# Causal Panel Analysis under Parallel Trends:

## Lessons from a Large Reanalysis Study

February 2025

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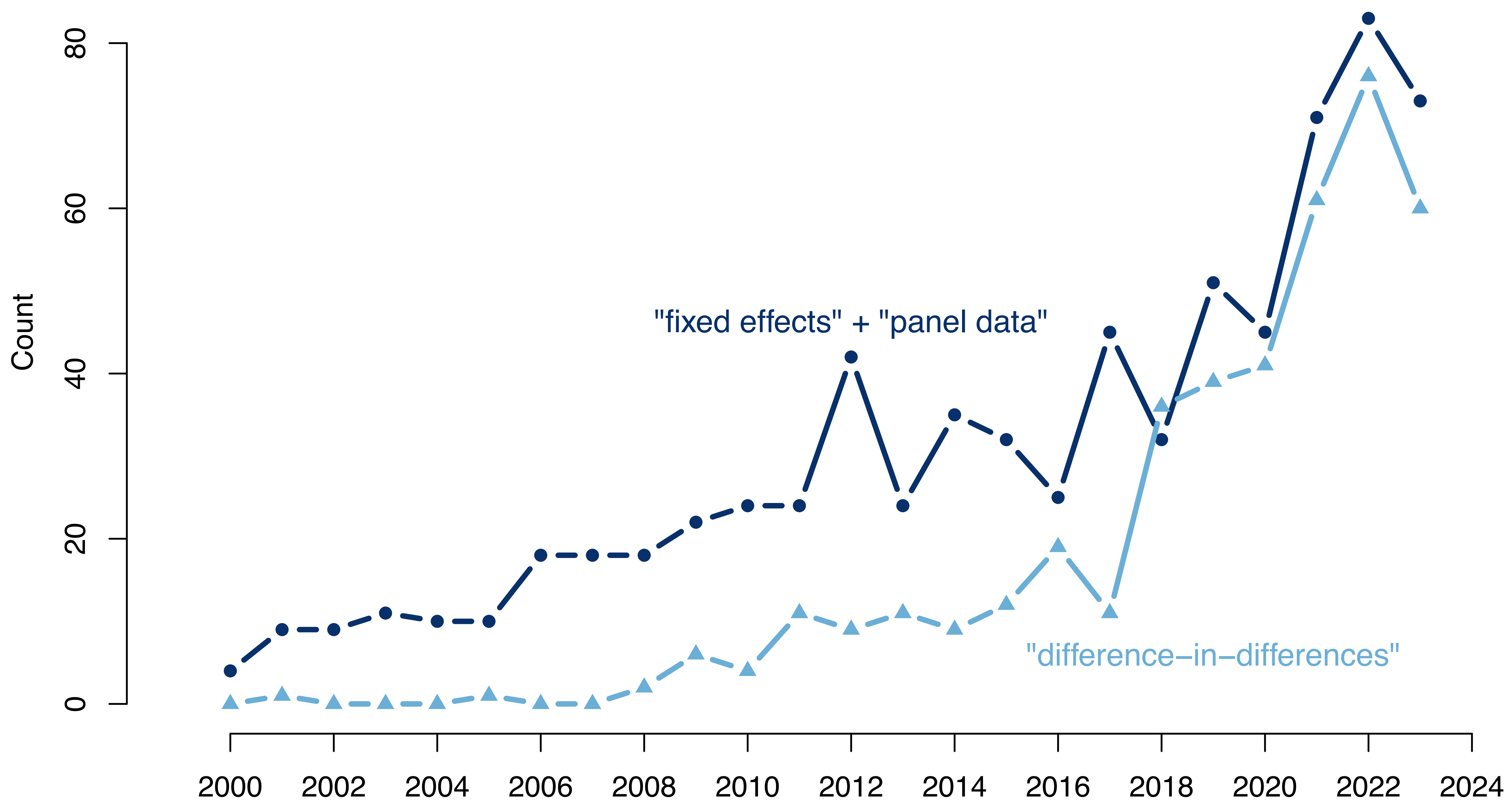


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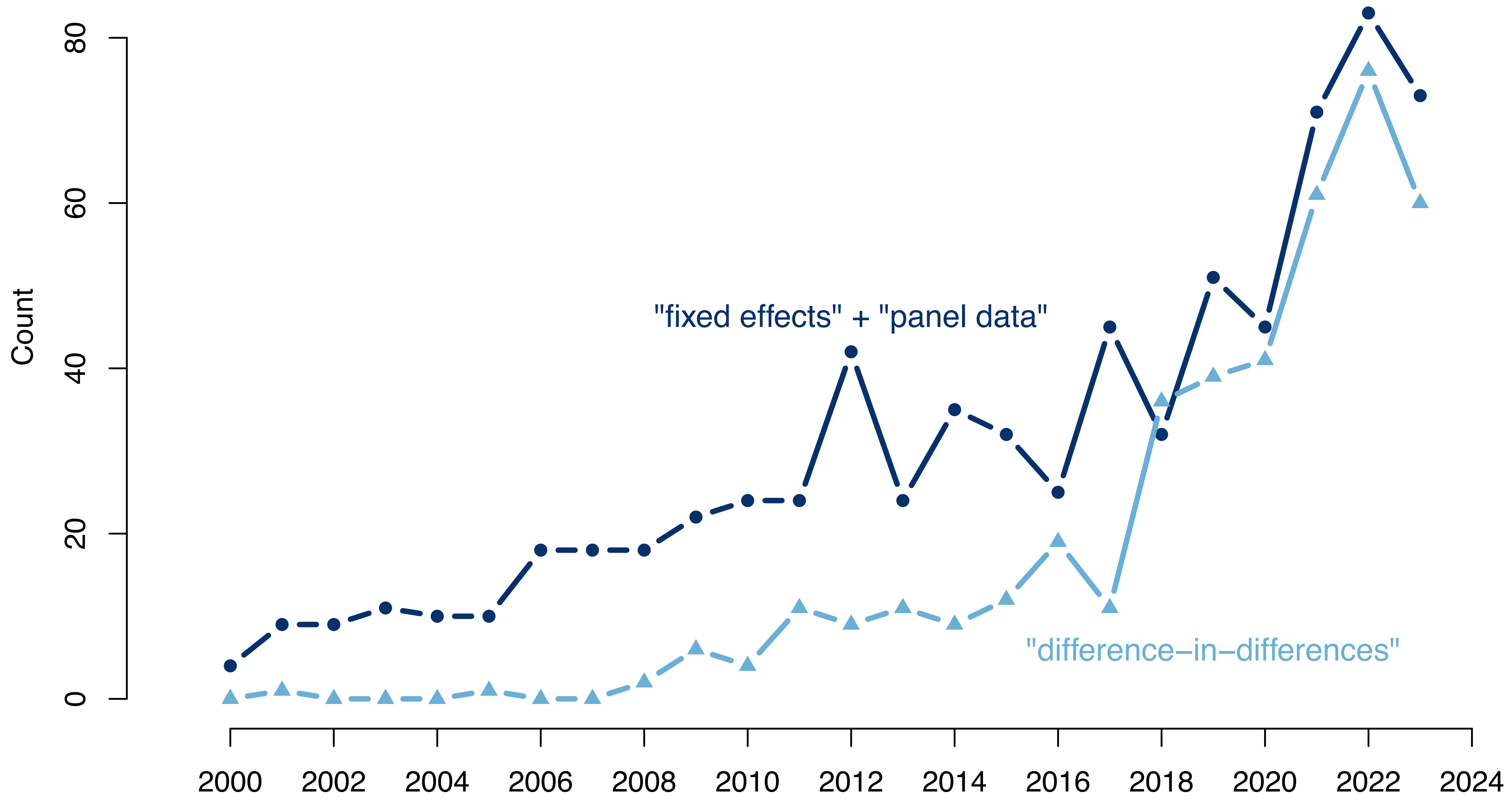


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    ➡ Many new estimators have been proposed...

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| Functional Form | $Y_{it} = \delta^{TWFE} D_{it} + X_{it}'\beta + \alpha_i + \xi_t + \epsilon_{it}$ |
| Strict Exogeneity | $D_{it} \perp\!\!\!\perp \epsilon_{js} \,\vert\, \mathbf{X}^{1:T}, \alpha, \xi^{1:T}, \quad \forall i, j, t, s$ |

**Related work:** Blackwell & Glynn (2018); Imai & Kim (2019); Athey & Imbens (2022); Liu, Wang & Xu (2022)

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  - Additive unobserved confounding
  - No "feedback"
- On interference (SUTVA)
  - No spatial spillover
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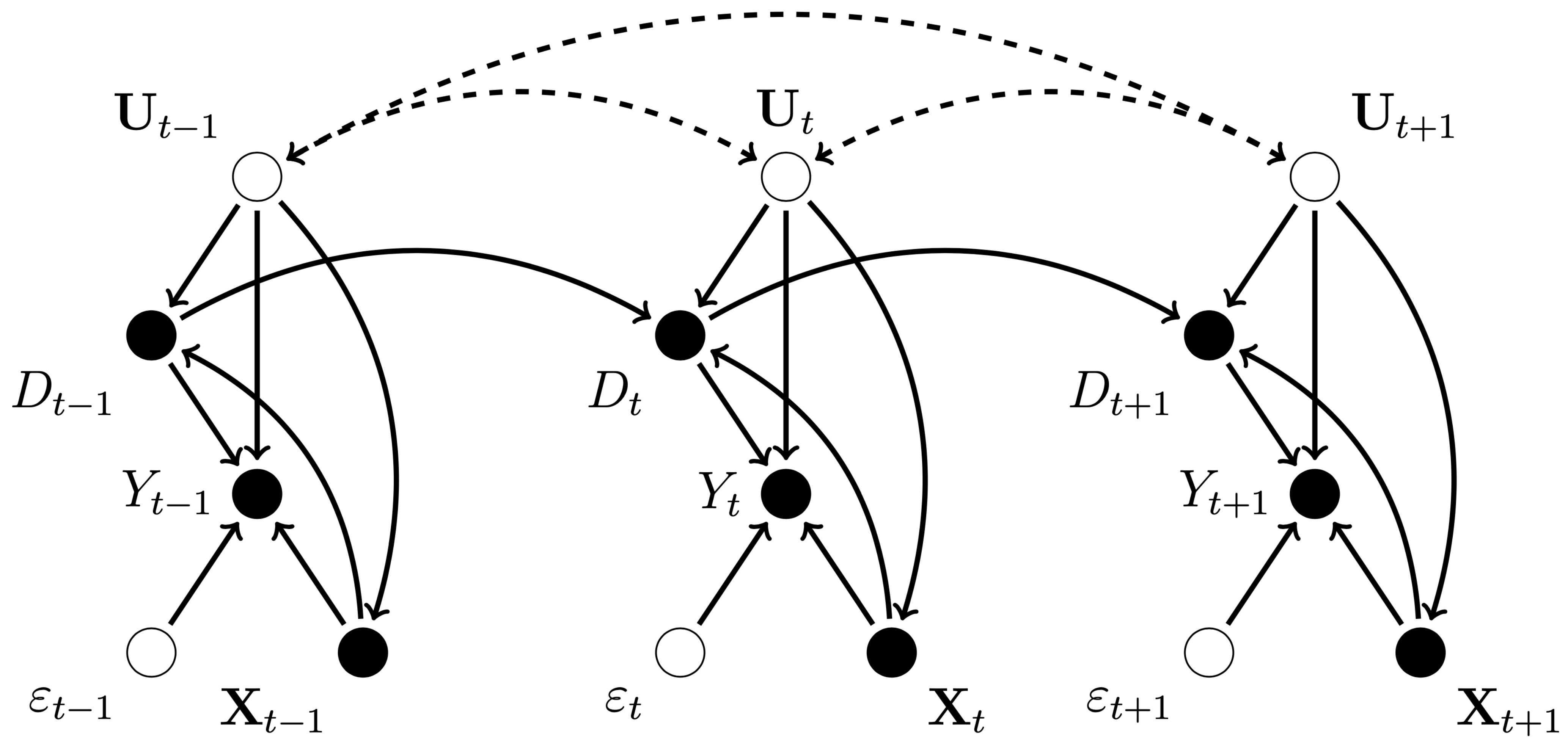


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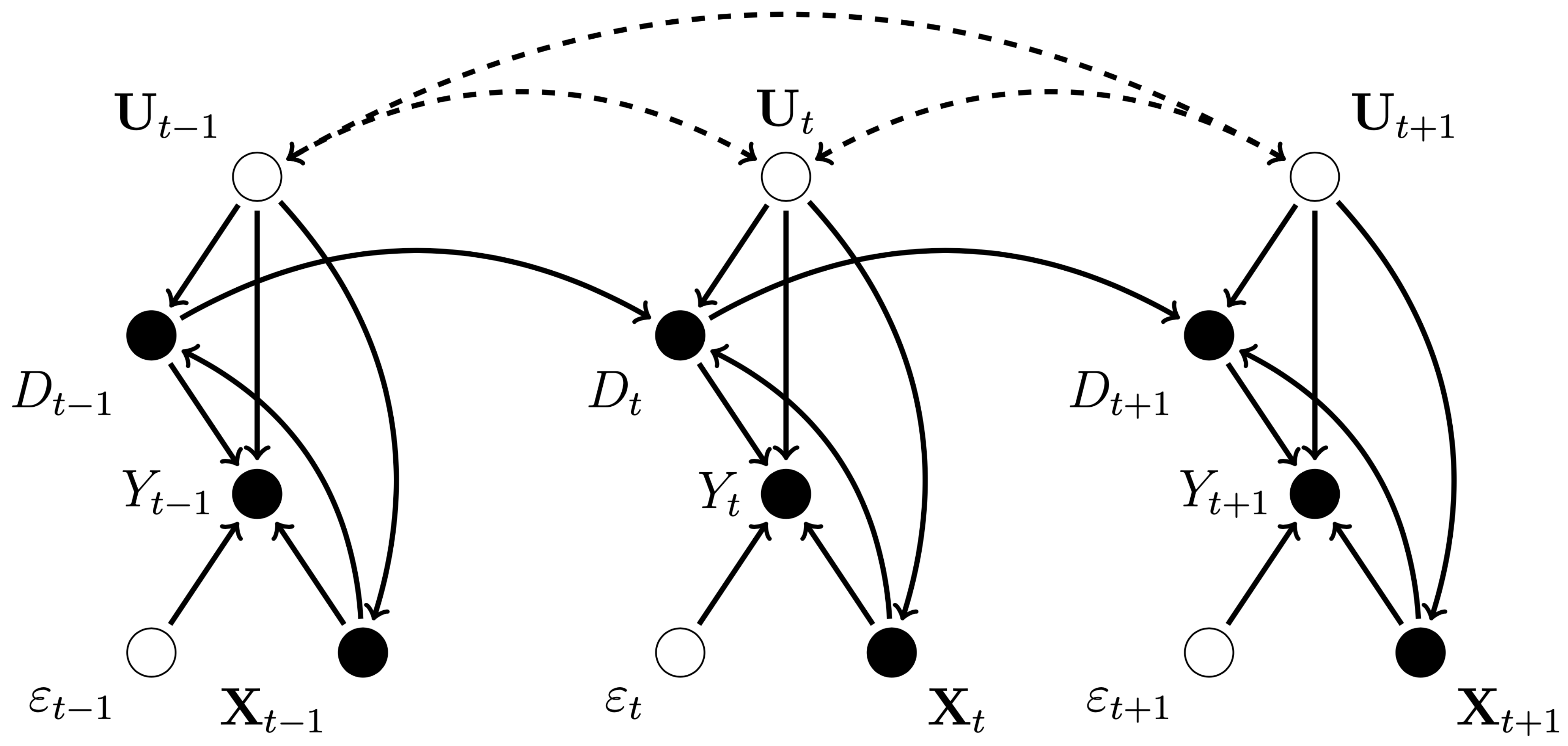


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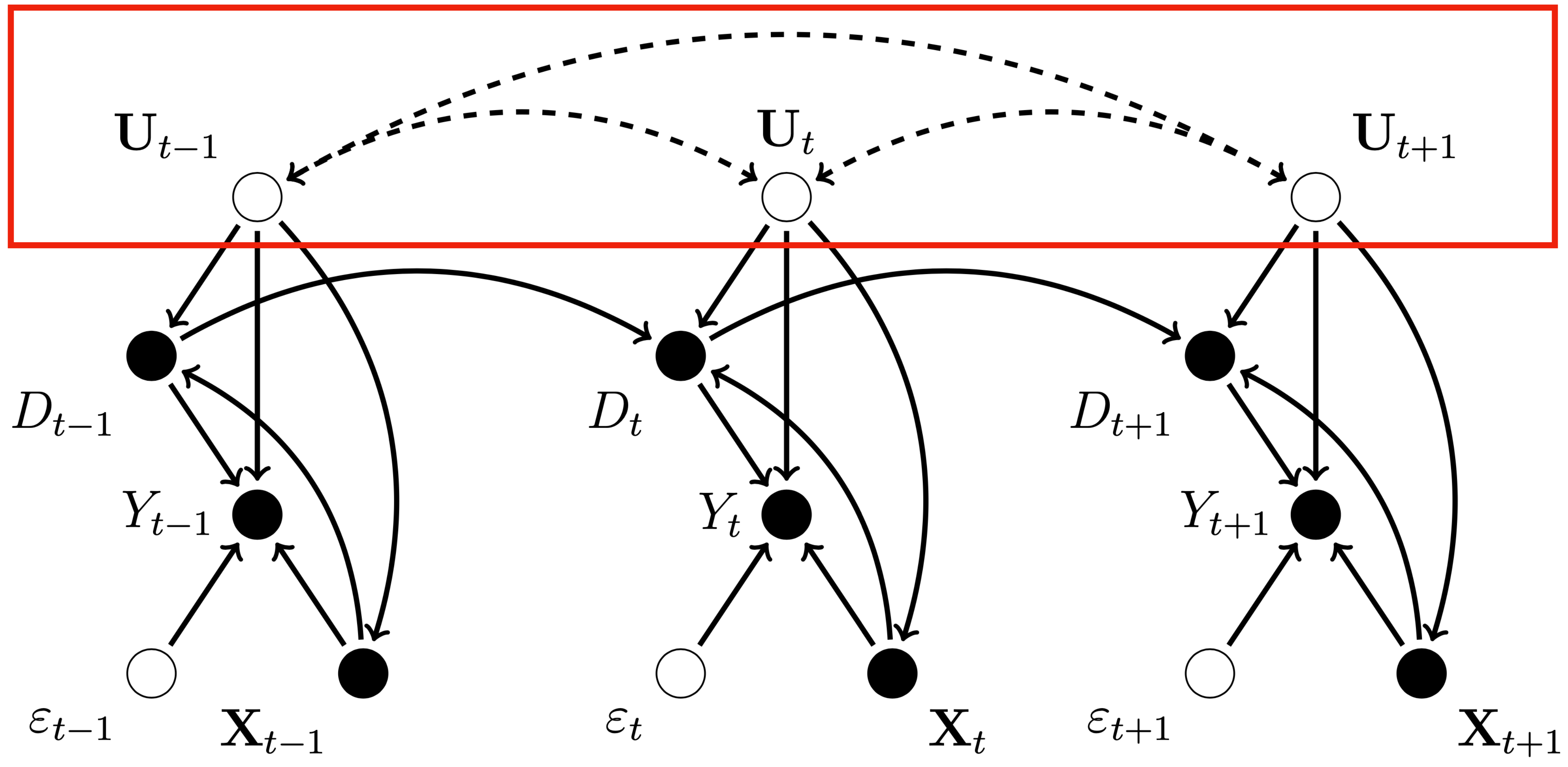


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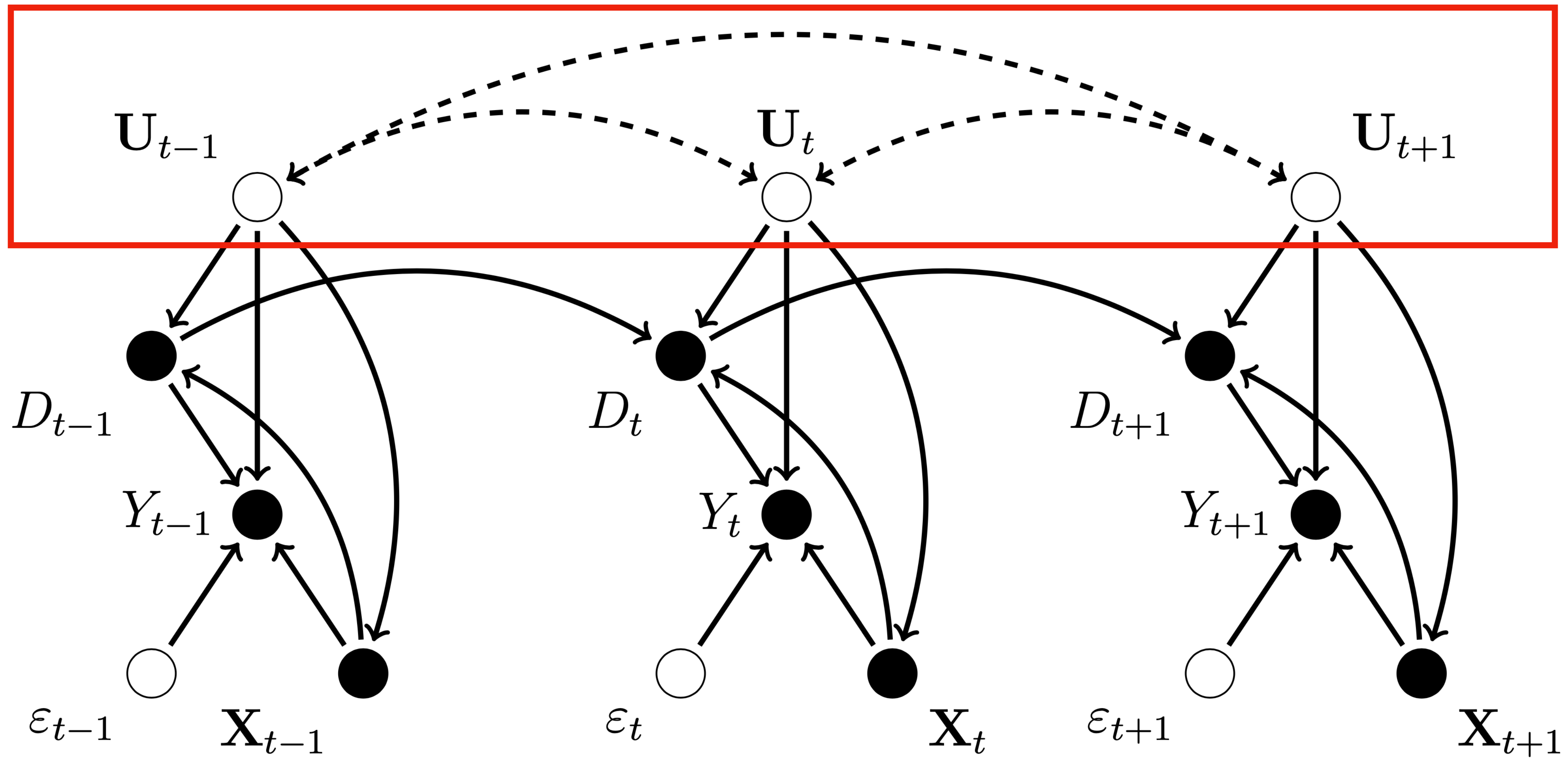


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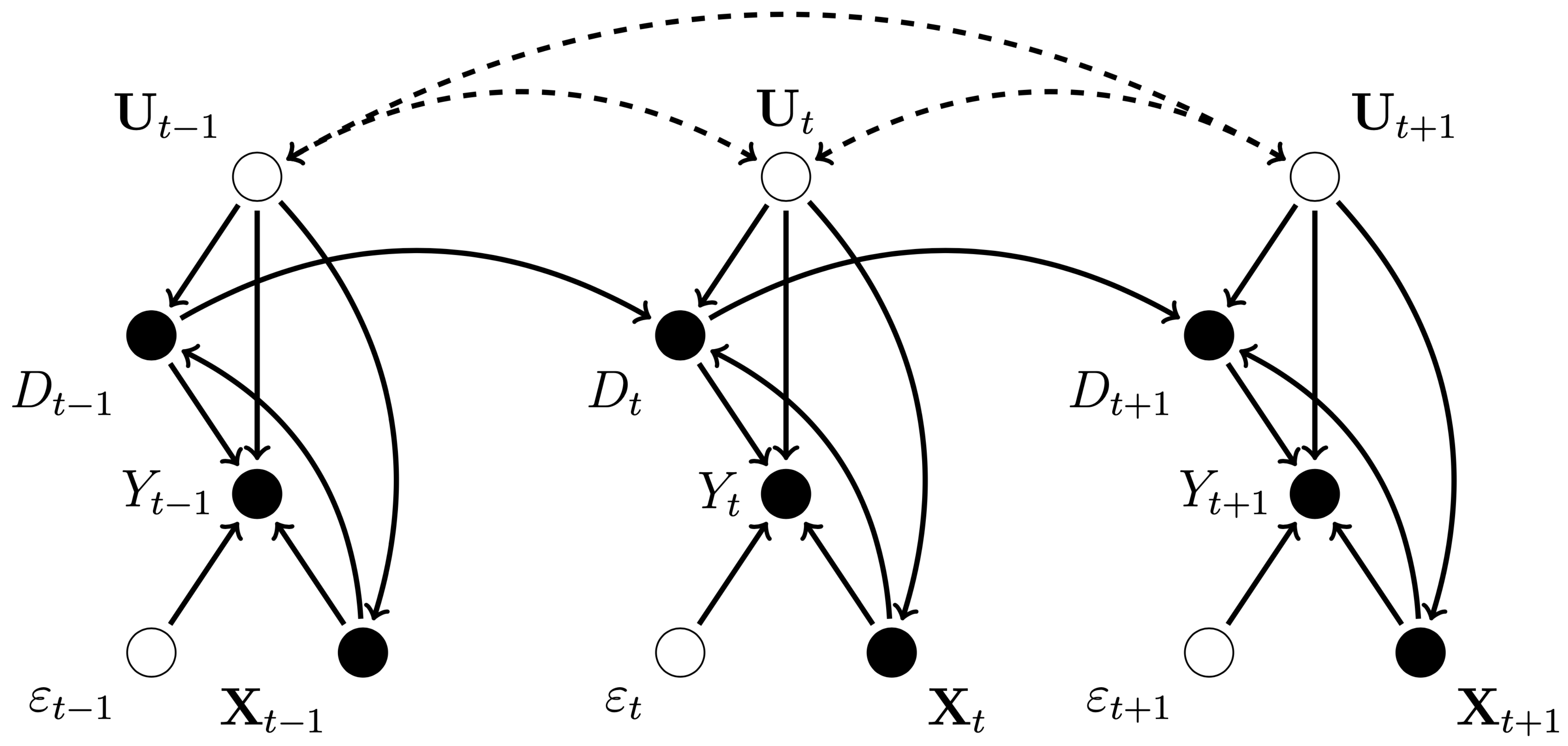


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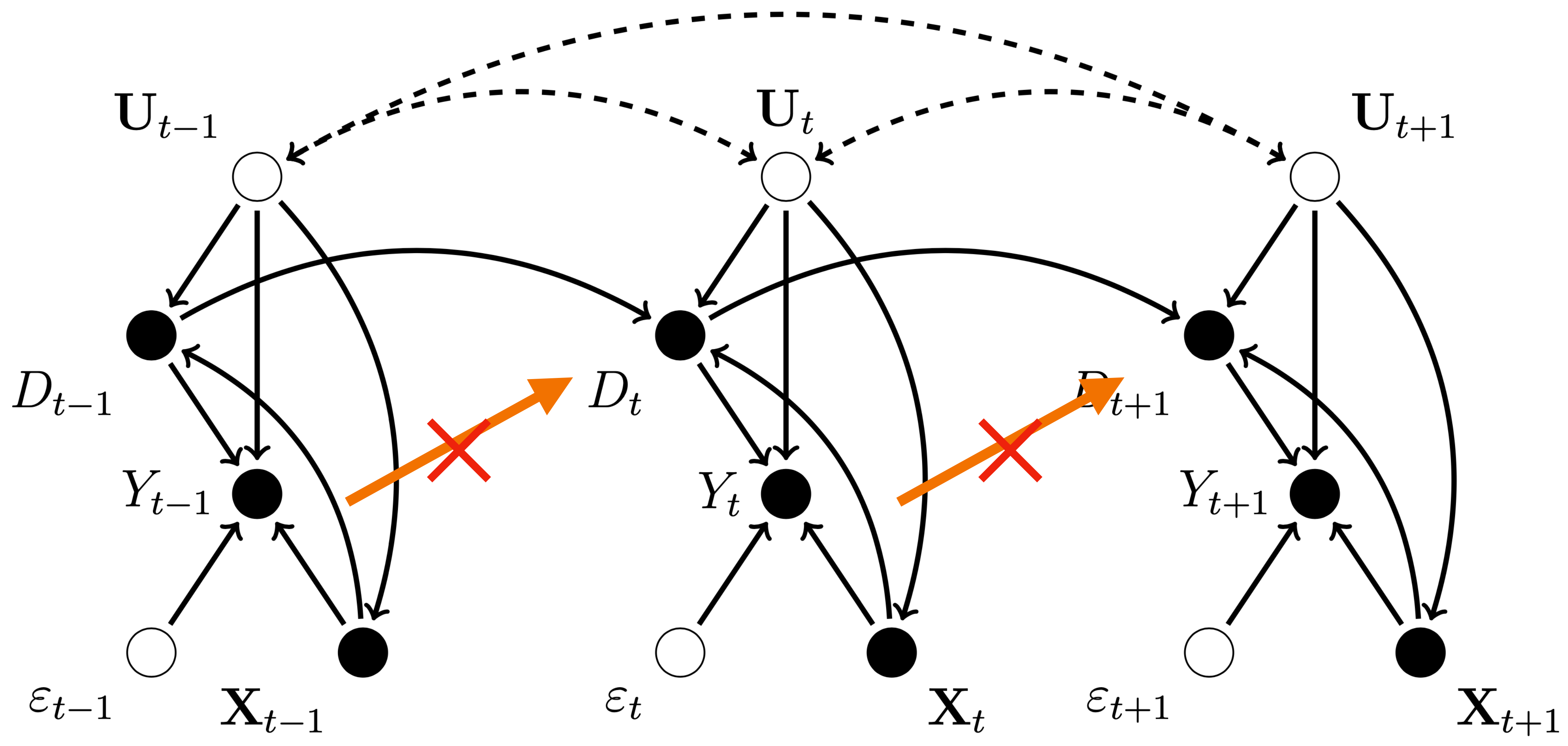


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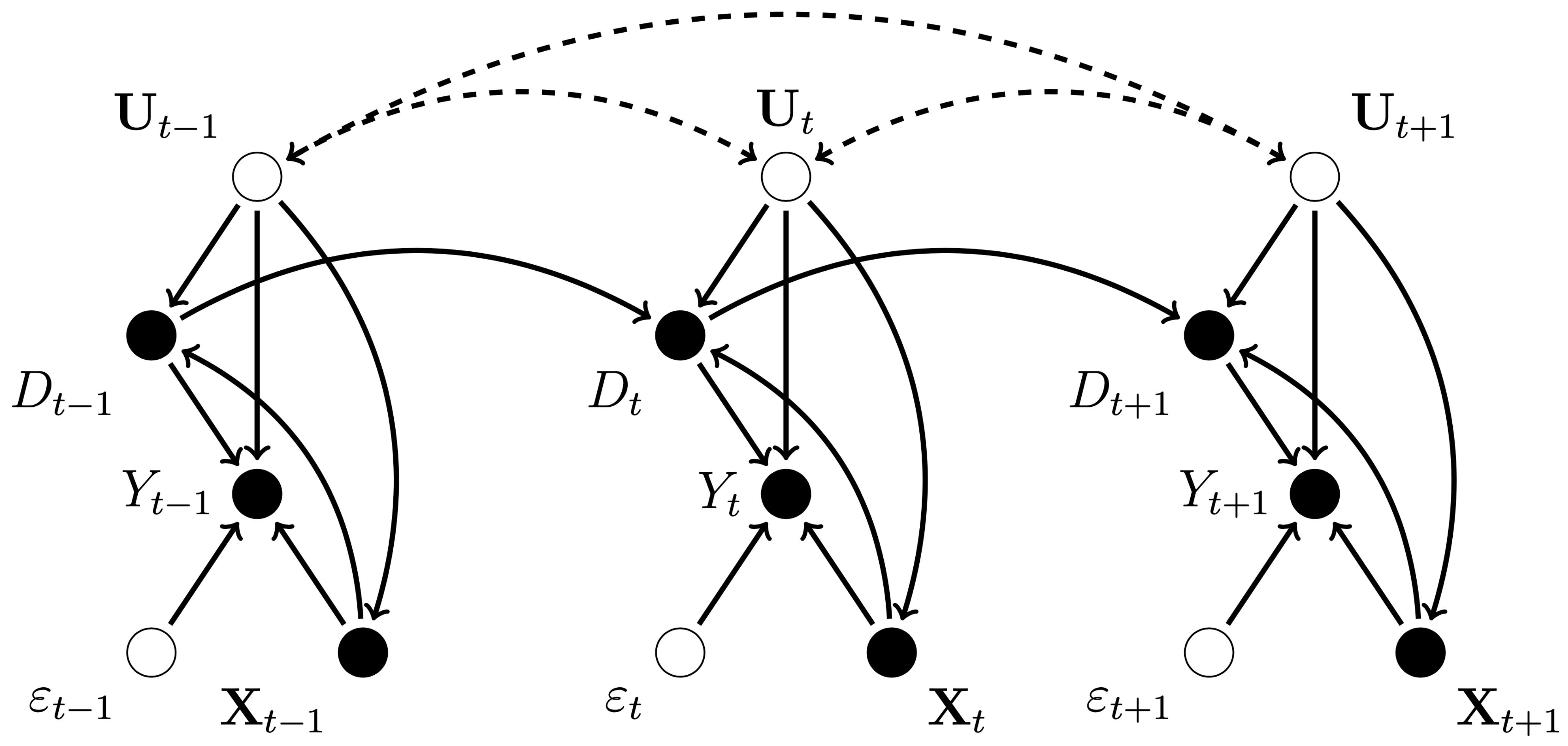


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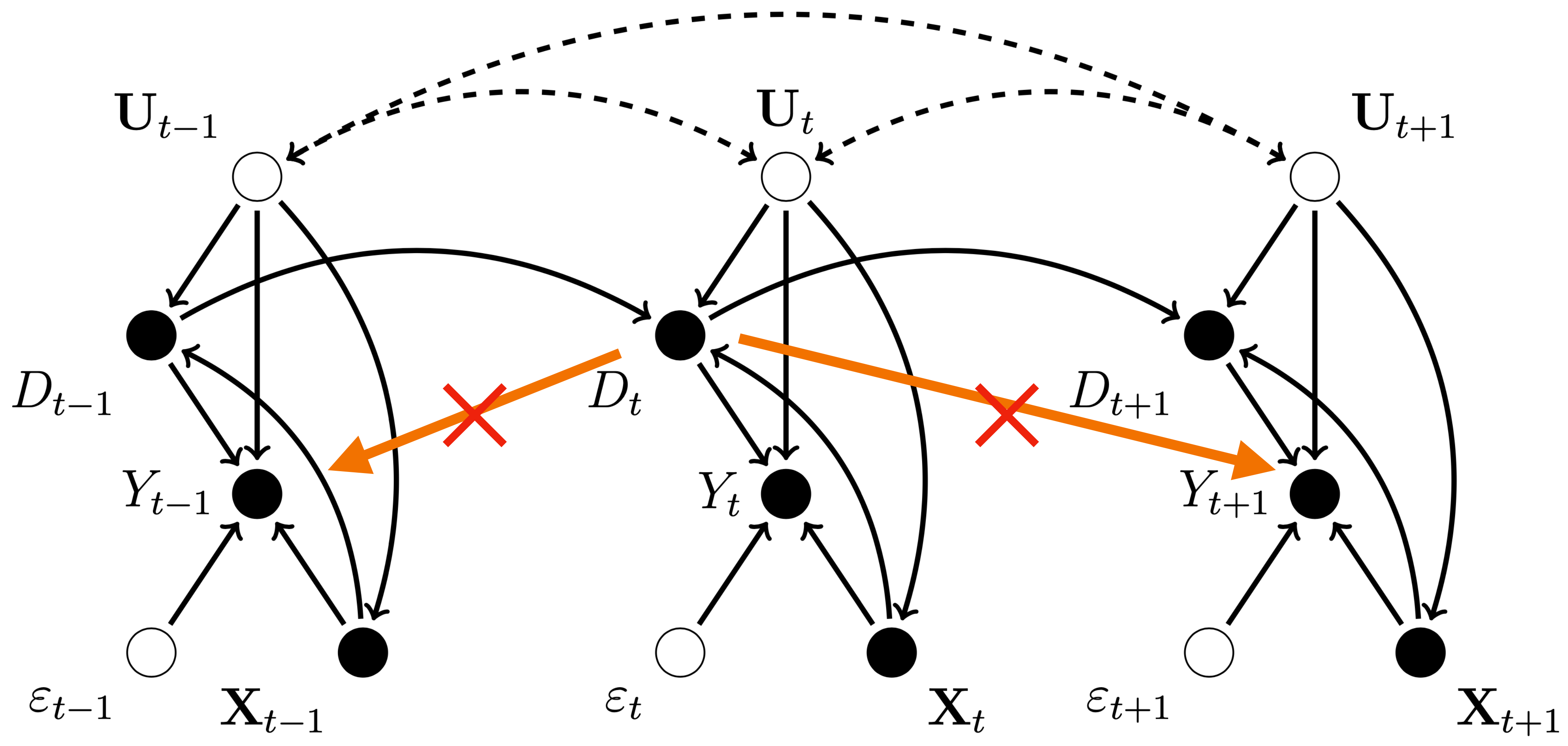


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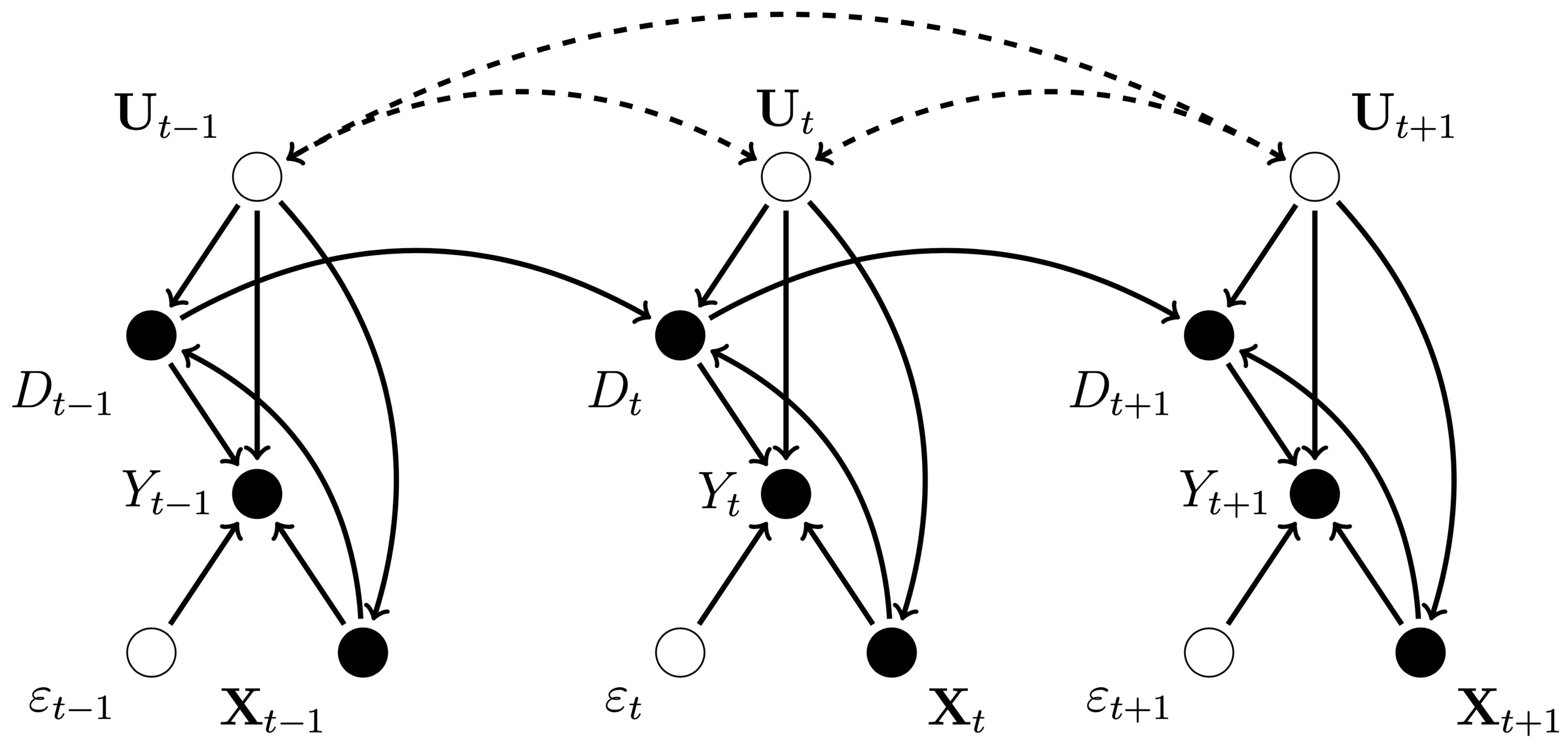


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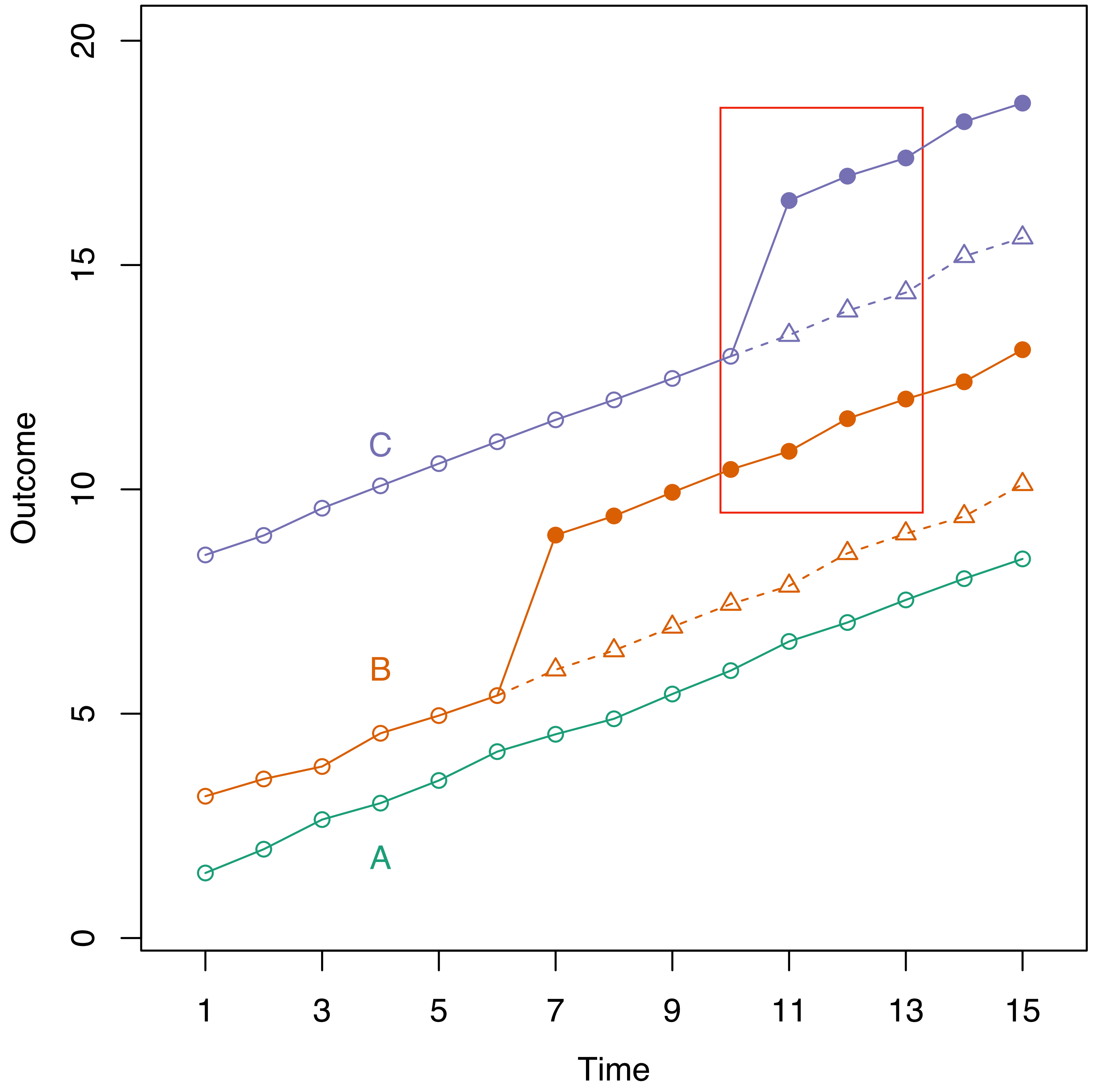


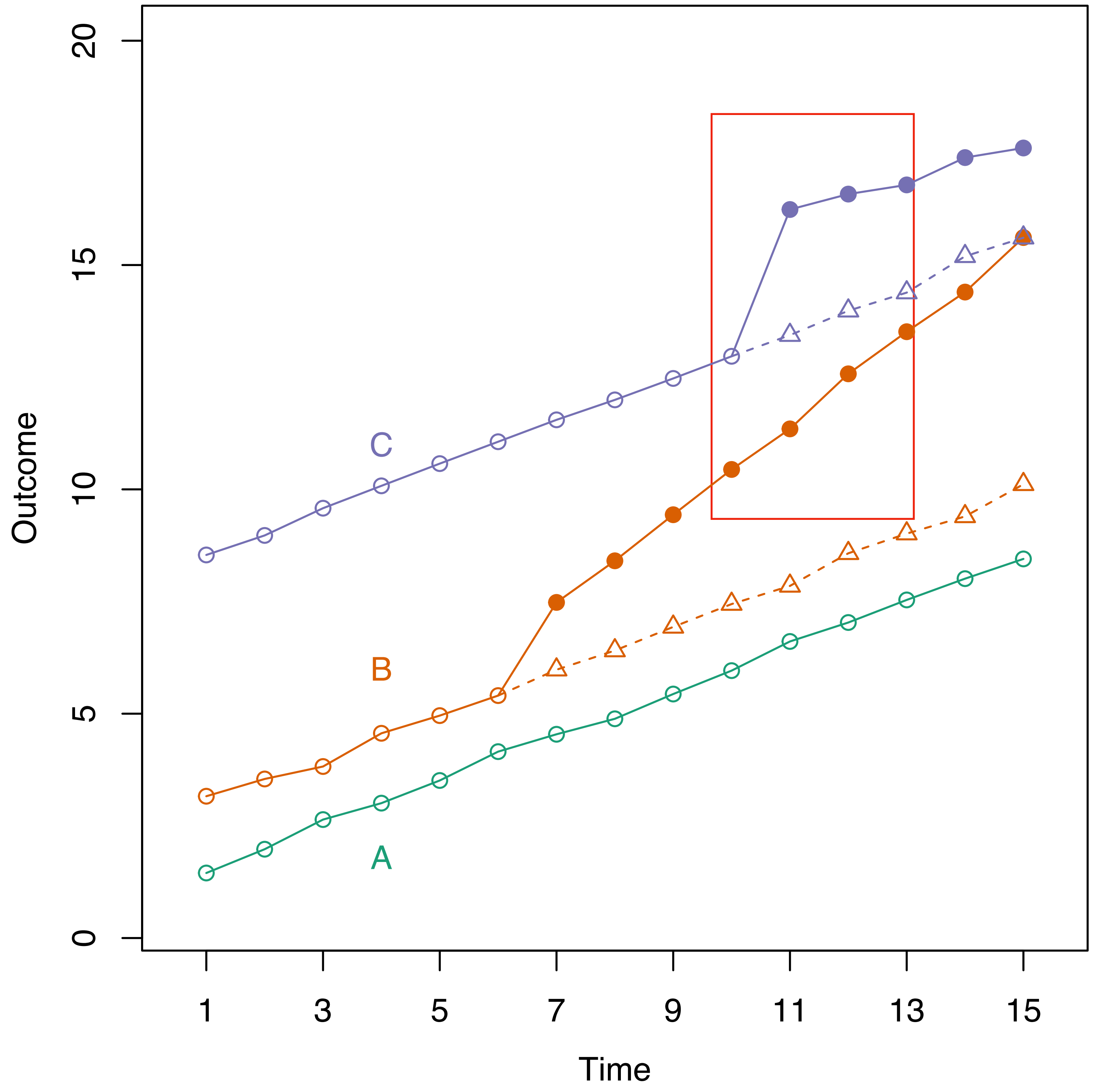

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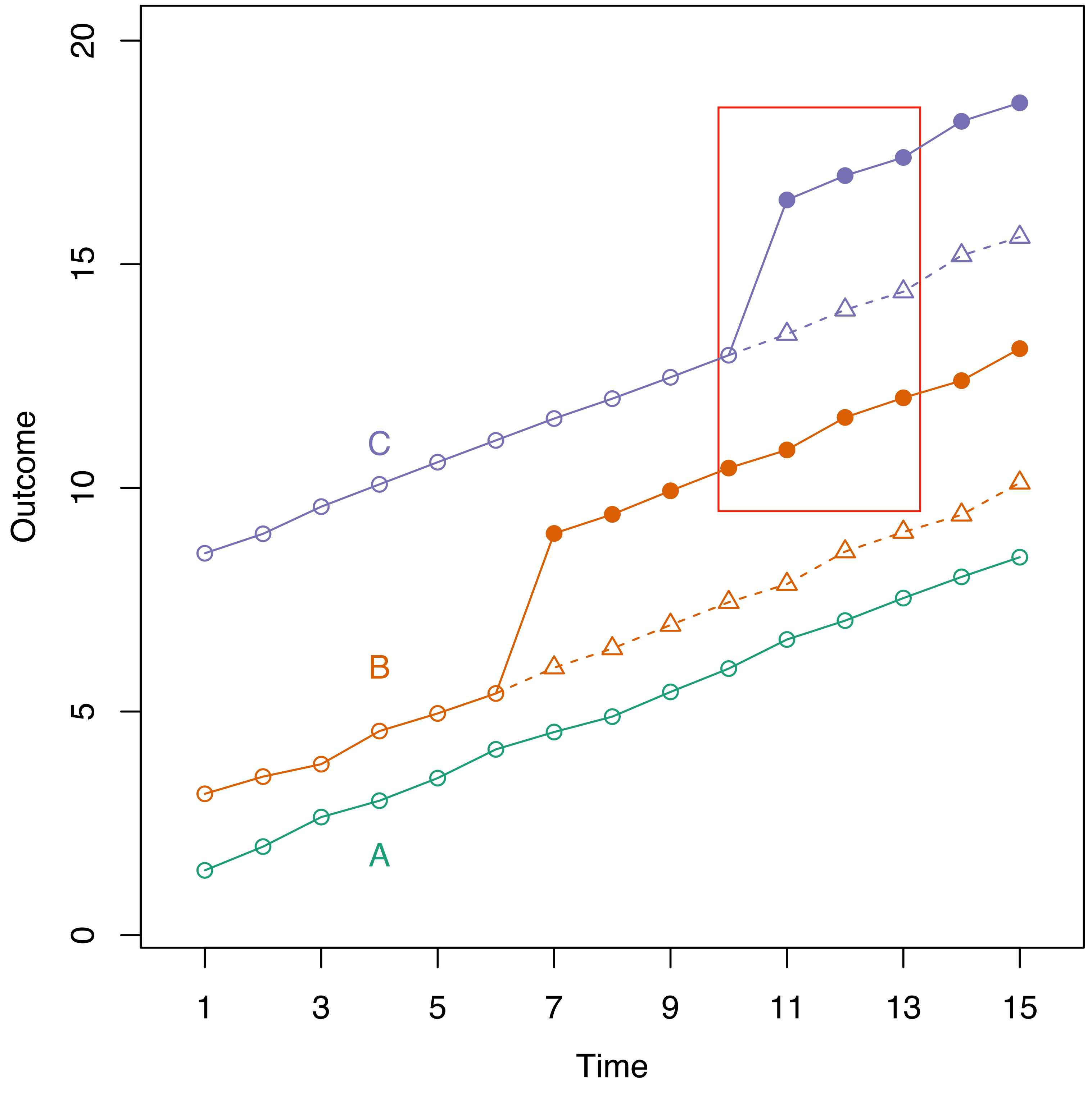
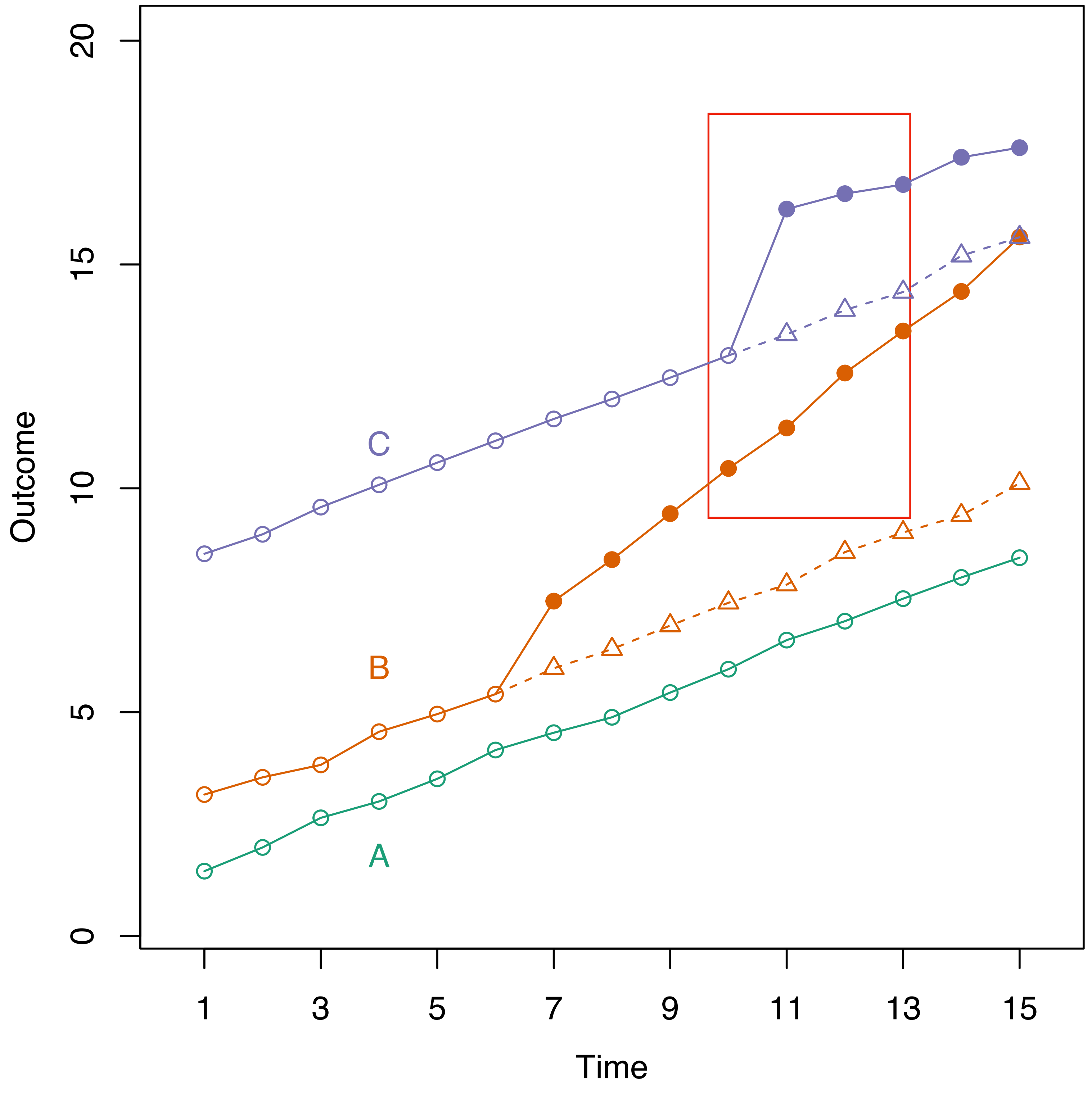
- **Complexity:** How important this issue is depends on many factors



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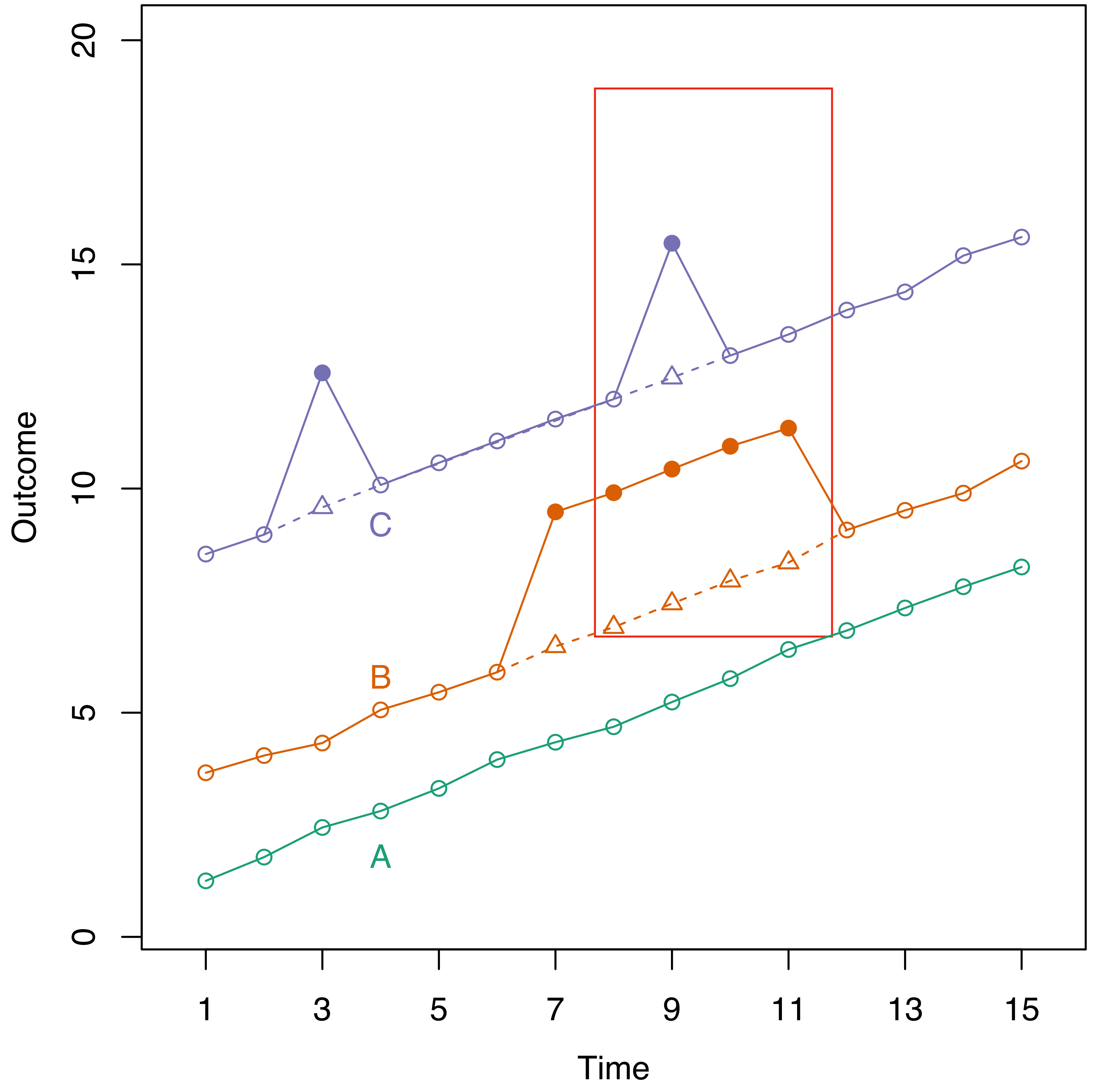

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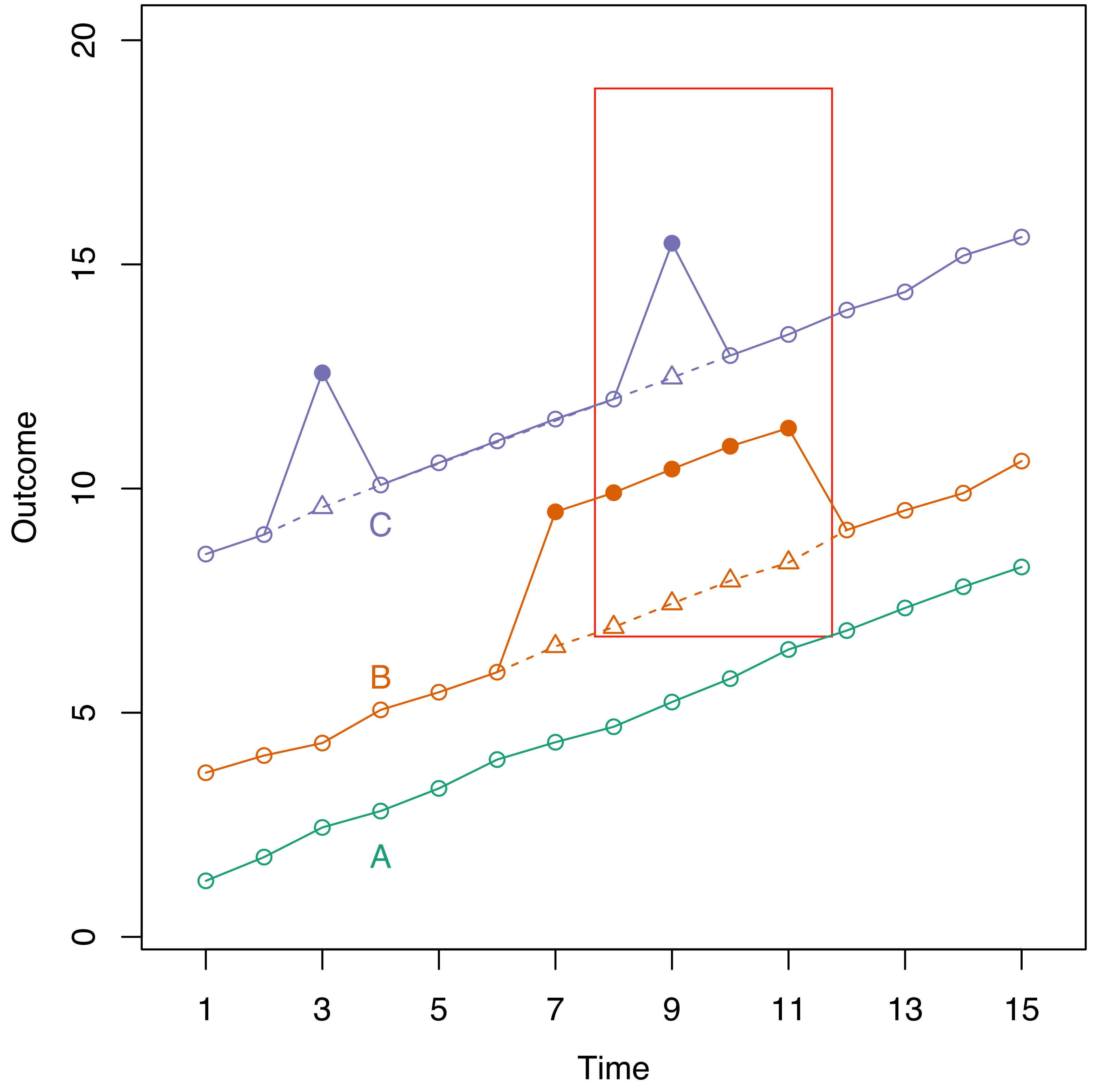

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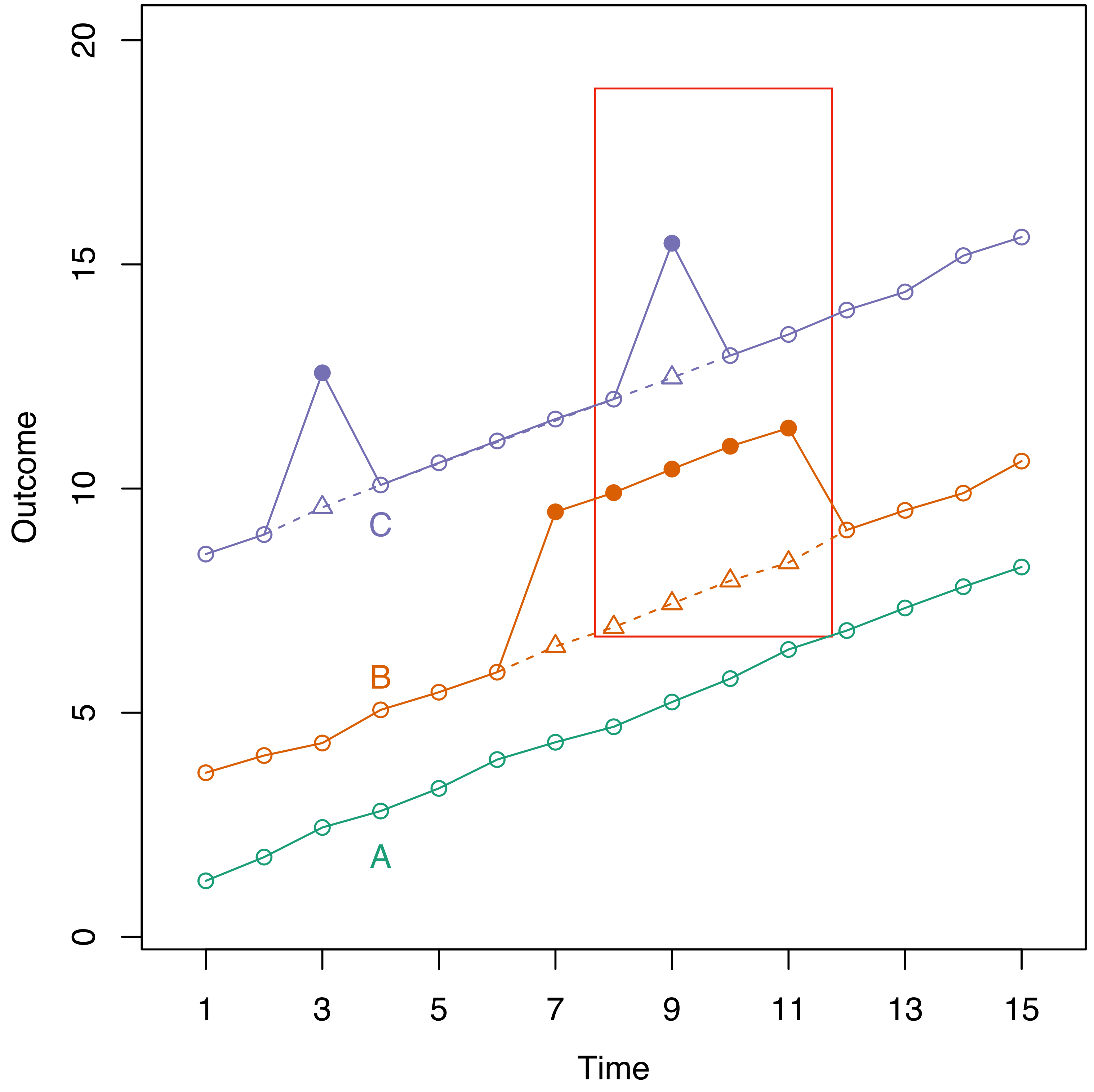

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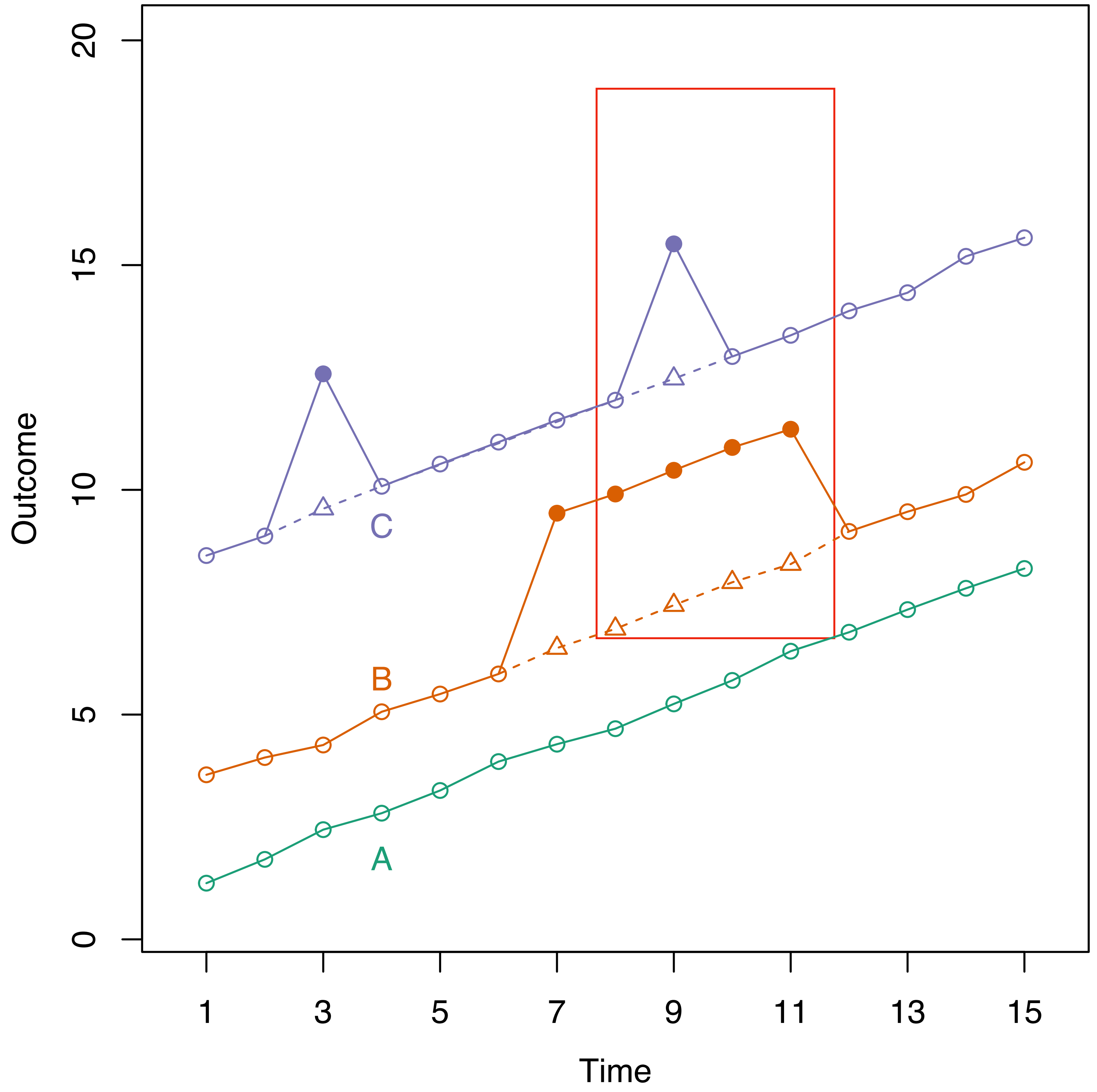


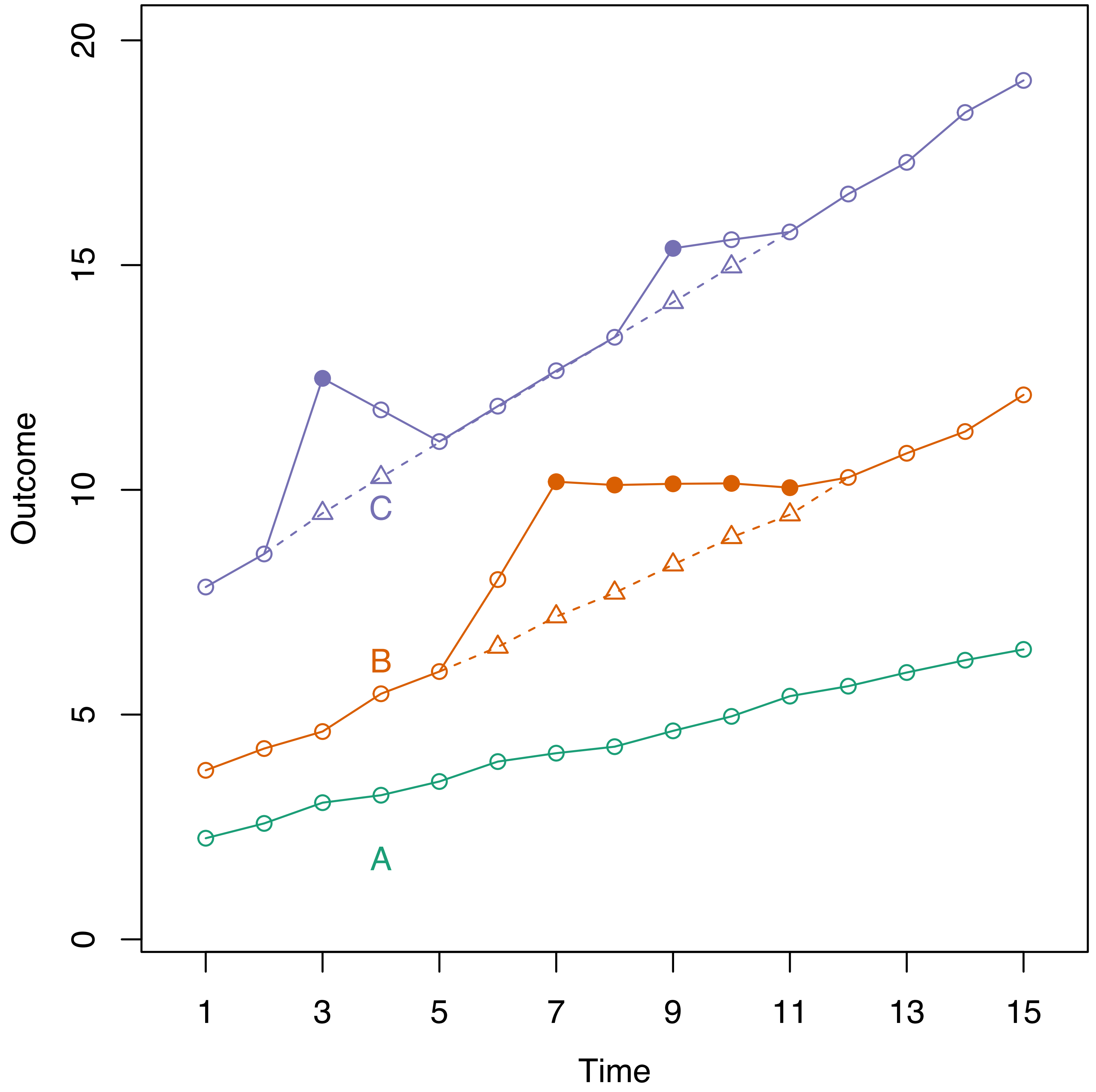

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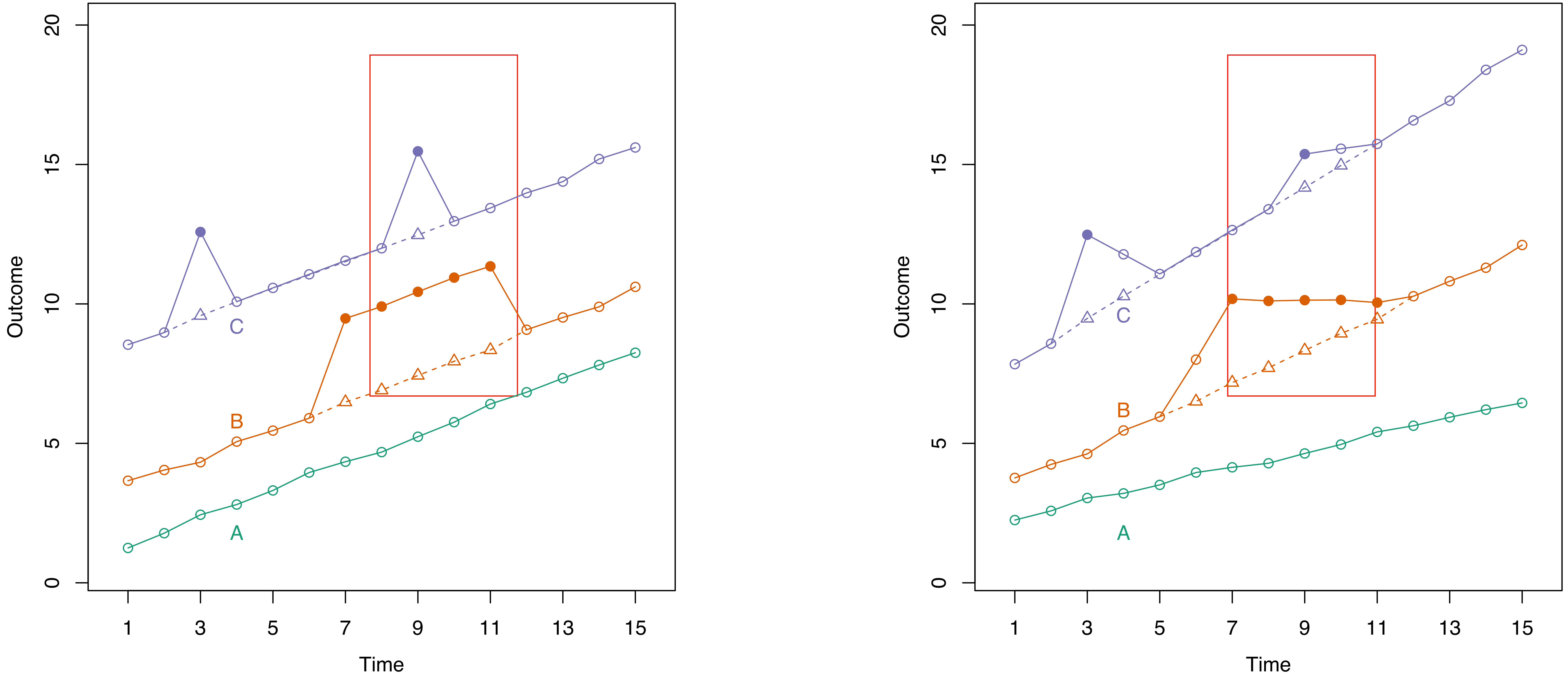

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  - Sensitivity analysis is helpful
- "Robust" DID requires a strong design and a lot of power

# Related Literature

- Review articles: Roth et al. (2023), Xu (2023), Arkhangelsky and Imbens (2023)
  - New diagnostic and estimation strategies not applied to data
  - Difficult to assess their relevance to empirical research

- Replication studies: Baker et al. (2022)
  - Replicated five economics and finance studies with staggered treatments
  - Focused on the consequence of HTE

# [roadmap]

- Estimators
  - Review 6 HTE-robust estimators
  - Typology & comparison
- Data and Procedure
  - Sample
  - Procedure
- Findings
  - Three examples
  - Overall assessment
- Recommendations

# Methods

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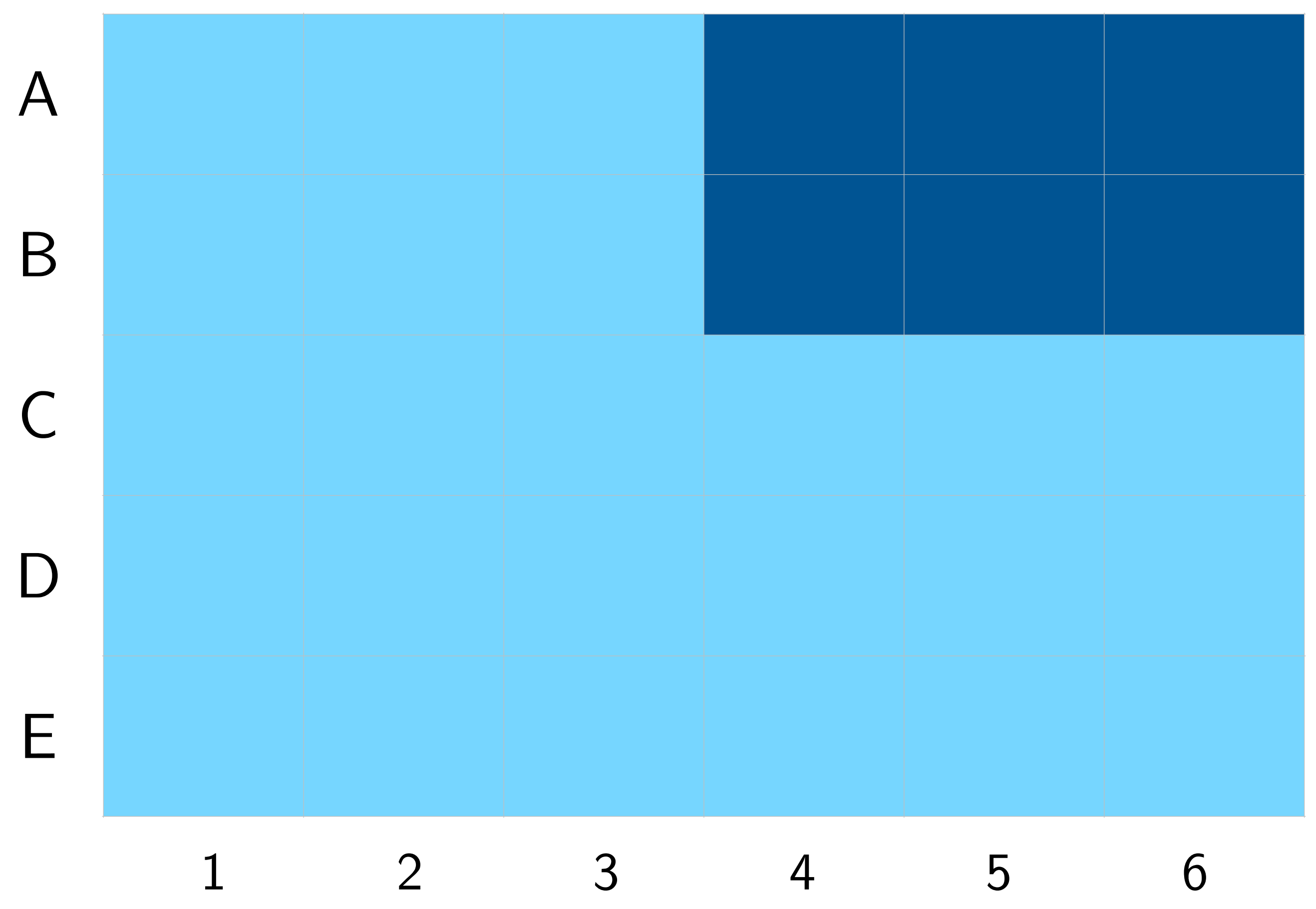


(Multi-Period) Block DID Setting

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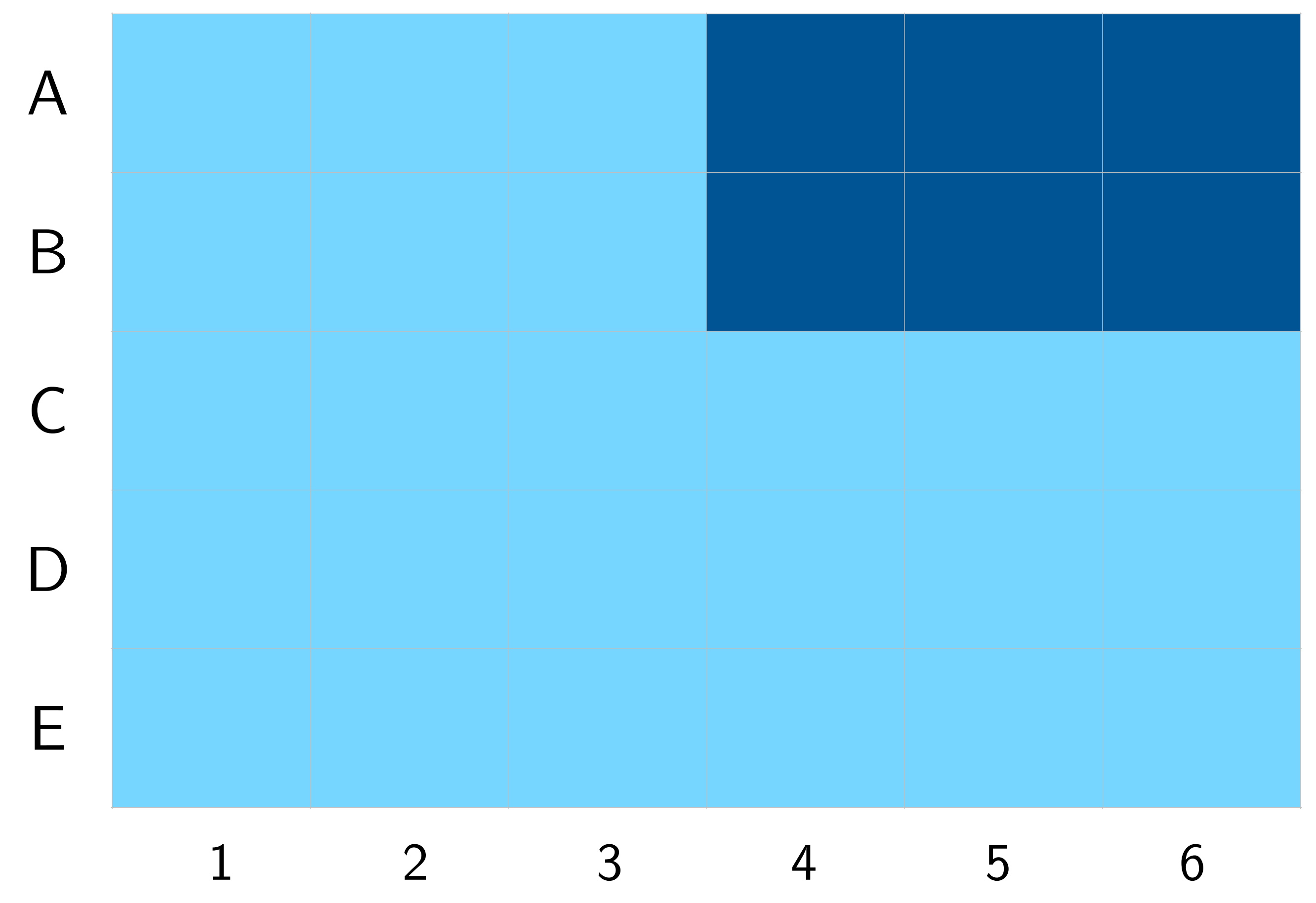


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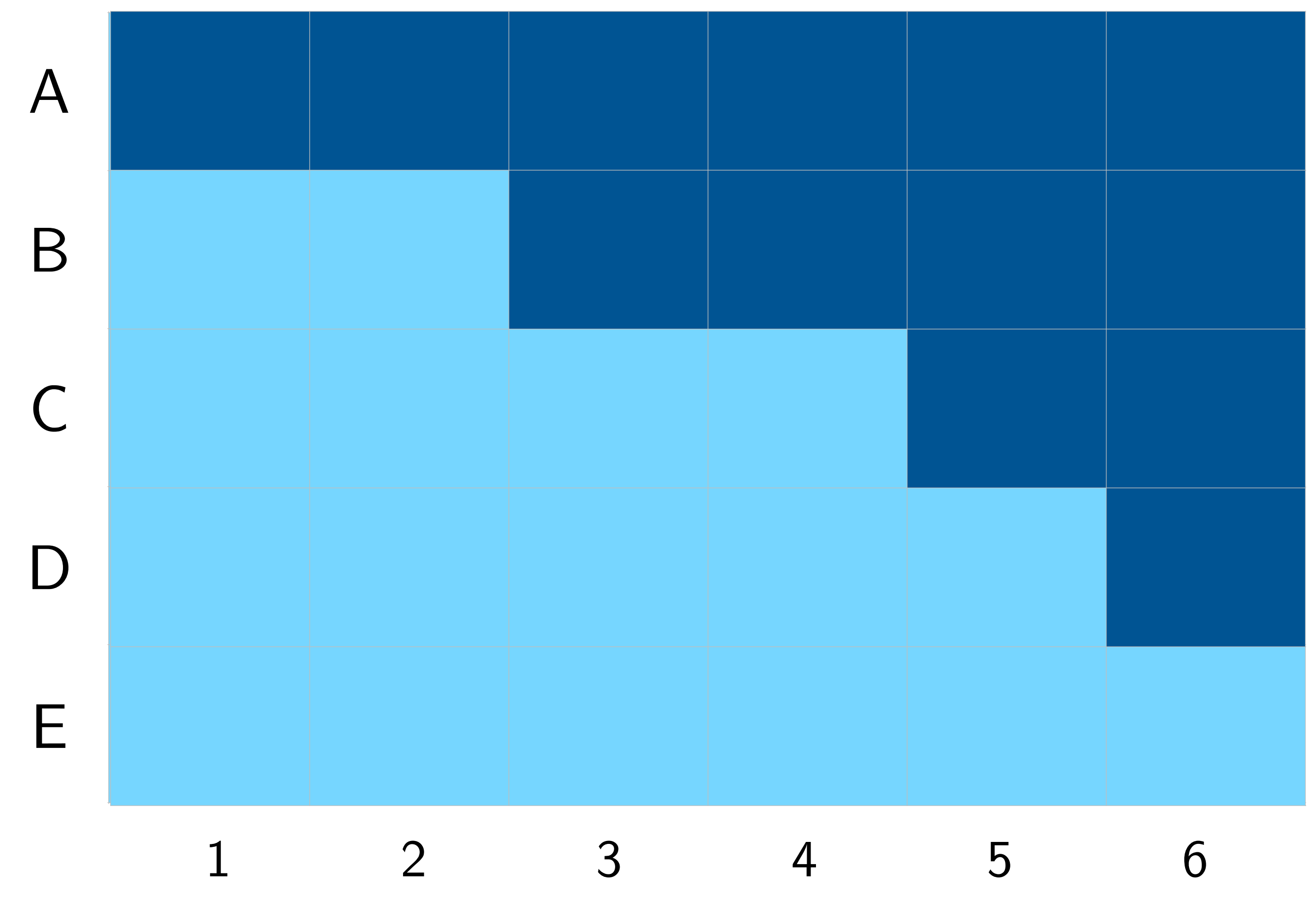
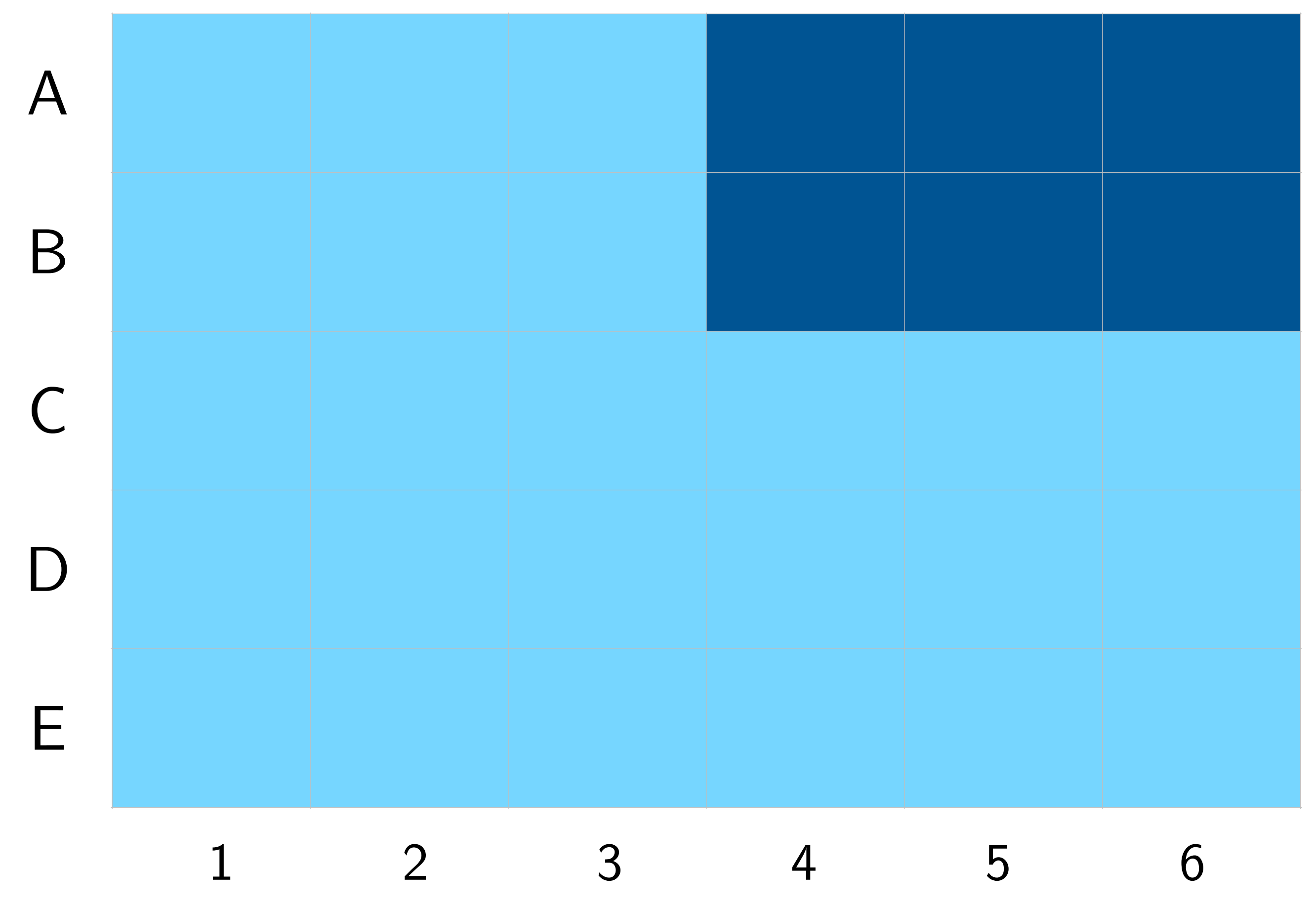



Staggered DID Setting

# Settings



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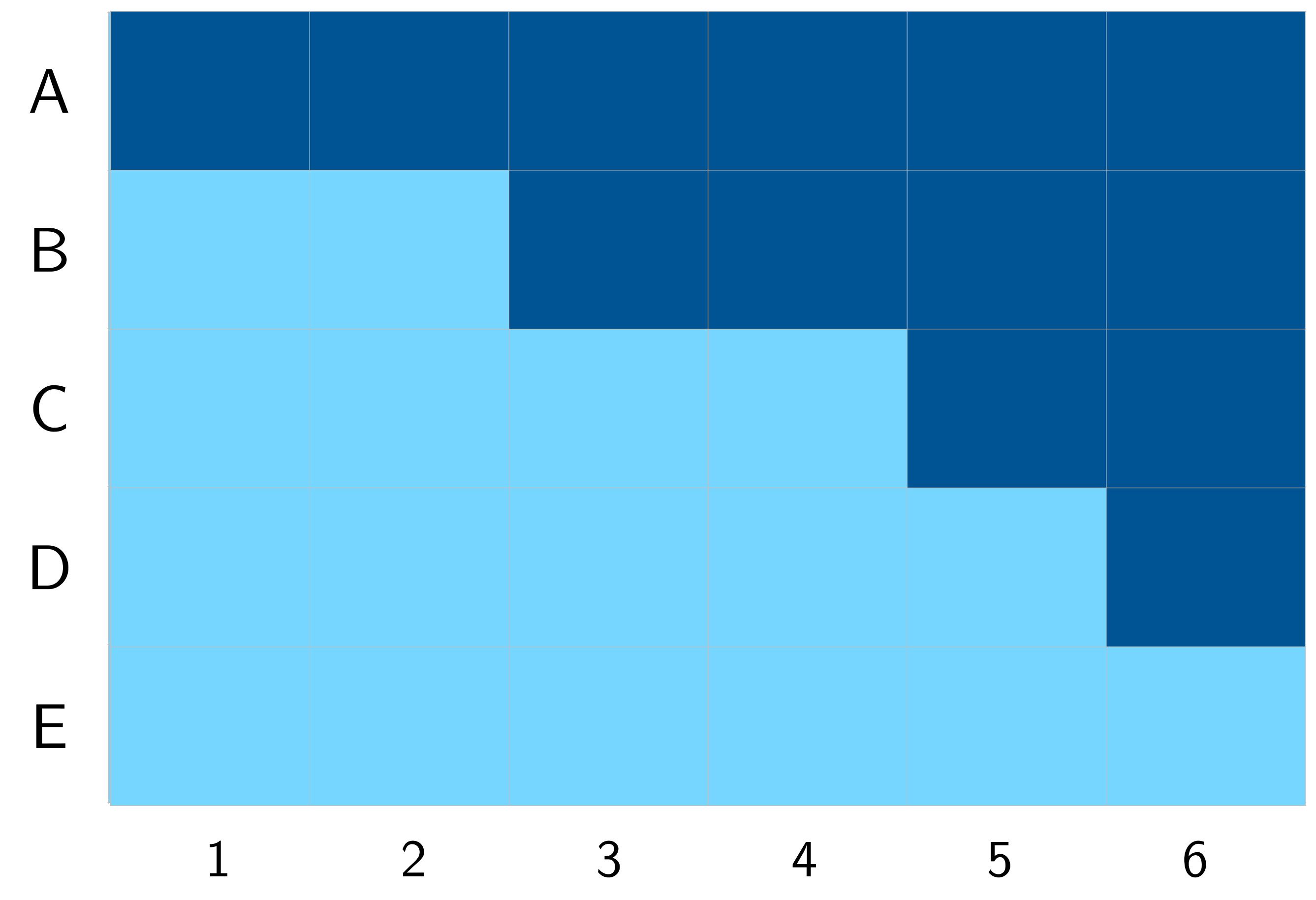

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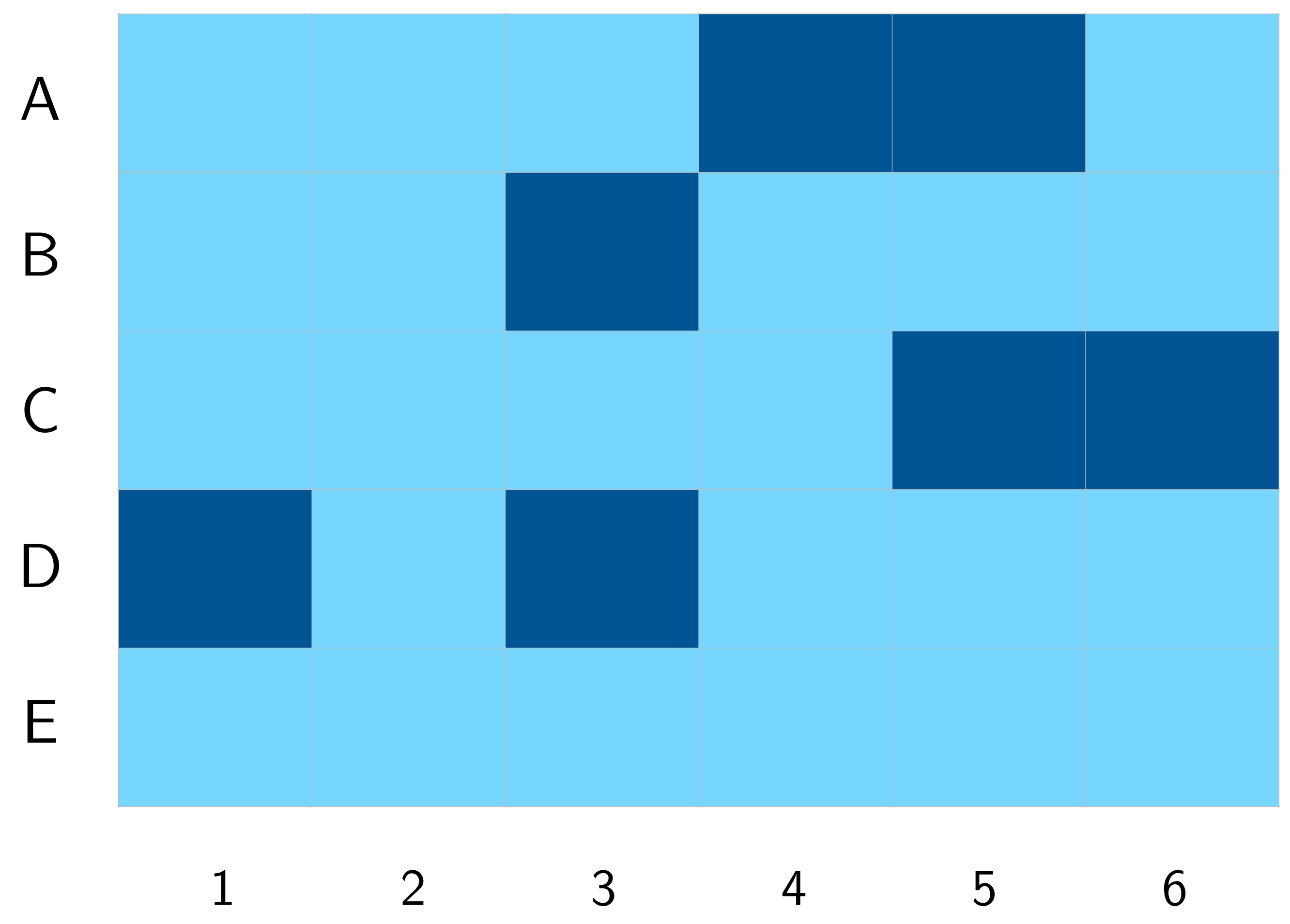

General Setting
(w/ Treatment Reversal)

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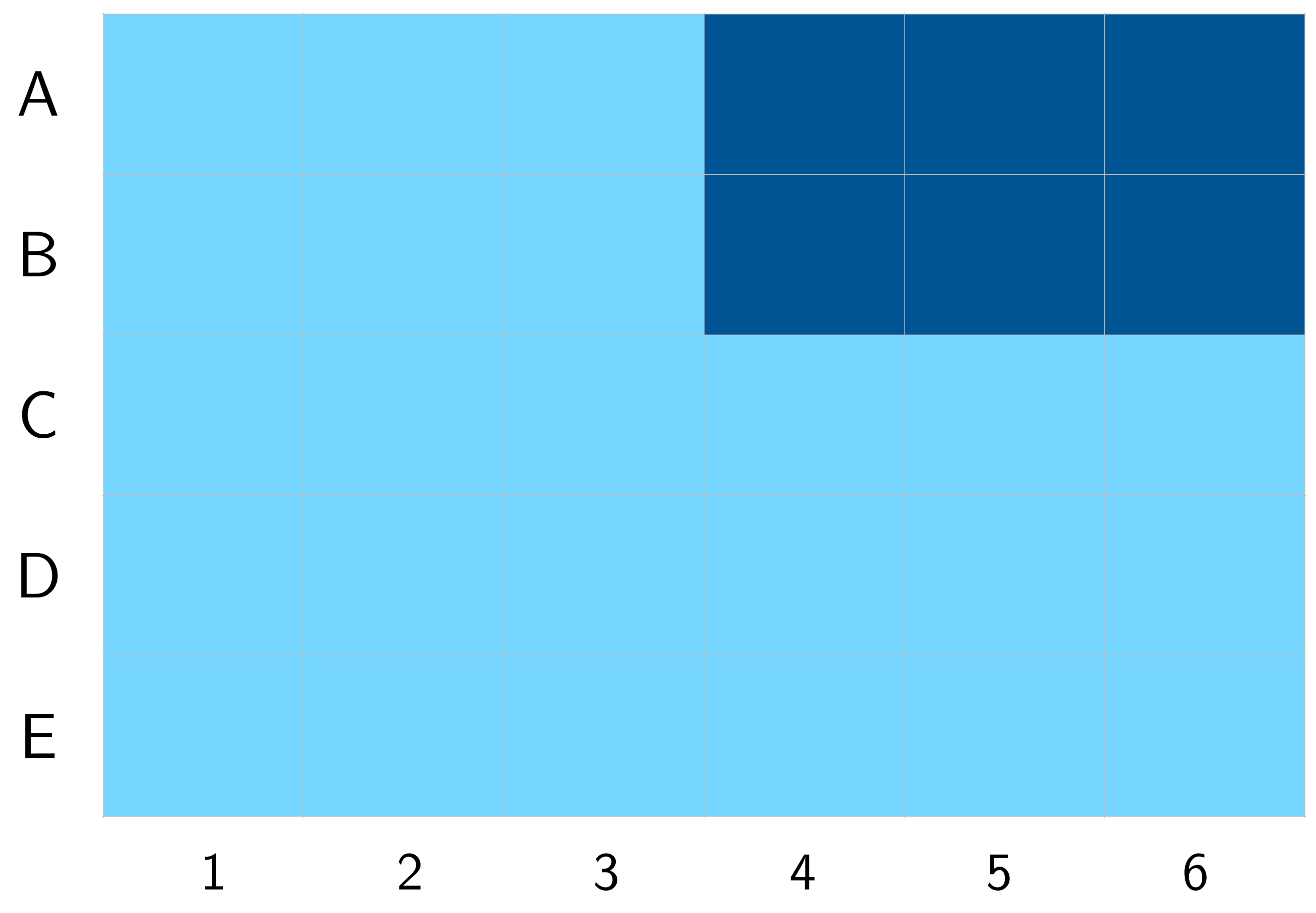


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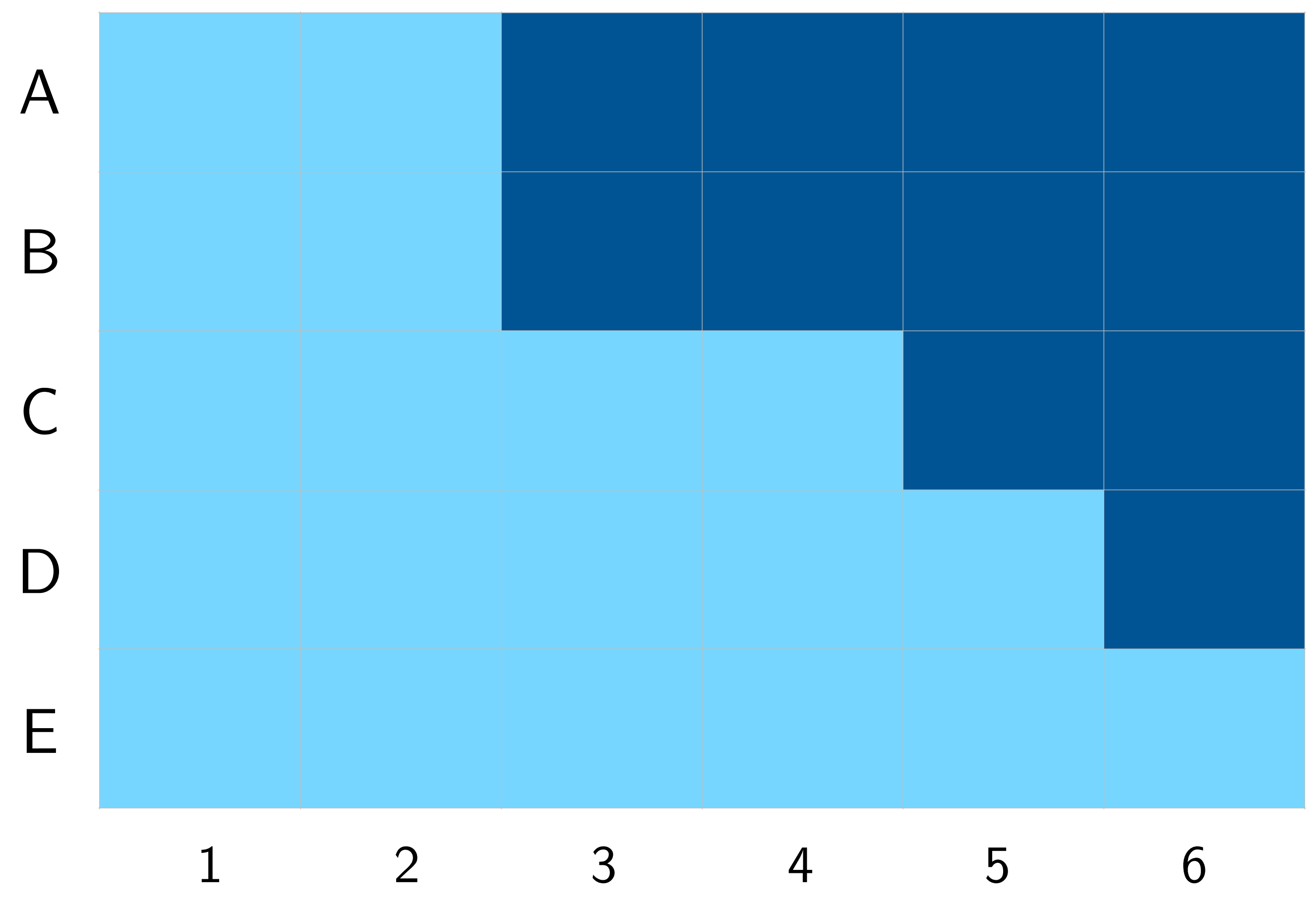


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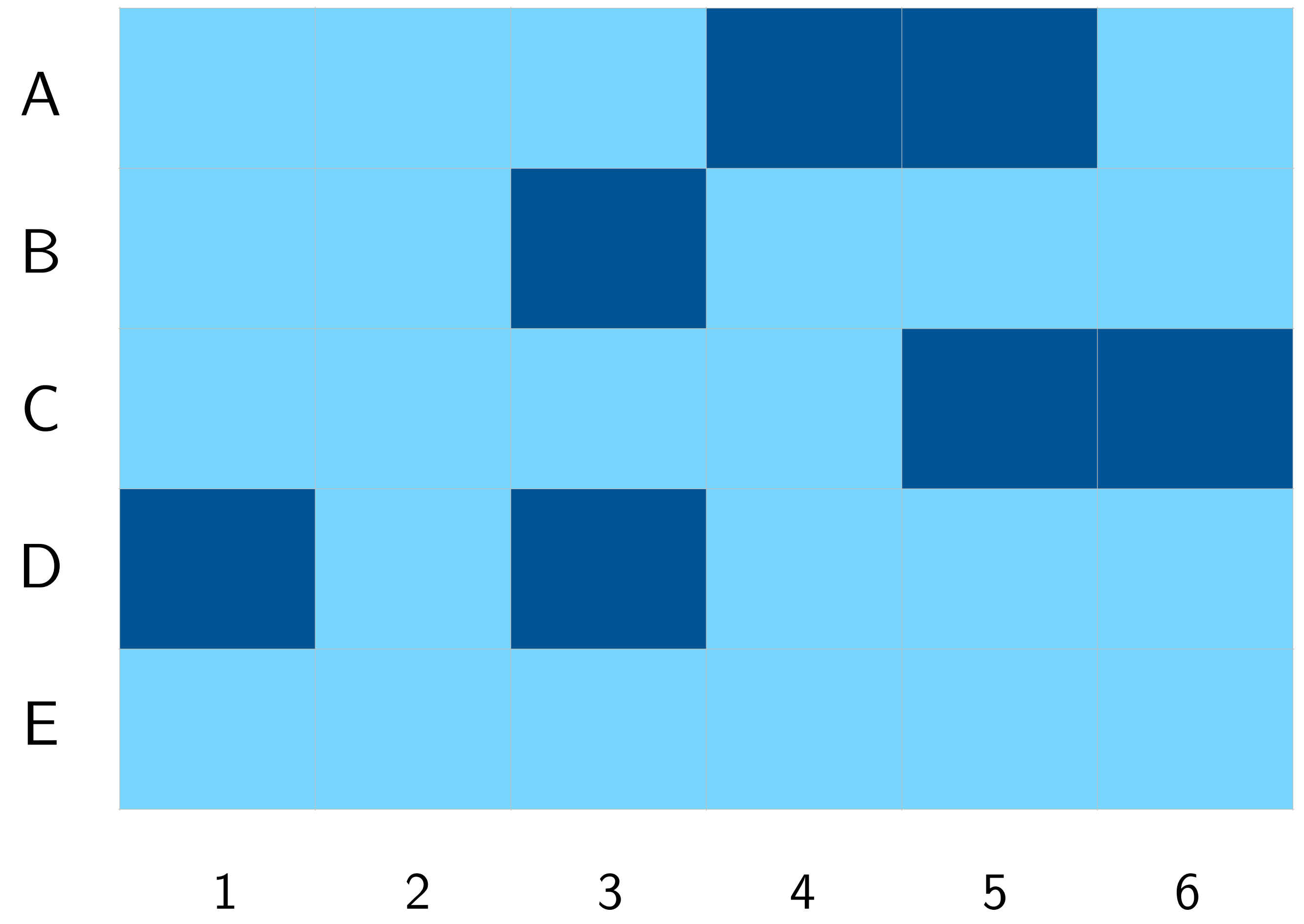


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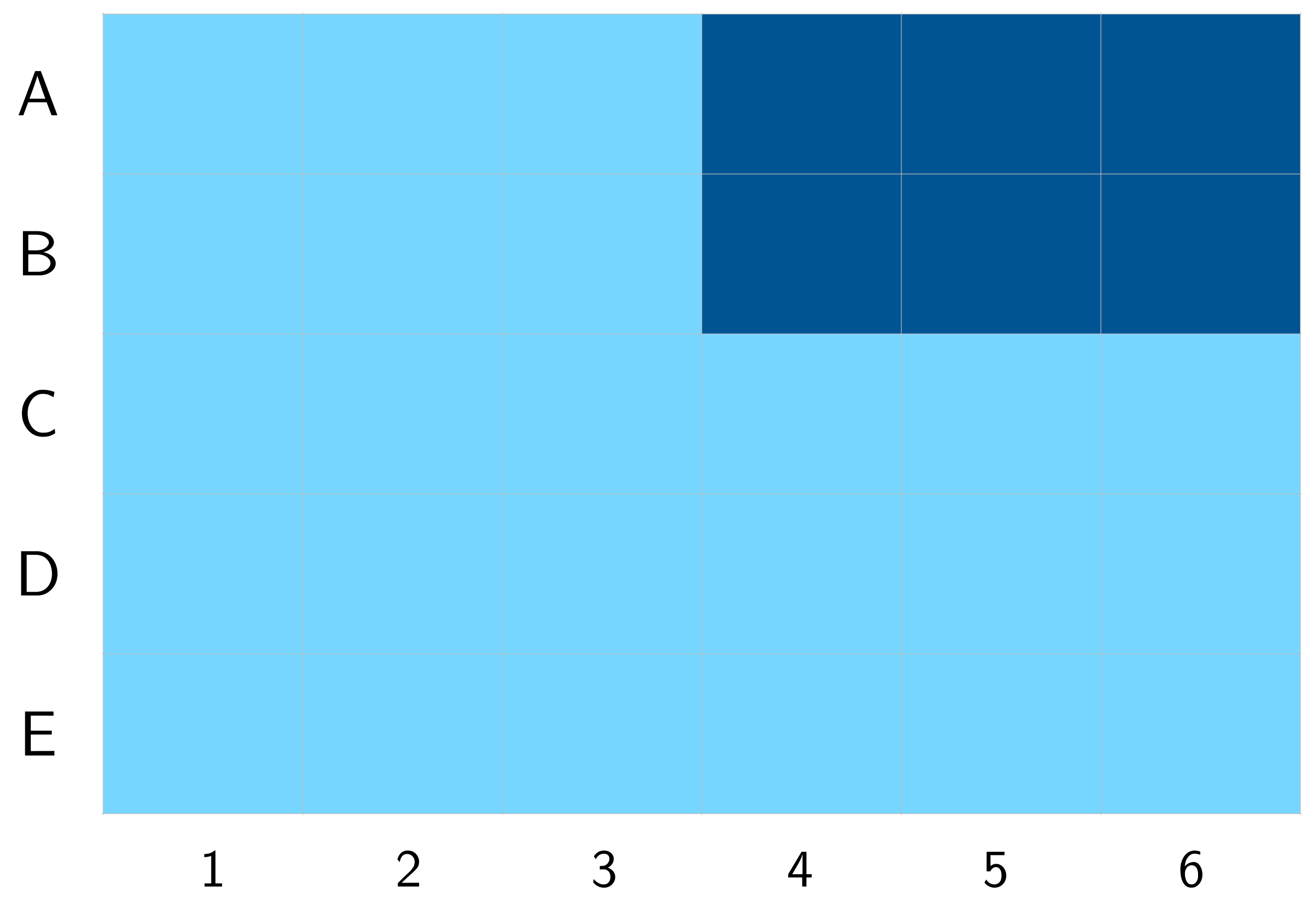


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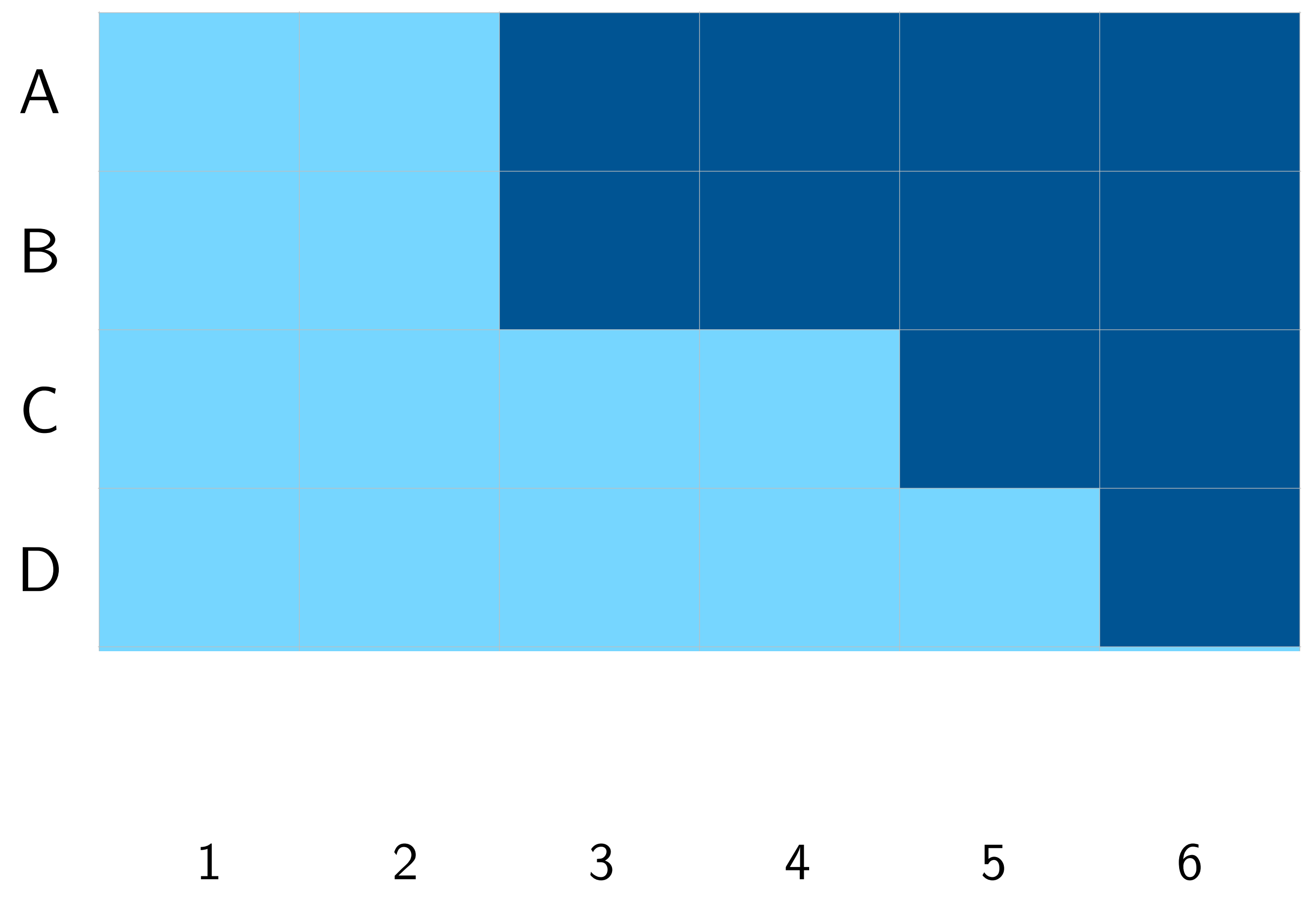


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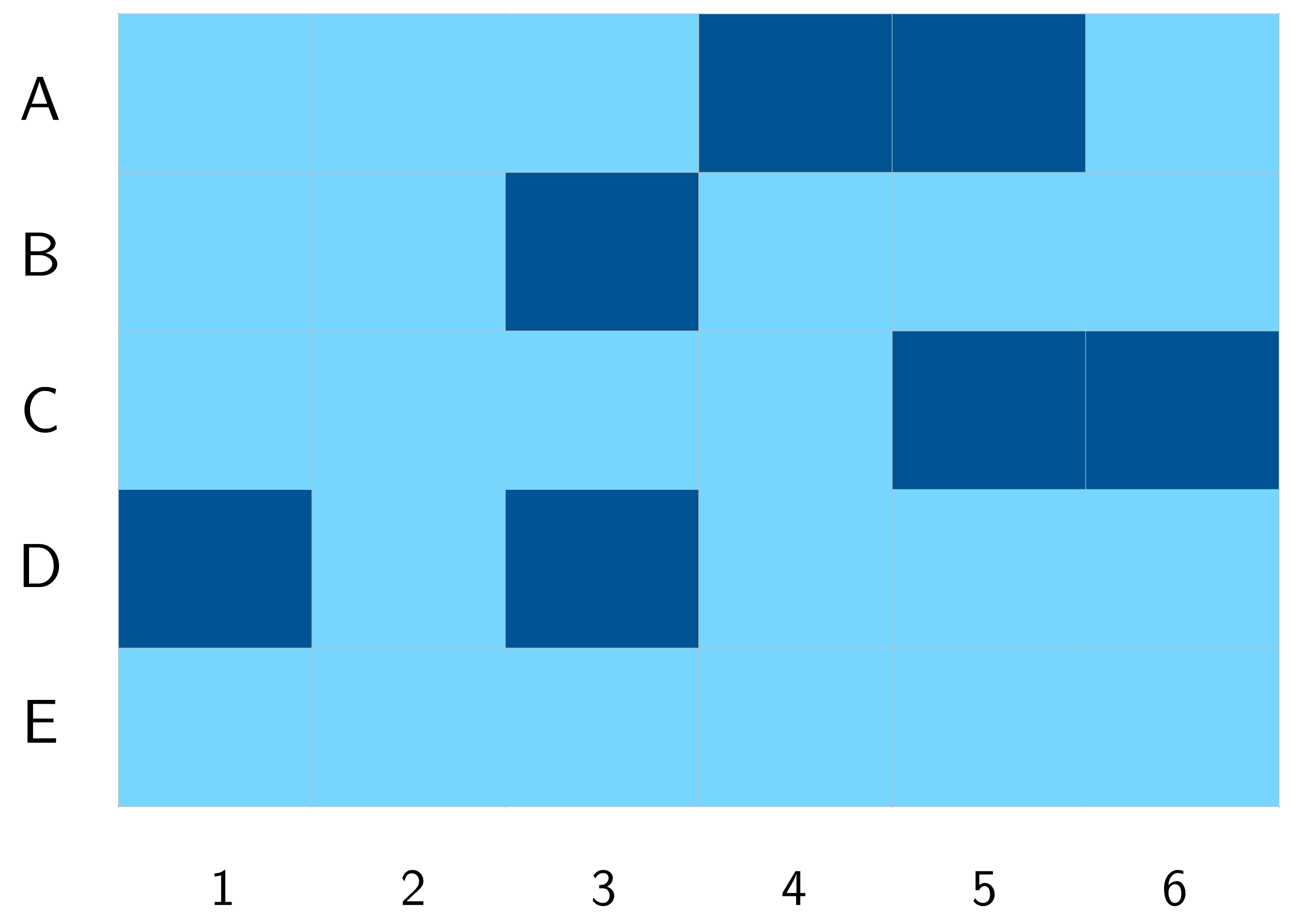


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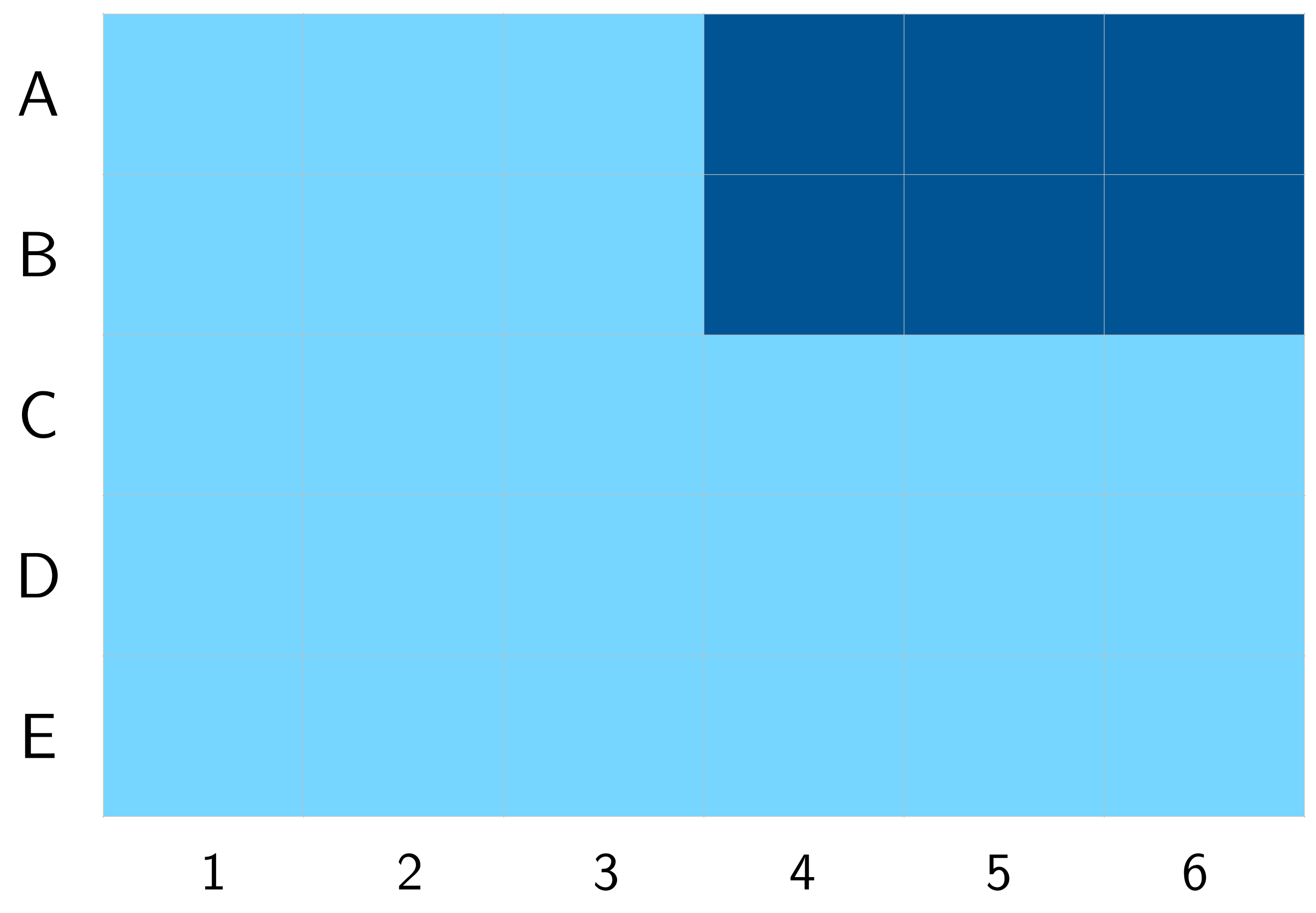


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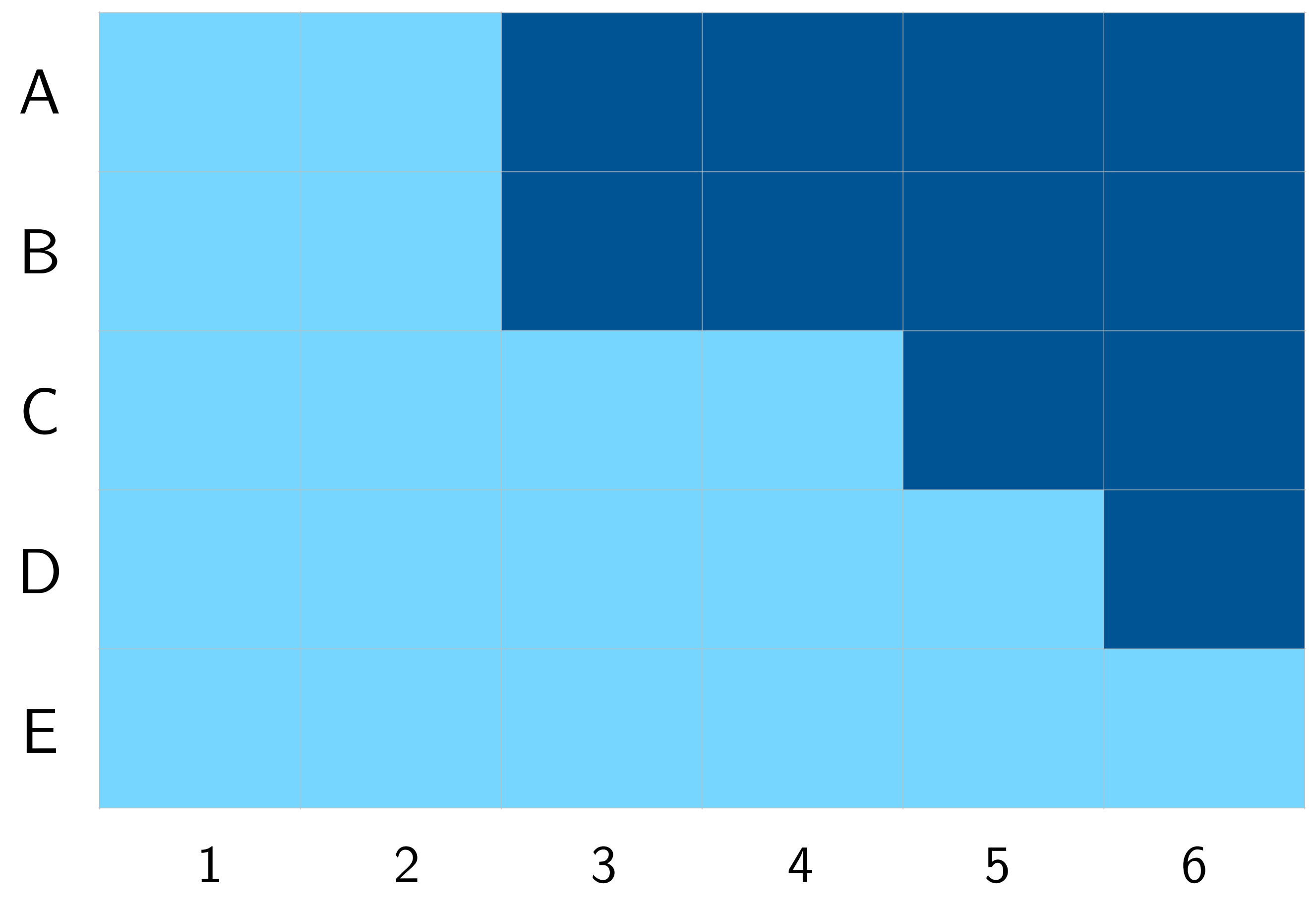


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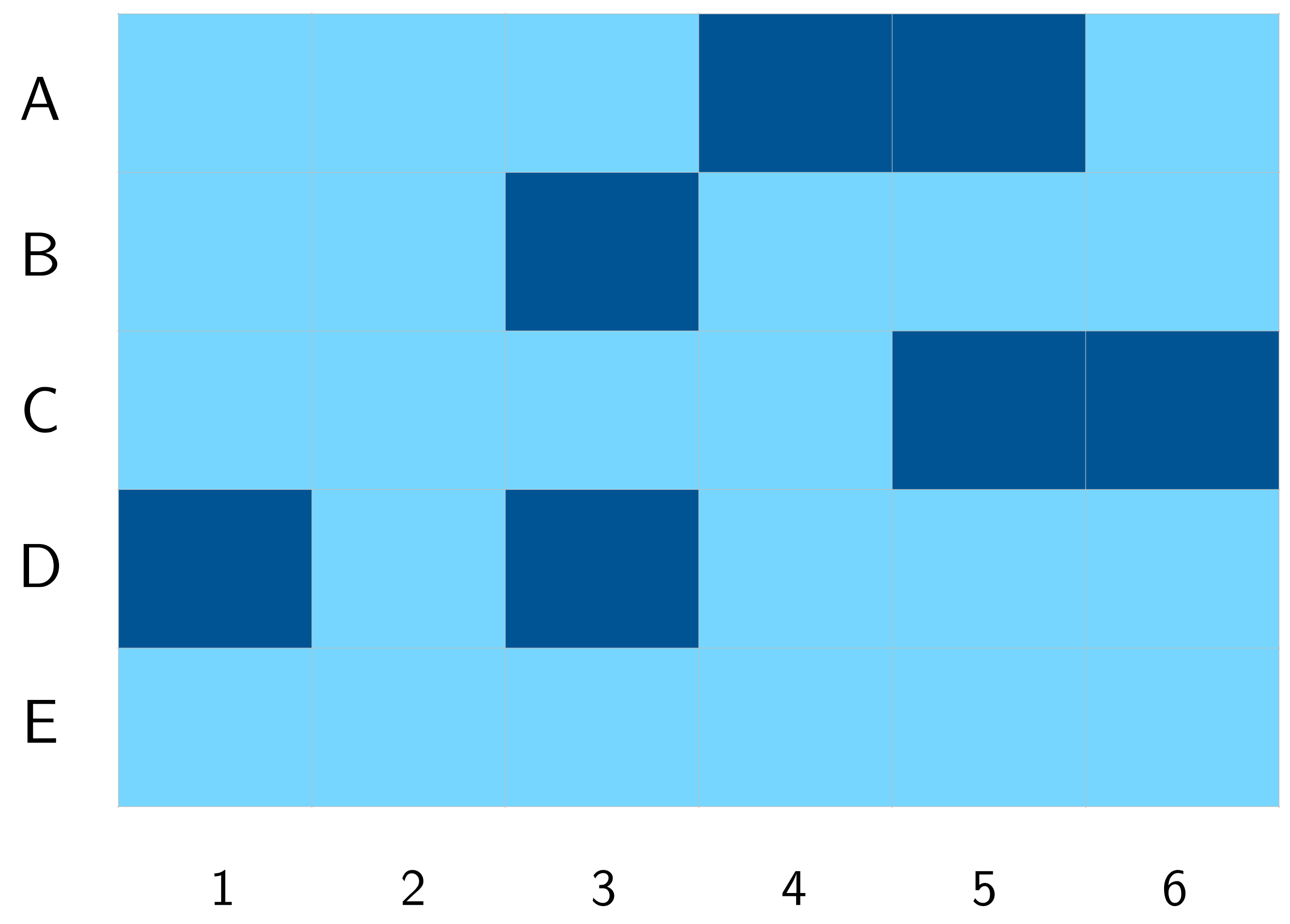


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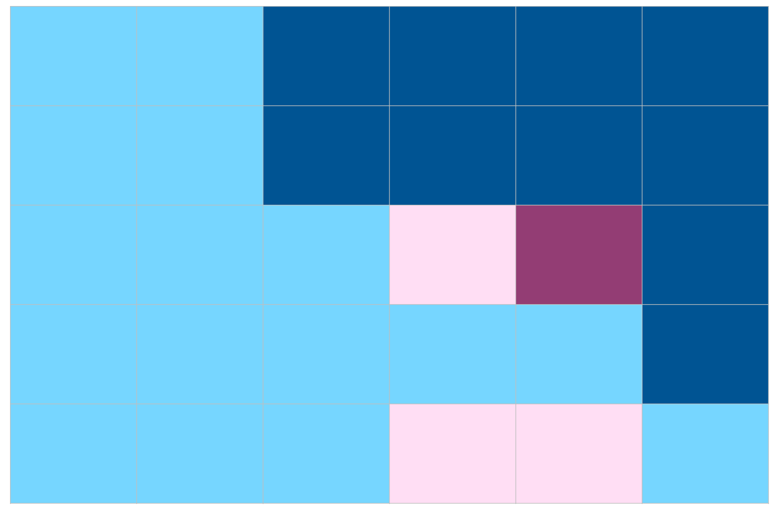
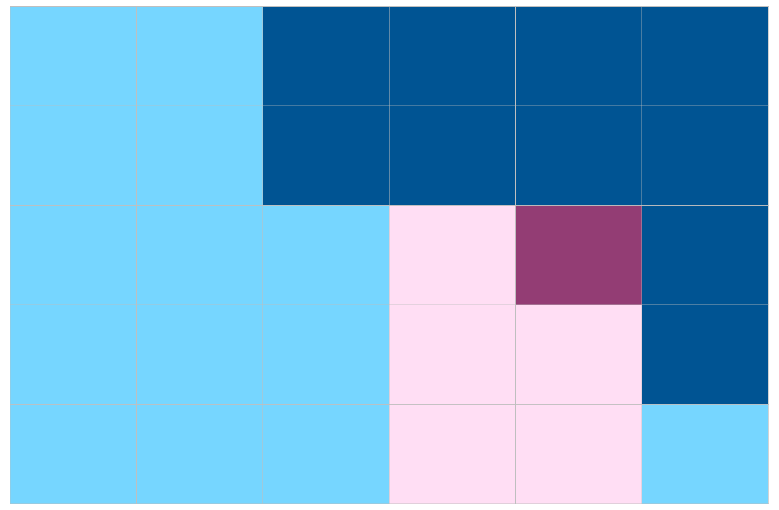
# Different Estimators Use Different Comparison Groups

DID Extension

Staggered



Interaction Weighted
& Stacked DID



CSDID

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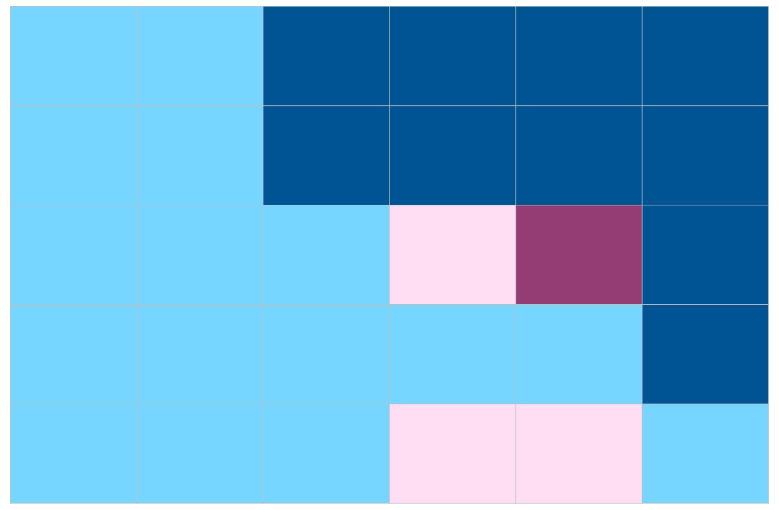

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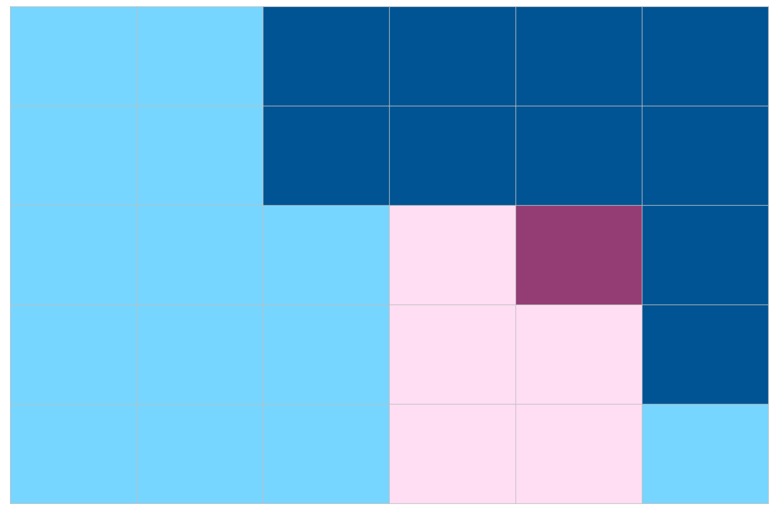
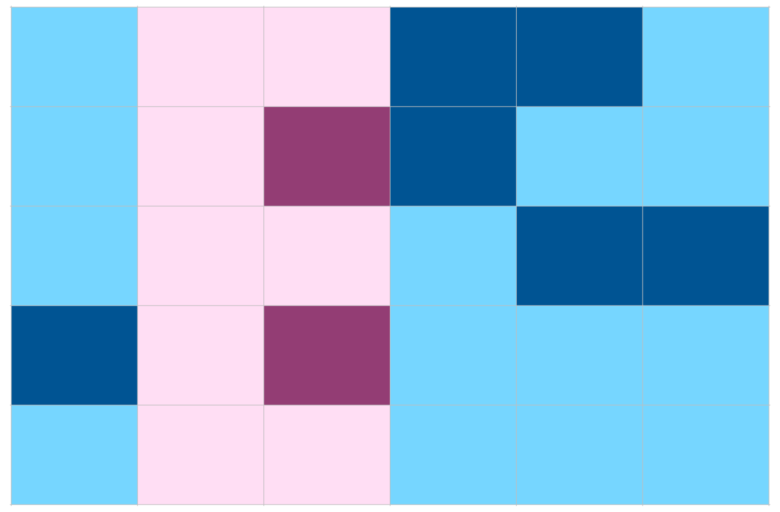


CSDID

General



DID multiple/PanelMatch

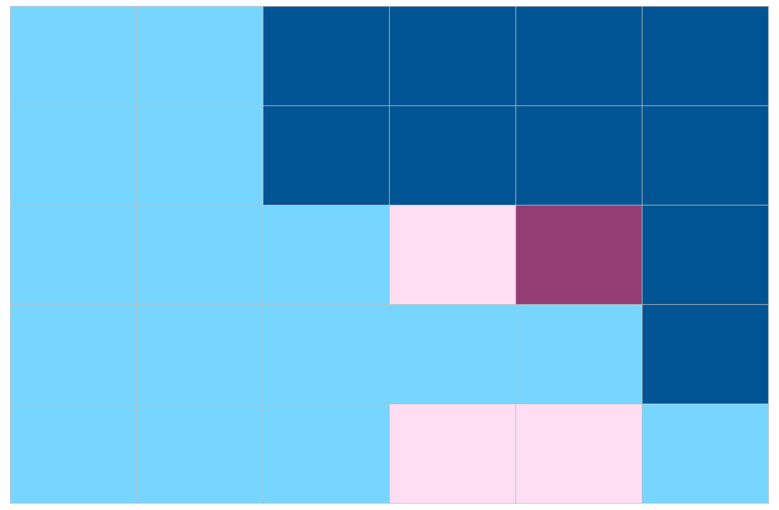
# Different Estimators Use Different Comparison Groups

DID Extension

Imputation

Staggered

General



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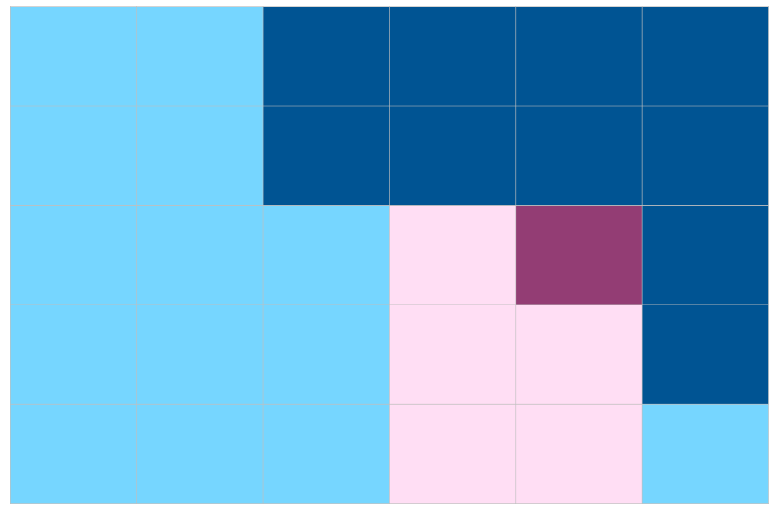

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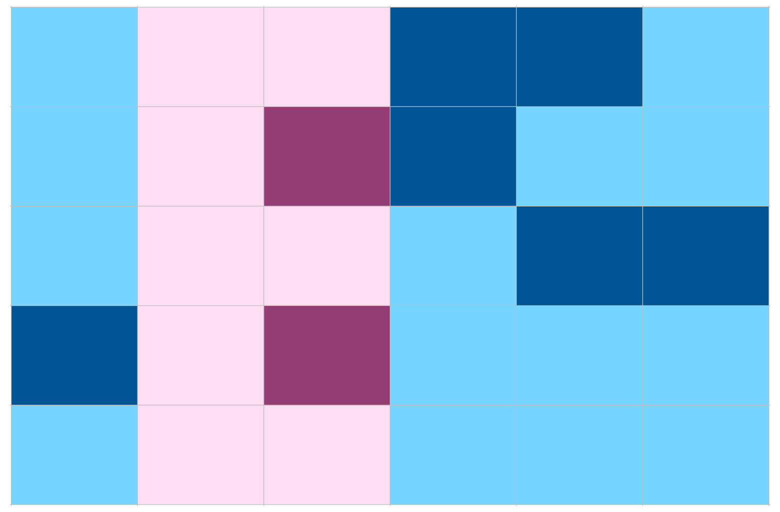

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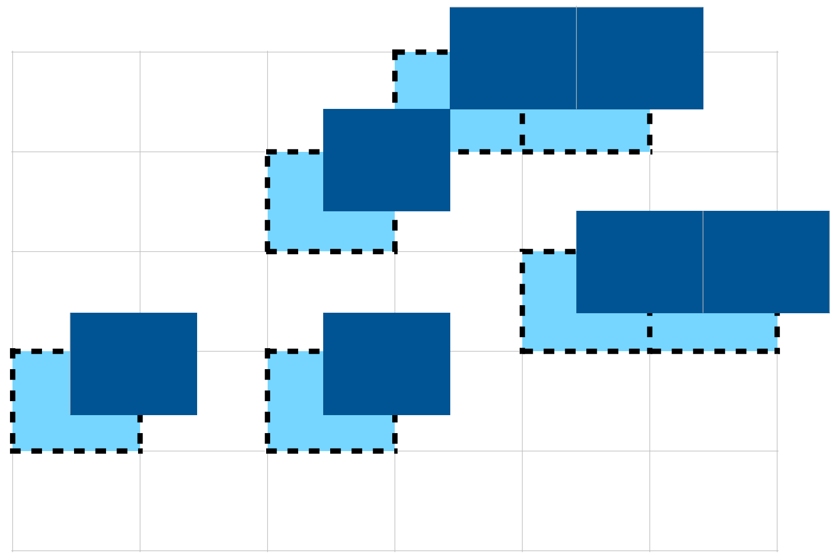

Imputation Method
$\text{DID}_{\text{impute}}$, FEct

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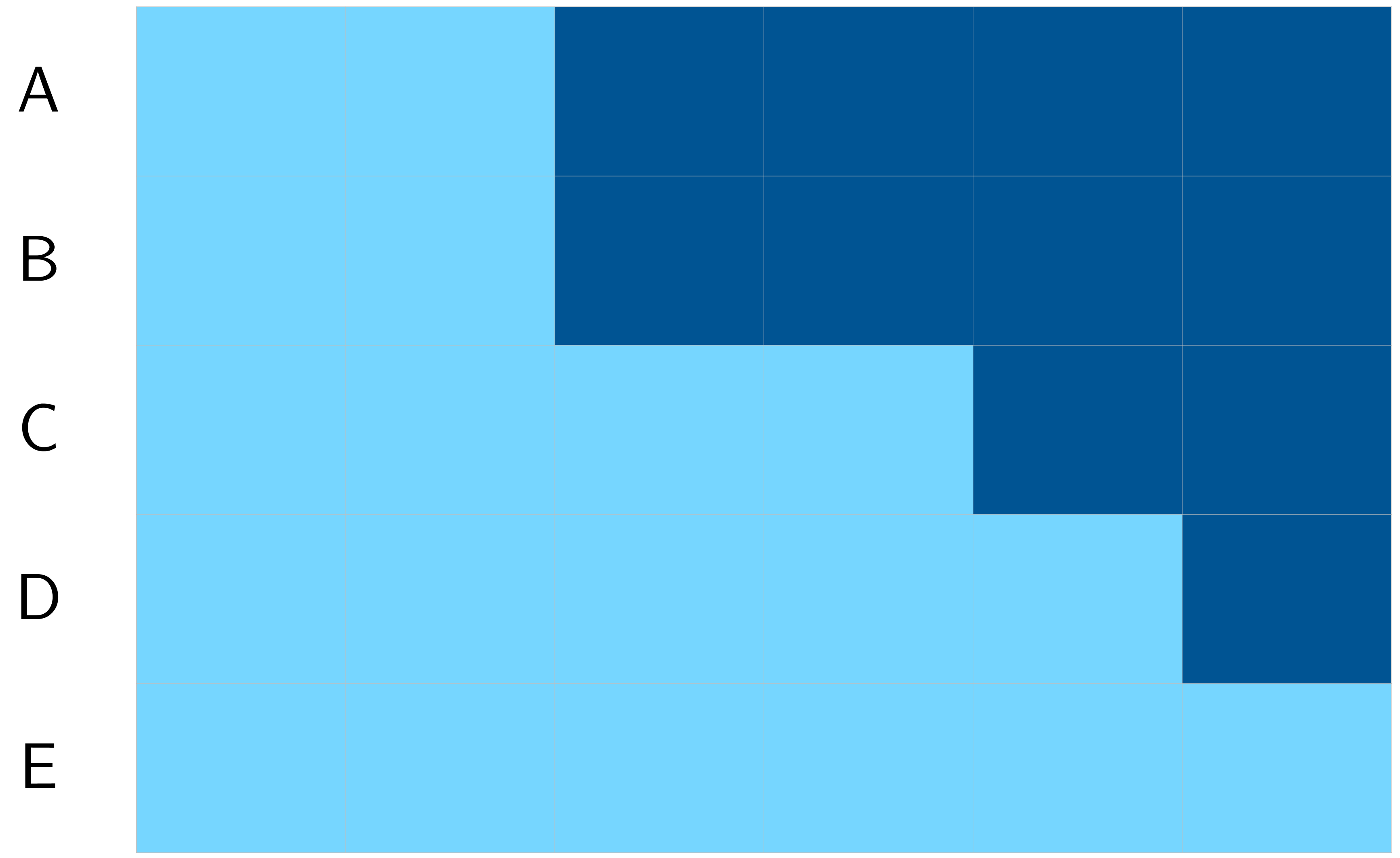

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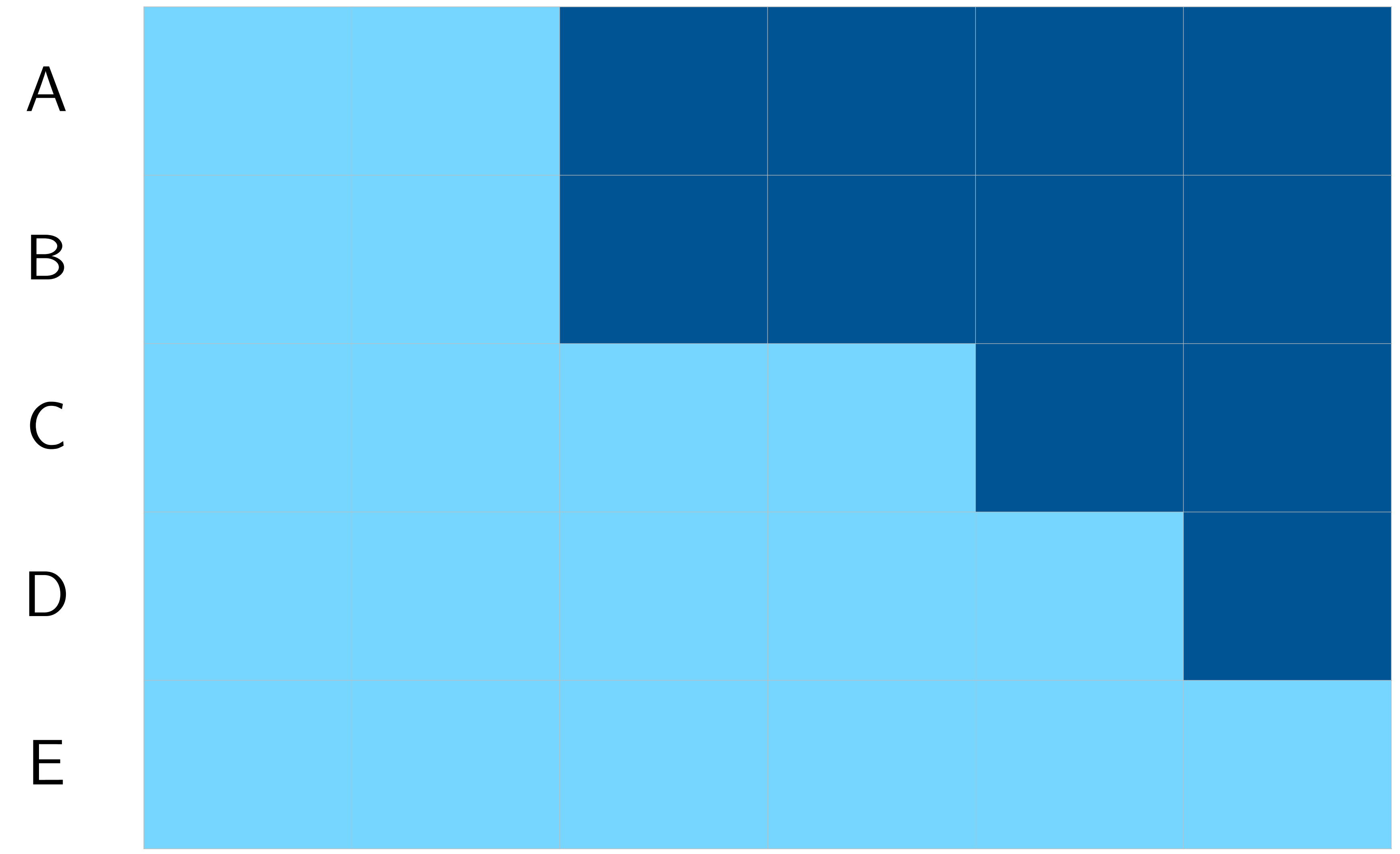

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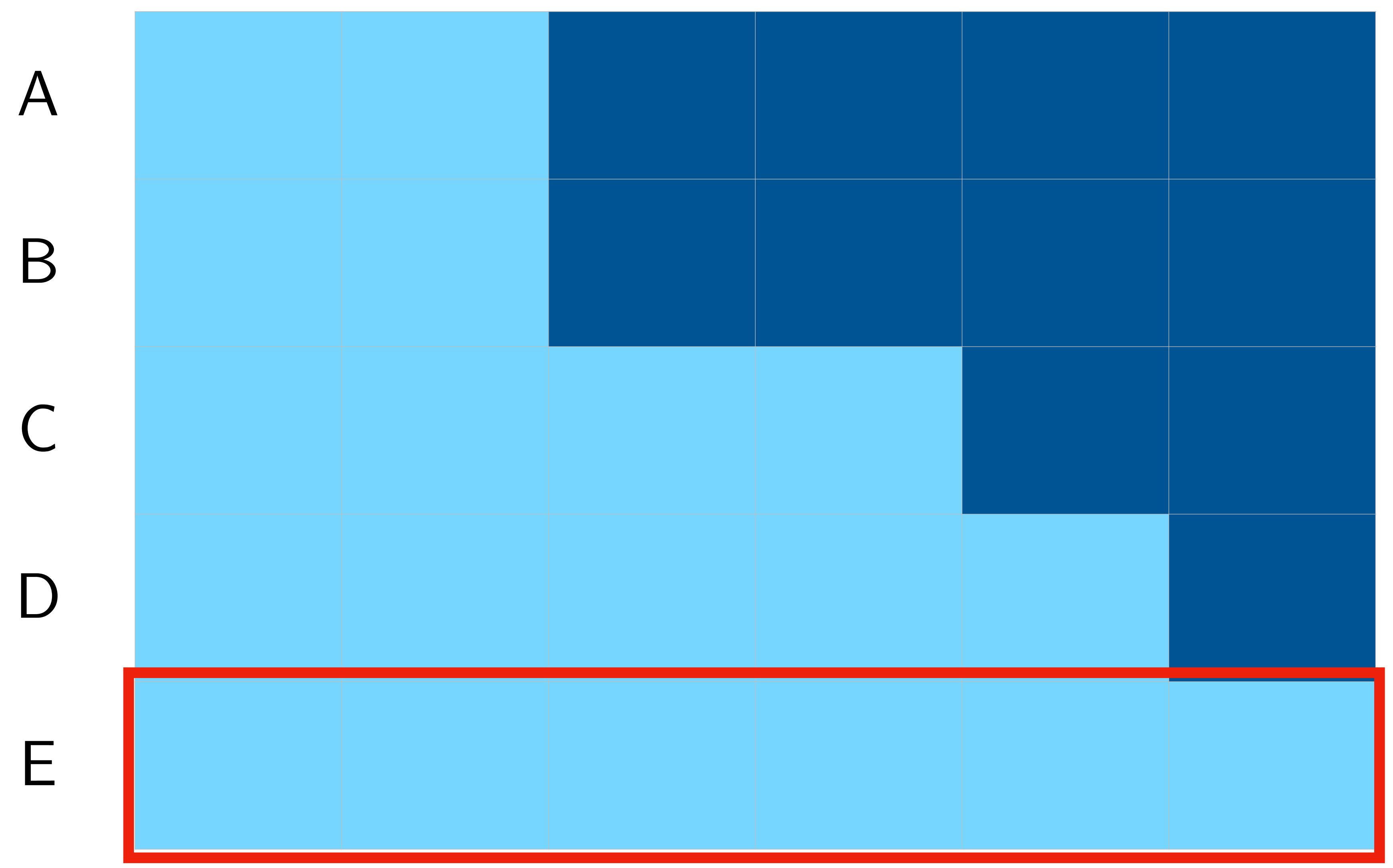

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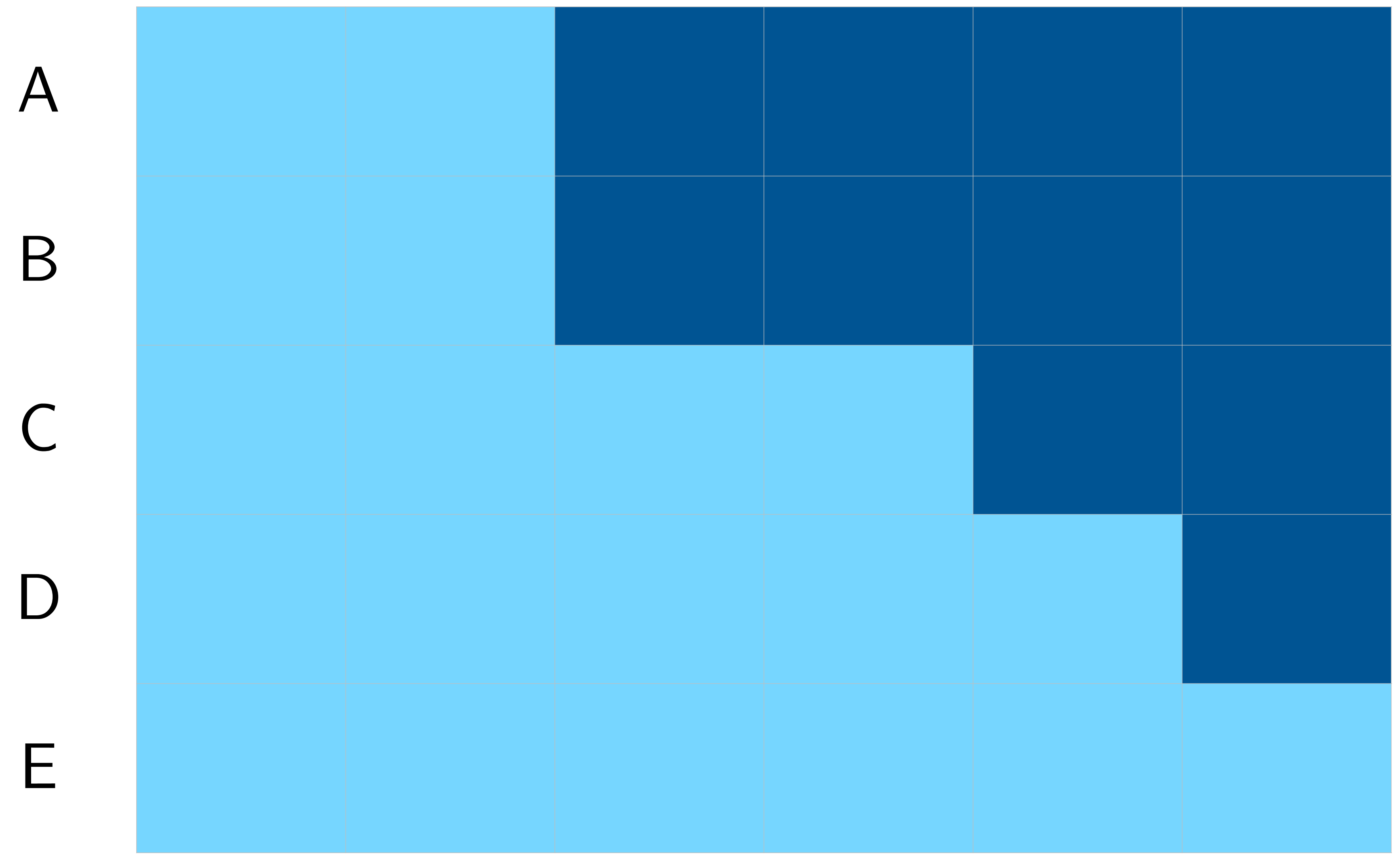

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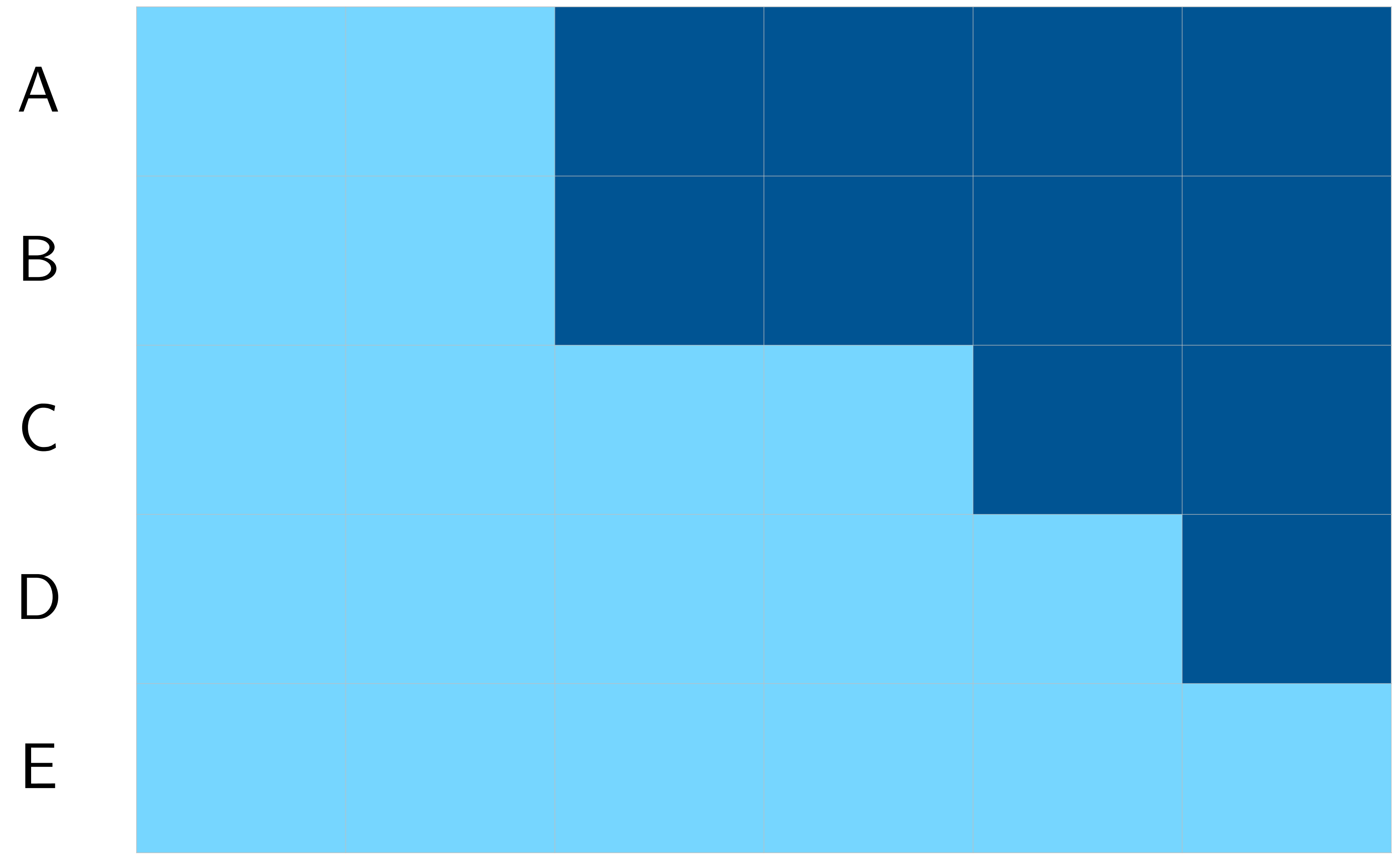

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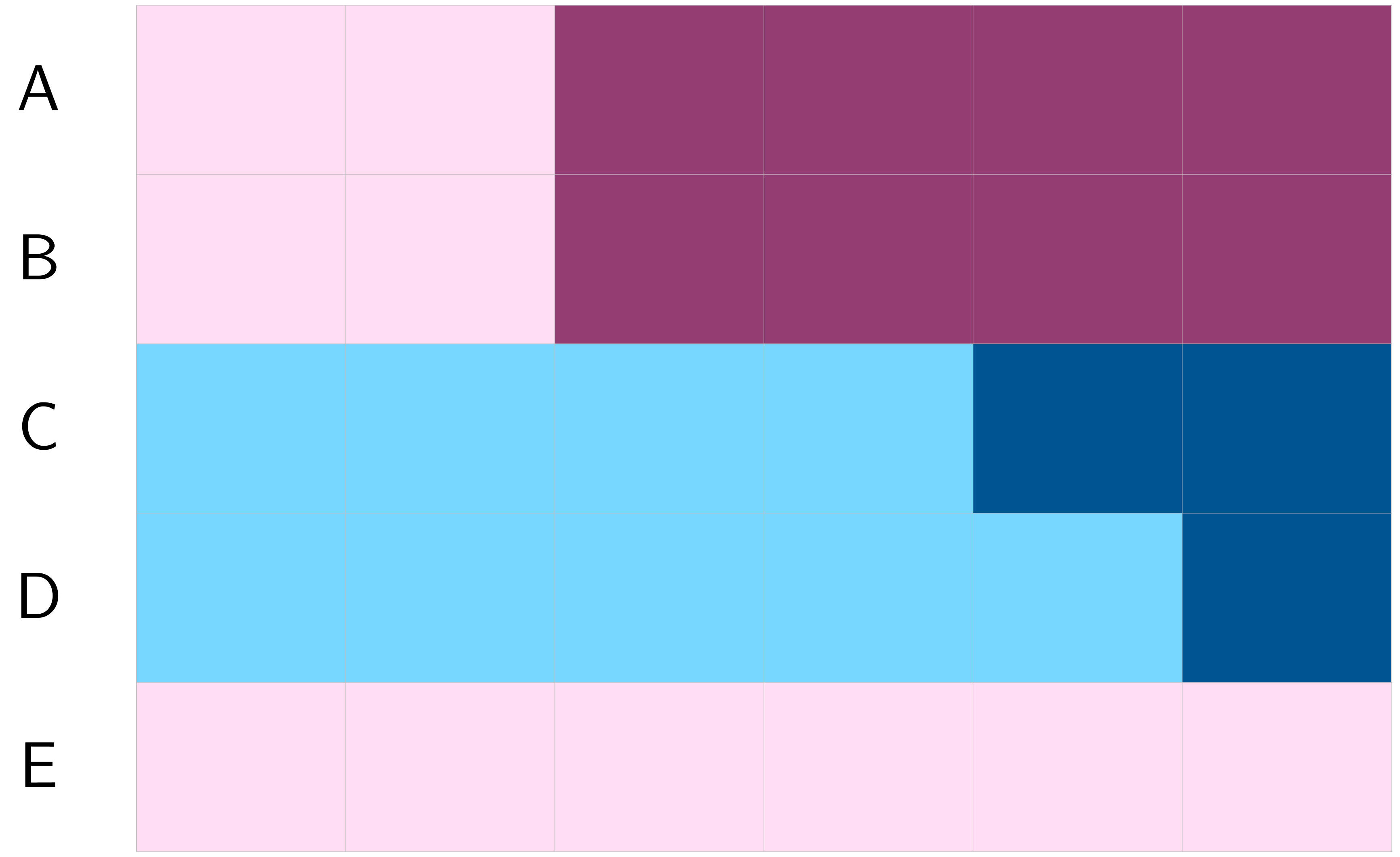

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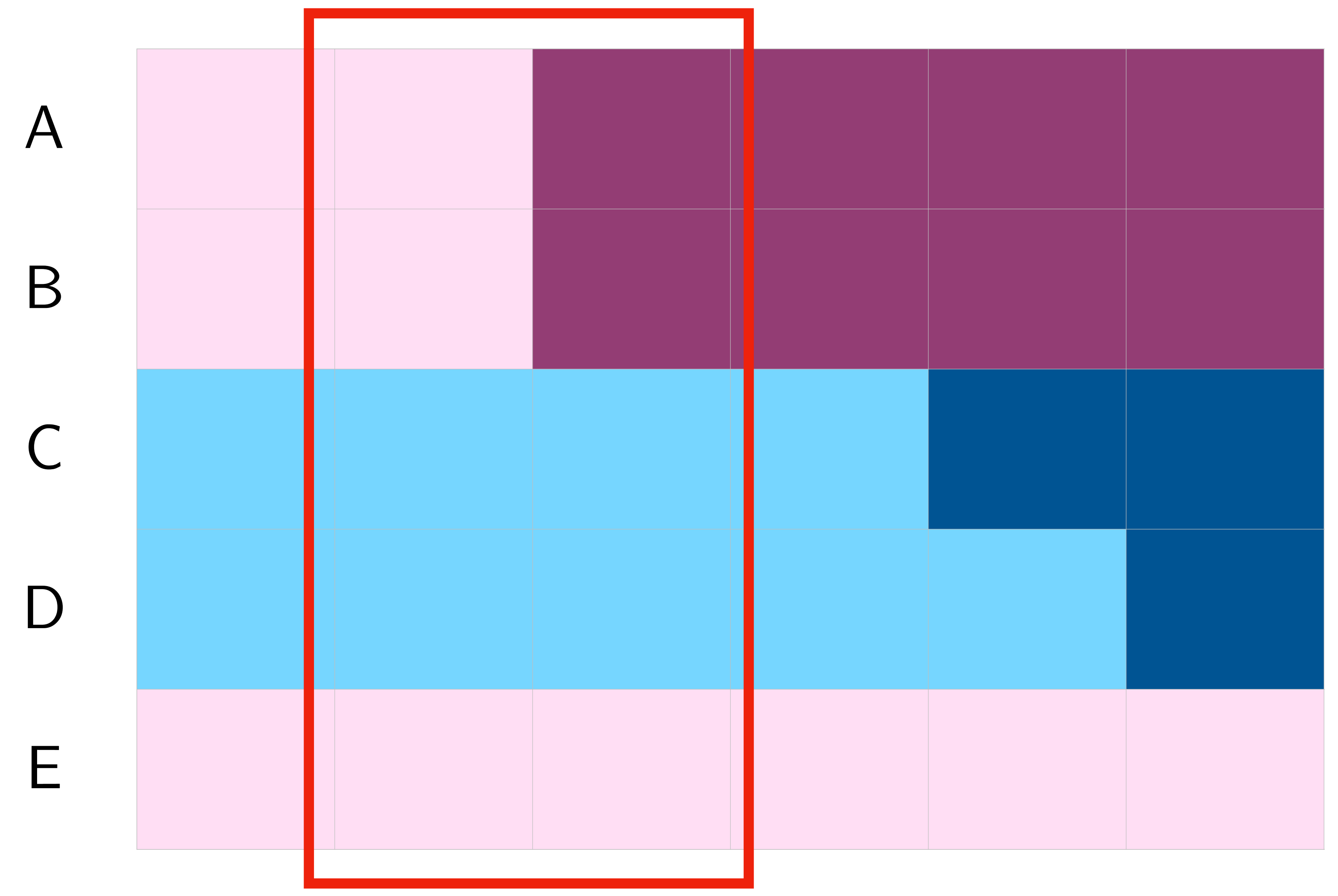

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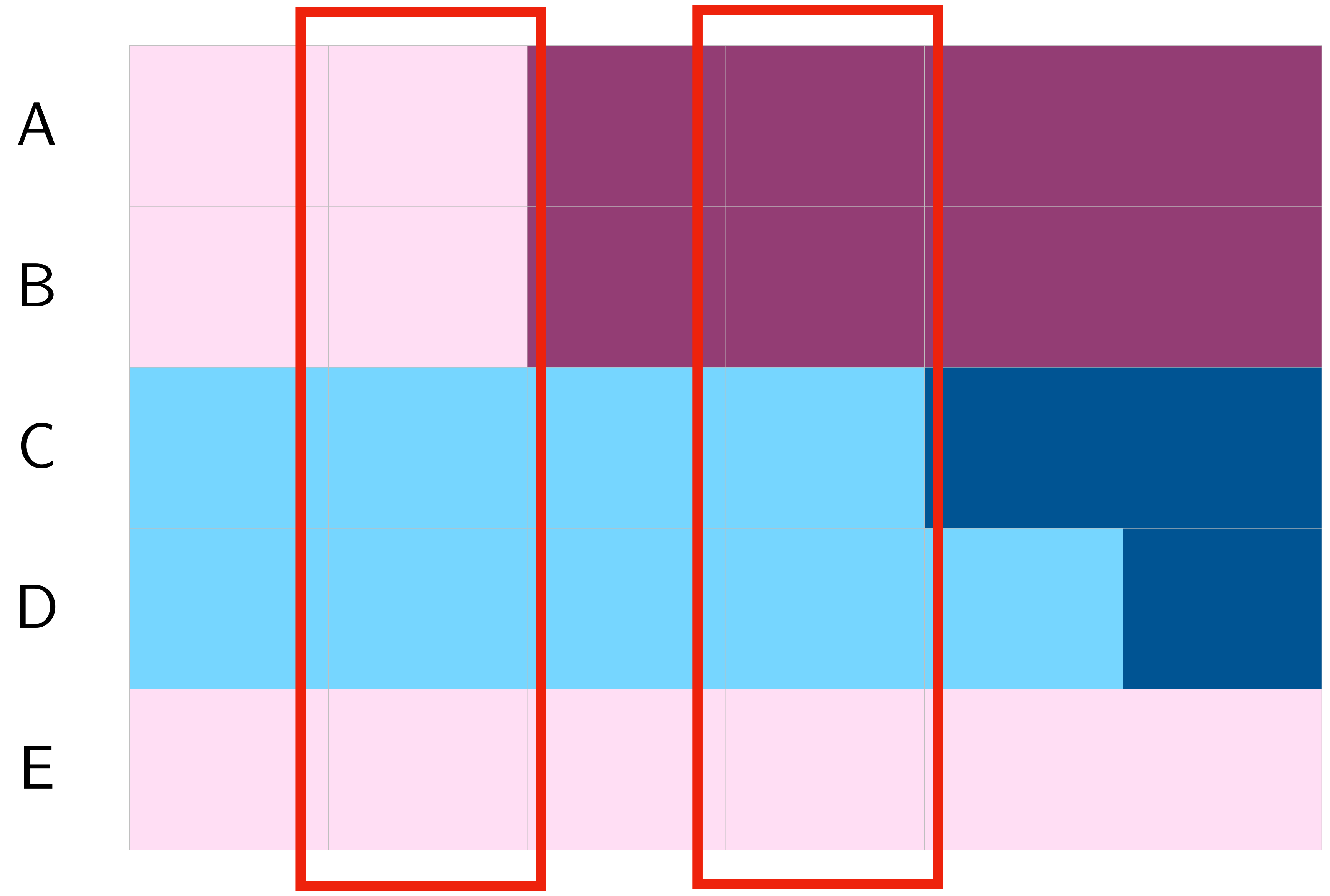

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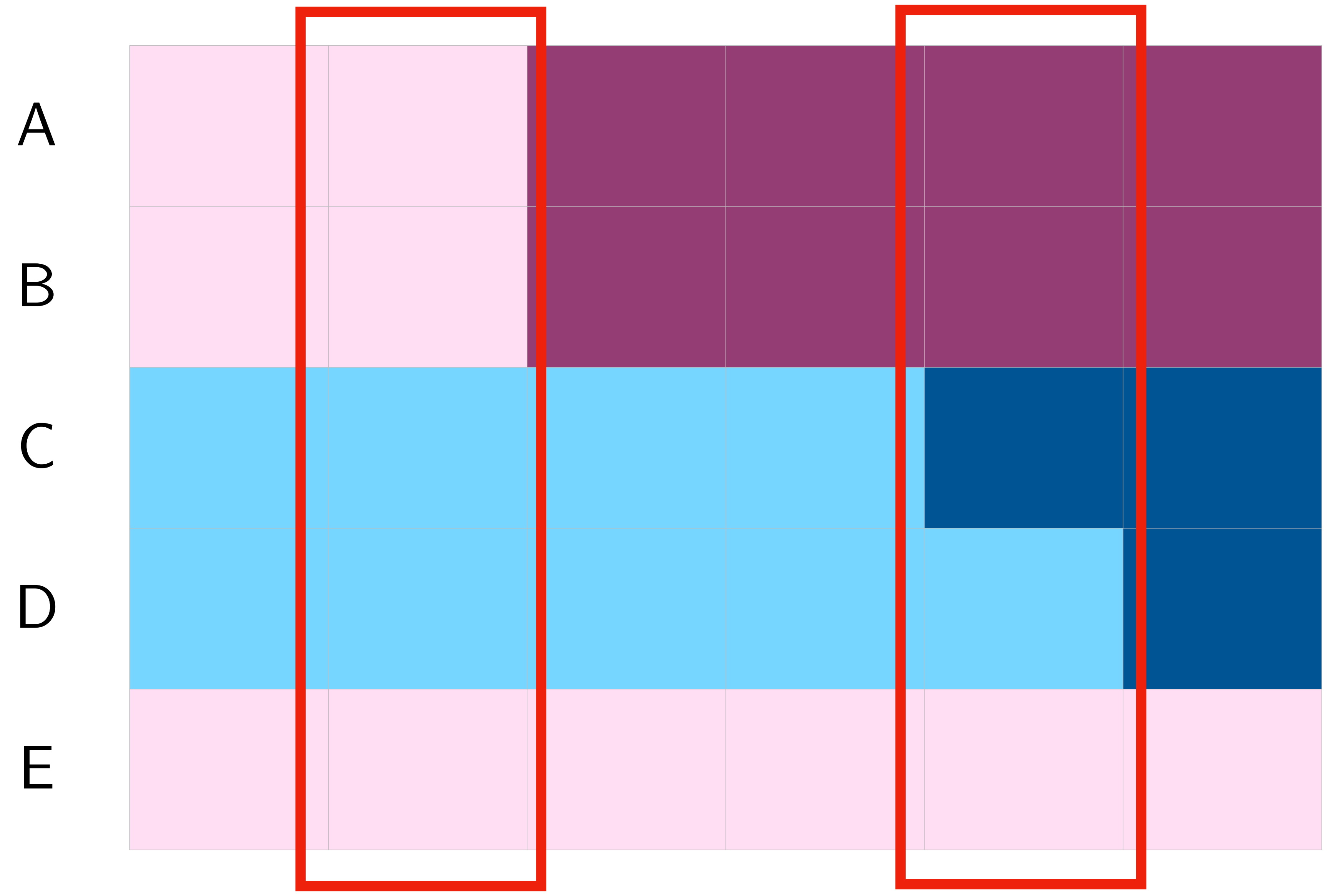

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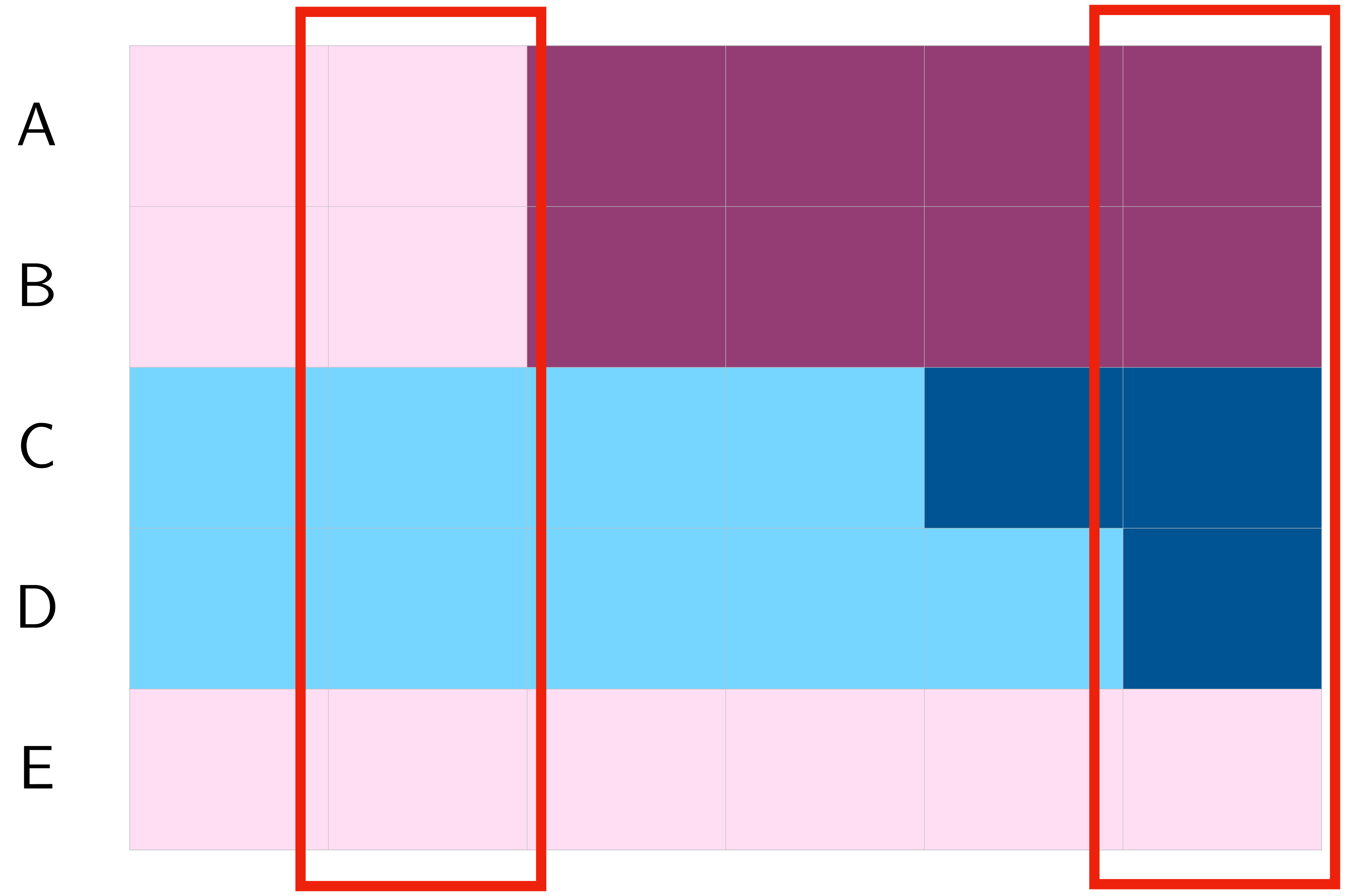

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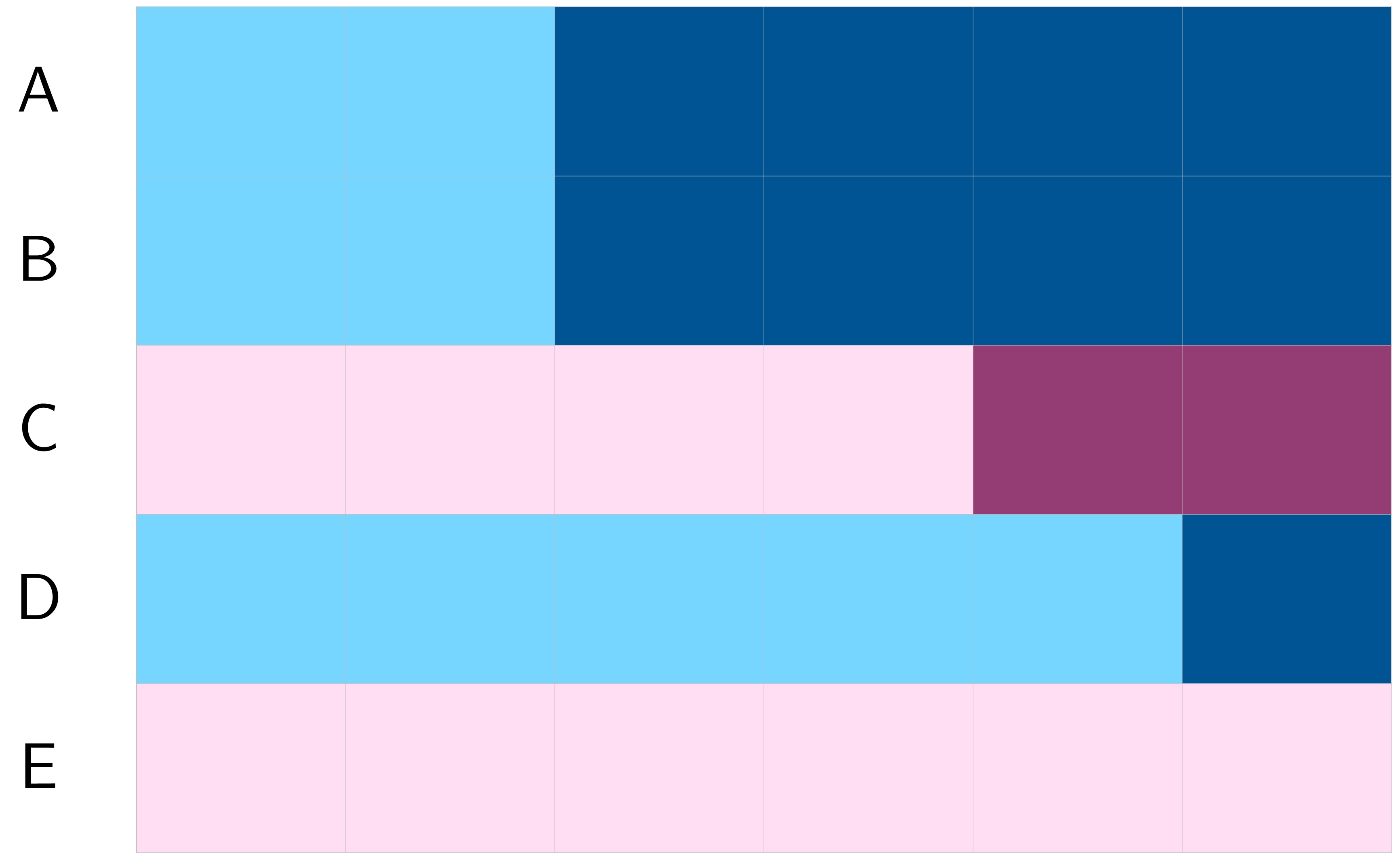

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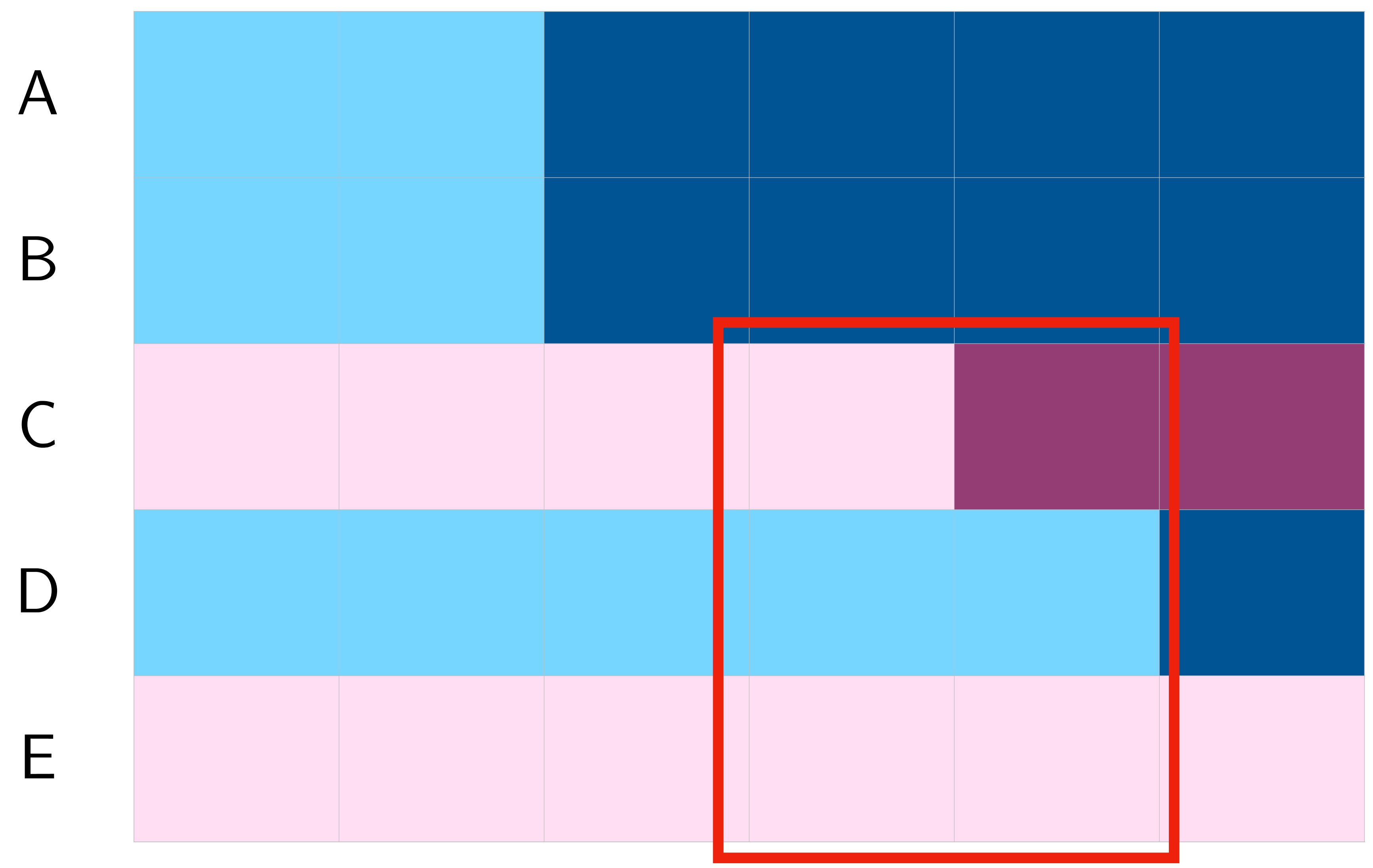

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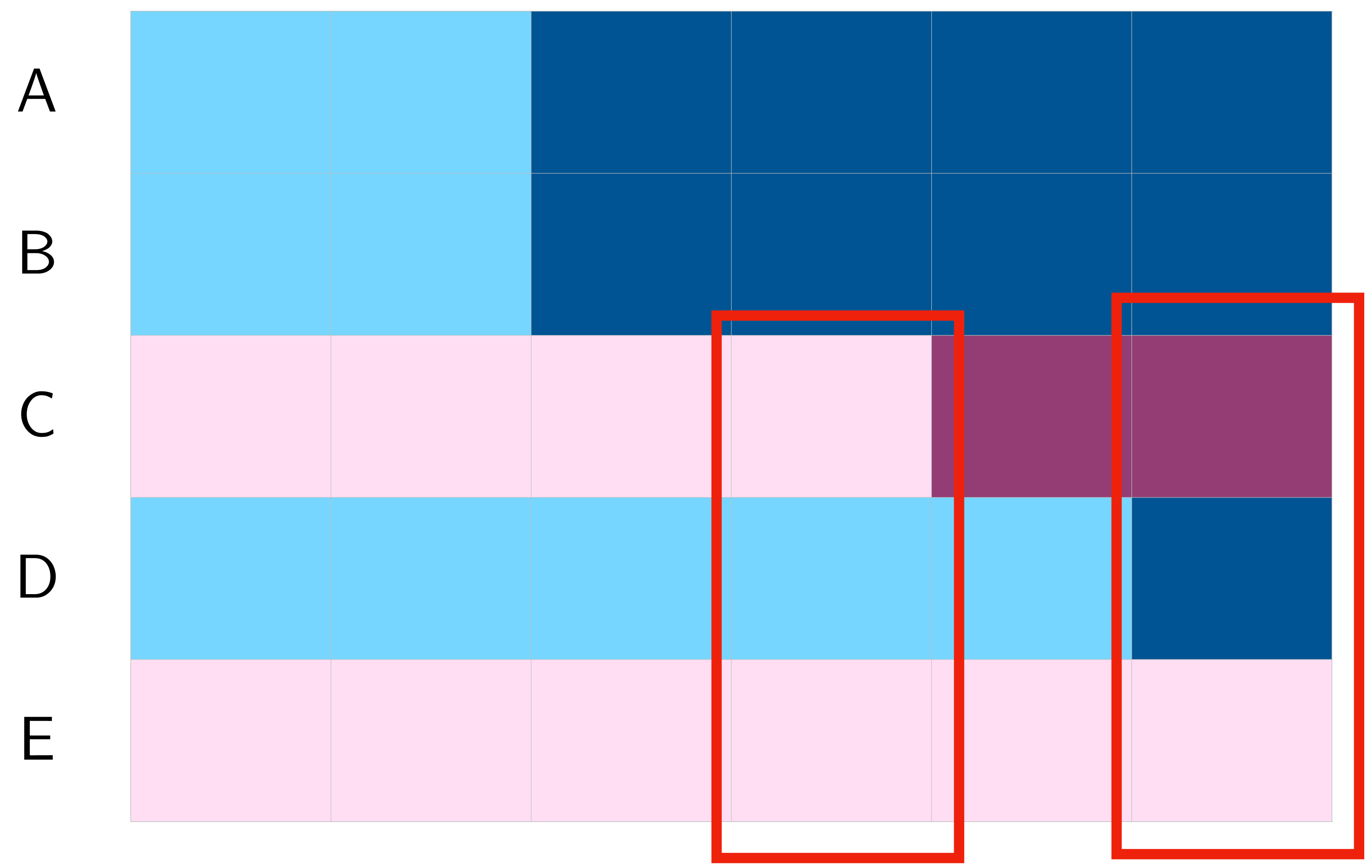

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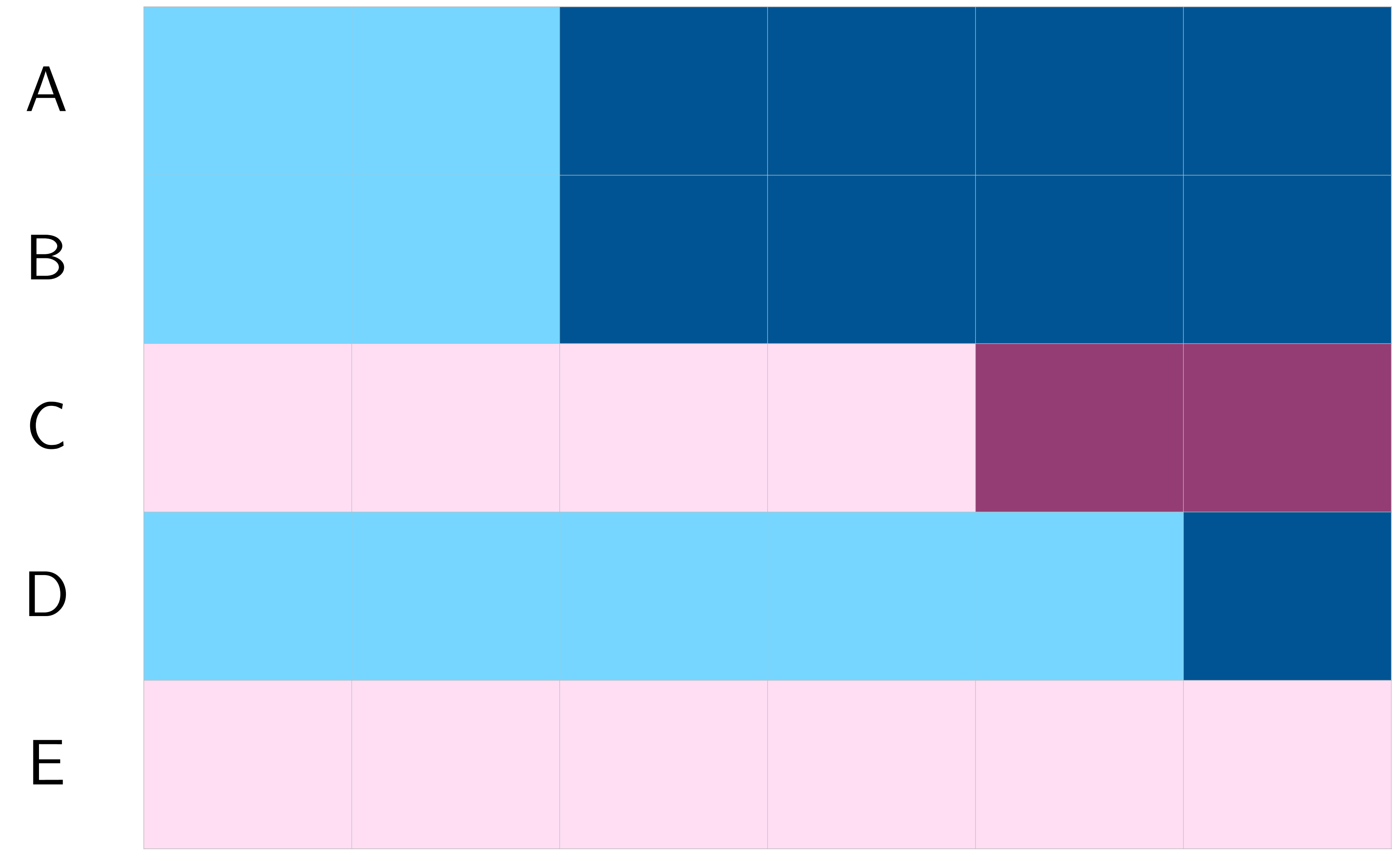
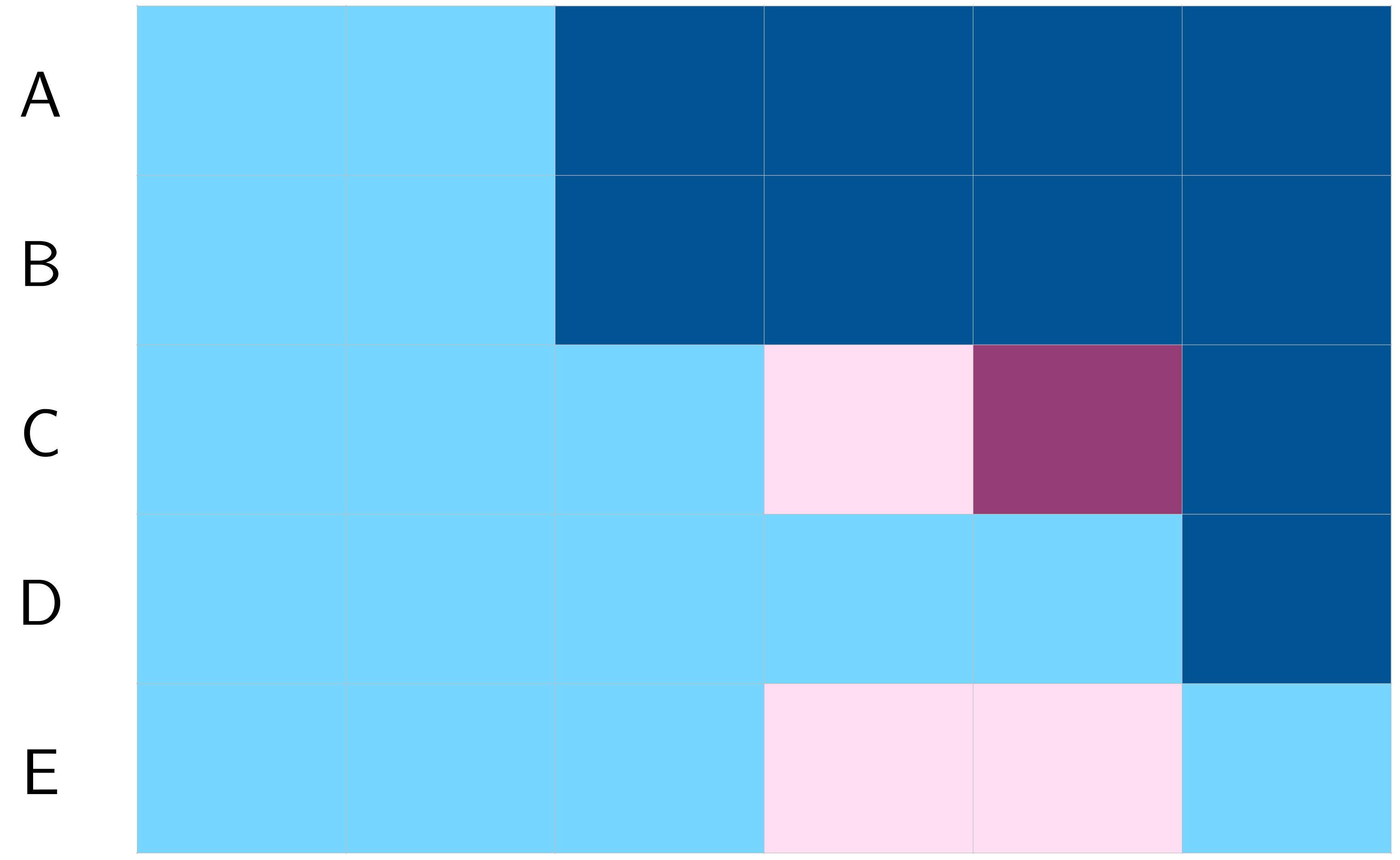


# Callaway & Sant'Anna (2021)

- Comparison group: <u>not-yet-treated</u> (in additional to never treated)
- "Doubly robust" with covariates

Sun & Abraham (2021)

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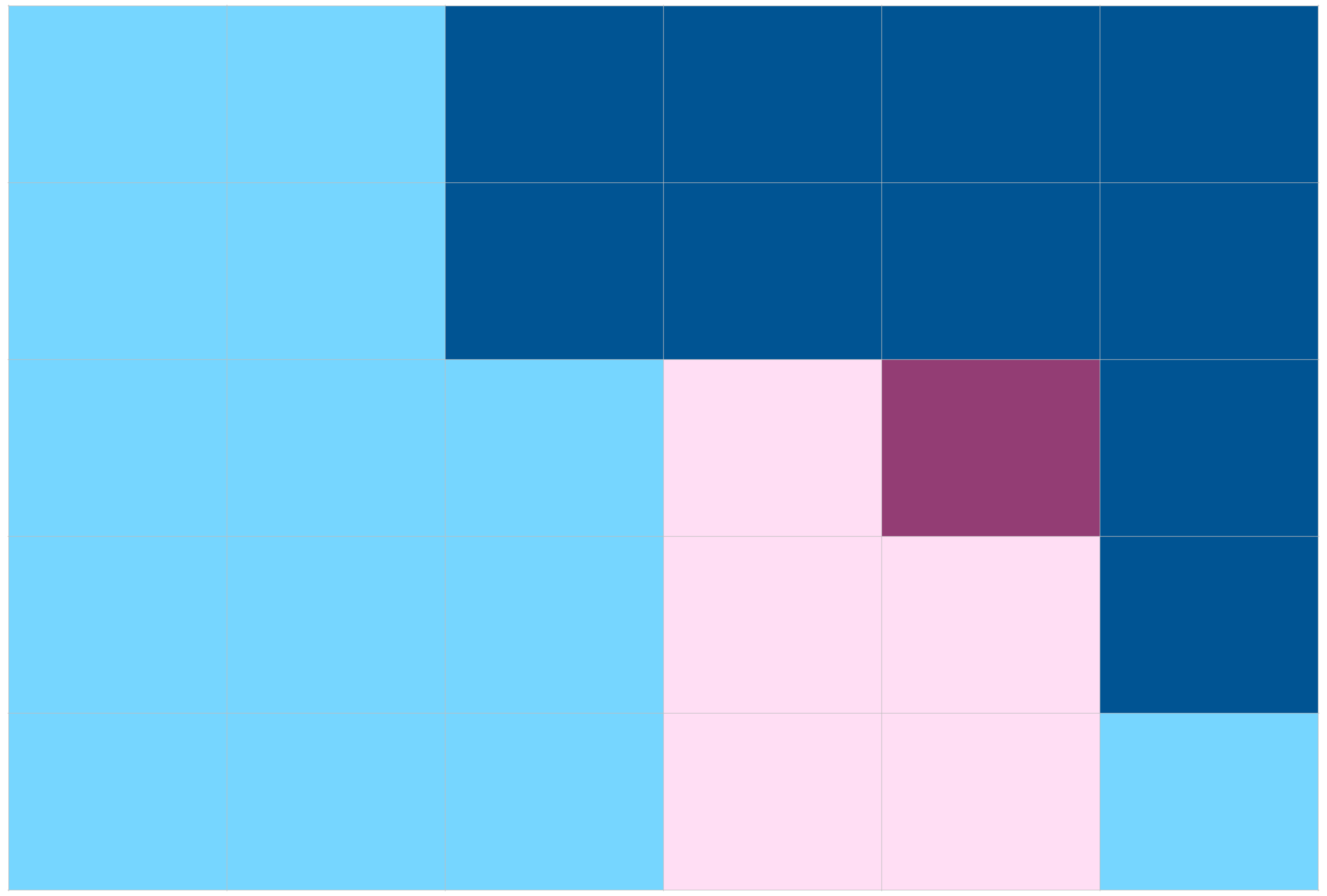
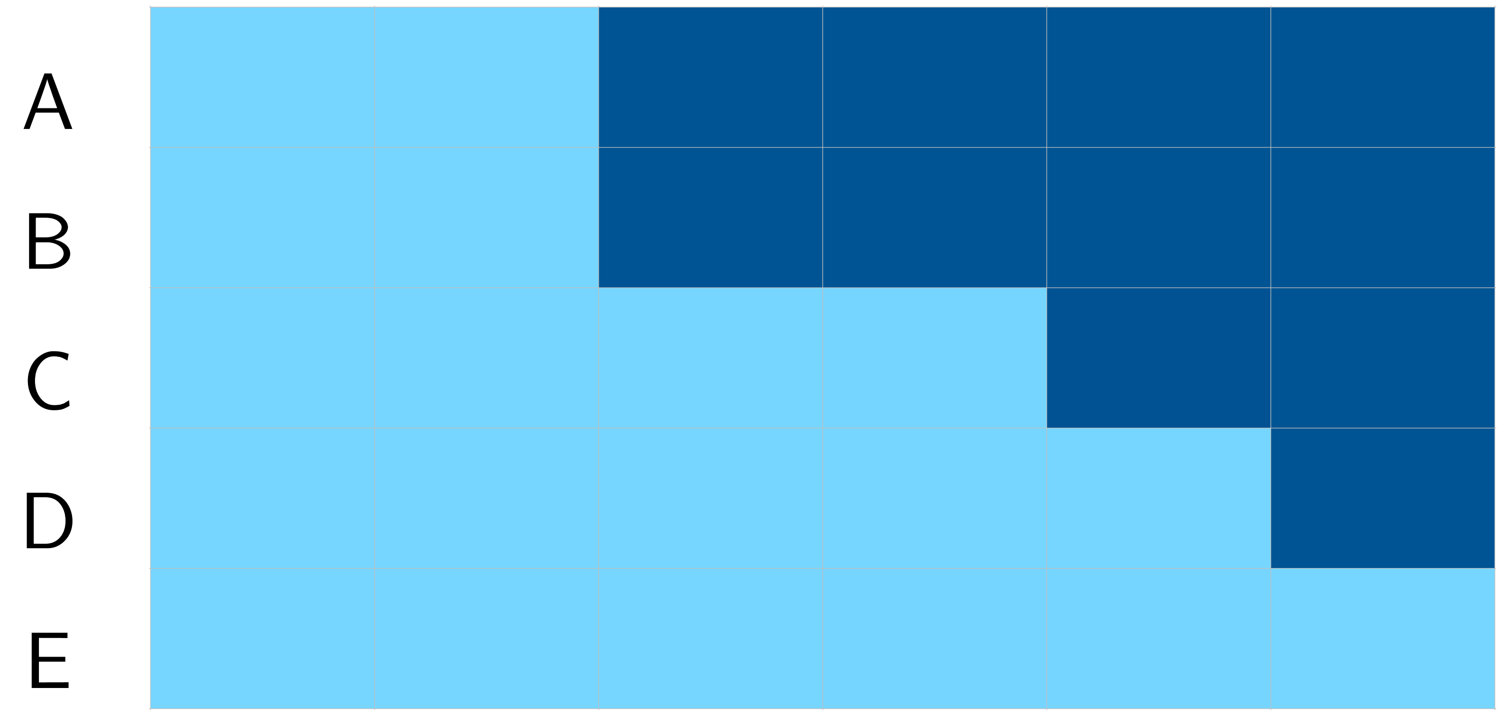


# Stacked DID: Cengiz et al. (2019)



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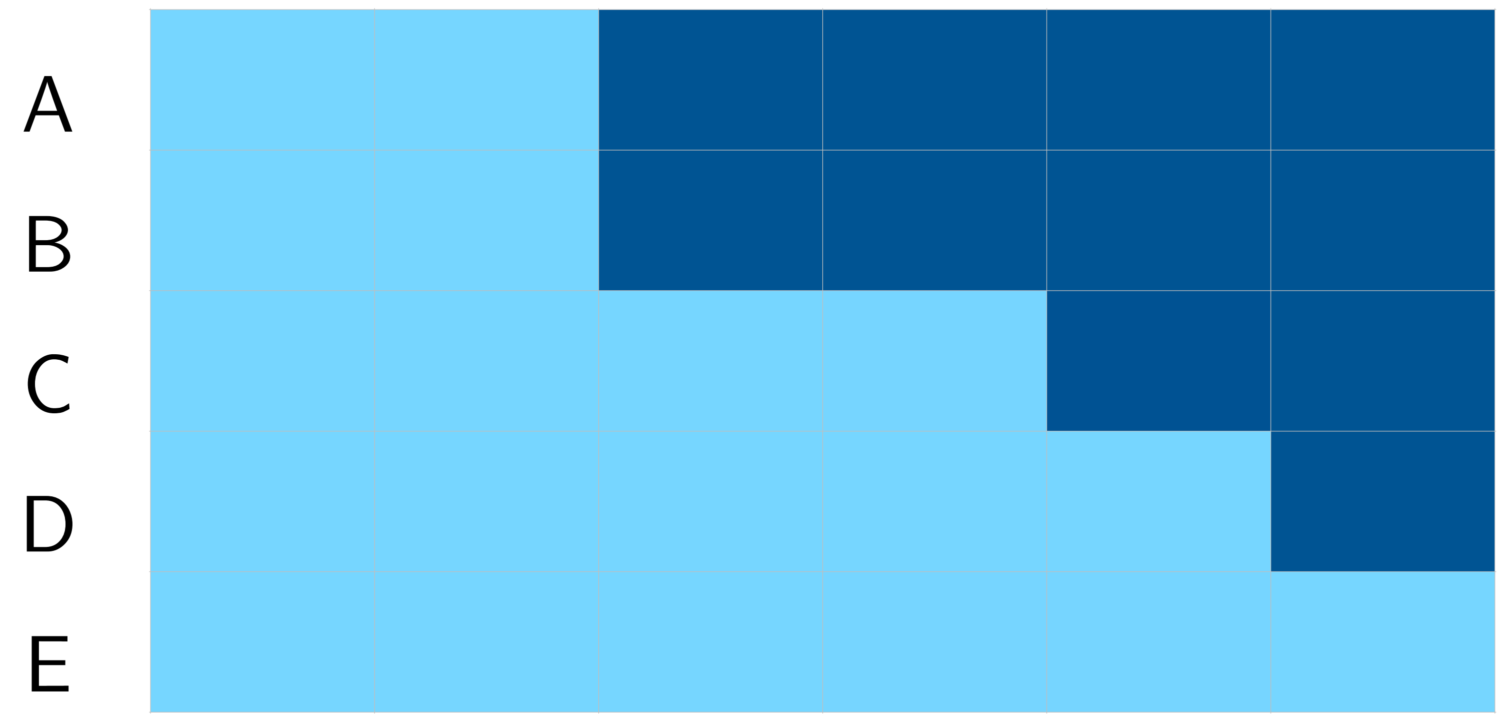


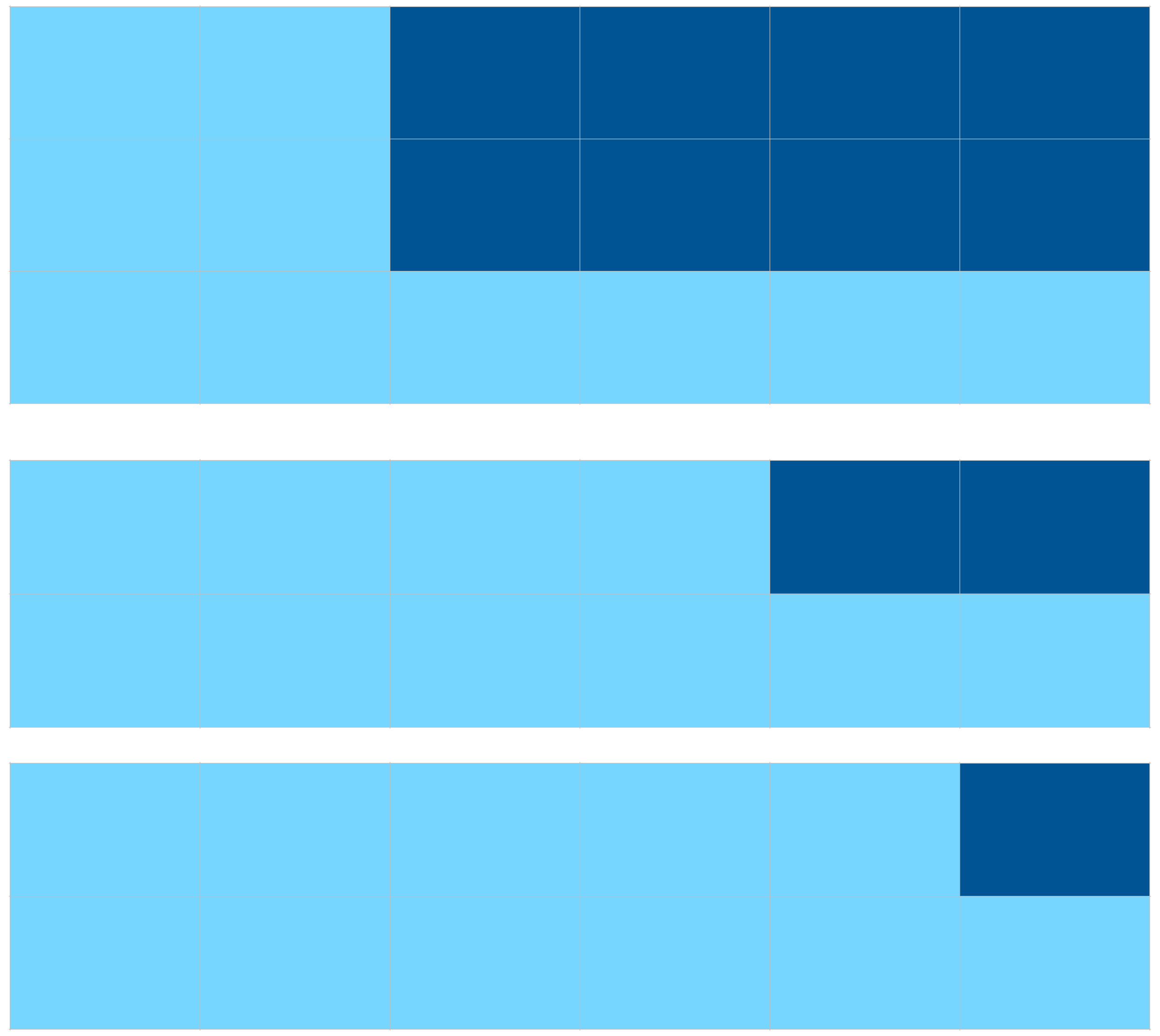

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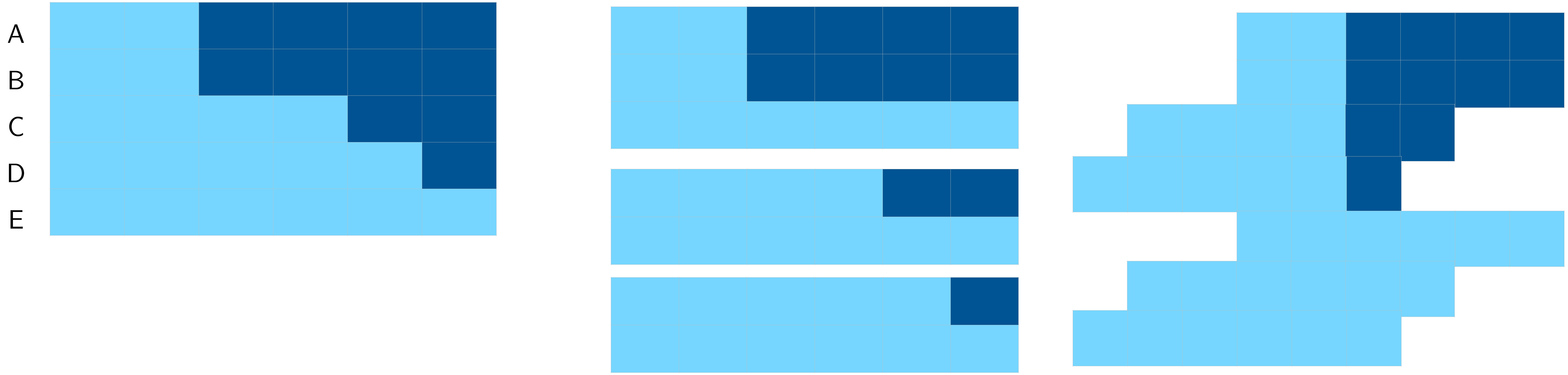
- Duplicate the pure control group for each cohort
- "Stack" on top of each other, align by relative time to treatment onset



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- Duplicate the pure control group for each cohort
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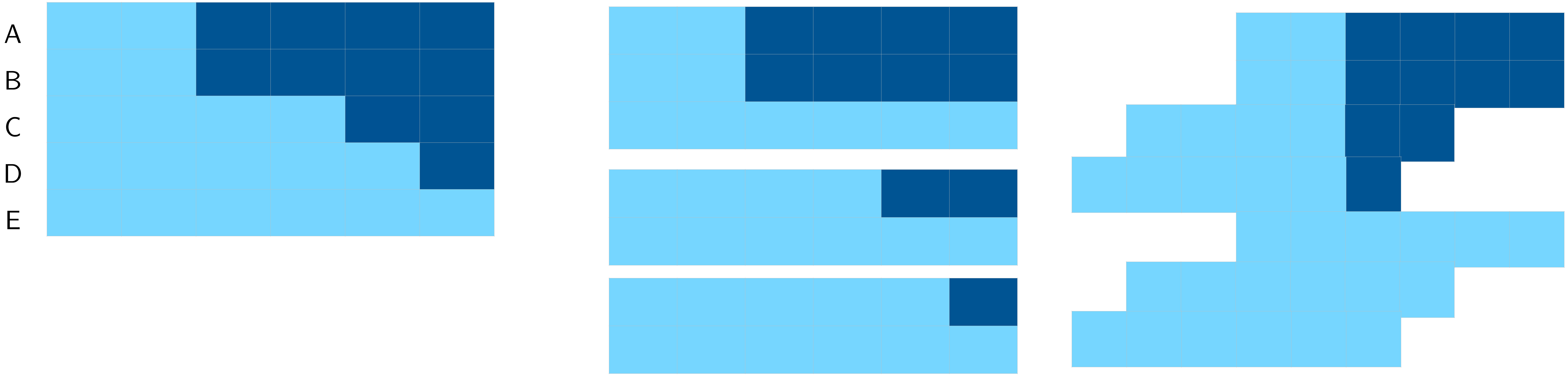

# Stacked DID: Cengiz et al. (2019)

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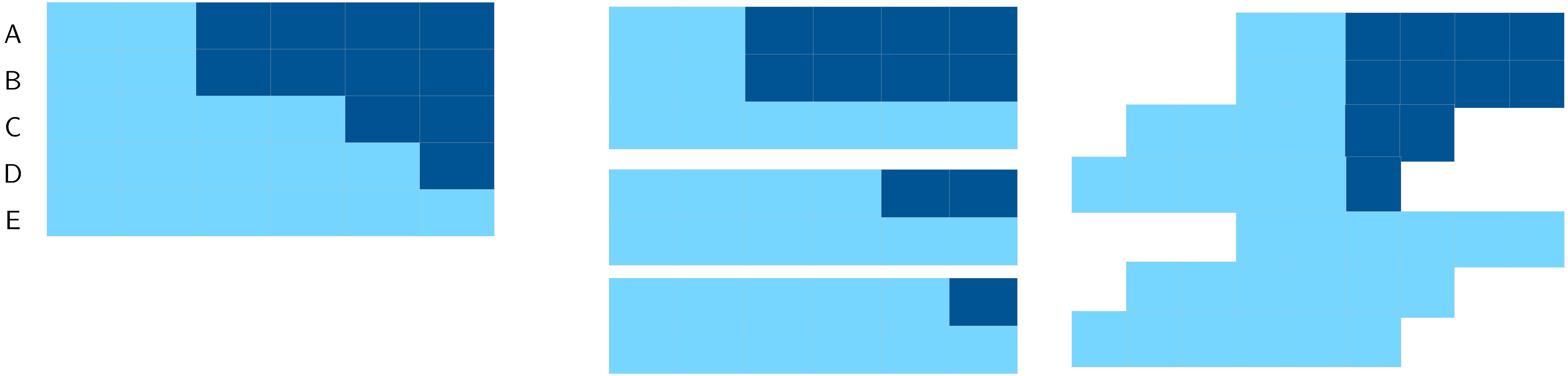
- Similar to IW with disproportionate weights



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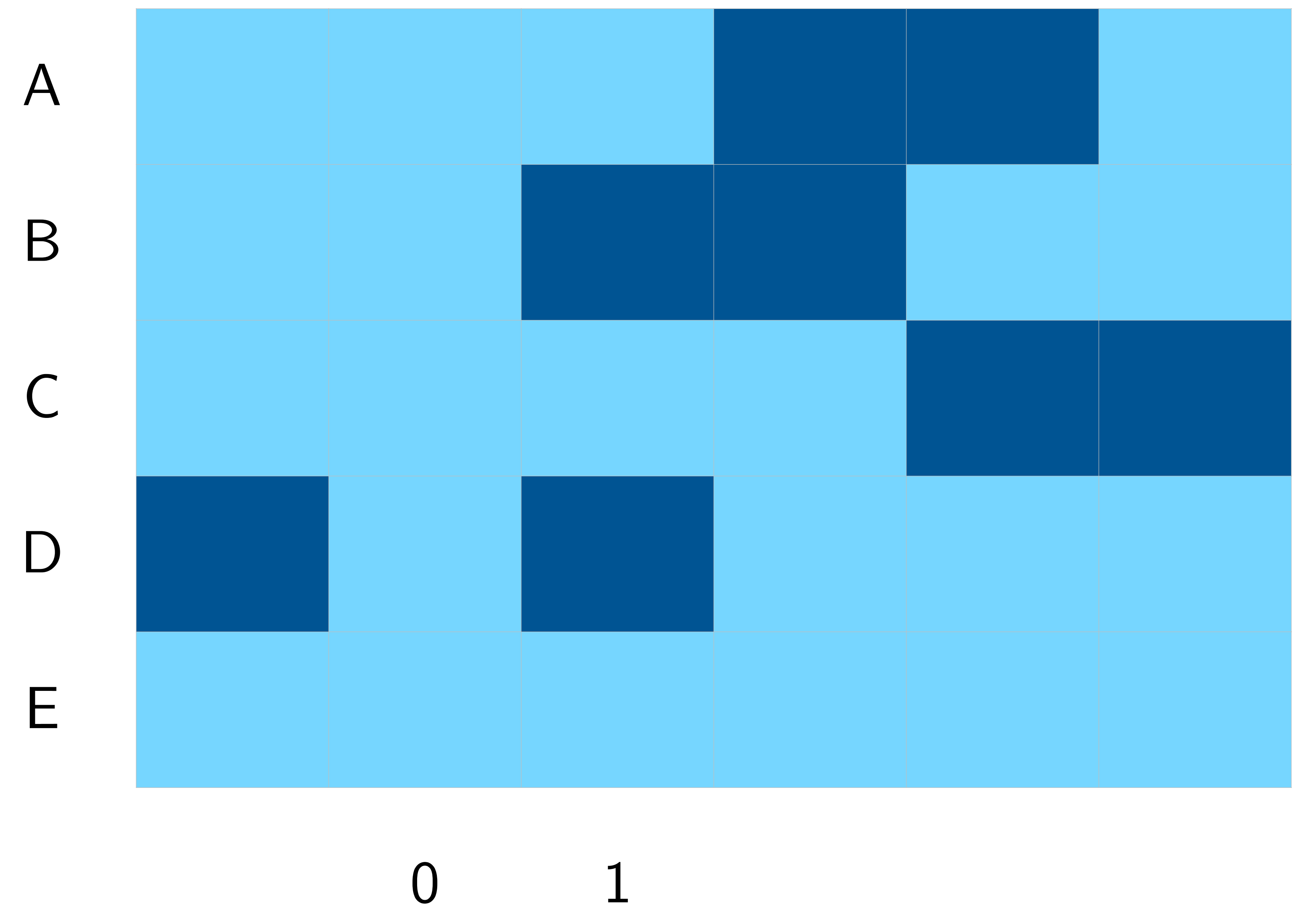

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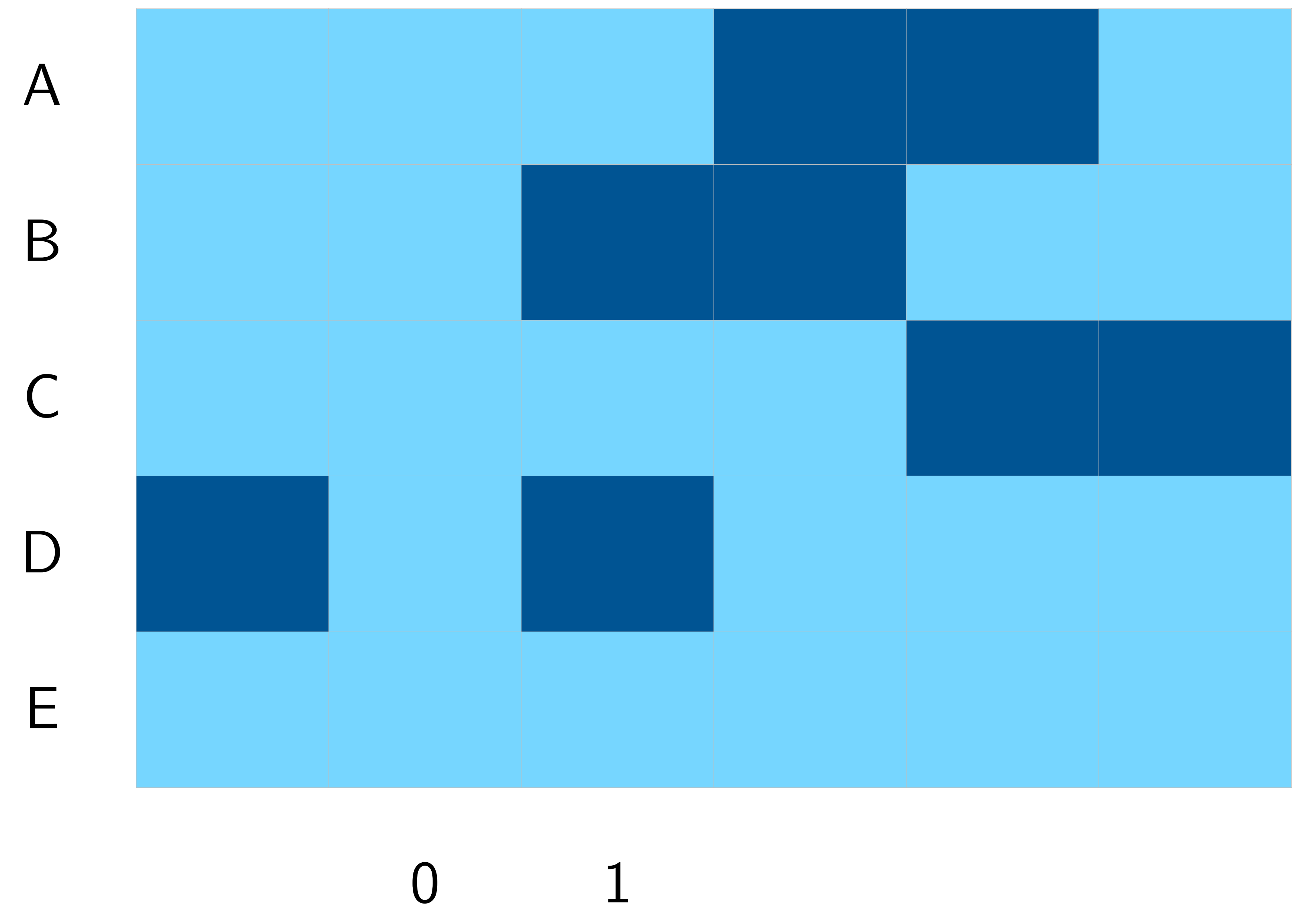

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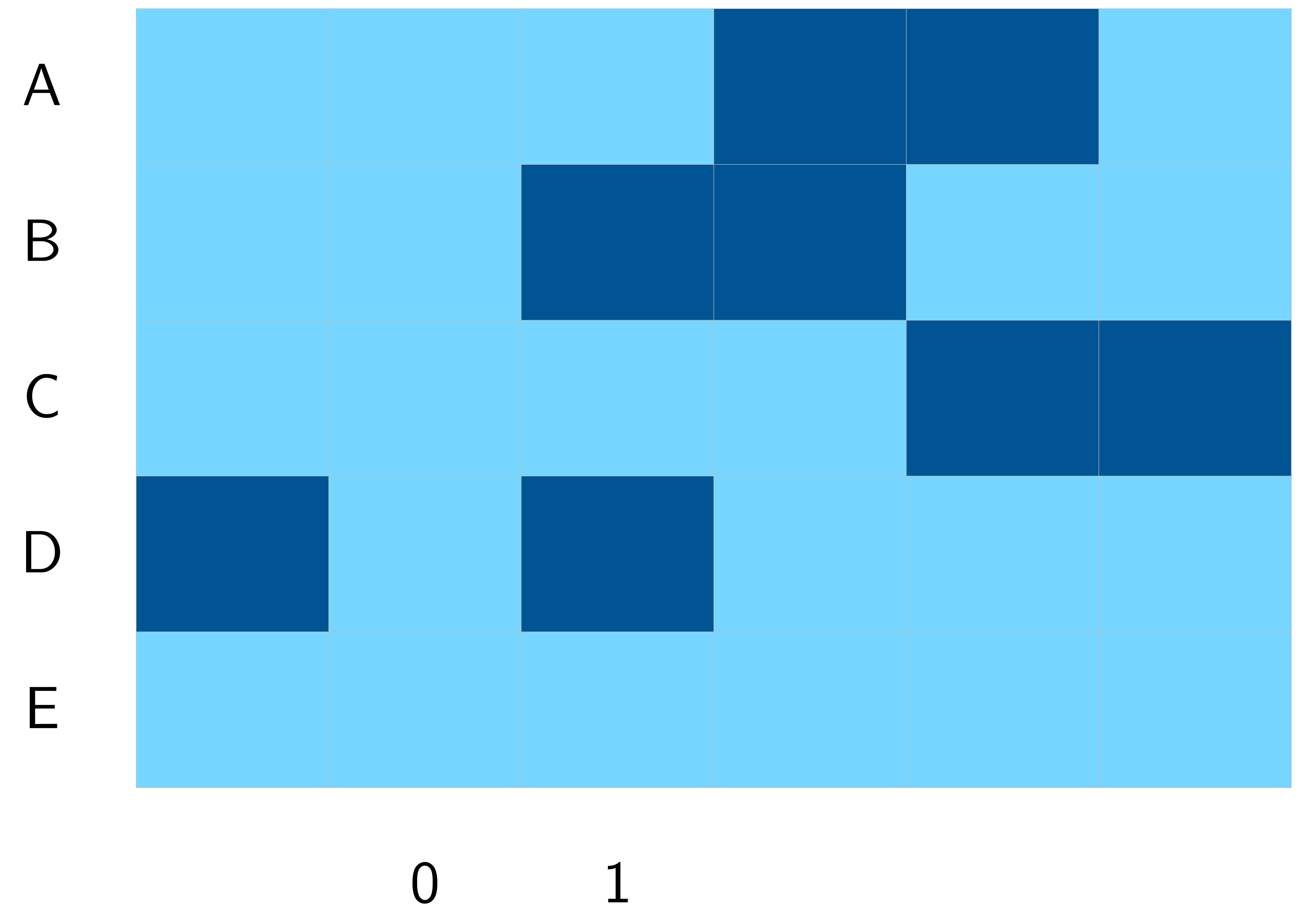

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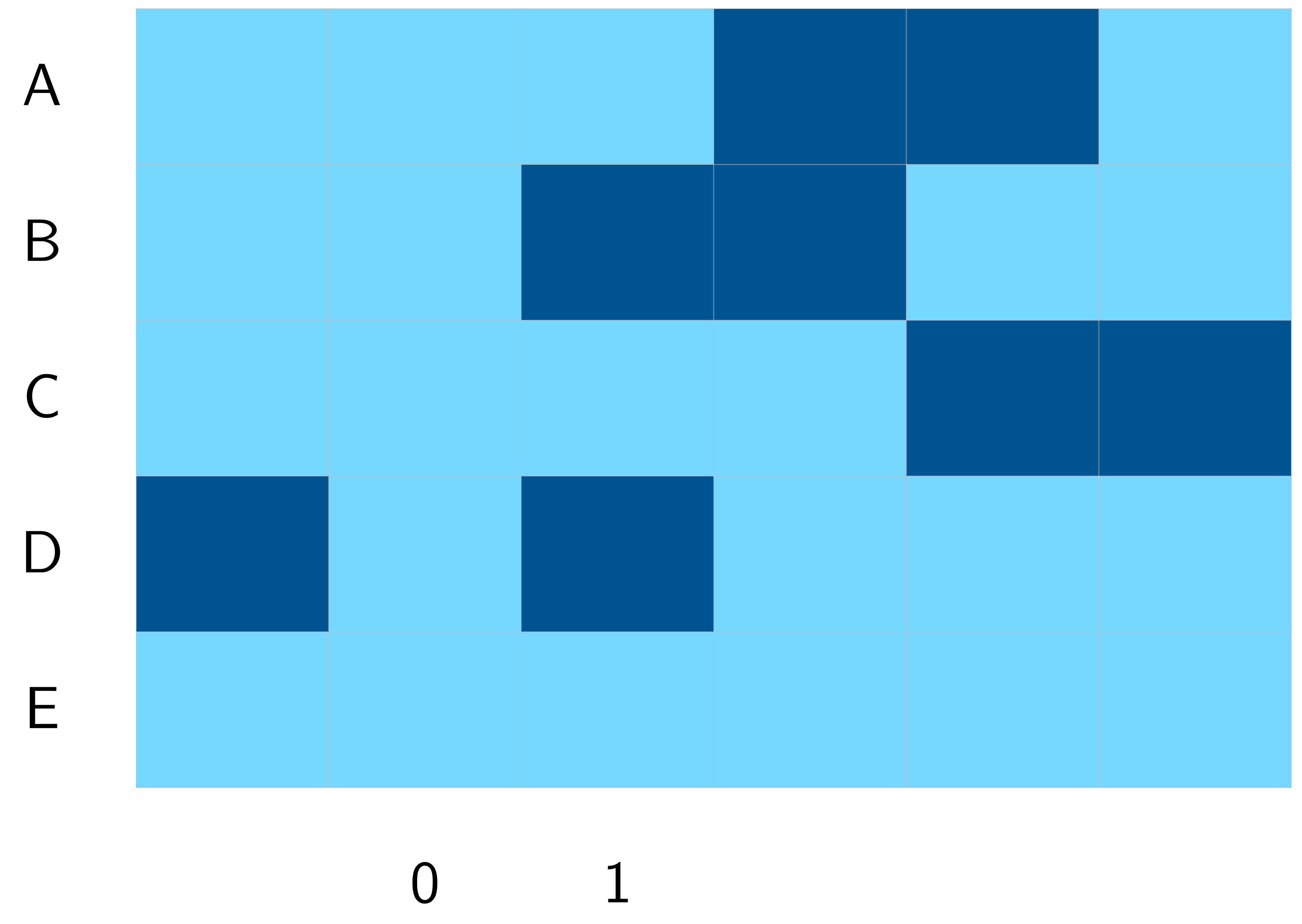

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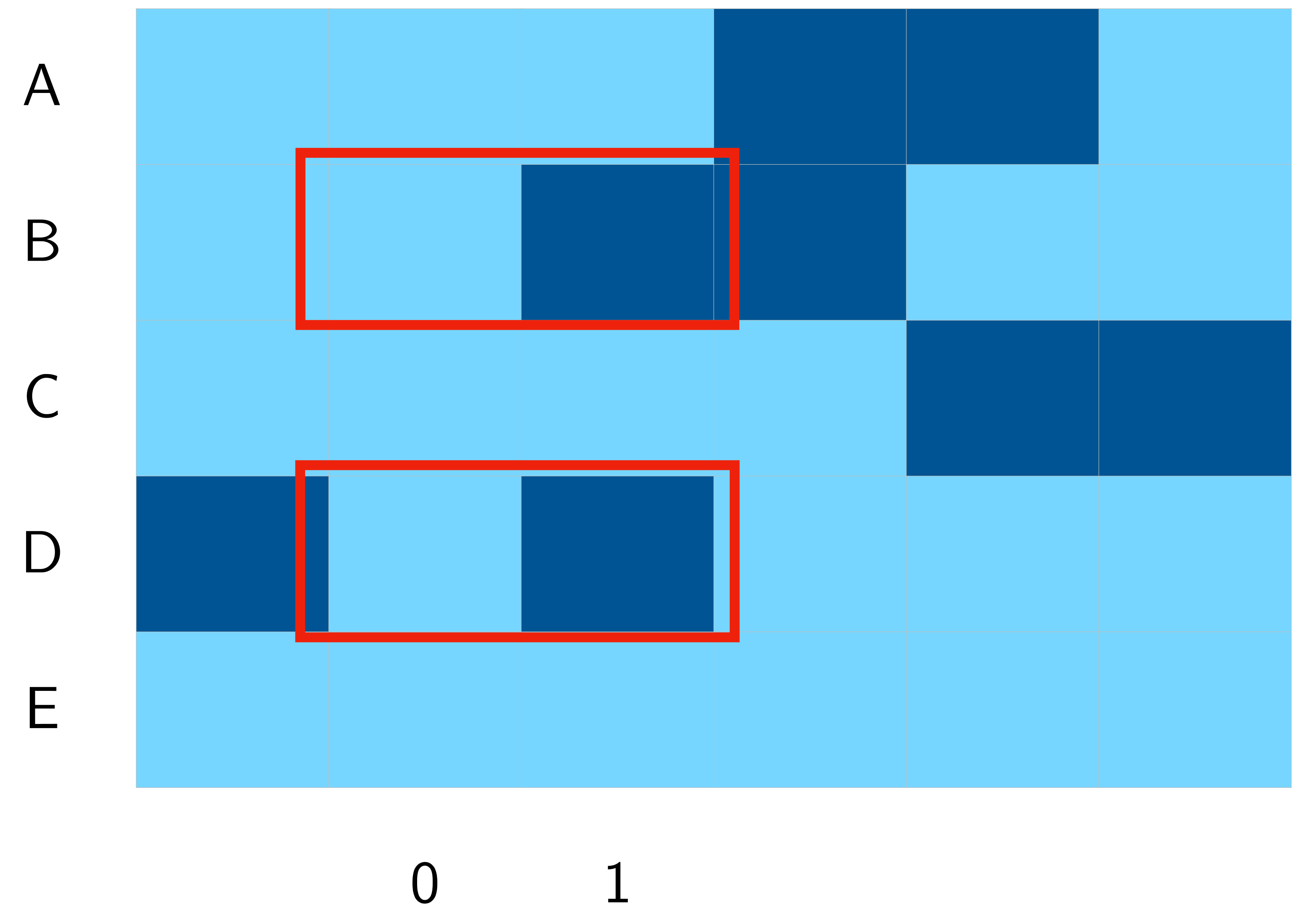

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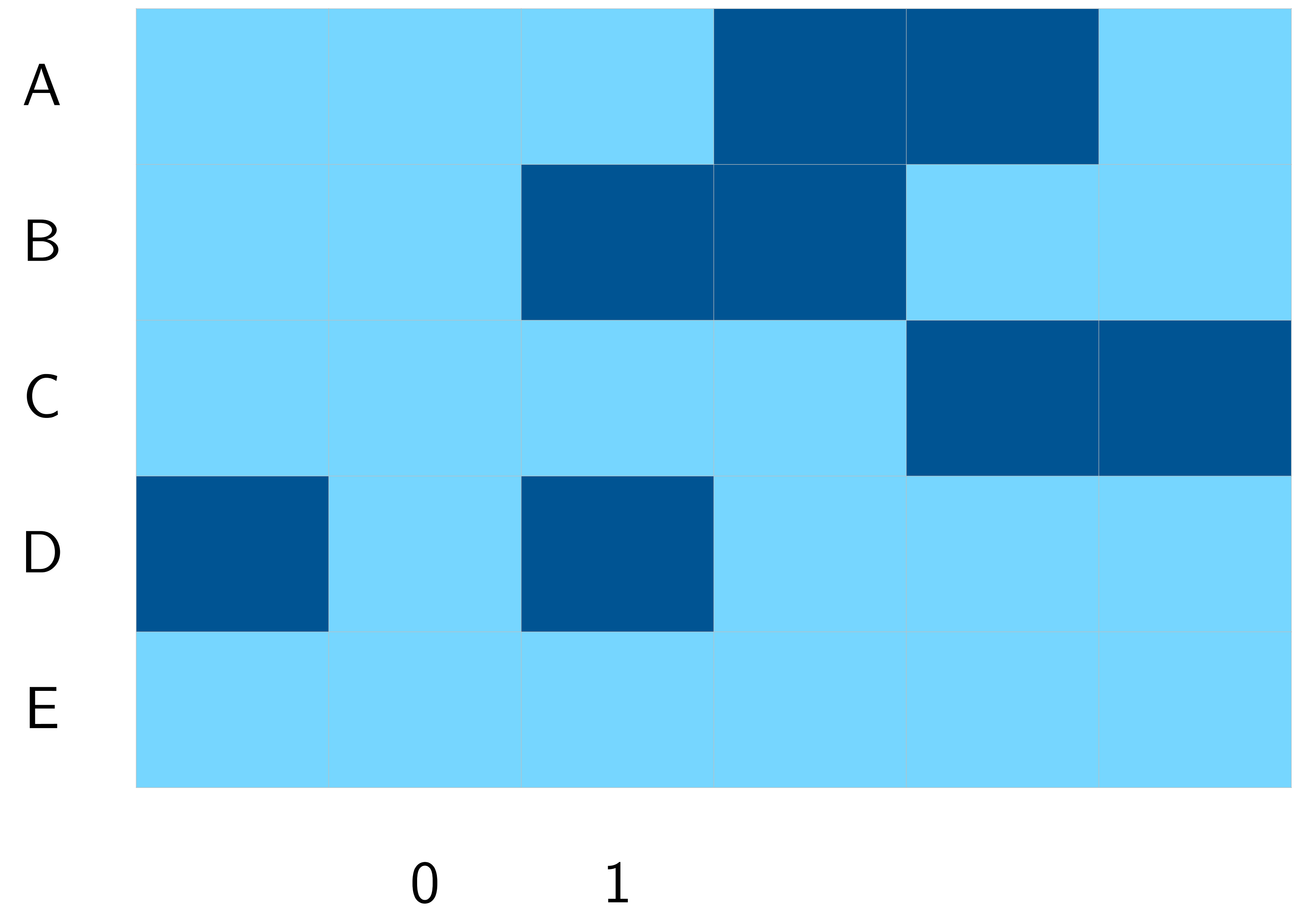

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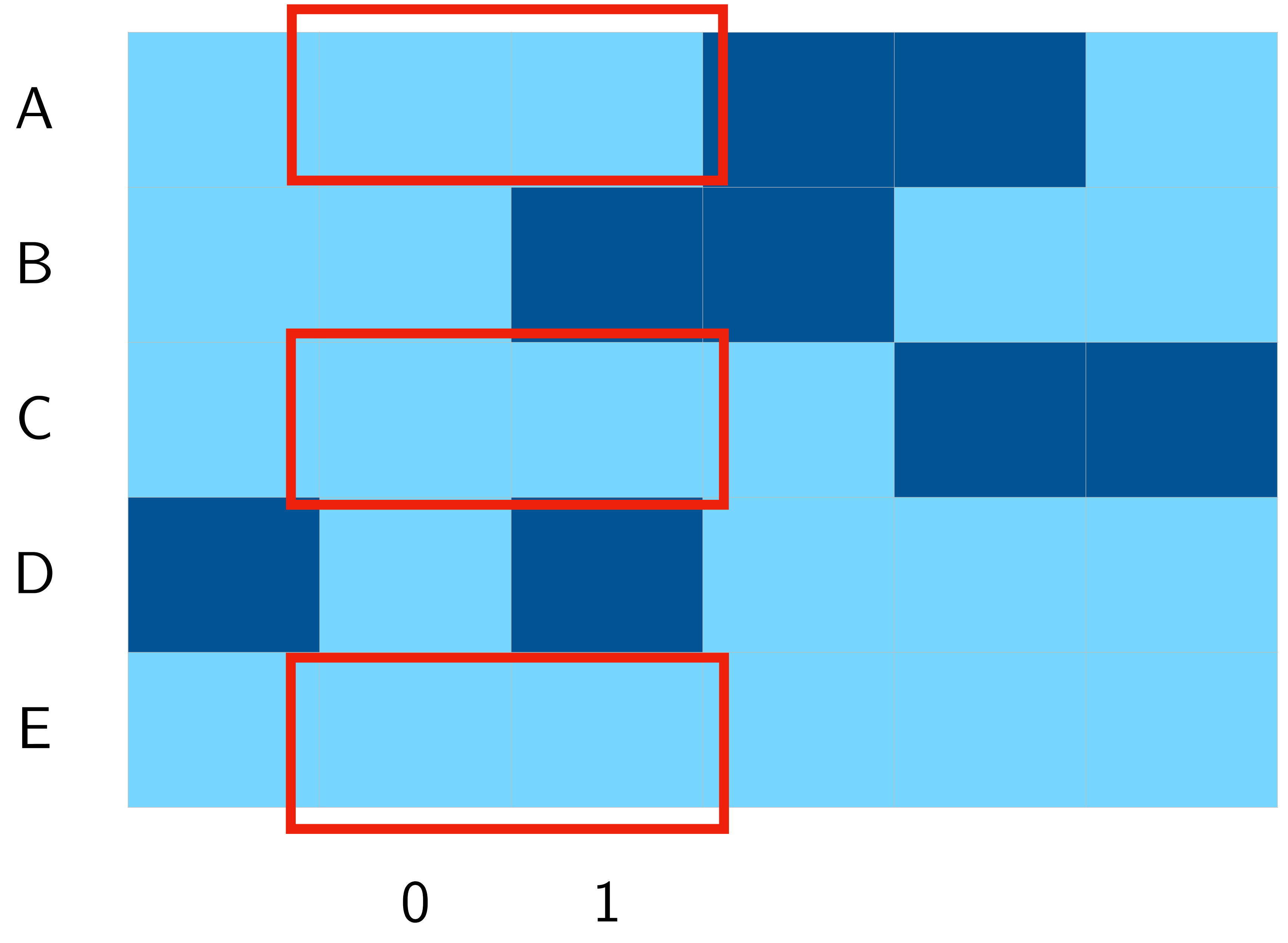

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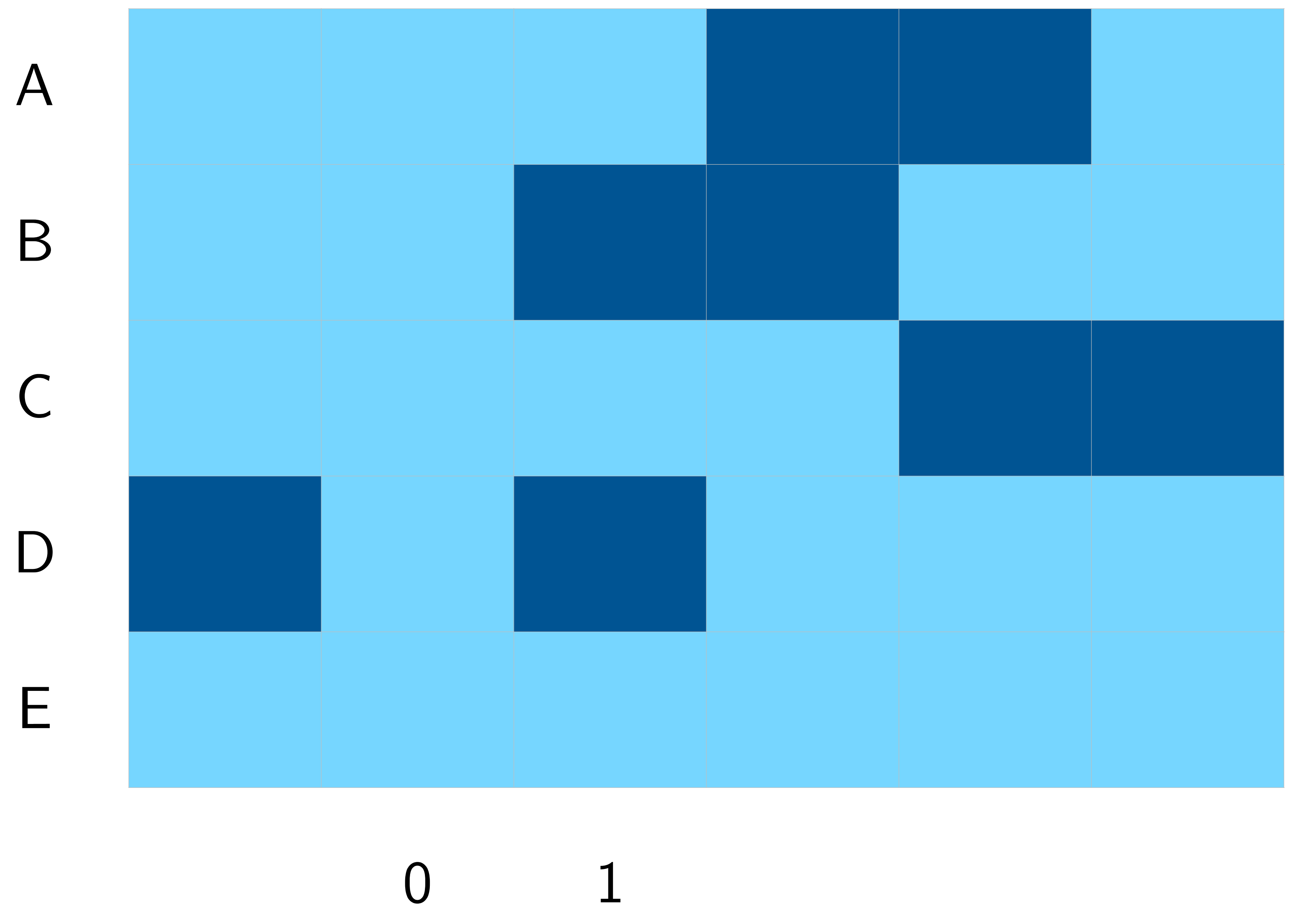

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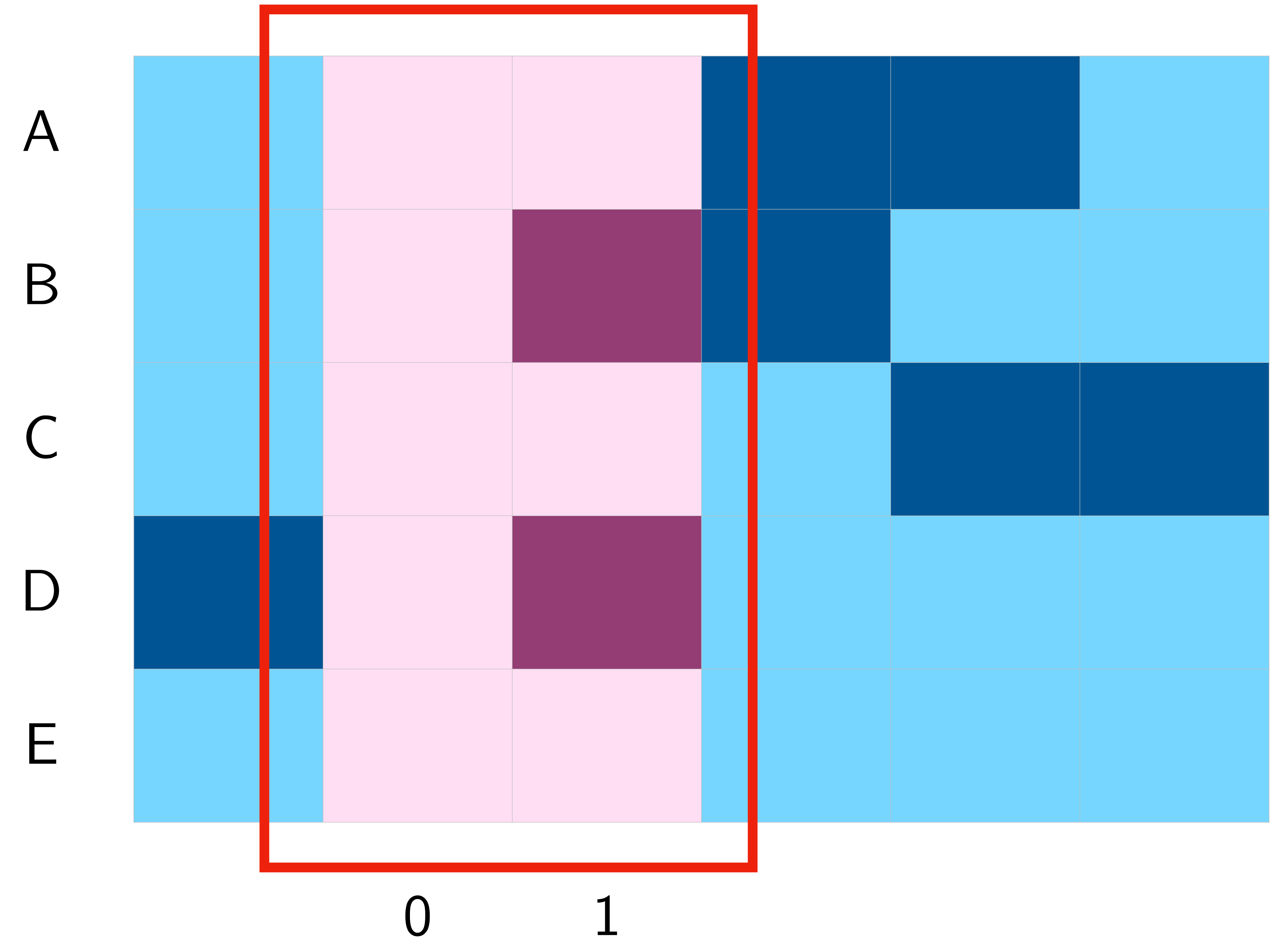

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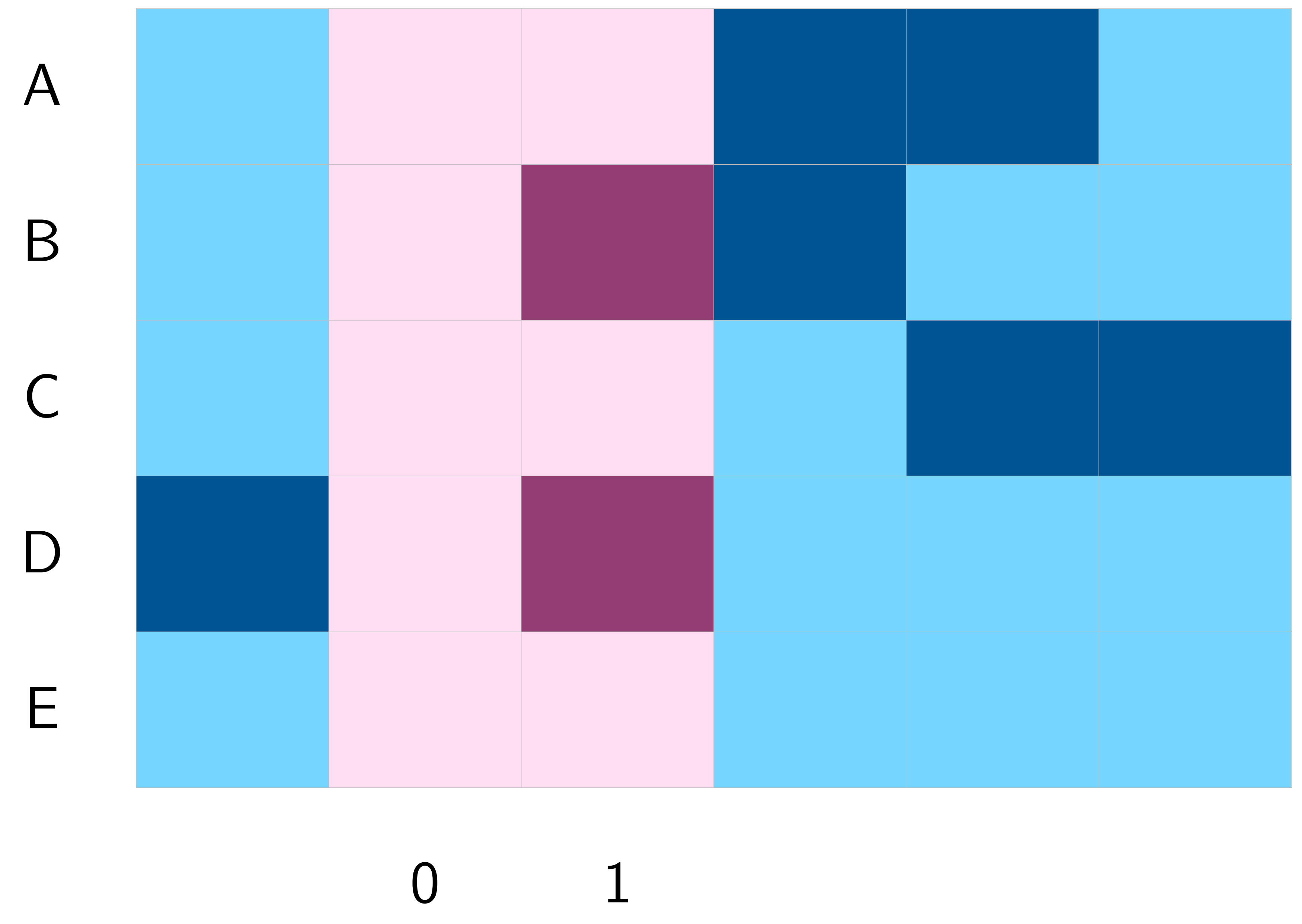

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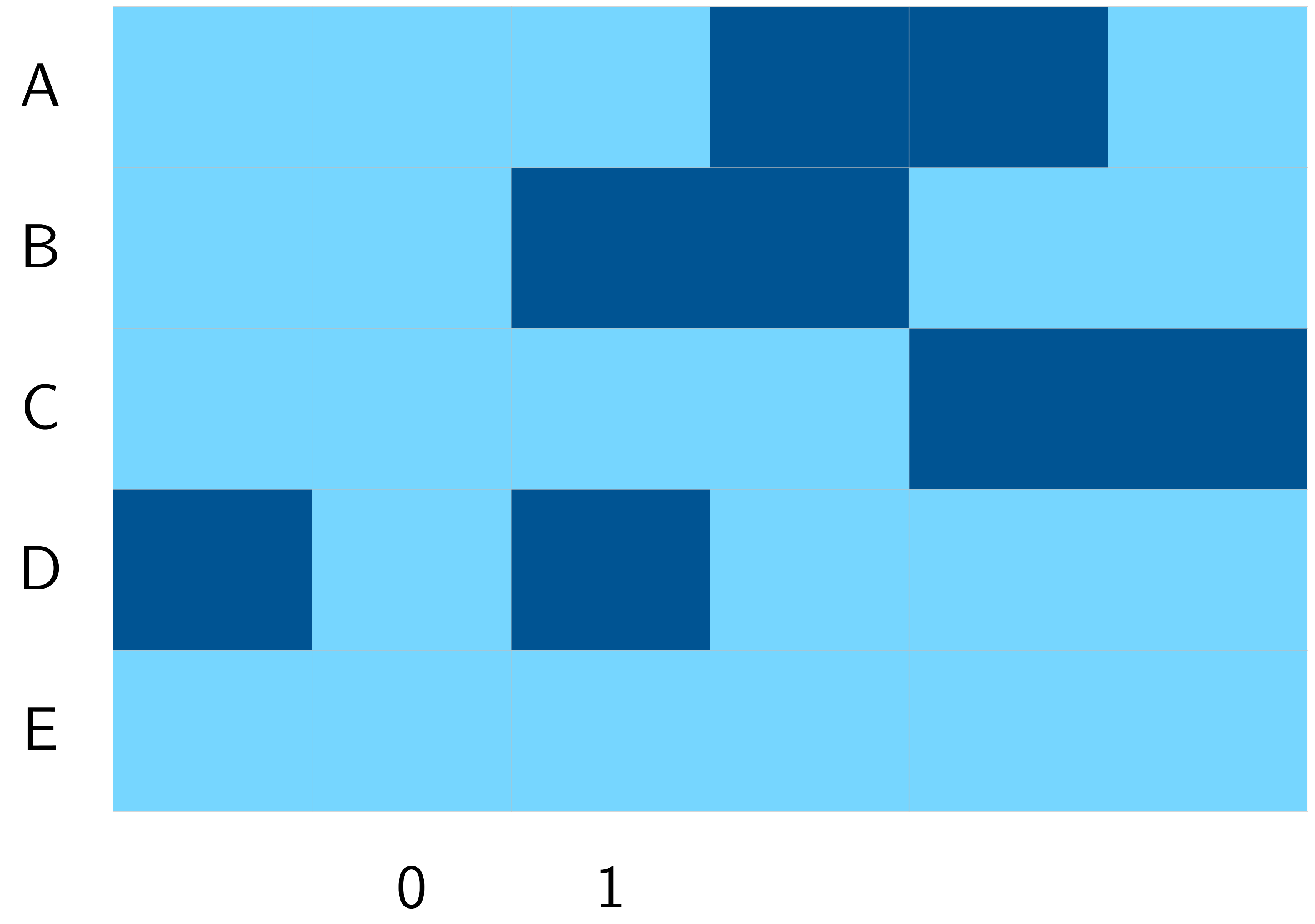

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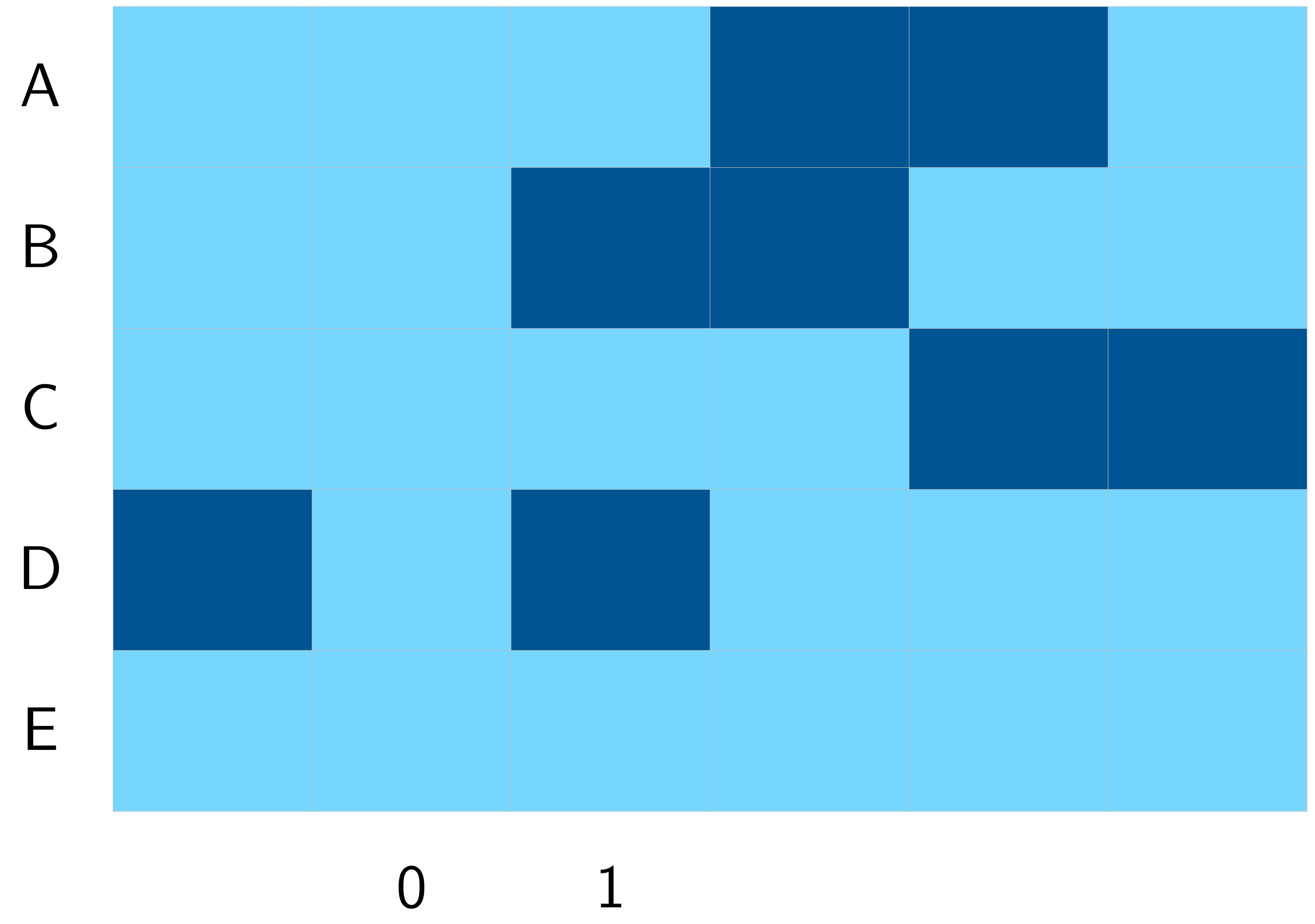

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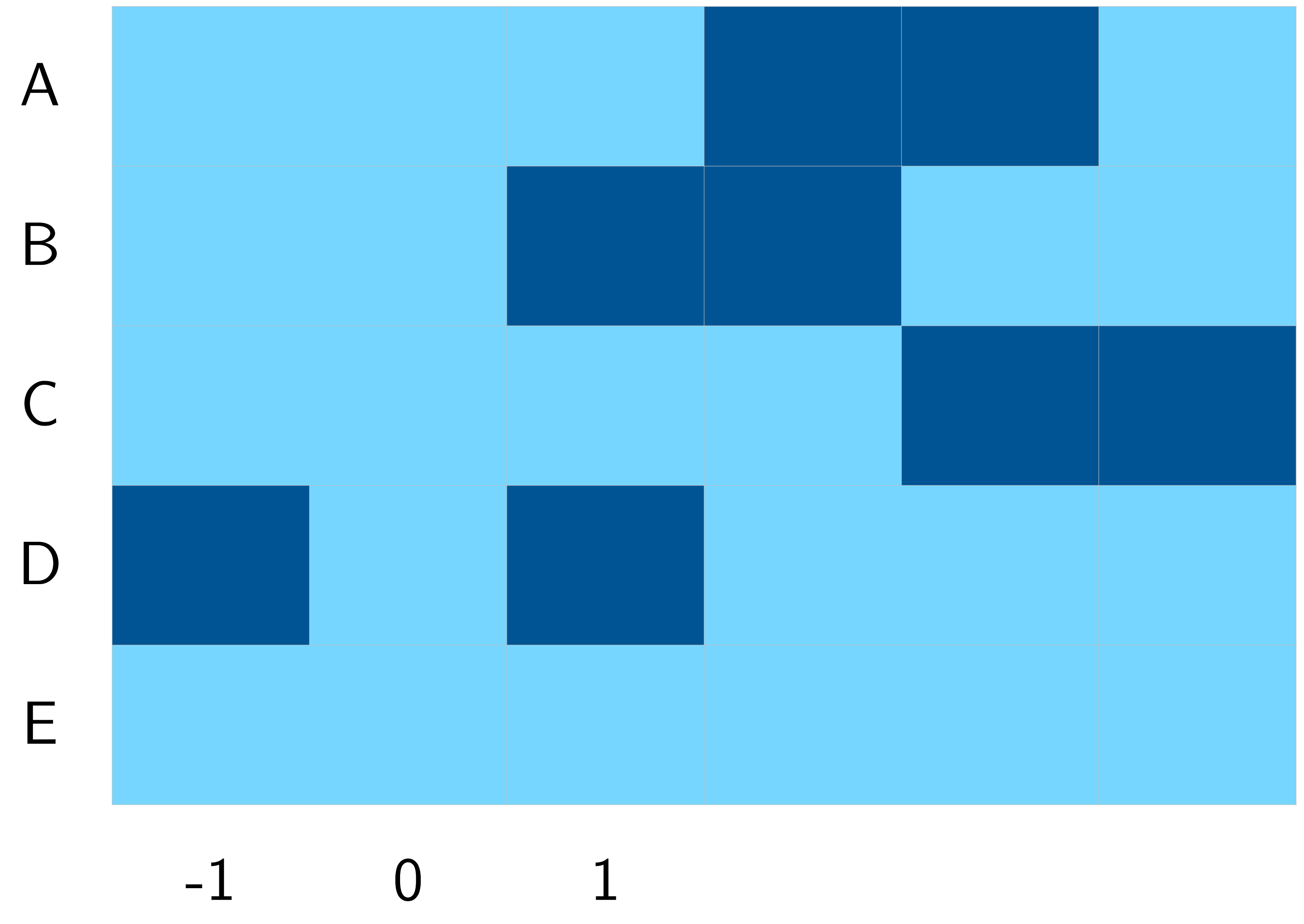

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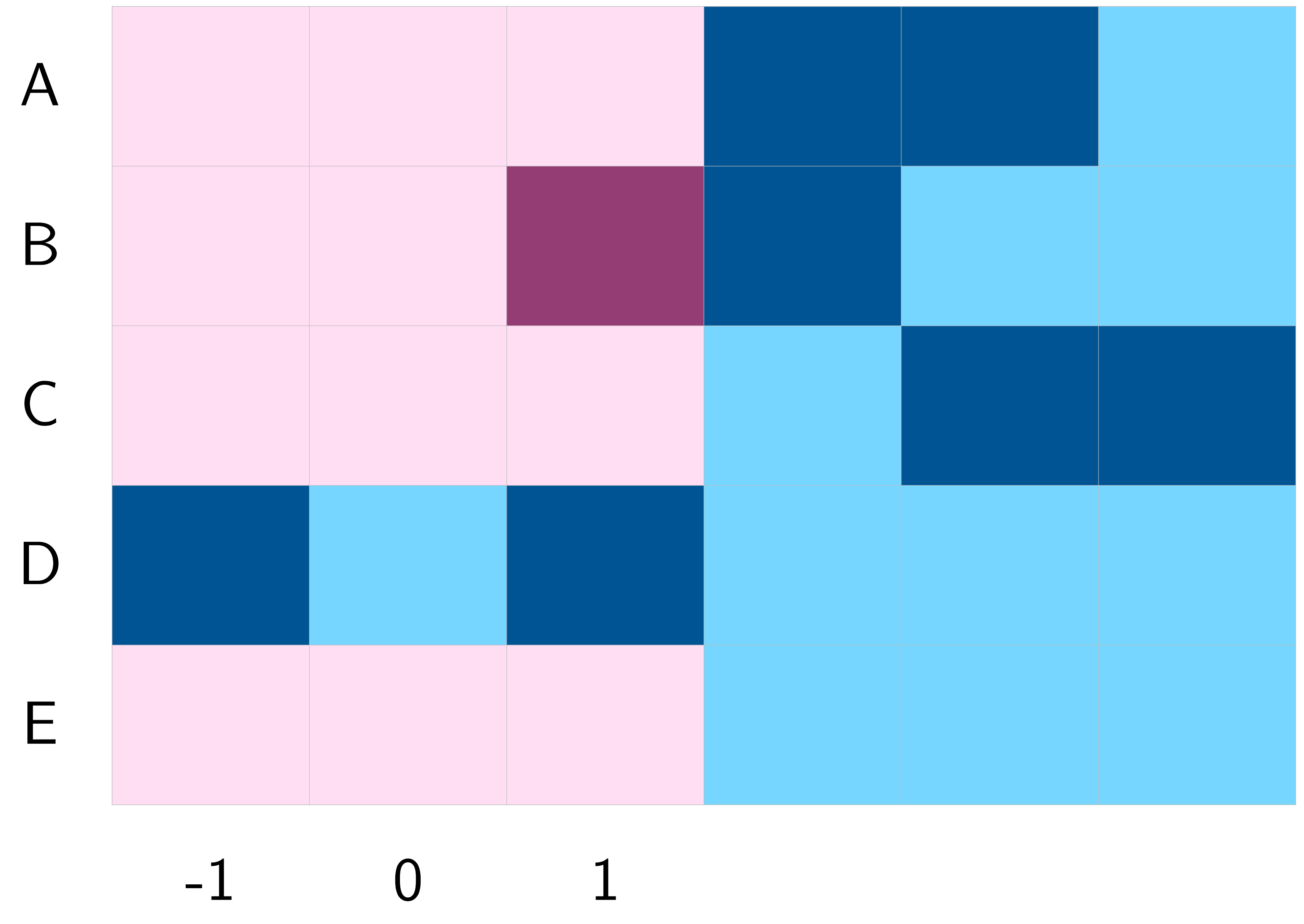

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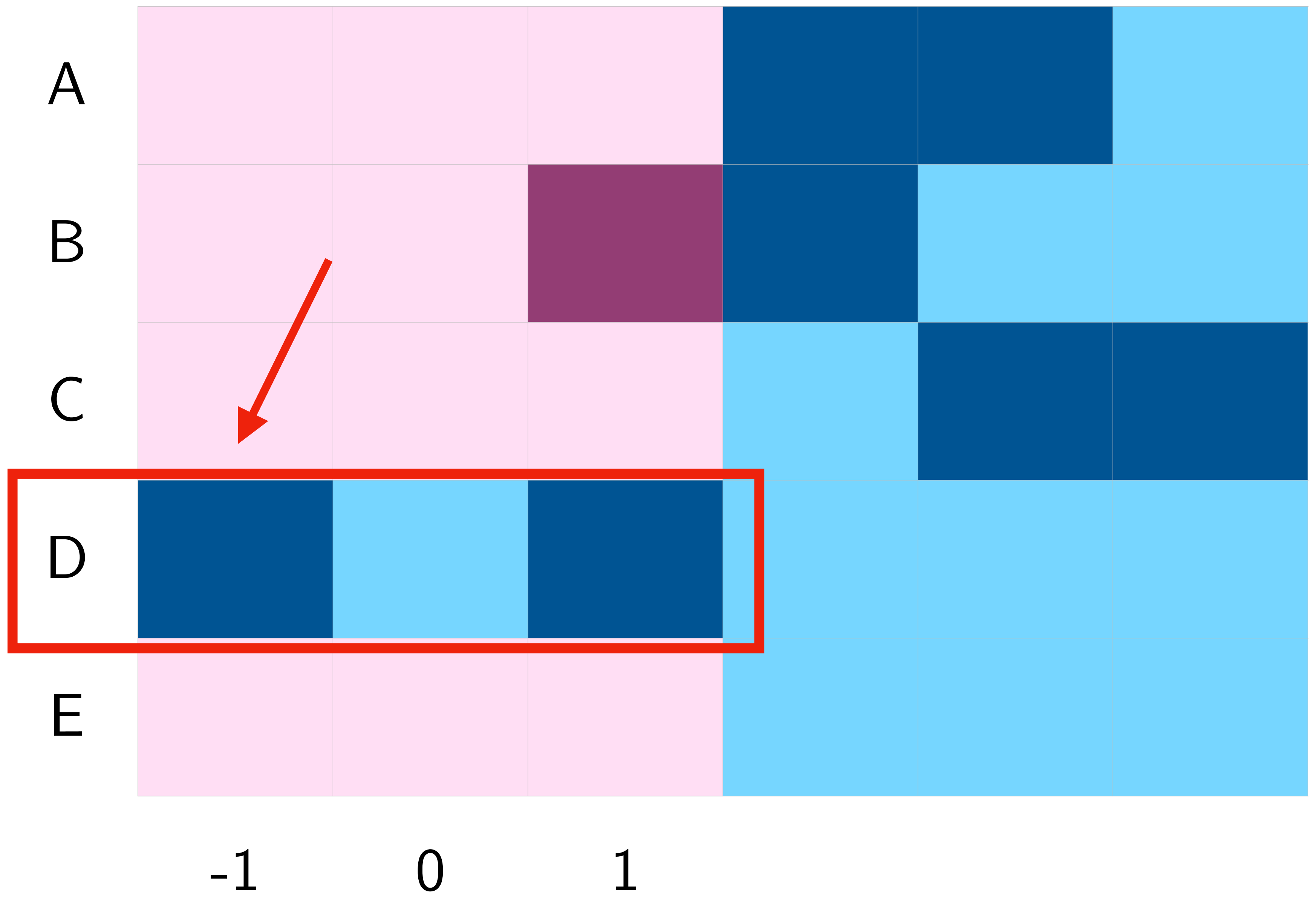

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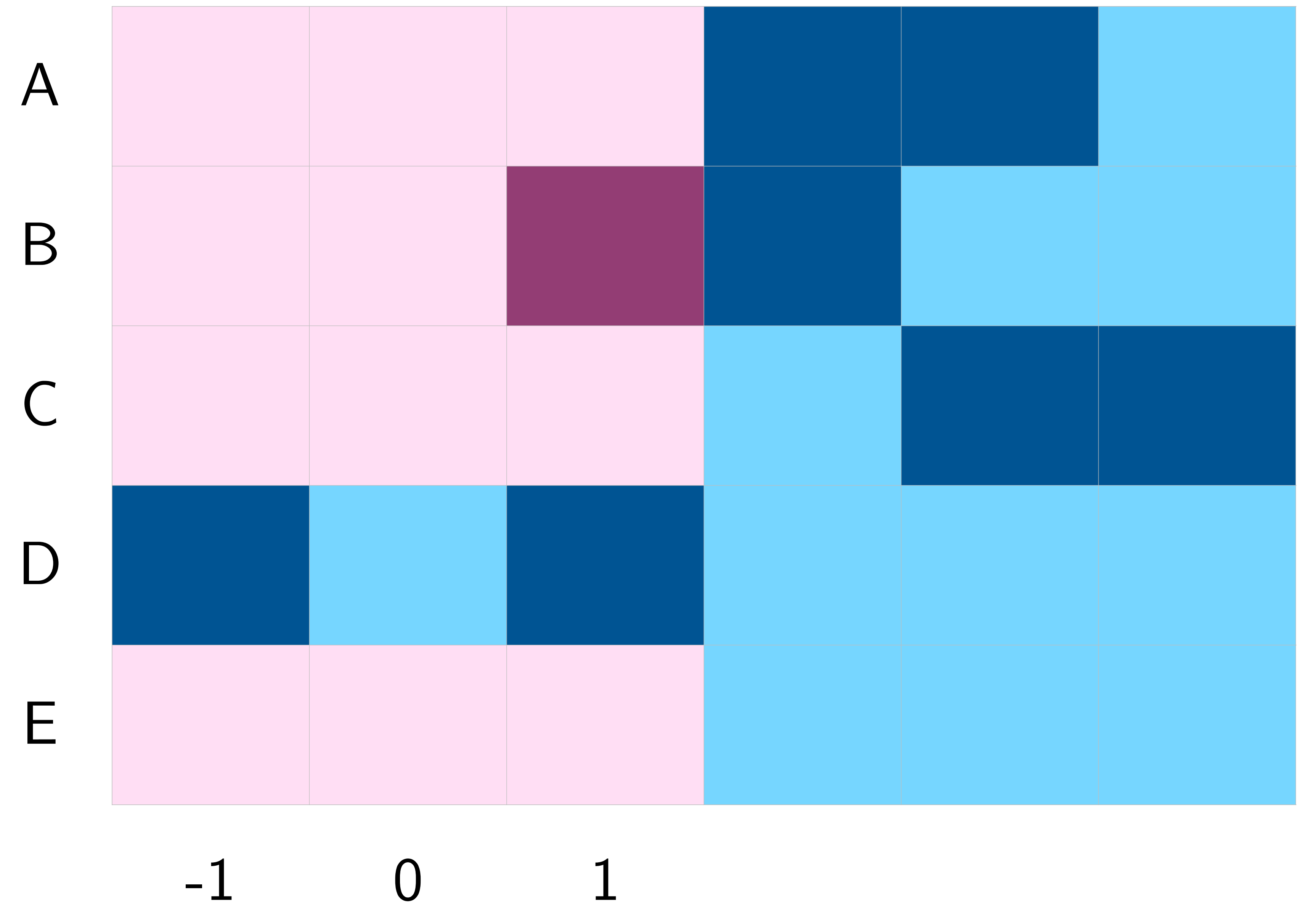

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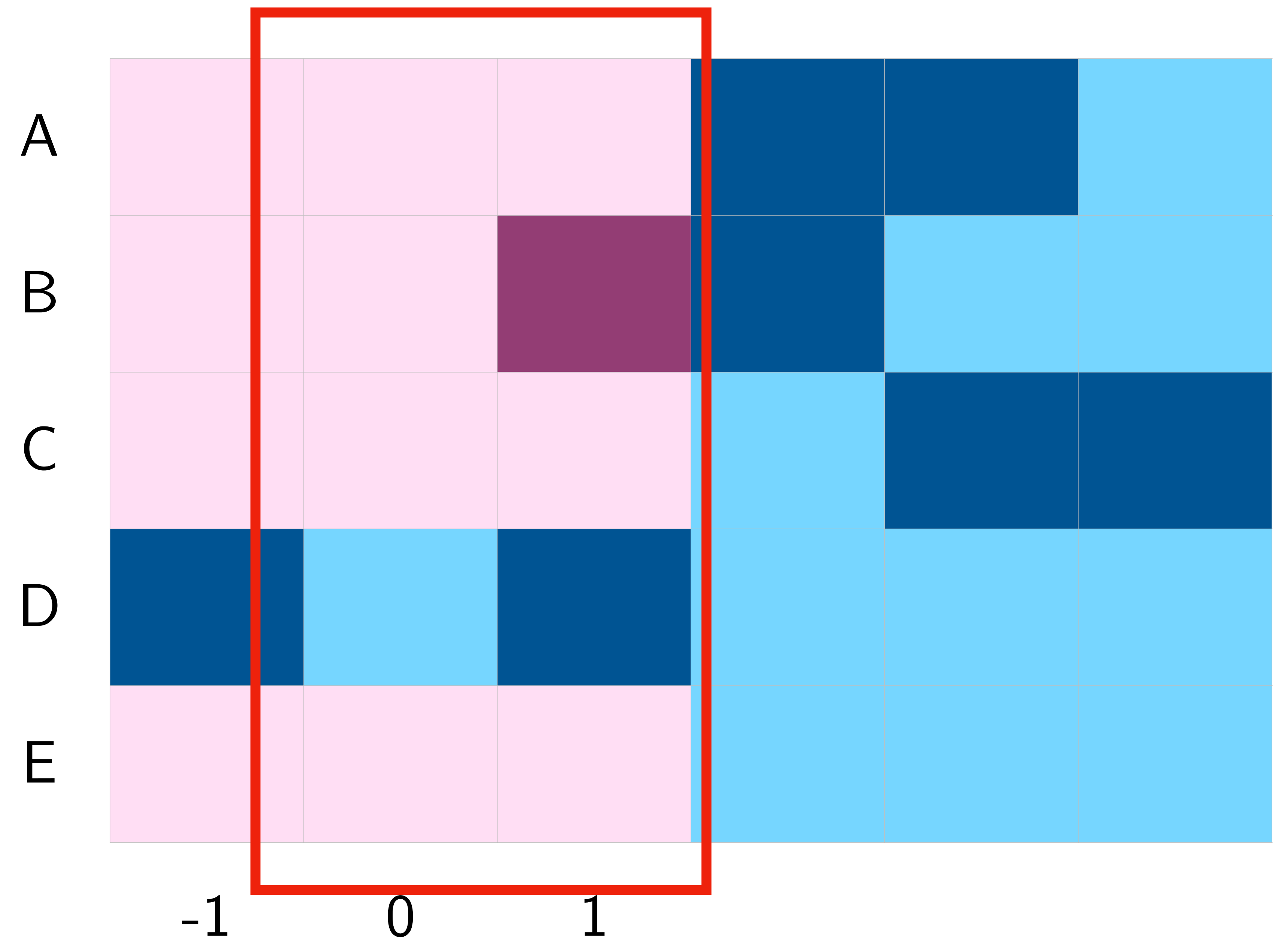

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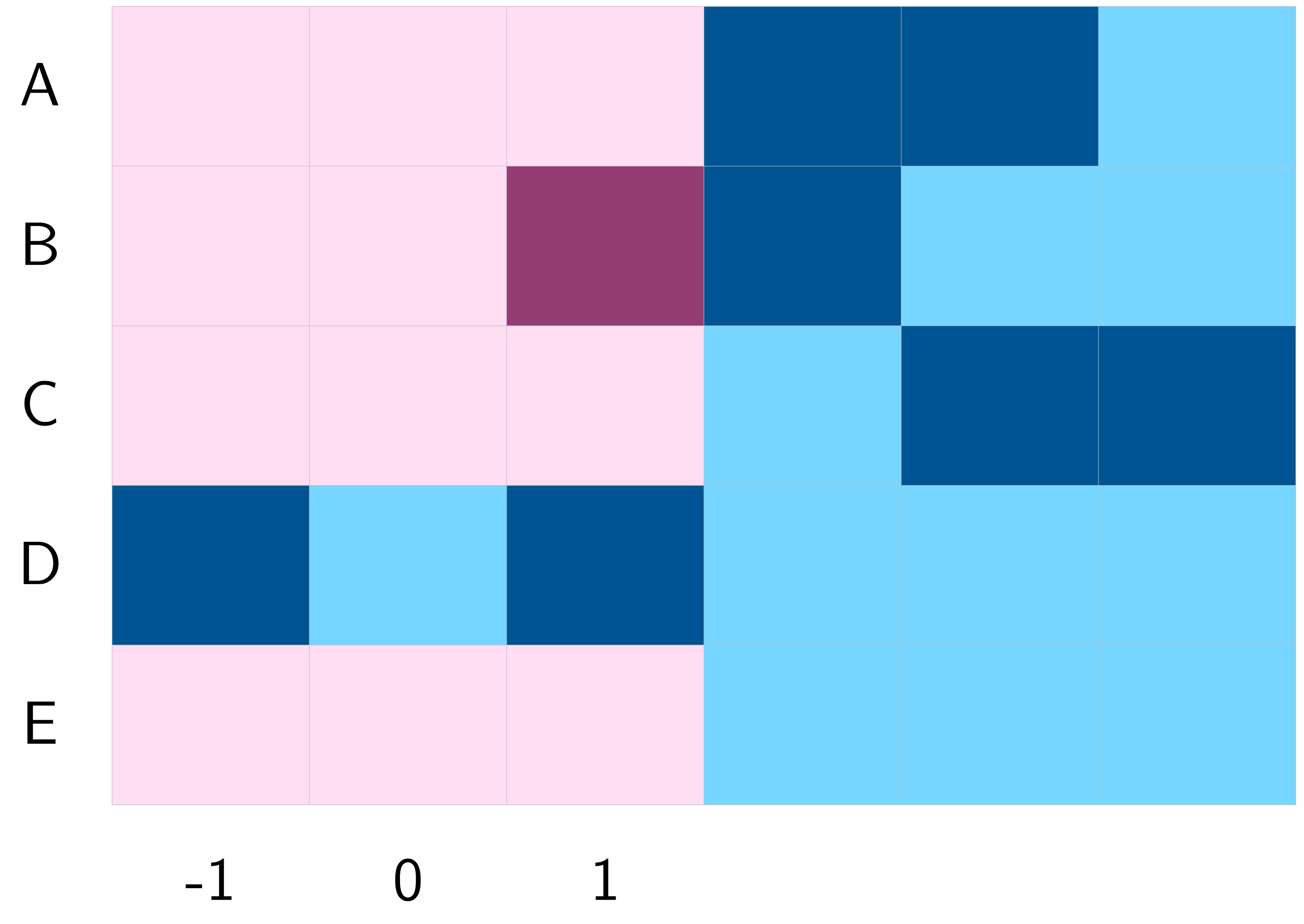

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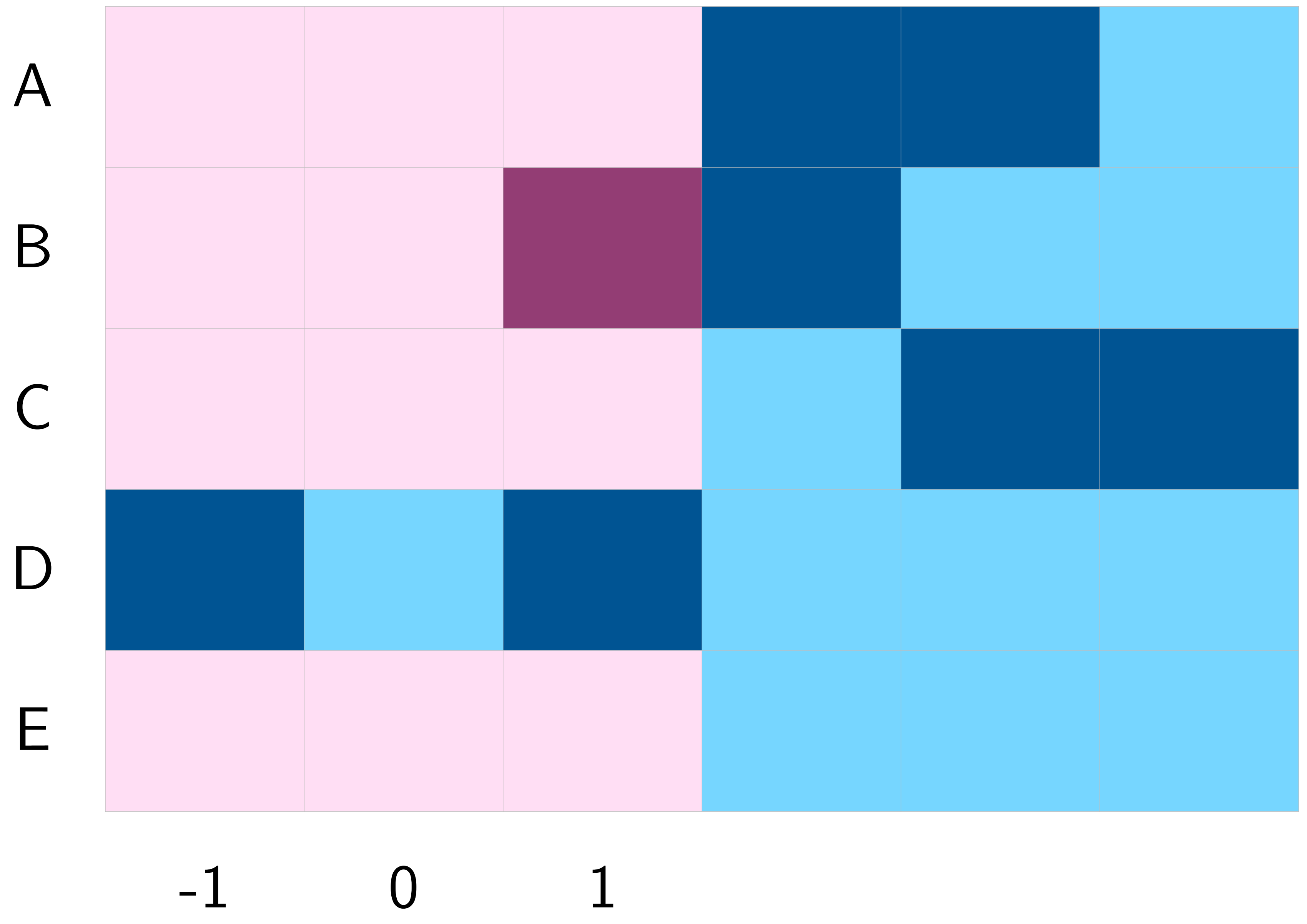

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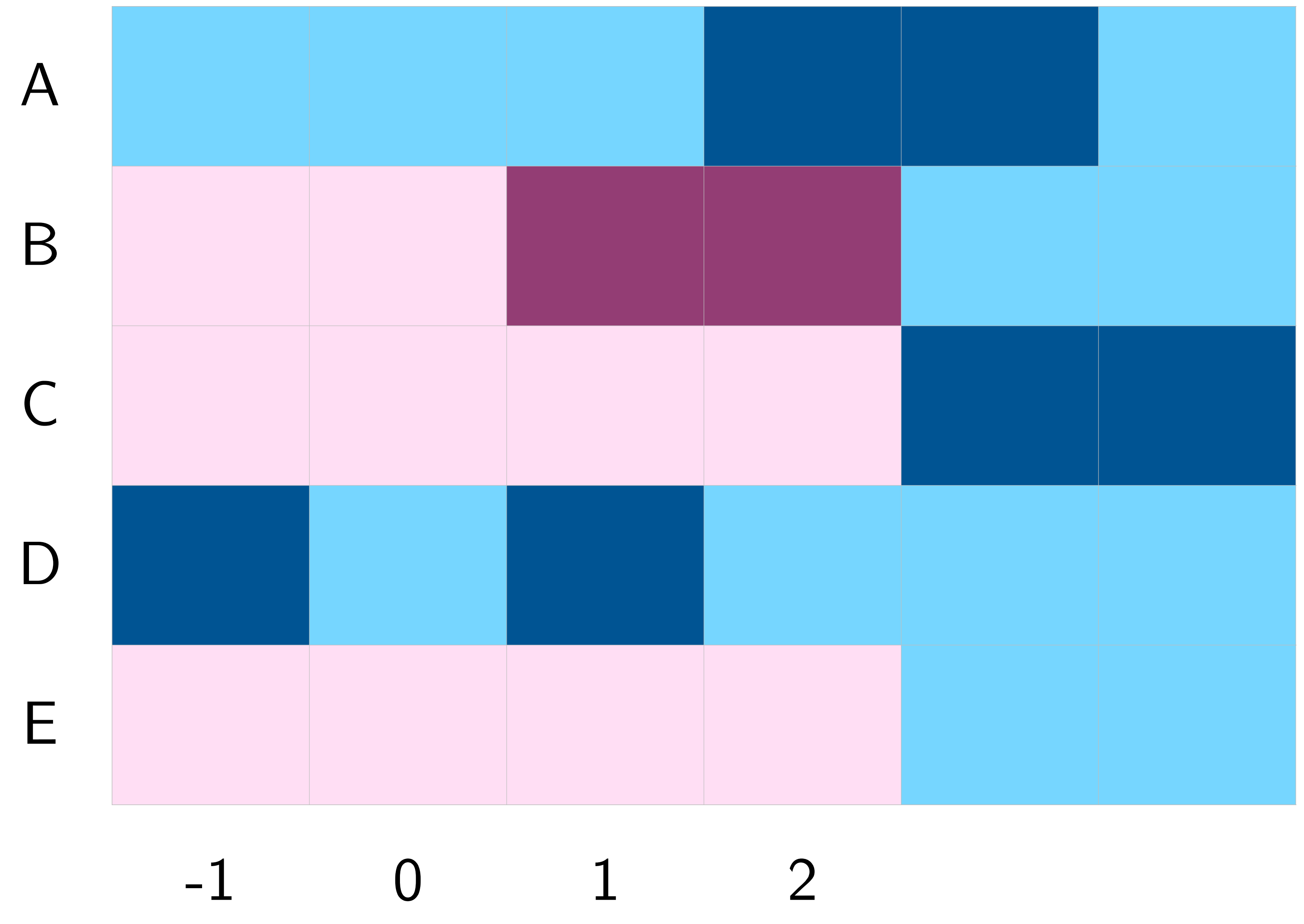

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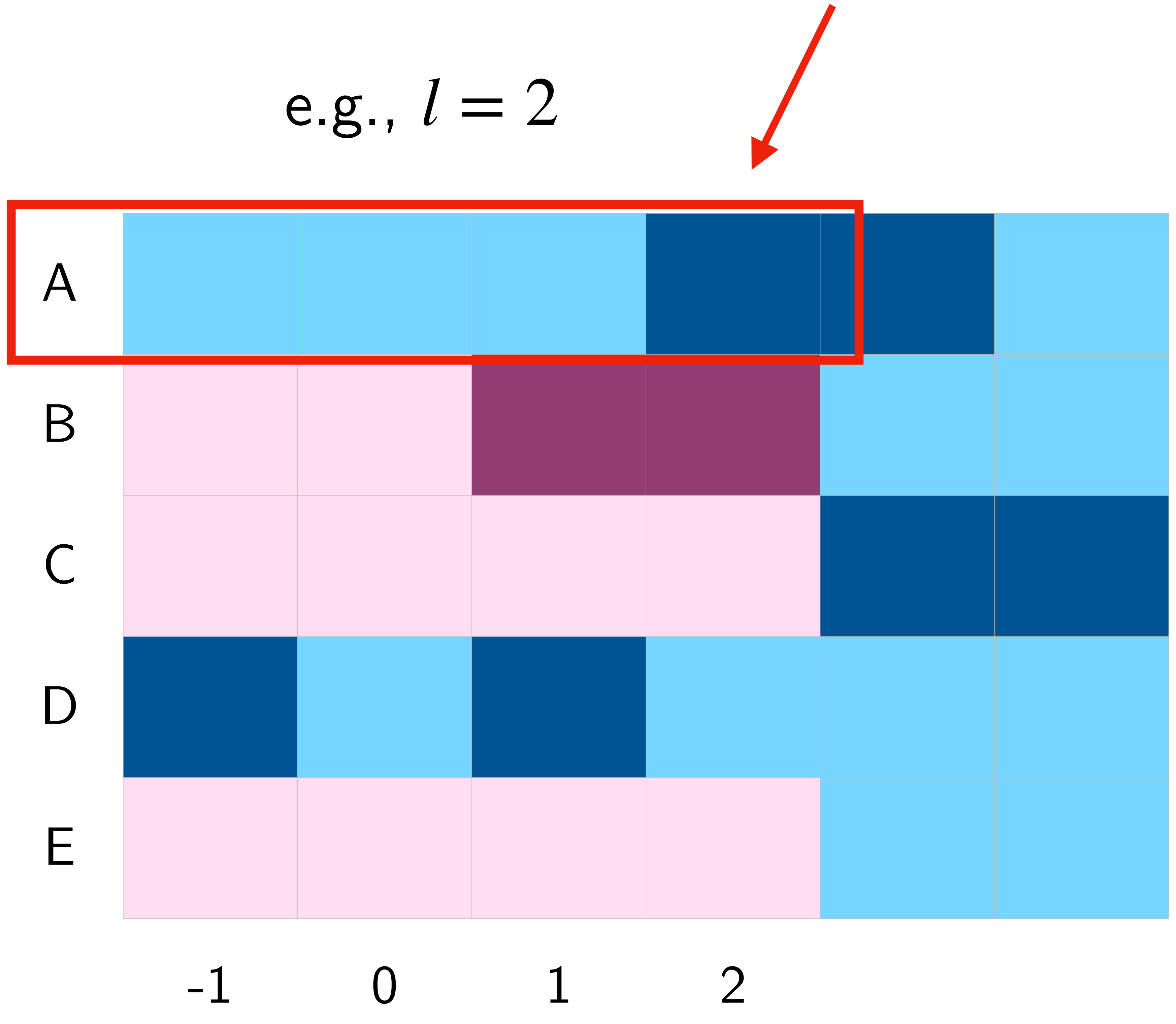

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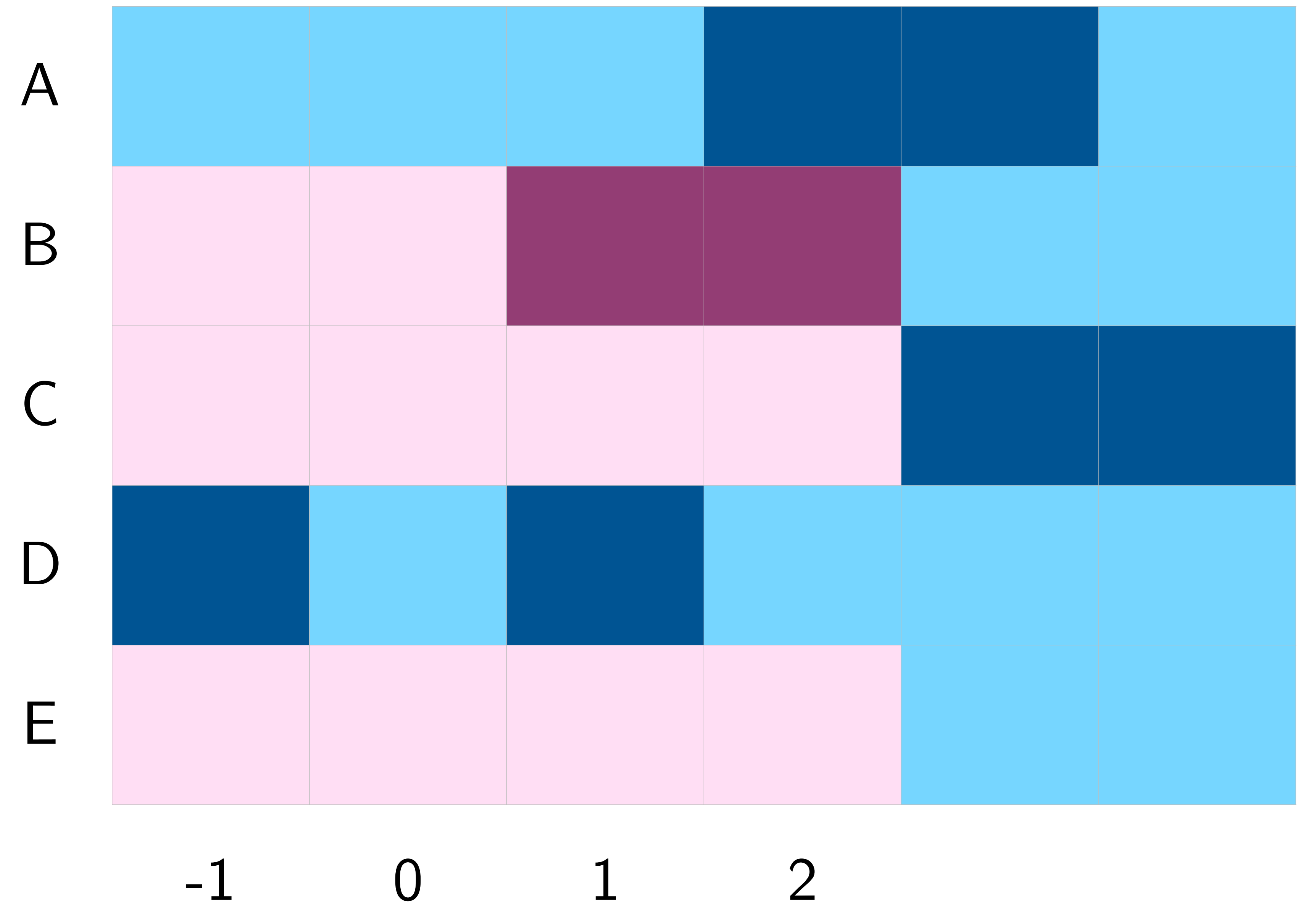

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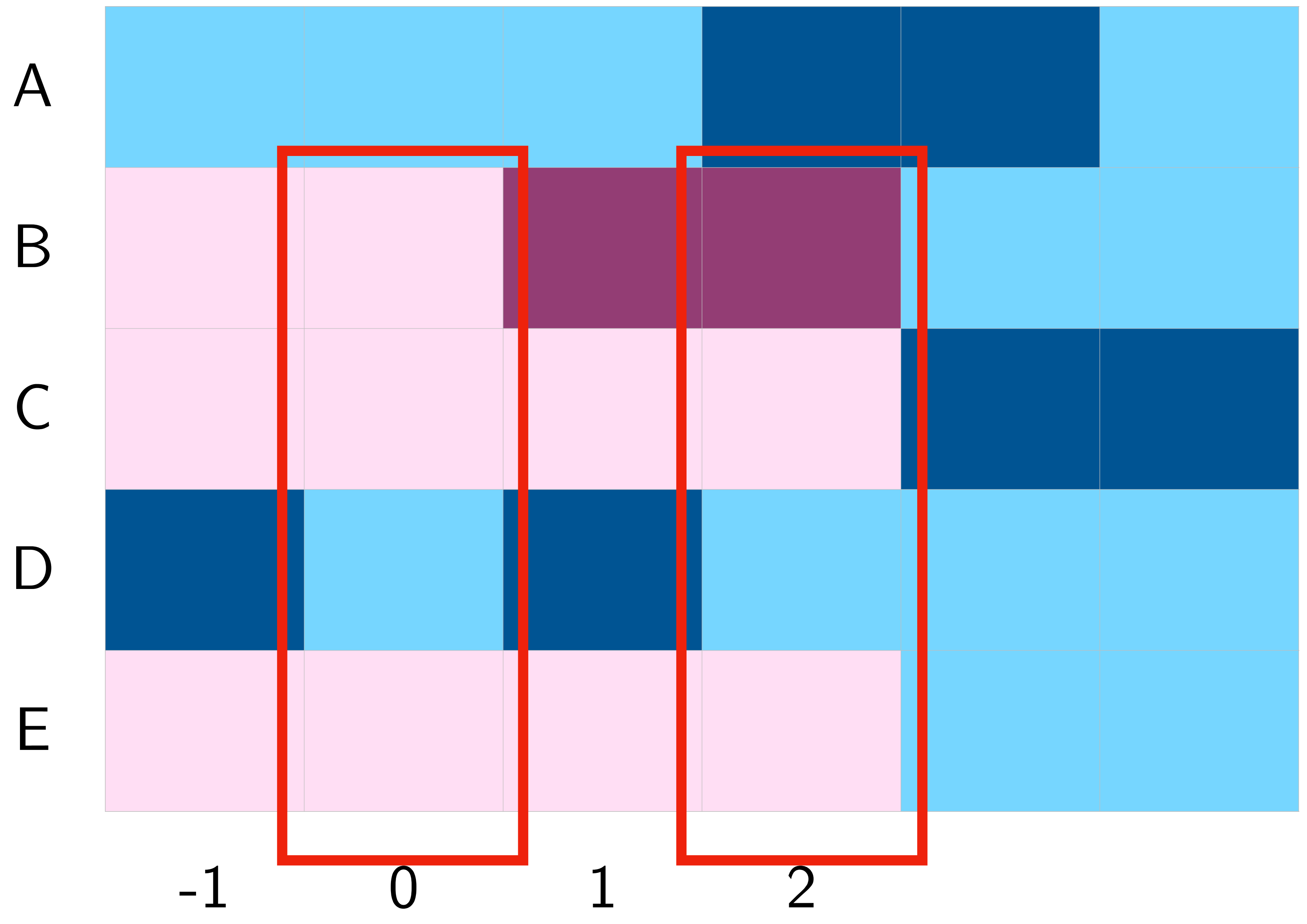

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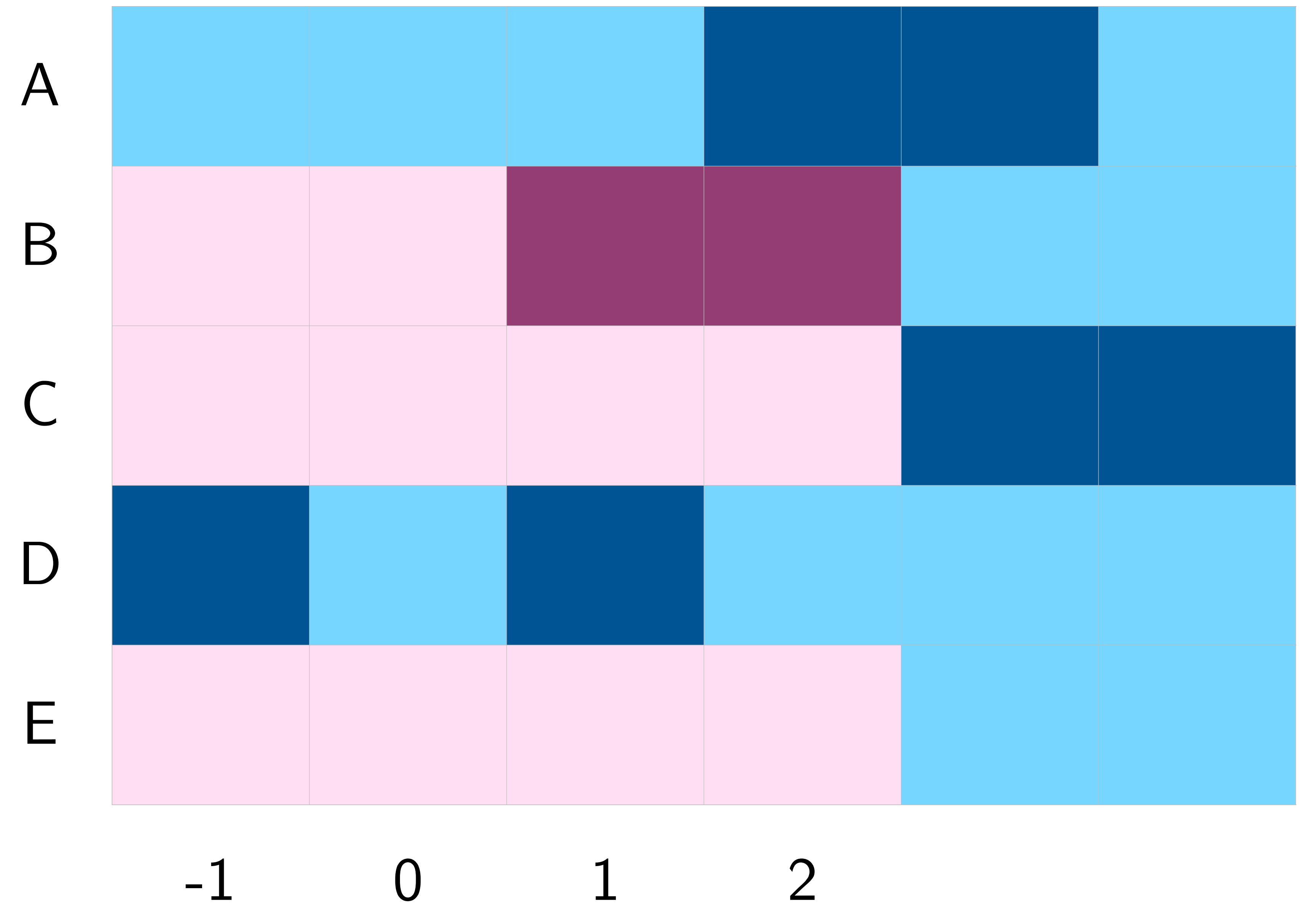

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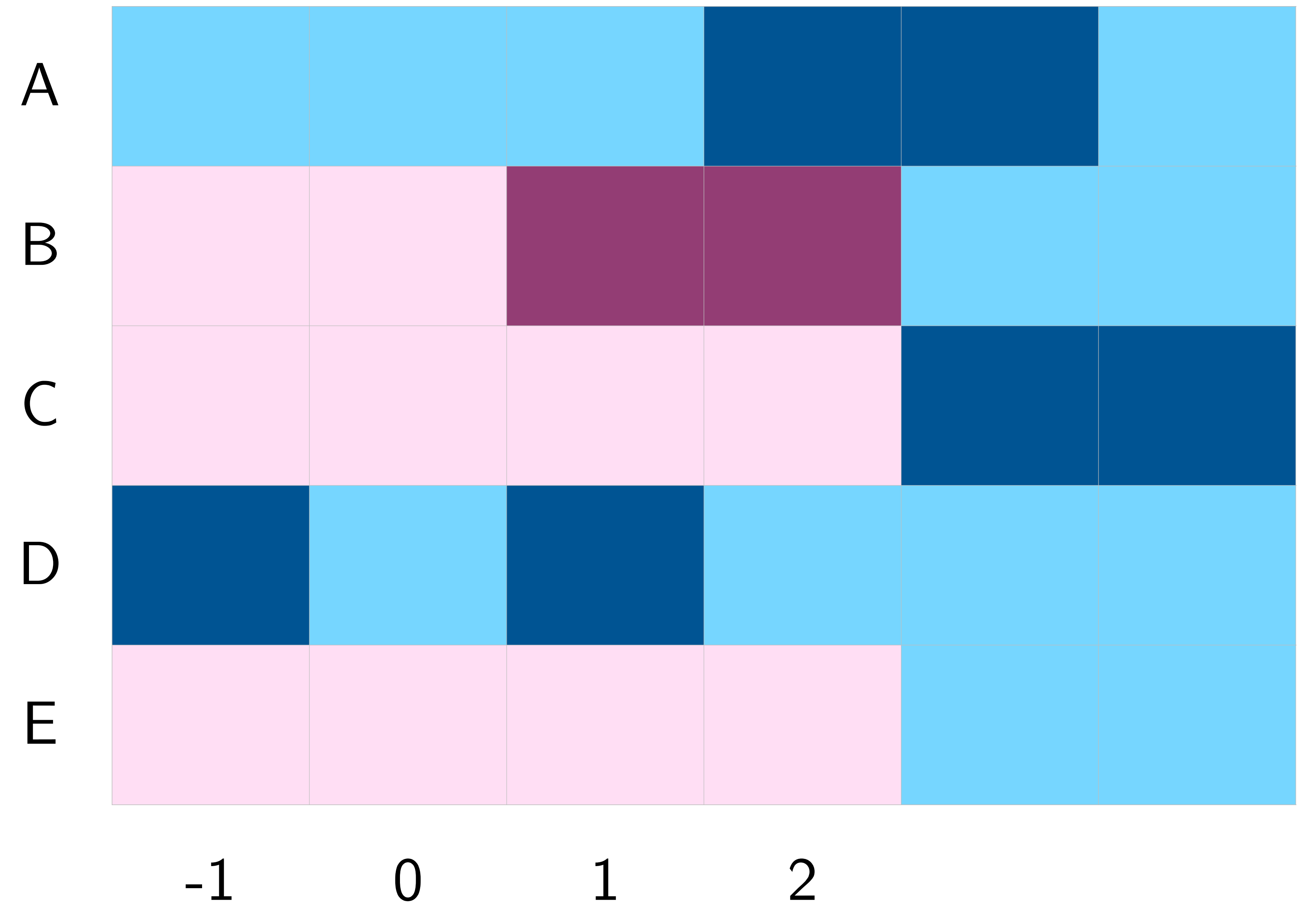

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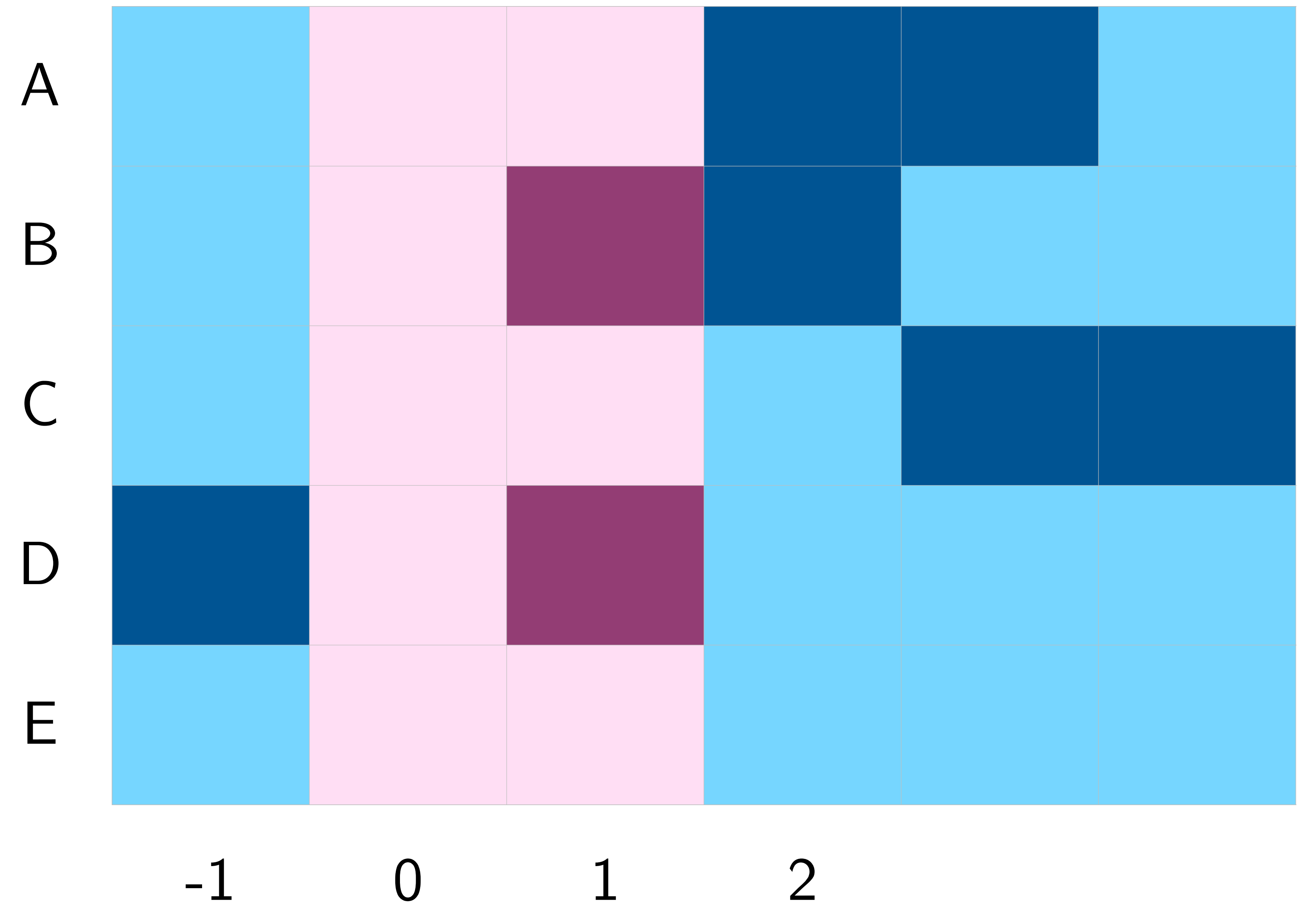

# Imputation Methods

Borusyak, Jaravel & Spiess (2023); Liu, Wang & Xu (2022)

- Fit model for $Y_{it}(0)$ on controls
  - TWFE: Fixed effects counterfactual estimator
- Impute $\hat{Y}_{it}(0)$ for treated
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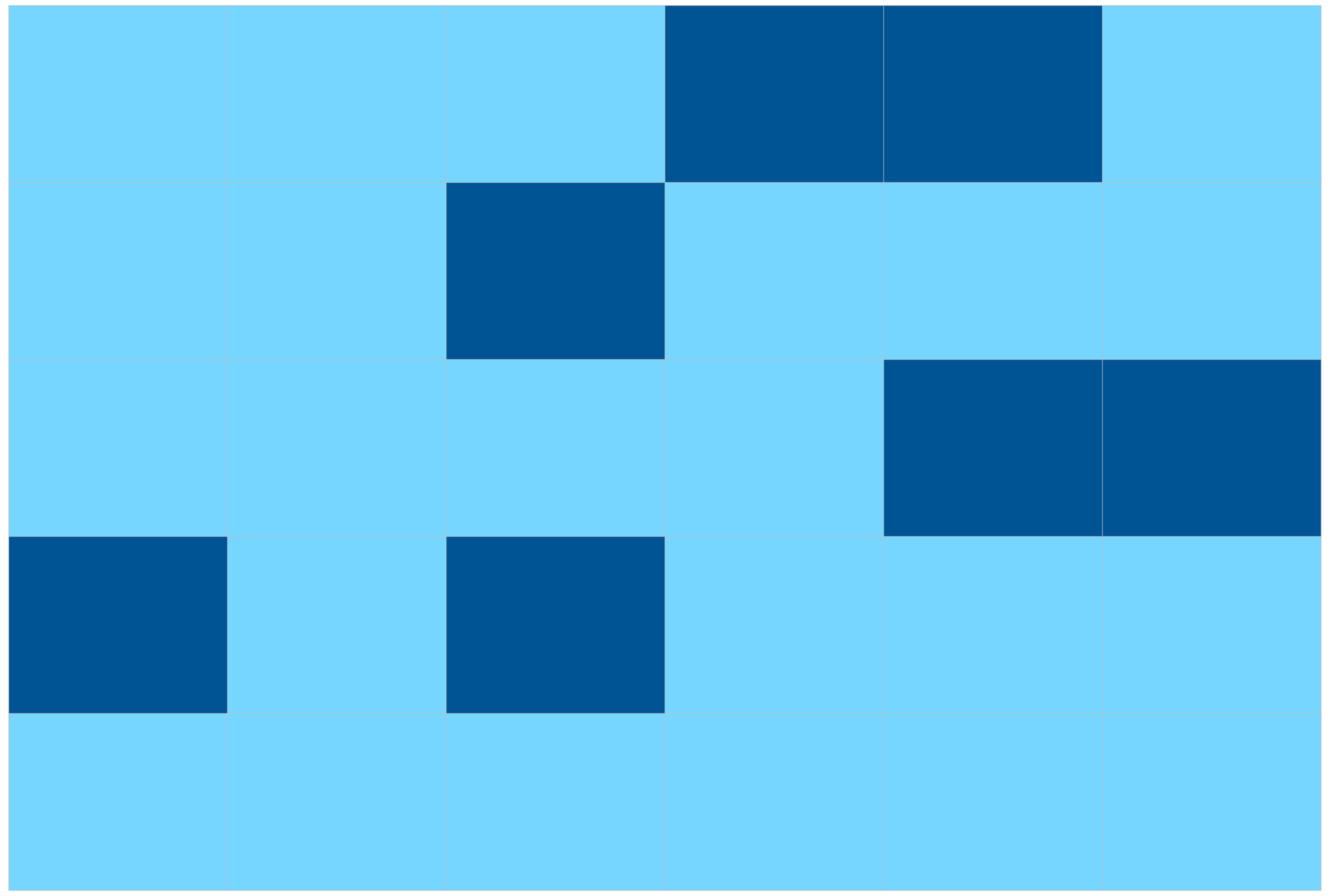

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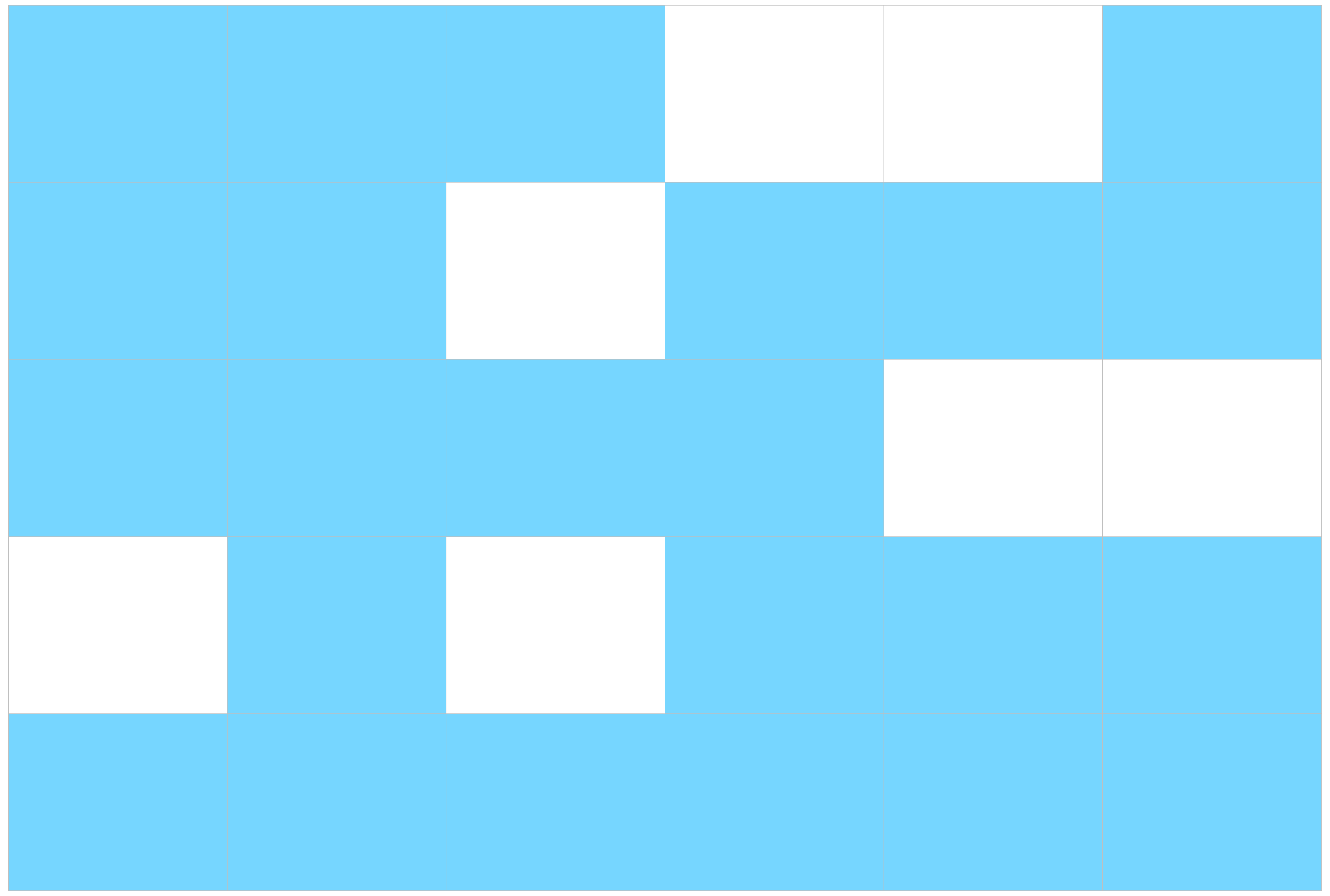

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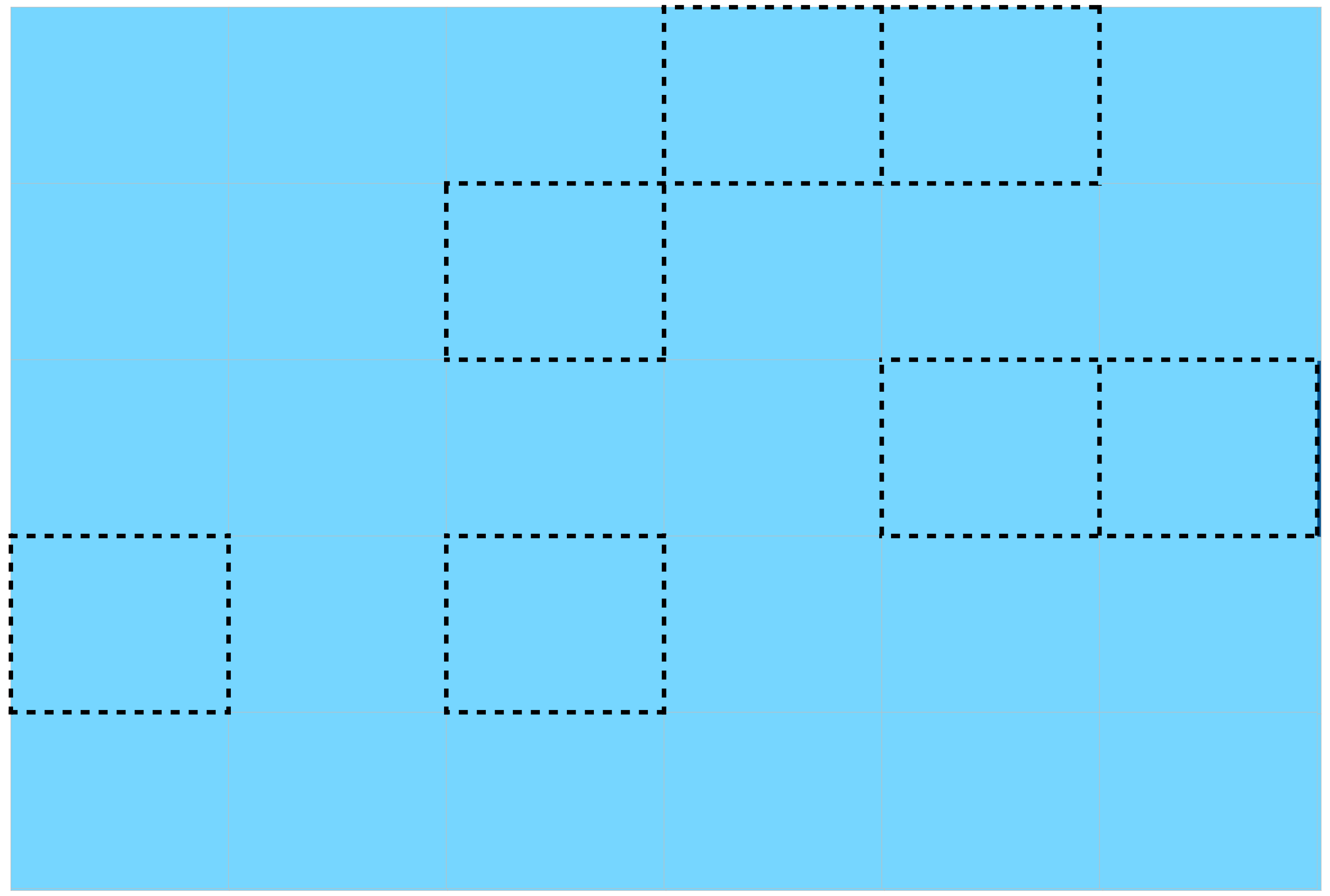

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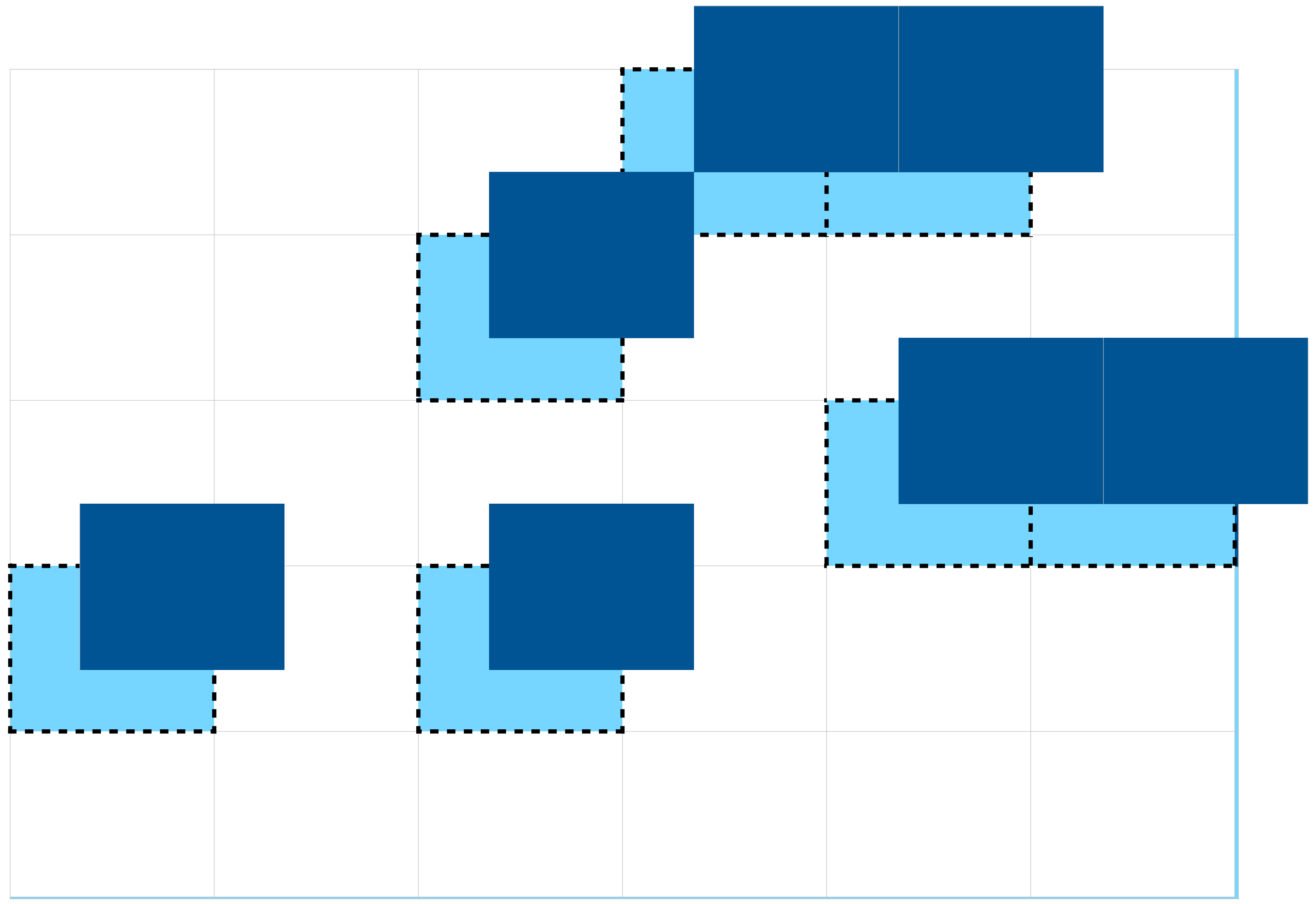

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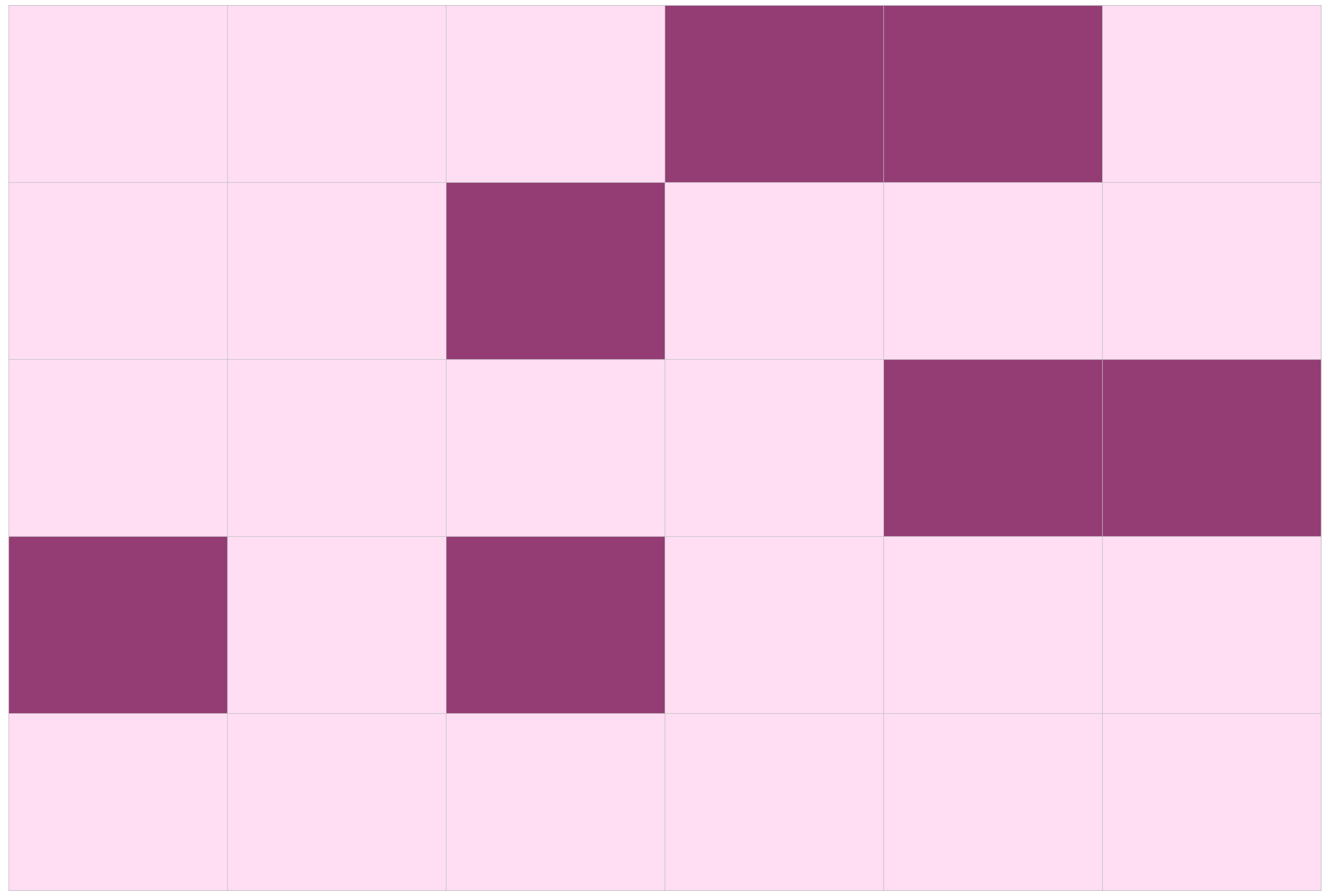
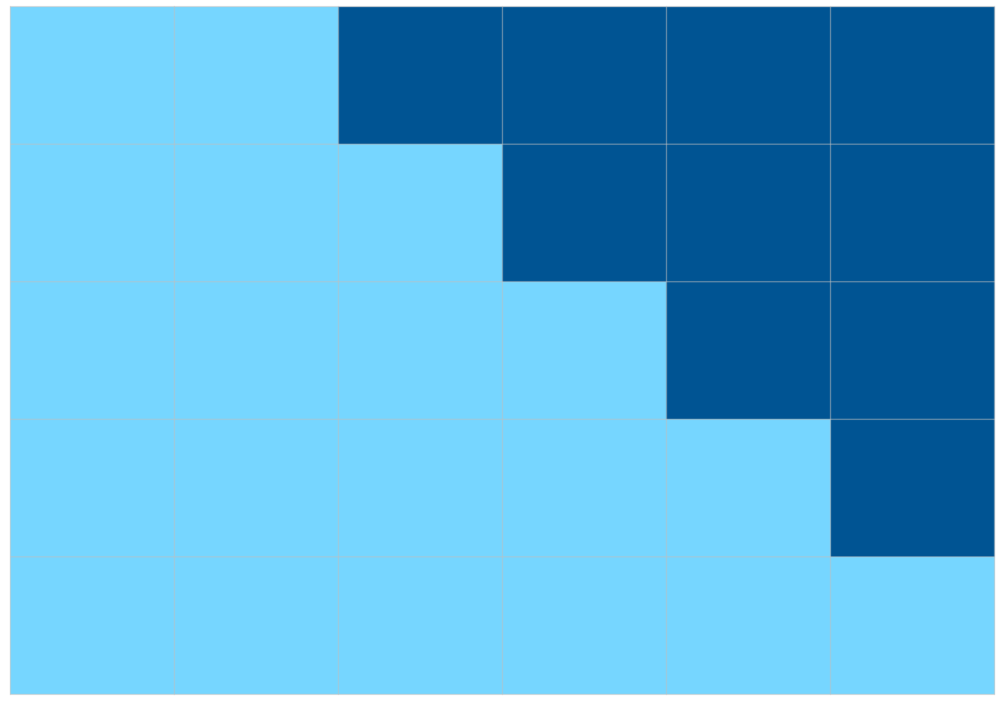
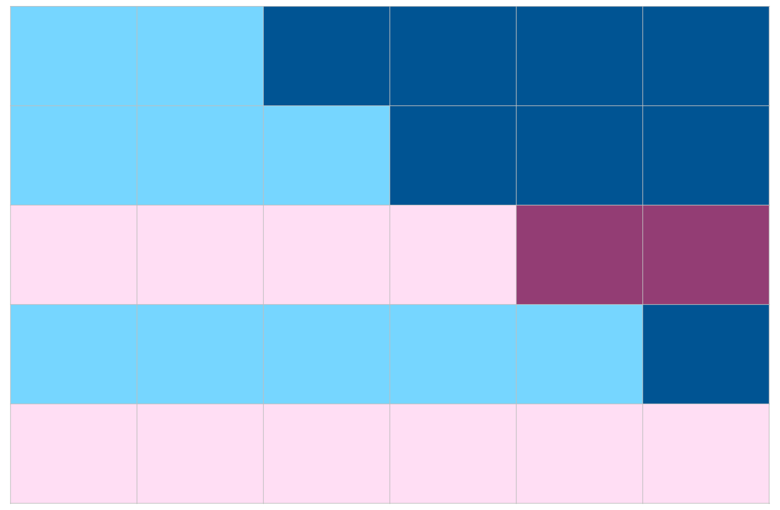


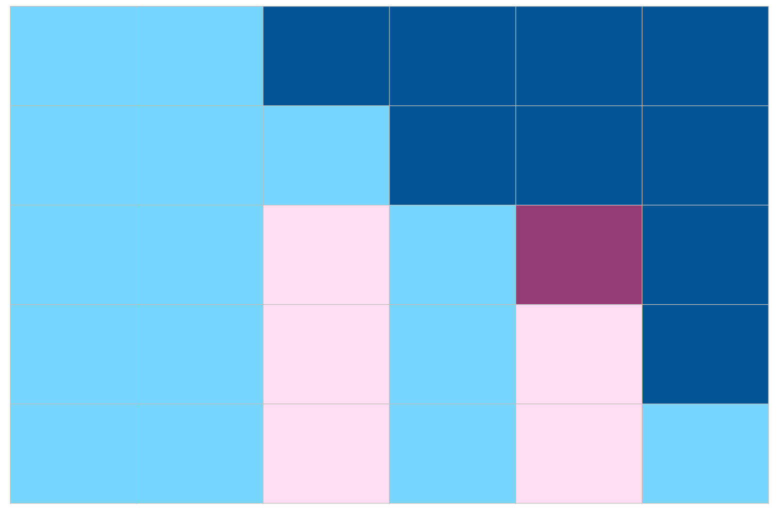
# Comparison — Staggered



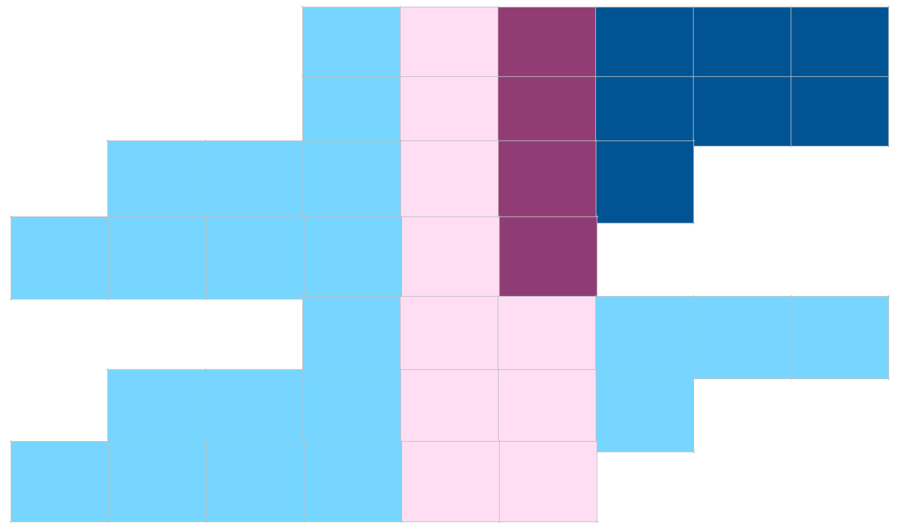
Original Data



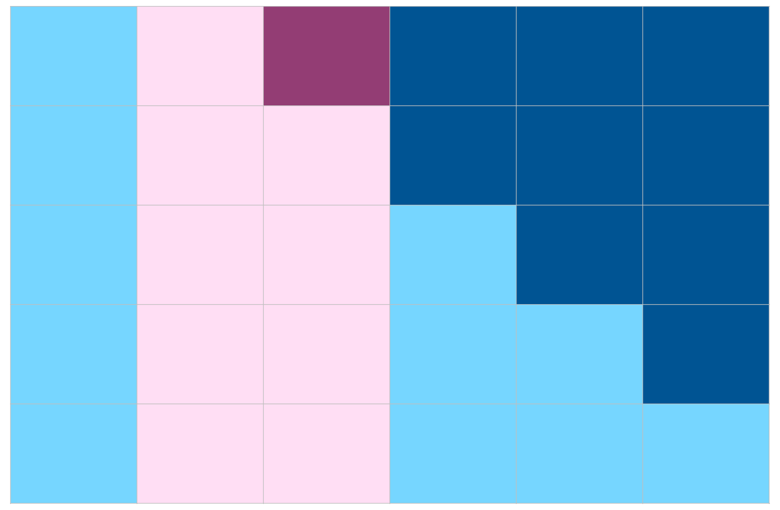
Interaction Weighted
& Stacked DID



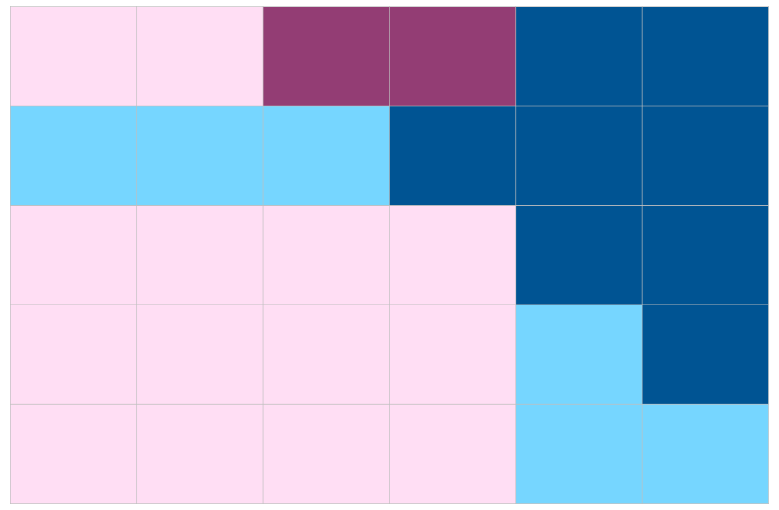
CSDID



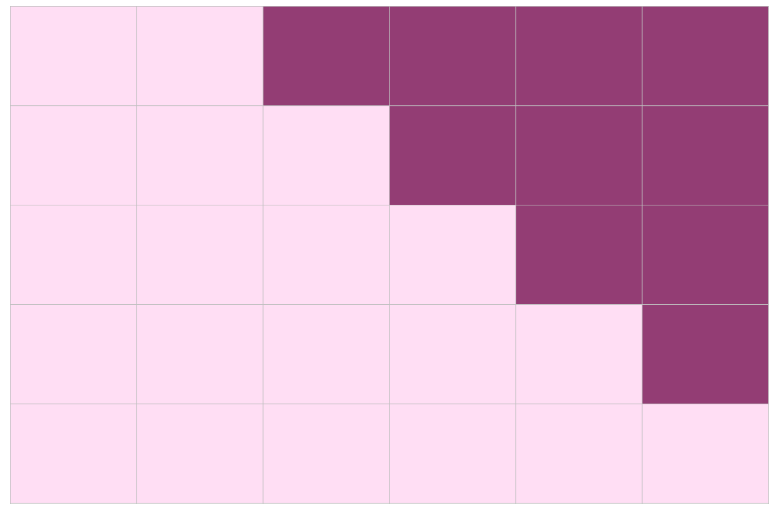
Stacked DID



DID multiple



PanelMatch



Imputation Method
e.g., FEct

# HTE-Robust Estimators

| | DID Extensions (2x2 DID as building blocks) | | Imputation Methods (outcome model w/ FE) |
|---|---|---|---|
| *Setting* | Staggered | General | General |
| *Estimand* | ATT | ATT for Switchers | ATT |
| *Estimator* | IW, CSDID, Stacked DID | PanelMatch, $\text{DID}_{\text{M}}$ | $\text{DID}_{\text{impute}}$, FEct |
| *Comparison Group* | Never/last/not-yet-treated | Matched set | Imputed counterfactual |
| *Key assumption* | Parallel Trends | Parallel Trends | Zero Conditional Mean or Parallel Trends |

# Replication & Reanalysis

Data

Procedure

# The Replication Sample (2017-2023)

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Case selection:

- Use panel data analysis as a critical piece of evidence to support a causal argument
- Binary treatment
- A "proper" TWFE (DID) research design
- Use a DID or TWFE estimator
- Focus on the authors' preferred specification

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| Journal | All Linear Panel |
|---|---|
| APSR | 22 |
| AJPS | 31 |
| JOP | 49 |
| **Total** | **102** |

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## Case selection:

- Use panel data analysis as a critical piece of evidence to support a causal argument
- Binary treatment
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| Journal | All Linear Panel | "Proper" TWFE |
|---|---|---|
| APSR | 22 | 13 |
| AJPS | 31 | 21 |
| JOP | 49 | 30 |
| **Total** | **102** | **64** |

# The Replication Sample (2017-2023)

## Case selection:

- Use panel data analysis as a critical piece of evidence to support a causal argument
- Binary treatment
- A "proper" TWFE (DID) research design
- Use a DID or TWFE estimator
- Focus on the authors' preferred specification

| Journal | All Linear Panel | "Proper" TWFE | Incomplete Data |
|---|---|---|---|
| APSR | 22 | 13 | 2 |
| AJPS | 31 | 21 | 3 |
| JOP | 49 | 30 | 6 |
| **Total** | **102** | **64** | **11 (17.2%)** |

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|---|---|---|---|---|---|
| **APSR** | 22 | 13 | 2 | 1 | 10 (76.9%) |
| **AJPS** | 31 | 21 | 3 | 3 | 15 (71.4%) |
| **JOP** | 49 | 30 | 6 | 0 | 24 (80%) |
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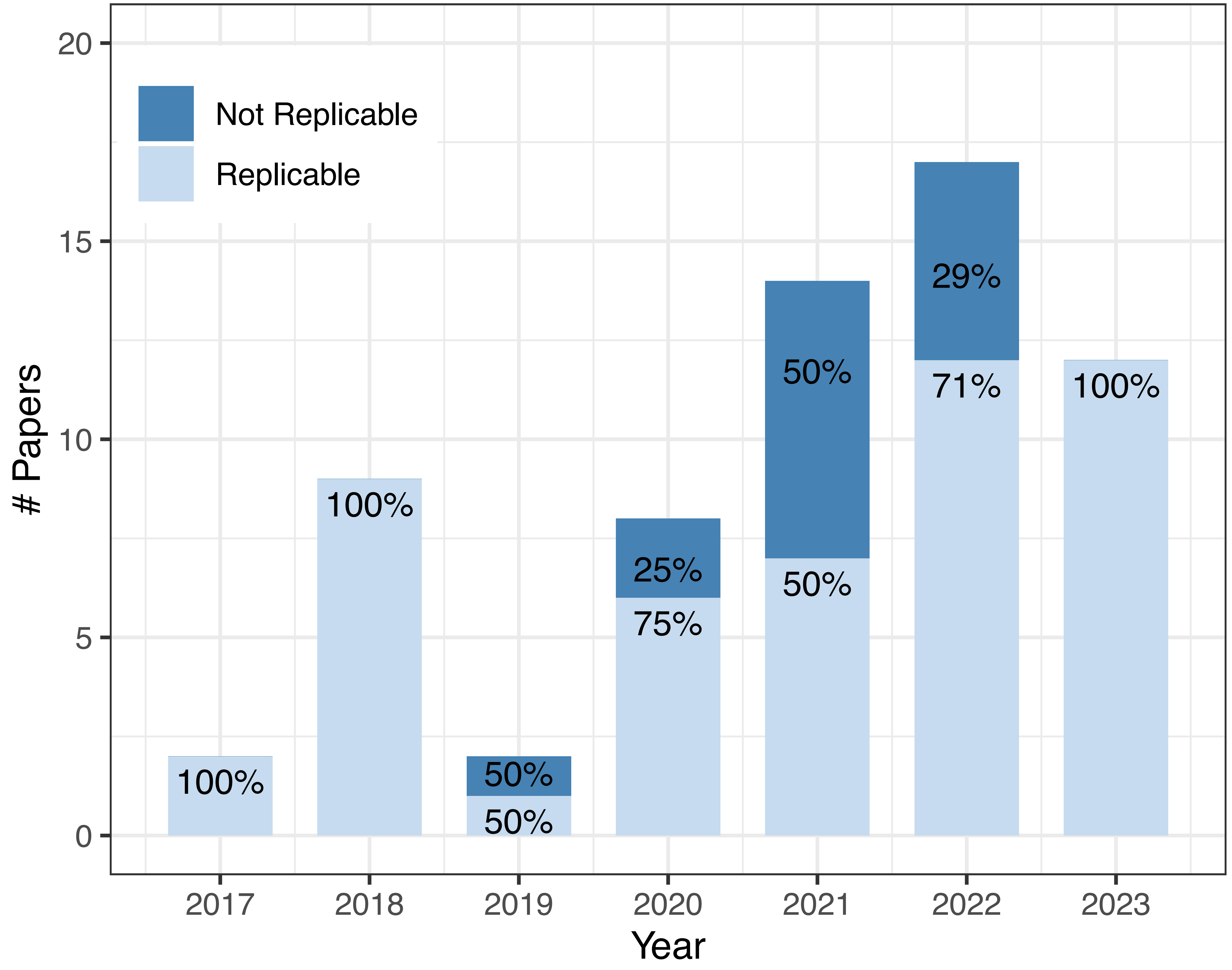

# Common Settings and Practice

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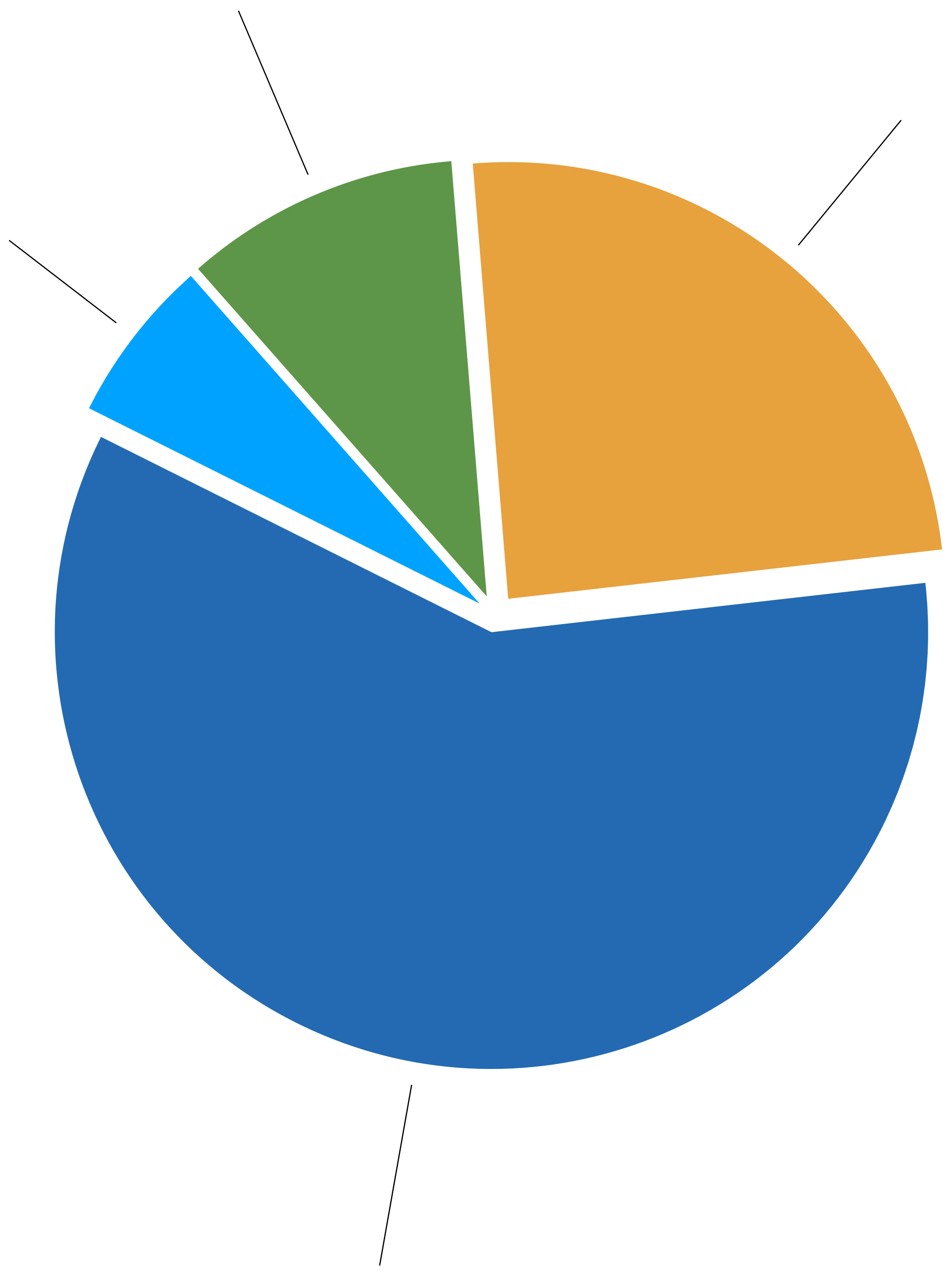

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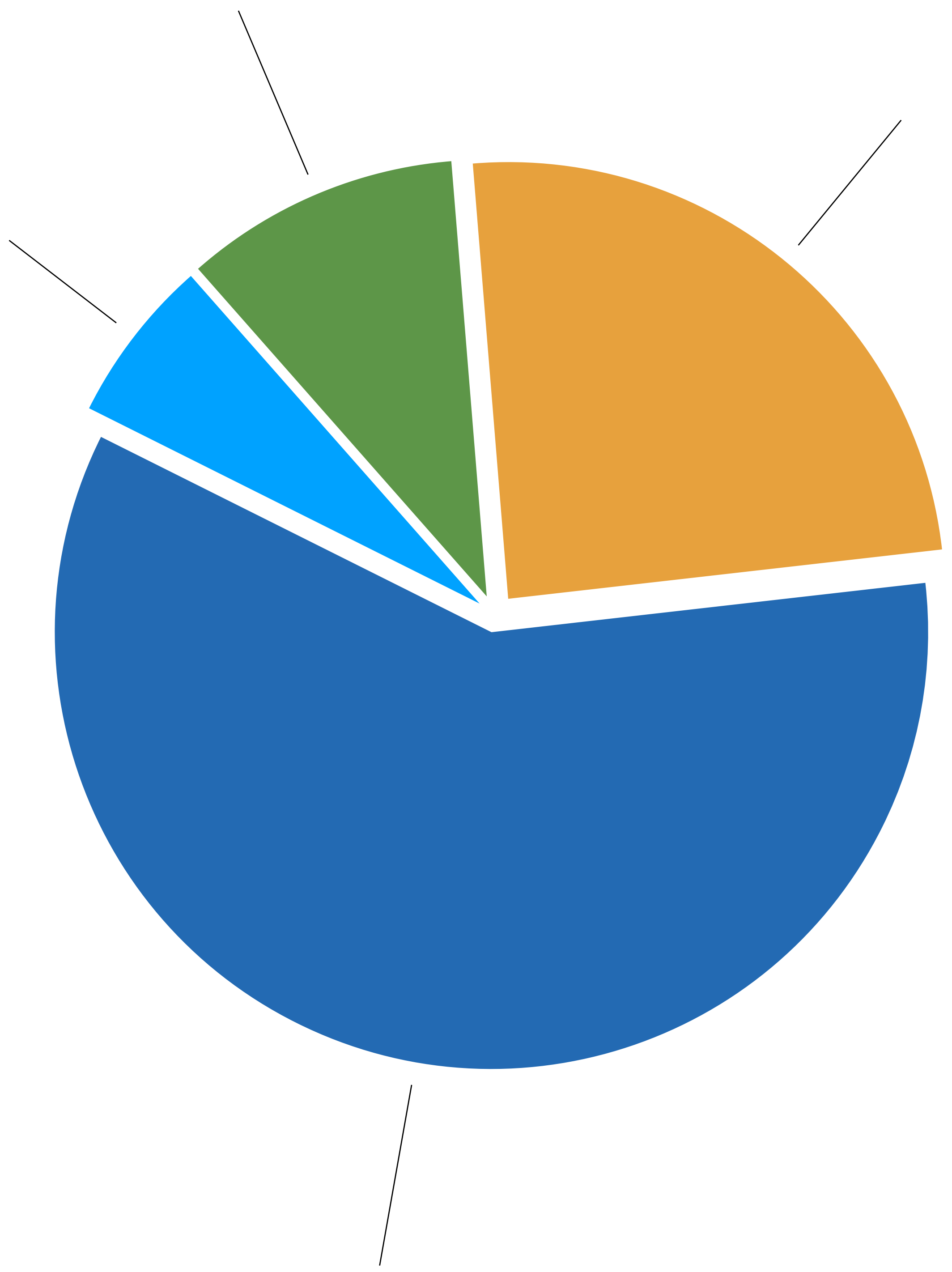
Among 49 Replicable Studies



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| Cluster-robust SE or PCSE | 48 | 98% |
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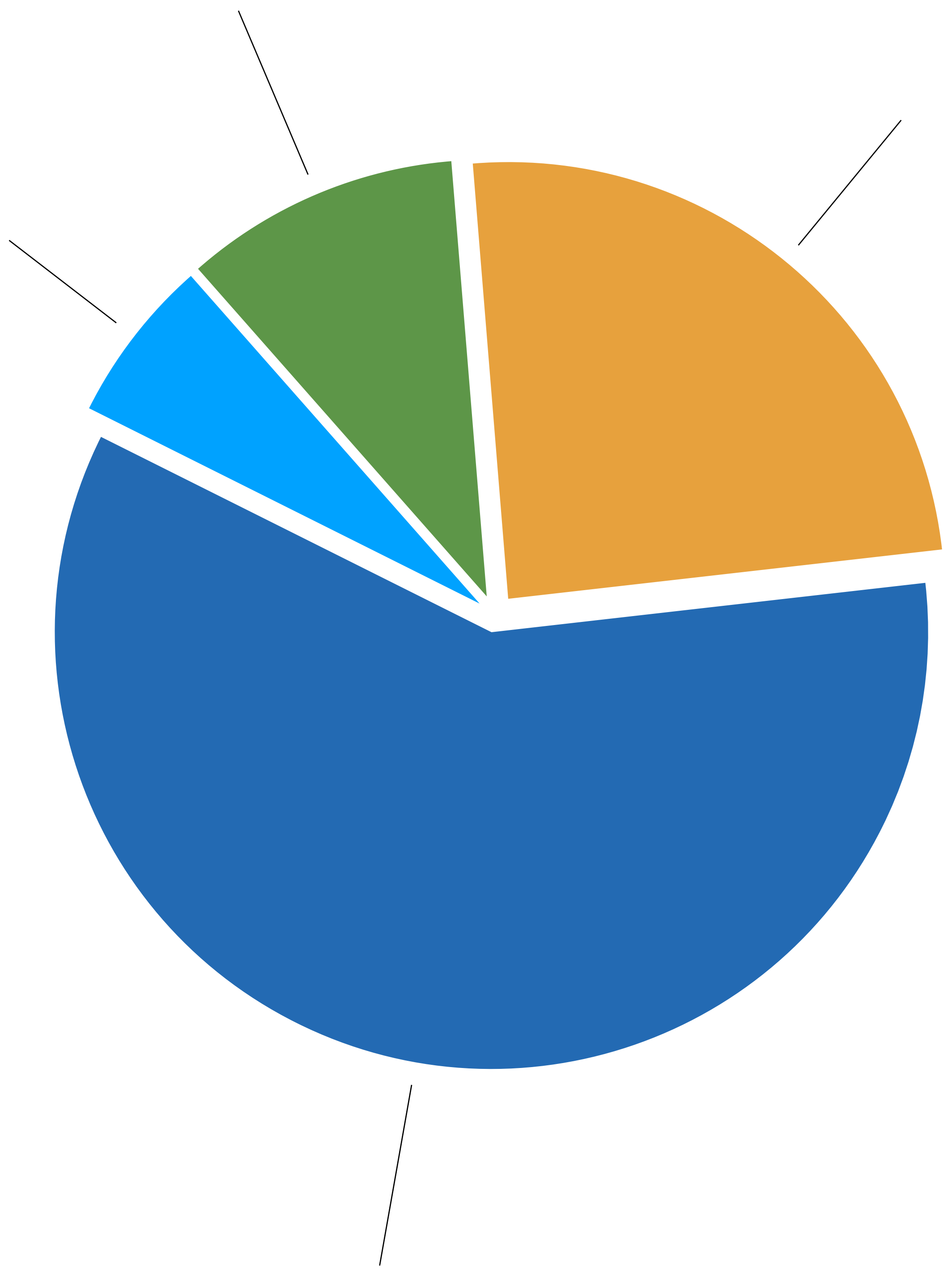

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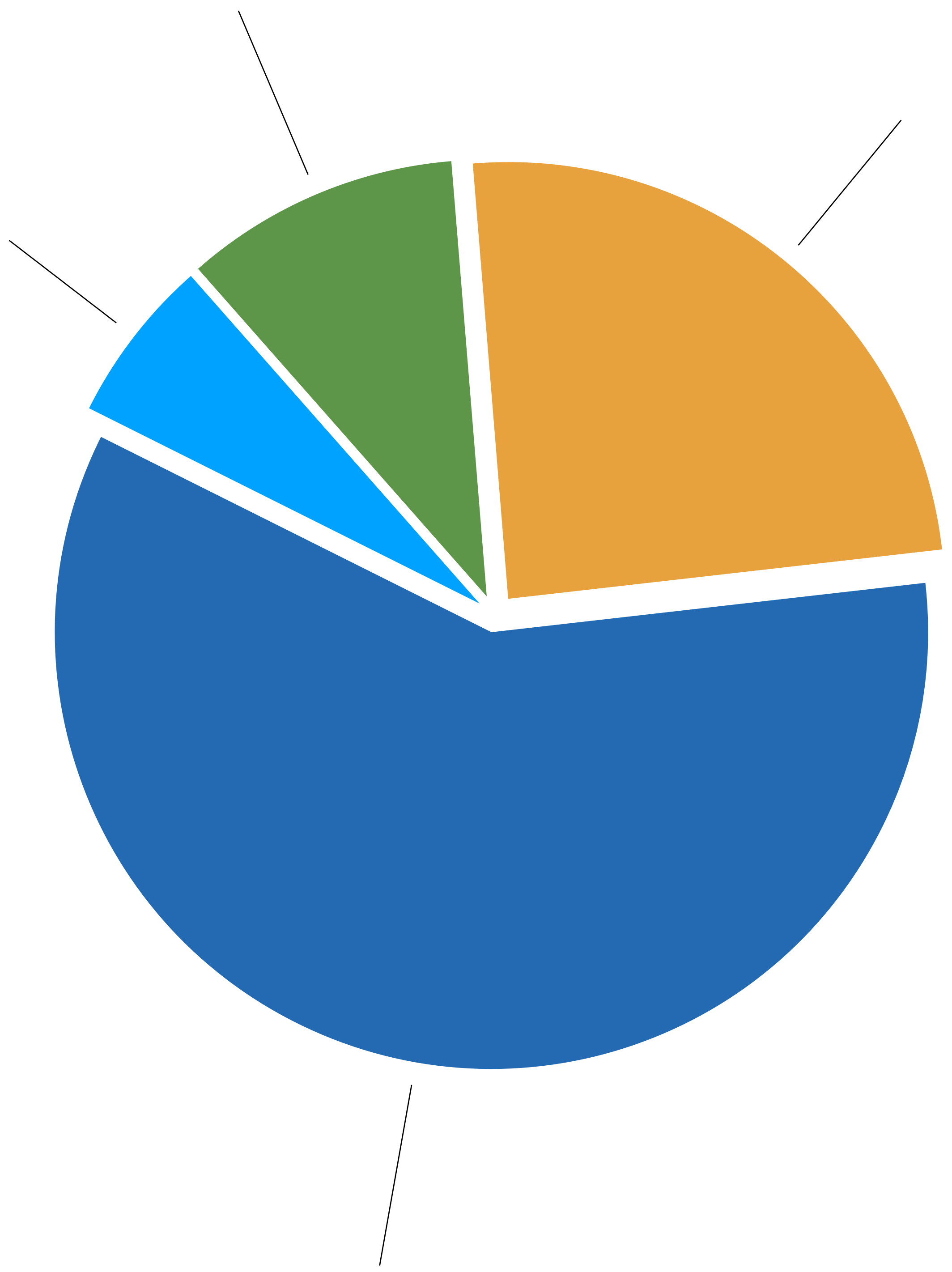

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| | | |
| **Visual Inspection** | | |
| Group average outcome trajectories | 19 | 39% |
| Event-study plots | 23 | 47% |
| Neither | 19 | 39% |

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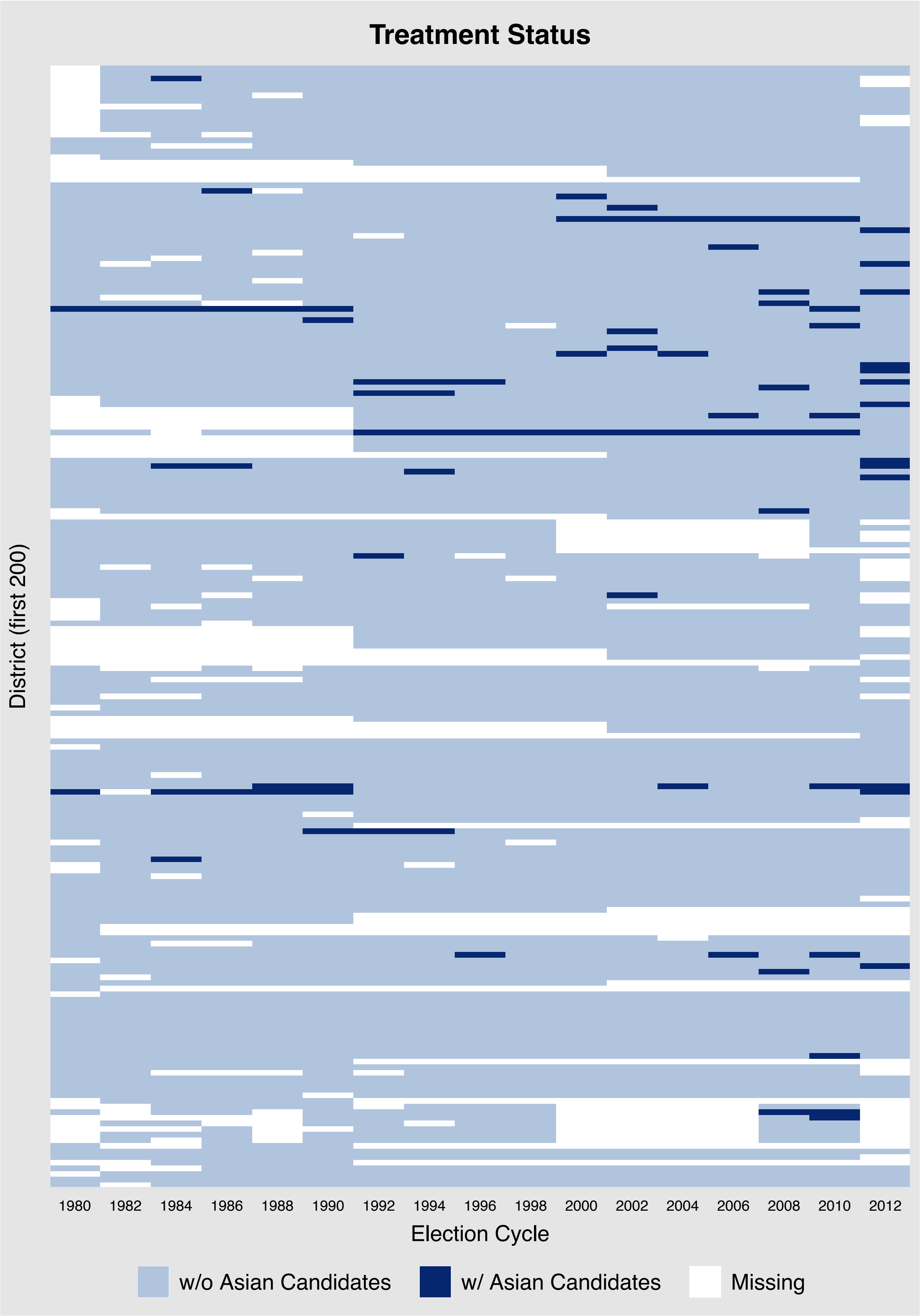
- Step 4. Conduct diagnostic test based on the imputation estimator (Liu, Wang & Xu 2022)
  - Tests for pretrend & carryover effects
  - Sensitivity analysis (Rambachan & Roth 2023)

# Findings

Three examples

Overall assessment

# Example 1: Coethnic Mobilization (APSR 2020)



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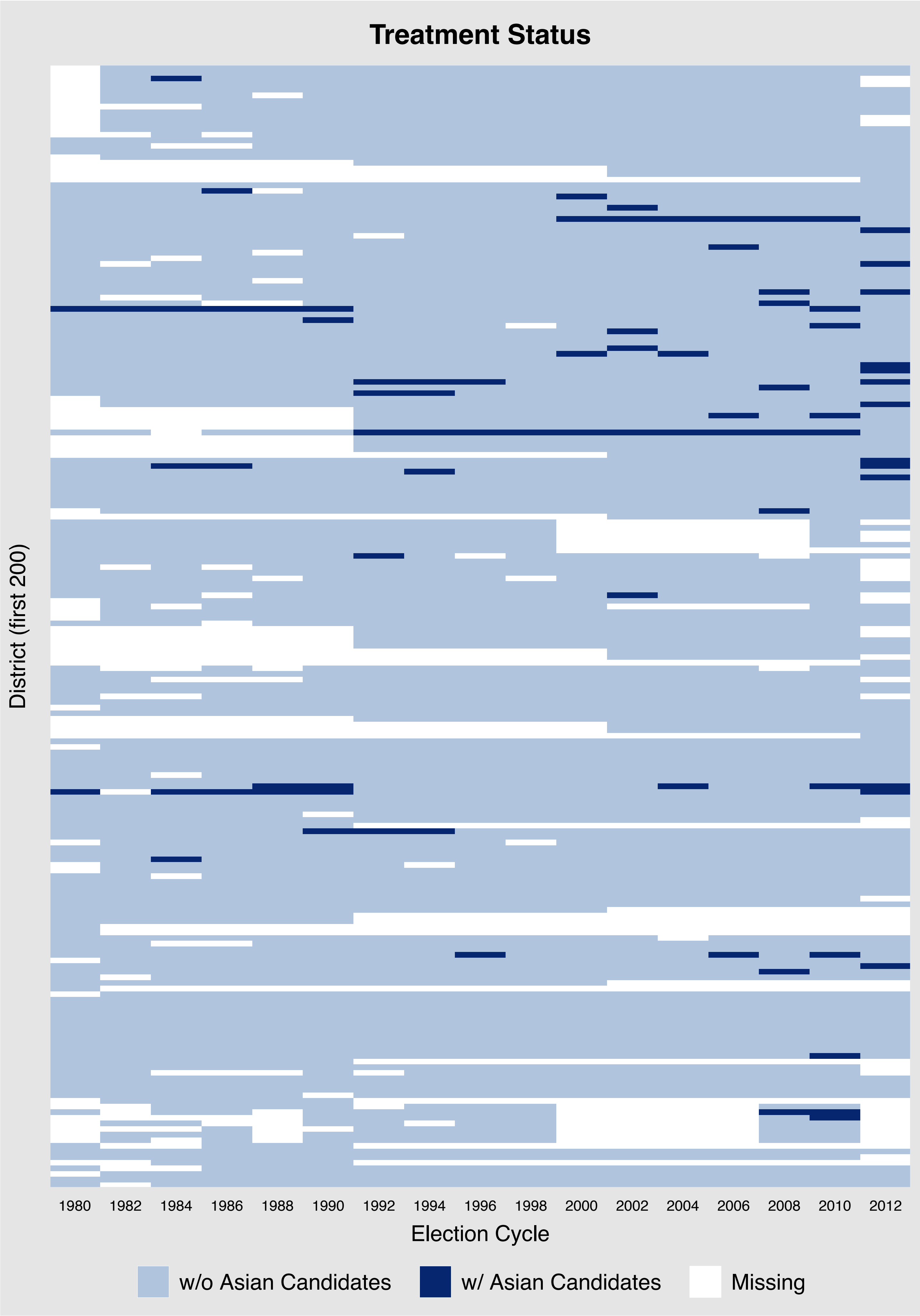

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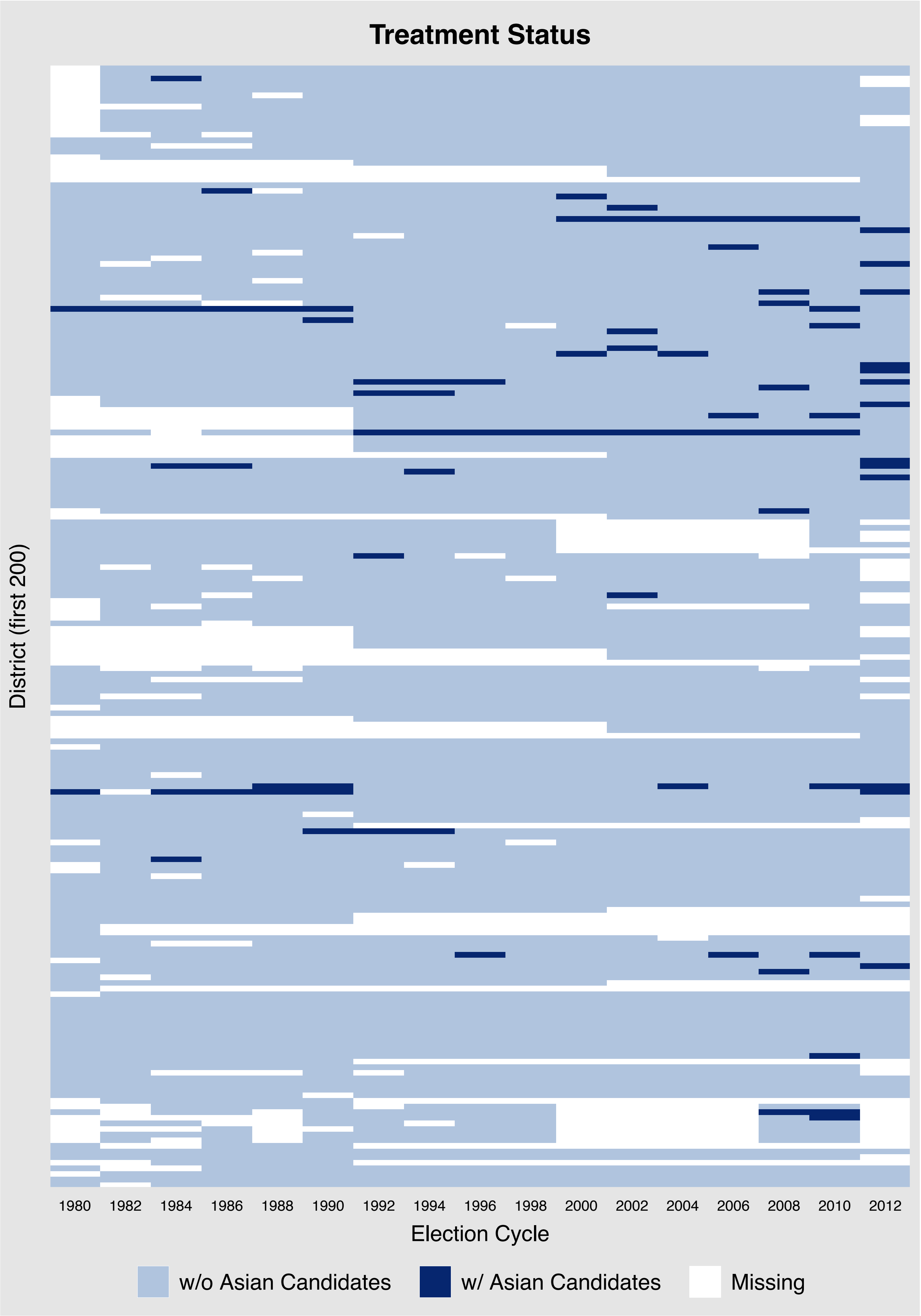

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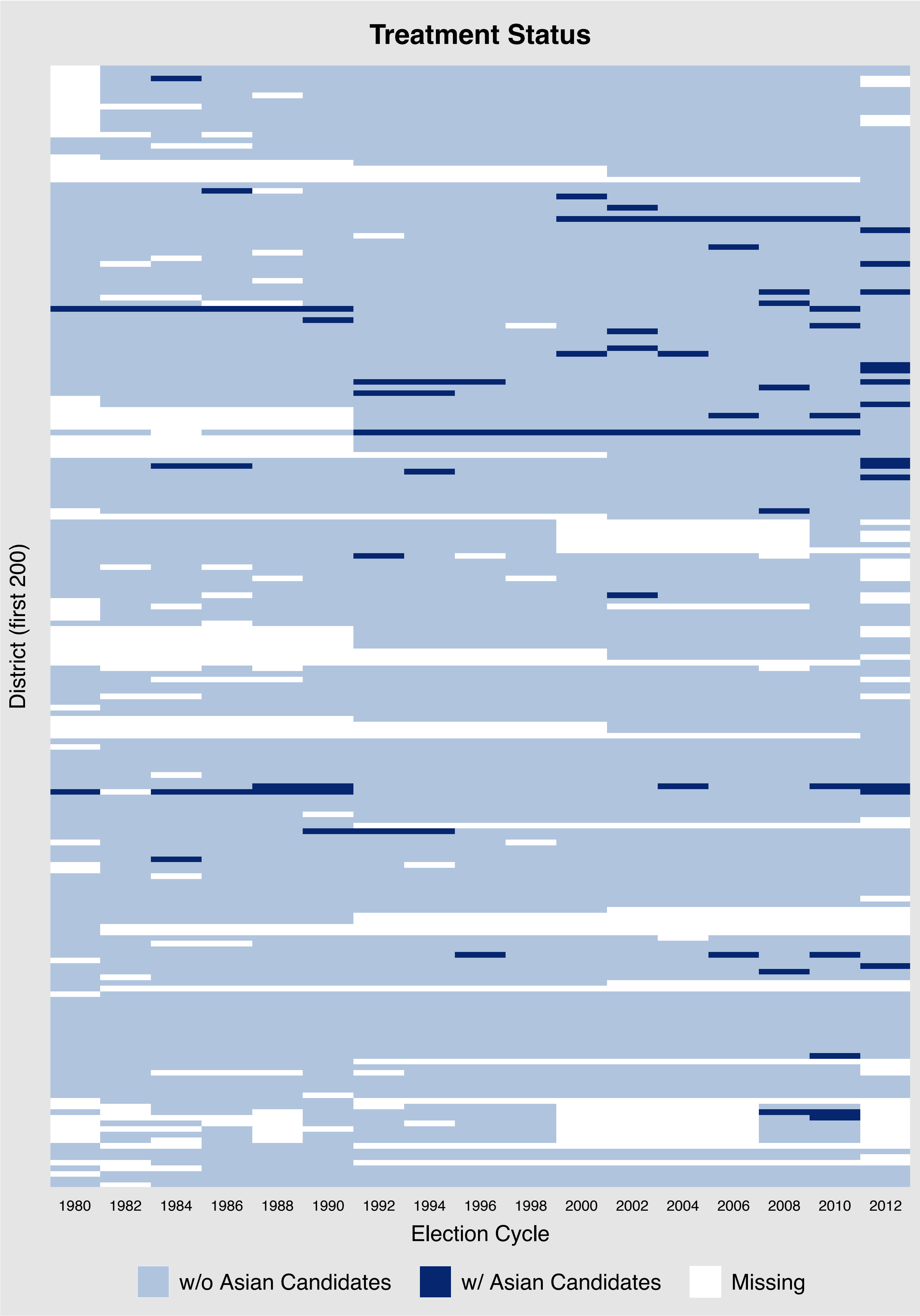

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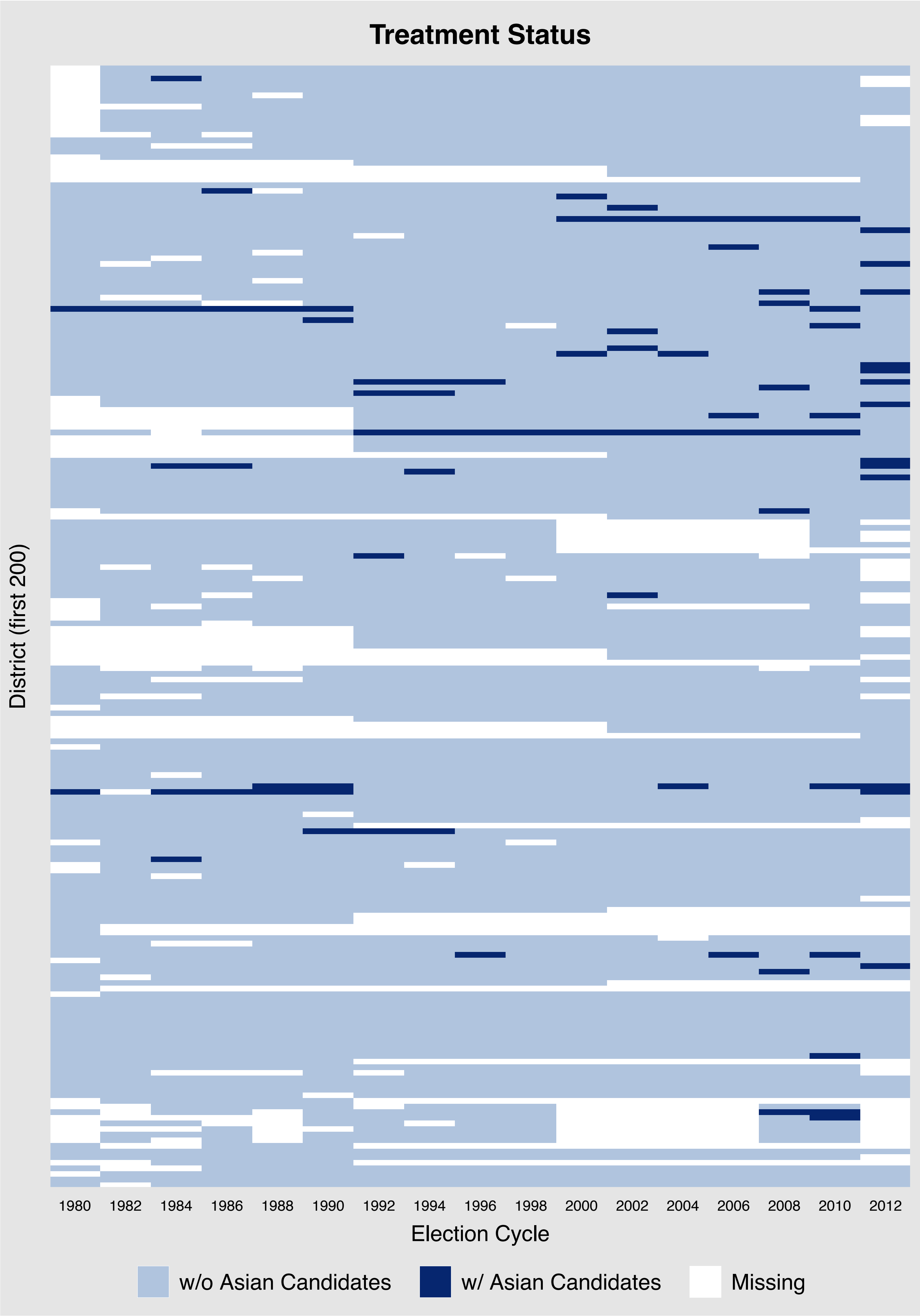
  - Outcome: share of Asian donations
  - Sample size:
    - N: 489
    - T: 17 (1980-2012)
    - #obs: 7,141



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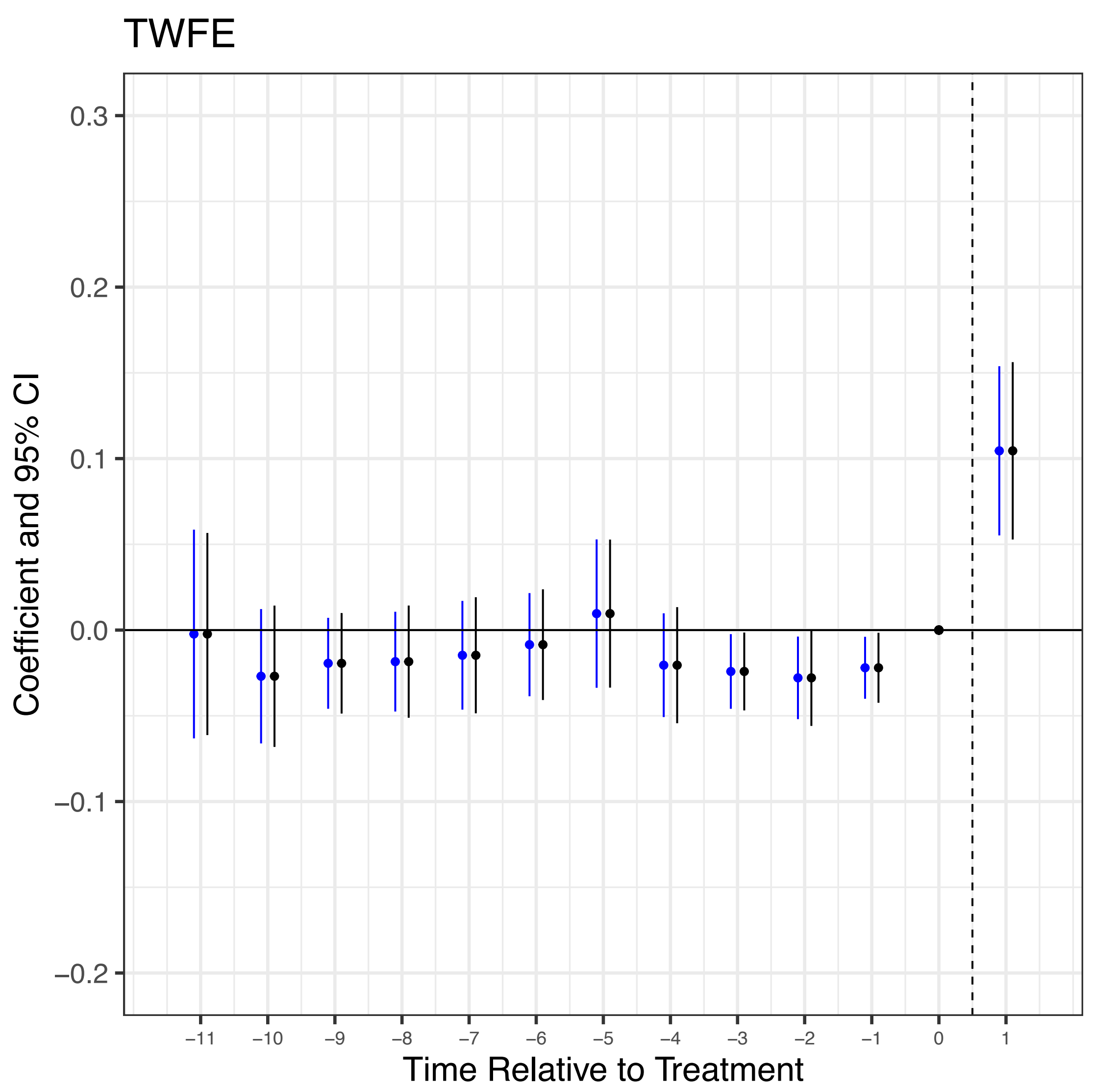

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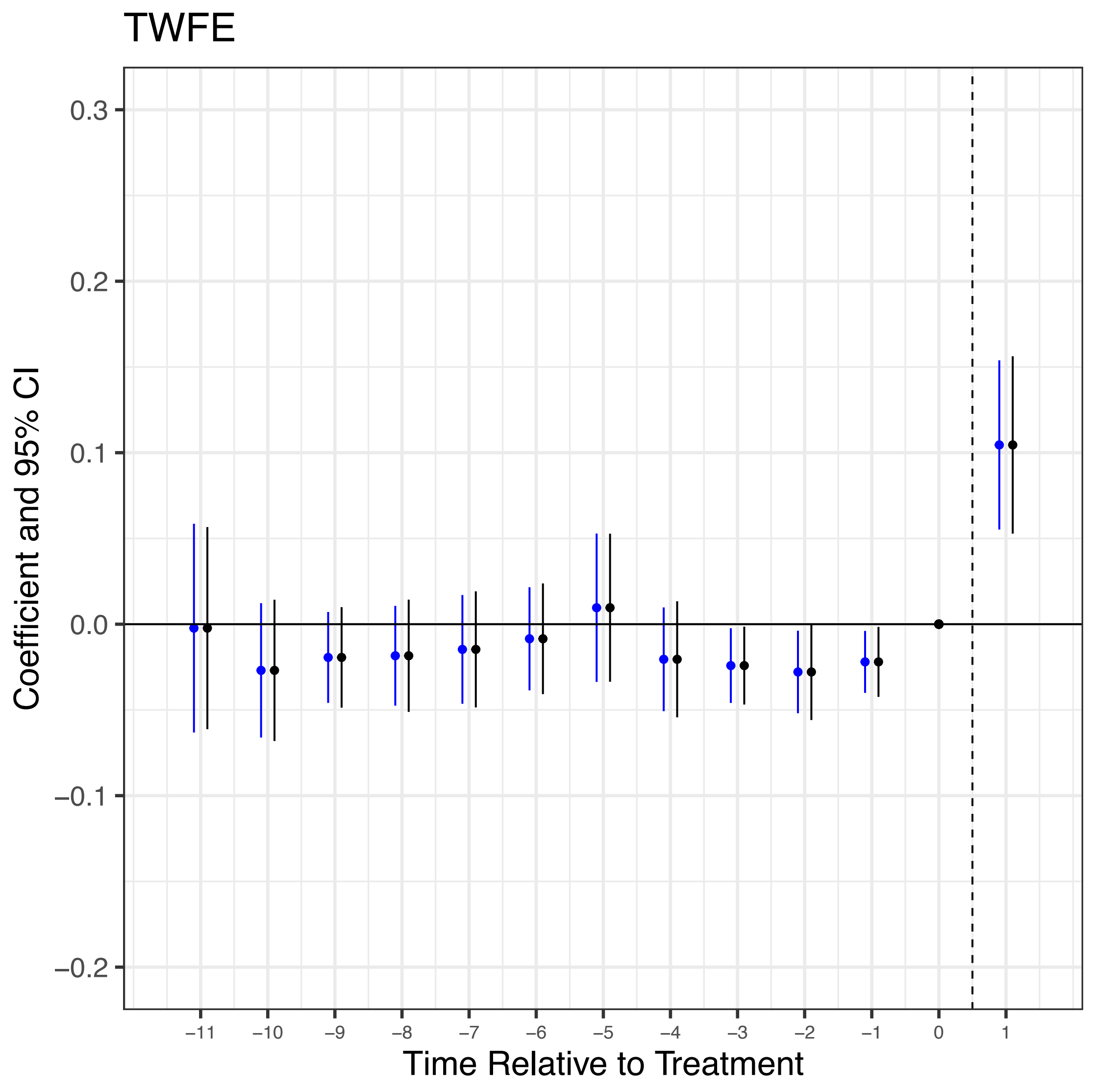


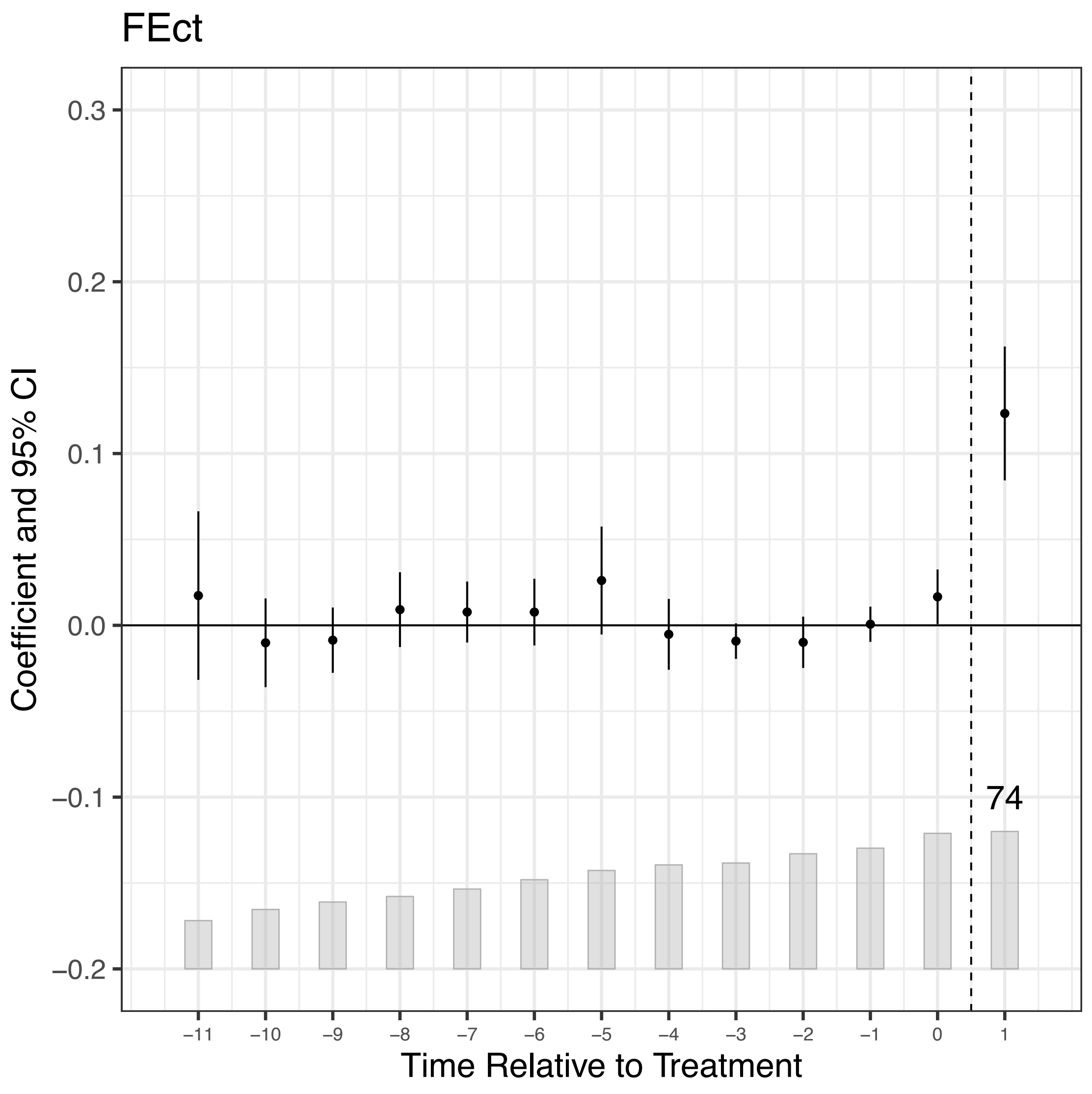

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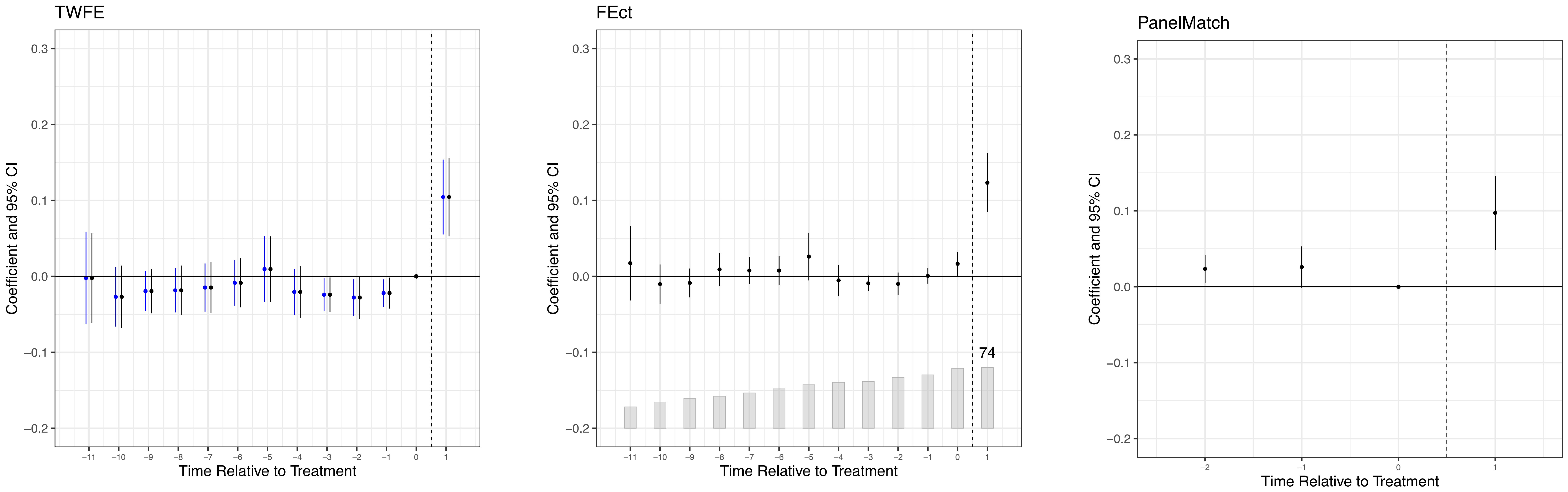

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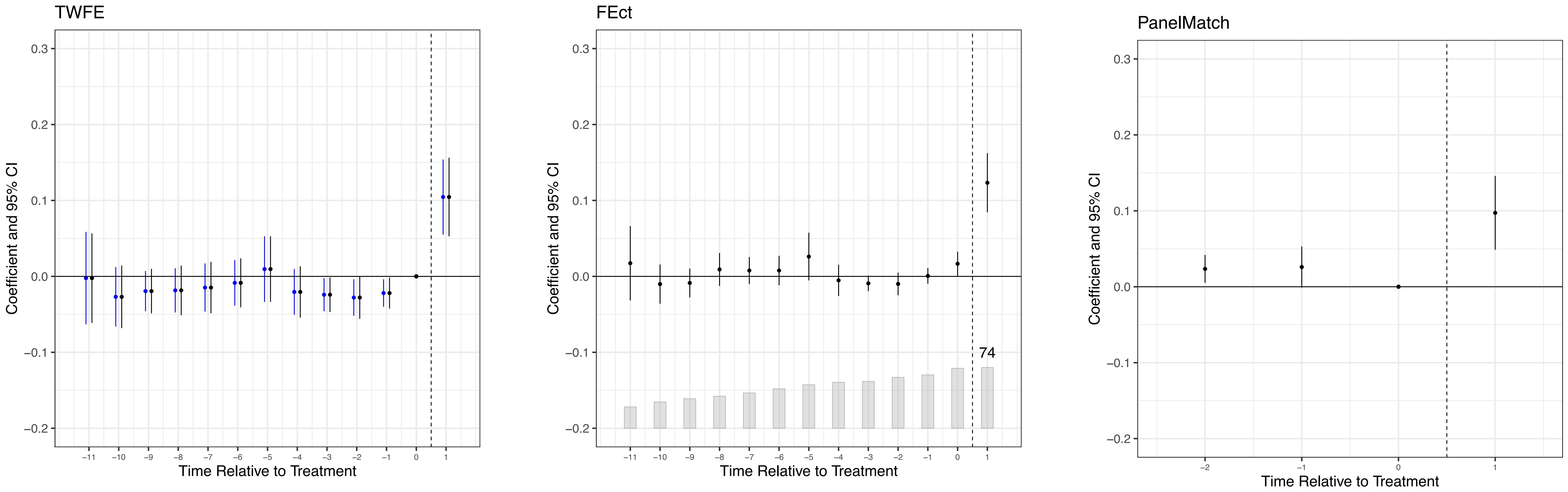
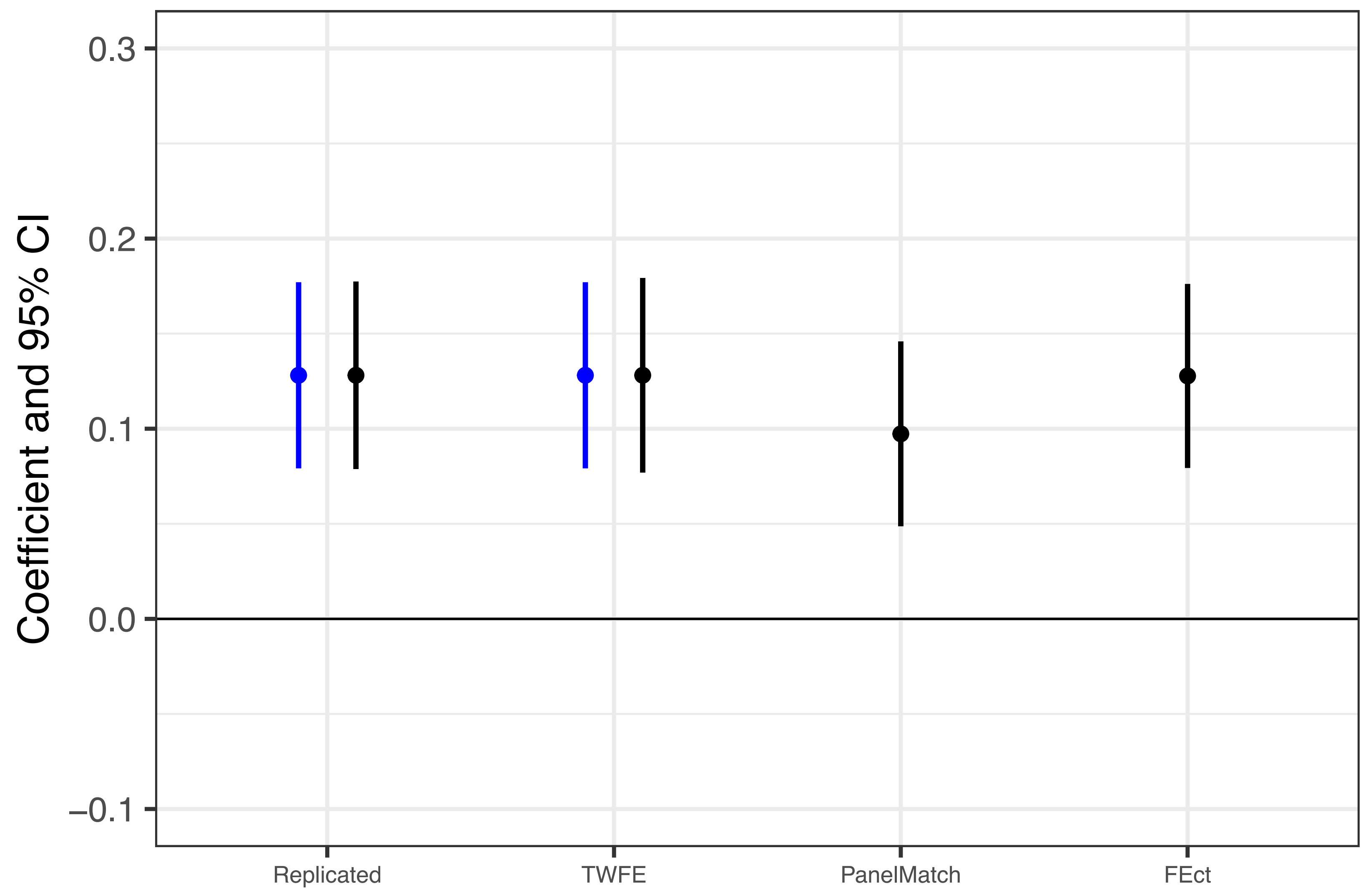



Comparison of Estimators



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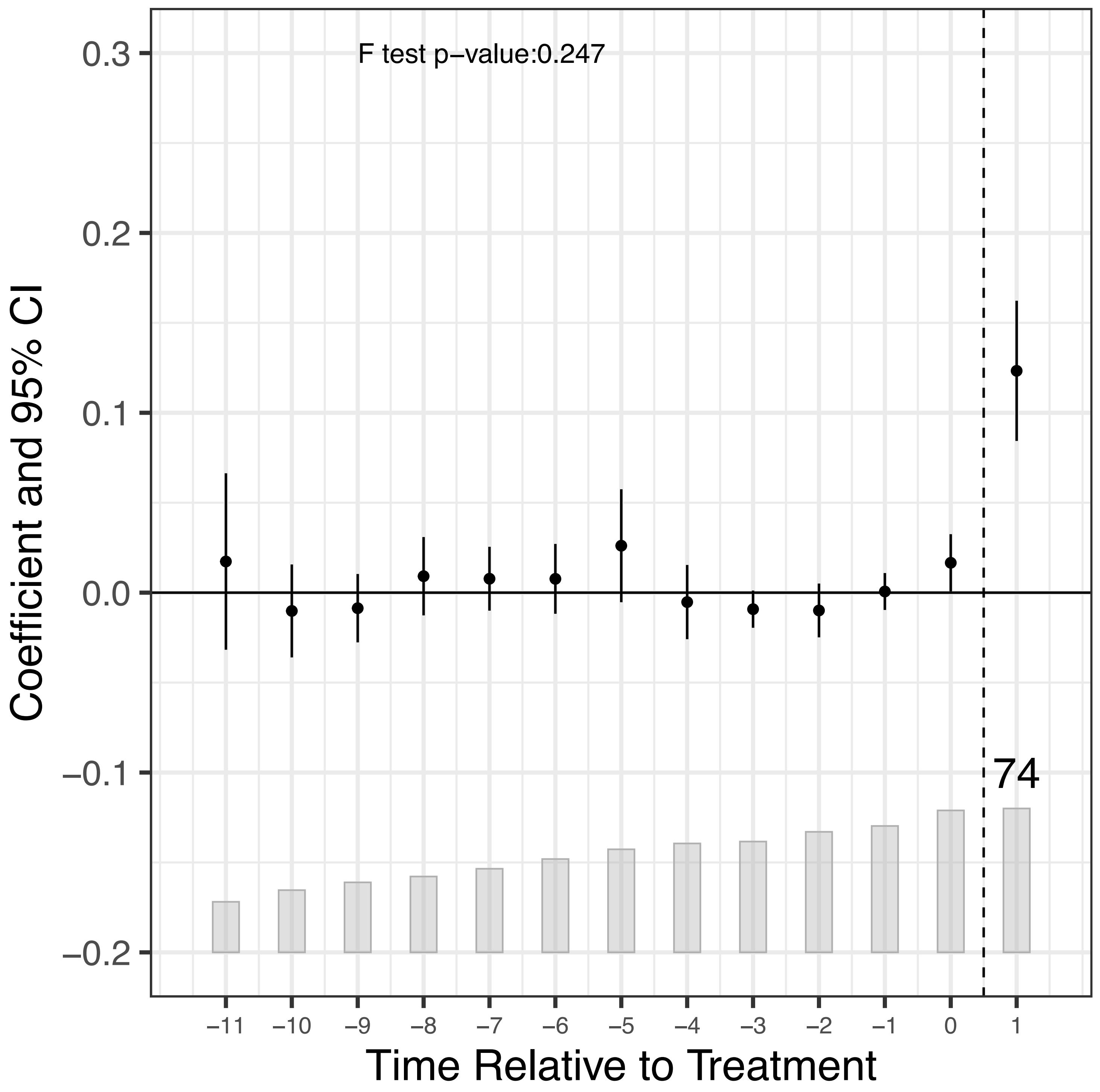

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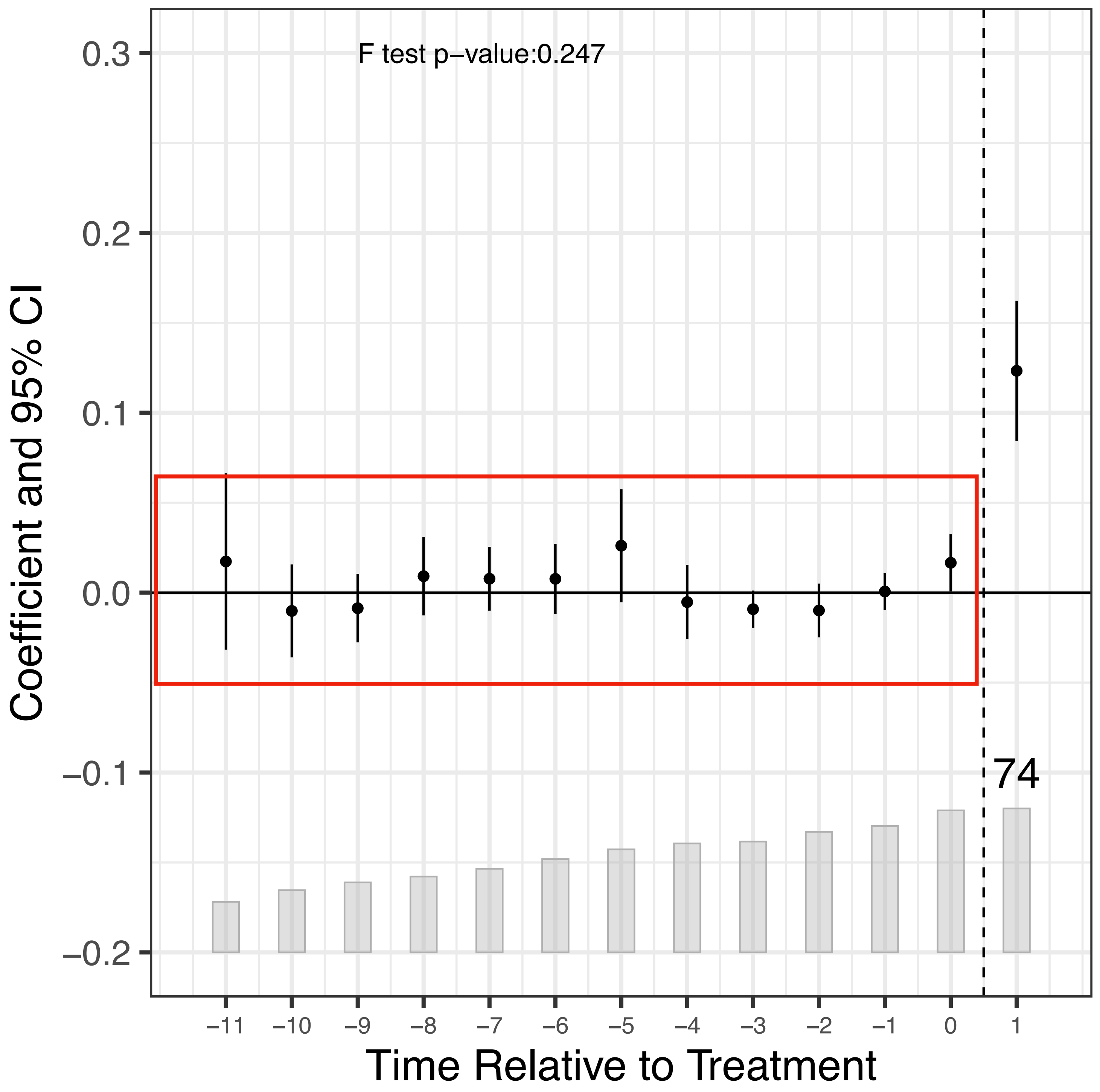


$F$ test for no pretrend
$p = 0.247$

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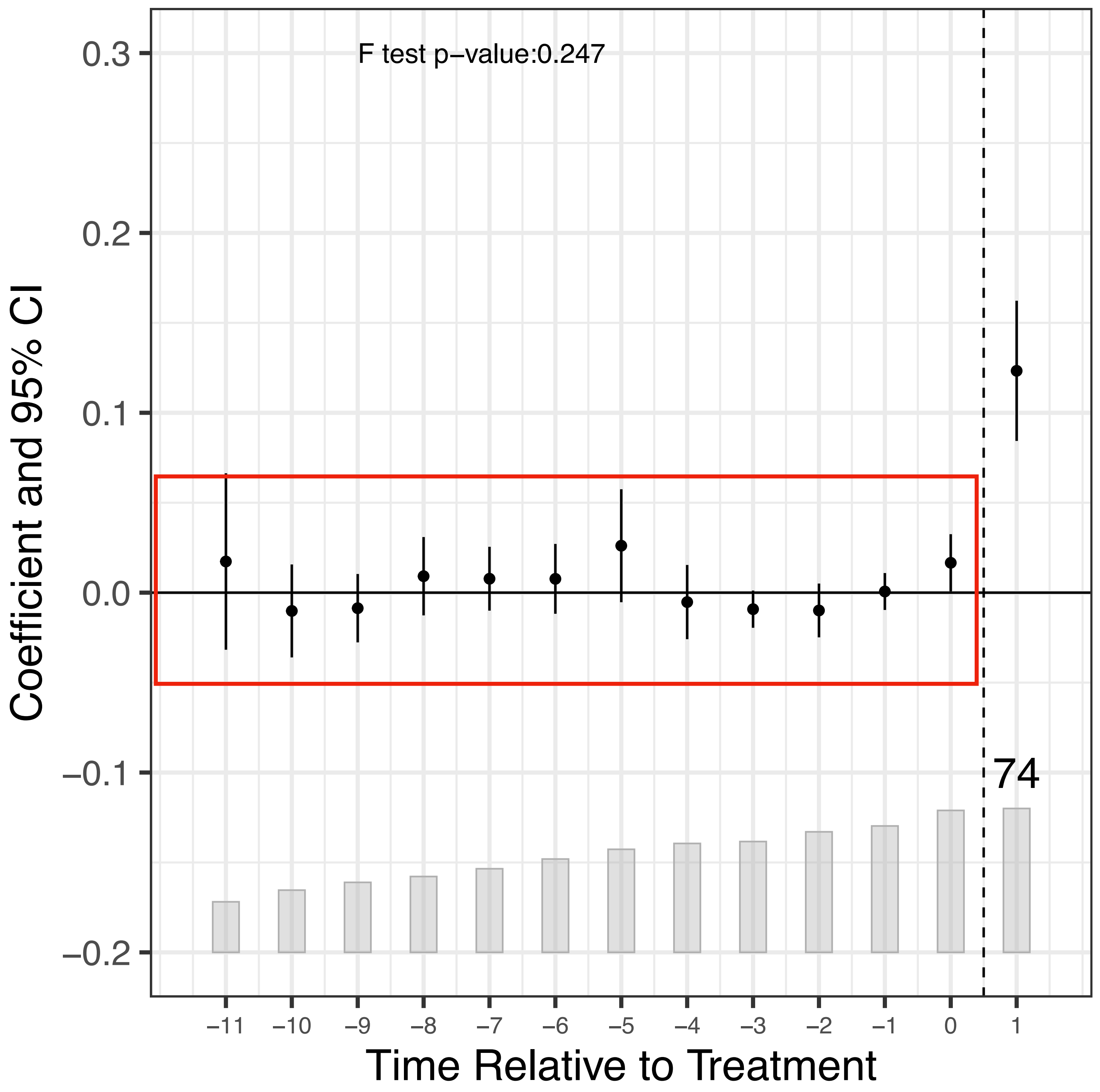


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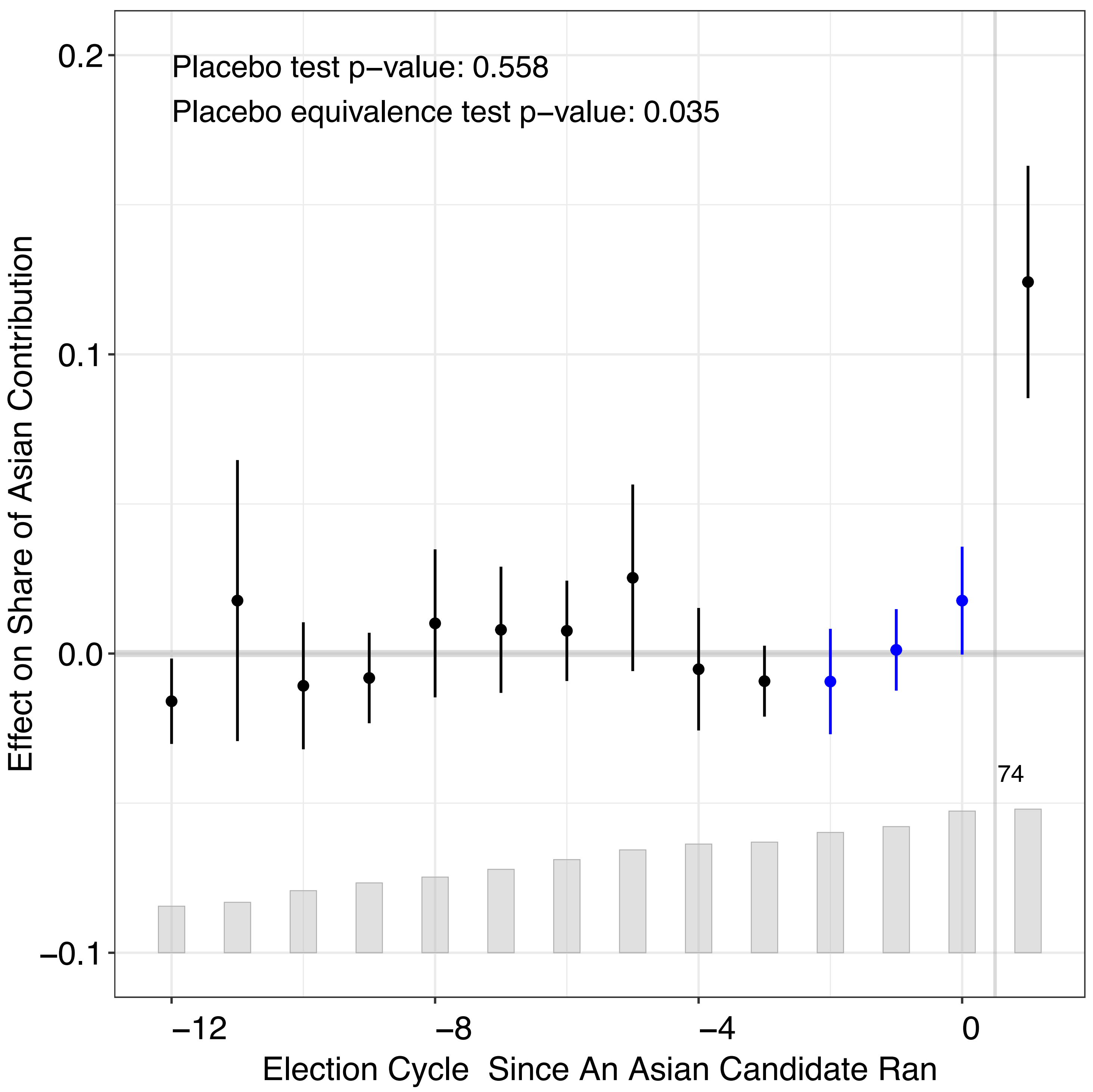


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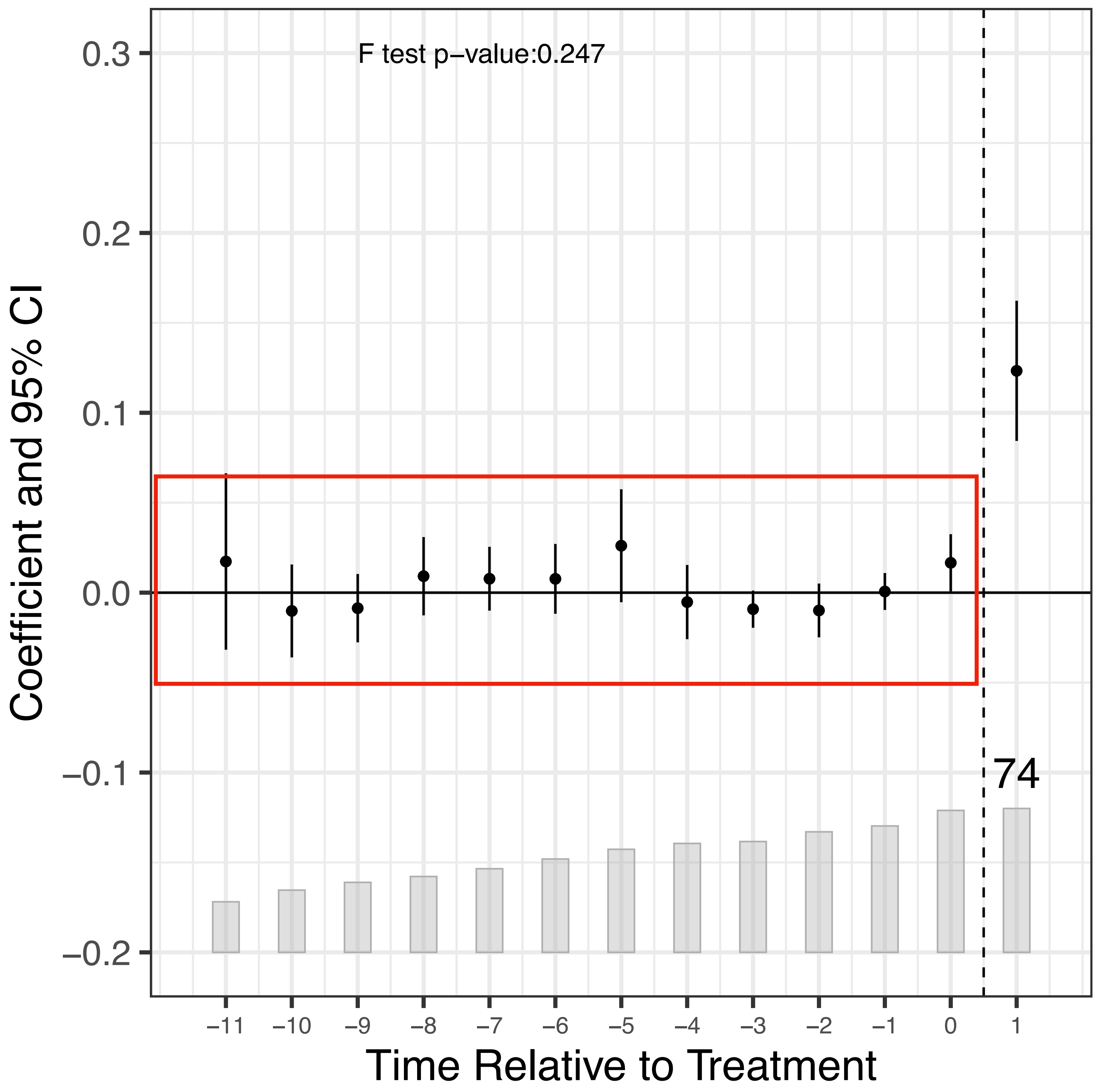


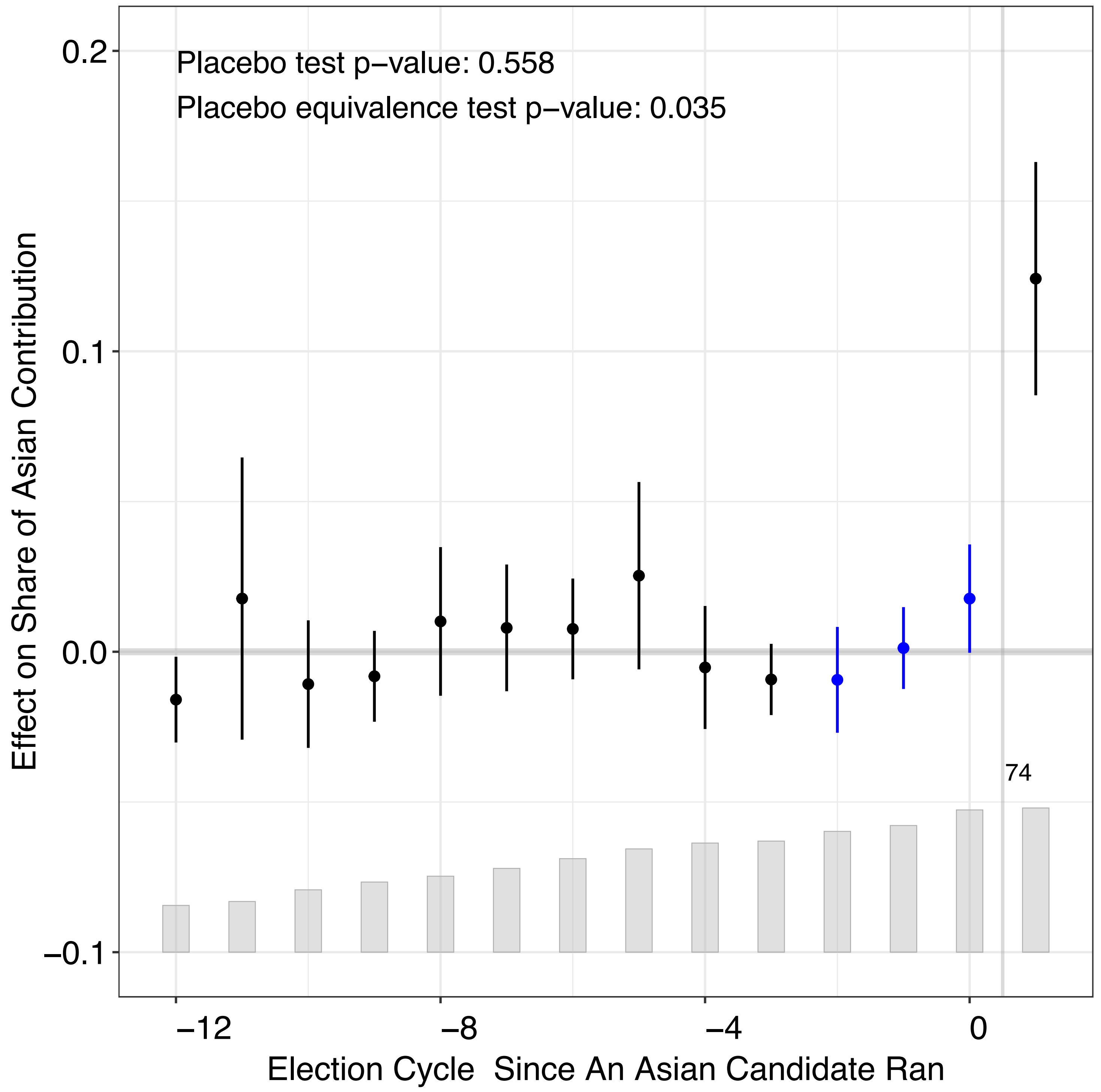


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*Assessing Pretrend*

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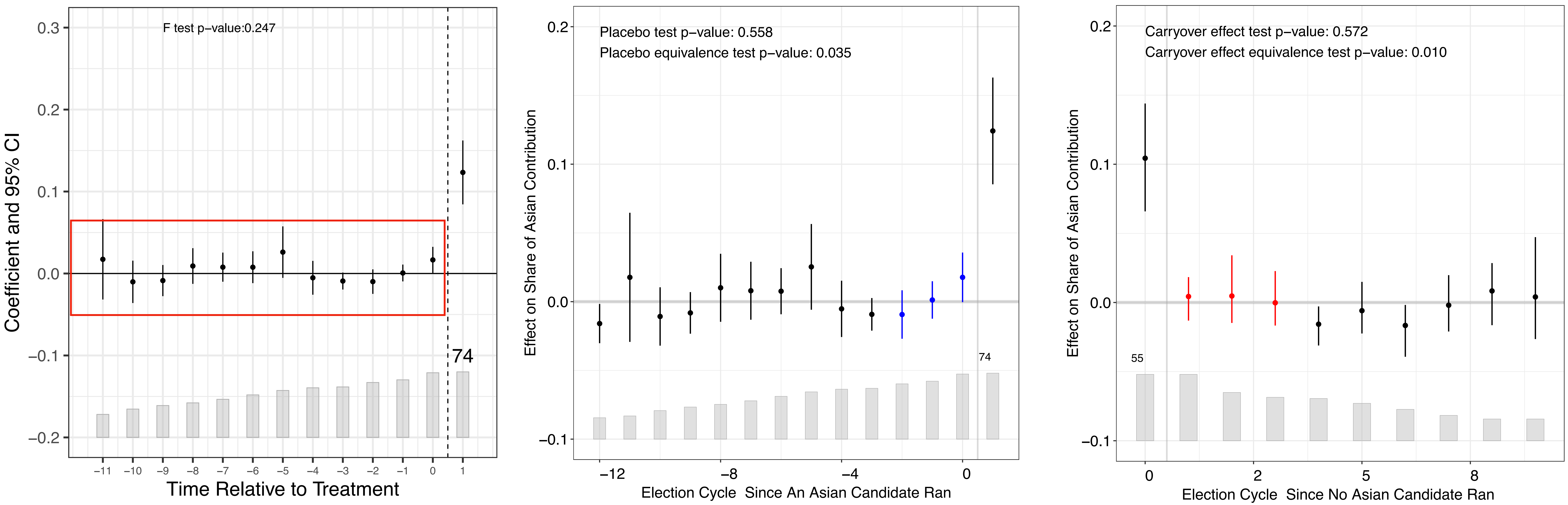


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*Test for carryover effects*
$p = 0.572$

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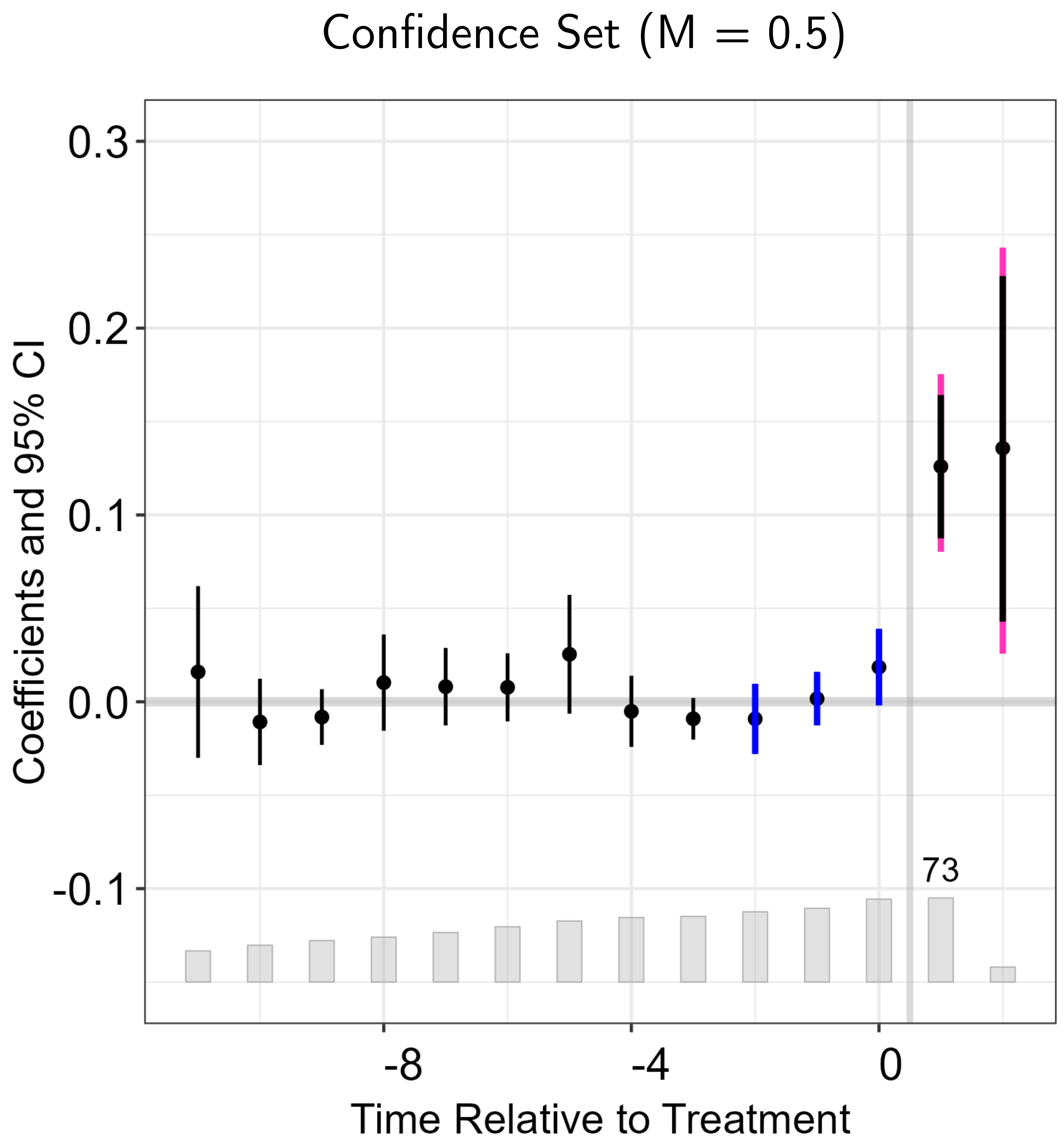

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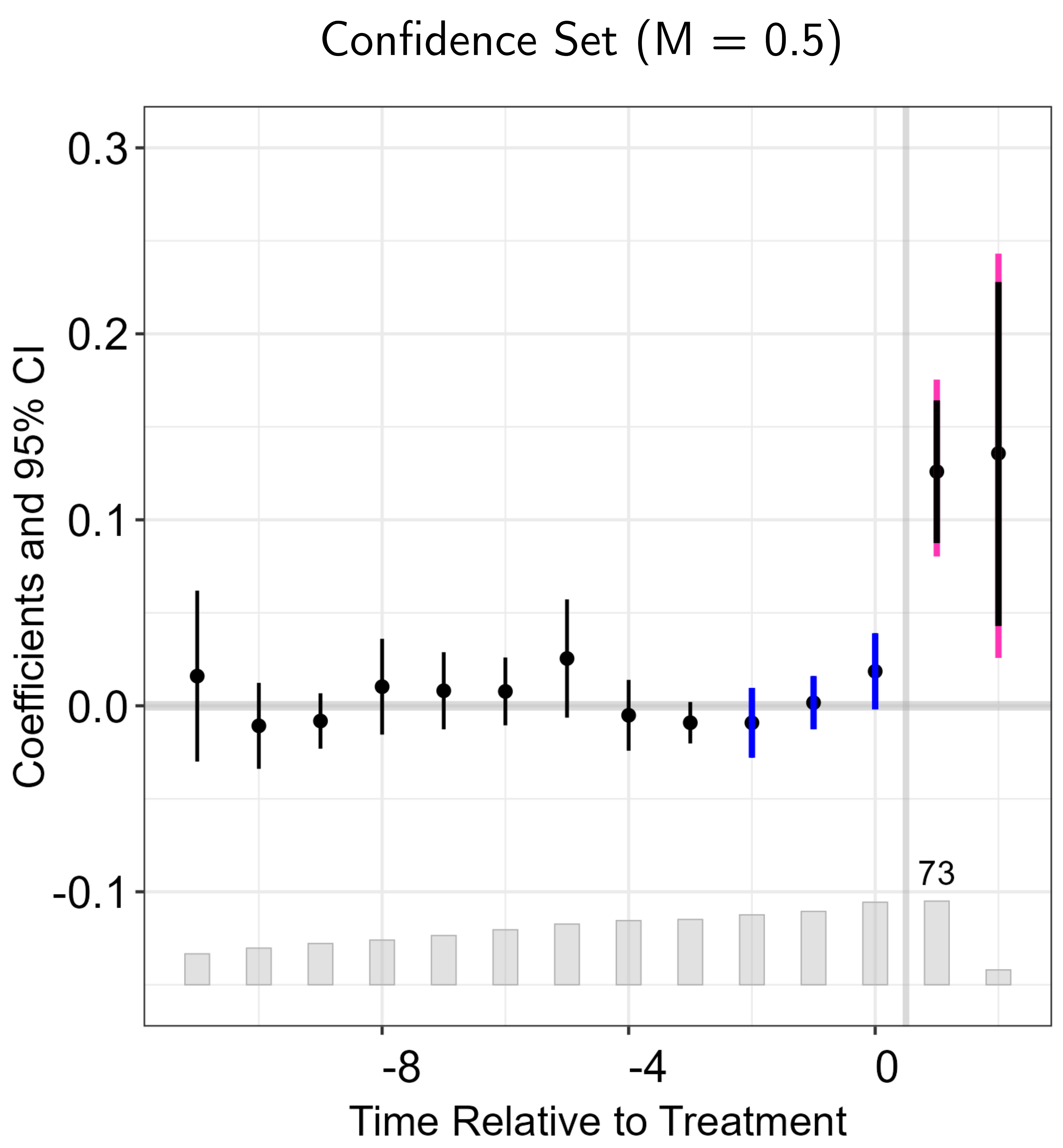


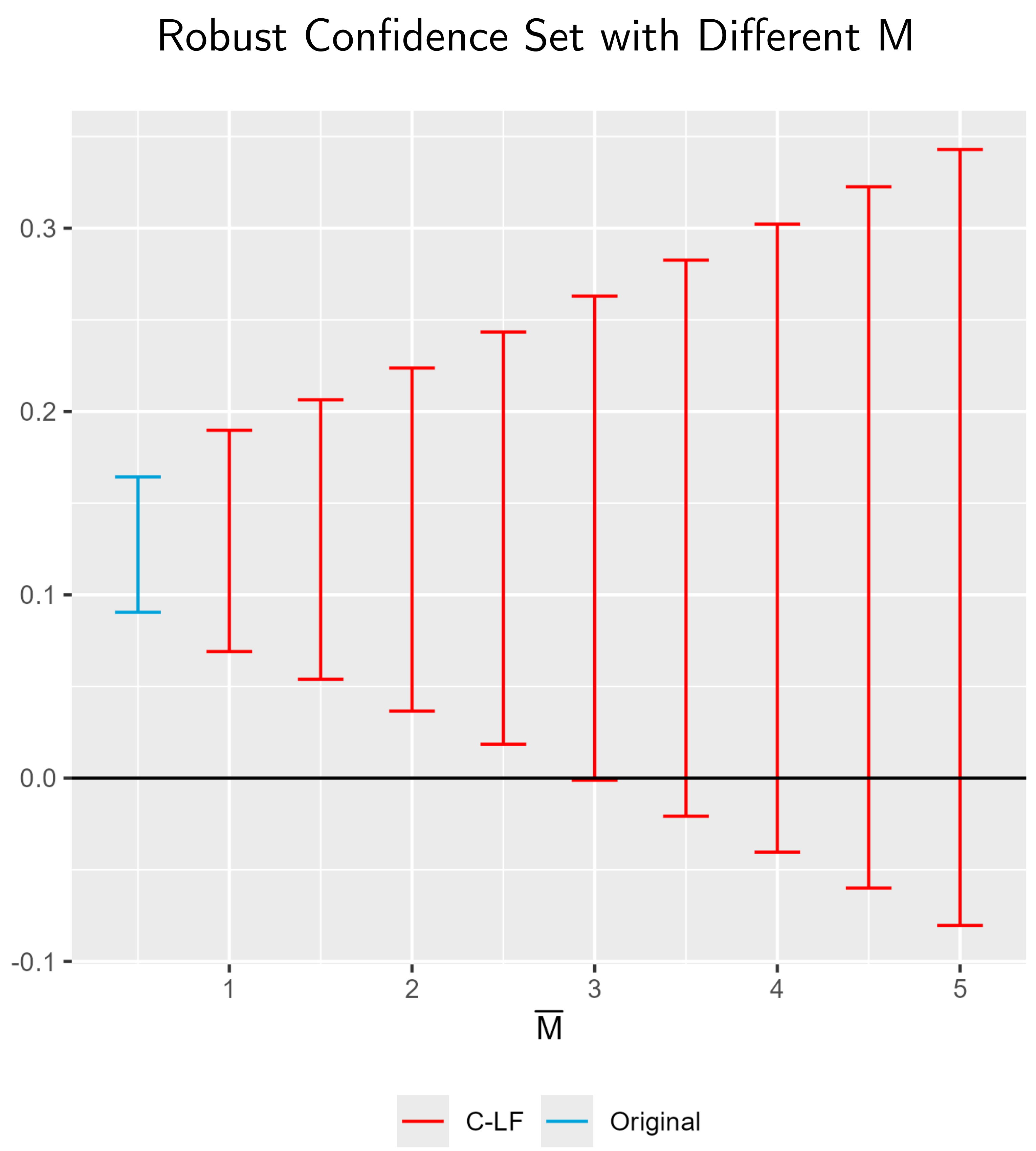

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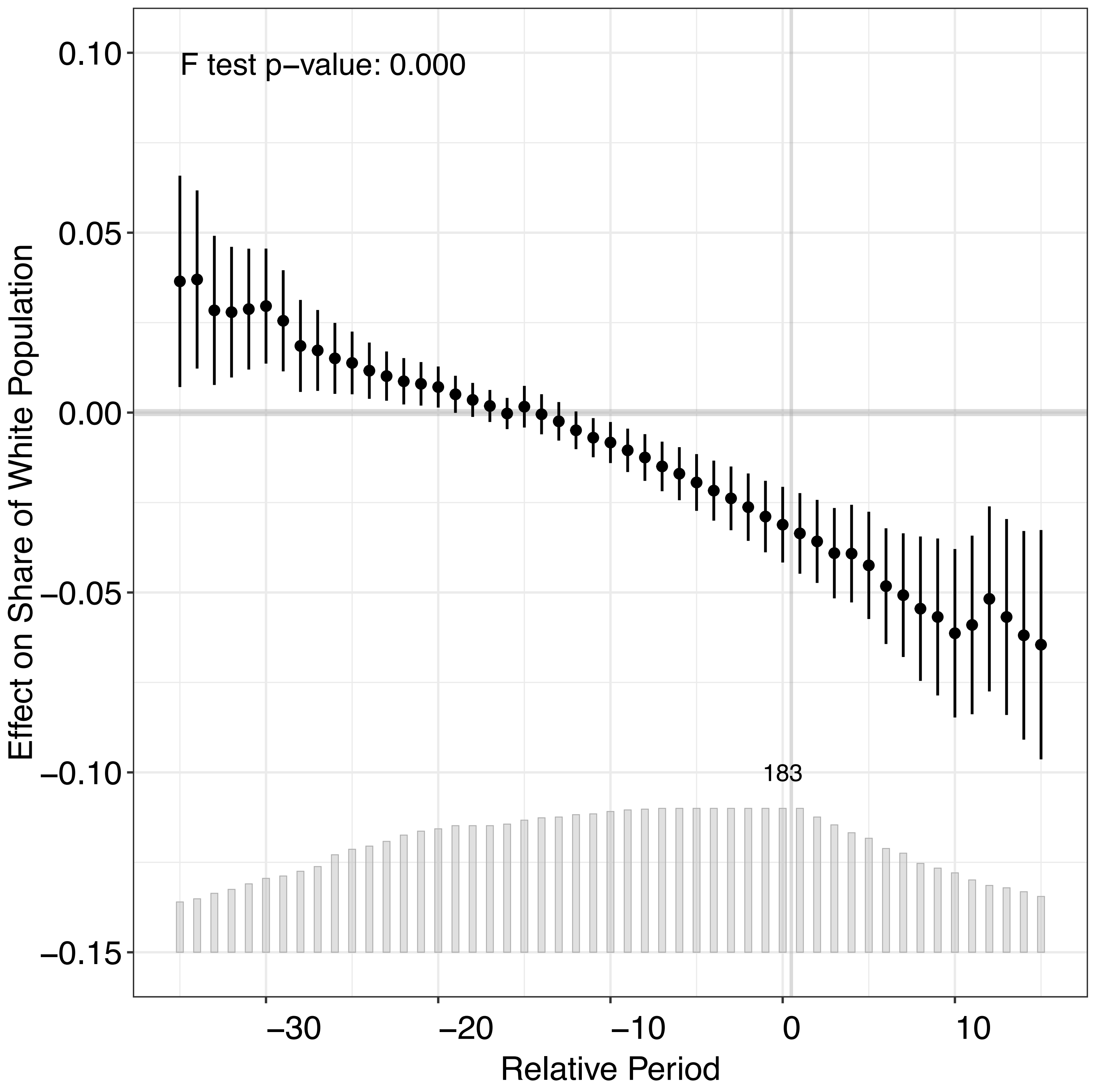

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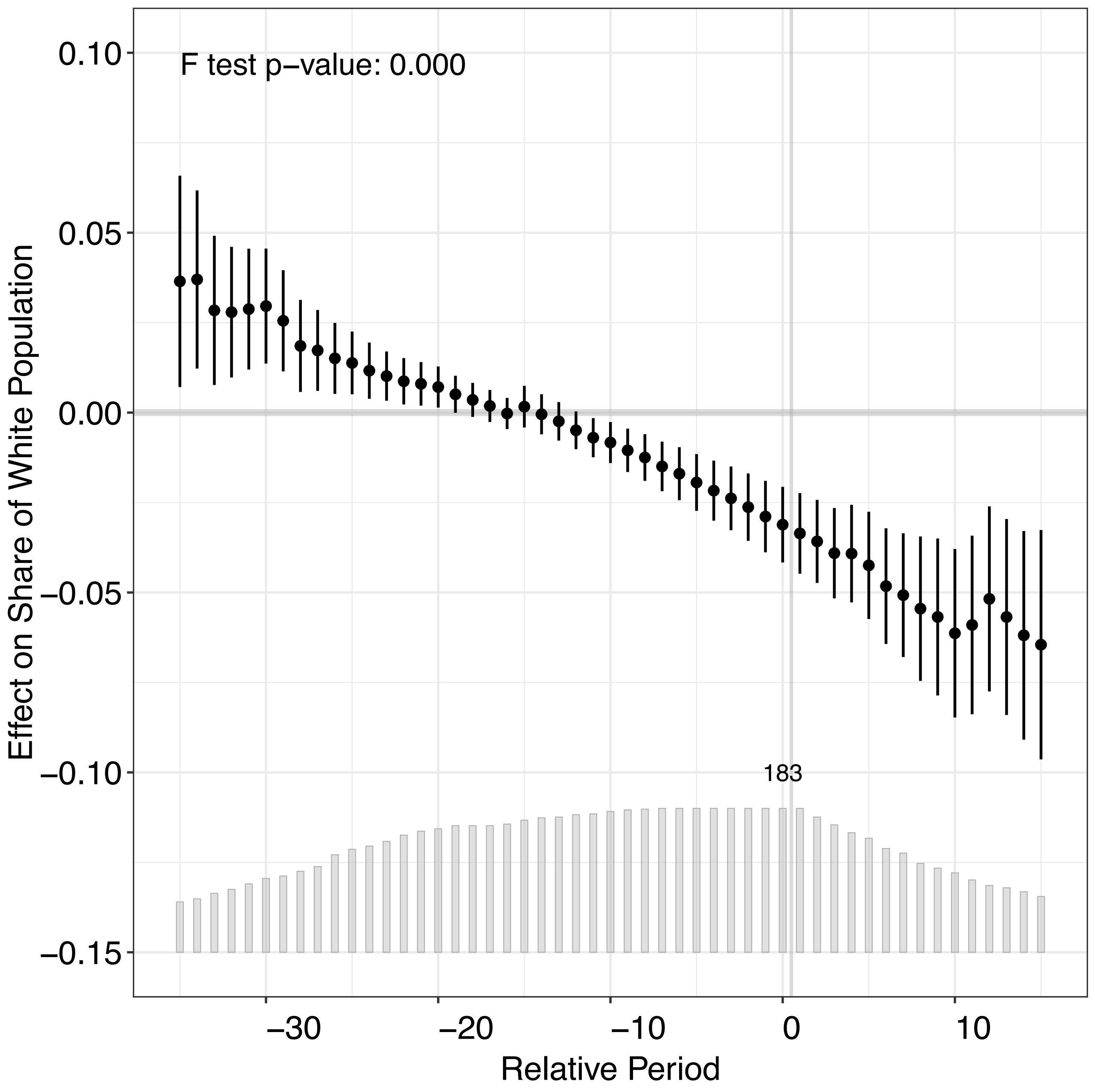


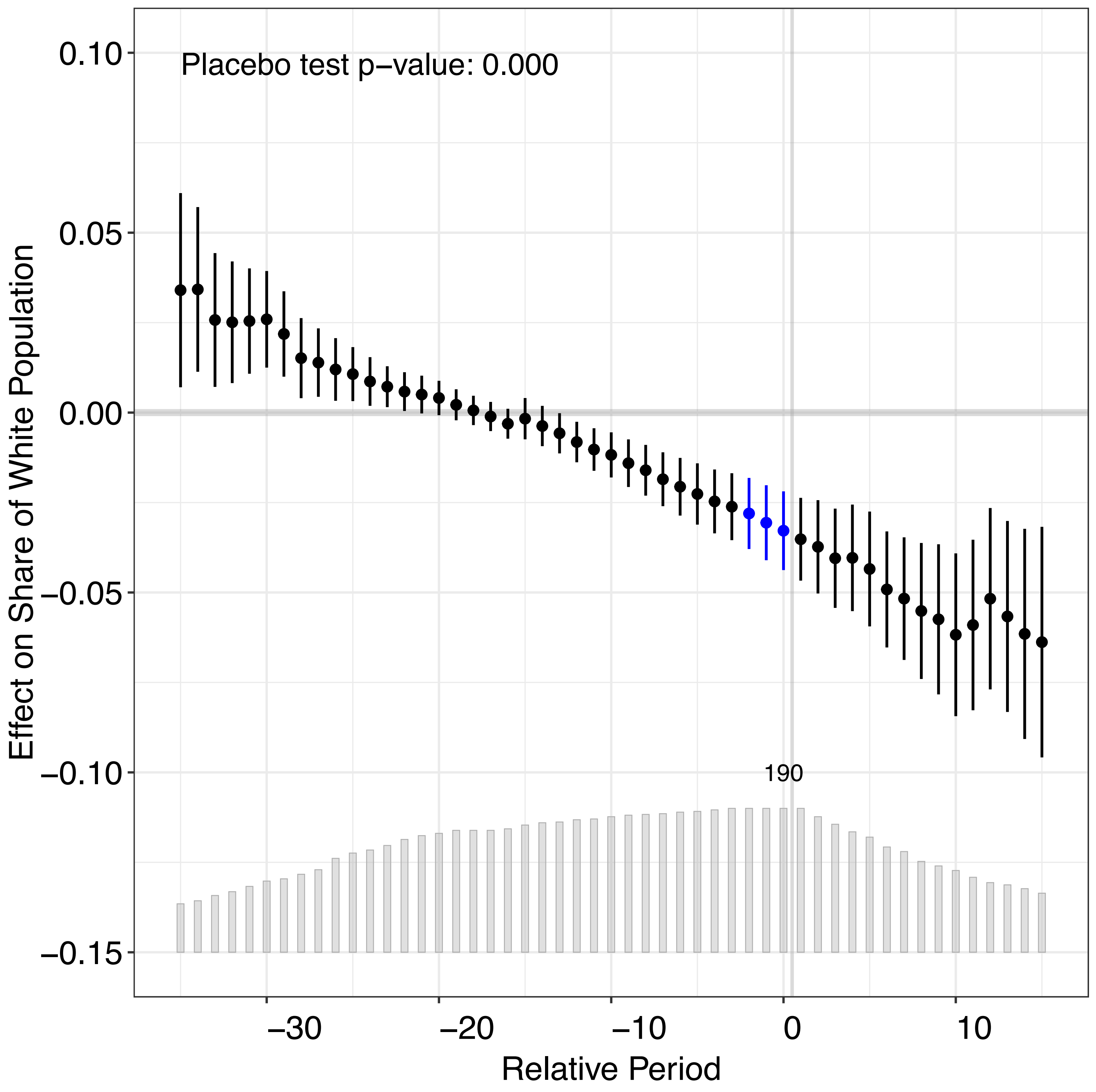

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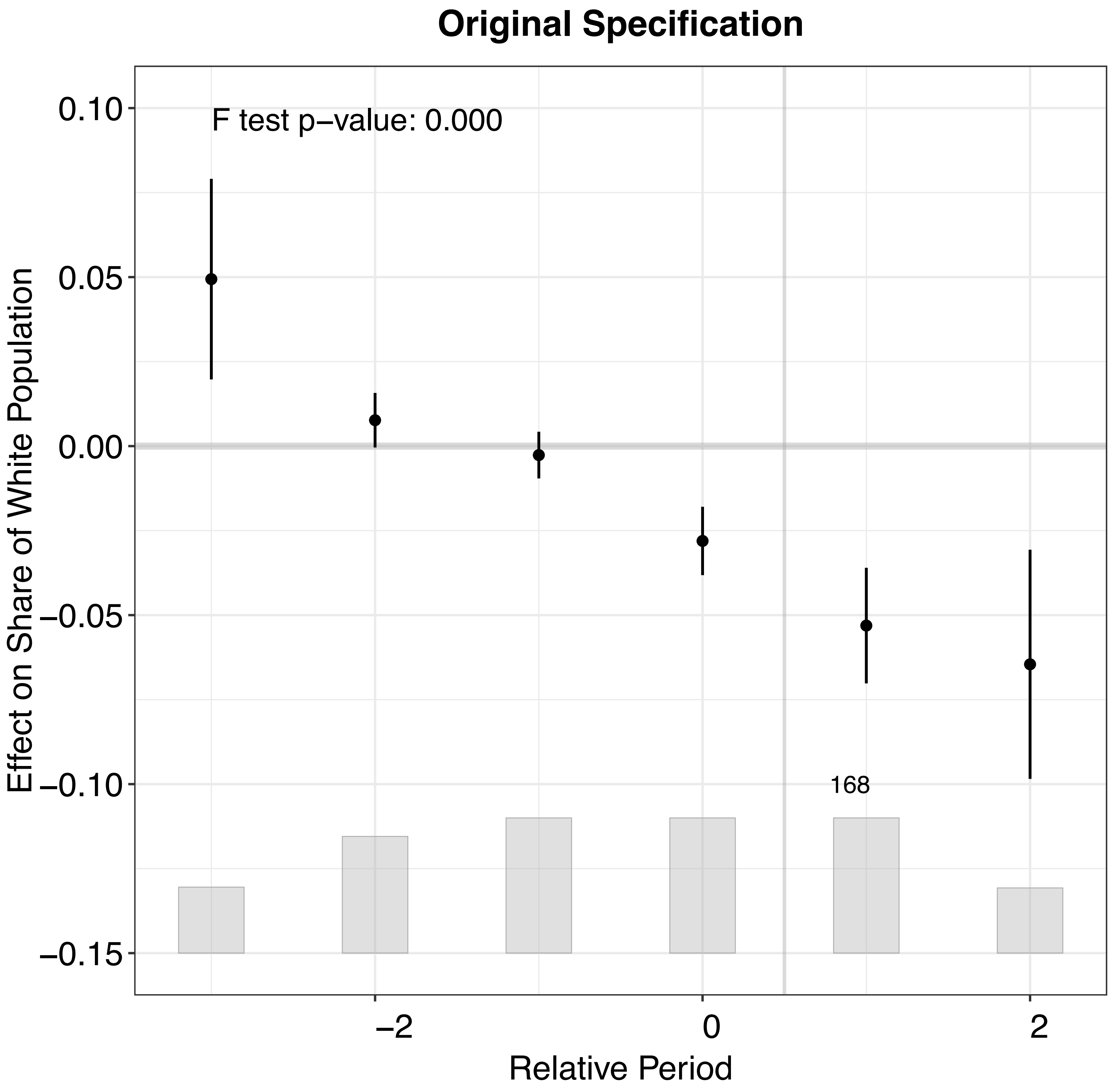

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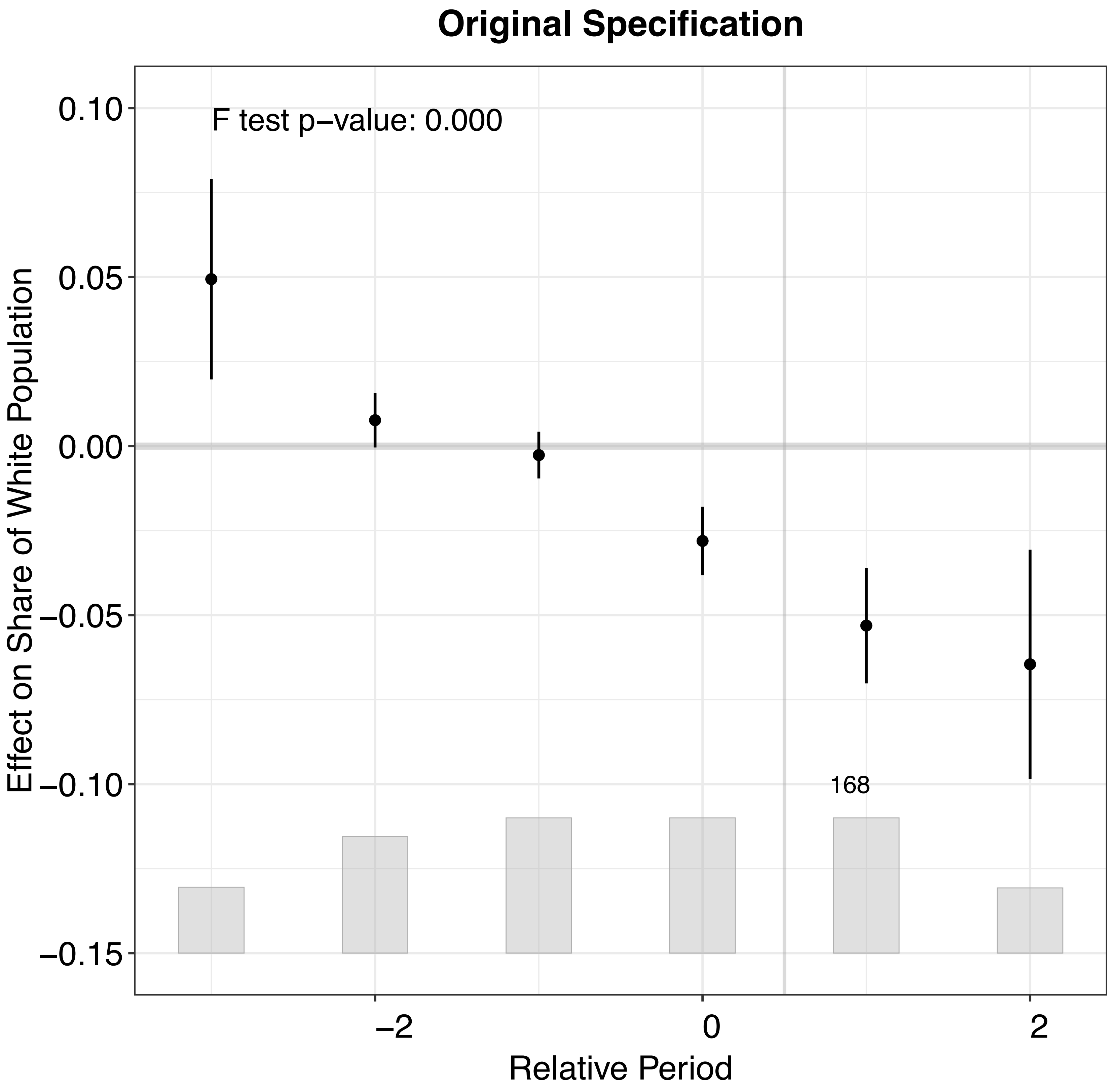
- The negative result is completely gone once we added city-specific linear time-trends



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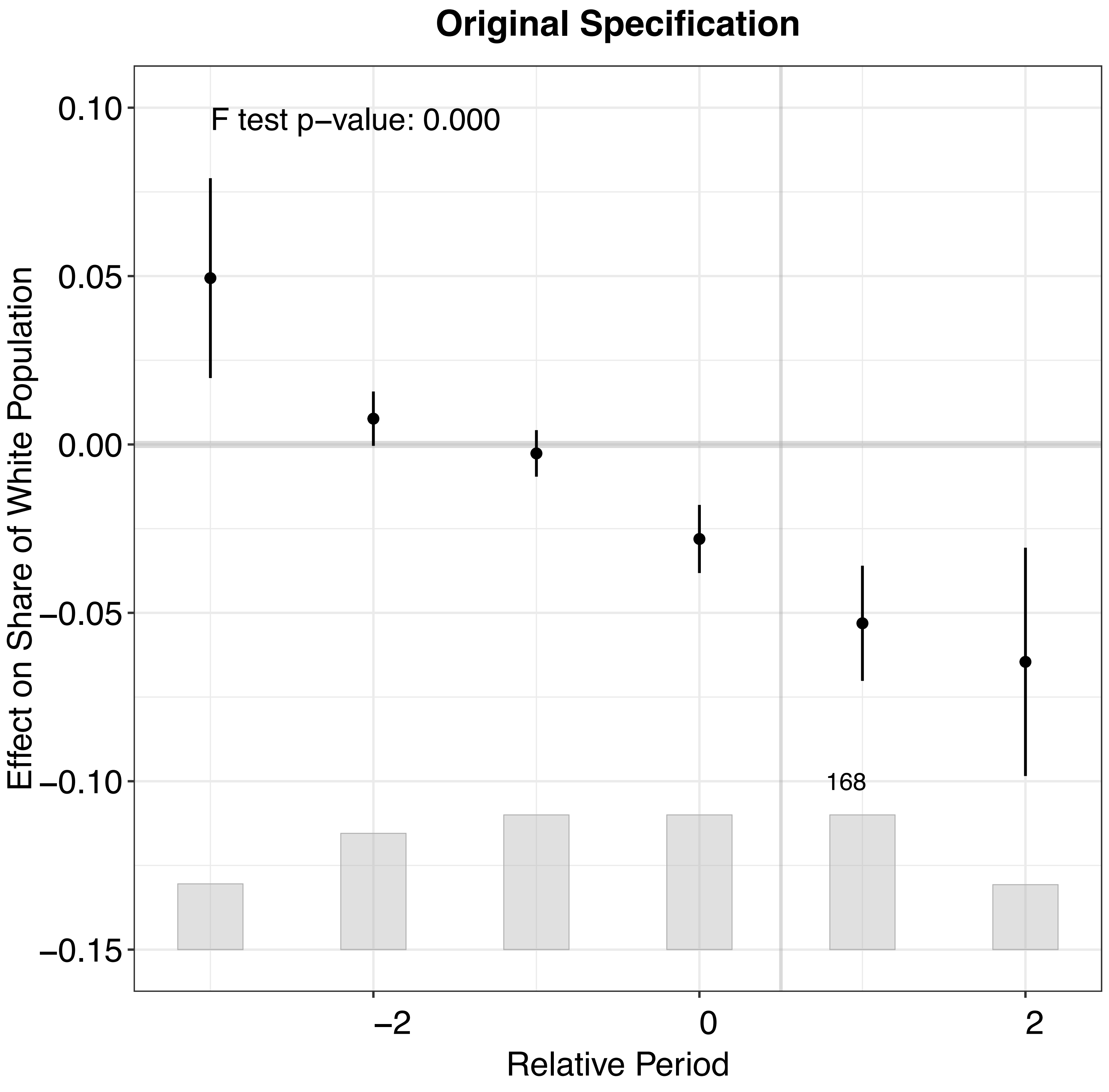
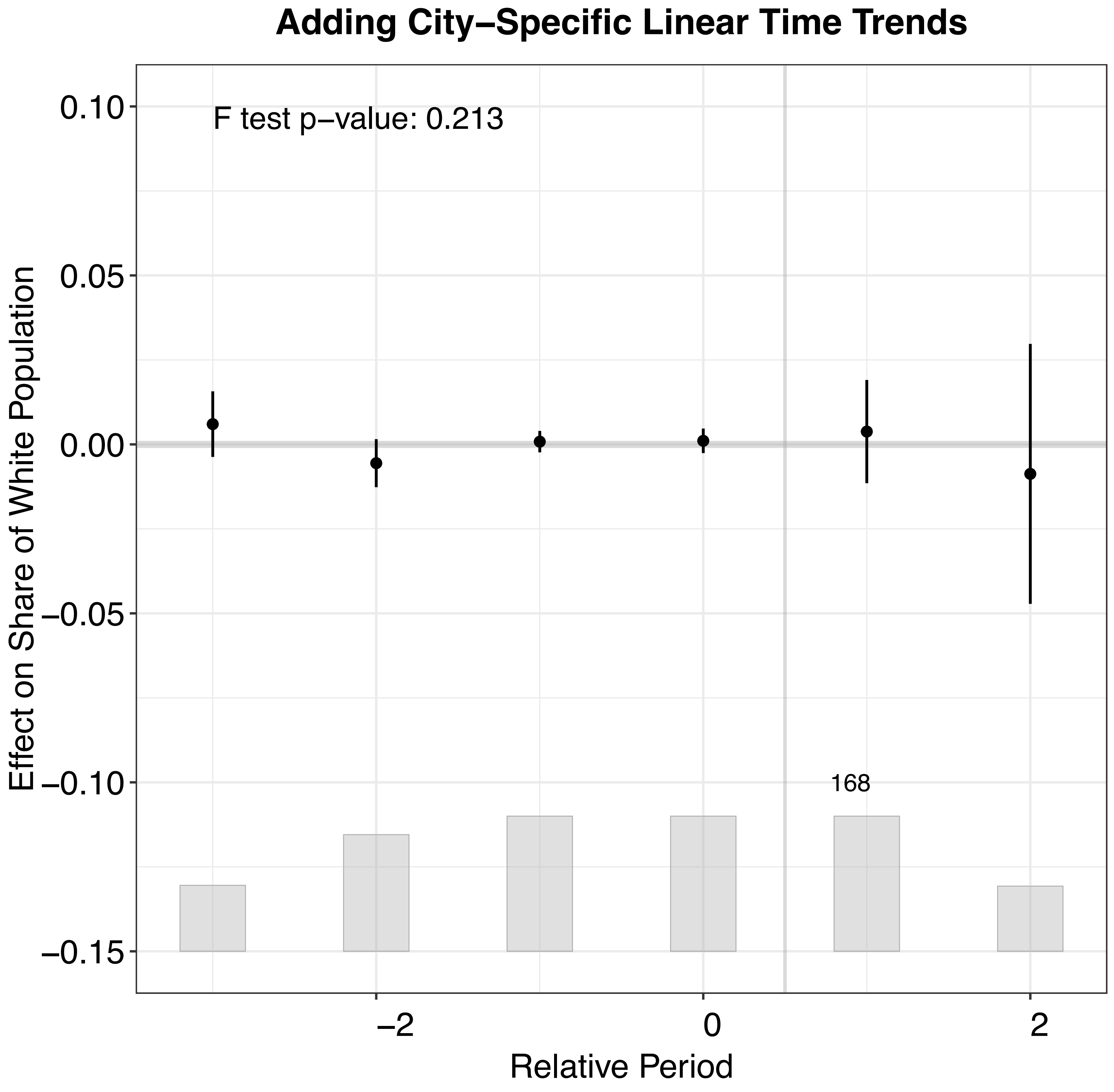
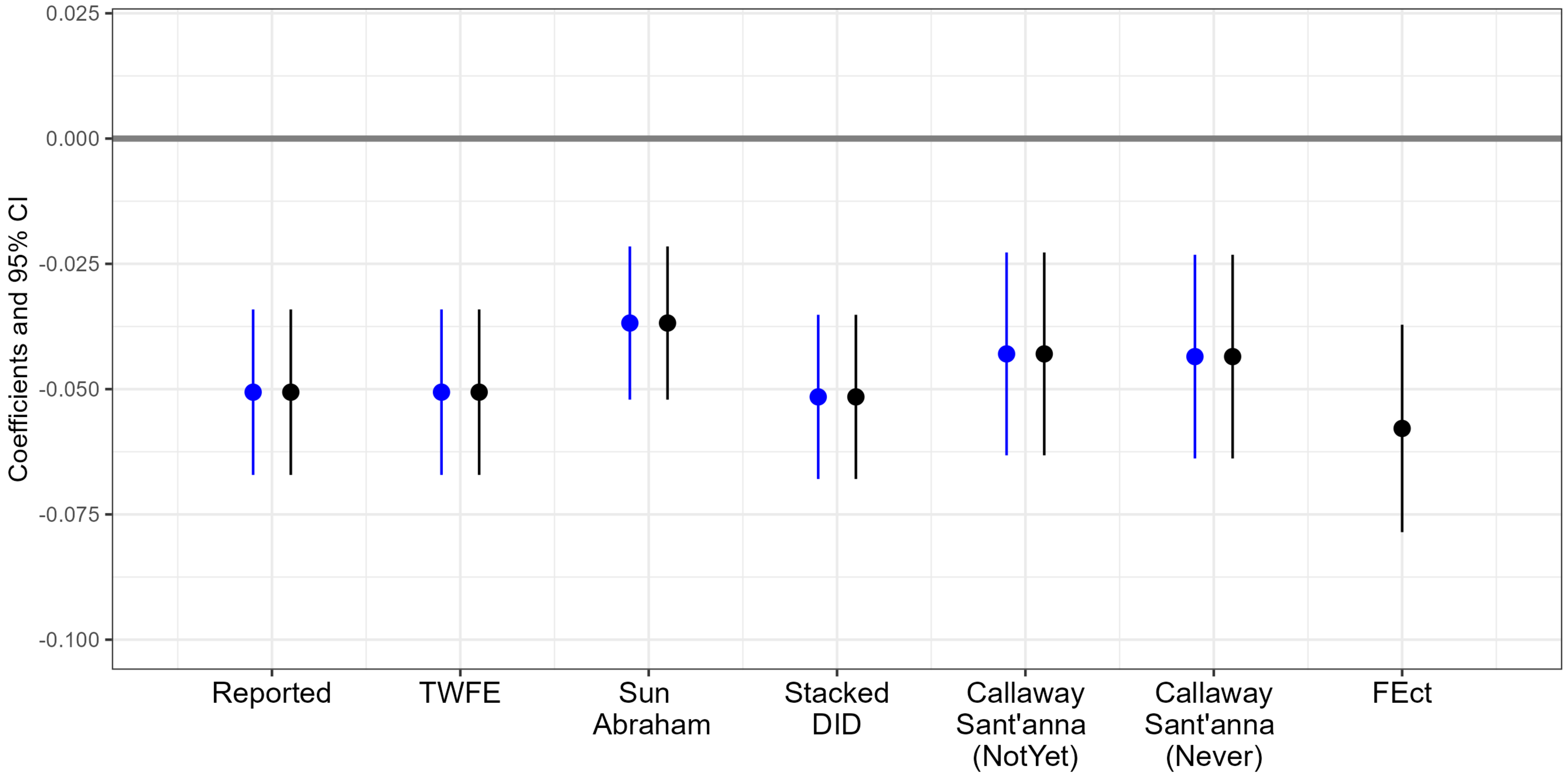


# Various Estimators Still Broadly Agree



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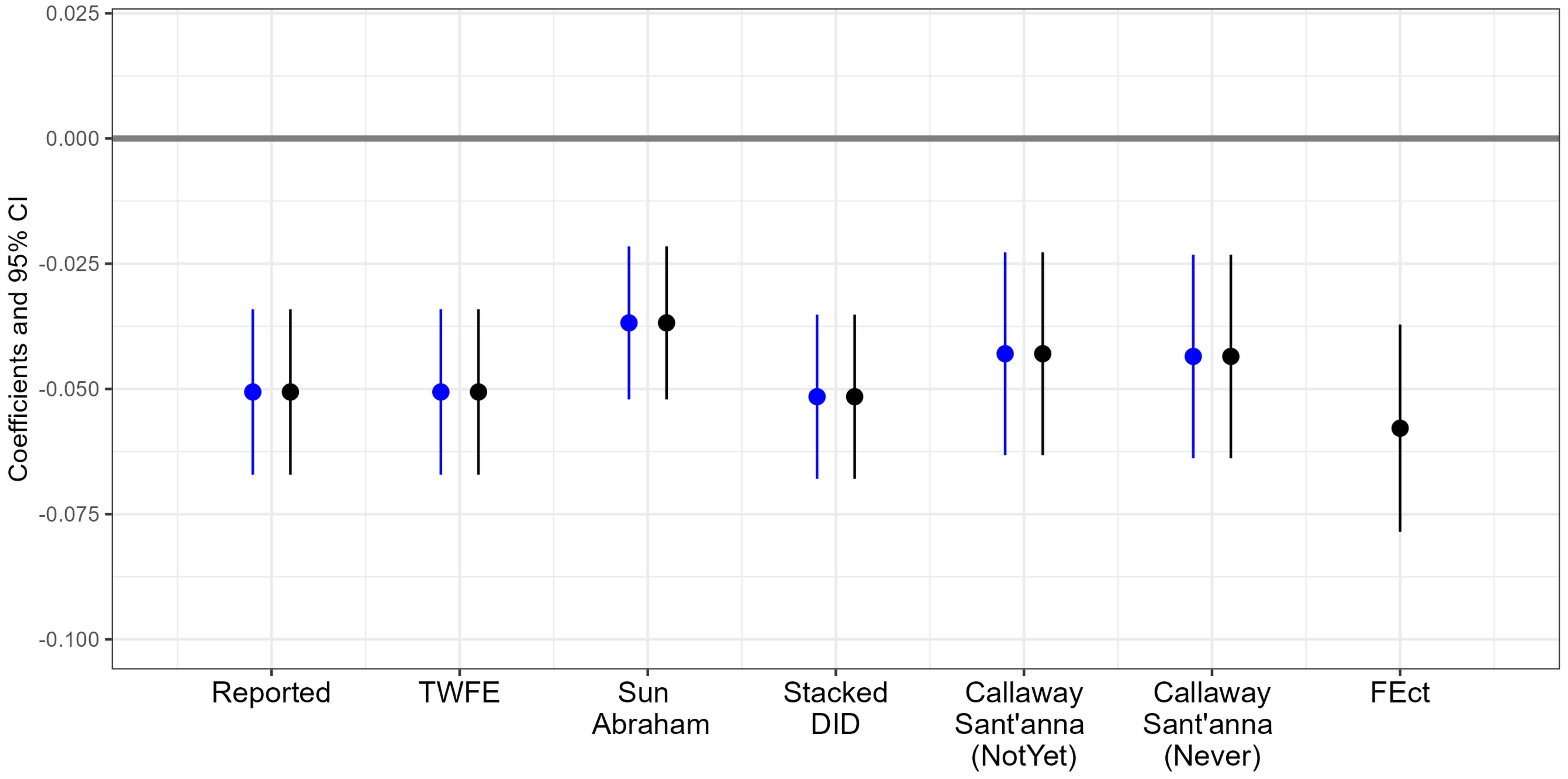
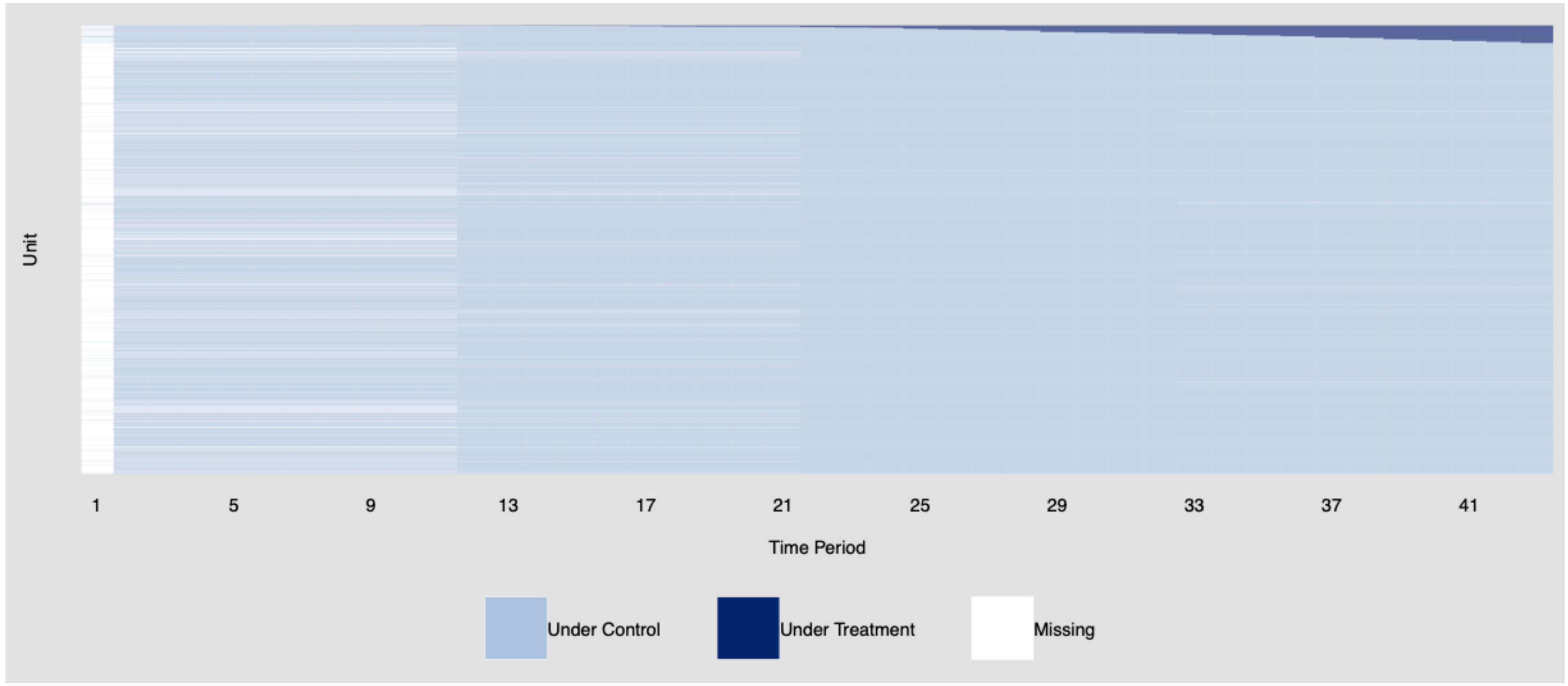
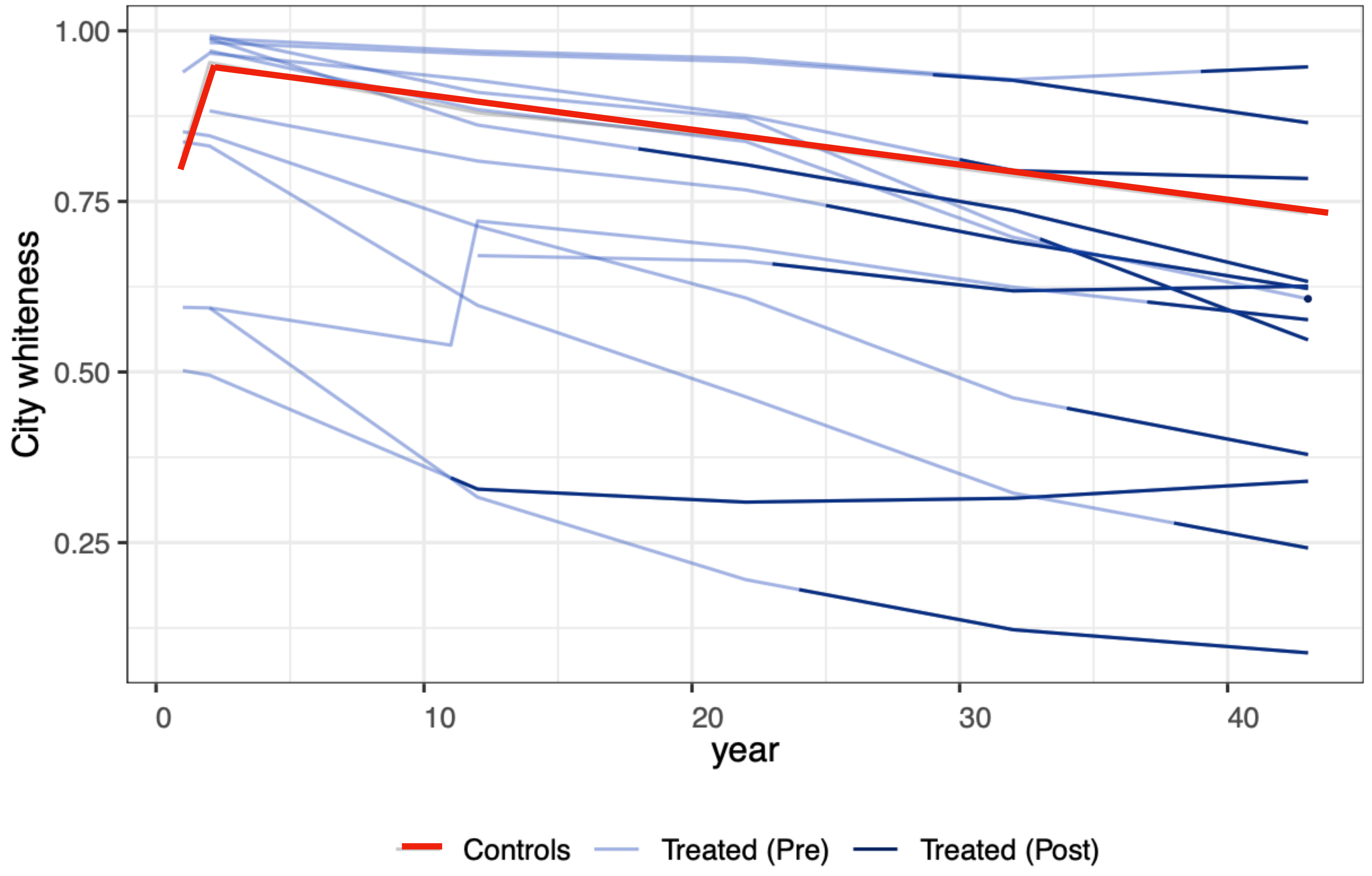


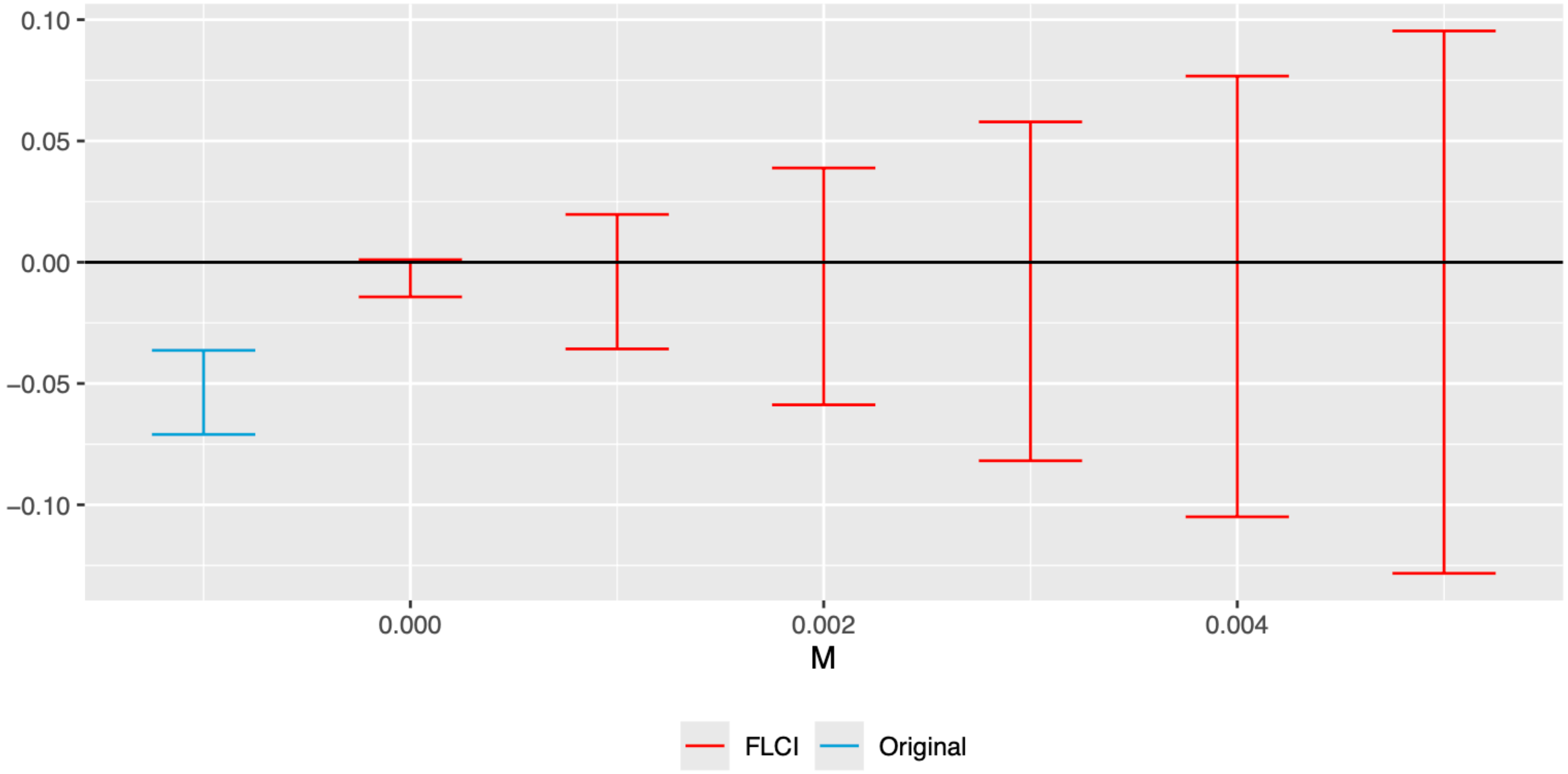

Treatment Status




Outcome Trajectories by Cohort

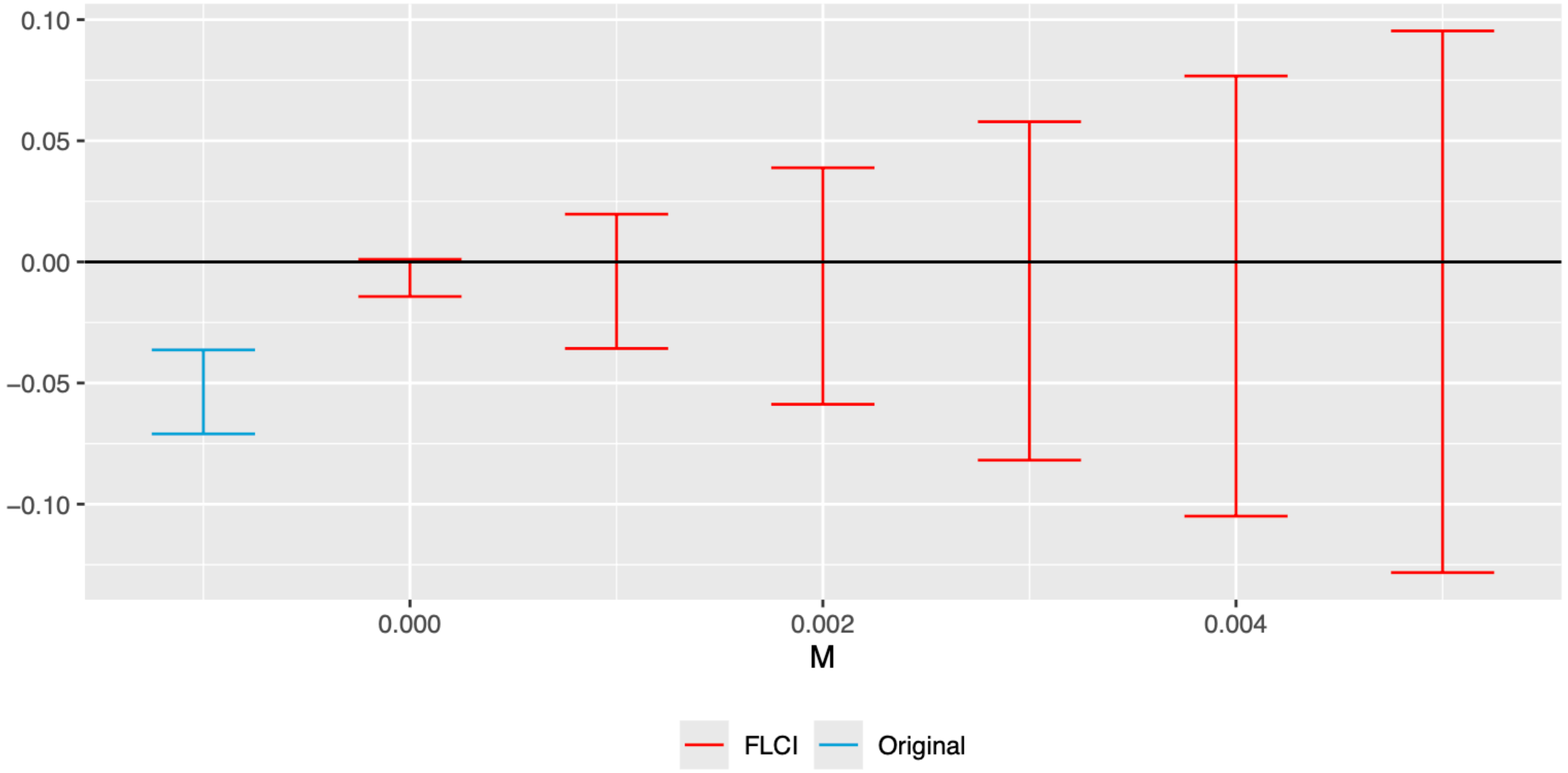


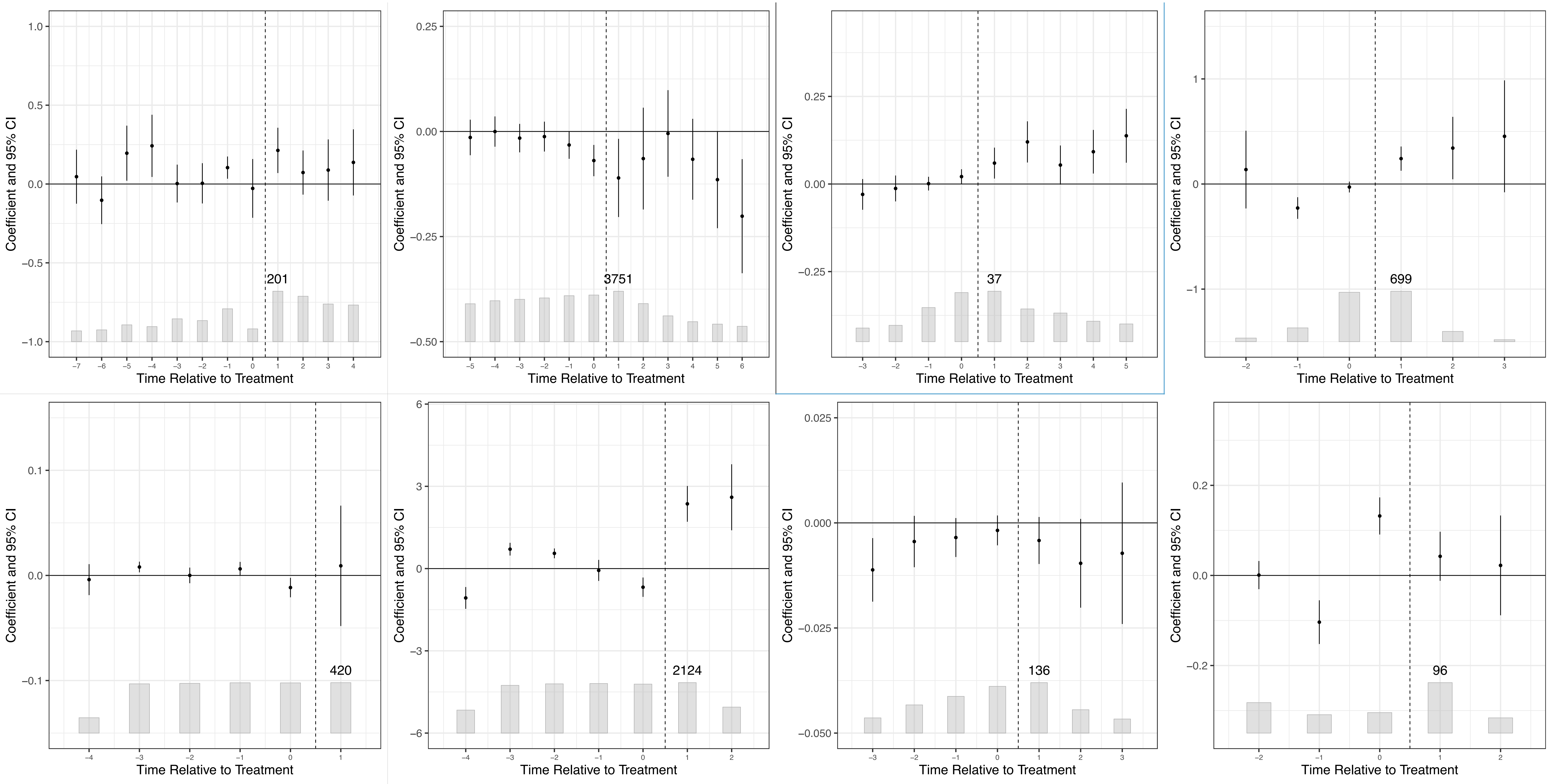
# Sensitivity Analysis w/ Smoothness Restriction

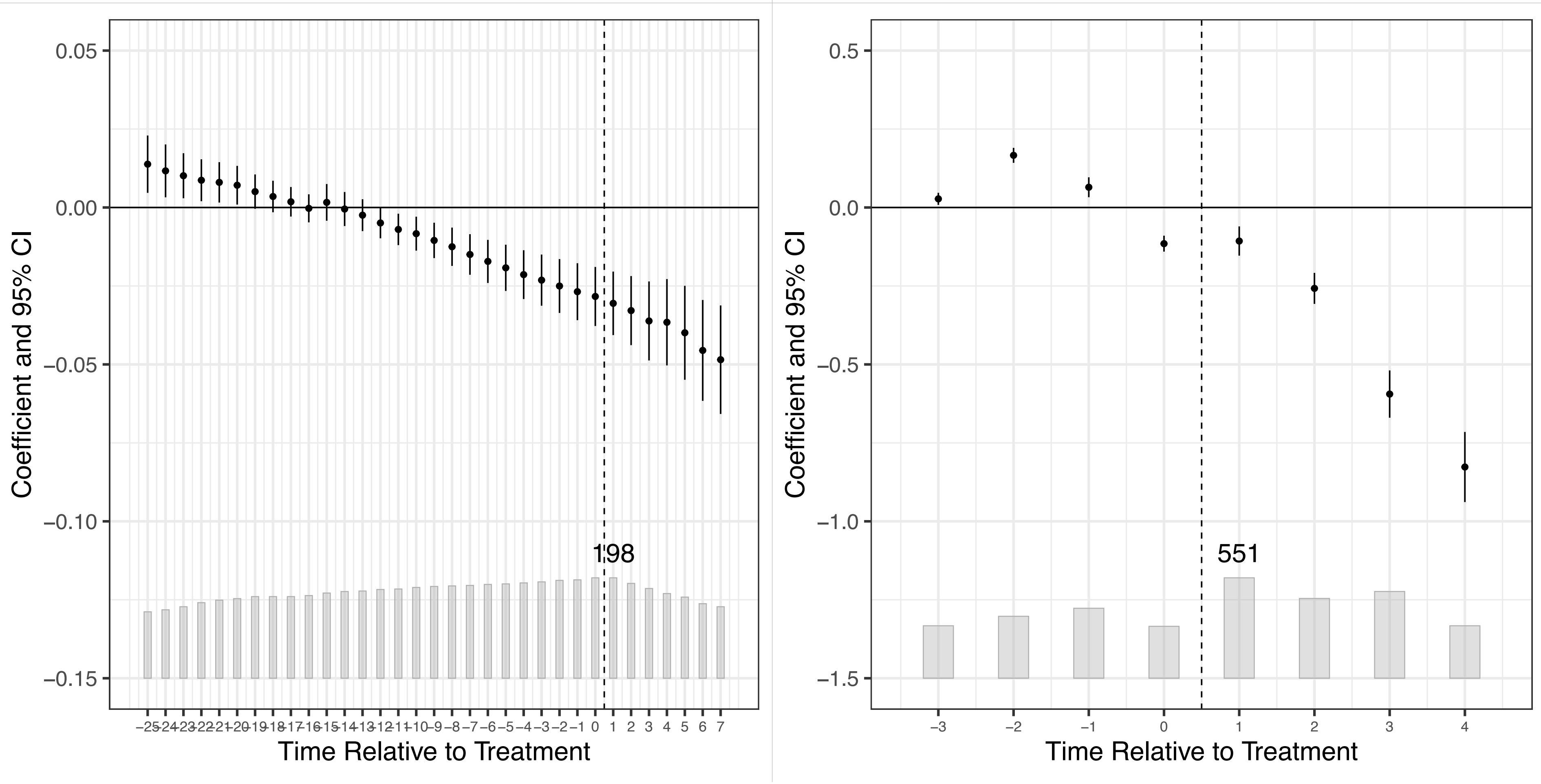


# Sensitivity Analysis w/ Smoothness Restriction

- Sensitivity analysis reveals that the result is not robust to a PT violation with a linear time trend.






# Problematic Pretrends Are Not Rare

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  - Simple plotting (and tests) will help spot the issue

# Example 3: Updating cadastral maps on Tax Revenue

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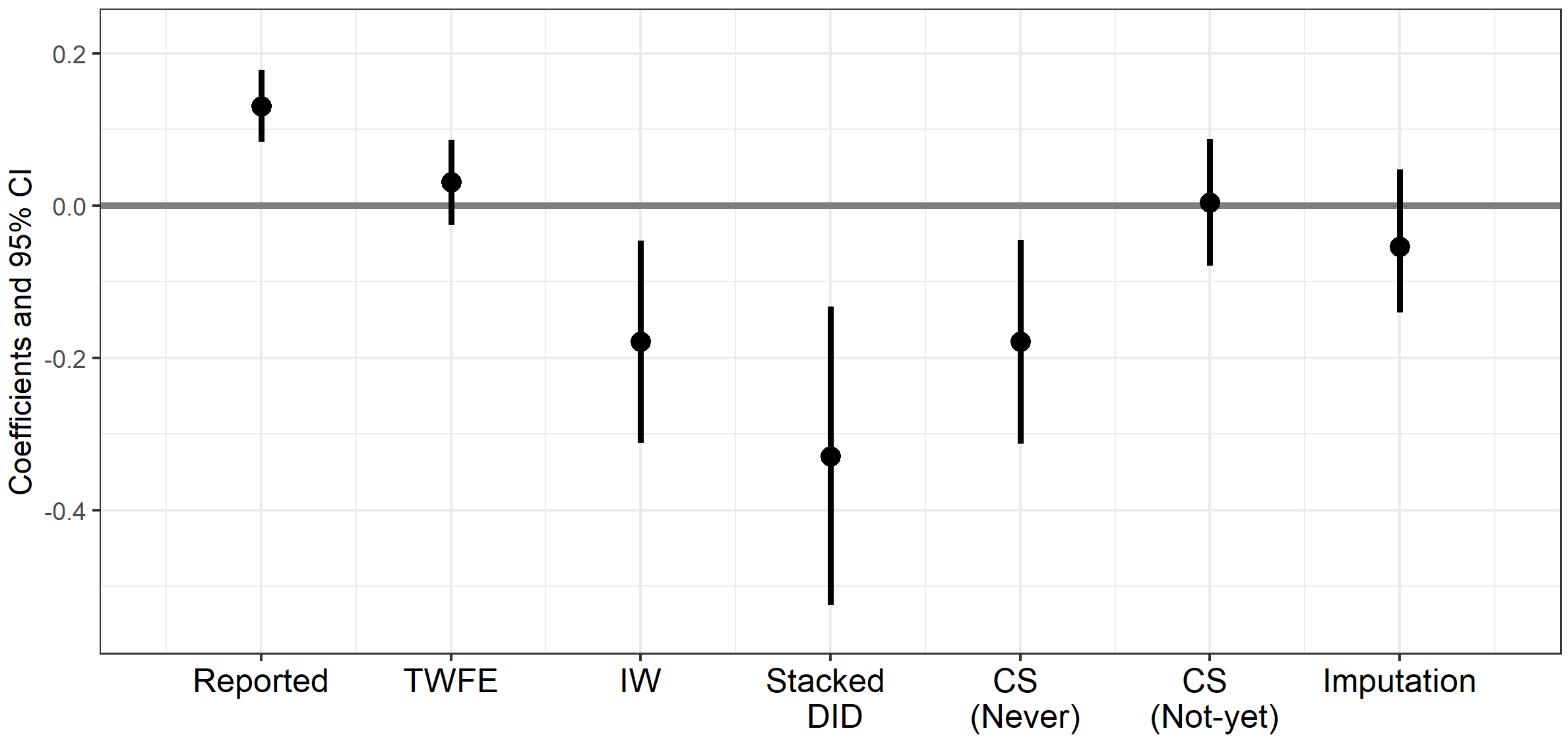


Replicated based on the Full Sample

# Example 3: Updating cadastral maps on Tax Revenue

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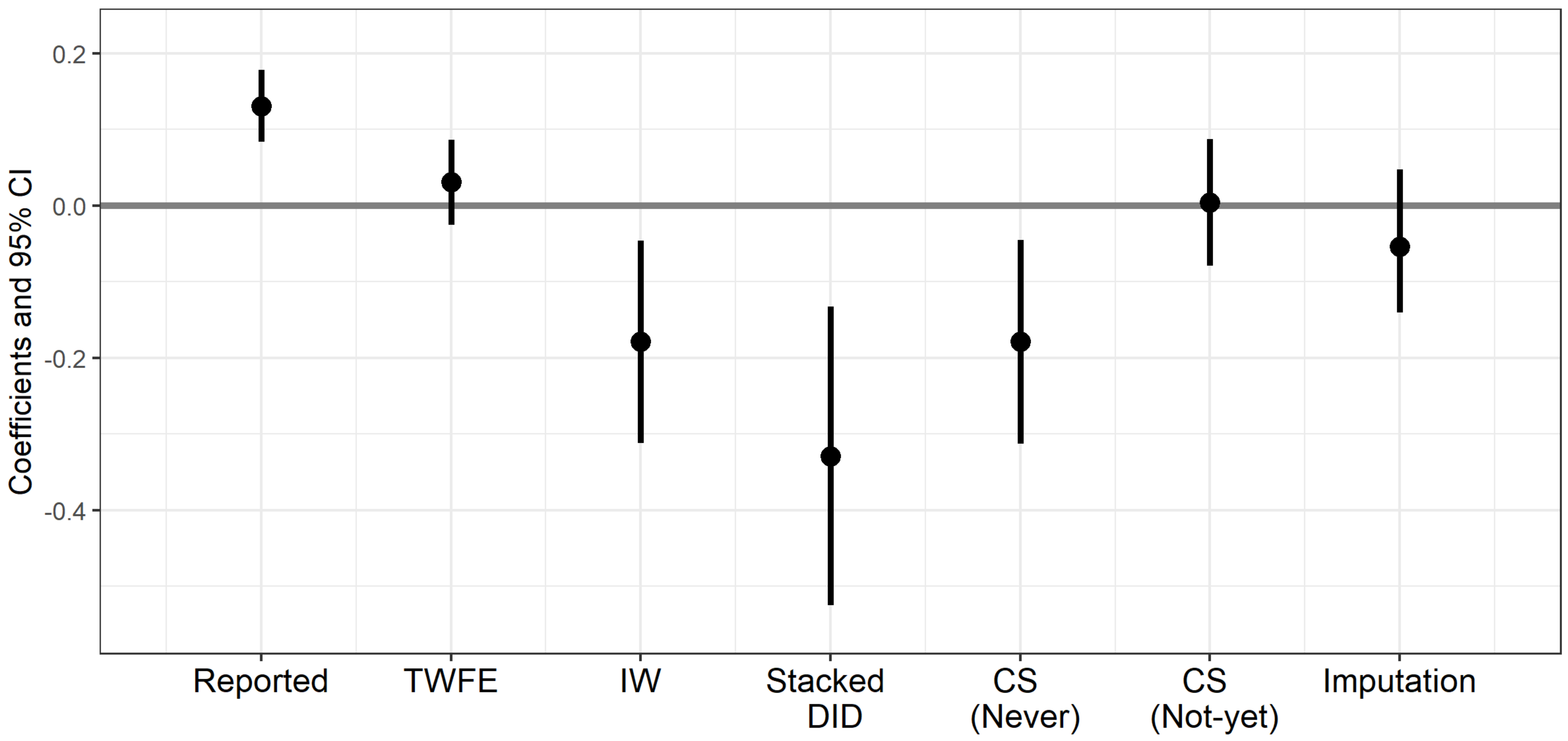


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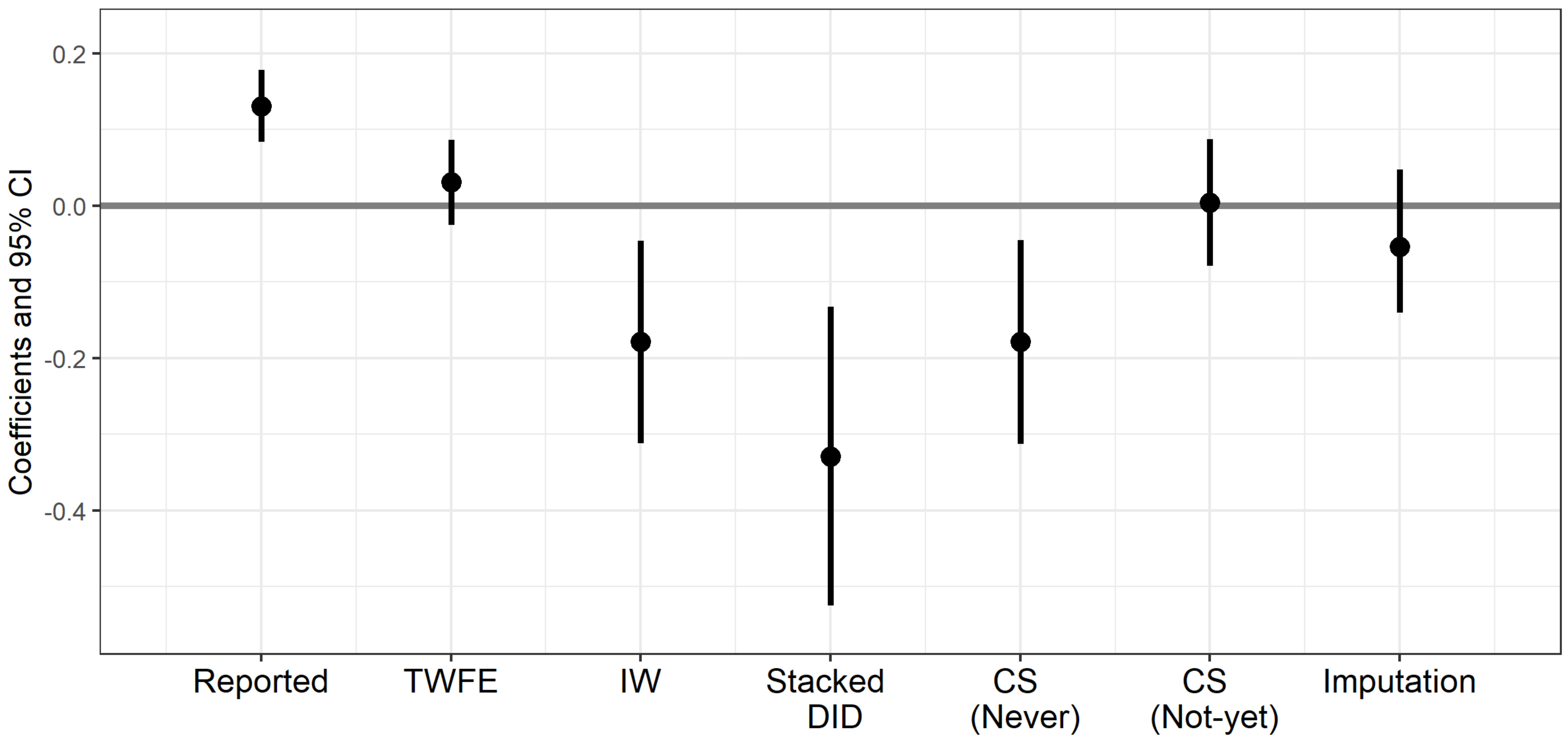


Replicated based on the Full Sample

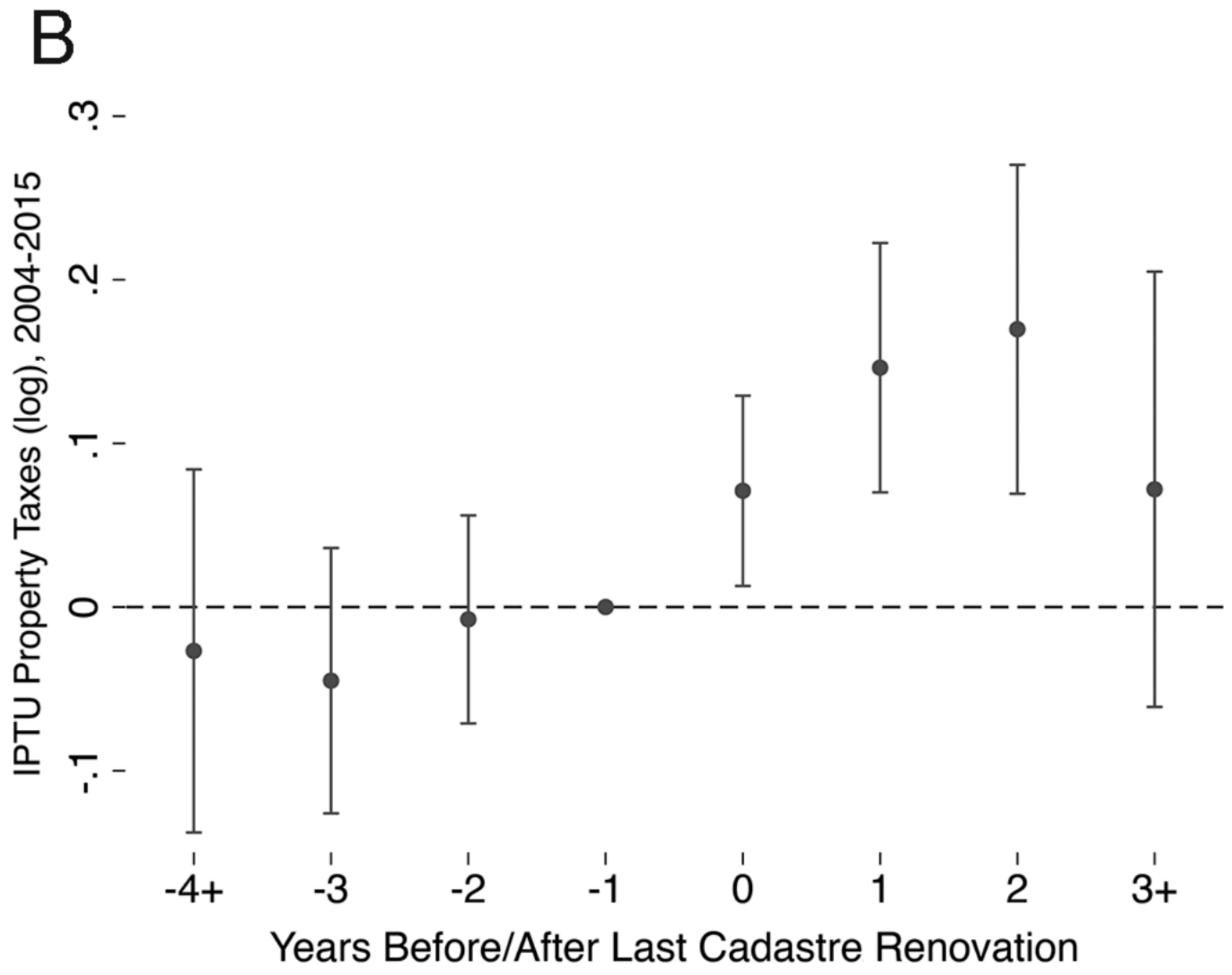


Figure 1B (based on a Subsample)

# Look Deeper...

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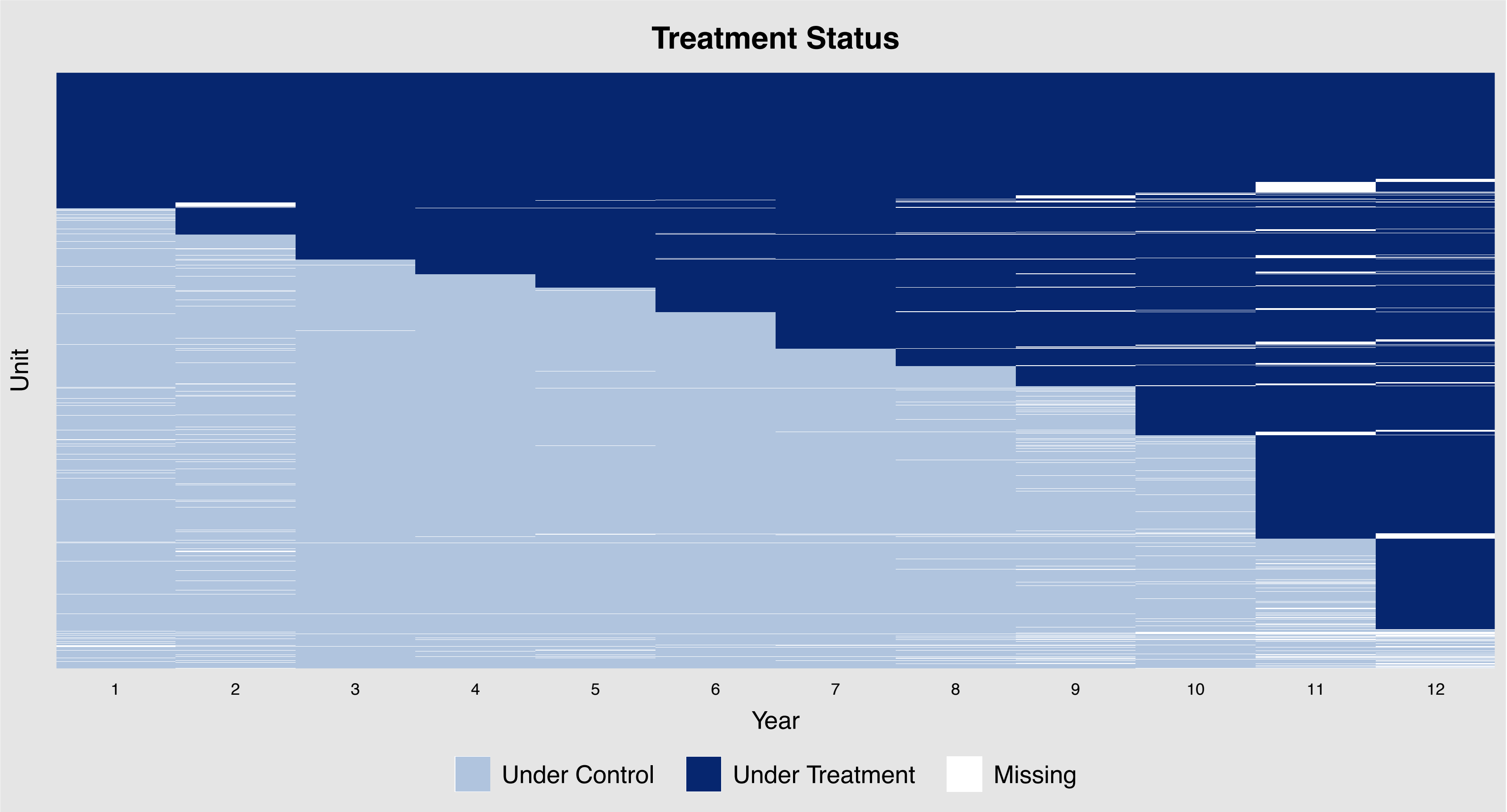
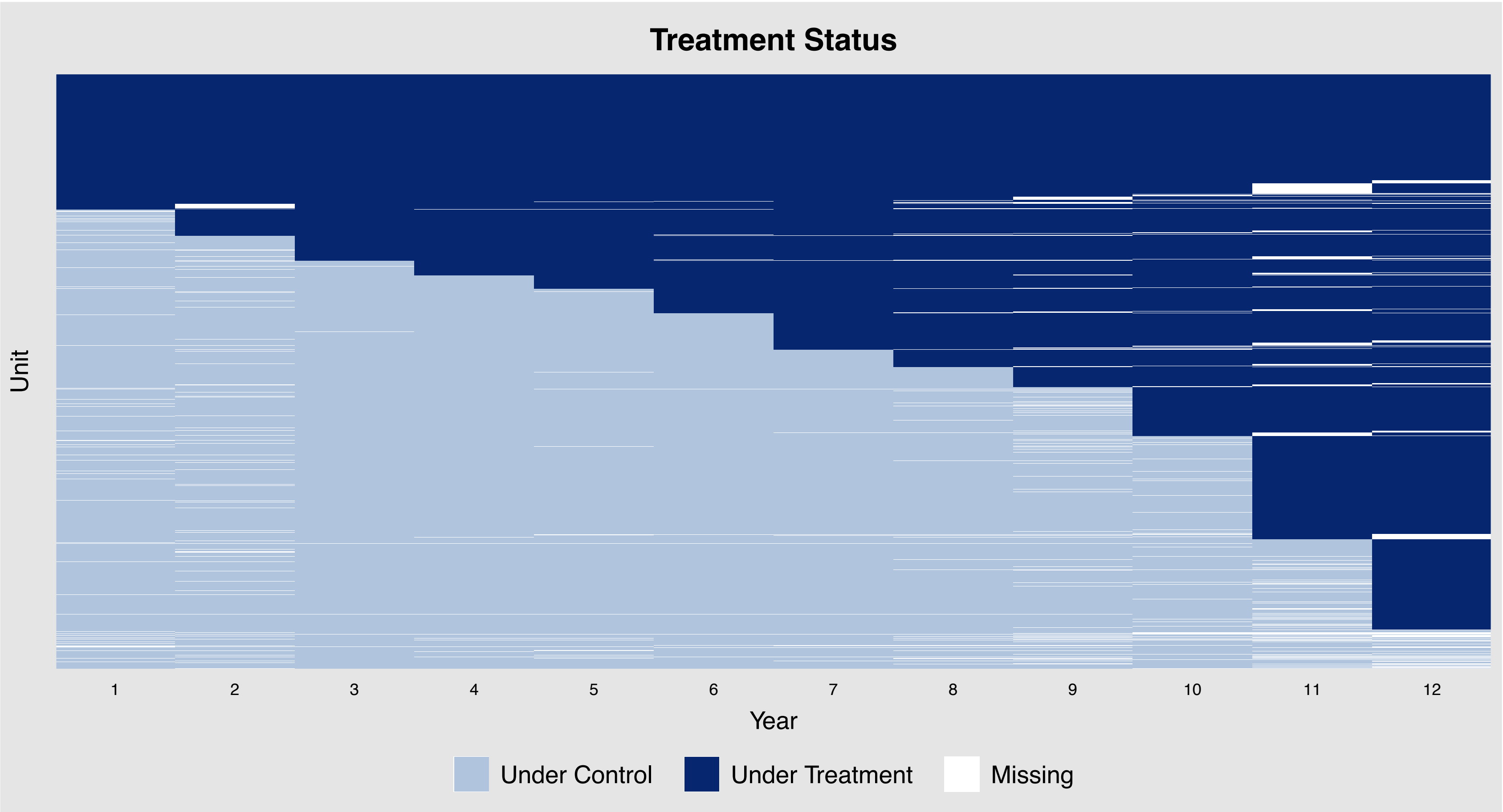


Treatment Status
Unit
1
2
3
4
5
6
7
8
9
10
11
12
Year
Under Control
Under Treatment
Missing

# Look Deeper...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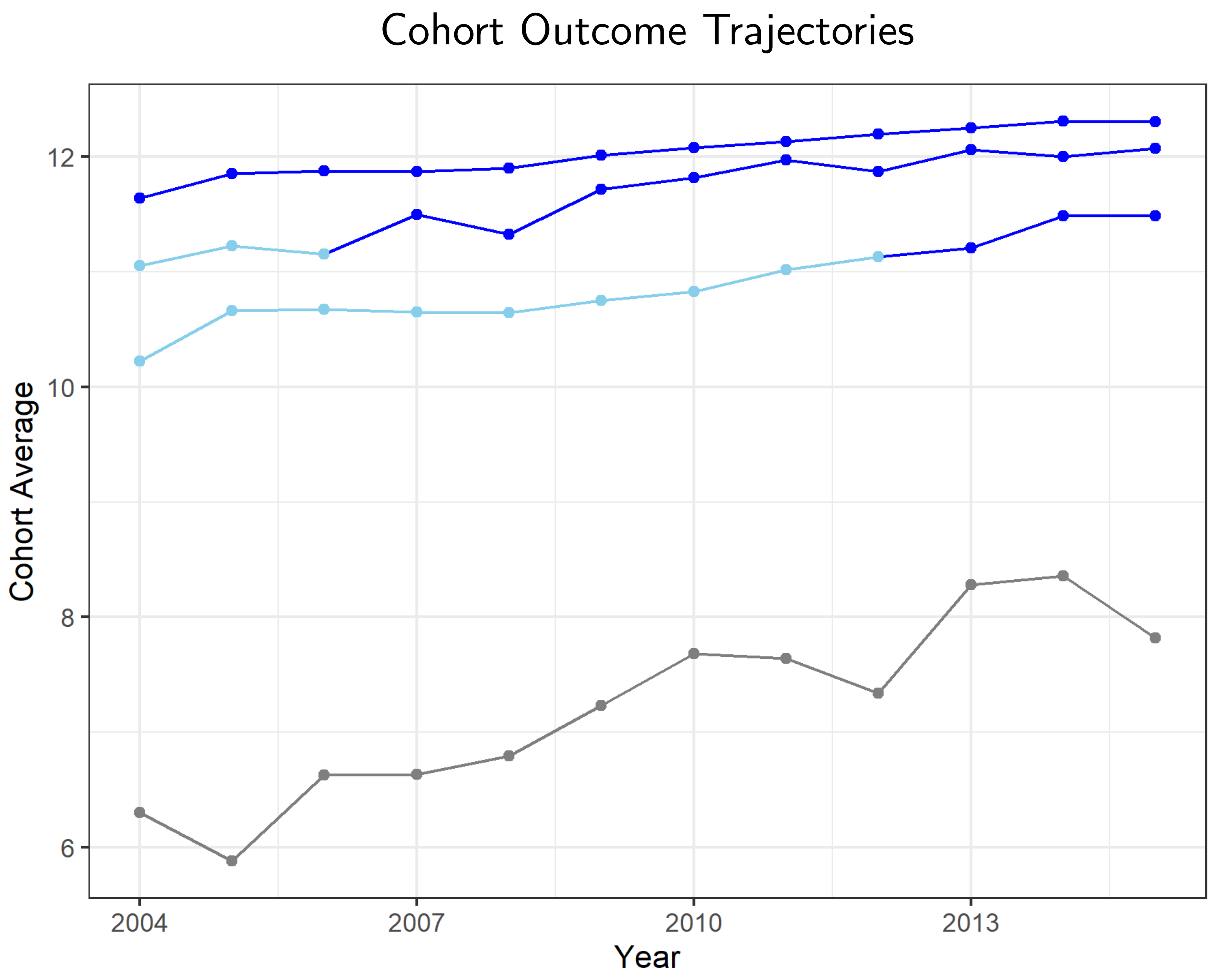
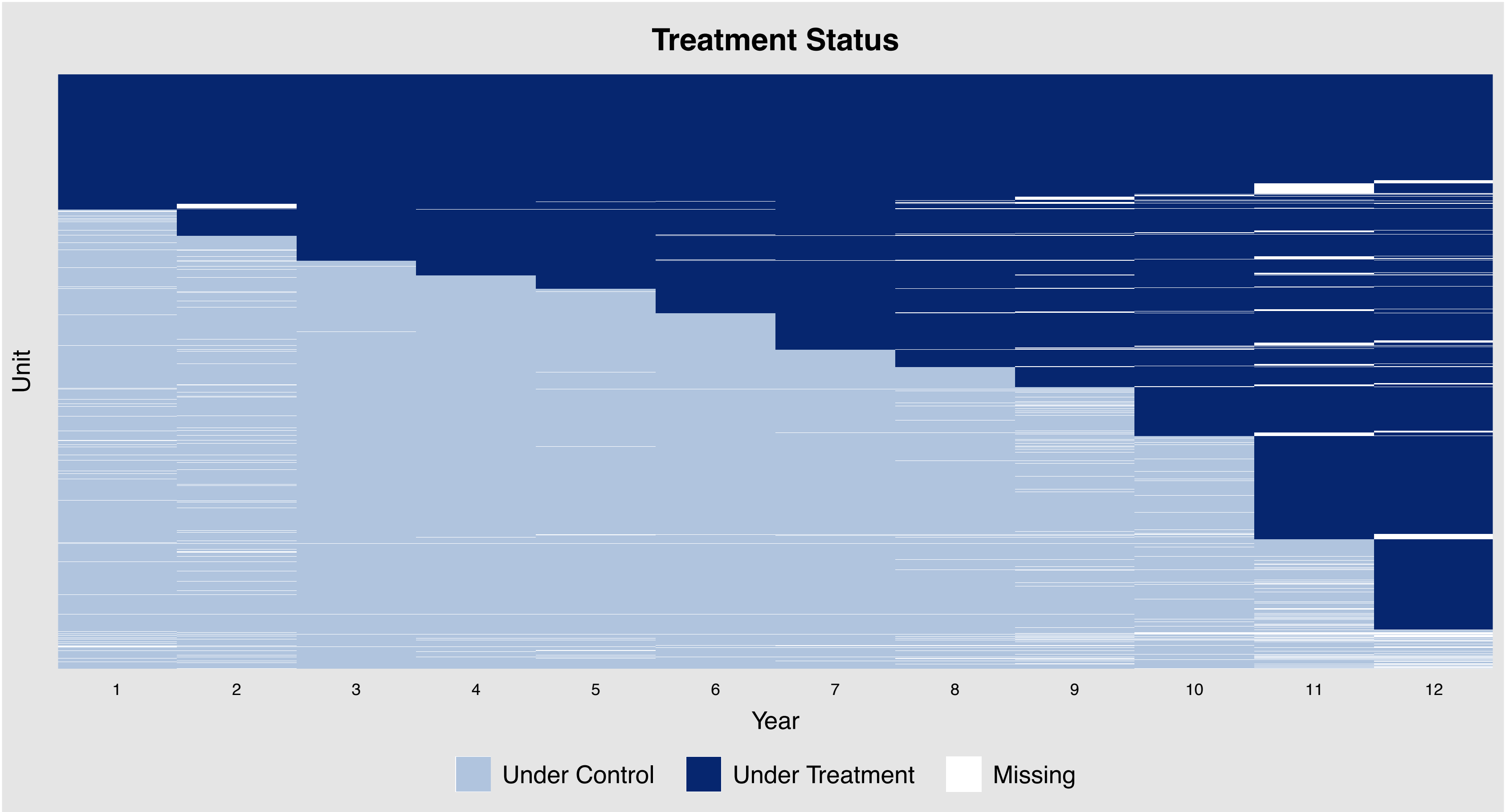

Cohort Outcome Trajectories
Cohort Average
12
10
8
6
2004
2007
2010
2013
Year

# Look Deeper...



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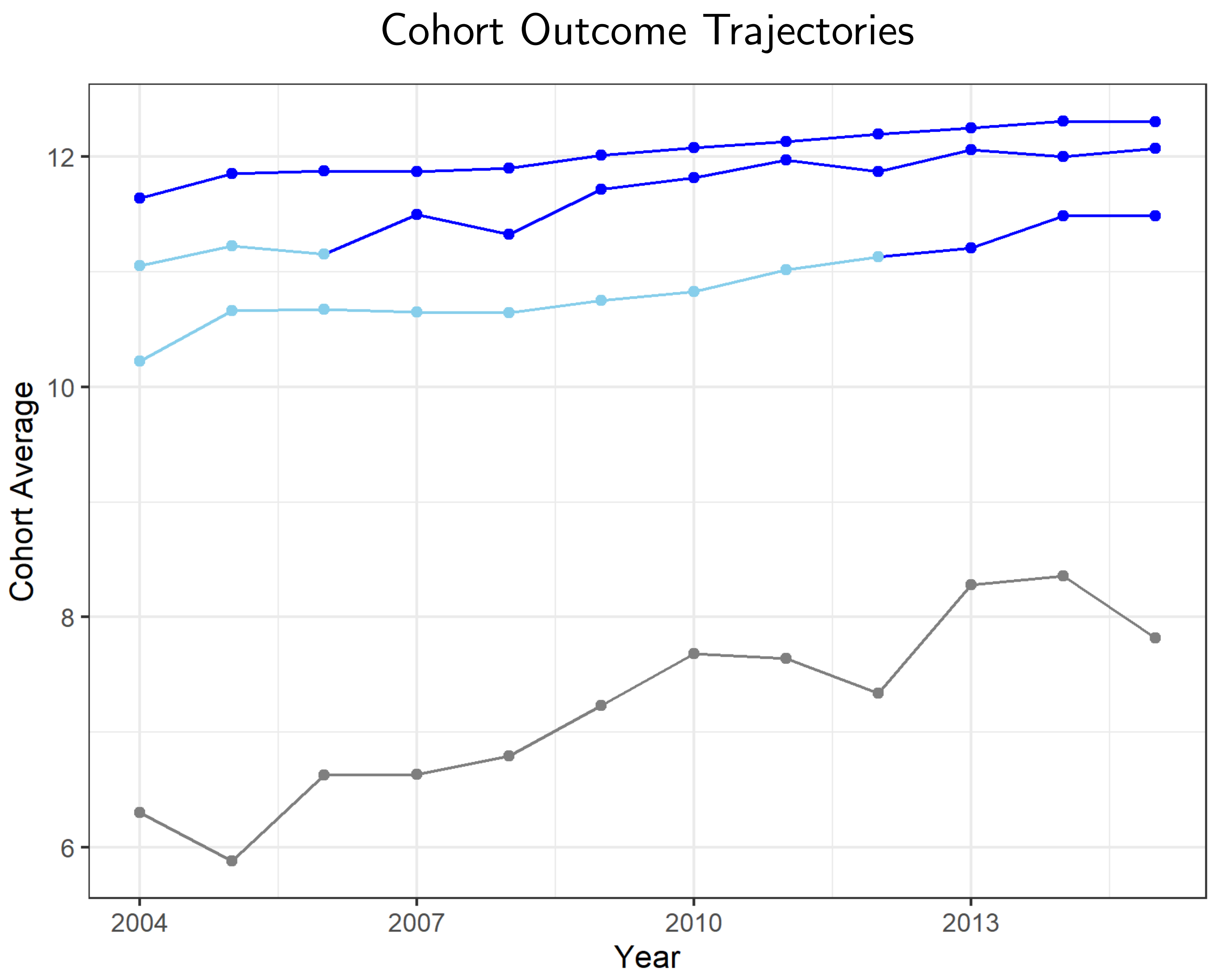

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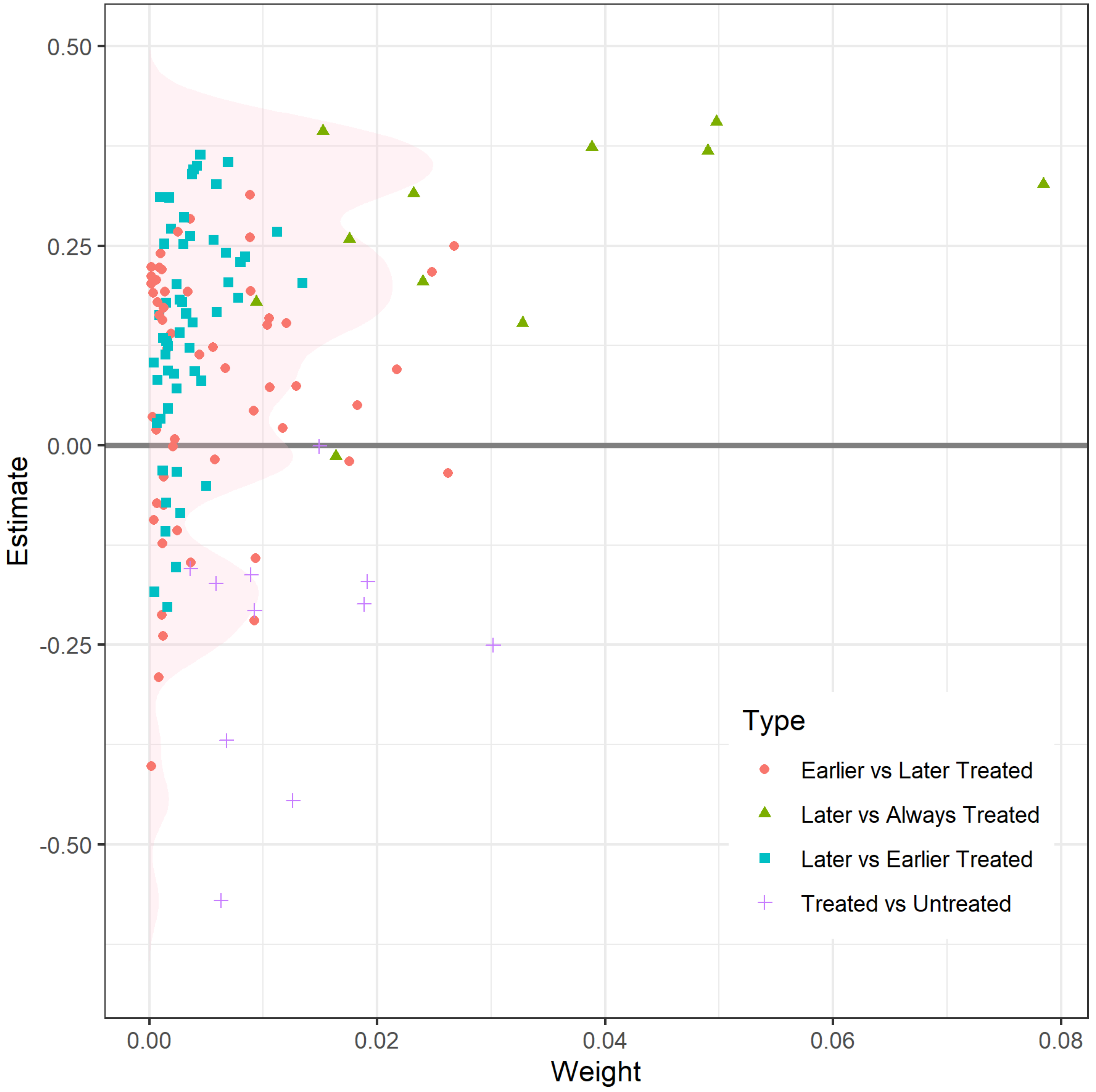
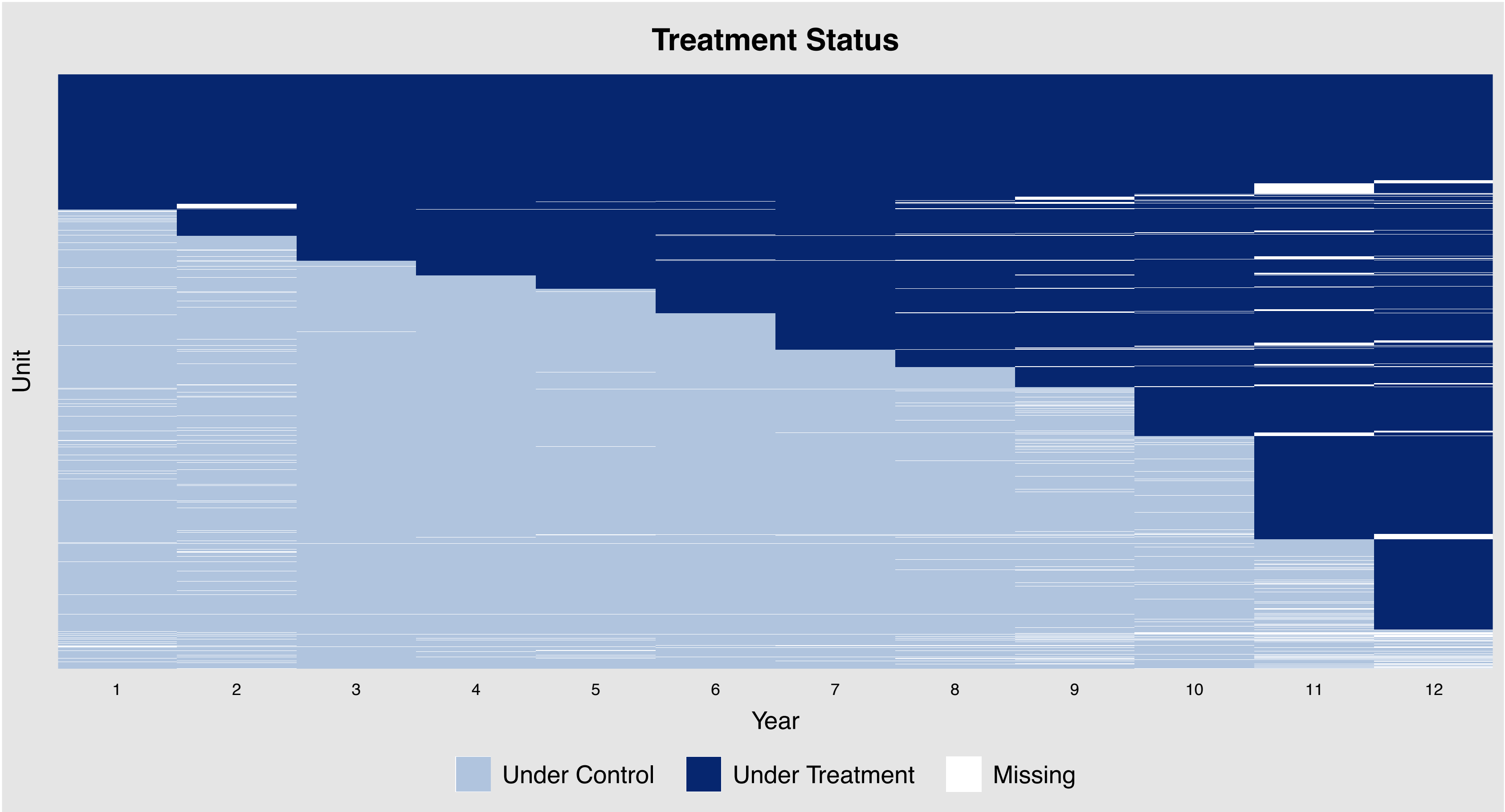


0.50
0.25
0.00
-0.25
-0.50
Estimate
0.00
0.02
0.04
0.06
0.08
Weight
Type
Earlier vs Later Treated
Later vs Always Treated
Later vs Earlier Treated
Treated vs Untreated

# Look Deeper...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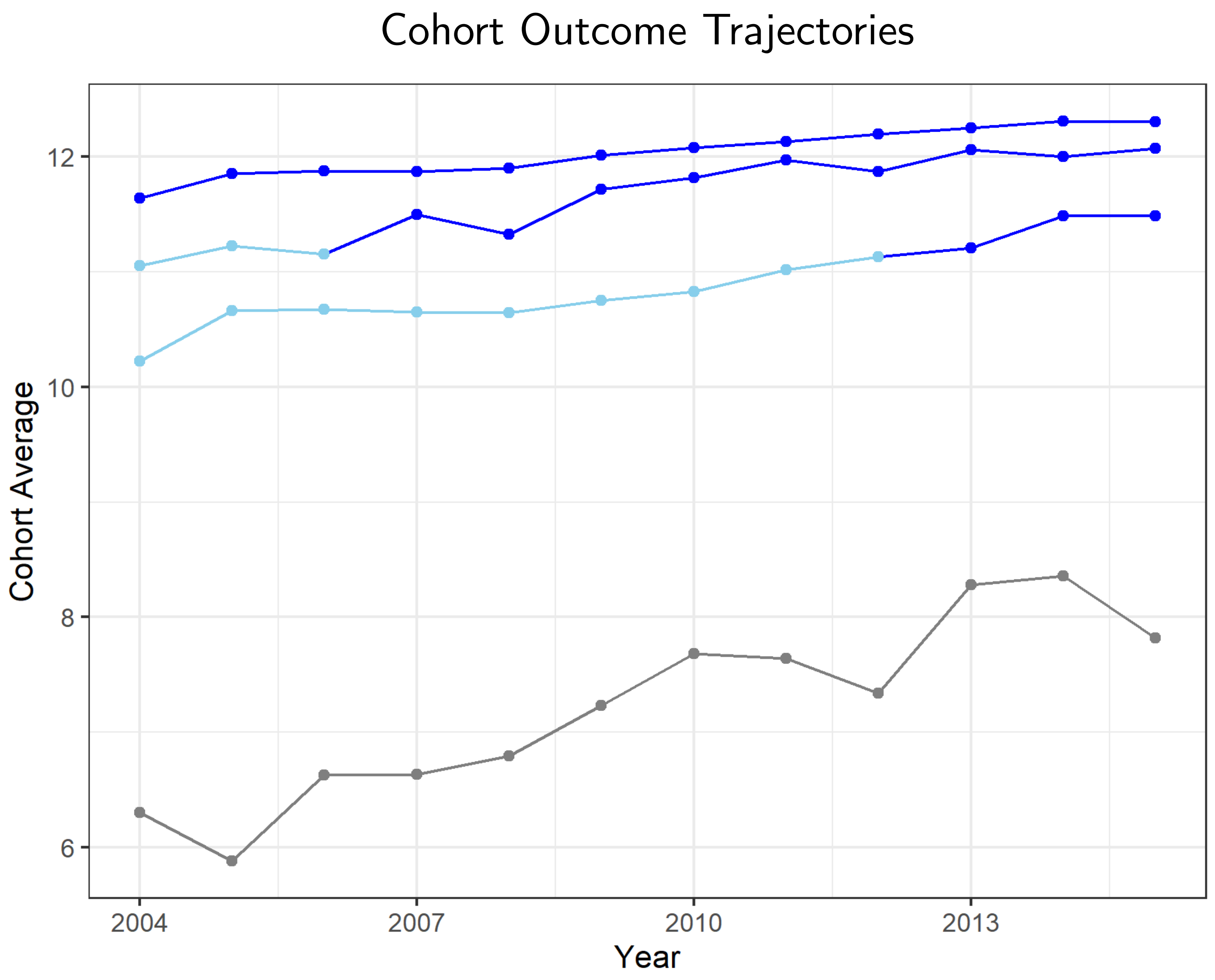
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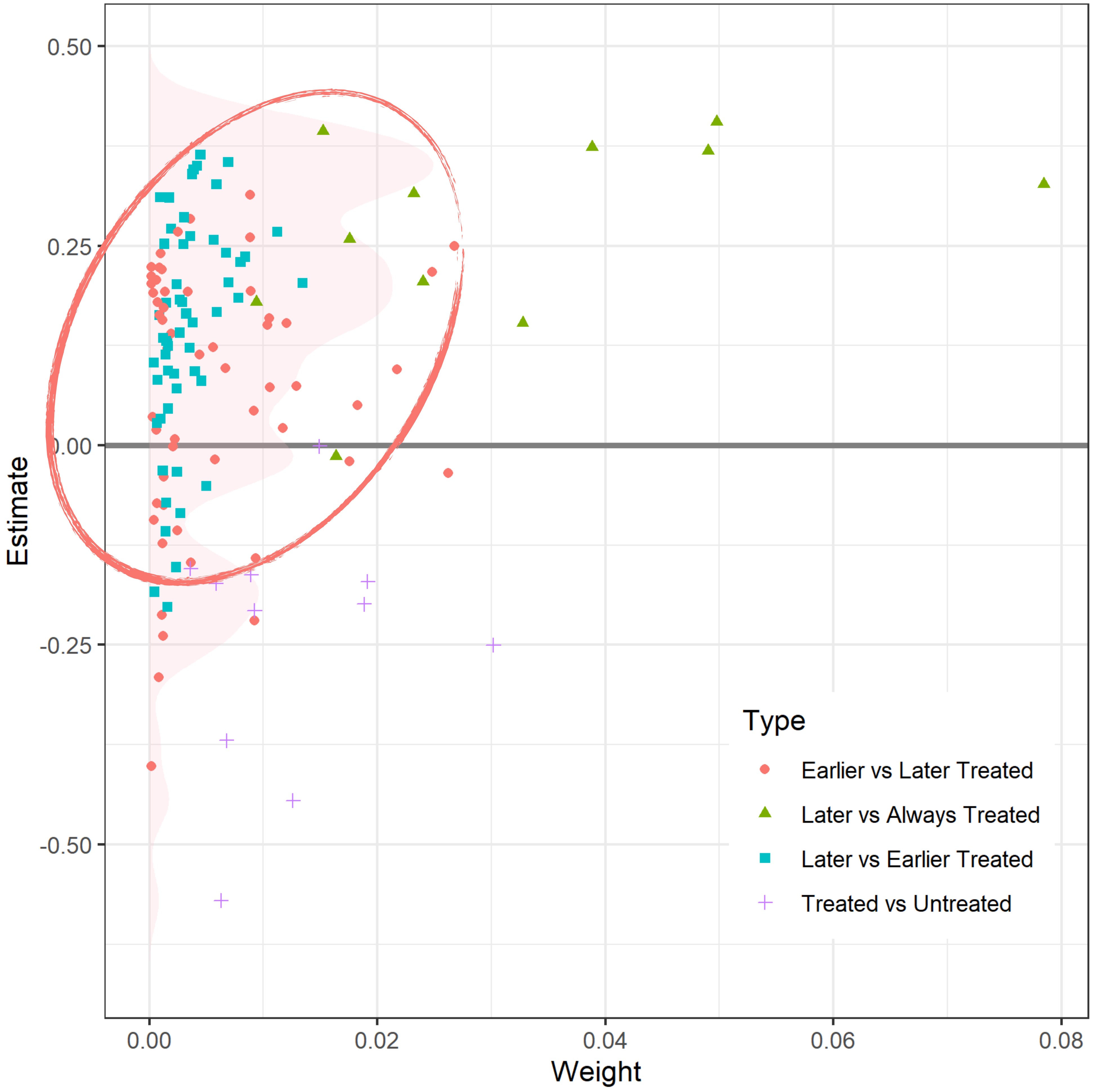
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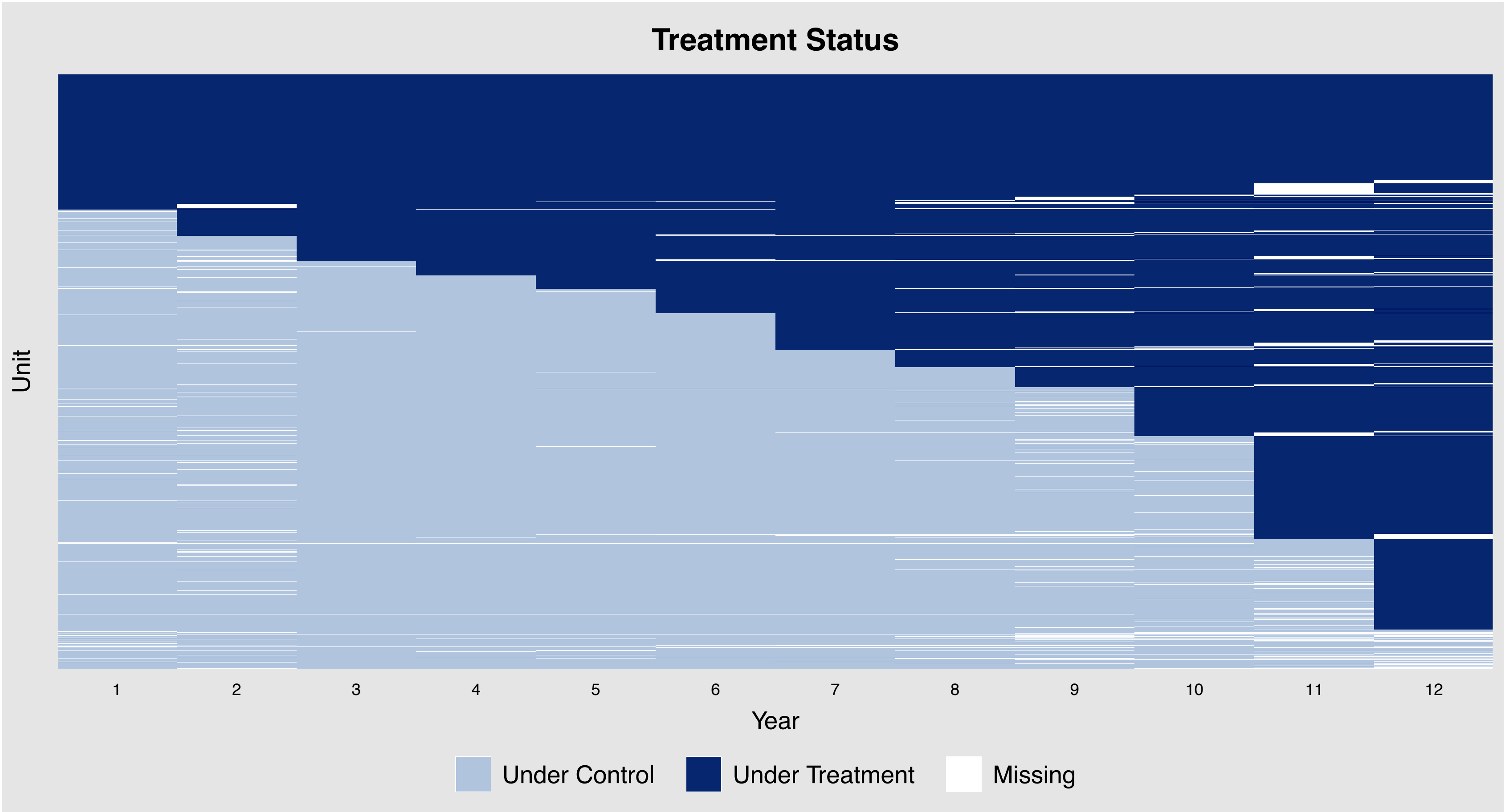

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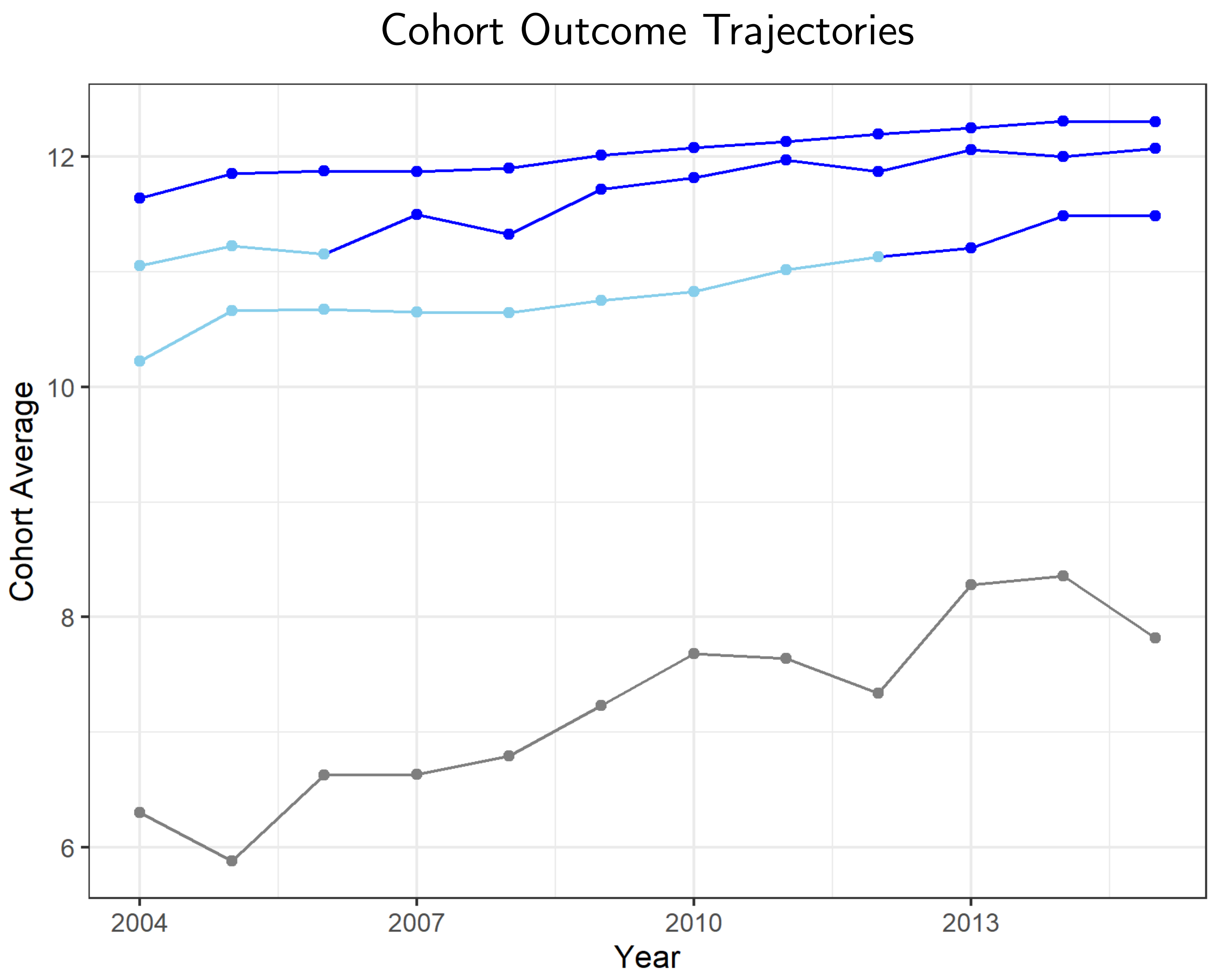

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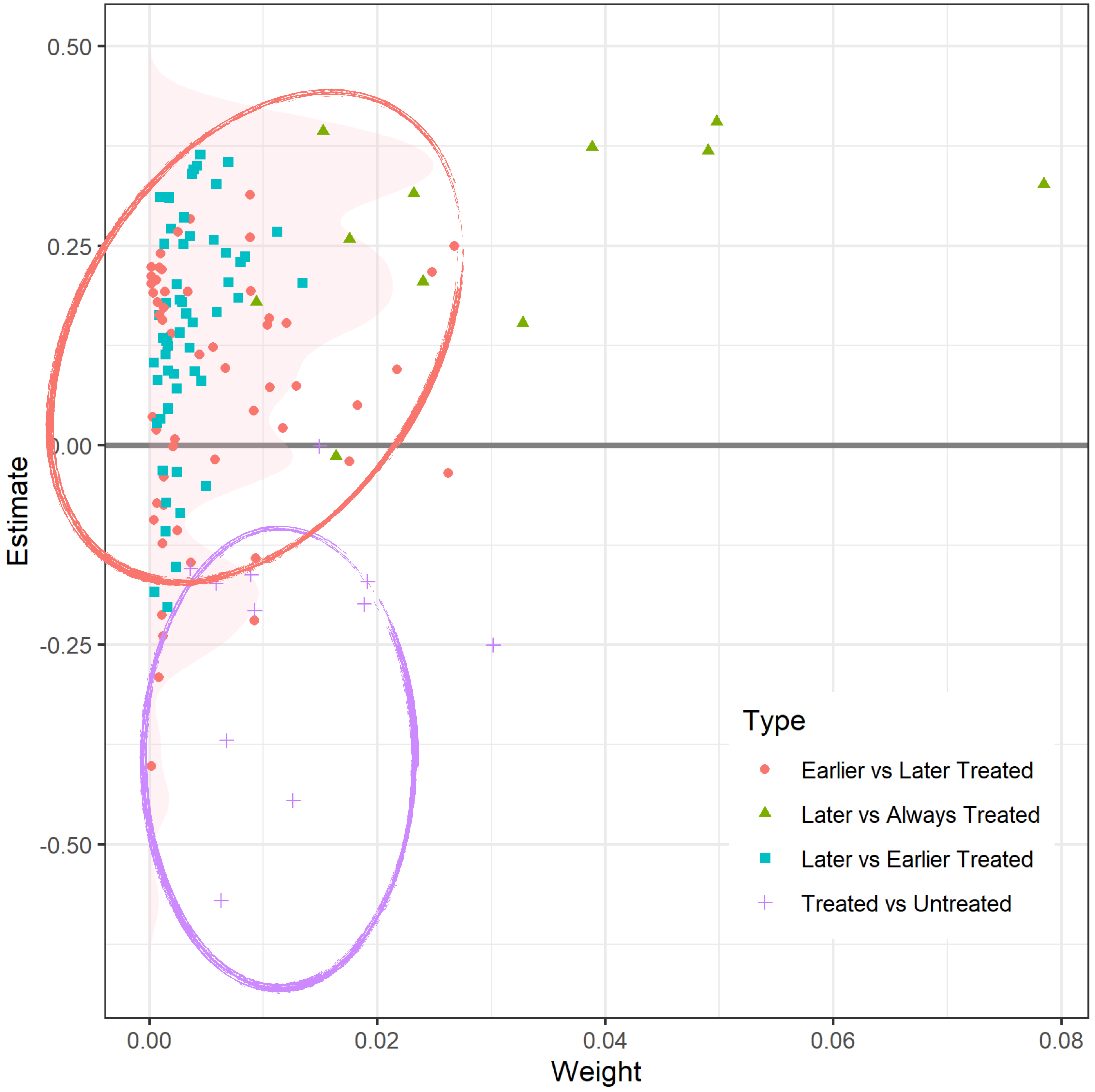

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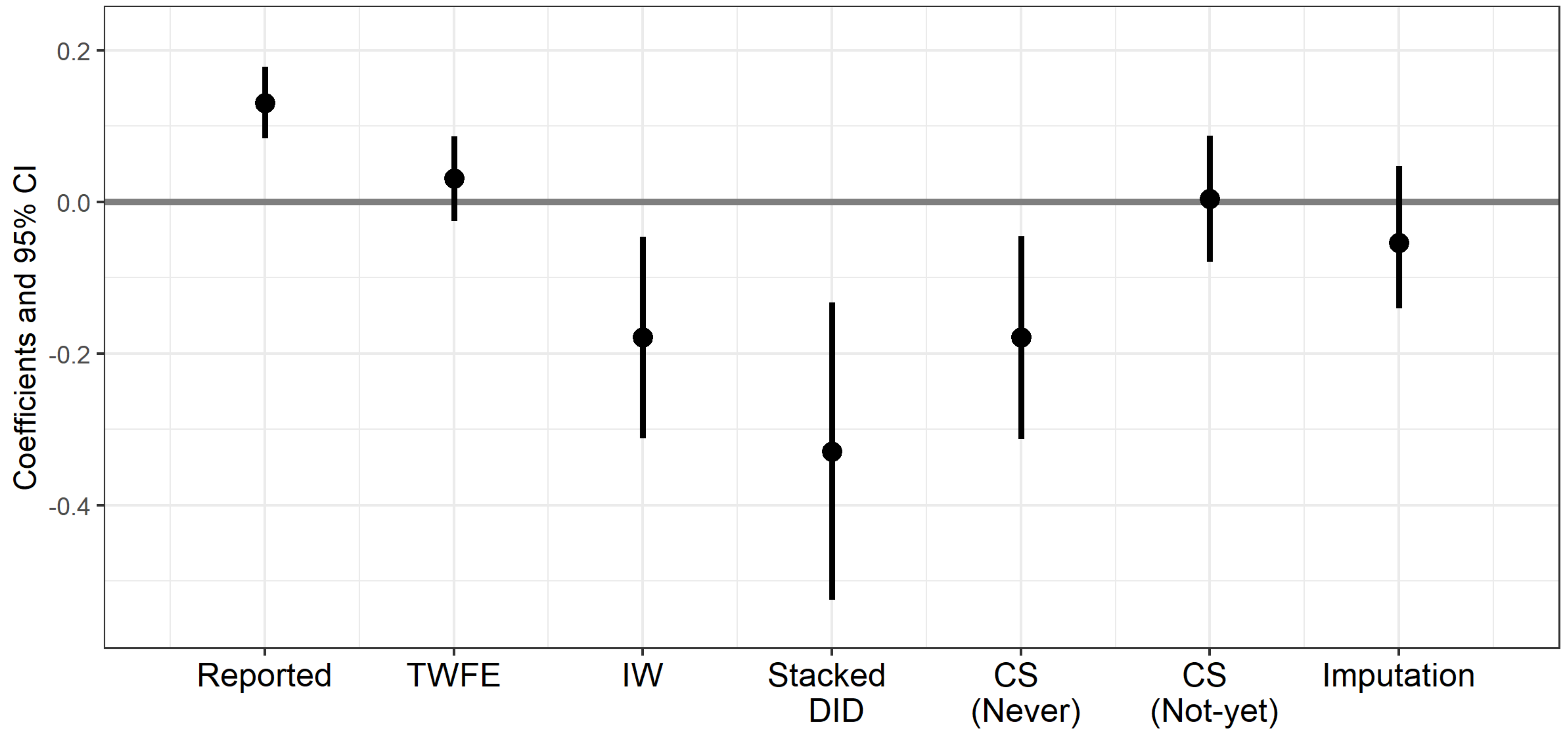
# Trimmed Subsample

Full Sample




Subsample

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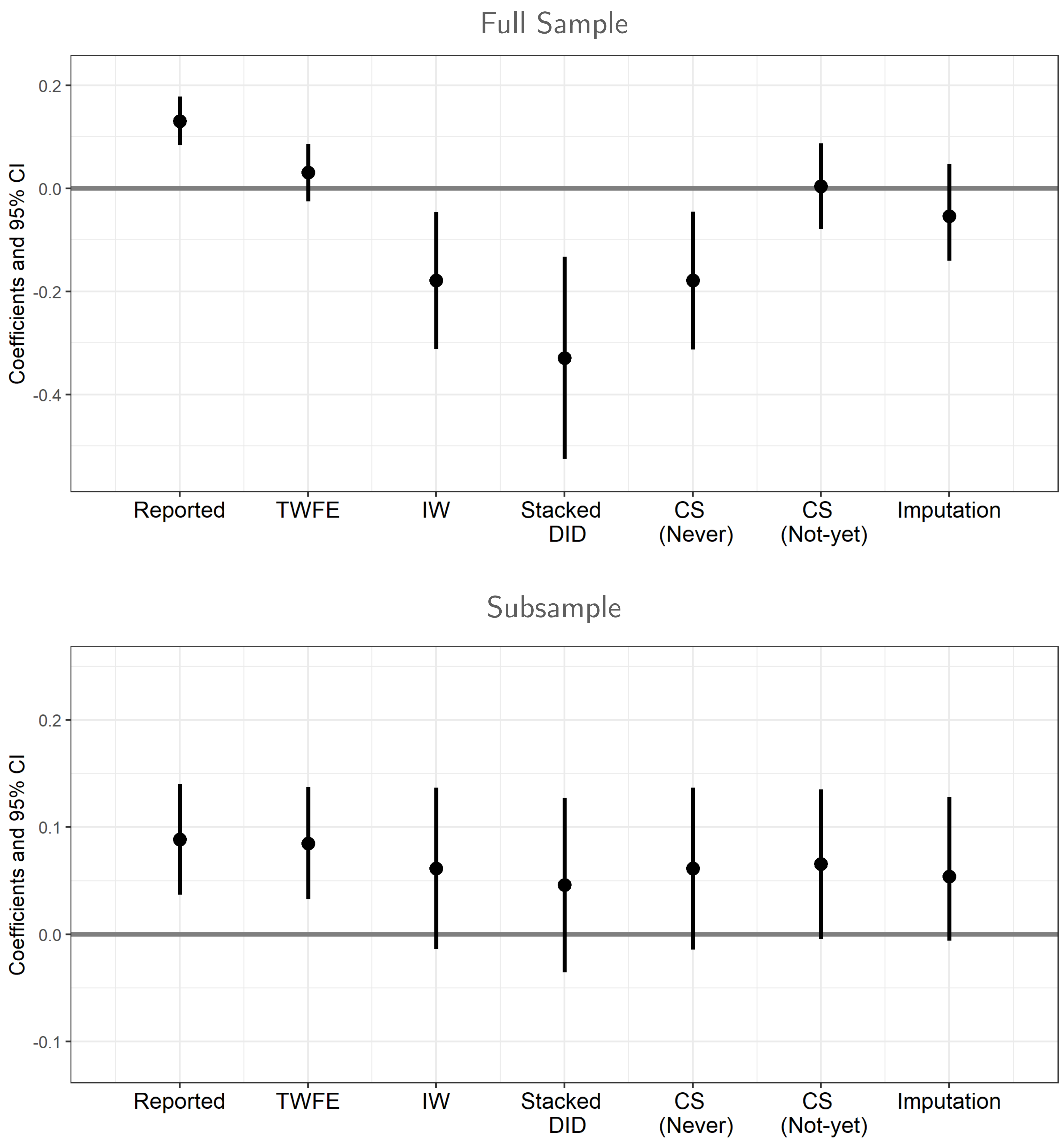

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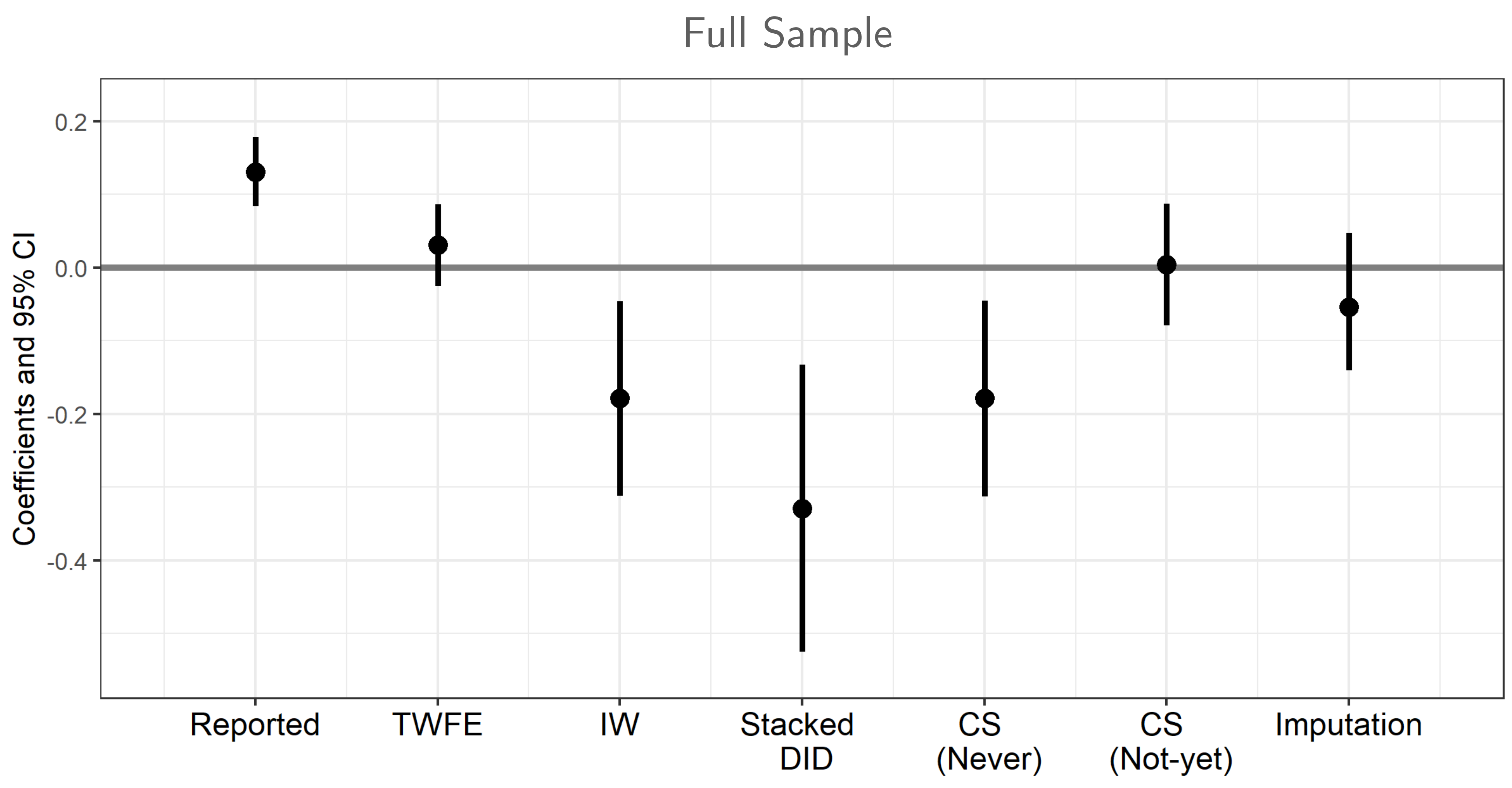


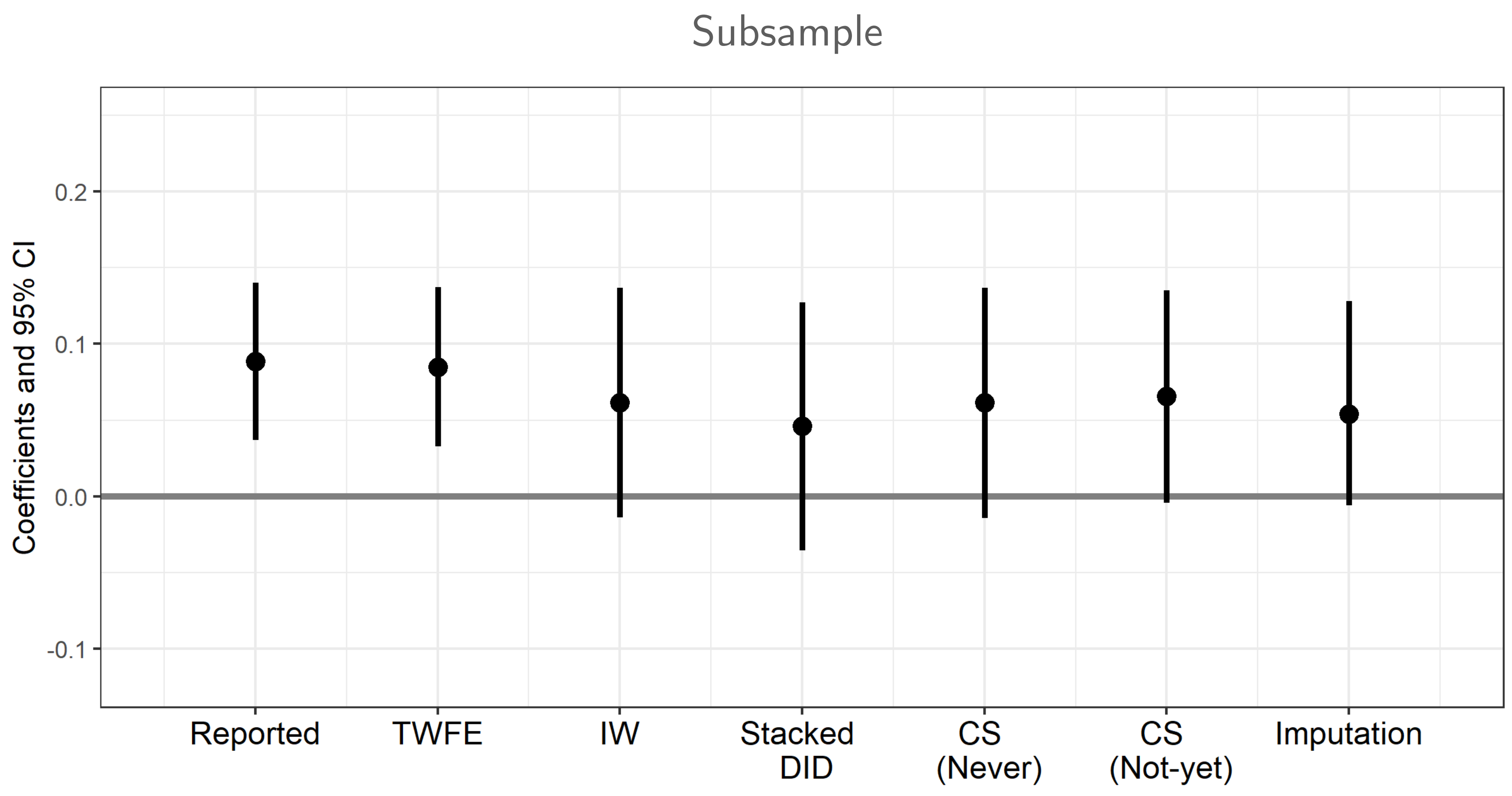


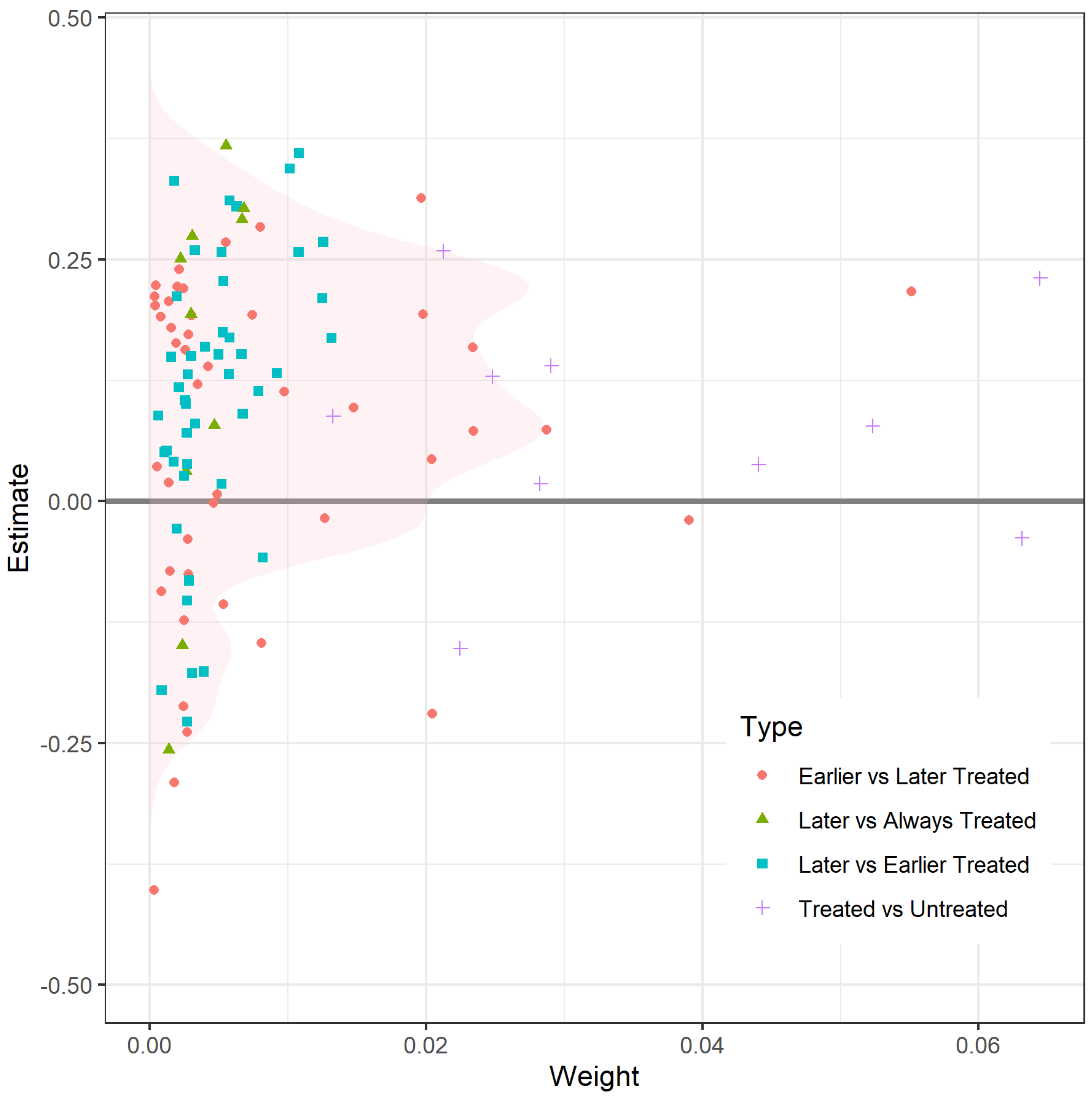

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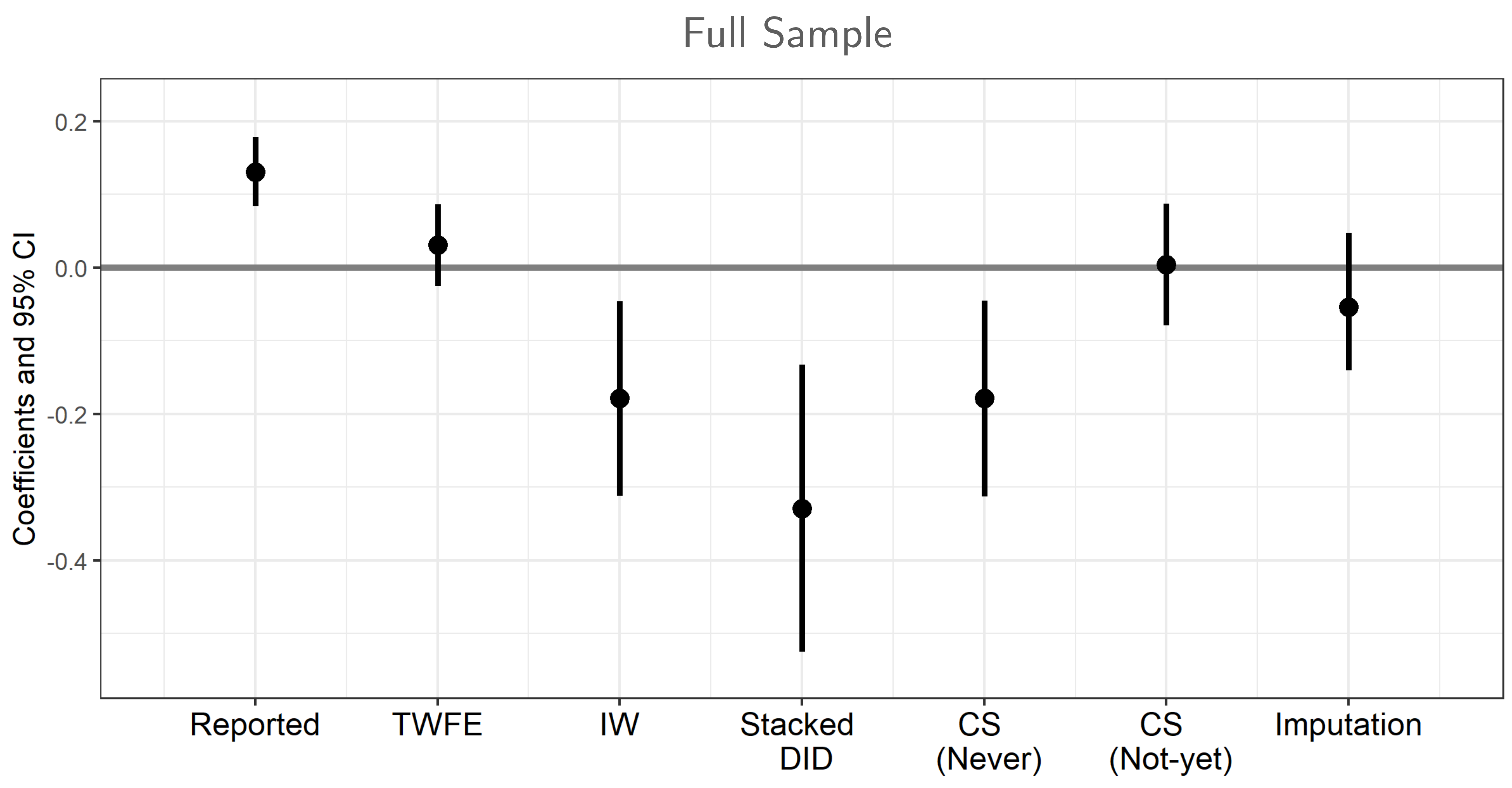


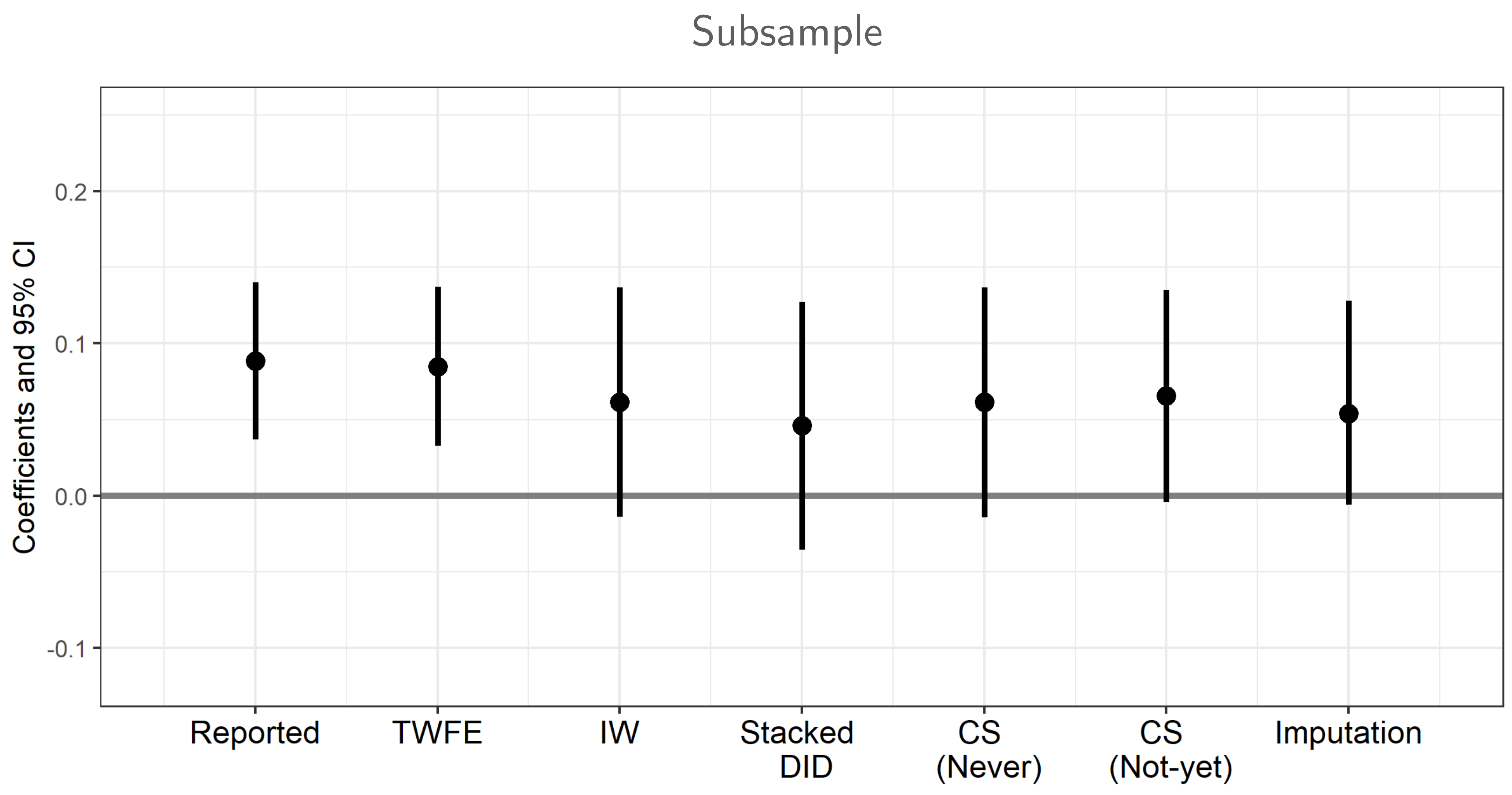


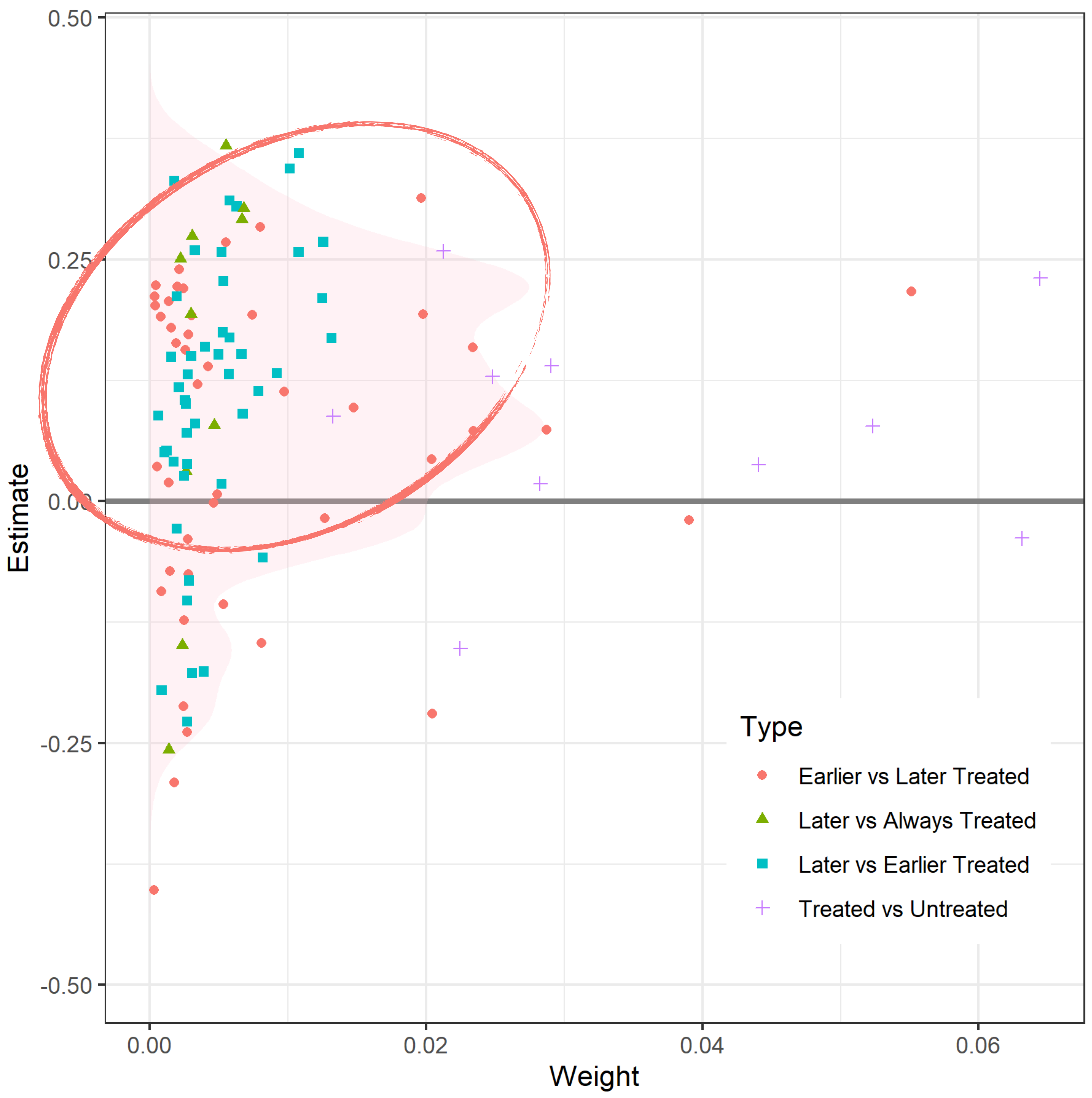

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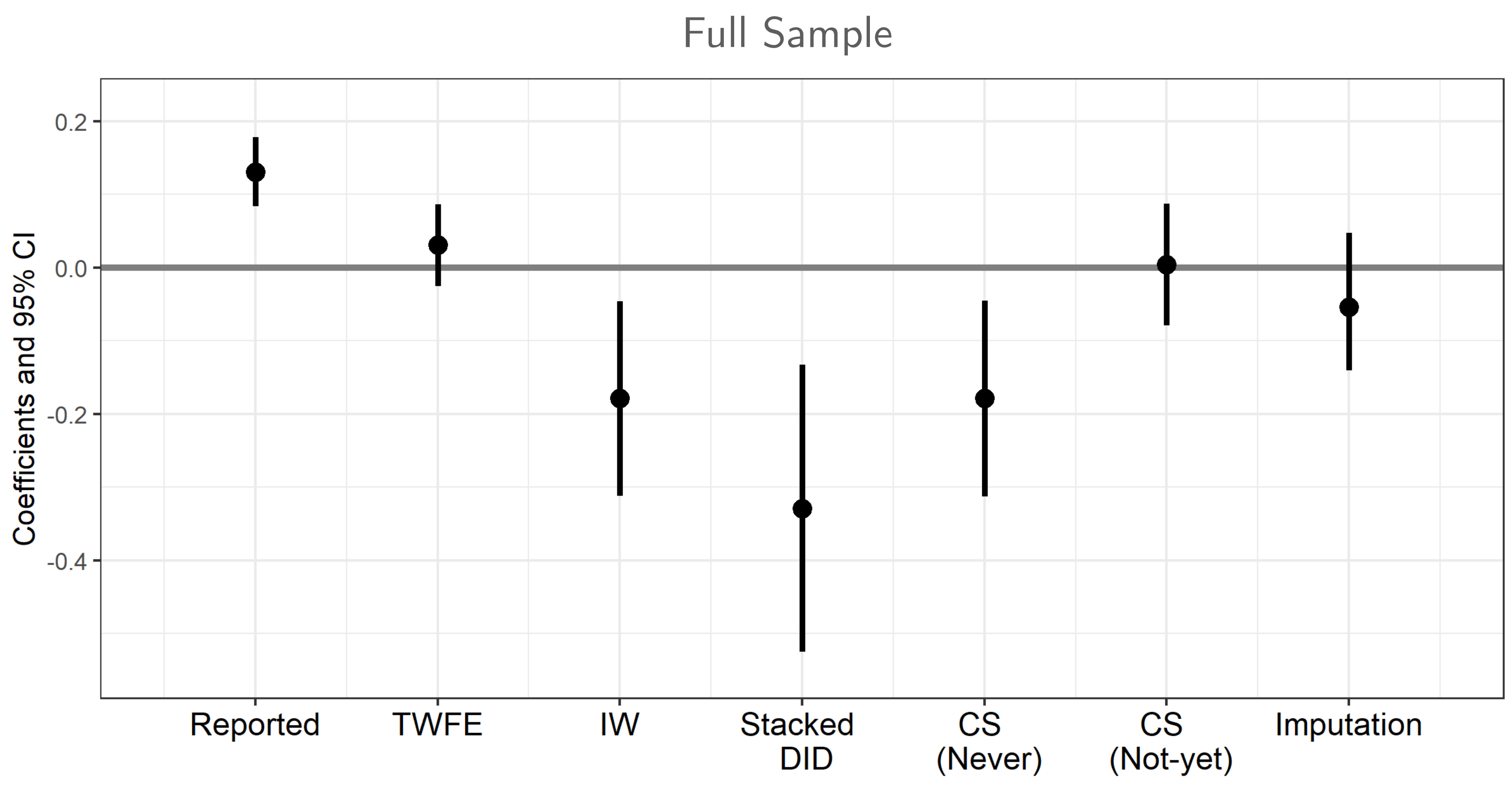
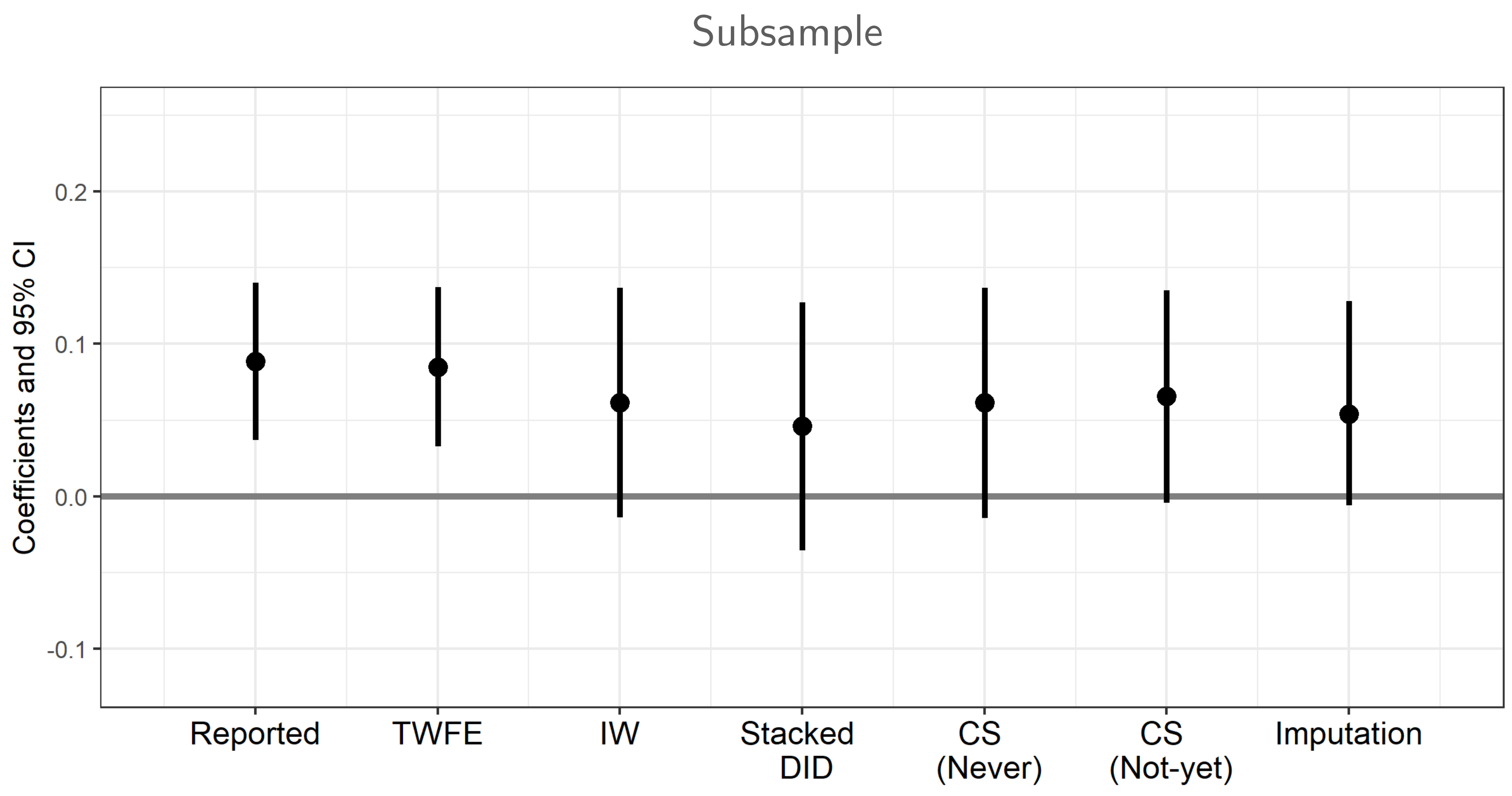
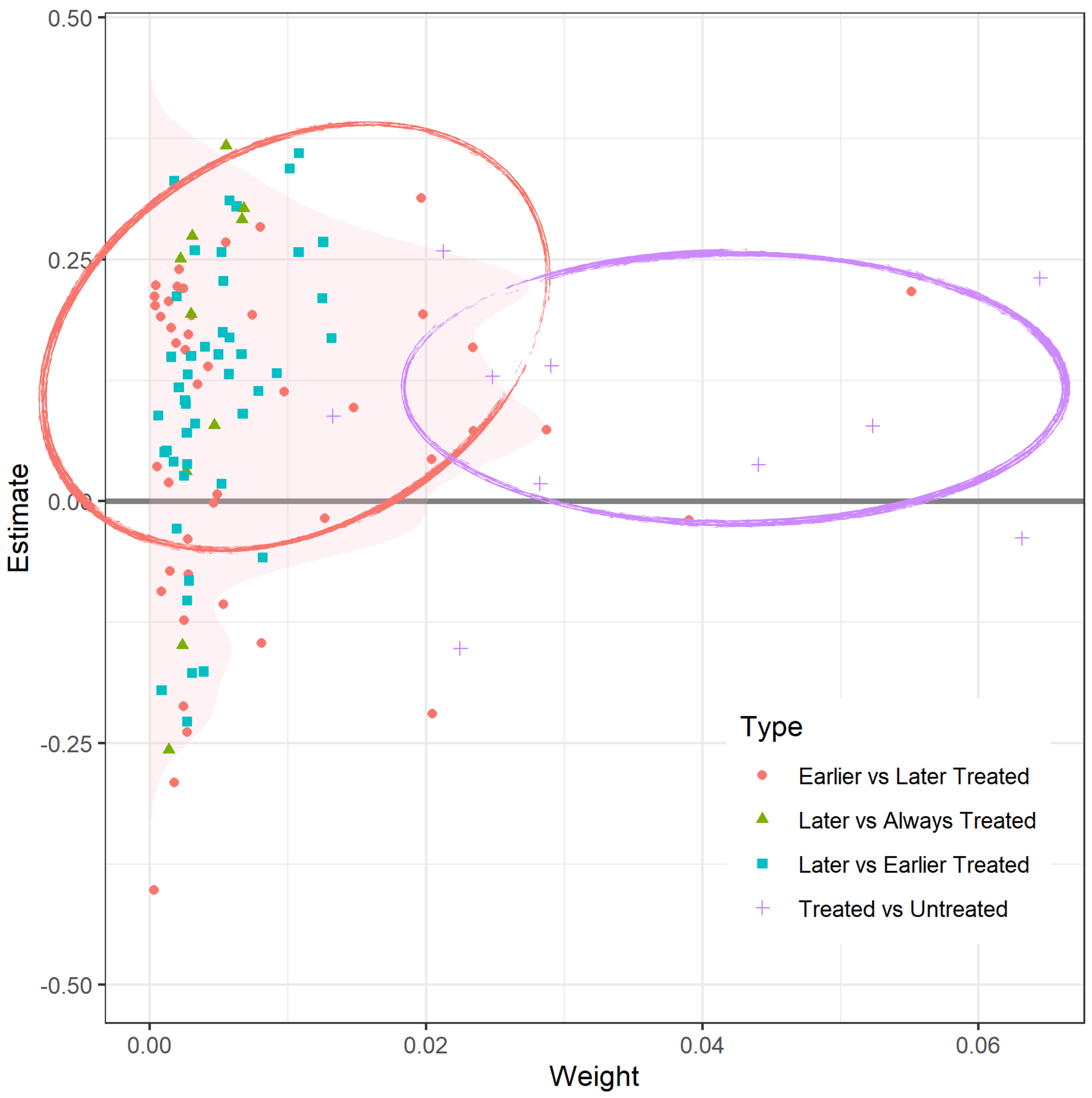
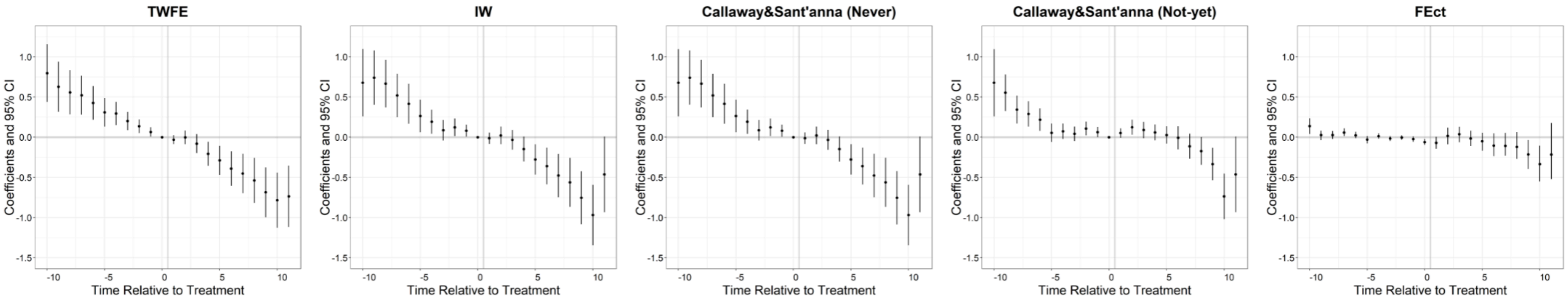


# Event Study Plots




Full Sample

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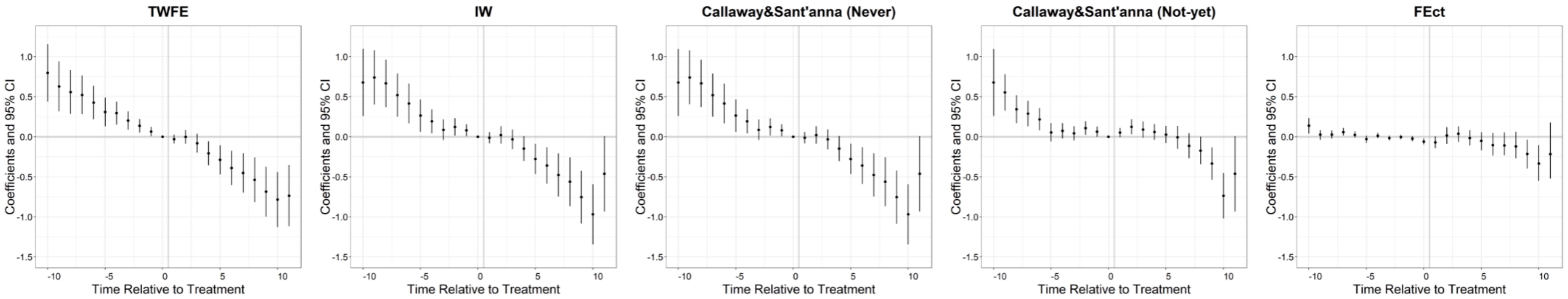
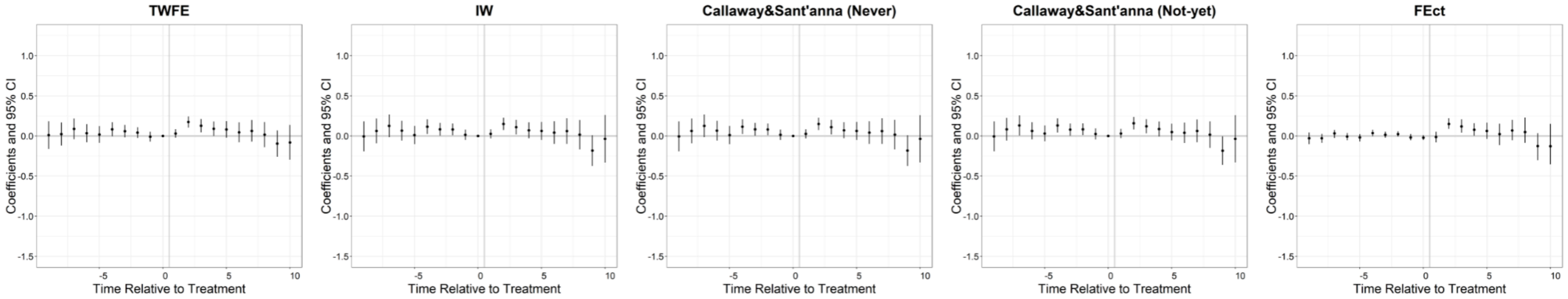



Full Sample




Trimmed Sample

# Three Examples

- **Example 1**: Coethnic Mobilization
  - Strong design; HTE matters marginally — estimators (including TWFE) broadly agree
- **Example 2**: Lawsuit against land use restriction
  - Clear signs of PT violations
  - HTE is a second-order issue; agreement does not mean robustness
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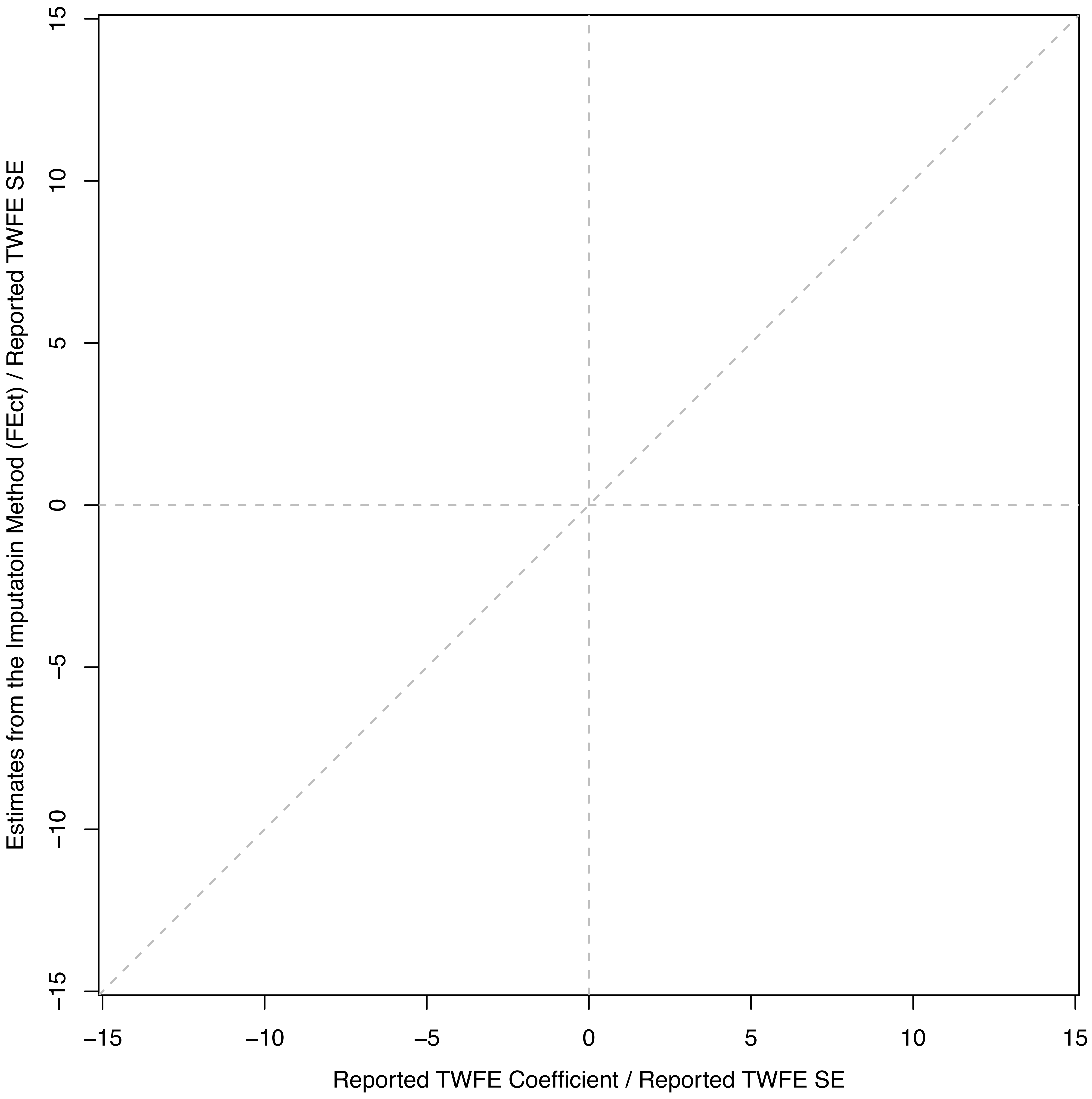
  - When estimators disagree, it may be a sign of PT violations
  - Design phrase, e.g. trimming, help improve inference (Imbens & Rubin 2015)

# Overal Assessment

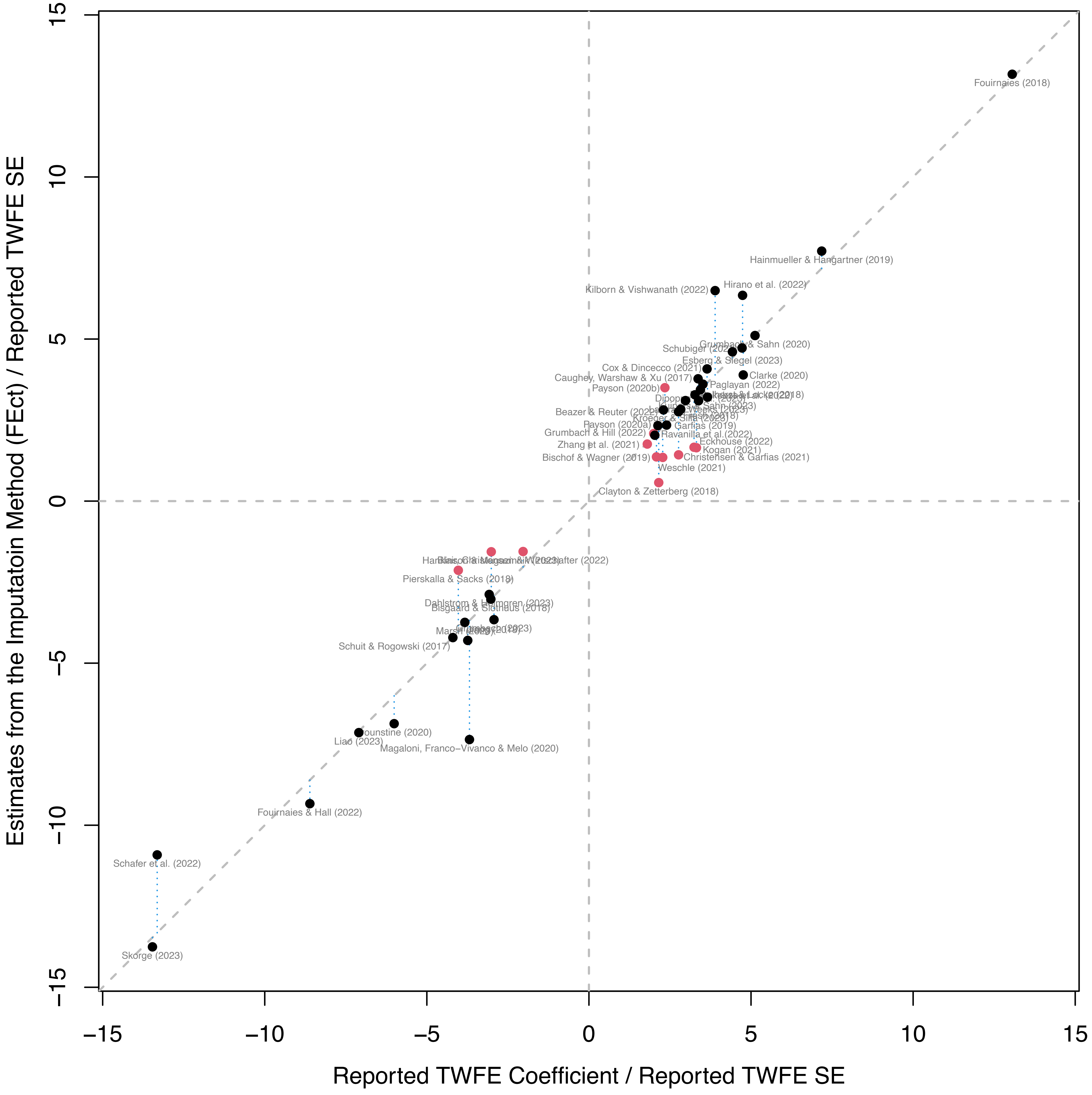
How much does HTE matter?

Why does “robust DID” require so much power?

# Do HTE-Robust Estimators Overturn Original Findings?

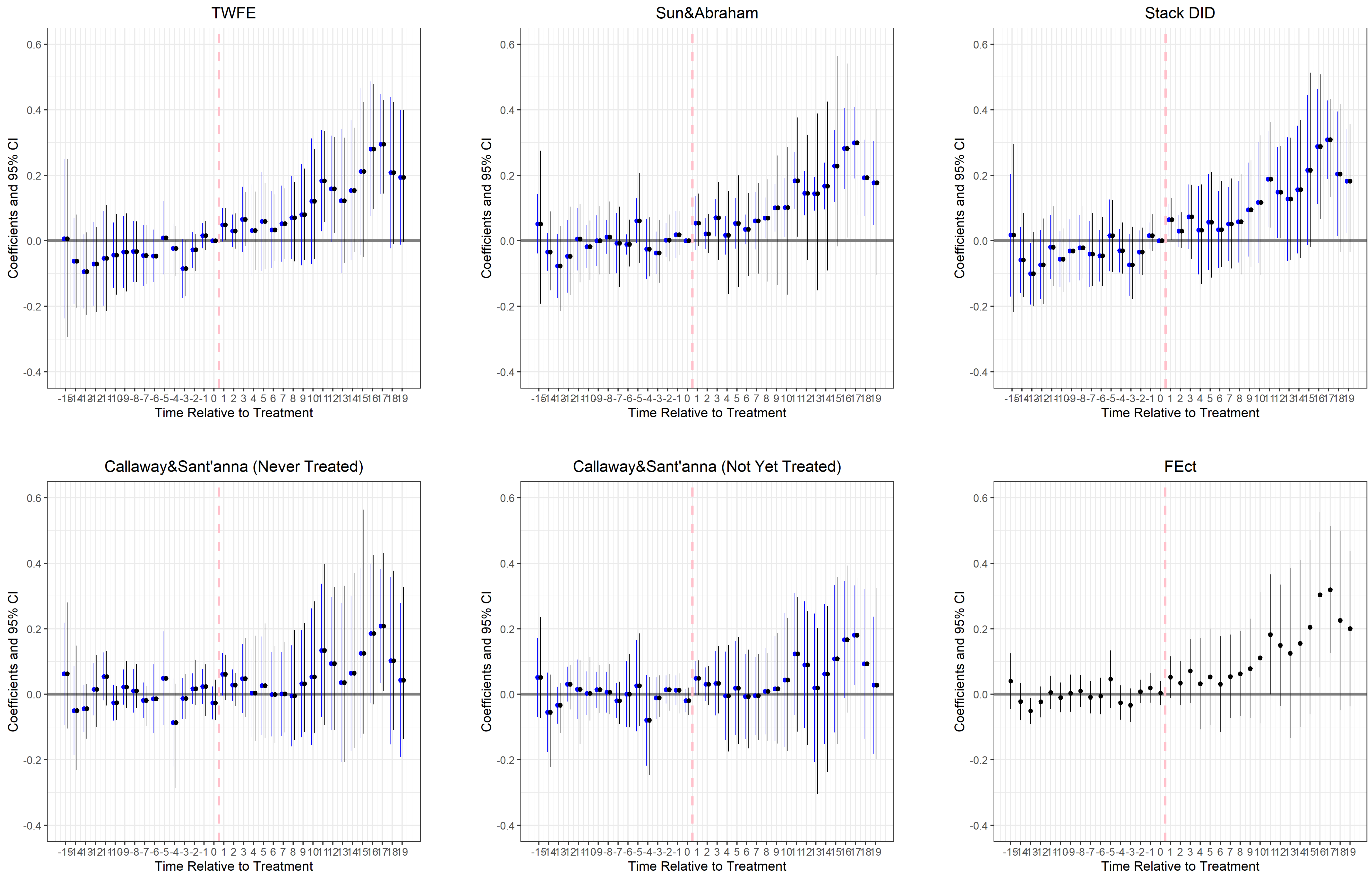


# Estimates from TWFE and Imputation Method Broadly Aligned



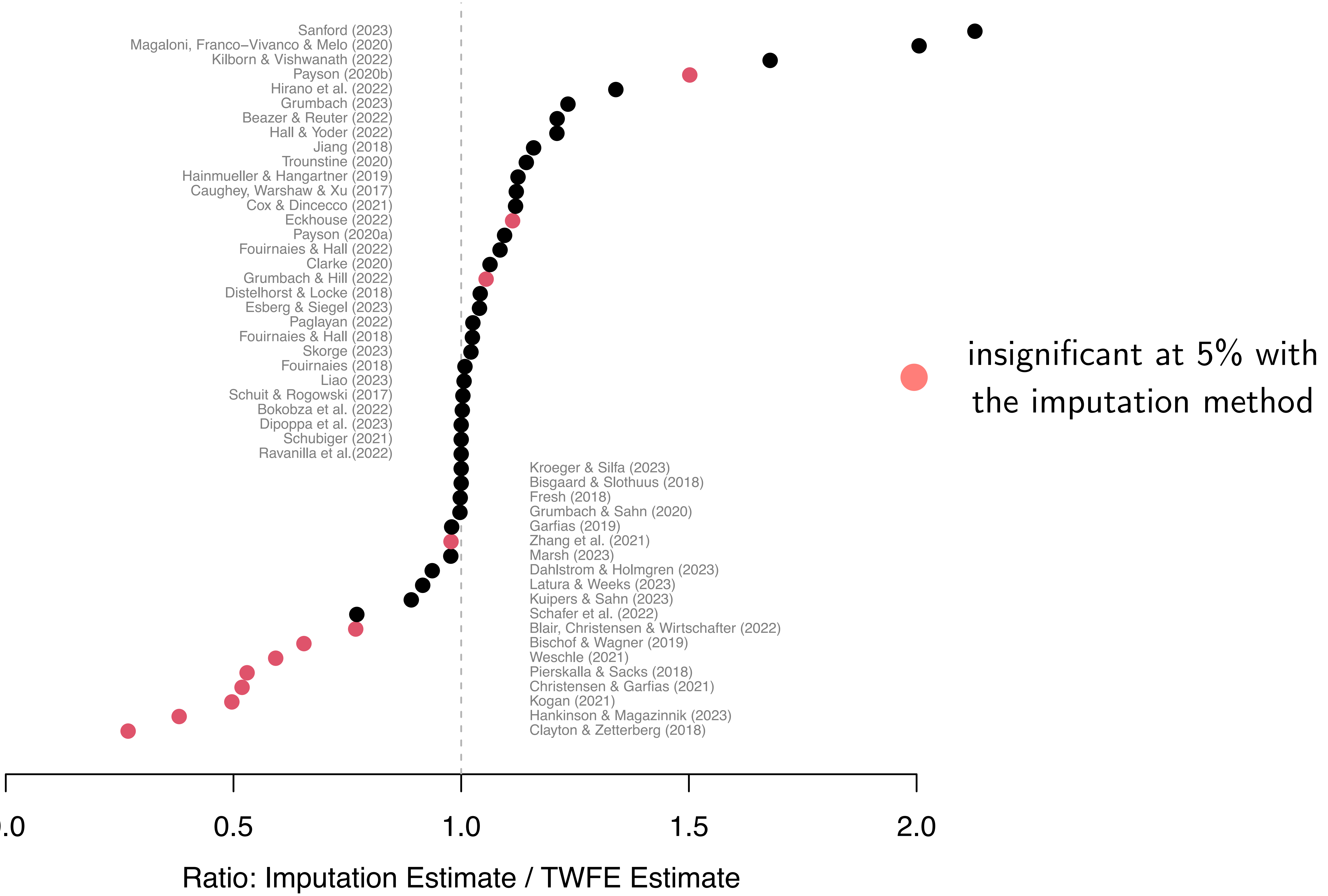

insignificant at 5% with the imputation method

# When PT Seems Plausible, Estimators Tend to Agree



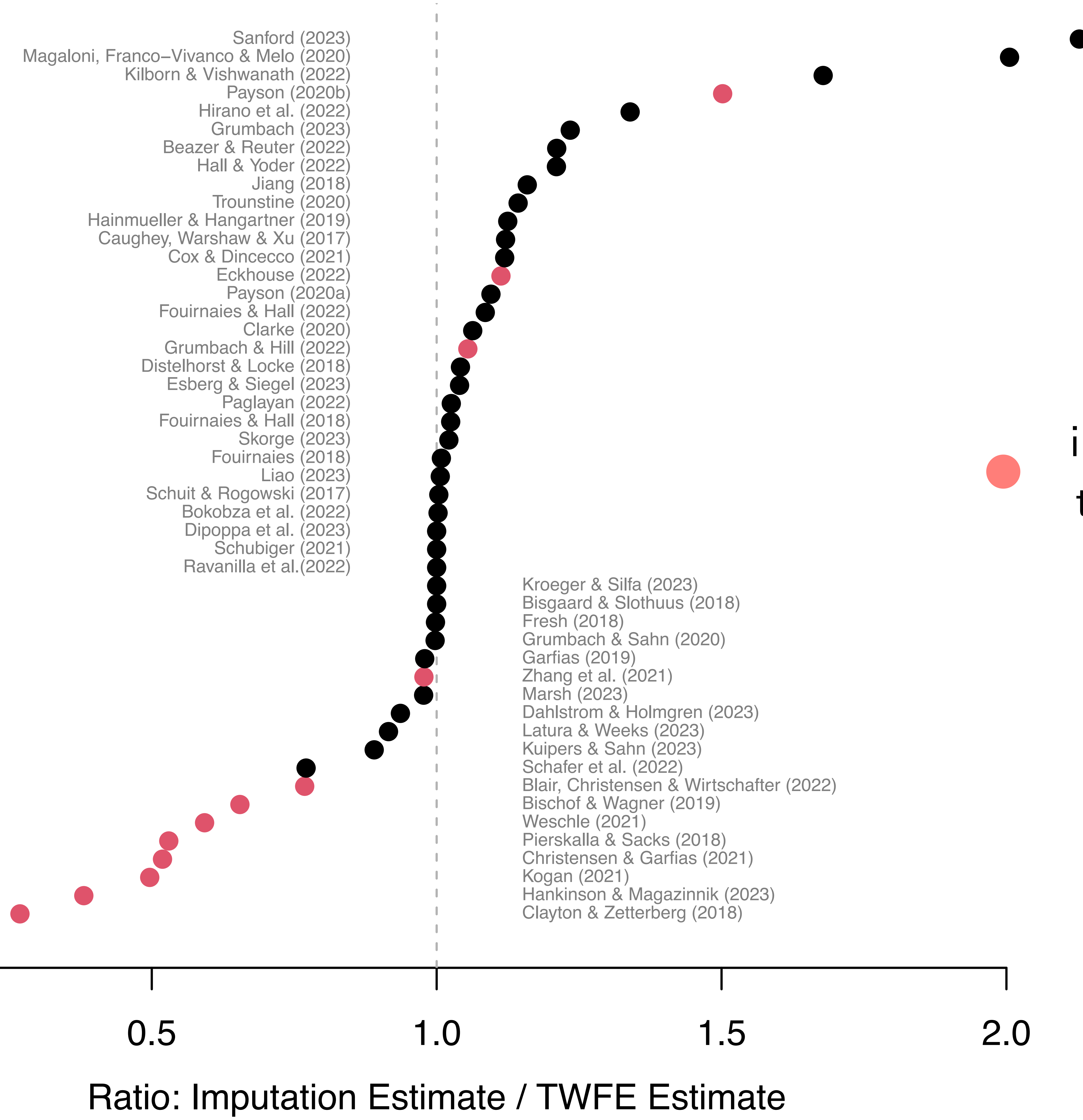

Bischof and Wagner (2019)

# However, Variability Cannot Be Overlooked



# However, Variability Cannot Be Overlooked

Mean(Ratio) = 1.02
Median(Ratio) = 1.00

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# Cost of Efficiency: TWFE versus Imputation SEs (Both Bootstrapped)

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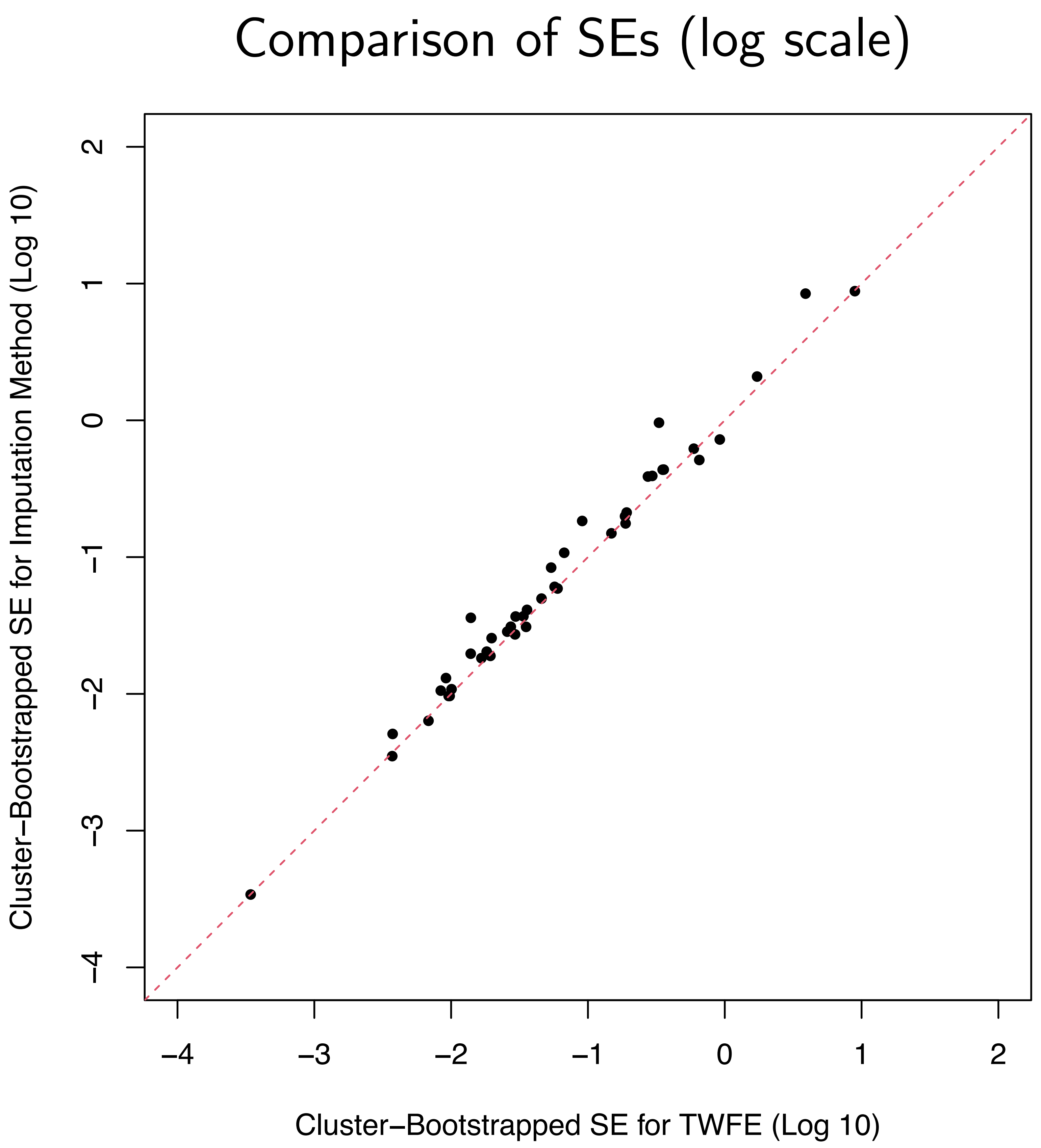

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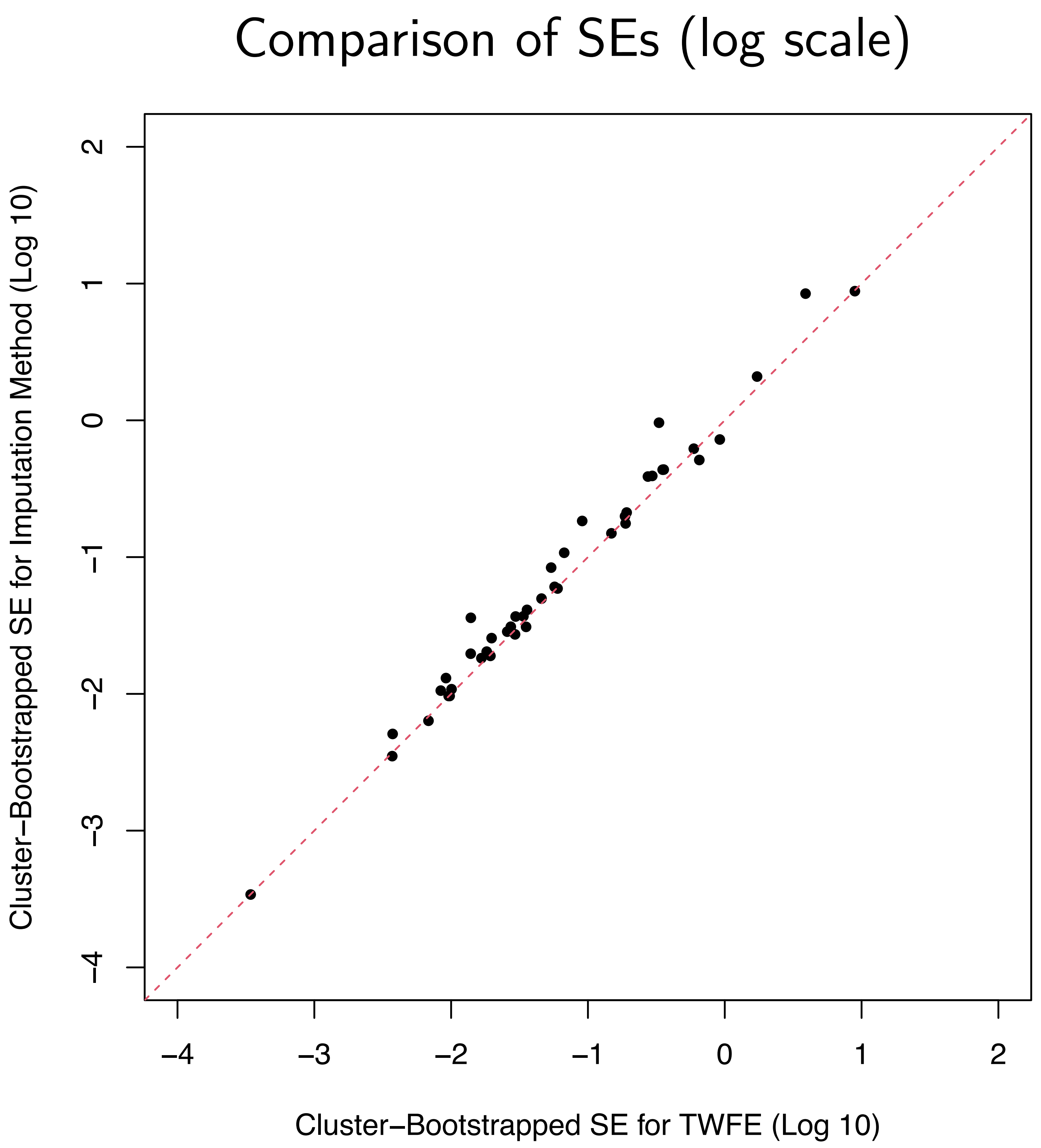


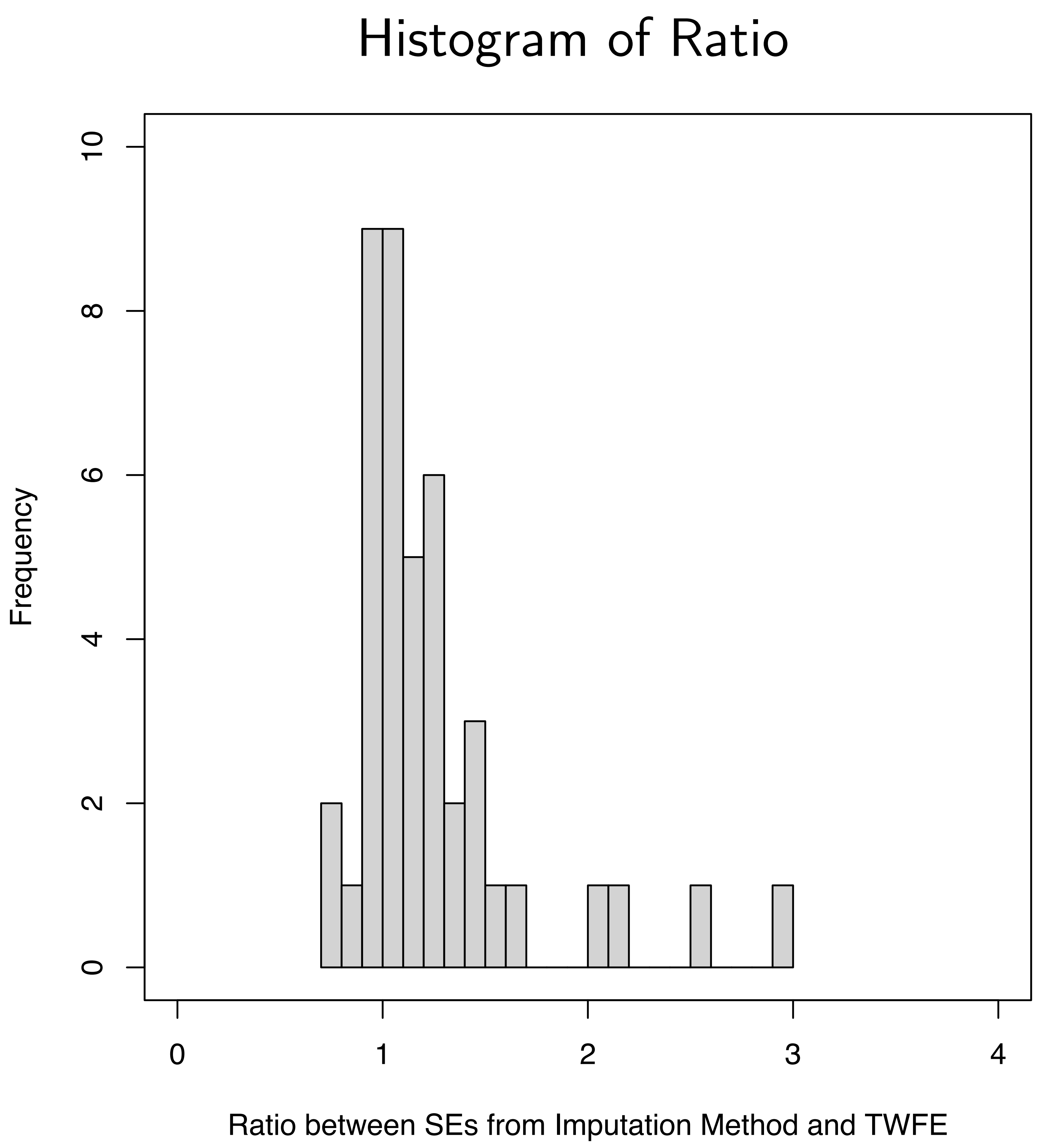

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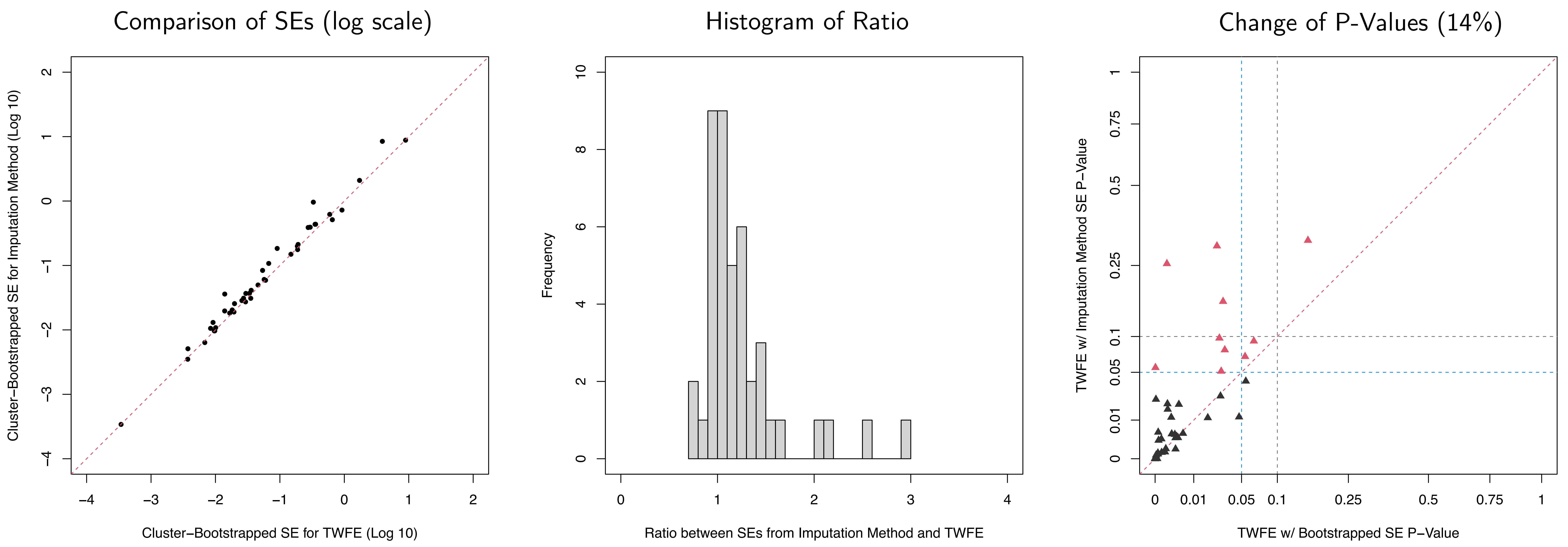
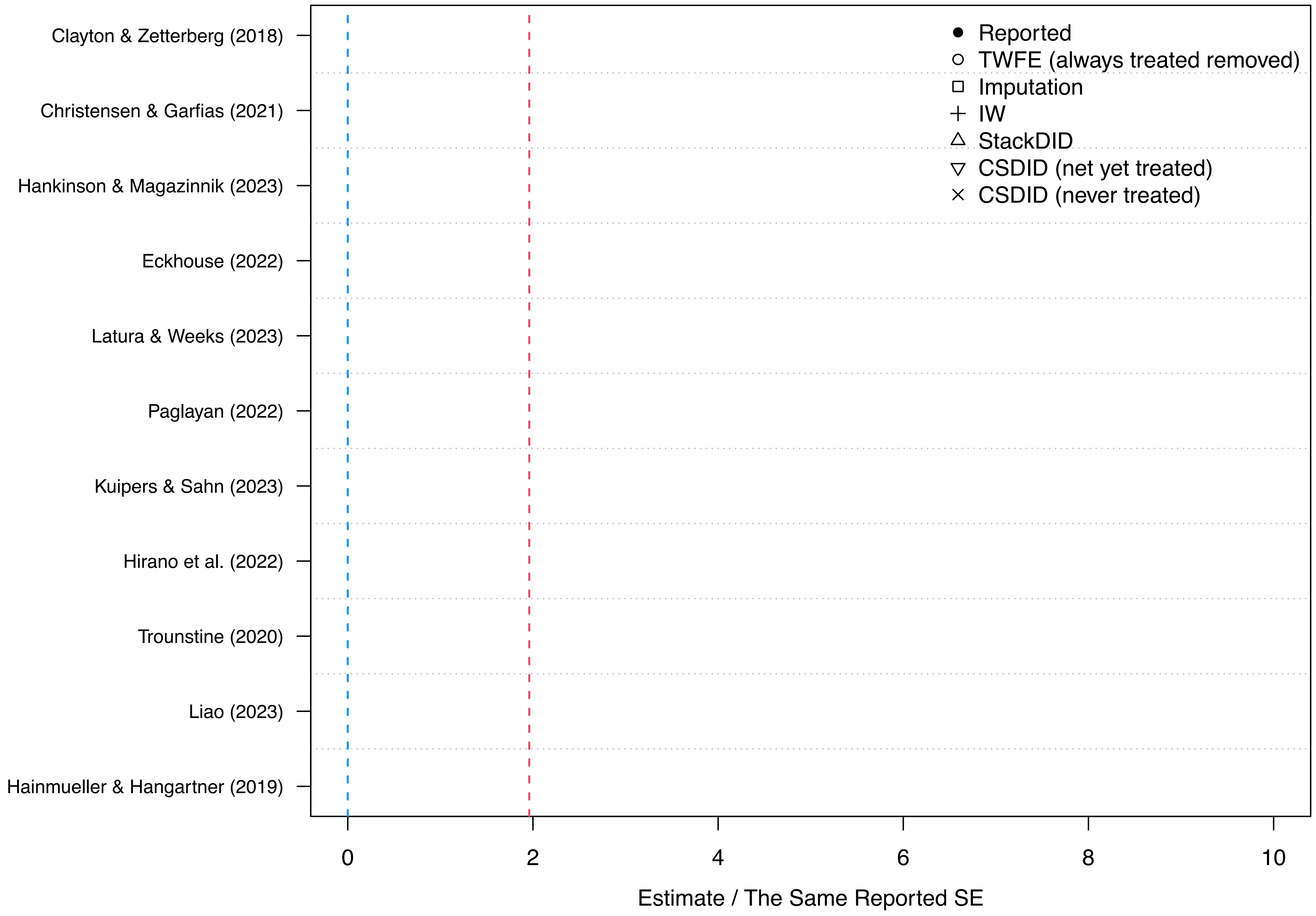


# The Staggered Cases — Coefficients



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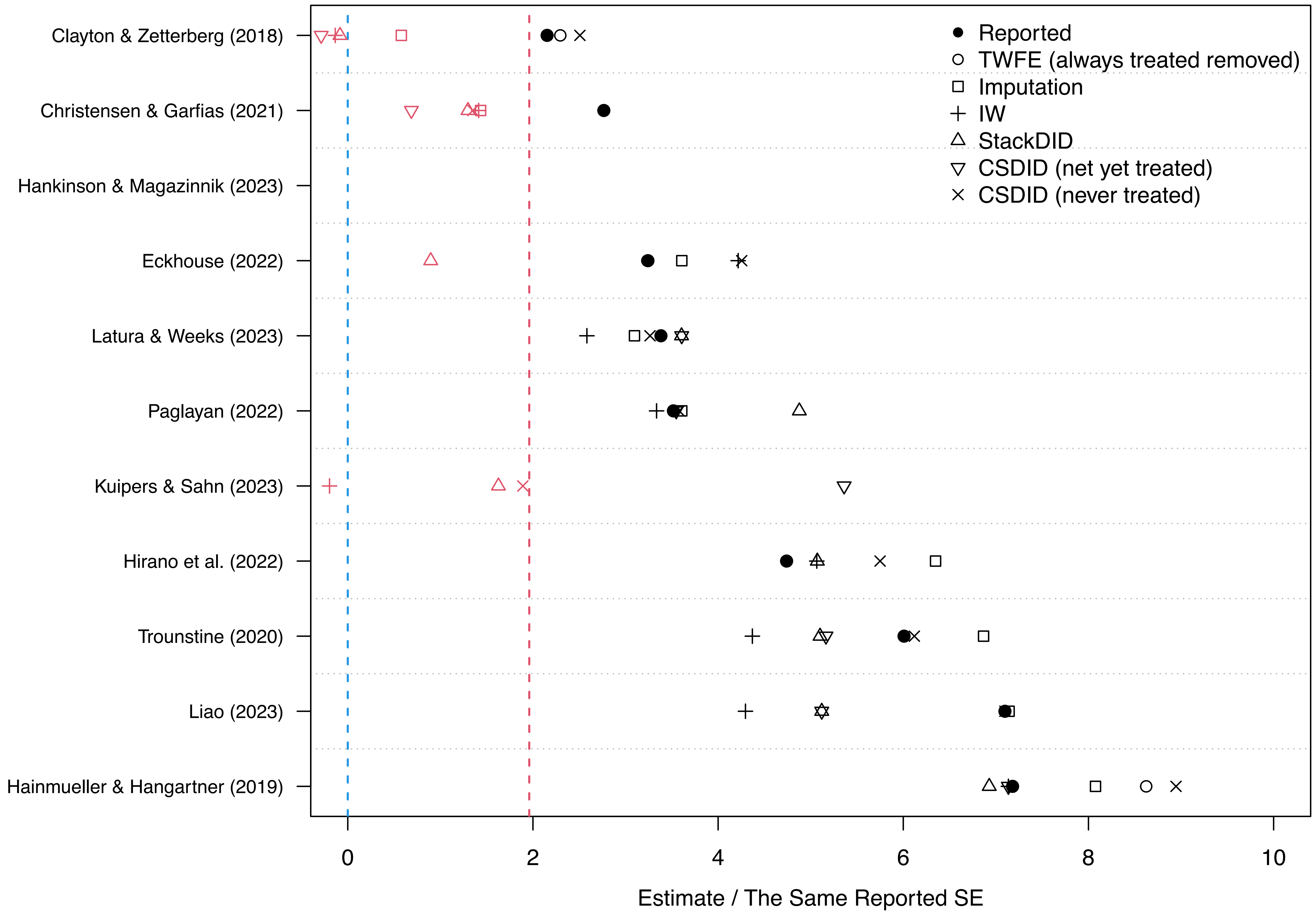
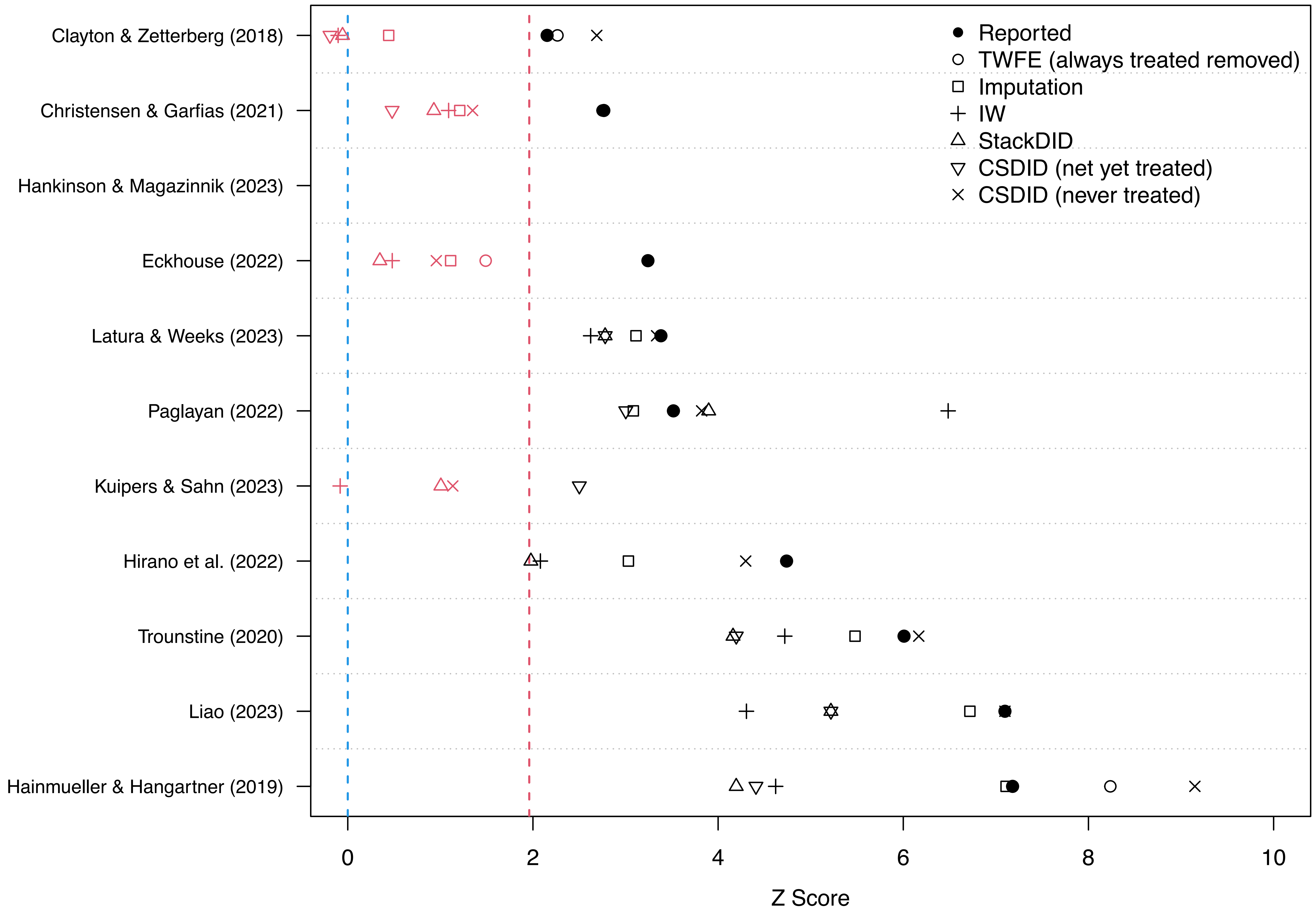


# The Staggered Cases — Z Scores



# Sensitivity Analysis with Relaxed PT $(M = 0.5)$

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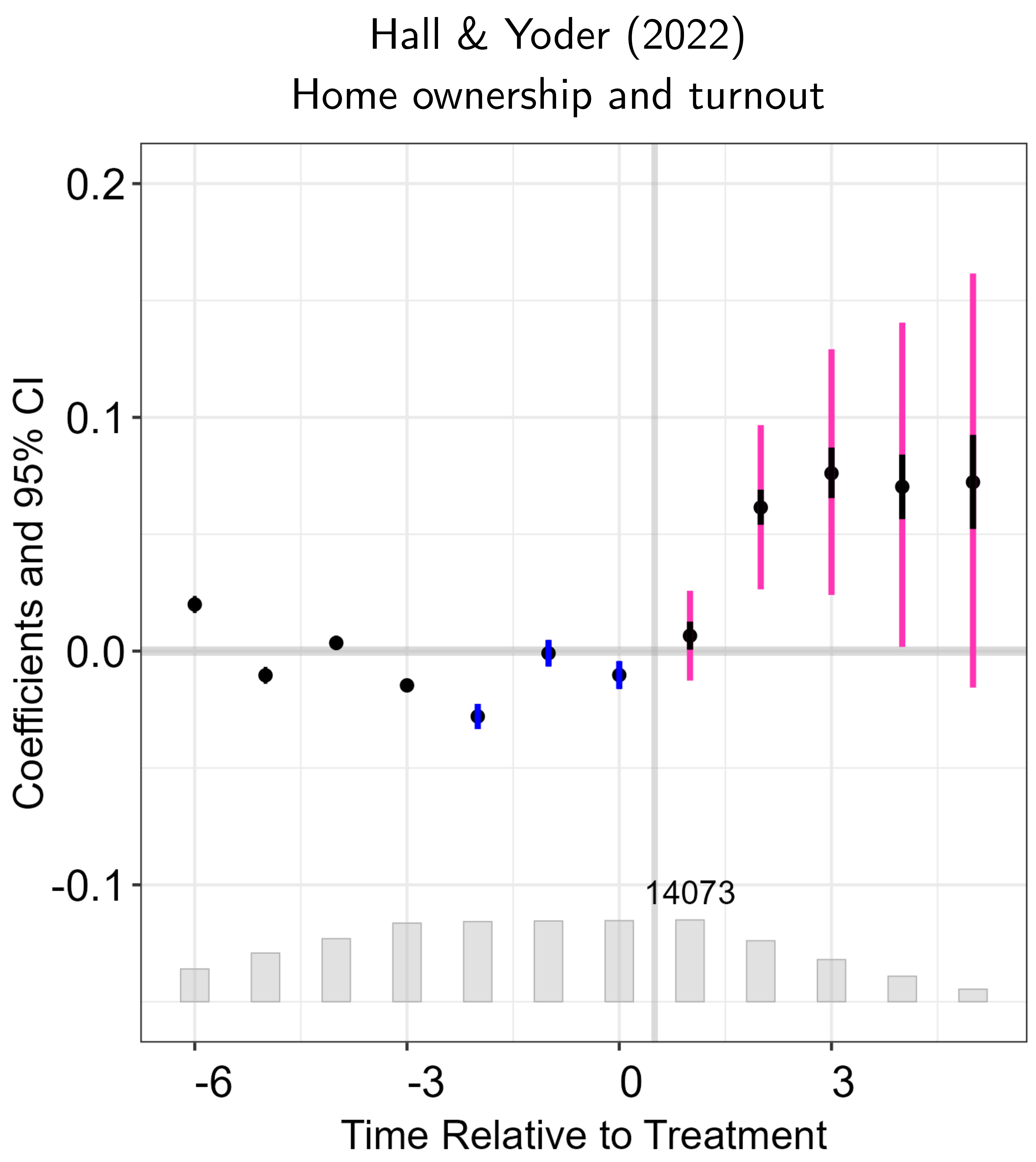

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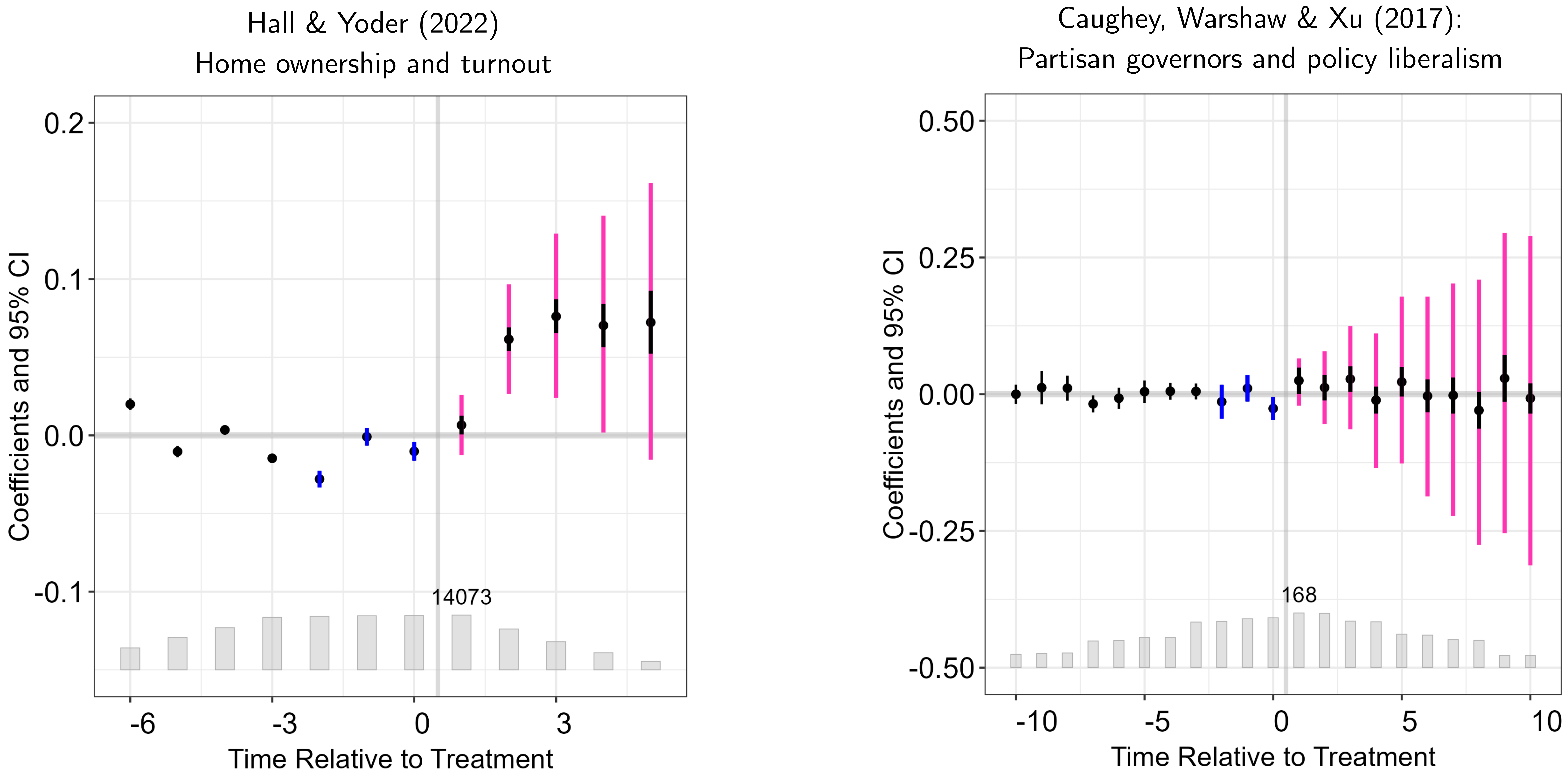

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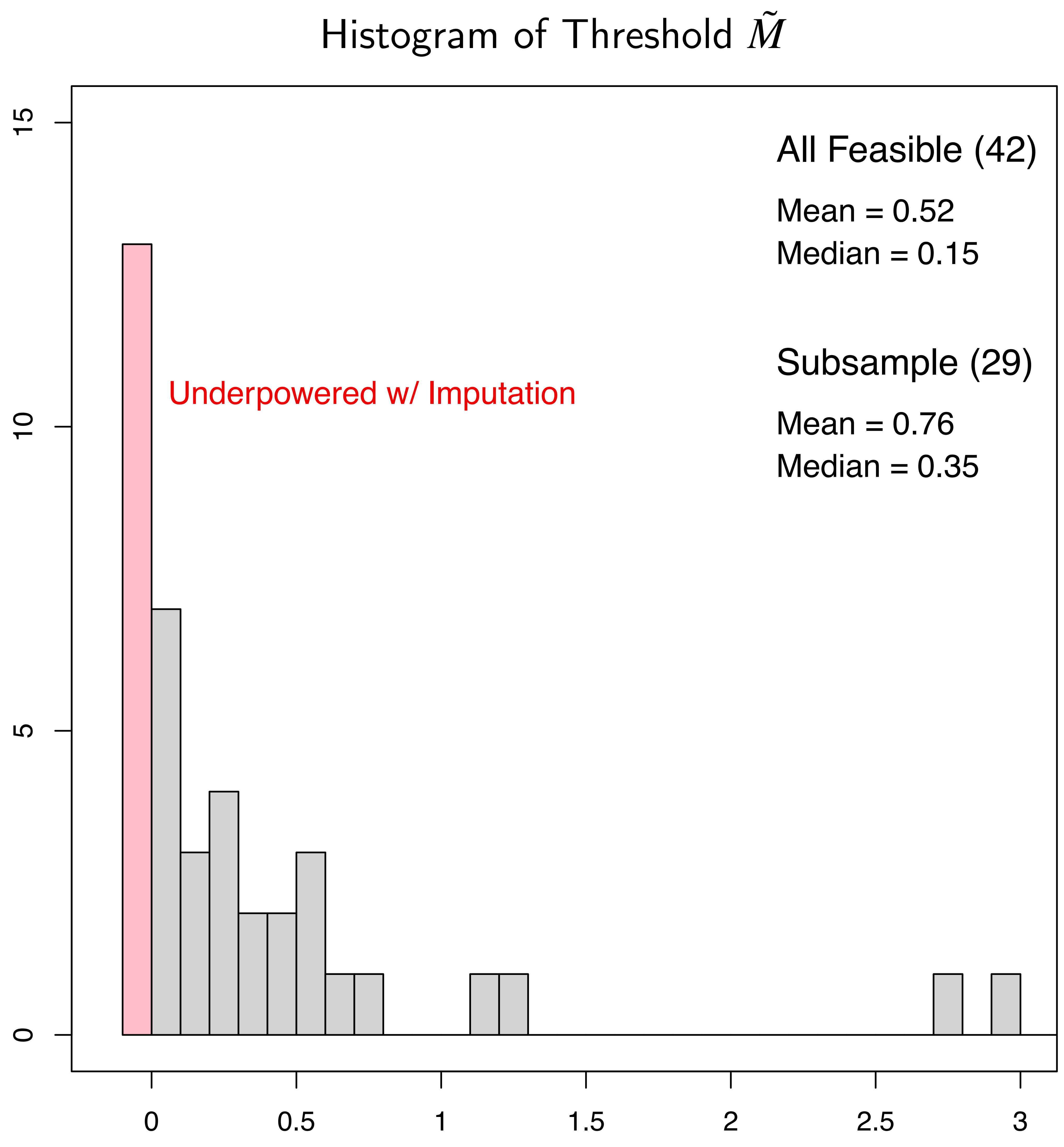

# “Robust” DID Is Power Hungry

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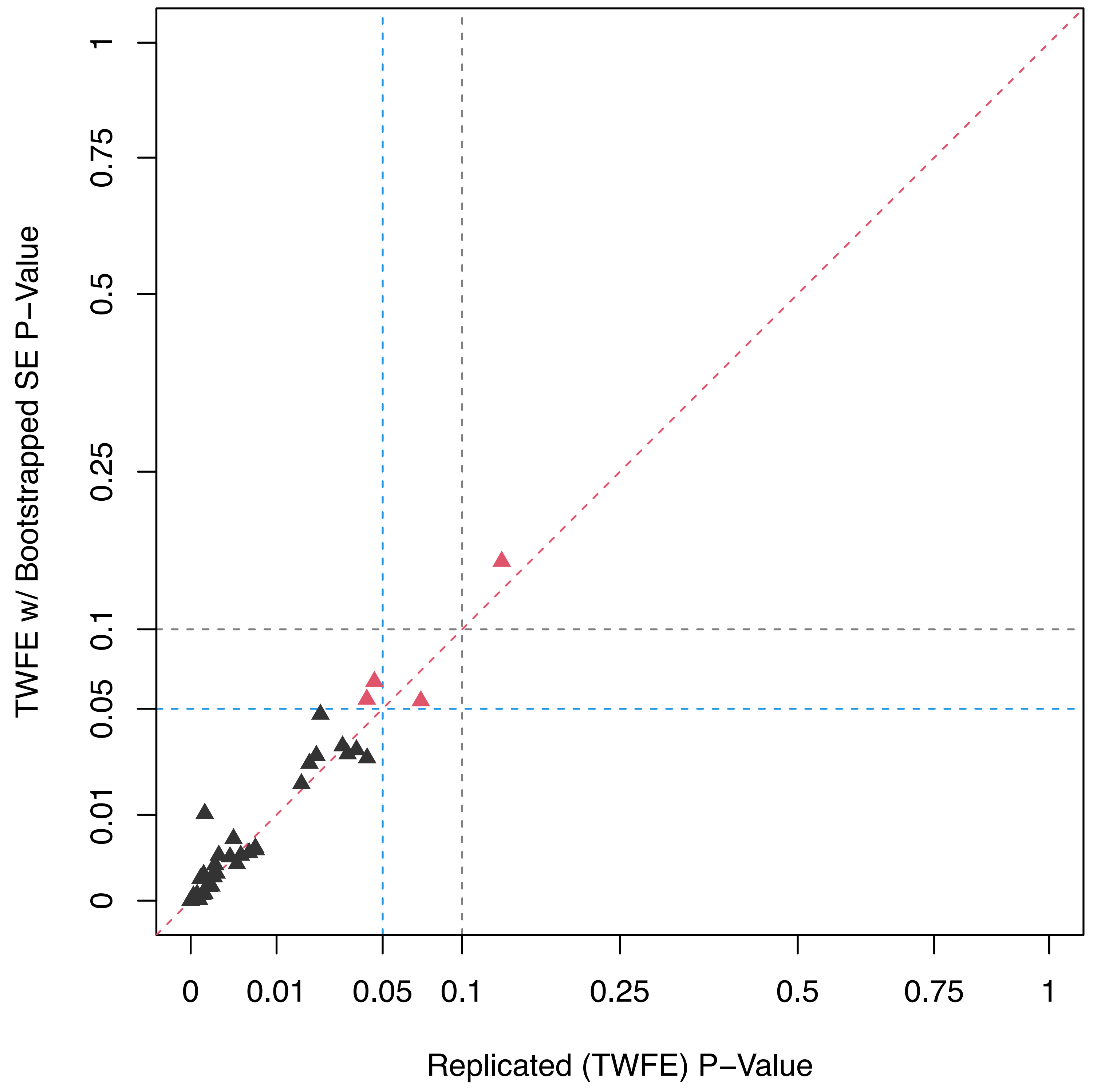
Using Bootstrapped SE (91%)



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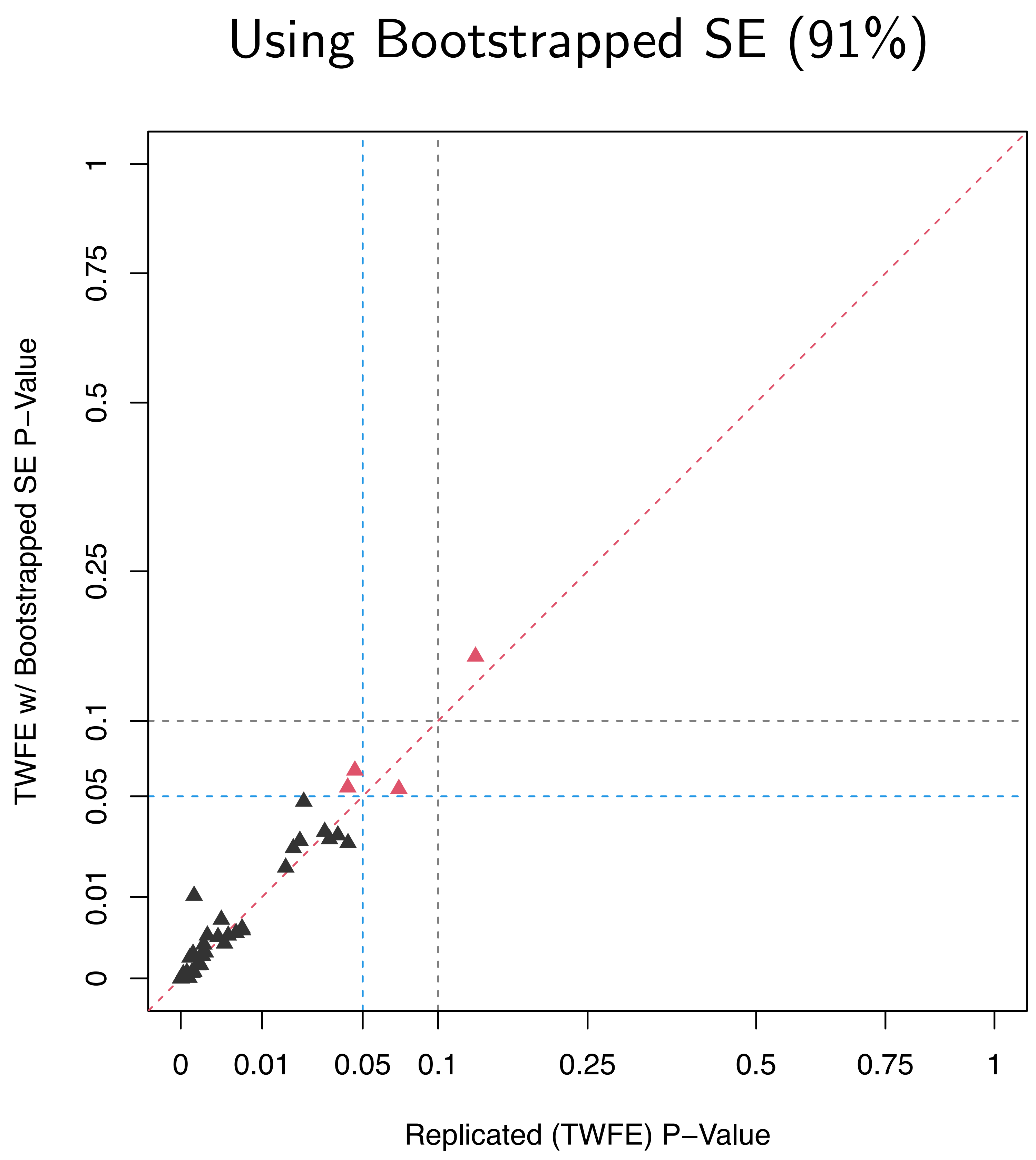


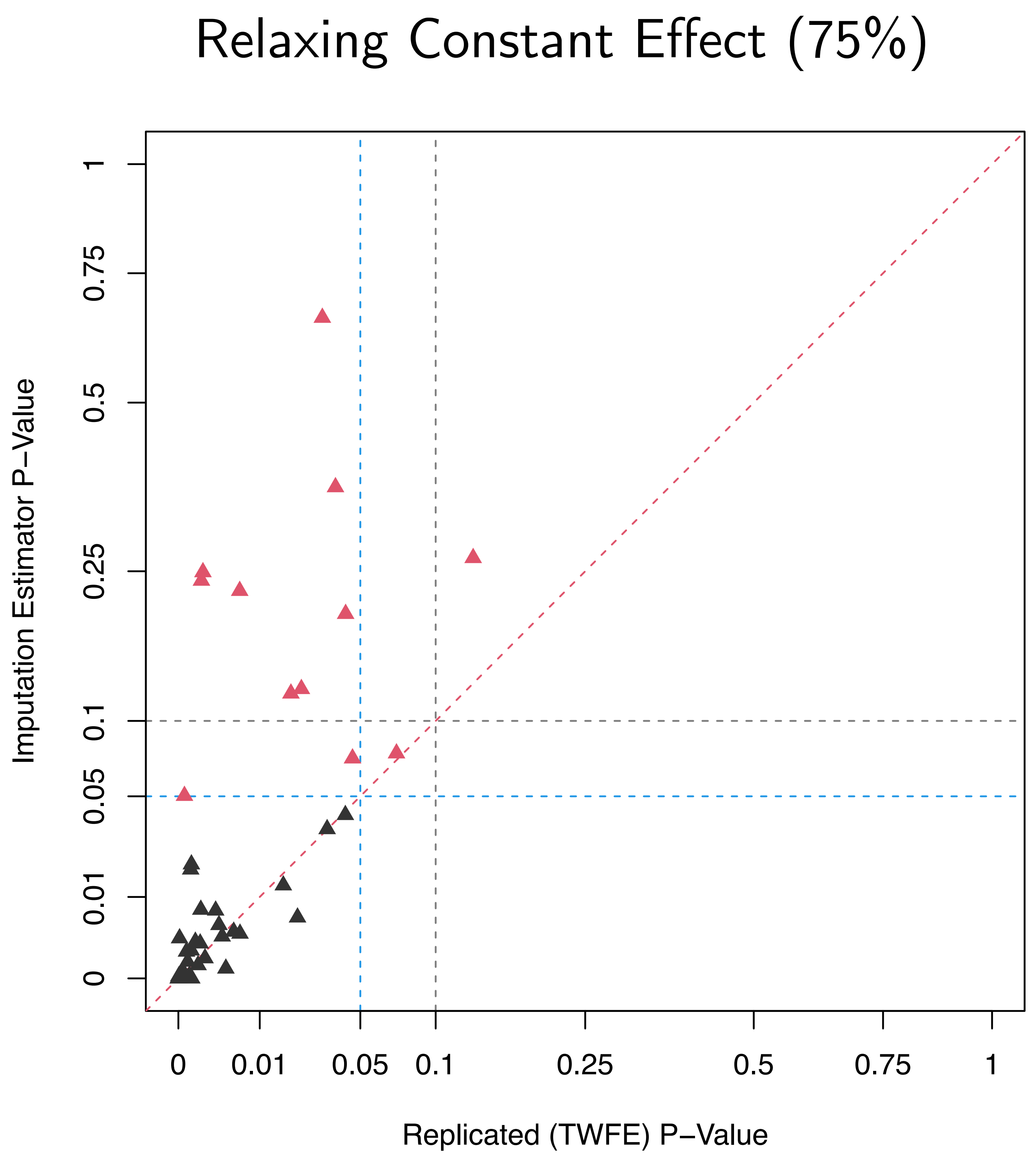

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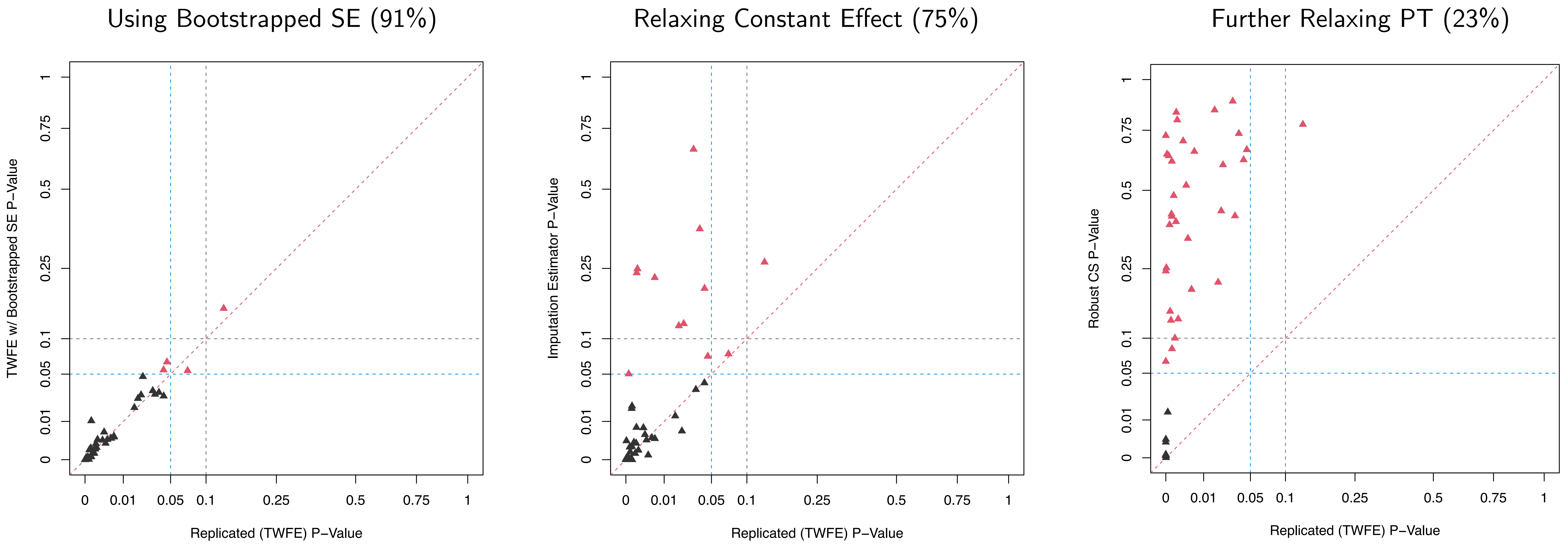

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"paradox" of committing to TWFE

w/ enough power, can afford HTE-robust estimators

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- Other Issues
  - Missing data (unlikely Missing-At-Random)
  - Carryover effects are common

Relaxing constant effects or the PT leads to insufficient power

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# Recommendations

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| | Do's | Don'ts |
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| **Design trumps analysis** | Start empirical analysis with a research design; proceed if "feedback" from past outcomes to treatment assignment is not a major concern | Start empirical analysis by blindly running regressions using existing data |
| **Discussion of designs** | Clearly specify designs and their corresponding identification assumptions | Equate designs with outcome models |
| **Plot raw data** | Plot raw data to better understand the research setting, missingness, sources of variations in the treatment and outcome variables, and univariate/bivariate distributions | Run regressions without looking at the data |
| **Estimators** | Choose HTE-robust estimators and always plot the estimated dynamic treatment effects | Choose models solely based on your beliefs; report regression coefficients only; no results visualization or diagnostics |
| **Diagnostics** | Conduct both visual and statistical tests to gauge the validity the identification and modeling assumptions | |
| **Level of clustering** | Cluster SEs at the level of treatment assignment or higher to account for potential spatial spillover | Cluster SEs at a level lower than treatment assignment |
| **Bootstrapping** | Use cluster-bootstrap procedures when the number of clusters is small (e.g., <50) | Use asymptotic SEs when the number of clusters is small |
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- Come up with a plausible research design ... estimators $\neq$ designs; “shocking” element; justify $\Delta_{s,t} Y_{i,t}(0) \perp\!\!\!\perp D_{i,t}, \forall s, t$

| | Do's | Don'ts |
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- Come up with a plausible research design ... estimators $\neq$ designs; "shocking" element; justify $\Delta_{s,t} Y_{i,t}(0) \perp\!\!\!\perp D_{i,t}, \forall s, t$
- Understand your data better ...before typing "reghdfe" in Stata

| | Do's | Don'ts |
|---|---|---|
| Design [illegible] analysis | [illegible] outcomes to treatment assignment is not a major concern | [illegible] data |
| Discussion of designs | Clearly specify designs and their corresponding identification assumptions | Equate designs with outcome models |
| Plot raw data | Plot raw data to better understand the research setting, missingness, sources of variations in the treatment and outcome variables, and univariate/bivariate distributions | Run regressions without looking at the data |
| Estimators | Choose HTE-robust estimators and always plot the estimated dynamic treatment effects | Choose models solely based on your beliefs; report regression coefficients only; no results visualization or diagnostics |
| Diagnostics | Conduct both visual and statistical tests to gauge the validity the identification and modeling assumptions | |
| Level of clustering | Cluster SEs at the level of treatment assignment or higher to account for potential spatial spillover | Cluster SEs at a level lower than treatment assignment |
| Bootstrapping | Use cluster-bootstrap procedures when the number of clusters is small (e.g., <50) | Use asymptotic SEs when the number of clusters is small |
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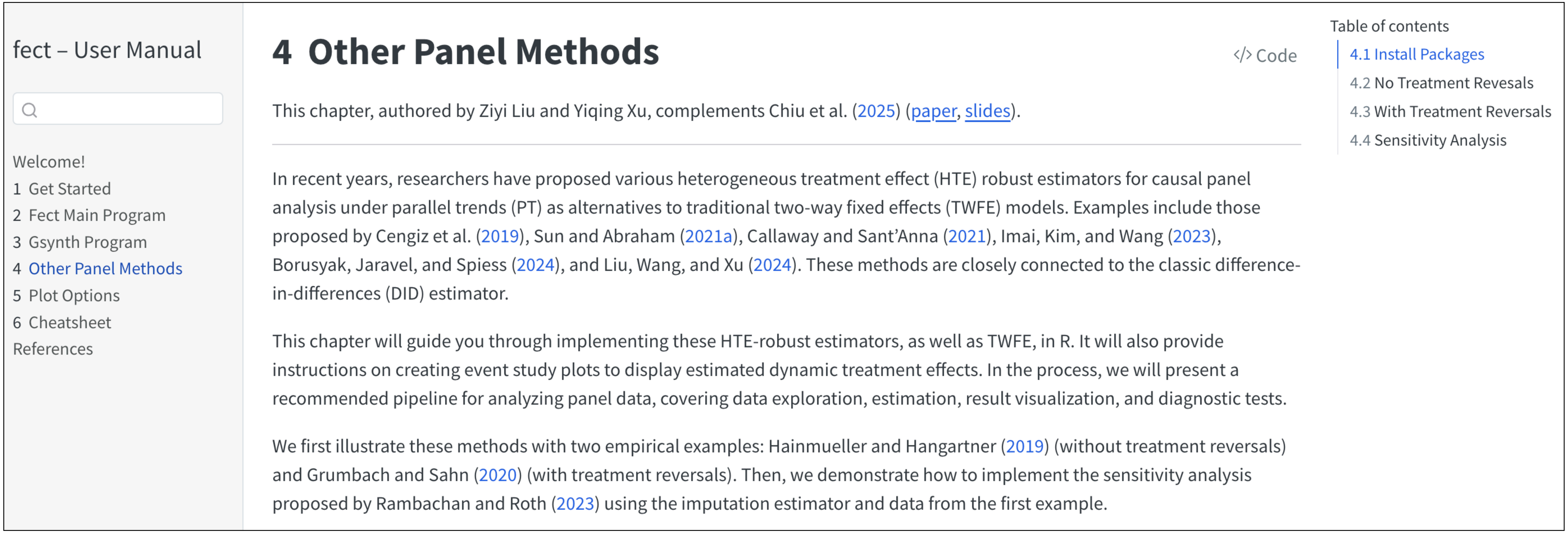
- Using HTE-robust estimators is safer ...and the choice of estimators shouldn't matter much
- Validate your assumptions ...knowing that power is a major concern

# Tools

- panelView (R & Stata), fect (R & Stata)
- Tutorial: https://yiqingxu.org/packages/fect/05-panel.html



fect – User Manual

Welcome!
1 Get Started
2 Fect Main Program
3 Gsynth Program
4 Other Panel Methods
5 Plot Options
6 Cheatsheet
References

# 4 Other Panel Methods

</> Code

This chapter, authored by Ziyi Liu and Yiqing Xu, complements Chiu et al. (2025) (paper, slides).

In recent years, researchers have proposed various heterogeneous treatment effect (HTE) robust estimators for causal panel analysis under parallel trends (PT) as alternatives to traditional two-way fixed effects (TWFE) models. Examples include those proposed by Cengiz et al. (2019), Sun and Abraham (2021a), Callaway and Sant'Anna (2021), Imai, Kim, and Wang (2023), Borusyak, Jaravel, and Spiess (2024), and Liu, Wang, and Xu (2024). These methods are closely connected to the classic difference-in-differences (DID) estimator.

This chapter will guide you through implementing these HTE-robust estimators, as well as TWFE, in R. It will also provide instructions on creating event study plots to display estimated dynamic treatment effects. In the process, we will present a recommended pipeline for analyzing panel data, covering data exploration, estimation, result visualization, and diagnostic tests.

We first illustrate these methods with two empirical examples: Hainmueller and Hangartner (2019) (without treatment reversals) and Grumbach and Sahn (2020) (with treatment reversals). Then, we demonstrate how to implement the sensitivity analysis proposed by Rambachan and Roth (2023) using the imputation estimator and data from the first example.

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Thank you!