Phantom Pre-Trends and Dynamic Effects in Event-Studies with Imbalanced Panels

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Research Question

- Can **TWFE event-studies** (ES) diagnose **parallel trends** (PT) and estimate **dynamic effects** under **panel imbalance** (differential entry/exit)?
- We study the **non-staggered** (common-onset t^*) setting, often considered "safe", since forbidden-comparison / negative-weighting bias arises only under *staggered* adoption (Goodman-Bacon 2021; de Chaisemartin & D'Haultfœuille 2020; Sun & Abraham 2021).

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k \neq t^*-1} \hat{\beta}_k D_i \mathbf{1}(t=k) + \varepsilon_{it}$$

with unit (α_i) FE, time (λ_t) FE, binary treatment D_i, and onset t^*. $\hat{\beta}_k$ $(k < t^*)$ diagnose PT; $\hat{\beta}_k (k > t^*)$ measure dynamics.

Key Results & Contribution

Even when treatment adoption is non-staggered (the "safe" setting), ES-estimator fails in two ways under panel imbalance:

1. **Phantom bias.** When effects are heterogeneous across entry- and/or exit-cohorts, ES-estimator may report pre-trends (under differential *entry*) and dynamic effects (under differential *exit*) even when none exist.
2. **Wrong estimand.** Even absent bias, coefficients are identified from *changing subsets* of units, so leads and lags are not comparable: they conflate pre-trends and dynamic effects with compositional change (analogous to Liu 2025's staggered-adoption comparability problem).

HTE-robust estimators **do not address** both; our proposed estimator **does**.

How the Bias Emerges

Setup and Notation.

- Treatment onset in common period t^*
- Differential-entry: unit i's cohort $c(i)$ enters at period e_c $(e_1 < \cdots < e_C < t^*)$ $\Rightarrow i$ is observed for $T_{\text{pre}}(c(i)) = t^* - e_c$ pre-periods.
- Same exit: all i observed for T_{post} post-periods $\Rightarrow T_{c(i)} = T_{\text{pre}}(c(i)) + T_{\text{post}}$
- DGP: PT holds exactly (within-cohort *and* pooled):

$$Y_{it} = \begin{cases} \mu_0 & D_i = 0 \\ \mu_1 & D_i = 1,\ t < t^* \\ \mu_1 + \tau_{c(i)} & D_i = 1,\ t \geq t^* \end{cases}$$

static, cohort-specific effects τ_c; *no pre-trend by construction.*

Demeaned quantities (FWL).

- By FWL, $\hat{\beta}_k$ equal the coefficients from regressing the demeaned outcome $\tilde{Y}_{it} = Y_{it} - \bar{Y}_i$ on demeaned interactions $\tilde{X}_{it}^{(k)}$ and time dummies.
- Unit FE demean over the *entire* history (including post-treatment), so a treated unit's $\bar{Y}$ is contaminated by its own effect and differential panel-length:

$$\bar{Y}_i = \frac{T_{\text{pre}}(c(i))\,\mu_1 + T_{\text{post}}\,(\mu_1 + \tau_{c(i)})}{T_{c(i)}} = \mu_1 + \frac{T_{\text{post}}}{T_{c(i)}}\tau_{c(i)}$$

$$\Rightarrow \quad \tilde{Y}_{it} = \begin{cases} -\frac{T_{\text{post}}}{T_{c(i)}}\tau_{c(i)}, & t < t^* \\ \frac{T_{\text{pre}}(c(i))}{T_{c(i)}}\tau_{c(i)}, & t \geq t^* \end{cases}$$

Mirrored by interactions: $\sum_{k \geq t^*} \tilde{X}_{it}^{(k)} = \tilde{Y}_{it}/\tau_{c(i)}\ \forall t$. Controls: $\tilde{Y}_{it} = 0$.

How the Bias Emerges (cont.)

Proposition 1 — No Bias Under Effect-Homogeneity. If $\tau_c = \tau$ for all c, then $\hat{\beta}_k = 0$ for all $k < t^*$, for *any* panel composition.

Why: $\tilde{Y}_{it} = \tau \sum_{k \geq t^*} \tilde{X}_{it}^{(k)}$ for *every* observed (i,t), so $\tilde{Y} \in \text{Col}(\tilde{X}_{it}^{(k \geq t^*)})$. Thus, $\beta_k = \tau$ $(k \geq t^*)$, $\beta_k = 0$ $(k < t^*)$ produces $SSR = 0 \Rightarrow \hat{\beta}_k = 0$.

Proposition 2 — Bias Under Effect-Heterogeneity. If $\tau_c \neq \tau_{c'}$ for some $c \neq c'$, then generically $\hat{\beta}_k \neq 0$ for $k < t^*$ — a **phantom pre-trend**.

Why: OLS fits the post-interactions with a common $\bar{\tau}$ (weighted mean of τ_c), which cannot reproduce $\tilde{Y}$ for all cohorts, leaving the pre-period residual:

$$\tilde{Y}_{it} - \bar{\tau} \sum_{k \geq t^*} \tilde{X}_{it}^{(k)} = -\frac{T_{\text{post}}}{T_c}\delta_{c(i)}, \qquad \text{where } \delta_{c(i)} \equiv \tau_{c(i)} - \bar{\tau}.$$

Averaging over treated units present in pre-period k:

$$\bar{r}_k = \frac{\sum_{c:\, e_c \leq k} n_{1,c}\,(T_{\text{post}}/T_c)\,\delta_c}{\sum_{c:\, e_c \leq k} n_{1,c}}.$$

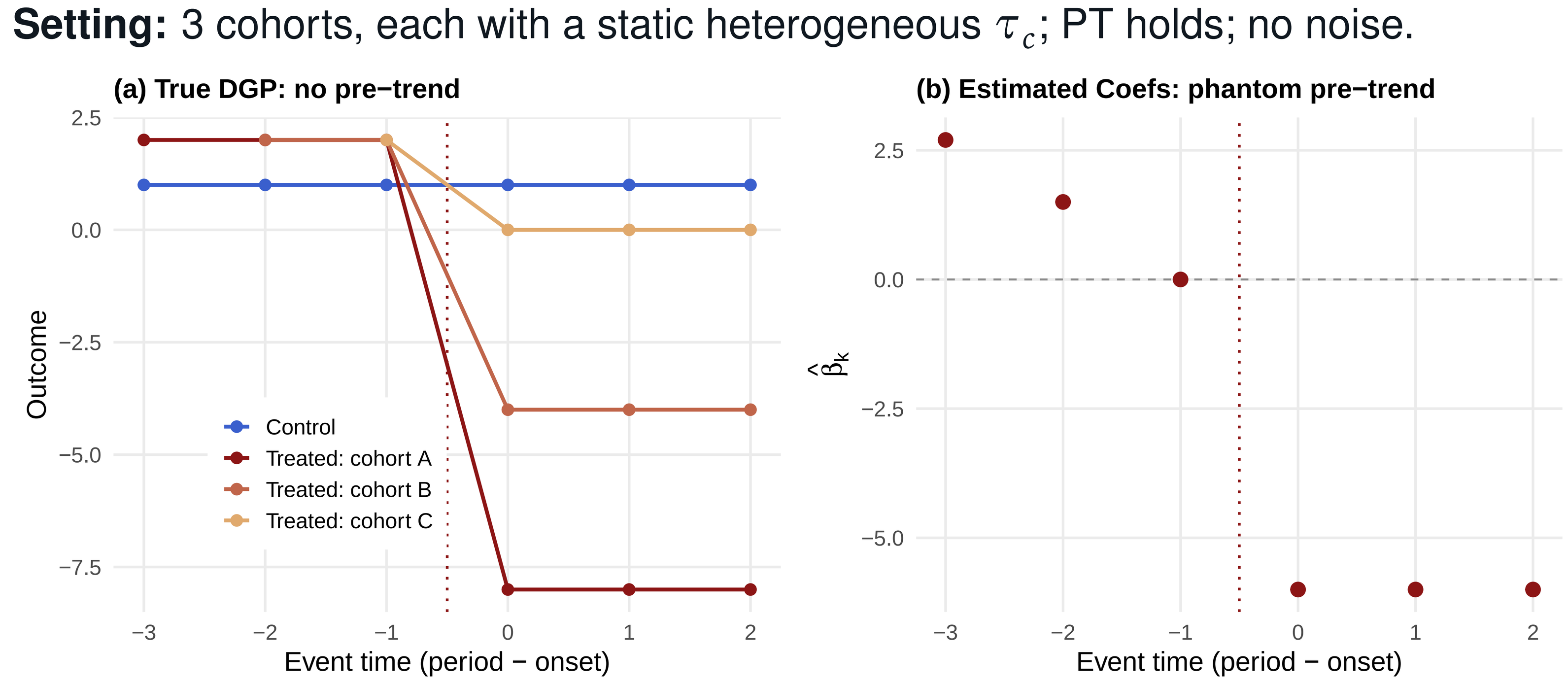
As k grows from e_1 to t^*-1, the set $\{c : e_c \leq k\}$ expands, so $\bar{r}_k$ shifts across pre-periods. The pre-treatment interactions, the only regressors distinguishing pre-periods, absorb it $\Rightarrow \hat{\beta}_k \neq 0$.

Corollary. *Prop. 2 implies the ES-estimator can obscure genuine pre-trends.*

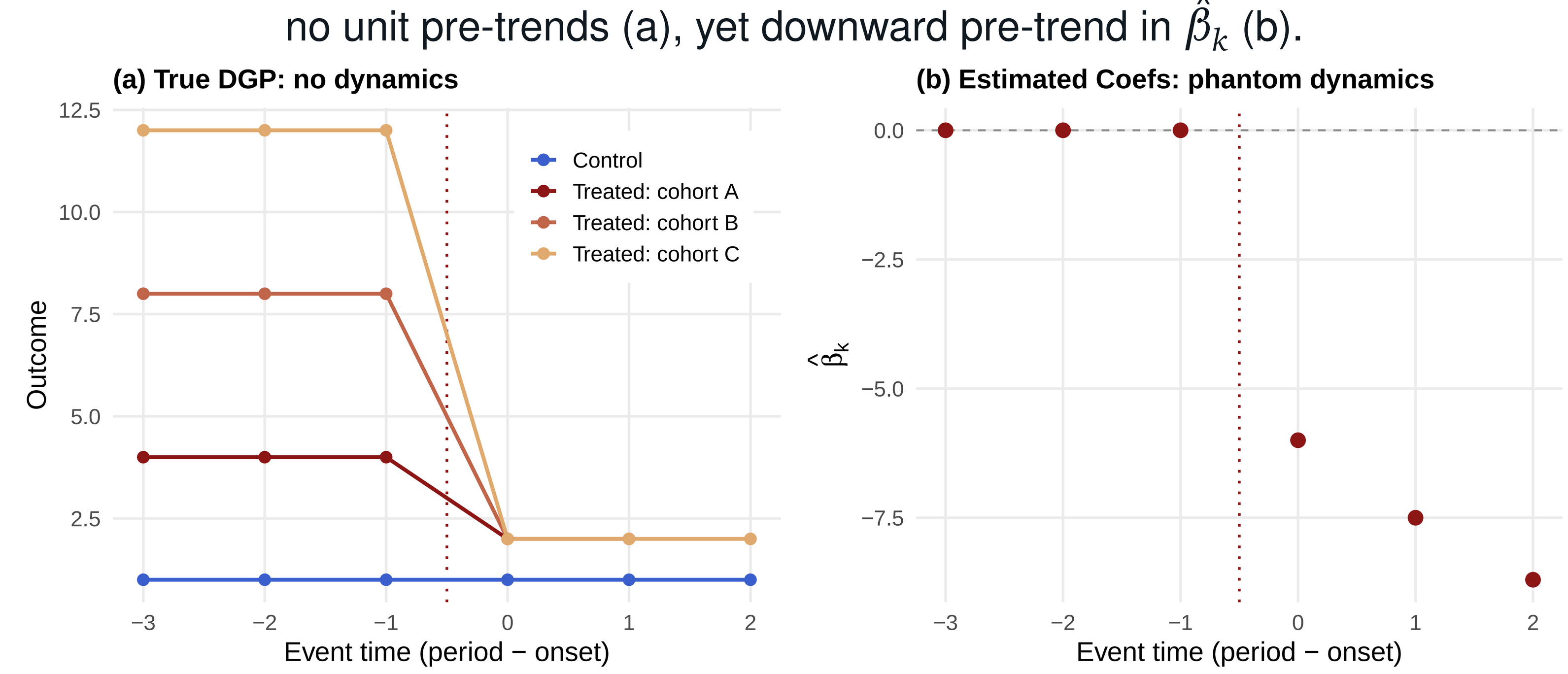
Extension: *Phantom dynamic effects emerge under differential-exit.* Follows analogously, because residual distortion now shifts across the post-periods.

Numerical Illustration

Setting: 3 cohorts, each with a static heterogeneous τ_c; PT holds; no noise.



Differential entry → phantom pre-trend:
no unit pre-trends (a), yet downward pre-trend in $\hat{\beta}_k$ (b).



Differential exit → phantom dynamics:
no unit has dynamic TE (a), yet $\hat{\beta}_k$ trends post-onset (b).

Do HTE-Robust Estimators Help?

Estimator	Unit-Demeaning	Pre-Trend Bias	Dyn-Effect Bias	Estimand Problem
Canonical TWFE	✓	phantom	phantom	✓
IW (Sun–Abraham)	✓	phantom	phantom	✓
Stacked DiD	✓	phantom	phantom	✓
CS-DiD	×	composition	composition	✓
$\text{DID}_{\text{impute}}$ / FEct	×	clean	composition	✓

Unit-demeaning: $\bar{Y}_i$ contaminated by T_i & $\tau_i \to$ phantom bias
Composition bias: estimates track the changing set of observed units, so baseline differences across cohorts may lead to spurious trends even when no unit trends.

A "Within-Cohort" Estimator

1. **Define cohorts** by same entry-exit periods → each a *balanced* sub-panel.
2. **Achieve PT:** match within cohort on pre-treatment trajectories + covariates (gains efficiency & identification).
3. **Estimate within-cohort ES**, *no unit FE* (matching, so α_i only costs efficiency).

Aggregation (weights $\propto n_{1,c}$):

- **Main effect:** aggregate over cohorts.
- **Dynamics:** aggregate over *surviving* cohorts; flag exiting cohorts each period.
- **Pre-trends:** diagnose cohort-by-cohort, avoid aggregation for clarity.

Empirical Application

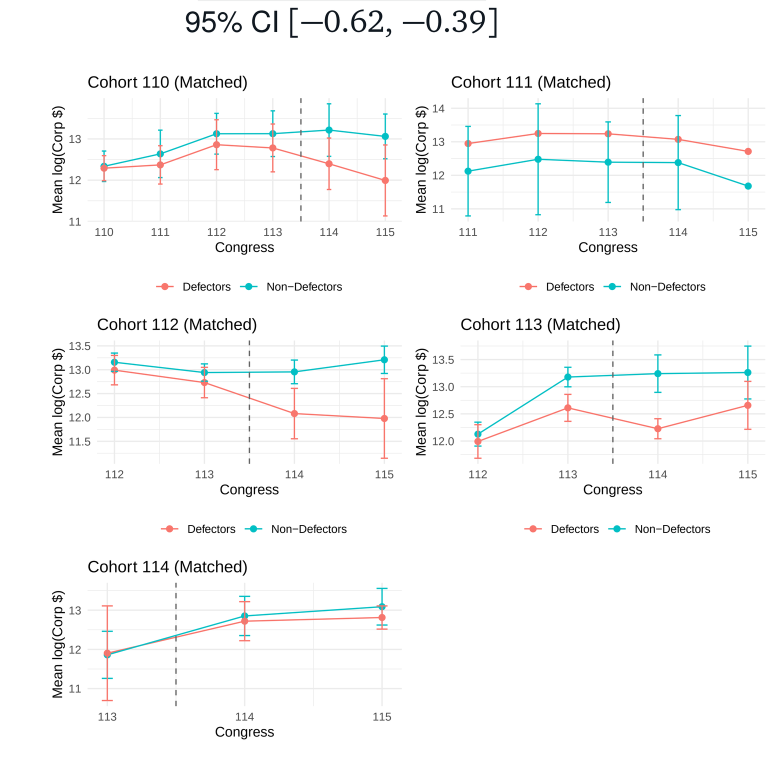
Q: Several Republicans voted against John Boehner's speaker election in 114th Congress. Did it cost them access to GOP leaders' corporate donors?

- $D = \mathbf{1}\{\text{defect}\}$; $Y = \text{log1p}(\$ \text{ from leaders' corporate donors})$; $t^* = 114$.
- **Imbalanced panel**: defectors first elected (enter panel) across 5 Congresses.
- **Approach**: within-cohort 1:2 PSM on pre-treatment fundraising, ideology, district vote-share; bootstrap entire procedure for ATT_{t^*} SE. (*Note:* ignoring dyn-effects here.)

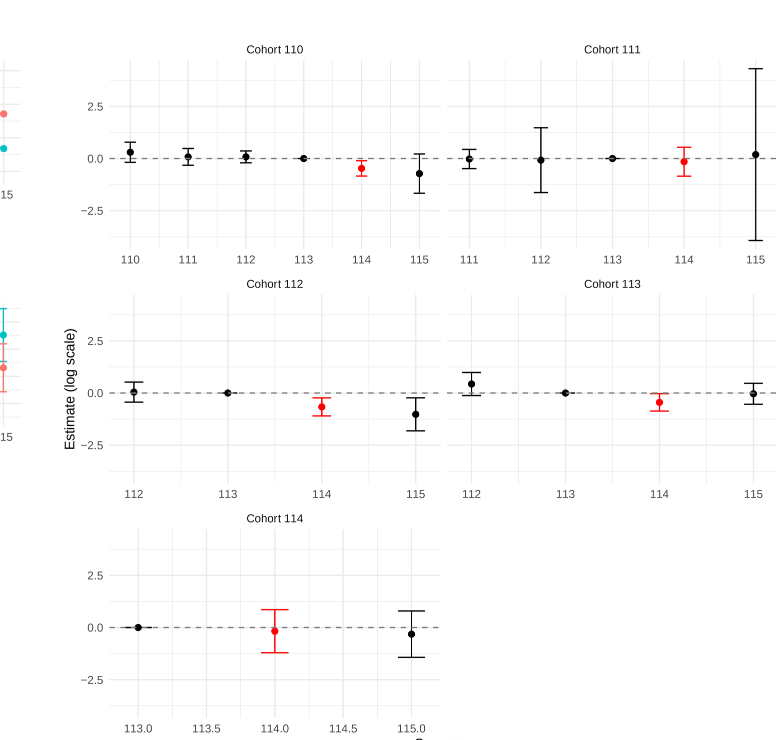
Cohort	$n_{1,c}$	ATT
110	6	−0.47
111	1	−0.15
112	6	−0.66
113	4	−0.45
114	3	−0.17
All	20	−0.46

95% CI $[-0.62, -0.39]$

- Defecting ⇒ **∼37% loss** in \$ from leaders' corporate network.
- Effects **heterogeneous** across cohorts (-0.17 to -0.66), the phantom bias condition.
- Within-cohort diagnostics show **clean pre-trends**:



Matched cohort trajectories



Within-cohort event studies (no unit FE)