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Duncan R. Hobbs
American Enterprise Institute

Michael R. Strain
American Enterprise Institute, Georgetown University and IZA

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ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Do Reemployment Bonuses Increase Employment? Evidence from the Idaho Return to Work Bonus Program*

In June 2020, Idaho announced the Return to Work Bonus program, which provided residents who returned to work with bonuses of up to $1,500. Using multiple data sources, we present difference-in-differences, triple differences, and synthetic control estimates suggesting the program may have increased individual employment and accelerated flows into employment from unemployment and from nonparticipation in the labor force. We show the program likely increased state-level employment rates. To the best of our knowledge, this is the first paper to study the effects of reemployment bonuses on the U.S. labor market outside an experimental setting.

JEL Classification: J08, J64, J68

Keywords: reemployment bonus, unemployment insurance, employment

Corresponding author:
Michael R. Strain
American Enterprise Institute
1789 Massachusetts Avenue, NW
Washington, DC 20036
USA

E-mail: michael.strain@aei.org

* We thank Jesse Rothstein for helpful comments.

1. Introduction

Reemployment bonuses are an interesting and novel policy option to support the incomes of unemployed workers while providing a direct incentive for them to secure employment (*e.g.*, Kugler, 2015). Like traditional UI benefits, reemployment bonuses would be available to unemployed workers. But unlike traditional UI benefits, they would be payable when unemployed workers transition into employment. This incentive structure might increase the pace at which the unemployment rate falls and the employment rate increases following economic downturns.

Despite this potentially attractive feature, reemployment bonuses are rare. Illinois, New Jersey, Washington, and Pennsylvania conducted reemployment bonus experiments during the 1980s, comparing employment and wage outcomes for unemployed individuals offered bonuses with those not offered bonuses.[1] These experiments led to modest reductions in the duration of unemployment and in the likelihood that recipients exhausted their benefits, along with decreases in benefit receipt (Woodbury and Speigelman 1987, Decker and O'Leary 1995, and Decker 1994). These experiments provide valuable information, but their usefulness in understanding the labor market and as a guide for policy is limited by the fact that they occurred over three decades ago, had relatively small samples, and had eligibility restrictions that likely would not apply in a non-experimental setting.

This paper adds to our understanding of the effects of reemployment bonuses by studying the Idaho Return to Work Bonus (RWB) program. To the best of our knowledge, this is the first paper to study the effects of reemployment bonuses on the U.S. labor market outside an

[1] Outside the U.S., the Netherlands, Taiwan, and South Korea have also trialed reemployment bonuses.

experimental setting. Introduced on June 5, 2020, this $100 million program provided Idaho residents who became unemployed due to the Covid-19 pandemic with bonuses of up to $1,500 if they returned to work with their previous non-governmental employer or a new employer between April 20 and July 15, 2020, and if they worked at least four consecutive weeks. Employers submitted applications on behalf of employees from July 13 – August 14, 2020 after completing four weeks of work.

Using difference-in-differences (DD) and triple-difference (DDD) estimators on Current Population Survey (CPS) data from January – October 2020, we estimate effects of the Idaho RWB program on a variety of labor market outcomes. Among prime-age workers, our most controlled DD models suggest that the Idaho RWB program increased employment by 3.2 percentage points, increased the flow of unemployed workers into employment by 18.2 percentage points, and increased the flow of nonparticipants into employment by 4.2 percentage points. Using state-level outcomes with a similar specification, we estimate that the Idaho RWB program is associated with the employment-population ratio increasing by 2.9 percentage points, the unemployment rate decreasing by 0.4 percentage points, the nonparticipation rate decreasing by 2.5 percentage points, and nonfarm payroll employment increasing by 2.4 percent.

In addition to conventional DD and DDD estimates, we also estimate placebo treatment effects under (the false) assumption that states other than Idaho enacted the program in June 2020. We find the estimated actual effects for Idaho are significantly larger than the placebo effects for states that did not enact the program.

The fact that Idaho was the only state to enact a return to work bonus during 2020 and the different effects of the initial outbreak of Covid-19 had the labor markets of different states raise the concern that labor market outcomes in Idaho and other states may have been evolving differently prior to the enactment of Idaho's program in ways that are not adequately controlled for by our DD, DDD, and placebo methods. To address this concern, we use synthetic control methods to construct a weighted average of the control states to best match Idaho for both the outcomes of interest and controls in the period before the bonus program was introduced. We find qualitatively similar results using synthetic control methods and DD and DDD methods.

2. Background on the Idaho Return to Work Bonus Program

Idaho Return to Work Bonus Program Background

On June 5, 2020, Idaho Governor Brad Little announced the "Return to Work Bonus" (RWB) program: a $100 million program that provided unemployed or non-employed residents who returned to work between April 20, 2020 and July 15, 2020 bonuses of up to $1,500, or roughly five times the average weekly Idaho unemployment benefit. Unemployed or non-employed individuals could receive the bonus if they returned to work for their immediate past employer or for a new employer.[2] Eligible employees had to be Idaho residents working for a non-governmental Idaho employer who previously received state UI due to the coronavirus pandemic, earned less than $75,000 in wages annually, and worked at least 20 hours per week for four consecutive weeks in a job that was intended to last longer than four weeks.[3]

Employers submitted applications on behalf of employees from July 13, 2020 to August 14, 2020 after the conclusion of four weeks of work. Employers could submit applications for

[2] https://gov.idaho.gov/pressrelease/gov-little-to-offer-back-to-work-cash-bonuses/
[3] https://web.archive.org/web/20220209222642/https://rebound.idaho.gov/return-to-work-bonuses/

multiple employees that met the criteria, but employees could only receive one return to work bonus based on the job with the highest number of hours worked per week. The size of the bonuses varied based on when a respondent started work and how many hours they worked during the four-week qualifying period. We present the schedule of available bonuses in Appendix Table A1. As of September 26, 2023, Idaho had approved 28,729 return to work bonuses representing roughly $43 million in payments.[4] We provide details of previous reemployment bonuses and research on their effects in Appendix A.

While other states introduced reemployment bonuses in the summer of 2021, we focus on the Idaho RWB program for several reasons. First, as shown in Table A2, Idaho's program is a clear outlier with respect to the availability of reemployment bonuses or the number of bonuses distributed to unemployed workers, measured as a share of the total number of unemployed and nonemployed workers. In addition, studying a program implemented in 2020 in the same analysis as a program implemented in 2021 introduces challenges. Finally, in 2021, other labor market policies were in flux, particularly extensions to Unemployment Insurance.

3. Data Sources

We use the basic monthly samples of the Current Population Survey (CPS) (Flood *et al.*, 2021). Respondents are interviewed for four consecutive months, not interviewed for the following eight months, and then interviewed again for another four consecutive months. The 4–8–4 short-panel structure allows us to examine levels of employment as well as transitions

[4] https://transparentshowcases.idaho.gov/pages/idaho-return-to-work-bonus-program (accessed September 26, 2023).

between employment, unemployment, and nonemployment across consecutive months. Our samples consist of all individuals ages 25–54 and ages 16 and over. We examine individual employment, transitions from unemployment into employment (U-to-E), and transitions from nonparticipation ("not in the labor force;" NILF) into employment (NILF-to-E).[5]

In addition to individual worker outcomes, we also examine state-level outcomes using the CPS: the employment-population ratio, the unemployment rate, and the rate of nonparticipation (the "NILF" rate). In addition, we use total monthly nonfarm employment from the Current Establishment Statistics (CES) and total employment from the Quarterly Census of Employment and Wages (QCEW) to study the effect of the RWB program on state-level employment. The CES produces detailed industry estimates of nonfarm employment, hours, and earnings of workers on payrolls. The QCEW provide monthly state employment levels based on quarterly reports submitted by nearly all employers in the U.S.[6]

We use data on the severity of restrictions designed to combat Covid-19 and the number of new cumulative Covid-19 cases as control variables. These data come from the Oxford Covid-19 Government Response Tracker (OxCGRT) and have been used in recent papers studying Covid-19 and the labor market (*e.g.*, Lee, Park, and Shin, 2021, Agrawal *et al.*, 2021, and Holzer, Hubbard, and Strain, 2023). OxCGRT calculates an index that recorded the strictness of social distancing policies that primarily restricted people's behavior — including restrictions on

[5] A concern with the matched CPS is mismeasurement of labor force status may lead to spurious transitions out of unemployment (Abowd and Zellner, 1985). To address this, we implement a recoding procedure for unemployment to employment transitions used, for example, in Farber, Rothstein, and Valletta (2015) and Petrosky-Nadau and Valleta (2021). For individuals who transition out of unemployment and into employment in one month, but then return to unemployment in the following month (i.e., U-E-U), we consider the transition spurious, and recode the respondent as having not had a U-E transition.

[6] The CES data is seasonally adjusted by the BLS, but the QCEW employment data is not.

gatherings, canceling public events, closing workplaces, restrictions on public transport, and school closures[7] — for all 50 states and the District of Columbia daily.

We also control for differences in macroeconomic conditions between Idaho and other U.S. states during this period that may have affected labor market outcomes during this period. Specifically we use quarterly state income *per capita* from the Bureau of Economic Analysis (BEA) and the monthly state-level job opening rate from the Job Openings and Labor Turnover Survey (JOLTS).

4. Empirical Strategy

Difference-in-differences

We estimate difference-in-differences (DD) regressions of the following form:

$$y_{i,s,m} = \beta_0 + \beta_1 Idaho_i + \beta_2 post_m + \beta_3 (Idaho_i * post_m) + Y_{i,s,m}\gamma + X_{s,m}\delta + \varepsilon_{i,s,m}, \qquad (1)$$

where $y_{i,s,m}$ is equal to one if an individual i living in state *s*, in month *m* is employed or transitions into employment from either unemployment or nonparticipation. $Idaho_i$ is a dichotomous variable indicating whether an individual lives in Idaho, $post_m$ is an indicator equal to zero from January – May 2020 (before the RWB program was announced) and equal to one from June – October 2020 (after the RWB program was announced). The vector $Y_{i,s,m}$ includes dummies for each age and education level in the CPS. We use January-October 2020 as

[7] The construction of the stringency index is described in Hale *et al.* (2021). The specific indicators in the stringency index include school, workplace, and public transportation closing, canceled public events, restrictions on gathering, shelter-in-place orders, restrictions on movement between cities or regions, restrictions on international travel, and public information campaigns. More details on how the index is calculated are available at: https://github.com/OxCGRT/covid-policy-tracker/blob/master/documentation/index_methodology.md. The data are frequently revised, and the data used in our analysis were last updated October 4, 2021.

the analysis period to focus on months affected by the initial outbreak of Covid and to balance the number of pre and post period months. The program ended in August, with bonuses paying out into September.

We include a large number of covariates in order to attempt to isolate the effect of the return to work program on labor market outcomes. The vector $X_{s,m}$ includes monthly state-level indices of Covid-19-related restrictions on activity as well as numbers of new Covid-19-related cases by state to address the possibility that differences in labor market outcomes are due to differences in the severity of Covid and associated restrictions on activity between Idaho and other states. For months prior to March 2020 where Covid-19 cases and restrictions were nonexistent, we set values to zero. $X_{s,m}$ also includes the monthly state-level job opening rate and the log of state quarterly income per capita to control for differences in the evolution of statewide economic conditions. We include state and month fixed effects in all regressions, which hold constant all time-invariant cross-state differences and state-invariant cross-time period differences.

The coefficients of interest are those associated with the $(Idaho_i * post_m)$ interaction term. These reveal how employment and transitions between unemployed or not in the labor force changed in Idaho on average relative to other states following the introduction of the RWB program in June 2020.

Triple difference-in-differences models

We also estimate triple difference (DDD) models of the following form:

$$y_{i,s,m,y} = \alpha + \gamma * \left(Idaho_s * Post_m * 2020_y\right)$$
$$+ \theta_1 * (Idaho_s * Post_m) + \theta_2 * \left(Idaho_s * 2020_y\right)$$
$$+\theta_3 * \left(Post_m * 2020_y\right) + \vartheta_1 * Idaho_s + \vartheta_2 * Post_m \tag{2}$$
$$+ \vartheta_3 * 2020_y + Y_{i,s,m,y}\varphi + X_{s,m,y}\delta + \varepsilon_{i,s,m,y.}$$

Similar to equation (1), $y_{i,s,m,y}$ is an indicator variable for whether an individual living in state s, in year y is employed in month m, or transitions from unemployment or out of the labor force in month $m - 1$ into employment in month m. Here, y is 2020 or 2019. The variable 2020_y is an indicator variable equal to one if the year is 2020 and equal to zero if the year is 2019. We include controls for the state job opening rate and the log of income per capita, but we drop controls for monthly Covid cases and the policy stringency index as nether existed in 2019 for any states. All other variables are defined as in equation (1).

This DDD model effectively controls for changes in Idaho and control states between the spring and summer of 2019. This could address concerns that Idaho could have different seasonal trends in employment, or labor market transitions relative to other states. It could also address concerns about relying solely on 2020 for our estimates, a year in the Covid pandemic massively disrupted social and economic activity.

State-level employment, labor force participation and unemployment

In addition to examining the effects of the RWB program on individual outcomes, we estimate the effects of the program on aggregate state-level outcomes. Specifically, we estimate variants of the following difference-in-difference model:

$$y_{s,m} = \beta_0 + \beta_1 Idaho_s + \beta_2 post_m + \beta_3 (Idaho_s * post_m) + Y_{s,m}\gamma + X_{s,m}\delta + \varepsilon_{s,m}, \qquad (3)$$

where $y_{s,m}$ is the state employment-population ratio, unemployment rate, nonparticipation (NILF) rate, or the log of total employment. $Idaho_s$ is a dichotomous variable indicating whether an individual lives in Idaho, $post_m$ is an indicator equal to zero from January – May 2020 (before the RWB program was announced) and equal to one from June – October 2020 (after the RWB program was announced). The vector $X_{s,m}$ includes monthly state-level indices of Covid-19-related restrictions on activity as well as numbers of new Covid-19-related cases and deaths by state, the monthly state-level job opening rate, the log of state quarterly income *per capita*. Rather than dummies for each age and education level, the vector $Y_{s,m}$ includes the average age of individuals in the CPS, and the share of individuals ages 16 and over with less than high school, high school, some college, and a BA or higher education in state *s* and month m.

We also estimate triple-differences models on state level outcomes of the following form:

$$
\begin{aligned}
y_{s,m,y} &= \alpha + \gamma * \left(Idaho_s * Post_m * 2020_y\right) \\
&+ \theta_1 * (Idaho_s * Post_m) + \theta_2 * \left(Idaho_s * 2020_y\right) \\
&+ \theta_3 * \left(Post_m * 2020_y\right) + \vartheta_1 * Idaho_s + \vartheta_2 * Post_m \\
&+ \vartheta_3 * 2020_y + Y_{s,m,y}\varphi + X_{s,m,y}\delta + \varepsilon_{s,m,y.}
\end{aligned}
\tag{4}
$$

This triple difference specification is identical to the specification we estimate on individual data presented in equation (2) except the outcome, denoted by $y_{s,m,y}$, and the age and education controls, denoted by $Y_{s,m,y}$, are measured at the state-level rather than being individual indicator variables.

Placebo tests of our main regression estimates

To provide additional support for hypothesis testing, we conduct a variation of placebo tests similar to Buchmuller, DiNardo, and Valletta (2011) in their analysis of Hawaii's Prepaid Healthcare Act.[8] We estimate placebo effects by assigning treatment status to one of the 49 states and Washington, D.C. from our control group and using the remaining control states as the control group. We then compare the magnitude of the estimated treatment effects with Idaho as the treated state and displayed in Tables 2 and 3 with the distribution of the fifty estimated placebo effects. We report the p-value in our regression tables.

[8] Many recent papers have used similar permutation tests as robustness checks for difference-in-differences regressions examining the effects of policy changes with a single affected group or a small number of affected groups including Balasubramanian *et al.* (2022), Cooper, Scott-Morton, and Shekita (2020), Cunningham and Shah (2018), and Goldin, Lurie, and McCubin (2021).

Synthetic controls to guard against differential trends.

Synthetic controls have become increasingly popular for estimating causal effects of policies affecting aggregate outcomes for a relatively small number of units.[9] With a single treated state and a large number of control states, synthetic controls are well-suited research question. As discussed in Abadie (2021), synthetic control estimators avoid extrapolation bias from linear regression, explicitly describe the contribution of each control unit to the counterfactual, use only pretreatment data to construct the estimated counterfactual, and do not require the treated and control units follow parallel trends in the pretreatment period. We use the same pre and post periods as in our DD and DDD specifications in order to maintain comparability.[10]

To probe the robustness of our synthetic control estimates and mitigate concerns about "cherry picking" (Ferman, Pinto, and Possebom, 2020), we implement two approaches to constructing synthetic controls. In the first "sparse" specification, we construct synthetic control groups using all values of the dependent variable as well as the average age and the share of individuals with less than high school, high school, some college, and BA or higher education, by state, from January-April 2020 as predictors. In the second "rich" specification, we add pretreatment levels of the Covid and macro variables as predictors to the specifications.[11] In both of these specifications, we do not include levels of the outcome or covariates in May 2020 as

[9] See Bohn, Lofdstrom, and Raphael (2014), Acemoglu *et al.* (2016), Cunningham and Shah (2018), Donohue, Aneja, and Weber (2019), Peri and Yasenov (2019), Jones and Marinescu (2022), Chen, Jain, and Yang (2023), Lang, Esbenshade, and Willer (2023), and Peri, Rury, and Wiltshire (forthcoming) for recent empirical papers using synthetic controls.

[10] Our results are qualitatively similar when we use January 2019 as the start of the preperiod.

[11] For the macro controls, we include levels of the job opening rate from January-April 2020, and levels of log per capita income in January and April 2020 (because the income data is quarterly). For the Covid controls, we include levels in March and April 2020 because levels of cases and stringency are equal to 0 for the vast majority of states in prior months.

predictors, following recommendations from Kaul *et al.* (2021). We employ these two specifications because they relate closely to the DD regression specifications we use, and because both contain a sufficient number of predictor variables to estimate a unique and sparse set of synthetic control weights for each outcome.

As a robustness check of our synthetic controls results, we estimate "placebo synthetic controls" by assigning treated status to one of the control states constructing synthetic control groups from the remaining control states and comparing the placebo effects to the "true" treatment effect estimated using Idaho as the treated state. We present these results in Appendix A. Additionally, we implement the synthetic-difference-in-differences (SDID) estimator from Arkhangelsky *et al.* (2021). The SDID estimator is more flexible than either the DD or synthetic control estimators. SDID relaxes the parallel trends assumption needed for DD estimates, optimally weights time periods when considering counterfactual outcomes, and allows for level differences between treatment and control groups, which the synthetic control estimator does not. We also construct event study style results using the synthetic difference-in-differences estimator following the procedure detailed in Clarke *et al*. (2023). We present more detail on the estimator and the associated results in Appendix B.

5. Results

We first examine summary statistics on individual employment, U-to-E, and NILF-to-E transitions, as well as state-level outcomes, in Table 1. These "unadjusted differences" can be used to compute simple difference-in-difference-style estimates, presented in column (4). These summary statistics indicate Idaho residents experienced substantial increases in the rate of transitions into employment between the “pre” and “post” periods relative to the residents of other states.

Specifically, Table 1 presents unadjusted differences in means in our main outcome variables using CPS data for Idaho and the rest of the United States before and after the introduction of the RWB program. Columns (1) and (2) present means in our pre and post periods and column (3) presents the absolute changes between the pre and post periods. Column (4) presents “unadjusted difference-in-differences” estimates between Idaho and the rest of the U.S., and column (5) presents the changes from column 3 relative to their baseline value from column (1).

The summary statistics indicate Idaho had strong increases in employment levels over our sample period (1.5, and 2.0 percentage points for individuals ages 25-54 and 16 and over, respectively), and that employment changed little in other states on average. Idaho also experienced far large increases in U-to-E transitions (17.3 and 14.4 percentage points for individuals ages 25-54, and 16 and over, respectively) and from nonparticipation (NILF) into employment (4.3 and 2.3 percentage points, respectively), while increases in the rest of the U.S. were far more modest.

Relative to baseline levels, the increase in employment is between 2 and 4 percent in Idaho compared with a decline in employment each age group in all other states. U-to-E transitions increased by 51-59 percent in Idaho compared with 7-13 percent in other states, and NILF-to-E transitions also increased substantially more in Idaho (65-80 percent) relative to other states (10-12 percent).

Difference-in-differences results

We present difference-in-difference estimates of the impacts of the RWB program in Columns (1), (2), (4), and (5), in Table 2. Each panel presents results for the effect the RWB program for a different outcome. Panel A presents results on employment, Panel B presents results for U-to-E transitions, and Panel C presents results for NILF-to-E transitions.

We present results for each outcome for two different age ranges: ages 25–54, and ages 16 and over. Focusing on workers ages 25–54 limits the confounding effects of changes in enrollment in higher education or in retirements in response to the pandemic — early retirements are likely a major pandemic-era labor market development (Faria e Castro, 2021), so this group is especially useful. Estimates for individuals and ages 16 and over give insight into the broader impacts of the return to work bonus on the labor market.

For each outcome, we present two estimates – one with only state, month, age, and education fixed effects (sparse controls) and one adding controls for the state-level Covid-19 variables, job opening rate, and log income per capita described above (rich controls). To asses statistical significance of our estimates, we present p-values estimated using standard errors clustered at the state level in parentheses and p-values calculated using the placebo test discussed above in brackets.

We estimate an increase in prime-age employment of 3.4 percentage points from the introduction of the RWB program before controlling for Covid and macro variables, and a slightly smaller increase of 3.2 percentage points after adding additional controls. We estimate slightly smaller impacts for individuals ages 16 and over (3.0 and 2.7 percentage points with and without controls). All of the four DD results are statistically significant using both conventional standard errors and using the placebo estimates.

Return to work bonuses are associated with increased U-to-E transitions. For conciseness, for the remainder of the discussion we limit the discussion to results with Covid and macro controls but report results with and without controls in the tables. U-to-E transitions increased following the introduction of Idaho's return to work program by 18.2 and 13.3 percentage points among prime-age individuals and individuals ages 16 and over, respectively. All of the DD results are significant using both conventional standard errors and using the placebo estimates From Panel C, the RWB program is associated with increases NILF-to-E transitions of between 2.4 and 4.2 percentage points depending on the demographic group. These results are all statistically significant using conventional standard errors, and two of the four DD estimates (the estimates including covid and macro controls) are statistically significant using the placebo estimates.

Columns (3) and (6) of Table 2 present DDD estimates from equation (2) using our two demographic groups. The estimated DDD effects of the RWB program are smaller than the DD results (with the exception of prime-age U-to-E transitions), but are relatively large in magnitude. All of the results are statistically significant using conventional standard errors except NILF-to-E transitions for prime-age individuals. Using the placebo estimates, the estimated effects on U-to-E transitions among prime age individuals and NILF-to-E transitions among individuals ages 16 and over are statistically significant.

State-level outcomes

We present difference-in-difference and triple-difference estimates of the impacts of the Idaho RWB program on various measures of state-level employment in Table 3. In Panel A, we present effects on the state employment-population ratio, unemployment rate, and share not in the labor force estimated using individuals ages 16 and over in the CPS. The DD coefficients suggest an increase in e-pop of 3.2 percentage points without macro or Covid controls and 2.9 percentage points including these controls, and the DDD coefficient suggests an increase in e-pop of 2.1 percentage points following the introduction of return to work bonuses.

The DD coefficients in Columns (3) and (4) imply a decrease in the unemployment rate of 0.8 percentage points without additional controls, a 0.4 percentage point decrease with Covid and macro controls, and the DDD coefficient suggests a decrease in the unemployment rate of 0.8 percentage points. The DD coefficients in Columns (7) and (8) suggest a decrease in nonparticipation of 2.4 and 2.5 percentage points, with and without controls. The DD coefficient in Column (9) suggests a 1.3 percentage point decrease in nonparticipation. All nine of these estimates are statistically significant using conventional standard errors and two out of nine are statistically significant using the placebo estimates.

In Columns (1)-(3) of Panel B, we estimate the effects of the RWB program on the log of aggregate monthly employment from the CES. The DD coefficients in Columns 4 and 5 suggest an increase in total employment of 3.7 percent without additional controls and 2.4 percent including Covid and macro controls, and the DDD coefficient in Column 6 suggests an increase of 2.7 percent including macro controls. In Columns (4)-(6) of panel B, we present results estimating the effects of the RWB program on the log of aggregate monthly employment from the QCEW. The DD coefficients in Columns (4) and (5) suggest an increase in total employment of 5.8 percent without additional controls and 4.6 percent including Covid and macro controls, and the DDD coefficient in Column (6) suggests an increase of 2.8 percent including macro controls. These results are all statistically significant using both conventional standard errors and using the placebo estimates.

Synthetic control estimates

The panels of Figure 1 present results for employment, U-to-E transitions and NILF-to-E transitions for individuals ages 25-54 using the “sparse” and “rich” predictor sets described above. From Panels A and B, the employment levels for Idaho and its synthetic control closely match during the months prior to June 2020. The employment level in Idaho rises substantially faster in July and August 2020 than its synthetic counterpart to roughly 0.4 percentage points higher for individuals ages 25-54. The largest differences are observed in July and August, which are the months individuals eligible for the return to work bonus had to be employed.

From Panels C and D, U-to-E transitions are higher in Idaho than its synthetic counterpart following the introduction of return to work bonuses. From Panels E and F, NILF-to-E transitions are higher in Idaho than synthetic Idaho following the introduction of the bonus program in June, but differences in subsequent months are smaller. These results are less stable than the employment results, most likely because the underlying U-to-E and NILF-to-E series are more volatile and based on smaller samples. From Figure 2, our results estimated using individuals 16 and over are generally similar to those estimated on samples ages 25-54, but the differences are slightly smaller. The magnitude of the effects is likely smaller for non-prime age workers because concerns about Covid or other factors may have outweighed the financial incentive to return to work for older individuals.

The panels of Figure 3 display synthetic control results for the unemployment rate, and nonparticipation rate for individuals ages 16 and over from the CPS as well as the log of total monthly employment from the CES and QCEW. From Panels A and B the unemployment rate for Idaho is not qualitatively different from synthetic Idaho following the introduction of return to work bonuses. From Panels C and D, the nonparticipation rate is visibly lower in Idaho than synthetic Idaho following the introduction of return to work bonuses. Finally, from Panels E, F, G, and H total employment is higher in Idaho than synthetic Idaho following the introduction of bonuses. The donor pool weights used to construct the synthetic counterfactual to Idaho for each outcome displayed in Figures 1-3 are in Table A3 (sparse predictors) and Table A4 (rich predictors).

Robustness

Appendix Figures A1, A2, and A3 plot the estimated "true" treatment effect for Idaho and the placebo treatment effects for the other states using synthetic controls.[12] The black line represents the treatment effect for Idaho and the light gray lines represent the placebo treatment effects. In these figures, we observe the estimated treatment effect for Idaho before the treatment occurs is very stable, close to zero, and roughly in the center of the distribution of the placebo estimates. We also observe estimated placebo treatment effects show no strong trends before or after treatment and are generally centered around zero.

In Appendix Table A5, we present statistics describing the fit of the synthetic control with Idaho as the treated state relative to the fit from synthetic controls using placebo treated

[12] These are calculated using version 0.20 of the *tidysynth* R package (Dunford 2021). https://github.com/edunford/tidysynth

states. We present the estimated pre and post treatment MSPE, post-pre MSPE ratio, the relative rank using Idaho as the treated state, and the p-value and implied by this rank for each outcome and control set. Idaho has the first, second, or third largest post-pre MSPE ratio for the employment-population ratio for all age groups, and for the U-to-E transition rate for prime-age individuals as well as the nonparticipation rate from the CPS, and total employment from the CES and QCEW. The p-values imply the estimated effects on employment, U-to-E transitions, and the nonparticipation rate using Idaho as the treatment state are generally larger than 90 percent of the placebo estimates. These results suggest the estimated effects on employment, nonparticipation, and U-to-E transitions for Idaho are significantly larger than estimated placebo effects and the RWB program had a substantial effect on these outcomes.

We also present synthetic difference-in-differences estimates for the employment-population ratio, U-to-E transitions, and NILF-to-E transitions among individuals ages 25-54 and 16 and over in Appendix Table B1, and for the unemployment rate, nonparticipation rate, and total employment Appendix Table B2. The coefficients on Idaho X post May in columns (2), (3), (5), and (6), (including age and education controls and macro and Covid covariates) are generally similar in magnitude to the coefficients from the DD regressions with the same covariates reported in Tables 2 and 3. For the e-pop and U-to-E outcomes, the coefficients are generally statistically significant, particularly in specifications including Covid and macro controls. For the unemployment rate, nonparticipation rate, and total employment outcomes, fewer of the coefficients are statistically significant. The significant coefficients for nonparticipation and total employment using the QCEW imply the bonus program reduced nonparticipation and increased total employment in Idaho relative to other states

Finally, we present event-study style synthetic difference-in-differences estimates exploring the dynamic effects of the program on labor market outcomes. From Appendix Figure B1 Panels A, B, C, and D, return to work bonuses increased prime-age employment and U-to-E transitions. These effects are strongest in July and August, when people had to work to receive the bonus, and remain strong in September and October. From Panels E and F, the return to work bonuses significantly increased prime-age NILF-to-E transitions in Idaho, but this effect is almost entirely concentrated in June 2020. From Appendix Figure B2, the effects of bonuses for all individuals ages 16 and over on employment, U-to-E transitions, and NILF-to-E transitions are smaller in magnitude than for prime-age individuals. The effects moderate more after July and August for all individuals 16 and over, but are still positive and significant though September and October of 2020. From Appendix Figure B3, the state nonparticipation rate significantly decreased and state aggregate employment significantly increased following the introduction of the program.

Section 6. Discussion and Conclusion

In this paper, we examine the labor-market effects of the Idaho Return to Work Bonus program. To the best of our knowledge, this is the first paper to study the effects of reemployment bonuses on the U.S. labor market outside an experimental setting.

The program, announced in June 2020, provided bonuses of up to $1,500 to non-employed and unemployed Idaho residents if they returned to work between April 20 and July 15, 2020. Using difference-in-difference, triple-difference, and synthetic control methods, we present a variety of estimates that suggest this program may have had a qualitatively important effect on supporting employment in Idaho.

The Return to Work Bonus program may have increased labor supply by increasing the return to working relative to receiving unemployment benefits and by compensating workers for Covid risks. In addition, it may be that for some groups of workers, market wage offers for job vacancies fell during the months the program was in effect (Bils, 1985; Pissarides, 2009). So the RWB payment may have been necessary to meet some workers' reservation wages. Finally, relative to the rest of the nation, labor demand (as measured by job openings) fell substantially less in Idaho in the spring of 2020, so a program focused on increasing labor supply was relatively better positioned to increase employment.

Across demographic samples, results using difference-in-differences and triple-difference models suggest that the program is associated with a 1 to 3 percent increase in the probability of employment, a 6 to 22 percent increase in unemployment-to-employment transitions, and a 1 to 4 percent increase in transitions into employment from nonparticipation. Our analysis of state-level outcomes produces similar results: the program's enactment is associated with a 2 to 3 percent increase in the employment-population ratio, a 2 to 6 percent increase in nonfarm payroll-survey employment, a 1 percent reduction in the unemployment rate, and a 1 to 3 percent reduction in the rate of nonparticipation. Synthetic control estimates produce qualitatively similar results and are robust to a variety of specifications. Synthetic differences-in-differences estimates produce results similar to both our difference-in-differences and synthetic control results.

Focusing on difference-in-difference estimates, we find the employment-population ratio rose by 2.9 to 3.2 percentage points, the unemployment rate fell by 0.4 to 0.8 percentage points, and the nonparticipation rate fell by 2.4 to 2.5 percentage points in Idaho relative to other states following the introduction of the RWB program. To place those magnitudes in perspective, during the 2001 recession, the national employment-population ratio fell by 1.3 percentage points, the unemployment rate increased by 1.2 percentage points, and the nonparticipation rate increased by 0.5 percentage points. So if the results from Idaho were to generalize to the economy as a whole — a strong claim that, to be clear, we are not making — then they would not be enough to arrest a moderate recession, but they could meaningfully accelerate labor market recovery.

While this paper documents the effect of return to work bonuses on employment, additional research is needed to investigate the welfare effects of return to work bonuses. Traditional unemployment insurance (UI) benefits might increase welfare by providing liquidity to unemployed workers, allowing them to smooth consumption (Chetty, 2008), and might allow unemployed workers to find jobs with better match quality (Farooq, Kugler, and Muratori, 2022). At the same time, traditional UI benefits might reduce welfare by "subsidizing unproductive leisure" (Gruber, 2007).

These complicated welfare dynamics suggest that accelerating transitions from unemployment to employment may or may not be welfare enhancing. Like traditional UI benefits, return to work bonuses offer liquidity to unemployed workers. But accelerating the transition to employment could be welfare enhancing if it truncates a period of unproductive leisure, or it could be welfare reducing if it results in worse match quality.[13] An additional area

[13] Using a dynamic job search model, Komatsu (2023) finds combining UI benefits with a reemployment bonus of roughly 50 percent of weekly wages would substantially increase welfare by mitigating the moral hazard associated with traditional UI benefits while preserving the ability of workers to smooth consumption.

for future work is the effects of the adoption of return to work bonuses — either alongside traditional UI benefits or in partial substitution for them — on state public finances and on UI program use.

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Tables and Figures

Table 1. Unadjusted Differences for Idaho and Other States Among Individuals Ages 25-54, Ages 16 and Over, and State Aggregate Outcomes

	(1) January-May 2020	(2) June-October 2020	(3) Change	(4) Change Relative to Other States	(5) Percent Change Relative to Baseline
Panel A. Ages 25-54					
Employed					
Residents of All Other States	0.762	0.749	-0.013		-1.71
Idaho Residents	0.781	0.796	0.015	0.028	1.92
U-E Transition					
Residents of All Other States	0.268	0.288	0.020		7.46
Idaho Residents	0.292	0.465	0.173	0.153	59.25
NILF-E Transition					
Residents of All Other States	0.077	0.084	0.008		10.05
Idaho Residents	0.054	0.097	0.043	0.035	79.89
Panel B. Ages 16 and Over					
Employed					
Residents of All Other States	0.571	0.565	-0.006		-1.05
Idaho Residents	0.597	0.617	0.020	0.026	3.35
U-E Transition					
Residents of All Other States	0.249	0.281	0.032		12.85
Idaho Residents	0.284	0.428	0.144	0.112	50.70
NILF-E Transition					
Residents of All Other States	0.040	0.045	0.005		11.69
Idaho Residents	0.035	0.057	0.023	0.018	65.32
Panel C. State-level Outcomes					
Unemployment Rate CPS					
Residents of All Other States	0.0491	0.0548	0.01		11.61
Idaho Residents	0.0345	0.0347	0.00	-0.01	0.58
Nonparticipation Rate CPS					
Residents of All Other States	0.380	0.380	0.00		0.00
Idaho Residents	0.368	0.348	-0.02	-0.02	-5.43
Employment CES (Thousands)					
Residents of All Other States	6331.1	6124.3	-206.80		-3.27
Idaho Residents	746.8	755.0	8.20	215.00	1.10
Employment QCEW (Thousands)					
Residents of All Other States	6236.7	6069	-167.70		-2.69
Idaho Residents	730.3	759.7	29.40	197.10	4.03

Notes: This table reports simple differences for our sample of individuals ages 25-54 and ages 16 and over living in Idaho or other states in the pre and post treatment period. Entries for employed, unemployed to employed, and not in labor force to employed in Panels A and B summarize individual data from the Current Population Survey (CPS). Entries in Panel C summarize state aggregate data from the CPS, Current Employment Statistics, and Quarterly Census of Employment and Wages. Column 1 reports the average value between January and May 2020 for each row, column 2 reports the average value between June and October 2020, and column 3 reports the difference between the two. Column 4 reports the change in the average value for each row relative to the relevant non-ending value. Column 5 reports the change in the average value from January-May 2020 to June-October 2020. Averages are weighted by the relevant state population.

Table 2. Estimated Effects of Idaho Return to Work Bonus Program on the Probability of Employment and Monthly Transitions From Unemployment or Out of the Labor Force to Employment Using Difference-in-Differences or Triple Differences Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Ages 25-54			Ages 16 and Over		
Estimator	DD	DD	DDD	DD	DD	DDD
Panel A. Dependent Variable: Employed						
Idaho X Post May 2020	0.034	0.032	0.013	0.030	0.027	0.018
p-value clustered SEs	(0.000)	(0.000)	(0.019)	(0.000)	(0.000)	(0.000)
p-value placebo test	[0.020]	[0.020]	[0.314]	[0.020]	[0.020]	[0.196]
Observations	388,062	388,062	834,146	851,819	851,819	1,805,695
Panel B. Dependent Variable: Unemployed to Employed Robust Transition						
Idaho X Post May 2020	0.139	0.182	0.220	0.101	0.133	0.063
p-value clustered SEs	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.018)
p-value placebo test	[0.098]	[0.039]	[0.098]	[0.098]	[0.039]	[0.333]
Observations	8,377	8,377	13,640	15,430	15,430	25,393
Panel D. Dependent Variable: Not in Labor Force to Employed Transition						
Idaho X Post May 2020	0.037	0.042	0.006	0.021	0.024	0.019
p-value clustered SEs	(0.000)	(0.000)	(0.280)	(0.000)	(0.000)	(0.000)
p-value placebo test	[0.118]	[0.078]	[0.490]	[0.118]	[0.078]	[0.039]
Observations	45,279	45,279	98,063	230,406	230,406	484,463
State and Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Age and Education FE	Yes	Yes	Yes	Yes	Yes	Yes
Covid Controls	No	Yes	No	No	Yes	No
Macro Controls	No	Yes	Yes	No	Yes	Yes
State-Month, Month-Year, and State-Year FE	No	No	Yes	No	No	Yes

Notes: This table reports regression results measuring the effect of the Idaho Return to Work Bonus program, introduced in June 2020, on individual labor market outcomes. The control group consists all of other states and Washington, D.C. The sample is from the Basic Monthly Current Population Survey (CPS) from January - October 2020 and January - October 2019. The outcome in Panel A is whether or not an individual is employed. In Panel B, the outcome is whether an unemployed individual in month m-1 becomes employed in month m, and not unemployed in month m+1. In Panel C, the outcome is whether an individual not in the labor force in month m-1 becomes employed in month m. Columns 1-3 all include individuals ages 25 to 54. Columns 4-6 include all individuals ages 16 and over. Columns 1, 2, 4, and 5 present results from difference-in-differences regressions, and Columns 3 and 6 present results from triple differences regressions with 2019 as the base year. All specifications include state, month, age, and education fixed effects. Covid controls include the asinh of monthly new Covid-19 cases and the monthly average policy stringency index from the Oxford Covid-19 Government Response Tracker (OxCGRT), Macro controls include quarterly state income per capita from the Bureau of Economic Analysis (BEA), and the monthly state job opening rate from the Job Openings and Labor Turnover Survey (JOLTS) The triple difference regressions in columns 3 and 6 also include year, month-year, state-year and state-month fixed effects. The p-values in parentheses are calculated using standard errors clustered at the state level. The p-values in brackets are calculated from a placebo test assuming that the states other than Idaho are the treated state.

Table 3. Estimated Effects of Idaho Return to Work Bonus Program on State-Level Labor Market Outcomes Using Difference in Differences or Triple Differences Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel A				
Dependent Variable	EPOP CPS 16+			UR CPS 16+			NILF CPS 16+		
Estimator	DD	DD	DDD	DD	DD	DDD	DD	DD	DDD
Idaho X Post May 2020	0.032	0.029	0.021	-0.008	-0.004	-0.008	-0.024	-0.025	-0.013
p-value clustered SEs	(0.000)	(0.000)	(0.001)	(0.001)	(0.037)	(0.052)	(0.000)	(0.000)	(0.002)
p-value placebo test	[0.059]	[0.020]	[0.176]	[0.667]	[0.725]	[0.725]	[0.980]	[0.980]	[0.824]
					Panel B				
Dependent Variable	Ln Employment CES Total Nonfarm			Ln Employment QCEW					
Estimator	DD	DD	DDD	DD	DD	DDD			
Idaho X Post May 2020	0.037	0.024	0.027	0.058	0.046	0.028			
p-value clustered SEs	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)			
p-value placebo test	[0.039]	[0.078]	[0.078]	[0.020]	[0.039]	[0.078]			
Observations	510	510	1,020	510	510	1,020	510	510	1,020
State and Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age and Education Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covid Controls	No	Yes	No	No	Yes	No	No	Yes	No
Macro Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
State-Month, Month-Year, and State-Year FE	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table reports regression results measuring the effect of the Idaho Return to Work Bonus program, introduced in June 2020, on state-level labor market outcomes. The control group consists of all other states and Washington, D.C. The samples are state-month aggregates from the Basic Monthly Current Population Survey (CPS), the Current Employment Statistics (CES), and the Quarterly Census of Employment and Wages (QCEW) from January - October 2020 and January - October 2019. Columns 1-3 of Panel A use the state employment-population ratio from the CPS as the dependent variable. Columns 4-6 of Panel B use the state unemployment rate from the CPS. Columns 7-9 of Panel A use the share not in the labor force from the CPS. Columns 1-3 of Panel B use the log of total monthly nonfarm employment from the CES. Columns 4-6 of Panel B use the log of total monthly employment from the QCEW. Columns 1, 2, 4, 5, 7, and 8 present results from difference-in-differences regressions, and Columns 3, 6, and 9 present results from triple differences regressions with 2019 as the base year. All specifications include state and month fixed effects. Age and education controls are the average age of individuals ages 16 and over, and the share of individuals ages 16 and over with less than high school, high school, some college, and BA or higher education by state. Covid controls include the asinh of state monthly new Covid-19 cases and the monthly average state policy stringency index from the Oxford Covid-19 Government Response Tracker (OxCGRT). Macro controls include quarterly state income per capita from the Bureau of Economic Analysis (BEA), and the monthly state job opening rate from the Job Openings and Labor Turnover Survey (JOLTS). The triple difference regressions in columns 3, 6, and 9 also include year, month-year, state-year and state-month fixed effects. The p-values in parentheses are calculated using standard errors clustered at the state level. The p-values in brackets are calculated from a placebo test assuming that the states other than Idaho are the treated state.

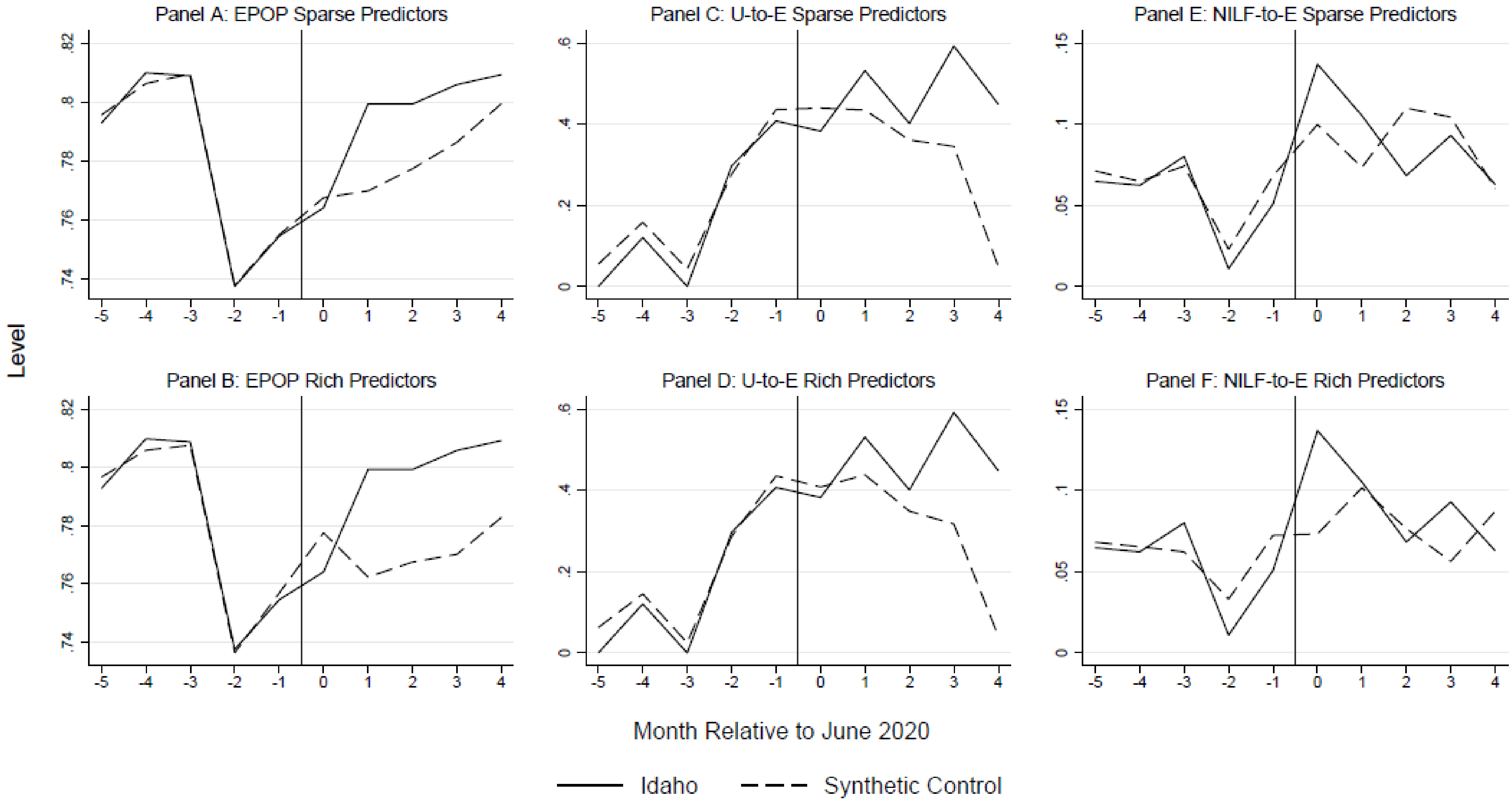

Figure 1. Synthetic Control Estimates of the Effects of the Idaho Return to Work Bonus Program on Prime Age Employment, Unemployed to Employed Transitions, and Not in the Labor Force to Employed Transitions Using Sparse or Rich Predictors. This figure plots the evolution of employment, unemployed to employed transitions, and not in the labor force to employed transitions for individuals ages 25-54 around the June 2020 introduction of the Idaho Return to Work Bonus program for Idaho and synthetic control groups constructed using the other 49 states and Washington, D.C. Panels A and B plot the employment population ratio, Panels C and D plot unemployed to employed transitions, and Panels E and F plot not in the labor force to employed transitions. Sparse predictors include levels of the outcome, as well as the average age and the share of prime-age individuals with less than high school, high school, some college, and BA or higher education by state from January-April 2020. Rich predictors add new monthly Covid cases, state policy stringency, income per capita and the job opening rate by state from January-April 2020 to the set of predictors used to generate the synthetic control estimate. June 2020 is month 0.

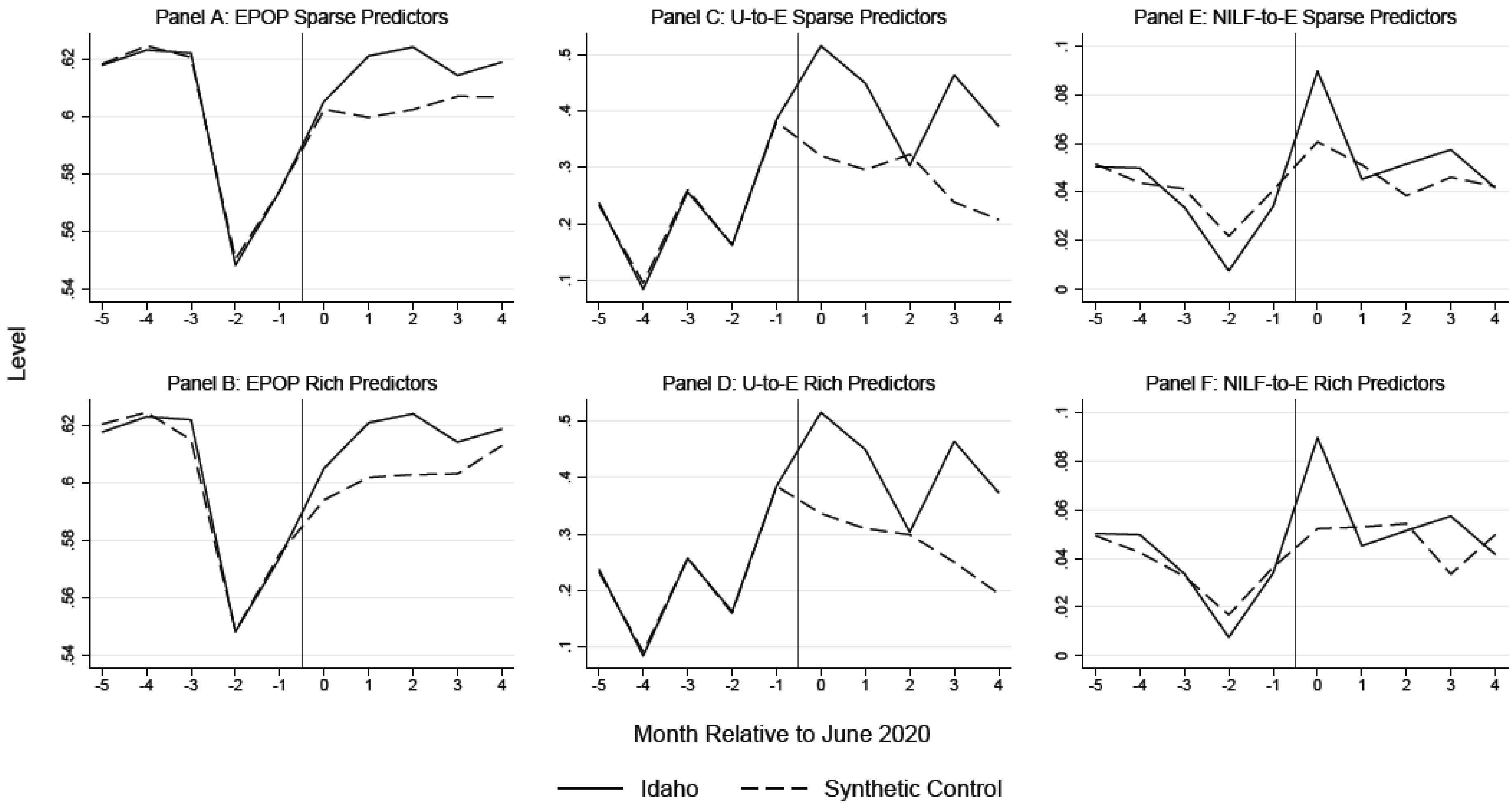

Figure 2. Synthetic Control Estimates of the Effects of the Idaho Return to Work Bonus Program on the Employment-Population Ratio, Unemployed to Employed, and Not in the Labor Force to Employed Transitions for Individuals Ages 16 and Over Using Sparse and Rich Predictors. This figure plots the evolution of employment, unemployed to employed and not in the labor force to employed transitions for individuals ages 16 and over around the June 2020 introduction of the Idaho Return to Work Bonus program for Idaho and synthetic control groups constructed using the other 49 states and Washington, D.C. Panels A and B plot the employment-population ratio, Panels C and D plot unemployed to employed transitions. Panels E and F plot not in the labor force to employed transitions. Sparse predictors include levels of the outcome, as well as the average age and the share of individuals with less than high school, high school, some college, and BA or higher education, by state from January-April 2020. Rich predictors add new monthly Covid cases, state policy stringency, income per capita and the job opening rate by state from January-April 2020 to the set of predictors used to generate the synthetic control estimate. June 2020 is month 0.

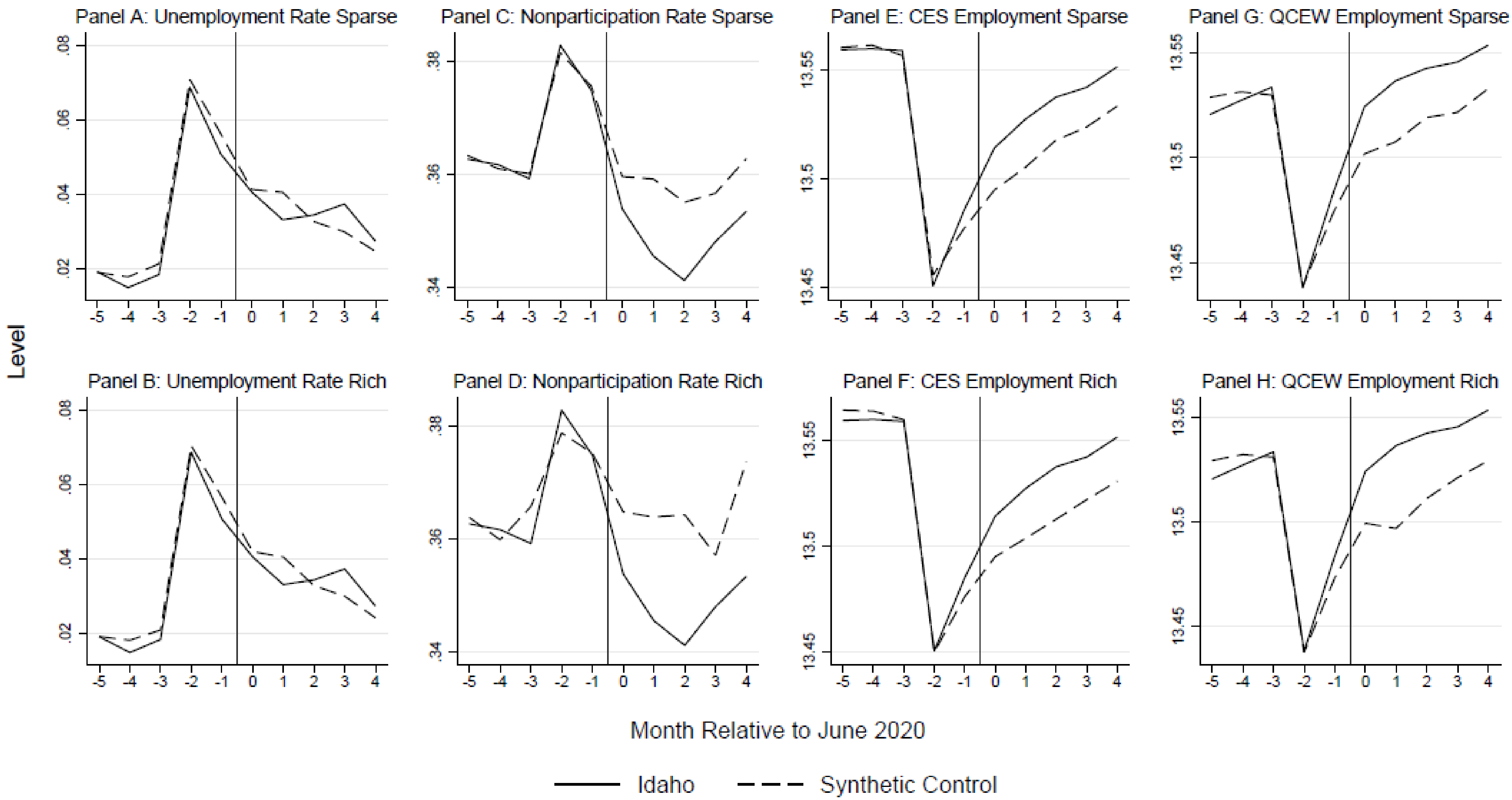

Figure 3. Synthetic Control Estimates of the Effects of the Idaho Return to Work Bonus Program on the State Unemployment Rate, Nonparticipation Rate and Log Total CES and QCEW Employment Using Sparse and Rich Predictors. This figure plots the evolution of the state unemployment rate, nonparticipation rate, and log of total employment from the CES and QCEW around the June 2020 introduction of the Idaho Return to Work Bonus program for Idaho and synthetic control groups constructed using the other 49 states and Washington, D.C. Panels A and B plot the unemployment rate, Panels C and D plot the nonparticipation rate, and Panels E and F plot the log of total nonfarm employment from the CES, and Panels G and H plot the log of total employment from the QCEW. Sparse predictors include levels of the outcome, as well as the average age and the share of individuals with less than high school, high school, some college, and BA or higher education by state from January-April 2020. Rich predictors add new monthly Covid cases, state policy stringency, income per capita and the job opening rate by state from January-April 2020 to the set of predictors used to generate the synthetic control estimate. June 2020 is month 0.

Appendix

A. Additional Information on the Return to Work Bonus Program, and additional Synthetic Control Estimates

This appendix provides some background on previous reemployment bonuses in the U.S. and around the world, additional details on how bonuses were determined in the Idaho Return to Work Bonus program, and some additional synthetic controls results to supplement what is in the main text.

Previous Reemployment Bonuses in the United States and Elsewhere

During the 1980s, Illinois, Washington, Pennsylvania, and New Jersey experimented with offering reemployment bonuses to Unemployment Insurance (UI) claimants. Between 1984 and 1985, Illinois conducted the Claimant Bonus Experiment. A random sample 4,186 of new unemployment insurance claimants were told they would qualify for a $500 cash bonus (roughly four times the average UI benefit) if they found a job working at least 30 hours per week within 11 weeks of filing their UI claim and remained in that job for at least four months. Analyzing the effects, Woodbury and Speigelman (1987) find individuals informed of the reemployment bonuses reduced benefits claimed by an average of $158-$194 per claimant, unemployment duration by 1.15 weeks, 5.5 percent more ended their UI spell within 11 weeks, and 3.2 percent fewer exhausted UI benefits, even though only 570 of the 4,186 individuals in the treated group actually claimed the bonus. Summarizing the effects of the Illinois bonus program, Meyer (1996) concludes though Illinois bonus reduced the duration of unemployment spells without reducing post-program earnings, many of those who received bonuses would have exited unemployment quickly without them.

New Jersey implemented a reemployment bonus in July 1986. In contrast to the flat Illinois bonus paid immediately on entering a new job, the New Jersey bonus was equal to half of the remaining UI benefit (which averaged $1,644). The bonus was offered seven weeks after the initial UI claim with the maximum bonus declining by ten percent each week later reemployment occurred, and individuals returning to their pre-UI employer were ineligible.[14] Decker (1994) compares the effects of the two programs and finds both the New Jersey and Illinois

[14] See Table 1 of Meyer (1995) for a detailed comparison of the features of each bonus program.

reemployment bonuses generated similar increases in the UI exit rate during the period in which claimants could qualify for the bonus. While the Illinois reemployment bonus had a substantial positive long-term impact on claimants, the New Jersey bonus had little effect on long-term claimants who exhausted their UI benefits. Pennsylvania and Washington also experimented with reemployment bonuses in 1988 and 1989. In contrast to the flat bonus amount in Illinois, Washington and Pennsylvania experimented with multiple bonuses ranging from two to six times the average weekly benefit amount. Examining the impacts of reemployment bonuses implemented in Pennsylvania and Washington in 1988 and 1989, Decker and O'Leary (1995) find the bonuses reduced UI receipt, but the impacts were not as large as estimated in Illinois.

Several countries outside the United States have trialed reemployment bonuses for people collecting unemployment insurance. Van Der Klaauw and Van Ours (2013) examine the effects of reemployment bonuses and reductions in UI benefits on exit from unemployment insurance in the Netherlands from 2000–2003. They find benefit reductions stimulated exit from unemployment insurance but the reemployment bonuses did not. Ahn (2018) examines the effects of larger reemployment bonuses for individuals over 55 on reemployment in South Korea using a regression discontinuity design. He finds reemployment bonuses increase the probability UI claimants find a new job early in their unemployment spell, reduce the average UI spell length, and do not negatively affect match quality. Huang and Yang (2021) examine the impacts of UI benefits and reemployment bonuses on search effort in Taiwan. Extending UI reduces exit from unemployment and generates negative fiscal externalities while reemployment bonuses increase job-finding rates and generate positive fiscal externalities.

Table A1. Bonuses Available in Idaho Return to Work Bonus Program

	Hours Worked per Week During Four Week Qualifying Period	
Date Returned to Work	20 Hours	30 Hours
April 20 - July 1	$750	$1,500
July 2 - July 8	$500	$1,000
July 9 - July 15	$250	$500

Source: https://rebound.idaho.gov/return-to-work-bonuses/ Last updated February 9, 2022. Last accessed May 3, 2022 using the Wayback Machine Archive.

Table A2. Measures of the Relative Size of Idaho's Return to Work Bonus Program in 2020 Compared With Programs Enacted in Other States in 2021

State	AZ	CO	CT	ID	KY	ME	MT	NH	NM	OK	VA
Panel A. Program Size Relative to Labor Market Conditions in February 2020											
Potential Bonuses % of Population	2.6	2.7	0.3	4.8	0.4	0.6	1.5	0.9	0.6	0.7	0.1
Potential Bonuses % of Unemployed	99.8	115.4	10.5	319.5	21.9	18.8	52.6	41.7	18.5	37.5	5.7
Potential Bonuses % of Nonemployed	6.7	8.2	0.9	12.7	1.0	1.5	3.7	2.6	1.3	1.7	0.3
Bonuses Disbursed % of Population	0.5	0.4	0.3	2.1	*	0.0	0.4	0.1	*	0.3	0.0
Bonuses Disbursed % of Unemployed	18.1	17.2	10.5	136.7	*	0.9	12.9	3.7	*	15.0	0.5
Bonuses Disbursed % of Nonemployed	1.2	1.2	0.9	5.4	*	0.1	0.9	0.2	*	0.7	0.0
Panel B. Program Size Relative to Labor Market Conditions in Month Before Implementation											
Potential Bonuses % of Population	2.5	2.7	0.3	4.8	0.4	0.6	1.4	0.9	0.6	0.7	0.1
Potential Bonuses % of Unemployed	53.0	84.0	8.9	93.8	9.8	25.9	64.1	36.7	14.8	27.6	3.0
Potential Bonuses % of Nonemployed	5.9	7.3	0.8	11.2	0.9	1.5	3.9	2.2	1.3	1.6	0.2
Bonuses Disbursed % of Population	0.5	0.4	0.3	2.0	*	0.0	0.4	0.1	*	0.3	0.0
Bonuses Disbursed % of Unemployed	9.6	12.6	8.9	40.1	*	1.3	15.7	3.3	*	11.1	0.2
Bonuses Disbursed % of Nonemployed	1.1	1.1	0.8	4.8	*	0.1	1.0	0.2	*	0.6	0.0

Notes: This table displays information on the size of the Idaho Return to Work Bonus program relative to population compared with the bonus programs announced by other states in 2021. We source data on program funding, number of bonuses disbursed, and amount of funding disbursed through direct contact with state agencies administering the bonus programs and data collection is ongoing. We calculate the number of potential bonuses as the total funds allocated divided by the dollars awarded per bonus if working full time. Population, labor force, and unemployment statistics from the month prior to implementation and from February 2020 for ages 16 and over are from the Current Population Survey. If the bonus was implemented during or after week containing the 12th day of the month, we use labor market information from the month of implementation from the CPS since these data reflect the most recent conditions before the bonus was implemented. Michigan provided a return to work bonus for individuals enrolled in the state's workshare program but has not provided information on total program funding or amount disbursed, so we drop it from the table. Where entries are missing, as indicated by (*), we have not yet received information on the number of bonuses or amount of funding disbursed and data collection is ongoing.

Table A3. Idaho Synthetic Control Weights from Analyses Using Sparse Predictors

State	EPOP 25-54	EPOP 16+	U-E 25-54	U-E 16+	N-E 25-54	N-E 16+	UR CPS	NILF CPS	EMP CES	EMP QCEW
AL	0	0	0	0	0	0	0.363	0	0	0
AK	0.274	0	0	0	0	0	0	0	0	0
AZ	0	0	0	0	0	0	0	0	0.062	0.034
CO	0	0	0	0.001	0	0	0	0	0	0.100
HI	0	0	0	0	0.374	0	0	0	0	0
IL	0.131	0	0	0	0	0	0	0	0	0
IA	0	0	0	0	0.188	0	0	0	0	0
KS	0	0.198	0	0	0	0	0	0	0	0.061
KY	0	0	0.649	0	0	0	0	0	0	0
ME	0.002	0	0	0	0	0	0	0	0	0
MI	0.033	0	0	0	0	0.471	0	0.255	0	0
MN	0	0	0	0	0.008	0	0	0	0	0
MI	0.030	0.071	0	0.042	0.006	0	0	0.193	0	0
MO	0.202	0	0	0	0	0	0	0	0	0
MT	0	0.264	0	0	0	0	0	0.083	0	0
NE	0	0	0.023	0	0	0	0	0	0	0
NV	0	0.164	0	0	0.323	0.365	0	0	0	0
NH	0	0	0	0.181	0	0	0	0.029	0	0
NM	0	0.022	0	0	0	0	0	0	0	0
ND	0.165	0	0	0.241	0	0	0.178	0.158	0.133	0.005
SD	0	0.071	0	0.449	0	0	0.303	0.085	0.364	0.599
TX	0	0	0	0	0	0.164	0	0	0	0
UT	0.085	0.209	0	0.047	0	0	0.156	0.117	0.371	0.198
WA	0	0	0	0.036	0	0	0	0.001	0	0
WI	0.079	0	0.239	0	0	0	0	0	0	0
WY	0	0.001	0.088	0.003	0.101	0	0	0.079	0.069	0.003

Notes: This table displays the synthetic control weights for each outcome using the sparse set of predictors including levels of the outcome, as well as the average age of individuals and the share of individuals with less than high school, high school, some college, and BA or higher education by state from January - April 2020.

Table A4. Idaho Synthetic Control Weights from Analyses Using Rich Predictors

State	EPOP 25-54	EPOP 16+	U-E 25-54	U-E 16+	N-E 25-54	N-E 16+	UR CPS	NILF CPS	EMP CES	EMP QCEW
AL	0	0	0	0	0	0	0.331	0	0	0
AK	0.164	0.216	0	0	0	0	0	0	0	0
AR	0	0	0	0.030	0	0	0	0	0	0
GA	0	0	0	0	0	0	0	0	0	0
HI	0	0	0	0	0	0.452	0	0	0	0
KS	0.269	0.154	0	0	0	0	0	0	0.003	0
KY	0	0	0.583	0	0.054	0	0	0	0	0
ME	0.311	0	0.015	0.001	0.376	0	0	0	0	0
MS	0	0	0	0.052	0	0	0	0.067	0	0
MT	0	0	0	0	0.025	0	0.012	0.110	0.292	0.166
NE	0	0	0	0.072	0	0	0	0	0	0
NV	0	0	0	0	0	0.270	0	0	0	0
NH	0.055	0.324	0	0.058	0	0	0	0	0	0
NJ	0	0	0	0	0	0.278	0	0	0	0
NM	0.201	0.173	0	0	0	0	0	0	0.167	0
ND	0	0	0	0.279	0.125	0	0.238	0	0	0
OK	0	0	0	0	0.206	0	0	0.31	0.006	0.107
OR	0	0	0	0.138	0	0	0	0	0	0
SD	0	0	0	0.227	0	0	0.214	0	0	0
UT	0	0.133	0	0.064	0	0	0.2	0.252	0.377	0.431
VT	0	0	0	0	0	0	0.005	0	0	0
WA	0	0	0	0.079	0	0	0	0	0	0
WV	0	0	0.267	0	0.215	0	0	0.071	0	0
WY	0	0	0.135	0	0	0	0	0.190	0.156	0.297

Notes: This table displays the synthetic control weights for each outcome using the rich set of predictors including levels of the outcome as well as the average age of individuals and the share of individuals with less than high school, high school, some college, and BA or higher education by state as well as state-level Covid and macro controls described in the paper from January - April 2020.

Table A5. Statistics For Synthetic Control Fit For Idaho Relative to Placebos For Each Outcome and Control Set

Outcome	Predictors	RMSPE Jan-May 2020	MSPE Jun-Oct 2020	Post-pre MSPE Ratio	Post Pre Ratio Rank	Fishers Exact P-value	Z-score
EPOP Ages 25-54	Sparse	5.54E-06	4.57E-04	82.519	1	0.020	6.563
EPOP Ages 16+	Sparse	2.81E-06	2.83E-04	100.595	1	0.020	6.501
U-E Ages 25-54	Sparse	1.80E-03	5.80E-02	32.304	2	0.039	0.412
U-E Ages 16+	Sparse	6.32E-03	2.54E-02	4.026	12	0.235	0.179
NILF-E Ages 25-54	Sparse	3.18E-04	7.20E-04	2.266	31	0.608	-0.359
NILF-E Ages 16+	Sparse	1.98E-04	8.32E-05	0.420	46	0.902	-0.586
EPOP Ages 25-54	Rich	3.56E-05	1.12E-03	31.498	1	0.020	5.334
EPOP Ages 16+	Rich	2.95E-05	2.32E-04	7.857	2	0.039	2.418
U-E Ages 25-54	Rich	1.12E-03	6.33E-02	56.443	1	0.020	5.658
U-E Ages 16+	Rich	4.40E-03	2.18E-02	4.962	10	0.196	0.403
NILF-E Ages 25-54	Rich	8.93E-04	5.05E-04	0.566	45	0.882	-0.919
NILF-E Ages 16+	Rich	2.58E-04	1.75E-04	0.676	46	0.902	-0.559
State UR	Sparse	8.52E-06	3.01E-05	3.530	7	0.137	0.069
State NILF	Sparse	6.12E-06	1.35E-04	21.995	3	0.059	3.131
State Emp CES	Sparse	7.90E-05	3.84E-04	4.864	1	0.020	3.634
State Emp QCEW	Sparse	1.16E-04	6.00E-04	5.174	2	0.039	3.547
State UR	Rich	9.41E-06	3.01E-05	3.200	12	0.235	0.201
State NILF	Rich	3.07E-05	3.41E-04	11.125	2	0.039	3.306
State Emp CES	Rich	8.03E-05	4.98E-04	6.198	1	0.020	6.482
State Emp QCEW	Rich	1.40E-04	9.33E-04	6.645	1	0.020	5.123

Notes: This table presents information on the synthetic control fit using Idaho as the treated state compared with the synthetic control fit using other states as placebos for each outcome and set of controls. For each outcome, we present the estimated pre and post treatment MSPE for Idaho as the treated state, the ratio of post to pretreatment MSPE, the relative rank of this ratio, the p-value implied by this rank, and Z-score.

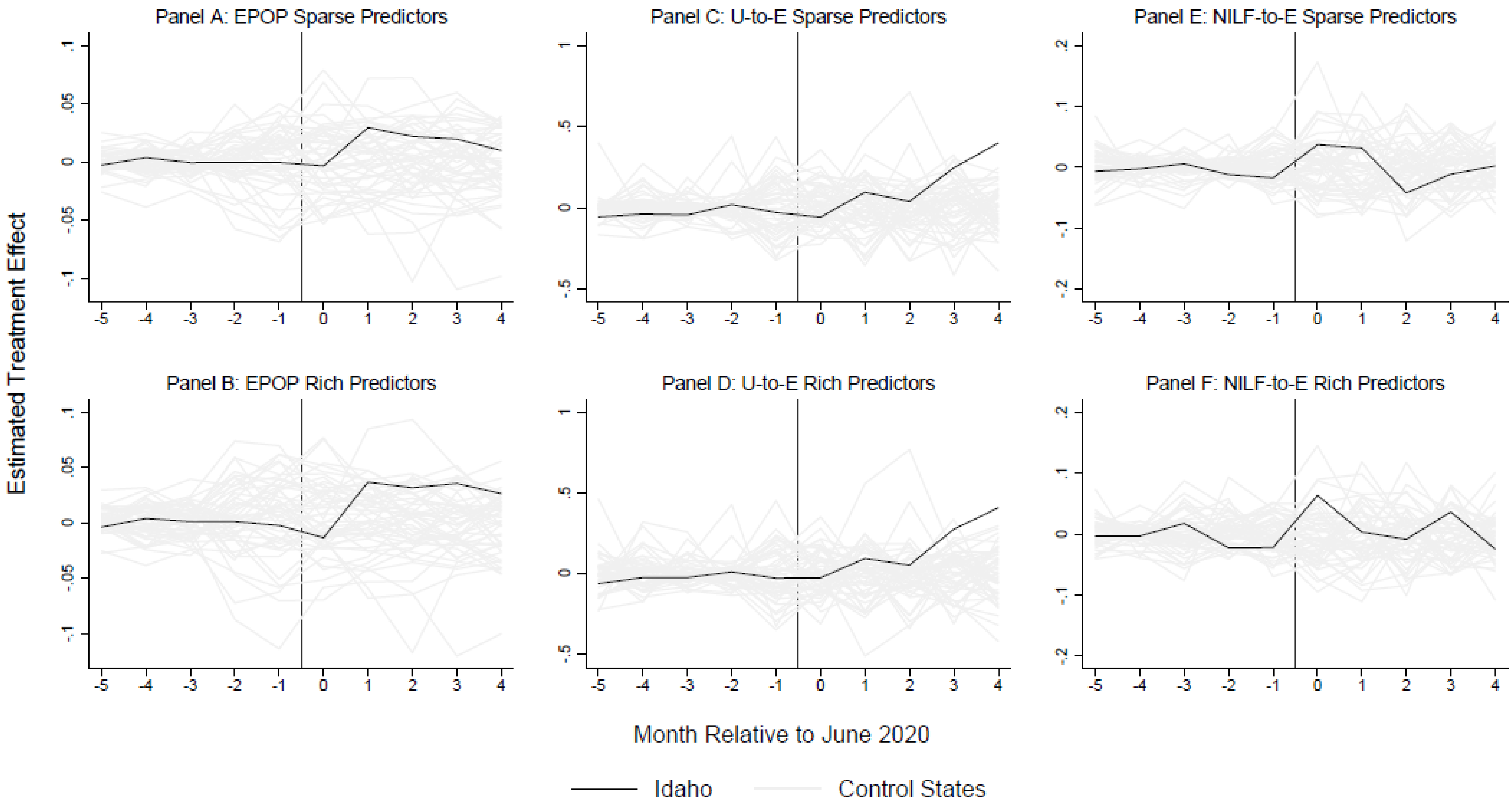

Figure A1. Synthetic Control Estimates of the Effects of the Idaho Return to Work Bonus Program on the Employment-Population Ratio, Unemployed to Employed Transitions, and Not in the Labor Force to Employed Transitions Compared with Estimated Placebo Effects For Individuals Ages 25-54 Using Sparse and Rich Predictors. This figure compares the differences in estimated effects of the Idaho Return to Work Bonus program between treated and synthetic control states. The black line represents the "true" treatment effect estimate calculated using Idaho as the treated state and all other states to construct the synthetic control. The light gray lines are estimated placebo treatment effects that we calculate by assigning one of the control states as the treated state and constructing a synthetic control state from the remaining control states. Panels A and B plot the employment population ratio, Panels C and D plot unemployed to employed transitions. Panels E and F plot not in the labor force to employed transitions. Models with sparse predictors (Panels A, C, and E) include pretreatment levels of the outcome, as well as the average age and the share of prime age individuals with less than high school, high school, some college, and BA or higher education by state from January-April 2020. Models with rich predictors (Panels B, D, and F) also include the state-level Covid and macro controls described in the paper as predictors. June 2020 is month 0.

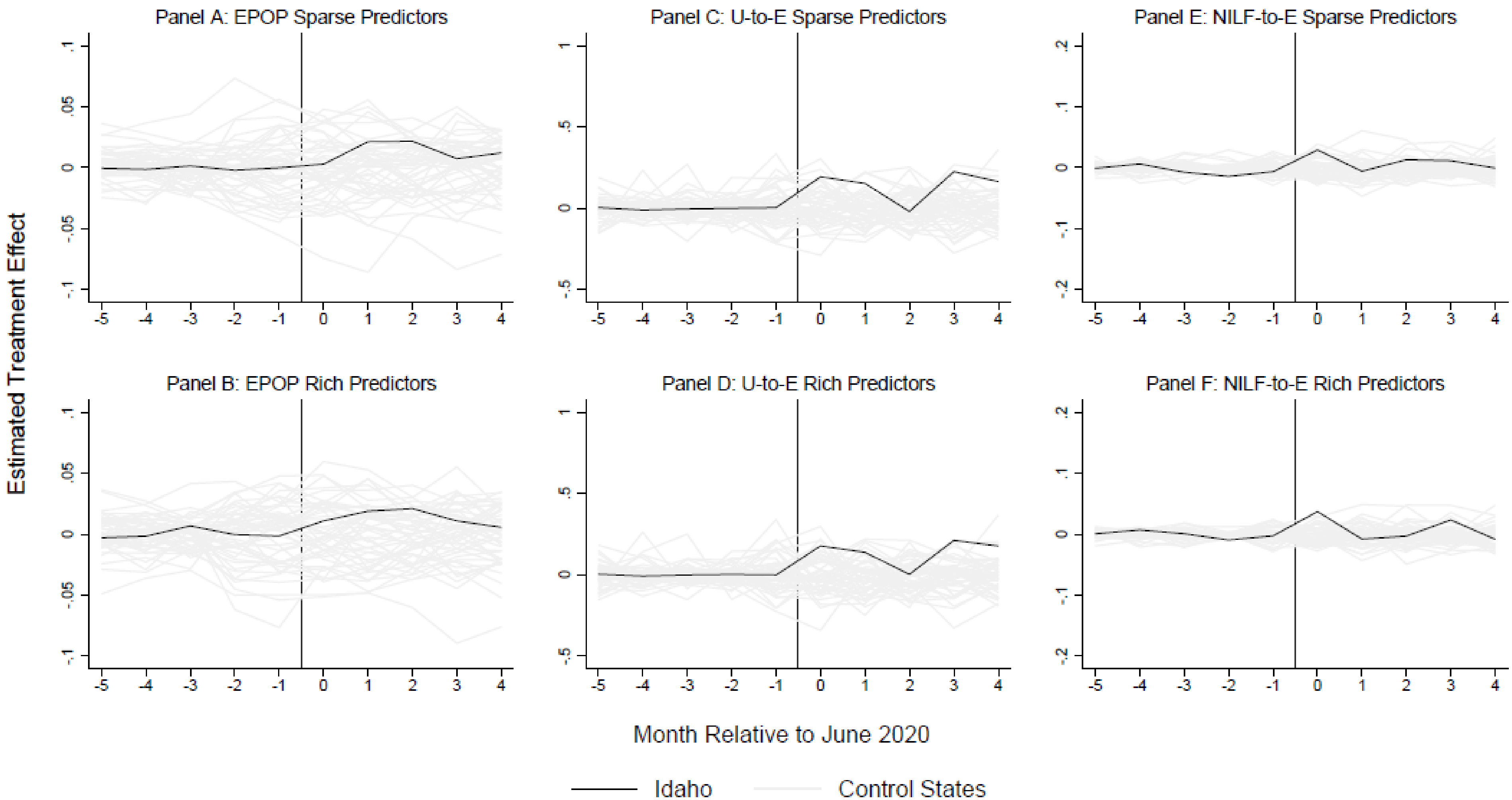

Figure A2. Synthetic Control Estimates of the Effects of the Idaho Return to Work Bonus Program on the Employment-Population Ratio, Unemployed to Employed Transitions, and Not in the Labor Force to Employed Transitions Compared with Estimated Placebo Effects For Individuals Ages 16 and Over Using Sparse and Rich Predictors. This figure compares the differences in estimated effects between treated and synthetic control states. The black line represents the "true" treatment effect estimate calculated using Idaho as the treated state and all other states to construct the synthetic control. The light gray lines are estimated placebo treatment effects that we calculate by assigning one of the control states as the treated state and constructing a synthetic control state from the remaining control states. Panels A and B plot the employment population ratio, Panels C and D plot unemployed to employed transitions. Panels E and F plot not in the labor force to employed transitions. Models with sparse predictors (Panels A, C, and E) include pretreatment levels of the outcome as well as the average age and the share of individuals with less than high school, high school, some college, and BA or higher education by state from January-April 2020. Models with rich predictors (Panels B, D, and F) also include the state-level Covid and macro controls described in the paper as predictors. June 2020 is month 0.

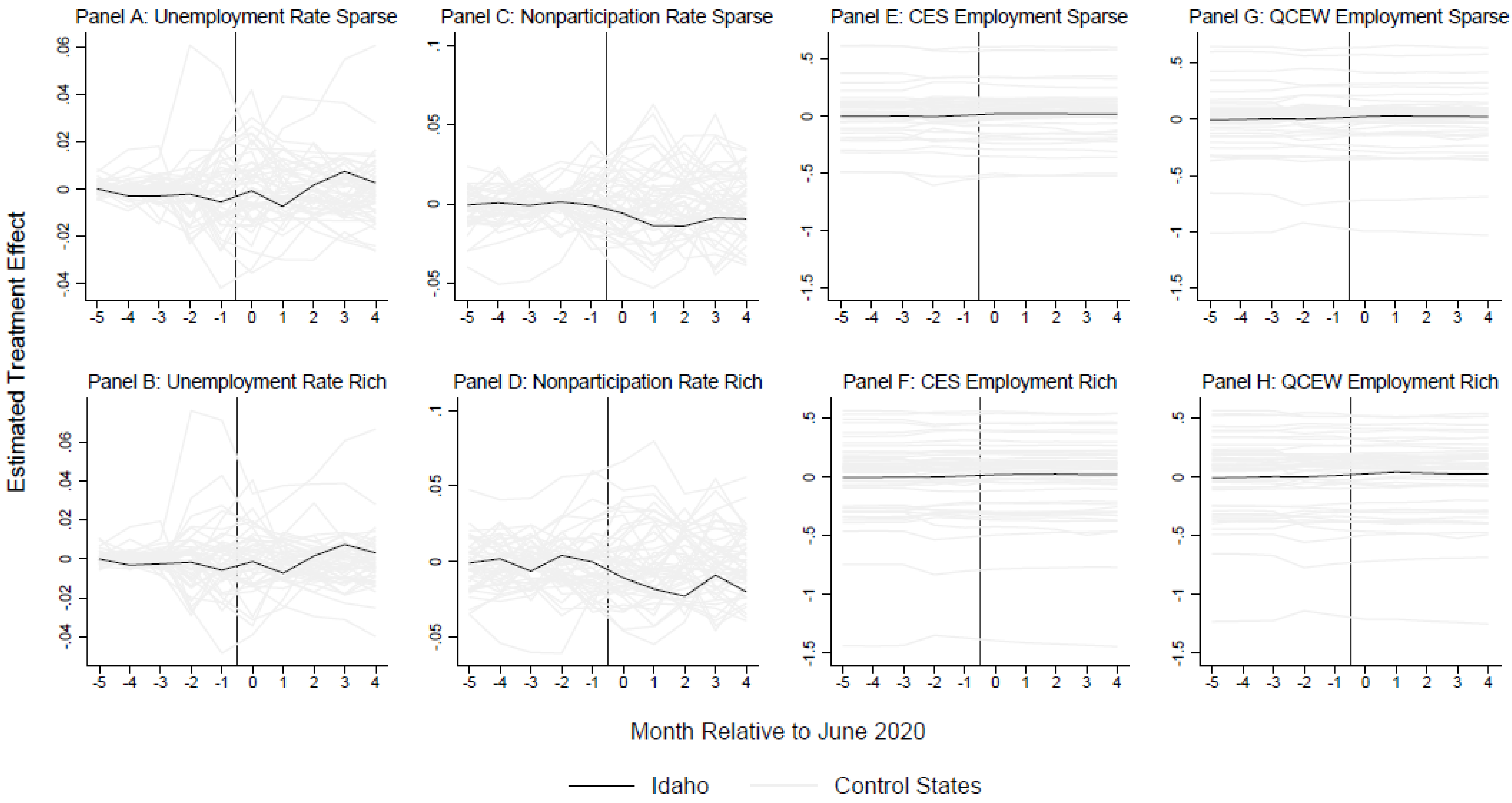

Figure A3. Synthetic Control Estimates of the Effects of the Idaho Return to Work Bonus Program on the State Unemployment Rate, Nonparticipation Rate and Log Total CES and QCEW Employment Compared with Estimated Placebo Effects Using Sparse and Rich Predictors. This figure compares the differences in estimated effects of the Idaho Return to Work Bonus program between treated and synthetic control states. The black line represents the "true" treatment effect estimate calculated using Idaho as the treated state and all other states to construct the synthetic control. The light gray lines are estimated placebo treatment effects that we calculate by assigning one of the control states as the treated state and constructing a synthetic control state from the remaining control states. Models with sparse predictors (Panels A, C, E, and G) include pretreatment levels of the outcome, as well as the average age and the share of individuals with less than high school, high school, some college, and BA or higher education by state from January-April 2020. Models with rich predictors (Panels B, D, F, and H) also include the state-level Covid and macro controls described in the paper as predictors. June 2020 is month 0.

B. Additional Robustness Analysis for Synthetic Control Estimates

Synthetic difference-in-differences

The synthetic difference-in-differences estimator from Arkhangelsky *et al.* (2021) adds state and month fixed effects to the synthetic control estimates. Adding these additional fixed effects can reduce bias and improve precision of synthetic control estimates, particularly in shorter panels. Relative to the two-way fixed effects estimator, the synthetic difference-in-differences estimator puts greater weight on control states with similar average outcomes to Idaho in the pretreatment period, and it emphasizes pretreatment months that are on average similar to the post-May 2020 treated months.

The standard difference-in-differences estimator with unit (α_i) and time (β_t) fixed effects assigns equal weights to all time periods and groups:

$$\left(\hat{\tau}^{did}, \hat{u}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,u,\alpha,\beta}{\operatorname{argmin}} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - u - \alpha_i - \beta_t - W_{it}\tau)^2 \right\}. \tag{B1}$$

The traditional synthetic control estimator chooses unit-specific weights $\widehat{\omega}_i^{sc}$ that seek to match treated and control units on pretreatment levels and pretreatment trends. The synthetic control estimator does not include unit fixed effects or time weights:

$$\left(\hat{\tau}^{sc}, \hat{u}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,u,\alpha,\beta}{\operatorname{argmin}} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - u - \beta_t - W_{it}\tau)^2 \widehat{\omega}_i^{sc} \right\}. \tag{B2}$$

The Synthetic Difference-in-Differences (SDID) estimator includes unit and time fixed effects like the DD estimator optimally chosen unit weights ($\widehat{\omega}_i^{sdid}$) like the synthetic control estimator and time weights ($\hat{\lambda}_t^{sdid}$):

$$\left(\hat{\tau}^{sdid}, \hat{u}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,u,\alpha,\beta}{\operatorname{argmin}} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - u - \alpha_i - \beta_t - W_{it}\tau)^2 \widehat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\}. \qquad \text{(B3)}$$

To generate the event study graphs, we follow the procedure outlined in section 4.4 of Clarke (2023). For each month *t*, we calculate:

$$(Y_t^{Tr} - Y_t^{Co}) - \left(Y_{baseline}^{Tr} - Y_{baseline}^{Co}\right), \qquad \text{(B4)}$$

where Y_t^{Tr} outcome for Idaho and Y_t^{Co} is the outcome for the synthetic control in month *t*. $Y_{baseline}^{Tr}$ and $Y_{baseline}^{Co}$ are the baseline values for Idaho and the synthetic control in the pretreatment period. In standard panel event studies, the baseline period is often the last period before treatment occurs.

With the SDID estimator, we can use the optimally chosen time weights ($\hat{\lambda}_t^{sdid}$) to calculate the average pretreatment outcome over all pretreatment months as the baseline rather than using the outcome from a single pretreatment month as the baseline.

$$Y_{baseline}^{Tr} = \sum_{t=1}^{T_{pre}} Y^{Tr} \hat{\lambda}_t^{sdid} \qquad\qquad Y_{baseline}^{Co} = \sum_{t=1}^{T_{pre}} Y^{Co} \hat{\lambda}_t^{sdid} \qquad \text{(B5)}$$

To calculate confidence intervals for each estimate, we use a block bootstrap and recalculate (B4) each iteration for each month t. We then calculate the confidence interval for each month based on the standard deviation of the 200 bootstrap resamples.

Table B1. Estimated Effects of Idaho Return to Work Bonus Program on the Employment-Population Ratio and Monthly Transitions From Unemployment or Not in the Labor Force to Employment Using Synthetic Difference-in-Differences

	(1)	(2)	(3)	(4)	(5)	(6)
Sample		Ages 25-54			Ages 16 and Over	
Estimator	SDID	SDID	SDID	SDID	SDID	SDID
	Panel A. Dependent Variable: Employment-Population Ratio					
Idaho X Post May 2020	0.030	0.036*	0.035**	0.020	0.023	0.022*
	(0.020)	(0.020)	(0.017)	(0.016)	(0.016)	(0.012)
Observations	510	510	510	510	510	510
	Panel B. Dependent Variable: Unemployed to Employed Transition Rate					
Idaho X Post May 2020	0.255**	0.258**	0.277***	0.144*	0.138*	0.150*
	(0.107)	(0.108)	(0.107)	(0.081)	(0.082)	(0.083)
Observations	510	510	510	510	510	510
	Panel C. Dependent Variable: Not in Labor Force to Employed Transition Rate					
Idaho X Post May 2020	0.031	0.026	0.032	0.015	0.017*	0.018*
	(0.025)	(0.025)	(0.025)	(0.010)	(0.010)	(0.009)
Observations	510	510	510	510	510	510
State and Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Age and Education Controls	No	Yes	Yes	No	Yes	Yes
Covid Controls	No	No	Yes	No	No	Yes
Macro Controls	No	No	Yes	No	No	Yes

Notes: This table reports results measuring the effect of Idaho Return to Work Bonus program on labor market outcomes using the synthetic differences-in-differences estimator in Arkhangelsky *et al.* (2021). The samples are state-month aggregates from the Current Population Survey from January-October 2020. The control group consists of all other states and Washington, D.C. Columns 1-3 all include individuals ages 25-54. Columns 4-6 include all individuals ages 16 and over. All specifications include state and month fixed effects. Age and education controls are the average age of individuals and the share of individuals with less than high school, high school, some college, and BA or higher education. Covid controls include the asinh of monthly new Covid-19 cases and the monthly average policy stringency index from the Oxford Covid-19 Government Response Tracker (OxCGRT). Macro controls include quarterly state income per capita from the Bureau of Economic Analysis (BEA), and the monthly state job opening rate from the Job Openings and Labor Turnover Survey (JOLTS) *** p<0.01, ** p<0.05, * p<0.1

Table B2. Estimated Effects of Idaho Return to Work Bonus Program on the Unemployment Rate, Nonparticipation Rate, and Total Employment Using Synthetic Difference-in-Differences

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A					
Dependent Variable	UR CPS 16+			NILF CPS 16+		
Estimator	SDID	SDID	SDID	SDID	SDID	SDID
Idaho X Post May 2020	0.000	-0.001	-0.000	-0.016	-0.021*	-0.022*
	(0.009)	(0.009)	(0.008)	(0.013)	(0.012)	(0.011)
Observations	510	510	510	510	510	510
	Panel B					
Dependent Variable	Ln Employment CES Total Nonfarm			Ln Employment QCEW		
Estimator	SDID	SDID	SDID	SDID	SDID	SDID
Idaho X Post May 2020	0.018	0.015	0.013	0.030	0.027	0.030*
	(0.017)	(0.017)	(0.016)	(0.019)	(0.019)	(0.017)
Observations	510	510	510	510	510	510
State and Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Age and Education Controls	No	Yes	Yes	No	Yes	Yes
Covid Controls	No	No	Yes	No	No	Yes
Macro Controls	No	No	Yes	No	No	Yes

Notes: This table reports results measuring the effect of the Idaho Return to Work Bonus program on labor market outcomes using the synthetic differences-in-differences estimator in Arkhangelsky *et al.* (2021). The control group consists of all other states and Washington, D.C. The samples are state-month aggregates from the Current Population Survey (CPS) the Current Employment Statistics (CES), and the Quarterly Census of Employment and Wages (QCEW) from January - October 2020. Columns 1-3 of Panel A use the state unemployment rate from the CPS as the outcome. Columns 4-6 of Panel A use the share not in the labor force from the CPS. Columns 1-3 of Panel B use the log of total monthly nonfarm employment from the CES. Columns 4-6 of Panel B use the log of total monthly employment from the QCEW. All specifications include state and month fixed effects. Age and education controls are the average age of individuals ages 16 and over, and the share of individuals ages 16 and over with less than high school, high school, some college, and BA or higher education .Covid controls include the asinh of monthly new Covid-19 cases and the monthly average policy stringency index from the Oxford Covid-19 Government Response Tracker (OxCGRT). Macro controls include quarterly state income per capita from the Bureau of Economic Analysis (BEA), and the monthly state job opening rate from the Job Openings and Labor Turnover Survey (JOLTS). *** p<0.01, ** p<0.05, * p<0.1

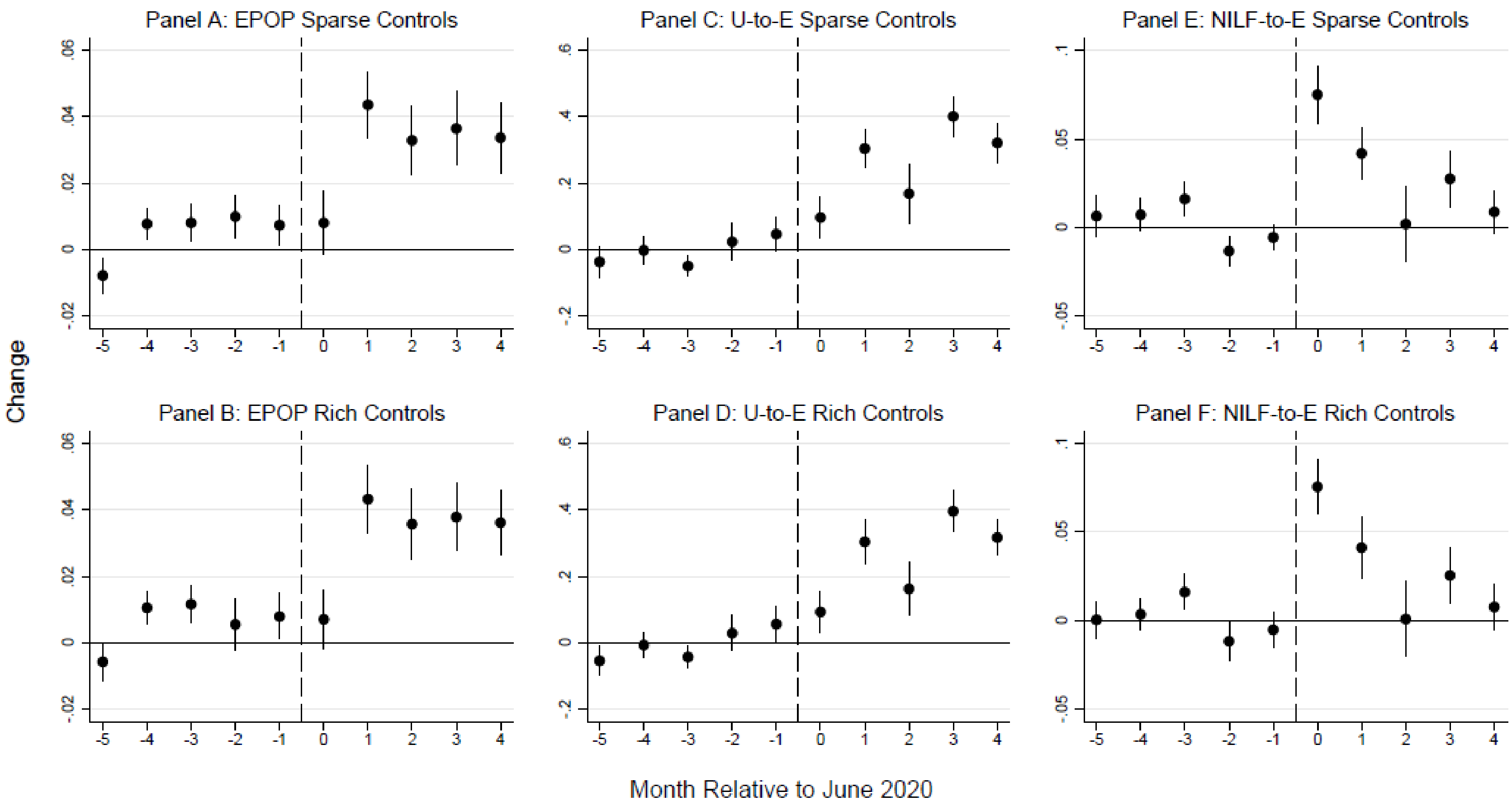

Figure B1. Event-Study Style Estimates for Employment, Unemployed to Employed Transitions, and Not in the Labor Force to Employed Transitions for Individuals Ages 25-54 Using Synthetic Difference-in-Differences. This figure displays estimated dynamic effects of the Idaho Return to Work Bonus program using the synthetic difference-in-differences estimator from Arkhangelsky *et al*. (2021). Panels A and B plot effects on the employment-population ratio. Panels C and D plot effects on unemployed to employed transitions. Panels E and F plot effects on not in the labor force to employed transitions. The estimates are calculated as described in Clarke *et al.* (2023). Regressions with sparse controls include average age and the share of prime age individuals with less than high school, high school, some college, and BA or higher education by state. Regressions with rich controls also include the state-level Covid and macro controls described in the paper. June 2020 is month 0.

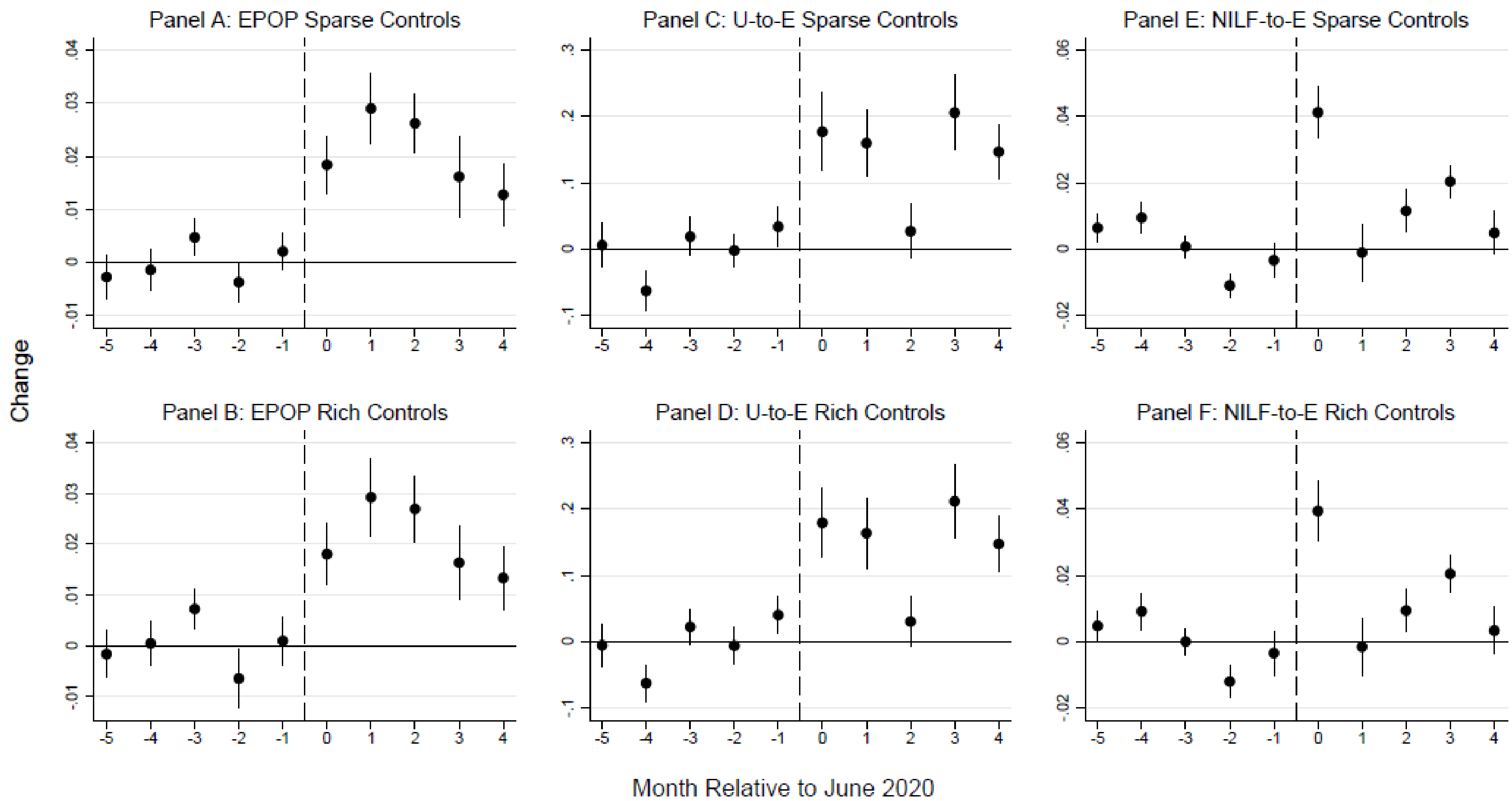

Figure B2. Event-Study Style Estimates for Employment, Unemployed to Employed Transitions, and Not in the Labor Force to Employed Transitions for Individuals Ages 16 and Over Using Synthetic Difference-in-Differences. This figure displays estimated dynamic effects of the Idaho Return to Work Bonus program using the synthetic difference-in-differences estimator from Arkhangelsky *et al*. (2021). Panels A and B plot effects on the employment-population ratio. Panels C and D plot effects on unemployed to employed transitions. Panels E and F plot effects on not in the labor force to employed transitions The estimates are calculated as described in Clarke *et al*. (2023). Regressions with sparse controls include average age and the share of individuals with less than high school, high school, some college, and BA or higher education by state. Regressions with rich controls also include the state-level Covid and macro controls described in the paper. June 2020 is month 0.

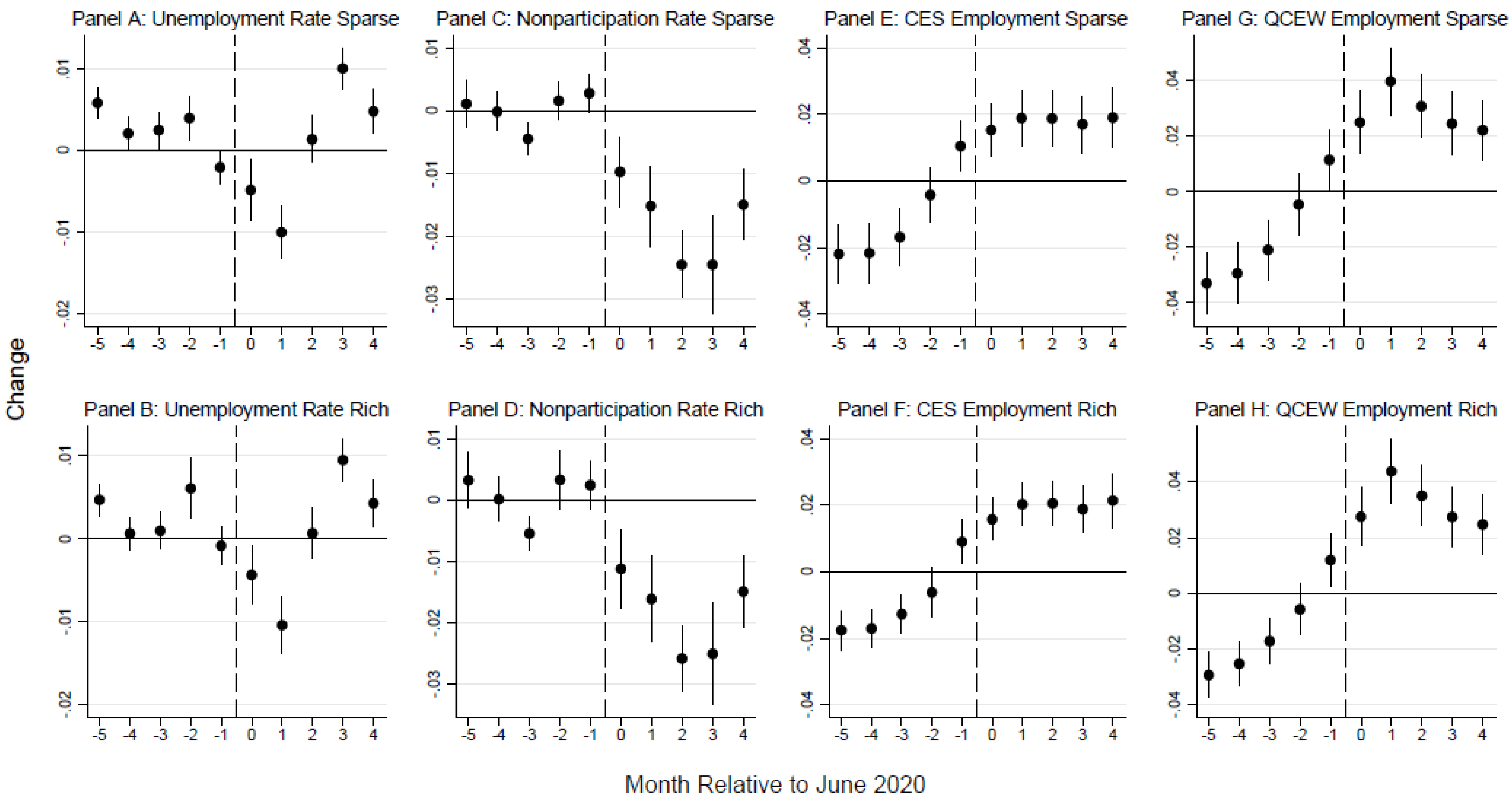

Figure B3. Event-Study Style Estimates for the State Unemployment Rate, the Nonparticipation Rate, and Log Total Employment from the CES and QCEW Using Synthetic Difference-in-Differences. This figure displays estimated dynamic effects of the Idaho Return to Work Bonus program on the state unemployment rate (Panels A and B), the state nonparticipation rate (Panels C and D), and the log of total employment from the CES (Panels E and F) and QCEW (Panels G and H) using the synthetic difference-in-differences estimator from Arkhangelsky *et al.* (2021). The estimates are calculated as described in Clarke *et al.* (2023). Regressions with sparse controls include average age and the share of individuals with less than high school, high school, some college, and BA or higher education by state. Regressions with rich controls also include the state-level Covid and macro controls described in the paper. June 2020 is month 0.