Investigating the Effect of 2011 Act 10 on Public Teacher Salaries in Wisconsin

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Introduction

In his seminal paper on unions, Hirschman (1970) suggests that workers dissatisfied with their current working conditions can choose one of two actions. First, they may "exit", meaning that they simply resign and seek another job. Second, they may use their "voice", staying at their job to demand better conditions. He postulates that unions enable workers to use their voice by providing collective bargaining, and grievance and arbitration systems. Freeman (1980) investigates this notion further, finding that union membership is positively associated with years spent at the same company and negatively associated with quitting behaviour or being fired.

U.S. labour unions gained strong legal protections with the National Labor Relations Act of 1935, but later laws and policy developments steadily reduced their power (Economic Policy Institute). The Labor Management Relations Act of 1947 (Taft-Hartley Act) weakened key union tactics by restricting secondary boycotts and jurisdictional strikes, and authorizing states to enact Right-to-Work (RTW) laws that prohibit union security agreements and undermine the unions' ability to collect membership fees for all covered workers (National Labor Relations Board). According to the U.S. Department of Labor, the Taft-Hartley Act marked a shift toward a legal outcome less favorable to unions, and subsequent measures, such as the Labor-Management Reporting and Disclosure Act of 1959, further increased federal oversight of internal union affairs and limited certain forms of picketing (U.S. Department of Labor).

Public-sector unions, especially teachers' unions, fit naturally into this "exit versus voice" framework because they are a primary channel through which educators can bargain over pay and working conditions in a setting where individual negotiation is limited. However, states have progressively narrowed unions' legal tools with reforms increasingly targeting public employees by restricting collective bargaining rights, limiting agency fee collection, and curbing strike activity. In K–12 education, such constraints directly affect teachers' bargaining leverage and can translate into changes in compensation, making teacher salaries a natural outcome for assessing the effects of weakening public-sector unions.

An especially notable reform is the Act 10, a Wisconsin state law first introduced in 2011 under former Republican governor Scott Walker. Act 10 dictated how state government employees, including teachers and nurses, could use their “voice” to bargain over work pay and conditions (PBS Wisconsin, 2024). Among the changes included restrictions to what unions could negotiate beyond wages and the impediment of member fee collections, all of which worked to undermine the survival of public sector unions (WPR). The enactment proved to be controversial, triggering weeks of large protests, a walkout of Democratic senators, numerous legal challenges, and even recall elections seeking to unseat then Governor Walker. However, supporters argued that before the act, the previous labour market for public employees was inefficient and diverted proper spending in school districts and local governments (PBS Wisconsin, 2024). As of writing this report, attempts to repeal the law are still playing out in the courts more than a decade after it took effect.

We are particularly interested in the effect of policies that weaken public-sector collective bargaining rights on teacher compensation. Thus, our research question is, **“How do laws obstructing public employee unions affect teacher wages?”** More specifically, we assess **how the 2011 Wisconsin Act 10 impacted salaries for elementary and secondary public school teachers.** We hypothesize that by impeding the collective bargaining power of public employees, Act 10 reduced teacher salaries.

Literature Review

The effect of unions remains a contentious topic. On one hand, the collective power of unions helps to give workers a seat at the table. Furthermore, studies like Green, Sand, and Snoddy (2022) suggest that unions even have positive spillover impacts on non-union wages. On the other hand, unions are said to inefficiently sustain high wages and prevent poor-performing workers from being fired. Furthermore, both its proponents and detractors claim that unions have an impact on efficiency.

There are several studies on the effects of Act 10 specifically. Using a differences-in-differences approach, Lyon (2021) found that the implementation of Act 10 led to substantial declines in teacher union power. Furthermore, the study found that weakened unions did not benefit student outcomes, and if anything, had negative effects on fourth grade reading scores. Litten (2017) found that the policy was associated with an eight percent reductaion in teacher salaries. This finding is corroborated by Baron (2018), which found that the policy reduced teacher salaries by approximately four percent. Like Lyon (2021), Baron (2018) also examined student outcomes, finding that Act 10 decreased average standardized exam scores by roughly 20% of a standard deviation. On the other hand, Jorgensen (2016) suggested that Act 10 was actually welfare increasing, and that the teacher labour market in Wisconsin was previously inefficient. Using piecewise regressions, the study agreed that the policy reduced teacher salaries and other benefits, but also increased the number of teachers per capita. Examining Act 10, Biasi and Sarsons (2021) conclude that unions lower inequality and gender pay gaps.

This report contributes to the literature in the following ways. First, whereas much of the existing literature relies on differences-in-differences or fixed effects approaches to estimate the impact of Act 10, we apply synthetic control. Synthetic control is particularly suitable because we have access to many time periods of data. Furthermore, it does not rely on the strong parallel trends assumption posited by many exisitng studies. Second, the teacher salary data this study uses is possibly more reliable than previous studies since it was collected and estimated by the National Education Association (NEA). NEA has decades of expertise, experience, and connections with the American education system, so it is arguably advantageous to use their vetted data than to attempt to aggregate teacher salary data ourselves. For example, Jorgensen (2016) used their own methodology to measure teacher salaries, but admit that differences in the quality and public availability of data across states led to irreconcilable inconsistencies.

Data

We obtained data from two sources. First is the Correlates of State Policy Project (CSPP) from the University of Michigan. It is a comprehensive state-level panel dataset containing a diverse range of policy indicator variables and outcomes such as education, healthcare, environment, and criminal justice. This dataset is assembled by compiling data from a plethora of different existing studies. The specific data that we use from the CSPP describes the timing of policies prohibiting agency fees for different states. Since we eventually narrowed our research scope to focus only on Wisconsin being treated, this dataset was not needed. Nevertheless, we mention it in this report because of its value and potential for future research. Second is a table on average teacher salaries by state from 1990 to 2020, obtained from the National Science Board (NSB). Their data was assembled using estimates from the National Education Association (NEA). We merged the two datasets by state and year.

In particular, our anaylsis uses data from the CSPP dataset in the time period of 1990 to 2017 for the following states: Alaska, California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Kentucky, Maine, Maryland, Massachusetts, Minnesota, Missouri, Montana, New Hampshire, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Vermont, and Washington. For a robustness check later in the paper, we use data for Michigan in the same time period.

In [24]:
```
import pandas as pd
import numpy as np
import pyfixest as pf
```

```
import scipy
from scipy import stats
import altair as alt
import statsmodels.formula.api as smf
import matplotlib.pyplot as plt
```

In [25]:
```
# Read state policy data
df = pd.read_csv('data/cspp_data_2025-11-20.csv')
df.columns
df
df_placebo = df.copy()

# Read NEA teacher salary data
salaries_df_wide = pd.read_csv('data/teacher_salaries.csv')
salaries_df = pd.melt(salaries_df_wide, id_vars = ['State'], var_name = 'Year', value_name = 'Salary')
salaries_df.rename(columns = {'State': 'state', 'Year': 'year', 'Salary': 'salary'}, inplace = True)

salaries_df['year'] = salaries_df['year'].astype(int)
salaries_df['salary'] = salaries_df['salary'].replace({',': ''}, regex=True).astype(int)
```

In [26]:
```
# Select relevant variables from CSPP
df_filtered = df[['year', 'st', 'state', 'agencyfeesprohibited']]

# Filter between 1990 and 2017
df_filtered = df_filtered[(df_filtered['year'] >= 1990) & (df_filtered['year'] <= 2017)]

df_merged = pd.merge(df_filtered, salaries_df, on = ['state', 'year'], how = 'left')

# Drop District of Columbia
df_merged = df_merged[df_merged['state'] != 'District of Columbia']

# Adjust variable types
df_merged['agencyfeesprohibited'] = df_merged['agencyfeesprohibited'].astype(int)
df_merged["state"] = df_merged["state"].astype("string")
df_merged["st"] = df_merged["st"].astype("string")

# Identify states that ever had a 1 under 'agencyfeesprohibited' i.e., ever had a RTW law pass
states_with_fees_prohibited = df_merged[df_merged['agencyfeesprohibited'] == 1]['st'].unique()

# Exclude states that ever had RTW except for Wisconsin (our sole treatment)
df = df_merged[~((df_merged['st'].isin(states_with_fees_prohibited)) & (df_merged['st'] != 'WI'))]
```

```
df
```

Out[26]:

	year	st	state	agencyfeesprohibited	salary
0	1990	AK	Alaska	0	43153
1	1991	AK	Alaska	0	43427
2	1992	AK	Alaska	0	44661
3	1993	AK	Alaska	0	46701
4	1994	AK	Alaska	0	47512
...	...	...	...	...	...
1367	2013	WI	Wisconsin	1	53797
1368	2014	WI	Wisconsin	1	53679
1369	2015	WI	Wisconsin	1	52264
1370	2016	WI	Wisconsin	1	54115
1371	2017	WI	Wisconsin	1	51439

672 rows × 5 columns

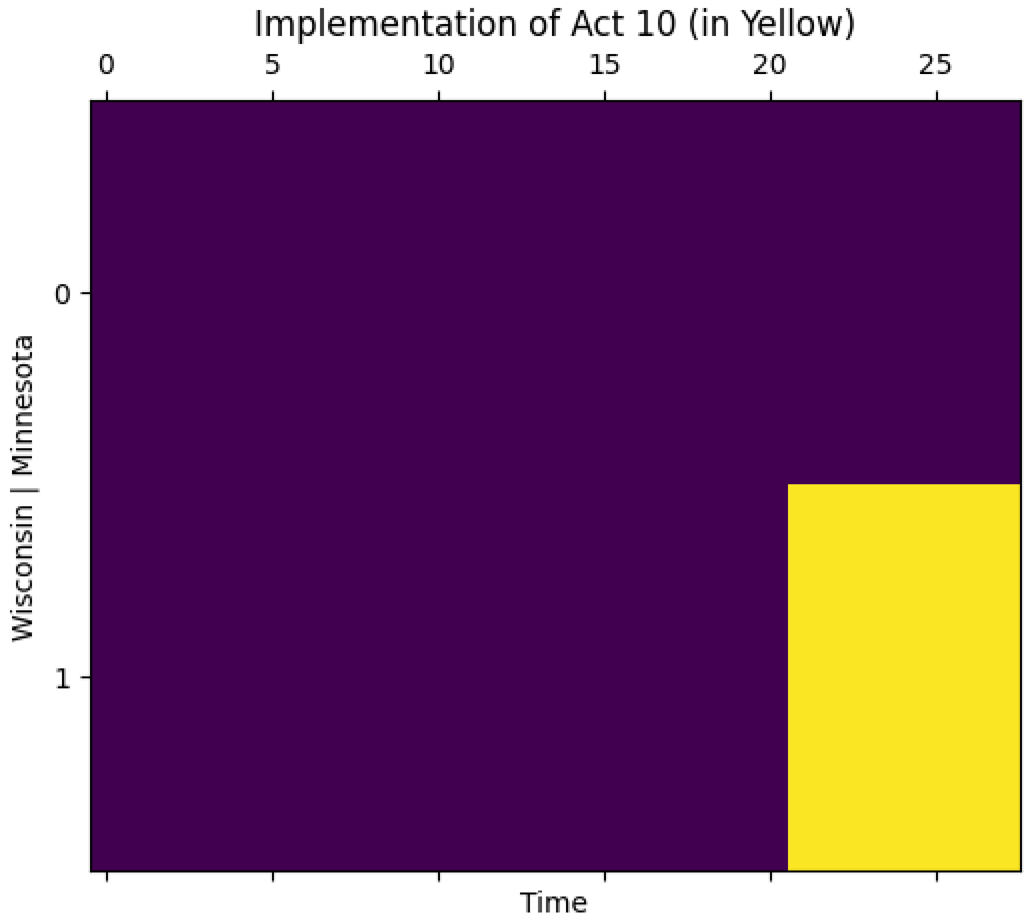
We plot the following graph to get a sense of the timing of the treatment, Act 10, in our data.

In [27]:

```
df_did1 = df[df['state'].isin(['Wisconsin', 'Minnesota'])]

pf.panelview(
    df,
    unit="state",
    time="year",
    treat="agencyfeesprohibited",
    collapse_to_cohort=True,
    sort_by_timing=True,
    ylab="Wisconsin | Minnesota",
    xlab="Time",
    title="Implementation of Act 10 (in Yellow)",
```

```
    figsize=(6, 5),
)
```

Out[27]: <Axes: title={'center': 'Implementation of Act 10 (in Yellow)'}, xlabel='Time', ylabel='Wisconsin | Minnesota'>



Methodology

We employ a differences-in-differences framework to find the causal effect of the RTW policy on teachers′ wages.

$$(E[Y(1) \mid D=1] - E[Y(1) \mid D=0]) - (E[Y(0) \mid D=1] - E[Y(0) \mid D=0])$$

where we define potential outcomes 1 = "post-treatment" and 0 = "pre-treatment."

An initial approach was to use two states: Wisconsin in the treatment group and Minnesota in the control group. However, the number of observations for each state in the time period 1990-2017 is 28. With such a small sample size, the standard errors may not be well defined. Thus, we kept it as a baseline result and turn to synthetic control for our main analysis. The treated unit is Wisconsin, and the donor pool consists of all the other states from the list in the “Data” section. The donor pool states never implemented the RTW or any similar policy affecting public unions within or prior to the time period being analyzed. We will combine the untreated states to build a fake state that closely resembles the pre-treatment trend of Wisconsin. We’ll see how the synthetic control behaves after the intervention.

We have $J+1$ units. Without loss of generality, assume that unit 1 (Wisconsin) is the unit that gets affected by an intervention. Units $j=2,\ldots,J+1$ are a collection of untreated units that we will refer to as the donor pool. Also assume that the data we have span T time periods, with T_0 periods before the intervention. For each unit j and each time t, we observe the outcome Y_{jt}. For each unit j and each time t, define Y_{jt}^N as the potential outcome without intervention and Y_{jt}^I the potential outcome with intervention.

To estimate Y_{1t}^N, we remember that a combination of units in the donor pool may approximate the characteristics of the treated unit much better than any untreated unit alone. Thus, a synthetic control is defined as a weighted average of the units in the control pool. Given the weights $\mathbf{W}=(w_2,\ldots,w_{J+1})$, the synthetic control estimate of Y_{1t}^N is:

$$\hat{Y}_{1t}^N = \sum_{j=2}^{J+1} w_j Y_{jt}$$

where,

$$\mathbf{W}=(w_2,\ldots,w_{J+1}) \quad \text{minimizes} \quad \|\mathbf{X}_1-\mathbf{X}_0\mathbf{W}\| = \left(\sum_{h=1}^{k} v_h \left(X_{h1}-\sum_{j=2}^{J+1} w_j X_{hj}\right)^2\right)^{1/2}$$

Then, the effect for the treated unit $j=1$ at time t, for $t>T_0$, is defined as

$$\tau_{1t} := Y_{1t} - \hat{Y}_{1t}^N, \qquad t > T_0$$

Results

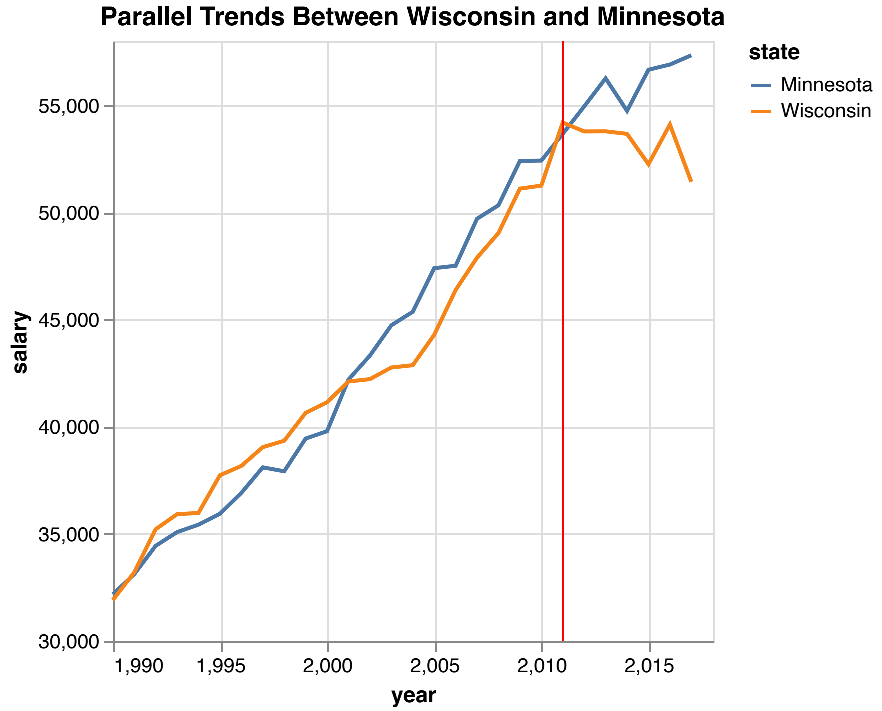
Baseline DID

We begin with a simple two way fixed effects model between Wisconsin and Minnesota. We justify Minnesota as a plausible counterfactual for Wisconsin in the same way that Card and Krueger (1993) justify using nearby Pennsylvania for New Jersey. The two states are geographically adjacent and economically integrated, so they are exposed to many of the same regional shocks (e.g., business cycles, inflation, and broader Upper Midwest labor-market conditions). In addition, both states have similar institutional environments for public education finance and labor markets (large public sectors, comparable urban–rural mixes, and similar school-district salary schedule structures), which makes it reasonable to expect common underlying trends in teacher pay absent the policy change. Plotting the pre-treatment data, the parallel trends assumption seems credible.

In [28]:

```
plot = alt.Chart(
    df_did1,
    title = alt.Title(
        'Parallel Trends Between Wisconsin and Minnesota'
        )).mark_line().encode(
    alt.X('year:Q'),
    alt.Y('salary:Q').scale(zero=False),
    alt.Color('state:N'))

vline = alt.Chart(pd.DataFrame({"year":[2011]})).mark_rule(color="red").encode(x='year:Q')

plot + vline
```

Out[28]:



In [29]:

```
fit_static_twfe1 = pf.feols(
    "salary ~ agencyfeesprohibited | state + year",
    df_did1
)
fit_static_twfe1.summary()
```

```
###

Estimation:  OLS
Dep. var.: salary, Fixed effects: state+year
Inference:  iid
Observations:  56

| Coefficient          |   Estimate |   Std. Error |   t value |   Pr(>|t|) |      2.5% |    97.5% |
|:---------------------|-----------:|-------------:|----------:|-----------:|----------:|---------:|
| agencyfeesprohibited |  -2208.000 |      715.136 |    -3.088 |      0.005 | -3677.983 | -738.017 |
---
RMSE: 789.489 R2: 0.989 R2 Within: 0.268
```

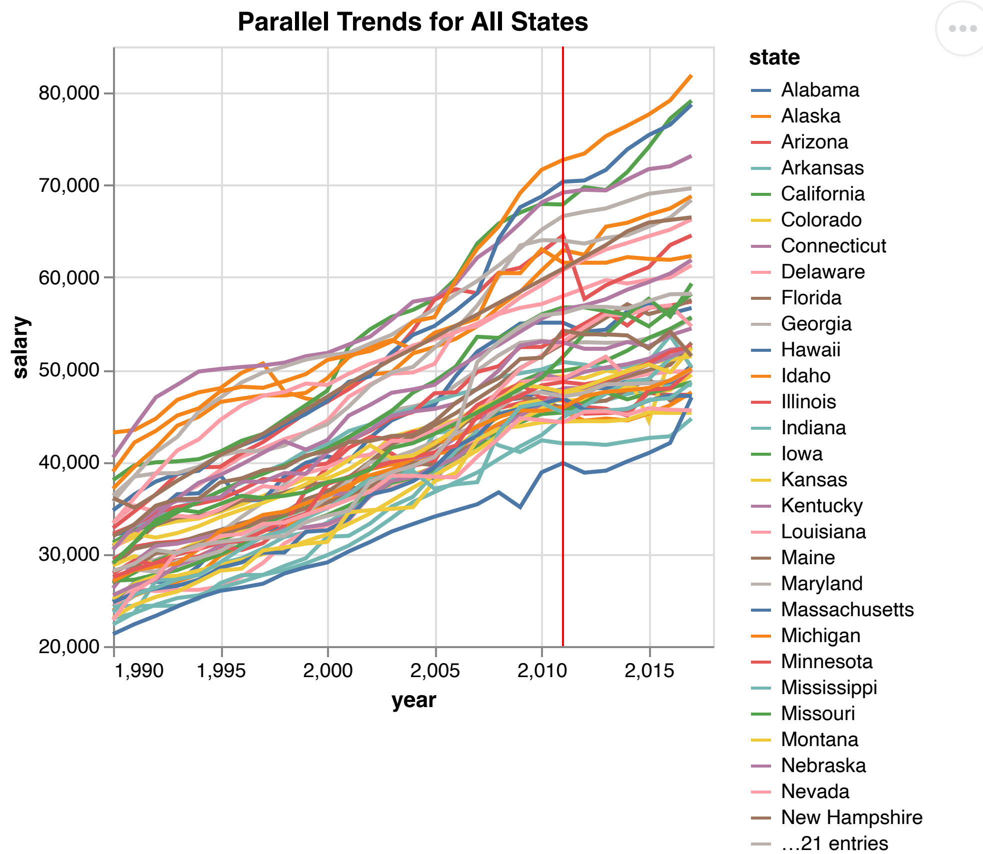
With state and year fixed effects, the coefficient on *agencyfeesprohibited* is $-2, 208(953$,678, −$738]), suggesting lower average teacher salaries after agency fees are prohibited in this sample. The “R2 Within” of 0.268 indicates the treatment explains a meaningful share of within-state changes in this restricted panel, but inference should be treated cautiously given the small N (56) and iid standard errors.

Baseline 2

Here, we get a bit bold and include all other control states (states that never received any sort of policy affecitng public unions) to see if parallel trends hold. Visually, we cannot conclude anything, and it is unreasonable to expect that the parallel trends assumption would hold across so many different states.

In [30]:
```
plot = alt.Chart(
    df_merged,
    title = alt.Title(
        "Parallel Trends for All States"
        )).mark_line().encode(
    alt.X('year:Q'),
    alt.Y('salary:Q').scale(zero=False),
    alt.Color('state:N'))

vline_WI = alt.Chart(pd.DataFrame({"year":[2011]})).mark_rule(color="red").encode(x='year:Q')

plot + vline_WI
```

Out[30]:



In [31]:

```
fit_static_twfe2 = pf.feols(
    "salary ~ agencyfeesprohibited | state + year",
    df
)
fit_static_twfe2.summary()
```

```
###

Estimation:  OLS
Dep. var.: salary, Fixed effects: state+year
Inference:  iid
Observations:  672

| Coefficient          |   Estimate |   Std. Error |   t value |   Pr(>|t|) |      2.5% |     97.5% |
|:---------------------|-----------:|-------------:|----------:|-----------:|----------:|----------:|
| agencyfeesprohibited |  -3827.867 |     1056.199 |    -3.624 |      0.000 | -5902.028 | -1753.707 |
---
RMSE: 2275.594 R2: 0.96 R2 Within: 0.021
```

We can create a similar story regarding the signifantly negative estimate seen here, but both baseline estimates provide unreliable inference since we cannot cluster on states.

Synthetic Control

We constructed a synthetic control for Wisconsin by treating Wisconsin as the exposed unit and using the remaining states as a donor pool. We first restricted attention to the pre-policy period and used that information to select a set of donor weights that makes a weighted average of donor states reproduce Wisconsin's pre-policy teacher-salary trajectory as closely as possible. This step is essentially a constrained least-squares matching problem: we choose weights to minimize pre-intervention discrepancies while imposing convexity constraints (weights are non-negative and sum to one), which ensures the synthetic unit is an interpretable mixture of observed states rather than an extrapolation.

Having fixed the weights using only pre-policy data, we then generated Wisconsin's counterfactual post-policy path by applying the same weights to the donor states' post-policy outcomes. This yields an estimated "no-intervention" salary trajectory for Wisconsin under the assumption that, absent the policy, Wisconsin would have continued to evolve like that weighted combination of comparable states. We then defined the estimated treatment effect in each post-policy year as the difference between Wisconsin's observed salary and the synthetic Wisconsin salary in that year, and we interpreted systematic post-policy divergence, conditional on strong pre-policy fit, as evidence consistent with an intervention effect.

In [32]:
```python
import random
from scpi_pkg.scdata import scdata
from scpi_pkg.scest import scest
from scpi_pkg.scplot import scplot
```

```
from sklearn.preprocessing import StandardScaler
from scpi_pkg.scpi import scpi
```

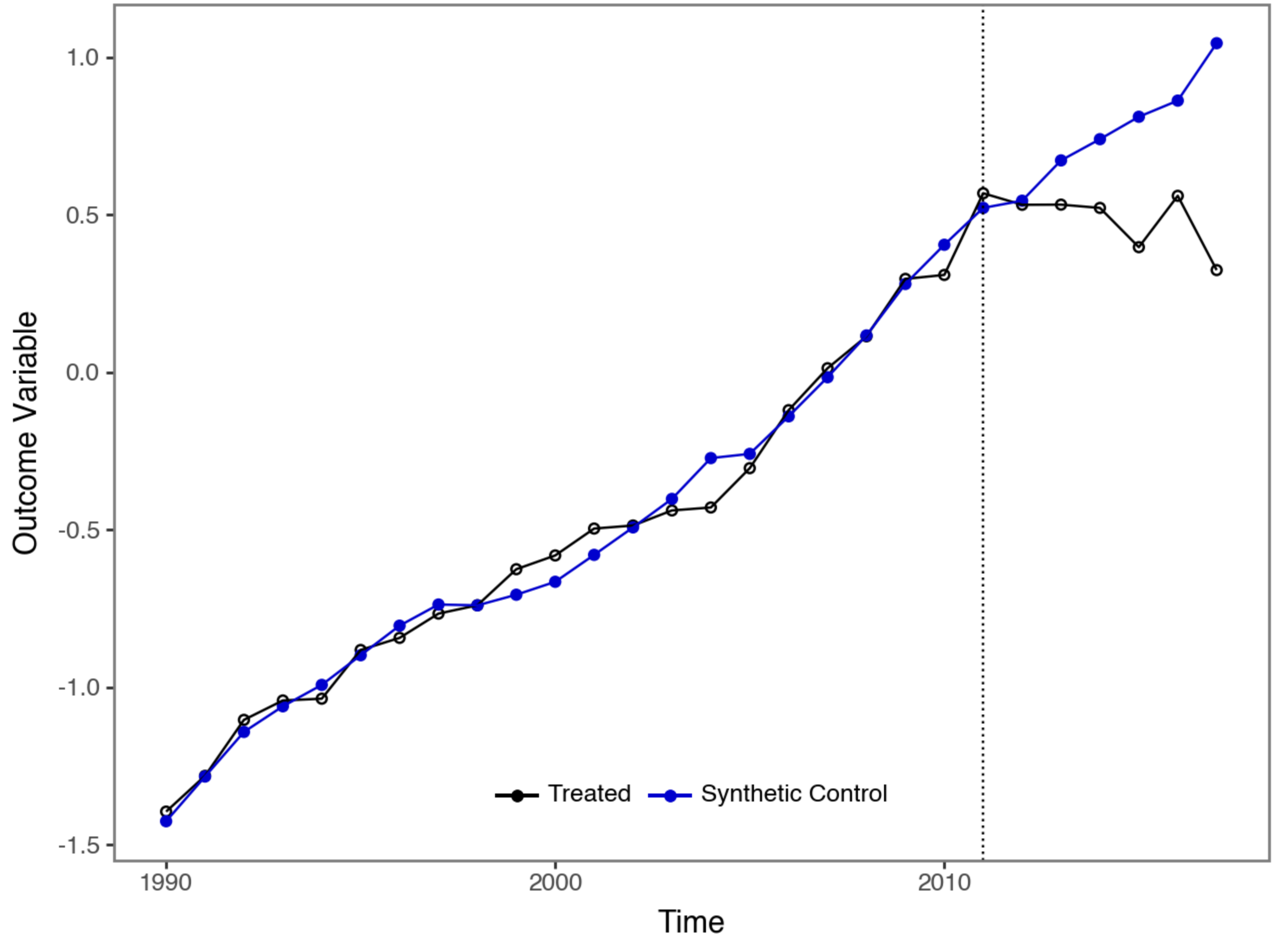
In [33]:
```
# Prepare data
df["wisconsin"] = (df["state"] == 'Wisconsin')
df["after_treatment"] = df["year"] > 2011

# Scale data since salary large values, otherwise scpi package can't calcluate
scaler = StandardScaler()
df["salary_scaled"] = scaler.fit_transform(df[["salary"]])
```

In [34]:
```
# Weights and estimates
scdf = scdata(df = df, id_var= "state", time_var="year", outcome_var="salary_scaled",
              period_pre=df.query("not after_treatment").year.unique(),
              period_post=df.query("after_treatment").year.unique(),
              unit_tr='Wisconsin',
              unit_co=df.query("not wisconsin").state.unique(),
              features=["salary_scaled"],
              cov_adj=None,
              cointegrated_data=True,
              constant=False)

est_si = scest(scdf, w_constr={'name': "simplex"})
print(est_si)
scplot(est_si)
```

```
-----------------------------------------------------------------------
Call: scest
Synthetic Control Estimation - Setup

Constraint Type:                                           simplex
Constraint Size (Q):                                             1
Treated Unit:                                            Wisconsin
Size of the donor pool:                                         23
Features                                                         1
Pre-treatment period                                     1990-2011
Pre-treatment periods used in estimation:                       22
Covariates used for adjustment:                                  0

Synthetic Control Estimation - Results

Active donors: 7

Coefficients:
                            Weights
Treated Unit Donor
Wisconsin    Alaska           0.181
             California       0.000
             Colorado         0.000
             Connecticut      0.000
             Delaware         0.000
             Hawaii           0.005
             Illinois         0.000
             Maine            0.565
             Maryland         0.000
             Massachusetts    0.000
             Minnesota        0.000
             Missouri         0.026
             Montana          0.000
             New Hampshire    0.000
             New Jersey       0.000
             New Mexico       0.000
             New York         0.078
             Ohio             0.000
             Oregon           0.000
             Pennsylvania     0.110
             Rhode Island     0.000
             Vermont          0.036
```

```
Washington       0.000
```

Out[34]:



Synthetic state donors

Donor state	Weight
Maine	0.565
Alaska	0.181
Pennsylvania	0.110

Donor state	Weight
New York	0.078
Vermont	0.036
Missouri	0.026
Hawaii	0.005

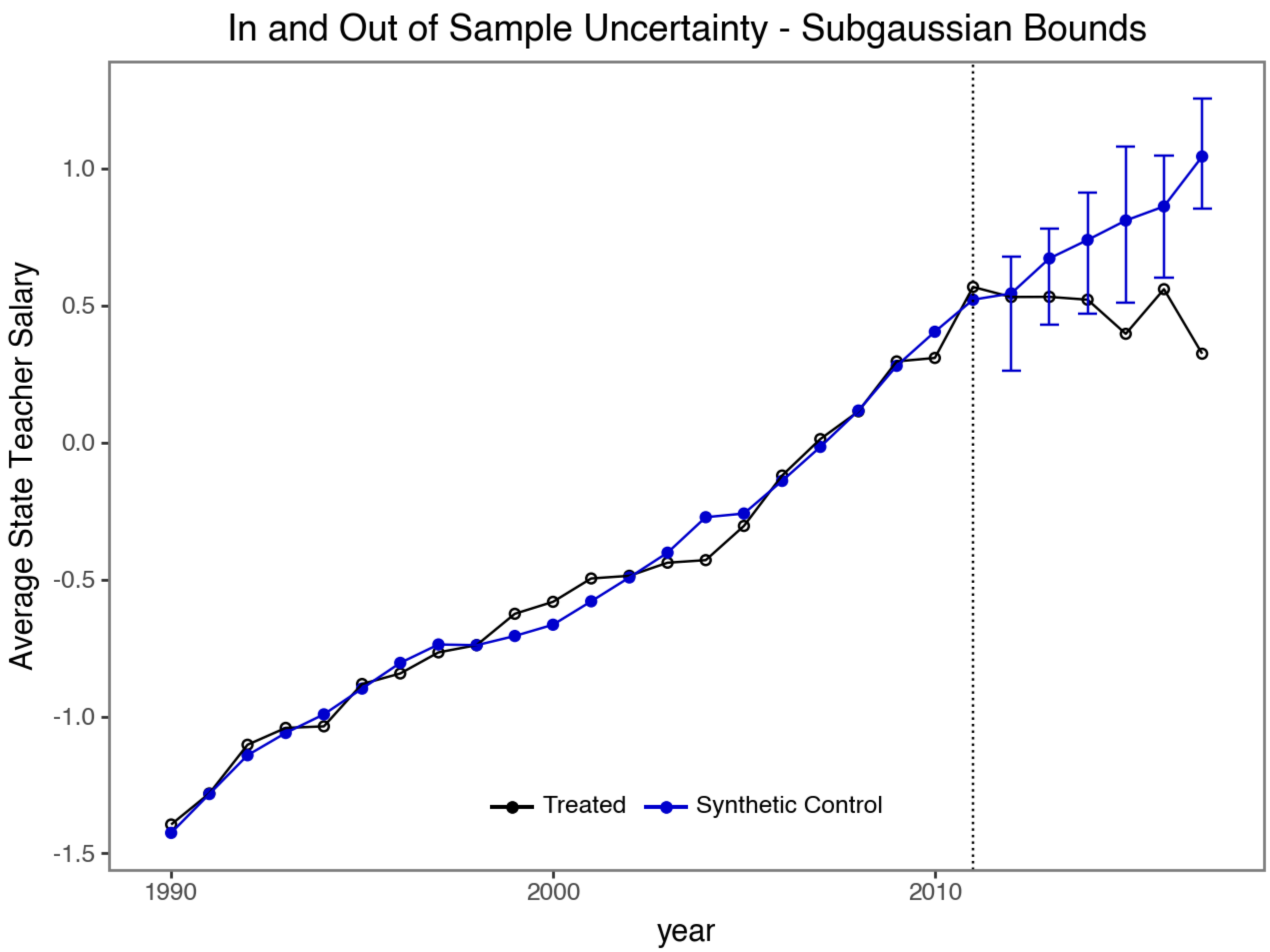
Wisconsin’s synthetic control is driven primarily by Maine (0.565) and Alaska (0.181), with additional weight on Pennsylvania (0.110) and New York (0.078). Because a large share of the weight is concentrated in one donor, the post-treatment gap should be interpreted as relying heavily on how closely Maine tracks Wisconsin absent treatment. Visually, the post-2011 divergence appears negative (Wisconsin below synthetic), consistent with a salary decline relative to the constructed counterfactual.

In [35]:
```
# Inference
w_constr = {'name': 'simplex', 'Q': 1}
u_missp = True
u_sigma = "HC1"
u_order = 1
u_lags = 0
e_method = "gaussian"
e_order = 1
e_lags = 0
e_alpha = 0.05
u_alpha = 0.05
sims = 200
cores = 1

random.seed(1)
result = scpi(scdf, sims=sims, w_constr=w_constr, u_order=u_order, u_lags=u_lags,
              e_order=e_order, e_lags=e_lags, e_method=e_method, u_missp=u_missp,
              u_sigma=u_sigma, cores=cores, e_alpha=e_alpha, u_alpha=u_alpha)
scplot(result, e_out=True, x_lab="year", y_lab="Average State Teacher Salary")
```

```
--------------------------------------------------
Estimating Weights...
Quantifying Uncertainty

terations completed (10%)
terations completed (20%)
terations completed (30%)
terations completed (40%)
iterations completed (50%)
iterations completed (60%)
iterations completed (70%)
iterations completed (80%)
iterations completed (90%)
200/200 iterations completed (100%)
```

```
/Users/rico/Documents/526-project/.venv/lib/python3.13/site-packages/plotnine/layer.py:374: PlotnineWarnin
g: geom_errorbar : Removed 22 rows containing missing values.
```

Out[35]:



In [36]:

```
# Extracting synthetic control estimates
sc = np.concatenate([est_si.Y_pre_fit, est_si.Y_post_fit]).reshape(-1, 1).flatten()

sc_original = scaler.inverse_transform(
    np.concatenate([est_si.Y_pre_fit, est_si.Y_post_fit]).reshape(-1, 1)).flatten()

synth_results = pd.DataFrame({
    'year': list(est_si.period_pre) + list(est_si.period_post),
    'synthetic_control': sc_original,
    'synthetic_control_scaled': sc})

df = pd.merge(df, synth_results, on = ['year'], how = 'left')
```

In [37]:
```
# Calculate treatment effects
df['te'] = df['salary'] - df['synthetic_control']

# Wisconsin results
df_te_wi = df[df['st'] == 'WI']
df_te_wi = df_te_wi[df_te_wi['after_treatment'] == True]
df_te_wi = df_te_wi[['year', 'state', 'salary', 'synthetic_control', 'te']]

# Calculate percentage change (between synthetic salary and actual salary)
df_te_wi["pct_change"] = df_te_wi["te"] / df_te_wi["synthetic_control"] * 100
```

In [38]:
```
# Extract confidence intervals
ci_df = (
    result.CI_all_gaussian[["Lower", "Upper"]]
    .reset_index()
    .rename(columns={"Time": "year"})
)

ci_df["ci_lower"] = scaler.inverse_transform(ci_df[["Lower"]]).ravel()
ci_df["ci_upper"] = scaler.inverse_transform(ci_df[["Upper"]]).ravel()

df_te_wi["year"] = df_te_wi["year"].astype(int)
ci_df["year"] = ci_df["year"].astype(int)

df_te_wi = df_te_wi.merge(
    ci_df[["year", "ci_lower", "ci_upper"]],
    on="year",
    how="left"
)

# Indicator for whether confidence interval overlaps with actual salary (i.e., insignificant effect)
df_te_wi["int_overlap"] = df_te_wi["salary"].between(df_te_wi["ci_lower"], df_te_wi["ci_upper"])

df_te_wi
```

Out[38]:

	year	state	salary	synthetic_control	te	pct_change	ci_lower	ci_upper	int_overlap
0	2012	Wisconsin	53792	53938.546452	-146.546452	-0.271692	50728.487244	55472.770841	True
1	2013	Wisconsin	53797	55391.564265	-1594.564265	-2.878713	52663.995670	56645.068684	True
2	2014	Wisconsin	53679	56156.723469	-2477.723469	-4.412158	53089.928294	58131.936438	True
3	2015	Wisconsin	52264	56964.046714	-4700.046714	-8.250900	53563.328251	60022.234924	False
4	2016	Wisconsin	54115	57546.301681	-3431.301681	-5.962680	54593.237161	59637.172806	False
5	2017	Wisconsin	51439	59615.684558	-8176.684558	-13.715660	57472.103053	62026.081372	False

Using uncertainty bands, we interpret statistical significance by checking whether the post-treatment confidence interval for the Wisconsin synthetic gap excludes zero. Visually, the post-Act 10 gap is not clearly distinguishable from zero in the first few post-treatment years, but it becomes clearly negative in the later period, most notably in 2015-2017 where the interval no longer overlaps zero, suggesting a statistically meaningful decline in teacher salaries relative to the counterfactual. Consistent with this, our point estimates imply average salary shortfalls of about $4,700(2015),$3,431 (2016), and $8,177 (2017) compared with the synthetic control, which correspond to roughly 8.3%, 6.0%, and 13.7% below the counterfactual salary level (about a 9% average decline over 2015-2017). Our estimated 9% decline (in the later post-treatment years) is very close to Litten's (2017) finding of about an 8% reduction in total teacher compensation following the Act 10-era weakening of union bargaining power. It is also consistent with Baron's (2018) evidence that Act 10 was associated with a 4% reduction in teacher salaries, which matches the direction of our synthetic-control gap.

Distribution of effects

To assess whether Wisconsin's estimated synthetic-control effect is substantially large, we conduct a placebo exercise: we re-estimate synthetic control effects for each donor state as if it were treated in the same year, generating a reference distribution of "placebo effects" that would arise even in the absence of treatment. We then compare Wisconsin's post-treatment effect to this placebo distribution. Following the textbook's randomization-inference logic (Facure), the reported proportion is interpreted as a one-sided p-value: it is the share of placebo effects that are at least as extreme as Wisconsin's effect (e.g., more negative if we test for a wage decline). A small proportion implies Wisconsin's effect is rare relative to what we typically see under placebo assignments.

In [39]:
```
# Used ChatGPT to adapt my previous code into a function and loop
actual_treated = "Wisconsin"
all_states = df["state"].unique()
control_states = [s for s in all_states if s != actual_treated]

def run_one_scpi(df, treated_state, donor_states, scaler, seed=1):
    # build scdata
    scdf = scdata(df=df, id_var="state", time_var="year", outcome_var="salary_scaled",
        period_pre=df.loc[~df["after_treatment"], "year"].unique(),
        period_post=df.loc[df["after_treatment"], "year"].unique(),
        unit_tr=treated_state,
        unit_co=np.array(donor_states),
        features=["salary_scaled"],
        cov_adj=None,
        cointegrated_data=True,
        constant=False
    )
    est_si = scest(scdf, w_constr={"name": "simplex"})

    random.seed(seed)
    result = scpi(
        scdf,
        w_constr={"name": "simplex", "Q": 1},
        u_order=1, u_lags=0,
        e_order=1, e_lags=0,
        e_method="gaussian",
        u_missp=True,
        u_sigma="HC1",
        cores=1,
        e_alpha=0.05,
        u_alpha=0.05
    )

    sc_scaled = np.concatenate([est_si.Y_pre_fit, est_si.Y_post_fit]).reshape(-1, 1)
    sc_orig = scaler.inverse_transform(sc_scaled).ravel()

    years = list(est_si.period_pre) + list(est_si.period_post)

    out = pd.DataFrame({
        "state": treated_state,
        "year": years,
```

```
        "synthetic_control": sc_orig,
        "synthetic_control_scaled": sc_scaled.ravel()
    })

    y = df.loc[df["state"] == treated_state, ["year", "salary"]].copy()
    out = out.merge(y, on="year", how="left")
    out["te"] = out["salary"] - out["synthetic_control"]

    return out

# Loop for all control states, excluding Wisconsin
placebo_rows = []
for pseudo in control_states:
    donors = [s for s in control_states if s != pseudo]
    try:
        placebo_rows.append(run_one_scpi(df, pseudo, donors, scaler, seed=1))
    except Exception as e:
        print(f"Skipping {pseudo} (failed): {e}")

placebos = pd.concat(placebo_rows, ignore_index=True)
placebos_post = placebos[placebos["year"] > 2011]
```

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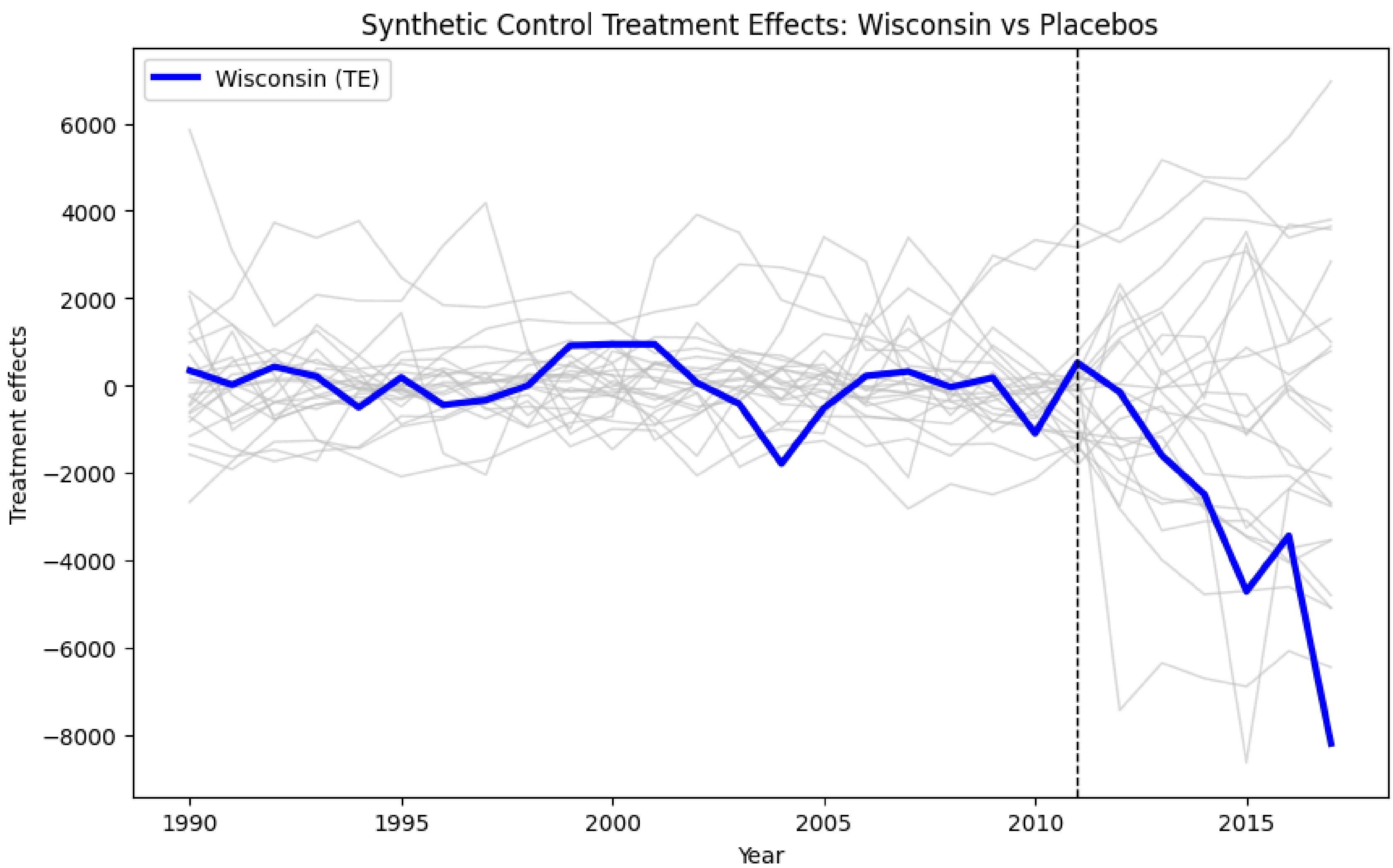
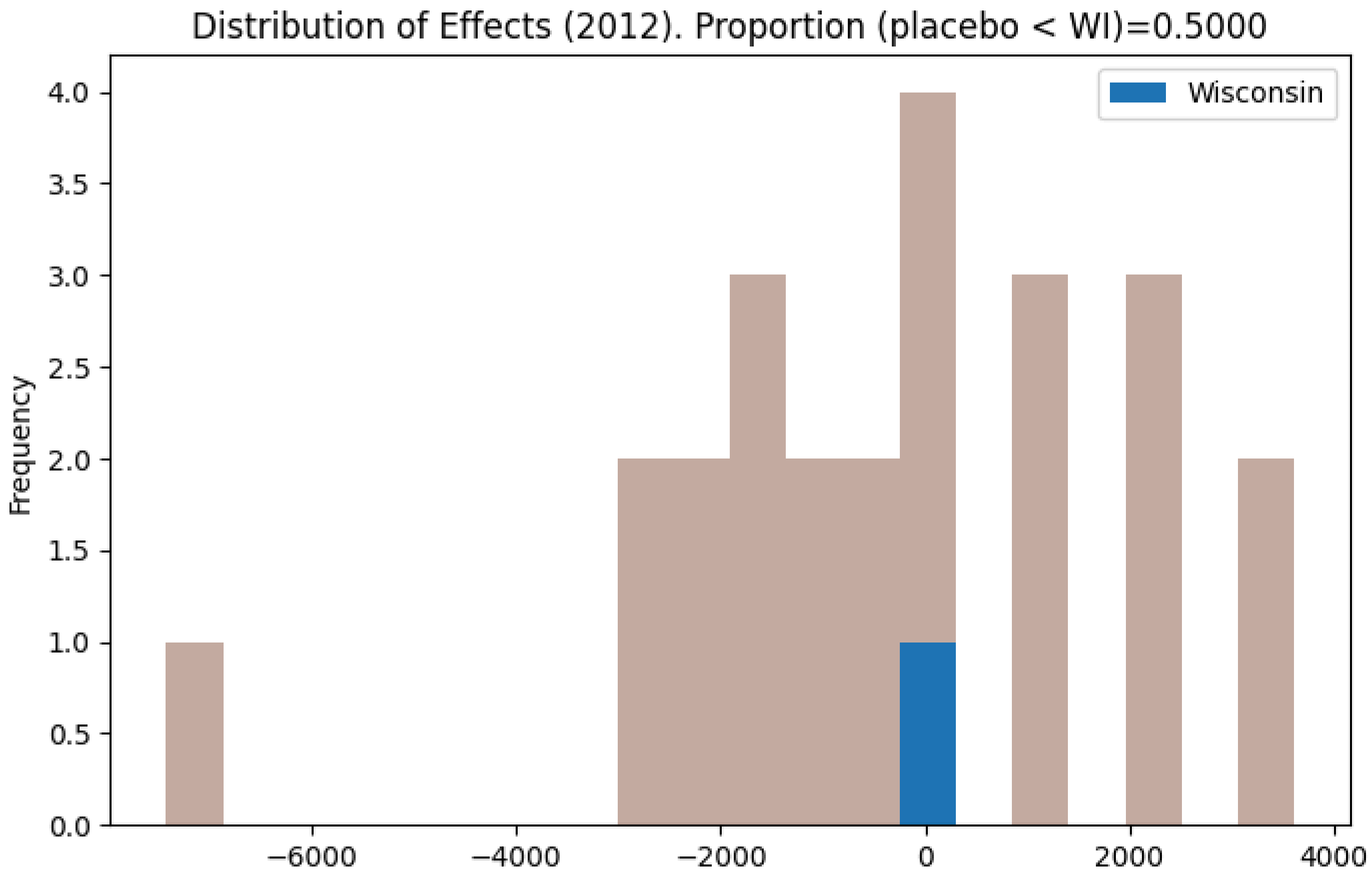
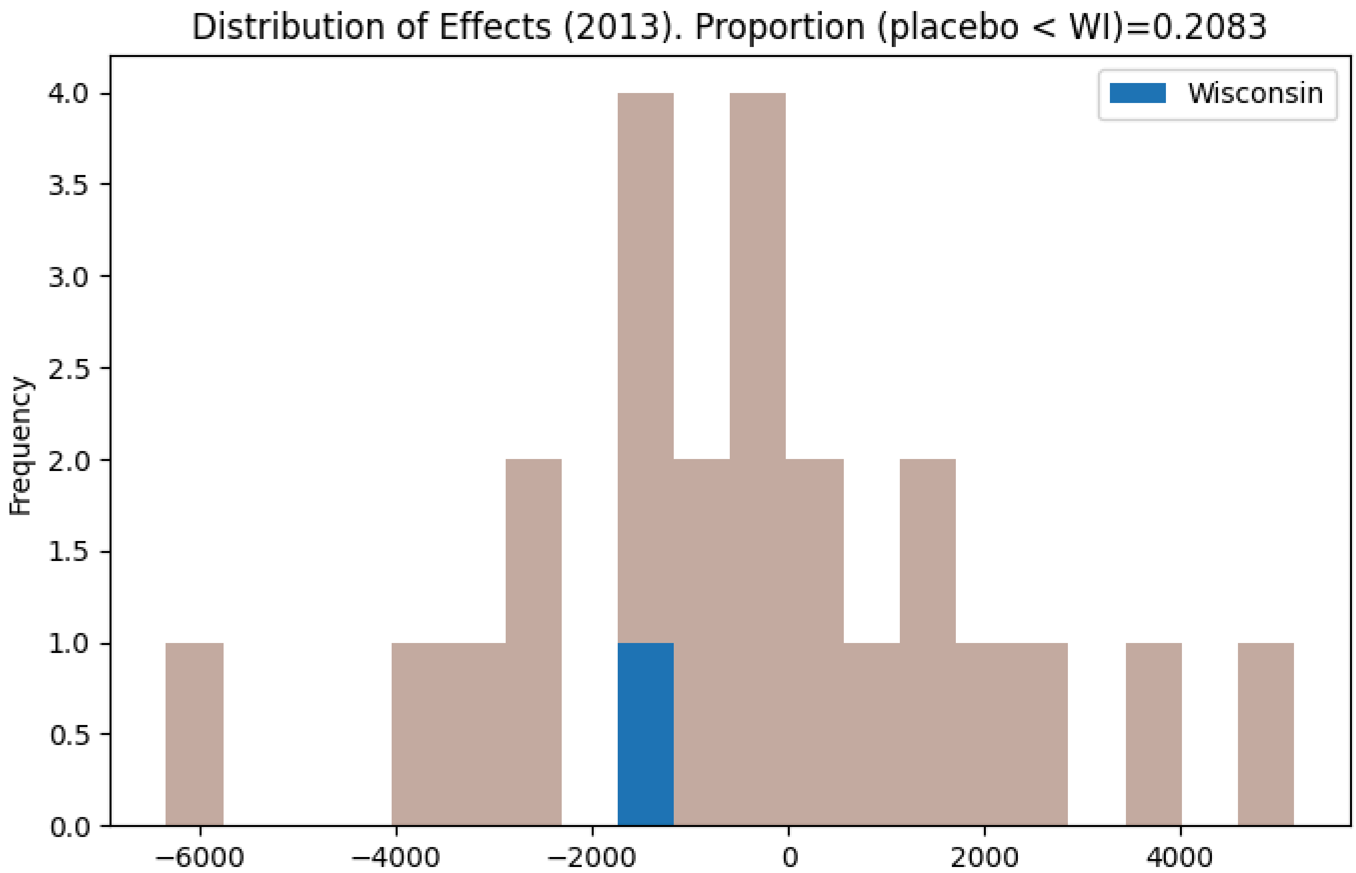
In [40]:
```
wi_filter = df[df["state"]=="Wisconsin"][["state", "year", "synthetic_control", "synthetic_control_scaled"
wi_filter

df_treat_placebo = pd.concat(
    [placebos[["state", "year", "te"]], wi_filter[["state", "year", "te"]]]
)

fig, ax = plt.subplots(figsize=(10, 6))

# Placebo lines
for s, g in df_treat_placebo[df_treat_placebo["state"] != "Wisconsin"].groupby("state"):
    ax.plot(g["year"], g["te"], color="0.75", linewidth=1, alpha=0.6)

# Wisconsin line
g_wi = df_treat_placebo[df_treat_placebo["state"] == "Wisconsin"].sort_values("year")
ax.plot(g_wi["year"], g_wi["te"], color="blue", linewidth=3, label="Wisconsin (TE)")

ax.axvline(2011, color="black", linestyle="--", linewidth=1)
ax.set_title("Synthetic Control Treatment Effects: Wisconsin vs Placebos")
ax.set_xlabel("Year")
ax.set_ylabel("Treatment effects")
ax.legend()
plt.show()
```

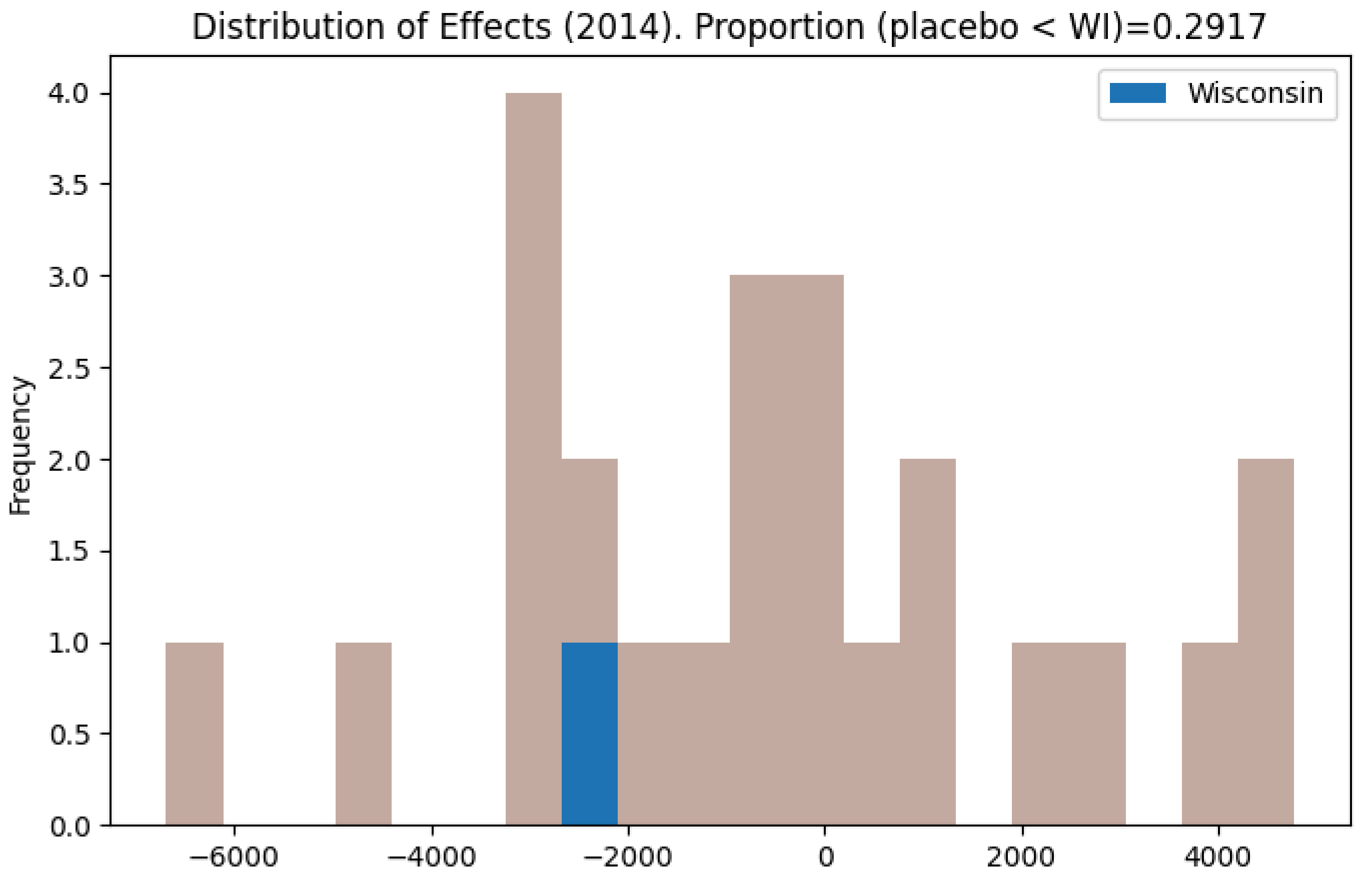


In [41]:
```
for year in range(2012, 2018):
    effects = df_treat_placebo.loc[df_treat_placebo["year"] == year, "te"].to_numpy()
    wi_effect  = df_te_wi.loc[df_te_wi["year"] == year, "te"].iloc[0]

    p_less = np.mean(effects < wi_effect)

    plt.figure(figsize=(8,5))
    _, bins, _ = plt.hist(effects, bins=20, color="C5", alpha=0.5)
    plt.hist([wi_effect], bins=bins, label="Wisconsin")
```

```
    plt.ylabel("Frequency")
    plt.title(f"Distribution of Effects ({year}). Proportion (placebo < WI)={p_less:.4f}")
    plt.legend()
    plt.show()
```




Distribution of Effects (2013). Proportion (placebo < WI)=0.2083
Wisconsin
Frequency
4.0
3.5
3.0
2.5
2.0
1.5
1.0
0.5
0.0
−6000
−4000
−2000
0
2000
4000


Distribution of Effects (2014). Proportion (placebo < WI)=0.2917
Wisconsin
Frequency
4.0
3.5
3.0
2.5
2.0
1.5
1.0
0.5
0.0
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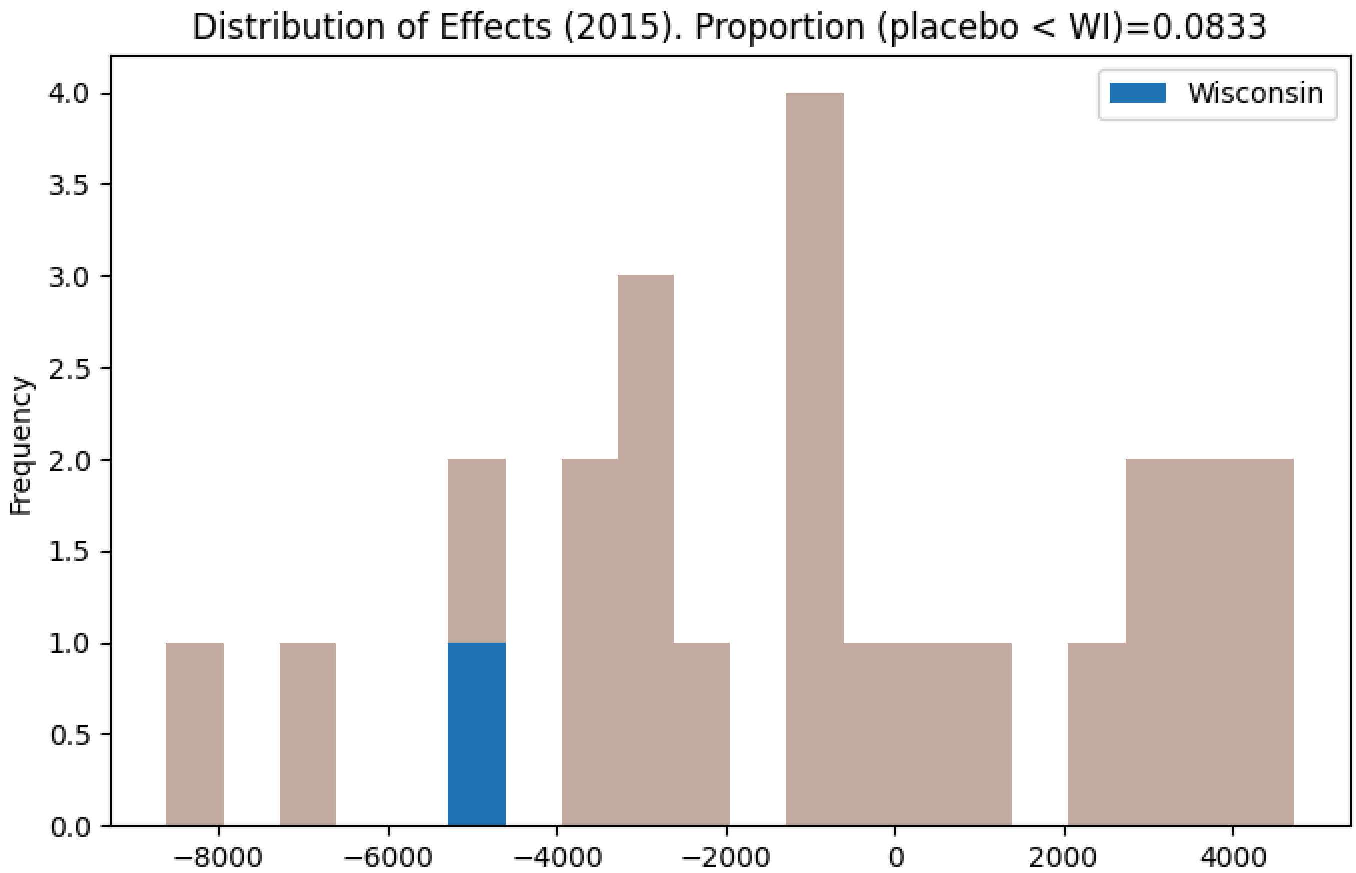
Distribution of Effects (2015). Proportion (placebo < WI)=0.0833
Wisconsin
Frequency
4.0
3.5
3.0
2.5
2.0
1.5
1.0
0.5
0.0
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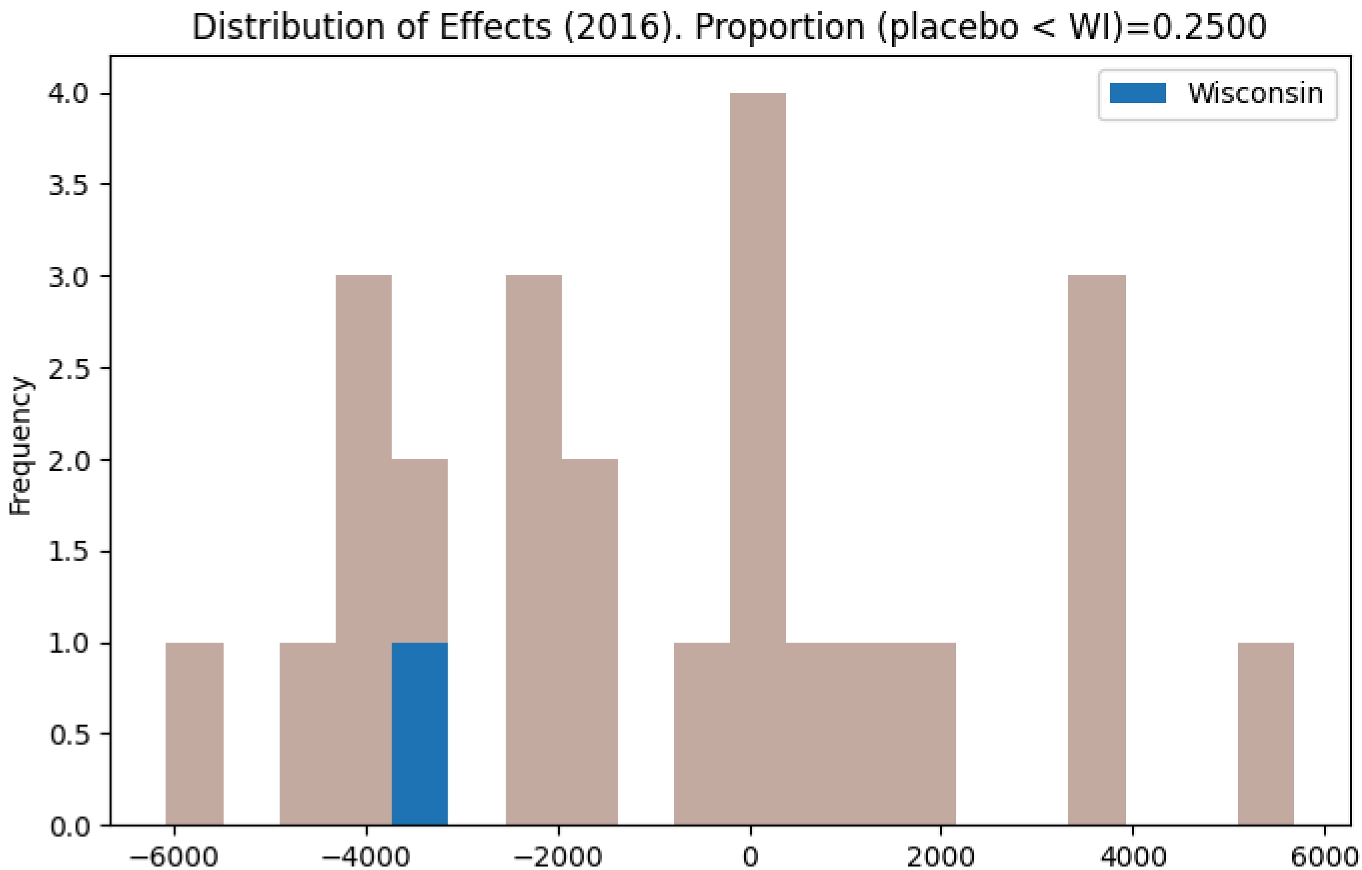
Distribution of Effects (2016). Proportion (placebo < WI)=0.2500
Wisconsin
Frequency
4.0
3.5
3.0
2.5
2.0
1.5
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0.0
−6000
−4000
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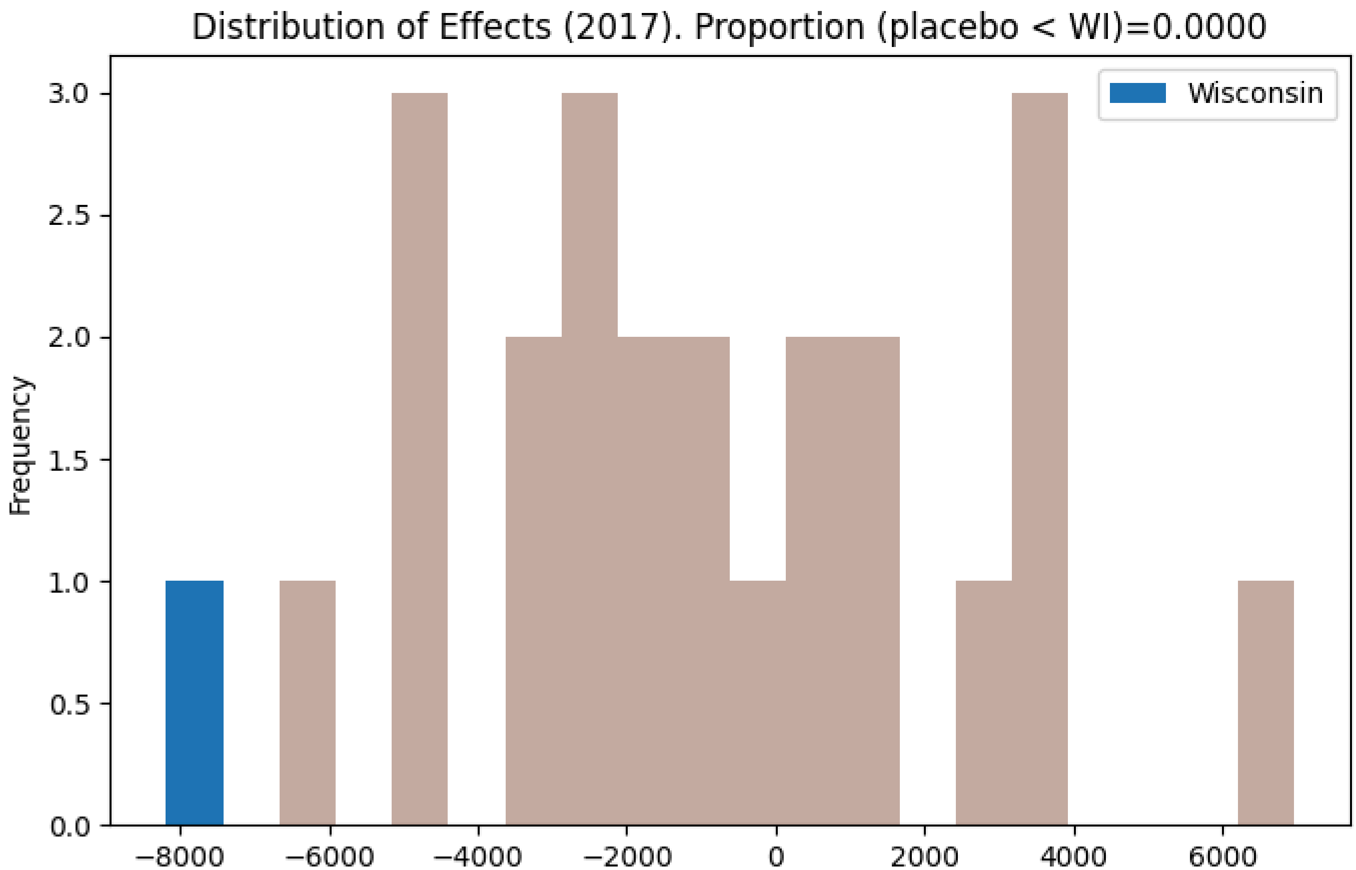


In 2015, only three placebo states exhibit an effect more negative than Wisconsin's, suggesting Wisconsin is already near the lower tail of the placebo distribution. In 2016, six placebo states are more negative than Wisconsin, indicating Wisconsin remains relatively extreme but not unique. By 2017, Wisconsin appears to be the most extreme (most negative) effect in the distribution, which corresponds to an extremely small placebo proportion i.e., an effect that is difficult to attribute to chance divergence alone.

Robustness

For a robustness check, we repeat the analysis using Michigan as an alternative treated unit. Michigan adopted a right-to-work (RTW) law in 2013 that plausibly weakened public-sector union resources by restricting the collection of agency fees. This exercise is not meant to imply that Michigan's reform is identical to Wisconsin's Act 10. Act 10 was a broader and more stringent overhaul that curtailed collective bargaining rights and imposed multiple constraints on public-sector unions, whereas RTW policy is narrower and primarily targets the financing channel by limiting mandatory fee collection. Because the Michigan policy shock is less comprehensive than Act 10, we would not expect identical magnitudes. Instead, this robustness check is aimed at assessing whether we observe a qualitatively similar pattern, in particular, whether teacher compensation tends to fall relative to a synthetic counterfactual following a policy that weakens unions. If the estimated effect for Michigan points in the same (negative) direction, it supports the interpretation that reductions in union bargaining power are associated with lower teacher pay, while differences in magnitude are consistent with the fact that the Michigan policy change was less severe than Wisconsin's Act 10.

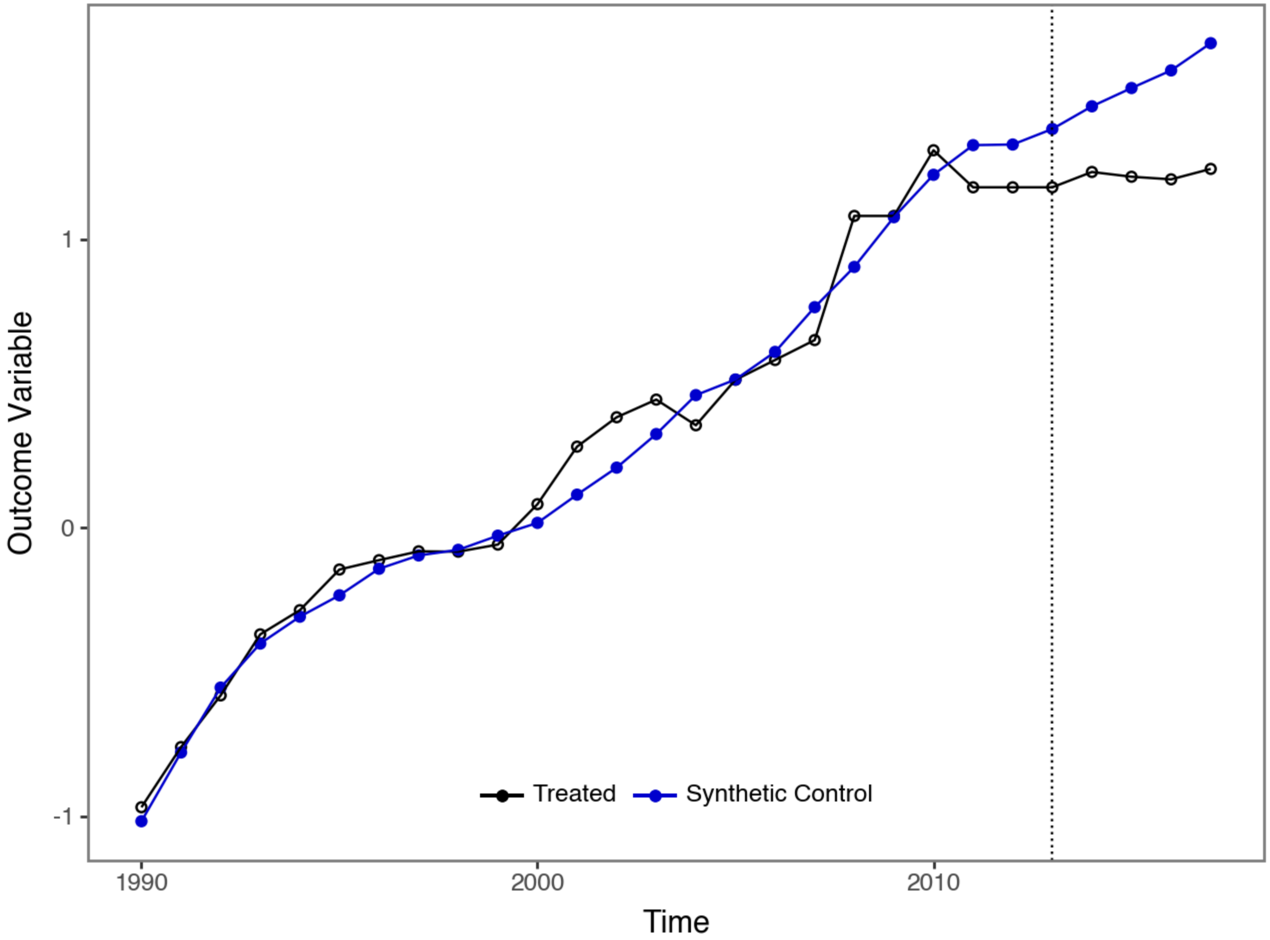
In [42]:
```
df = df_merged[~((df_merged['st'].isin(states_with_fees_prohibited)) & (df_merged['st'] != 'MI'))]
df["michigan"] = (df["st"] == 'MI')
df["after_treatment"] = df["year"] > 2013
```

In [43]:
```
df["salary_scaled"] = scaler.fit_transform(df[["salary"]])

scdf = scdata(df = df, id_var= "state", time_var="year", outcome_var="salary_scaled",
              period_pre=df.query("not after_treatment").year.unique(),
              period_post=df.query("after_treatment").year.unique(),
              unit_tr='Michigan',
              unit_co=df.query("not michigan").state.unique(),
              features=["salary_scaled"],
              cov_adj=None,
              cointegrated_data=True,
              constant=False)

est_si = scest(scdf, w_constr={'name': "simplex"})
print(est_si)
scplot(est_si)
```

```
-----------------------------------------------------------------------
Call: scest
Synthetic Control Estimation - Setup

Constraint Type:                                                simplex
Constraint Size (Q):                                                  1
Treated Unit:                                                  Michigan
Size of the donor pool:                                              23
Features                                                              1
Pre-treatment period                                          1990-2013
Pre-treatment periods used in estimation:                            24
Covariates used for adjustment:                                       0

Synthetic Control Estimation - Results

Active donors: 5

Coefficients:
                            Weights
Treated Unit Donor
Michigan     Alaska           0.081
             California       0.000
             Colorado         0.000
             Connecticut      0.403
             Delaware         0.000
             Hawaii           0.000
             Illinois         0.045
             Maine            0.000
             Maryland         0.000
             Massachusetts    0.000
             Minnesota        0.000
             Missouri         0.000
             Montana          0.000
             New Hampshire    0.000
             New Jersey       0.000
             New Mexico       0.000
             New York         0.000
             Ohio             0.000
             Oregon           0.000
             Pennsylvania     0.309
             Rhode Island     0.000
             Vermont          0.000
```

```
Washington        0.162
```

Out[43]:



In [44]:

```python
from scpi_pkg.scpi import scpi

w_constr = {'name': 'simplex', 'Q': 1}
u_missp = True
u_sigma = "HC1"
u_order = 1
u_lags = 0
e_method = "gaussian"
e_order = 1
e_lags = 0
e_alpha = 0.05
```

```python
u_alpha = 0.05
sims = 200
cores = 1

random.seed(1)
result = scpi(scdf, sims=sims, w_constr=w_constr, u_order=u_order, u_lags=u_lags,
              e_order=e_order, e_lags=e_lags, e_method=e_method, u_missp=u_missp,
              u_sigma=u_sigma, cores=cores, e_alpha=e_alpha, u_alpha=u_alpha)
scplot(result, e_out=True, x_lab="year", y_lab="Average State Teacher Salary")
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Estimating Weights...
Quantifying Uncertainty

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g: geom_errorbar : Removed 24 rows containing missing values.
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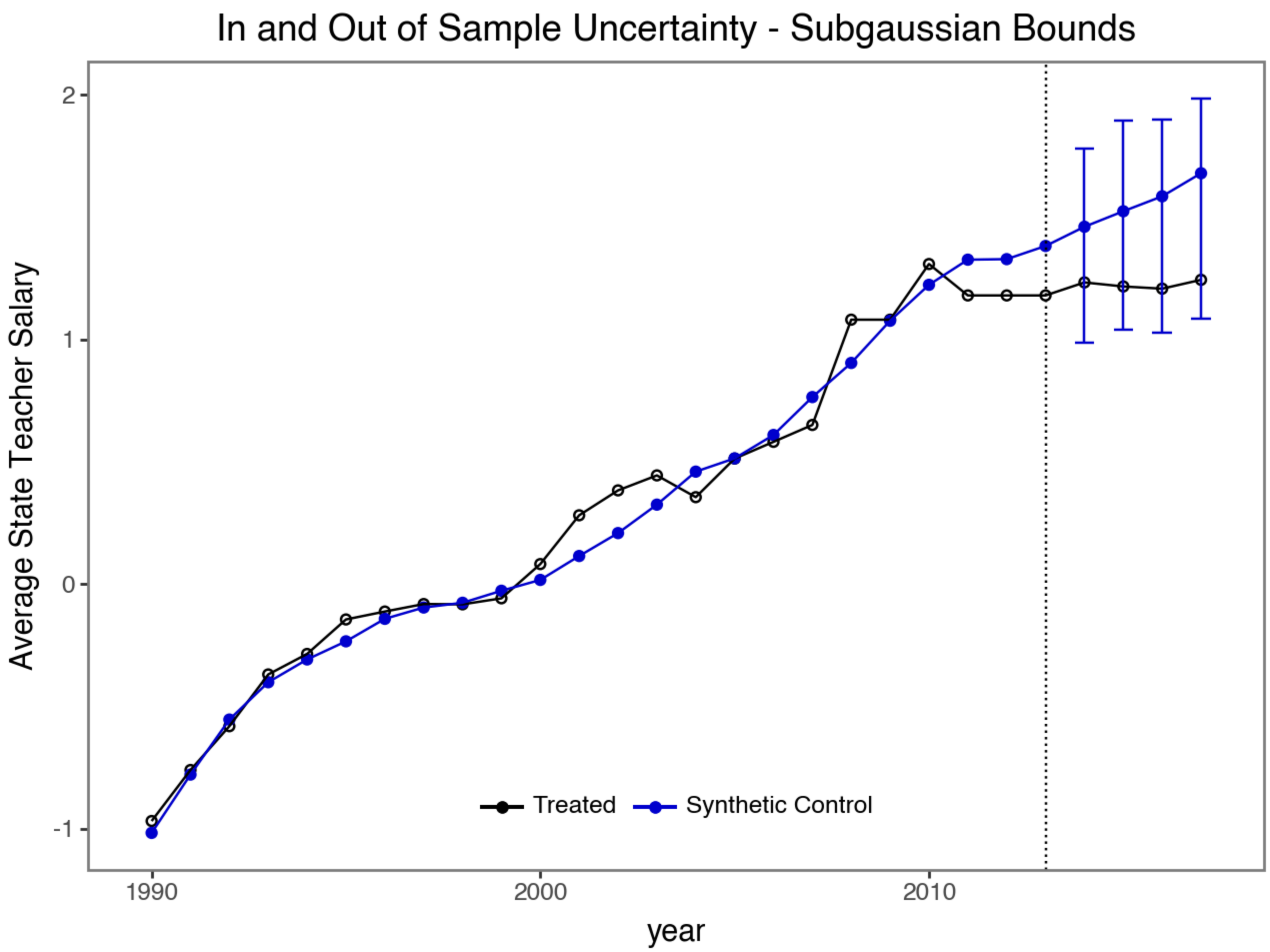
Out[44]:



In [45]:

```
sc = np.concatenate([est_si.Y_pre_fit, est_si.Y_post_fit]).reshape(-1, 1).flatten()

sc_original = scaler.inverse_transform(
    np.concatenate([est_si.Y_pre_fit, est_si.Y_post_fit]).reshape(-1, 1)).flatten()

synth_results = pd.DataFrame({
    'year': list(est_si.period_pre) + list(est_si.period_post),
    'synthetic_control': sc_original,
    'synthetic_control_scaled': sc})

df = pd.merge(df, synth_results, on = ['year'], how = 'left')
```

In [46]:
```
# Calculate treatment effects
df['te'] = df['salary'] - df['synthetic_control']

df_te_mi = df[df['st'] == 'MI']
df_te_mi = df_te_mi[df_te_mi['after_treatment'] == True]
df_te_mi = df_te_mi[['year', 'state', 'salary', 'synthetic_control', 'te']]
df_te_mi

df_te_wi["pct_change"] = df_te_wi["te"] / df_te_wi["synthetic_control"] * 100

# Extract confidence intervals
ci_df = (
    result.CI_all_gaussian[["Lower", "Upper"]]
    .reset_index()
    .rename(columns={"Time": "year"})
)

ci_df["ci_lower"] = scaler.inverse_transform(ci_df[["Lower"]]).ravel()
ci_df["ci_upper"] = scaler.inverse_transform(ci_df[["Upper"]]).ravel()

df_te_mi["year"] = df_te_mi["year"].astype(int)
ci_df["year"] = ci_df["year"].astype(int)

df_te_mi = df_te_mi.merge(
    ci_df[["year", "ci_lower", "ci_upper"]],
    on="year",
    how="left"
)

df_te_mi["int_overlap"] = df_te_mi["salary"].between(df_te_mi["ci_lower"], df_te_mi["ci_upper"])

df_te_mi
```

Out[46]:

	year	state	salary	synthetic_control	te	ci_lower	ci_upper	int_overlap
0	2014	Michigan	62166	64759.357784	-2593.357784	59353.632153	68408.924638	True
1	2015	Michigan	61978	65481.497288	-3503.497288	59958.136319	69725.219246	True
2	2016	Michigan	61875	66178.210357	-4303.210357	59813.795254	69740.679031	True
3	2017	Michigan	62287	67252.473112	-4965.473112	60455.724438	70741.231465	True

In our robustness check, we apply the same synthetic-control framework to Michigan, treating the 2013 right-to-work change as the intervention and constructing a synthetic Michigan from the donor pool (donor pool size 23; pre-treatment period 1990–2013) to approximate Michigan's pre-policy salary trajectory as closely as possible. The resulting post-treatment comparisons show a consistently negative and increasingly large gap between observed Michigan salaries and the synthetic counterfactual: Michigan's salary is below the synthetic path by roughly $2,593 in 2014$,3,503 in 2015, $4,303 in 2016, and$ 4,965 in 2017, indicating that the divergence is not a one-time fluctuation but instead accumulates over time.

This pattern aligns with what we would expect if weakening union financing and bargaining leverage puts downward pressure on teacher compensation, and it strengthens our main interpretation by showing that a similar union-weakening reform in another state generates effects in the same direction when evaluated with the same methodology. At the same time, because Michigan's RTW policy is narrower than Wisconsin's Act 10, we treat this as a directional robustness check i.e., it is evidence that the negative salary effects are not unique to Wisconsin, rather than as a claim that the magnitudes should match across the two reforms.

Conclusion

We studied whether Wisconsin's 2011 Act 10, an institutional change that substantially weakened public-sector unions, reduced teacher salaries. To motivate our design, we first estimated a transparent differences-in-differences comparison between Wisconsin and Minnesota, where shared regional conditions and similar pre-2011 patterns make the parallel-trends assumption more plausible than in a broad multi-state setting. We also explored an all-states DID as a descriptive benchmark but treat it cautiously because heterogeneity across states makes a common parallel-trends assumption much harder to defend.

Our main analysis relies on synthetic control. We constructed a "synthetic Wisconsin" as a weighted combination of donor states chosen to match Wisconsin's pre-2011 salary trajectory as closely as possible, then compared Wisconsin to this counterfactual after 2011. The results point to a sustained negative divergence following Act 10, with the clearest separation in the later post-treatment years (especially 2015-2017). Interpreted in relative terms, the gaps in those years imply a significant decline of about 9% in average teacher salary, suggesting that the policy’s effects accumulated over time rather than a one-time shock.

To assess whether Wisconsin’s estimated effect is significantly large, we implemented placebo tests that reassign the treatment to each donor state and recompute synthetic-control gaps. Wisconsin’s post-treatment gap lies in the extreme tail of this placebo distribution in the later years, strengthening the interpretation that the observed divergence is not typical of what we see when applying the same method to untreated states. Finally, as an external robustness check, we repeated the synthetic-control exercise for Michigan around its 2013 right-to-work change. Because Michigan’s reform is narrower and less sweeping than Act 10, we do not expect identical magnitudes. Nonetheless, the estimated post-period gaps move in the same (negative) direction, providing additional support that union-weakening reforms are associated with lower teacher salaries relative to a constructed counterfactual.

In conclusion, the evidence is most consistent with Act 10 being followed by meaningfully lower average teacher salaries in Wisconsin relative to what would have been expected under comparable pre-policy trends. At the same time, our conclusions should be interpreted with appropriate caution: causal interpretation depends on the quality of the pre-treatment fit and the validity of the donor pool, and broader institutional changes after our sample window could affect longer-run dynamics beyond what we evaluate here.

Weaknesses/Further studies

A key limitation of our analysis is that the synthetic control is constructed primarily to match pre-treatment salary trends, but we do not match on a broader set of covariates (e.g., school finance variables, district composition, local labor-market conditions, or teacher workforce characteristics) that could also shape salary dynamics. Further studies could strengthen identification by incorporating additional predictors into the synthetic-control matching step and by using more disaggregated data (for example, by district, experience level, or salary percentile) to examine whether the effects are heterogeneous across higher- and lower-earning teachers. In addition, our data do not allow us to separately identify impacts on unionized versus non-unionized teachers, which matters because union-weakening policies may affect wages both directly through bargaining and indirectly through spillovers to nonunion labor markets, as emphasized by Green, Sand, and Snoddy (2022). Finally, as we focus on public-school teachers, extending the analysis to include private-school teacher compensation would help clarify whether the policy operates mainly through public-sector bargaining institutions or whether broader labor-market adjustments also affect teacher pay outside the public system.

References

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