# Difference-in-differences with Economic Factors and the Case of Housing Returns*

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February 28, 2023

**Abstract**

This paper studies how to incorporate observable factors in difference-in-differences and documents its empirical relevance. We show that even under random assignment directly adding factors with unit-specific loadings into the difference-in-differences estimation results in biased estimates. This bias, which we term the "bad time control problem" arises when the treatment effect covaries with the factor variation. Applied researchers partially control for the factor structure by using: (i) unit time trends, (ii) pre-treatment covariates interacted with a time trend and (iii) group-time dummies. We show that all these methods suffer from the bad time control problem and/or omitted factor bias. We propose two solutions to the bad time control problem. To evaluate the relevance of the factor structure we study US housing returns. Adding macroeconomic factors shows that factors have additional explanatory power and estimated factor loadings differ systematically across geographic areas. This results in substantially altered treatment effects.

Keywords: Difference-in-differences, Factor models, House prices.

JEL: C22, C54, G28, R30.

*We are grateful for financial support from the Swiss National Science Foundation (SNSF) through the grant "Trading and Financing during Market Stress #100018_172679". Additionally, we thank Christoph Basten, Brantly Callaway, Michel Habib, Mathias Hoffmann, Xiangying Huang, Philipp Lentner, Steven Ongena, Michael Wolf, Alexander Wagner and seminar participants at the KOF-ETH-UZH International Economic Policy Seminar and the University of Zurich for valuable comments.

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# 1 Introduction

Arguably, over the last decades with the adoption of quasi-experimental techniques identification in economics and finance has significantly improved. We have surveyed papers in the *American Economic Review* and 12% of the published papers in 2015 and 2016 use difference-in-differences for identification.[1,2] The majority (71%) of these papers use the two-way fixed effect (TWFE) estimator that only captures very restricted factor variation. The TWFE estimator has also been extensively employed when studying variables that have been documented to have a factor structure (e.g., stock returns and housing returns) and it has been shown theoretically that its omission leads to biased estimates.[3] This raises a number of questions that this paper addresses. First, if factors are observable how should they be included in the difference-in-differences framework? Second, applied researchers use techniques that partially control for the factor structure (e.g., unit time trends), but are these techniques sufficient and unbiased? Third, if we include a factor structure does this alter the conclusions of the difference-in-differences analysis?

An intuitive way to control for the factor structure is to augment the TWFE by allowing for unit specific loadings ($\lambda_i$) interacted with the factor realizations ($F_t$) in the difference-in-differences estimation. We call this estimation technique the full-sample estimator. Despite its intuitive appeal, we show that the full-sample estimator in general leads to biased estimates of the average treatment effect on treated (ATT). Intuitively, if the *true* treatment effect is time-varying ($\Delta_t$), but a non-dynamic estimator is used and the *true* treatment effect covaries with the factor realizations then the *estimated* factor loadings will capture some of the treatment effect. The end result is biased estimates of factor loadings that in turn result in a biased estimate of the ATT, and we term this bias as the "bad time control problem." The bad time control problem only depends on the covariance between the treatment effect and the factor realizations implying that it exists even under random assignment.

In fact, variants of the full sample estimator are commonly used. For example, it is common to augment the TWFE estimator by introducing unit-specific time trends (we

[1] A summary of our survey is attached at the end of the paper.

[2] de Chaisemartin, and D'Haultfoeuille (2020) report that 19% of papers published in the AER in years 2010 to 2012 use two-way fixed effects regressions.

[3] Gobillon and Magnac (2016) show that in the presence of an omitted factor structure the two-way fixed effect estimator is inconsistent.

denote this UTT), which is in effect the full sample estimator where factor realizations are replaced by a time trend. A related alternative is to interact pre-treatment covariates with a time-trend (we denote this CTT - covariates time-trend), this restricts the unit-specific loadings to a linear function of pre-treatment covariates. Since these two augmentations are restricted versions of the full-sample estimator, they are susceptible to the bad time control problem if a non-dynamic estimator is used. In our survey, six (five) out of 21 DiD papers use the UTT (CTT) estimators. However, only 18 out of 217 specifications combine time-trends (unit and covariate) with a dynamic estimator.[4,5] That implies for the remaining specifications, if the true treatment effect covaries with trends the estimated ATT's are biased.

There are two simple methods of avoiding the bad time control problem. First, when using the full-sample estimator (or variants thereof) if you estimate dynamic treatment effects then you are not subject to the bad time control problem since then time-variation in treatment effects cannot not be captured by the estimated loadings. As an alternative to using dynamic treatment effects, we propose a two-step procedure termed pre-treatment estimator. Initially, loadings are estimated using only pre-treatment sample and subsequently factor variation ($\hat{\lambda}_i \times F_t$) is subtracted from the dependent variable before the difference-in-difference regression is estimated. Since no treatment variation is used to estimate loadings, the pre-treatment estimator is unbiased.

Another commonly used augmentation that controls for factor variation is to introduce group-time dummies. Each group ($G_r$) is assigned a dummy (e.g., firms in the same industry) and then these dummies are interacted with dummies for time periods ($H_s$, which could either represent single or multiple time periods). We call this control procedure the dummy factor method. 7 out of 21 DiD papers in our survey use group-time dummies. It reduces the bias from omitting the factor structure by removing the between group factor variation. If the dummies aggregate over multiple periods (e.g., $H_s$, are five year periods) then the dummy factor estimator suffers from the bad time control problem since it is possible that the dummy factor variation covaries with the treatment effect. Additionally, since

[4] For details see Appendices C and D. Dynamic treatment effects are most often used in event-strudy graphs.

[5] Bailey and Goodman-Bacon (2015) interact pre-treatment covariates with a time trend while estimating treatment effects for various event periods. Additionally, in Figures 3 and 4 in Currie, Davis, Greenstone and Walker (2015) there are treatment effects estimated per period while factor variation is saturated using time trends. Finally, columns 1-3 of Table 5 in Bøler, Moxnes and Ulltveit-Moe (2015) use dynamic treatment effects and a unit time-trend.

it only uses *within* dummy group variation it also assigns treated observations different weights than the TWFE estimator. Fortunately, the dummy factor always assigns positive weights to all treated observations (unlike the TWFE in staggered treatment settings, see de Chaisemartin and D'Haultfœuille, 2020, and Goodman-Bacon, 2021). Put differently, dummy factors that are comprised of block of multiple periods suffer from the bad time control problem, but it is much less severe than under the full sample estimator since all treated observations still have positive weights.

To evaluate the importance of controlling for factor variation and the relative performance of the different methods we study housing returns. There is extensive evidence that they have been shown to exhibit a factor structure. For example, Cotter, Gabriel and Roll (2014) document the explanatory ability of macroeconomic factors in the cross-section of MSA (metropolitan statistical area) housing returns.[6] We use county-level real estate price data from the Federal Housing Finance Agency (FHFA) combined with the macroeconomic factors founded in Cotter et al. (2014).

First, we examine the performance of the TWFE, full-sample and pre-treatment estimators with randomly generated placebo interventions. Even with two-way clustered standard errors we find that the TWFE rejects 50% more than expected if the true treatment effects are zero. The full-sample estimator rejects 100% more than expected. This suggests that in our setting, controlling for factor variation using the full-sample estimator results in worse performance than ignoring it. The pre-treatment estimator with optimally selected economic factors rejects roughly as expected.

Second, we revisit the main difference-in-differences specifications of Favara and Imbs (2015) and Zevelev (2021). In the case of Favara and Imbs (2015), introducing optimally selected factors renders estimated treatment effects insignificant both when using the full and pre-treatment estimators. We also considered all possible factor combinations. For the full-sample estimator adding up to 3 factors implies that only 41.9% of estimated treatment effects remain statistically significant. For the pre-treatment estimator adding up to 3 factors implies that only 53.3% of the treatment effects are still significant. In addition, we show that the estimated state loadings differ systematically across U.S. regions.

[6]Further, arbitrage pricing theory (APT) models with macroeconomic factors have been used in Chan et al. (1990), statistical factors (PCA) are employed by Titman and Warga (1986) while equity based factors such as the Fama-French factors, momentum and liquidity have studied in the real estate context by Peterson and Hsieh (1997), Hung and Glascock (2010) and Cannon and Cole (2010).

Zevelev (2021) studies a Texas collateral reform. Given the importance of oil for Texas he uses the full-sample estimator with the oil price as a factor. In addition he introduces state time-trends. In the absence of factor controls the estimated treatment effect has the opposite sign. This highlights the importance of using factors especially when treatment controls are such diverse units as states. In most cases, introducing our economic factors reduces estimated treatment effect, suggesting that economic factors are important controls. One strength of Zevelev's paper is that he analyses the treatment effect only considering neighboring counties. In this case, the pre-treatment estimator with economic factors results in treatment effects that are very similar to the full sample estimator of Zevelev (2021), suggesting the importance of internal validity in research design. Overall, our empirical replications highlight the importance of factor controls, but also suggest that factor controls are not a substitute for strong internal validity.

This paper makes a number of contributions. First, we introduce the bad time control problem that is a sibling of the bad control problem. Bad controls are controls that are affected by treatment *status* while the bad time control problem arises due to a correlation between the treatment effect *value* and controls (factors in our case). Second, we characterize the bias of the full-sample estimator and commonly used augmentations of TWFE estimators. Third, we show that for housing returns the TWFE estimator does not capture sufficient factor variation, especially when interventions are at the state level and the sample is comes from the entire United States.

The rest of the paper is organized as follows. Section 2 examines the related literature while section 3 describes our theoretical results and Section 4 presents our simulations. Our empirical evidence can be found in Section 5 and Section 6 concludes.

## 2 Related Literature

We contribute to the growing literature that studies methodological choices of economics and finance researchers. For example, Bertrand et al. (2004) provide guidance how to treat standard errors in difference-in-differences, while Karpoff and Wittry (2017) show that past work using state level interventions on anti-takeover laws is not robust to controlling for hisorical and institutional context as well as political economy.

In this paper, like Gobillon and Magnac (2016) we depart from parallel trends assumption due to an omitted factor structure and we use their result that the TWFE estimator is inconsistent when the factor structure is omitted.[7] However, our paper assumes that the omitted factor is observable and explores the consequences of including it in the difference-in-difference regression.

Caetano et al. (2022) study the performance of the TWFE estimator with time varying covariates. Among other things, they show that TWFE estimator is biased if the covariates are confounded with, rather than affected by, treatment. Essentially, they carefully extend the bad control problem in multiple new dimensions while we extend the bad control problem in the time dimension due to factor variation.

In an elegant paper, Callaway and Karami (2022) show that in an interactive fixed effect setting (i.e., factor structure) GMM can estimate the ATT consistently under the assumption that there exists covariates with time invariant effects. We do not focus on a comprehensive solution in a general setting but more on the validity of currently widely-used methods. Our proposed pre-treatment estimator, provides the researcher with an easily implementable solutions when treatment effect covaries with the factor.

There is a burgeoning literature highlighting drawbacks of the two-way fixed effect estimator. Even under parallel trends this literature documents that the TWFE estimators may assign wrong and even negative weights to treated observations. The intuition for this is that with heterogeneous and staggered treatment already treated units may become comparison units and resulting in very poor treatment effect estimates (de Chaisemartin and d'Haultfoueille (2020), Borosyak, Jaravel and Spiess (2021), Goodman-Bacon (2021), Sun and Abraham (2021)). The relevance of these issues for accounting and finance is highlighted by the impressive survey contained in Baker et al. (2022).

# 3 Combining Factors with the DiD

In this section we describe the implications for inference using difference-in-difference when the true data generating process has a linear factor structure. First, we characterize the bias when the factor is omitted. Second, we establish that the full-sample estimator suffers from the bad time control problem. Additionally, we characterize the degree to which commonly used techniques such as unit time trends, covariate time trends, and dummy factors suffer

[7] Roth (2022) argues that the power of testing for parallel trends is often low implying that we may often fail to reject even if trends are not parallel.

from the bad time control problem. Finally, we provide two solutions to researchers that want to control for time trends. First, we show that combining dynamic treatment effects with the full-sample estimator eliminates a bad time control problem. Second, we propose and prove that the two-step pre-treatment estimator results in unbiased estimation of both the treatment effect and factor loadings.

Suppose the observed sample consists $\{Y_{it}, D_i, P_t, F_t\}$ for $N (N \geq 2)$ $(i = 1, \ldots, N)$ units across $T (T \geq 2)$ $(t = 1, \ldots, T)$ periods. $Y_{it}$ is the observed outcome and $Y_{it}(0)$ and $Y_{it}(1)$ are potential outcome if not treated and if treated, respectively. $\lambda_i$ is $r \times 1$ vector of individual-specific **unobserved** loadings and $F_t$ is $r \times 1$ vector of **observed** time-specific factors, where $r$ is the number of factors[8,9]. Including covariates that are independent from treatment would have made the notation significantly more complicated, but our results would remain qualitatively unchanged[10]. $D_i$ is a binary treatment indicator such as $D_i = 1$ if unit $i$ is in the treatment group. Suppose treatment happens at time $t^*$ and $P_t$ is a binary time indicator such as $P_t = 1$ if $t \geq t^*$.

**Assumption 1** (Random sampling)**.** *Observed sample consists of* $\{Y_{it}, D_i, P_t, F_t\}_{i=1,t=1}^{N,\ T}$ *which are independent and identically distributed.*

**Assumption 2** (Conditional parallel trend)**.** *Conditional on factor structure, the parallel trend assumption holds.*

$$\begin{aligned}
&\mathbb{E}\left[Y_{it}(0) - Y_{is}(0) - Y_{jt}(0) + Y_{js}(0) \middle| \lambda_i, \lambda_j, F_t, F_s, D_i = d_1, D_j = d_2, P_t = p_1, P_s = p_2\right] \\
=&\mathbb{E}\left[Y_{it}(0) - Y_{is}(0) - Y_{jt}(0) + Y_{js}(0) \middle| \lambda_i, \lambda_j, F_t, F_s\right] \qquad \forall (d_1, d_2, p_1, p_2) \in \{0,1\}^4
\end{aligned}$$

**Assumption 3** (Strict exogeneity)**.** *Potential outcomes are mean independent with factor loadings and factor realizations assigned to other units and periods.*

$$\mathbb{E}\left[Y_{it}(0) \middle| \lambda_i, F_t\right] = \mathbb{E}\left[Y_{it}(0) \middle| \boldsymbol{\lambda}, \boldsymbol{F}\right]$$

$$\mathbb{E}\left[Y_{it}(1) \middle| \lambda_i, F_t\right] = \mathbb{E}\left[Y_{it}(1) \middle| \boldsymbol{\lambda}, \boldsymbol{F}\right]$$

[8] For simplicity we consider a single factor, but all of our results can be generalized into a multi-factor setting.

[9] Note that a time fixed effects can be represented as a factor that all units have identical loadings to. Symmetrically, the unit fixed effect can be seen as time-invariant factor, but with unit specific loadings.

[10] Caetano et al. (2022) carefully studies the difference-in-differences estimator with covariates affected with treatment status. Also see Roth et al. (2022) section 4.2 for a review.

**Assumption 4** (Bilinear factor structure)**.** *The conditional expectation of the potential outcome if not treated is a bilinear function of $\lambda$ and $F$.*

$$\mathbb{E}\left[Y_{it}(0) - Y_{is}(0) - Y_{jt}(0) + Y_{js}(0)\right] = (\lambda_i - \lambda_j)'(F_t - F_s)$$

**Assumption 5** (Stable unit treatment value assumption)**.** *Potential outcomes are independent with the treatment assigned to other units and periods.*

$$Y_{it}(0), Y_{it}(1) \perp\!\!\!\perp \boldsymbol{D}_{-i}, \boldsymbol{P}_{-t}$$

**Assumption 6** (Stable loading and factor assumption)**.** *Factor loadings and factor realizations are independent with the treatment assigned to other units and periods.*

$$\lambda_i \perp\!\!\!\perp \boldsymbol{D}_{-i} \qquad F_t \perp\!\!\!\perp \boldsymbol{P}_{-t}$$

Define $\Delta_{it} = Y_{it}(1) - Y_{it}(0)$ as the *unobserved* true treatment effect for each unit and period. The goal of Difference-in-Difference (DID) estimators is to measure the average treatment effect on treated (ATT)

$$\alpha^{\text{ATT}} = \mathbb{E}[\Delta_{it} | D_i = 1, P_t = 1]$$

### 3.1 Two-Way Fixed Effect estimator

The classical Two-Way Fixed Effect (TWFE) estimator can be defined as,

$$(\hat{\alpha}^{\text{TWFE}}, \hat{\gamma}_i^{\text{TWFE}}, \hat{\eta}_t^{\text{TWFE}}) = \underset{\alpha,\gamma,\eta}{argmin} \Big\{ \sum_{t=1}^{T} \sum_{i=1}^{N} (Y_{i,t} - \gamma_i - \eta_t - \alpha D_i P_t)^2 \Big\}$$

**Proposition 1.** *Given an omitted linear factor structure, the Two-Way Fixed Effect estimator estimates $\hat{\alpha}^{TWFE}$ as,*

$$\mathbb{E}[\hat{\alpha}^{TWFE}] = \alpha^{ATT} + (\mathbb{E}[\lambda_i | D_i = 1] - \mathbb{E}[\lambda_i | D_i = 0])(\mathbb{E}[F_t | P_t = 1] - \mathbb{E}[F_t | P_t = 0]). \qquad (1)$$

**Proof** See Appendix A.2.

Thus, the TWFE estimator is unbiased unless $\mathbb{E}[\lambda_i | D_i = 1] - \mathbb{E}[\lambda_i | D_i = 0] = 0$ or $\mathbb{E}[F_t | P_t = 1] - \mathbb{E}[F_t | P_t = 0] = 0$. This expression is non-zero if loadings of treated and control units are not equal and the factor exhibits time-series variation. This result is equivalent to Eq. (21) in Gobillon and Magnac (2016).[11] Given the construction of the

[11] They prove inconsistency of the difference-in-differences estimator even when the factor is deterministic. For our purposes, we are going to treat the factor as a random variable (similar to (Bai, 2009) Bai, 2009) since this corresponds closer to the economic setting we are interested in.

counterfactual any level differences between treated and control and any level differences between before and after treatment are controlled for, but the interaction of differences in the two dimensions may affect identification. Given that the TWFE is generically biased there are a number of estimators that can potentially control for the omitted factor structure.

### 3.2 Full-sample estimator

If our factors are observable we can simply introduce the factors and estimate unit loadings which we refer to as the full-sample (FS) estimator,

$$(\hat{\alpha}^{\text{FS}}, \hat{\gamma}_i^{\text{FS}}, \hat{\eta}_t^{\text{FS}}, \hat{\lambda}_i^{\text{FS}}) = \underset{\alpha,\gamma,\eta,\lambda}{argmin} \Big\{ \sum_{t=1}^{T} \sum_{j=1}^{N} (Y_{i,t} - \gamma_i - \eta_t - \lambda_i F_t - \alpha D_i P_t)^2 \Big\}$$

where $\lambda_i$ are unit specific loadings and $F_t$ are observable factor realizations. In practice it is straightforward to estimate unit specific loadings, one interacts unit dummies with the time-series of factor realizations to allow for unit specific sensitivities to the factors.

**Proposition 2.** *When the factor realizations are exogenously determined, the FS estimator can be expressed as*

$$\begin{aligned} \mathbb{E}[\hat{\alpha}^{FS}] &= \alpha^{ATT} + w^{FS} cov(F_t, \Delta_{it} | D_i P_t = 1). \\ where\ w^{FS} &= \mathbb{E}\left[ \frac{N_T T_P - 1}{N_T T_P} \cdot \frac{\overline{F}_{pre} - \overline{F}}{\sigma_F^2 + (\overline{F}_{post} - \overline{F})(\overline{F}_{pre} - \overline{F})} \right] \end{aligned} \tag{2}$$

**Proof** See Appendix A.3.

Proposition 2 illustrates that only in partcular circumstances will the full-sample estimator uncover the true ATT. It will be unbiased if there is no difference in factor realizations between the pre and post treatment periods ($\mathbb{E}[F_t|P_t = 0] - \mathbb{E}[F_t|P_t = 1]$) or the factor realizations do not covary with the treatment effect ($\text{Cov}(F_t, \Delta_{it}|D_i P_t = 1) = 0$).

**Corollary 1.** *If the treatment effect is time invariant ($\forall t,\ \Delta_{it} = \Delta_i$) then the full-sample estimator is unbiased since $Cov(F_t, \Delta_i|D_i P_t = 1) = 0$*

Corollary 1 implies that treatment heterogeneity across units does not result in bias. The bias is driven by the covariance in the time dimension.

**Corollary 2.** *The bias of the full-sample estimator is independent of the unit loadings ($\lambda_i$)*

One implication of Corollary 2 is that the full-sample estimator may be more biased than the classical TWFE estimator. The intuition is that if the loadings of treated and control units are sufficiently close ($\mathbb{E}[\lambda_i|D_i = 1] \simeq \mathbb{E}[\lambda_i|D_i = 0]$) then the bias in the TWFE estimator will not be large while if the treatment effect significantly covaries with the factor then the full-sample may be more biased than the TWFE.

It is important to consider what happens to the bad time control problem as the number of units increase and as the number of time periods increase. First, the number of units does not affect the size of the bad time control problem. Second, to evaluate the impact of the increase in the number of time periods note that $\mathbb{E}[F_t]$ can be expressed as $\mathbb{E}[P_t]\,\mathbb{E}[F_t|P_t = 1] + (1 - \mathbb{E}[P_t])\,\mathbb{E}[F_t|P_t = 0]$. If we keep the conditional expectations of factor realizations given treatment constant as $T$ increases we can focus on the term $\mathbb{E}[P_t]$ which in turn can be expressed as $T_P/T$ (the ratio of treated periods to total time periods). When $T$ increases $T_P/T$ tends to 0 and $\mathbb{E}[F_t]$ tends to $(\mathbb{E}[F_t|P_t = 0])$ which in turn means the numerator $(\mathbb{E}[F_t|P_t = 0] - \mathbb{E}[F_t])$ tends to 0. This is not surprising since effectively there is no post-treatment period. However, a more likely case, is when we add more time periods the ratio of treated time periods remains constant (e.g., half the time periods are still treated) then the full-sample bias is unaffected when $T$ is increased.

In the admittedly unrealistic setting with observable loadings and unobservable factors the full-sample estimator would still be biased, but the bias would depend on the covariance of the loadings with treatment heterogeneity in the unit dimension.

In practice, researchers add unit-specific time trend, termed unit time trend (UTT) estimator, to control for time-varying heterogeneity. However, it is a special case of the full sample estimator and have the same issue of "bad time control problem."

We take unit-specific linear time trend $F_t = t$ as an example. If the time trend is polynomial, it does not make an essential difference. Similar to the bias of the full sample estimator, the unit time trend estimator is generally biased.

**Corollary 3.** *The unit time trend (UTT) estimator is unbiased if and only if the treatment effect is orthogonal to the linear time trend over the treated observations ($Cov(\Delta_{it}, t|\, D_iP_t = 1) = 0$).*

**Proof** See Appendix A.4.

Another common way to augment TWFE estimator is by adding covariates interacted with a time trend. To avoid the bad control problem, researchers often use pre-treatment covariates and control for covariate time trend ($X_{i0} \cdot t$). We show that this augmentation may circumvent the bad control problem, but leads to a bad time control problem.

**Proposition 3.** *The covariate time trend estimator leads to a bad time control problem as long as* $Cov(\Delta_{it}, X_{i0} \cdot t | D_i P_t = 1) \neq 0$

**Proof** See Appendix A.5.

### 3.3 Dummy factor estimator

The dummy factor is defined by the granularity chosen along the unit and time dimension. We examine two limiting cases of the dummy factor. First, we consider the case where we define the group dummies $R_i$ for $|R| < N$ groups interacted with $T$ period dummies (i.e., we have the most amount of granularity in the time dimension and less than full granularity in the unit dimension). Second, we consider the case when we have $N$ groups (i.e., full granularity in the unit dimension) interacted with $S_t$ time periods, where $|S| < T$.

**Proposition 4.** *(i) The dummy factor with $R \times T$ dummies does not suffer from the full-sample bias. The weighting of observations is altered, implying that the ATT is generally not estimated. (ii) The dummy factor with $N \times S$ suffers from the full-sample bias and weighting error.*

**Proof** See Appendix A.6.

In conclude, the dummy factor estimate is a convex combination of of TWFE estimates for each group. Omitted factor bias of the dummy factor estimator, compared to the TWFE estimator, is reduced because factor structure variation across groups is eliminated. When all groups have the same treated observation ratio, the dummy factor estimator degenerates into the TWFE estimator.

When loadings are balanced within each group, the omitted factor bias will be equal to zero, even though the dummy factor estimator is still biased because of the weighting issue. The weighting issue is not extremely severe as in staggered DiD, because the weighting is grantee to be between 0 and 1. In other words, the estimated treatment effect term is a convex combination of the true treatment (unlike in de Chaismartin and D'Haullfulle, 2020) so it guarantee that the estimator does not become negative if true treatment effects are all positive.

The weighting issue can transform into the bad time control problem. Though only $N \times S$ dummy factor suffers from it because $R \times T$ dummy factor captures factor variation through full granularity in the time dimension.

### 3.4 Pre-treatment estimator

Given a sufficiently long time-series another possible solution is to estimate factor loadings only using pre-treatment variation and then use the estimated loadings when estimating the ATT in the full sample. We refer to this two step procedure as the pre-treatment (PT) estimator. First loadings are estimated over pre-treatment periods,

$$(\hat{\lambda}_i^{\mathrm{PT}}) = \underset{\lambda}{argmin}\Big\{\sum_{t=1}^{T}(1-P_t)\sum_{j=1}^{N}(Y_{i,t}-\gamma_i-\eta_t-\lambda_i F_t)^2\Big\} \tag{3}$$

and the estimated loadings $(\hat{\lambda}_i)$ are then used in the full sample when estimating the ATT,

$$(\hat{\alpha}^{PT}, \hat{\gamma_i}^{\mathrm{PT}}, \hat{\eta_t}^{\mathrm{PT}}) = \underset{\alpha,\gamma,\eta}{argmin}\Big\{\sum_{t=1}^{T}\sum_{j=1}^{N}(Y_{i,t}-\gamma_i-\eta_t-\hat{\lambda}_i^{\mathrm{PT}} F_t-\alpha D_i P_t)^2\Big\}. \tag{4}$$

The pre-treatment estimator avoids estimated loadings capture the treatment effect variation and therefore does not lead to biased estimation.

**Proposition 5.** *The pre-treatment estimator results in an unbiased estimate of the Average Treatment on Treated (ATT).*

**Proof** See Appendix A.7.

The pre-treatment estimator has a number of advantages over the GMM estimator with dynamic treatment effect proposed by Callaway and Karami (2022). First, recovering the ATT and standard error using the dynamic estimator requires additional calculations. Second, it is not completely clear how the dynamic estimator performs when treatment is staggered. The advantages of the dynamic estimator is that it does not require a long time-series to estimate loading and provides useful information about the treatment effect over time.

# 4 Simulation Evidence: Different factor estimation methods

To illustrate our theoretical findings, we simulate data according to the following data generating process,

$$Y_{it} = \gamma_i + \eta_t + \lambda_i F_t + \Delta_{it} D_i \times P_t + \varepsilon_{it} \tag{5}$$
$$\gamma_i \sim N(0, \sigma_\gamma^2) \qquad \eta_t \sim N(0, \sigma_\eta^2) \qquad \varepsilon_{it} \sim N(0, \sigma_\varepsilon^2)$$

$\gamma_i$ are unit fixed effects, $\eta_t$ are time fixed effects, $\lambda_i F_t$ represents the factor structure, and $\Delta_{it}$ is the heterogeneous and time-varying treatment effects. By construction the unit and time fixed effects are independent from our other key quantities. In order to allow for loading differences between treated and control we simulate loadings as follows,

$$\lambda_i = P_i + \mu(D_i - \mathbb{E}[D_i]) \quad P_i \sim N(0,1)$$

where the parameter $\mu$ allows us to shift the loading of treated units while maintaining a mean loading of zero. T Our factor realizations are given by,

$$F_t = Q_t + \nu(t - \mathbb{E}[t]) \quad Q_t \sim N(0,1)$$

where the key parameter is $\nu$ that captures the time-trend of our factor. Like with our loadings our factors are modeled to have mean zero. he last ingredient of our simulation is our treatment effects which are simulated as follows,

$$\Delta_{it} = ATT + \sigma_\Delta(\psi U_i + \phi V_t + (1 - \psi^2 - \phi^2) W_{it})$$
$$\text{corr}(U_i, P_i) = \rho_{\Delta,\lambda} \qquad corr(V_t, Q_t) = \rho_{\Delta,F} \qquad U_i, V_t, W_{it} \sim N(0,1)$$

where the true ATT is 1.0, $U_i$ is a unit specific treatment effect, $V_t$ is the time specific treatment effect and the term $(1 - \psi^2 - \phi^2)W_{it}$ ensures that the total variance is kept constant. Crucially, the parameter $\text{corr}(U_i, P_i) = \rho_{\Delta,\lambda}$ allows for a possible correlation between the loadings and the treatment effect and similarly $corr(V_t, Q_t) = \rho_{\Delta,F}$ allows for a correlation between the treatment effect and the factor realization.

We perform 1000 iterations for each cell. In our simulation analysis, we set number of units $N = 1000$, number of treated unites $N_T = 0.3N = 300$, number of periods $T = 20$, number of post-treatment periods $T_P = 0.5T = 10$. We also set the baseline value of stardard deviation of unit fixed effect $\sigma_\gamma = 1$, standard deviation of time fixed effect

**Table 1:** Homogeneous Treatment Effect

We set $\sigma_\Delta = 0$ (homogeneous treatment effects). The loading difference $\mu$ and factor trend $\nu$ are the parameters of interest. All other parameter are the same as baseline setting. The mean of two-way fixed effect estimators is displayed in the first line without any parentheses, the mean of full sample estimator is displayed in the second line with round parentheses, and the mean of pre-treatment estimators is displayed in the third line with square parentheses.

| **TWFE Estimator (FS Estimator) [PT Estimator]** | | Loading Difference $\mu$ | | | | | |
|---|---|---|---|---|---|---|---|
| | | 0 | 0.2 | 0.4 | 0.6 | 0.8 | 1 |
| Factor Trend $\nu$ | 0 | 0.9958 | 1.0075 | 0.9824 | 1.0070 | 1.0029 | 0.9969 |
| | | (0.9988) | (1.0044) | (0.9993) | (1.0036) | (1.0031) | (0.9941) |
| | | [0.9986] | [1.0037] | [0.9998] | [1.0022] | [1.0041] | [0.9936] |
| | 0.5 | 0.9947 | 1.0437 | 1.1183 | 1.1434 | 1.2331 | 1.2454 |
| | | (0.9977) | (0.9964) | (1.0033) | (0.9995) | (0.9976) | (1.0000) |
| | | [0.9983] | [0.9967] | [1.0055] | [1.0001] | [0.9976] | [0.9999] |
| | 1 | 1.0030 | 1.1072 | 1.1973 | 1.3525 | 1.4156 | 1.5557 |
| | | (1.0018) | (0.9975) | (1.0015) | (0.9982) | (0.9966) | (1.0000) |
| | | [1.0020] | [0.9978] | [1.0012] | [0.9979] | [0.9964] | [0.9997] |
| | 1.5 | 0.9998 | 1.1387 | 1.3233 | 1.4781 | 1.6147 | 1.7812 |
| | | (0.9991) | (1.0025) | (1.0032) | (0.9960) | (1.0007) | (0.9949) |
| | | [0.9973] | [1.0038] | [1.0029] | [0.9975] | [1.0005] | [0.9954] |
| | 2 | 1.0059 | 1.2167 | 1.4339 | 1.6442 | 1.8459 | 2.0769 |
| | | (0.9969) | (0.9960) | (1.0048) | (1.0059) | (0.9909) | (1.0000) |
| | | [0.9971] | [0.9963] | [1.0042] | [1.0061] | [0.9916] | [1.0011] |
| | 2.5 | 1.0051 | 1.2607 | 1.5119 | 1.7841 | 2.0723 | 2.3543 |
| | | (1.0005) | (0.9957) | (0.9975) | (0.9989) | (1.0010) | (1.0055) |
| | | [1.0017] | [0.9981] | [0.9970] | [1.0000] | [0.9997] | [1.0011] |

$\sigma_\eta = 3$, standard deviation of error term $\sigma_\varepsilon = 3$, standard deviation of treatment effect $\sigma_\Delta = 2$, loadings difference between treatment group and control group $\mu = 0.4$, linear time trend $\nu = 1.5$, degree of time-varition in the treatment effect $\phi = 0.6$, degree of unit heterogeneity in the treatment effect $\psi = 0.6$, correlation between treatment effect and loadings $\rho_{\Delta,\lambda} = 0.6$, and the correlation between treatment effect and factors $\rho_{\Delta,F} = 0.6$.

Table 1 is designed to illustrate Proposition 1, that is we assume the treatment effect is homogeneous. On the horizontal axis we change the loading difference of treated and control units. In the left most column there is no loading differential while on in the right most the loading differential is one corresponding to one standard deviation of the loading ($\lambda$). On the vertical axis we allow for an increasing factor trend, from none to 1.6 factor deviations. If there is no loading difference or no factor trend the TWFE is unbiased (there is only sampling error around the true ATT of 1). However, as we move diagonally introducing both a loading differential and factor trend the TWFE estimator gets significantly biased. In the bottom right cell it estimates an ATT of 2.25.

**Table 2:** Heterogeneous Treatment Effect

The loading difference $\mu$ and factor trend $\nu$ are the parameters of interest. All other parameter are the same as baseline setting. The mean of two-way fixed effect estimators is displayed in the first line without any parentheses, the mean of full sample estimator is displayed in the second line with round parentheses, and the mean of pre-treatment estimators is displayed in the third line with square parentheses.

| **TWFE Estimator (FS Estimator) [PT Estimator]** | | Loading Difference $\mu$ | | | | | |
|---|---|---|---|---|---|---|---|
| | | 0 | 0.2 | 0.4 | 0.6 | 0.8 | 1 |
| Factor Trend $\nu$ | 0 | 0.9810 | 1.0048 | 0.9698 | 1.0018 | 1.0122 | 0.9860 |
| | | (0.9895) | (1.0091) | (0.9948) | (0.9981) | (1.0156) | (0.9859) |
| | | [0.9839] | [1.0009] | [0.9872] | [0.9971] | [1.0133] | [0.9827] |
| | 0.5 | 1.0187 | 1.0356 | 1.1295 | 1.1334 | 1.2078 | 1.2415 |
| | | (0.9796) | (0.9509) | (0.9651) | (0.9459) | (0.9198) | (0.9476) |
| | | [1.0222] | [0.9886] | [1.0167] | [0.9900] | [0.9724] | [0.9959] |
| | 1 | 1.0046 | 1.1134 | 1.2058 | 1.3700 | 1.4069 | 1.5719 |
| | | (0.9160) | (0.9108) | (0.9176) | (0.9088) | (0.8905) | (0.9196) |
| | | [1.0036] | [1.0040] | [1.0098] | [1.0154] | [0.9877] | [1.0160] |
| | 1.5 | 1.0067 | 1.1228 | 1.3278 | 1.4740 | 1.6078 | 1.7869 |
| | | (0.8638) | (0.8522) | (0.8641) | (0.8494) | (0.8577) | (0.8593) |
| | | [1.0042] | [0.9880] | [1.0074] | [0.9934] | [0.9936] | [1.0011] |
| | 2 | 1.0125 | 1.2299 | 1.4341 | 1.6485 | 1.8406 | 2.0701 |
| | | (0.8126) | (0.8236) | (0.8177) | (0.8165) | (0.7948) | (0.8029) |
| | | [1.0037] | [1.0095] | [1.0045] | [1.0104] | [0.9863] | [0.9943] |
| | 2.5 | 1.0221 | 1.2675 | 1.4957 | 1.8047 | 2.0882 | 2.3723 |
| | | (0.7775) | (0.7756) | (0.7622) | (0.7974) | (0.7796) | (0.7882) |
| | | [1.0187] | [1.0049] | [0.9808] | [1.0207] | [1.0156] | [1.0191] |

Turning to Table 2, we now set the $\sigma_\Delta = 2$ so the treatment effect is heterogeneous. The bias of the TWFE remains the same as with homogeneous treatment effect. Also the pre-treatment estimator performs as before. However, treatment heterogeneity implies that the full sample estimator (within parentheses) becomes biased. The bias is only present when there is a factor trend (in the first row the estimated ATT of the full-sample estimator is 1). However as the factor trend increases and it reaches 2.5 the estimated ATT using the full sample estimator is 0.78. Finally, the estimated ATT using the full sample estimator is independent of the loading difference as we move along columns verifying corollary 1.

In Table 3 we vary degree of treatment effect heterogeneity in the unit (columns) and time (rows) dimension. Regarding the full sample estimator (within parentheses), it performs the same irrespective of the amount of variance explained by unit heterogeneity

**Table 3:** Treatment Effect Heterogeneity Asymmetry

The treatment effect unit heterogeneity $\psi$ and time heterogeneity $\phi$ are the parameters of interest. All other parameter are the same as baseline setting. The mean of two-way fixed effect estimators is displayed in the first line without any parentheses, the mean of full sample estimator is displayed in the second line with round parentheses, and the mean of pre-treatment estimators is displayed in the third line with square parentheses.

| **TWFE Estimator** (**FS Estimator**) [**PT Estimator**] | TE Time Heterogeneity $\phi$ | TE Unit Heterogeneity $\psi$ = 0 | 0.2 | 0.4 | 0.6 | 0.8 | 1 |
|---|---|---|---|---|---|---|---|
| | 0 | 1.3058 | 1.3183 | 1.3084 | 1.3133 | 1.3069 | 1.3071 |
| | | (1.0004) | (1.0002) | (0.9938) | (1.0007) | (1.0016) | (1.0018) |
| | | [0.9995] | [0.9996] | [0.9921] | [1.0032] | [1.0006] | [1.0008] |
| | 0.2 | 1.3364 | 1.3320 | 1.3163 | 1.3011 | 1.3406 | |
| | | (0.9508) | (0.9507) | (0.9590) | (0.9569) | (0.9551) | |
| | | [1.0006] | [0.9971] | [1.0051] | [1.0011] | [1.0048] | |
| | 0.4 | 1.2982 | 1.2910 | 1.3265 | 1.3445 | 1.3000 | |
| | | (0.9036) | (0.8956) | (0.9083) | (0.9050) | (0.9056) | |
| | | [0.9932] | [0.9904] | [1.0018] | [1.0072] | [0.9950] | |
| | 0.6 | 1.3183 | 1.3198 | 1.3011 | 1.3181 | 1.3062 | |
| | | (0.8652) | (0.8617) | (0.8456) | (0.8640) | (0.8610) | |
| | | [1.0054] | [1.0029] | [0.9887] | [1.0008] | [0.9999] | |
| | 0.8 | 1.3468 | 1.3248 | 1.2973 | 1.3057 | | |
| | | (0.8226) | (0.8005) | (0.8023) | (0.8139) | | |
| | | [1.0224] | [0.9962] | [0.9802] | [0.9994] | | |
| | 1 | 1.3545 | | | | | |
| | | (0.7575) | | | | | |
| | | [1.0132] | | | | | |

(moving across columns), but performs worse the more variance is explained by time heterogeneity (moving between rows). Throughout this table we have kept the baseline assumption of a factor trend of $\nu = 1.5$ illustrating that the performance of the full sample estimator can either be degraded by the increasing factor trend (as in Table 2) or as in this table through changing the degree of time variation in the treatment effect.

Appendix B provides additional simulations. The first table in Appendix B decomposes $cov(F_t, \Delta_{it}|D_{it} = 1)$ into two components, first the correlation between the treatment effect and the factor realization ($\rho_{\Delta,F}$) and second the time-variation in treatment effect ($\phi$). The second Table in Appendix B illustrates that the performance of the full-sample estimator is independent of loading differentials and the correlation between the treatment effect and loadings verifying corollary 2.

## 5 Empirical Evidence

The goal of this section is to provide an empirical illustration of the importance of including factors in difference-in-difference models. We choose to focus on housing returns for several reasons. First, there is an extensive literature highlighting the importance of factors in real estate returns (see below). Second, the TWFE estimator is used frequently. Third, these studies often consider up to 10 years of data which means significant factor variation. Fourth, the data and code is on occasion available.

We identified two papers using data from the Federal Housing Finance Agency (FHFA) where all additional data needed is readily available. Table 4 of Favara and Imbs (2015) studies the impact of interstate bank branching deregulation on housing returns and Table B2 of Zevelev (2020) that studies the impact of allowing home equity loans on Texas house prices. We revisit these two results using the full-sample and pre-treatment estimators.

There are a significant amount of factors that have been shown to be relevant in housing returns. For example, models based on the arbitrage pricing theory (APT) with macroeconomic factors have been used in Chen et al. (1990), and Cotter et al. (2014), statistical factors (PCA) are employed by Titman and Warga (1986) while equity based factors such as the Fama-French factors, momentum and liquidity have studied in the real estate context by Peterson and Hsieh (1997), Hung and Glascock (2010) and Cannon and Cole (2010). Given the plethora of choice, we decided to pick the economic factors used by Cotter, Gabriel and Roll (2014). The factors are: the loan-to-value ratio (LTV), mortgage-backed securities issuance (PrivMBS), payroll employment (Payems), equity markets (S&P500), industrial production (Indpro), PPI materials prices (PPIitm), personal Income (Income), consumer sentiment (Umcsent), building permits (Permit1), and the Federal Funds rate (Fedfunds).

In including factors we need to address which factors should we include? We use two criteria. First, we select the factors that are able to significantly price the cross-section of county housing returns. Second, we use Bayesian Information Criterion (BIC) in our difference-in-difference regressions.

### 5.1 Factor Selection based on BIC

Bai and Ng (2002) note that when the factors are observable then the factor selection boils down to a model selection problem and the penalty term does not have to take into account the size of the cross-section. Additionally, the Bayesian Information Criterion (BIC) consistently estimates the number of factors while the Akaike Infomation Criterion (AIC) may overestimate the number of factors. Given this we use the BIC for model selection with penalty parameters based on the number of periods T. It is important to note that factor selection based on the BIC will not neccessarily select the factors that have the largest impact on the treatment effect.

**Table 4:** Factor selection based on BIC

The factor in this table are selected based on Bayesian information criterion. We consider all possible factor combinations and select the specification with the minimum BIC. In Panel A we consider the full-sample estimator and in Panel B we consider the pre-treatment estimator. *unit*-level (*range*) indicates the data contains observations in *range* and minimun data unit is *unit*. For example, County-level (US) sample indicates the BIC value based on county-level data among all the united states.

| Sample | Optimal factor combination |
|---|---|
| *Section A: Full sample estimator* | |
| County-level (US) | Fedfunds Indpro Payems Permit1 PPIitm S&P500 |
| ZIP5-level (US) | Fedfunds Indpro Payems Permit1 PPIitm Umcsent S&P500 Income OilPrice |
| ZIP5-level (BorderState) | Fedfunds Indpro Payems PPIitm S&P500 Income OilPrice |
| ZIP5-level (Border) | Fedfunds Indpro Permit1 PPIitm Income OilPrice |
| *Section B: Pre-treatment estimator* | |
| County-level (US) | Fedfunds Payems Income |
| ZIP5-level (US) | Income Umcsent S&P500 |
| ZIP5-level (BorderState) | Indpro Payems Permit1 PPIitm Income |
| ZIP5-level (Border) | Indpro PPIitm |

## 5.2 Cross-sectional Pricing of Real Estate Returns

We use Fama and MacBeth (1973) procedure to estimate risk premia. All factors are standardized. We divided the sample into three parts to avoid using overlapping data. We divide the time-series into three equally long time periods. The initial period is used to estimate loadings which are used for ranking and portfolio formation. In the second period we estimate factor loadings of the portfolio and in the final period cross-sectional pricing regressions are run.

Based on the results in Table 5, we find factors are priced differently in different settings. Generally speaking, Fedfunds, Indpro, PPIitm, Umcsent, SP500, and Income are priced factors.

**Table 5:** Esimated factor premium using Fama-MacBeth regression

This table presents factor premium estimated using the Fama-MacBeth two step procedure. Newey-west t-values based on two lags are presented in parentheses. ***, **, * represent significance at 10%, 5%, and 1% respectively.

| Factors | County 1987–2019, Yearly | 3-digit Zip code 1995–2020, Quarterly | MSA 1987–2020, Quarterly |
|---|---|---|---|
| Fedfunds | 0.091** | 0.586*** | 0.189 |
| | (2.21) | (2.79) | (0.55) |
| Indpro | 0.060 | 0.003 | -0.006*** |
| | (0.51) | (1.23) | (-4.25) |
| Payems | 0.213 | 0.001 | 0.0004 |
| | (1.57) | (1.14) | (0.82) |
| Permit1 | -0.006 | -0.038 | -0.011 |
| | (-0.10) | (-1.19) | (-0.33) |
| PPIitm | 0.232** | 0.013** | 0.006 |
| | (2.06) | (2.20) | (1.48) |
| Umcsent | 0.001 | -0.070*** | -0.042*** |
| | (0.01) | (-3.97) | (-3.25) |
| S&P500 | 0.074 | -0.132*** | -0.053 |
| | (0.65) | (-3.67) | (-1.32) |
| Income | −0.215*** | -0.002*** | -0.001 |
| | (−3.33) | (-2.61) | (-0.32) |

## 5.3 Placebo Interventions

An implication of using a misspecified benchmark model is some form of bias. In turn, the bias implies that even when the true treatment effect is zero the null hypothesis may be rejected. The TWFE estimator suffers from omitted factor bias and as result may imply significant coefficients and some degree of over-rejection. In contrast, the full sample estimator, if well specified, reduces the omitted factor bias, but introduces the bad time control problem - another form of bias and hence a source of over-rejection. That is, the relative over-rejection rates of the two estimators are likely to vary according to the dependent variable studied and the level at which the interventions are undertaken (e.g., state vs firm).

It is important to note that using two-way clustered standard errors does not deal with the over-rejection since the source of the bias that we are studying comes from the product of the unit and time dimension. Dealing with serial correlation in each of the dimensions separately does not deal with the product and therefore neither the omitted factor bias or the bad time control problem.

To evaluate the relative performance of these two classes of estimators, we study real estate returns while introducing state-level random interventions. We use county-level real estate price data from the Federal Housing Finance Agency (FHFA). The data spans the period from 1975 to 2020. Each state has a 20% chance of being treated. All states are treated simultaneously (the intervention is non-staggered) and the treatment year is random, but selected such that there are at least seven years prior and post treatment. So the earliest treatment year is 1982 and the last possible treatment year is 2013. For each intervention, we only keep keep 15 years of data (seven years pre and post-treatment).

We simulate 3,000 interventions and estimate four different benchmark models. First, we use the plain TWFE estimator:

$$lnP_{i,t} - lnP_{i,t-1} = \gamma_i + \eta_t + \alpha D_i P_t + \varepsilon_{it},$$

where $lnP_{i,t} - lnP_{i,t-1}$ is the real estate return from year $t-1$ to $t$ for county $i$, $\gamma_i$ and $\eta_t$ are county and year fixed effects, respectively. Second, we add a linear unit time-trend to the TWFE estimator:

$$lnP_{i,t} - lnP_{i,t-1} = \gamma_i + \eta_t + \alpha D_i P_t + \lambda_i \times t + \varepsilon_{it},$$

where $t$ is a linear time trend and $\lambda_i$ is a state-specific loading. Third, we use the full-sample estimator with three economic factors:

$$lnP_{i,t} - lnP_{i,t-1} = \gamma_i + \eta_t + \alpha D_i P_t + \sum_{k=1}^{3} \lambda_{ki} F_{kt} + \varepsilon_{it},$$

where $F_{kt}$ is the factor realization of factor $k$ at time $t$ and $\lambda_i$ is the factor loading for state $i$. We use the factors selected in 5.1. Lastly, we use the pre-treatment estimator (described in Eqns. 3 and 4) with factor selection for the pre-treatment period (also found in 5.1). In all specifications we cluster standard errors at the state and time levels.

The results are presented in Table 6. When using TWFE estimator we reject the null hypothesis 7.6% of the time while in the absence of true interventions we would expect a 5% rejection rate. The high rejection rate could be the cause of true interventions happen to be sampled or factor variation that is uncontrolled for. Strikingly, when we use the full-sample estimator the rejection rate increases. When we include a unit time trend the rejection rate is 10.3% while when we include economic factors the rejection rate is 8.8%. This suggests that bad time control bias outweighs the omitted factor bias in this setting.

**Table 6:** Rejection rates of placebo Interventions

This table presents rejection rates estimated using TWFE estimator without controlling factors, full-sample and pre-treatment estimator using state-specific time trends and economic factors Fedfunds, PPIitm and Income. The standard errors calculated are clustered by state and year level. The significance level is set at 5%.

| | No Factor | Time Trend | Economic Factors |
|---|---|---|---|
| Two-way fixed effect estimator | 7.6% | | |
| Full-sample estimator | | 10.3% | 8.8% |
| Pre-treatment estimator | | 10.2% | 5.5% |

This is plausible since we know that the TWFE is unbiased under random assignment while the bad time control bias exists under random assignment.

However, when we use the pre-treatment estimator with economic factors the rejection rate falls to 5.3%. Taken together our placebo interventions suggest: (1) the choice of method matters substantially, (2) the bad time control problem can be substantial - almost doubling the rejection rate and (3) the pre-treatment estimator with economic factors has the rejection rate closest to our expectation.

## 5.4 Favara and Imbs (2015)

For economists and policy makers it is important to understand the impact of local credit expansions on local asset prices. An increase in local house prices following a local credit expansion provides evidence that non-local assets are not perfect substitutes. Favara and Imbs (2015) use the state deregulation index introduced by Rice and Strahan (2010) to relate increases in local credit supply to local house prices.[12] Using a staggered difference-in-difference they find that an increase in the deregulation index results in an increase in local house prices by 1.2%. [13]

As with many quasi-natural experiments it is likely that deregulation is not randomly assigned. Indeed, Kroszner and Strahan (1999) study the causes of interstate banking deregulation and comment “We find that deregulation occurs earlier in states with fewer small banks, in states where small banks are financially weaker, and in states with more small,

[12] The effect of interstate banking deregulation has been extensively studied, among other things it has been documented to lead to less pronounced business cycles (Morgan, Rime and Strahan, 2004) per capital growth in Income and output (Jayaratne and Strahan, 1996), credit costs of borrowers (Rice and Strahan, 2010), lower Income inequality (Beck, Levine and Levkov, 2010) and reallocation across sectors (Acharya, Imbs and Sturgess, 2011)

[13] Other papers that analyze house prices in a difference-in-difference setting includes Blickli (2018) and Di Maggio and Kermani (2017).

presumably bank-dependent, firms. Also, a larger insurance industry delays deregulation when banks may compete in the sale of insurance products. Interest group factors related to the relative strength of potential winners (large banks and small firms) and losers (small banks and the rival insurance firms) thus can explain the timing of branching deregulation across states.” This suggests that treated and control units may have different loadings to factors.

We incorporate a factor structure into Eq. (2) of Favara and Imbs (2015). This implies we estimate the following,

$$lnP_{c,t} - lnP_{c,t-1} = \beta_1 D_{s,t-1} + \beta_2 D_{s,t-1} \times \eta_c^s + \beta_3 \mathbf{X}_{c,t} + \alpha_c + \gamma_t + \sum_{k=1}^{K} \lambda_{c,k} \times F_{t,k} + \varepsilon_{c,t}$$

where $P_{c,t}$ denotes house price index, $D_{s,t-1}$ denotes deregulation index, $\eta_c^s$ denotes housing supply (in)elasticity, $\mathbf{X}_{c,t}$ denotes county-level control variables, $\alpha_c$ and $\gamma_t$ are county and year fixed effects respectively. Indexes $c$ refers to counties, $s$ to states, and $t$ to years. We add factors based on the selection procedure described above where $\lambda_{c,k}$ refers to the loading to factor $k$ of county $c$ and $F_{t,k}$ is the factor realization of factor $k$ at date $t$.

**Table 7:** Incorporating factors into Favara and Imbs (2015)

This table presents the original results of Favara and Imbs (2015) and our full sample estimators as well as pre-treatment estimators. Column (1) replicates column (3) of table 4 in Favara and Imbs (2015). Columns (2) and (3) add the factors with significant premia. Columns (4) and (5) introduce the factors selected in Panel A of Table 3. Columns (6) and (7) add the factors selected in Panel B of Table 3. The standard errors reported are clustered by state. ***, **, * represent significance at 10%, 5%, and 1% respectively.

| Variable | Original (1) | FS Est. (2) | PT Est. (3) | FS Est. (4) | PT Est. (5) | FS Est. (6) | PT Est. (7) |
|---|---|---|---|---|---|---|---|
| Deregulation index | 0.0122*** | -0.0002 | 0.0009 | -0.0034 | -0.0029 | 0.0005 | 0.0017 |
| | (0.002) | (0.006) | (0.006) | (0.013) | (0.014) | (0.006) | (0.006) |
| Deregulation index | -0.005*** | -0.003 | -0.004 | -0.002 | -0.024 | -0.003 | -0.003 |
| × house supply elasticity | (0.000) | (0.002) | (0.0024) | (0.004) | (0.005) | (0.002) | (0.002) |
| County-level controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County & Year Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Fedfunds × State dummies | | ✔ | ✔ | ✔ | ✔ | ✔ | ✔ |
| Indpro × State dummies | | | | ✔ | ✔ | | |
| Payems × State dummies | | | | ✔ | ✔ | ✔ | ✔ |
| Permit1 × State dummies | | | | ✔ | ✔ | | |
| PPIitm × State dummies | | ✔ | ✔ | ✔ | ✔ | | |
| Umcsent × State dummies | | | | | | | |
| SP500 × State dummies | | | | ✔ | ✔ | | |
| Income × State dummies | | ✔ | ✔ | | | ✔ | ✔ |

The results are presented in Table 7. In all specifications, the introduction of the factors

**Table 8:** Combinatorial Factor Selection

This table uses all possible factor combinations and record the number of significant point estimates of the treatment effect as well as the number of factors. In the full sample case, we use all 9 factors while in the pre-treatment case we exclude PrivMBS due to data availability.

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: FS estimator* | | | | | | | | | |
| # of factors | 9 | 8 | 7 | 6 | 5 | 4 | 3 | 2 | 1 |
| # significant | 0/1 | 0/9 | 0/36 | 0/84 | 1/126 | 7/126 | 20/84 | 25/36 | 9/9 |
| | [0%] | [0%] | [0%] | [0%] | [0.8%] | [5.6%] | [23.8%] | [69.4%] | [100%] |
| *Panel B: PT estimator* | | | | | | | | | |
| # of factors | 3 | 2 | 1 | | | | | | |
| # significant | 29/56 | 14/28 | 6/8 | | | | | | |
| | [51.8%] | [50%] | [75%] | | | | | | |

renders the estimated treatment effect economically and statistically insignificant.

Since factor selection could be argued is somewhat arbitrary we performed the same analysis using all possible factor combinations while using from one to nine factors. In Table 8 we report the number of factors used and the number of significant treatment effects and the total of factor combinations. We present results separately for the full and pre treatment estimators. Given the shorter time horizon there is a lower maximum number of factors for the pre treatment estimators and PrivMBS is not available before 1994 implying that it cannot be used with the pre-treatment estimator. Using the full-sample estimator only 7 out of 126 combinations are significant when using four factors. For the pre-treatment estimator when we include 3 factors only 29 out of 56 combinations are statistically significant.

To examine whether there are systematic differences in loadings we display the deregulation index of Rice and Strahan (2010) taken from Favara and Imbs (2015) in Figure 1 and in Figure 2. we display our estimated state loadings. Examining the loadings it seems as if they cluster geographically.

### 5.5 Zevelev (2021)

Zevelev (2021) studies the effect of a constitutional amendment in Texas that legalized home equity loans. He finds that this increases Texas house prices by 4%. We introduce a factor structure into the Zevelev's equation (static DID) which implies we estimate,

$$y_{i,s,t} = \alpha_i + \theta_t + \beta_{DID} Texas_s \times Post_t + \Gamma X_{i,s,t} + \sum_{k=1}^{K} \lambda_{s,k} \times F_{t,k} + \varepsilon_{i,s,t}$$

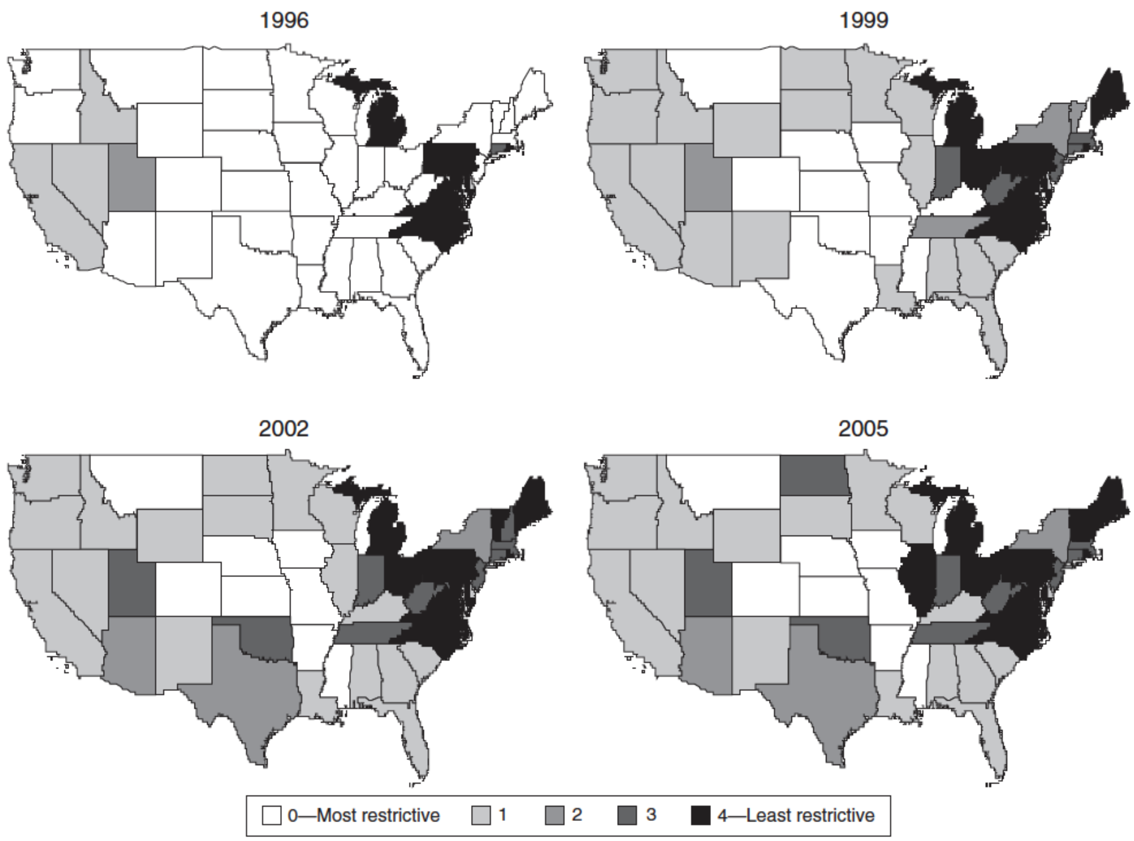


Figure B1. Rice-Strahan (2010) Deregulation Index by State and Year

*Source:* Rice and Strahan (2010).

**Figure 1:** Interstate Branching Deregulation Index

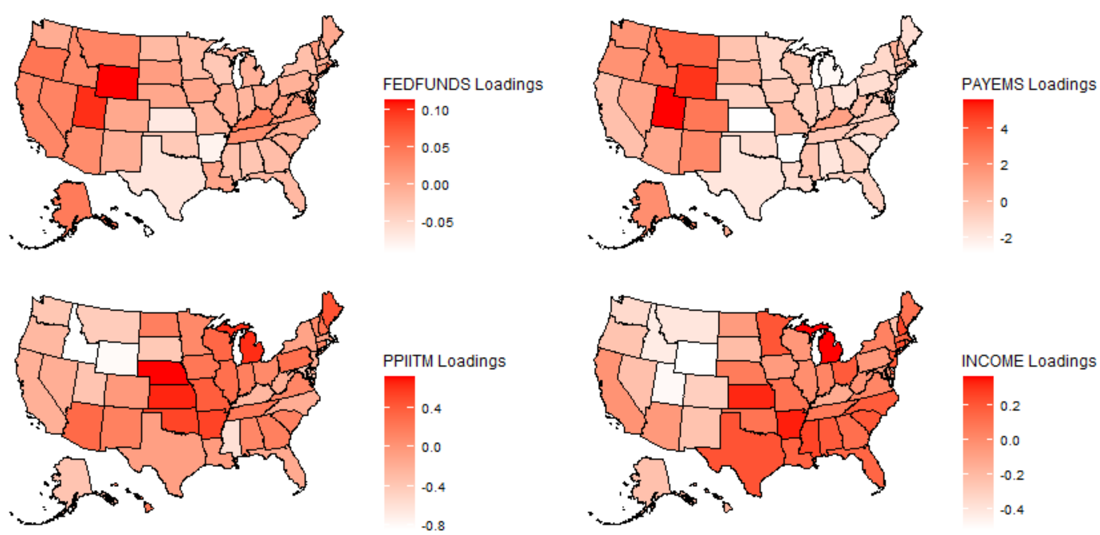


**Figure 2:** Estimated State Loadings

where $y_{i,s,t}$ is log real house price index. Index $i$ refers to five digit zip code, $s$ refers to state and year $t$. $\alpha_i$ is zip code fixed effect while $\theta_t$ are time fixed effects. $Texas_s$ is a dummy variable that takes the value 1 if it is the state Texas and $Post_t$ takes the value 1 if it is in post-treatment preiod ($t \geq 1998$).

Interestingly Zevelev (2021) introduces a factor structure in some of his specifications. He controls for the interaction between the oil price and MSA dummies as well as a time-trend that is interacted with state dummies.

In Table 9 we introduce economic factors into Table B2 of Zevelev (2021). Panel A provides results when we consider the entire United States. We show that without any factor controls Zevelev's result changes sign and is statistically significant. This highlights the importance of including factor controls. Additionally, the point estimates of both the full and pre-treatment estimators are negative and significant.

In Panel B, we present replication results for border states. Without factor controls the treatment effect is rendered insignificant. Introducing optimally selected factors reduces the treatment effects from 0.0616 to 0.039 (pre) and 0.0372 (full), but the point estimates remain statistically significant.

Finally, in Panel C we replicate the results for border counties. Again, without factor controls the treatment effect is rendered insignificant. This is also the case for the full sample estimator. Interestingly, the point estimate of the pre treatment estimator is very close to what is found in the original paper. Although the introduction of factors provides mixed results, it is clear that they are essential for the estimated treatment effects.

# 6 Conclusion

For almost 30 years factor models were the standard methodology used to analyze housing returns. The advent of quasi-experimental techniques that offer improved identification has resulted in a shift in research methodology from factor models to difference-in-differences estimators. We show that it is far from obvious how to incorporate the factor model into the difference-in-differences framework. The TWFE estimator is generally biased when factors are omitted, but so is the full-sample estimator. The TWFE estimator is preferred when assignment is close to random while the full sample estimator is unbiased when treatment is time-invariant.

**Table 9:** Incorporating factors into Zevelev (2021)

This table presents the original results of Zevelev (2021) and our full sample estimators as well as pre-treatment estimators. Column (1), (5), (9) replicates column (1) to (3) of table B.2 in Zevelev (2021). Columns (2), (6), (10) remove factor controls from Zevelev (2021). Columns (3), (7), (11) introduce the factors selected in Panel A of Table 4. Columns (4), (8), (12) add the factors selected in Panel B of Table 4. The standard errors of full sample esitmators and pre-treatment estimators are clustered by zip-code level. ***, **, * represent significance at 10%, 5%, and 1% respectively.

| Variable | Original Paper | Without Factor | FS Estimator | PT Estimator |
|---|---|---|---|---|
| *Panel A: United States* | (1) | (2) | (3) | (4) |
| TexasPost | 0.0350*** | -0.0387 | -0.0123** | -0.0147*** |
| | (0.0099) | (0.0040) | (0.0062) | (0.0040) |
| Zipcode & Year FE | ✔ | ✔ | ✔ | ✔ |
| State time trend | ✔ | | | |
| Oil× MSA dummies | ✔ | | ✔ | |
| Fedfunds × State dummies | | | ✔ | ✔ |
| Indpro × State dummies | | | ✔ | |
| Payems × State dummies | | | ✔ | |
| Permit1 × State dummies | | | ✔ | |
| PPIitm × State dummies | | | ✔ | |
| Umcsent × State dummies | | | ✔ | ✔ |
| SP500 × State dummies | | | ✔ | ✔ |
| Income × State dummies | | | ✔ | |
| *Panel B: Border States* | (5) | (6) | (7) | (8) |
| TexasPost | 0.0616*** | 0.0015 | 0.0372*** | 0.0390*** |
| | (0.0221) | (0.0051) | (0.0041) | (0.0048) |
| Zipcode & Year FE | ✔ | ✔ | ✔ | ✔ |
| State time trend | ✔ | | | |
| Oil× MSA dummies | ✔ | | ✔ | |
| Fedfunds × State dummies | | | ✔ | |
| Indpro × State dummies | | | ✔ | ✔ |
| Payems × State dummies | | | ✔ | ✔ |
| Permit1 × State dummies | | | | ✔ |
| PPIitm × State dummies | | | ✔ | ✔ |
| Umcsent × State dummies | | | | |
| SP500 × State dummies | | | ✔ | |
| Income × State dummies | | | ✔ | ✔ |
| *Panel C: Border Counties* | (9) | (10) | (11) | (12) |
| TexasPost | 0.0476** | 0.0024 | 0.0032 | 0.0450*** |
| | (0.0151) | (0.0113) | (0.0095) | (0.0097) |
| Zipcode & Year FE | ✔ | ✔ | ✔ | ✔ |
| State time trend | ✔ | | | |
| Oil× MSA dummies | ✔ | | ✔ | |
| Fedfunds × State dummies | | | ✔ | |
| Indpro × State dummies | | | ✔ | |
| Payems × State dummies | | | | |
| Permit1 × State dummies | | | ✔ | |
| PPIitm × State dummies | | | ✔ | ✔ |
| Umcsent × State dummies | | | | |
| SP500 × State dummies | | | | |
| Income × State dummies | | | ✔ | ✔ |

Applied researchers frequently augment the TWFE estimator to control for factor variation. We show that the resulting estimators often suffer from the bad time control problem. In our placebo analysis we find that the full sample estimator performs worse than the TWFE estimator suggesting that the bad time control problem is significant when studying housing returns. Further, we revisit the results of Favara and Imbs (2015) and Zevelev (2021) while incorporating relevant factors. In both cases we find that the factor model explains significant variation and should therefore be included. Additionally, depending on method and specification the estimated treatment effect may be significantly changed. Overall, this paper provides methods for incorporating factor models into difference-in-differences regressions while showing that it is also necessary when studying housing returns.

Future work should consider other dependent variables which have been shown to have a factor structure where difference-in-differences are often used. Given the importance of factors for interest rates (e.g., Litterman and Scheinkman, 1991) and yields (e.g., Duffie and Kan, 1996) we suspect that in these applications it is particularly beneficial to augment the difference-in-differences analysis to control for factor variation.

# Appendix A  Proof

## A.1  Useful lemmas

Denote the sample mean of treatment effects on treated $\overline{\Delta}^{\text{ATT}}$ as

$$\overline{\Delta}^{\text{ATT}} = \frac{\sum_{i,t} D_i P_t \Delta_{it}}{\sum_{i,t} D_i P_t}$$

**Lemma 1.** *The expectation of the sample mean of treatment effects on treated is equal to the average treatment effect on treated.*

$$\mathbb{E}[\overline{\Delta}^{ATT}] = \alpha^{ATT}$$

*Proof.*

$$\begin{aligned}
&\mathbb{E}\left[\overline{\Delta}^{\text{ATT}}\right] \\
=&\mathbb{E}\left[\frac{\sum_{i,t} D_i P_t \Delta_{it}}{\sum_{i,t} D_i P_t}\right] \\
=&\mathbb{E}\left[\mathbb{E}\left[\left.\frac{\sum_{i,t} D_i P_t \Delta_{it}}{\sum_{i,t} D_i P_t}\right| \boldsymbol{D}, \boldsymbol{P}\right]\right] \\
=&\mathbb{E}\left[\frac{1}{\sum_{i,t} D_i P_t}\sum_{i,t}\mathbb{E}\left[D_i P_t \Delta_{it} \middle| \boldsymbol{D}, \boldsymbol{P}\right]\right]
\end{aligned}$$

Since $Y_{it}(0)$ and $Y_{it}(1)$ are independent from $D_j$ and $P_s$ when $j \neq i$ and $s \neq t$,

$$\begin{aligned}
&\mathbb{E}\left[D_i P_t \Delta_{it} \middle| \boldsymbol{D}, \boldsymbol{P}\right] \\
=&\mathbb{E}\left[D_i P_t \Delta_{it} \middle| D_i, P_t\right] \\
=&D_i P_t \, \mathbb{E}\left[\Delta_{it} \middle| D_i, P_t\right] \\
=&\mathbb{1}_{\{D_i P_t = 1\}} \, \mathbb{E}\left[\Delta_{it} \middle| D_i P_t = 1\right]
\end{aligned}$$

In reason that $\mathbb{E}\left[\Delta_{it} \middle| D_i P_t = 1\right]$ is a constant,

$$\begin{aligned}
&\mathbb{E}\left[\overline{\Delta}^{\text{ATT}}\right] \\
=&\mathbb{E}\left[\frac{1}{\sum_{i,t} D_i P_t}\sum_{i,t}\mathbb{E}\left[D_i P_t \Delta_{it} \middle| \boldsymbol{D}, \boldsymbol{P}\right]\right] \\
=&\mathbb{E}\left[\Delta_{it} \middle| D_i P_t = 1\right]\mathbb{E}\left[\frac{\sum_{i,t} \mathbb{1}_{\{D_i P_t = 1\}}}{\sum_{i,t} D_i P_t}\right] \\
=&\alpha^{\text{ATT}}
\end{aligned}$$

□

Denote the sample mean of $\lambda$ for the treated (control) group $\overline{\lambda}_D$ ($\overline{\lambda}_C$) as

$$\overline{\lambda}_D = \frac{\sum_i D_i \lambda_i}{\sum_i D_i} \qquad \overline{\lambda}_C = \frac{\sum_i (1-D_i)\lambda_i}{\sum_i 1 - D_i}$$

Denote the sample mean of $F$ for the post-treatment (pre-treatment) period $\overline{F}_{\text{post}}$ ($\overline{F}_{\text{pre}}$) as

$$\overline{F}_{\text{post}} = \frac{\sum_t P_t F_t}{\sum_i P_t} \qquad \overline{F}_{\text{pre}} = \frac{\sum_i (1-P_t) F_t}{\sum_i 1 - P_t}$$

Using the method similar to lemma 1, it is not hard to verify that

$$\begin{aligned}
\mathbb{E}\left[\overline{\lambda}_D\right] &= \mathbb{E}\left[\lambda_i \mid D_i = 1\right] \\
\mathbb{E}\left[\overline{\lambda}_C\right] &= \mathbb{E}\left[\lambda_i \mid D_i = 0\right] \\
\mathbb{E}\left[\overline{F}_{\text{post}}\right] &= \mathbb{E}\left[F_t \mid P_t = 1\right] \\
\mathbb{E}\left[\overline{F}_{\text{pre}}\right] &= \mathbb{E}\left[F_t \mid P_t = 0\right]
\end{aligned}$$

Denote the sample covariance between factor realizations $F_t$ and individual treatment effect $\Delta_{it}$ as

$$Q^{\text{ATT}}_{F,\Delta} = \frac{\sum_{i,t} D_i P_t F_t (\Delta_{it} - \overline{\Delta}^{\text{ATT}})}{\sum_{i,t} D_i P_t}$$

**Lemma 2.** *The expectation of the sample covariance between factor realizations and individual treatment effect is equal to the overall covariance adjusted by degree of freedom.*

$$\mathbb{E}\left[Q^{ATT}_{F,\Delta}\right] = \left(1 - \frac{1}{E\left[\sum_{it} D_i P_t\right]}\right) cov(F_t, \Delta_{it} \mid D_i P_t = 1)$$

*Proof.*

$$\begin{aligned}
&\mathbb{E}\left[Q^{\text{ATT}}_{F,\Delta}\right] \\
=&\mathbb{E}\left[\frac{\sum_{i,t} D_i P_t F_t (\Delta_{it} - \overline{\Delta}^{\text{ATT}})}{\sum_{i,t} D_i P_t}\right] \\
=&\mathbb{E}\left[\mathbb{E}\left[\left.\frac{\sum_{i,t} D_i P_t F_t (\Delta_{it} - \overline{\Delta}^{\text{ATT}})}{\sum_{i,t} D_i P_t}\right| \boldsymbol{D}, \boldsymbol{P}\right]\right] \\
=&\mathbb{E}\left[\frac{\sum_{i,t} \mathbb{E}\left[D_i P_t F_t (\Delta_{it} - \overline{\Delta}^{\text{ATT}}) \middle| \boldsymbol{D}, \boldsymbol{P}\right]}{\sum_{i,t} D_i P_t}\right]
\end{aligned}$$

Since both $F_t$ and $\Delta_{it}$ are independent from $D_j$ and $P_s$ when $j \neq i$ and $s \neq t$,

$$
\begin{aligned}
&\mathbb{E}\left[D_i P_t F_t \Delta_{it} \middle| \boldsymbol{D}, \boldsymbol{P}\right] \\
=&\mathbb{1}_{\{D_i P_t = 1\}} \mathbb{E}\left[F_t \Delta_{it} \middle| D_i = 1, P_t = 1\right] \\
=&\mathbb{1}_{\{D_i P_t = 1\}} \left(\operatorname{cov}\left(F_t, \Delta_{it} \middle| D_i = 1, P_t = 1\right) + \mathbb{E}\left[F_t \middle| P_t = 1\right] \mathbb{E}\left[\Delta_{it} \middle| D_i = 1, P_t = 1\right]\right)
\end{aligned}
$$

On the other hand, $\Delta_{it}$ is independent from $F_s$ when $s \neq t$ and therefore $E[F_t \Delta_{js}] = E[F_t]E[\Delta_{js}] + \mathbb{1}_{\{t=s\}}\text{cov}(F_t, \Delta_{jt})$. Thus, we obtain

$$
\begin{aligned}
&\sum_{i,t} \mathbb{E}\left[D_i P_t F_t \overline{\Delta}^{\text{ATT}} \middle| \boldsymbol{D}, \boldsymbol{P}\right] \\
=&\sum_{i,t} \mathbb{E}\left[D_i P_t F_t \frac{\sum_{j,s} D_j P_s \Delta_{js}}{\sum_{j,s} D_j P_s} \middle| \boldsymbol{D}, \boldsymbol{P}\right] \\
=&\frac{1}{N_D T_P} \mathbb{E}\left[\left(\sum_{(i,t):D_i P_t = 1} F_t\right)\left(\sum_{(j,s):D_j P_s = 1} \Delta_{js}\right)\right] \\
=&\frac{1}{N_D T_P}\left(\mathbb{E}\left[\sum_{(i,t):D_i P_t = 1} F_t\right]\right)\left(\mathbb{E}\left[\sum_{(j,s):D_j P_s = 1} \Delta_{js}\right]\right) \\
&+ \frac{1}{N_D T_P}\left(\mathbb{E}\left[\sum_{(i,t):D_i P_t = 1} \text{cov}(F_t, \Delta_{js})\right]\right) \\
=&N_D T_P\, \mathbb{E}\left[F_t \middle| P_t = 1\right] \mathbb{E}\left[\Delta_{it} \middle| D_i = 1, P_t = 1\right] + \operatorname{cov}\left(F_t, \Delta_{it} \middle| D_i = 1, P_t = 1\right)
\end{aligned}
$$

Therefore, the expectation of sample covariance is

$$
\begin{aligned}
&\mathbb{E}\left[Q_{F,\Delta}^{\text{ATT}}\right] \\
=&\mathbb{E}\left[\frac{N_D T_P\, \mathbb{E}\left[F_t \middle| P_t = 1\right] \mathbb{E}\left[\Delta_{it} \middle| D_i = 1, P_t = 1\right] + N_D T_P \operatorname{cov}\left(F_t, \Delta_{it} \middle| D_i = 1, P_t = 1\right)}{N_D T_P}\right] \\
&- \mathbb{E}\left[\frac{N_D T_P\, \mathbb{E}\left[F_t \middle| P_t = 1\right] \mathbb{E}\left[\Delta_{it} \middle| D_i = 1, P_t = 1\right] + \operatorname{cov}\left(F_t, \Delta_{it} \middle| D_i = 1, P_t = 1\right)}{N_D T_P}\right] \\
=&\left(1 - \frac{1}{\mathbb{E}\left[\sum_{i,t} D_i P_t\right]}\right) \operatorname{cov}\left(F_t, \Delta_{it} \middle| D_i = 1, P_t = 1\right)
\end{aligned}
$$

□

## A.2 Proof of Proposition 1

The traditional way to estimate treatment effect is the two-way fixed effect difference-in-difference estimator. The definition of two-way fixed effect is the following.

$$(\hat{\alpha}^{\text{TWFE}}, \hat{\gamma}_i^{\text{TWFE}}, \hat{\eta}_t^{\text{TWFE}}) = \underset{\alpha,\gamma,\eta}{argmin}\Big\{\sum_{t=1}^{T}\sum_{i=1}^{N}(Y_{it} - \gamma_i - \eta_t - \alpha D_i P_t)^2\Big\} \tag{6}$$

Given observed $\{D_i\}$ and $\{P_t\}$, we can regress $D_i P_t$ on unit dummies, and time dummies. The residuals are defined as $u_{it}^{\text{TWFE}}$.

$$D_i P_t = \kappa_i^{\text{TWFE}} + \zeta_t^{\text{TWFE}} + u_{it}^{\text{TWFE}} \tag{7}$$

It can be verified that the residual $u_{it}^{\text{TWFE}}$ is the two-way demeaned $D_i P_t$,

$$\begin{gathered} u_{it}^{\text{TWFE}} = (D_i - \overline{D})(P_t - \overline{P}) \\ \text{where} \quad \overline{D} = \frac{1}{N}\sum_{i=1}^{N} D_i \quad \overline{P} = \frac{1}{T}\sum_{t=1}^{N} P_t \end{gathered}$$

In reason that $u_{it}^{\text{TWFE}}$ is the residual in regression (7), we can obtain

$$\begin{aligned} &\forall t, \quad \sum_i u_{it}^{\text{TWFE}} = 0 \quad \Rightarrow \quad \sum i, t u_{it}^{\text{TWFE}} Y_{1t} = 0 \\ &\forall i, \quad \sum_t u_{it}^{\text{TWFE}} = 0 \quad \Rightarrow \quad \sum i, t u_{it}^{\text{TWFE}} Y_{i1} = 0 \\ &\forall (i,t) : D_i P_t = 1 \quad u_{it}^{\text{TWFE}} = (1 - \overline{D})(1 - \overline{P}) \end{aligned}$$

Then we can derive that

$$\begin{aligned} &\mathbb{E}\left[\sum_{i,t} u_{it}^{\text{TWFE}} Y_{it} \middle| \boldsymbol{D}, \boldsymbol{P}\right] \\ =&\mathbb{E}\left[\sum_{i,t} u_{it}^{\text{TWFE}} Y_{it}(0) \middle| \boldsymbol{D}, \boldsymbol{P}\right] + \mathbb{E}\left[\sum_{i,t} u_{it}^{\text{TWFE}} D_i P_t \Delta_{it} \middle| \boldsymbol{D}, \boldsymbol{P}\right] \\ =&\mathbb{E}\left[\sum_{i,t} u_{it}^{\text{TWFE}} (Y_{it}(0) - Y_{i1}(0) - Y_{1t}(0) + Y_{11}(0)) \middle| \boldsymbol{D}, \boldsymbol{P}\right] + \mathbb{E}\left[\sum_{i,t} u_{it}^{\text{TWFE}} D_i P_t \Delta_{it} \middle| \boldsymbol{D}, \boldsymbol{P}\right] \\ =&\mathbb{E}\left[\sum_{i,t} u_{it}^{\text{TWFE}} (\lambda_i - \lambda_1)(F_t - F_1) \middle| \boldsymbol{D}, \boldsymbol{P}\right] + \mathbb{E}\left[\sum_{i,t} u_{it}^{\text{TWFE}} D_i P_t \Delta_{it} \middle| \boldsymbol{D}, \boldsymbol{P}\right] \\ =&\mathbb{E}\left[\sum_{i,t} u_{it}^{\text{TWFE}} \lambda_i F_t \middle| \boldsymbol{D}, \boldsymbol{P}\right] + \mathbb{E}\left[\sum_{i,t} u_{it}^{\text{TWFE}} D_i P_t \Delta_{it} \middle| \boldsymbol{D}, \boldsymbol{P}\right] \end{aligned}$$

Following the Frisch-Waugh-Lovell theorem, the two-way fixed effect estimator can be written as

$$
\begin{aligned}
&\mathbb{E}\left[\hat{\alpha}^{\text{TWFE}}\middle|\boldsymbol{D},\boldsymbol{P}\right]\\
=&\mathbb{E}\left[\frac{\sum_{i,t}u_{it}^{\text{TWFE}}Y_{it}}{\sum_{i,t}u_{it}^{\text{TWFE}}D_iP_t}\middle|\boldsymbol{D},\boldsymbol{P}\right]\\
=&\mathbb{E}\left[\frac{\sum_{i,t}u_{it}^{\text{TWFE}}D_iP_t\Delta_{it}}{\sum_{i,t}u_{it}^{\text{TWFE}}D_iP_t}\middle|\boldsymbol{D},\boldsymbol{P}\right]+\mathbb{E}\left[\frac{\sum_{i,t}u_{it}^{\text{TWFE}}\lambda_iF_t}{\sum_{i,t}u_{it}^{\text{TWFE}}D_iP_t}\middle|\boldsymbol{D},\boldsymbol{P}\right]\\
=&\mathbb{E}\left[\frac{\sum_{(i,t):D_iP_t=1}u_{it}^{\text{TWFE}}\Delta_{it}}{\sum_{(i,t):D_iP_t=1}u_{it}^{\text{TWFE}}}\middle|\boldsymbol{D},\boldsymbol{P}\right]+\mathbb{E}\left[\frac{\sum_i\left((D_i-\overline{D})\lambda_i\right)\sum_t\left((P_t-\overline{P})F_t\right)}{\sum_{(i,t):D_iP_t=1}(1-\overline{D})(1-\overline{P})}\middle|\boldsymbol{D},\boldsymbol{P}\right]\\
=&\mathbb{E}\left[\frac{\sum_{(i,t):D_iP_t=1}\Delta_{it}}{\sum_{(i,t):D_iP_t=1}1}\middle|\boldsymbol{D},\boldsymbol{P}\right]+\mathbb{E}\left[\frac{\sum_i\left((D_i-\overline{D})\lambda_i\right)\sum_t\left((P_t-\overline{P})F_t\right)}{NT\overline{D}(1-\overline{D})\overline{P}(1-\overline{P})}\middle|\boldsymbol{D},\boldsymbol{P}\right]\\
=&\mathbb{E}\left[\overline{\Delta}^{\text{ATT}}\middle|\boldsymbol{D},\boldsymbol{P}\right]+\mathbb{E}\left[(\overline{\lambda}_{\text{D}}-\overline{\lambda}_{\text{C}})(\overline{F}_{\text{Post}}-\overline{F}_{\text{Pre}})\middle|\boldsymbol{D},\boldsymbol{P}\right]
\end{aligned}
$$

According to lemma 1, we can get

$$
\begin{aligned}
&\mathbb{E}\left[\hat{\alpha}^{\text{TWFE}}\right]\\
=&\mathbb{E}\left[\mathbb{E}\left[\hat{\alpha}^{\text{TWFE}}\middle|\boldsymbol{D},\boldsymbol{P}\right]\right]\\
=&\alpha^{\text{ATT}}+(E[\lambda_i|D_i=1]-E[\lambda_i|D_i=0])(E[F_t|P_t=1]-E[F_t|P_t=0])
\end{aligned}
$$

## A.3 Proof of Proposition 2

Two-way fixed effect estimator totally ignores the presence of the factor structure. A straight-forward idea is to add all the factors as control variables. To be explicit, full sample estimator is defined as

$$
(\hat{\alpha}^{\text{FS}},\hat{\gamma}_i^{\text{FS}},\hat{\eta}_t^{\text{FS}},\hat{\lambda}_i^{\text{FS}})=\underset{\alpha,\gamma,\eta,\lambda}{argmin}\Big\{\sum_{t=1}^{T}\sum_{i=1}^{N}(Y_{it}-\gamma_i-\eta_t-\lambda_iF_t-\alpha D_iP_t)^2\Big\} \tag{8}
$$

Compared to TWFE estimator, a full sample estimator adds observed factor realizations as covariates. Suppose treatment group $D_i$, treatment time $P_t$ and factor realizations $F_t$ is given, we regress $D_iP_t$ on unit dummies, time dummies and factor realizations, and define the residual as $u_{it}^{\text{FS}}$.

$$
D_iP_t=\kappa_i^{\text{FS}}+\zeta_t^{\text{FS}}+\xi_i^{\text{FS}}F_t+u_{it}^{\text{FS}} \tag{9}
$$

In reason that $u^{\mathrm{FS}}$ is the residual of regression (9), it satisfies the following equations.

$$
\begin{aligned}
\forall t, \quad & \sum_i u_{it}^{\mathrm{FS}} = 0 \\
\forall i, \quad & \sum_t u_{it}^{\mathrm{FS}} = 0 \\
\forall i, \quad & \sum_t u_{it}^{\mathrm{FS}} F_t = 0
\end{aligned}
$$

Then it implies that

$$
\begin{aligned}
& \mathbb{E}\left[\sum_{i,t} u_{it}^{\mathrm{FS}} Y_{it} \,\middle|\, \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] \\
= & \mathbb{E}\left[\sum_{i,t} u_{it}^{\mathrm{FS}} Y_{it}(0) \,\middle|\, \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] + \mathbb{E}\left[\sum_{i,t} u_{it}^{\mathrm{FS}} D_i P_t \Delta_{it} \,\middle|\, \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] \\
= & \mathbb{E}\left[\sum_{i,t} u_{it}^{\mathrm{FS}} (Y_{it}(0) - Y_{i1}(0) - Y_{1t}(0) + Y_{11}(0)) \,\middle|\, \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] + \mathbb{E}\left[\sum_{i,t} u_{it}^{\mathrm{FS}} D_i P_t \Delta_{it} \,\middle|\, \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] \\
= & \mathbb{E}\left[\sum_{i,t} u_{it}^{\mathrm{FS}} (\lambda_i - \lambda_1)(F_t - F_1) \,\middle|\, \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] + \mathbb{E}\left[\sum_{i,t} u_{it}^{\mathrm{FS}} D_i P_t \Delta_{it} \,\middle|\, \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] \\
= & \mathbb{E}\left[\sum_{i,t} u_{it}^{\mathrm{FS}} D_i P_t \Delta_{it} \,\middle|\, \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right]
\end{aligned}
$$

Due to the degree of freedom, there exists multiple solutions of regression (9). However, it does not change the values of residuals, which is what we are interested in. One of the possible solutions is

$$
\begin{aligned}
\xi_i^{\mathrm{FS}} &= 0 && \text{if } D_i = 0 \\
\xi_i^{\mathrm{FS}} &= \overline{P}(1 - \overline{P}) \frac{\overline{F}_{\text{post}} - \overline{F}_{\text{pre}}}{\sigma_F^2} && \text{if } D_i = 1 \\
\kappa_i^{\mathrm{FS}} &= 0 && \text{if } D_i = 0 \\
\kappa_i^{\mathrm{FS}} &= \overline{P} - \xi_i^{\mathrm{FS}} \overline{F} && \text{if } D_i = 1 \\
\zeta_t^{\mathrm{FS}} &= -\overline{\kappa}^{\mathrm{FS}} - \overline{\xi}^{\mathrm{FS}} F_t && \text{if } P_t = 0 \\
\zeta_t^{\mathrm{FS}} &= -\overline{\kappa}^{\mathrm{FS}} - \overline{\xi}^{\mathrm{FS}} F_t + \overline{D} && \text{if } P_t = 1
\end{aligned}
$$

where

$$
\sigma_F^2 = \frac{1}{T} \sum_{t=1}^{T} (F_t - \overline{F})^2 \qquad \overline{\kappa}^{\mathrm{FS}} = \frac{1}{N} \sum_{i=1}^{N} \kappa_i^{\mathrm{FS}} \qquad \overline{\xi}^{\mathrm{FS}} = \frac{1}{N} \sum_{i=1}^{N} \xi_i^{\mathrm{FS}}
$$

In this case, we can estimate the value of a full sample estimator. Unfortunately, the full sample estimator is biased

$$
\begin{aligned}
&\mathbb{E}\left[\hat{\alpha}^{\mathrm{FS}}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{F}\right]\\
=&\mathbb{E}\left[\frac{\sum_{i,t}u_{it}^{\mathrm{FS}}Y_{it}}{\sum_{i,t}u_{it}^{\mathrm{FS}}D_iP_t}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{F}\right]\\
=&\mathbb{E}\left[\frac{\sum_{i,t}u_{it}^{\mathrm{FS}}D_iP_t\Delta_{it}}{\sum_{i,t}u_{it}^{\mathrm{FS}}D_iP_t}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{F}\right]\\
=&\mathbb{E}\left[\frac{\sum_{(i,t):D_iP_t=1}u_{it}^{\mathrm{FS}}\overline{\Delta}^{\mathrm{ATT}}}{\sum_{(i,t):D_iP_t=1}u_{it}^{\mathrm{FS}}}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{F}\right]+\mathbb{E}\left[\frac{\sum_{(i,t):D_iP_t=1}u_{it}^{\mathrm{FS}}(\Delta_{it}-\overline{\Delta}^{\mathrm{ATT}})}{\sum_{(i,t):D_iP_t=1}u_{it}^{\mathrm{FS}}}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{F}\right]\\
=&\mathbb{E}\left[\overline{\Delta}^{\mathrm{ATT}}\frac{\sum_{(i,t):D_iP_t=1}u_{it}^{\mathrm{FS}}}{\sum_{(i,t):D_iP_t=1}u_{it}^{\mathrm{FS}}}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{F}\right]+\mathbb{E}\left[\frac{\sum_{(i,t):D_iP_t=1}(1-\kappa_i^{\mathrm{FS}}-\zeta_t^{\mathrm{FS}}-\xi_i^{\mathrm{FS}}F_t)(\Delta_{it}-\overline{\Delta}^{\mathrm{ATT}})}{\sum_{(i,t):D_iP_t=1}u_{it}^{\mathrm{FS}}}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{F}\right]\\
=&\mathbb{E}\left[\overline{\Delta}^{\mathrm{ATT}}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{F}\right]+\frac{(\overline{F}_{\mathrm{pre}}-\overline{F})}{\sigma_F^2+(\overline{F}_{\mathrm{post}}-\overline{F})(\overline{F}_{\mathrm{pre}}-\overline{F})}\mathbb{E}\left[Q_{F,\Delta}^{\mathrm{ATT}}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{F}\right]
\end{aligned}
$$

We can not get the analytical solution of $\mathbb{E}\left[\hat{\alpha}^{\mathrm{FS}}\right]$ without adding assumption, because the variation of the factor $\sigma_F^2$ and the sample covariance $Q_{F,\Delta}^{\mathrm{ATT}}$ both depend on the factor realizations in a specific sample. If we assume the factor realizations $F_t$ are exogenously determined (i.e. they are not random across samples), we can get a simplified expression.

$$
\begin{aligned}
&\mathbb{E}\left[\hat{\alpha}^{\mathrm{FS}}\right]\\
=&\mathbb{E}\left[\mathbb{E}\left[\hat{\alpha}^{\mathrm{FS}}\middle|\boldsymbol{D},\boldsymbol{P}\right]\right]\\
=&\mathbb{E}\left[\mathbb{E}\left[\overline{\Delta}^{\mathrm{ATT}}\middle|\boldsymbol{D},\boldsymbol{P}\right]\right]\\
&+\mathbb{E}\left[\frac{\overline{F}_{\mathrm{pre}}-\overline{F}}{\sigma_F^2+(\overline{F}_{\mathrm{post}}-\overline{F})(\overline{F}_{\mathrm{pre}}-\overline{F})}\mathbb{E}\left[Q_{F,\Delta}^{\mathrm{ATT}}\middle|\boldsymbol{D},\boldsymbol{P}\right]\right]\\
=&\alpha^{\mathrm{ATT}}+w^{FS}\operatorname{cov}\left(F_t,\Delta_{it}\middle|D_i=1,P_t=1\right)
\end{aligned}
$$

where

$$
w^{FS}=\mathbb{E}\left[\frac{N_TT_P-1}{N_TT_P}\cdot\frac{\overline{F}_{\mathrm{pre}}-\overline{F}}{\sigma_F^2+(\overline{F}_{\mathrm{post}}-\overline{F})(\overline{F}_{\mathrm{pre}}-\overline{F})}\right]
$$

## A.4 Proof of Corollary 3

In practice, researchers add unit-specific time trend, termed unit time trend (UTT) estimator, to control for time-varying heterogeneity. However, it is a special case of the full sample estimator and have the same issue of "bad time control problem".

We take unit-specific linear time trend $F_t = t$ as an example. If the time trend is polynomial, it does not make an essential difference. Since time trend is exogenously determined and non-random across different samples, we can plug $F_t = t$ into the estimation result of the full sample estimator and get the bias of unit time trend estimator. Suppose the treatment happens at time $T - T_P + 1$.

$$\begin{aligned}
&\mathbb{E}\left[\hat{\alpha}^{\text{UTT}}\right] \\
=&\alpha^{\text{ATT}} + \mathbb{E}\left[\frac{N_T T_P - 1}{N_T T_P} \cdot \frac{\frac{-T_P}{2}}{\frac{T^2-1}{12} + \frac{T-T_P}{2}\frac{-T_P}{2}}\right] \text{cov}(\Delta_{it}, t | D_i P_t = 1) \\
=&\alpha^{\text{ATT}} + w^{\text{UTT}} \text{cov}(\Delta_{it}, t | D_i P_t = 1)
\end{aligned}$$

where

$$w^{\text{UTT}} = \mathbb{E}\left[\frac{N_T T_P - 1}{N_T T_P} \cdot \frac{-6T_P}{T^2 - 1 - 3T_P(T - T_P)}\right] < 0$$

## A.5 Proof of Proposition 3

In order to avoid bad control problem, researchers add pre-treatment covariate interacted with time trend termed covariate time trend (CTT) estimator. However, CTT has a similar bias as the full sample estimator, because of "bad time control problem". For simplicity, we assume the time trend is a linear time trend, but the argument is also valid for polynomial time trend. Let $Z_{it} = X_{i0} \cdot t$. The definition of covariate time trend estimator is:

$$(\hat{\alpha}^{\text{CTT}}, \hat{\gamma}_i^{\text{CTT}}, \hat{\eta}_t^{\text{CTT}}, \hat{\beta}^{\text{CTT}}) = \underset{\alpha,\gamma,\eta,\beta}{argmin} \Big\{ \sum_{t=1}^{T} \sum_{i=1}^{N} (Y_{it} - \gamma_i - \eta_t - \beta Z_{it} - \alpha D_i P_t)^2 \Big\} \tag{10}$$

We regress $D_i P_t$ on unit dummies, time dummies, and $Z_{it}$:

$$D_i P_t = \kappa_i^{\text{CTT}} + \zeta_t^{\text{CTT}} + \xi^{\text{CTT}} Z_{it} + u_{it}^{\text{CTT}} \tag{11}$$

The estimate of $\xi$ is the coefficient of a covariant in the two-way fixed effect estimator.

$$\hat{\xi}^{\text{CTT}} = \frac{\sum_{it}(D_i - \overline{D})(P_t - \overline{P})(Z_{it} - \overline{Z}_{i.} - \overline{Z}_{.t} + \overline{Z})}{\sum_{it}(Z_{it} - \overline{Z}_{i.} - \overline{Z}_{.t} + \overline{Z})^2}$$

Thus, the residual of regression 11 can be written as:

$$u_{it}^{\text{CTT}} = \left(D_i - \overline{D}\right)\left(P_t - \overline{P}\right) - \hat{\xi}^{\text{CTT}} Z_{it}$$

where

$$\overline{Z}_{i.} = \frac{\sum_{t=1}^{T} Z_{it}}{T} \qquad \overline{Z}_{.t} = \frac{\sum_{i=1}^{N} Z_{it}}{N} \qquad \overline{Z} = \frac{\sum_{i=1}^{N}\sum_{t=1}^{T} Z_{it}}{NT} \qquad \overline{Z}^{\text{ATT}} = \frac{\sum_{i,t} D_i P_t Z_{it}}{\sum_{i,t} D_i P_t}$$

Denote the sample covariance between covariate time trend $Z_{it}$ and individual treatment effect $\Delta_{it}$ as

$$Q^{\text{ATT}}_{Z,\Delta} = \frac{\sum_{i,t} D_i P_t Z_{it}(\Delta_{it} - \overline{\Delta}^{\text{ATT}})}{\sum_{i,t} D_i P_t}$$

In the best possible setting, researchers correctly identify the factor structure (i.e. $\lambda_i = X_{i0}$ and $F_t = t$). However, the covariate time trend estimator is still subject to the bad time control problem:

$$\begin{aligned}
&\mathbb{E}\left[\hat{\alpha}^{\text{CTT}}\middle| \boldsymbol{D},\boldsymbol{P},\boldsymbol{Z}\right]\\
=&\mathbb{E}\left[\frac{\sum_{i,t} u^{\text{CTT}}_{it} Y_{it}}{\sum_{i,t} u^{\text{CTT}}_{it} D_i P_t}\middle| \boldsymbol{D},\boldsymbol{P},\boldsymbol{Z}\right]\\
=&\mathbb{E}\left[\frac{\sum_{i,t} u^{\text{CTT}}_{it} D_i P_t \Delta_{it}}{\sum_{i,t} u^{\text{CTT}}_{it} D_i P_t}\middle| \boldsymbol{D},\boldsymbol{P},\boldsymbol{Z}\right] + \mathbb{E}\left[\frac{\sum_{i,t} u^{\text{CTT}}_{it} \lambda_i F_t}{\sum_{i,t} u^{\text{CTT}}_{it} D_i P_t}\middle| \boldsymbol{D},\boldsymbol{P},\boldsymbol{Z}\right]\\
=&\mathbb{E}\left[\frac{\sum_{i,t} u^{\text{CTT}}_{it} D_i P_t \overline{\Delta}^{\text{ATT}}}{\sum_{i,t} u^{\text{CTT}}_{it} D_i P_t}\middle| \boldsymbol{D},\boldsymbol{P},\boldsymbol{Z}\right] + \mathbb{E}\left[\frac{\sum_{i,t} u^{\text{CTT}}_{it} D_i P_t \left(\Delta_{it} - \overline{\Delta}^{\text{ATT}}\right)}{\sum_{i,t} u^{\text{CTT}}_{it} D_i P_t}\middle| \boldsymbol{D},\boldsymbol{P},\boldsymbol{Z}\right]\\
=&\mathbb{E}\left[\overline{\Delta}^{\text{ATT}}\middle| \boldsymbol{D},\boldsymbol{P},\boldsymbol{Z}\right] + \frac{-\hat{\xi}^{\text{CTT}}}{(1-\overline{D})(1-\overline{P}) - \hat{\xi}^{\text{CTT}}\overline{Z}^{\text{ATT}}}\,\mathbb{E}\left[Q^{\text{CTT}}_{Z,\Delta}\middle| \boldsymbol{D},\boldsymbol{P},\boldsymbol{Z}\right]
\end{aligned}$$

It shows that the covariate time trend estimator exists the bad time control problem in general. Besides, the covariate time trend estimator may suffer from the misspecification problem and generate additional bias terms if the pre-treatment covariate does not fully correlated with the factor loading or the factor realization is not a linear time trend.

## A.6 Proof of Proposition 4

### A.6.1 Unit group interacted with time

Suppose $g_i$ indicates the group unit $i$ belongs to and we define dummy factor estimator with unit group interacted with time as

$$(\hat{\alpha}^{\text{DF1}}, \hat{\gamma}^{\text{DF1}}_i, \hat{\omega}^{\text{DF1}}_{rt}) = \underset{\alpha,\gamma,\omega}{argmin}\Big\{\sum_{t=1}^{T}\sum_{i=1}^{N}(Y_{it} - \gamma_i - \omega_{g_i,t} - \alpha D_i P_t)^2\Big\} \tag{12}$$

we regress $D_i P_t$ on unit dummies, and group dummies interacted with time dummies.

$$D_i P_t = \kappa^{\text{DF1}}_i + \theta^{\text{DF1}}_{rt} + u^{\text{DF1}}_{it} \tag{13}$$

Let $R_g$ be the ratio of treated units overall all units in group $g$,

$$R_g = \frac{\sum_i D_i \mathbb{1}_{\{i\in g\}}}{\sum_i \mathbb{1}_{\{i\in g\}}}$$

Then, the residual of regression 13 is:

$$u_{it}^{\text{DF1}} = (D_i - R_{g_i})(P_t - \overline{P})$$

Denote $N_{D,g}$ is the number of treated observations in group $g$, $\overline{\lambda}_{D,g}$ is the mean of factor loadings $\lambda$ of the treated observations in group $g$, $\overline{\lambda}_{C,g}$ is the mean of factor loadings $\lambda$ of the non-treated observations in group $r$, and $\overline{\Delta}_g^{\text{ATT}}$ is the mean of the treatment effects $\Delta_{it}$ for the treated observations in group $g$.

Following the Frisch-Waugh-Lovell theorem, we can show that the dummy factor estimator is biased in two ways.

$$\begin{aligned}
&\mathbb{E}\left[\hat{\alpha}^{\text{DF1}}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{G}\right] \\
=&\mathbb{E}\left[\frac{\sum_{i,t} u_{it}^{\text{DF1}} Y_{it}}{\sum_{i,t} u_{it}^{\text{DF1}} D_i P_t}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{G}\right] \\
=&\mathbb{E}\left[\frac{\sum_{i,t} u_{it}^{\text{DF1}} \Delta_{it} D_i P_t}{\sum_{i,t} u_{it}^{\text{DF1}} D_i P_t}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{G}\right] + \mathbb{E}\left[\frac{\sum_{i,t} u_{it}^{\text{DF1}} \lambda_i F_t}{\sum_{i,t} u_{it}^{\text{DF1}} D_i P_t}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{G}\right] \\
=&\mathbb{E}\left[\frac{\sum_{i,t}(1-R_{g_i})(1-\overline{P})D_i P_t \Delta_{it}}{\sum_{i,t}(1-R_{g_i})(1-\overline{P}))D_i P_t}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{G}\right] \\
&+\mathbb{E}\left[\frac{\left(\sum_i (D_i - R_{g_i})\lambda_i\right)\left(\sum_t (P_t-\overline{P})F_t\right)}{\sum_{i,t} u_{it}^{\text{DF1}} D_i P_t}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{G}\right] \\
=&\mathbb{E}\left[\frac{\sum_{i,t}(1-R_{g_i})D_i P_t \Delta_{it}}{\sum_{i,t}(1-R_{g_i})D_i P_t}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{G}\right] \\
&+\mathbb{E}\left[\frac{\left(\sum_i (D_i - R_{g_i})\lambda_i\right) T\overline{P}(\overline{F}_{\text{post}} - \overline{F})}{\left(\sum_i (1-R_{g_i})D_i\right)(1-\overline{P})T\overline{P}}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{G}\right] \\
=&\mathbb{E}\left[\frac{\sum_g N_g R_g (1-R_g)\overline{\Delta}_g^{\text{ATT}}}{\sum_g N_g R_g(1-R_g)}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{G}\right] \\
&+\mathbb{E}\left[\frac{\sum_g N_g R_g (1-R_g)\left(\overline{\lambda}_{D,r} - \overline{\lambda}_{C,r}\right)\left(\overline{F}_{\text{post}} - \overline{F}_{\text{pre}}\right)}{\sum_g N_g R_g(1-R_g)}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{G}\right]
\end{aligned}$$

Given the assignment of groups, the expectation of a dummy factor estimator can be expressed as a convex combination of TWFE estimates for each group.

$$\begin{aligned}
&\mathbb{E}\left[\hat{\alpha}^{\text{DF1}}\middle|\boldsymbol{G}\right] \\
=&\sum_g \omega_g^{\text{DF1}}\,\mathbb{E}\left[\Delta_{it}\middle| g_i = g, D_i = 1, P_t = 1\right] \\
&+\sum_g \omega_g^{\text{DF1}}\left(\mathbb{E}\left[\lambda_i\middle| g_i = g, D_i = 1\right] - \mathbb{E}\left[\lambda_i\middle| g_i = g, D_i = 0\right]\right)\left(\mathbb{E}\left[F_t\middle| P_t = 1\right] - \mathbb{E}\left[F_t\middle| P_t = 0\right]\right)
\end{aligned}$$

where

$$\omega_g^{\mathrm{DF1}} = \frac{N_g R_g(1-R_g)}{\sum_g N_g R_g(1-R_g)}$$

We find that weighting problem can be rewritten as the true ATT plus a covariance term like bad time control problem. The weighting issue in Chaismartin and d'Haullfulle can be also rewrite in this way. Let $\overline{R}^{\mathrm{ATT}}$ be the sample mean of treated unit ratios for treatment observations and $Q_{G,\Delta}^{\mathrm{ATT}}$ be the sample covariance of group treatment ratio $R_{g_i}$ and treatment effect $\Delta_{it}$ for the treated observations.

$$\overline{R}^{\mathrm{ATT}} = \frac{\sum_{i,t} D_i P_t R_{g_i}}{\sum_{i,t} D_i P_t}$$

$$Q_{G,\Delta}^{\mathrm{ATT}} = \frac{\sum_{i,t} D_i P_t \left(R_{g_i} - \overline{G}^{\mathrm{ATT}}\right)\left(\Delta_{it} - \overline{\Delta}^{\mathrm{ATT}}\right)}{\sum_{i,t} D_i P_t}$$

When loadings are balanced within each group ($\forall$group $g$, $\mathbb{E}\left[\lambda_i \middle| g_i = g, D_i = 1\right] - \mathbb{E}\left[\lambda_i \middle| g_i = g, D_i = 0\right]$), the omitted factor bias will be equal to zero. But then, the dummy factor estimator is still biased because of the weighting issue. The dummy factor estimator will be shown as

$$\begin{aligned}
&\mathbb{E}\left[\mathbb{E}\left[\frac{\sum_{i,t}(1-R_{g_i})D_i P_t \Delta_{it}}{\sum_{i,t}(1-R_{g_i})D_i P_t}\middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{G}\right]\right] \\
=&\mathbb{E}\left[\mathbb{E}\left[\overline{\Delta}_{\mathrm{ATT}}\middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{G}\right]\right] - \mathbb{E}\left[\frac{1}{1-\overline{G}^{\mathrm{ATT}}}\mathbb{E}\left[Q_{G,\Delta}^{\mathrm{DF1}}\middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{G}\right]\right] \\
=&\alpha^{\mathrm{ATT}} - \frac{\mathrm{cov}\left(\Delta_{it}, R_{g_i}\middle| D_i P_t = 1\right)}{1 - \mathbb{E}\left[R_{g_i}\middle| D_i P_t = 1\right]}
\end{aligned}$$

Fortunately, the weighting issue is not extremely severe in the dummy factor estimator, because the weighting is grantee to be between 0 and 1 (unlike in Chaismartin and d'Haullfulle) and the estimator does not become negative if true treatment effects are all positive.

$$\begin{aligned}
&\alpha^{\mathrm{ATT}} - \frac{\mathrm{cov}\left(\Delta_{it}, R_{g_i}\middle| D_i P_t = 1\right)}{1 - \mathbb{E}\left[R_{g_i}\middle| D_i P_t = 1\right]} \\
=&\alpha^{\mathrm{ATT}} - \frac{\mathbb{E}\left[\Delta_{it} R_{g_i}\middle| D_i P_t = 1\right] - \mathbb{E}\left[\Delta_{it}\middle| D_i P_t = 1\right]\mathbb{E}\left[R_{g_i}\middle| D_i P_t = 1\right]}{1 - \mathbb{E}\left[R_{g_i}\middle| D_i P_t = 1\right]} \\
\geq&\alpha^{\mathrm{ATT}} - \frac{\mathbb{E}\left[\Delta_{it}\middle| D_i P_t = 1\right] - \mathbb{E}\left[\Delta_{it}\middle| D_i P_t = 1\right]\mathbb{E}\left[R_{g_i}\middle| D_i P_t = 1\right]}{1 - \mathbb{E}\left[R_{g_i}\middle| D_i P_t = 1\right]} \\
=&\alpha^{\mathrm{ATT}} - \alpha^{\mathrm{ATT}} \\
=&0
\end{aligned}$$

### A.6.2 Time group interacted with unit

Suppose $h_t$ indicates the time group that time $t$ belongs to and we define dummy factor estimator with time group interacted with unit as

$$(\hat{\alpha}^{\mathrm{DF2}}, \hat{\eta}_t^{\mathrm{DF2}}, \hat{\omega}_{is}^{\mathrm{DF2}}) = \underset{\alpha,\eta,\omega}{argmin} \Big\{ \sum_{t=1}^{T} \sum_{i=1}^{N} (Y_{it} - \eta_t - \omega_{i,h_t} - \alpha D_i P_t)^2 \Big\} \tag{14}$$

we regress $D_i P_t$ on time dummies, and time group dummies interacted with unit dummies.

$$D_i P_t = \zeta_t^{\mathrm{DF2}} + \theta_{is_t}^{\mathrm{DF2}} + u_{it}^{\mathrm{DF2}}$$

Define $K_h$ is the ratio of treated time periods overall all time periods in group $h$,

$$K_h = \frac{\sum_t P_t \mathbb{1}_{\{t \in h\}}}{\sum_i \mathbb{1}_{\{t \in h\}}}$$

Similarly, we can get

$$u_{it}^{\mathrm{DF2}} = (D_i - \overline{D})(P_t - K_{h_t})$$

Following the Frisch-Waugh-Lovell theorem, we can show that the dummy factor estimator with time group interacted with unit.

$$\begin{aligned}
&\mathbb{E}\left[ \hat{\alpha}^{\mathrm{DF2}} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{K} \right] \\
=&\mathbb{E}\left[ \frac{\sum_{i,t} u_{it}^{\mathrm{DF2}} Y_{it}}{\sum_{i,t} u_{it}^{\mathrm{DF2}} D_i P_t} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{K} \right] \\
=&\mathbb{E}\left[ \frac{\sum_{i,t} u_{it}^{\mathrm{DF2}} \Delta_{it} D_i P_t}{\sum_{i,t} u_{it}^{\mathrm{DF2}} D_i P_t} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{K} \right] + \mathbb{E}\left[ \frac{\sum_{i,t} u_{it}^{\mathrm{DF2}} \lambda_i F_t}{\sum_{i,t} u_{it}^{\mathrm{DF2}} D_i P_t} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{K} \right] \\
=&\mathbb{E}\left[ \frac{\sum_{i,t} (1-\overline{D})(1-K_{h_t}) D_i P_t \Delta_{it}}{\sum_{i,t}(1-\overline{D}) D_i P_t (1-K_{h_t})} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{K} \right] \\
&+ \mathbb{E}\left[ \frac{\left(\sum_i (D_i - \overline{D})\lambda_i\right)\left(\sum_t (P_t - K_{h_t}) F_t\right)}{\sum_{i,t} u_{it}^{\mathrm{DF2}} D_i P_t} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{K} \right] \\
=&\mathbb{E}\left[ \frac{\sum_{i,t} (1-K_{h_t}) D_i P_t \Delta_{it}}{\sum_{i,t}(1-K_{h_t}) D_i P_t} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{K} \right] \\
&+ \mathbb{E}\left[ \frac{N\overline{D}(\overline{\lambda}_D - \overline{\lambda})\left(\sum_t (P_t - K_{h_t}) F_t\right)}{N\overline{D}(1-\overline{D})\left(\sum_t (P_t - K_{h_t}) P_t\right)} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{K} \right] \\
=&\mathbb{E}\left[ \frac{\sum_{i,t} (1-K_{h_t}) D_i P_t \Delta_{it}}{\sum_{i,t}(1-K_{h_t}) D_i P_t} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{K} \right] \\
&+ \mathbb{E}\left[ \frac{\sum_t (P_t - K_{h_t}) F_t}{\sum_t (P_t - K_{h_t}) P_t} \left(\overline{\lambda}_{\mathrm{D}} - \overline{\lambda}_{\mathrm{C}}\right) \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{K} \right]
\end{aligned}$$

Because time groups are continuous in time, most of the time groups are either fully before the treatment date or fully after the treatment date except for one group within which the treatment takes place. This suggests most of $K_h$ are either 0 or 1. For all time $t$ whose $K_{h_t} = 0$ or $K_{h_t} = 0$, it does contributes to neither the treatment effect part nor the bias part. It means that when using a time group dummy factor only keeps the data of one time group - the time group within which the treatment happens. However, for that time group, the dummy factor estimator with time group interacted with unit suffers from the bad time control problem like the one with unit group interacted with time.

### A.7 Proof of Proposition 5

Given a sufficiently long time-series it is possible to estimate factor loadings only using pre-treatment variation and then use the estimated loadings when estimating the ATT in the full sample. We refer to this two step procedure as the pre-treatment (PT) estimator. First loadings are estimated over pre-treatment periods,

$$(\hat{\lambda}_i^{\mathrm{PT}}) = \underset{\lambda}{argmin}\Big\{ \sum_{t=1}^{T}(1 - P_t) \sum_{i=1}^{N}(Y_{it} - \gamma_i - \eta_t - \lambda_i F_t)^2 \Big\}$$

and the estimated loadings $(\hat{\lambda_i})$ are then used in the full sample when estimating the ATT,

$$(\hat{\alpha}^{PT}, \hat{\gamma_i}^{\mathrm{PT}}, \hat{\eta_t}^{\mathrm{PT}}) = \underset{\alpha,\gamma,\eta}{argmin}\Big\{ \sum_{t=1}^{T} \sum_{i=1}^{N}(Y_{it} - \gamma_i - \eta_t - \hat{\lambda}_i^{\mathrm{PT}} F_t - \alpha D_i P_t)^2 \Big\}. \tag{15}$$

The pre-treatment estimator estimates the loadings using the sample of the pre-treatment period in order to avoid estimated loadings captures the treatment effect variation.

Due to collinearity, we, without loss of generality, assume the estimated loading of unit 1 is 0 and the average of time fixed effects is 0, i.e. $\hat{\lambda}_1^{PT} = 0$, $\sum_t \zeta_t^{PT,k} = 0$. Define $w_{it}^{PT,k}$ is the residual of $F_t \cdot \mathbb{1}_{i=k}$ on unit dummies, time dummies, and the rest of factors in the pre-treatment period.

$$F_t \mathbb{1}[i = k] = \kappa_i^{PT,k} + \zeta_t^{PT,k} + \sum_{j \neq k \wedge j \neq 1} \xi_j^{PT,k} F_t \mathbb{1}[i = j] + v_{it}^{PT,k}$$

we can verify that

$$
\begin{aligned}
&\xi_i^{PT,k} = \xi_j^{PT,k} && \text{if} \quad i \neq k \wedge i \neq 1 \wedge j \neq k \wedge j \neq 1 \\
&\phi_i^{PT,k} = -\frac{\xi_i^{PT,k}}{T - T_P} \sum_{t:P_t=0} F_t && \text{if} \quad i \neq k \wedge i \neq 1 \\
&v_{it}^{PT,k} = 0 && \text{if} \quad i \neq k \wedge l \neq 1 \\
&v_{kt}^{PT,k} = -v_{1t}^{PT,k}
\end{aligned}
$$

Based on the Frisch-Waugh-Lovell theorem, the loading estimate of unit $k$ ($\hat{\lambda}_k^{PT}$) can be written as:

$$
\begin{aligned}
\hat{\lambda}_k^{PT} =& \frac{\sum_{(i,t):P_t=0} v_{it}^{PT,k} Y_{it}}{\sum_{(i,t):P_t=0} v_{it}^{PT,k} F_t \mathbb{1}_{i=k}} \\
=& \frac{\sum_{(i,t):P_t=0} v_{it}^{PT,k} \lambda_i F_t + \sum_{(i,t):P_t=0} v_{it}^{PT,k} \varepsilon_{it}}{\sum_{(i,t):P_t=0} v_{it}^{PT,k} F_t \mathbb{1}_{i=k}} \\
=& \frac{\sum_{t:P_t=0} v_{1t}^{PT,k} \lambda_1 F_t + \sum_{t:P_t=0} v_{kt}^{PT,k} \lambda_k F_t}{\sum_{t:P_t=0} v_{kt}^{PT,k} F_t} + \frac{\sum_{(i,t):P_t=0} v_{it}^{PT,k} \varepsilon_{it}}{\sum_{(i,t):P_t=0} v_{it}^{PT,k} F_t \mathbb{1}_{i=k}} \\
=& \frac{-\lambda_1 \left(\sum_{t:P_t=0} v_{kt}^{PT,k} F_t\right) + \lambda_k \left(\sum_{t:P_t=0} v_{kt}^{PT,k} F_t\right)}{\sum_{t:P_t=0} v_{kt}^{PT,k} F_t} + \frac{\sum_{(i,t):P_t=0} v_{it}^{PT,k} \varepsilon_{it}}{\sum_{(i,t):P_t=0} v_{it}^{PT,k} F_t \mathbb{1}_{i=k}} \\
=& \lambda_k - \lambda_1 + \frac{\sum_{(i,t):P_t=0} v_{it}^{PT,k} \varepsilon_{it}}{\sum_{(i,t):P_t=0} v_{it}^{PT,k} F_t \mathbb{1}_{i=k}}
\end{aligned}
$$

Then, we regress $Y_{it} - \hat{\lambda}_i^{PT} F_t$ on unit dummies, time dummies and treatment dummies and get the pre-treatment estimator.

$$
\begin{aligned}
& \mathbb{E}\left[\hat{\alpha}^{PT} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] \\
=& \mathbb{E}\left[\frac{\sum_{i,t} u_{it}^{\text{TWFE}} (Y_{it} - \hat{\lambda}_i^{PT} F_t)}{\sum_{i,t} u_{it}^{\text{TWFE}} D_i P_t} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] \\
=& \mathbb{E}\left[\frac{\sum_{i,t} u_{it}^{\text{TWFE}} \Delta_{it} D_i P_t}{\sum_{i,t} u_{it}^{\text{TWFE}} D_i P_t} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] + \mathbb{E}\left[\frac{\sum_{i,t} u_{it}^{\text{TWFE}} (\lambda_i - \hat{\lambda}_i^{PT}) F_t}{\sum_{i,t} u_{it}^{\text{TWFE}} D_i P_t} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] \\
=& \mathbb{E}\left[\frac{\sum_{i,t:} D_i P_t \Delta_{it}}{\sum_{i,t} D_i P_t} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] + \mathbb{E}\left[\frac{\sum_{i,t} u_{it}^{\text{TWFE}} \lambda_1 F_t}{\sum_{i,t} u_{it}^{\text{TWFE}} D_i P_t} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right] \\
=& \mathbb{E}\left[\overline{\Delta}^{\text{ATT}} \middle| \boldsymbol{D}, \boldsymbol{P}, \boldsymbol{F}\right]
\end{aligned}
$$

We can show that pre-treatment estimator is unbiased.

$$
\begin{aligned}
&\mathbb{E}\left[\hat{\alpha}^{PT}\right] \\
=&\mathbb{E}\left[\mathbb{E}\left[\hat{\alpha}^{PT}\middle|\boldsymbol{D},\boldsymbol{P},\boldsymbol{F}\right]\right] \\
=&\alpha^{\mathrm{ATT}}
\end{aligned}
$$

# Appendix B Additional Simulations

**Table B1:** Correlation between Treatment effects and Factor Structure

This table presents how estimates change along with the correlation between loadings and treatment effects $\rho_{\Delta,\lambda}$ and correlation between factors and treatment effects $\rho_{\Delta,F}$. All other parameter are the same as baseline setting. The mean of two-way fixed effect estimators is displayed in the first line without any parentheses, the mean of full sample estimator is displayed in the second line with round parentheses, and the mean of pre-treatment estimators is displayed in the third line with square parentheses.

| **TWFE Estimator** **(FS Estimator)** **[PT Estimator]** | | Correlation between Loadings and TE $\rho_{\Delta,\lambda}$ | | | | | |
|---|---|---|---|---|---|---|---|
| | | 0 | 0.2 | 0.4 | 0.6 | 0.8 | 1 |
| Correlation between Factors and TE $\rho_{\Delta,F}$ | 0 | 1.3161 | 1.3252 | 1.3179 | 1.3424 | 1.3471 | 1.3005 |
| | | (1.0021) | (1.0016) | (1.0020) | (0.9998) | (1.0024) | (1.0035) |
| | | [1.0098] | [1.0066] | [1.0067] | [1.0052] | [1.0024] | [1.0054] |
| | 0.2 | 1.3315 | 1.3285 | 1.3212 | 1.3178 | 1.2856 | 1.3155 |
| | | (0.9485) | (0.9469) | (0.9492) | (0.9578) | (0.9318) | (0.9583) |
| | | [0.9956] | [0.9936] | [0.9965] | [1.0005] | [0.9825] | [0.9979] |
| | 0.4 | 1.2988 | 1.2862 | 1.3037 | 1.2961 | 1.3558 | 1.2758 |
| | | (0.9037) | (0.8900) | (0.8943) | (0.8886) | (0.9294) | (0.8857) |
| | | [0.9938] | [0.9856] | [0.9914] | [0.9804] | [1.0273] | [0.9774] |
| | 0.6 | 1.3206 | 1.3189 | 1.2996 | 1.2873 | 1.2742 | 1.3026 |
| | | (0.8673) | (0.8607) | (0.8487) | (0.8405) | (0.8621) | (0.8630) |
| | | [1.0077] | [1.0019] | [0.9825] | [0.9809] | [0.9919] | [0.9969] |
| | 0.8 | 1.3433 | 1.3232 | 1.3067 | 1.3387 | 1.3221 | 1.2988 |
| | | (0.8202) | (0.8002) | (0.8105) | (0.8251) | (0.8295) | (0.8122) |
| | | [1.0189] | [0.9946] | [0.9966] | [1.0158] | [1.0121] | [0.9926] |
| | 1 | 1.3476 | 1.3134 | 1.2950 | 1.2967 | 1.3757 | 1.3014 |
| | | (0.7554) | (0.7690) | (0.7658) | (0.7679) | (0.7731) | (0.7719) |
| | | [1.0063] | [0.9971] | [0.9950] | [0.9910] | [1.0327] | [0.9987] |

**Table B2:** Unit Dimension Irrelevance

This table presents how estimates change along with the loading difference $\mu$ and correlation between loadings and treatment effects $\rho_{\Delta,\lambda}$. All other parameter are the same as baseline setting. The mean of two-way fixed effect estimators is displayed in the first line without any parentheses, the mean of full sample estimator is displayed in the second line with round parentheses, and the mean of pre-treatment estimators is displayed in the third line with square parentheses.

| **TWFE Estimator** **(FS Estimator)** **[PT Estimator]** | | Loading Difference $\mu$ | | | | | |
|---|---|---|---|---|---|---|---|
| | | 0 | 0.2 | 0.4 | 0.6 | 0.8 | 1 |
| Correlation between Loadings and TE $\rho_{\Delta,\lambda}$ | 0 | 0.9957 | 1.1476 | 1.3190 | 1.5148 | 1.6653 | 1.7614 |
| | | (0.8554) | (0.8637) | (0.8699) | (0.8566) | (0.8532) | (0.8751) |
| | | [0.9923] | [0.9970] | [1.0079] | [1.0099] | [1.0023] | [1.0091] |
| | 0.2 | 1.0069 | 1.1718 | 1.3267 | 1.4874 | 1.5940 | 1.7955 |
| | | (0.8571) | (0.8581) | (0.8621) | (0.8511) | (0.8386) | (0.8711) |
| | | [1.0074] | [1.0020] | [1.0020] | [0.9951] | [0.9803] | [1.0023] |
| | 0.4 | 1.0085 | 1.1402 | 1.3010 | 1.4801 | 1.6821 | 1.7797 |
| | | (0.8631) | (0.8458) | (0.8455) | (0.8574) | (0.8884) | (0.8419) |
| | | [1.0001] | [0.9928] | [0.9886] | [0.9962] | [1.0328] | [0.9851] |
| | 0.6 | 1.0001 | 1.1574 | 1.3002 | 1.4449 | 1.5691 | 1.8105 |
| | | (0.8668) | (0.8566) | (0.8493) | (0.8392) | (0.8648) | (0.8470) |
| | | [1.0054] | [1.0002] | [0.9831] | [0.9809] | [0.9943] | [0.9932] |
| | 0.8 | 1.0211 | 1.1664 | 1.3131 | 1.4811 | 1.6185 | 1.7735 |
| | | (0.8709) | (0.8520) | (0.8637) | (0.8689) | (0.8757) | (0.8617) |
| | | [1.0215] | [0.9980] | [1.0031] | [1.0116] | [1.0111] | [0.9981] |
| | 1 | 1.0044 | 1.1412 | 1.2972 | 1.4659 | 1.6616 | 1.7427 |
| | | (0.8558) | (0.8436) | (0.8635) | (0.8798) | (0.8657) | (0.8638) |
| | | [1.0091] | [0.9803] | [0.9972] | [1.0052] | [1.0107] | [0.9935] |

# Appendix C Detailed literature review

We review 21 papers that use difference-in-difference or closely related methodology that we found in our literature review. For each paper, we use the following presentation:

**Authors (year)**, Title.
**DiD:** Which tables use difference-in-differences methodology.
**Estimator:** What difference-in-difference estimator is used in the respective difference-in-differences tables.
**Dimension:** The unit dimension (e.g., plant), time frequency (e.g., monthly)) of the paper.
**Factor Control:** A description of the factor control that is used in the respective tables.
**Description of variables and regressions:** Brief description of the independent and dependent variables or mechanism being tested.
**Heterogeneous:** Whether the paper estimates heterogeneous treatment effects. We take a liberal classification here and describe also subsample analysis that acknowledges that the treatment effects are heterogeneous.
**Dynamic:** Whether a dynamic difference-in-differences estimator is used and if so which specifications with a factor structure use the dynamic estimator.
**Staggered:** Whether the difference-in-differences is staggered.

1. **Collard-Wexler and de Loecker (2015)**, Reallocation and Technology: Evidence from the US Steel Industry.
   **DiD:** Tables 5, 9.
   **Estimator:** Dummy Factor (Table 5, columns 2, 3, 4, Table 9, Columns 3, 4, 6, 7) TWFE (Table 9, Column 8).
   **Dimensions:** Plant × Year.
   **Description of factor control:** Dummy Factor: Year × Firm, Year × State, Firm × Year × State.
   **Description of variables and regressions:** Main independent variable is a dummy variable indicating whether a plant is vertically integrated interacted with a time dummy. The tables measure the (negative) technology premium associated with old technology.

**Heterogeneous:** No

**Dynamic:** No

**Staggered:** The implied DiD could be staggered since plants could theoretically be classified as vertically integrated and then change status at a later date.

2. **Cicala (2015)**, When Does Regulation Distort Costs? Lessons from Fuel Procurement in US Electricity Generation. **DiD:** Tables 2, 3, 4, 5, 6, 7.

   **Estimator:** TWFE (Table 2, columns 1-6, Table 3, columns 2, 4-6, Table 4, columns 1-6, Table 6, columns 1-6, Table 7, columns 1-6) Dummy Factor (Table 3, column 3) Unit time trend (Online Appendix Table B.5).

   **Dimensions:** Facility × Month.

   **Description of factor control:** Dummy Factor (Table 3, column 3) – Facility × Year. Unit time trend (Online Appendix Table B.5) State-specific quadratic time trend.

   **Description of variables and regressions:** DiD that relates deregulation to the price paid for coal by power plants.

   **Heterogeneous:** Yes p.432 discusses the heterogeneity of treatment effects.

   **Dynamic:** Yes, Figure 5 presents dynamic treatment effects.

   **Dynamic used with factor control:** No

   **Staggered:** The deregulation is staggered.

3. **Currie, Davis, Greenstone and Walker (2015)**, Environmental Health Risks and Housing Values: Evidence from 1,600 Toxic Plant Openings and Closings.

   **DiD:** Tables 2, 3, 4, 5, 6.

   **Estimator:** Dummy Factor (Table 2, columns 1-8, Table 4, columns 1-8, Table 6, columns 1-8), Covariates time trend (Table 2-6).

   **Dimensions:** Plant × Year.

   **Description of factor control:** Dummy Factor: Plant × Distance-bin, State × year, Plant × year, County × year. Covariates time trend: 1990 census tract characteristics interacted with quadratic time trends.

   **Description of variables and regressions:** Dependent variable pollution / birthweight and independent variable is plant openings and closings.

**Heterogeneous:** No

**Dynamic:** Yes, Figure 3 and 4.

**Dynamic used with factor control:** Yes, Figure 3 and 4 which use Dummy Factor and Covariates time trend. No, (Table 2, columns 1-8, Table 4, columns 1-8, Table 5, columns 1-5, Table 6, columns 1-8)

**Staggered:** Yes

4. **Favara and Imbs (2015)**, Credit Supply and the Price of Housing. Staggered.

   **DiD:** Table 2, Table 3, Table 4, Table 6.

   **Estimator:** TWFE (Table 2, 3, 4, 6)

   **Dimensions:** County $\times$ Year.

   **Description of factor control:** None

   **Description of variables and regressions:** Dependent variables loan outcomes and housing returns. Independent variable is state-wide banking deregulation index (developed by Rice and Strahan, 2010).

   **Heterogeneous:** Yes, allows for different treatment effects across counties depending on their house price elasticity.

   **Dynamic:** No

   **Staggered:** Yes

5. **Hackmann, Kolstad and Kowalski (2015)**, Adverse Selection and an Individual Mandate: When Theory Meets Practice.

   **DiD:** Table 2, 4.

   **Estimator:** DiD (Table 2,4)

   **Dimensions:** State $\times$ Year.

   **Description of factor control:** None

   **Description of variables and regressions:** Dependent variables are insurance coverage, log premiums or log average costs and independent variable is regulation change in Massachusetts that mandated insurance.

   **Heterogeneous:** No

**Dynamic:** No

**Staggered:** No

6. **Bailey and Goodman-Bacon (2015)**, The War on Poverty's Experiment in Public Medicine: Community Health Centers and the Mortality of Older Americans.

   **DiD:** Table 2- 5.

   **Estimator:** Dummy Factor (Table 2, Table 3, Table 4, Table 5) Covariate time trend (Table 2, column 2, column 3, Table 3, Table 4, Table 5)

   **Dimensions:** County × Year.

   **Description of factor control:** Dummy Factor: Urban × Year, State × Year.

   **Covariate time trend:** 1960 characteristics interacted with linear time trend: share of population: in urban area, in rural area, under 5 years of age, 65 or older, non-white, with 12 or more years of education, with less than 4 years of education, in households with Income less than $3000. In households with Incomes greater than $10000, total active MDs.

   **Description of variables and regressions:** Dependent variable average mortality rate and main independent variable is a dummy variables indicating the introduction of community health centres.

   **Heterogeneous:** Yes, Table 2, Panel A considers all ages while Panel B only considers people over 50 years. Table 3 stratifies treatment effects on mortality causes (e.g., heart disease). Table 4 stratifies treatment effects over 1960 characteristics and census regions. Table 5 stratifies results over household Income.

   **Dynamic:** Yes, Table 2, 3, and 4 consider dynamic treatment effects. Figures 5, 6 and 7 are dynamic.

   **Dynamic used with factor control:** Yes, Table 2, 3, and 4 consider dynamic treatment effects (treatment effects are estimated over different event time buckets). Figure 5, 6 and 7. No, Table 5.

   **Staggered:** Yes

7. **Burgess, Jedwab, Miguuel, and Morjaria Padró I Miquel (2015)**, The Value of Democracy: Evidence from Road Building in Kenya. **DiD:** Table 1, 2, 3, 5.

**Estimator:** TWFE (Table 1, column 1, Table 2, column 2) Covariate time trend (Table 1, column 2-5, Table 2, column 2 -5, Table 3, Table 5) Unit time trend (Table 1, column 5, Table 2, column 5, Table, 5 columns 3-4)

**Dimensions:** District × Year.

**Description of factor control:** Covariate time trend: Table 1, 2, 5: (Population, area, urbanization rate) × trend, (earnings, employment, cash crops) × trend, (Main highway, border, dist. Nairobi) × trend, District time trends. Table 3, Initial controls × trend. Unit time trend: (Table 1, column 5, Table 2, column 5, Table, 5 columns 3-4)

**Description of variables and regressions:** The dependent variable is the share of road expenditure normalized by population share. The independent variable is co-ethnicity of president.

**Heterogeneous:** No

**Dynamic:** No

**Staggered:** Yes

8. **Braguinsky, Ohyama, Okazaki, and Syverson (2015)**, Acquisitions, Productivity, and Profitability: Evidence from the Japanese Cotton Spinning Industry.

   **DiD:** Table 2, 3 and 6.

   **Estimator**: TWFE (Table 2, Table 3, Table 6, Columns 4-6)

   **Dimensions:** Plant × Year.

   **Description of factor control:** None

   **Description of variables and regressions:** The dependent variable is the economic performance. The main independent variable indicates whether the particular plant was acquired.

   **Heterogeneous:** Yes, Table 2, 3: treatment effects are stratified according to whether the acquisition is undertaken by a serial acquirer.

   **Dynamic:** No

   **Staggered:** Yes

9. **Pomeranz (2015)**, No Taxation without Information: Deterrence and Self-Enforcement

in the Value Added Tax

**DiD:** Table 4, 5, 6, 7

**Estimator:** TWFE (Table 4, 5, 6, 7)

**Dimensions:** Firm $\times$ Month.

**Description of factor control:** None

**Description of variables and regressions:** The dependent variable is the line item increase the main independent variable is letter from the tax office interacted with line item.

**Heterogeneous:** No

**Dynamic:** Yes, Figure 2 is dynamic, tracking treatment effects over time.

**Dynamic used with factor control:** No factor used.

**Staggered:** Yes

10. **de Janvry, Emerick, Gonzalez-Navarro and Sadoulet (2015)**, Delinking Land Rights from Land Use: Certification and Migration in Mexico

    **DiD:** Table 1, 4, 5, 6.

    **Estimator:** TWFE (Table 1, Columns 1,2,3,5,6 Table 4, Columns 1-2, Table 5, Table 6 Column 1) Dummy Factor (Table 1, Column 4, Table 4, Column 3, Table 6, Column 2)

    **Dimensions:** Household $\times$ Ejido $\times$ Year.

    **Description of factor control:** Dummy Factor: Table 1, Column 4 (State $\times$ Time), Table 4, Column 3 (High-yield $\times$ Time), Table 6, Column 2 (Progresa Treatment Locality $\times$ Time)

    **Description of variables and regressions:** The main dependent variable is in an indicator variable for whether households have a migrant and the main independent variable is whether a geographic area has been certified.

    **Dynamic:** No

    **Staggered:** Yes

11. **Yagan (2015)**, Capital Tax Reform and the Real Economy: The Effects of the 2003 Dividend Tax Cut

**DiD:** Table 2, 3, 4.

**Estimator:** DiD (Table 2, Column 1, 3, 4, 5, 7, 8, 10, 11, Table 3, Table 4, Columns 1-2) TWFE (Table 1, Column 3, 6, 9, 12, Table 4, Column 3, Column 6).

**Dimensions:** Firm × Year.

**Description of factor control:** None. The main dependent variables are firm investment, employ compensation and firm payout. The main independent variable is whether the firm is a C-Corp interacted with a time dummy indicating the 2003 tax cut.

**Heterogeneous:** No

**Dynamic:** Yes, Table 4, Columns 1-6 includes dummies for each of the treatment years.

**Dynamic used with factor control:** No factor used.

**Staggered:** No

12. **Lalive, Landais and Zweimüller (2015)**, Market Externalities of Large Unemployment Insurance Extension Programs

    **DiD:** Table 2, 3, 4.

    **Estimator:** TWFE (Table 2, Columns 1-2, Table 3, Table 4) Unit time trends (Table 2, Columns 3-6).

    **Dimensions:** Firm × Year.

    **Description of factor control:** Unit time trends: Region specific trends

    **Description of variables and regressions:** The main dependent variable is unemployment duration and the main independent variable indicated eligibility of the Regional Extension Benefit Program (REBP) which extended unemployment benefits for a large subset of Austrian workers.

    **Heterogeneous:** Yes, treatment effects are evaluated across employment and age.

    **Dynamic:** No

    **Staggered:** Effectively yes since there are two treatments.

13. **Muhlenbachs, Spiller, Timmins (2015)**, The Housing Market Impacts of Shale Gas Development

**DiD:** Table 2, 3, 4.

**Estimator:** Dummy Factor (Table 2, 3, 4) TWFE (Table 4)

**Dimensions:** Quarter × House

**Description of factor control:** Dummy Factor: Table 2 Panel A (County × year), Panel B (Census tract × year), Table 3 Panel B (County × year)

**Description of variables and regressions:** The main dependent variable is log sale prices of houses and the main independent variable is the number of wells at different distances from the property as well as whether the property is reliant on ground water.

**Heterogeneous:** Yes, both Table 3 and 4 considers subsamples in different panels.

**Dynamic:** No

**Staggered:** Yes, the number of wells are changing.

14. **Bøler, Moxnes and Ulltveit-Moe (2015)**, R&D, International Sourcing, and the Joint Impact on Firm Performance

    **DiD:** Table 4, 5, 6, 7, 8, 9.

    **Estimator:** TWFE (Table 4, Table 5, Columns 1-3, Table 7, Column 1, Table 8, Column 5) Unit time trends (Table 5, Columns 4-7, Table 6, Table 7, Column 2, Table 8, Columns 1-4, Table 9)

    **Dimensions:** Firm × Year.

    **Description of factor control:** Unit time trends, Table 5, Columns 4-7, Table 6, Table 7, Column 2, Table 8, Columns 1-4, Table 9

    **Description of variables and regressions:** The paper considers as dependent variables R&D expenditure and number of imported products and the main independent variable captures whether the firm is eligible for tax credits.

    **Heterogeneous:** Yes, Table 8 considers the origins of imported products.

    **Dynamic:** Yes, Table 4, columns 1-3, Table 5, columns 1-3

    **Dynamic used with factor control:** No (Table 4 no factor factor used, Table 5, columns 4-7, Table 6, Table 7, columns 2, Table 8, Columns 1-4, Table 9) Yes (Table 5, columns 1-3) .

    **Staggered:** No

15. **Duggan, Garthwaite and Goyal (2016)**, The Market Impacts of Pharmaceutical Product Patents in Developing Countries: Evidence from India

    **DiD:** Table 4, 5, 6, 7, 8.

    **Dimensions:** Molecules $\times$ Quarter

    **Estimator:** TWFE (Table 4, Columns 2, 4, 6, 8, Table 6, Columns 2, 4, Table 7, Columns 2, 4, 6, 8) Unit time trends (Table 4, Columns 1, 3, 5, 7, Table 5, Table 6, Columns 1, 3, Table 7, Columns 1, 3, 5, 7, Table 8)

    **Description of factor control:** Unit time trends: $\lambda \times t \times I_{EverPatent}$, where $t$ is a time indicator and $I_{EverPatent}$ is an indicator of whether the molecule ever has had a patent. (Table 4, Columns 1, 3, 5, 7, Table 5, Table 6, Columns 1, 3, Table 7, Columns 1, 3, 5, 7, Table 8)

    **Description of variables and regressions:** The paper examines the effect of molecule patents on prices and quantities sold.

    **Heterogeneous:** No.

    **Dynamic:** Yes, Figure 1, 2, 5, 6, 7, 8 and 9 are event studies.

    **Dynamic used with factor control:** No (Table 4, Columns 1, 3, 5, 7, Table 5, Table 6, Columns 1, 3, Table 7, columns 1, 3, 5, 7, Table 8).

    **Staggered:** Yes

16. **Jayaraman, Ray and De Véricourt (2016)**, Anatomy of a Contract Change

    **DiD:** Table 2, Columns 4, 5

    **Estimator:** DiD (Table 2, Columns 4, 5)

    **Dimensions:** Rice output in kg $\times$ day

    **Description of factor control:** None

    **Description of variables and regressions:** This paper studies the effect of a contract change on tea worker's productivity.

    **Heterogeneous:** No.

    **Dynamic:** Yes, Figure 9 is estimated with time-varying treatment effects (one for each of 17 weeks).

    **Staggered:** No

17. **Hoynes, Whitmore Schanzenbach and Almond (2016)**, Long-Run Impacts of Childhood Access to the Safety Net

    **DiD:** Table 2, 3, 4, 5, 6, 7, 8, 9.

    **Estimator:** Unit time trends & Covariate time trends (Table 2, 3, 4, 5, 6, 7, 8, 9)

    **Dimensions:** Individual × County × Birth year

    **Description of factor control:** Unit time trends & Covariate time trends (Table 2, 3, 4, 5, 6, 7, 8, 9), State specific cohort trend, county pre-treatment characteristics trend.

    **Description of variables and regressions:** This paper studies the effect of food stamps programs on long-run health outcomes.

    **Heterogeneous:** Yes, it is stratified across gender.

    **Dynamic:** No

    **Staggered:** Yes

18. **Pierce and Schott (2016)**, The Surprisingly Swift Decline of US Manufacturing Employment

    **DiD:** Table 1, 2, 3, 4, 5, 6, 7, 8, 9.

    **Estimator:** TWFE (Table 1, Table 2, Columns 1-4, 5, 6, Table 3, Columns 2-3, Table 7, Table 8, Table 9), Covariate time trend (Table 2, Column 5) Dummy factor (Table 3, Column 1, Table 4, Columns 1-4, Table 5, Columns 1-4, Table 6, Columns 1-4)

    **Dimensions:** Industry × year.

    **Description of factor control:** Covariate time trend (Table 2, Column 5): $ln(RDGP)_t \times ln(NP/Emp_{i,t})$, effectively a GDP factor interacted with a covariate. Dummy factor (Table 3, Column 1): Country × time, Country × industry, Industry × year (Table 4, 5, 6) Product × country, Country × time, Product × time.

    **Description of variables and regressions:** This paper studies the effect of tariff reduction on employment.

    **Heterogeneous:** No

    **Dynamic:** No.

    **Staggered:** No

19. **Muralidharan, Niehaus and Sukhtankar (2016)**, Building State Capacity: Evidence from Biometric Smartcards in India

    **DiD:** Table 2, 7.

    **Estimator:** TWFE (Table 2, Columns 5-8, Table 7, Columns 3-4)

    **Dimensions:** Household / Individual × mandal-district × week.

    **Description of factor control:** None.

    **Description of variables and regressions:** This paper studies the impact of "smart cards" on the functioning of the financial system.

    **Heterogeneous:** No

    **Dynamic:** No

    **Staggered:** Yes

20. **Sequiera, (2016)**, Corruption, Trade Costs, and Gains from Tariff Liberalization: Evidence from Southern Africa

    **DiD:** Table 5, 8, 9, 10, 11, 13, 15, 18, 19.

    **Estimator:** DiD (Table 5, Columns 4-6, Table 8, Table 9, Table 10, Table 11, Table 13, Table 18, Table 19, Panel B), ≈ TWFE (Table 15)

    **Dimensions:** Trade Gap × Year

    **Description of factor control:** None.

    **Description of variables and regressions:** This paper studies the effect of tariff changes on trade and bribery.

    **Heterogeneous:** No

    **Dynamic:** No

    **Staggered:** No

21. **Koudjis and Voth (2016)**, Leverage and Beliefs: Personal Experience and Risk-Taking in Margin Lending

    **DiD:** Table 5, 6, 7 (Panel B), 8, 9, 11, 12

    **Estimator:** ≈ TWFE (Table 5, Table 6, Table 7 Columns 3 & 6, Table 8, Table 9, Table 11, Table 12)

    **Dimensions:** Haircut × year

**Description of factor control:** None.

**Description of variables and regressions:** This paper examines of unexpected investor losses on the interest rates and haircuts charged.

**Heterogeneous:** Yes, treatment effects are stratified across exposure and whether it is a loans consortium.

**Dynamic:** No

**Staggered:** No

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