Helping out the outsourced: A pre-registered evaluation of the New Jersey temporary worker bill of rights*

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Abstract

We pre-register an observational study of New Jersey's New Jersey's Temporary Workers' Bill of Rights, which requires that temporary workers are paid the same as their full-time counterparts. Because the data for studying the effects is currently unavailable, this presents an opportunity for a pre-registered synthetic control design. Typically, observational studies contain hidden analytical decisions, potentially affecting the reliability of the estimates. A pre-registered synthetic control can address this issue by pinning down researcher decisions before the event takes places. This document establishes the exact data, code, outcomes, time periods, inference procedure, and robustness checks that we will use to study the reform. When the final period of data is available, we will run the code and report the results.

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1 Introduction

In this document, we pre-register an observational study of New Jersey's Temporary Workers' Bill of Rights, signed in February 2023, which mandates that temporary workers earn the same wages as full-time employees performing the same job (NJ Governor's Office, 2023). At the time of this pre-registration, the data for studying the effects of the law have not been released. This presents an opportunity for a **pre-registered synthetic control** (Abadie, 2021). Below, we will specify the synthetic control procedure, outcomes, time horizon, and all key features of the analysis. Once the specified data is available, we will perform the analysis as planned.

2 Background

On February 2nd, 2023, New Jersey Governor Phil Murphy signed A1474/S511, known as the "Temporary Workers' Bill of Rights" (NJ Governor's Office, 2023). The bill applies to temporary help service firms[1] that staff "designated classification placements."[2] The most significant provision pertains to wages, and went into effect August 5, 2023. Under the law, covered temporary workers need to be paid the same as full-time employees performing the same job, including the cost any benefits provided.[3]

The new law is meant to address wage disparities between full-time employees and contractors. Domestic outsourcing has increased over time (Weil, 2014; Segal and Sullivan, 1997; Handwerker, 2023), and outsourced workers make less. Studying German data, Goldschmidt and Schmieder (2017)

[1] This is defined as

> any person or entity who operates a business which consists of employing individuals directly or indirectly for the purpose of assigning the employed individuals to assist the firm's customers in the handling of the customers' temporary, excess or special workloads, and who, in addition to the payment of wages or salaries to the employed individuals, pays federal social security taxes and State and federal unemployment insurance; carries workers' compensation insurance as required by State law; and sustains responsibility for the actions of the employed individuals while they render services to the firm's customers. (NJ State Legislature, 2023)

[2] The full list of occupations is: Other Protective Service Workers; Food Preparation and Serving Related Occupations; Building and Grounds Cleaning and Maintenance Occupations; Personal Care and Service Occupations; Construction Laborers; Helpers, Construction Trades; Installation, Maintenance, and Repair Occupations; Production Occupations; Transportation and Material Moving Occupations.

[3] The wage requirement, as reported by State of New Jersey (2023), is:

> Covered temporary workers cannot be paid less than the average rate of pay and average cost of benefits, or the cash equivalent thereof, of employees of the third-party client performing the same or substantially similar work on jobs where the performance requires equal skill, effort, and responsibility, and which are performed under similar working conditions.

show that outsourced workers get paid 10-15% less than their counterparts performing similar jobs in the same firm. In the US, Dube and Kaplan (2010) find similar wage penalties using survey data. Song et al. (2019) argue that the rise of outsourcing likely explains a large share of increasing wage inequality in the US.

The new law sets a minimum wage for temporary workers. This should increase the wages paid to temporary workers. However, employers of temporary workers face both higher wage bills and compliance costs (e.g., Horner, 2023). This may decrease the demand for temporary workers, and reduce employment in the temporary help industry. Our study will investigate the effect of the bill on average weekly wages and employment in the temporary help industry.

3 Pre-analysis plan

We will perform a synthetic control analysis studying the causal effect of the Temporary Workers' Bill of Rights on employment and wages in New Jersey's temporary help industry. Below, we pre-register the key features of our design.

3.1 Data, sample, unit of observation, and length of follow-up

We use state-level data from the Quarterly Census of Employment and Wages ("QCEW NAICS-Based Data Files") (US BLS, 2023). This provides, at the state-by-industry level, monthly employment and quarterly average weekly wages. While the data go back to 1975, the industry classification system switched from SIC to NAICS in 2001. This introduced some transcription errors creating spurious spikes in employment (Chodorow-Reich and Wieland, 2020, Appendix F). To avoid this, we will restrict our analysis to 2001 and after. We show below that the synthetic control design attains a good fit over this duration and that we are powered to detect reasonable effect sizes.

We restrict to privately-owned firms in the Temporary Help Services industry (5-digit NAICS code: 56132) based on the coverage of the law. Our primary data set will be a state-by-quarter panel of employment and average weekly earnings in this industry from 2001 to the most current data. Employment is measured monthly; earnings only quarterly. The panel covers the 50 states and Washington DC; we will exclude Puerto Rico and the Virgin Islands. Table 1 shows descriptives for all variables used in the analysis.

The law was passed on February 6, 2023. It specified that the wage requirements would go into

effect on August 5, 2023. The New Jersey Department of Labor and Workforce Development published proposed regulations implementing the law on July 21, 2023. Although New Jersey law allows for a 60-day comment period before the regulations become final, the state stated that it intended to enforce the law in accordance with the proposed regulations.

We will assume that the law takes effect in August 2023. We will conduct our analysis using QCEW data through 2024Q3, when those data become available (typically with a 5-6 month lag). This will allow us to measure the impact over a full four quarters after the law's implementation.

Table 1: QCEW Temporary Help Services State-level descriptives

	N	Mean	SD	Min	Max	P25	P50	P75
Month 1 Emps (thousands)	4,488	49.24	59.53	1	408	9	30	63
Month 2 Emps (thousands)	4,488	50.02	60.39	1	416	9	30	64
Month 3 Emps (thousands)	4,488	50.41	60.69	1	418	9	31	64
Qtr Emps (thousands)	4,488	49.88	60.20	0	414	9	30	64
Qtr Wages (millions)	4,488	370,285.64	529,385.63	0.00	5,650,470.86	63,151.75	202,483.90	460,792.75
Avg Weekly Wage	4,488	727.81	179.18	382.87	1,614.18	595.32	697.46	822.60
Not Disclosed	4,488	0.00	0.02	0.00	1.00	0.00	0.00	0.00
Year	4,488	2012	6.34	2001	2022	2006	2012	2017

Notes: Data source is the QCEW NAICS-Based, Quarterly CSV Files. In the build, each row refers to the Temporary Help Services industry (5-digit NAICS code: 56132) in a specific state × quarter. Month 1-3 Emps is as provided by the QCEW in the variables `month1_emplvl`–`month3_emplvl`, giving total employees as of the 12th of the first, second, or third month of the given quarter. Qtr Emps is the average of Month 1-3 Emps. Qtr wages is `total_qtrly_wages` from the QCEW. Avg weekly wage is `avg_wkly_wage` from the QCEW. Not Disclosed is a dummy equal to 1 if a `disclosure_code` is "N" for "not disclosed" in that quarter. See QCEW variable descriptions at US BLS (2023).

3.2 Outcomes

We will construct and analyze the following outcomes:

- **Monthly employment**: This is based on the variables `month1_emplvl`, `month2_emplvl`, and `month3_emplvl` (US BLS, 2023) in the quarterly QCEW release. After constructing a state-by-month panel, we will seasonally adjust each state's monthly employment using X-13ARIMA-SEATS (US Census, 2023) as implemented in the `seasonal` package in R.

- **Average weekly wages**: Average weekly wages are provided at the quarterly level by the QCEW in the variable `avg_wkly_wage`. We will seasonally adjust using the same X-13ARIMA-SEATS algorithm and deflate to January 2023 prices using the Consumer Price Index for All Urban Consumers (CPIAUCSL; FRED, 2023).

This implies that our analysis will be done at two different time units: months and quarters. We

will choose separate donor weights for each outcome / time unit. The QCEW does not provide a measure of monthly wages, but preliminary power calculations suggested that estimates for employment effects would be more precise using the monthly estimates.

3.3 Definition of treatment and the estimand of interest

The only treated state is New Jersey. We specify (1) the period used for calculating the weights, (2) the period used to measure outcomes for the core estimand of interest, and (3) the period over which any secondary outcomes will be examined.

This requires some judgment. The law was signed in February 2023 and will go into effect in August 2023. It is possible that firms may have altered their labor demand immediately after the law was signed, in anticipation of higher wages. However, we think this is unlikely, in part because the regulations implementing the law were not published until late July and in part because the law was challenged and it was not clear if it would take effect. Accordingly, we make decisions allowing for anticipation effects but with a view that treatment effects are most likely to appear after August 2023, perhaps well after.

The possibility of small anticipation effects suggests that we may want to allow for a gap between the pre-treatment periods used to identify the synthetic control comparison group and the post-treatment periods that we will use to measure the law's impact. However, there is a trade-off, as the longer the gap we allow, the lower the statistical power of the analysis, as New Jersey might diverge from its synthetic control over time due to chance.

To balance this, we will use data from before the law was passed and shortly afterward to estimate the synthetic control weights. We will then allow for a gap, and begin measuring outcomes in August 2023, after the law was implemented. Our primary outcomes will be based on averages over the first full year after this point, but we will also consider secondary outcomes that exclude periods right after the law took effect to allow for phase-in.

The exact periods used will be different for monthly employment than for quarterly wages:

- **Monthly employment**:
  - **Estimation of weights** We will use data from the beginning of the panel through March 2023 to select the synthetic control donor states.

- **Primary outcome** Our primary measure of the law's effect will be the average treatment effect over the 12-month period August 2023-July 2024. This allows a four-month gap between the pre-treatment and post-treatment data.
- **Secondary outcomes** A secondary analysis will examine the average treatment effect over the six-month period February 2024-July 2024. This allows for the possibility that the treatment may grow over time, and that the primary analysis will be under-powered due to the inclusion of low-effect early months.
- **Testing for anticipation** Our weights will be estimated using data through March 2023, which raises the possibility that the law could have impacted the outcomes during the last two months of this period. We will estimate a secondary set of weights using data through December 2022, and use these weights to estimate the treatment effect on outcomes in January-March 2023. We will report this effect as a secondary outcome. If a test that the average impact over these three months equals zero has p-value less than 0.05, we will use these secondary weights to calculate the treatment effects of interest, excluding all post-law-signing data from estimation of the weights, and the resulting estimates will be our primary ones.

- **Quarterly wages**:
  - **Estimation of weights** We will use data through 2023Q1 to estimate weights.
  - **Primary outcome** We will measure the average ATT over 2023Q4-2024Q3.
  - **Secondary outcome** Our secondary outcome will be the average ATT over 2024Q2 and 2024Q3.
  - **Testing for anticipation** We will estimate a secondary set of weights using data through 2022Q4, and use these weights to test for a non-zero average treatment effect on outcomes in 2023Q1. If the p-value for this test is less than 0.05, we will use these secondary weights to calculate the treatment effects of interest, and the resulting estimates will be our primary ones.

3.4 Missing data

NAICS-level data at the state and local level are subject to disclosure restrictions. In full QCEW data from 2001-2022, there are two state-quarter observations where average weekly wages and monthly employment are not provided. For those months, we linearly interpolate using the nearest previous and first subsequent quarters with non-missing data. It is highly unlikely that any of our post-treatment observations in 2023 and 2024 will be below the disclosure threshold. If this happens in any of the states needed for estimating the treatment effect, we will linearly interpolate the missing periods. If this happens in the *last* period of our outcome window, we will wait an additional quarter so that we can use a quarter after our outcome window for interpolation.

3.5 Weights for synthetic control method

We use an augmented synthetic control algorithm (Ben-Michael et al., 2021), implemented in the `augsynth` package, to calculate weights. Augmentation will be via a Ridge regression, using cross-validation to select the Ridge hyper-parameter via the `augsynth` package's default settings. The entire R script and pre-treatment data from the QCEW is included in our pre-registration repository.

3.6 Robustness

We will report the following robustness checks for the primary and secondary estimates.

1. **Exclude bordering states and states passing similar laws**: We will run the same analysis omitting the states bordering New Jersey (Pennsylvania, New York, and Delaware) in case there are meaningful spillovers. We also exclude Illinois, which passed and implemented a similar law, HB2862, in August 2023 (Wallin, 2023).[4] If more states pass laws mandating equal pay for temporary workers in our post-treatment period, we will exclude them as well in this robustness check. This is difficult to foresee, but we will amend the pre-registration as soon as these laws become known.

2. **Generalized synthetic control**: Use the Generalized Synthetic Control Method as implemented in the `gsynth` package, using defaults (Xu, 2017).

[4] A major difference is that the equal pay provision applies after the temporary worker has been at that job for 90 days.

3. **Standard synthetic control**: Run `augsynth` without an outcome model (this is vanilla synthetic control).

4. **Alternate treatment date**: Run our main `augsynth` procedure where the weights only use data through December 2022. This is our test for anticipation effects (see Section 3.3).

3.7 Inference

We will calculate confidence intervals and p-values using the conformal inference procedure (Chernozhukov et al., 2021). While we generally will rely on the implementation in `augsynth`, it will require some modification to generate p-values for mean estimated treatment effects over a period not immediately following the period used to select weights (as discussed above). We will update this pre-registration with code to implement this before 2023Q4 data are released.

4 Power

While not part of our pre-registration steps, we document here using some basic checks that the design should be well-powered to detect plausible effects based on the existing literature.

4.1 What share of workers are affected by the law?

The law does not cover all occupations in the temporary help services industry (see Section 2). We downloaded data from the BLS's Occupational Employment and Wage Statistics (US BLS, 2022), which provides national and state-level counts of the number of workers in each occupation-by-industry, including temporary help services. These estimates from March 2022 suggest that 61% of New Jersey workers in temporary help services will be covered by the law.

4.2 Plausible effects

Based on the raw estimates in Dube and Kaplan (2010), wages are 17% higher for full-time janitors compared to outsourced janitors, and 28% higher for full-time guards.[5] This is from their cross-sectional analysis, which could be driven in part by difference across workers and firms. Their analysis using individual fixed effects, demographic controls, and switchers in the Current Population Survey finds a penalty of 6.5% for janitors (Table 3a, row 8) and 8.3% for guards (Table 3b, row 8). Also using

[5]This is calculated as follows. Janitors: $100 \times 1.33/7.89$, Guards: $100 \times 2.34/8.50$ using the real wage estimates in the top row of Table 2. Both of these occupations are covered by the law.

US data, Dorn et al. (2018) find a roughly 5% wage penalty following domestic outsourcing events. It's unclear ex ante whether the cross-sectional or within-person estimates are better for measuring the potential wage effects, since firms who switch their workers from full-time to temporary might be different from firms who have stably employed a temporary workforce. The latter group will have to comply with the law and would not contribute to the within-person estimates from Dube and Kaplan (2010) or Dorn et al. (2018).

The law also requires that temp workers are paid the cash value of benefits the employer grants to the full-time counterparts that are not legally required.[6] Based on a recent BLS report (US DOL, 2023, Table A), benefits amount to 11.5% of hourly wages for workers at the 10th percentile of wages even excluding legally required benefits.[7] This increases to 30% for workers with median wages.

Depending on which wage estimates are used, the benefits portion of the law may have a larger impact on wages for temporary workers. Using the lowest possible estimated wage penalty (5%) and the lowest possible value for the cash value of benefits (11.5%) suggests a wage increase of 16.5% for temporary workers. This could be a lower bound given that temporary workers are on average about the 10th percentile of the wage distribution.

What would be a reasonable own-wage demand elasticity to expect in this context? Research on the minimum wage could be a guide here, but there is no clear consensus view on employment responses. Besides, temporary workers might be especially substitutable since they can be converted to full-time employees.

4.3 Power calculations

We ran several placebo tests to gauge the power of the design. Our core placebo procedure assigns a range of placebo treatment dates to New Jersey, leaving at least one year as pre-treatment for monthly employment and five years as pre-treatment for quarterly average wages. This gives 240 placebo treatment dates for employment and 64 for wages. For each of these placebo dates, we calculate the average ATT for first year of the post-treatment period. This distribution of average ATTs is used to

[6] It is defined as follows (NJ Labor, 2023, p. 21):

> "Benefits" means employee fringe benefits, including but not limited to, health insurance, life insurance, disability insurance, paid time off (including vacation, holidays, personal leave and sick leave in excess of what is required by law) training, and pension. The term "benefits" does not include employee fringe benefits that an employer is required by law to provide to its employees (e.g., earned sick leave under N.J.S.A. 34:11D-1 et seq.).

[7] This is calculated as (0.48+0.25+0.65+0.11)/12.97 = 11.5% using the numbers in Table A.

calculate the confidence interval. For the last placebo treatment date, we also calculate the jackknife (for gsynth runs) and jackknife+ (Barber et al., 2021) (for augsynth runs) confidence interval to not rely entirely on the placebo treatment dates.

Table 2: Confidence interval widths from placebo runs

Type	Outcome	Technique	CI/2	NJ Mean	N dates	Jackknife	As Percent
Emp.	Emp. (seas. adj.)	augsynth	5618.09	108917.04	240	4572.52	5.16
Emp.	Emp.	augsynth	6087.30	108802.33	240	5887.79	5.59
Emp.	Emp.	none	6697.96	108802.33	240	7515.19	6.16
Emp.	Emp. (seas. adj.)	none	6767.49	108917.04	240	6164.22	6.21
Emp.	Emp. (seas. adj.)	GSYN	7873.08	108917.04	240	4839.59	7.23
Emp.	Emp.	GSYN	8226.05	108802.33	240	5337.42	7.56
Emp.	Ln(Emp.) - Ln(State Emp.)	augsynth	0.08	-3.64	240	0.05	7.57
Emp.	Ln(Emp.) (seas. adj.)	augsynth	0.08	11.60	240	0.05	7.66
Emp.	Ln(Emp.) - Ln(State Emp.)	GSYN	0.09	-3.64	240	0.06	8.93
Emp.	Ln(Emp.) (seas. adj.)	GSYN	0.09	11.60	240	0.06	8.99
Emp.	Ln(Emp.) - Ln(State Emp.)	none	0.09	-3.64	240	0.09	9.20
Emp.	Ln(Emp.) (seas. adj.)	none	0.11	11.60	240	0.08	10.70
Wage	Avg. wage (seas. adj.)	none	31.09	947.44	64	39.30	3.28
Wage	Avg. wage (seas. adj.)	augsynth	35.23	947.44	64	39.39	3.72
Wage	Avg. wage	augsynth	35.90	943.48	64	45.21	3.81
Wage	Ln(Avg. wage) (seas. adj.)	none	0.04	6.85	64	0.05	3.83
Wage	Avg. wage	none	37.66	943.48	64	49.36	3.99
Wage	Ln(Avg. wage) - Ln(State wage)	none	0.04	-0.47	64	0.09	4.42
Wage	Ln(Avg. wage) (seas. adj.)	augsynth	0.05	6.85	64	0.05	4.63
Wage	Ln(Avg. wage) - Ln(State wage)	augsynth	0.05	-0.47	64	0.08	4.94
Wage	Ln(Avg. wage) - Ln(State wage)	GSYN	0.05	-0.47	64	0.09	5.41
Wage	Avg. wage (seas. adj.)	GSYN	59.71	947.44	64	95.92	6.30
Wage	Avg. wage	GSYN	61.90	943.48	64	105.58	6.56
Wage	Ln(Avg. wage) (seas. adj.)	GSYN	0.07	6.85	64	0.17	7.48

Notes: This table shows the confidence intervals from several placebo runs in order to probe the power of our design. Each row is a set of placebo runs where we vary the date that New Jersey is "treated" within the currently available data (2001-2022), and leaving periods at the beginning and end for fitting the weights and estimating an effect. From each placebo treatment date, we calculate the average ATT using the first year of post-treatment ATT estimates. The confidence interval is based on this distribution of average ATTs. Across rows, we change the outcome and synthetic control method that we use. The columns are as follows: **Type**: Monthly employment (Emp.) or average weekly wages (Wage). **Outcome**: Which adjustment of the outcome is used, where "seas. adj."indicates using seasonal adjustment; State wage and Sate Emp. are the state-level aggregates of those variables (agglvl_code=50 in the QCEW); and Ln() indicates the natural logarithm. **Technique:** the synthetic control technique used. Ridge is `augsynth` using "Ridge" regression for the outcome model, "GSYN" uses the `gsynth` package with defaults (Xu, 2017), "none" is vanilla synthetic control (estimated in `augsynth`). **CI/2**: half of the confidence interval width based on the distribution of average one-year ATTs. **NJ mean**: the mean of the outcome in New Jersey in 2022. **N dates**: the number of placebo treatment dates used. **Jackknife**: An alternate confidence interval, this is the jackknife+ estimate of the confidence interval for the last of the placebo treatment dates, divided by 2, except when the technique is GSYN it's the regular jackknife. **As percent**: this shows either the **CI/2** $\times$ 100 for logged outcomes or 100 $\times$(**CI/2**) / (**NJ mean**) for outcomes that are not in logs, so that the confidence intervals can be more easily compared.

We ran these tests with several transformations of the outcomes: levels, seasonally adjusted levels, seasonally adjusted logs, and seasonally adjusted logs minus the seasonally adjusted log of the

state-level outcome. Finally, we varied the synthetic control technique. We either used: `augsynth` augmented with Ridge, gsynth (Xu, 2017), or vanilla synthetic control (as implemented in `augsynth`).

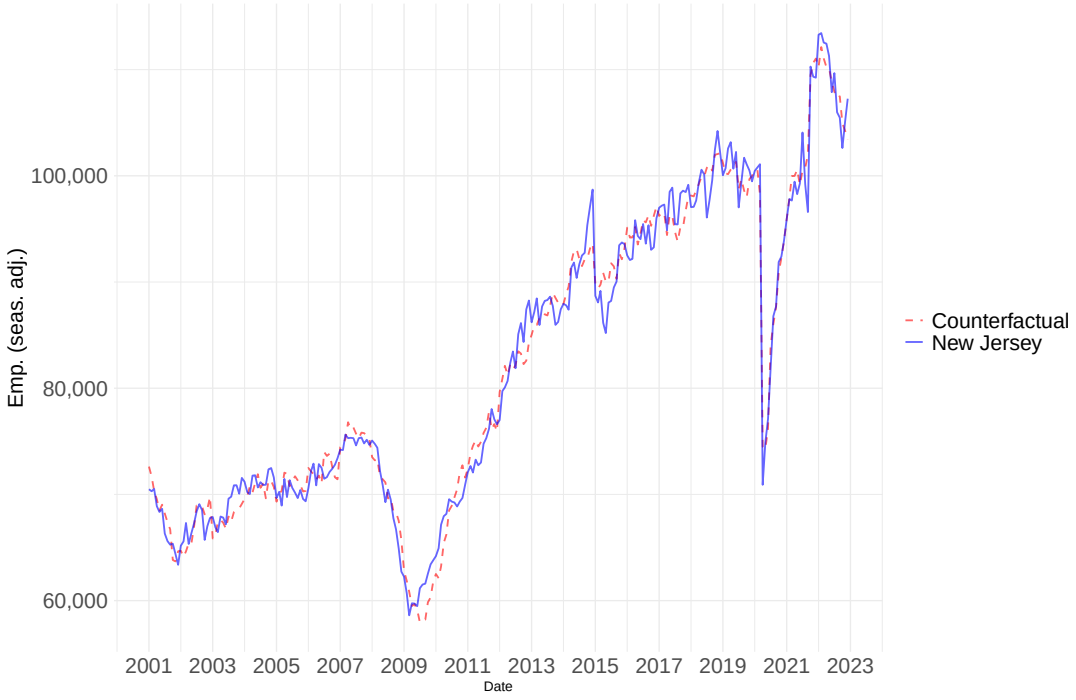
The results are in Table 2. The fourth column reports half the width of the 95% confidence interval derived using the placebo treatment dates. The last column shows this as a percent of the New Jersey average outcome as of 2022. So in the top row, the half-width of the implied confidence interval is 5.16% of the New Jersey mean, implying that we would be 50% powered assuming the policy changes employment by 5.16%. For both employment and wages, the seasonally adjusted levels gave the narrowest confidence interval. For employment, the lowest width was achieved using `augsynth` augmented with Ridge. The half-width of the confidence interval was 5.16% of the New Jersey mean in 2022. For wages, the lowest half-width was 3.28% of the New Jersey 2022 mean, this time estimated using vanilla synthetic control.

This suggests sufficient power to detect the effects on wages discussed above, at least given some simplifying assumptions. We arrived at 16.5% as a guess at the lower bound of potential wage increases, and using the OES we estimated that 61% of workers are covered by the law. The minimum detectable effect for 80% power with a normal sampling distribution is roughly 2.8 times the standard error. We do not have any guarantee of normality in this context, but using a quarter confidence interval as a crude standard error, this would mean we are powered to detect a \$44 ($= 2.8 * \$31.09/2$) change in average wages or 4.6% of the New Jersey average in 2022, which is less than half of the lower bound wage increase we might expect ($10\% = 0.61 * 16.5\%$). This appears promising for measuring the wage impacts. By the same reasoning, we would be powered to detect a 7,865 ($= 2.8 * 5618.09/2$) change in monthly employment, or 7.2% of the New Jersey average in 2022, corresponding to an own-wage labor demand elasticity of about 0.7 assuming an overall wage increase of 10%.

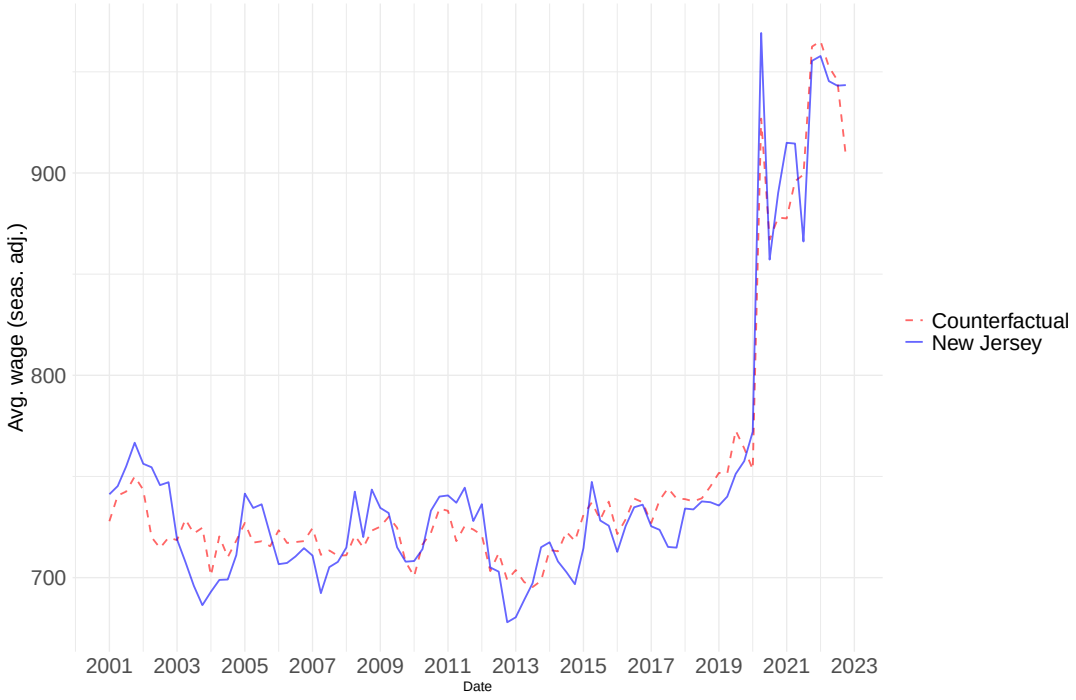
4.4 Visual assessment

In Figure 1, we show New Jersey and its synthetic control for the two primary outcomes, using `augsynth` with Ridge and with seasonal adjustment. In both cases, the two series track each other closely, confirming the impression from the power calculations.

Figure 1: New Jersey and synthetic New Jersey in 2001-2022



(a) Monthly employment in the temporary help services industry



(b) Average weekly wages in the temporary help services industry

Notes: Panel (a) shows seasonally adjusted monthly employment in the temporary help services in New Jersey and its synthetic counterfactual for 2001-2022, where the synthetic control is derived using the augmented sythetic control method, augmented with ridge regression. Panel (b) shows the same series but using average weekly wages (measured at the quarterly level in the QCEW). Wages deflated to January 2023.

Table A1: Pre-analysis checklist

	Item	Brief Description	Followed plan?
1	**Sample and Data**	State-level (50 states + DC) QCEW data on wages and employment in the Temporary Help Services industry (5-digit NAICS code: 56132) from 2001Q1 to 2024Q3.	
2	**Primary Outcomes**	Employment (measured monthly) and average weekly wages (measured quarterly), both seasonally adjusted.	
3	**Treatment definition**	New Jersey is treated beginning 2023Q2.	
4	**Causal design / controls**	We use an augmented synthetic control design, augmented with a Ridge regression. Weights will be obtained via the pre-specified procedure described in Section 3.5.	
5	**Primary estimates**	Our estimates of interest are the average ATTs from `augsynth` with Ridge over the first full year following the law's implementation in August 2023 for the two primary outcomes. This means August 2023 - July 2024 for monthly employment, 2023Q4-2024Q3 for wages.	
6	**Secondary estimates**	For both of the primary estimates above, we will report (i) the ATT for the latter half of the one-year period used for its primary estimate and (ii) the period-by-period ATT and confidence intervals for the full post-treatment period.	
7	**Inference**	Conformal inference using `augsynth`, modified to examine average treatment effects over the first year following implementation.	
8	**Robustness checks**	We will report the primary and secondary estimates with the following adjustments: (1) Exclude bordering states and states that passed similar laws since January 1, 2022 (augsynth+Ridge), (2) GYSNTH (Xu, 2017), (3) Vanilla synthetic control (4) Using a treatment date of January 2023.	
9	**Missing data and cleaning**	We will drop any negative or zero values for employment or wages. In the unlikely event of missing values, we will use linear interpolation.	

Notes: This table gives a brief checklist of analysis steps, with references to the full explanation of the decisions. When our analysis is complete, we will fill in the "Followed plan?" column with either "Yes" or an explanation.

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