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Abstract

This study investigates the effects of state expansions of Unemployment Insurance (UI) eligibility criteria under the American Reinvestment and Recovery Act on UI recipiency among unemployed workers. Using a difference-in-differences approach and data from the Current Population Survey (2003–2020), we find evidence largely consistent with the expected overall and differential effects of these provisions. The adoption of the Alternative Base Period (ABP) increases UI take-up overall by approximately five percentage points. Some evidence suggests CFR provisions increase take-up among caregivers, but not non-caregivers. Part-Time (PT) provisions increase take-up among previously part-time workers, with no effect on previously full-time workers. The estimated magnitudes are around six percentage points. Additionally, we observe some evidence of differential impacts by gender. Our findings contribute insights into UI policy conversations, including federal mandates for ABP and PT provisions in eligibility determinations.

Keywords: Unemployment Insurance, alternative base period, compelling family reasons, part-time provision, American Reinvestment and Recovery Act, difference-in-difference, two-way fixed effects

Introduction

The Federal-State Unemployment Compensation Program, better known as Unemployment Insurance (UI), has provided income support for unemployed workers since its establishment under the Social Security Act of 1935. The primary goals of the UI program are to reduce individual income loss from unemployment and to stabilize the national economy during times of economic crisis when rates of joblessness increase. For example, during the Great Recession and early recovery years (2008 to 2012), UI prevented approximately 11 million Americans from falling into poverty (Council of Economic Advisers 2014).

UI benefits are not available to all workers who lose their jobs. All states require that workers meet certain criteria to receive benefits, and there is considerable variation in the design of eligibility criteria (and in other aspects of program administration) across states. These eligibility criteria reflect the need to balance the social welfare maximizing effects of UI benefits with the potential negative behavioral effects associated with moral hazard. In essence, they help policymakers ensure that enough workers receive benefits for the program to act as a robust macroeconomic stabilizer while avoiding providing benefits to workers who could otherwise find employment.

However, since the early 1950s, substantial shifts in the U.S. economy (the decrease in manufacturing jobs and the increase in service sector jobs) and the labor force (the increase in female labor force participation) have resulted in declines in the share of unemployed workers receiving UI benefits (U.S. Department of Labor 2021).[1] Because low recipiency rates can limit

[1] The UI recipiency rate was the highest in the 1950s, averaging about 50 percent. It hovered around 41.2 percent in the 1960s and 1970s before dropping to 33.8 percent in the 1980s (U.S. Department of Labor, 2021). Even in 2009, as the national unemployment rate was reaching a historical high after the Great Recession, the national UI recipiency rate did not exceed 40 percent. In the wake of the Great Recession, from 2010 to 2019, UI recipiency averaged 27.4 percent (U.S. Department of Labor 2021).

the UI system's ability to ensure the economic security of workers experiencing unexpected job loss and its capacity to stabilize the macroeconomy during economic downturns, some policymakers have interpreted declining UI recipiency rates as evidence in support of broadening eligibility criteria so that more workers qualify for benefits.

Along these lines, Congress passed several modernization provisions under the American Recovery and Reinvestment Act of 2009 (ARRA) with the intention of expanding the scope of the UI program.[2] With the Special Transfers for Unemployment Compensation Modernization (Sec. 2003), the federal government set aside $7 billion to be allocated across states conditional on expansions to their eligibility criteria and benefit provisions (American Recovery and Reinvestment Act 2009). In this paper we focus specifically on how UI recipiency was impacted by state adoptions of three eligibility provisions: the use of an alternative base period (ABP) in determining monetary eligibility for benefits, referred to hereafter as the ABP provision; allowing workers leaving work for compelling family reasons (CFR) to claim benefits, hereafter the CFR provision; and allowing workers seeking part-time (PT) work to claim benefits, hereafter the PT provision.

Conversations leading up to the passage of the Unemployment Insurance Modernization Act (UIMA) suggested that policymakers expected the ABP, CFR, and PT provisions to increase UI recipiency rates among specific groups of unemployed workers who historically had limited access to benefits. This included low-wage workers, caregivers, and part-time workers. The provisions were also expected to increase recipiency rates among female workers, since they are over-represented among each of the targeted groups. In announcing the 2007 Congressional hearing on UI modernization legislation, Representative Jim McDermott, the Chairman of the

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[2] The provisions were introduced under the Unemployment Insurance Modernization Act of 2009 but ultimately became part of the American Recovery and Reinvestment Act of 2009 (ARRA).

U.S. House of Representatives, Ways & Means Committee stated, “Too many workers, especially those in low-wage and part-time employment, are excluded from the Unemployment Insurance system. Women in particular are hampered by policies that were crafted five, six and seven decades ago. We should actively encourage states to make further progress in covering all unemployed workers who have worked hard and who have had taxes paid into the system on their behalf” (Modernizing Unemployment Insurance to Reduce Barriers for Jobless Workers 2007:2). [3]

Considering these policy expectations, this paper uses data from the Current Population Survey (CPS) and a difference-in-differences (DinD) design to examine whether the three provisions (ABP, CFR, and PT) led to increases in UI receipt among unemployed workers, particularly among the targeted groups. In the limited research that has used a nationally representative sample to examine the behavioral effects of these provisions, one study using a DinD design and administrative data found that the share of job losers filing new UI claims increased following the implementation of the three provisions (Bleemer 2013). Another study using a DinD design and CPS data found that ABP provisions increased UI receipt among a subsample of less-educated part-time workers (Gould-Werth & Shaefer 2013). No prior study of which we are aware has focused on the impacts of CFR and PT provisions on benefit receipt for caregivers, part-time workers, or female workers.

UI Program Background & Policy Context

[3] The original design of the UI program was guided by conventional work-family assumptions: that men were the primary breadwinners and women were the primary caregivers of the family, that the breadwinners tended to have full-time jobs and stable work histories, and that only breadwinners would require income support during periods of unexpected job loss (Meyers, Plotnick, and Romich 2011).

Under the current design of the UI system, not all unemployed workers are eligible to receive UI benefits. Workers claiming benefits must have been employed in a job covered by the UI system and they must meet both monetary and non-monetary criteria for eligibility.

A worker is covered under the UI system if their employer pays UI taxes on a portion of their wages. Covered work typically excludes self-employed workers, freelancers or independent contractors (non-W2 employees) (McKay, Pollack, and Fitzpayne 2018). Workers in the on-demand or gig economy are often classified as independent contractors rather than traditional employees (Berg 2015).[4]

Monetary criteria stipulate the minimum prior labor force attachment (work hours and earnings) a worker must have in the period prior to unemployment to qualify for benefits. Most states use the first four of the five calendar quarters prior to the quarter of unemployment as the standard base period for determining whether a worker's labor force attachment meets monetary eligibility conditions (U.S. Department of Labor 2020a).

Non-monetary criteria identify whether a worker's reason for unemployment qualifies as involuntary. Most states require UI recipients to be job losers, individuals who have become unemployed due to layoff or employer bankruptcy, rather than job leavers, individuals who have voluntarily exited from employment. Once initial eligibility and benefit amounts have been determined, beneficiaries also must meet guidelines for continuing eligibility. Broadly, these guidelines require that a UI recipient be available for work, actively seeking work, and willing to accept reasonable employment offers (U.S. Department of Labor, 2020a).[5]

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[4] Although temporary changes to the UI program in response to the sharp economic downturn in the early months of the COVID-19 pandemic expanded UI to more workers, workers in the gig economy would not be eligible to receive benefits under the status quo.

[5] Importantly, multiple job holders may claim UI benefits if they are seeking more work and their wages are less than or equal to their benefit amount. For these individuals, the weekly benefit amount is adjusted to account for earnings from their second job. In most cases total earnings (wages and weekly benefit amount) cannot be greater than one and a half times the weekly benefit amount. This rule applies similarly to individuals who work only one

As the US labor market has changed over time, fewer workers are able to meet these eligibility criteria. Today, many workers earn low hourly wages (Ross and Bateman 2019; Berstein and Hartman 1999; Cooper 2018), work part-time (U.S. Bureau of Labor Statistics 2021a, 2021b), and/or have caregiving responsibilities for children and adults with special needs (AARP and National Alliance for Caregiving 2020). Women are overrepresented among low-wage workers (Chaudry et al. 2016) and caregivers (AARP and National Alliance for Caregiving 2020) and are more likely to work part-time, less likely to work overtime, and more likely to be absent from work than men (U.S. Bureau of Labor Statistics 2016a, 2016b).

Policy conversations on options for expanding the scope of the UI program have typically included modifying states' eligibility criteria so that more workers qualify for benefits. As early as the 1980s, commissions and taskforces raised concerns that low-wage workers, part-time workers, and caregivers represented a growing share of the labor force but were less like to qualify for benefits (National Commission on Unemployment Compensation,1980; Advisory Council on Unemployment Compensation 1996). They also raised concerns that female workers represented a growing share of the labor force, but were overrepresented among these groups, and therefore less likely to qualify for benefits. The committees recommended that modifications to eligibility criteria take place at the federal level to reduce variation in eligibility determinations for workers across states.

Although a few states modified their eligibility criteria in the 1990s and early 2000s, the most significant changes to UI eligibility criteria occurred with the passage of the ARRA in 2009. Under the ARRA, the federal government set aside $7 billion to be allocated across states conditional on changes to their eligibility criteria. To receive the first share (one-third) of the 7

job but have had their hours reduced and for workers who find limited work with a new employer and are seeking additional work.

billion dollar stimulus funds, states were required to adopt an alternative base period (ABP) when making monetary eligibility determinations. An ABP shifts the window during which earnings requirements are examined, typically from the first four of the five prior calendar quarters—the standard base period— to the last four of the five prior calendar quarters—the alternative base period. Essentially, a worker failing to qualify for UI under the standard base period could use wages in the most recent four completed calendar quarters to qualify (U.S. Department of Labor 2020a).

To receive the remaining share (two-thirds) of the federal stimulus funding, states had the option to modify their eligibility criteria to include provisions that would allow workers who were seeking part-time employment to claim benefits (PT provisions) or to allow exits from employment due to domestic violence or due to compelling family reasons, such as care-taking for a spouse or loved one or moving because of a spouse's job transfer, to qualify as good cause (CFR provisions). In addition to PT and CFR provisions, to receive the second portion of the federal stimulus funding, states could provide additional benefits to permanently laid-off workers who were participating in job training programs, or incorporate dependent allowances into weekly benefit amounts (American Recovery and Reinvestment Act 2009). In total, 34 states (including Washington DC) received the full allocation of the UI Modernization funding (ABP plus two out of four optional provisions), 5 states received the first share (1/3) of funding (ABP only), and 12 states did not receive any funding (Chang 2020b). See Table 1 for information on implementation dates and incentive funding by state.

[Table 1 Here]

As of 2019, 39 states had adopted an ABP, including 18 states who had implemented an ABP before the federal UIMA and 21 states who adopted an ABP provision or modified their existing ABP to meet the requirements for federal UIMA incentive funding. Fewer states (29) had adopted an ARRA-compliant PT provision allowing workers seeking part-time employment to claim benefits, and 21 states had adopted an ARRA-compliant CFR provision allowing workers who exited employment for compelling family reasons to claim benefits.

Previous Research and the Current Study

Limited research has used a nationally representative sample to examine the effects of the ABP, CFR, and PT provisions included in the ARRA on benefit receipt among unemployed workers. Of the provisions, the ABP has been the most studied, though with more focus on changes to UI eligibility than benefit receipt. For example, in one simulation study, Stettner, Boushey, and Wenger (2005) used nationally representative data from the SIPP and simulated the increase in the share of workers that would be eligible for UI if all states were to adopt the ABP. They estimated that a nationally adopted ABP would increase the annual number of UI recipients by 6 percentage points (or 9 percent) and would have a larger impact on workers at the lower end of the wage distribution. At the time that their study was conducted, 19 states had implemented an ABP provision.

In two other simulation studies, Lindner and Nichols (2012) and Callan, Lindner, and Nichols (2015) took similar approaches to Stettner and colleagues but simulated the increase in eligibility due to the PT and CFR provisions in addition to the ABP provision using data from the SIPP. Relying on SIPP data collected from 1997 to 2007 (i.e., prior to the Great Recession), Linder and Nichols (2012) estimated that if the three provisions were nationally adopted, then rates of eligibility would increase by 20 percentage points (from 53.6 to 75.8 percent). Following up on this work, Callan et al. (2015) extended the study period to 2013 and reported a similar overall increase in eligibility if all states adopted all three provisions. Speaking to the targeted aims of the three eligibility provisions, they also found that workers who would become eligible for benefits under any of the three provisions had lower household incomes, lower levels of education, more children, on average, and were more likely to be female.

Several studies have used state administrative records to report on the share of claimants qualifying for benefits due to an ABP provision. Vroman (1995) was among the first to take this approach, analyzing administrative data from six states that had implemented an ABP, and finding that between 6 and 10 percent of claimants were determined eligible for benefits under the ABP depending on the state. Drawing on additional demographic information from three of the six states, Vroman found that a higher share of less-educated claimants qualified under the ABP, consistent with the policy goal of increasing recipiency among low-wage workers. However, findings related to the gendered effects of the ABP provision were mixed. In Washington, a greater share of women claimants than men claimants obtained eligibility under the ABP, while in Vermont and Maine the pattern was reversed.

Studies such as Vroman's that rely on administrative records of UI claimants typically find smaller policy effects of the UI provisions than studies that rely on survey data to simulate eligibility among unemployed workers. This is largely to be expected since the simulation studies implicitly assume complete take-up of benefits and no behavioral response. In reality, not all unemployed workers apply for benefits, and this distinction may be particularly relevant for more disadvantaged groups of workers, who may be less likely to apply for benefits because they are not aware of the program or because they perceive that they are ineligible for benefits (Gould-Werth and Shaefer 2012). In a study using data from the National Evaluation of the Welfare-to-Work Grants Program Evaluation, Rangarajan and Razafindrakoto (2004) found that ABP provisions increased the likelihood that former welfare recipients—those that previously received cash payments from the Aid to Families with Dependent Children (AFDC) program—would *qualify* for UI if they experienced a spell of unemployment in the year after transitioning from welfare (AFDC) to work. However, of those who did experience a spell of unemployment, very few *received* benefits.

In a study focused on the behavioral effects of UI modernizations, Bleemer (2013) used state-level administrative records from 2005 through 2011 and a difference-in-differences research design to study the impact of the ARRA provisions on UI utilization (the ratio of new claimants to job losers). The study found that the ratio of new claimants to job losers increased by 14 percent following implementation of ABP provisions and by 10 percent and 5.4 percent, respectively, following implementation of PT and CFR provisions.

Finally, in the study most closely related to our own, Gould-Werth and Shaefer (2013) used pooled cross-sectional samples from the CPS and a DinD design to capture changes in individual-level UI receipt following state adoptions of ABP provisions from 1987 through 2011. Unlike Bleemer (2013), Gould-Werth and Shaefer (2013) did not observe an increase in UI receipt among their full sample of unemployed workers. They, however, observed that ABP provisions increased UI receipt among less-educated part-time workers—by about 3 percentage points, on average.

We build on the work of Gould-Werth and Shaefer (2013) by using data from the CPS from 2002 to 2019 to examine the impact of state adoptions of ABP, CFR, and PT provisions on individual UI receipt for the overall unemployed population and targeted groups. We also investigate the extent to which these provisions had a "gendered effect." Like the prior work, we use state-time variation in adoptions of the modernizations as a natural experiment where the states that did not change their policies serve as the counterfactual (what would have happened had states not adopted the provisions). However, the prior work did not capture state implementations of these provisions that occurred after 2011. We use data through 2019, which allows additional time for state adoptions and implementation as well as more time for workers to become aware of these new program rules, which is an important factor in the take-up of benefits (Vroman 2008a).

We pay particular attention to the effects of non-monetary eligibility criteria (CFR and PT provisions) on UI take-up, since no prior study of which we are aware has used up-to-date nationally representative survey data to examine the impacts of CFR and PT provisions on UI receipt or to consider whether these non-monetary eligibility provisions increased UI receipt among their targeted groups. Moreover, ABP provisions and CFR and PT provisions differ in how they are used in eligibility determination. ABP provisions affect *monetary* eligibility determinations—whether an individual has sufficient earnings and work history to qualify for benefits. Making this determination does necessarily require the applicant to provide more information and it may not require any discretion on the part of UI agency staff. CFR and PT provisions affect *non-monetary* eligibility determinations—whether reasons for unemployment qualify as "good cause"—and may require more documentation, discretion, or interpretation of UI agency staff, and certain actions on the part of the worker (e.g. negotiation for accommodation with the employer before leaving work for family reasons (Ben-Ishai, McHugh, and Ujvari 2015). If changes to non-monetary eligibility criteria did not result in increased take-up of benefits among caregivers and part-time workers, this is useful information to policymakers aiming to expand the scope of the program, since it signals that additional policy or administrative changes may be required to increase UI recipiency rates for these groups of workers.

Methods

Data and Sample

The CPS is a monthly survey of approximately 60,000 households conducted by the U.S. Census Bureau and the Bureau of Labor Statistics and designed to be representative of the non-institutionalized population in the United States. As part of the Annual Social and Economic Supplement (ASEC), households participating in the CPS in March provide additional socioeconomic information from the prior calendar year (not just the survey month), including information on employment status and UI benefit receipt.

We used CPS ASEC (2003–2020) data merged with state-level data from multiple governmental data sources spanning from 2002 through 2019.[6] This allows us to capture several years before and following the period that many states made substantial changes to their UI eligibility criteria in response to provisions in the ARRA. We retrieved multi-year CPS ASEC data through IPUMS USA: Version 12.0 (Ruggles, Flood, Goeken, Schouweiler, and Sobek 2022).

The CPS ASEC survey is appealing for this study because it has a higher UI reporting rate (approximately 70% since 2002) than other national surveys on average (Meyer, Mok, and Sullivan 2009, 2015). Additionally, cross-sections of the CPS ASEC can be pooled to cover more years than panel survey data, such as the SIPP, which surveys the same households over time but for a shorter period. One limitation of using the CPS ASEC data is that the measures of employment status and UI benefit receipt are annual, which means that we cannot precisely identify the timing of a job separation relative to UI receipt within a year. We also cannot

[6] Because individuals are asked to report detailed information on income and earnings from the prior calendar year, the 2003 to 2020 waves of the CPS ASEC cover the period from 2002 through 2019. We chose 2019 as our ending year because the substantial UI policy changes beginning in March 2020 in response to the COVID-19 pandemic may confound our estimates of the policy effects of the changes induced by the ARRA provisions.

precisely identify whether a job separation or benefit receipt happened before, during, or after a state implemented a UIM provision within a year. A dataset with monthly data like the 2008 SIPP panel could provide more precision on the timing of job separation, UI receipt, and UI policy changes. However, there are known issues with the SIPP. We were particularly concerned that the relatively small sample size of the 2008 panel, particularly for subgroup analyses, and the high sample attribution rate over the five-year period would produce biased estimates of the UIM policy effects.

From the CPS ASEC, we identified an analytic sample of 99,311 individuals between the ages 25 to 64 who experienced a spell of unemployment lasting at least four weeks in the prior calendar year.[7] See Appendix Table A1 for sample characteristics. We excluded individuals unemployed for less than one month because they may be in a job transition or taking temporary leave and have a low need for unemployment benefits (U.S. Bureau of Labor Statistics, 2019). Moreover, those unemployed for a relatively short period of time might perceive the costs of filing a UI claim to be quite high (Ben-Ishai, McHugh, & McKenna, 2015). Generally, it takes two to three weeks after filing a claim to receive the first benefit payment because most states require a one-week waiting period (DOLETA, 2022).

Some closely related studies have used a broader sample of individuals based on the official definition of unemployment, which includes anyone who is jobless, actively seeking work, and available to take a job (e.g., Shaefer 2010; Shaefer and Wu 2011), while others have used a more narrow sample that excludes those who did not report working in the prior year even

[7] We focus on individuals ages 25 to 64 because they are likely to have the strongest attachments to the labor force and therefore earnings and work histories sufficient to qualify for UI benefits. We excluded individuals ages 65 and older because older adults were more like to leave the workforce and retire during the Great Recession (Coile and Levine 2011). We excluded individuals less than 25 years old because many are in school (Davis and Baumen 2013). Moreover, workers aged 25-64 are more likely to be primary caregivers, marking them a relevant population to evaluate the effectiveness of compelling family reasons provisions.

if they were looking for work (Gould-Werth and Shaefer 2013). Following Gould-Werth & Shaefer (2013), we selected individuals who were unemployed and looking for work in the reference year but exclude individuals who did not reported any work in the reference year for our analytic sample. During the Great Recession, unemployed workers could receive benefits for up to 99 weeks (almost 2 years), making it difficult to determine whether someone unemployed for the year-long reference period of the CPS ASEC was a new entrant to the labor market or whether they had previously established work history. Because we exclude workers younger than 25 years old, it is more likely that these individuals, who represent about 20 percent of our sample, are long-term unemployed rather than new labor market entrants. Finally, we excluded individuals with imputed UI information (approximate 5 percent of our sample) because empirical work using the CPS data has shown the imputed dependent variables can result in biases of estimated coefficients (Bollinger and Hirsch 2006, 2013). We consider whether our main findings are sensitive to alternative sample selections in the sensitivity analysis section.

Research Design

To evaluate the effectiveness of state adoptions of ABP, CFR and PT provisions, a randomized approach would be ideal. Of course, there are numerous reasons why states would object to such an approach, not least that if the policies resulted in higher take-up the state agency would incur higher costs.[8] The adoption of these provisions was not random, and states that adopted them differ from non-adopters in terms of their region, government ideology, and other factors. For example, more than half of the states that have not adopted the ABP are in the Southern region of the U.S,[9] while two-thirds of states that did not adopt PT provisions are in the

[8] The ARRA provided funds to states that adopted these policies as a way of mitigating the increased costs associated with them, but the decision to adopt was not random.
[9] States in the south region of the U.S. that did not adopt the ABP provision include Alabama, Florida, Kentucky, Louisiana, Mississippi, and Texas.

Midwest and South. Furthermore, several non-adopting states have UI programs that offer limited protection, as indicated by lower accessibility and insufficient program financing[10] (Chang, 2020a). Compared to adopters, non-adopters had higher poverty rates, food insecurity, and uninsured low-income children over the study period.

Although the ARRA-induced policy were not randomly assigned, we exploit the temporal variation in the adoption of ABP, CFR, and PT provisions across states to evaluate their effectiveness using a DinD research design. States that did not adopt the UI provisions during the study period serve as the comparison group, providing a counterfactual for what would have happened in adopting states without a policy change. Table 1 provides the dates of UIM provision adoptions by state. We use a linear probability regression model (LPM) with statute and year fixed effects, as shown in equation 1, to estimate the overall effects of UIM provisions on UI receipt among unemployed workers. We used a LPM because it provides an intuitive interpretation of the effect sizes as changes in the probability of the outcome. Furthermore, the LPM does not impose functional form assumptions on the relationship between the independent and dependent variables. While heteroscedasticity is a common issue in LPMs, it can be addressed by applying robust standard errors to address it (Wooldridge, 2015). We conducted LPMs using Stata/MP 17.0. LPMs with an option of robust cluster standard errors.

$$P\,(Receipt\_{ist} = 1) = \alpha + \Sigma\beta UIM_{st} + \Sigma\gamma(State_{st}) + \Sigma\delta(Individual_{ist}) + \tau_t + \lambda_s + \eta_{st} + \varepsilon_{ist} \quad (1)$$

In equation 1, i indexes individuals, s indexes state, and t indexes year. $P\,(Receipt\_{ist} = 1)$ is the probability that individual i received a UI benefit. α is the intercept term. $\Sigma\beta$ represents a

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[10] These states include Alabama, Florida, Louisiana, Mississippi, and Missouri.

summation of coefficients (β) for each UIM provision (ABP, CFR, and PT). β is interpreted as the average treatment effect of a provision on the probability of UI receipt among unemployed workers in the treated states (ATT). UIM_{st} is a set of indicators for the three provisions (ABP, CFR, PT) that correspond with the states and years in which they were implemented.[11] To identify ARRA-induced policy effects of ABP provisions on UI receipt, we estimate equation 1 using a sample that excludes 19 states that adopted ABP provisions before the ARRA. To identify ARRA-induced policy effects of PT provisions on UI receipt, we estimate equation 1 using a sample that excludes 9 states that adopted PT provisions before the ARRA (see Table 1).[12]

The year fixed effects τ_t control for unobserved time-varying factors that are common to all states, and the state fixed effects λ_s control for unobserved time-invariant factors that differ across states. Both of which could lead to variation in UI receipt and therefore introduce bias into our estimates of the policy effects (β) if not taken into account. A central assumption of a DinD design is that the pre-intervention trends in outcomes (in our case receipt of UI benefits) do not differ. Because the common trends model is nested with the state-specific linear trends model in a two-way fixed effects (TWFE) regression model, our main model includes state-specific linear

[11] In our main specification of formula (1), states never implemented a provision were coded as 0 throughout the study period. States implementing a provision before June 30 of year t were coded as 1 in year t and subsequent years. States implementing a provision after June 30 were coded as 1 in year t+1 following Gould-Werth and Shaefer's (2013) rounding approach.

[12] New research on estimating DinD designs using two-way fixed-effects (TWFE) models indicates that when treatment is time-varying, TWFE estimator(s) may assign negative weights to some treated cases—typically unit-time observations occurring late in the study period for units that were treated early in the study period (Goodman-Bacon 2021; Jakiela 2021). These negative weights can produce fixed-effects estimators that are severely biased or even in the reverse direction of the “true” effect when treatment effects are heterogeneous (Callaway and Sant’Anna 2021; Goodman-Bacon 2021; Jakiela 2021). We address this issue by excluding pre-ARRA adopters of ABP and PT provisions from our main models estimating ABP and PT effects. In additional sensitivity analysis, we include early adopters in our models. We also consider whether our estimates are robust to exclusion of later data points, which is also recommended in the recent DinD literature.

time trends (η_{st}) to relax the common trends assumption (recommended by Wing, Simon, and Bello-Gomez, 2018).

We also control for other factors that may be related to UI receipt. $State_{st}$ represents a set of state-by-year covariates that including the unemployment rate, minimum wage, UI trust fund reserve ratio, UI benefit replacement rate, and average UI benefit duration, as well as the training benefit provision that states could have adopted under the ARRA—an extension of maximum UI benefit durations for additional 26 weeks for individuals in certified job training programs. This provision aimed to improve the adequacy of UI benefits for the long-term unemployed but might confound the policy effects of eligibility expansions on UI receipt. $\Sigma\gamma$ represents a summation of coefficients (γ) for each of the state-level covariates. $Individual_{ist}$ represents a set of individual-level covariates, including sex, education, caregiving as the main activity during job separation, full-time or part-time employment status, race, citizenship status, marital status, age, main occupation, disability, union status, homeownership, geography (urban or rural), and receipt of food assistance. $\Sigma\delta$ represents a summation of coefficients (δ) for each of your individual-level covariates. The error term, $\varepsilon_{ist,}$ represents individual-specific errors, and is assumed to be correlated within states. We adjust the standard errors to account for the correlation between observations within each state. All analyses were weighted by using the person-level weights provided in the CPS-ASEC, which adjusts for the complex sampling process used in the CPS so that the sample is a closer representation of the U.S. population.

We tested the null hypothesis that the adoption of a provision (ABP, CFR, and PT, respectively) did not impact the probability of UI benefit receipt among unemployed workers. We used a two-sided test to avoid inflating the Type I error rate. While a p-value below 0.1 does not meet the conventional threshold for statistical significance, typically set at 0.05, we interpret it as marginally statistically significant or approaching significance, thereby maintaining its potential relevance to our findings. We also performed a joint test for the hypothesis that all effects of three provisions are zero using the "suest"[13] command in Stata.

Differential Effects of Provisions

Because conversations leading up to the passage of the ARRA suggest that policymakers designed these provisions with certain groups of workers in mind, we add two-way and three-way interaction terms to our base models to explore (1) whether the provisions had a differential effect on UI receipt for low-wage workers, family caregivers, and part-time workers relative to their counterparts, (2) whether the provisions had differential effects on UI receipt for female workers relative to male workers, and (3) whether the provisions had a differential effects on UI receipt by sex and target groups. Given the presence of gender disparities in the labor market concerning wages, occupational segregation, work hours impacted by caregiving responsibilities, and other factors, it is important to acknowledge that UI, as a component of labor market policy, may have differential effects on men and women. We used three three-way interaction models to examine the gendered policy effect on each target group, respectively (i.e., low-wage workers, family caregivers, and part-time workers). See equation (2) for the general notation of the three-way interaction model.

$$P\,(\text{Receipt\_}_{ist} = 1) = \alpha + \Sigma\boldsymbol{\beta}\text{UIM}_{st}\,{*}\text{Target}{*}\text{Sex} + \Sigma\gamma(\text{State}_{st}) + \Sigma\delta(\text{Individual}_{ist}) + \tau_t + \lambda_s + \eta_{st} + \varepsilon_{ist} \quad (2)$$

In each model, we tested three null hypotheses: first, that the adoption of a provision did not yield a differential effect on the probability of UI benefit receipt between its target and non-

[13] The 'suest' command in Stata allows for the testing of cross-model hypotheses after estimating a set of models.

target groups; second, second, that the adoption of a provision did not yield a differential effect on the probability of UI benefit receipt between male and female workers; and third, that the adoption of a provision did not yield a differential effect on the probability of UI benefit receipt between its target and non-target groups within each sex group. These tests were conducted while controlling for the effects of the other two provisions and all other variables present in the model. Additionally, we performed a joint test for the hypothesis that all differential effects of three provisions are zero.

We used workers without a college degree[14] as a proxy measure for low-wage workers because the CPS ASEC data did collect information on workers' wages before job separation. We identified unemployed family caregivers in the CPS ASEC data by focusing on part-year workers who worked for less than 52 weeks and whose main activity during job separation was family caregiving in the reference year. We classified part-time workers as those whose usual weekly working hours were less than 35 hours in the last year. It should be noted that a mere 4.4% (4,382 individuals) of the sample identified family caregiving as their primary activity during job separation. Of this target group, only 911 individuals were located in CFR-adopting states during the treatment years. The limited number of treated unemployed family caregivers potentially reduces our statistical power, consequently diminishing our capacity to accurately discern true CFR effects for this group when utilizing a conventional statistical significance level at 0.05.[15] An examination of the 95% confidence interval of the effect size suggests potential

[14] We explored a three-level education variable (no high school degree, with a high school degree and some college, and with a college degree and found a similar ABP effect size for workers without high school degree and workers with a high school degree and come college. Considering the simplicity of the analysis and interpretation, we opted to use without a college degree as a proxy measure for low-wage workers.

[15] The probability of Type II error (a failure to reject the null hypothesis when the alternative hypothesis is true in the population) is greater when the sample size is small and the effect size is not much different from the null (Serdar, Cihan, Yücel, and Serdar, 2021).

underpowering in our analyses. Considering the small sample size of the target group and the effect size, we have chosen to adopt a significance level of 0.1 when interpreting CFR effects.

In addition to reporting the tests of differential effects, we further estimated ATT for each subgroup, utilizing the "margins" command in Stata. This estimation incorporated a joint test (F-test) for a null hypothesis that all policy effects of subgroups are zero and a set of tests of a null hypothesis that the adoption of a provision did not impact the probability of UI receipt for a specific subgroup of unemployed workers. A positively signed and statistically significant ATT indicates a UIM provision increased the probability of UI receipt among unemployed workers of that particular subgroup in the treated states. These estimates offer empirical evidence of the policy effect on a given group. We used the mcompare(Bonferroni) option of the "margins" command to adjust our estimates for multiple comparison across subgroups.[16]

Results

The Effects of the UIM Provisions on UI Benefit Receipt: Overall and by Target Group

The first row of Table 2 reports coefficients(β) indicating the ATTs of three UIM provisions on UI receipt for the unemployed. Each column corresponds to a β for a provision retrieved from a model with a sample that excludes individuals in states with a pre-ARRA adoption of that provision. Each model controls for the other two provisions and all variables in the equation (1). A joint test across three models confirms a significant UIM impact on the unemployed workers. The second row of Table 2 reports the coefficients from models with two-way interaction terms

[16] The Bonferroni method divides the desired overall alpha level by the number of tests being performed (Schochet, 2009). For example, for a test among four groups (male full-time workers, female full-time workers, male part-time workers, and female part-time workers), the Bonferroni method considers an effect to be statistically significant only if its p-value is less than 0.0125, rather than the conventional threshold of 0.05. When interpreting the policy effects of subgroups, one should be cautious about the conservative nature of the Bonferroni adjustment, which could potentially increase risk of Type II errors.

between the UIM provisions and our targeted groups (unemployed low-wage workers, family caregivers, or part-time workers) added to the base model. A joint test across three models confirms a significant differential UIM impact on subgroups of unemployed workers. Lastly, the third and fourth rows report the ATTs for each subgroup estimated from the two-way interaction models.

We find evidence that ABP increased UI take-up. On average, the adoption of an ABP provision after the ARRA was associated with a 4.7 percentage point (equivalent to 14.1 percent) increase in UI receipt ($p < .01$) (See Appendix Table A2 for the equivalent percent changes). Contrary to our hypothesis, we observed no significant differences in the effects of ABP on UI take-up between individuals with a college degree and those without such educational attainment.

Our analysis showed no significant overall effect of CFR among the full sample of unemployed workers. However, consistent with our hypothesis, there is some evidence indicating that the effect was 4 percentage points larger among caregivers compared to non-caregivers, though this difference was only significant at the 0.1 level. On average, CFR adoption increased the likelihood of UI receipt among caregivers by 5.1 percentage points ($p <.1$, See Table 2)

We did not find a discernible overall PT effect among the full sample of unemployed workers. However, we found robust evidence supporting our hypothesis that PT has a larger effect on workers previously employed part-time than for those previously employed full-time (a differential of 5.9 percentage points, $p < .001$, See Table 2). On average, a PT adoption was associated with a 6.4 percentage point increase (40 percent increase, See Appendix Table A2) in the likelihood of a part-time worker receiving UI.

The Gendered Effects of the UIM Provisions on UI Benefit Receipt: Overall and by Target Group

We examined whether there was a gendered effect of the UIM provisions by adding interaction terms between the provisions and sex (with male as the reference category) and adding three-way interactions terms between the provisions, sex, and the targeted group to our base models. In the top panel of Table 3, we present the test results of differential effects between male and female workers in the first row, followed by the estimated ATTs of the three provisions on male and female workers. In the bottom panel of the same table, we display the test results regarding differences in target groups, stratified by sex, alongside the estimated ATTs by both sex and target group.

[Table 3 Here]

Our analysis revealed evidence of the gendered ABP effect, however, contrary to our hypotheses, male workers experienced a significantly larger effect (3.7 percentage points, $p < .001$). ABP provisions increased the likelihood of UI receipt among unemployed male workers by 6.1 percentage points (22.7 percent, $p < .001$), whereas a neglectable effect was observed among female counterparts (See Column 1, top panel of Table 3). We did not observe a differential ABP effect between those with a college degree and those without a college degree within each sex group. When analyzing the ATTs by education and sex, we found a statistically significant ABP effect of 6.6 percentage points among non-college-educated male workers (p<.001) compared to a marginally significant effect of 5 percentage points among college educated male workers (p<.1, see Column 1, bottom panel of Table 3).

Our analysis showed no gender-specific differential in CFR effects among the full sample of unemployed workers (See Column 2, top panel of Table 3). However, we observed some evidence of gendered CFR effect when cross-referencing sex and caregiving status. The differential effect of CFR between caregivers and non-caregivers showed marginal significance among female workers (4.6 percentage points, p <.1), while such an effect remained absent among their male counterparts. The estimations of the ATTs by caregiving status and sex show some evidence that the CFR effect was concentrated among female caregivers (5.6 percentage, p<.1 see Column 2, bottom panel of Table 3).

Lastly, our findings showed gendered effects of PT that align with our hypotheses, with female workers experiencing a significantly larger effect (2.8 percentage points, p < .05) than male workers. On average, PT adoptions increased the probability of UI receipt among unemployed female workers by 3.8 percentage points, whereas the effect among male workers was negligible (See Column 3, top panel of Table 3). Nevertheless, when analyzing the PT effects by gender and part-time status, we observed more robust differential PT effects between full-time and part-time male workers compared to their female counterparts (7.8 percentage points, p<.001 vs. 3.3 percentage points, p< .1). The PT effect was significant among both male and female part-time workers (7.8 percentage points, p<.001 and 5.8 percentage points, p<.05, See Column 3, bottom panel of Table 3).

Sensitivity Analyses: DinD Design Assumptions

We tested the robustness of our main findings to several DinD design assumptions and alternative samples. Considering simplicity, this section focuses on comparing the estimated ATTs, both overall and by subgroup, across various model specifications. The results of this comparison are presented in Table 4a-4c.

[Tables 4a-4c Here]

A central assumption of a DinD design is that the pre-intervention trends in outcomes (in our case receipt of UI benefits) do not differ. We estimate an alternative model specification that removes state-specific linear trends to assess whether results are sensitive to enforcing the common trends assumption. The second column of Tables 4a-4c present estimated ATTs from models without state-specific linear trends. The results of ABP and PT effects from this alternative model show similar patterns to those from our main specification. We note the CFR effect among caregivers became smaller (5.1 percentage points vs. 3.6 percentage points) and did not reach statistical significance. This was also the case for female caregivers (5.6 percentage points vs. 4.1 percentage points). This suggests a model without state-specific linear trends may yield biased estimates of CFR effects because it does not account for the unobserved time-varying state-specific factors might impact the outcome.

Next, considering negative weights associated with states' early adopting status and potential heterogeneous effects, we followed suggestions from recent advancements in D-in-D literature (Callaway and San't Anna, 2021; Jakiela, 2021). When the timing of treatment adoptions varies across units, negative weights may be assigned to early treated cases in later time periods, because those units more often serve as comparison group observations for later treated units than as treatment group observations Jakiela (2021). Negative weights may lead to bias in the fixed-effects estimator when treatment effects are heterogeneous—that is, they change over time within the treated units. Specifically, we examined whether our findings remained robust when including pre-ARRA early adopters, and upon exclusion of later time periods from our sample.[17] This set sensitivity analyses integrates insights drawn from the Callway and San't Anna (2021), which estimates of heterogenous effects in terms of treatment cohort and time progression.

The third column of Table 4a presents results from the model adding a sample from the seven states (Connecticut, DC, Georgia, Hawaii, Illinois, New Mexico, and Virginia) adopting an ABP provision between 2003 and 2008 (the early treatment cohort) were included into to the original ABP model. The third column of Table 4c presents results from the model adding a sample from the four states (Maine, Massachusetts, New Jersey, and New Mexico) adopting a PT provision between 2003 and 2008 in addition to the original PT model.[18] When we included early adopters, we found that the ATTs of ABP and PT provisions changed. The ABP effects became smaller and insignificant overall and among subgroups. These changes suggested negative weights assigned to seven early ABP-adopting states attenuate the policy effect (column 3, Table 4a).

In contrast to the attenuated overall ABP effect, the overall PT effect became larger and achieved statistical significance after including the early adoption cohort (as shown in column 3 in Table 4c). Within in the alternative model that includes early PT adopters, we also observe larger PT effects among female workers (both overall and by full-time/part-time status), compared to those in the main model. In contrast, we noticed a smaller PT effect for male part-

[17] We dropped years after 2013 from the sample, considering negative weights assigned to the relatively early adopters in the later years and possible heterogenous treatment effects after 2013 due to the end of the Emergency Unemployment Compensation (July 2008 – December 2013), which provided extended UI benefits for unemployed workers.

[18] We did not include the 12 states adopting an ABP provision and the 5 states adopting a PT provision before 2002 in the alternative ABP and PT models respectively because they were coded 1 over the study period.

time workers after including early PT adopters. These findings suggest potential heterogeneous treatment effects between pre- and post-ARRA PT adoption. We discuss possible explanations in the discussion section.

When we removed years after 2013 from the sample, we found that the overall effects of the three provisions in this alternative model remained similar to those obtained in the main models. These findings suggest no discernable heterogeneous effects between early and later years of ARRA implementation. See the fourth column of Table 4a -c. We prefer the results from our original model because excluding later years' data resulted in a loss of statistical power and less precise estimates for our subgroups.

Sensitivity Analyses: ARRA mechanisms & Alternative Subgroup Specifications

We conducted additional robustness checks considering mechanisms specific to ARRA provision adoptions and alternative target groups. Under the ARRA, adopting an ABP provision was a prerequisite for states to receive additional funding if they adopted CFR and PT provisions (or other possible provisions such as extending benefit duration for job training or allowing for dependents in calculating benefit amounts). Therefore, we may see clearer policy effects for adoptions of CFR and PT provisions if we focus only on states who adopted ABPs or already had them in place. This makes the comparison group those that were eligible for additional funding if they adopted PT or CFR provisions, but still chose not to do so. This alternative model is similar to Callaway and Sant'Anna (2021)'s alternative estimation, which imposes conditional parallel trends relying on a "not-yet treated" group as the basis for comparison. We present results from this alternative model in the fifth column of Tables 4a-4c. When we exclude individuals in the 12 states that never adopted an ABP, the results do not substantively alter our conclusions of the policy effects of three provisions.

Next, we consider whether the conclusions from our main models are sensitive to including those who did not work at all but reported at least one week looking for work in the reference year into our sample in the sixth column of Table 4a-c. The estimated ABP effects based on the broadened sample remained similar, while the estimates for the two non-nonmonetary provisions decreased in magnitude. These attenuated effects may be explained by the weaker labor market attachments among workers who did not report work in the reference year (e.g., the lack of active search for jobs to meet the requirement of UI eligibility).

Alternative Measures of the Targeted Groups

As a final robustness check, we consider whether our results for subgroup analyses are robust to alternative measures of our target groups, using SNAP (food stamp) receipt[19] as a proxy for low-wage workers and using the age of the youngest resident child in the household as a proxy for having significant caregiving responsibilities. We anticipate a more pronounced ABP effect among SNAP recipients compared to non-SNAP recipients, and the most substantial effect expected among workers whose youngest residing child is under five years old.

Similar to our main ABP model using no college degree as a proxy for low-wage worker, we did not find that ABP adoption had a differential impact on workers receiving SNAP benefits relative to workers not receiving SNAP benefits. At the same time, the estimates show a significant ABP effect among SNAP recipients (4.3 percentage points, $p < .05$), but no such impact among those not receiving SNAP benefits. This effect was particularly notable among male SNAP recipients, with an increase of 6.9 percentage points. This aligns with our original

[19] To qualify for SNAP benefits, households must have incomes at or below 135 percent of the federal poverty line. Approximately 21 percent of our sample of unemployed workers reported that their household received SNAP benefits whereas 26 percent who reported that they did not have a college degree.

model's findings of a significant effect on male workers without a college degree (6.6 percentage points increase). See the left panel of Table 5.

[Table 5 Here]

We did not find evidence of CFR provisions' effectiveness in increasing take-up of UI among caregivers using an alternative measure of caregiving based on the age of the youngest child living in the individual's home. The concern of insufficient statistical power to detect effects, which was present in our main analysis, remains relevant when breaking down the sample into these three smaller groups (no resident children living in the home, youngest resident child less than five years old, and youngest resident child 5 to 17 years old). We note that point estimates suggest a gender-specific trend in effect sizes, mirroring our main specification's findings. The estimated CFR effect appears concentrated among female workers with the youngest child under the age of 5 (2.7 percentage points). See the right panel of Table 5.

Limitations

Our findings should be seen in the context of several limitations. First, although survey data provides more detailed information about unemployed workers than we would have if we relied on administrative records, we were still challenged to identify workers in the targeted groups, such as low-wage workers and caregivers. We were challenged to identify low-wage workers because the CPS-ASEC does not have information on earned income in the year prior to the reference year. We used the level of education and household SNAP receipt in the reference year as proxy measures of whether a worker earns low wages, and both measures may include some workers who are not low-wage and exclude others who are. We were challenged to identify caregivers because there is limited information in the CPS ASEC to identify these workers with significant caretaking responsibilities. Our caregiving variable captures part-year workers who worked less than 52 weeks and reported their main activity during job separation was taking care of family in the reference year. We did not know whether caregiving was the main reason forcing a worker to leave their job or whether a worker had significant caregiving responsibilities prior to job separation.

Second, because we relied on survey data for our analysis rather than administrative records, our estimates may be biased due to the underreporting or misreporting of receipt of UI benefits in the CPS-ASEC. Prior studies on report of government benefits/transfers has shown that for unemployment insurance benefit in particular, underreporting is likely higher among individuals who receive UI benefits for short periods of time or at low amounts (Gabe and Whittaker 2012) and individuals who are low wage especially during recessionary periods (Larrimore, Mortenson, Splinter, 2022), which suggest we may be underestimating rates of UI receipt among workers in the targeted groups, which could attenuate our estimates of the impacts of the UI provisions. For example, Larrimore et al. (2022) show that from 2000 through 2020, IRS administrative records show more UI receipt than the CPS data. For the years covered in our study period, the IRS data reported by Larrimore et al. (2022) shows the highest number of UI recipients in 2009 (20 million), while the CPS shows the highest number of UI recipients in 2010 (13 million). Going forward, linkages between survey data such as the CPS and administrative records of UI benefit receipt may be an avenue for addressing misreporting while retaining the detailed information available in the survey data that is not available from administrative records alone. Although, to date, most of the studies linking survey and administrative records to adjust for misreporting in the CPS have focused on a single state (e.g., Meyer and Mittag, 2019). Alternatively, Larrimore et al. (2022) have recently suggested an imputation approach to adjust for underreporting of UI benefit receipt in the public-use CPS data that does not require individual level administrative data, which could be a promising avenue for future nationally-representative research on this topic.

Third, it is important to note that certain states that applied for UI modernization funds under the ARRA already had pre-existing CPR provisions in place. These states could apply for and receive ARRA funding so long as their existing provisions included all three family-related reasons as good cause for leaving a job: (1) domestic violence, (2) spouse relocation, and (3) caregiving obligations. Several states modified their existing provisions to better meet these criteria and the suggested languages offered by the Department of Labor. For example, California and Oklahoma modified the existing statutory language to allow caregiving of any family member, not only children, and Illinois and Washington choose to redefine leaving work for illness or disability in a more inclusive way. In our main analysis, the variation in our CFR measure comes from states newly adopting the component of caregiving obligations, as well as from states modifying the existing languages for this component in order to receive the second portion of ARRA funds, so these are likely lower-bound estimates of the effects of CFR adoptions on caregivers.

Finally, despite leveraging recent advancements in DinD estimations to refine our analyses of the UIM effects, the constraints of available statistical packages posed certain limitations to our study. Notably, Callaway and Sant'Anna's (2021) substantial contributions to DinD models, allowing for nuanced analysis of heterogeneous treatment effects, could not be fully utilized due to the specific structure of our data. Their methodology, while implementable through the csdid package for Stata (Rios-Avila, Callaway, and Sant'Anna, 2021), was incompatible with our dataset consisting of repeated individual observations at given time-points within a state panel. Further, the current version of the csdid package did not support an interaction between the treatment identifier and the target group identifier—a crucial aspect given the focus of our study. Our choice to adhere to the TWFE estimate were driven by our research questions and hypotheses. Wooldridge (2021) posits that TWFE proves to be more flexible and efficient when considering the appropriate comparison group and timeframe. This flexibility is critical given the complexities of the our study, which involves with three policy adoptions and their policy effects on target populations. Given the dynamic nature of the field of the DinD models, we anticipate forthcoming insights on DinD models with multiple staggered policy treatments and a multi-level data structure, and hope for updated statistical tools capable of accommodating these research design elements.

Discussion and Policy Implications

Policy conversations on improving the effectiveness of the UI program have often focused on modifying states' eligibility criteria as a mechanism for increasing the number of workers that qualify for benefits. A common concern is that eligibility criteria that are too restrictive prevent the UI program from being able to meet the needs of workers in the modern economy during times of major economic upheaval, such as the Great Recession or the most recent economic downturn in the early months of the COVID-19 pandemic. This study examined whether state adoptions of alternative base period (ABP), compelling family reasons (CFR), and part-time (PT) eligibility provisions under the ARRA increased rates of UI receipt among unemployed workers. We focused on increases in take up of UI benefits among unemployed workers generally and among specific groups of workers who were targeted by these provisions, including low-wage workers, part-time workers, caregivers, and female workers.

Our study observed an increase in UI receipt by 4-5 percentage points following an ABP adoption, regardless of education. This finding is divergent from both the overall and the subgroup findings of Gould-Werth and Shaefer (2013), which identified concentrated ABP effects of 3 percentage points on a small subset of part-time workers with less than a high school diploma. The divergence in findings between our study and theirs may be attributable to the different data timeframes and distinct cohort effects of ABP observed in each respective study. Over the course of the ABP implementing years, substantial modifications in labor force characteristics and economic conditions could have influenced the impact of ABPs on UI receipt. Specifically, the growing incidence of non-traditional employment structures, evolving educational makeup of the labor force, and the fluctuations in economic conditions could all influence the effectiveness of ABPs. Further, our main analysis centered on the influence of ARRA-induced ABP adoption, contrasting with their study that included the early ABP adopters, which could attenuate their estimates. Our sensitivity analysis indicates that the effect of ABP becomes more discernible when early ABP adopters are excluded from the sample. The broader impact observed in our research indicates a dynamic nature of ABP effects over time, a finding that significantly adds to the body of knowledge on ABPs and UI receipt.

When we further stratified by gender we found that the ABP effect was largely concentrated among male workers without a college degree (6-7 percentage points). This finding was contrary to the expressed policy aims (increasing take-up among low-wage female workers) and evidence from the previous simulation studies which found that workers who would become eligible for benefits under any of the three provisions were more likely to be female (Linder and Nichols, 2012; Callen et al., 2015). Our study differs from these simulation studies in that our estimates include a behavioral effect while the simulation studies assume that a change in eligibility results in take up of UI benefits. Unobserved factors such as perceived ineligibility (Vroman 2008b; Shaefer and Wu 2011); barriers in the UI claim-making process (Gould-Werth 2016; Wentworth 2018); and gaps between policy adoption, implementation, and policy-learning (Vroman 2008a) might attenuate the effects of these provisions on take-up of UI receipt among those who are eligible and targeted.

Regarding the effects of CFR provisions, our findings did not indicate a significant overall impact on unemployed workers. However, some evidence suggests differential impact between caregivers and non-caregivers. Specifically, CFR provisions increase take-up of 5 percentage points among caregivers, an effect not observed among non-caregivers. We also found that increases in UI receipt following adoption of CFR provisions were concentrated among female caregivers, which is in keeping with the stated policy goals and with conclusion from Callen et al. (2015) regarding increases in UI eligibility among female workers who had more children. Although our study is the first to explicitly examine the CFR effects on caregivers, and to contribute to current understanding of the extent to which these provisions facilitating access to the UI program and providing income support to workers juggling employment and family responsibilities during spells of unemployment, the value of non-monetary provisions has also been demonstrated by Ventor (2020) who found that CFR provisions improve post-move labor market outcomes for trailing spouses. It is also important to note that under the CFR provisions, eligibility is only extended to those who leave work to attend to caregiving obligations if they satisfy the “able” and “available” for work criteria. Workers who voluntarily leave their positions and are engaged in intensive caregiving remain ineligible for UI.

In considering the impact of PT provisions, our research did not discern a statistically significant average effect among all unemployed workers. Consistent with our hypothesis, our findings indicate increases in UI receipt among part-time workers compared to their full-time counterparts, and among female workers compared to male workers following a PT adoption. Our estimates suggest the average effect of PT provisions for part-time workers to be around 6 percentage points. This effect was largely concentrated among part-time male workers (7 percentage points) in our primary model, while it was largely concentrated among part-time female workers (6.6 percentage points) when pre-ARRA PT adopters were included in the alternative model. Greater incidence of unemployment among part-time male workers following the Great Recession (U.S. Bureau of Labor Statistics 2018) might have increased the likelihood that these workers received unemployment benefits in states that implemented a PT provision after the passage of the ARRA.

These ARRA-induced policy changes occurred over a decade ago, and some might consider them relatively minor reforms, especially in comparison to recent temporary changes enacted in response to the COVID-19 pandemic. The Coronavirus Aid, Relief, and Economic Security (CARES) Act of 2020 temporarily increased benefit amounts, extended the number of weeks that workers could claim benefits, and expanded eligibility to workers who would normally not qualify for benefits. These changes resulted a historically high UI recipiency rate of 78 percent in 2020 (U.S. Department of Labor 2021). Most of these temporary programmatic changes have ended, returning state programs within the UI system to their status quo policies, and returning the recipiency rate to about 37 percent in 2021 (U.S. Department of Labor 2021).

There has been some interest in making permanent changes to the UI system in the wake of the pandemic that appear to similar in scale and scope to the ARRA policy changes examined in this paper. President Biden's FY2022 budget proposal supported reforms to the UI system to advance equity and security (The White House 2021). Additionally, both houses of the 117th Congress introduced the Unemployment Insurance Improvement Act (2021, S.2865 and H.R.5507), which would federally mandate the inclusion of ABP and the PT provisions in eligibility determinations. Notably, the CFR provision was discussed in a draft proposal of the Unemployment Insurance Improvement Act of 2021 by the Senate Committee on Finance (2021) but was not included in the introduced Act (i.e., S.2865 and H.R.5507). Although this might have been due to a lack of political consensus on whether UI should apply to workers who leave work due to family circumstances, it could reflect the lack of evidence of the provision's effectiveness at the time the bill was drafted and the potential for studies such as ours to inform future conversations.

Conclusion

This study examined the impact of three UI eligibility provisions adopted by states under the ARRA on rates of UI receipt among unemployed workers. Although this paper does not directly engage with the broader theoretical issue about whether these changes result in optimal rates of UI receipt, it significantly enhances our understanding of the heterogenous effects of the expansion of UI eligibility. We found the implementation of the ABP increased UI take-up by about 5 percentage points broadly. While our investigation did not discern significant CFR and PT effects across all unemployed workers, it is critical to emphasize the impacts observed for specific subgroups. CFR provisions appeared to increase take-up among caregivers by approximately 5 percentage points, and PT provisions increase take-up among previously part-time workers by approximately 6 percentage points. In addition to these findings, our study we found some evidence of differential impacts based on gender, suggesting the importance to account for this demographic heterogeneity in the future UI research and policymaking.

For those concerned about the potential for expansions of UI eligibility criteria to increase administrative costs and deplete state UI funds, our evidence does not indicate large increases in overall take up of UI benefits among unemployed workers. However, for those interested in making UI broadly available to the largest group of unemployed workers, particularly those economically disadvantages, these findings may suggest the need to consider additional policy levers, such as addressing barriers to applying for benefits and conducting outreach or education to better inform unemployed workers of their eligibility. Empirical evidence from this study substantively contributes to a more nuanced understanding of UI policy implications.

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TABLE 1. UI modernization provisions - Implementation dates and incentive funding by state

State	Alternative Base Period	Compelling Family Reasons	Part-Time Workers	UIM Incentive Funding (millions)
Alabama				$0.0
Alaska	01/2010	04/2010		$15.6
Arizona				$0.0
Arkansas	07/2009	07/2009	07/2009	$60.0
California	04/2012	04/2011	at least 01/2002	$838.7
Colorado	07/2009	07/2009	08/2009	$127.5
Connecticut	01/2003	04/2009		$87.8
Delaware	01/2010	01/2010	01/2010	$21.9
DC	03/2003	07/2010	1978	$27.6
Florida				$0.0
Georgia	01/2003		04/2009	$220.3
Hawaii	01/2004	07/2009	07/2009	$30.5
Idaho	10/2009		01/2010	$32.3
Illinois	01/2008	01/2010		$301.2
Indiana				$0.0
Iowa	07/2009		07/2009	$70.8
Kansas	01/2010		01/2010	$69.0
Kentucky				$0.0
Louisiana			at least 01/2002	$0.0
Maine	09/1992	04/2009	01/2004	$28.2
Maryland	03/2011		03/2011	$126.8
Massachusetts	01/1994		01/2004	$162.7
Michigan	10/2000			$69.4
Minnesota	08/2009	08/2009	08/2009	$130.1
Mississippi				$0.0
Missouri				$0.0
Montana	05/2009		05/2009	$19.5
Nebraska	07/2011		07/2011	$43.6
Nevada	04/2009	05/2009	05/2009	$76.9
New Hampshire	04/2001	09/2009	08/2008	$31.4
New Jersey	07/1995		01/2004	$206.8
New Mexico	01/2005		01/2005	$39.0
New York	04/1999	05/2009	05/2009	$412.7
North Carolina	09/1997	01/2010[d]	01/2010[d]	$205.1
North Dakota				$0.0

TABLE 1. UI modernization provisions - Implementation dates and incentive funding by state

State	Alternative Base Period	Compelling Family Reasons	Part-Time Workers	UIM Incentive Funding (millions)
Ohio	01/1995			$88.2
Oklahoma	11/2009	11/2009	11/2009	$75.9
Oregon	07/2009	05/2009		$85.6
Pennsylvania				$0.0
Rhode Island	10/1992	01/2011		$23.5
South Carolina	01/2011	01/2011	01/2011	$97.5
South Dakota	07/2009		07/2010	$17.6
Tennessee	06/2010[d]		06/2010[d]	$141.8
Texas				$0.0
Utah	01/2011			$20.3
Vermont	01/1988		1976	$13.9
Virginia	07/2003			$62.8
Washington	04/1994	07/2012		$146.6
West Virginia	04/2009			$11.1
Wisconsin	01/2000	05/2009		$133.9
Wyoming			at least 01/2002	$0.0
Total	39	21	29	$4,417.2

Note.—Adapted from “Unemployment Insurance modernization incentive payment state certifications,” by U.S. Department of Labor, 2011(http://www.doleta.gov/recovery/). Also see references: Callan et al. (2015); Gould-Werth and Shaefer (2013). d: a provision was deleted in TN's and NC's UI laws in July 2013.

TABLE 2. Average Treatment Effects of UI Modernization Provisions on UI benefit receipt among Treated States, Overall and by Subgroup

(1) Alternative Base Period		(2) Compelling Family Reasons		(3) Part-Time Provision		Joint Test (chi2)
Overall	0.047 (0.015)**	Overall	0.012 (0.009)	Overall	0.021 (0.013)	12.48**
	[0.016, 0.078]		[-0.006, 0.031]		[-0.006, 0.049]	
Diff.	0.009 (0.010)	Diff.	0.040 (0.023)†	Diff.	0.059 (0.013)***	23.4***
	[-0.012, 0.030]		[-0.005, 0.085]		[0.034, 0.085]	
College Degree	0.041 (0.017)*	Non-Caregiver	0.011 (0.009)	Full-Time	0.005 (0.013)	
	[0.0002, 0.082]		[-0.010, 0.033]		[-0.027, 0.037]	
No College Degree	0.050 (0.015)**	Caregiver	0.051 (0.023)†	Part-Time	0.064 (0.017)**	
	[0.014, 0.086]		[-0.002, 0.104]		[0.024, 0.105]	
Joint Test (F)	5.61**		2.73†		11.53***	
N	61400		99311		75564	

Note. † p < .10; * p < .05; ** p < .01; *** p < .001. The table presents estimates from the linear probability model. Excluding states with pre-ARRA-compliant adoptions yields different sample sizes for provision. Robust cluster standard errors in parentheses. Diff. refers to the differential ATT effect for low-wage workers, family caregivers, and part-time workers relative to their counterparts. The p-values for subgroups are adjusted for multiple comparisons using the Bonferroni method. Bonferroni-adjusted 95% confidence intervals in brackets. An F-test is reported for the joint hypothesis that all policy effects of subgroups are zero. A chi2 test is reported for the joint hypothesis that all three UIM effects are zero.

TABLE 3. Average Treatment Effects of UI Modernization Provisions on UI benefit receipt among Treated States, by Sex and Subgroup

(1) Alternative Base Period		(2) Compelling Family Reasons		(3) Part-Time Provision	
Diff.	-0.037(0.009)***	Diff.	0.001 (0.014)	Diff.	0.028 (0.013)*
	[-0.054, -0.019]		[-0.027, 0.029]		[0.001, 0.055]
Male	0.061 (0.015)***	Male	0.011 (0.011)	Male	0.010 (0.014)
	[0.026, 0.096]		[-0.013, 0.036]		[-0.023, 0.043]
Female	0.024 (0.018)	Female	0.012 (0.013)	Female	0.038 (0.016)*
	[-0.018, 0.066]		[-0.017, 0.042]		[0.0003, 0.075]
Alternative Base Period		Compelling Family Reasons		Part-Time Provision	
Male, Diff.	0.015 (0.012)	Male, Diff.	0.024 (0.035)	Male, Diff.	0.078 (0.017)***
	[-0.014, 0.044]		[-0.058, 0.105]		[0.042, 0.114]
Male, College Degree	0.051 (0.020)†	Male, Non-Caregiver	0.011 (0.011)	Male, Full-Time	-0.007 (0.016)
	[-0.002, 0.104]		[-0.016, 0.039]		[-0.049, 0.034]
Male, No College Degree	0.066 (0.014)***	Male, Caregiver	0.035 (0.037)	Male, Part-Time	0.070 (0.016)***
	[0.030, 0.102]		[-0.062, 0.131]		[0.029, 0.112]
Female, Diff.	-0.011 (0.020)	Female, Diff.	0.046 (0.025)†	Female, Diff.	0.033 (0.017)†
	[-0.057, 0.035]		[-0.012, 0.104]		[-0.009, 0.075]
Female , College Degree	0.033 (0.018)	Female, Non-Caregiver	0.010 (0.013)	Female, Full-Time	0.025 (0.016)
	[-0.014, 0.081]		[-0.025, 0.044]		[-0.016, 0.067]
Female, No College Degree	0.022 (0.022)	Female, Caregiver	0.056 (0.023)†	Female, Part-Time	0.058 (0.022)*
	[-0.036, 0.080]		[-0.005, 0.116]		[0.002, 0.115]
Joint Test (F)	10.57***		1.49		7.99***
N	61400	N	99311	N	75564

Note. † p < .10; * p < .05; ** p < .01; *** p < .001. The table presents estimates from the linear probability model. The p-values are adjusted for multiple comparisons using the Bonferroni method. Robust cluster standard errors in parentheses. Diff. refers to the differential ATT effect for females relative to males in the top panel, and for low-wage workers, family caregivers, and part-time workers relative to their counterparts in the bottom panel. Bonferroni-adjusted 95% confidence intervals in brackets. An F-test is reported for the joint hypothesis that all policy effects of four subgroups are zero.

TABLE 4a. Sensitivity Tests for the Effects of the Alternative Base Period (ABP) Provision

	(1)	(2)	(3)	(4)	(5)	(6)
ABP Effect	Main models	Exclude linear time trend	Include early adopters	Exclude later treatment years	Exclude non-ABP adopters	Exclude individuals with no work history
Overall	0.047**	0.040*	0.017	0.050*	0.045*	0.048**
	(0.016)	(0.017)	(0.010)	(0.020)	(0.019)	(0.017)
College Degree	0.042†	0.035	0.017	0.054†	0.046†	0.046*
	(0.018)	(0.020)	(0.011)	(0.024)	(0.020)	(0.019)
No College Degree	0.049**	0.042*	0.017	0.048*	0.045†	0.048*
	(0.016)	(0.018)	(0.012)	(0.020)	(0.019)	(0.017)
Male	0.062***	0.055**	0.021	0.067**	0.058*	0.059**
	(0.015)	(0.018)	(0.012)	(0.018)	(0.019)	(0.017)
Female	0.026	0.019	0.011	0.026	0.028	0.034
	(0.018)	(0.019)	(0.012)	(0.026)	(0.020)	(0.019)
Male , College Degree	0.051†	0.044	0.014	0.068†	0.051	0.049†
	(0.020)	(0.025)	(0.012)	(0.028)	(0.023)	(0.021)
Female , College Degree	0.033	0.026	0.019	0.041	0.040	0.044
	(0.018)	(0.023)	(0.012)	(0.026)	(0.019)	(0.020)
Male, No College Degree	0.066***	0.060**	0.024	0.066**	0.061**	0.063**
	(0.014)	(0.019)	(0.014)	(0.018)	(0.017)	(0.016)
Female, No College Degree	0.022	0.015	0.005	0.018	0.019	0.028
	(0.022)	(0.021)	(0.015)	(0.030)	(0.024)	(0.022)
N	61400	61400	74040	46434	38197	75546

Note. † p < .10; * p < .05; ** p < .01; *** p < .001. The table presents estimates from the linear probability model. The p-values are adjusted for multiple comparisons using the Bonferroni method. Robust cluster standard errors in parentheses. All coefficients are two-way fixed effect estimates of the average treatment effects among treated states.

TABLE 4b. Sensitivity Tests for the Effects of the Compelling Family Reasons Provision

	(1)	(2)	(3)	(4)	(5)	(6)
CFR Effect	Main models	Exclude linear time trend	Include early adopters	Exclude later treatment years	Exclude non-ABP adopters	Exclude individuals with no work history
Overall	0.012	-0.002	N/A	0.015	0.015	0.005
	(0.009)	(0.007)		(0.011)	(0.010)	(0.009)
Non-Caregiver	0.011	-0.004	N/A	0.014	0.014	0.004
	(0.009)	(0.007)		(0.011)	(0.010)	(0.010)
Caregivers	0.051†	0.036	N/A	0.054†	0.050	0.030
	(0.022)	(0.020)		(0.037)	(0.024)	(0.016)
Male	0.012	-0.003	N/A	0.015	0.015	0.006
	(0.011)	(0.008)		(0.014)	(0.011)	(0.011)
Female	0.013	-0.002	N/A	0.016	0.016	0.005
	(0.013)	(0.009)		(0.012)	(0.013)	(0.011)
Male, Non-Caregiver	0.011	-0.003	N/A	0.014	0.015	0.006
	(0.011)	(0.008)		(0.013)	(0.011)	(0.011)
Female, Non-Caregiver	0.010	-0.005	N/A	0.013	0.013	0.000
	(0.013)	(0.009)		(0.013)	(0.014)	(0.012)
Male, Caregiver	0.035	0.019	N/A	0.055	0.032	0.009
	(0.037)	(0.040)		(0.066)	(0.038)	(0.020)
Female, Caregiver	0.056†	0.041	N/A	0.053	0.055	0.035
	(0.023)	(0.022)		(0.036)	(0.025)	(0.017)
N	99311	99311		75952	76108	123150

Note. † p < .10; * p < .05; ** p < .01; *** p < .001. The table presents estimates from the linear probability model. The p-values are adjusted for multiple comparisons using the Bonferroni method. Robust cluster standard errors in parentheses. All coefficients are two-way fixed effect estimates of the average treatment effects among treated states.

TABLE 4c. Sensitivity Tests for the Effects of the Part-Time (PT) Provision

	(1)	(2)	(3)	(4)	(5)	(6)
PT effects	Main models	Exclude linear time trend	Include early adopters	Exclude later treatment years	Exclude non-ABP adopters	Exclude individuals with no work history
Overall	0.020	0.009	0.028*	0.010	0.023	0.014
	(0.014)	(0.010)	(0.012)	(0.021)	(0.016)	(0.014)
Full-time Worker	0.005	-0.006	0.016	-0.005	0.007	-0.01
	(0.014)	(0.011)	(0.013)	(0.020)	(0.016)	(0.014)
Part-time Worker	0.064**	0.053***	0.062***	0.055†	0.066**	0.050**
	(0.018)	(0.012)	(0.014)	(0.025)	(0.021)	(0.015)
Male	0.008	-0.003	0.014	-0.006	0.01	0.001
	(0.014)	(0.011)	(0.013)	(0.023)	(0.017)	(0.015)
Female	0.037†	0.026*	0.047**	0.033	0.039†	0.031
	(0.016)	(0.011)	(0.013)	(0.021)	(0.018)	(0.015)
Male, Full-time Worker	-0.007	-0.018	0.003	-0.022	-0.005	-0.022
	(0.016)	(0.012)	(0.015)	(0.024)	(0.018)	(0.016)
Female, Full-time Worker	0.025	0.015	0.037*	0.023	0.028	0.01
	(0.016)	(0.014)	(0.014)	(0.021)	(0.018)	(0.016)
Male, Part-time Worker	0.070***	0.060***	0.058**	0.061†	0.073**	0.047**
	(0.016)	(0.015)	(0.014)	(0.024)	(0.020)	(0.015)
Female, Part-time Worker	0.058*	0.048**	0.066**	0.051	0.060†	0.052**
	(0.022)	(0.014)	(0.017)	(0.029)	(0.024)	(0.018)
N	75564	75564	82005	58315	54705	93432

Note. † p < .10; * p < .05; ** p < .01; *** p < .001. The table presents estimates from the linear probability model. The p-values are adjusted for multiple comparisons using the Bonferroni method. Robust cluster standard errors in parentheses. All coefficients are two-way fixed effect estimates of the average treatment effects among treated states.

TABLE 5. Average Treatment Effects of UI Modernization Provisions on UI benefit receipt among Treated States Using Alternative Measures for Target Group

Alternative Base Period		Compelling Family Reasons	
Diff. (SNAP vs. no)	0.012(0.017)	Diff. (<5 vs. No)	0.002 (0.019)
	[-0.022, 0.047]		[-0.042, 0.045]
		Diff. (5-17 vs. No)	0.011 (0.012)
			[-0.017, 0.039]
No SNAP Receipt	0.031 (0.016)	No Children < 18	0.010 (0.012)
	[-0.006, 0.069]		[-0.02, 0.041]
SNAP Receipt	0.043 (0.017)*	Children < 5	0.012 (0.016)
	[0.004, 0.083]		[-0.027, 0.051]
		Children 5 to 17	0.021 (0.01)
			[-0.003, 0.045]
Male, Diff. (SNAP vs. no)	0.009 (0.017)	Male, Diff. (<5 vs. No)	-0.016 (0.020)
	[-0.032, 0.050]		[-0.068, 0.037]
		Male, Diff. (5-17 vs. No)	0.009 (0.018)
			[-0.037, 0.054]
Male , No SNAP Receipt	0.061 (0.015)***	Male, No Children < 18	0.013 (0.012)
	[0.022, 0.100]		[-0.019, 0.045]
Male, SNAP Receipt	0.069 (0.02)**	Male, Children < 5	-0.003 (0.020)
	[0.017, 0.122]		[-0.057, 0.051]
		Male, Children 5 to 17	-0.021(0.016)
			[-0.023, 0.066]
Female, Diff. (SNAP vs. no)	0.034 (0.027)	Female, Diff. (<5 vs. No)	0.022 (0.025)
	[-0.029, 0.034]		[-0.044, 0.087]
		Female, Diff. (5-17 vs. No)	0.014 (0.016)
			[-0.028, 0.056]
Female , No SNAP Receipt	0.014 (0.019)	Female, No Children < 18	0.006 (0.018)
	[-0.038, 0.065]		[-0.044, 0.055]
Female, SNAP Receipt	0.048 (0.024)	Female, Children < 5	0.027 (0.016)
	[-0.015, 0.111]		[-0.018, 0.72]
		Female, Children 5 to 17	0.019 (0.015)
			[-0.021, 0.060]
Joint Test (F)	12.61***	Joint Test (F)	1.210
N	61400	N	99311

Note. † p < .10; * p < .05; ** p < .01; *** p < .001. The table presents estimates from the linear probability model. The p-values are adjusted for multiple comparisons using the Bonferroni method. Robust cluster standard errors in parentheses. Bonferroni-adjusted 95% confidence intervals in brackets.. An F-test is reported for the joint hypothesis that all policy effects of subgroups are zero.

Online Appendix (not for print publication)

TABLE A1. Sample characteristics (*N*=99,311 Unemployed Workers)

Individual and Household Measures			State-Level Measures	
		Outcome		
UI receipt (%)		32.63		
		Covariates of Interest		
Targeted Groups (%)			UI modernizations (%)	
Low-wage worker	(Less than high school)	15.20	ABP provision	53.77
Caregiver		4.41	CFR provision	23.45
Part-time worker		25.90	PT provision	37.18
Female worker		44.49		
		Other Covariates		
Age of Youngest Child in HH (%)	No children < age 18	52.37	Unemployment Rate (%)	6.39
	Youngest child < age 5	17.21	Minimum wage ($)	7.00
	Youngest child age 5 to 17	30.41	Training benefit provision (%)	16.23
Household received SNAP (%)		19.30	Avg. UI duration (weeks)	16.11
Race & Ethnicity (%)	White, non-Hispanic	58.10	UI replacement rate (%)	41.00
	Black, non-Hispanic	13.20	UI reserve ratio	0.73
	Hispanic of any race	20.37		
	Other race, non-Hispanic	8.33		
U.S. citizen (%)		87.27		
Marital status (%)	Married	51.75		
	Widowed, divorced, or separated	20.12		
	Never married	28.13		
Age (years)		40.98		
Weeks unemployed		20.71		
Occupation	Management & professional	23.36		
	Service, sales, & office	40.58		
	Construction	36.06		
Work-limiting disability		6.12		
Union member		1.14		
Homeowner		42.06		
Lives in metro area		79.58		

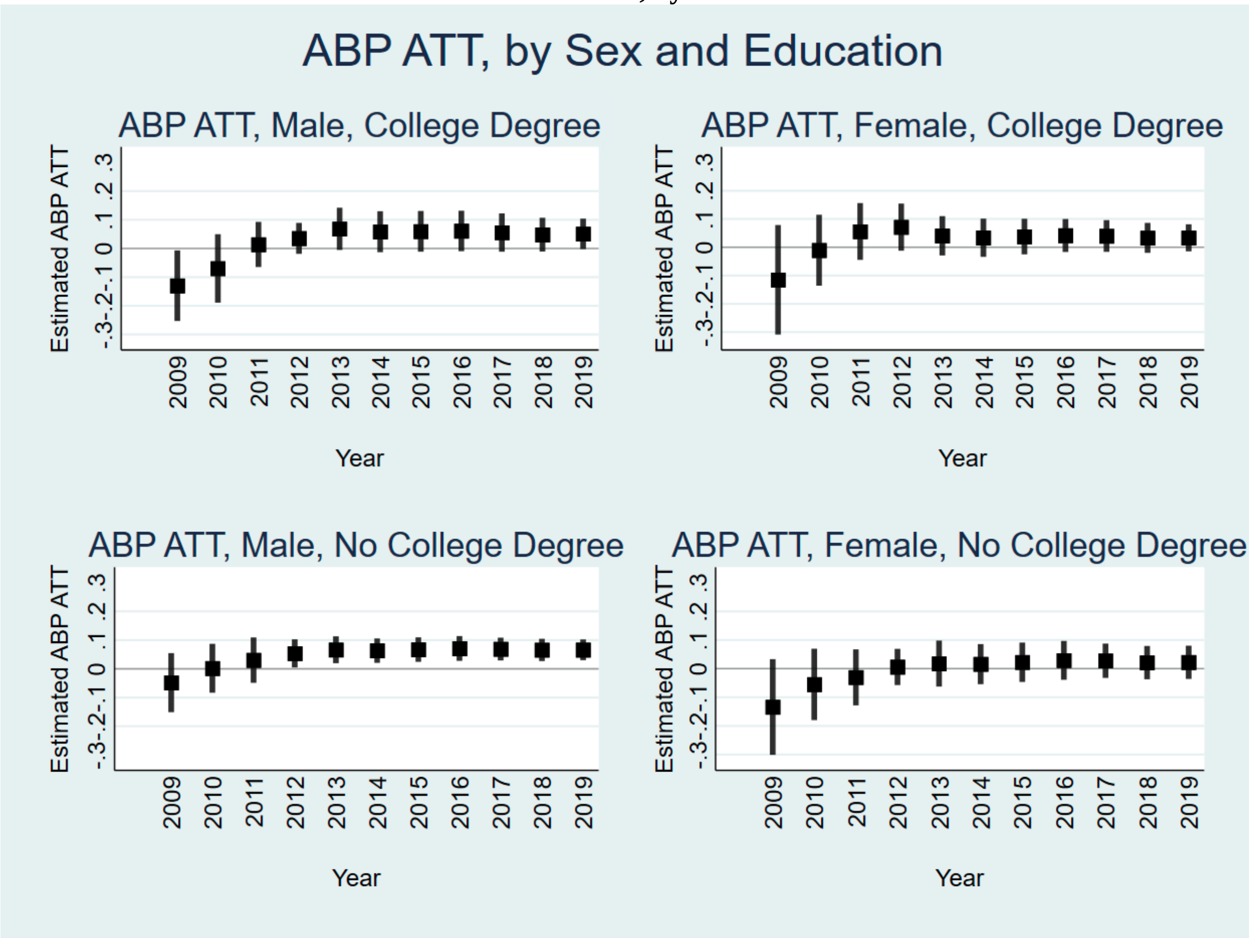
Note. ABP: alternative base period; CFR: compelling family reasons; PT: part-time

TABLE A2. Predicted Probabilities and Percent Changes for Average Treatment Effects among the Treated States Estimated in TABLES 2 & 3

ABP=0, Overall	0.284	CFR=0, Overall	0.308	PT=0, Overall	0.302
	(0.004)		(0.002)		(0.003)
ABP=1, Overall	0.331	CFR=1, Overall	0.32	PT=1, Overall	0.324
	(0.011)		(0.007)		(0.011)
Percent Increase	16.5%	*Percent Increase*	3.9%	*Percent Increase*	7.3%
ABP=0, College Degree	0.291	CFR=0, Non-caregiver	0.313	PT=0, Full-time	0.351
	(0.007)		(0.002)		(0.003)
ABP=1, College Degree	0.332	CFR=1, Non-caregiver	0.324	PT=1, Full-time	0.356
	(0.013)		(0.007)		(0.011)
Percent Increase	14.1%	*Percent Increase*	3.5%	*Percent Increase*	1.4%
ABP=0, No College Degree	0.281	CFR=0, Caregiver	0.171	PT=0, Part-time	0.163
	(0.005)		(0.010)		(0.006)
ABP=1, No College Degree	0.331	CFR=1, Caregiver	0.222	PT=1, Part-time	0.228
	(0.011)		(0.020)		(0.013)
Percent Increase	17.8%	*Percent Increase*	29.8%	*Percent Increase*	39.9%
ABP=0, Male	0.269	CFR=0, Male	0.299	PT=0, Male	0.298
	(0.005)		(0.003)		(0.004)
ABP=1, Male	0.33	CFR=1, Male	0.31	PT=1, Male	0.307
	(0.011)		(0.008)		(0.011)
Percent Increase	22.7%	*Percent Increase*	3.7%	*Percent Increase*	0.030
ABP=0, Female	0.306	CFR=0, Female	0.319	PT=0, Female	0.309
	(0.005)		(0.004)		(0.003)
ABP=1, Female	0.33	CFR=1, Feale	0.332	PT=1, Female	0.346
	(0.014)		(0.010)		(0.014)
Percent Increase	7.8%	*Percent Increase*	4.1%	*Percent Increase*	12.0%
ABP=0, College Degree, Male	0.297	CFR=0, Non-caregiver, Male	0.304	PT=0, Full-time Worker, Male	0.35
	(0.009)		(0.003)		(0.004)
ABP=1, College Degree, Male	0.347	CFR=1, Non-caregiver, Male	0.315	PT=1, Full-time Worker, Male	0.343
	(0.016)		(0.008)		(0.013)
Percent Increase	16.8%	*Percent Increase*	3.6%	*Percent Increase*	-2.0%
ABP=0, No College Degree, Male	0.256	CFR=0, Caregiver, Male	0.161	PT=0, Part-time Worker, Male	0.140
	(0.005)		(0.013)		(0.006)
ABP=1, No College Degree, Male	0.323	CFR=1, Caregiver, Male	0.196	PT=1, Part-time Worker, Male	0.210
	(0.010)		(0.034)		(0.013)
Percent Increase	26.2%	*Percent Increase*	21.7%	*Percent Increase*	50.0%
ABP=0, No College Degree, Male	0.289	CFR=0, Non-caregiver, Female	0.325	PT=0, Part-time Worker, Male	0.351
	(0.006)		(0.004)		(0.004)
ABP=1, No College Degree, Male	0.323	CFR=1, Non-caregiver, Female	0.335	PT=1, Part-time Worker, Male	0.376
	(0.013)	*Percent Increase*	(0.010)		(0.015)
Percent Increase	11.8%	*Percent Increase*	3.1%	*Percent Increase*	7.1%
ABP=0, No College Degree, Female	0.317	CFR=0, Caregiver, Female	0.184	PT=0, Part-time Worker, Female	0.185
	(0.007)		(0.011)		(0.007)
ABP=1, No College Degree, Female	0.339	CFR=1, Caregiver, Female	0.239	PT=1, Part-time Worker, Female	0.244
	(0.017)		(0.020)		(0.017)
Percent Increase	6.9%	*Percent Increase*	29.9%	*Percent Increase*	31.9%

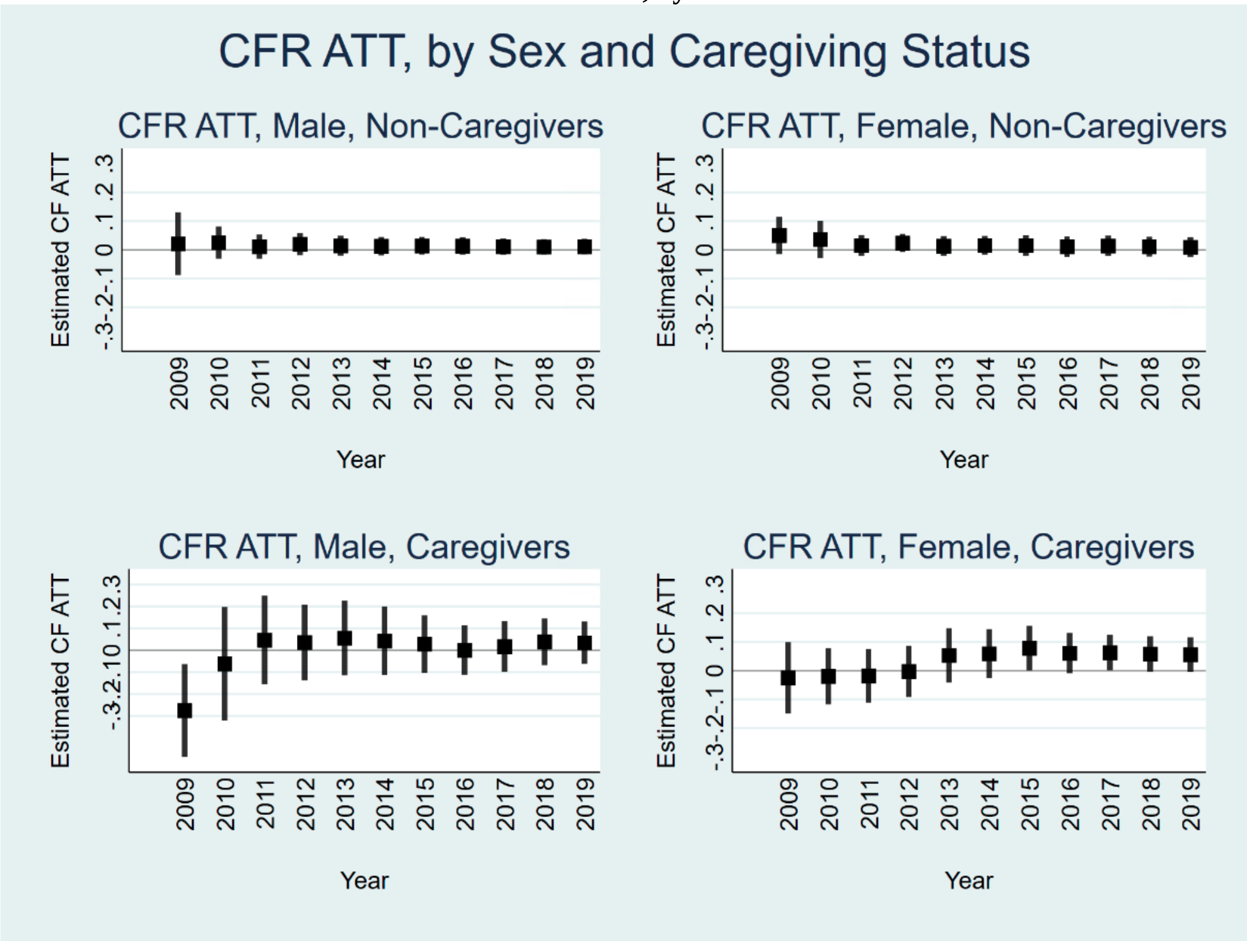
Note. ABP: alternative base period; CFR: compelling family reasons; PT: part-time. The table presents estimates from the linear probability model. Robust cluster standard errors in parentheses.

FIGURE A1a. Estimated ABP Effects over Time, by Sex and Education



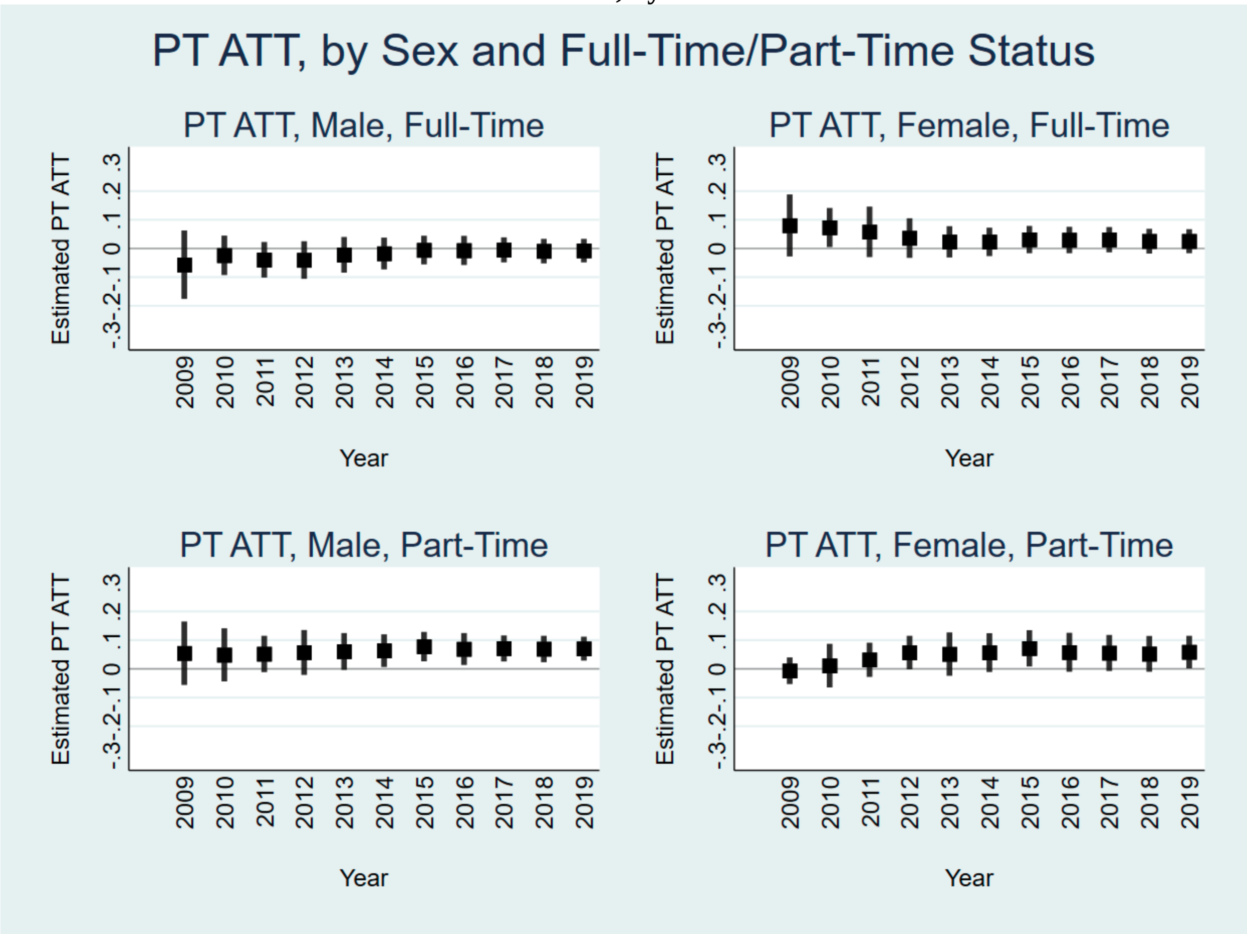
Note. ABP: alternative base period provision; ATT: average treatment effect among the treated states. The table presents two-way fixed effect estimates and the Bonferroni-adjusted 95% confidence intervals from the linear probability model.

FIGURE A1b. Estimated CFR Effects over Time, by Sex and Education



Note. CFR: compelling family reasons provision; ATT: average treatment effect among the treated states. The table presents two-way fixed effect estimates and the Bonferroni-adjusted 95% confidence intervals from the linear probability model.

FIGURE A1c. Estimated PT Effects over Time, by Sex and Education



Note. PT: part-time provision; ATT: average treatment effect among the treated states. The table presents two-way fixed effect estimates and the Bonferroni-adjusted 95% confidence intervals from the linear probability model.