

Difference-in-Differences with Unequal Baseline

Treatment Status*

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Abstract

We study difference-in-differences (DiD) settings where groups experience differential treatment status in the baseline period, commonly employed in empirical studies. We show that unless the treatment effect of the studied policy is immediate and constant over time, the standard DiD approach fails to recover the average treatment effect. Furthermore, the usual parallel trends test is invalid, meaning one may find pre-trends when the parallel trends assumption holds, or may fail to detect differences in pre-trends because these differences cancel out with the treatment effect. We further show that intuitive solutions such as estimation in reverse or including a linear trend term do not resolve these problems in most general settings.

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Keywords: differences-in-differences, parallel trends assumption, panel data, policy evaluation

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In a standard difference-in-differences (DiD) framework, the comparison group remains untreated throughout the period of study, while the treatment group is treated at some point in time. Thus, both groups start off untreated, and then only one of the groups experiences treatment. Many empirical studies, however, employ this setup loosely, instead comparing a group that experiences a change to a group that does not, which typically implies comparing groups with different treatment status in the pre-policy period. We will refer to these types of frameworks as “unequal baseline DiD.” The goal of this paper is to evaluate the validity of the unequal baseline DiD approach. We show that under a wide range of assumptions, the traditional DiD estimation approach does not recover the average treatment effect on the treated (ATT) and that the usual parallel trends test is inappropriate.

Unequal baseline DiD approach is ubiquitously used by empirical tax economists to measure causal effects of taxes. A typical study, e.g. [Kleven and Schultz \(2014\)](#), exploits differential changes in marginal tax rates (MTRs) across different tax brackets and changes in bracket cutoffs to estimate elasticities of taxable income. In such studies, all individuals are treated (i.e. subject to a positive MTR) in both periods, but the intensity of treatment (i.e. MTRs) differs across time and groups. Importantly, in most cases, individuals experience different MTRs and hence treatment intensity in the baseline period. Similar approaches have been used to study wealth taxes, dividend taxes, corporate taxes, and other taxes (e.g., [Jakobsen et al., 2020](#); [Yagan, 2015](#); [Fuest et al., 2018](#)).

Beyond tax studies, unequal baseline DiD studies have been employed in other settings: in economics of education to study effects of affirmative action bans ([Bleemer, 2022](#)); in health economics to study consequences of lead exposure ([Grönqvist et al., 2020](#)); in labor economics to study employment effects of working hour reductions ([Chemin and Wasmer, 2009](#)); in environmental economics to study energy effects of daylight savings time ([Kotchen and Grant, 2011](#)); in development economics to study rebellions ([Cao and Chen, 2022](#)), malaria eradication programs ([Bleakley, 2010](#); [Cutler et al., 2010](#); [Lucas, 2010](#); [Rossi and Villar, 2020](#)), and management institutions ([Sawada et al., 2022](#)); to name just a few.

Our analysis proceeds as follows. First, we characterize theoretically the parameters that can be recovered using an unequal baseline DiD and discuss under which conditions they have a causal interpretation. Second, we illustrate implications of our theoretical results in empirical settings. In particular, we show how two frequently

featured settings – policy reversals and universal policy adoptions – diverge from the canonical DiD setting in which groups experience equal baseline status. Finally, we evaluate the validity of two solutions – estimation in reverse and inclusion of a linear trend, and provide practical recommendations for empirical researchers.

Our theoretical framework considers a population with two groups. The first group, the stayers, starts and remains at treatment level d_0 , while the second group, the switchers, starts at treatment level d_{pre} and then switches to treatment level d_{post} starting at time period t^* . We analyze the OLS estimators from the commonly used event-study specification:

$$Y_{it} = \alpha_i + \delta_t + \sum_{\ell < t^*, \ell \neq \tilde{t}} \theta_\ell S_i \mathbb{1}(t = \ell) + \sum_{\ell \geq t^*} \beta_\ell S_i \mathbb{1}(t = \ell) + \varepsilon_{it},$$

where period $\tilde{t} < t^*$ is the baseline (omitted) period. We refer to the coefficients β_ℓ as the “lag coefficients” (post-policy change coefficients), and the coefficients θ_ℓ as the “lead coefficients” (pre-policy change coefficients), also sometimes known as placebo coefficients. We show that under the traditional parallel trends assumption, i.e. that the evolution of outcomes is similar for the two groups at the *stayers’ baseline treatment status* d_0 , the ATT is generally not identified, even under the usual parallel trends assumption. Furthermore, the usual pre-treatment trends (“parallel trends”) test does not provide evidence in support of the parallel trends assumption. The conclusions are qualitatively equivalent under an alternative definition of parallel trends – where the evolution of outcomes is similar under the *switchers’ baseline treatment status* d_{pre} . Finally, we demonstrate that although the ATT is identified if average potential outcomes evolve similarly under *each own baseline treatment statuses* d_0 and d_{pre} , this parallel trends assumption effectively rules out non-constant treatment effects.

Next, we apply our theoretical results to several common empirical frameworks. Figure 1 provides a visual illustration of the issues for a hypothetical policy that is initially introduced in period t_{-1} for a subgroup of the population, after which it is cancelled in period t^* .¹ Panel A shows the evolution of outcome of interest for truly comparable treatment and comparison groups (under stayers’ treatment status), while Panel B assumes groups are actually not comparable.

¹Such settings have been studied by [Bleemer \(2022\)](#), [Cao and Chen \(2022\)](#), [Jakobsen et al. \(2020\)](#), [Bleakley \(2010\)](#), [Cutler et al. \(2010\)](#), [Lucas \(2010\)](#), and [Rossi and Villar \(2020\)](#) among others. Figure 3 illustrates the case of universal adoptions, which have been studied by [Chemin and Wasmer \(2009\)](#), [Kotchen and Grant \(2011\)](#), [Sawada et al. \(2022\)](#).

Figure 1 illustrates why DiD analysis around policy cancellation may yield misleading results: in the baseline period, i.e. after t_{-1} but before t^* , one may find pre-trends even if treatment and control groups are truly comparable, in the sense that their average outcomes would have followed the same trend in the absence of the policy. Indeed, Figures (b) and (c) show pre-trends because of policy’s non-constant treatment effects.² At the same time, one may fail to detect differences in pre-trends because these differences cancel out with the average treatment effect, as shown in Figure (e) and approximately (f). The problem arises from the fact that the lead coefficients estimate not only the difference in trends between the two groups if the policy never applied but also the effect of the policy over time. At the same time, the lag coefficients estimate the sum of the ATT plus the average difference between the trend that would have been observed had the switchers remained at their initial treatment level d_{pre} , and the trend that is observed for the stayers at treatment level d_0 . When the policy leads to non-constant treatment effects, the second difference is typically not zero, and the ATT estimates are inconsistent. When groups are comparable and the treatment effect is immediate, a typical event study specification will not recover dynamic treatment effects. Instead, for cases shown in Figures (b) and (c), post-treatment indicators (i.e. $t \geq t^*$) will be constant.

The hypothetical scenarios considered in Figure 1 illustrate that the only cases where a researcher does not run into trouble is when policy treatment effect is constant and immediate – as in Figures (a) and (d). Therefore, without explicit knowledge of the nature of treatment effects – either from data on earlier periods, from previous empirical work in related settings, or theoretical models – one cannot interpret the observed pre-trends.

Our results demonstrate that the ability of DiD approach to recover the true ATT in unequal baseline settings depends on the nature of treatment effects. In some settings, researchers would naturally expect a constant treatment effect, perhaps, once allowed for a short adjustment period. In these cases, standard event study design will correctly estimate the ATT as long as sufficient time has passed from the initial treatment to allow for the treatment effect to apply.

However, in cases where constant treatment effects are unlikely, alternative solutions are needed. We consider the validity of two intuitive solutions: estimation in reverse and inclusion of a linear trend. First, in settings where treatment status con-

²Treatment effects may be non-linear because of policy’s nature or due to frictions.

verges after policy change (e.g. illustrated in Figures 1–3), an appealing solution is to estimate event studies in reverse, by designating a reference period to be some period after policy change. We show that this estimation approach will only recover the ATT if the trends are parallel under the stayers’ treatment status *and* if the potential outcomes do not depend on the previous status. Therefore, the reverse estimation strategy is hard to justify, unless researchers have prior knowledge on the nature of outcome dynamics.

Second, we consider a frequently used solution to account for differential trends in the baseline period – inclusion of a linear trend term. We provide a formal characterization of this estimator and its corresponding estimand, and show that this approach can work well if differences in baseline period trends are constant over time or zero. In such settings, the linear trend term serves both as a “test” of parallel trends assumption, and it works as a correction thus allowing to recover the ATT. However, inclusion of a linear trend is not a cure-all: if differences in trends are not constant over time, then estimates are biased and inconsistent, and it is generally not possible to determine which asymptotic bias is larger – when using a simple event study or an event study with a linear trend. Therefore, the choice of specification should be guided by economic theory, prior empirical evidence, or direct observation of long pre-reform trends, if the data series are sufficiently long.

Our paper contributes to a growing literature analyzing identification and estimation of treatment effects using DiD methods under different scenarios (de Chaisemartin and D’Haultfœuille, 2020; Sant’Anna and Zhao, 2020; Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak et al., 2021; Roth and Sant’Anna, 2021; de Chaisemartin et al., 2022). These papers consider the standard case in which all units start from the same treatment level and diverge in later (and possibly different) periods (see Steigerwald et al., 2021; de Chaisemartin and D’Haultfœuille, 2022; Roth et al., 2022, for recent surveys). In recent work, de Chaisemartin et al. (2022) and Callaway et al. (2021) consider DiD with continuous treatments where units can start from different treatment levels. While we also allow for continuous treatments (e.g. marginal tax rates), our setup in this paper is different from theirs, as they focus on other estimands such as average derivatives or response functions that require more variation in the treatment, different estimation methods and different parallel trends assumptions. Instead, we focus interpreting the estimators from a two-way fixed effects specification where units start from different baselines

and the treatment changes discretely and by the same magnitude for all switchers. In related work, [Kim and Lee \(2019\)](#) consider a two-period DiD where the stayers remain treated while the switchers move from untreated to treated, a scenario that we refer to as “universal adoption” (see [Section 2.2](#)). They provide conditions under which it is possible to identify the effect of a binary treatment in the pre-policy change period using a “reverse DiD” strategy (see [Section 3.2](#)). Our analysis generalizes their results in multiple dimensions. First, we consider a more general setting with both converging and diverging treatment status, of which universal policy adoption is a particular case. We also allow potential outcomes to depend on past treatment status, and our results show that the strategy in [Kim and Lee \(2019\)](#) requires the (often restrictive) assumption that potential outcomes only depend on current treatment status. Finally, we consider multiple periods and more general estimation strategies including event study designs, which are one of the most commonly used empirical tools for estimation in DiD, and characterize the behavior of the lead coefficient estimators typically used to test for pre-policy change trends in outcomes.

Our paper is also related to the literature that focuses more directly on the parallel trends assumption. In particular, our results show that DiD designs with unequal baseline treatment status typically suffer from non-parallel trends, a topic that has received increased attention in recent years ([Manski and Pepper, 2018](#); [Kahn-Lang and Lang, 2019](#); [Bilinski and Hatfield, 2018](#); [Rambachan and Roth, 2022](#); [Roth and Sant’Anna, 2020](#); [Roth, forthcoming](#)). To the best of our knowledge, while other studies have analyzed similar forms of adjustments ([Bilinski and Hatfield, 2018](#); [Borusyak et al., 2021](#)), we are the first to derive a closed-form characterization of the OLS estimators and estimands from an event-study specification with a linear adjustment for the differences in trends. We formally show that while this approach leads to consistent estimates of causal effects when differences in trends are constant (including zero), the inclusion of a linear trend could result in more biased estimates when the differences in trends are non-constant.

1 Setup and Main Results

We consider balanced panel data with units $i = 1, \dots, n$ and time periods $t = 1, \dots, T$. There is a treatment of interest D_{it} , not necessarily binary,³ taking values in

³While our results allow for a multivalued or even continuous treatment (e.g. marginal tax rates), we only consider cases in which the treatment switches discretely from one value to another one. We

a set \mathcal{D} . The population consists of two groups. The first group, the stayers, starts at treatment level d_0 , which we refer to as the “baseline treatment status”, and remains at that treatment level throughout the observed period. The second group in the population, the switchers, starts at a treatment level d_{pre} and switches to treatment level $d_{\text{post}} \neq d_{\text{pre}}$ in period t^* , where $1 < t^* \leq T$, and stays in treatment status d_{post} for the remaining periods. We refer to t^* as the policy change period. We define the variable S_i which is equal to one if unit i switches treatment status in period t^* (the switchers) and zero if unit i remains at treatment level d_0 for the whole period (the stayers). The observed outcome of interest of unit i in period t is denoted by Y_{it} . We will assume our observed data follows the following sampling scheme, which is standard in panel data settings.

Assumption 1 (Sampling and moments)

1. *Observations $(Y_{i1}, Y_{i2}, \dots, Y_{iT}, D_{i1}, D_{i2}, \dots, D_{iT})_{i=1}^n$ are iid draws from an infinite superpopulation of units.*
2. $0 < \mathbb{E}[S_i] < 1$ and for all $d \in \mathcal{D}$ and $t = 1, \dots, T$, $\mathbb{E}[Y_{it}(d)^2] < \infty$.

To define causal parameters, we now introduce the potential outcomes which we view as random. In general, the potential outcome for unit i in period t can depend on the path of treatments from period 1 up to period t , (d_1, d_2, \dots, d_t) . Because we consider a simultaneous adoption design in which stayers remain at the same treatment level and all the switchers change status in the same time period, we can represent potential outcomes as follows. For any period $t < t^*$, we write the potential outcome as $Y_{it}(d_{\text{pre}})$, which is shorthand notation for a path of treatments where treatment status remains at d_{pre} , $(d_1, d_2, \dots, d_t) = (d_{\text{pre}}, d_{\text{pre}}, \dots, d_{\text{pre}})$. For any period $t \geq t^*$, we write the potential outcome as $Y_{it}(d, d_{\text{pre}})$, which is shorthand notation for a path of treatment that starts at d_{pre} over the pre-policy change period and then switches to d in period t^* , that is, $(d_1, d_2, \dots, d_{t^*-1}, d_{t^*}, d_{t^*+1}, \dots, d_t) = (d_{\text{pre}}, d_{\text{pre}}, \dots, d_{\text{pre}}, d_{\text{post}}, d_{\text{post}}, \dots, d_{\text{post}})$. In our notation, the first argument indicates current treatment status and the second argument indicates past treatment status.

In this setup, the potential outcomes for a stayer are $Y_{it}(d_0)$ for $t < t^*$ and $Y_{it}(d_0, d_0)$ for $t \geq t^*$. On the other hand, the potential outcomes for a switcher are $Y_{it}(d_{\text{pre}})$ for

do not consider identification and estimation of average derivatives or other more general estimands. See [de Chaisemartin et al. \(2022\)](#) and [Callaway et al. \(2021\)](#) for studies focusing specifically on DiD with continuous treatments.

$t < t^*$ and $Y_{it}(d_{\text{post}}, d_{\text{pre}})$ for $t \geq t^*$. The individual-specific treatment effect of treatment status d over treatment status d' in the pre-policy change period is $Y_{it}(d) - Y_{it}(d')$, and the treatment effect of treatment status d over treatment status d' in the post-policy change period given pre-policy change treatment status d_{pre} is $Y_{it}(d, d_{\text{pre}}) - Y_{it}(d', d_{\text{pre}})$. Notice that this allows for effect heterogeneity both across units over time. That is, we allow, for example, for $Y_{it}(d, d_{\text{pre}}) - Y_{it}(d', d_{\text{pre}}) \neq Y_{it+1}(d, d_{\text{pre}}) - Y_{it+1}(d', d_{\text{pre}})$ and $Y_{it}(d, d_{\text{pre}}) - Y_{it}(d', d_{\text{pre}}) \neq Y_{jt}(d, d_{\text{pre}}) - Y_{jt}(d', d_{\text{pre}})$.

Researchers relying on DiD strategies commonly assume, often implicitly, that potential outcomes do not depend on previous treatments. We refer to this assumption as the static potential outcomes assumption.

Assumption 2 (Static potential outcomes) *For all $i, t \geq t^*$ and $d, d' \in \mathcal{D}$, $Y_{it}(d, d') = Y_{it}(d)$.*

This assumption states that potential outcomes in the post-policy periods do not depend on their second argument, which is pre-policy change treatment status. This assumption is strong, as it implies, for example, that if a unit is subject to the policy in periods $t < t^*$ and the policy is removed in period t^* , the outcomes would switch immediately to the value that would have been observed had the policy never been in place (see Figure 1 for examples). Our results in upcoming sections do not rely on this assumption, but we discuss how identified parameters change if one is willing to impose it.

Empirical researchers commonly estimate the effect of the policy change by comparing the evolution of outcomes between switchers and stayers before and after the policy change. We consider the following event study specification:

$$Y_{it} = \alpha_i + \delta_t + \sum_{\ell < t^*, \ell \neq \tilde{t}} \theta_\ell S_i \mathbb{1}(t = \ell) + \sum_{\ell \geq t^*} \beta_\ell S_i \mathbb{1}(t = \ell) + \varepsilon_{it}, \quad (1)$$

We set period $\tilde{t} < t^*$ as the baseline (omitted) period. A common choice for \tilde{t} in empirical studies is $\tilde{t} = t^* - 1$, the last pre-policy change period. We refer to the estimated coefficients $\hat{\beta}_\ell$ as the “lag coefficients”, and the estimated coefficients $\hat{\theta}_\ell$ as the “lead coefficients”. We view the regression coefficients in Equation (1) as linear projection coefficients, and we do not make any assumptions on the shape of the conditional expectation of the observed outcome.

The following results characterizes the OLS estimators from the event study design in Equation (1). Throughout the paper, we assume that $0 < \sum_{i=1}^n S_i < n$ so that some,

but not all, units are switchers in any sample, so that all estimated coefficients are well defined.

Lemma 1 *Let $\{\hat{\beta}_\ell\}_\ell$ and $\{\hat{\theta}_\ell\}_\ell$ be the OLS estimators from Equation (1). For any $\ell \geq t^*$,*

$$\hat{\beta}_\ell = \frac{\sum_{i=1}^n (Y_{i\ell} - Y_{i\tilde{t}}) S_i}{\sum_{i=1}^n S_i} - \frac{\sum_{i=1}^n (Y_{i\ell} - Y_{i\tilde{t}}) (1 - S_i)}{\sum_{i=1}^n (1 - S_i)}$$

and for any $\ell < t^$, $\ell \neq \tilde{t}$,*

$$\hat{\theta}_\ell = \frac{\sum_{i=1}^n (Y_{i\ell} - Y_{i\tilde{t}}) S_i}{\sum_{i=1}^n S_i} - \frac{\sum_{i=1}^n (Y_{i\ell} - Y_{i\tilde{t}}) (1 - S_i)}{\sum_{i=1}^n (1 - S_i)}.$$

Lemma 1 shows that, with balanced panel data, the coefficient estimators from the event-study specification in Equation (1) are DiD estimators between periods ℓ and the baseline period \tilde{t} . Our next result characterizes the probability limit of the event-study estimators in terms of potential outcomes. Our results are based on an asymptotic panel data setting where $n \rightarrow \infty$ with T fixed and we let “ $\rightarrow_{\mathbb{P}}$ ” represent convergence in probability.

Proposition 1 *Under Assumption 1, as $n \rightarrow \infty$ with T fixed, for $\ell \geq t^*$,*

$$\begin{aligned} \hat{\beta}_\ell &\rightarrow_{\mathbb{P}} \mathbb{E} [Y_{i\ell}(d_{\text{post}}, d_{\text{pre}}) - Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) | S_i = 1] \\ &\quad + \mathbb{E} [Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i\tilde{t}}(d_{\text{pre}}) | S_i = 1] - \mathbb{E} [Y_{i\ell}(d_0, d_0) - Y_{i\tilde{t}}(d_0) | S_i = 0] \end{aligned}$$

and for $\ell < t^$, $\ell \neq \tilde{t}$,*

$$\hat{\theta}_\ell \rightarrow_{\mathbb{P}} \mathbb{E} [Y_{i\ell}(d_{\text{pre}}) - Y_{i\tilde{t}}(d_{\text{pre}}) | S_i = 1] - \mathbb{E} [Y_{i\ell}(d_0) - Y_{i\tilde{t}}(d_0) | S_i = 0].$$

If, in addition, Assumption 2 holds,

$$\begin{aligned} \hat{\beta}_\ell &\rightarrow_{\mathbb{P}} \mathbb{E} [Y_{i\ell}(d_{\text{post}}) - Y_{i\ell}(d_{\text{pre}}) | S_i = 1] \\ &\quad + \mathbb{E} [Y_{i\ell}(d_{\text{pre}}) - Y_{i\tilde{t}}(d_{\text{pre}}) | S_i = 1] - \mathbb{E} [Y_{i\ell}(d_0) - Y_{i\tilde{t}}(d_0) | S_i = 0]. \end{aligned}$$

Proposition 1 shows that the lead coefficients $\hat{\theta}_\ell$ converge in probability to the pre-policy change difference in trends had the switchers remained at treatment status d_{pre} and stayers remained at treatment level d_0 . The lag coefficients $\hat{\beta}_\ell$ converge in probability to the ATT in period ℓ , plus the difference in trends that would be observed between

switchers and stayers, again keeping their corresponding baseline statuses fixed. A parallel trends assumption is typically invoked to “erase” these differences in trends and thus recover an unbiased estimate of ATT. A traditional DiD parallel trends assumption states that the evolution of outcomes is expected to be similar for the two groups at the *stayers’ treatment status* d_0 , that is:

Assumption 3 (Parallel trends - PT(d_0)) For any $\ell \geq t^*$ and $\tilde{t} < t^*$,

$$\mathbb{E}[Y_{i\ell}(d_0, d_0) - Y_{i\tilde{t}}(d_0)|S_i = 1] = \mathbb{E}[Y_{i\ell}(d_0, d_0) - Y_{i\tilde{t}}(d_0)|S_i = 0].$$

In canonical DiD settings, the baseline status of treated and comparison group is the same, and therefore the parallel trends Assumption 3 is natural. However, in settings with unequal baseline status, the parallel trends assumption is less obvious. One may assume that outcomes would exhibit the same trajectory across groups at the pre-policy status of the *stayers* (as in Assumption 3) or at the pre-policy status of the *switchers*:

Assumption 4 (Parallel trends - PT(d_{pre})) For any $\ell \geq t^*$ and $\tilde{t} < t^*$,

$$\mathbb{E}[Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i\tilde{t}}(d_{\text{pre}})|S_i = 1] = \mathbb{E}[Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i\tilde{t}}(d_{\text{pre}})|S_i = 0].$$

An alternative assumption and one that appears most intuitive in unequal baseline settings is to require that outcomes would exhibit the same trajectory at each group’s pre-policy status, that is:

Assumption 5 (Parallel trends - PT(d_0, d_{pre})) For any $\ell \geq t^*$ and $\tilde{t} < t^*$,

$$\mathbb{E}[Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i\tilde{t}}(d_{\text{pre}})|S_i = 1] = \mathbb{E}[Y_{i\ell}(d_0, d_0) - Y_{i\tilde{t}}(d_0)|S_i = 0].$$

Which assumption is appropriate depends on the studied setting, and economic reasoning. For example, if researchers study the roll out of some new policy, then requiring parallel trends in absence of the policy may seem more reasonable than in presence of the policy. The reverse appears more appropriate if one studies changes to a policy that has been in place for a long time.

It can be readily seen, that besides being intuitive, Assumption 5 also ensures that the lag coefficients $\hat{\beta}_\ell$ of Proposition 1 recover an ATT. However, while Assumption 5 is appealing at first sight, it is actually highly restrictive. To see this, note that we can rewrite Assumption 5 as follows:

$$\begin{aligned} & \mathbb{E}[Y_{i\ell}(d_0, d_0) - Y_{i\bar{\ell}}(d_0)|S_i = 1] - \mathbb{E}[Y_{i\ell}(d_0, d_0) - Y_{i\bar{\ell}}(d_0)|S_i = 0] \\ & + \mathbb{E}[Y_{i\bar{\ell}}(d_0) - Y_{i\bar{\ell}}(d_{\text{pre}})|S_i = 1] - \mathbb{E}[Y_{i\ell}(d_0, d_0) - Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}})|S_i = 1] = 0. \end{aligned}$$

In other words, Assumption 5 holds if Assumption 3 holds *and* the treatment effect of switching from status d_{pre} to status d_0 for the switchers is constant over time.⁴ Alternatively, Assumption 5 holds if Assumption 4 holds *and* the effect of treatment status d_0 compared to d_{pre} for the stayers is constant over time:

$$\begin{aligned} & \mathbb{E}[Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i\bar{\ell}}(d_{\text{pre}})|S_i = 1] - \mathbb{E}[Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i\bar{\ell}}(d_{\text{pre}})|S_i = 0] \\ & + \mathbb{E}[Y_{i\bar{\ell}}(d_0) - Y_{i\bar{\ell}}(d_{\text{pre}})|S_i = 0] - \mathbb{E}[Y_{i\ell}(d_0, d_0) - Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}})|S_i = 0] = 0. \end{aligned}$$

Therefore, researchers cannot simultaneously presume that Assumption 5 holds and expect to recover (possibly) nonconstant treatment effects of the policy in question. For this reason, we consider Assumption 5 as stronger requirement that adds a treatment effect homogeneity restriction on top of Assumptions 3 or 4.

1.1 Pre-Trends Tests and Parallel Trends Assumptions

Assessing the validity of parallel trends assumptions based on pre-trends differences, both visually – by plotting the evolution of outcomes for switchers and stayers, or formally with t- or F-tests for lead coefficients, has become standard practice in empirical work. It is well known, however, that the absence of differences in trends before the policy change does not imply that trends would have been the same in the post period in the absence of the policy change. Furthermore, recent literature has pointed out that pre-trends tests often have limited statistical power to detect violations of parallel pre-trends (Bilinski and Hatfield, 2018; Freyaldenhoven et al., 2019; Kahn-Lang and Lang, 2019; Roth, forthcoming). These issues are particularly important in an unequal baseline DiD, where different treatment statuses before the policy change are likely to generate different outcome and treatment effect dynamics for switchers and stayers.

To see why Assumption 5 can fail even in the absence of pre-trends, consider the following example. Suppose the data contains four time periods (years), $t = 1, 2, 3, 4$ and the policy change occurs in period $t^* = 3$. Consider a universal policy adoption scenario where the stayers are always treated whereas the switchers only receive

⁴Alternatively, the treatment effect could be such that it offsets the difference in trends between stayers and switchers precisely – intuitively, a knife-edge scenario that is hard to defend in practice.

treatment in period $t = 3$. Furthermore, suppose that the effect of the treatment is non-constant, and that treatment effects only appear after a unit is treated for three or more years. These patterns are common, for example, in agricultural extension programs, since effects in outcomes like yields or the introduction of new seed varieties may only materialize after an adjustment period (see e.g. [Maffioli et al., 2011](#)). In this case, a pre-trends test would find no significant differences between switchers and stayers, since the switchers are untreated and the treatment effect for the stayers is zero in the first two years. In periods three and four, however, the true treatment effect for the switchers is zero (since the effect does not kick in until the third year of treatment), but an event study design would estimate a non-zero coefficient because the stayers start to experience the effect of the treatment, which creates a difference in observed outcomes after the policy change.

This simple example highlights the importance of clearly distinguishing between testing for pre-trends, which is based on observable data, and assessing the validity of the parallel trends assumption, which involves counterfactual unobserved magnitudes. The validity of the identification assumption cannot be assessed solely on statistical grounds, and empirical evidence needs to be complemented with institutional knowledge and economic theory. While this is true for canonical and unequal baseline DiDs, we highlight that this difference is particularly important in unequal baseline DiDs, since the assumption that provides identification of ATTs (Assumption 5) imposes restrictions on both the evolution of outcomes and treatment effects.

1.2 Canonical DiD: Equal Baseline Status

In the canonical DiD setting, both groups start at the same treatment level and then diverge, i.e. $d_{\text{pre}} = d_0$ and $d_{\text{post}} \neq d_0$. In such settings, it is common to assume that, had all units remained at the pre-policy change treatment status d_0 , the average outcomes would have followed the same evolution over time, i.e. Assumption 3 holds. The next result follows directly from Proposition 1.

Corollary 1 (Equal baseline treatment status) *When $d_{\text{pre}} = d_0$ and $d_{\text{post}} \neq d_0$, under Assumptions 1 and 3, for $\ell \geq t^*$*

$$\hat{\beta}_\ell \rightarrow_{\mathbb{P}} \mathbb{E}[Y_{i\ell}(d_{\text{post}}, d_0) - Y_{i\ell}(d_0, d_0) | S_i = 1],$$

and for $\ell < t^*$, $\ell \neq \tilde{t}$,

$$\hat{\theta}_\ell \rightarrow_{\mathbb{P}} \mathbb{E}[Y_{i\ell}(d_0) - Y_{i\tilde{t}}(d_0) | S_i = 1] - \mathbb{E}[Y_{i\ell}(d_0) - Y_{i\tilde{t}}(d_0) | S_i = 0].$$

Corollary 1 shows that if the parallel trends Assumption 3 is satisfied, then the lag coefficients converge to the average effect on the switchers of moving from treatment status d_0 to d_{post} in each post-policy change period $\ell \geq t^*$. At the same time, the lead coefficients $\hat{\theta}_\ell$ estimate the difference in pre-policy change trends between groups for the same treatment level. Notice that in general, the parallel trends Assumption 3 does not necessitate that the lead coefficients $\hat{\theta}_\ell$ converge to zero. However, because lag coefficients mimic the parallel trends equation (which cannot be observed directly), researchers typically provide evidence in support of Assumption 3 by showing that $\hat{\theta}_\ell$ are close to zero and statistically insignificant.

1.3 DiD with Converging Treatment Status

Now consider DiD settings in which groups start from different treatment levels and then converge to the same treatment. In other words, suppose $d_{\text{pre}} \neq d_0$ and $d_{\text{post}} = d_0$. Applying Assumption 3 to Proposition 1 generates the following result:

Corollary 2 (Convergent treatment status under PT(d_0)) *When $d_{\text{pre}} \neq d_0$ and $d_{\text{post}} = d_0$, under Assumptions 1 and 3, for $\ell \geq t^*$,*

$$\hat{\beta}_\ell \rightarrow_{\mathbb{P}} \mathbb{E}[Y_{i\tilde{t}}(d_0) - Y_{i\tilde{t}}(d_{\text{pre}}) | S_i = 1] + \mathbb{E}[Y_{i\ell}(d_0, d_{\text{pre}}) - Y_{i\ell}(d_0, d_0) | S_i = 1],$$

and for $\ell < t^*$, $\ell \neq \tilde{t}$,

$$\hat{\theta}_\ell \rightarrow_{\mathbb{P}} \mathbb{E}[Y_{i\tilde{t}}(d_0) - Y_{i\tilde{t}}(d_{\text{pre}}) | S_i = 1] - \mathbb{E}[Y_{i\ell}(d_0, d_0) - Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) | S_i = 1].$$

If, in addition, Assumption 2 holds,

$$\hat{\beta}_\ell \rightarrow_{\mathbb{P}} \mathbb{E}[Y_{i\tilde{t}}(d_0) - Y_{i\tilde{t}}(d_{\text{pre}}) | S_i = 1].$$

Corollary 2 demonstrates that the lead coefficients $\hat{\theta}_\ell$ estimate the difference in treatment effect on the switchers from switching from treatment status d_{pre} to d_0 in period ℓ versus in period \tilde{t} . Therefore, even if Assumption 3 holds, lead coefficients will not converge to zero unless the treatment effect on the switchers is constant over time.

At the same time, the lag coefficients do not recover an ATT unless such treatment effect is constant over time.⁵ Under the additional assumption of static potential outcomes, which can be seen as a best-case scenario, all the lag coefficients $\{\hat{\beta}_\ell\}_\ell$ estimate the same parameter, namely, the average effect of status d_0 versus d_{pre} at the baseline period \tilde{t} , and are not able to recover the dynamic effect of policy change.

Applying Assumption 4 to Proposition 1 generates the following result:

Corollary 3 (Converging treatment status under PT(d_{pre})) *When $d_{\text{pre}} \neq d_0$ and $d_{\text{post}} = d_0$, under Assumptions 1 and 4, for $\ell \geq t^*$,*

$$\begin{aligned} \hat{\beta}_\ell \rightarrow_{\mathbb{P}} & \mathbb{E} [Y_{i\ell}(d_0, d_{\text{pre}}) - Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) | S_i = 1] \\ & + \mathbb{E} [Y_{i\tilde{t}}(d_0) - Y_{i\tilde{t}}(d_{\text{pre}}) | S_i = 0] - \mathbb{E} [Y_{i\ell}(d_0, d_0) - Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) | S_i = 0], \end{aligned}$$

and for $\ell < t^*$, $\ell \neq \tilde{t}$,

$$\hat{\theta}_\ell \rightarrow_{\mathbb{P}} \mathbb{E} [Y_{i\tilde{t}}(d_0) - Y_{i\tilde{t}}(d_{\text{pre}}) | S_i = 0] - \mathbb{E} [Y_{i\ell}(d_0, d_0) - Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) | S_i = 0].$$

If, in addition, Assumption 2 holds,

$$\begin{aligned} \hat{\beta}_\ell \rightarrow_{\mathbb{P}} & \mathbb{E} [Y_{i\ell}(d_0) - Y_{i\ell}(d_{\text{pre}}) | S_i = 1] \\ & - \{\mathbb{E} [Y_{i\ell}(d_0) - Y_{i\ell}(d_{\text{pre}}) | S_i = 0] - \mathbb{E} [Y_{i\tilde{t}}(d_0) - Y_{i\tilde{t}}(d_{\text{pre}}) | S_i = 0]\}. \end{aligned}$$

Corollary 3 demonstrates that the lead coefficients estimate the difference in treatment effect of switching from status d_{pre} to status d_0 on the stayers in period ℓ versus in period \tilde{t} . Therefore, even if Assumption 4 holds, lead coefficients will not converge to zero unless the treatment effect on the stayers is constant over time. At the same time, the lag coefficients do not recover the ATT unless such treatment effect is constant over time.

2 Empirical Applications

In this section we apply our results to two common empirical settings: policy reversals and universal policy adoptions. Finally, we discuss settings with non-convergent

⁵To see this note that Corollary 2 implies that $\hat{\beta}_\ell \rightarrow_{\mathbb{P}} \mathbb{E} [Y_{i\ell}(d_0, d_{\text{pre}}) - Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) | S_i = 1] + \mathbb{E} [Y_{i\tilde{t}}(d_0) - Y_{i\tilde{t}}(d_{\text{pre}}) | S_i = 1] - \mathbb{E} [Y_{i\ell}(d_0, d_0) - Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) | S_i = 1]$. Here the first term measures an ATT while the second two terms measure the difference in treatment effects on the switchers, comparing status d_{pre} to status d_0 between periods ℓ and \tilde{t} .

treatment status, where individuals experience different treatment status both in pre- and post periods.

2.1 Policy Reversal

In a policy reversal studies, in the baseline period, the policy applies to the treatment group but not to the comparison group, before being cancelled in the post period.⁶ This type of research design is common. For example, [Bleemer \(2022\)](#) studies the effects of race-based affirmative action bans on student outcomes. This study compares outcomes of the underrepresented minority (URM) applicants to the outcomes of non-URM students with similar prior academic opportunity and preparation, before and after the 1998 affirmative action ban at California public universities, which removed preferential treatment for URMs. [Slattery et al. \(2022\)](#) study how the cancellation of independent campaign contribution bans due to *Citizens United* ruling affected state tax policies, by comparing tax policies in states that had bans to states that did not. [Cao and Chen \(2022\)](#) study the abandonment of the Grand Canal in China on rebellions. Their treatment group consists of counties through which the canal ran before it was abandoned while the control group includes distant counties. [Grönqvist et al. \(2020\)](#) study the effects of leaded gasoline phaseout by comparing life outcomes of children born in Swedish neighborhoods with high vs low lead exposures, before and after 1981, when lead levels per liter of gasoline were rapidly reduced in all areas of Sweden. Relatedly, [Bleakley \(2010\)](#), [Cutler et al. \(2010\)](#), [Lucas \(2010\)](#), and [Rossi and Villar \(2020\)](#) study malaria eradication campaigns, and exploit variation in malaria prevalence prior to anti-malaria programs as a measure of intensity of treatment.

In the policy reversal settings, d_{pre} identifies episodes when the policy applies and d_0 when the policy does not apply. In this setting, it seems natural to assume that the parallel trends assumption holds at the comparison group’s treatment status d_0 (i.e. Assumption 3), since all groups eventually revert to this status. Figure 1 illustrates the potential empirical challenges under a variety of assumptions, for a policy that is initially introduced in period t_{-1} and then cancelled in period t^* . Panel A considers truly comparable treatment and control groups, i.e. groups that show parallel trends prior to t_{-1} , while Panel B assumes groups are not comparable.

⁶Settings where the policy is first applied to all individuals but then is cancelled for a subgroup of people fall under the canonical DiD framework, summarized in Section 1.2, since in the baseline period both treatment and control group are subject to the same status.

Figure 1 illustrates why the parallel trend test over the baseline period may fail: in the baseline period, i.e. after t_{-1} but before t^* , one may find pre-trends even if the groups are truly comparable – e.g. in (b) and (c), or may fail to detect differences in pre-trends because these differences cancel out with the policy treatment effect – e.g. in (e) and approximately (f). Figure 1 also illustrates why the lag coefficients $\hat{\beta}_l$ do not identify the ATT when the policy leads to non-constant treatment effects as in (b)-(c) and (e)-(f). Note that in the hypothetical scenarios of Figure 1, the potential outcomes do not depend on previous treatments, i.e. Assumption 2 holds. In such cases Corollary 2 shows that lag coefficients converge to $\mathbb{E}[Y_{it}(d_0) - Y_{it}(d_{\text{pre}}) | S_i = 1]$. In other words, all lag coefficients estimate the same parameter – the effect of removing the policy in period \tilde{t} . Hence, even under a stronger assumption, the lag coefficients do not allow the researcher to estimate how the effect of interest varies over time and thus recover dynamic ATT.

Jakobsen et al. (2020) provide a compelling illustration of these issues in an empirical setting. This study explores how wealthy individuals respond to wealth taxes in Denmark using two approaches. Their first approach exploits a 1989 reform that increased the wealth tax exemption threshold for couples relative to singles. As a result of the reform, some married individuals who were previously subject to wealth taxation became exempt from it. The authors estimate causal effects of this change by comparing couples in the affected wealth range to two plausible comparison groups: (a) single individuals in the same wealth range who were always subject to wealth tax (their preferred specification) and (b) couples in a lower wealth range who were always exempt from wealth tax.⁷

Figure 2 reproduces Jakobsen et al. (2020) findings. Note that comparing treated couples to untreated singles in Figure 2(a) follows the canonical DiD framework: both groups experience the same treatment status (a positive wealth tax) in the baseline period. Therefore, Corollary 1 applies and the DiD approach recovers the ATT as long as the parallel trends assumption holds. However, comparing treated couples to couples who were not subject to wealth tax in the first period in Figure 2(b) violates the canonical assumptions because treatment and control experience unequal status in the first period, necessitating the application of Corollary 2 (or Corollary 3). Thus this approach identifies the ATT on the switchers if and only if the wealth tax results

⁷For simplicity and lack of relevance, our discussion abstracts away from other practical issues that may affect individuals’ treatment status, e.g. fluctuations of wealth and changes of marital status. Authors address these separately.

in a constant treatment effect. This is unlikely in this setting because wealth taxes lead to mechanical changes in wealth: even if individuals choose not to respond to tax incentives, their wealth accumulates slower (faster) in presence of higher (lower) wealth tax rates. Indeed, as Figure 2 shows, the first control group appears to provide a better comparison than the second, with the raw data showing parallel trends in (a) but divergent trends in (b). The divergent trends observed in Figure 2(b) could either be due to continuous treatment effect of wealth taxation (similar to Figures 1(b)-(c)), or because groups are actually not comparable (similar to Figure 1(d)), with no way of telling these apart. The authors assume the former is true, and account for the differential trends by including a linear trend term in their specification. We evaluate the validity of this approach in Section 3.3

2.2 Universal Adoption

Corollaries 2 and 3 can also be applied to studies that explore universal adoptions of policies that were previously applied only to a subset of the population. In these settings, the comparison group consists of individuals that are subject to some policy in both periods, while the treatment group adopts the policy in the post period only. This setting has been explored in environmental economics to study energy effects of daylight savings time (DST) by Kotchen and Grant (2011), who exploit a 2006 reform that resulted in a universal adoption of DST in Indiana. Prior to the studied policy change, 77 counties did not practice DST while 15 did. In the post-treatment period, all counties switched to DST. Sawada et al. (2022) study introduction of a formal, democratic local school-based management (SBM) institution in rural Burkina Faso. This study compares schools that implemented SBM in the first period to schools that implement SBM in the second period. In labor economics, Chemin and Wasmer (2009) study the employment effects of working hour reductions. They exploit a change in working hours laws in France that resulted in a switch from a 39 hours per week to 35 in all areas of France except for Alsace-Moselle where working hours decreased from a lower starting point.

In settings with universal policy adoptions, d_{pre} represents episodes when the policy does not apply, while status d_0 when the policy does apply. In these settings, Assumption 3 implies that the trends would be parallel if the policy applied to both groups. One can interpret Assumption 3 as restricting the evolution of the treatment effect over time, since it requires the group of switchers and stayers to evolve in the same way

under the policy. In fact, a sufficient condition for this version of the parallel trends assumption is that (i) the outcomes of both groups in the absence of treatment evolves in the same way over time and (ii) the average treatment effects for both groups exhibit the same trend. In this sense, Assumption 4 is more in line with the usual parallel trends assumption employed in the DiD literature. Nonetheless, Assumption 3 may be an intuitive choice in some settings, e.g. in the case of Chemin and Wasmer (2009), while Assumption 4 may be better suited for other settings, e.g. Kotchen and Grant (2011) and Sawada et al. (2022).

Figure 3 illustrates the issues for a policy that is adopted by the stayers in period t_{-1} and by the switchers in period t^* . Panel A considers truly comparable treatment and control groups under Assumption 4, i.e. groups that show parallel trends prior to t_{-1} , while Panel B assumes groups are not comparable. As Figures 3 (b) and (c) demonstrate, even if groups are truly comparable, the lead coefficients will not be zero unless the policy effect on the stayers/switchers is constant over time. Alternatively, the change in treatment for the stayers/switchers may offset differences in trends, resulting in incorrect conclusion that groups are indeed similar, as in (e).

Under Assumption 4, Corollary 3 shows that the lag coefficients converge to the sum of the average effect on the switchers of adopting the policy after being untreated, and the difference in treatment effect of switching from status d_{pre} to status d_0 on the stayers in period ℓ versus in period \tilde{t} . This latter term is non-zero when treatment effects are non-constant, as in Figures 3 (b) and (c), irrespective of whether Assumption 2 holds in addition or not.

If Assumption 3 is more appropriate in the studied setting, then the results of Section 2.1 apply: the lag coefficients are biased unless the policy results in constant treatment effects, while the lead coefficients do not provide suggestive evidence for or against Assumption 3.

2.3 DiD with Non-converging Treatment Status

Finally, Proposition 1 applies to the most general case, where treatment and control groups experience differential treatment statuses in both periods. This version of DiD is ubiquitously used by empirical tax economists to measure causal effects of taxes. For example, Kleven and Schultz (2014) measure causal effects of income taxes while Jakobsen et al. (2020) measure effects of wealth taxes by comparing individuals in different income tax brackets who experience differential changes in relative MTRs.

[Yagan \(2015\)](#) studies the effect of dividend taxes on investment by comparing firms that are subject to dividend taxes (C-corporations) to firms that are not (S-corporations). [Zwick and Mahon \(2017\)](#) study the effects of bonus depreciation scheme on investment by exploiting differences in relative magnitude of incentives, while [Fuest et al. \(2018\)](#) study the effect of corporate taxes on wages using variation in local corporate rates. In all these studies, treatment and control groups experience different levels of taxes in the baseline period and in the post-period.

Proposition 1 applies to these settings. As discussed in Section 1, the lead coefficients estimate difference in trends between switchers and stayers keeping their corresponding different pre-policy change status fixed, and therefore not providing an evaluation of Assumption 3 nor Assumption 4. The lag coefficients estimate the ATT plus the difference in trends that would be observed between switchers and stayers, had the switchers remained at treatment status d_{pre} . Again, neither Assumption 3 nor Assumption 4 alone can guarantee that this latter term is zero. In addition to one of the parallel trend assumptions, one must assume that the treatment effect is constant over time in order to recover unbiased ATT.

[Jakobsen et al. \(2020\)](#) provide an empirical illustration of this case. Their second set of analysis exploits a 1989 reform that reduced the wealth tax rate from 2.2% to 1% on the very wealthy. To estimate a causal effect, the authors compare wealthy individuals to a group subject to zero marginal tax rate because of a tax ceiling provision. This approach does not conform to the classical DiD setup because the treatment and control group experience differential wealth tax treatment in the pre-period, and in contrast to the analysis discussed in Section 2.1, groups' statuses do not converge in a post-policy period. Again, unless we expect treatment effect of wealth taxes to be constant over time, this approach would not recover the ATT and the parallel trend test would fail. Figure 4 confirms our expectations: again, the raw wealth series show differential trends which authors adjust for with a linear trend. We discuss the validity of such linear trend adjustment in Section 3.3.

3 Practical Recommendations

The inability of researchers to verify the validity of parallel trends assumption and thus establish to what extent treatment and control groups are actually comparable is a well-known shortcoming of the DiD approach is. A growing literature has pointed out

the limitations of pre-trends tests in canonical DiD settings (e.g., [Roth, forthcoming](#); [Rambachan and Roth, 2022](#); [Kahn-Lang and Lang, 2019](#); [Bilinski and Hatfield, 2018](#); [Freyaldenhoven et al., 2019](#)), but it is particularly troublesome in settings with unequal baseline DiD.

Sections 1 and 2 demonstrate that the ability of DiD approach to recover the true ATT in such settings depends on the nature of treatment effects. The unequal baseline DiD is best applied to settings where treatment effects are expected to be fairly immediate and constant in nature. Having data that covers pre-baseline period in which groups experience equal treatment status can be useful to establish the nature of treatment effects in the specific setting studied. Alternatively, economic reasoning and prior research on the topic can be used to form expectations about treatment effects, and any frictions that may delay effects.

In some settings, researchers would naturally expect a constant treatment effect, perhaps, once allowed for a short adjustment period. For example, an increase in marginal income tax is expected to permanently decrease individual’s labor supply. With the exception of a plausibly short adjustment period (e.g. due to information and labor market frictions), the treatment effect should be constant in nature. In these cases, the approach will correctly estimate the ATT as long as sufficient time has passed from the initial treatment to allow for the treatment effect to apply.

On the other hand, in other settings we would expect non-constant treatment effects. For example, a wealth tax should plausibly lead to a continuous treatment effect through tax’s mechanical effect on wealth accumulation. Therefore, even in absence of individuals’ responses, one would expect a divergence of wealth outcomes among individuals subject to different levels of wealth tax. Generally speaking, non-constant treatment effects are expected in settings where treatment effects tend to accumulate or dissipate over time, or where adjustment frictions are particularly large and lead to delayed responses. For example, non-constant treatment effects have been observed 10 years after policy changes in studies of knowledge production and invention (e.g. [Furman and Stern, 2011](#); [Moser and Voena, 2012](#); [Akcigit et al., 2022](#)), international migration ([Kleven et al., 2013](#)), labor outcomes (e.g. [Walker, 2013](#)), and health outcomes (e.g. [Greenstone and Hanna, 2014](#); [Alsan and Goldin, 2019](#)).

In what follows, we discuss a few alternatives for analyzing unequal baseline DiD.

3.1 Additional Data on Pre-Baseline Periods

The most straightforward, although often infeasible, solution to the limitations of unequal baseline DiD is to obtain more data on earlier periods where both groups had a common treatment status, as in the periods before t_{-1} in Figure 1. Because groups start from the same treatment status in this pre-baseline periods, this can be seen as a canonical DiD, and all the standard results apply. In this case, it is possible to split the time periods into three groups: one initial period in which both groups share the same treatment status, one intermediate period in which they diverge, and a final period in which they converge. This allows the researcher to estimate both the effects of the policy change and its reversion, and also test whether outcomes immediately revert back to their initial trend.

For example, [Namen \(2021\)](#) studies an education reform in Colombia that capped schools' grade retention rates at 5 percent. The policy was implemented in 2002 and later removed in 2009. By exploiting the extent to which schools were affected by this policy, this study compares the periods 2002-2008 and 2009-2012 to the period 1998-2001 to estimate the effects of implementing and removing the grade retention cap on students' outcomes.

3.2 Estimation in Reverse for DiD with Converging Treatment Status

In settings where treatment status converges after policy change, visual examination of Figures 1 and 3 suggests an alternative estimation approach: perhaps, it is possible to estimate ATT “in reverse”, simply by re-ordering time periods. Intuitively, if one designates the reference period \tilde{t} to be some period after the policy change, then pre-policy change coefficients $\hat{\theta}_\ell$ may estimate the ATT of the policy change. In this section we discuss the validity of this approach.

Consider a setting where groups start from different treatment levels and then converge to the same treatment status, i.e. suppose $d_{\text{pre}} \neq d_0$ and $d = d_0$ as in Section 1.3. Let the baseline period \tilde{t} be some post-policy change period, i.e. let $\tilde{t} \geq t^*$. The following proposition is equivalent to Proposition 1 applied to convergent treatment status cases. It characterizes the probability limit of the event study estimators under the modified assumption $\tilde{t} \geq t^*$.

Proposition 2 (Reverse DiD) *When $d_{\text{pre}} \neq d_0$ and $d = d_0$, $\tilde{t} \geq t^*$, and under Assumption 1, as $n \rightarrow \infty$ with T fixed, for $\ell \geq t^*$, $\ell \neq \tilde{t}$,*

$$\hat{\beta}_\ell \rightarrow_{\mathbb{P}} \mathbb{E} [Y_{i\tilde{t}}(d_0, d_0) - Y_{i\ell}(d_0, d_0) | S_i = 0] - \mathbb{E} [Y_{i\tilde{t}}(d_0, d_{\text{pre}}) - Y_{i\ell}(d_0, d_{\text{pre}}) | S_i = 1]$$

and for $\ell < t^*$,

$$\begin{aligned} \hat{\theta}_\ell \rightarrow_{\mathbb{P}} & \mathbb{E} [Y_{i\ell}(d_{\text{pre}}) - Y_{i\ell}(d_0) | S_i = 1] \\ & + \mathbb{E} [Y_{i\tilde{t}}(d_0, d_0) - Y_{i\ell}(d_0) | S_i = 0] - \mathbb{E} [Y_{i\tilde{t}}(d_0, d_{\text{pre}}) - Y_{i\ell}(d_0) | S_i = 1]. \end{aligned}$$

If, in addition, Assumption 2 holds, for $\ell \geq t^*$, $\ell \neq \tilde{t}$,

$$\hat{\beta}_\ell \rightarrow_{\mathbb{P}} \mathbb{E} [Y_{i\tilde{t}}(d_0) - Y_{i\ell}(d_0) | S_i = 0] - \mathbb{E} [Y_{i\tilde{t}}(d_0) - Y_{i\ell}(d_0) | S_i = 1]$$

and for $\ell < t^*$,

$$\begin{aligned} \hat{\theta}_\ell \rightarrow_{\mathbb{P}} & \mathbb{E} [Y_{i\ell}(d_{\text{pre}}) - Y_{i\ell}(d_0) | S_i = 1] \\ & + \mathbb{E} [Y_{i\tilde{t}}(d_0) - Y_{i\ell}(d_0) | S_i = 0] - \mathbb{E} [Y_{i\tilde{t}}(d_0) - Y_{i\ell}(d_0) | S_i = 1]. \end{aligned}$$

Proposition 2 demonstrates that the pre-policy change coefficients $\hat{\theta}_\ell$ estimate the ATT of the policy change, $\mathbb{E} [Y_{i\ell}(d_{\text{pre}}) - Y_{i\ell}(d_0) | S_i = 1]$, if the parallel trends Assumption 3 holds and if we are willing to assume that potential outcomes are static (i.e. Assumption 2 holds). Therefore, for settings demonstrated in Panel A of Figures 1 and 3, one can indeed recover the ATT by choosing an alternative reference point.

However, the result does not hold when potential outcomes are dynamic. If switching treatment status leads to different outcome trends compared to the case in which treatment status remains constant throughout the period, then $Y_{i\tilde{t}}(d_0, d_{\text{pre}}) \neq Y_{i\tilde{t}}(d_0, d_0)$, and therefore $\hat{\theta}_\ell$ will not recover the ATT.

The assumption of static potential outcomes is very strong, as it requires, for instance, that average potential outcomes immediately return to their pre-treatment trend once the treatment is removed. Therefore, the reverse estimation strategy is hard to justify, unless researchers have prior knowledge on the nature of outcome dynamics.

3.3 Linear Trend Adjustment

An appealing and frequently used solution to account for differential trends in the baseline period is to include a linear trend term in the usual event study specification. For example, [Jakobsen et al. \(2020\)](#) account for differential trends in Figures 2(b) and 4 by including “a linear differential pretrend identified based on n prereform years (i.e. the omitted years in the first term on the right-hand side).” Intuitively, the goal of the linear trends adjustment is to relax a parallel trends assumption such as Assumption 5 from requiring no differences in trends to allowing for differences that are constant over time, that is:

Assumption 6 (Constant Differences in Parallel Trends) *For any $\ell \geq t^*$ and $\tilde{t} < t^*$, and for some constant κ that does not depend on ℓ or \tilde{t} ,*

$$\mathbb{E}[Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i\tilde{t}}(d_{\text{pre}})|S_i = 1] - \mathbb{E}[Y_{i\ell}(d_0, d_0) - Y_{i\tilde{t}}(d_0)|S_i = 0] = \kappa.$$

Note that the parallel trends Assumption 5 amounts to setting $\kappa = 0$. In this section, we evaluate the validity of this approach.

Let $\mathcal{T}_{\text{LA}} \subseteq \{1, \dots, t^* - 1\}$ be the subset of periods used to estimate the linear trend adjustment and let $T_{\text{LA}} \geq 2$ be the number of elements in \mathcal{T}_{LA} . We assume that \mathcal{T}_{LA} consists of consecutive periods, $\mathcal{T}_{\text{LA}} = \{t_m, t_m + 1, \dots, t_M - 1, t_M\}$, where $1 \leq t_m < t_M \leq t^* - 1$. Define the average and the variance of time periods in set \mathcal{T}_{LA} as $\bar{t} = \sum_{t \in \mathcal{T}_{\text{LA}}} t / T_{\text{LA}}$, $V = \sum_{t \in \mathcal{T}_{\text{LA}}} (t - \bar{t})^2 / T_{\text{LA}}$, and let $\Delta Y_{it} = Y_{it} - Y_{it-1}$ denote one-period differences. We now consider the following extension of the basic event-study specification from Equation (1):

$$Y_{it} = \alpha_i + \delta_t + \gamma S_{it} + \sum_{\ell < t^*, \ell \notin \mathcal{T}_{\text{LA}}} \theta_{\ell}^{\text{LA}} S_i \mathbb{1}(t = \ell) + \sum_{\ell \geq t^*} \beta_{\ell}^{\text{LA}} S_i \mathbb{1}(t = \ell) + u_{it} \quad (2)$$

where we refer to γ as the linear adjustment coefficient. The following result characterizes the OLS estimators from this equation and is similar to Lemma 1.

Lemma 2 *Let $\hat{\gamma}$, $\{\hat{\beta}_{\ell}\}_{\ell}$ and $\{\hat{\theta}_{\ell}\}_{\ell}$ be the OLS estimators from Equation (2). Then,*

$$\hat{\gamma} = \sum_{t=t_m+1}^{t_M} \omega_t^{\gamma} \left(\frac{\sum_{i=1}^n \Delta Y_{it} S_i}{\sum_{i=1}^n S_i} - \frac{\sum_{i=1}^n \Delta Y_{it} (1 - S_i)}{\sum_{i=1}^n (1 - S_i)} \right) \quad \text{with} \quad \omega_t^{\gamma} = \frac{(t - t_m)(t_M + 1 - t)}{2T_{\text{LA}}V},$$

and for any $\ell \geq t^*$,

$$\hat{\beta}_\ell^{\text{LA}} = \sum_i \frac{(S_i - \bar{S})}{n\bar{S}(1 - \bar{S})} \left(Y_{i\ell} - Y_{it_M} - \sum_{t=t_m+1}^{t_M} \Delta Y_{it} \omega_t^\ell \right)$$

and for any $\ell < t^*, \ell \notin \mathcal{T}_{\text{LA}}$,

$$\hat{\theta}_\ell^{\text{LA}} = \sum_i \frac{(S_i - \bar{S})}{n\bar{S}(1 - \bar{S})} \left(Y_{i\ell} - Y_{it_M} - \sum_{t=t_m+1}^{t_M} \Delta Y_{it} \omega_t^\ell \right),$$

where

$$\omega_t^\ell = \left(\frac{\ell - \bar{t}}{T_{\text{LA}} V} \right) \frac{t_m(t_m - 1)}{2} + \left(\frac{t - t_m}{T_{\text{LA}}} \right) \left(\frac{\bar{t}(\ell - \bar{t})}{V} - 1 \right) - \left(\frac{\ell - \bar{t}}{2T_{\text{LA}} V} \right) t(t - 1).$$

Note that $\sum_{t=t_m+1}^{t_M} \omega_t^\ell = \ell - t_M$, while weights $\omega_t^\gamma \geq 0$ and satisfy $\sum_{t=t_m+1}^{t_M} \omega_t^\gamma = 1$. Also notice that the weights are non-random and observable, since they are functions of the time periods only. Next, we characterize the probability limits of the estimators from Equation (2), similarly to Proposition 1.

Proposition 3 (Linear trend adjustment) *Under Assumption 1, as $n \rightarrow \infty$ with T fixed, estimators from Equation (2) satisfy*

$$\hat{\gamma} \rightarrow_{\mathbb{P}} \sum_{t=t_m+1}^{t_M} \omega_t^\gamma (\mathbb{E}[\Delta Y_{it}(d_0) | S_i = 1] - \mathbb{E}[\Delta Y_{it}(d_{\text{pre}}) | S_i = 0]),$$

and for $\ell \geq t^*$,

$$\begin{aligned} \hat{\beta}_\ell^{\text{LA}} &\rightarrow_{\mathbb{P}} \mathbb{E}[Y_{i\ell}(d_0, d_{\text{pre}}) - Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) | S_i = 1] \\ &\quad + \mathbb{E}[Y_{i\ell}(d_{\text{pre}}, d_{\text{pre}}) - Y_{it_M}(d_{\text{pre}}) | S_i = 1] - \mathbb{E}[Y_{i\ell}(d_0, d_0) - Y_{it_M}(d_0) | S_i = 0] \\ &\quad - \sum_{t=t_m+1}^{t_M} \omega_t^\ell (\mathbb{E}[\Delta Y_{it}(d_{\text{pre}}) | S_i = 1] - \mathbb{E}[\Delta Y_{it}(d_0) | S_i = 0]) \end{aligned}$$

and for $\ell < t^*, \ell \notin \mathcal{T}_{\text{LA}}$,

$$\hat{\theta}_\ell^{\text{LA}} \rightarrow_{\mathbb{P}} \mathbb{E}[Y_{i\ell}(d_{\text{pre}}) - Y_{it_M}(d_{\text{pre}}) | S_i = 1] - \mathbb{E}[Y_{i\ell}(d_0) - Y_{it_M}(d_0) | S_i = 0]$$

$$- \sum_{t=t_m+1}^{t_M} \omega_t^\ell (\mathbb{E}[\Delta Y_{it}(d_{\text{pre}})|S_i = 1] - \mathbb{E}[\Delta Y_{it}(d_0)|S_i = 0]),$$

with weights ω_t^γ and ω_t^ℓ defined as in Lemma 2.

If, in addition, Assumption 2 holds,

$$\begin{aligned} \hat{\beta}_\ell^{\text{LA}} &\rightarrow_{\mathbb{P}} \mathbb{E}[Y_{i\ell}(d_0) - Y_{i\ell}(d_{\text{pre}})|S_i = 1] \\ &+ \mathbb{E}[Y_{i\ell}(d_{\text{pre}}) - Y_{it_M}(d_{\text{pre}})|S_i = 1] - \mathbb{E}[Y_{i\ell}(d_0) - Y_{it_M}(d_0)|S_i = 0] \\ &- \sum_{t=t_m+1}^{t_M} \omega_t^\ell (\mathbb{E}[\Delta Y_{it}(d_{\text{pre}})|S_i = 1] - \mathbb{E}[\Delta Y_{it}(d_0)|S_i = 0]) \end{aligned}$$

Proposition 3 shows that the linear trend coefficient $\hat{\gamma}$ estimates a weighted average of pre-policy differences in trends between switchers and non-switchers. These weights ω_t^γ are non-negative, sum up to one, and are quadratic in nature, with the largest weight given to the middle of the estimation period \mathcal{T}_{LA} (specifically, period $\bar{t} + 0.5$), and are decreasing towards early and late periods t_m and t_M , respectively.

The estimator $\hat{\gamma}$ satisfies $\hat{\gamma} \rightarrow_{\mathbb{P}} 0$ if the pre-trends are parallel, that is, $\mathbb{E}[\Delta Y_{it}(d_0)|S_i = 1] = \mathbb{E}[\Delta Y_{it}(d_{\text{pre}})|S_i = 0]$, in the chosen estimation period \mathcal{T}_{LA} . Therefore, the inclusion of a linear trend helps evaluate the validity of Assumption 5: a non-zero and statistically significant estimate of γ would imply that this assumption is unlikely to hold. As discussed previously, this assumption imposes restrictions on both the evolution of outcomes under the baseline treatment status and on the evolution of treatment effects.

However, the test may fail in two circumstances. First, if differences in trends are volatile over time, the negative differences may cancel out with positive differences resulting in an approximately zero estimate of $\hat{\gamma}$ even though pre-trends are not parallel. Similarly, the quadratic nature of weights ω_t^γ imply that the test may fail to detect differences in trends if these differences are small in the vicinity of period $\bar{t} + 0.5$ but large otherwise. Visual examination of the data is useful to rule out such possibilities.

Proposition 3 further shows that event study coefficients $\hat{\beta}_\ell^{\text{LA}}$ and $\hat{\theta}_\ell^{\text{LA}}$ converge to the same estimand as the event-study coefficients $\hat{\beta}_\ell$ and $\hat{\theta}_\ell$ from Proposition 1 with $\tilde{t} = t_M$, minus an adjustment term consisting of a linear combination of differences in pre-policy trends between switchers and non-switchers. This linear combination is governed by weights ω_t^ℓ that satisfy $\sum_{t=t_m+1}^{t_M} \omega_t^\ell = \ell - t_M$ and where some weights can be negative or larger than one.

Most importantly, $\hat{\beta}_\ell^{\text{LA}}$ recovers the ATT if the parallel trend assumption holds in

all periods or if the differences in trends are constant over time, i.e. if Assumption 5 holds, or if the change in the pre-trends is linear, that is Assumption 6 holds. Practically this means that including a linear adjustment factor will not bias estimates in settings where the parallel trend assumption actually holds (although doing so may reduce the precision of the estimates). The upside of including a linear trend term is that it will eliminate the asymptotic bias in settings with constant differences of parallel trends, e.g. in settings with unequal baseline status and linear treatment effects (illustrated in Figure 1(b) and 3(b)).

However, inclusion of a linear trend adjustment is no panacea: if the differences in trends between the switchers and non-switchers are not constant over time, then both estimates $\hat{\beta}_\ell$ and $\hat{\theta}_\ell$ from Proposition 1 and estimates $\hat{\beta}_\ell^{\text{LA}}$ and $\hat{\theta}_\ell^{\text{LA}}$ from Proposition 3 are inconsistent, and it is not possible in general to determine which asymptotic bias is larger. Appendix A illustrates this fact by considering a simple three-period setting. Under one assumption simple event study produces estimates with smaller bias while under a different assumption the bias is smaller for a specification with a linear term trend.

To conclude, inclusion of a linear trend in event study specification will result in consistent estimates in settings where the parallel trend assumption holds or where the differences in trends are constant over time. However, if the differences in trends are non-constant, using specification (2) instead of (1) may increase the bias. Therefore, the choice of specification should be guided by economic theory or prior empirical evidence, or direct observation of pre-reform trends, if the data series are sufficiently long.

How many periods should one use to estimate the linear adjustment term, in other words, what periods should be included in the set \mathcal{T}_{LA} ? Unfortunately, it is not possible to provide a precise recommendation without making further assumptions on the evolution of potential outcomes and treatment effects. It is worth emphasizing, however, that the larger the number of periods used to estimate $\hat{\gamma}$, the more the strategy relies on the validity of the linearity assumption. On the other hand, when only two periods are used to estimate the difference in trends, this approach can be thought of as a “non-parametric” adjustment that subtracts an additional difference term from the DiD estimand (see Appendix A for details).

3.4 Sensitivity Analysis

In the context of canonical DiDs, recent studies by [Manski and Pepper \(2018\)](#) and [Rambachan and Roth \(2022\)](#) have proposed that researchers evaluate the sensitivity of treatment effect estimates to deviations from traditional parallel trends assumption, instead of imposing such an assumption outright. This type of sensitivity analysis may be particularly useful in unequal baseline DiD settings because of the likely violations of the parallel trends assumption. For example, researchers may wish to assess the sensitivity of the estimates to different treatment effect dynamics, instead of assuming that treatment effects are constant over time. We refer the reader to the aforementioned papers for further details on this type of sensitivity analysis.

4 Conclusion

In this paper we consider DiD settings in which treatment and control group experience unequal treatment status. Our analysis demonstrates that the usual event study estimation approach will not recover the ATT unless the treatment effect is constant over time. Furthermore, we show that the traditional parallel trend test is inappropriate and cannot provide adequate support for or against the parallel trends assumption.

We evaluate two intuitive solutions. First, we show that in settings with convergent treatment status, estimation in reverse will recover the ATT, but only when the outcomes are static. Second, correcting for divergent trends by including a linear term trend works but only in settings where differences in trends are constant over time. Otherwise, event studies with a linear trend may lead to more biased estimates than event studies without a linear trend.

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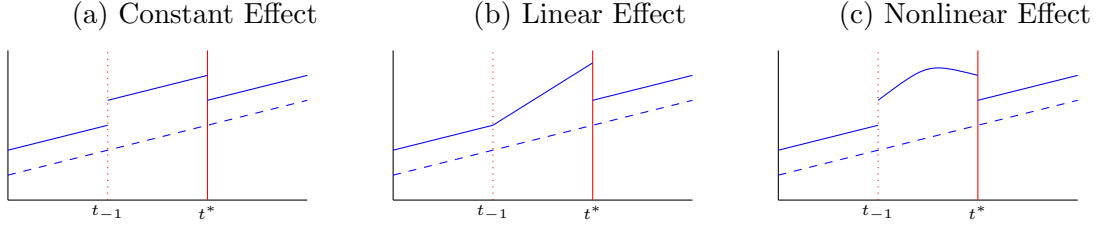
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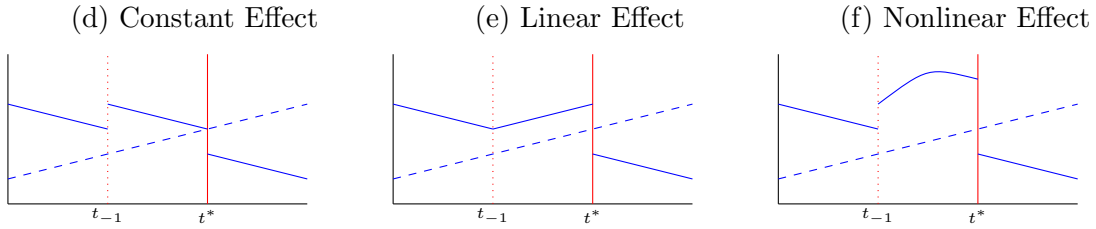
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Figure 1: Policy Reversal: Examples of Possible Outcome Paths

Panel A: Comparable Groups, Immediate Treatment Effect



Panel B: Not Comparable Groups, Immediate Treatment Effect



Notes: This figure illustrates the possible evolution of outcomes for treatment (solid line) and control (dashed line) groups under different scenarios, in a setting where a policy is adopted for some individuals in period t_{-1} and then is cancelled in period t^* . Panel A assumes that groups are truly comparable and therefore satisfy the “parallel trends” assumption in the pre-baseline period under Assumption 3, while Panel B assumes the opposite. All figures assume that potential outcomes are static, i.e. Assumption 2 holds. Three general types of treatment effects are considered: an immediate constant effect in Figures (a) and (d), a persistent linear effect in Figures (b) and (e), and a nonlinear effect in Figures (c) and (f). For simplicity, all figures assume that treatment leads to an increase in outcome, and that treatment effects are homogenous across groups.

Figure 2: Empirical Illustration – Convergent Treatment Status: [Jakobsen et al. \(2020\)](#)

(a) Equal baseline status:
both control and treated are taxed

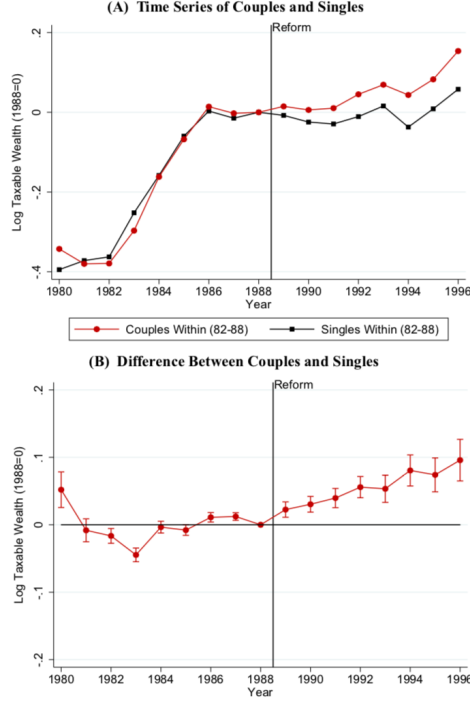
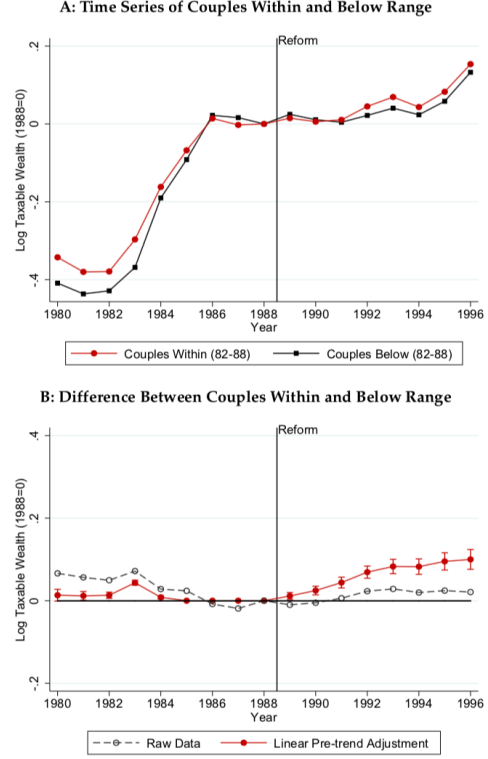


FIGURE IV
Difference-in-Differences Comparing Couples and Singles within Exempted Range

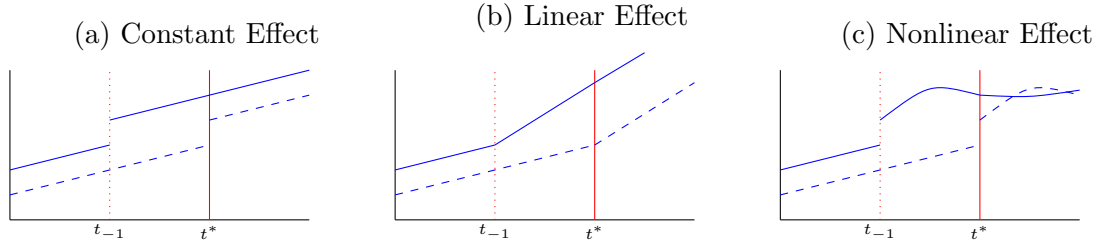
(b) Unequal baseline status:
treated are taxed but control are not



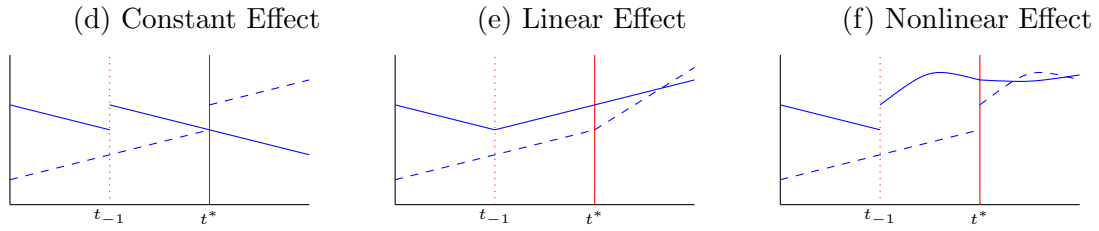
Notes: This figure reproduces (a) Figure IV and (b) Appendix Figure A.VII from [Jakobsen et al. \(2020\)](#). These figures show evolution of taxable wealth and difference between treatment and control groups before and after 1989 reform that increased the exemption threshold for couples but not for single individuals. Loosely speaking, in both figures, treatment group consists of couples who were subject to wealth tax before 1989 but not after 1989. In (a), the control group consists of single individuals who were subject to wealth tax before and after 1989 – “Singles” group, while in (b), the control group consists of couples who were exempt from wealth tax both before and after 1989 – the “Below Range” group. In all figures, the treatment group is shown in red dots, while the control group in black squares.

Figure 3: Universal Adoption: Examples of Possible Outcome Paths

Panel A: Comparable Groups, Immediate Treatment Effect



Panel B: Not Comparable Groups, Immediate Treatment Effect



Notes: This figure illustrates the possible evolution of outcomes for treatment (solid line) and control (dashed line) groups under different scenarios, in a setting where a policy is adopted for some individuals in period t_{-1} and then is adopted for all individuals in period t^* . Panel A assumes that groups are truly comparable and therefore satisfy the “parallel trends” under Assumption 4, while Panel B assumes the opposite. All figures assume that potential outcomes are static, i.e. Assumption 2 holds. Three general types of treatment effects are considered: an immediate constant effect in Figures (a) and (d), a persistent linear effect in Figures (b) and (e), and a nonlinear effect in Figures (c) and (f). For simplicity, all figures assume that treatment leads to an increase in outcome, and that treatment effects are homogenous across groups.

Figure 4: Empirical Illustration – Non-Convergent Treatment Status: [Jakobsen et al. \(2020\)](#)

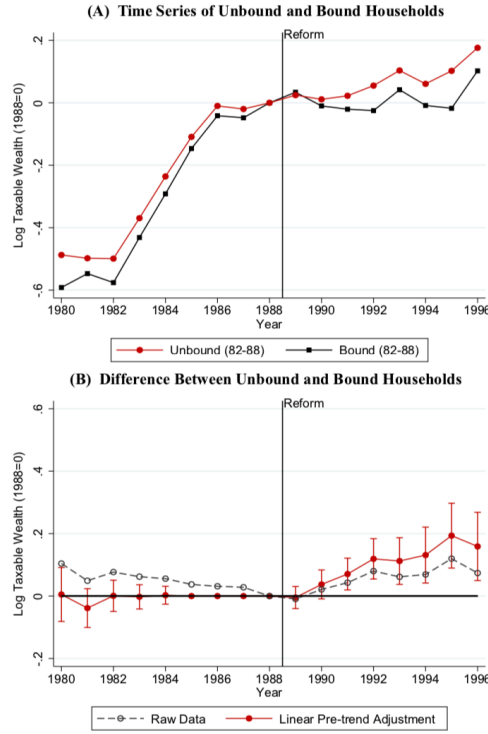


FIGURE VI
Difference-in-Differences Comparing Households Unbound and Bound
by Tax Ceiling

Notes: This figure reproduces Figure VI from [Jakobsen et al. \(2020\)](#). This figures show evolution of taxable wealth and difference between treatment and control groups before and after 1989 reform that reduced the wealth tax rate from 2.2% to 1% on the very wealthy – “the Unbound” group. The authors compare wealthy individuals to a control group subject to 0% marginal tax rate because of a tax ceiling provision (“Det Vandrette Skatteloft”), both before and after 1989 – the “Bound” group. The treatment group is shown in red dots, while the control group in black squares.

A Event Studies With or Without Linear Trend

Assumption – Illustration

If there are non-linear trends between switchers and non-switchers, then both the event study estimator and the linear adjustment estimator are inconsistent. Importantly, it is not possible to determine which asymptotic bias is larger in general.

To see this, consider the following simple example. Suppose there are two pre-change periods, and one post-change period, i.e. $t = 1, 2, 3$ and $t^* = T = 3$. Consider a policy reversal setting where the switchers are treated in periods 2 and 3 and then the treatment is removed in period 3 while the stayers remain untreated, i.e. $d = d_0$. Also suppose that the parallel trends Assumption 3 holds.

We will use the first two periods to estimate the linear trend, so that $\mathcal{T}_{\text{LA}} = \{1, 2\}$. The effect of the policy is then estimated in period $\ell = 3$. Applying Proposition 3, we find that the event-study and linear-adjusted estimators converge to:

$$\begin{aligned} \hat{\beta}_\ell \rightarrow_{\mathbb{P}} \beta_\ell^{\text{ES}} &= \mathbb{E}[Y_{i3}(d_0, d_{\text{pre}}) - Y_{i3}(d_{\text{pre}}, d_{\text{pre}}) | S_i = 1] \\ &\quad + \mathbb{E}[Y_{i3}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i3}(d_0, d_0) | S_i = 1] - \mathbb{E}[Y_{i2}(d_{\text{pre}}) - Y_{i2}(d_0) | S_i = 1], \end{aligned}$$

and

$$\begin{aligned} \hat{\beta}_\ell^{\text{LA}} \rightarrow_{\mathbb{P}} \beta_\ell^{\text{LA}} &= \mathbb{E}[Y_{i3}(d_0, d_{\text{pre}}) - Y_{i3}(d_{\text{pre}}, d_{\text{pre}}) | S_i = 1] \\ &\quad + \mathbb{E}[Y_{i3}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i3}(d_0, d_0) | S_i = 1] - \mathbb{E}[Y_{i2}(d_{\text{pre}}) - Y_{i2}(d_0) | S_i = 1] \\ &\quad - (\mathbb{E}[Y_{i2}(d_{\text{pre}}) - Y_{i2}(d_0) | S_i = 1] - \mathbb{E}[Y_{i1}(d_{\text{pre}}) - Y_{i1}(d_0) | S_i = 1]). \end{aligned}$$

Then the bias of event-study estimator equals to the change in the ATT between periods 2 and 3, whereas the bias of the linear-adjusted estimator is the difference in changes of the ATT between periods 2 and 3 and periods 1 and 2.

We now consider the following four cases, each resulting in an asymptotic bias summarized in Table 1.

Case 1: constant TE. Suppose that

$$\begin{aligned} 0 &= \mathbb{E}[Y_{i2}(d_{\text{pre}}) - Y_{i2}(d_0) | S_i = 1] - \mathbb{E}[Y_{i1}(d_{\text{pre}}) - Y_{i1}(d_0) | S_i = 1] \\ 0 &= \mathbb{E}[Y_{i3}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i3}(d_0, d_0) | S_i = 1] - \mathbb{E}[Y_{i2}(d_{\text{pre}}) - Y_{i2}(d_0) | S_i = 1]. \end{aligned}$$

so that the biases are zero for both estimators.

Case 2: linear TE. Suppose that for some constant κ

$$\begin{aligned}\kappa &= \mathbb{E}[Y_{i2}(d_{\text{pre}}) - Y_{i2}(d_0) | S_i = 1] - \mathbb{E}[Y_{i1}(d_{\text{pre}}) - Y_{i1}(d_0) | S_i = 1] \\ \kappa &= \mathbb{E}[Y_{i3}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i3}(d_0, d_0) | S_i = 1] - \mathbb{E}[Y_{i2}(d_{\text{pre}}) - Y_{i2}(d_0) | S_i = 1].\end{aligned}$$

Case 3: nonlinear decreasing TE. Suppose that for some constant κ

$$\begin{aligned}3\kappa &= \mathbb{E}[Y_{i2}(d_{\text{pre}}) - Y_{i2}(d_0) | S_i = 1] - \mathbb{E}[Y_{i1}(d_{\text{pre}}) - Y_{i1}(d_0) | S_i = 1] \\ \kappa &= \mathbb{E}[Y_{i3}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i3}(d_0, d_0) | S_i = 1] - \mathbb{E}[Y_{i2}(d_{\text{pre}}) - Y_{i2}(d_0) | S_i = 1].\end{aligned}$$

Case 4: nonlinear increasing TE. Suppose that for some constant κ

$$\begin{aligned}\kappa &= \mathbb{E}[Y_{i2}(d_{\text{pre}}) - Y_{i2}(d_0) | S_i = 1] - \mathbb{E}[Y_{i1}(d_{\text{pre}}) - Y_{i1}(d_0) | S_i = 1] \\ 3\kappa &= \mathbb{E}[Y_{i3}(d_{\text{pre}}, d_{\text{pre}}) - Y_{i3}(d_0, d_0) | S_i = 1] - \mathbb{E}[Y_{i2}(d_{\text{pre}}) - Y_{i2}(d_0) | S_i = 1].\end{aligned}$$

	ES estimator	LA estimator
Case 1: constant TE	0	0
Case 2: linear TE	κ	0
Case 3: nonlinear decreasing TE	κ	-2κ
Case 4: nonlinear increasing TE	3κ	2κ

Table 1: Asymptotic bias - ES vs LA estimators