowing for heterogeneity and explicitly specifying the target estimand.¹

From the above discussion, the reader should not conclude that event study designs are plagued by fundamental problems. On the contrary, these challenges only arise due to a mismatch between the regression estimators and the underlying assumptions. We therefore use our framework to circumvent these issues and derive robust and efficient estimators from first principles.

In Section 4, we first establish a simple characterization for the most efficient linear unbiased estimator of any pre-specified weighted sum of treatment effects, in the baseline case of homoskedasticity. This estimator explicitly incorporates the researcher’s estimation goal and assumptions about parallel trends, anticipation effects, and restrictions on treatment effect heterogeneity. It is constructed by estimating a flexible high-dimensional regression that differs from conventional event study specifications, and aggregating its coefficients appropriately. While homoskedasticity is only a natural starting point, the principled construction of this estimator yields attractive efficiency properties more generally, as we confirm in simulations and later in the application. We find that the estimator has sizable efficiency advantages: the variances of alternative robust estimators are 15–41% higher under homoskedasticity; moreover, we find in simulations that these gains are preserved under

¹We show that these challenges are not resolved by trimming the sample to a fixed window around the event date.

heteroskedasticity and serial correlation of residuals.

The efficient robust estimator can be implemented using a transparent “imputation” procedure in our leading case where the heterogeneity of treatment effects is not restricted. First, the unit and period fixed effects $\hat{\alpha}_i$ and $\hat{\beta}_t$ are fitted by regressions using untreated observations only. Second, these fixed effects are used to impute the untreated potential outcomes and therefore obtain an estimated treatment effect $\hat{\tau}_{it} = Y_{it} - \hat{\alpha}_i - \hat{\beta}_t$ for each treated observation. Finally, a weighted sum of these treatment effect estimates is taken, with weights corresponding to the estimation target.

To relate our efficient estimator to other unbiased estimators, we derive two additional, more general results. First, any other linear estimator that is unbiased in our framework with unrestricted causal effects can be represented as *an* imputation estimator, albeit with an inefficient way of imputing untreated potential outcomes. Second, even when assumptions that restrict treatment effect heterogeneity are imposed, any unbiased estimator can still be understood as an imputation estimator for an adjusted estimand. Together, these two results allow us to characterize estimators of treatment effects in event studies as a combination of how they impute unobserved potential outcomes and which weights they put on treatment effects.

For the efficient estimator in our framework, we provide tools for valid inference. Specifically, we derive conditions under which the estimator is consistent and asymptotically Normal and propose standard error estimates. Inference is challenging under arbitrary treatment effect heterogeneity, because causal effects cannot be separated from the error terms. We instead show how asymptotically conservative standard errors can be derived, by attributing some variation in estimated treatment effects to the error terms. Our inference results apply under mild conditions in short panels. Expanding the existing literature on DiD estimation with staggered adoption, we also provide conditions for consistency and inference that extend to panels where the number of time periods grows, as long as growth is not too fast. We also propose a leave-one-out modification to our conservative variance estimates with improved finite-sample performance.

In addition, we provide a principled way of testing the identifying assumptions of parallel trends and no anticipation effects, based on ordinary least squares (OLS) regressions with untreated observations only. Compared to conventional OLS regressions with leads and lags of treatment, this approach avoids contamination of the tests by treatment effect heterogeneity, shown by Sun and Abraham (2021). Moreover, relative to placebo-based tests (e.g., de Chaisemartin and D’Haultfoeulle, 2021), our strategy clearly separates estimation and testing. This property allows the estimator to remain efficient while also avoiding a power loss in testing. Finally, the separation of testing from estimation, along with the properties of our efficient estimator, allows our test to avoid the pre-testing problems pointed out by Roth (2019), under homoskedasticity.

While our baseline setting is for panel data with two-way fixed effects, we show how our formal results extend naturally in a number of ways. We allow for more general panel-type specifications that can include, for instance, unit-specific trends and time-varying covariates. In Section 5 we consider extensions to repeated cross-sections, data defined by two cross-sectional dimensions (e.g., regions and age groups), triple-differences designs, and other data structures. We further discuss

the implications of our results when treatment is simultaneous rather than staggered, when it can switch on and off, and when multiple treatment events can happen in the same unit. Settings with non-binary treatments (i.e., variable treatment intensity) are also covered, provided each unit is observed without treatment in some earlier periods.

It is also useful to point out the limitations of our analysis. First, all event study designs assume a restrictive parametric model for untreated outcomes. We do not evaluate when these assumptions may be applicable, and therefore when the event study design are *ex ante* appropriate, as Roth and Sant’Anna (2020) do. We similarly do not consider estimation that is robust to violations of parallel-trend type assumptions, as Rambachan and Roth (2020) propose. We instead take the standard assumptions of event study designs as given and derive optimal estimators, valid inference, and practical tests to assess whether parallel-trend assumptions hold. Second, we also do not consider event studies as understood in the finance literature, based on high-frequency panel data, which typically do not use period fixed effects (MacKinlay, 1997).

In Section 6, we illustrate the practical relevance of our theoretical insights by revisiting the estimation of the marginal propensity to spend (MPX) out of tax rebates in the event study of Broda and Parker (2014). First, we show that the choice of a binned OLS specification used by Broda and Parker (2014) leads to a substantial upward bias in estimated MPXs. Indeed, we find that the binned OLS specification puts a large weight on the effects happening in the first week after the rebate receipt, and negative weights on some longer-run effects, biasing the estimate upwards because the spending response quickly decays over time. Second, we highlight that, due to the extrapolation of treatment effects implicit in the OLS estimator, some dynamic specifications could be mistakenly interpreted as evidence for a large and persistent increase in spending. Our imputation estimator eliminates unstable patterns found across OLS specifications. Third, we illustrate the underidentification problem with the fully-dynamic OLS specification: the dynamic path of estimates is very sensitive to the choice of leads to drop. Fourth, we find large efficiency gains of our imputation estimator relative to the alternative robust estimators: the confidence interval is about 50% longer for all periods for de Chaisemartin and D’Haultfoeuille (2021), and 2–3.5 times longer for Sun and Abraham (2021).

We also draw the implications of our findings for the macroeconomics literature. While commonly used estimates of the quarterly MPX covering all expenditures range from 50-90% and estimates of the quarterly MPX for nondurable expenditure range from 15-25%,² our estimates are about half as large, at 25–37% for the MPX one quarter after tax rebate receipt for all expenditure and 8–11% for the quarterly MPX for nondurables. Our preferred estimates are also more short lived, falling to zero beyond the first month after receiving the tax rebate. Thus, our new estimates imply that fiscal stimulus may be less potent than predicted by leading macroeconomic models targeting benchmark estimates.³

²Broda and Parker (2014), Parker et al. (2013) and Johnson et al. (2006) estimate different versions of the MPX out of tax rebates. Laibson et al. (2022), Kaplan and Violante (2021) and Di Maggio et al. (2020) provide recent reviews of the literature on the estimation of the marginal propensity to spend and consume.

³See also Baker et al. (2021) for evidence that using robust, rather than regression-based, event study estimation matters in other empirical contexts.

For convenient application of our results, we supply a Stata command, `did_imputation`, which implements the imputation estimator and inference for it in a computationally efficient way. Our command handles a variety of practicalities, such as additional covariates and fixed effects, observation weights, and repeated cross-sections. We also provide a second command, `event_plot`, for producing “event study plots” that visualize the estimates with both our estimator and the alternative ones.

Our paper contributes to a growing methodological literature on event studies. To the best of our knowledge, our paper is the first and only one to characterize the underidentification and spurious identification of long-run treatment effects that arise in regression-based implementations of event study designs. The negative weighting problem has received more attention. It was first shown by de Chaisemartin and D’Haultfoeuille (2015, Supplement 1). The earlier manuscript of our paper (Borusyak and Jaravel, 2017) independently pointed it out and additionally explained how it arises because of forbidden comparisons and why it affects long-run effects in particular, which we now discuss in Section 3.3 below. The issue has since been further investigated by Goodman-Bacon (2018), Strezhnev (2018), and de Chaisemartin and D’Haultfoeuille (2020), while Sun and Abraham (2021) have shown similar problems with dynamic specifications. Sun and Abraham (2021) and Roth (2019) have further uncovered problems with conventional pre-trend tests, and Schmidheiny and Siegloch (2020) have characterized the problems which arise from binning multiple lags and leads in dynamic specifications. Besides being the first to point out some of these issues, our paper provides a unifying econometric framework which explicitly relates these issues to the conflation of the target estimand and the underlying identification assumptions.

Several papers have proposed ways to address these problems, introducing estimators that remain valid when treatment effects can vary arbitrarily (de Chaisemartin and D’Haultfoeuille, 2021; Sun and Abraham, 2021; Callaway and Sant’Anna, 2021; Marcus and Sant’Anna, 2020; Cengiz et al., 2019). An important limitation of these robust estimators is that their efficiency properties are not known.⁴ A key contribution of our paper is to derive a practical, robust and finite-sample efficient estimator from first principles. We show that this estimator takes a particularly transparent form under unrestricted treatment effect heterogeneity, while our construction also yields efficiency when some restrictions on treatment effects are imposed. By clearly separating the testing of underlying assumptions from the estimation step imposing these assumptions, we simultaneously increase estimation efficiency and avoid pre-testing bias under homoskedasticity.⁵ Our estimator uses all pre-treatment periods for imputation, as appropriate under the standard DiD assumptions, while alternative estimators use more limited information; see Section 4.5 for a detailed discussion.⁶

⁴A notable exception is Marcus and Sant’Anna (2020), who consider a two-stage generalized method of moments (GMM) estimator and establish its semiparametric efficiency under heteroskedasticity in a large-sample framework with a fixed number of periods. However, they find this estimator to be impractical, as it involves many moments, e.g. almost as many as the number of observations in the application they consider.

⁵This efficiency gain is obtained without stronger assumptions than in de Chaisemartin and D’Haultfoeuille (2021) and Sun and Abraham (2021). Callaway and Sant’Anna (2021) restrict pre-trends only for some units and periods, which is technically a weaker assumption (see Marcus and Sant’Anna, 2020).

⁶A separate strand of literature has considered design-based inference with event studies, where randomness arises in the treatment assignment and timing, following a known model (Athey and Imbens, 2018; Roth and Sant’Anna,

Finally, our paper is related to a nascent literature that develops robust estimators similar to the imputation estimator. To the best of our knowledge, this idea has been first proposed for factor models (Gobillon and Magnac, 2016; Xu, 2017). Athey et al. (2021) consider a general class of “matrix-completion” estimators for panel data that first impute untreated potential outcomes by regularized factor- and fixed-effects models and then average over the implied treatment-effect estimates.⁷ In work independent from ours, the imputation idea has been explicitly applied to fixed-effect estimators in event studies by Liu et al. (2020) and Gardner (2021). Specifically, the counterfactual estimator of Liu et al. (2020) and the two-stage estimator of Gardner (2021) coincide with the imputation estimator in our model for the specific class of estimands their papers consider. Relative to these papers, we make four contributions: we *derive* a general imputation estimator from first principles, show its efficiency, provide tools for valid asymptotic inference when unit fixed effects are included, and show its robustness to pre-testing. Subsequently to our work, Wooldridge (2021) derives a two-way Mundlak estimator, which is also equivalent to the imputation estimator in complete panels with no controls and for a restricted class of estimands. The robustness and efficiency properties of our estimator are not limited to those situations.

2 Setting

We consider estimation of causal effects of a binary treatment D_{it} on some outcome Y_{it} in a panel of units i and periods t . We focus on “staggered rollout” designs, in which being treated is an absorbing state. That is, for each unit there is an event date E_i when D_{it} switches from 0 and 1 and stays there forever: $D_{it} = \mathbf{1}[K_{it} \geq 0]$, where $K_{it} = t - E_i$ is the number of periods since the event date (“relative time”). We also allow that some units are never treated, denoted by $E_i = \infty$.⁸ Units with the same event date are referred to as a cohort.

We do not make any random sampling assumptions and work with a fixed set of N observations $it \in \Omega$, which may or may not form a complete panel. We similarly view the event date for each unit, and therefore all treatment indicators, as fixed. We define the set of treated observations by $\Omega_1 = \{it \in \Omega: D_{it} = 1\}$ of size N_1 and the set of untreated (i.e., never-treated and not-yet-treated) observations by $\Omega_0 = \{it \in \Omega: D_{it} = 0\}$ of size N_0 .⁹

2022; Arkhangelsky and Imbens, 2019). Our identification and inference approach differs in that we condition on treatment timing and take the panel as given. With this approach, we follow Abadie et al. (2014) in focussing on an in-sample estimand, which in our case is a weighted sample average treatment effect for those observations that were actually treated.

⁷Earlier work on synthetic controls (Abadie et al., 2010) and interactive fixed effects (Hsiao et al., 2012) features estimators with similar structure in settings with just one treated unit, in which treatment effect heterogeneity is not a main focus.

⁸In principle always-treated units are also allowed for, by $E_i = -\infty$, but in practice they will not be useful for causal identification with flexible treatment effect heterogeneity.

⁹Viewing the set of observations and event times as non-stochastic is for notational convenience: Appendix A.10 shows how this framework can be derived from one in which both are stochastic, by conditioning on Ω and $\{E_i\}_i$. Furthermore, our conditional framework avoids random sampling assumptions made in other work on DiD designs (e.g. de Chaisemartin and D’Haultfœuille, 2020, Sun and Abraham, 2021, and Callaway and Sant’Anna, 2021). In Appendix A.11 we show how the assumptions in this section can be derived from such population models; in this sense, our approach is more general since it nests common alternatives..

We denote by $Y_{it}(0)$ the period- t (possibly stochastic) potential outcome of unit i if it is never treated. We are then interested in causal effects $\tau_{it} = \mathbb{E}[Y_{it} - Y_{it}(0)]$ on the treated observations $it \in \Omega_1$. We suppose a researcher is interested in a statistic which sums or averages treatment effects $\tau = (\tau_{it})_{it \in \Omega_1}$ over the set of treated observations with pre-specified non-stochastic weights $w_1 = (w_{it})_{it \in \Omega_1}$ that can depend on treatment assignment and timing, but not on realized outcomes:

Estimation Target. $\tau_w = \sum_{it \in \Omega_1} w_{it} \tau_{it} \equiv w_1' \tau$.

For notation brevity we consider scalar estimands.

Different weights are appropriate for different research questions. The researcher may be interested in the overall ATT, formalized by $w_{it} = 1/N_1$ for all $it \in \Omega_1$. In “event study” analyses a common estimand is the average effect h periods since treatment for a given “horizon” $h \geq 0$: $w_{it} = \mathbf{1}[K_{it} = h] / |\Omega_{1,h}|$ for $\Omega_{1,h} = \{it : K_{it} = h\}$. Our approach also allows researchers to specify target estimands that place unequal weights on units within the same cohort-by-horizon cell. For example, one may be interested in weighting units by their size, or in estimating a “balanced” version of horizon-average effects: the average treatment effects at horizon h computed only for the subset of units also observed at horizon h' , such that the gap between two or more estimates is not confounded by compositional differences. Finally, we do not require the w_{it} to add up to one; for example, a researcher may be interested in the difference between average treatment effects at different horizons or across some groups of units (e.g. women and men), corresponding to $\sum_{it \in \Omega_1} w_{it} = 0$.¹⁰

To identify τ_w , we consider three assumptions. We start with the parallel trends assumption, which imposes a two-way fixed effect (TWFE) model on the untreated potential outcomes.

Assumption 1 (Parallel trends). *There exist non-stochastic α_i and β_t such that $\mathbb{E}[Y_{it}(0)] = \alpha_i + \beta_t$ for all $it \in \Omega$.*¹¹

An equivalent formulation requires $\mathbb{E}[Y_{it}(0) - Y_{it'}(0)]$ to be the same across units i for all periods t and t' (whenever it and it' are observed).

Parallel trend assumptions are standard in DiD designs, but their details may vary.¹² First, our framework extends immediately to richer models of $Y_{it}(0)$, which may include time-varying controls, unit-specific trends, or additional fixed effects (see Assumption 1' below). Similarly, it applies in settings where unit FEs are not appropriate, as with repeated cross-sections, or even if the data

¹⁰We note that in practice researchers are not always explicit about their estimand of interest and may be willing to work with any “reasonable” average of treatment effects, informally understood (e.g., no negative weights). In Section 3 we will show why specifying the estimand (or perhaps a class of estimands) is important.

¹¹In estimation, we will set the fixed effect of either one unit or one period to zero, such as $\beta_1 = 0$. This is without loss of generality, since the TWFE model is otherwise over-parameterized.

¹²A recent line of work by Athey and Imbens (2018) and Roth and Sant’Anna (2022) develops methods for causal inference in staggered adoption designs which do not require parallel trends, but are instead based on randomness of the event date. Sant’Anna and Zhao (2020), Arkhangelsky and Imbens (2019), and Arkhangelsky et al. (2021) consider double-robust estimation of treatment effects in panel data that simultaneously estimates an outcomes model and a propensity-score model. Rambachan and Roth (2020) instead consider a relaxed version of Assumption 1 which imposes bounds on non-parallel trends and leads to set identification of causal effects. Arkhangelsky et al. (2020) relax the parallel-trend assumption in a difference-in-differences setting by assuming it only holds relative to a weighted average of control units, where weights can be learned from the data.

do not have a panel structure; see Section 5. Second, we impose the TWFE model on the entire sample. Although weaker assumptions can be sufficient for identification of τ_w (e.g., Callaway et al., 2021), those alternative restrictions depend on the realized treatment timing. Since parallel trends is an assumption on *potential* outcomes, we prefer its stronger version which can be made *a priori*.¹³ Moreover, Assumption 1 can be tested by using pre-treatment data, while minimal assumptions cannot. Finally, we impose Assumption 1 at the unit level, while sometimes it is imposed on cohort-level averages. Our approach is in line with the practice of including unit, rather than cohort, FEs in DiD analyses and allows us to avoid biases in incomplete panels where the composition of units changes over time. Moreover, we show in Appendix A.11 that, under random sampling and without compositional changes, assumptions on cohort-level averages imply Assumption 1.

To be able to identify α_i and β_t , we next rule out anticipation effects, i.e. the causal effects of being treated in the future on current outcomes (e.g. Abbring and Van den Berg, 2003):

Assumption 2 (No anticipation effects). $Y_{it} = Y_{it}(0)$ for all $it \in \Omega_0$.

It is straightforward to weaken this assumption, e.g. by allowing anticipation for some k periods before treatment: this simply requires redefining event dates to earlier ones. However, some form of this assumption is necessary for DiD identification, as there would be no reference periods for treated units otherwise. Assumptions 1 and 2 together imply that the observed outcomes Y_{it} for untreated observations follow the TWFE model.

Finally, researchers sometimes impose restrictions on causal effects, explicitly or implicitly. For instance, τ_{it} may be assumed to be homogeneous for all units and periods, or only depend on the number of periods since treatment (but be otherwise homogeneous across units and periods), or perhaps to be time-invariant for each unit. We will consider such restrictions as a possible auxiliary assumption:

Assumption 3 (Restricted causal effects). $B\tau = 0$ for a known $M \times N_1$ matrix B of full row rank.

It will be more convenient for us to work with an equivalent formulation of Assumption 3, based on $N_1 - M$ free parameters driving treatment effects rather than M restrictions on them:

Assumption 3' (Model of causal effects). $\tau = \Gamma\theta$, where θ is a $(N_1 - M) \times 1$ vector of unknown parameters and Γ is a known $N_1 \times (N_1 - M)$ matrix of full column rank.

Assumption 3' imposes a parametric model of treatment effects. For example, the assumption that treatment effects all be the same, $\tau_{it} \equiv \theta_1$, corresponds to $N_1 - M = 1$ and $\Gamma = (1, \dots, 1)'$. In contrast, a “null model” $\tau_{it} \equiv \theta_{it}$ that imposes no restrictions is captured by $M = 0$ and $\Gamma = \mathbb{I}_{N_1}$.

¹³Specifically, Assumption 4 in Callaway and Sant’Anna (2021) requires that the TWFE model only holds for all treated observations ($D_{it} = 1$), observations directly preceding the treatment onset ($E_i = -1$), and in all periods for never-treated units. Similarly, Goodman-Bacon (2018) proposes to impose parallel trends on a “variance-weighted” average of units, as the weakest assumption under which static OLS estimators we discuss in Section 3 identify some average of causal effects. While technically weaker, this assumption may be hard to justify *ex ante* without imposing parallel trends on all units as it is unlikely that non-parallel trends will cancel out by averaging. See Section 4.5 for further discussion.

If restrictions on the treatment effects are implied by economic theory, imposing them will increase estimation power.¹⁴ Often, however, such restrictions are implicitly imposed without an *ex ante* justification, but just because they yield a simple model for the outcome. We will show in Section 3 how estimators that rely on this assumption can fail to estimate reasonable averages of treatment effects, let alone the specific estimand τ_w , when the assumption is violated. We therefore view the null Assumption 3 can be viewed as a conservative default. We note, however, that this makes the assumptions inherently asymmetric in that they impose restrictive models on potential control outcomes $Y_{it}(0)$ (Assumption 1) but not on treatment effects τ_{it} . This asymmetry reflects the standard practice in staggered rollout DiD designs and is natural when the structure of treatment effects is *ex ante* unknown, while our framework also accommodates the case where the researcher is willing to impose such structure.

3 Challenges with Conventional Practice

In this section, we first present the Ordinary Least Squares (OLS) regressions with two-way fixed effects that have traditionally been used in DiD designs. We then discuss several estimation challenges that are unaddressed in these specifications, including underidentification of dynamic causal effects, negative weighting, and spurious identification of long-run causal effects.

3.1 Conventional OLS Specifications in Staggered Adoption DiD

Causal effects in staggered adoption DiD designs have traditionally been estimated via Ordinary Least Squares (OLS) regressions with two-way fixed effects. While details may vary, the following specification covers many studies:

$$Y_{it} = \tilde{\alpha}_i + \tilde{\beta}_t + \sum_{\substack{h=-a \\ h \neq -1}}^{b-1} \tau_h \mathbf{1}[K_{it} = h] + \tau_{b+} \mathbf{1}[K_{it} \geq b] + \tilde{\varepsilon}_{it}, \quad (2)$$

Here $\tilde{\alpha}_i$ and $\tilde{\beta}_t$ are the unit and period (“two-way”) fixed effects, $a \geq 0$ and $b \geq 0$ are the numbers of included “leads” and “lags” of the event indicator, respectively, and $\tilde{\varepsilon}_{it}$ is the error term. The first lead, $\mathbf{1}[K_{it} = -1]$, is often excluded as a normalization, while the coefficients on the other leads (if present) are interpreted as measures of “pre-trends,” and the hypothesis that $\tau_{-a} = \dots = \tau_{-2} = 0$ is tested visually or statistically. Conditionally on this test passing, the coefficients on the lags are interpreted as a dynamic path of causal effects: at $h = 0, \dots, b-1$ periods after treatment and, in the case of τ_{b+} , at longer horizons binned together.¹⁵ We will refer to this specification as “*dynamic*” (as long as $a + b > 0$) and, more specifically, “*fully-dynamic*” if it includes all available leads and lags except $h = -1$, or “*semi-dynamic*” if it includes all lags but no leads.

¹⁴In some cases τ_w may even not be identified without such restrictions, while it is identified with them.

¹⁵Schmidheiny and Siegloch (2020) discuss the issues that arise when the bin $\mathbf{1}[K_{it} \geq b]$ is not included. Other restrictions on coefficients are also sometimes used: Broda and Parker (2014), for instance, bin groups of four consecutive horizons together, requiring $\tau_0 = \dots = \tau_3$, $\tau_4 = \dots = \tau_7$, etc. (see Section 6.2).

Viewed through the lens of the Section 2 framework, these specifications make implicit assumptions on potential outcomes, anticipation and treatment effects, and the estimand of interest. First, they make Assumption 1 but, for $a > 0$, do not fully impose Assumption 2, allowing for anticipation effects for a periods before treatment.¹⁶ Typically this is done as a means to *test* Assumption 2 rather than to *relax* it, but the resulting specification is the same.

Second, Equation (2) imposes strong restrictions on causal effect heterogeneity (Assumption 3), with treatment (and anticipation) effects assumed to only vary by horizon h and not across units and periods otherwise. Most often, this is done without an *a priori* justification. If the lags are binned into the term with τ_{b+} , the effects are further assumed to be time-invariant once b periods have elapsed since the event.

Finally, dynamic specifications do not explicitly define the estimands τ_h as particular averages of heterogeneous causal effects, even though researchers often admit that the effects may indeed vary across observations. Instead, the OLS coefficients for τ_h are interpreted as averages of causal effects for horizon h with unspecified weights, which are presumed to be reasonable (e.g., weighted by some treatment variance, as in the case of OLS with saturated controls; Angrist (1998)).

Besides dynamic specifications, Equation (2) also nests a very common specification used when a researcher is interested in a single parameter summarizing all causal effects. With $a = b = 0$, we have the “*static*” specification in which a single treatment indicator is included:

$$Y_{it} = \tilde{\alpha}_i + \tilde{\beta}_t + \tau^{\text{static}} D_{it} + \tilde{\varepsilon}_{it}. \quad (3)$$

In line with our Section 2 setting, the static equation imposes the parallel trends and no anticipation Assumptions 1 and 2. However, it also makes a particularly strong version of Assumption 3 — that all treatment effects are the same. Moreover, the target estimand is again not written out as an explicit average of potentially heterogeneous causal effects.

In the rest of this section we turn to the challenges associated with OLS estimation of Equations (2) and (3). We explain how these issues result from the conflation of the target estimand, Assumption 2 and Assumption 3, providing a new and unified perspective on the problems of static and dynamic OLS-based methods. Specifically, in Sections 3.2, 3.3 and 3.4 we focus on three challenges: underidentification of the fully-dynamic regression, negative weighting in the static regression, and spurious identification of the long-run effects. To the best of our knowledge, our paper is the first and only one to characterize the underidentification and spurious identification issues. The negative weighting issue was originally identified in de Chaisemartin and D’Haultfœuille (2015, Supplement 1). It was pointed out independently in the earlier manuscript of our paper (Borusyak and Jaravel, 2017), which also provided an intuitive framework to understand the issue in terms of “forbidden comparisons”. We present this intuition below and also derive novel necessary and sufficient conditions for this problem to arise (Proposition 4). In Section 3.5, we discuss how our framework also relates to other problems that have since been pointed out by Roth (2019) and Sun and Abraham

¹⁶One can alternatively view this specification as imposing Assumption 2 but making a weaker Assumption 1 which includes some pre-trends into $Y_{it}(0)$. This difference in interpretation is immaterial for our results.

(2021).

3.2 Under-Identification of the Fully-Dynamic Specification

The first problem pertains to fully-dynamic specifications and arises because a strong enough Assumption 2 is not imposed. We show that those specifications are under-identified if there is no never-treated group:

Proposition 1. *If there are no never-treated units, the path of $\{\tau_h\}_{h \neq -1}$ coefficients is not point identified in the fully-dynamic OLS specification. In particular, for any $\kappa \in \mathbb{R}$, the path $\{\tau_h + \kappa(h + 1)\}$ fits the data equally well, with the fixed effect coefficients appropriately modified.*

Proof. All proofs are given in Appendix B. □

This result can be illustrated with a simple example, which we intentionally make extreme and free from additional effects and noise. Consider Figure 1, which plots the outcomes for a simulated dataset with two units (which can be understood as cohort averages): one treated early at $t = 2$ (solid line) and the other one later at $t = 4$ (dashed line). Both units are observed for periods $t = 1, \dots, 7$, and the outcomes exhibit linear growth with the same slope of one, but starting from different levels. There are two interpretations of what could cause such dynamics. On one hand, treatment could have no impact on the outcome, in which case the level difference corresponds to the unit FEs, while trends are just a common feature of the environment, formalized by the period FEs. On the other hand, note that the outcome equals the number of periods since the event for both groups and all time periods: it is zero at the moment of treatment, negative before and positive after. So a possible interpretation is that the outcome is entirely driven by causal effects of treatment and anticipation of treatment. One cannot hope to distinguish between unrestricted dynamic causal effects and a combination of unit effects and time trends.¹⁷

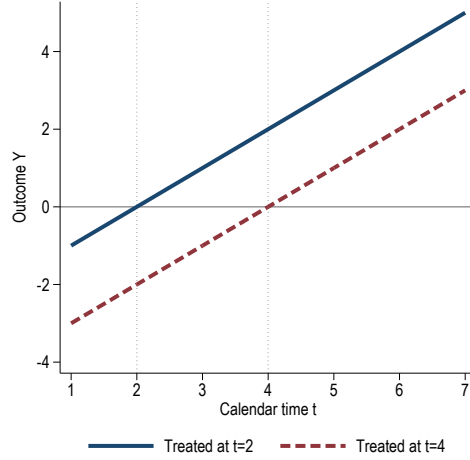
Formally, the problem arises because a linear time trend t and a linear term in the cohort E_i (subsumed by the unit FEs) can perfectly reproduce a linear term in relative time $K_{it} = t - E_i$. Therefore, a complete set of treatment leads and lags, which is equivalent to the FE of relative time, is collinear with the unit and period FEs.¹⁸

The problem may be important in practice, as statistical packages may resolve this collinearity by dropping an arbitrary unit or period indicator. Some estimates of $\{\tau_h\}$ would then be produced, but because of an arbitrary trend in the coefficients they may suggest a violation of parallel trends even when the specification is in fact correct, i.e. Assumptions 1 and 2 hold and there is no heterogeneity of treatment effects for each horizon (Assumption 3).

¹⁷Note that in the former case, the solid line is a vertical shift up (a level shift) from the dashed line, while in the latter case it is a horizontal shift to the left that is due to the differential timing. With straight lines, these are observationally equivalent.

¹⁸The mechanics of this issue are essentially the same as in the well-known “age-cohort-time” problem, where the set of units treated at a given time $E_i = e$ can be viewed as a birth cohort and the relative time $K_{it} = t - E_i$ serves as age.

Figure 1: Underidentification of Fully-Dynamic Specification



To break the collinearity problem, stronger restrictions on anticipation effects, and thus on Y_{it} for untreated observations, have to be introduced. One could consider imposing minimal restrictions on the specification that would make it identified. In typical cases, only a linear trend in $\{\tau_h\}$ is not identified in the fully dynamic specification, while nonlinear paths cannot be reproduced with unit and period fixed effects. Therefore, just one additional normalization, e.g. $\tau_{-a} = 0$ in addition to $\tau_{-1} = 0$, breaks multicollinearity.¹⁹

However, minimal identified models rely on *ad hoc* identification assumptions which are *a priori* unattractive. For instance, just imposing $\tau_{-a} = \tau_{-1} = 0$ means that anticipation effects are assumed away 1 and a periods before treatment, but not in other pre-periods. This assumption therefore depends on the realized event times. Instead, a systematic approach is to impose the assumptions — some forms of no anticipation effects and parallel trends — that the researcher has an *a priori* argument for and which motivated the use of DiD to begin with. Such assumptions on anticipation effects give much stronger identification power relative to the unnecessarily flexible specifications, of which the fully-dynamic specification is an extreme example. Importantly, our suggestion to impose identification assumptions at the estimation stage does not mean that those assumptions should not also be tested; we discuss testing in detail in Section 4.4 below. Rather, the separation of estimation and testing makes the identification argument explicit, while stronger-than-minimal assumptions allow for more powerful testing.

3.3 Negative Weighting in the Static Regression

We now show how, by imposing Assumption 3 instead of specifying the estimation target, the static TWFE specification does not identify a reasonably-weighted average of heterogeneous treatment

¹⁹There are some exceptions in which additional collinearity arises, e.g. when treatment is staggered but happens at periodic intervals.

Table 1: Two-Unit, Three-Period Example

$\mathbb{E}[Y_{it}]$	$i = A$	$i = B$
$t = 1$	α_A	α_B
$t = 2$	$\alpha_A + \beta_2 + \tau_{A2}$	$\alpha_B + \beta_2$
$t = 3$	$\alpha_A + \beta_3 + \tau_{A3}$	$\alpha_B + \beta_3 + \tau_{B3}$
Event date	$E_i = 2$	$E_i = 3$

Notes: without loss of generality, we normalize $\beta_1 = 0$.

effects. The underlying weights may be negative, particularly for the long-run causal effects.²⁰ Although we focus on the static specification here, the same issues arise in various specifications that bin multiple lags together.

First, we note that, if the parallel trends and no anticipation assumptions hold, static OLS identifies *some* weighted average of treatment effects:²¹

Proposition 2. *If Assumptions 1 and 2 hold, then the estimand of the static OLS specification in (3) satisfies $\tau^{static} = \sum_{it \in \Omega_1} w_{it}^{OLS} \tau_{it}$ for some weights w_{it}^{OLS} that do not depend on the outcome realizations and add up to one, $\sum_{it \in \Omega_1} w_{it}^{OLS} = 1$.*

The underlying weights w_{it}^{OLS} can be easily computed from the data using the Frisch–Waugh–Lovell theorem (see Equation (18) in the proof of Proposition 2) and only depend on the timing of treatment for each unit and the set of observed units and periods.

The static OLS estimand, however, cannot be interpreted as a *proper* weighted average, as some weights can be negative. We illustrate this problem with a simple example:

Proposition 3. *Suppose Assumptions 1 and 2 hold and the data consist of two units (or equal-sized cohorts), A and B , treated in periods 2 and 3, respectively, both observed in periods $t = 1, 2, 3$ (as shown in Table 1). Then the estimand of the static OLS specification (3) can be expressed as $\tau^{static} = \tau_{A2} + \frac{1}{2}\tau_{B3} - \frac{1}{2}\tau_{A3}$.*

This example illustrates the severe short-run bias of TWFE OLS: the long-run causal effect, corresponding to the early-treated unit A and the late period 3, enters with a negative weight ($-1/2$). Thus, the larger the effects are in the long-run, the smaller the coefficient will be.

This problem results from what we call “forbidden comparisons” performed by OLS. Recall that the original idea of DiD estimation is to compare the evolution of outcomes over some time interval for the units which got treated during that interval relative to a reference group of units which

²⁰Since de Chaisemartin and D’Haultfœuille (2015) and our earlier draft pointed out this issue, it has been extensively studied. In a special case of complete panels, Strezhnev (2018) and Goodman-Bacon (2018) provide two decompositions of the static regression’s estimand, while de Chaisemartin and D’Haultfœuille (2020) characterize the weights analytically. We complement this discussion by attributing the fundamental cause of the problem to the conflation of target and assumptions delineated in our framework from Section 2. Early insights into this problem in specific applications can be found in Wolfers (2006) and Meer and West (2016).

²¹This result extends Theorem 1 in de Chaisemartin and D’Haultfœuille (2020) to incomplete panels and restates the Appendix C result in Borusyak and Jaravel (2017).

didn't, identifying the period FEs. In the Proposition 3 example, such an "admissible" comparison is between units A and B in periods 2 and 1, $(Y_{A2} - Y_{A1}) - (Y_{B2} - Y_{B1})$. However, panels with staggered treatment timing also lend themselves to a second type of comparisons — which we label forbidden — in which the reference group has been treated throughout the relevant period. For units in this group, the treatment indicator D_{it} does not change over the relevant period, and so OLS uses them to identify period FEs, too. The comparison between units B and A in periods 3 and 2, $(Y_{B3} - Y_{B2}) - (Y_{A3} - Y_{A2})$, in Proposition 3 is a case in point. While a comparison like this is appropriate and increases efficiency when treatment effects are homogeneous (which OLS was designed for), forbidden comparisons are problematic under treatment effect homogeneity. For instance, subtracting $(Y_{A3} - Y_{A2})$ not only removes the gap in period FEs, $\beta_3 - \beta_2$, but also deducts the evolution of treatment effects $\tau_{A3} - \tau_{A2}$, placing a negative weight on τ_{A3} . OLS leverages comparisons of both types and estimates the treatment effect by $\hat{\tau}^{static} = (Y_{B2} - Y_{A2}) - \frac{1}{2}(Y_{B1} - Y_{A1}) - \frac{1}{2}(Y_{B3} - Y_{A3})$.²²

Fundamentally, this problem arises because OLS estimation imposes very strong restrictions on treatment effect homogeneity, i.e. Assumption 3, instead of acknowledging the heterogeneity and specifying a particular target estimand (or perhaps a class of estimands that the researcher is indifferent between). Such conflation is likely a consequence of the common perception that regression estimators generally recover reasonably-weighted averages of treatment effects. Even if one gets some variance-weighted average instead of the policy-relevant ATT, the benefit of convenience may dominate. This perception is correct in regressions with saturated controls, as shown by the seminal paper of Angrist (1998). However, it fails in staggered adoption DiD designs where the set of controls (i.e. unit and period FEs) is non-saturated and complex.

With a large number of never-treated units or a large number of periods before any unit is treated (relative to other units and periods), our setting becomes closer to a classical non-staggered DiD design, and therefore negative weights disappear, as our next result illustrates:²³

Proposition 4. *Suppose all units are observed for all periods $t = 1, \dots, T$ and the earliest treatment happens at $E_{first} > 1$. Let N_1^* be the number of observations for never-treated units before period E_{first} and N_0^* be the number of untreated observations for ever-treated units since E_{first} . Then there is no negative weighting, i.e. $\min_{it \in \Omega_1} w_{it}^{OLS} \geq 0$, if and only if $N_1^* \geq N_0^*$.*

Even when weights are non-negative, they may remain highly unequal and diverge from the estimands that the researcher is interested in. Our preferred strategy is therefore to commit to the estimation target and explicitly allow for treatment effect heterogeneity, except when some form of Assumption 3 is *ex ante* appropriate.

²²The proof of Proposition 2 shows why long-run effects in particular are subject to the negative weights problem. In general, negative weights arise for the treated observations, for which the residual from an auxiliary regression of D_{it} on the two-way FEs is negative. de Chaisemartin and D'Haultfoeuille (2020) show that, in complete panels, the unit FEs are higher for early-treated units (which are observed treated for a larger shares of periods) and period FEs are higher for later periods (in which a larger shares of units are treated). The early-treated units observed in later periods correspond to the long-run effects.

²³ N_1^* and N_0^* respectively correspond to the numbers of admissible and forbidden 2x2 DiD comparisons available for the earliest-treated units in the latest period T . The gap between them drives negative weights with complete panels, as in Strezhnev (2018, Proposition 1).

3.4 Spurious Identification of Long-Run Causal Effects in Dynamic Specifications

Another consequence of inappropriately imposing Assumption 3 concerns estimation of long-run causal effects. Dynamic OLS specifications (except those subject to the underidentification problem) yield *some* estimates for all τ_h coefficients. Yet, for large enough h , no averages of treatment effects are identified under Assumptions 1 and 2 with unrestricted treatment effect heterogeneity. Therefore, OLS estimates are fully driven by unwarranted extrapolation of treatment effects across observations and may not be reliable, unless strong *ex ante* reasons for Assumption 3 exist.

This issue is well illustrated in the example of Proposition 3. To identify the long-run effect τ_{A3} under Assumptions 1 and 2, one needs to form an admissible DiD comparison, of the outcome growth over some period between unit A and another unit not yet treated in period 3. But by period 3 both units have been treated. Mechanically, this problem arises because the period fixed effect β_3 is not identified separately from the treatment effects τ_{A3} and τ_{B3} in this example, absent restrictions on treatment effects. Yet, the dynamic OLS specification

$$Y_{it} = \tilde{\alpha}_i + \tilde{\beta}_t + \tau_0 \mathbf{1}[t = E_i] + \tau_1 \mathbf{1}[t = E_i + 1] + \tilde{\varepsilon}_{it}$$

will produce an estimate $\hat{\tau}_1$ via extrapolation. Specifically, two different parameters, $\tau_{A3} - \tau_{B3}$ and τ_{A2} , are identified by comparing the two units in periods 2 or 3, respectively, with period 1. Therefore, OLS that imposes the homogeneity of short-run effects across units, $\tau_{A2} = \tau_{B3} \equiv \tau_0$, estimates the long-run effect $\tau_{A3} \equiv \tau_1$ as the sum of $\tau_1 - \tau_0$ and τ_0 :

$$\hat{\tau}_1 = [(Y_{A3} - Y_{A1}) - (Y_{B3} - Y_{B1})] + [(Y_{A2} - Y_{A1}) - (Y_{B2} - Y_{B1})].$$

However, when $\tau_{A2} \neq \tau_{B3}$, this estimator is biased.

In general, the gap between the earliest and the latest event times observed in the data puts an upper bound on the number of dynamic coefficients that can be identified without extrapolation of treatment effects. This result, which follows by the same logic of non-identification of the later period effects, is formalized by our next proposition:

Proposition 5. *Suppose there are no never-treated units and let $\bar{H} = \max_i E_i - \min_i E_i$. Then, for any non-negative weights w_{it} defined over the set of observations with $K_{it} \geq \bar{H}$ (that are not identically zero), the weighted sum of causal effects $\sum_{it: K_{it} \geq \bar{H}} w_{it} \tau_{it}$ is not identified by Assumptions 1 and 2.²⁴*

This result applies to all estimators, not just those based on OLS. However, robust estimators, including the one we characterize in Section 4, would not be possible to compute for non-identified estimands, never resulting in spurious estimates.

²⁴The requirement that the weights are non-negative rules out some estimands on the *gaps* between treatment effects which are in fact identified. For instance, adding period $t = 4$ to the Table 1 example, the difference $\tau_{A4} - \tau_{B4}$ would be identified (by $(Y_{A4} - Y_{B4}) - (Y_{A1} - Y_{B1})$), even though neither τ_{A4} nor τ_{B4} is.

3.5 Other Limitations of Conventional Practice

We finally discuss three limitations of conventional practice pointed out in other work. We use our framework to connect these issues to the conflation of assumptions about treatment effect heterogeneity and parallel trends.

First, Roth (2019) uncovers undesirable consequences of conditioning on a pre-trend test passing. He shows that OLS pre-trend estimates are correlated with the estimates of treatment effects obtained from the same dynamic specification. As a consequence, when Assumptions 1 and 2 are satisfied, pre-trend testing makes statistical inference on the coefficients invalid; if the assumptions are instead violated, this can exacerbate estimation bias. In Section 4.4 we will show that this problem can be attributed to the fact that Assumption 2 is not imposed when estimating the treatment effects by dynamic OLS specifications, and testing is not separated from estimation—similarly to the underidentification problem discussed above.

Second, Sun and Abraham (2021) show that the negative weighting problem from Section 3.3 is also relevant in dynamic OLS specifications. Specifically, causal estimates for one horizon may be confounded by heterogeneous effects across treatment cohorts at other horizons.²⁵ This may not be surprising given that long-run effects are not identified absent Assumption 3, and thus dynamic OLS coefficients have to be based on extrapolations of treatment effects. In light of our framework, this problem arises because Assumption 3 is imposed instead of specifying a target estimand.

Finally, Sun and Abraham (2021) further show that conventional pre-trend tests may reject Assumption 2 when the assumption is satisfied or pass when the assumption fails, all because of heterogeneous treatment effects. That problem is another variation on how the Section 2 assumptions may be conflated: the conventional pre-trend test for Assumptions 1 and 2 is in fact a joint test of those assumptions together with implicit Assumption 3 and can reject because of the latter.

3.6 Similar Problems Persist When Trimming around Event Times

We finally point out that the challenges above apply even if the sample is “trimmed” around the event time. By trimming, we mean the relatively common practice (sometimes also referred to as “balancing”) of dropping observations more than a periods before or b periods after the event, for some $a > 0$ and $b \geq 0$ (e.g. Miller (2017), Bartik and Nelson (2021)).²⁶

One may think that the static and dynamic TWFE OLS estimands may be close to their desirable targets in trimmed samples because by construction the composition of units is unchanged across horizons (i.e., across all leads and lags of the event). However, we show that, with staggered adoption, the weights implied by TWFE regressions remain complex and highly skewed. Intuitively, this follows because trimmed panels are necessarily unbalanced in terms of units and periods; for

²⁵This issue is distinct from the one of Section 3.3, as it stems from the heterogeneity of treatment effects across cohorts and periods for a given horizon, rather than from the heterogeneity across horizons, which is explicitly allowed for in the dynamic specifications.

²⁶One scenario in which trimming is justified within our framework is when Assumptions 1' and 2 are imposed only on observations within a certain window of the event only.

instance, the composition of units varies across periods by construction. Perhaps surprisingly, OLS estimands can even have worse properties in trimmed samples.

To illustrate how the challenges of OLS persist with trimming — and can be made worse — in Appendix A.1 we present a numerical example. In the example, negative weighting in the static two-way fixed-effect OLS regression is even more severe after trimming. Similarly, weights implied by the dynamic specification with trimming show an even larger skew than without trimming. Finally, trimming can exacerbate the issue of spurious identification of long-run effects. The costs of trimming — in particular from reducing the sample size and thus estimation efficiency — are therefore difficult to justify.

In sum, Section 3 has shown that OLS estimation of both static and dynamic specifications suffers from a host of challenges, which all arise from the conflation of the target estimand, Assumptions 1, 2 and 3. Importantly, these problems pertain only to OLS estimation of event studies, not to the event study design itself, as we show next.

4 Imputation-Based Estimation and Testing

To overcome the challenges of conventional practice we now derive the robust and efficient estimator, and show that it takes a particularly transparent “imputation” form when no restrictions on treatment-effect heterogeneity are assumed. We then perform asymptotic analysis, establishing the conditions for the estimator to be consistent and asymptotically Normal, deriving conservative standard error estimates for it, and discussing appropriate pre-trend tests. Next, we compare the imputation estimator with other methods recently proposed in response to some problems with OLS estimation. We conclude the section by providing simulation evidence that the efficiency gains from using our estimator are sizable, that its sensitivity to some parallel trend violations is no larger than that of the alternatives, and that our inference tools perform well in finite samples.

Our theoretical results apply in a class of models more general than those that satisfy Assumption 1. We therefore relax it to have:

Assumption 1' (General model of $Y(0)$). *For all $it \in \Omega$, $\mathbb{E}[Y_{it}(0)] = A'_{it}\lambda_i + X'_{it}\delta$, where λ_i is a vector of unit-specific nuisance parameters, δ is a vector of nuisance parameters associated with common covariates, and A_{it} and X_{it} are known non-stochastic vectors.*

The first term in this model of $Y_{it}(0)$ nests unit FEs, but also allows to interact them with some observed covariates unaffected by the treatment status, e.g. to include unit-specific trends.²⁷ The second term nests period FEs but additionally allows any time-varying covariates, i.e. $X'_{it}\delta = \beta_t + \tilde{X}'_{it}\tilde{\delta}$ (again, for the covariates unaffected by the treatment). We note that Assumption 1' looks similar to a factor model, but differs crucially in that regressors A_{it} are observed.

²⁷Without further restrictions on $X'_{it}\delta$, there is redundancy in this formulation. However, we will impose such restrictions when considering the asymptotic behavior of the estimator below. We note that Assumption 1' also nests specifications that exclude unit or period FEs.

As previously, we suppose that a researcher has chosen estimation target τ_w and made Assumption 2. Some model of treatment effects (Assumption 3) is also assumed, although our main focus is on the null model, under which treatment effect heterogeneity is unrestricted. Letting $\varepsilon_{it} = Y_{it} - \mathbb{E}[Y_{it}]$ for $it \in \Omega$, we thus have under Assumptions 1', 2 and 3':

$$Y_{it} = A'_{it}\lambda_i + X'_{it}\delta + D_{it}\Gamma'_{it}\theta + \varepsilon_{it}. \quad (4)$$

We assume throughout that the model is parameterized in a way that all parameters are identified.²⁸

4.1 Efficient Estimation

For our efficiency result we impose an additional homoskedasticity assumption:

Assumption 4 (Homoskedastic errors). *Error terms ε_{it} are homoskedastic and mutually uncorrelated across all $it \in \Omega$: $\mathbb{E}[\varepsilon\varepsilon'] = \sigma^2\mathbb{I}_N$.*

While this assumption is strong, our efficiency results also apply without change under dependence that is due to unit random effects, i.e. if $\varepsilon_{it} = \eta_i + \tilde{\varepsilon}_{it}$ for $\tilde{\varepsilon}_{it}$ that satisfy Assumption 4 and for some η_i . Moreover, these results are straightforward to relax to any known form of heteroskedasticity or mutual dependence.²⁹ Under Assumption 4 and allowing for restrictions on causal effects, we have:

Theorem 1 (Efficient estimator). *Suppose Assumptions 1', 2, 3' and 4 hold. Then among the linear unbiased estimators of τ_w , the (unique) efficient estimator $\hat{\tau}_w^*$ can be obtained with the following steps:*

1. *Estimate θ by $\hat{\theta}$ from the linear regression (4) (where we assume that θ is identified);*
2. *Estimate the vector of treatment effects τ by $\hat{\tau} = \Gamma\hat{\theta}$;*
3. *Estimate the target τ_w by $\hat{\tau}_w^* = w'_1\hat{\tau}$.*

Moreover, this estimator $\hat{\tau}_w^$ is unbiased for τ_w under Assumptions 1', 2 and 3' alone, even when error terms are not homoskedastic.*

Under Assumptions 1', 2 and 3', regression (4) is correctly specified. Thus, this estimator for θ is unbiased by construction, and efficiency under homoskedasticity of error terms is a direct consequence of the Gauss–Markov theorem. Moreover, OLS yields the most efficient estimator for any linear combination of θ , including $\tau_w = w'_1\Gamma\theta$. While assuming homoskedasticity may be unrealistic in practice, we think of this assumption as a natural benchmark to decide between the many unbiased

²⁸Ensuring that all parameter are identified may involve restricting some of them. For example, in the baseline case of two-way fixed effects we can set the fixed effect of either one unit or one period to zero, since the model is otherwise over-parameterized.

²⁹For instance, if error terms are independent with known variances σ_{it}^2 , efficiency requires the estimation step (Step 1) of Theorems 1 and 2 to be performed with weights proportional to σ_{it}^{-2} . One example for this is when the data are aggregated from n_{it} individuals randomly drawn from group i in period t and homoskedastic individual-level errors, in which case efficiency is obtained with weights proportional to n_{it} .

estimators of τ_w . Theorem 1 also assumes that the parameter vector θ is identified in the regression model in (4). This, for instance, rules out estimation of long-run treatment effects (absent strong restrictions on treatment effects), in line with Proposition 5.³⁰

In the important special case of unrestricted treatment effect heterogeneity, $\hat{\tau}_w^*$ has a useful “imputation” representation. The idea is to estimate the model of $Y_{it}(0)$ using the untreated observations $it \in \Omega_0$ and extrapolate it to impute $Y_{it}(0)$ for treated observations $it \in \Omega_1$. Then, observation-specific causal effect estimates can be averaged appropriately. Perhaps surprisingly, the estimation and imputation steps are identical regardless of the target estimand. Applying any weights to the imputed causal effects yields the efficient estimator for the corresponding estimand. We have:

Theorem 2 (Imputation representation for the efficient estimator). *With a null Assumption 3 (that is, if $\Gamma = \mathbb{I}_{N_1}$), the unique efficient linear unbiased estimator $\hat{\tau}_w^*$ of τ_w from Theorem 1 can be obtained via an imputation procedure:*

1. *Within the untreated observations only ($it \in \Omega_0$), estimate the λ_i and δ (by $\hat{\lambda}_i, \hat{\delta}$) by OLS in the regression (where we assume there is no perfect multicollinearity)*

$$Y_{it}(0) = A'_{it}\lambda_i + X'_{it}\delta + \varepsilon_{it}; \quad (5)$$

2. *For each treated observation ($it \in \Omega_1$) with $w_{it} \neq 0$, set $\hat{Y}_{it}(0) = A'_{it}\hat{\lambda}_i + X'_{it}\hat{\delta}$ and $\hat{\tau}_{it} = Y_{it} - \hat{Y}_{it}(0)$ to obtain the estimate of τ_{it} ;*
3. *Estimate the target τ_w by a weighted sum $\hat{\tau}_w^* = \sum_{it \in \Omega_1} w_{it}\hat{\tau}_{it}$.*

The “imputation” structure of the estimator is related to the “direct estimation of the counterfactual” form considered by Gobillon and Magnac (2016) for linear factor models. In the special case of two-way fixed effects as Assumption 1', the estimator from this theorem yields the “counterfactual” and “two-stage” estimators proposed in independent work by Liu et al. (2020) and Gardner (2021) for the specific choices of the weights w_1 they consider, and can in this case also be understood as a matrix-completion estimator in the framework of Athey et al. (2021) without a factor model or regularization. Here, we derive a general imputation estimator for arbitrary estimands as the unique efficient linear estimator in our setting.

The imputation representation offers computational and conceptual benefits. First, it is computationally efficient as it only requires estimating a simple TWFE model, for which fast algorithms are available (Guimarães and Portugal, 2010; Correia, 2017). This is in contrast to the OLS estimator from Theorem 1, as Equation (4) has regressors $\Gamma_{it}D_{it}$ in addition to the fixed effects, which are high-dimensional unless a low-dimensional model of treatment effect heterogeneity is imposed.

Second, the imputation approach is intuitive and transparently links the parallel trends and no anticipation assumptions to the estimator. Indeed, Imbens and Rubin (2015) write: “At some level,

³⁰We note that for the Theorem 1 result we do not necessarily need to identify all fixed effects in $Y_{it}(0)$ separately; it is sufficient that the fitted value $\mathbb{E}[Y_{it}(0)] = A'_{it}\lambda_i + X'_{it}\delta$ is identified. For example, we could shift all unit FEs up and all period FEs down by the same constant, but this change is immaterial for the fitted values.

all methods for causal inference can be viewed as imputation methods, although some more explicitly than others” (p. 141). We formalize this statement in the next proposition, which shows that *any* estimator unbiased for τ_w can be represented in the imputation way, but their way of imputing the $Y_{it}(0)$ may be less explicit and no longer efficient.

Proposition 6 (Imputation representation for all unbiased estimators). *Under Assumptions 1' and 2, any unbiased linear estimator $\hat{\tau}_w$ of τ_w that allows for arbitrary treatment-effect heterogeneity (that is, a null Assumption 3) can be written as an imputation estimator:*

1. *For every treated observation, estimate expected potential untreated outcomes $A'_{it}\lambda_i + X'_{it}\delta$ by some unbiased linear estimator $\hat{Y}_{it}(0)$ using data from the untreated observations only;*
2. *For each treated observation, set $\hat{\tau}_{it} = Y_{it} - \hat{Y}_{it}(0)$;*
3. *Estimate the target by a weighted sum $\hat{\tau}_w = \sum_{it \in \Omega_1} w_{it} \hat{\tau}_{it}$.*

This result establishes an imputation representation when treatment effects can vary arbitrarily. Proposition A1 in the appendix establishes that the imputation structure applies even when restrictions $\tau = \Gamma\theta$ are imposed, albeit with an additional step in which the weights w_1 defining the estimand are adjusted in a way that does not change τ_w under the imposed model. In this sense, unbiased causal inference is equivalent to imputation in our framework.

4.2 Asymptotic Properties

Having derived the linear unbiased estimator $\hat{\tau}_w^*$ for τ_w in Theorem 1 that is also efficient under homoskedastic errors terms, we now consider its asymptotic properties without imposing homoskedasticity. We study convergence along a sequence of panels indexed by the sample size N , where randomness only stems from the error terms ε_{it} , as in Section 2. Our approach applies to asymptotic sequences where both the number of units and the number of time periods may grow, but the assumptions are least restrictive when the number of time periods remains constant or grows slowly, as in short panels. By viewing the set of observations Ω , treatment timing, and all FEs and controls as non-stochastic, we do not have to impose assumptions on the sampling of the weights w_1 themselves, but can take them as given.

Instead of making Assumption 4, that error terms are homoskedastic and uncorrelated, we now assume that error terms are clustered by units i .

Assumption 5 (Clustered error terms). *Error terms ε_{it} are uncorrelated across units and have bounded variance: $\text{Cov}[\varepsilon_{it}, \varepsilon_{js}] = 0$ for $i \neq j$, and $\text{Var}[\varepsilon_{it}] \leq \bar{\sigma}^2$ uniformly.*

The key role in our results is played by the weights that the Theorem 1 estimator places on each observation. Since the estimator is linear in the observed outcomes Y_{it} , we can write it as $\hat{\tau}_w^* = \sum_{it \in \Omega} v_{it}^* Y_{it}$ with non-stochastic weights v_{it}^* , derived in Proposition A2 in the appendix.

We now formulate high-level conditions on the sequence of weight vectors that ensure consistency, asymptotic Normality, and will later allow us to provide valid inference. These results apply to any

unbiased linear estimator $\hat{\tau}_w = \sum_{it \in \Omega} v_{it} Y_{it}$ of τ_w , not just the efficient estimator $\hat{\tau}_w^*$ from Theorem 1 – that is, if the respective conditions are fulfilled for the weights v_{it} , then consistency, asymptotic Normality, and valid inference follow as stated. For the specific estimator $\hat{\tau}_w^*$ introduced above, we then provide sufficient low-level conditions for short panels.

First, we obtain consistency for $\hat{\tau}_w = \sum_{it \in \Omega} v_{it} Y_{it}$ under a Herfindahl condition on the weights v that takes the clustering structure of error terms into account.

Assumption 6 (Herfindahl condition). *Along the asymptotic sequence,*

$$\|v\|_H^2 \equiv \sum_i \left(\sum_{t; it \in \Omega} |v_{it}| \right)^2 \rightarrow 0.$$

The condition on the clustered Herfindahl index $\|v\|_H^2$ states that the sum of squared weights vanishes, where weights are aggregated by units. One can think of the inverse of the sum of squared weights, $n_H = \|v\|_H^{-2}$, as a measure of effective sample size, which Assumption 6 requires to grow large along the asymptotic sequence. If it is satisfied, and variances are uniformly bounded, then the difference between the estimator $\hat{\tau}_w$ and the estimand τ_w vanishes asymptotically, and we obtain consistency:

Proposition 7 (Consistency of $\hat{\tau}_w$). *Under Assumptions 1', 2, 3', 5 and 6, $\hat{\tau}_w - \tau_w \xrightarrow{\mathcal{L}_2} 0$ for an unbiased estimator $\hat{\tau}_w$ of τ_w , such as $\hat{\tau}_w^*$ in Theorem 1.³¹*

We next consider the asymptotic distribution of the estimator around the estimand.

Proposition 8 (Asymptotic Normality). *Under the assumptions of Proposition 7, a balance assumption on higher moments of the weights (Assumption A1), and if $\liminf n_H \sigma_w^2 > 0$ for $\sigma_w^2 = \text{Var}[\hat{\tau}_w]$, we have that*

$$\sigma_w^{-1}(\hat{\tau}_w - \tau_w) \xrightarrow{d} \mathcal{N}(0, 1).$$

This result establishes conditions under which the difference between estimator and estimand is asymptotically Normal. Besides regularity, this proposition requires that the estimator variance σ_w^2 does not decline faster than $1/n_H$. It is violated if the clustered Herfindahl formula is too conservative: for instance, if the number of periods is growing along the asymptotic sequence while the within-unit over-time correlation of error terms remains small. Alternative sufficient conditions for asymptotic Normality can be established in such cases, e.g. along the lines of Footnote 31.

So far, we have formulated high-level conditions on the weights v_{it} of any linear unbiased estimator of τ_w . Appendix A.4 presents low-level sufficient conditions for consistency and asymptotic

³¹The Herfindahl condition can be restrictive since it allows for a worst-case correlation of error terms within units. When such correlations are limited, other sufficient conditions may be more appropriate instead, such as $R(\sum_{it \in \Omega} v_{it}^2) \rightarrow 0$ with $R = \max_i (\text{largest eigenvalue of } \Sigma_i) / \bar{\sigma}^2$, where $\Sigma_i = (\text{Cov}[\varepsilon_{it}, \varepsilon_{is}])_{t,s}$. Here R is a measure of the maximal joint covariation of all observations for one unit. If error terms are uncorrelated, then $R \leq 1$, since the maximal eigenvalue of Σ_i corresponds to the maximal variance of an error term ε_{it} in this case, which is bounded by $\bar{\sigma}^2$. An upper bound for R is the maximal number of periods for which we observe a unit, since the maximal eigenvalue of Σ_i is bounded by the sum of the variances on its diagonal.

Normality of the imputation estimator $\hat{\tau}_w^*$ for the benchmark case of a panel with unit and period fixed effects, a fixed or slowly growing number of periods, and no restrictions on treatment effects. Unlike Propositions 7 and 8, these conditions are imposed directly on the weights w_1 chosen by the researcher, and not on the implied weights v_{it}^* , and are therefore easy to verify. In particular, the estimator achieves consistency and asymptotic Normality in the common case where the number of time periods is fixed, the size of all cohorts increases, and the weights on treatment effects do not vary within the same period and cohort. In addition, the sufficient conditions are also fulfilled when the number of periods grows slowly and when weights differ across observations within the same cohort and period, but not by too much.

4.3 Conservative Inference

Our goal next is to estimate the variance of $\hat{\tau}_w = \sum_{it \in \Omega} v_{it} Y_{it}$, which equals

$$\sigma_w^2 = \mathbb{E} \left[\sum_i \left(\sum_{t; it \in \Omega} v_{it} \varepsilon_{it} \right)^2 \right]$$

with clustered error terms (Assumption 5). We start with the case where treatment effect heterogeneity is unrestricted (i.e. $\Gamma = \mathbb{I}$). As in Section 4.2, the inference tools we propose apply to a generic linear unbiased estimator but we use them for the efficient estimator $\hat{\tau}_w^*$. Our strategy is to estimate individual error terms by some $\tilde{\varepsilon}_{it}$ and then use a plug-in estimator,

$$\hat{\sigma}_w^2 = \sum_i \left(\sum_{t; it \in \Omega} v_{it} \tilde{\varepsilon}_{it} \right)^2. \quad (6)$$

Estimating the error terms presents two challenges, which become apparent when we consider the benchmark choice $\tilde{\varepsilon}_{it} = \hat{\varepsilon}_{it}$ based on the regression residuals $\hat{\varepsilon}_{it} = Y_{it} - A_i' \hat{\lambda}_i - X_{it}' \hat{\delta} - D_{it} \hat{\tau}_{it}$ in the regression (4). The first challenge is that the fixed effects λ_i are not generally estimated consistently (e.g., in short panels). This inconsistency, however, turns out to be inconsequential because the candidate variance estimator $\sum_i \left(\sum_{t; it \in \Omega} v_{it} \hat{\varepsilon}_{it} \right)^2$ is numerically invariant to the underlying $\hat{\lambda}_i$ estimates. This invariance property of $\hat{\sigma}_w^2$ derives from the invariance of $\hat{\tau}_w^*$ to adding arbitrary unit fixed effects (or, more generally, $A_i' \tilde{\lambda}_i$ terms for some i and $\tilde{\lambda}_i$) to the outcomes; this was first pointed out by Stock and Watson (2008).

A second challenge arises from unrestricted treatment-effect heterogeneity. In Theorem 2, treatment effects are estimated by fitting the corresponding outcomes Y_{it} perfectly, with residuals $\hat{\varepsilon}_{it} \equiv 0$ for all treated observations. This issue is not specific to our estimation procedure: one generally cannot distinguish between τ_{it} and ε_{it} from observations of $Y_{it} = A_i' \lambda_i + X_{it}' \delta + \tau_{it} + \varepsilon_{it}$ for treated observations, making it impossible to produce unbiased estimates of σ_w^2 (see Lemma 1 in Kline et al. (2020) for a similar impossibility result).

While unbiased estimation of σ_w^2 is not possible, we show that this variance can be estimated

conservatively. Our estimator is based on an auxiliary parsimonious model of treatment effects. We do not require this model to be correct, in the sense that inference is weakly asymptotically conservative under misspecification. However, auxiliary models which better approximate τ_{it} will make confidence intervals tighter and closer to asymptotically exact. In the computation of $\hat{\sigma}_w^2$ we set $\tilde{\varepsilon}_{it}$ for the treated observations equal to the residuals of the auxiliary model. We require the model to be parsimonious, such that it does not overfit and the residuals include ε_{it} . When the model is incorrect, $\tilde{\varepsilon}_{it}$ also include a component due to the misspecification of τ_{it} , leading to conservative inference.

We formalize the auxiliary model by considering estimators $\tilde{\tau}_{it}$ for each $it \in \Omega_1$ which satisfy two properties: (1) $\tilde{\tau}_{it}$ converges to *some* non-stochastic limit $\bar{\tau}_{it}$ and (2) if the auxiliary model is correct, $\bar{\tau}_{it} = \tau_{it}$. The following theorem presents conditions applicable to short panels under which our construction yields asymptotically conservative inference:

Theorem 3 (Conservative clustered standard error estimates). *Assume that the assumptions of Proposition 7 hold, that the model of treatment effects is trivial ($\Gamma = \mathbb{I}$), that the estimates $\tilde{\tau}_{it}$ converge to some non-random $\bar{\tau}_{it}$ in the sense that $\|v\|_H^{-2} \sum_i \left(\sum_{t; it \in \Omega_i} v_{it} (\tilde{\tau}_{it} - \bar{\tau}_{it}) \right)^2 \xrightarrow{P} 0$, that $\hat{\delta}$ from Theorem 1 is sufficiently close to δ in the sense of a mean-squared error of weighted fitted values (Assumption A2), and that additional regularity conditions on the moments of the model parameters and weights hold (Assumption A3). Then the variance estimate*

$$\hat{\sigma}_w^2 = \sum_i \left(\sum_{t; it \in \Omega} v_{it} \tilde{\varepsilon}_{it} \right)^2, \quad \tilde{\varepsilon}_{it} = Y_{it} - A_i' \hat{\lambda}_i - X_{it}' \hat{\delta} - D_{it} \tilde{\tau}_{it} \quad (7)$$

is asymptotically conservative: $\|v\|_H^{-2} (\hat{\sigma}_w^2 - \sigma_w^2 - \sigma_\tau^2) \xrightarrow{P} 0$ where $\sigma_\tau^2 = \sum_i \left(\sum_{t; D_{it}=1} v_{it} (\tau_{it} - \bar{\tau}_{it}) \right)^2 \geq 0$. If $\bar{\tau}_{it} = \tau_{it}$ for all $it \in \Omega_1$, $\sigma_\tau^2 = 0$.

It remains to be discussed how to choose the estimates $\tilde{\tau}_{it}$. We focus on auxiliary models that impose the equality of treatment effects across large groups of treated observations: for a partition $\Omega_1 = \bigcup_g G_g$, $\tau_{it} \equiv \tau_g$ for all $it \in G_g$. The τ_g can then be estimated by some weighted average of $\hat{\tau}_{it}$ among $it \in G_g$. Specifically, we propose averages of the form

$$\hat{\tau}_g = \frac{\sum_i \left(\sum_{t; D_{it}=1, it \in G_g} v_{it} \right) \left(\sum_{t; D_{it}=1, it \in G_g} v_{it} \hat{\tau}_{it} \right)}{\sum_i \left(\sum_{t; D_{it}=1, it \in G_g} v_{it} \right)^2}. \quad (8)$$

In the appendix, we show that this choice of weights leads to minimal excess variance σ_τ^2 in the case where there is only a single group g . The corresponds to a conservative auxiliary model which requires all treatment effects to be the same. In general, the choice of the partition aims to maintain a balance between avoiding overly conservative variance estimates and ensuring consistency. If the sample is large enough, one may want to partition Ω_1 into multiple groups of observations, where treatment effect heterogeneity is expected to be smaller within them than across. For instance,

with many units, a group may consist of observations with the same number of periods relative to treatment. If cohorts are large, one can further partition observations into groups with the same cohort and period.

While sufficiently large groups in (8) avoid overfitting asymptotically (under appropriate conditions), in finite samples these $\tilde{\tau}_{it}$ still use $\hat{\tau}_{it}$ and thus partially overfit to ε_{it} . In Appendix A.6 we therefore also consider leave-out versions of these $\tilde{\tau}_{it}$.

We make four final remarks on Theorem 3. First, our strategy for estimating the variance extends directly to conservative estimation of variance-covariance matrices for vector-valued estimands, e.g. for average treatment effects at multiple horizons h . Second, the result applies in short panels under the low-level conditions of Appendix A.4 (see Proposition A6). Third, while we have focused here on the case of unrestricted heterogeneity ($\Gamma = \mathbb{I}$), Theorem 3 can be extended to the case with a non-trivial treatment-effect model imposed in Assumption 3.³² Finally, computation of $\hat{\sigma}_w^2$ for the estimator $\hat{\tau}_w^*$ from Theorem 1 involves the implied weights v_{it}^* , which becomes computationally challenging with multiple sets of high-dimensional FEs. In Appendix A.7 we develop a computationally efficient algorithm for computing v_{it}^* based on the iterative least squares algorithm for conventional regression coefficients (Guimarães and Portugal, 2010).

4.4 Testing for Parallel Trends

Above, we have considered efficient estimation of weighted average treatment effects in event studies when the parallel-trend and no-anticipation assumptions hold. In this section, we discuss testing these assumptions. Specifically, we now discuss a principled way to falsify parallel trends with no anticipatory effects or, more generally, Assumptions 1' and 2. Our testing procedure is based on OLS regressions with untreated observations only, departing from both traditional regression-based tests and more recent placebo tests. We establish three attractive properties of our procedure: it is robust to treatment effect heterogeneity and, under homoskedasticity, has attractive power properties and avoids the pre-testing problem explained by Roth (2019).

We start by reviewing two existing strategies to test parallel trends. Traditionally, researchers estimated a dynamic specification which includes both lags and leads of treatment onset, and test — visually or statistically — that the coefficients on leads are equal to zero. More recent papers (e.g. de Chaisemartin and D'Haultfœuille, 2020; Liu et al., 2020) replace it with a placebo strategy: pretend that treatment happened k periods earlier for all eventually treated units, and estimate the average effects $h = 0, \dots, k - 1$ periods after the placebo treatment using the same estimator as for actual estimation. Liu et al. (2020) use the imputation estimator with that approach.

Both of these strategies have drawbacks. Because the traditional regression-based test uses the full sample, including treated observations, it is *not* a test for Assumptions 1' and 2 only. Rather, it

³²By Proposition A1, the general efficient estimator can be represented as an imputation estimator for a modified estimand, i.e. by changing w_1 to some w_1^* . Theorem 3 then yields a conservative variance estimate for it. We note that under sufficiently strong restrictions on treatment effects, asymptotically exact inference may be possible, as the residuals $\hat{\varepsilon}_{it}$ in (4) may be estimated consistently even for treated observations (except for the inconsequential noise in $\hat{\lambda}_i$), alleviating the need for an additional auxiliary model.

is a joint test that is also sensitive to violations of the implicit Assumption 3: that treatment effects τ_{it} for $it \in \Omega_1$ are homogeneous within each horizon (Sun and Abraham, 2021). Even if a researcher has reasons to impose a non-trivial Assumption 3 in estimation, a robust test for parallel trends and no anticipation *per se* should avoid those restrictions on treatment effect heterogeneity.

With a null Assumption 3, treated observations are not useful for testing, and our test only uses the untreated ones, for which $Y_{it}(0)$ is directly observed under Assumption 2. Tests based on placebo estimates appropriately use untreated observations only, but they suffer from a conceptual problem that may result in a power loss. The problem arises because the placebo strategy does not make a distinction between estimation and testing, and specifies a precise placebo estimation target — typically the placebo ATT. However, according to the standard logic of statistical assumption testing, the choice of the test should be based on a guess about the class of most plausible alternatives, while mimicking the estimator does not generally correspond to an efficient test of any such class.³³ We therefore propose the following joint test for Assumption 1' and Assumption 2:

Test 1.

1. Choose an alternative model for $Y_{it}(0)$ that is richer than the Assumption 1' imposed a priori:

$$Y_{it}(0) = A'_{it}\lambda_i + X'_{it}\delta + W'_{it}\gamma + \tilde{\varepsilon}_{it}; \quad (9)$$

2. Estimate γ by $\hat{\gamma}$ using OLS on untreated observations $it \in \Omega_0$ only, which is justified by Assumption 2;
3. Test $\gamma = 0$ using the F-test.

A natural choice for W_{it} , which parallels conventional pre-trend tests, is a set of indicators for observations $1, \dots, k$ periods before treatment,³⁴ with periods before $E_i - k$ serving as the reference group.³⁵ This choice is appropriate, for instance, if the researcher's main worry is the possible effects of treatment anticipation, i.e. violations of Assumption 2.³⁶ If the violation of Assumptions 1' and 2 is misspecified by (9), the test will still be powerful against many, although not all, alternatives. This choice of W_{it} also lends itself to making “event study plots,” which combine the ATT estimates by horizon $h \geq 0$ with a series of pre-trend coefficients; we supply the `event_plot` Stata command for this goal.³⁷

³³In contrast, the F -test we propose possesses well-known efficiency properties: e.g., it is uniformly most powerful invariant test when $\tilde{\varepsilon}_{it}$ are homoskedastic and normal (Lehmann and Romano, 2006, Ch. 7.6).

³⁴The optimal choice of k is a challenging question that we leave to future research. As usual with F -tests, choosing a k that is too large can lead to low power against many alternatives, in particular those that generate large biases in treatment effect estimates that impose invalid Assumption 1.

³⁵Relatedly, Liu et al. (2020) propose an F -test that is based on the average differences between actual and imputed outcomes across different periods before treatment. Their test is based on residuals from the original imputation estimator, while our test is based on fitting an alternative model among the untreated observations.

³⁶Alternatively, the researcher may focus on possible violations of the parametric structure in Assumption 1'. For instance, with data spanning many years one could test for the presence of a structural break, e.g. that α_i are the same before and after the Great Recession (up to a constant shift captured by the period FEs).

³⁷Since our estimation and testing procedures are different, `event_plot` shows the two sets of coefficients in different colors by default.

We now show an additional advantage of the proposed test: if the researcher conditions on the test passing (i.e. does not report the results otherwise), inference on $\hat{\tau}_w^*$ is still asymptotically valid under the null of no violations of Assumptions 1' and 2 and under homoskedasticity. This stands in contrast with the results by Roth (2019) who find distorted inference in the context of dynamic event study regressions.

Proposition 9 (Pre-test robustness). *Suppose Assumptions 1, 2 and 4 hold. Consider $\hat{\tau}_w^*$ constructed as in Theorem 1 for some estimation target and with some Γ . Then $\hat{\tau}_w^*$ is uncorrelated with any vector $\hat{\gamma}$ constructed as in Test 1. Consequently, if the vector $(\hat{\tau}_w^*, \hat{\gamma})$ is asymptotically Normal, $\hat{\tau}_w^*$ and $\hat{\gamma}$ are asymptotically independent, and conditioning on $\hat{\gamma} \notin R_\gamma$ for a non-stochastic rejection region R_γ does not asymptotically affect the distribution of $\hat{\tau}_w^*$.³⁸*

Key to this proposition is the separation of estimation and testing: estimation imposes Assumptions 1' and 2, while testing uses untreated observations only, which are not confounded by treatment effects. The formal logic behind it is similar to that of Hausman tests: under Assumptions 1, 2 and 4 and additionally imposing appropriate Assumption 3, $\hat{\tau}_w^*$ is efficient for τ_w . Under the same assumptions, $\hat{\gamma}$ is unbiased for zero and thus should be uncorrelated with $\hat{\tau}_w^*$, or else $\hat{\tau}_w^* + \zeta' \hat{\gamma}$ would be more efficient for some ζ .

While we use Proposition 9 for the efficient estimators from Theorem 1, we note that it applies well beyond. This is because it does not require that Assumption 3 used to construct $\hat{\tau}_w^*$ (via Γ) actually holds. Thus, the result applies, for instance, to the static TWFE estimator which is a special case of Theorem 1 with all treatment effects assumed (perhaps incorrectly) to be the same. Similarly, it applies to dynamic OLS specifications that include only lags of the event and no leads.³⁹

In contrast, Proposition 9 fails when estimation and testing are done simultaneously. This point is most simply illustrated in conventional DiD designs where treatment happens at the same time in the treatment group and never in the control group. Then the period right before treatment is used as the sole reference period for both the estimates and the test statistics with a fully-dynamic regression, creating a correlation between them.

4.5 Comparison to Other Estimators

Since the problems of OLS event study regressions were first discussed, several alternative approaches robust to treatment effect heterogeneity have been developed. We now analyze these proposals through the lens of our framework and contrast our efficient imputation-based estimator with strategies based on manual aggregation of admissible DiD comparisons, focusing on the case

³⁸For estimators that do not satisfy this property, an early version of Roth (2019) shows how to construct an adjustment that removes the dependence, provided the covariance matrix between $\hat{\tau}^*$ and $\hat{\gamma}$ can be estimated. By Proposition 9, this adjustment is not needed for the Theorem 1 estimator under homoskedasticity.

³⁹Furthermore, Proposition 9 also holds when testing is done with the placebo imputation estimator, rather than by OLS as we suggested above. This is because placebo treatment effects γ_{it} can be recovered from (9), by stacking the dummies for all individual observations up to k periods before treatment into W_{it} . Thus, the placebo test is a special case of Test 1, except with $\hat{\gamma}_{it}$ averaged for each horizon before applying the F -test.

where treatment-effect heterogeneity is not constrained ($\Gamma = \mathbb{I}$). We also connect our imputation strategy to a related idea proposed for factor models.

de Chaisemartin and D’Haultfœuille (2021), Sun and Abraham (2021) and Callaway and Sant’Anna (2021) develop alternative estimators that are robust to arbitrary treatment effect heterogeneity. In the first step, they estimate “cohort-average treatment effects” $CATT_{e,t}$, i.e. ATTs for all units first treated in e and observed at $t \geq e$. Specifically, they propose (different) explicit formulas that leverage DiD comparisons for the cohort e in period t against some reference set of cohorts (e.g. never-treated or not-yet-treated by t) and against period $e - 1$, which directly precedes treatment. The $CATT_{e,t}$ estimates are then appropriately aggregated across cohorts and periods.⁴⁰

The key advantage of the imputation estimator is that it imputes the potential untreated outcomes $Y_{it}(0)$ for treated observations $it \in \Omega_1$ from the full set of untreated observations, yielding the efficiency properties formalized by Theorem 2. By Proposition 6, these alternative estimators, which are linear and robust, can also be viewed as relying on *some* estimates of $Y_{it}(0)$. However, those estimates are not the efficient choices under Assumptions 1, 2 and 4 since they only use data from period $e - 1$ for $CATT_{e,t}$, while our imputation estimator leverages all pre-periods; the reference group is also restricted by some of them. Although the imputation estimator is only most efficient under homoskedasticity, the use of all available data should make it more efficient with other heteroskedasticity and mutual correlation structures, as we find in the simulation in Section 4.6 and in the application in Section 6.3.

In principle, there could be a trade-off between efficiency and robustness, if other estimators were valid under weaker identification assumptions. While de Chaisemartin and D’Haultfœuille (2021) and Sun and Abraham (2021) impose parallel trends and no anticipation assumptions equivalent to ours, the assumptions in Callaway and Sant’Anna (2021) may appear weaker: they require outcome trends to be parallel between treated unit i and some reference group only since period $E_i - 1$ but not before. However, in the important case where all not-yet-treated cohorts are used as reference group, their pre-trends are implicitly assumed equal to each other (Marcus and Sant’Anna, 2020), bridging the gap between assumptions.⁴¹ Therefore, more efficient imputation of $Y_{it}(0)$ in Theorem 2 is not just a consequence of stronger assumptions.

The imputation estimator offers four additional benefits over the alternatives discussed above. First, by transparently mapping the model of $Y_{it}(0)$ into the estimator, it is immediately extended to models richer than TWFE, such as with unit-specific trends or in triple-difference strategies, with the same efficiency properties (see Section 5). Second, by separating testing of parallel trends from estimation, it mitigates the pre-testing problem of Roth (2019), as we have shown in Section 4.4. This

⁴⁰Fadlon and Nielsen (2015) propose a similar approach based on matching treated and not-yet-treated units, while Cengiz et al. (2019) develop a related “stacked regressions” approach.

⁴¹More precisely, the results of Marcus and Sant’Anna (2020) imply that Assumption 5 in Callaway and Sant’Anna (2021) is equivalent to our Assumption 1 when there are no extra covariates and when some units are treated at $t = 2$, such that $E_i - 1$ for them is the earliest period observed in the data. Appendix A.8 provides an illustrative example of how the imputation estimator achieves efficiency gains in that case. When $\min_i E_i > 2$, the assumptions diverge. However, while a researcher may have *a priori* reasons to think that some older periods should not be used (e.g. because of a structural break), we prefer to make any assumption on potential outcomes *ex ante* and not based on the realized period E_i of first treatment.

is in contrast to estimators which use the same reference period, from $E_i - 1$ to E_i , for both estimation and testing (e.g. de Chaisemartin and D’Haultfœuille, 2021; Sun and Abraham, 2021), generating a correlation between the resulting coefficients. Third, since our framework does not require random sampling, we allow for a more general class of estimands. Specifically, any weighted sum of treated observations is allowed for, e.g. the difference between the average effects on women and men when units are individuals, or the average treatment effect weighted by state populations when units are states. Other frameworks analyze cohort-average treatment effects and their combinations only. Finally, we provide analytical formulas for valid standard errors with a computationally efficient procedure to implement them. This complements the use of the bootstrap in de Chaisemartin and D’Haultfœuille (2020) and Callaway and Sant’Anna (2021), as well as the derivation of analytical standard errors for the case of large cohorts in de Chaisemartin and D’Haultfœuille (2020) and Sun and Abraham (2021).⁴²

Our imputation approach is closely related to the “direct estimation of the counterfactual” approach proposed by Gobillon and Magnac (2016) in a different context, for linear factor models. Like in Theorem 2, they estimate the model of $Y_{it}(0)$ — albeit a different model — on untreated observations only and extrapolate it to the treated observations. Xu (2017) notes the applicability of this approach in DiD settings too. A follow-up paper by Liu et al. (2020) further develops the counterfactual estimation approach for staggered rollout DiD, and Gardner (2021) similarly proposes a “two-stage DiD estimator” based on a specification with cohort and period FEs. Both the Liu et al. (2020) and Gardner (2021) estimators, developed independently from our work, coincide with the imputation estimator for the estimands they consider: the overall ATT and the average effect at a given number of periods after treatment. For the overall ATT, the imputation estimator is also a matrix-completion estimator without a factor model or regularization in the class considered by Athey et al. (2021). Compared with these studies, we *derive* the imputation estimator as the most efficient one for a class of problems, allow for restrictions on treatment effects for a general class of target estimands, and consider a series of extensions. Moreover, we develop asymptotic theory for the estimator and provide analytical standard errors for the standard case when fixed effects are on the individual level.⁴³

4.6 Monte-Carlo Simulations

We now quantify the efficiency properties of the imputation estimator in a simulated dataset, both under homoskedastic, serially uncorrelated error terms and without those assumptions, in comparison to the alternative robust estimators of de Chaisemartin and D’Haultfœuille (2021) and Sun and Abraham (2021). We also verify correct coverage of our inference procedure and check sensitivity of

⁴²Our standard errors are only conservative, while the analytical standard errors of Sun and Abraham (2021) are asymptotically exact. However, this difference arises because we consider a fixed sample. In the case where the estimand is based on cohort average treatment effects (with $\bar{\tau}_{it}$ similarly specified) and deviations from these averages are seen as random (as would be the case for random sampling), then our standard errors are exact, too.

⁴³Gardner (2021) derives large-sample theory by interpreting the estimator as a Generalized Method of Moments estimator with cohort fixed effects, while we consider unit fixed effects, which unlike cohort fixed effects cannot be estimated consistently in the case of short panels.

different estimators to anticipation effects.

In our baseline simulation we consider a complete panel of $I = 250$ units observed for $T = 6$ periods. The event happens for each unit in one of the periods 2–7 with equal probabilities; units with $E_i = 7$ are therefore never treated in the observed sample. Treatment effects depend on the horizon, as $\tau_{it} = K_{it} + 1$ for $it \in \Omega_1$, but are otherwise homogeneous. We impose Assumptions 1 and 2 and set the FEs to $\alpha_i = -E_i$ and $\beta_t = 3t$. Finally, in each of the 500 simulations we generate homoskedastic and mutually independent error terms, $\varepsilon_{it} \stackrel{\text{iid}}{\sim} \mathcal{N}(0, 1)$. In line with Section 2, we generate the E_i realizations only once, viewing them (along with α_i and β_t) as non-stochastic. Our target estimands are the ATTs τ_h for each horizon $h = 0, \dots, 4$ observed in the data; while τ_0 is an average of the short-run effects on 205 units, τ_4 corresponds to 41 units only (those treated at $E_i = 2$ and observed at $t = 6$).

Besides the imputation estimator of Theorem 2, we consider the de Chaisemartin and D’Haultfoeulle (2021) estimator (denoted dCDH) which uses all non-yet-treated units as the reference group but only $t = E_i - 1$ as the reference period, as well the Sun and Abraham (2021) estimator (denoted SA) which further restricts the reference group to the latest-treated cohort $E_i = 7$. Two versions of the Callaway and Sant’Anna (2021) estimator which differ by the reference group are equivalent to dCDH and SA, respectively, in this setting with no additional covariates. Importantly, the estimands are exactly the same for all three estimators we consider. We implement the imputation estimator via our Stata command `did_imputation`, and the alternative estimators by the commands provided by the authors: `did_multipligt` and `eventstudyinteract`, respectively. We also compute the weights v_{it} underlying these estimators to calculate exact properties of the estimators, such as their finite-sample variance. For inference on the imputation estimator, we use the results from Section 4.3 with $\bar{\tau}_{it}$ defined as simple averages of treatment effects by cohort-period cells. In the absence of treatment effect heterogeneity within these cells, inference is exact rather than conservative. Standard errors for the dCDH estimator are based on bootstrap with 100 replications, while SA standard errors are based on large-cohort asymptotic results, as described in the respective papers.

Column 1 of Table 2 reports the exact variance of each estimator, for each horizon-specific estimand. In line with Theorem 1, the imputation estimator is most efficient in all cases, but the simulation is useful in quantifying the magnitude of the efficiency gain. Under homoskedasticity, the variances of the dCDH and SA estimators are 15–41% higher than the variance of the imputation estimator, i.e. a 15–41% larger sample would be needed to obtain confidence intervals of a similar length if these estimators are used. Relative to the dCDH estimator, the efficiency gains tend to be stronger at shorter horizons, while they are non-monotonic relative to SA.

Column 2 shows that the inference procedures accompanying each of the three estimators perform well. Specifically, we report the simulated coverage: the fraction of the 500 simulations in which a t -test does not reject the null of $\tau_h = h + 1$ at the 5% significance level. Coverage is close to 95% in all simulations of Table 2, and therefore is not reported later.

Since the imputation estimator uses all pre-periods as reference periods, while the alternative estimators only use the period directly preceding treatment, it is not surprising that the efficiency

Table 2: Efficiency and Bias of Alternative Estimators

Horizon	Estimator	Baseline simulation		More pre-periods	Heterosk. errors	AR(1) errors	Anticipation effects
		Variance (1)	Coverage (2)	Variance (3)	Variance (4)	Variance (5)	Bias (6)
$h = 0$	Imputation	0.0099	0.942	0.0080	0.0347	0.0072	-0.0569
	dCDH	0.0140	0.938	0.0140	0.0526	0.0070	-0.0915
	SA	0.0115	0.938	0.0115	0.0404	0.0066	-0.0753
$h = 1$	Imputation	0.0145	0.936	0.0111	0.0532	0.0143	-0.0719
	dCDH	0.0185	0.948	0.0185	0.0703	0.0151	-0.0972
	SA	0.0177	0.948	0.0177	0.0643	0.0165	-0.0812
$h = 2$	Imputation	0.0222	0.956	0.0161	0.0813	0.0240	-0.0886
	dCDH	0.0262	0.958	0.0262	0.0952	0.0257	-0.1020
	SA	0.0317	0.950	0.0317	0.1108	0.0341	-0.0850
$h = 3$	Imputation	0.0366	0.928	0.0255	0.1379	0.0394	-0.1101
	dCDH	0.0422	0.930	0.0422	0.1488	0.0446	-0.1087
	SA	0.0479	0.952	0.0479	0.1659	0.0543	-0.0932
$h = 4$	Imputation	0.0800	0.942	0.0546	0.3197	0.0773	-0.1487
	dCDH	0.0932	0.950	0.0932	0.3263	0.0903	-0.1265
	SA	0.0932	0.954	0.0932	0.3263	0.0903	-0.1265

Notes: See Section 4.6 for a detailed description of the data-generating processes and reported statistics.

gains are even higher when we add four more pre-periods, $t = -3, \dots, 0$, in Column 3. In this simulation the true variances of the dCDH and SA estimators are 44–97% higher.

In Columns 4 and 5 of Table 2, we report estimator variances under deviations from Assumption 4, such that the relative efficiency of the imputation estimator is no longer guaranteed by Theorem 1. In Column 4 we make the error terms heteroskedastic (while still mutually independent): $\varepsilon_{it} \sim \mathcal{N}(0, \sigma_{it}^2)$ for $\sigma_{it}^2 = t$, such that the variance is higher in later periods. In Column 5 we instead suppose that ε_{it} follow a stationary AR(1) process with $\text{Var}[\varepsilon_{it}] = 1$ and $\text{Cov}[\varepsilon_{it}, \varepsilon_{it'}] = 0.5^{|t-t'|}$, with ε_{it} still Normally distributed and independent across units. The imputation estimator remains the most efficient of the three, with variances of dCDH and SA higher by 2–51% in Column 4 and 5–42% in Column 5. The only exception is the estimator for $h = 0$ in Column 5, where the alternative estimators are 3–8% more efficient.

Finally, in Column 6 we consider the sensitivity of the three estimators to violations of Assumptions 1 and 2. Specifically, we add an anticipation effect of $1/\sqrt{I} = 0.0632$ to the outcomes of each unit corresponding to the period right before treatment, $t = E_i - 1$, and report the exact bias of

each estimator.⁴⁴ While the dCDH estimator is always more sensitive to anticipation effects than SA (except $h = 4$ where the two estimators coincide), we find no clear relationship between the imputation estimator and its alternatives. The imputation estimator is more sensitive for longer horizons $h = 3, 4$ more robust for $h = 0, 1$, and is in between dCDH and SA for $h = 2$.⁴⁵

Taken together, these results suggest that the imputation estimator has sizable efficiency advantages that extend even to heteroskedasticity and serial correlation of error terms; in Section 6.3 we confirm these findings in an application with real data. The Monte Carlo simulations here further highlight that the efficiency gains do not come at a cost of systematically higher sensitivity to parallel trend violations. Moreover, our analytical inference tools perform well in finite samples. Naturally, these results may be specific to the data-generating processes we considered, and we recommend that researchers perform similar simulations based on their data.

5 Extensions

Our benchmark setting assumed panel data and treatments that happen at different times but stay on forever. We now show that our results extend naturally to a range of related settings used in applied economics, deviating from these benchmark conditions in various ways. We consider three alternative data structures: repeated cross-sections, datasets with two-dimensional cross-sectional variation in one period, and triple-difference designs. We also comment on how our assumptions and results translate to generic datasets which need not have a panel structure. On treatment timing, we discuss plain vanilla DiD designs without staggered timing, scenarios where treatment switches on and off, and settings where the same unit goes through multiple events. We focus on robust and efficient estimation throughout the section but also point out how OLS-based procedures continue to be problematic.

5.1 Deviations from the Panel Data Structure

Repeated Cross-Sections. Suppose in each period data are available for different random samples of units i (e.g., individuals) belonging to the same set of groups $g(i)$ (e.g., regions), and treatment timing varies at the group level (e.g. Sansone, 2019). In that case no estimation with units FEs is possible, as only one observation is available for each unit. However, DiD strategies are still applicable to repeated cross sections, with group FEs replacing unit FEs. In our framework this means replacing Assumption 1 with $Y_{it}(0) = \alpha_{g(i)} + \beta_t + \varepsilon_{it}$. Theorem 1 then directly extends to this setting.

⁴⁴The \sqrt{T} normalization makes the bias comparable in magnitude to standard errors.

⁴⁵There is similarly no clear ranking between the sensitivity of our estimator relative to dCDH and SA when we consider parallel trend violations at other horizons $K_{it} = -k$, $k = 2, \dots, 6$ (for the same estimands). This is not surprising since all linear estimators that are robust to arbitrary treatment effect heterogeneity have the same sensitivity to the sum of these anticipation effects, $Y_{it} = \sum_{k=1}^6 \mathbf{1}[K_{it} = -k]$ (Proposition A9).

Two-Dimensional Cross-Sections. DiD designs are also used when the outcome is measured in a single period but across two cross-sectional dimensions, such as regions i and birth cohorts g (e.g. Hoynes et al. (2016); Bennett et al. (2020)). To fix ideas, suppose some policy is implemented in a set of regions always for the cohorts born later after some cutoff period E_i that varies across regions, such that the treatment indicator is defined as $D_{ig} = \mathbf{1}[g \geq E_i]$. With the untreated potential outcomes modeled in a TWFE way, as $Y_{ig} = \alpha_i + \beta_g + \varepsilon_{ig}$, the setting is isomorphic to our benchmark. Thus, TWFE OLS may suffer from negative weighting, in particular for the oldest groups in the regions with lower cutoffs. Moreover, the imputation estimator is robust to heterogeneous effects and efficient under homoskedasticity of ε_{it} .

Triple-Differences. In triple-difference designs, the data have three dimensions, such as regions i , demographic groups g , and periods t . Conventional static OLS estimation is based on the regression

$$Y_{igt} = \alpha_{ig} + \alpha_{it} + \alpha_{gt} + \tau D_{igt} + \varepsilon_{igt} \quad (10)$$

(Angrist and Pischke, 2008, p.181) and dynamic versions have been leveraged (e.g. Bau, 2021). A prominent class of applications with staggered adoption, following Baier and Bergstrand (2007), is in gravity equations, where Y_{igt} is (log) value of exports from country i to country g in period t , and D_{igt} is an indicator for a free trade agreement between them. For the same reasons as in Section 3.3, the OLS estimand for τ may not properly average heterogeneous effects τ_{igt} when different regions and groups are treated at different times. The imputation estimator based on the model $Y_{igt}(0) = \alpha_{ig} + \alpha_{it} + \alpha_{gt} + \varepsilon_{igt}$ is robust and efficient under homoskedasticity.

Generic Data. Ultimately, Theorems 1 and 2 apply to generic datasets with observations indexed by j (which may or may not include a time dimension), if one assumes the analogs of Assumptions 1', 2 and 3':

- a model of untreated potential outcomes: $Y_j(0) = Z_j' \pi + \varepsilon_j$ for some covariates Z_j (perhaps including one or more sets of group FEs) and $\mathbb{E}[\varepsilon_j] = 0$;
- that the observed outcome $Y_j = Y_j(0)$ if the treatment indicator D_j is zero;
- and a (possibly trivial) model of treatment effects $\tau_j \equiv Y_j - Y_j(0) = \Gamma_j \theta$.

The asymptotic results generalize similarly.

5.2 Deviations from Staggered Treatment Timing

Simultaneous Treatment. Consider plain vanilla DiD designs in which treatment happens at a single date (in the treatment group) or never (in the control group), and the panel is complete. In this case there are no forbidden DiD comparisons, in which a unit switches its treatment status in a period when another unit has already been treated. Thus, only admissible comparisons are available,

and OLS estimation does not suffer from negative weights. The presence of a never-treatment group also prevents the underidentification problem.

These nice properties of conventional DiD designs break if more covariates are included (Sant’Anna and Zhao, 2020), in particular in presence of unit-specific time trends or at least a treatment group-specific trend (as in, e.g., Duggan et al., 2016). Static OLS estimates unit-specific trends by using the data both pre- and post-treatment, and therefore contaminates them with the dynamics of treatment effects—an observation first made by Wolfers (2006).⁴⁶ Similarly, the underidentification problem reappears, as the fully-dynamic specification cannot distinguish between a linear path in treatment effects and a combination of a time trend and a treatment group-specific effect.

In contrast, the imputation estimator continues to apply, providing robust estimates which are efficient under homoskedasticity.

Treatment Switching On and Off. In some applications, the treatment of interest may switch on and off for the same unit over time. For instance, Martínez et al. (2021) study the effects of a temporary tax holiday introduced across Swiss cantons in a staggered way (see Dean, 2021, for another example). Our results extend directly if it is appropriate to write $Y_{it} = \alpha_i + \beta_t + \tau_{it}D_{it} + \varepsilon_{it}$ and therefore extrapolate the TWFE outcome structure from the untreated to the treated periods, regardless of how they are ordered relative to each other.

This characterization of Y_{it} , however, here requires an additional assumption that there are no within-unit spillovers from the periods of treatment to the future untreated periods. If the lags of the event affect current outcomes in periods with $D_{it} = 0$, the observed outcomes may not equal the never-treated potential outcome $Y_{it}(0)$ even if there are no anticipation effects. All periods since the first treatment date may thus be contaminated by treatment. The imputation approach is therefore applicable when treatment effects are heterogeneous across units and periods but not dynamic, i.e. the potential outcome today depends only on the treatment status today.⁴⁷

Non-Binary Treatments. Our framework also applies to treatments that switch from zero to multiple (discrete or continuous) values, which may or may not change over time once non-zero. For instance, Broda and Parker (2014) analyze the marginal propensity to spend (MPX) out of the 2008 economic stimulus payments (ESP), which was disbursed in a staggered way. While some specifications use the indicator of any ESP as a binary treatment, others leverage the dollar amount of the ESP receipt, ESP_{it} . The imputation estimator applies with no change: the FEs can be estimated using untreated observations only, yielding unbiased estimates of treatment effects, $Y_{it} - \hat{Y}_{it}(0)$. The richer structure of treatment allows for a broader class of estimands: one could, for

⁴⁶Interestingly, negative weights may also appear in a non-staggered setting without covariates, when the panel is incomplete. In particular, this would happen if, for a certain treated observation it (from the treatment group in a post-treatment period), many pre-treatment outcomes for unit i and many control group outcomes in period t are missing. The mechanics of the issue is similar to the staggered case; see the proof of Proposition 4.

⁴⁷Another assumption that becomes less natural when the treatment may switch on and off is the asymmetry between $\mathbb{E}[Y_{it}(0)]$ and $\mathbb{E}[Y_{it}(1)] = \mathbb{E}[Y_{it}(0)] + \tau_{it}$ — the asymmetry that motivates us to impose parametric assumptions on $\mathbb{E}[Y_{it}(0)]$ while keeping treatment effects and therefore $\mathbb{E}[Y_{it}(1)]$ unrestricted.

instance, compute the average per-dollar MPX, by averaging $(Y_{it} - \hat{Y}_{it}(0)) / ESP_{it}$ across treated observations, or the total effect of the stimulus package by averaging $Y_{it} - \hat{Y}_{it}(0)$ without normalizing them.

Multiple Events per Unit. A related scenario arises when units experience more than one event of interest. For instance, Adda (2016) considers the effects of school holidays on the epidemics. He leverages school holidays that happen in a staggered manner across regions, but his data span several years and therefore several holidays. If each holiday is viewed as a separate event that may have its own effects which potentially last forever and may change over time, causal identification is clearly impossible: one cannot distinguish between the effects of all past holidays.

However, with the events sufficiently spaced out in time, natural restrictions may be introduced via Assumption 3. For instance, one may be willing to assume that holidays can have no effects more than a few weeks after, which is still well before the next holiday. Alternatively, one may assume that the effects stabilize for each unit after that period of time, even if not at zero, or that the path of treatment effects is the same across different events. Such assumptions imply different “true” models of outcomes Y_{it} which have a lot of flexibility, yet enough structure for identification. Analogs of Theorem 1 then apply.

6 Application

In this section, we revisit the estimation of the marginal propensity to spend in the event study of Broda and Parker (2014).

6.1 Setting

The marginal propensity to spend out of tax rebates is a crucial parameter for economic policy. The Economic Stimulus Act of 2008 consisted primarily of a 100 billion dollar program that sent tax rebates to approximately 130 million US tax filers; the tax rebate component of the Coronavirus Aid, Relief, and Economic Security (CARES) Act of 2020 amounted to around 300 billion dollars. Whether such policies were effective at boosting the economy and counteracting economic crises depends crucially on the extent to which they directly raised household spending.

A recent literature, in particular Parker et al. (2013) and Broda and Parker (2014), estimates the marginal propensity to spend (MPX) out of the 2008 tax rebates.⁴⁸ The tax cuts varied across households in amount, method of disbursement, and timing. Typically, single individuals received between \$300 and \$600, while couples received between \$600 and \$1,200; households additionally received \$300 per child who qualified for the child tax credit. The rebate was disbursed in one of two methods: via direct deposit to a bank account, if known by the IRS, or a paper check mailed to

⁴⁸Earlier work by Johnson et al. (2006) analyzes similar payments under The Economic Growth and Tax Relief Reconciliation Act of 2001, while a recent paper by Parker et al. (2022) evaluates the Economic Impact Payments of 2020.

the home address otherwise. Within each method, the timing of disbursement was determined by the final two digits of the recipient’s Social Security Number, according to a schedule announced in advance.

Broda and Parker (2014, henceforth BP) use event study designs to examine the response of nondurable spending to tax rebate receipt. They leverage the quasi-experimental variation in the *timing* of the receipt, limiting their sample to households who report receiving the payment within the planned period. The rationale for this choice is that the last two digits of the Social Security Number, which determined the timing, are as-good-as-randomly assigned, justifying parallel trends in expenditures.⁴⁹ The no anticipation assumption may also be expected to hold: although the disbursement schedule was known in advance, households were directly notified by mail only several days before disbursement. Event studies are especially useful for estimating MPXs because of the focus on the dynamics of the impulse response, which is crucial for both policy analysis and calibrating macroeconomic models. We note that, although the MPX is a very informative moment to discipline macroeconomic models of stabilization policy, it can only be used to estimate the change in household spending directly caused by the receipt of the tax rebates and inherently ignores general equilibrium effects, e.g. due to Keynesian multipliers and price changes.

We estimate the performance of various estimators at estimating the impulse response function of nondurable spending to tax rebate receipt using the same data as BP.⁵⁰ While earlier work by Parker et al. (2013) estimate the impulse responses using quarterly spending data from the Consumer Expenditure Survey, BP leverage more detailed data from the Nielsen Homescan Consumer Panel (NCP). The Nielsen dataset tracks transactions at a much higher (in principle, daily) frequency, which is why we choose it as our main dataset. The Nielsen data cover expenditures on consumer packaged goods (food, beverages, beauty and health products, household supplies, and general merchandise), representing around 15% of total household expenditures. Our dataset, identical to that of BP, is a complete panel of 21,760 households (including 21,690 with non-missing disbursement method information) observed over 52 weeks of year 2008. Appendix Figure A1 shows the distribution of receipt weeks, by disbursement method.

6.2 Comparison between Robust and OLS Estimates

6.2.1 Negative Weighting and Upward Bias with Binning

We replicate BP’s estimates of the MPX and show how they suffer from an upward bias due to the choice of a binned OLS specification. For now we follow BP in focusing on the first three months

⁴⁹Thakral and Tô (2020) point out that for the paper check group pre-rebate household characteristics (in *levels*), such as average weekly spending, are not balanced with respect to the timing of the receipt, possibly because of misreporting of the receipt day. While this is problematic for randomization-based approaches to DiD (e.g. Arkhangelsky and Imbens (2019) and Roth and Sant’Anna (2022)), parallel *trends* in expenditures may still hold. Indeed, we fail to reject them with pre-trend tests below.

⁵⁰The literature provides other estimates of the marginal propensity to spend using variation from lottery winners (e.g., Fagereng et al. (2021)) or earnings shocks (e.g., Baker (2018) and Kueng (2018)). The MPX estimates from the Economic Stimulus Act of 2008 are widely used to calibrate macroeconomic models because they offer direct evidence on the impact of tax policy on consumption behavior.

since the receipt; we leave the longer-run effects to Section 6.2.2.

BP estimate dynamic OLS specifications of the form:

$$Y_{it} = \alpha_i + \beta_t + \sum_{h=-a}^b \tau_h \mathbf{1}[K_{it} = h] + \varepsilon_{it}, \quad (11)$$

where Y_{it} is the dollar amount of spending in calendar week t for household i , α_i are household FEs, and β_t are week FEs. In some specifications, week FEs are interacted with the disbursement method $m(i)$ (i.e., $\beta_{m(i)t}$ is included instead of β_t in (11)) to leverage the variation in timing within each disbursement method and not across; we refer to those specifications as “with disbursement method FEs.” The set of $\mathbf{1}[K_{it} = h]$ are the lead/lag indicator variables tracking the number of weeks $K_{it} = t - E_i$ since the week of the tax rebate receipt for the household, E_i ; b is chosen such that all possible lags in the sample are covered (and a varies, as discussed below). MPXs for each week, as well as pre-trend coefficients, are captured by τ_h . Regressions are weighted by the Nielsen projection weights.

BP’s preferred specification is a binned version of (11) which constrains τ_h to be constant across four-week periods — “months” — around the event, starting with the week of tax rebate receipt: e.g., $\tau_0 = \dots = \tau_3$. This specification also includes one monthly pre-trend coefficient, i.e. $a = 4$ with $\tau_{-1} = \dots = \tau_{-4}$. These estimates are replicated in Table 3: Column (1) of Panel A reports the results without disbursement methods FEs, while Column (1) of panel B includes these FEs.⁵¹ According to these estimates, tax rebate receipt led to an increase in spending in the contemporaneous month of \$42.6 (s.e. 7.2) in Panel A to \$47.6 (s.e. 9.2) in Panel B, and a cumulative increase over three months of \$60.5 (s.e. 25.7) in Panel A to \$94.4 (s.e. 33.5) in Panel B. Expressed as a fraction of the average tax rebate amount, these estimates correspond to a three-month cumulative MPX of 6.7% in Panel A and 10.5% in Panel B.⁵² BP then scale these MPX estimates to extrapolate from the sample of consumer packaged goods to the overall expenditure patterns. As discussed in greater detail in Section 6.2.3, their calculations using the preferred specification without disbursement method FEs imply that the propensity to spend from a tax rebate is between 51 and 75 percent over a quarter. These very large MPX estimates (and similar estimates by Parker et al. (2013)) have been influential to calibrate the latest generation of macroeconomic models of fiscal stabilization.

Next, we show that the choice of binned OLS leads to a substantial upward bias in estimated MPXs. In Column (2) of Table 3 we report the estimates from specification (11) without binning and with one weekly lead ($a = 1$), as in BP’s Table 3. We report the coefficients aggregated to the monthly level. The MPX for the contemporaneous month falls from \$42.6 (s.e. 7.2) to \$35.0 (s.e. 5.8) in Panel A and from \$47.6 (s.e. 9.2) to \$27.9 (s.e. 7.8) in Panel B. The fall in MPX is

⁵¹The results are identical to BP’s Table 4, Columns (1) and (4), respectively.

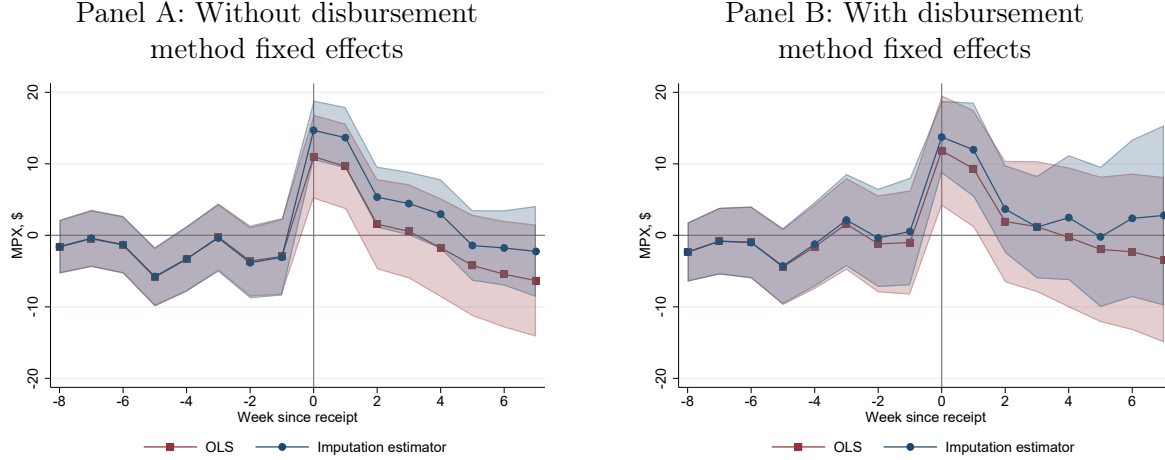
⁵²These estimates in percent are obtained by dividing the dollar estimates by the average rebate amount of \$897.86. They differ slightly from the estimates reported by BP (Table 4, columns 3 and 6, where the corresponding estimates are 6.9% for our Panel A and 12.3% for our Panel B) who reestimate the regressions rescaling the treatment lead and lags by the average rebate amount, overall or by disbursement type. Their method changes both the sample (dropping households with a missing rebate amount) and, with disbursement FEs, the estimand.

Table 3: Estimates of the Monthly and Quarterly MPX out of Tax Rebates

Panel A: Without disbursement method fixed effects			
	Dollars spent after tax rebate receipt		
	OLS	OLS	Imputation
	Monthly binned (1)	No binning (2)	Estimator (3)
Contemporaneous month	42.59 (7.19)	35.02 (5.75)	38.13 (5.68)
First month after	9.31 (9.00)	−2.28 (7.59)	−2.47 (7.81)
Second month after	8.63 (11.17)	−5.96 (10.06)	13.08 (22.51)
Three-month total	60.53 (25.73)	26.79 (21.43)	48.75 (30.97)
N observations	1,131,520	1,131,520	631,040
N households	21,760	21,760	21,760
Panel B: With disbursement method fixed effects			
	Dollars spent after tax rebate receipt		
	OLS	OLS	Imputation
	Monthly binned (1)	No binning (2)	Estimator (3)
Contemporaneous month	47.57 (9.15)	27.88 (7.75)	30.54 (9.08)
First month after	26.26 (11.95)	−4.48 (12.48)	7.43 (16.17)
Second month after	20.52 (14.57)	−13.82 (16.38)	4.01 (29.89)
Three-month total	94.35 (33.54)	9.58 (34.42)	41.97 (46.56)
N observations	1,127,880	1,127,880	536,553
N households	21,690	21,690	21,690

Notes: In Panel A, Column (1) estimates the binned version of OLS specification (11) with $a = 4$ imposing that the coefficients are the same in each month, i.e. four weeks since the rebate receipt. Column (2) estimates the same specification without binning, with $a = 1$. These specifications are identical to Broda and Parker (2014), Tables 3 and 4, column 1. Column (3) reports the efficient imputation estimator. All columns aggregate coefficients by month for the first three months after the rebate receipt and suppress the other coefficients. The estimates in Column (3), Panel B exclude the last week of the quarter ($h = 11$) due to insufficient sample size. All estimates use projection weights from the Nielsen Consumer Panel, and standard errors are clustered by household. Panel B additionally interacts week fixed effects with the disbursement method dummies. Columns (1) and (2) are identical to Broda and Parker (2014), Tables 3 and 4, column 4.

Figure 2: Dynamic Specifications and Pre-Trends



Notes: Panel A reports estimates of the response of spending to tax rebate receipts and pre-trend coefficients, using OLS specification (11) with $a = 8$ and without binning (“OLS”) and the efficient imputation estimator and the pre-trend test from Section 4.4 (“Imputation”). Panel B additionally interacts week fixed effects with the disbursement method. Observations 8 or more weeks since the rebate receipt are excluded. Estimation is weighted by the projection weights from the Nielsen Consumer Panel. 95% confidence bands are shown, using standard errors clustered by household.

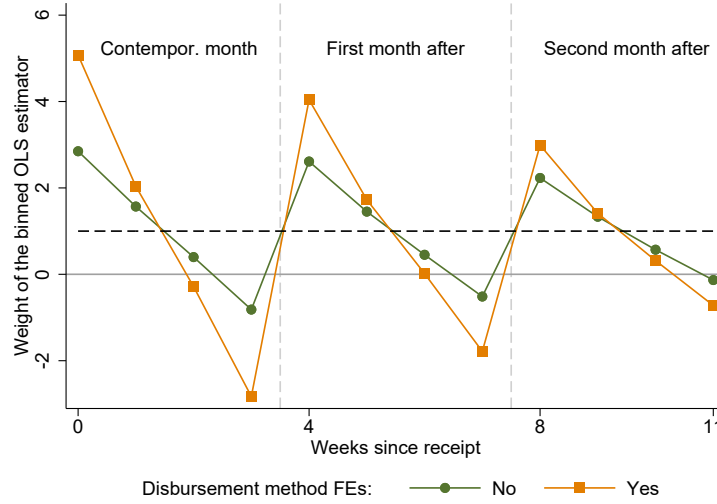
much larger for the cumulative three-month changes, from \$60.5 (s.e. 25.7) to \$26.8 (s.e. 21.4) in Panel A and from \$94.4 (s.e. 33.5) to \$9.6 (s.e. 34.4) in Panel B. In Column (3), we use the robust imputation estimator to estimate weekly average responses and aggregate them to the monthly level. The point estimates are similar to column (2) for the contemporaneous month response, while for the quarterly MPX they are in between the results obtained with monthly binned OLS and dynamic OLS.

The difference between the binned OLS and other specifications could originate from three distinct sources. First, if the DiD assumptions are violated, different estimators can produce different results. Second, as shown in (3), restricting treatment effect heterogeneity through binning can lead to undesirable estimands. Finally, the differences can simply result from sampling noise, if different estimators are differentially sensitive to it.

Figure 2 provides evidence against the first possibility. It reports the imputation estimates for 8 weeks since the rebate along with the pre-trend test from Section 4.4 that allow for 8 weeks of anticipation effects. The pre-trend coefficients are close to zero both without and with disbursement method FEs, with a joint significance p-value of 0.185 and 0.402, respectively. The figure also reports OLS specifications without binning augmented to include $a = 8$ weeks of pre-trends (and with the observations at 8+ weeks after the receipt dropped). There are no signs of pre-trends with any strategy.

Figure 2 also shows that the increase in spending after the receipt is concentrated in the month contemporaneous to the tax receipt, especially in the first two weeks, while in the following month the point estimates are close to zero (and insignificant). Why then does the binned OLS specification

Figure 3: Short-term Bias in Weights for Binned OLS



Notes: This figure reports the cumulative weight that the binned OLS estimator of the quarterly MPX from Table 3, column 1, with or without disbursement methods fixed effects, places on the true effects at each horizon $h = 0, \dots, 11$ weeks since the rebate receipt. These weights are computed using the Frisch-Waugh-Lovell theorem, as in (18), and aggregated across the first three months since the rebate receipt. The black dashed line indicates the weight corresponding to the true quarterly MPX, i.e. a simple sum of the effects at each horizon.

of Table 3, column (1), produce sizable (and sometimes significant) MPX estimates one and even two months after the receipt, and larger contemporaneous month effects?

Using the theoretical insights developed in this paper, we find that this discrepancy is explained by the short-run bias of the estimand implicitly chosen by the binned OLS specification. Figure 3 shows the weights with which the quarterly MPX estimated from the binned OLS specification of Table 3, column (1), aggregates the MPXs at each weekly horizon.⁵³ These weights show how the estimand of binned OLS diverges from the true quarterly MPX, which is a simple sum of the effects at each horizon $h = 0, \dots, 11$ weeks, i.e. with constant weights of one on each week.⁵⁴

The binned OLS specification puts a large weight on the first week, which generates a large upward bias given that the spending response is concentrated in the first weeks, as shown in Figure 2. In the specification without disbursement FEs, the weight placed on the first-week response is three times larger than it would be with an equally weighted sum; it is five times larger with the FEs. Furthermore, within each month the weights become negative for the last weeks of the month.

To verify that the short-run biased weighting scheme due to binning explains nearly all the difference between Columns (1) and (2) of Panel A of Table 3, we apply the weights of binned

⁵³For each monthly coefficient in the binned OLS specification, the weights on individual observations are computed using the Frisch-Waugh-Lovell theorem, as in Equation (18). Then they are aggregated across households and across the three monthly coefficients.

⁵⁴The binned OLS estimand also diverges from the true MPX in how it weights different households for the same weekly horizon. We focus on the variation across horizons here because MPXs have a very strong dynamic pattern (Figure 2) which, as we show, turns out to explain most of the gap between the estimates with and without binning.

OLS across weeks (underlying the specification in Column (1) of Panel A of Table 3) to the OLS estimates without binning (underlying column (2)). We obtain a point estimate of \$42.6 for the contemporaneous month (almost identical to Column (1), instead of \$35.0 in Column (2)) and \$60.4 for the quarter (compared with \$60.5 in Column (1), instead of \$26.8 in Col. (2)). Thus, the short-run biased weighting scheme due to binning explains nearly all the difference between Columns (1) and (2) of Table 3, Panel A.^{55 56}

6.2.2 Spurious Identification of Long-Run Causal Effects

In this section, we examine the long-run dynamics of MPXs obtained with OLS estimators and the robust imputation estimator. The timing of the tax rebate is such that we simultaneously observe treated and untreated households for at most 13 weeks.⁵⁷ Per Proposition 5, it is not possible to estimate causal effects beyond 12 weeks since the receipt via DiD methods without restrictive assumptions on treatment effect heterogeneity. In what follows, we highlight that dynamic OLS specifications still produce estimates for longer horizons via extrapolation, and we examine whether, in our application, the estimates obtained in this way could paint a misleading picture of the long-run dynamics of MPXs.

In Figure 4, we use the same specifications as in Table 3 but we report the full set of dynamic estimates for the treatment effects. Panel A reports the estimates from the binned OLS specification. With disbursement method fixed effects, the point estimates are large and positive for all nine months following the receipt of the tax rebate. Thus, due to the extrapolation implicit in the OLS estimator, this specification could be mistakenly interpreted as evidence for a very large and persistent increase in spending. Without these fixed effects, the estimates tend to hover around zero.

In Panel B, we show OLS estimates without binning, as in Table 3, Column (2). Here both specifications with and without disbursement method fixed effects yield point estimates that are almost all negative in the long run. Taken at face value, these estimates could misleadingly suggest that households intertemporally substitute consumption by making purchases at the time of tax rebate that they would have made 20 to 30 weeks later. As in Panel A, these point estimates are noisy but could lend themselves to some economic interpretation.

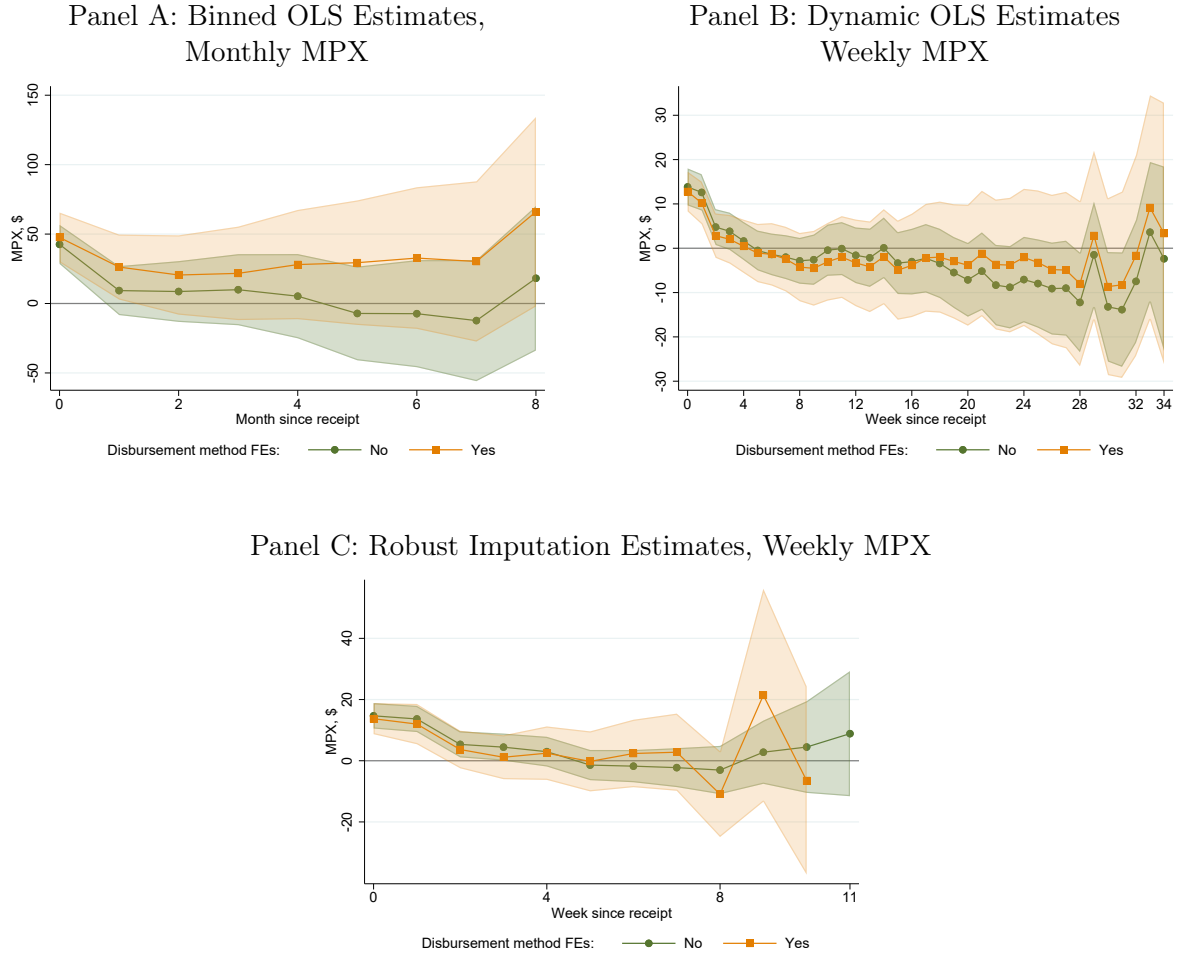
In contrast, Panel C describes the results from the robust imputation estimator, which does not allow extrapolation in the absence of an explicit control group. This panel shows that, for the horizons for which imputation is possible, there is no evidence of any impact on spending beyond the first two weeks after tax rebate receipt. The patterns are the same both with and

⁵⁵Short-run biased weighting also explains the majority, although not all, of the difference between Columns (1) and (2) of Table 3, Panel B, especially for the more precisely estimated contemporaneous month. Specifically, applying the binned OLS weights to the OLS specifications without binning, we get an estimate of \$40.0 for the contemporaneous month and \$69.0 for the quarter, thus reducing the discrepancy between Columns (1) and (2) by 62% and 70%, respectively.

⁵⁶BP motivate binning by statistical precision: “*by estimating fewer parameters, longer-term spending effects of the receipt may be estimated more precisely*” (p. 29). A comparison of Columns (1) and (2) of Table 3 does not find support for that rationale, and thus for any trade-off between a more desirable estimand and estimation efficiency.

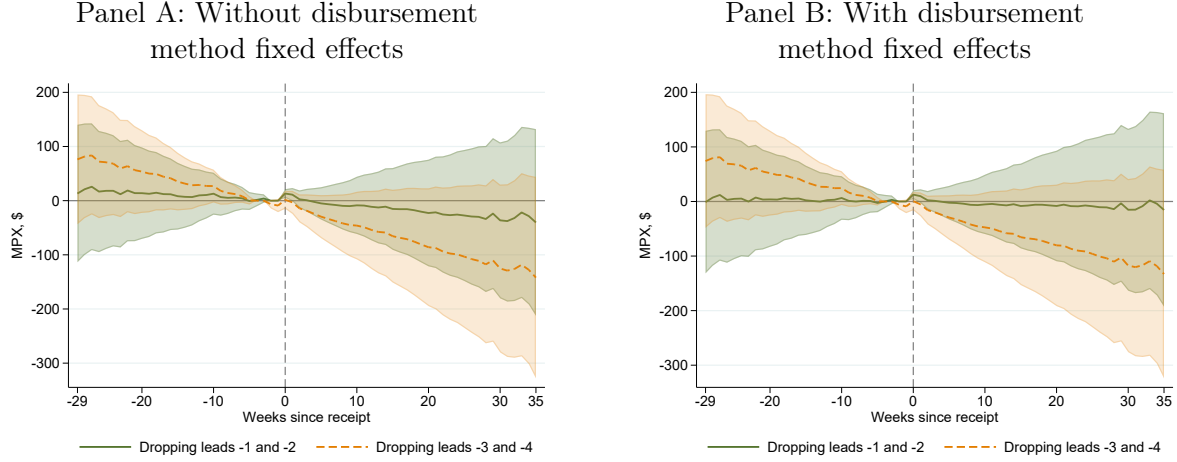
⁵⁷Appendix Figure A1 shows that the first treated households received the rebate during week 17 of 2008 (week ending April 26), while the last treated households received it during week 30 (week ending July 26).

Figure 4: Long-run MPXs, OLS vs. Robust Imputation Estimator



Notes: Panels A–C plot all MPX coefficients and 95% confidence bands for Table 3, columns 1–3, respectively, both with and without interactions between disbursement method and week FEs. Coefficients on the leads of treatment (for one month in Panel A and one week in Panel B) are not shown. The last horizon in Panel B ($h = 35$ weeks) and Panel C ($h = 12$ weeks without disbursement methods FEs or $h = 11$ with them) are also suppressed because of the very large standard errors, due to a limited sample size. Standard errors are clustered by household.

Figure 5: Underidentification of the Fully-Dynamic OLS Specification



Notes: This figure reports MPX and pre-trend estimates and 95% confidence bands for specification (11) with all leads and lags of the tax receipt included, except for two chosen as indicated. Panel A proceeds without disbursement methods fixed effects interacted with week fixed effects, while Panel B includes them. Standard errors are clustered by household.

without disbursement fixed effects. These results highlight the practical relevance of the insights from Section 3.4: the imputation estimators avoid extrapolation, thus eliminating seemingly unstable patterns found across OLS specifications.

Finally, in Figure 5 we illustrate the importance of the insights from Section 3.2 on the underidentification of fully-dynamic OLS specifications. Unlike earlier specifications, which only included a small number of treatment leads, here we run the OLS specification (11) with a full set of weekly leads and lags around tax rebate receipt. We drop two leads since the set of dynamics lead and lag coefficients is only identified up to a linear trend, as discussed in Section 3.2. We find that the fully-dynamic OLS estimates change drastically depending on which two leads are dropped. We illustrate this by comparing the MPXs when dropping leads $h = -1$ and -2 , or $h = -3$ and -4 . With or without disbursement method fixed effects, the dynamic path is very sensitive to the choice of leads to drop. This illustrates another source of instability in conventional practice, which the robust imputation estimator directly avoids.

6.2.3 Preferred Robust Estimates and Implications for the Macroeconomics Literature

We now discuss the implications of our findings for the macroeconomics literature. We proceed in two steps: selecting our preferred MPX estimate for the Nielsen products based on Section 6.2.1 and then extrapolating it to the entire consumption basket, following the strategy of BP.

Our preferred estimate for the average cumulative MPX out of the tax rebate for the Nielsen products is \$30.5, corresponding to the imputation estimator with disbursement method FEs (Table 3, Panel B, column 3) in the first month since the rebate. This constitutes 3.4% of the average rebate amount. We choose the specification with disbursement method FEs because the variation

in timing is more plausibly exogenous within disbursement methods and across (both *ex ante*, due to the institutional setting, and *ex post*, as shown by Thakral and Tô (2020)). We focus on the first (i.e., contemporaneous) month and impose zero effects for the following months based on the evidence from Figures 2 and 4 that the MPXs rapidly decay to zero, while estimation noise increases.⁵⁸ Finally, we choose the imputation estimator over OLS for its robustness properties. In contrast to column 1 of Table 3, it avoids the short-term bias due to binning. Moreover, in contrast to column 2 it ensures equal weighting across all post-rebate observations for which imputation is possible, although the estimates are similar for the contemporaneous month.⁵⁹ Conveniently, robustness to treatment effect heterogeneity is gained without an efficiency loss in this application: the standard errors are similar across columns of Table 3 for the effects in the contemporaneous month (and even one month after).

To obtain MPX estimates covering the full consumption baskets, BP propose to rescale the estimates obtained with the Nielsen data. In BP, this scaling is done in three different ways: (i) by the ratio of spending per capita in the National Income and Product Account (NIPA) and Nielsen data; (ii) by the ratio of the self-reported change in spending on all goods after the rebate relative to that on Nielsen goods alone; (iii) by a factor based on the relative shares of spending and relative responsiveness to the rebate across subcategories of goods as measured in Consumer Expenditure Survey. Using these three approaches and BP’s preferred MPX estimate (based on the specification of our Table 3, Panel A, Column 1), they estimate that the tax rebate raised the annualized expenditure growth rate by 1.3–1.9 percentage points (p.p.) in 2008Q2 and by 0.6–0.9 p.p. in 2008Q3, depending on the choice of rescaling. Panel A of Figure 6 reproduces these results, showing the counterfactual path of personal consumption expenditure absent the ESP, for each scaling method.

Applying the same scaling methods to our preferred MPX estimate for the contemporaneous month and assuming zero response in the following months paints a very different picture, with an increase in annualized expenditure growth of only 0.8–1.1p.p. in 2008Q2 and 0.15–0.22p.p. in 2008Q3. Panel B of Figure 6 reports these patterns. Thus, our estimate implies a 40% smaller response of consumption expenditures in 2008Q2, and 75% smaller in 2008Q3. This difference stems from the divergence between our preferred estimates at the individual level: while BP conclude that the propensity to spend at the individual level from a tax rebate over three months since the rebate is between 51 and 75 percent, our preferred estimates are half of that: between 25 and 37 percent.⁶⁰

In Table 4, we summarize the MPX estimates for the first quarter after tax rebate obtained with BP’s preferred specification and with the efficient imputation estimator. The first row reports the observed marginal propensity to spend on products included in the Nielsen sample during that quarter. The next rows rescale these estimates to extrapolate the marginal propensity to spend to

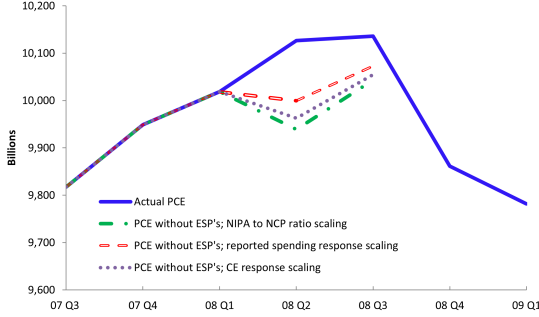
⁵⁸Our preferred estimate is robust to the choice of the time window: the cumulative MPX would have been similar (at \$25.7 instead of \$30.5) if we focused on the first two weeks only.

⁵⁹The set of households for whom the MPXs are estimable without restricting heterogeneity is smaller for longer horizons. In an unreported robustness check, we have verified that the MPX estimates are very similar (at \$28.5 instead of \$30.5) if restricting the set of households to be balanced across horizons $h = 0, \dots, 3$ weeks.

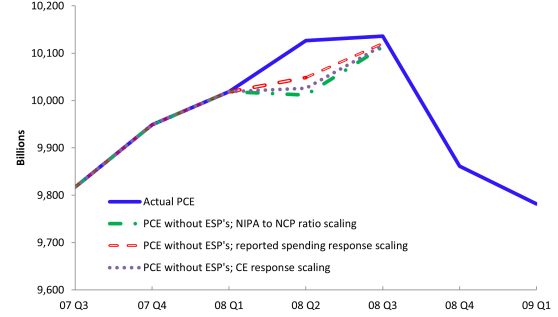
⁶⁰We obtain these estimates by replicating the first row of Panel A of BP’s Table 5 and using our preferred event study estimates.

Figure 6: Macroeconomic Implications of Tax Rebates for Personal Consumption Expenditures during the Great Recession

Panel A: Replication of Broda and Parker (2014)



Panel B: With the Preferred Robust MPX Estimate



Notes: This figure reports the estimated impact of the 2008 fiscal stimulus on total personal consumption expenditure in the U.S., using the partial equilibrium MPX estimates for consumer packaged goods covered by the Nielsen Consumer Panel (NCP) and three rescaling methods. Panel A, which replicates Figure 1 from Broda and Parker (2014), uses their preferred MPX estimate, while panel B uses our preferred MPX estimate from the robust imputation estimator (Table 3, Panel B, column 3 for the contemporaneous month, with zero MPX thereafter).

broader samples, i.e. non-durables (second row) and the full consumption basket (third row). As in Figure 6, we implement the scaling in three different ways and report the higher and upper bound of MPXs. The estimates from BP are closely in line with the literature on MPXs: estimates of the quarterly MPX for all expenditure range from 50-90%, while estimates of the quarterly MPX for nondurable expenditure range from 15-25%.⁶¹ The fourth row follows the methodology of Laibson et al. (2022) and reports the model-consistent, or “notional,” marginal propensity to consume (MPC) that can be used as a target for macroeconomic models.⁶² In all rows, the efficient imputation estimator delivers the effects that are about half as large as those based on Broda and Parker (2014). These smaller estimates imply a lower effectiveness of fiscal stimulus.

Thus, besides providing new estimates for the impact of the 2008 fiscal stimulus on the U.S. economy, our approach yields two lessons for the calibration of macroeconomic models: (1) that the targeted MPC should be significantly smaller — about half as large — and (2) that it is best to calibrate the model using weekly-level estimates of the MPC, as we report in Figure 2, rather than monthly or, especially, quarterly MPC estimates, which are much noisier. Indeed, models should reflect that most of the spending response occurs in the very short run, in the first two to four weeks after tax rebate receipt. This strong lack of consumption smoothing may be inconsistent with some of the economic channels in state-of-the-art macroeconomic models.⁶³

⁶¹Laibson et al. (2022) provide a recent review of the literature. Kaplan and Violante (2021) review non-durables MPX, and Di Maggio et al. (2020) review total MPX.

⁶²Standard macroeconomic models assume a notional consumption flow that does not distinguish between non-durables and durable consumption. Prior to the Laibson et al. (2022) work showing that the notional MPC should be the relevant target, state-of-the-art macroeconomic models (e.g., Kaplan and Violante (2014)) targeted non-durable MPX estimates (e.g., from Johnson et al. (2006)).

⁶³For completeness, in Appendix Figure A2, we report imputation-based MPX estimates by household group, defined

Table 4: MPX and MPC Estimates for Calibration of Macroeconomic Models

Statistic	Replication of Broda and Parker (2014)	Efficient Imputation Estimator
Nielsen MPX	6.7%	3.4%
Non-durable MPX	16.7% to 22.1%	8.2% to 10.8%
Total MPX	50.8% to 74.8%	24.8% to 36.6%
Notional MPC	15.9% to 23.4%	7.8% to 11.4%

Notes: This table reports the first-quarter MPX and MPC using the preferred binned OLS specification of Broda and Parker (2014) and our preferred specification based on the efficient imputation estimator. The first row reports the marginal propensity to spend on products included in the Nielsen sample. The next rows rescale these estimates to extrapolate the marginal propensity to spend to broader samples, i.e. durables (second row) and all goods (third row). The fourth row follows the methodology of Laibson et al. (2022) and reports the model-consistent MPC that can be used as a target for macro models, as total MPX divided by 3.2. The ranges correspond to the lowest and highest values among the three rescaling methods of Broda and Parker (2014).

6.3 Efficiency Gains relative to Alternative Robust Estimators

Finally, we compare the efficiency of the imputation estimator to the alternative robust estimators of Sun and Abraham (2021) and de Chaisemartin and D’Haultfoeulle (2021) (abbreviated SA and dCDH, as before). These results validate the findings from the Monte Carlo simulations of Section 4.6 using real-world data.

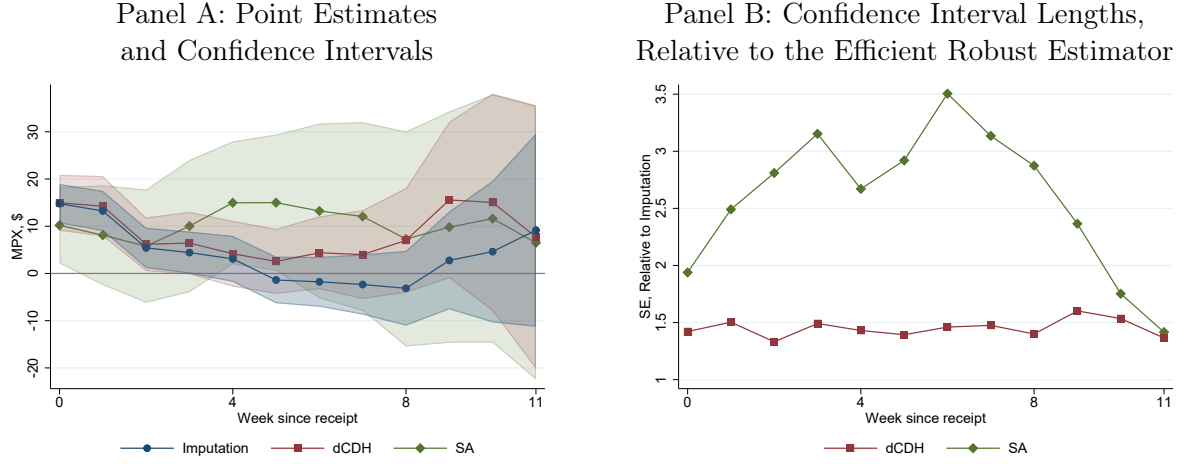
We document the in-sample efficiency gains in Figure 7 by showing the point estimates and confidence intervals for weekly average MPXs based on the imputation estimator and the two alternatives in Panel A. We use the specification without disbursement fixed effects.⁶⁴ The point estimates are very similar for dCDH and the efficient imputation estimator, but they differ from those of SA, because this estimator uses a much smaller control group (only the households who received the rebate in the latest possible week) and is therefore much noisier. Panel B zooms in on the efficiency comparison by reporting the lengths of the confidence intervals for SA and dCDH relative to that of the efficient imputation estimator.

The differences are large: the confidence interval from dCDH is about 50% longer for all periods, and 2–3.5 times longer for SA. In Appendix A.12 we confirm these efficiency gains, obtained from a single sample, using a Monte Carlo study based on a data-generating process that closely resembles the actual data.

by income or liquidity, paralleling BP’s Tables 7 and 8. These moments can also help discipline macroeconomic models. We find that illiquid households have a significantly higher MPX, consistent with the results of BP. While BP suggest that the MPX (when measured in percent of the average rebate amount) may be non-monotonic in income, our estimates suggest that the MPX is declining with income, although the difference is less pronounced across income groups than across liquidity groups and more noisy. The key result that MPXs quickly decay to zero is preserved.

⁶⁴Unlike Section 4.6, here we implement the dCDH method using the `csdid` Stata command developed for the Callaway and Sant’Anna (2021) estimator: the two estimators are identical absent additional controls (see Section 4.5), and `csdid` allows for projection weights.

Figure 7: Alternative Robust MPXs Estimates and In-Sample Efficiency



Notes: Panel A shows the estimates and 95% confidence bands for the average MPXs by horizon since rebate using three robust estimators: the efficient imputation estimator, de Chaisemartin and D’Haultfœuille (2021) (dCDH) and Sun and Abraham (2021) (SA). The specifications do not include disbursement method fixed effects. Panel B reports the ratios of the length of confidence intervals for dCDH and SA relative to the imputation estimator. Standard errors are clustered by household.

7 Conclusion

In this paper we revisited a popular class of empirical designs: event studies, or difference-in-differences with staggered rollout. We provided a unified framework that formalizes an explicit set of goals and assumptions underlying event study designs, reveals and explains challenges with conventional practice, and yields an efficient estimator. Focusing on robustness to treatment effect heterogeneity, we first showed that conventional OLS methods suffer from identification and negative weighting issues that we related to the conflation of estimation goals, fundamental assumptions on no-treatment potential outcomes and anticipation effects, and auxiliary assumptions on treatment effect homogeneity.

We then solved for the efficient estimator within our framework. In a baseline case where treatment-effect heterogeneity remains unrestricted, this robust and efficient estimator takes a particularly simple “imputation” form that estimates fixed effects among the untreated observations only, imputes untreated outcomes for treated observations, and then forms treatment-effect estimates as weighted averages over the differences between actual and imputed outcomes. We then developed results for asymptotic inference and testing and compared our approach to other estimators. Our characterization of efficient unbiased estimators naturally extends to a number of related settings. In particular, it applies to any linear model of untreated potential outcomes and unrestricted treatment effect heterogeneity, including models that do not explicitly include fixed effects. We also highlighted the importance of separating testing of identification assumptions from estimation based on them; such separation both increases estimation efficiency and helps address inference

biases due to pre-testing.

Finally, we illustrated the practical relevance of our theoretical insights by revisiting the estimation of the MPX out of tax rebates in the event study of Broda and Parker (2014). Our preferred estimates are about half of the benchmark quarterly MPX estimates used to calibrate state-of-the-art macroeconomic models and are more short-lived, implying that fiscal stimulus may be significantly less effective than predicted by existing calibrations.

References

- Abadie, Alberto, Alexis Diamond, and Jens Hainmueller**, “Synthetic control methods for comparative case studies: Estimating the effect of California’s Tobacco control program,” *Journal of the American Statistical Association*, 2010, *105* (490), 493–505.
- , **Guido W. Imbens, and Fanyin Zheng**, “Inference for Misspecified Models With Fixed Regressors,” *Journal of the American Statistical Association*, 2014, *109* (508), 1601–1614.
- Abbring, Jaap H. and Gerard J. Van den Berg**, “The nonparametric identification of treatment effects in duration models,” *Econometrica*, 2003, *71* (5), 1491–1517.
- Adda, Jerome**, “Economic Activity and the Spread of Viral Diseases: Evidence from High Frequency Data,” *Quarterly Journal of Economics*, 2016, *131* (2), 891–941.
- Angrist, Joshua**, “Estimating the labor market impact of voluntary military service using social security data on military applicants,” *Econometrica*, 1998, *66* (2), 249–288.
- Angrist, Joshua D. and JS Pischke**, *Mostly harmless econometrics: An empiricist’s companion*, Princeton University Press, 2008.
- Arkhangelsky, Dmitry and Guido W. Imbens**, “Double-Robust Identification for Causal Panel Data Models,” 2019.
- , —, **Lihua Lei, and Xiaoman Luo**, “Double-Robust Two-Way-Fixed-Effects Regression For Panel Data,” 2021.
- , **Susan Athey, David A Hirshberg, Guido W Imbens, and Stefan Wager**, “Synthetic Difference in Differences,” *Working Paper*, 2020.
- Athey, Susan and Guido W. Imbens**, “Design-based Analysis in Difference-In-Differences Settings with Staggered Adoption,” *Working Paper*, 2018.
- , **Mohsen Bayati, Nikolay Doudchenko, Guido W. Imbens, and Khashayar Khosravi**, “Matrix Completion Methods for Causal Panel Data Models,” *NBER Working Paper 25132*, 2021.
- Baier, Scott L. and Jeffrey H. Bergstrand**, “Do free trade agreements actually increase members’ international trade?,” *Journal of International Economics*, 2007, *71* (1), 72–95.

- Baker, Andrew C., David F. Larcker, and Charles C. Y. Wang**, “How Much Should We Trust Staggered Difference-In-Differences Estimates?,” *Working Paper*, 2021.
- Baker, Scott R.**, “Debt and the response to household income shocks: Validation and application of linked financial account data,” *Journal of Political Economy*, 2018, *126* (4), 1504–1557.
- Bartik, Alexander and Scott Nelson**, “Deleting a Signal: Evidence from Pre-Employment Credit Checks,” *Mimeo*, 2021.
- Bau, Natalie**, “Can Policy Change Culture? Government Pension Plans and Traditional Kinship Practices,” *American Economic Review*, 2021, *111* (6), 1880–1917.
- Bennett, Partick, Richard Blundell, and Kjell G. Salvanes**, “A second chance? Labor market returns to adult education using school reforms,” *IFS Working Paper*, 2020.
- Borusyak, Kirill and Xavier Jaravel**, “Revisiting Event Study Designs,” *Working Paper*, 2017.
- Broda, Christian and Jonathan A. Parker**, “The economic stimulus payments of 2008 and the aggregate demand for consumption,” *Journal of Monetary Economics*, 2014, *68* (S), S20–S36.
- Callaway, Brantly and Pedro H.C. Sant’Anna**, “Difference-in-Differences with Multiple Time Periods and an Application on the Minimum Wage and Employment,” *Journal of Econometrics*, 2021.
- , **Andrew Goodman-Bacon, and Pedro H. C. Sant’Anna**, “Difference-in-Differences with a Continuous Treatment,” 2021.
- Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zippere**, “The Effect of Minimum Wages on Low-Wage Jobs,” *The Quarterly Journal of Economics*, 2019, pp. 1405–1454.
- Correia, Sergio**, “Linear Models with High-dimensional Fixed Effects: An Efficient and Feasible Estimator,” *Working Paper*, 2017, (March).
- de Chaisemartin, Clément and Xavier D’Haultfoeulle**, “Fuzzy Differences-in-Differences,” *arXiv preprint*, 2015.
- and —, “Two-way fixed effects estimators with heterogeneous treatment effects,” *American Economic Review*, 2020, *110* (9), 2964–2996.
- and —, “Difference-in-Differences Estimators of Intertemporal Treatment Effects,” *Working Paper*, 2021.
- Dean, Joshua T.**, “Noise, Cognitive Function, and Worker Productivity,” *Working Paper*, 2021, pp. 1–92.
- Di Maggio, Marco, Amir Kermani, and Kaveh Majlesi**, “Stock Market Returns and Consumption,” *Journal of Finance*, 2020, *75* (6), 3175–3219.

- Duggan, Mark, Craig Garthwaite, and Aparajita Goyal**, “The Market Impacts of Pharmaceutical Product Patents in Developing Countries: Evidence from India,” *American Economic Review*, 2016, *106* (1), 99–135.
- Fadlon, Itzik and Torben Heien Nielsen**, “Household Responses to Severe Health Shocks and the Design of Social Insurance,” *Working Paper*, 2015.
- Fagereng, Andreas, Martin B Holm, and Gisle J Natvik**, “MPC heterogeneity and household balance sheets,” *American Economic Journal: Macroeconomics*, 2021, *13* (4), 1–54.
- Gardner, John**, “Two-stage differences in differences,” *Working Paper*, 2021.
- Gobillon, Laurent and Thierry Magnac**, “Regional policy evaluation: Interactive fixed effects and synthetic controls,” *Review of Economics and Statistics*, 2016, *98* (3), 535–551.
- Goodman-Bacon, Andrew**, “Difference-in-Differences with Variation in Treatment Timing,” *Working Paper*, 2018.
- Guimarães, Paulo and Pedro Portugal**, “A simple feasible procedure to fit models with high-dimensional fixed effects,” *Stata Journal*, 2010, *10* (4), 628–649.
- Hoynes, Hilary W, Diane Whitmore Schanzenbach, and Douglas Almond**, “Long Run Impacts of Childhood Access to the Safety Net,” *American Economic Review*, 2016, *106* (4), 903–934.
- Hsiao, Cheng, H. Steve Ching, and Shui Ki Wan**, “A Panel Data Approach for Program Evaluation: Measuring the Benefits of Political and Economic Integration of Hong Kong with Mainland China,” *Journal of Applied Econometrics*, 2012, *27*, 705–740.
- Imbens, Guido W. and Donald B Rubin**, *Causal inference in statistics, social, and biomedical sciences*, Cambridge University Press, 2015.
- Johnson, David S., Jonathan A. Parker, and Nicholas S. Souleles**, “Household expenditure and the income tax rebates of 2001,” *American Economic Review*, 2006, *96* (5), 1589–1610.
- Kaplan, Greg and Giovanni L. Violante**, “A Model of the Consumption Response to Fiscal Stimulus Payments,” *Econometrica*, 2014, *82* (4), 1199–1239.
- and —, “The Marginal Propensity to Consume in Heterogeneous Agent Models,” *Working Paper*, 2021.
- Kline, Patrick, Raffaele Saggio, and Mikkel Solvsten**, “Leave-Out Estimation of Variance Components,” *Econometrica*, 2020, *88* (5), 1859–1898.
- Kueng, Lorenz**, “Excess sensitivity of high-income consumers,” *The Quarterly Journal of Economics*, 2018, *133* (4), 1693–1751.

- Laibson, David, Peter Maxted, and Benjamin Moll**, “A Simple Mapping from MPCs to MPXs,” *Working Paper*, 2022.
- Lehmann, Erich L and Joseph P Romano**, *Testing statistical hypotheses*, Springer Science & Business Media, 2006.
- Liu, Licheng, Ye Wang, and Yiqing Xu**, “A Practical Guide to Counterfactual Estimators for Causal Inference with Time-Series Cross-Sectional Data,” *SSRN Electronic Journal*, 2020.
- MacKinlay, A. Craig**, “Even Studies in Economics and Finance,” *Journal of Economic Literature*, 1997, *XXXV*, 13–39.
- Mackinnon, James G. and Halbert White**, “Some heteroskedasticity-consistent covariance matrix estimators with improved finite sample properties,” *Journal of Econometrics*, 1985, *29*, 305–325.
- Marcus, Michelle and Pedro H. C. Sant’Anna**, “The role of parallel trends in event study settings: An application to environmental economics,” *Journal of the Association of Environmental and Resource Economists*, 2020, pp. 1–41.
- Martínez, Isabel Z., Emmanuel Saez, and Michael Siegenthaler**, “Intertemporal Labor Supply Substitution? Evidence from the Swiss Income Tax Holidays,” *American Economic Review*, 2021, *111* (2), 506–546.
- Meer, Jonathan and Jeremy West**, “Effects of the Minimum Wage on Employment Dynamics,” *Journal of Human Resources*, 2016, *51* (2), 500–522.
- Miller, Conrad**, “The Persistent Effect of Temporary Affirmative Action,” *American Economic Journal: Applied Economics*, 2017, *9* (3), 152–190.
- Parker, Jonathan A., Jake Schild, Laura Erhard, and David Johnson**, “Household Spending Responses to the Economic Impact Payments of 2020: Evidence from the Consumer Expenditure Survey,” *Working Paper*, 2022.
- , **Nicholas S. Souleles, David S. Johnson, and Robert McClelland**, “Consumer Spending and the Economic Stimulus Payments of 2008,” *American Economic Review*, 2013, *103* (6), 2530–2553.
- Rambachan, Ashesh and Jonathan Roth**, “An Honest Approach to Parallel Trends,” *Working Paper*, 2020.
- Roth, Jonathan**, “Pre-test with Caution: Event-study Estimates After Testing for Parallel Trends,” *Working Paper*, 2019.
- and **Pedro H. C. Sant’Anna**, “Efficient Estimation for Staggered Rollout Designs,” *Working Paper*, 2022.

- **and Pedro H.C. Sant’Anna**, “When Is Parallel Trends Sensitive to Functional Form?,” *Working Paper*, 2020.
- Sansone, Dario**, “Pink work: Same-sex marriage, employment and discrimination,” *Journal of Public Economics*, 2019, *180*, 104086.
- Sant’Anna, Pedro H.C. and Jun Zhao**, “Doubly robust difference-in-differences estimators,” *Journal of Econometrics*, 2020, *219* (1), 101–122.
- Schmidheiny, Kurt and Sebastian Siegloch**, “On Event Study Designs and Distributed-Lag Models: Equivalence , Generalization and Practical Implications,” *Working Paper*, 2020.
- Stock, James H. and Mark W. Watson**, “Heteroskedasticity-robust standard errors for fixed effects panel data regression,” *Econometrica*, 2008, *76* (1), 155–174.
- Strezhnev, Anton**, “Semiparametric weighting estimators for multi-period difference-in-differences designs,” *Working Paper*, 2018.
- Sun, Liyang and Sarah Abraham**, “Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects,” *Journal of Econometrics*, 2021.
- Thakral, Neil and Linh T. Tô**, “Anticipation and Consumption,” *Working Paper*, 2020.
- Wolfers, Justin**, “Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results,” *American Economic Review*, 2006, pp. 1802–1820.
- Wooldridge, Jeffrey M**, “Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Event Study Estimators,” *Working Paper*, 2021.
- Xu, Yiqing**, “Generalized synthetic control method: Causal inference with interactive fixed effects models,” *Political Analysis*, 2017, *25* (1), 57–76.

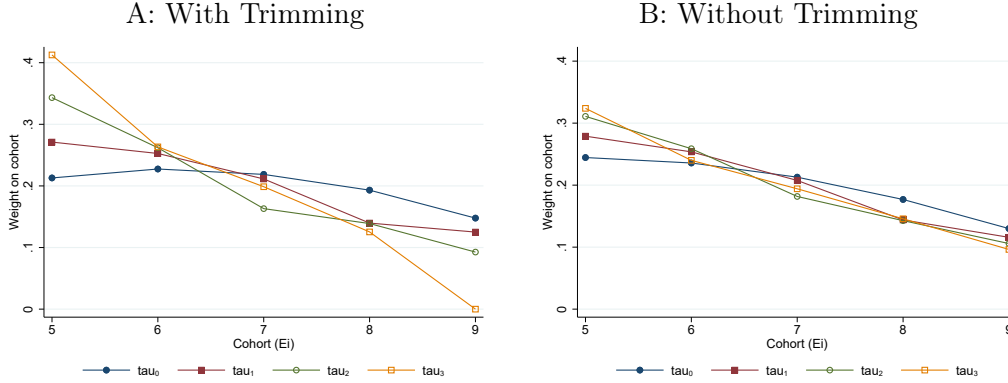
A Details and Additional Results

A.1 An Example of Trimming around Event Time

In this section, we consider the consequences of trimming around event time in a numerical example. We consider five equal-sized cohorts treated in periods $E_i = 5, \dots, 9$ and observed in periods $t = 1, \dots, 12$, with Assumptions 1’ and 2 satisfied in the complete panel. We suppose the researcher decides to trim the sample, keeping four untreated ($K_{it} = -4, \dots, -1$) and four treated ($K_{it} = 0, \dots, 3$) periods for each unit.

The short-term bias and negative weighting of static TWFE OLS persist with trimming. Using the Frisch–Waugh–Lowell theorem, we compute that cumulative weights that static OLS puts on horizons $0, \dots, 3$ are 0.875, 0.425, 0.025, and -0.325 , respectively (with the penultimate weight

Figure 8: Weights Implied by Dynamic Specifications with and without Trimming



Notes: For the numerical example described in Appendix A.1 this figure reports the total weight that the $\hat{\tau}_h$ estimator from the semi-dynamic TWFE OLS regression, for each horizon $h = 0, \dots, 3$, places on the treated observations from each cohort e observed h periods after treatment. The horizontal axis and the lines correspond to the cohort e and horizon h , respectively. Panel A trims the sample to include observations with $K_{it} \in [-4, 3]$ only, while Panel B includes all data (but does not report the weights for the coefficients τ_4, \dots, τ_7). The weights placed by $\hat{\tau}_h$ on observations at horizons other than h (as described in Section 3.5) are not shown.

combining positive and negative weights on different cohorts). There is more negative weighting with trimming than in the complete panel: the total of all negative weights is -0.367 with trimming, compared to -0.316 without.

The challenges pertaining to dynamic specifications persist, too. We consider the semi-dynamic specification which includes all lags and no leads of the event, with or without trimming.⁶⁵ In our example, we find that the OLS estimands τ_0, \dots, τ_3 are less homogeneous in trimmed samples in terms of the composition of cohorts underlying them. Figure 8 reports the weights that τ_h places on the observations h periods after treatment across various cohorts, for $h = 0, \dots, 3$. Panel A, which corresponds to the trimmed sample, shows that all estimands place higher weights on earlier-treated cohorts, but much more so for larger h . This is in contrast to the complete panel (Panel B), where the differences both across cohorts and across h are much smaller.

Finally, spurious identification of long-run effects can also be reinforced by trimming, as the observations for late-treated units in early periods are dropped. In our example, there are no admissible DiD comparisons for any unit observed three periods after treatment in the trimmed sample, making τ_3 identified through extrapolation of treatment effects only. In contrast, admissible comparisons are available in the complete panel: the cohort treated at $E_i = 5$ and observed at $t = 8$ can be compared to the cohort treated at $E_i = 9$ and to any period $t = 1, \dots, 4$ when both cohorts are not yet treated.⁶⁶

⁶⁵We also note that underidentification of fully-dynamic TWFE specifications in the absence of never-treated units applies directly with trimmed samples. In fact, this challenge is only more relevant as trimming may involve dropped never-treated units.

⁶⁶A less extreme version of this problem is that, like in complete panels, τ_0, τ_1 and τ_2 are confounded by the

A.2 Imputation and Weight Representations for Efficient Unbiased Estimators

In this section, we provide representations of unbiased and efficient estimators, including for the case when a non-trivial treatment-effect model is imposed. For brevity of notation, we write $A'_{it}\lambda_i + X'_{it}\delta \equiv Z'_{it}\pi$, where all parameters λ_i and δ are collected into a single column vector π , with the corresponding covariates collected in Z_{it} . We further let Z be the matrix with N rows Z'_{it} , and Z_1 and Z_0 its restrictions to observations Ω_1 and Ω_0 , respectively. Under Assumptions 1' and 2, we can therefore write $Y_{it} = Z'_{it}\pi + D_{it}\tau_{it} + \varepsilon_{it}$.

We first show that even when a non-trivial model $\tau = \Gamma\theta$ is imposed, the imputation result for unbiased estimators from Proposition 6 applies with respect to an adjusted estimand.

Proposition A1 (Imputation representation of unbiased estimators with a non-trivial treatment effect model). *Under Assumptions 1' and 2, any linear estimator $\hat{\tau}_w$ of τ_w that is unbiased when the model $\tau = \Gamma\theta$ is imposed can be written as a linear estimator of some alternatively weighted estimand $\tau_v = \sum_{it \in \Omega_1} v_{it}\tau_{it}$ that is unbiased without restrictions on the treatment effects. In particular, the imputation representation in Proposition 6 still applies with $\hat{\tau}_w = \sum_{it \in \Omega_1} v_{it}\hat{\tau}_{it}$ in the third step. The weights $v_1 = (v_{it})_{it \in \Omega_1}$ satisfy $\Gamma'w_1 = \Gamma'v_1$, such that $\tau_v = \tau_w$ when the model $\tau = \Gamma\theta$ is correct.*

We now provide explicit expressions for the weights implied by the the efficient estimator $\hat{\tau}_w^*$, both with and without Assumption 3.

Proposition A2 (Weight representation of efficient estimator). *The efficient estimator from Theorem 1 can be represented as $\hat{\tau}_w^* = v^{*'}Y$ with the weight vector $v^* = (v_1^{*'}, v_0^{*'})'$ satisfies*

$$v^* = \begin{pmatrix} \mathbb{I} - Z_1(Z'_1Z_1)^{-1}Z'_1 \\ -Z_0(Z'_1Z_1)^{-1}Z'_1 \end{pmatrix} \Gamma(\Gamma'(\mathbb{I} - Z_1(Z'_1Z_1)^{-1}Z'_1)\Gamma)^{-1}\Gamma'w_1$$

that does not depend on the realization of the Y_{it} . In the special case of $\Gamma = \mathbb{I}_{N_1}$, $v_1^* = w_1$ and

$$v_0^* = -Z_0(Z'_1Z_1)^{-1}Z'_1w_1.$$

We can characterize these weights by a combination of variance minimization for the treated observations and imputation for the untreated observations.

Proposition A3 (Characterization of weights in terms of imputation and variance minimization). *With a non-trivial model $\tau = \Gamma\theta$ for the treatment effects, the efficient estimator from Theorem 1 can be written as the efficient imputation estimator from Theorem 2 under unrestricted heterogeneity with alternative weights v_1^* on the treatment effects, which solve the variance-minimization problem*

$$\min_{v_1} v_1' \Phi^{-1} v_1 \quad \text{subject to } \Gamma'v_1 = \Gamma'w_1. \quad (12)$$

heterogeneity of treatment effects at other horizons (Sun and Abraham, 2021). The argument here focuses on the horizons which are present in the trimmed sample; naturally, trimming eliminates some horizons, such as $h = 4, \dots, 7$ in our example, for which causal effects are not identified in the complete panel.

where $\Phi = \mathbb{I} - Z_1(Z'_1 Z_1)^{-1} Z'_1$ is the variance of the OLS estimator of τ in the case of homoskedastic errors with unit variance and unrestricted treatment-effect heterogeneity.

The characterization of efficient weights in terms of the minimization problem in the second part clarifies that weights v_1^* can differ from weights w_1 if treatment-effect estimates $\hat{\tau}_{it}$ can be combined in a way that reduces variance according to the variance-covariance matrix Φ^{-1} of the unrestricted OLS estimator of τ (under homoskedasticity and normalizing the variance of errors to one), while ensuring that the expectation of the resulting estimator is still τ_w .

A.3 Asymptotic Conditions on Weights

In this section, we spell out the high-level conditions on weights used in Section 4.2. In order to extend consistency to asymptotic Normality, we impose an additional assumption on the concentration of weights:

Assumption A1 (Higher moments of weights). *There exists $\delta > 0$ such that $\mathbb{E}[|\varepsilon_{it}|^{2+\delta}]$ is uniformly bounded and*

$$\sum_i \left(\frac{\sum_{t; it \in \Omega} |v_{it}|}{\|v\|_H} \right)^{2+\delta} \rightarrow 0.$$

In order to establish valid inference, we require additional conditions. First, we assume that $\hat{\delta}$ is close to δ in the sense of a mean-squared error of weighted fitted values:

Assumption A2 (Consistent estimation of $\hat{\delta}$). *For the estimate $\hat{\delta}$ of δ we assume that*

$$\|v\|_H^{-2} \sum_i \left(\sum_{t; it \in \Omega} v_{it} X'_{it} (\hat{\delta} - \delta) \right)^2 \xrightarrow{p} 0.$$

This condition expresses that the fitted values $X'_{it} \hat{\delta}$ are close to $X'_{it} \delta$ according to a norm given by the weights on related units. We develop sufficient conditions below for the case of short panels when δ expresses time fixed effects.

Second, we impose additional moment bounds:

Assumption A3 (Higher moment bounds for inference). *$|\tau_{it}|$, $|\bar{\tau}_{it}|$ and $\mathbb{E}[\varepsilon_{it}^4]$ are uniformly bounded and $\sum_i \left(\frac{\sum_{t; it \in \Omega} |v_{it}|}{\|v\|_H} \right)^4 \rightarrow 0$.*

A.4 Sufficient Asymptotic Conditions for Inference

In this section we develop low-level sufficient conditions on the weights w_1 on treated observations and cohort sizes in the case of a panel with I units and T time periods with respective fixed effects. We first state sufficient condition for consistency in a panel where the number of periods T is allowed to grow slowly.

Assumption A4 (Low-level sufficient conditions for consistency). *Assume that in the first period every unit is observed and not treated, and that*

1. $\sum_{i=1}^I \left(\sum_{t; D_{it}=1} |w_{it}| \right)^2 \rightarrow 0$, that is, the weights on treatment effects fulfill a (clustered) Herfindahl condition;
2. $T \sum_{i=1}^I \left(\sum_{t; D_{it}=1} w_{it} \right)^2 \rightarrow 0$, that is, the Herfindahl concentration of unit net weights decreases fast enough;
3. $T^2 \sum_{t=2}^T \frac{(\sum_{i; D_{it}=1} w_{it})^2}{\sum_{i; D_{it}=0} 1} \rightarrow 0$, that is, the sum of squared total weight on observations treated at t relative to the number of untreated observations in the same period vanishes sufficiently quickly.

The first two conditions express that the weights do not concentrate on too few units. They are similar, but not redundant unless T is fixed; when some weights within a unit are negative and some positive, the weights may cancel out within units, yielding the second condition even when the first one is not fulfilled. The three conditions address different sources of variation of the efficient estimator $\hat{\tau}_w^*$: the first condition bounds the variation from the treated observations themselves; the second, from estimating unit FEs from the untreated observations; and the third, from estimating time FEs from the untreated observations. Together with the conditions in the main text, Assumption A4 yields consistency.

Proposition A4 (Consistency under low-level sufficient conditions). *Consider the estimator $\hat{\tau}_w^*$ for a panel with two-way fixed effects (Assumption 1), no anticipation effects (Assumption 2), treatment effects that vary arbitrarily (trivial Assumption 3'), and clustered error terms (Assumption 5). Suppose Assumption A4 holds. Then $\hat{\tau}_w^*$ from Theorem 1 is consistent for τ_w .*

Next, we develop sufficient conditions that will imply asymptotic Normality and valid inference in the special case of a complete panel with fixed number T of periods and staggered adoption.

Assumption A5 (Low-level sufficient conditions for asymptotic Normality and inference). *The panel is complete, T is fixed, $|\tau_{it}|$, $|\bar{\tau}_{it}|$ and $\mathbb{E}[\varepsilon_{it}^4]$ are uniformly bounded, and*

1. *There is some uniform constant C such that for all t and i, j with $E_i = E_j$, $D_{it} = 1 = D_{jt}$ and $w_{it} \neq 0$, we have that $w_{jt} \neq 0$ and $\frac{|w_{it}|}{|w_{jt}|} \leq C$, that is, weights within cohort and period do not vary too much;*
2. $\sum_{i; E_i=t} 1 \rightarrow \infty$ for all t (including $t = \infty$), that is, the size of all cohorts grows.⁶⁷

We note that this assumption is fulfilled in particular in the common case where the weights are constant within cohort–period cells, $w_{it} = w_{jt}$ for $t \geq E_i = E_j$. If we add these conditions, we also achieve asymptotic Normality.

Proposition A5 (Asymptotic Normality in short panels). *Consider the estimator $\hat{\tau}_w^*$ for a panel with two-way fixed effects (Assumption 1), no anticipation effects (Assumption 2), treatment effects*

⁶⁷This assumption that *all* cohorts grow can be relaxed in the context of Proposition A5 for some of the cohorts on which the estimand does not put any weight, as long as we retain enough data to estimate period fixed effects consistently.

that vary arbitrarily (trivial Assumption 3'), and clustered error terms (Assumption 5). Suppose Assumptions A4 and A5 both hold. Then $\hat{\tau}_w^*$ from Theorem 1 fulfills the conditions of Proposition 8, and is therefore asymptotically Normal for $\liminf n_H \sigma_w^2 > 0$ in the sense that $\sigma_w^{-1}(\hat{\tau}_w - \tau_w) \xrightarrow{d} \mathcal{N}(0, 1)$.

Finally, we show that these conditions are also sufficient for obtaining consistent variance estimates.

Proposition A6 (Consistent variance estimation in short panels). *Consider the estimator $\hat{\sigma}_w^2$ from Theorem 3, and suppose Assumptions 1, 2, 3', 5, A4 and A5 hold. Then $\hat{\tau}_w^*$ from Theorem 1 fulfills the conditions of Theorem 3 when $\tilde{\tau}_{it}$ are equal to an overall average or to cohort-horizon averages calculated as in (8).*

A.5 Optimal Choices for Treatment Averages in Variance Estimation

The variance estimator in Theorem 3 is asymptotically conservative, since it includes the variation $\sigma_\tau^2 = \sum_i \left(\sum_{t; D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{it}) \right)^2 \geq 0$ of the treatment effects around their averages $\bar{\tau}_{it}$. Here, we consider reasonable choices for these averages. As discussed in Section 4.3, a natural conservative choice is to estimate a single average $\bar{\tau}$, and set $\bar{\tau}_{it} = \bar{\tau}$. The choice of $\bar{\tau}$ that minimizes $\sigma_\tau^2(\bar{\tau}) = \sum_i \left(\sum_{t; D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}) \right)^2$ is

$$\bar{\tau} = \frac{\sum_i \left(\sum_{t; D_{it}=1} v_{it} \right) \left(\sum_{t; D_{it}=1} v_{it} \tau_{it} \right)}{\sum_i \left(\sum_{t; D_{it}=1} v_{it} \right)^2}. \quad (13)$$

Indeed, $\sigma_\tau^2(\bar{\tau})$ is convex in $\bar{\tau}$, and the first-order condition

$$0 = \frac{\partial}{\partial \bar{\tau}} \sigma_\tau^2(\bar{\tau}) = -2 \sum_i \left(\sum_{t; D_{it}=1} v_{it} \right) \left(\sum_{t; D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}) \right)$$

locates the above solution. A natural estimator is its sample analog.⁶⁸

When multiple group-wise averages are estimated, (8) should be viewed as a heuristic extension of (13). The optimal solution for $\tilde{\tau}_g$ is generally more complex, as the optimal choice of weights for one group-wise average may depend on the weights in the other groups. For concreteness, consider the case where averages $\bar{\tau}_{et}$ vary by cohort $E_i = e$ and period t , yielding excess variance $\sigma_\tau^2 = \sum_i \left(\sum_{t; D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{E_i t}) \right)^2$. Then the first-order conditions

$$0 = \frac{\partial}{\partial \bar{\tau}_{es}} \sum_i \left(\sum_{t; D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{E_i t}) \right)^2 = -2 \sum_{i; E_i=e} v_{is} \left(\sum_{t; D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{et}) \right)$$

⁶⁸ Also note that the denominator of (13) is zero if and only if the estimand makes only within-unit comparisons of treatment effects over time; in that case the choice of $\bar{\tau}$ is inconsequential, as it cancels out in (7).

have to be solved for each cohort simultaneously across t , provided that the estimator puts non-zero weight on multiple periods within the same cohort. The exception is when only one period for every cohort receives non-zero weight, as when estimating the ATT for a given number of periods since treatment. In that situation the optimal solution

$$\bar{\tau}_{et} = \frac{\sum_{i; E_i=e} v_{it}^2 \tau_{it}}{\sum_{i; E_i=e} v_{it}^2},$$

coincides with Equation (13).

A.6 Leave-Out Conservative Variance Estimation

Here we formalize the leave-out conservative variance estimator for τ_w , contrast it to leave-out variance estimators from prior work, and provide a computationally efficient way of obtaining them.

As in Equation (8), suppose Ω_1 is partitioned into groups of treated observations given by G_g . Let $v_{ig} = \sum_{t; it \in G_g} v_{it}$ and $\hat{T}_{ig} = \left(\sum_{t; it \in G_g} v_{it} \hat{\tau}_{it} \right) / v_{ig}$ (with an arbitrary value if $v_{ig} = 0$). Then our *non-leave-out* variance estimator is based on

$$\tilde{\tau}_{it} \equiv \tilde{\tau}_g = \frac{\sum_j v_{jg}^2 \hat{T}_{jg}}{\sum_j v_{jg}^2},$$

for $it \in G_g$. The leave-out version is defined as⁶⁹

$$\tilde{\tau}_{it}^{LO} = \frac{\sum_{j \neq i} v_{jg}^2 \hat{T}_{jg}}{\sum_{j \neq i} v_{jg}^2}.$$

Our leave-out strategy differs from leave-out variance estimation procedures of Mackinnon and White (1985) and Kline et al. (2020). Those papers assume that the OLS parameter vector is still identified when dropping individual units. In fact, Lemma 1 in Kline et al. (2020) shows that unbiased variance estimation is impossible outside that case. Our results, in contrast, provide conservative inference in models where that condition is violated. Indeed, the imputation estimator of Theorem 2 is a special case of Theorem 1 for the model in which each treated observation gets its own treatment effect parameter τ_{it} . Naturally, τ_{it} cannot be unbiasedly estimated without unit i in the data, and thus the results from Kline et al. (2020) do not apply.

While computing $\tilde{\tau}_{it}^{LO}$ directly may be computationally intensive, a more efficient procedure is available based on a simple rescaling of residuals $\tilde{\varepsilon}_{it}$ in Equation (7). For $it \in G_g$ consider

$$\tilde{\varepsilon}_{it}^{LO} = \tilde{\varepsilon}_{it} \cdot \frac{1}{1 - \left(v_{ig}^2 / \sum_j v_{jg}^2 \right)}.$$

Then replacing $\tilde{\varepsilon}_{it}$ with $\tilde{\varepsilon}_{it}^{LO}$ in Equation (7) implements the leave-out adjustment (the proof is based on straightforward algebra and available by request). The leave-out variance estimator based on $\tilde{\varepsilon}_{it}^{LO}$

⁶⁹It is well-defined whenever there are no groups in which only one unit receives a non-zero total weight.

is available as an option in our `did_imputation` command.

When all residuals use only out-of-cluster observations for estimating δ , too, the resulting variance estimator is exactly unbiased for an upper bound on the true variance:

Proposition A7. *Assume that the variance of τ_w is estimated by $\hat{\sigma}_w^2 = \sum_i \left(\sum_{t; it \in \Omega} v_{it} \tilde{\varepsilon}_{it}^{LO} \right)^2$ with $\tilde{\varepsilon}_{it}^{LO} = Y_{it} - A'_i \hat{\lambda}_i - X'_{it} \hat{\delta}^{-i} - \tilde{\tau}_{it}^{-i}$, where $\hat{\delta}^{-i}$ and $\tilde{\tau}_{it}^{-i}$ are estimated based on outcomes Y_{js} with $j \neq i$ only. Then $\mathbb{E} [\hat{\sigma}_w^2] \geq \sigma_w^2$.*

A.7 Computationally Efficient Calculation of v_{it}^* Weights

We first establish a general result about regressions: that any linear combination of estimated coefficients can be represented as a weighted sum of outcomes, where the weights are themselves a linear combination of the regressors.

Proposition A8. *Consider some scalar estimator $\hat{\psi}_w = w' \hat{\psi}$ obtained from an arbitrary point-identified regression $y_j = \psi' z_j + \varepsilon_j$. Like every linear estimator, it can be uniquely represented as $\hat{\psi}_w = v' y$, with y collecting y_j and with implied weights $v = (v_j)_j$ that do not depend on the outcome realizations. Then weights v_j can be represented as a linear combination of z_j in the sample, i.e. $v_j = z'_j \check{\psi}$ for some vector $\check{\psi}$, the same for all j .*

Proof. By standard OLS results, $\hat{\psi}_w = w' (z' z)^{-1} z' y$. Thus, $\hat{\psi}_w = v' y$ for weights $v = z (z' z)^{-1} w$ which do not depend on y . Moreover, these weights can be rewritten as $v_j = z'_j \check{\psi}$ for $\check{\psi} = (z' z)^{-1} w$. \square

We now apply this proposition to Theorem 1, with the general model of $Y_{it}(0) = Z'_{it} \pi + \varepsilon$. Then $\hat{\tau}_w^* = v^* Y$, where weights v^* can be represented as

$$v_{it}^* = Z'_{it} \check{\pi} + D_{it} \Gamma'_{it} \check{\theta}.$$

It remains to find the unknown $\check{\pi}$ and $\check{\theta}$ to obtain the v weights. To do so, we use the properties of $\hat{\tau}_w^*$. First, it equals to zero if Y_{it} is linear in Z_{it} . Second, letting $\mu = \Gamma' w_1$, $\hat{\tau}_w^* = \mu_j$ if $Y_{it} = \Gamma_{it,j} D_{it}$ for all $it \in \Omega$, as in that case $\hat{\theta}_j = 1$ and $\hat{\theta}_{-j} = 0$. Thus we have a system of equations which determine $\check{\pi}$ and $\check{\theta}$:

$$\sum_{it \in \Omega} Z_{it} (Z'_{it} \check{\pi} + D_{it} \Gamma'_{it} \check{\theta}) = 0; \quad (14)$$

$$\sum_{it \in \Omega_1} \Gamma_{it} (Z'_{it} \check{\pi} + D_{it} \Gamma'_{it} \check{\theta}) = \Gamma' w_1. \quad (15)$$

When Z_{it} has a block structure in which some covariates are FEs, solving this system iteratively is most convenient and computationally efficient. For instance, suppose $Z'_{it} \pi \equiv \alpha_i + X'_{it} \delta$ and $\Gamma = \mathbb{I}_{N_1}$ (i.e. Assumption 3 is trivial). Then Proposition A8 implies $v_{it}^* = \check{\alpha}_i + X'_{it} \check{\delta} + D'_{it} \check{\theta}_{it}$ for all $it \in \Omega$, and (15) simplifies to $v_{it}^* = w_{it}$ for all $it \in \Omega_1$. Using this and the structure of Z_{it} , we rewrite (14)

as a system

$$\sum_{t, it \in \Omega_0} (\check{\alpha}_i + X'_{it} \check{\delta}) = - \sum_{t, it \in \Omega_1} w_{it}, \quad \text{for all } i; \quad (16)$$

$$\sum_{it \in \Omega_0} X_{it} (\check{\alpha}_i + X'_{it} \check{\delta}) = - \sum_{it \in \Omega_1} X_{it} w_{it}. \quad (17)$$

This system suggests an iterative algorithm, similar to iterative OLS (e.g. Guimarães and Portugal, 2010):

1. Given a guess of $\check{\delta}$, set $\check{\alpha}_i$ for each unit to satisfy (16);
2. Given $\check{\alpha}_i$, set $\check{\delta}$ to satisfy (17);
3. Repeat until convergence.

A.8 Increased Efficiency without Stronger Assumptions: An Example

To illustrate how the imputation estimator generates efficiency gains relative to the alternative robust estimators without strengthening the parallel trend assumptions, we extend the Table 1 example by adding unit (or cohort) C treated in period 4; see Table A1. The Callaway and Sant’Anna (2021) estimator that uses not-yet-treated units as the reference group (and the de Chaisemartin and D’Haultfœuille (2021) estimator alike) would leverage $Y_{B2} - Y_{B1}$ and $Y_{C2} - Y_{C1}$ as comparisons for $Y_{A2} - Y_{A1}$ (when estimating τ_{A2}) and similarly $Y_{C3} - Y_{C2}$ for $Y_{B3} - Y_{B2}$ (when estimating τ_{B3}). This rules out non-parallel trends and anticipation effects for units B and C . However, these estimators would stop short of also using $Y_{C3} - Y_{C1}$ as a comparison to $Y_{B3} - Y_{B1}$ to estimate τ_{B3} . The imputation estimator leverages this comparison, and therefore increases efficiency without stronger assumptions.

We note that the assumptions underlying other versions of the Callaway and Sant’Anna (2021) estimator are weaker. Marcus and Sant’Anna (2020) explain that in the version that only uses never-treated units as reference group treatment effects are just-identified, and thus the imputation estimator cannot deliver an efficiency gain; in this case, the Callaway and Sant’Anna (2021) estimator is equivalent to the efficient imputation estimator based only on the set of observations on which parallel trends are imposed (i.e. those with $t \geq E_i - 1$ or $E_i = \infty$). However, this minimal parallel-trend assumption depends on the realized event timing and is not testable. As discussed in Section 2, we prefer the stronger Assumption 1, which can be made *a priori* and allows for pre-trend testing.

A.9 Equal Sensitivity of Robust Estimators to Linear Pre-Trends

Proposition A9. *Suppose there are no never-treated units. Then all linear estimators $\hat{\tau}_w$ of τ_w that are unbiased under Assumptions 1 and 2 with a trivial Assumption 3, and thus robust to arbitrary treatment effect heterogeneity, have the same sensitivity to linear anticipation trends. Specifically, if $Y_{it} = (\kappa_0 + \kappa_1 K_{it}) \cdot \mathbf{1}[D_{it} = 0]$ for some $\kappa_0, \kappa_1 \in \mathbb{R}$, then $\mathbb{E}[\hat{\tau}_w] = - \sum_{it \in \Omega_1} w_{it} (\kappa_0 + \kappa_1 K_{it})$.*

Proof. Any linear estimator unbiased under Assumptions 1 and 2 has to be numerically invariant to adding any combination of unit and period FEs to the outcome. Thus, $\hat{\tau}_w$ would be the same with the data

$$\begin{aligned}\tilde{Y}_{it} &= Y_{it} - \kappa_0 - \kappa_1 E_i + \kappa_1 t \\ &= -(\kappa_0 + \kappa_1 K_{it}) D_{it}.\end{aligned}$$

These data satisfy Assumptions 1 and 2 with the corresponding $\tau_{it} = -(\kappa_0 + \kappa_1 K_{it})$ for $it \in \Omega_1$, and thus

$$\mathbb{E}[\hat{\tau}_w] = \sum_{it \in \Omega_1} w_{it} \tau_{it} = - \sum_{it \in \Omega_1} w_{it} (\kappa_0 + \kappa_1 K_{it}).$$

□

A.10 Stochastic Event Timing

In this extension we suppose event times E_i are stochastic. Moreover, the set of observations Ω may be stochastic too—e.g. due to the stochasticity of the set of periods for which the data are missing (as in the case of panels trimmed around the event time). We continue to make no random sampling assumptions. The main results in the paper go through by conditioning on $\mathcal{I} = (\{E_i\}_i, \Omega)$. That is:

- The target is defined as $w'_1 \tau$ where w_1 is a function of \mathcal{I} —such as the average treatment effect one period after treatment for the set of units for which the outcomes are available both one period after and in some periods before treatment;
- Assumption 1 is reformulated as $\mathbb{E}[Y_{it}(0) \mid \mathcal{I}] = \alpha_i(\mathcal{I}) + \beta_t(\mathcal{I})$ or, equivalently, $\mathbb{E}[Y_{it}(0) - Y_{it'}(0) \mid \mathcal{I}] = \beta_t(\mathcal{I}) - \beta_{t'}(\mathcal{I})$ is the same across units; Assumption 1' is rewritten in a similar way;
- Assumption 4 is reformulated in a conditional way: $\mathbb{E}[\varepsilon \varepsilon' \mid \mathcal{I}] = \sigma^2(\mathcal{I}) \mathbb{I}_{N(\mathcal{I})}$;
- Theorems 1 and 2 guarantee efficiency in the class of linear conditionally-on- \mathcal{I} unbiased estimators.

A.11 Derivation from a Sampling Model

In our setup in Section 2 and in Appendix A.10, we assume a two-way fixed-effects model of $Y_{it}(0)$ conditional on treatment. In this section, we show that such a model can be obtained from a population model with random sampling where parallel-trend assumptions are formulated in terms of group-wise averages. Our model in Section 2 is therefore more general; it captures common sampling and parallel-trend assumptions, while it allows for more flexible modeling of heterogeneity and may be preferable when the panel is incomplete.

To relate our approach to random sampling, assume now that for a given number T of periods we observe a sample of I units with complete outcomes $Y_i = (Y_{i1}, \dots, Y_{iT})$ and treatment time E_i , where $E_i = \infty$ denotes a never-treated unit. We write $Y_i(0) = (Y_{i1}(0), \dots, Y_{iT}(0))$ for the

corresponding vector of potential outcomes if the unit was never to be treated, and $D_{it} = \mathbf{1}[t \geq E_i]$ for the treatment status of unit i in period t . We assume that $(Y_i, Y_i(0), E_i)$ are iid across units. We can then impose assumptions on the distribution of $(Y_i, Y_i(0), E_i)$:

1. Parallel trends: $\mathbb{E}[Y_{i,t+1}(0) - Y_{it}(0) \mid E_i]$ does not vary with E_i .
2. No anticipation: $Y_{it} = Y_{it}(0)$ for $t < E_i$, a.s.

In this model, once we condition on event timing $\{E_i\}_i$ above, we obtain the assumptions from Appendix A.10 with unit fixed effects $\alpha_i = \mathbb{E}[Y_{i1}(0) \mid E_i]$, time fixed effects $\beta_t = \mathbb{E}[Y_{it}(0) - Y_{i1}(0)]$, and treatment effects $\tau_{it} = \mathbb{E}[Y_{it} - Y_{it}(0) \mid E_i]$, where unit-fixed and treatment effects do not vary within cohorts. In particular, when all units are observed in all periods, then the sampling model with a parallel-trend assumption defined on the cohort level implies our assumptions in Section 2.

Relative to a sampling-based approach, we see three main advantages of our more general conditional fixed-effects model. First, considering units individually allows us to estimate their treatment effects separately, which permits the estimation of weighted treatment effects that put non-constant weights on units with the same event time (e.g. when estimating the gap between average treatment effects for men and women). Second, we can more naturally capture settings in which the convenience assumption of sampling from a population is unrealistic, such as when we observe all US states. Third, our unit fixed-effects model can handle missing observations even when the composition of units changes over time, while cohort-based parallel trend assumptions (e.g. that $\mathbb{E}[Y_{i,t+1}(0) - Y_{it}(0) \mid E_i]$ does not vary with E_i) may be unattractive; similarly, estimation with cohort, rather than individual, fixed effects can lead to biases with incomplete panels.

A.12 Monte Carlo Study based on the Broda and Parker Application

To complement the results in Section 6.3 obtained from a single sample, we further validate them with a Monte Carlo study. We choose a data-generating process (DGP) that closely resembles the actual data, using wild clustered bootstrap. Specifically, we obtain residuals $\tilde{\varepsilon}_{it}$ as in Section 4.3 and simulate the errors as $\varepsilon_{it}^* = z_i^* \tilde{\varepsilon}_{it}$, for z_i^* drawn independently across households and taking values ± 1 with equal probabilities. Panel A of Figure A3 plots the true standard deviations of the three alternative estimators for this DGP, computed as in Section 4.6, while Panel B normalizes them by the standard deviation of the imputation estimator.⁷⁰ The efficiency gains are very similar to those from Figure 7: the standard deviation of the dCDH estimator is about 50% larger than that of the efficient imputation estimator, and 2 to 3.5 times larger for Sun and Abraham (2021). In a robustness check we find similar efficiency gains with homoskedastic errors ε_{it}^* .

⁷⁰The tight connection between the chosen DGP and the actual data reveals itself in the fact that the standard deviation of the imputation estimator in Figure A3 is identical to its in-sample clustered standard error from Figure 7. This is not the case for the other estimators, making our Monte Carlo analysis informative.

B Proofs

In this appendix, we collect proofs for the results in the main text and in the appendix. We first extend the matrix notation from Section 4 to simplify notation. Specifically, we stack the vectors λ_i into a single vector $\lambda = (\lambda_i)_i$. We set $Z_{it} = \begin{pmatrix} (1_{i=j}A_{jt})_j \\ X_{it} \end{pmatrix}$, $\pi = \begin{pmatrix} \lambda \\ \delta \end{pmatrix}$ to summarize the nuisance component of the model. In matrix-vector notation, we write Y for the vector of outcomes, $Z = (A, X)$ for the covariate matrix, D for the matrix of indicators for treated units, ε for the vector of error terms, and $\Sigma = \text{Var}[\varepsilon]$ for their variance. We write $Y_1, Z_1 = (A_1, X_1), D_1, \varepsilon_1$ for the rows corresponding to treated observations ($it \in \Omega_1$); in particular, $D_1 = \mathbb{I}$. Analogously, we write $Y_0, Z_0 = (A_0, X_0), D_0, \varepsilon_0$ for the rows corresponding to untreated observations ($it \in \Omega_0$); in particular, $D_0 = \mathbb{O}$. We write $\tau = (\tau_{it})_{it \in \Omega_1}$ for the vector of treatment effects of the treated units, $\theta = (\theta_m)_{m=1}^{N_1-M}$ for the vector of underlying parameters, $\Gamma = (\Gamma_{it,j})_{it \in \Omega_1, j \in \{1, \dots, N_1-M\}}$ for the matrix linking the two, and $w_1 = (w_{it})_{it \in \Omega_1}$ for the weight vector. Then we can write model and estimand as

$$Y = Z\pi + D\tau + \varepsilon, \quad \tau = \Gamma\theta, \quad \tau_w = w_1'\tau$$

with $\mathbb{E}[\varepsilon] = \mathbf{0}$, $\text{Var}[\varepsilon] = \Sigma$, where Σ has block structure according to units i . For unit i , we write

$$A_i = (A_{it})_t, X_i = (X_{it})_t, Y_i = (Y_{it})_t, \varepsilon_i = (\varepsilon_{it})_t, v_i = (v_{it})_t$$

and denote by $\Sigma_i = \text{Var}[\varepsilon_i]$ the within-unit variance-covariance matrix of error terms.

B.1 Proofs from Main Text

Proof of Proposition 1. In the absence of never-treated units and defining $\tau_{-1} = 0$, we can write $\sum_{h \neq -1} \tau_h \mathbf{1}[K_{it} = h] = \tau_{K_{it}}$.

Now consider some collection of τ_h (with $\tau_{-1} = 0$) and FEs $\tilde{\alpha}_i$ and $\tilde{\beta}_t$. For any $\kappa \in \mathbb{R}$, let $\tau_h^* = \tau_h + \kappa(h+1)$, $\tilde{\alpha}_i^* = \tilde{\alpha}_i + \kappa(E_i - 1)$, and $\tilde{\beta}_t^* = \beta_t - \kappa t$. Then for any observation it ,

$$\begin{aligned} \tilde{\alpha}_i^* + \tilde{\beta}_t^* + \tau_{K_{it}}^* &= \tilde{\alpha}_i + \beta_t + \tau_h + \kappa(E_i - 1) - \kappa t + \kappa(t - E_i + 1) \\ &= \tilde{\alpha}_i + \beta_t + \tau_h, \end{aligned}$$

and Equation (2) has exactly the same fit under the original and modified FEs and τ_h coefficients, indicating perfect collinearity. \square

Proof of Proposition 2. By the Frisch–Waugh–Lovell theorem, τ^{static} can be obtained by a regression of $\mathbb{E}[Y_{it}] = \alpha_i + \beta_t + \tau_{it}D_{it}$ on \tilde{D}_{it} (without a constant), where $\tilde{D}_{it} = D_{it} - \tilde{\alpha}_i - \tilde{\beta}_t$ are the residuals from the auxiliary regression of D_{it} on the unit and period FEs. Thus,

$$\tau^{\text{static}} = \frac{\sum_{it \in \Omega} \tilde{D}_{it} (\alpha_i + \beta_t + \tau_{it}D_{it})}{\sum_{it \in \Omega} \tilde{D}_{it}^2}.$$

We have $\sum_{it \in \Omega} \tilde{D}_{it} \alpha_i = \sum_i \alpha_i \sum_{t; it \in \Omega} \tilde{D}_{it} = 0$ because the residuals in the auxiliary regression are orthogonal to all unit indicators. Analogously, $\sum_{it \in \Omega} \tilde{D}_{it} \beta_t = 0$. Defining

$$w_{it}^{\text{OLS}} = \frac{\tilde{D}_{it}}{\sum_{it \in \Omega} \tilde{D}_{it}^2}, \quad (18)$$

we have that $\tau^{\text{static}} = \sum_{it \in \Omega} w_{it}^{\text{OLS}} \tau_{it} D_{it} = \sum_{it \in \Omega_1} w_{it}^{\text{OLS}} \tau_{it}$, as required.

Clearly, w_{it}^{OLS} do not depend on the outcome realizations. Moreover, these weights add up to one:

$$\begin{aligned} \sum_{it \in \Omega_1} \tilde{D}_{it} &= \sum_{it \in \Omega} \tilde{D}_{it} D_{it} \\ &= \sum_{it \in \Omega} \tilde{D}_{it} (\tilde{D}_{it} + \check{\alpha}_i + \check{\beta}_t) \\ &= \sum_{it \in \Omega} \tilde{D}_{it}^2, \end{aligned}$$

where the last equality holds because \tilde{D}_{it} are orthogonal to the unit and period FEs. \square

Proof of Proposition 3. We use the characterization of OLS weights in Equation (18). Given the complete panel, the regression of D_{it} on TWFE produces residuals

$$\tilde{D}_{it} = D_{it} - \bar{D}_{i\cdot} - \bar{D}_{\cdot t} + \bar{D}_{\cdot\cdot},$$

where $\bar{D}_{i\cdot} = \frac{1}{3} \sum_{t=1}^3 D_{it}$, $\bar{D}_{\cdot t} = \frac{1}{2} \sum_{i=A,B} D_{it}$, and $\bar{D}_{\cdot\cdot} = \frac{1}{6} \sum_{i=A,B} \sum_{t=1}^3 D_{it}$ (de Chaisemartin and D'Haultfœuille, 2020). Plugging in $\bar{D}_{A\cdot} = 2/3$, $\bar{D}_{B\cdot} = 1/3$, $\bar{D}_{\cdot 1} = 0$, $\bar{D}_{\cdot 2} = 1/2$, $\bar{D}_{\cdot 3} = 1$, and $\bar{D}_{\cdot\cdot} = 1/2$, and computing $\sum_{i,t \in \Omega_1} \tilde{D}_{it} = 1/3$, we have

$$\begin{aligned} \hat{\tau}^{\text{static}} &= \frac{\sum_{i,t \in \Omega} \tilde{D}_{it} Y_{it}}{\sum_{i,t \in \Omega_1} \tilde{D}_{it}} \\ &= (Y_{A2} - Y_{B2}) - \frac{1}{2} (Y_{A1} - Y_{B1}) - \frac{1}{2} (Y_{A3} - Y_{B3}). \end{aligned}$$

Similarly, the OLS estimand equals

$$\begin{aligned} \tau^{\text{static}} &= \frac{\sum_{i,t \in \Omega_1} \tilde{D}_{it} \tau_{it}}{\sum_{i,t \in \Omega_1} \tilde{D}_{it}} \\ &= \tau_{A2} - \frac{1}{2} (\tau_{A3} - \tau_{B3}). \end{aligned}$$

\square

Proof of Proposition 4. Let I_{ever} be the number of ever-treated units (i.e., those treated by $t = T$).

As in the proofs of Propositions 2 and 3, $w_{it}^{OLS} = \tilde{D}_{it} / \sum_{it \in \Omega} \tilde{D}_{it}^2$ with

$$\tilde{D}_{it} = D_{it} - \bar{D}_{i\cdot} - \bar{D}_{\cdot t} + \bar{D}, \quad (19)$$

for

$$\begin{aligned} \bar{D}_{i\cdot} &= \frac{1}{T} \sum_{t=1}^T D_{it} = \frac{T - (E_i - 1)}{T}, \\ \bar{D}_{\cdot t} &= \frac{1}{I} \sum_{i=1}^I D_{it} = \frac{\sum_i \mathbf{1}[E_i \leq t]}{I}, \\ \bar{D}_{\cdot\cdot} &= \frac{1}{IT} \sum_{i,t} D_{it} = \frac{I_{\text{ever}} (T - (E_{\text{first}} - 1)) - N_0^*}{IT}, \end{aligned}$$

where the last expression holds because treated observations are those belonging to ever-treated units in periods since E_{first} , excluding the N_0^* pre-treatment observations in those periods.

Since $\bar{D}_{i\cdot}$ monotonically declines in E_i and $\bar{D}_{\cdot t}$ increases in t , Equation (19) implies that the lowest weight on any treated observation corresponds to $E_i = E_{\text{first}}$ and $t = T$. Thus, there is no negative weighting if and only if $\tilde{D}_{it} \geq 0$ for such observations. Considering Equation (19) for one of those observations and using $\bar{D}_{\cdot T} = \frac{I_{\text{ever}}}{I}$, there is no negative weighting if and only if

$$\begin{aligned} 0 &\leq 1 - \frac{T - (E_{\text{first}} - 1)}{T} - \frac{I_{\text{ever}}}{I} + \frac{I_{\text{ever}} (T - (E_{\text{first}} - 1)) - N_0^*}{IT} \\ &= \frac{(E_{\text{first}} - 1)(I - I_{\text{ever}}) - N_0^*}{IT} \\ &= \frac{N_1^* - N_0^*}{IT}, \end{aligned}$$

where the second line simplifies the terms, and the last line uses the definition of N_1^* . This is equivalent to $N_1^* \geq N_0^*$, as required. \square

Proof of Proposition 5. For any observation it , $K_{it} = t - E_i \geq \bar{H}$ implies $t \geq E_i + \bar{H} \geq \min_i E_i + \bar{H} \geq \max_i E_i$. Thus, all observations considered by the estimand correspond to the periods in which all units are already treated. Consider one such period t^* for which the total weights are non-zero, $\sum_{i: K_{it^*} \geq \bar{H}} w_{it^*} \neq 0$. (It exists because all weights w_{it} are assumed non-negative and are not identically zero.) Then consider a data-generating process (DGP) in which β_{t^*} is replaced with $\beta_{t^*} - \kappa$ for some $\kappa \neq 0$ and τ_{it^*} is replaced with $\tau_{it^*} + \kappa$ for all i . This DGP is observationally equivalent in terms of the observed Y_{it} and continues to satisfy Assumptions 1 and 2. Yet, the estimand differs by any arbitrary $\kappa \sum_{i: K_{it^*} \geq \bar{H}} w_{it^*} \neq 0$, and it therefore not identified. \square

Proof of Theorem 1. The result is a consequence of the Gauss–Markov theorem. By itself, the Gauss–Markov theorem establishes the efficiency of the underlying OLS estimator for θ . Here, we extend efficiency of the estimation of θ to efficiency for the estimation of the weighted treatment effect $\tau_w = w_1' \Gamma \theta$. By construction, $\hat{\tau}_w^* = w_1' \Gamma \hat{\theta}$ for the OLS estimator $\hat{\theta}$ of θ . For every linear

estimator $\tilde{\tau}_w$ that is unbiased for τ_w for all θ there is a linear unbiased estimator $\tilde{\theta}$ of θ (with variance $\Sigma_{\tilde{\theta}}$) for which $\tilde{\tau}_w = w_1' \Gamma \tilde{\theta}$, for example the estimator $\tilde{\theta} = \hat{\theta} + \Gamma' w_1 (w_1' \Gamma \Gamma' w_1)^{-1} (\tilde{\tau}_w - w_1' \Gamma \hat{\theta})$. Indeed, for that choice,

$$\begin{aligned} w_1' \Gamma \tilde{\theta} &= w_1' \Gamma \hat{\theta} + w_1' \Gamma \Gamma' w_1 (w_1' \Gamma \Gamma' w_1)^{-1} (\tilde{\tau}_w - w_1' \Gamma \hat{\theta}) = \tilde{\tau}_w, \\ \mathbb{E} [\tilde{\theta}] &= \mathbb{E} [\hat{\theta}] + \Gamma' w_1 (w_1' \Gamma \Gamma' w_1)^{-1} (\mathbb{E} [\tilde{\tau}_w] - \mathbb{E} [w_1' \Gamma \hat{\theta}]) \\ &= \theta + \Gamma' w_1 (w_1' \Gamma \Gamma' w_1)^{-1} (w_1' \Gamma \theta - w_1' \Gamma \theta) = \theta. \end{aligned}$$

Under homoskedasticity, the OLS estimator $\hat{\theta}$ is the BLUE for θ in the regression $Y = Z\pi + D\Gamma\theta + \varepsilon$, with variance $\Sigma_{\hat{\theta}}$ that is minimal (in the partial ordering implied by positive semi-definiteness) among the variance of linear unbiased estimators of θ by Gauss–Markov. Hence, $\text{Var}(w_1' \Gamma \hat{\theta}) - \text{Var}(w_1' \Gamma \tilde{\theta}) = w_1' (\Sigma_{\hat{\theta}} - \Sigma_{\tilde{\theta}}) w_1 \leq 0$, establishing efficiency. The efficient linear estimator of τ_w is also unique; indeed, if there was some unbiased linear estimator $\tilde{\tau}_w$ with $\text{Var} [\tilde{\tau}_w] = \text{Var} [\hat{\tau}_w^*]$ but $\mathbb{E} [(\tilde{\tau}_w - \hat{\tau}_w^*)^2] > 0$ (and thus $\text{Cov} [\tilde{\tau}_w, \hat{\tau}_w^*] < \text{Var} [\hat{\tau}_w^*]$), then $\frac{\hat{\tau}_w^* + \tilde{\tau}_w}{2}$ would be an unbiased linear estimator with lower variance.

It remains to argue unbiasedness in the heteroskedastic case. Since the OLS estimator $\hat{\theta}$ remains unbiased for θ even without homoskedasticity, so does $\hat{\tau}_w^* = w_1' \Gamma \hat{\theta}$ for $\tau_w = w_1' \Gamma \theta$. \square

Proof of Theorem 2. The efficient estimator from Theorem 1 is obtained from the OLS estimator $\hat{\tau}$ of τ in $Y = Z\pi + D\tau + \varepsilon$ by setting $\hat{\tau}_w^* = w_1' \hat{\tau}$. We now show that the OLS estimator $\hat{\tau}_w^*$ has the desired imputation form. By Frisch–Waugh–Lovell applied to residualization of Y and Z with respect to D , the estimate $\hat{\pi}$ of π in the linear regression $Y = Z\pi + D\tau + \varepsilon$ is the same as the estimate of π in the linear regression $Y_0 = Z_0\pi + \varepsilon$ restricted to Ω_0 . Indeed,

$$\mathbb{I}_N - D(D'D)^{-1}D = \mathbb{I}_N - \begin{pmatrix} \mathbb{I}_{N_1} & \mathbb{O} \\ \mathbb{O} & \mathbb{O} \end{pmatrix} = \begin{pmatrix} \mathbb{O} & \mathbb{O} \\ \mathbb{O} & \mathbb{I}_{N_0} \end{pmatrix}$$

and thus

$$\begin{aligned} \hat{\pi} &= (Z'(\mathbb{I}_N - D(D'D)^{-1}D)Z)^{-1} Z'(\mathbb{I}_N - D(D'D)^{-1}D)Y \\ &= (Z_0'Z_0)^{-1} Z_0'Y_0. \end{aligned}$$

The OLS estimator $\hat{\tau}$ of τ in $Y = Z\pi + D\tau + \varepsilon$ is then

$$\begin{aligned} \hat{\tau} &= (D'D)^{-1} D'(Y - Z\hat{\pi}) \\ &= Y_1 - Z_1\hat{\pi}, \end{aligned}$$

which has the desired imputation form. \square

Proof of Proposition 6. As in the proof of Theorem 1, there exists an unbiased linear estimator $\hat{\tau}$ of τ such that $\hat{\tau}_w = w_1' \hat{\tau}$. We now construct a linear estimator \hat{C} that is unbiased for $Z_1\pi$, does not

depend on Y_1 , and yields $\hat{\tau}_w = w'_1(Y_1 - \hat{C})$. To this end, let $\hat{C} = Y_1 - \hat{\tau}$. Then \hat{C} is a linear estimator with $\mathbb{E}[\hat{C}] = \mathbb{E}[Y_1] - \mathbb{E}[\hat{\tau}] = Z_1\pi$. Since \hat{C} is linear, we can write $\hat{C} = UY_1 + VY_0$ for matrices U, V . Since

$$\begin{aligned} Z_1\pi &= \mathbb{E}[\hat{C}] = U\mathbb{E}[Y_1] + V\mathbb{E}[Y_0] \\ &= U\tau + (UZ_1 + VZ_0)\pi \end{aligned}$$

for all τ, π , we must have $U = \mathbb{O}$. Therefore, \hat{C} satisfies the requirement of the proposition. \square

Proof of Proposition 7. Writing $v_i = (v_{it})_t$, consistency follows from

$$\begin{aligned} \mathbb{E}[\hat{\tau}_w] &= 0, \\ \text{Var}(\hat{\tau}_w) &= \sigma_w^2 = \sum_{i=1}^I v'_i \Sigma_i v_i \leq \min \left\{ \sum_i \left(\sum_{t; it \in \Omega} |v_{it}| \right)^2, R \left(\sum_{it \in \Omega} v_{it}^2 \right) \right\} \bar{\sigma}^2 \rightarrow 0, \end{aligned}$$

where the first case covers the condition $\sum_i \left(\sum_{t; it \in \Omega} |v_{it}| \right)^2 \rightarrow 0$ from Assumption 6 and the second case covers the alternative condition $R \left(\sum_{it \in \Omega} v_{it}^2 \right) \rightarrow 0$ from Footnote 31. \square

Proof of Proposition 8. Write

$$\hat{\tau}_w - \tau_w = \sum_{it \in \Omega} v_{it} \varepsilon_{it} = \sum_i \eta_i$$

with

$$\eta_i = v'_i \varepsilon_i, \quad \mathbb{E}[\eta_i] = 0, \quad \text{Var}(\eta_i) = v'_i \Sigma_i v_i.$$

Write $p = 2 + \delta$ and let q be the solution to $\frac{1}{p} + \frac{1}{q} = 1$ (so in particular $1 < q < 2 < p$). Using Hölder's inequality to establish that, for any i ,

$$\sum_{t; it \in \Omega} |v_{it}|^{\frac{1}{q}} \left(|v_{it}|^{\frac{1}{p}} |\varepsilon_{it}| \right) \leq \left(\sum_{t; it \in \Omega} |v_{it}|^{\frac{q}{q}} \right)^{\frac{1}{q}} \left(\sum_{t; it \in \Omega} |v_{it}|^{\frac{p}{p}} |\varepsilon_{it}|^p \right)^{\frac{1}{p}},$$

and using $\mathbb{E}[|\varepsilon_{it}|^p] \leq C$ and $\frac{p}{q} + 1 = p$, we have that

$$\begin{aligned}\mathbb{E}[|\eta_i|^{2+\delta}] &= \mathbb{E}\left[\left|\sum_{t;it \in \Omega} v_{it} \varepsilon_{it}\right|^p\right] \leq \mathbb{E}\left[\left(\sum_{t;it \in \Omega} |v_{it} \varepsilon_{it}|\right)^p\right] = \mathbb{E}\left[\left(\sum_{t;it \in \Omega} |v_{it}|^{\frac{1}{q}} |v_{it}|^{\frac{1}{p}} |\varepsilon_{it}|\right)^p\right] \\ &\leq \mathbb{E}\left[\left(\sum_{t;it \in \Omega} |v_{it}|^{\frac{q}{q}}\right)^{\frac{p}{q}} \left(\sum_{t;it \in \Omega} |v_{it}|^{\frac{p}{p}} |\varepsilon_{it}|^p\right)^{\frac{p}{p}}\right] = \left(\sum_{t;it \in \Omega} |v_{it}|^{\frac{p}{q}}\right)^{\frac{p}{q}} \sum_{t;it \in \Omega} |\varepsilon_{it}|^p \\ &\leq \left(\sum_{t;it \in \Omega} |v_{it}|^{\frac{p}{q}+1}\right)^{\frac{p}{q}} C = \left(\sum_{t;it \in \Omega} |v_{it}|^p\right)^{\frac{p}{q}} C.\end{aligned}$$

Hence,

$$\begin{aligned}\frac{\sum_i \mathbb{E}[|\eta_i|^{2+\delta}]}{(\sum_i \text{Var}(\eta_i))^{\frac{2+\delta}{2}}} &= \frac{\sum_i \mathbb{E}[|\eta_i|^{2+\delta}]}{\sigma_w^{2+\delta}} \\ &\leq \frac{\|v\|_{\text{H}}^{2+\delta}}{\sigma_w^{2+\delta}} \frac{\sum_i \left(\sum_{t;it \in \Omega} |v_{it}|\right)^{2+\delta} C}{\|v\|_{\text{H}}^{2+\delta}} = \frac{\|v\|_{\text{H}}^{2+\delta}}{\sigma_w^{2+\delta}} \sum_i \left(\frac{\sum_{t;it \in \Omega} |v_{it}|}{\|v\|_{\text{H}}}\right)^{2+\delta} C \rightarrow 0,\end{aligned}$$

where we have used that $\limsup \|v\|_{\text{H}}^2 / \sigma_w^2 < \infty$ and that $\sum_i \left(\frac{\sum_{t;it \in \Omega} |v_{it}|}{\|v\|_{\text{H}}}\right)^{2+\delta} \rightarrow 0$, and so by the Lyapunov central limit theorem we have that

$$\sigma_w^{-1}(\hat{\tau}_w - \tau_w) \xrightarrow{d} \mathcal{N}(0, 1). \quad \square$$

Proof of Theorem 3. We have that

$$\begin{aligned}\text{for } D_{it} = 0, \quad \hat{\varepsilon}_{it} &= Y_{it} - A'_{it} \hat{\lambda}_i - X'_{it} \hat{\delta} = \varepsilon_{it} - A'_{it}(\hat{\lambda}_i - \lambda_i) - X'_{it}(\hat{\delta} - \delta), \\ \text{for } D_{it} = 1, \quad \hat{\tau}_{it} - \tilde{\tau}_w &= Y_{it} - A'_{it} \hat{\lambda}_i - X'_{it} \hat{\delta} - \tilde{\tau}_w = \varepsilon_{it} + \tau_{it} - \tilde{\tau}_w - A'_{it}(\hat{\lambda}_i - \lambda_i) - X'_{it}(\hat{\delta} - \delta)\end{aligned}$$

so

$$\begin{aligned}&\sum_{t;D_{it}=0} v_{it} \hat{\varepsilon}_{it} + \sum_{t;D_{it}=1} v_{it} (\hat{\tau}_{it} - \tilde{\tau}_w) \\ &= v'_i \varepsilon_i - v'_i A_i (\hat{\lambda}_i - \lambda_i) - v'_i X_i (\hat{\delta} - \delta) + \sum_{t;D_{it}=1} v_{it} (\tau_{it} - \tilde{\tau}_w).\end{aligned}$$

Since the estimator $\hat{\tau}_w$ is invariant with respect to a change in λ_i , and λ_i only appears within unit i with covariates A_i , we must have that

$$0 = \frac{\partial}{\partial \lambda_i} \hat{\tau}_w = \frac{\partial}{\partial \lambda_i} v'_i Y_i = v'_i \left(\frac{\partial}{\partial \lambda_i} Y_i \right) = v'_i A_i.$$

Hence,

$$\hat{\sigma}_w^2 = \sum_{i=1}^I \left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{it}) - v'_i X_i(\hat{\delta} - \delta) \right)^2.$$

We now want to show that $\hat{\sigma}_w^2$ is asymptotically close to $\sum_{i=1}^I \left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{it}) \right)^2$, for which we will establish consistency with respect to $\sigma_w^2 + \sigma_\tau^2$ below. We have

$$\begin{aligned} & \|v\|_{\mathbf{H}}^{-2} \left| \sum_i \left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{it}) \right)^2 - \hat{\sigma}_w^2 \right| \\ & \leq \|v\|_{\mathbf{H}}^{-2} \sum_i \left(\sum_{t, D_{it}=1} v_{it}(\tilde{\tau}_{it} - \bar{\tau}_{it}) + v'_i X_i(\hat{\delta} - \delta) \right)^2 \\ & \quad + 2\|v\|_{\mathbf{H}}^{-2} \left| \sum_i \left(\sum_{t, D_{it}=1} v_{it}(\tilde{\tau}_{it} - \bar{\tau}_{it}) + v'_i X_i(\hat{\delta} - \delta) \right) \left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{it}) \right) \right| \\ & \leq \|v\|_{\mathbf{H}}^{-2} \sum_i \left(\sum_{t, D_{it}=1} v_{it}(\tilde{\tau}_{it} - \bar{\tau}_{it}) + v'_i X_i(\hat{\delta} - \delta) \right)^2 \\ & \quad + 2\sqrt{\|v\|_{\mathbf{H}}^{-2} \sum_i \left(\sum_{t, D_{it}=1} v_{it}(\tilde{\tau}_{it} - \bar{\tau}_{it}) + v'_i X_i(\hat{\delta} - \delta) \right)^2} \sqrt{\|v\|_{\mathbf{H}}^{-2} \sum_i \left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{it}) \right)^2}. \end{aligned}$$

To bound the first term and the first factor of the second term, note that

$$\begin{aligned} & \|v\|_{\mathbf{H}}^{-2} \sum_i \left(\sum_{t, D_{it}=1} v_{it}(\tilde{\tau}_{it} - \bar{\tau}_{it}) + v'_i X_i(\hat{\delta} - \delta) \right)^2 \\ & \leq 2\|v\|_{\mathbf{H}}^{-2} \sum_i \left(\sum_{t, D_{it}=1} v_{it}(\tilde{\tau}_{it} - \bar{\tau}_{it}) \right)^2 + 2\|v\|_{\mathbf{H}}^{-2} \sum_i \left(\sum_{t; it \in \Omega} v'_i X_i(\hat{\delta} - \delta) \right)^2 \\ & \xrightarrow{p} 0 \end{aligned}$$

since $\|v\|_{\mathbf{H}}^{-2} \sum_i \left(\sum_{t, D_{it}=1} v_{it}(\tilde{\tau}_{it} - \bar{\tau}_{it}) \right)^2 \xrightarrow{p} 0$ and Assumption A2 holds. For the second factor of the second term, as well as the consistency of $\sum_{i=1}^I \left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{it}) \right)^2$ itself, note that

$$\mathbb{E} \left[\left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{it}) \right)^2 \right] = v'_i \Sigma_i v_i + \left(\sum_{t, D_{it}=1} v_{it}(\tau_{it} - \bar{\tau}_{it}) \right)^2.$$

and, for $\mathbb{E} [\varepsilon_{it}^4], |\tau_{it}|^4, |\bar{\tau}_{it}|^4 \leq C$,

$$\begin{aligned} \text{Var} \left(\left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it} (\tau_{it} - \bar{\tau}_{it}) \right)^2 \right) &\leq \mathbb{E} \left[\left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it} (\tau_{it} - \bar{\tau}_{it}) \right)^4 \right] \\ &\leq 16 \mathbb{E} \left[(v'_i \varepsilon_i)^4 + \left(\sum_{t, D_{it}=1} v_{it} (\tau_{it} - \bar{\tau}_{it}) \right)^4 \right] \leq 16 (1 + 2^4) \left(\sum_{t; it \in \Omega} |v_{it}| \right)^4 C. \end{aligned}$$

Since $\sum_i \left(\frac{\sum_{t; it \in \Omega} |v_{it}|}{\|v\|_H} \right)^4 \rightarrow 0$, we therefore have that

$$\text{Var} \left(\|v\|_H^{-2} \sum_i \left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it} (\tau_{it} - \bar{\tau}_{it}) \right)^2 \right) \leq 16 (1 + 2^4) \sum_i \left(\frac{\sum_{t; it \in \Omega} |v_{it}|}{\|v\|_H} \right)^4 C \rightarrow 0$$

and thus $\|v\|_H^{-2} \left(\sum_i \left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it} (\tau_{it} - \bar{\tau}_{it}) \right)^2 - \sigma_w^2 - \sigma_\tau^2 \right) \xrightarrow{P} 0$. This establishes consistency of $\sum_{i=1}^I \left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it} (\tau_{it} - \bar{\tau}_{it}) \right)^2$. Since we also have that $\|v\|_H^{-2} (\sigma_w^2 + \sigma_\tau^2)$ is bounded asymptotically, the consistency result also ensures that the last term in the difference $\|v\|_H^{-2} \left| \hat{\sigma}_w^2 - \sum_i \left(v'_i \varepsilon_i + \sum_{t, D_{it}=1} v_{it} (\tau_{it} - \bar{\tau}_{it}) \right)^2 - \sigma_w^2 - \sigma_\tau^2 \right|$ vanishes. Hence, $\|v\|_H^{-2} (\hat{\sigma}_w^2 - \sigma_w^2 - \sigma_\tau^2) \xrightarrow{P} 0$. \square

B.2 Proofs of Appendix Results

Proof of Proposition A1. Since $\hat{\tau}_w$ is linear by assumption we can write $\hat{\tau}_w = v'_1 Y_1 + v'_0 Y_0$. Unbiasedness implies that

$$\mathbb{E} [\hat{\tau}_w] = v'_1 \Gamma \theta + (v'_1 Z_1 + v'_0 Z_0) \pi = w'_1 \Gamma \theta$$

for any θ and π . Hence, $\Gamma' v_1 = \Gamma' w_1$ and $v'_1 Z_1 + v'_0 Z_0 = \mathbf{0}'$. It follows that $\hat{\tau}_w$ is an unbiased estimator of $\tau_v = v'_1 \tau$ for all τ . The remaining proposition is a direct consequence of Proposition 6. \square

Proof of Proposition A2. Write $\Phi_Z = \mathbb{I} - Z(Z'Z)^{-1}Z'$ for the annihilator matrix with respect to the control variables, then

$$\begin{aligned} \Phi &= \Phi_Z|_{\Omega_1 \times \Omega_1} = \mathbb{I} - Z_1(Z'_1 Z_1)^{-1}Z'_1 \\ &= \mathbb{I} - Z_1(Z'_1 Z_1 + Z'_0 Z_0)^{-1}Z'_1 \\ \Phi_0 &= \Phi_Z|_{\Omega_0 \times \Omega_1} = -Z_0(Z'_1 Z_1)^{-1}Z'_1 \\ &= -Z_0(Z'_1 Z_1 + Z'_0 Z_0)^{-1}Z'_1. \end{aligned}$$

By Frisch–Waugh–Lovell for the OLS estimator $\hat{\theta}$ of θ ,

$$\hat{\theta} = ((D\Gamma)' \Phi_Z (D\Gamma))^{-1} (D\Gamma)' \Phi_Z Y$$

and, since $v^{*'}Y = \hat{\tau}_w^* = w_1'\Gamma\hat{\theta}$,

$$\begin{aligned} v^* &= \Phi_Z D\Gamma ((D\Gamma)'\Phi_Z(D\Gamma))^{-1} \Gamma' w_1 \\ &= \begin{pmatrix} \Phi \\ \Phi_0 \end{pmatrix} \Gamma(\Gamma'\Phi\Gamma)^{-1} \Gamma' w_1 \\ &= \left(\begin{pmatrix} \mathbb{I} \\ \mathbb{O} \end{pmatrix} - Z(Z'Z)^{-1}Z_1' \right) \Gamma(\Gamma'(\mathbb{I} - Z_1(Z'Z)^{-1}Z_1')\Gamma)^{-1} \Gamma' w_1, \end{aligned}$$

as required by the proposition.

When Γ is invertible then $\Gamma(\Gamma'\Phi\Gamma)^{-1}\Gamma' = \Phi^{-1}$ (where we note that Φ is invertible by the assumption that τ is identified, which requires that $\Phi_Z D$ is not collinear or, equivalently, that $D'\Phi_Z D = \Phi$ is not singular), simplifying the expression to

$$v^* = \begin{pmatrix} \Phi \\ \Phi_0 \end{pmatrix} \Gamma(\Gamma'\Phi\Gamma)^{-1} \Gamma' w_1 = \begin{pmatrix} w_1 \\ \Phi_0 \Phi^{-1} w_1 \end{pmatrix}.$$

To simplify $\Phi_0 \Phi^{-1}$, we note that

$$\begin{aligned} (Z'Z)^{-1}Z_1' &= (Z_0'Z_0)^{-1}Z_0'Z_0(Z'Z)^{-1}Z_1' \\ &= (Z_0'Z_0)^{-1}(Z'Z(Z'Z)^{-1} - Z_1'Z_1(Z'Z)^{-1})Z_1' \\ &= (Z_0'Z_0)^{-1}Z_1'(\mathbb{I} - Z_1(Z'Z)^{-1}Z_1') \end{aligned}$$

and thus

$$\Phi_0 \Phi^{-1} = -Z_0(Z'Z)^{-1}Z_1'(\mathbb{I} - Z_1(Z'Z)^{-1}Z_1')^{-1} = -Z_0(Z_0'Z_0)^{-1}Z_1',$$

which shows the expression for $\Gamma = \mathbb{I}_{N_1}$ and thus concludes the proof. \square

Proof of Proposition A3. Write $\Phi_Z = \mathbb{I} - Z(Z'Z)^{-1}Z'$ for the annihilator matrix with respect to fixed effects, and $\Phi = \Phi_Z|_{\Omega_1 \times \Omega_1}$ (a $N_1 \times N_1$ matrix) and $\Phi_0 = \Phi_Z|_{\Omega_1 \times \Omega_0}$ (a $N_1 \times N_0$ matrix) for its relevant components, where components are ordered such that $D'\Phi_Z = (\Phi, \Phi_0)$. The OLS estimator in Theorem 1 is

$$\begin{aligned} \hat{\theta} &= (\Gamma'D'\Phi_Z D\Gamma)^{-1} \Gamma'D'\Phi_Z Y \\ &= (\Gamma'\Phi\Gamma)^{-1} \Gamma'(\Phi Y_1 + \Phi_0 Y_0), \end{aligned}$$

which also implies that

$$\hat{\tau}_w = w_1' \Gamma(\Gamma'\Phi\Gamma)^{-1} \Gamma'(\Phi Y_1 + \Phi_0 Y_0). \quad (20)$$

We now show that the weights that this estimator puts on Y_1 and Y_0 , respectively, are the same as those stated in the proposition. We can solve the optimization problem for v_1^* e.g. from the

Lagrangian relaxation

$$\min_v v' \Phi^{-1} v - 2\lambda' \Gamma'(v - w_1)$$

with FOC $\Gamma\lambda = \Phi^{-1}v$, which is solved for $v_1^* = \Phi\Gamma(\Gamma'\Phi\Gamma)^{-1}\Gamma'w_1$ with $\lambda = (\Gamma'\Phi\Gamma)^{-1}\Gamma'w_1$. Here, Φ^{-1} is the variance

$$\text{Var}((D'(\mathbb{I} - Z(Z'Z)^{-1}Z')D)^{-1}D'(\mathbb{I} - Z(Z'Z)^{-1}Z')Y) = (D'(\mathbb{I} - Z(Z'Z)^{-1}Z')D)^{-1} = \Phi^{-1}$$

of the OLS estimator of τ with unrestricted heterogeneity, homoskedasticity, and unit variance. The solution v_1^* is the same weight as the weight the estimator (20) puts on Y_1 . The weight on Y_0 is then implied by imputation. Indeed, the imputation estimator from Theorem 2 with weights v_1^* is

$$(v_1^*)'(Y_1 + \Phi^{-1}\Phi_0Y_0)$$

e.g. by Proposition A2 with $\Gamma = \mathbb{I}_{N_1}$, which is the same as $\hat{\tau}_w$ in (20) for $v_1^* = \Phi\Gamma(\Gamma'\Phi\Gamma)^{-1}\Gamma'w_1$. \square

Proof of Proposition A4. In order to establish consistency, we show that the Herfindahl condition from Assumption 6 is fulfilled for $\hat{\tau}_w^*$, that is, $\|v^*\|_H^2 \rightarrow 0$, which allows us to invoke Proposition 7. As a preliminary result, note that for any $\kappa \geq 1$ and any a_1, \dots, a_T Jensen's inequality implies

$$\left(\sum_{t=1}^T a_t\right)^\kappa \leq T^{\kappa-1} \sum_{t=1}^T |a_t|^\kappa. \quad (21)$$

Assume without loss of generality that $\beta_1 = 0$. To establish consistency of $\hat{\tau}_w^*$, we want to bound its implied weights v_{it}^* . Consider the alternative unbiased linear estimator

$$\hat{\alpha}_i^\# = Y_{i1}, \quad \hat{\beta}_t^\# = \frac{\sum_{i;it \in \Omega_0} (Y_{it} - Y_{i1})}{\sum_{i;it \in \Omega_0} 1}$$

of the unit and time fixed effects. Since $\sum_{it \in \Omega_0} v_{it}^* Y_{it}$ is the best linear unbiased estimator for

$(-\sum_{it \in \Omega_1} w_{it}(\alpha_i + \beta_t))$ under homoskedasticity,

$$\begin{aligned}
\sum_{it \in \Omega_0} (v_{it}^*)^2 &\leq \text{Var}^\# \left[-\sum_{it \in \Omega_1} w_{it}(\hat{\alpha}_i^\# + \hat{\beta}_t^\#) \right] \\
&= \text{Var}^\# \left[\sum_{i=1}^I \left(\sum_{t; it \in \Omega_1} w_{it} \right) \hat{\alpha}_i^\# + \sum_{t=2}^T \left(\sum_{i; it \in \Omega_1} w_{it} \right) \hat{\beta}_t^\# \right] \\
&= 2\text{Var}^\# \left[\sum_{i=1}^I \left(\sum_{t; it \in \Omega_1} w_{it} \right) Y_{i1} \right] + 2\text{Var}^\# \left[\sum_{t=2}^T \left(\sum_{i; it \in \Omega_1} w_{it} \right) \left(\frac{\sum_{i; it \in \Omega_0} (Y_{it} - Y_{i1})}{\sum_{i; it \in \Omega_0} 1} \right) \right] \\
&\leq 2 \sum_{i=1}^I \text{Var}^\# \left[\left(\sum_{t; it \in \Omega_1} w_{it} \right) Y_{i1} \right] + 2T \sum_{t=2}^T \text{Var}^\# \left[\left(\sum_{i; it \in \Omega_1} w_{it} \right) \left(\frac{\sum_{i; it \in \Omega_0} (Y_{it} - Y_{i1})}{\sum_{i; it \in \Omega_0} 1} \right) \right] \\
&= 2 \sum_{i=1}^I \left(\sum_{t; it \in \Omega_1} w_{it} \right)^2 \text{Var}^\# [Y_{i1}] + 2T \sum_{t=2}^T \left(\sum_{i; it \in \Omega_1} w_{it} \right)^2 \frac{\text{Var}^\# [\sum_{i; it \in \Omega_0} (Y_{it} - Y_{i1})]}{\left(\sum_{i; it \in \Omega_0} 1 \right)^2} \\
&= 2 \sum_{i=1}^I \left(\sum_{t; it \in \Omega_1} w_{it} \right)^2 \text{Var}^\# [Y_{i1}] + 2T \sum_{t=2}^T \left(\sum_{i; it \in \Omega_1} w_{it} \right)^2 \frac{\sum_{i; it \in \Omega_0} \text{Var}^\# [Y_{it} - Y_{i1}]}{\left(\sum_{i; it \in \Omega_0} 1 \right)^2} \\
&\leq 2 \sum_{i=1}^I \left(\sum_{t; it \in \Omega_1} w_{it} \right)^2 + 8T \sum_{t=2}^T \frac{\left(\sum_{i; it \in \Omega_1} w_{it} \right)^2}{\sum_{i; it \in \Omega_0} 1}
\end{aligned}$$

for $\text{Var}^\#$ the variance for homoskedastic errors ε_{it} with unit variance, where we use (21) for $\kappa = 2$. Hence we have that

$$\begin{aligned}
\sum_i \left(\sum_{t; it \in \Omega} |v_{it}^*| \right)^2 &\leq 2 \sum_{i=1}^I \left(\sum_{t; it \in \Omega_1} |v_{it}^*| \right)^2 + 2 \sum_{i=1}^I \left(\sum_{t; it \in \Omega_0} |v_{it}^*| \right)^2 \\
&\leq 2 \sum_{i=1}^I \left(\sum_{t; it \in \Omega_1} |v_{it}^*| \right)^2 + 2T \sum_{it \in \Omega_0} (v_{it}^*)^2 \\
&\leq 2 \sum_{i=1}^I \left(\sum_{t; it \in \Omega_1} |w_{it}| \right)^2 + 4T \sum_{i=1}^I \left(\sum_{t; it \in \Omega_1} w_{it} \right)^2 + 16T^2 \sum_{t=2}^T \frac{\left(\sum_{i; it \in \Omega_1} w_{it} \right)^2}{\sum_{i; it \in \Omega_0} 1} \rightarrow 0
\end{aligned}$$

under Assumption A4, which implies the Herfindahl condition (Assumption 6) and thus consistency by Proposition 7. \square

Proof of Proposition A5. In order to establish asymptotic Normality, we want to establish that $\sum_i \left(\frac{\sum_{t; it \in \Omega} |v_{it}|}{\|v\|_{\text{H}}} \right)^{2+\delta} \rightarrow 0$ for some $\delta > 0$ (Assumption A1), which allows us to invoke Proposition 8. By construction and since $\hat{\alpha}_i$ is the least-squares solution to $\sum_{it; D_{it}=0} (Y_{it} - \hat{\alpha}_i - \hat{\beta}_t)^2$,

$$\hat{\tau}_w^* = \sum_{it; D_{it}=1} w_{it} (Y_{it} - \hat{\alpha}_i - \hat{\beta}_t), \quad \hat{\alpha}_i = \frac{\sum_{t; D_{it}=0} (Y_{it} - \hat{\beta}_t)}{\sum_{t; D_{it}=0} 1},$$

so we can express

$$\begin{aligned} \hat{\tau}_w^* &= \sum_{it; D_{it}=1} w_{it} \left(Y_{it} - \frac{\sum_{s; D_{is}=0} (Y_{is} - \hat{\beta}_s)}{\sum_{s; D_{is}=0} 1} - \hat{\beta}_t \right), \\ &= \sum_{it; D_{it}=1} w_{it} Y_{it} + \sum_{it; D_{it}=0} \frac{-\sum_{s; D_{is}=1} w_{is}}{\sum_{s; D_{is}=0} 1} Y_{it} + \sum_{t=2}^T \left(\sum_{i; D_{it}=0} \frac{\sum_{s; D_{is}=1} w_{it}}{\sum_{s; D_{is}=0} 1} - \sum_{i; D_{it}=1} w_{it} \right) \hat{\beta}_t \\ &= \sum_{it; D_{it}=1} w_{it} Y_{it} + \sum_{it; D_{it}=0} \frac{-\sum_{s; D_{is}=1} w_{is}}{\sum_{s; D_{is}=0} 1} Y_{it} + \sum_{it; D_{it}=0} u_{it} Y_{it} \\ &= \sum_{i=1}^I \left(\sum_{t=1}^{E_i-1} \left(u_{it} - \frac{\sum_{s; D_{is}=1} w_{is}}{\sum_{s; D_{is}=0} 1} \right) Y_{it} + \sum_{t=E_i}^T w_{it} Y_{it} \right) \end{aligned}$$

where the u_{it} are defined as the weights on the Y_{it} , $it \in \Omega_0$ in $\sum_{t=2}^T \left(\sum_{i; D_{it}=0} \frac{\sum_{s; D_{is}=1} w_{it}}{\sum_{s; D_{is}=0} 1} - \sum_{i; D_{it}=1} w_{it} \right) \hat{\beta}_t$, and we write $E_i = T + 1$ for the never-treated cohort. Hence, we can write

$$v_{it}^* = \begin{cases} w_{it}, & it \in \Omega_1, \\ u_{it} - \frac{\sum_{s; D_{is}=1} w_{is}}{\sum_{s; D_{is}=0} 1}, & it \in \Omega_0. \end{cases} \quad (22)$$

It follows with (21) in the previous proof for $\kappa = 2 + \delta$ and some $\delta > 0$ (and specifically for $\delta = 2$, which we will use in the proof of Proposition A6) that

$$\begin{aligned} \sum_{i=1}^I \left(\sum_{t=1}^T |v_{it}^*| \right)^{2+\delta} &\leq T^{1+\delta} \sum_{it \in \Omega} |v_{it}^*|^{2+\delta} = T^{1+\delta} \left(\sum_{it \in \Omega_1} |w_{it}|^{2+\delta} + \sum_{it \in \Omega_0} \left| u_{it} - \frac{\sum_{s; D_{is}=1} w_{is}}{\sum_{s; D_{is}=0} 1} \right|^{2+\delta} \right) \\ &\leq T^{1+\delta} \left(\sum_{it \in \Omega_1} |w_{it}|^{2+\delta} + 2^{1+\delta} \sum_{it \in \Omega_0} |u_{it}|^{2+\delta} + 2^{1+\delta} \sum_{it \in \Omega_0} \left| \frac{\sum_{s; D_{is}=1} w_{is}}{\sum_{s; D_{is}=0} 1} \right|^{2+\delta} \right) \\ &\leq (2T)^{1+\delta} \left(\sum_{it \in \Omega_1} |w_{it}|^{2+\delta} + \sum_{it \in \Omega_0} |u_{it}|^{2+\delta} + \sum_{i=1}^I \frac{\left| \sum_{t; D_{it}=1} w_{is} \right|^{2+\delta}}{\left| \sum_{t; D_{it}=0} 1 \right|^{1+\delta}} \right) \\ &\leq (2T)^{1+\delta} \left(\sum_{it \in \Omega_1} |w_{it}|^{2+\delta} + \sum_{it \in \Omega_0} |u_{it}|^{2+\delta} + T^{1+\delta} \sum_{it \in \Omega_1} |w_{it}|^{2+\delta} \right) \\ &\leq (2T)^{2(1+\delta)} \left(\sum_{it \in \Omega_1} |w_{it}|^{2+\delta} + \sum_{it \in \Omega_0} |u_{it}|^{2+\delta} \right). \end{aligned}$$

We now consider the two parts of this sum. First, note that there is only a finite number of cohort-period cells kt with $E_i = k \geq t$, and within every such cell

$$\begin{aligned} \frac{\sum_{i; E_i=k} |w_{it}|^{2+\delta}}{(\sum_{i; E_i=k} |w_{it}|^2)^{\frac{2+\delta}{2}}} &\leq \frac{\max_{i; E_i=k} |w_{it}|^{2+\delta} \sum_{i; E_i=k} 1}{\min_{i; E_i=k} |w_{it}|^{2+\delta} (\sum_{i; E_i=k} 1)^{\frac{2+\delta}{2}}} \\ &\leq C^{2+\delta} \frac{1}{(\sum_{i; E_i=k} 1)^{\frac{\delta}{2}}} \rightarrow 0, \end{aligned}$$

where we have used At the same time, the u_{it} only depend on cohort and period (since the time fixed effects are invariant to exchanging units identities within cohorts), $u_{it} = U_{E_i, t}$, so similarly, for $E_i = k < t$,

$$\frac{\sum_{i; E_i=k} |u_{it}|^{2+\delta}}{(\sum_{i; E_i=k} |u_{it}|^2)^{\frac{2+\delta}{2}}} = \frac{\sum_{i; E_i=k} |U_{kt}|^{2+\delta}}{(\sum_{i; E_i=k} |U_{kt}|^2)^{\frac{2+\delta}{2}}} = \frac{1}{(\sum_{i; E_i=k} 1)^{\frac{\delta}{2}}} \rightarrow 0.$$

This shows, in particular, that the $\frac{\sum_{i; E_i=k} |w_{it}|^{2+\delta}}{(\sum_{i; E_i=k} |w_{it}|^2)^{\frac{2+\delta}{2}}}$ and $\frac{\sum_{i; E_i=k} |u_{it}|^{2+\delta}}{(\sum_{i; E_i=k} |u_{it}|^2)^{\frac{2+\delta}{2}}}$ vanish for all k and t , from which we now derive that $\frac{\sum_{it \in \Omega} |v_{it}^*|^{2+\delta}}{(\sum_{it \in \Omega} |v_{it}^*|^2)^{\frac{2+\delta}{2}}}$ also vanishes. To this end, we note that

$$\sum_{it \in \Omega_0} \left| \frac{\sum_{s; D_{is}=1} w_{is}}{\sum_{s; D_{is}=0} 1} \right|^2 \leq \sum_{i=1}^I \left| \sum_{t; it \in \Omega_1} w_{it} \right|^2 \leq \sum_{it \in \Omega_1} w_{it}^2.$$

Using (22), we obtain that

$$\begin{aligned} \frac{1}{2} \sum_{it \in \Omega_0} u_{it}^2 &= \frac{1}{2} \sum_{it \in \Omega_0} \left(v_{it}^* + \frac{\sum_{s; D_{is}=1} w_{is}}{\sum_{s; D_{is}=0} 1} \right)^2 \\ &\leq \sum_{it \in \Omega_0} \left| \frac{\sum_{s; D_{is}=1} w_{is}}{\sum_{s; D_{is}=0} 1} \right|^2 + \sum_{it \in \Omega_0} |v_{it}^*|^2 \\ &\leq \sum_{it \in \Omega_1} w_{it}^2 + \sum_{it \in \Omega_0} |v_{it}^*|^2 = \sum_{it \in \Omega} |v_{it}^*|^2. \end{aligned}$$

At the same time, $\sum_{it \in \Omega} |v_{it}^*|^2 \geq \sum_{it \in \Omega_1} w_{it}^2$. Putting everything together, we conclude that

$$\begin{aligned} \frac{\sum_{it \in \Omega} |v_{it}^*|^{2+\delta}}{(\sum_{it \in \Omega} |v_{it}^*|^2)^{\frac{2+\delta}{2}}} &\leq (2T)^{2(1+\delta)} \frac{\sum_{it \in \Omega_1} |w_{it}|^{2+\delta} + \sum_{it \in \Omega_0} |u_{it}|^{2+\delta}}{(\sum_{it \in \Omega} |v_{it}^*|^2)^{\frac{2+\delta}{2}}} \\ &\leq (2T)^{2(1+\delta)} \sum_{kt; k \leq t} \frac{\sum_{i; E_i=k} |w_{it}|^{2+\delta}}{(\sum_{i; E_i=k} |w_{it}|^2)^{\frac{2+\delta}{2}}} + 2(2T)^{2(1+\delta)} \sum_{kt; k > t} \frac{\sum_{i; E_i=k} |u_{it}|^{2+\delta}}{(\sum_{i; E_i=k} |u_{it}|^2)^{\frac{2+\delta}{2}}} \rightarrow 0. \end{aligned}$$

Since also $\|v\|_H^2 = \sum_i \left(\sum_{t; it \in \Omega} |v_{it}^*| \right)^2 \geq \sum_{it \in \Omega} |v_{it}^*|^2$, we conclude that

$$\sum_i \left(\frac{\sum_{t; it \in \Omega} |v_{it}|}{\|v\|_H} \right)^{2+\delta} \leq \frac{\sum_{it \in \Omega} |v_{it}^*|^{2+\delta}}{(\sum_{it \in \Omega} |v_{it}^*|^2)^{\frac{2+\delta}{2}}} \rightarrow 0,$$

yielding Assumption A1. This allows us to invoke Proposition 8 to obtain asymptotic Normality. \square

Proof of Proposition A6. We show that the assumptions of this proposition imply the assumptions of Theorem 3, which guarantee that standard errors are asymptotically conservative. The two proceeding proofs (of Propositions A4 and A5) establish that Assumptions 6 and A3 hold. It remains to show that

$$\|v\|_H^{-2} \sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* (\tilde{\tau}_{it} - \bar{\tau}_{it}) \right)^2 \xrightarrow{p} 0, \quad \|v\|_H^{-2} \sum_i \left(\sum_{t; it \in \Omega} v_{it}^* (\hat{\beta}_t - \beta_t) \right)^2 \xrightarrow{p} 0. \quad (23)$$

We first consider the left expression for $\tilde{\tau}_{it} = \tilde{\tau}_{it}^A \equiv \tilde{\tau} = \frac{\sum_i (\sum_{t; it \in \Omega_1} v_{it}^*) (\sum_{t; it \in \Omega_1} v_{it}^* \tilde{\tau}_{it})}{\sum_i (\sum_{t; it \in \Omega_1} v_{it}^*)^2}$, with the corresponding $\bar{\tau}_{it} \equiv \bar{\tau}$. In this case,

$$\begin{aligned} \sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* (\tilde{\tau}_{it} - \bar{\tau}_{it}) \right)^2 &= (\tilde{\tau} - \bar{\tau})^2 \sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* \right)^2 \\ &= \frac{\left(\sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* \right) \left(\sum_{t; it \in \Omega_1} v_{it}^* (\tilde{\tau}_{it} - \tau_{it}) \right) \right)^2}{\sum_i (\sum_{t; it \in \Omega_1} v_{it}^*)^2}. \end{aligned}$$

When $\tilde{\tau}_{it}$ are cohort-horizon cell averages, specifically $\tilde{\tau}_{it} = \tilde{\tau}_{it}^B = \tilde{\tau}_{et} = \frac{\sum_{i; E_i=e} |v_{it}^*|^2 \hat{\tau}_{it}}{\sum_{i; E_i=e} |v_{it}^*|^2}$, and $\bar{\tau}_{it} \equiv \tau_{et}$ are defined correspondingly,

$$\begin{aligned} \sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* (\tilde{\tau}_{it} - \bar{\tau}_{it}) \right)^2 &= \sum_e \sum_{i; E_i=e} \left(\sum_{t \geq e} v_{it}^* (\tilde{\tau}_{et} - \bar{\tau}_{et}) \right)^2 \\ &\leq T \sum_e \sum_{i; E_i=e} \sum_{t \geq e} (v_{it}^* (\tilde{\tau}_{et} - \bar{\tau}_{et}))^2 = T \sum_e \sum_{t \geq e} (\tilde{\tau}_{et} - \bar{\tau}_{et})^2 \sum_{i; E_i=e} |v_{it}^*|^2 \\ &= T \sum_e \sum_{t \geq e} \frac{\left(\sum_{i; E_i=e} |v_{it}^*|^2 (\hat{\tau}_{it} - \tau_{it}) \right)^2}{\sum_{i; E_i=e} |v_{it}^*|^2}. \end{aligned}$$

We finally consider the expression involving the time-fixed effects $\hat{\beta}_t$. Here,

$$\begin{aligned} \sum_i \left(\sum_{t; it \in \Omega} v_{it}^* (\hat{\beta}_t - \beta_t) \right)^2 &\leq T \sum_{it \in \Omega} (\hat{\beta}_t - \beta_t)^2 |v_{it}^*|^2 \\ &\leq T \sum_t (\hat{\beta}_t - \beta_t)^2 \sum_{i; it \in \Omega} |v_{it}^*|^2. \end{aligned}$$

From these three expressions, we conclude that

$$\begin{aligned} \mathbb{E} \left[\sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* (\tilde{\tau}_{it}^A - \bar{\tau}_{it}) \right)^2 \right] &\leq \frac{\text{Var} \left[\sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* \right) \left(\sum_{t; it \in \Omega_1} v_{it}^* \hat{\tau}_{it} \right) \right]}{\sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* \right)^2}, \\ \mathbb{E} \left[\sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* (\tilde{\tau}_{it}^B - \bar{\tau}_{it}) \right)^2 \right] &\leq T \sum_e \sum_{t \geq e} \frac{\text{Var} \left[\sum_{i; E_i = e} |v_{it}^*|^2 \hat{\tau}_{it} \right]}{\sum_{i; E_i = e} |v_{it}^*|^2}, \\ \mathbb{E} \left[\sum_i \left(\sum_{t; it \in \Omega} v_{it}^* (\hat{\beta}_t - \beta_t) \right)^2 \right] &\leq T \sum_t \text{Var} [\hat{\beta}_t] \sum_{i; it \in \Omega} |v_{it}^*|^2. \end{aligned}$$

We now bound the three variances above. Each of them contains a linear estimator. Such a linear estimator $\hat{\eta} = \sum_{it \in \Omega} a_{it} Y_{it}$ has variance

$$\begin{aligned} \text{Var} [\hat{\eta}] &= \sum_i \mathbb{E} \left(\sum_{t; it \in \Omega} a_{it} \varepsilon_{it} \right)^2 \\ &\leq T \sum_{it \in \Omega} a_{it}^2 \mathbb{E} \varepsilon^2 \leq T \bar{\sigma}^2 \sum_{it \in \Omega} a_{it}^2. \end{aligned}$$

We can therefore bound it by $T \bar{\sigma}^2$ times the variance $\text{Var}^\#(\hat{\eta}) = \sum_{it \in \Omega} a_{it}^2$ in data with uncorrelated and homoskedastic errors terms with unit variance. In such data, the estimators $\hat{\tau}_{it}$ and $\hat{\beta}_t$ have minimal variance (by Gauss–Markov), which extends to their weighted averages as in the proof of Theorem 1. Under $\text{Var}^\#$, we can therefore bound the respective variances by the variances using any other linear unbiased estimators of the τ_{it} and β_t . Assuming that we normalize $\beta_1 = 0$, we can

use the estimators $\tilde{\beta}_t = \frac{\sum_{i;E_i=\infty}(Y_{it}-Y_{i1})}{\sum_{i;E_i=\infty}1}$ of β_t and $Y_{it} - Y_{i1} - \tilde{\beta}_t$ of τ_{it} . We find that

$$\begin{aligned}
& \text{Var}^\# \left[\sum_i \left(\sum_{t;it \in \Omega_1} v_{it}^* \right) \left(\sum_{t;it \in \Omega_1} v_{it}^* \hat{\tau}_{it} \right) \right] \leq \text{Var}^\# \left[\sum_i \left(\sum_{t;it \in \Omega_1} v_{it}^* \right) \left(\sum_{t;it \in \Omega_1} v_{it}^* (Y_{it} - Y_{i1} - \tilde{\beta}_t) \right) \right] \\
& \leq 3\text{Var}^\# \left[\sum_i \left(\sum_{t;it \in \Omega_1} v_{it}^* \right)^2 Y_{i1} \right] + 3\text{Var}^\# \left[\sum_i \left(\sum_{t;it \in \Omega_1} v_{it}^* \right) \left(\sum_{t;it \in \Omega_1} v_{it}^* Y_{it} \right) \right] \\
& \quad + 3\text{Var}^\# \left[\sum_i \left(\sum_{t;it \in \Omega_1} v_{it}^* \right) \left(\sum_{t;it \in \Omega_1} v_{it}^* \tilde{\beta}_t \right) \right] \\
& = 3 \sum_i \left(\sum_{t;it \in \Omega_1} v_{it}^* \right)^4 + 3 \sum_i \left(\sum_{t;it \in \Omega_1} v_{it}^* \right)^2 \sum_{t;it \in \Omega_1} |v_{it}^*|^2 + 3\text{Var}^\# \left[\sum_t \tilde{\beta}_t \sum_{i;it \in \Omega_1} v_{it}^* \left(\sum_{s;is \in \Omega_1} v_{it}^* \right) \right] \\
& \leq 3(T+1) \sum_i \left(\sum_{t;it \in \Omega_1} v_{it}^* \right)^2 \sum_{t;it \in \Omega_1} |v_{it}^*|^2 + 3T \sum_t \left(\sum_{i;it \in \Omega_1} v_{it}^* \left(\sum_{s;is \in \Omega_1} v_{it}^* \right) \right)^2 \text{Var}^\# [\tilde{\beta}_t] \\
& \leq 3(T+1) \sum_e \sum_{i;E_i=e} \left(\sum_{t;it \in \Omega_1} v_{it}^* \right)^2 \sum_{t \geq e} \max_{j;E_j=e} w_{jt}^2 + 3T \sum_t \text{Var}^\# [\tilde{\beta}_t] \sum_{i;it \in \Omega_1} |v_{it}^*|^2 \sum_{i;it \in \Omega_1} \left(\sum_{s;is \in \Omega_1} v_{it}^* \right)^2 \\
& \leq 3(T+1) \sum_e \sum_{i;E_i=e} \left(\sum_{t;it \in \Omega_1} v_{it}^* \right)^2 \frac{C \sum_{j;E_j=e} \sum_{t \geq e} w_{jt}^2}{\sum_{j;E_j=e} 1} + 3T \sum_i \left(\sum_{t;it \in \Omega_1} v_{it}^* \right)^2 \sum_{it \in \Omega_1} |v_{it}^*|^2 \sum_t \text{Var}^\# [\tilde{\beta}_t] \\
& \leq 3(T+1) \sum_i \left(\sum_{t;it \in \Omega_1} v_{it}^* \right)^2 \sum_{it \in \Omega_1} |v_{it}^*|^2 \left(\frac{C \sum_{j;E_j=1} \sum_{t \geq e} w_{jt}^2}{\max_e \sum_{j;E_j=e} 1} + \sum_t \text{Var}^\# [\tilde{\beta}_t] \right), \\
& \text{Var}^\# \left[\sum_{i;E_i=e} |v_{it}^*|^2 \hat{\tau}_{it} \right] \leq \text{Var}^\# \left[\sum_{i;E_i=e} |v_{it}^*|^2 (Y_{it} - Y_{i1} - \tilde{\beta}_t) \right] \\
& \leq 3\text{Var}^\# \left[\sum_{i;E_i=e} |v_{it}^*|^2 Y_{it} \right] + 3\text{Var}^\# \left[\sum_{i;E_i=e} |v_{it}^*|^2 Y_{i1} \right] + 3\text{Var}^\# \left[\sum_{i;E_i=e} |v_{it}^*|^2 \tilde{\beta}_t \right] \\
& = 6 \sum_{i;E_i=e} |v_{it}^*|^4 + 3 \left(\sum_{i;E_i=e} |v_{it}^*|^2 \right)^2 \text{Var}^\# [\tilde{\beta}_t] \\
& \leq 6 \sum_{i;E_i=e} |v_{it}^*|^2 \sum_{it \in \Omega_1} |v_{it}^*|^2 \left(\frac{C}{\sum_{i;E_i=e} 1} + \text{Var}^\# [\tilde{\beta}_t] \right), \\
& \text{Var}^\# [\hat{\beta}_t] \leq \text{Var}^\# [\tilde{\beta}_t] = \frac{\text{Var}^\# [\sum_{i;E_i=\infty} (Y_{it} - Y_{i1})]}{\left(\sum_{i;E_i=\infty} 1 \right)^2} \leq \frac{4}{\sum_{i;E_i=\infty} 1} \rightarrow 0.
\end{aligned}$$

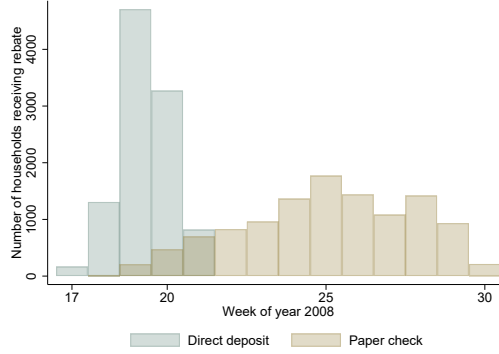
Putting everything together,

$$\begin{aligned}
& \mathbb{E} \left[\|v\|_{\mathbf{H}}^{-2} \sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* (\tilde{\tau}_{it}^A - \bar{\tau}_{it}) \right)^2 \right] \leq T \bar{\sigma}^2 \frac{\text{Var}^\# \left[\sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* \right) \left(\sum_{t; it \in \Omega_1} v_{it}^* \hat{\tau}_{it} \right) \right]}{\|v\|_{\mathbf{H}}^2 \sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* \right)^2} \\
& \leq 3(T+1)T \bar{\sigma}^2 \frac{\sum_{it \in \Omega_1} |v_{it}^*|^2}{\|v\|_{\mathbf{H}}^2} \left(\frac{C \sum_{j; E_j=1} \sum_{t \geq e} w_{jt}^2}{\max_e \sum_{j; E_j=e} 1} + \sum_t \text{Var}^\# [\tilde{\beta}_t] \right) \rightarrow 0, \\
& \mathbb{E} \left[\|v\|_{\mathbf{H}}^{-2} \sum_i \left(\sum_{t; it \in \Omega_1} v_{it}^* (\tilde{\tau}_{it}^B - \bar{\tau}_{it}) \right)^2 \right] \leq T \bar{\sigma}^2 \frac{T \sum_e \sum_{t \geq e} \frac{\text{Var}^\# \left[\sum_{i; E_i=e} |v_{it}^*|^2 \tilde{\tau}_{it} \right]}{\sum_{i; E_i=e} |v_{it}^*|^2}}{\|v\|_{\mathbf{H}}^2} \\
& \leq 6T^2 \bar{\sigma}^2 \frac{\sum_{it \in \Omega_1} |v_{it}^*|^2}{\|v\|_{\mathbf{H}}^2} \sum_e \sum_{t \geq e} \left(\frac{C}{\sum_{i; E_i=e} 1} + \text{Var}^\# [\tilde{\beta}_t] \right) \rightarrow 0, \\
& \mathbb{E} \left[\|v\|_{\mathbf{H}}^{-2} \sum_i \left(\sum_{t; it \in \Omega} v_{it}^* (\hat{\beta}_t - \beta_t) \right)^2 \right] \leq T \bar{\sigma}^2 \sum_t \text{Var}^\# [\hat{\beta}_t] \frac{\sum_{i; it \in \Omega} |v_{it}^*|^2}{\|v\|_{\mathbf{H}}} \rightarrow 0.
\end{aligned}$$

This establishes (23), and thus the conditions of Theorem 3. \square

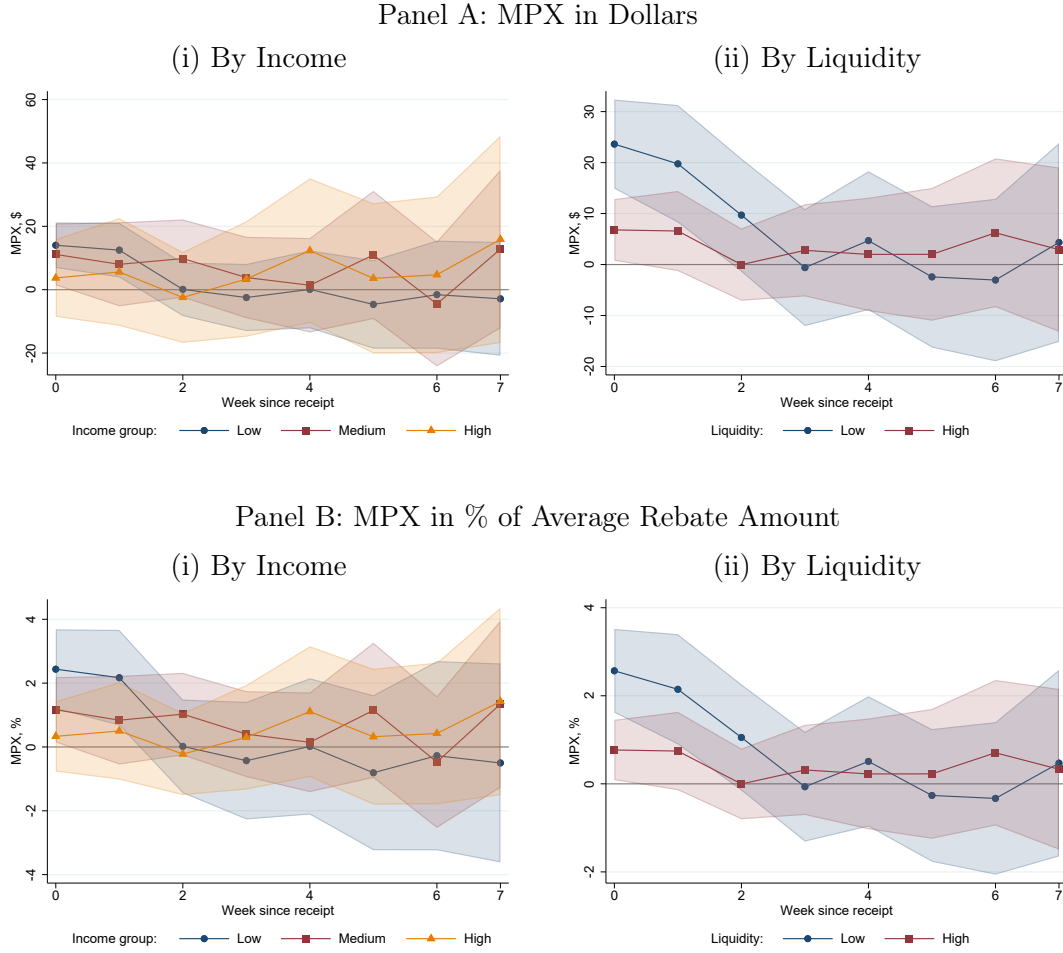
Appendix Figures and Tables

Figure A1: Timing of Rebate Receipt by Disbursement Method



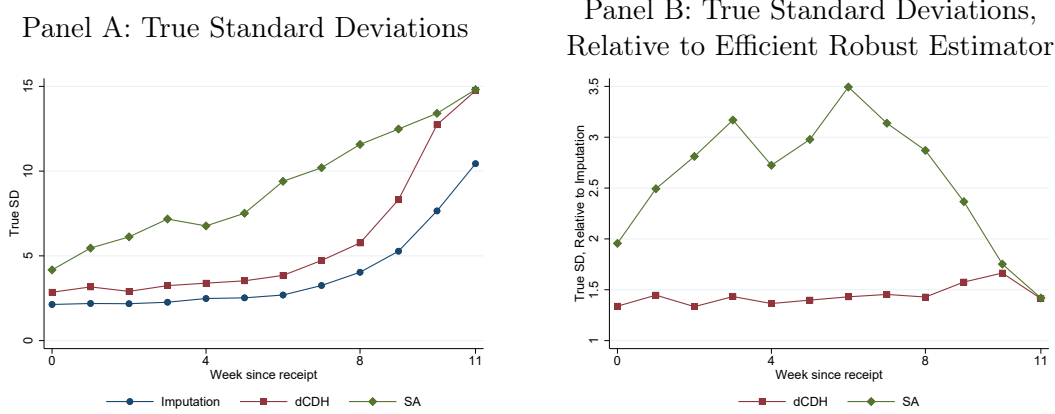
Notes: This figure reports the number of households in the sample who report receiving the tax rebate. by week of year 2008 and by disbursement method: direct deposit and paper check. Seventy households in the sample with unknown disbursement method are not shown. Week 17 ends April 26; week 30 ends July 26.

Figure A2: Heterogeneity in MPX across Household Groups, by Liquidity and Income



Notes: Panel A reports MPX coefficients and 95% confidence bands using the robust imputation estimator for subsamples based on household characteristics, in parallel to Tables 7 and 8 of Broda and Parker (2014). Figure (i) splits households into three groups by 2007 household income: below \$35,000 (“low”), between \$35,000 and \$70,000 (“medium”), and above \$70,000 (“high”). Figure (ii) splits them into two groups according to the survey question: “*In case of an unexpected decline in income or increase in expenses, do you have at least two months of income available in cash, bank accounts, or easily accessible funds?*” Answer “No” corresponds to low liquidity, and “Yes” to high liquidity. All specifications include interactions between disbursement method and week FEs. In Panel B, the MPX estimates in dollars are rescaled by the average tax rebate amount in each household group.

Figure A3: Efficiency of Alternative Robust MPX Estimators, Wild Clustered Bootstrap



Notes: Panel A reports the true standard deviations of three robust estimators — the efficient imputation estimator, de Chaisemartin and D’Haultfœuille (2021) (dCDH), and Sun and Abraham (2021) (SA) — under the wild clustered bootstrap data-generating process detailed in Appendix A.12. These standard deviations are computed based on the weights underlying each estimator, without any simulation, as in Figure 7. The specifications do not include disbursement method fixed effects. Panel B reports the ratios of the standard deviations for dCDH and SA relative to the imputation estimator.

Table A1: A Three-Unit, Three-Period Example

$\mathbb{E}[Y_{it}]$	$i = A$	$i = B$	$i = C$
$t = 1$	α_A	α_B	α_C
$t = 2$	$\alpha_A + \beta_2 + \tau_{A2}$	$\alpha_B + \beta_2$	$\alpha_C + \beta_2$
$t = 3$	$\alpha_A + \beta_3 + \tau_{A3}$	$\alpha_B + \beta_3 + \tau_{B3}$	$\alpha_C + \beta_3$
Event date	$E_i = 2$	$E_i = 3$	$E_i = 4$