Wage Insurance for Displaced Workers*

Benjamin Hyman
Federal Reserve Bank of New York

Brian Kovak
Carnegie Mellon University and NBER

Adam Leive
University of California - Berkeley

March 29, 2024

Abstract

Wage insurance provides income support to displaced workers who find reemployment at a lower wage. We analyze wage insurance in the context of the U.S. Trade Adjustment Assistance (TAA) program by merging linked employer-employee Census data to TAA petitions and leveraging a discontinuity in eligibility based on worker age. Wage insurance eligibility increases short-run employment probabilities and leads to higher long-run cumulative earnings. We find shorter non-employment durations largely drive increased long-term earnings among workers eligible for wage insurance. Our results are quantitatively consistent with a standard non-stationary partial equilibrium search model. The program is self-financing even under conservative assumptions.

*We thank Theodore Naff and Kelsey Pukelis for outstanding research assistance. We thank Richard Audoly, Andy Garin, Hilary Hoynes, Felix Koenig, Pawel Krolikowski, Dorothy Kronick, Tuomas Matikka, Amanda Michaud, David Molitor, Jesse Rothstein, Johannes Schmieder, Lowell Taylor, Nathanael Velekoop, and seminar participants at Boston University, Carnegie Mellon University, Chicago-Booth, Duke, the Federal Reserve Banks of New York, San Francisco, and the Board of Governors, University of Georgia, University of Nottingham, Oregon, Penn State, Princeton NLS-E, the NTA meetings, UC-Berkeley, UC-Santa Cruz, the IRP Summer Research Workshop, and the Globalization and Workforce Composition Workshop for helpful comments. We are grateful to Jooyoun Park and Kara Reynolds for sharing TAPR data. This research was supported by the National Science Foundation under Grant No. SES-1851679. Leive acknowledges support from the UVA Bankard Fund for Political Economy. Kovak acknowledges support from the Block Center for Technology and Society at Carnegie Mellon. This paper was made possible (in part) by a grant from the Carnegie Corporation of New York. This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by National Science Foundation Grants SES-9978093, SES-0339191 and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the authors and do not necessarily reflect the views of the National Science Foundation, the Federal Reserve Bank of New York, the Federal Reserve System, or the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 1762. (CBDRB-FY23-P1762-R10162, CBDRB-FY23-P1762-R10429, CBDRB-FY23-P1762-R10845, CBDRB-FY24-P1762-R11193).

1 Introduction

Workers who lose their jobs often experience substantial earnings losses (Jacobson et al. 1993; Stevens 1997; Kletzer 1998; Couch and Placzek 2010; Schmieder and von Wachter 2010; Davis and von Wachter 2011; Flaaen et al. 2019; Schmieder et al. 2023), persistent non-employment (Ruhm 1991; Chan and Stevens 2001), and lower wealth (Stevens and Moulton 2013). While standard policies like unemployment insurance temporarily cushion the impacts of job loss, and retraining can help some workers re-skill, in many cases these policies have proven insufficient at compensating workers whose livelihoods are lost.[1] In fact, studies show a causal link between job displacement and broader societal problems, including lower educational attainment of children (Oreopoulos et al. 2008; Rege et al. 2011; Stevens and Schaller 2011), political polarization (Autor et al. 2020), and higher mortality (Sullivan and Von Wachter 2009; Pierce and Schott 2020). Given the likelihood of ongoing labor market disruption from emerging technologies including artificial intelligence and decarbonization, developing alternative policies to address job displacement is a key goal for policymakers.

In this paper, we study the effects of an innovative policy known as wage insurance, which temporarily subsidizes the earnings of displaced workers whose new job pays less than their old one. Because the subsidy amount is proportional to the earnings decline, the policy is designed to shorten unemployment durations by making reemployment more attractive, particularly in lower-wage jobs. Wage insurance therefore aims to avoid the negative consequences of long unemployment durations documented in the prior literature (Krueger and Mueller 2011; Kroft et al. 2013; Schmieder et al. 2016) and supports workers for whom training is ineffective, infeasible, or unavailable.[2] However, the subsidy may also lead to worse job matches and persistently low wages after benefits expire. These potentially countervailing effects call for empirical assessment, but evidence on the impact of wage insurance programs remains scarce (Cahuc 2018).

We estimate the causal effects of wage insurance on displaced workers' employment and earnings in the U.S. using linked administrative data and a regression discontinuity design. We study the wage insurance provisions of the Trade Adjustment Assistance (TAA) program, which compensates workers who lose employment as a result of international trade. Displaced workers in the traditional TAA program participate in

[1] For example, the adverse effects of Chinese import competition on U.S. workers were more consequential than many economists once thought, revealing the shortcomings of existing policies in overcoming adjustment frictions and compensating losses (Autor et al. 2013, 2014, 2016; Pierce and Schott 2016).

[2] In a meta-analysis, Card et al. (2018) find that the returns to active labor market policies are weaker for older workers, consistent with concave earnings profiles in age and experience found throughout the literature (Heckman et al. 2006). Retraining programs also require both household liquidity to cover expenses during training and foresight about the sectors and geographic locations of future job growth.

mandatory job training and receive extended unemployment insurance payments. Workers age 50 or older are additionally eligible for an alternative program known as Reemployment Trade Adjustment Assistance (RTAA), which does not require job training and instead provides wage insurance, paying up to half of the difference between the worker's pre- and post-separation wages for up to two years. To implement this analysis, we merge administrative data on TAA petitions with the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) dataset to measure employment and earnings outcomes for 76,500 workers at approximately 1,000 TAA-petitioning firms.

Our regression discontinuity (RD) design compares outcomes for workers just above and below the age-50 eligibility cutoff, finding substantial increases in employment and earnings for wage insurance-eligible workers. However, this RD estimate may understate the true effect of wage insurance eligibility because other programs' eligibility rules also change at age 50, most notably disability insurance (Chen and van der Klaauw 2008; Deshpande et al. 2021; Carey et al. 2022). We therefore also estimate a difference-in-discontinuities (D-RD) to net out the negative effects of these other programs, using a sample of displaced workers whose petitions for TAA were denied by the Department of Labor. We present evidence that this denied sample is a credible counterfactual for the TAA-certified sample and note that if workers displaced from certified firms have weaker labor market opportunities than those displaced from denied firms, our D-RD estimate will be understated.[3] The effects of wage insurance eligibility estimated using this D-RD approach are qualitatively similar to the RD results but larger in magnitude.

We find that wage insurance eligibility substantially increases workers' employment probabilities and cumulative earnings.[4] Wage insurance eligibility increases employment probabilities by 8 to 17 percentage points during the two years following displacement before fading to zero after four years. Program eligibility also increases earnings replacement rates by 10 percentage points and cumulative earnings by over $18,000 during the four years following displacement (not including the value of the subsidies); this is a large effect relative to the 4-year average cumulative earnings of $68,630 among marginally ineligible workers. These earnings effects are largely driven by shorter non-employment durations among wage insurance eligible workers; wage insurance eligibility reduces the initial non-employment spell duration by approximately 1 calendar quarter and reduces the total time out of employment

[3] We show (1) observable balance at the age-50 discontinuity in both samples, (2) similar observable characteristics across both samples, and (3) similar effects at age 55 (when only disability insurance eligibility varies by age) in both samples.

[4] We focus on intent-to-treat (ITT) estimates because wage insurance eligibility affects workers' incentives and therefore their search behavior irrespective of whether their new job makes them eligible for subsidy payments. As a result, eligibility for wage insurance can affect outcomes even for workers who do not receive subsidy payments, making eligibility an invalid instrument, as in Jones (2015). See Section 5 for details.

across all non-employment spells by 1.26 quarters over 4 years.

While wage insurance eligibility leads workers to return to work more quickly, we find little evidence of effects on other employment outcomes. Proponents of wage insurance hoped that it might encourage workers to leave declining industries and shift into expanding industries, with the wage insurance subsidy facilitating this transition while workers accumulate on-the-job experience and industry-specific human capital (U.S. Trade Deficit Review Commission 2000; Kletzer and Litan 2001; U.S. White House 2016). Although the majority of displaced workers in our sample switch industries, we find no difference in industry switching rates between eligible and ineligible workers. We also do not find effects on the worker's number of unique employers, geographic mobility, job quality (measured by firm age, firm size, and earnings growth rates), or the length of the employment spell at the first job after displacement. The lack of responsiveness along these forward-looking margins may reflect the fact that our regression-discontinuity approach identifies the effects of wage insurance eligibility displaced at age 50, a relatively late point in many careers.

Our findings are robust to a variety of alternative approaches and pass standard specification checks. The density of the age distribution at separation is smooth at the age-50 eligibility cutoff, which is expected given that most TAA applicants separate in mass layoffs. Observables are balanced across the age-50 eligibility threshold, including pre-displacement earnings levels and earnings growth.[5] Placebo tests using an age cutoff of 55 provide further empirical validation. The results are robust to varying the particulars of the regression discontinuity estimator including the functional form for local regression, kernel type, bandwidth, controlling for baseline covariates, and clustering standard errors by petition. We also find very similar results when addressing partial treatment of workers who turn 50 shortly after displacement using either a one-sided "donut RD" approach or a regression-kink design. Finally, the results are robust to relaxing our sample restrictions.

While the program we study is part of TAA, covered workers span a wide range of industries and locations, including the service sector, in part because the program applies to upstream and downstream firms. We find no evidence for heterogeneity in effects across locations with high vs. low unemployment rates, which suggests we are not simply finding large favorable effects of wage insurance because displaced workers in our sample are in particularly distressed labor markets. We also find no evidence for heterogeneity in effects by gender, race, or education level, suggesting that the policy is similarly effective across these groups.

[5] Observables are also balanced in the difference-in-discontinuities design comparing workers whose layoffs were certified vs. denied for TAA.

We interpret our empirical findings through the lens of a non-stationary partial equilibrium search model with endogenous search effort and duration dependence. In this model, workers receive a wage insurance subsidy if they obtain reemployment at a wage below their pre-displacement wage. Eligibility affects search behavior in two ways: wage insurance eligible workers lower their reservation wages (since the subsidy makes lower wages more attractive) and increase their search effort (since the subsidy increases the expected marginal value of obtaining a job offer). By changing job search behavior along both of these margins, wage insurance eligibility reduces non-employment durations, helping workers avoid the potentially negative effects of duration-dependent wage offers. The calibrated model delivers quantitative predictions that are consistent with our D-RD estimates. Moreover, varying parameters of UI generosity suggests that our results do not depend on the outside option offered by the TAA program. Model simulations also imply that wage insurance is likely to be particularly effective in settings with negative duration dependence.

Using our regression estimates, we calculate the marginal value of public funds (MVPF) as developed by Hendren (2016) and Hendren and Sprung-Keyser (2020). The MVPF is the ratio of willingness to pay for wage insurance benefits to net government costs, defined as program costs less savings to government budgets ("fiscal externalities"). We find the net costs to the government are negative, as fiscal externalities (such as reduced unemployment insurance payments and increased tax revenues on higher earnings) exceed wage insurance payments, even under conservative assumptions.[6] However, this calculation assumes partial equilibrium; if the program were scaled up to cover a much larger share of displaced workers, general equilibrium effects would become relevant.[7]

Our paper contributes to several literatures. Despite persistent interest in wage insurance since the 1980s, our study is the first to estimate the causal impacts of wage insurance in the U.S. labor market.[8,9] We are aware of evaluations of only two prior wage

[6]This "negative cost" result is consistent with Kostøl and Mogstad (2014) who studied a Norwegian reform that paid out disability benefits upon reemployment, and similarly found that the tax revenue levied on program-induced labor force participation outweighed fiscal costs of the reform.

[7]Davidson and Woodbury (1995) and Lise et al. (2004) use equilibrium search models to quantify various crowd-out factors, while Crépon et al. (2013) and Gravoueille (2022) use reforms and randomized trials to study labor-market level interventions in France.

[8]The earliest wage insurance proposal we have uncovered appears in Lawrence et al. (1984). Many subsequent proposals recommended a similar structure to the one we study in terms of the subsidy rate, benefit duration, and maximum benefit amount, and some would extend the policy to a much broader set of workers. Examples include Lawrence and Litan (1986), Kletzer and Litan (2001); Brainard et al. (2005); LaLonde (2007); Burtless (2007); and Litan (2015). See Wandner (2016) for a detailed history of wage insurance proposals in the U.S. and legislative history of RTAA.

[9]As part of an evaluation of the TAA Program commissioned by the U.S. Department of Labor, Schochet et al. (2012) implement a propensity score matching analysis of the effects of the Alternative Trade

insurance programs in other countries, both of which were smaller than RTAA and included important barriers to participation. Bloom et al. (2001) examined Canada's Earnings Supplement Project, which provided two years of wage insurance with a 75 percent subsidy rate to a random sample of workers in five Canadian cities. The measured effects of wage insurance were modest, but the study had a small sample size and low rates of program take-up among eligible participants, partly due to the requirement that workers find a full-time job within 26 weeks.[10] Stephan et al. (2016) use an information intervention to study the German Entgeltsicherung (EGS) program, which offered wage insurance with a subsidy rate of 50 percent in the first year and 30 percent in the second year to displaced workers age 50 or over. While the information intervention increased awareness of the program, take-up increased only slightly and the level of participation remained low, perhaps because of the requirement to apply for the program before taking up the new job. These issues led to noisy and wide-ranging estimates of the program's impact on employment outcomes.[11]

An alternative policy designed to reduce unemployment durations is the reemployment bonus, in which benefit payments are fixed and do not depend upon reemployment wages. A large literature evaluates the effects of experiments providing reemployment bonuses to workers who quickly found and maintained a job for a specified period.[12] Our estimated magnitudes compare favorably to these experiments; the cash bonuses were an order of magnitude smaller than average wage insurance payments and, correspondingly, the positive effects on employment were much smaller than our estimates. While wage insurance and reemployment bonuses both subsidize reemployment, the two programs are distinct; by increasing insurance payments when reemployment wages are lower, wage insurance amplifies the incentive to quickly find a job.[13]

More broadly, our paper contributes new evidence to the literature on active labor

Adjustment Assistance (ATAA) wage insurance program, which preceded the RTAA program we study. Yet, because they define treatment partly based upon post-displacement employment outcomes, they conclude "... we have serious doubts about the quality of the comparison group matches for the ATAA subgroup analysis."

[10] Based on a follow-up survey, Bloom et al. (2001) conclude that "...the main barrier to supplement use was an inability to find suitable new full-time work in time."

[11] A suite of closely related papers (written in German) use a difference-in-differences approach to study the EGS program (alongside other German active labor market policies) (Ammermüller et al., 2006; Brussig et al., 2006; Zwick, 2006). These papers find minimal effects of program eligibility and use surveys and interviews to highlight barriers to participation including job placement agencies' lack of engagement with programs for older workers, the requirement to receive approval prior to starting the job, and difficulty gaining approval in jobs not covered by collective bargaining contracts.

[12] Prominent examples include Woodbury and Spiegelman (1987), Corson (1989), Lise et al. (2004), and O'Leary et al. (2005). Decker and O'Leary (1995) and Meyer (1995) synthesize results across studies.

[13] See Davidson and Woodbury (1995) for an investigation of the parameters under which reemployment bonuses and wage subsidies yield similar outcomes in the context of a standard search framework.

market policies, showing that wage insurance is a promising option for supporting displaced workers. The employment effects we document are larger and more immediate than the average effects of training, job search assistance, or employer subsidy programs documented in Card et al. (2018). Our estimated effects of wage insurance are also larger than those of partial UI, which provides reduced unemployment benefits to workers in low-paying, part-time jobs (Boeri and Cahuc 2023). Partial UI is intended to encourage quick reemployment in temporary work, unlike wage insurance which is intended to lead to a new permanent position.

Finally, our study also relates to the literature on optimal targeting and tagging (Akerlof 1978; Currie and Gahvari 2008; Alcott et al. 2015; Kroft and Notowidigdo 2016; Lieber and Lockwood 2019). By construction, wage insurance targets people who experience large earnings losses. Although this subsidy structure may lead to moral hazard in which people take less-demanding, lower-paying jobs, in practice we find no evidence for lower reemployment wages. Other features of the wage insurance program we study also steer payments to displaced workers with reduced labor market opportunities. Restricting eligibility to workers over 50 was conceived as way to tag workers for whom retraining might be less effective and more socially costly (U.S. Trade Deficit Review Commission 2000). Workers eligible for wage insurance under TAA are heavily concentrated in particular locations (see Appendix Figure A.2). Since the program speeds reemployment without differential impacts on mobility, our results suggest that place-based targeting of wage insurance may be effective (Bartik 2020).

The paper proceeds as follows. Section 2 provides background on TAA and the associated wage insurance program. Section 3 develops a pedagogical model of job search with wage insurance to help build intuition. Section 4 describes the TAA petition data and LEHD, and also presents descriptive statistics. Section 5 details our identification strategy, and Section 6 presents our main results. We empirically examine mechanisms in Section 7 and perform quantitative analysis in a realistic non-stationary search model in Section 8. Section 9 evaluates benefits and costs based on the marginal value of public funds, and Section 10 concludes and discusses areas for future research.

2 Institutional Setting

This section briefly summarizes the key features of Trade Adjustment Assistance (TAA) and its wage insurance program, Reemployment Trade Adjustment Assistance (RTAA). We provide additional details in Appendix A, including citations to relevant legislation and regulations, as well as details on how wage insurance payment amounts are verified and collected.

2.1 Trade Adjustment Assistance

The U.S. Trade Adjustment Assistance (TAA) program was in place from 1962 to 2022 (with substantial amendments in 1974), providing benefits to workers "who lose their jobs or whose hours of work and wages are reduced as a result of increased imports."[14] The program was designed to compensate workers who are negatively affected by trade liberalization and to help maintain support for continued reductions in trade barriers. The central program benefits cover expenses for qualified retraining programs and provide extended unemployment insurance (UI) benefits for up to three years.[15] Training is required in order to maintain extended UI benefits. To qualify for TAA, displaced workers or their representatives must petition the Department of Labor to certify that their displacement resulted from foreign competition. TAA petitions are assigned to case investigators tasked with determining whether layoffs were linked to: (1) a direct reduction in sales from import competition; (2) a shift in production to outside the U.S.; (3) being an upstream supplier or downstream client of firms affected by (1) or (2). While investigators have subpoena power to request confidential information to inform their decision to "certify" (approve) a petition for TAA, considerable discretion is required to disentangle the firm's trade exposure from contemporaneous technology or automation shocks which may also result in separations (Hyman 2018). Since TAA's inception, 60.6 percent of petitions have been certified, with higher rates in more recent years.[16] Eligibility is determined at the plant level, so all workers displaced from a certified plant during the relevant time window are eligible for TAA benefits. As discussed below, this plant-level certification process enables us to identify TAA-eligible workers in Census Bureau data.

Aggregate spending on TAA is low relative to other social insurance programs, with less than $1 billion expended annually on training, extended UI payments, and other benefits (see Appendix A and Figure A.1). The small size of the program is due to relatively few workers receiving benefits, rather than low spending per worker. In 2021, $441 million was spent on 21,286 participants. After the Great Recession, around 200,000 workers received services in both 2010 and 2011, though aggregate annual spending still did not surpass $1 billion. Training and extended unemployment insurance benefits account for the large majority of program spending. Between 2009 and 2022, the TAA program spent a cumulative total of $9.2 billion on 341,311 displaced workers.

[14] See `https://www.dol.gov/general/topic/training/tradeact` (accessed March 27, 2023) for details.

[15] See Appendix A for a discussion of additional benefits available under TAA.

[16] See `https://www.dol.gov/agencies/eta/tradeact/data/petitions-determinations` (accessed August 23, 2023).

Various studies have examined TAA, including Magee (2003), Baicker and Rehavi (2004), Dolfin and Berk (2010), Reynolds and Palatucci (2012), Park (2012), Monarch et al. (2017), Kondo (2018), and Hyman (2018). However, neither these studies nor the widely known TAA evaluation conducted jointly by Social Policy Research Associates and Mathematica Policy Research from 2004-2011 (D'Amico and Schochet 2012) included a systematic evaluation of TAA's wage insurance program, which is our focus.[17]

2.2 Wage Insurance

The wage insurance (WI) portion of TAA provides an alternative way to compensate older TAA-eligible workers, who are less likely to retrain, while providing incentives for reemployment. The wage insurance program was introduced as a pilot in 2002, and we focus on the permanent version introduced in 2009 under the name Reemployment Trade Adjustment Assistance (RTAA) which changed eligibility criteria in ways that markedly increased take-up. Between 2009 and 2021, more than 30,000 workers received subsidy payments through RTAA, with the average recipient claiming about $5,600 in subsidies.[18]

Wage insurance benefits under RTAA are restricted to TAA-certified workers aged 50 and older at reemployment, and cover up to half of the difference between pre-displacement and post-displacement wages, so the dollar value of the benefit is larger when reemployment wages are lower. The maximum cumulative benefit amount is capped at $10,000 over a two-year period, and only workers who earn up to $50,000 (pre-tax) upon reemployment are eligible.[19]

The benefit eligibility period lasts for two years, starting with the earlier of either reemployment or the exhaustion of Unemployment Insurance (UI) payments (26 weeks in most states absent extensions during recessions). Therefore, a worker who finds reemployment relatively early has the full two years of benefit eligibility, while a worker finding reemployment after exhausting UI benefits has a shorter benefit eligibility window.[20] This rule implies that workers may be displaced prior to age 50 and receive wage insurance payments if they are 50 when reemployed, provided they have not yet exhausted the two-year benefit eligibility period. As discussed in Section 5, our analysis

[17] See footnote 9.

[18] Authors' calculation based on TAA annual reports.

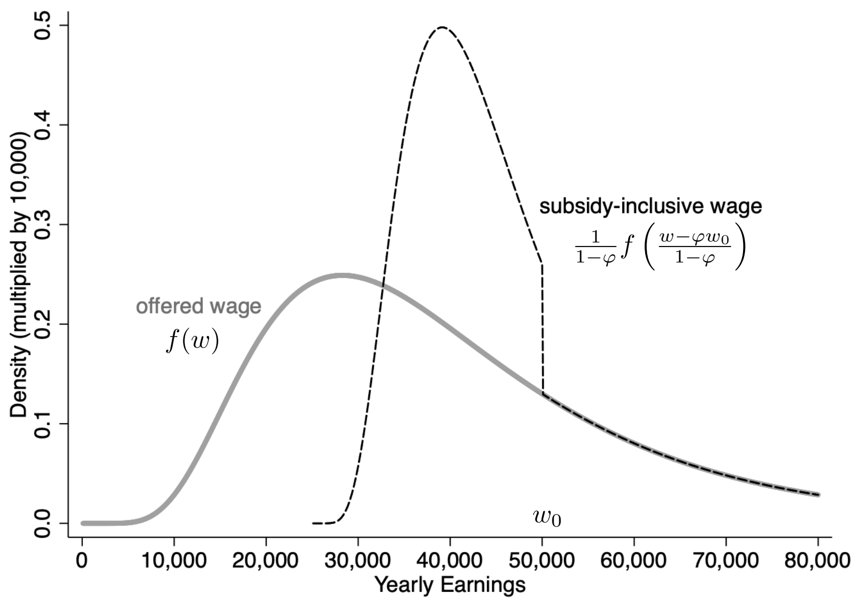
[19] The program parameters were relaxed from 2009 to 2011, increasing the maximum benefit cap to $12,000 and maximum earnings to $55,000.

[20] During the pilot phase of the program, prior to 2009, workers were required to find a new job within 26 weeks of displacement to receive wage insurance benefits. However, few workers received WI payments during this pilot phase. This was attributed to the 26-week deadline for reemployment, lack of awareness of the program at the time, and the requirement to choose either wage insurance or training and extended-UI (D'Amico and Schochet 2012).

accommodates this rule using either a one-sided "donut RD" approach or a regression-kink design (both yielding similar results).

As an example, consider a worker initially earning \$50,000 per year and who finds reemployment in a job paying \$40,000 per year. The yearly wage subsidy represents half of the gap between the old and new earnings, i.e. \$5,000 per year. Figure 1 shows how this benefit structure affects the worker's perception of potential wage offers, ignoring benefit caps for simplicity (Section 3 defines the notation in Figure 1 and derives the subsidy-inclusive wage distribution).[21]

Figure 1 – Example of Perceived Wage Offer Distribution with Wage Insurance



Notes: Illustrative yearly wage offer distribution: $f(w)$. Pre-displacement yearly earnings: $w_0 = \$50,000$. Wage insurance subsidy rate: $\varphi = 0.5$.

Assume the worker faces the wage offer distribution shown with the solid gray line. Wage insurance eligibility compresses the subsidy-inclusive wage offer distribution from below, up to the pre-displacement earnings of \$50,000, above which the worker does not receive benefits. The perceived subsidy-inclusive wage distribution is shown by the black dashed line in Figure 1. As discussed in Section 3, in the context of a standard partial-equilibrium search model, workers will lower their reservation wages and increase their search effort in response to this distortion in the perceived wage offer distribution. Both responses result in shorter average non-employment durations.

Note that Figure 1 assumes the wages offered by firms are not affected by the presence

[21] In their study of the German EGS wage insurance program, Stephan et al. (2016) conceptualize the effects of wage insurance on effective offers using a similar figure.

of the wage insurance program. As discussed in Appendix A, this assumption is justified by (1) the program's small scale relative to the population of job seekers (less than 0.3 percent of those filing new UI claims were eligible for wage insurance), (2) the fact that employers do not know which workers are eligible, and (3) that benefits are calculated and delivered to workers without employer knowledge or participation. We discuss limitations to this partial equilibrium setting in Section 10.

There is no meaningful private market for wage insurance. Two market failures likely explain this absence. First, imperfections in credit markets may prevent workers from pledging future earnings as collateral. This market failure is similar to the case of student loans, in which securitizing human capital is challenging. The second possibility is adverse selection. Workers likely have private information about their probability of unemployment, and those who expect to face unemployment would be more likely to purchase wage insurance policies than those who believe their job is safer. Private information about future job loss can explain the absence of a market for unemployment insurance that supplements government benefits (Hendren 2017).

3 Pedagogical Model

To build intuition for how wage insurance eligibility affects an unemployed worker's search behavior, this section introduces wage insurance into a stationary partial-equilibrium job search model (McCall 1970) with endogenous search effort. We characterize how wage insurance eligibility influences a worker's reservation wage, optimal search effort, and ultimately their non-employment duration. To permit a simple graphical representation of optimal search behavior, the pedagogical model in this section assumes a stationary setting in which there is no on-the-job search, employment is an absorbing state, and wage insurance payments are permanent and uncapped for eligible workers.[22] We postpone adding more realistic non-stationary features including benefit expiry and duration dependence in wage offers until Section 8, where we demonstrate that the magnitudes of our empirical estimates are consistent with standard search theory.

Setup: Time is discrete, and we consider a infinitely-lived worker who is displaced at $t = 0$ after earning w_0 in the previous job. The subsidy rate is φ, and we define the worker's

[22] In practice, 85 percent of payment recipients do not hit maximum benefit caps. In the context of more complex search models incorporating on-the-job search or job ladders, wage insurance may have additional predicted effects. For example, eligible workers may be more likely to switch industries or occupations or to take jobs with lower initial wages but faster wage growth. As discussed in Section 7, we do not find evidence for these effects.

subsidy-inclusive wage when employed at wage w as $\widetilde{w}(w)$, where

$$\widetilde{w}(w) = \begin{cases} w + \varphi(w_0 - w) & \text{if } w < w_0 \\ w & \text{if } w \geq w_0. \end{cases} \tag{1}$$

The worker is forward-looking with a discount factor β and receives a payment b in each period of non-employment. The worker optimally chooses a search intensity λ, which equals the probability of receiving a wage offer and comes at a convex cost $c(\lambda)$, where $c'(\lambda) > 0$, and $c''(\lambda) > 0$. For simplicity, there is no on-the-job search, employment is an absorbing state, and workers draw wage offers from a fixed and exogenous cumulative distribution function $F(w)$. Figure 1 shows an example in which $F(w)$ is lognormal, the subsidy rate $\varphi = 0.5$, and the pre-displacement annual wage $w_0 = \$50,000$. Because wages below w_0 are subsidized, the subsidy-inclusive wage distribution is compressed upward by a factor of 0.5 below w_0.[23]

Value of Employment: The indirect utility of employment at wage w is

$$V_t^e(w) = \widetilde{w}(w) + \beta V_{t+1}^e(w). \tag{2}$$

Since employment is an absorbing state and there is no on-the-job search, the value of employment at a given wage is deterministic and there is no expectation in the continuation value. If $w \geq w_0$, then the worker receives no subsidy and earns w in all subsequent periods. If $w < w_o$, then the worker receives the subsidized wage $w + \varphi(w_0 - w)$ in all subsequent periods. In both cases, the setting is stationary and $V_t^e(w) = V_{t+1}^e(w)$. Therefore,

$$V^e(w) = \begin{cases} \frac{w+\varphi(w_0-w)}{1-\beta} & \text{if } w < w_0 \\ \frac{w}{1-\beta} & \text{if } w \geq w_0 \end{cases} \tag{3}$$

Value of Unemployment: Since the problem is stationary, the indirect utility of unemployment V^u is equal in all time periods, as are the optimal reservation wage $\overline{w}$ and

[23] Using equation (1) it is straightforward to show that the subsidy-inclusive wage distribution is given by

$$\widetilde{f}(w) = \begin{cases} \frac{1}{1-\varphi} f\left(\frac{w-\varphi w_0}{1-\varphi}\right) & \text{if } w < w_0 \\ f(w) & \text{if } w \geq w_0, \end{cases}$$

with a jump at w_0 because the CDF of the subsidy-inclusive wage in (1) has a kink at w_0.

optimal search effort λ^*. The value of unemployment is then

$$V^u = b + \max_{\lambda} \left[-c(\lambda) + (1-\lambda)\beta V^u + \lambda\beta \int_0^\infty \max\{V^e(w), V^u\} dF(w) \right] \tag{4}$$

Optimal Search Behavior: Define λ^* as the optimal search effort and $\overline{w}$ as the reservation wage, such that $V^u = V^e(\overline{w})$. Equation (4) then implies[24]

$$(1-\beta)V^e(\overline{w}) - b + c(\lambda^*) = \lambda^*\beta \int_{\overline{w}}^\infty \left(V^e(w) - V^e(\overline{w})\right) dF(w) \tag{5}$$

which determines the reservation wage $\overline{w}$ given the optimal search effort λ^*. Again using $V^u = V^e(\overline{w})$, the first-order condition for optimal search effort in equation (4) is

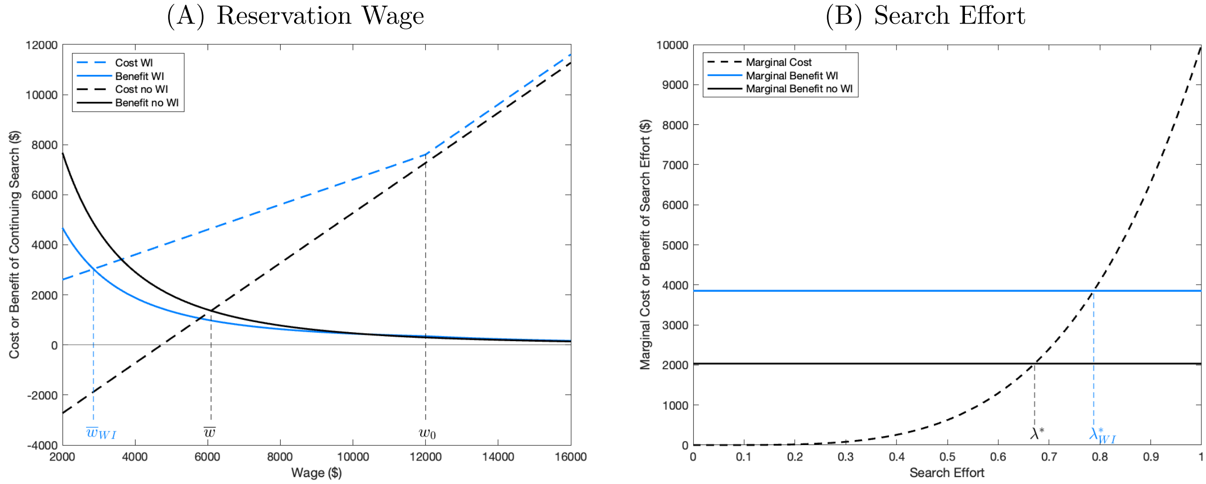
$$c'(\lambda^*) = \beta \int_{\overline{w}}^\infty (V^e(w) - V^e(\overline{w})) dF(w), \tag{6}$$

which determines the optimal search effort λ^* given the reservation wage $\overline{w}$. Equations (5) and (6) therefore simultaneously determine the optimal search effort and reservation wage.

Effect of Wage Insurance Eligibility on Search Behavior: In Appendix E, we show analytically that wage insurance eligibility reduces the reservation wage and increases optimal search effort in this model. Figure 2 presents a graphical analysis highlighting the intuition behind this result. Panel (A) plots the reservation wage condition in equation (5). The left side of equation (5) reflects the cost of turning down a wage offer $\overline{w}$ to continue searching. In the absence of wage insurance, this cost (dashed black line) is an increasing straight line, since the value of employment is proportional to the wage. The right side of equation (5) is the benefit of turning down a wage offer $\overline{w}$ to continue searching, which equals the probability of receiving an offer at the optimal search effort times the discounted value of the expected wage increase if an offer is received. This benefit is decreasing and convex in $\overline{w}$, as shown in the figure (solid black line). The intersection of these two profiles yields the worker's reservation wage.

[24] $V^u = V^e(\overline{w})$ and $dV^e(w)/dw > 0$ imply $\int_0^\infty \max\{V^e(w), V^u\}dF(w) = V^u + \int_{\overline{w}}^\infty \left(V^e(w) - V^e(\overline{w})\right) dF(w)$.

Figure 2 – Optimal Search Behavior With and Without Wage Insurance



Notes: Panel (A) plots the optimal reservation wage condition in equation (5) with wage insurance (in blue) and without (in black). Panel (B) plots the first order condition determining optimal search effort in equation (6), where marginal cost (the left side of the equation) is invariant to wage insurance. For illustration, we use a lognormal quarterly wage distribution $F(w)$ with $\mu = 6.5$ and $\sigma = 1$; a convex search cost function $c(\lambda) = \kappa \cdot \lambda^{1+\gamma}/(1+\gamma)$ with $\kappa = 10{,}000$ and γ=4; β=0.98; b=5,000; w_0=12,000; and φ=0.5.

Now consider the same profiles for a worker eligible for wage insurance, shown in blue in Figure 2 Panel (A). For wage offers above w_0 (in this example \$12,000 per quarter), the worker does not receive any subsidy payment, and the profiles are identical to those for an ineligible worker, except for the change in optimal value of λ^*. For offers below w_0, the cost of turning down a wage offer is now higher, because the worker loses the subsidy as well. Since wage insurance subsidies are larger for lower wages, the slope of the cost profile (dashed blue line) is flatter to the left of w_0. The benefit of continued search (solid blue line) falls for wage offers below w_0. Wage insurance provides larger subsidies when wages are lower, so it increases $V^e(\overline{w})$ by weakly more than it increases $V^e(w)$ when $w \geq \overline{w}$. Examining the right side of equation (5), this implies a reduction in the benefit of continued search, and that reduction is larger for lower values of $\overline{w}$. As is clear in Panel (A), by tilting the cost and benefit profiles below w_0, wage insurance eligibility lowers the reservation wage.[25]

Panel (B) shows that wage insurance also increases optimal search effort by increasing the value of receiving a wage offer, which appears on the right side of equation (6). Under the assumption of a convex search cost function, which is unaffected by wage insurance, the optimal search effort, λ^*, increases. This increase accounts for the slight differences in the cost and benefit profiles in Panel (A) for wage values above w_0.

[25] This tilting of the cost and benefit profiles distinguishes the incentives resulting from wage insurance from those of a reemployment bonus, which does not vary with the pre-displacement or reemployment wages (c.f. Woodbury and Spiegelman 1987; Meyer 1995).

This simple framework yields intuitive predictions regarding how wage insurance eligibility affects worker's search behavior and in turn their employment outcomes. Eligible workers have a lower reservation wage and exert greater search effort, both of which lead to shorter average unemployment durations. To the extent that the reservation wage is binding, eligible workers will exhibit lower reemployment wages, all else equal. However, as discussed in Section 8, in a more realistic setting where wage offers exhibit negative duration dependence, eligible workers' shorter unemployment durations may offset this expected reduction in reemployment wages (Schmieder et al., 2016; Nekoei and Weber, 2017).

4 Data

An empirical analysis of wage insurance requires that we (1) identify workers involved in a TAA-certified displacement episode, (2) observe workers' age at displacement to determine wage insurance eligibility, and (3) measure worker-level labor market outcomes in the years preceding and following displacement. We build such a database by combining administrative data from the TAA program with longitudinal matched employer-employee data from the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD). This section first provides an overview of these two data sources, with additional details provided in Appendix B, and then presents descriptive statistics.

4.1 TAA Petition and Worker Data

We begin with the universe of TAA petitions (1974-2016), acquired through Freedom of Information Act (FOIA) requests from the U.S. Department of Labor (see Hyman 2018). For each petition, this dataset reports the plant (establishment) name and address, the petition filing date, determination status (TAA certification or denial) and date, impact (separation) date, and eligibility expiry date. We use these dates to identify the set of workers laid off in the eligibility window who qualify for TAA benefits. Each petition also contains an estimate of the number of workers eligible for the program under the relevant petition, allowing us to corroborate the number of eligible workers measured in Census data.

From 1998 to 2011, the Department of Labor retained individual-level data on program participants in the Trade Adjustment Participant Report (TAPR) dataset.[26] These data include anonymized records of all individuals receiving TAA-related benefits, and indicate which individuals participated in the RTAA wage insurance program. This

[26] These data were obtained through two separate FOIA requests at the Department of Labor, which feature in Park (2012) and Reynolds and Palatucci (2012).

information allows us to calculate take-up rates for the wage insurance program and to observe the characteristics of those workers relative to the broader population of TAA participants through 2011, but is not sufficiently detailed to match with workers in the LEHD.

4.2 Census Data

We merge the TAA petition data to the U.S. Census Bureau's LEHD administrative files, which provide detailed person-level panel data tracking workers' quarterly employment status and pre-tax earnings across employers, industries, geographies, and time. The core data are compiled from employer-reported UI filings at the state level for every paid employee. While the LEHD data partnership spans all 50 states, for this project 24 states and the District of Columbia approved data access.[27] Our main sample uses quarterly earnings from 2007 to 2014 for these 25 states from the 2014 LEHD Snapshot. We also observe an indicator for UI-covered employment in any state, allowing us to observe employment status even if workers move outside the participating 25 states.[28] The LEHD files also include the worker's date of birth, gender, race, and educational attainment, along with the employing firm's age and size.[29] Together, the TAA petition and Census databases allow us to identify TAA-eligible workers just above and below the RTAA wage insurance eligibility age cutoff and to observe their labor market outcomes over a period of many years.

The vast majority of plants that petition for TAA are part of firms experiencing mass layoffs. Figure 3, Panel (A) shows that many petitioning firms close shortly after filing a TAA petition. Panel (B) documents that among surviving firms, median employment drops precipitously, with substantially larger declines among firms that are certified. A potential concern is that workers displaced from certified firms may have weaker labor market opportunities than workers displaced from denied firms; however, in the context of our D-RD research design (Section 5), this difference would bias us against finding favorable effects of wage insurance eligibility. That is, using denied workers as a counterfactual for outcomes absent certification will, if anything, understate the positive effects of wages insurance eligibility.

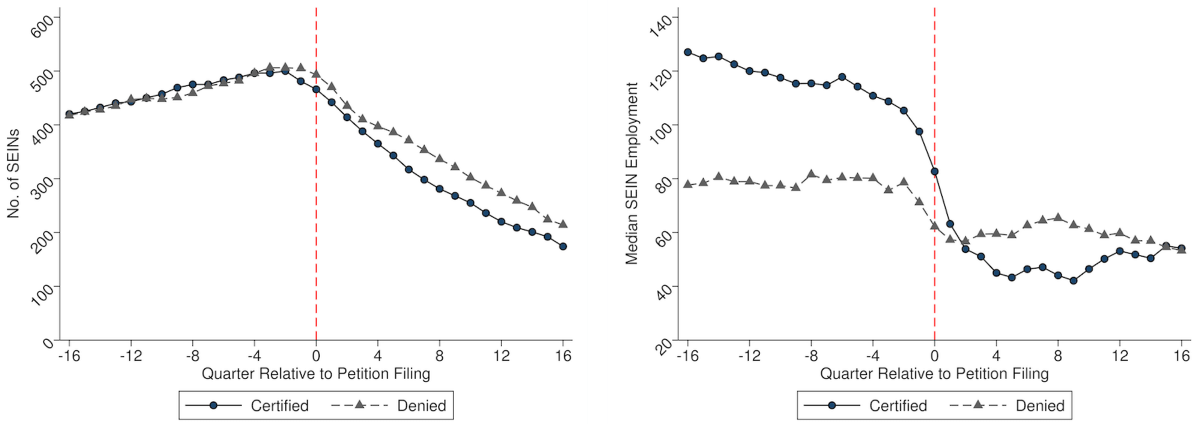
[27]These include the following states: AR, AZ, CA, CO, DC, DE, FL, IA, ID, IL, IN, KS, MD, ME, MO, MT, NM, NV, OK, OR, PA, SC, TN, WA, WV. These states account for just under half of total TAA spending and participation (see Appendix A).

[28]For more details on how we construct our earnings and employment measures, see Appendix B. Also see Abowd et al. (2009) and Vilhuber and McKinney (2009) for further details on the LEHD.

[29]Educational attainment is calculated based on Census Bureau multiple-imputation and probabilistic record linking methods when the worker is not in either the decennial Census or annual American Community Survey (ACS).

Figure 3 – Firm Exits and Employment Around TAA Petition Filing

(A) Number of Firms (B) Median Number of Employees per Firm



Notes: Panel A plots the number of firms (state employer identification numbers (SEINs)) that are active in each quarter relative to when the petition is filed, separately by petitions that are certified and those that are denied. The increasing number of firms prior to petition filing is due to firm entry. The increasing exit rate of firms after the petition filing date is not driven by firm reorganizations as reported in Census Successor-Predecessor Files. Panel B plots the median number of employees at surviving firms, which may include multi-establishment firms that lose an establishment.

4.3 Sample Selection

We consider the sample of TAA-certified workers covered by petitions that were filed on or after May 18, 2009 and who were displaced by December 31, 2013. These restrictions ensure that workers were eligible for RTAA, while also allowing us to observe earnings and employment for at least one year following separation. Because we study the effects of wage insurance *eligibility* on worker outcomes, our analysis may not identify the effects of the program if take-up is very low. Program reports and discussions with administrators raise concerns that many eligible workers were not aware of the wage insurance program, potentially explaining low take-up rates (Dolfin and Berk 2010). A null effect may therefore reflect low take-up rather than the causal effect of the policy. To address this issue, we identify types of firms in which wage insurance take-up is predicted to be relatively high and restrict attention to these firms in our main analysis. We do so by building a machine learning (ML) classifier that uses data from the TAPR, which records the number of wage insurance participants associated with each approved TAA petition in 2009-2011. Appendix D describes the procedure for identifying high-take-up petitions based on their observable characteristics. We refer to this sample, which contains about half of all certified petitions, as the "certified sample." In robustness tests, we show that our qualitative conclusions are unchanged when we relax the high take-up restriction or simply use the full sample of petitions.

We supplement this sample of TAA-certified petitions with a sample of petitions denied by the Department of Labor. Hyman (2018) shows that many rejected petitions have observably similar characteristics to certified petitions, and randomized investigator assignment plays an important role in determining certification. This sample of denied petitions (hereafter the "denied sample") helps us account for changes in other relevant programs that also occur at age 50. Most notably, eligibility requirements for disability insurance—Supplemental Security Income (SSI) and Social Security Disability Income (SSDI)—loosen at age 50 due to the occupational grids used to determine disability status (Chen and van der Klaauw 2008; Deshpande et al. 2021; Carey et al. 2022). A large body of work using various identification strategies consistently finds disability insurance reduces employment and earnings (Bound 1989; von Wachter et al. 2011; Maestas et al. 2013; French and Song 2014; Gelber et al. 2017; Low and Pistaferri 2020; Abraham and Kearney 2020). As a result of these changes in disability insurance, trade-displaced workers might experience a drop in employment at age 50 in the absence of wage insurance. The next section describes how we incorporate the denied sample using a difference-in-discontinuity design to isolate the effect of wage insurance from other programs.[30]

In both the certified and denied samples, we include workers age 22 to 60 at the date of separation to allow for at least 4 years of observed labor market outcomes before and after separation, within working age range (18 to 65). We restrict attention to those with high labor force attachment, defined as earning at least $3,000 in each quarter from 8 to 5 quarters prior to separation (targeting the $12,000 annual filing cutoff requirement used by the IRS). We impose this condition in the second year before separation to avoid endogenous sample selection from any anticipatory changes in earnings in the year before displacement. Finally, our main analysis focuses on workers with at least one full quarter of non-employment after displacement. This restriction ensures that we do not include workers who voluntarily switched employers for reasons unrelated to the trade shock, rather than being involuntarily displaced. While this definition of displaced workers follows previous literature (Jacobson et al. 1993; Couch and Placzek 2010; Sullivan and Von Wachter 2009), one concern is that excluding these workers conditions on an outcome. In robustness tests, we include workers who switched employers without a full quarter of non-employment and obtain qualitatively similar estimates, suggesting this restriction does not meaningfully change our findings.

[30] While work requirements for childless adults enrolled in the Supplemental Nutrition Assistance Program (SNAP) stop at age 50 (Gray et al. 2023), that policy was suspended for nearly all of our sample period.

4.4 Descriptive Statistics

Table 1 presents means and standard deviations of key worker characteristics and earnings prior to separation in the certified and denied samples (columns 1–4). In addition, columns 5–6 show similar statistics for a nationally representative sample of displaced workers, using the Displaced Worker Supplement (DWS) of the Current Population Survey. In this section, we highlight several comparisons between the two samples used in our analysis and the broader sample of displaced workers in the U.S.

Table 1 – Descriptive Statistics

	TAA-certified sample		TAA-denied sample		CPS DWS	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
Age at separation	45.38	[10.47]	43.38	[10.91]	39.56	[10.85]
Less Than High School	0.11	[0.32]	0.13	[0.33]	0.11	[0.31]
High School	0.39	[0.49]	0.32	[0.47]	0.29	[0.46]
Some College	0.33	[0.47]	0.33	[0.47]	0.32	[0.47]
College or higher	0.17	[0.38]	0.22	[0.42]	0.28	[0.45]
Female	0.35	[0.48]	0.39	[0.49]	0.42	[0.49]
Black	0.12	[0.32]	0.14	[0.35]	0.13	[0.33]
White	0.82	[0.38]	0.77	[0.42]	0.80	[0.40]
Other Race	0.06	[0.24]	0.09	[0.29]	0.07	[0.26]
Hispanic	0.07	[0.25]	0.12	[0.32]	0.18	[0.38]
Prior Earnings in -8Q to -5Q	45,160	[21,900]	48,620	[25,700]	40,927	[40,491]
Δ Prior Earnings from -8Q to -5Q	17.84	[3,623]	-8.05	[4,091]	-	-
Overall Tenure (years)	14.69	[4.56]	14.42	[5.15]	-	-
Prior-Firm Tenure (years)	7.24	[5.39]	6.14	[5.13]	4.86	[5.84]
Firm Age (years)	29.65	[9.89]	31.85	[8.63]	-	-
Log Firm Size	7.85	[1.88]	9.50	[2.15]	-	-

Notes: The TAA-certified and TAA-denied samples are restricted to high labor force attachment, defined as earning at least $3,000 in each quarter from 8 to 5 quarters prior to separation. Observation counts for the TAA-certified sample ($N = 28,000$) and TAA-denied-sample ($N = 48,500$) are rounded due to Census disclosure rules. $N = 76,500$ pooling both certified and denied samples. Earnings are deflated to 2010Q1. Firm age corresponds to the age of the parent firm. All variables besides prior earnings are measured at time of separation. The last two columns present statistics from the 2010, 2012, and 2014 Displaced Workers Supplement of the Current Population Survey, obtained from IPUMS-CPS (Flood et al., 2022). We include workers age 22-60 at displacement for consistency with the sample definition columns (1)–(4). Means and standard deviations are calculated using supplement-specific weights provided by the CPS. Topcode imputation for prior earnings follows Armour et al. (2016). Weekly earnings are converted to yearly equivalents assuming 50 weeks employed per year, on average.

First, the mean separation age is 45 in the certified sample and 43 in the denied sample, while the DWS sample average is 40. These averages are a bit younger than the age 50 discontinuity, suggesting the treatment effects we estimate will correspond to ages close to, but somewhat older than, the average among displaced workers. Second, workers in both of our analysis samples worked for their prior employers for approximately 6–7 years, on average, before displacement. These tenures are slightly longer than the DWS average of 5 years, and reflect both the high attachment sample restriction and the types of firms that experience trade shocks and petition for TAA. Average earnings 5 to 8 quarters prior to separation are $45,160 in the certified sample and $48,620 in the denied sample, while the DWS average is $40,927. Finally, workers in both analysis samples are more likely to be men and to have a high school degree without attending college than the average displaced worker in the U.S. Overall, workers in our sample have features that make displacement particularly disruptive.

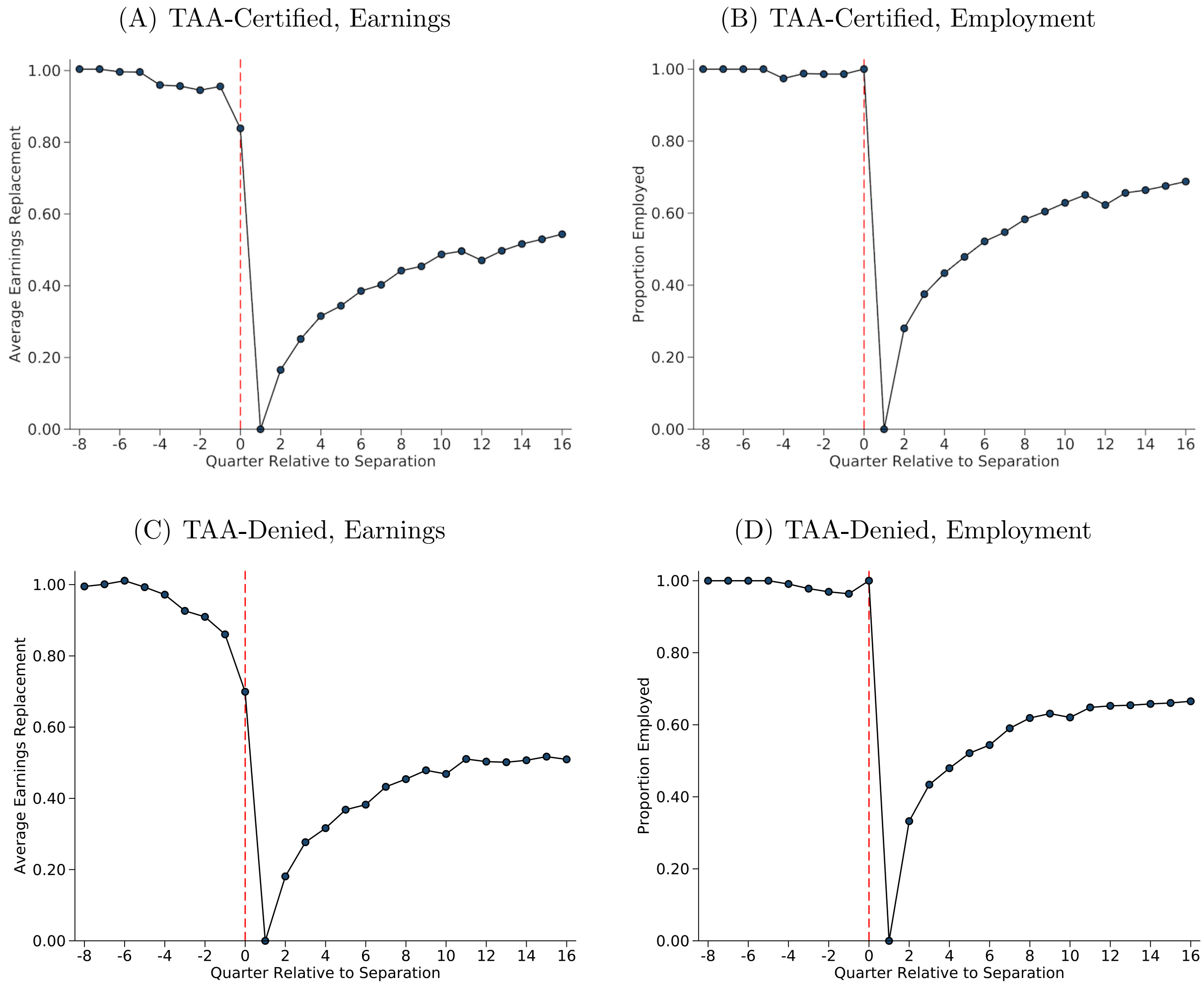
Workers in both certified and denied samples experience large and persistent declines in employment and earnings, consistent with trajectories documented in prior research (Jacobson et al. 1993, Lachowska et al. 2020, Hyman et al. 2021). Figure 4 presents descriptive event study plots of employment and earnings replacement rates for workers aged 47–53.[31] About 60 percent of workers are reemployed after four years. Workers replace slightly over half of their pre-separation earnings (excluding any subsidies) by this time, indicating that even those who become reemployed experience a decline in earnings. The magnitude of this earnings loss is sizable and suggests a potential role for wage insurance. We next describe our empirical approach to estimate the effect of wage insurance on worker outcomes.

5 Regression Discontinuity Design

To estimate the causal effect of wage insurance, we leverage the requirement that workers must be age 50 or older when reemployed to be eligible. After a TAA petition is certified by Department of Labor investigators, all of the associated workers qualify for the baseline TAA benefits of training and extended UI payments, while those aged 50 or older have the additional option of receiving wage insurance. The dates determining eligibility apply to all workers covered by the same petition, so an individual worker is unable to manipulate their displacement date to influence wage insurance eligibility. Therefore, workers who are laid off just above the age threshold should be, on average, otherwise identical to those laid off just before age 50, while only the slightly older group is immediately eligible for wage insurance. This administrative structure facilitates a regression-discontinuity (RD) design estimating

[31] Appendix Figure C.2 shows separate plots for workers aged 46-49 and aged 50-53 at displacement. Note that simple comparisons between these two age groups are not sufficient to identify the causal effect of wage insurance eligibility because employment and earnings outcomes decline with age at displacement (see Figure 5), necessitating our regression-discontinuity design.

Figure 4 – Descriptive Earnings and Employment Trajectories



Notes: Panel A plots earnings replacement rates among the sample of displaced workers aged 47–53, combining both certified and denied samples. Earnings replacement is calculated as quarterly earnings divided by the average from quarters 8 to 5 prior to displacement, inclusive of zero earnings. Panel B plots the corresponding change in employment probabilities for the same workers. Samples are restricted to high labor force attachment, defined as earning at least \$3,000 in each quarter from 8 to 5 quarters prior to separation. All displaced workers are employed in quarter 0 and not employed in quarter 1. Panels C and D present corresponding plots for the denied sample.

the intent-to-treat (ITT) effect of wage insurance on worker outcomes.

We focus on the effect of wage insurance *eligibility* rather than the effect of receiving wage insurance *payments* because the latter is not a well-defined causal parameter in our context. While it is possible to assign wage insurance eligibility, it is not possible to force workers into a job paying less than their pre-displacement job, which is a necessary condition to receive wage insurance payments. From another perspective, any attempt to estimate the effect of receiving wage insurance payments would face an exclusion restriction violation, as in Jones (2015). For example, an eligible worker may be induced to increase search effort and may find a position paying more than their pre-displacement job, in which case they do not receive subsidy payments.[32] In the context of an instrumental-variables analysis seeking to estimate the effect of receiving wage insurance payments and using eligibility as an instrument, this behavior would constitute an exclusion restriction violation. This issue would apply to any wage insurance program, not just the RTAA program we analyze. We therefore focus on identifying the ITT parameter, and show that our qualitative conclusions are robust across sample restrictions with varying predicted takeup rates.

Our preferred RD specification is a local linear model, following Gelman and Imbens (2017), with age at separation (the running variable) centered around 50.

$$y_{it} = \beta_0^t + \beta_1^t \cdot \mathbb{1}(age_i \geq 50) + \beta_2^t \cdot (age_i - 50) + \beta_3^t \cdot \mathbb{1}(age_i \geq 50) \cdot (age_i - 50) + \varepsilon_{it} \qquad (7)$$

where y_{it} is one of several labor market outcomes for individual i in quarter t, measured relative to separation. We observe each worker's precise date of birth, and the term $\mathbb{1}(age_i \geq 50)$ is an indicator for worker i being older than 50 at separation (i.e. older than 50 on the first day of the quarter in which the separated worker has moved to zero quarterly earnings). The coefficient of interest is β_1^t, which measures the jump in the regression function at the discontinuity. In order to avoid ad hoc bandwidth selection for the RDs, we follow the systematic procedure of Calonico et al. (2014) to select (potentially asymmetric) optimal bandwidths for each regression. Our main analysis does not cluster standard errors since age is measured in days, which we treat as continuous. We run a separate regression for each quarter relative to displacement $t \in \{-8, -7, ..., 15, 16\}$.

We first present RD estimates from (7) separately for the certified and denied samples. We then pool the two samples to estimate a difference in discontinuities (D-RD) via the following specification:

$$\begin{aligned} y_{it} = {} & \gamma_0^t + \gamma_1^t \cdot Cert_i + \gamma_2^t \cdot \mathbb{1}(age_i \geq 50) + \gamma_3^t \cdot Cert_i \cdot \mathbb{1}(age_i \geq 50) \\ & + \gamma_4^t \cdot (age_i - 50) + \gamma_5^t \cdot \mathbb{1}(age_i \geq 50) \cdot (age_i - 50) \\ & + \gamma_6^t \cdot Cert_i \cdot (age_i - 50) + \gamma_7^t \cdot Cert_i \cdot \mathbb{1}(age_i \geq 50) \cdot (age_i - 50) + e_{it}, \end{aligned} \qquad (8)$$

where $Cert_{it}$ is an indicator for worker i being in the certified sample. By setting $Cert_i$ equal to zero, the equation collapses to equation (7) for the denied sample. The key coefficient of interest in the D-RD specification is γ_3^t, which measures the difference in the outcome discontinuities between the certified and denied samples. The terms in the second and third

[32] This is not an uncommon occurrence; in our sample, 32 percent of reemployed workers below age 50 find employment in a job that pays more than their past job or exceeds the program salary cap.

lines of equation (8) allow for different slopes of the regression function on either side of the cutoff and for these slopes to differ between the two samples.[33]

In estimating both equations (7) and (8), our preferred specification excludes a donut of workers who turn 50 between separation and quarter t following separation. As discussed in Section 2.2, these workers are partially treated relative to a worker who is displaced at age 50, since they only become eligible for wage insurance once they turn 50 (if reemployed). Including displaced workers who cross the eligibility threshold prior to relative quarter t would otherwise attenuate any effect of wage insurance. We set the maximum donut length at 6 quarters to avoid extrapolating the regression function far away from the cutoff.[34] Prior research using RDs to evaluate eligibility rules in disability insurance (Deshpande et al. 2021) and SNAP (Gray et al. 2023) employ a similar approach to capture the fact that some individuals age into eligibility and therefore are treated only for a portion of the post-displacement period.

We consider several alternative specifications in robustness tests, including estimating a quadratic polynomial in age; employing a triangular kernel that weights observations closer to the cutoff more heavily; clustering standard errors by petition, varying the bandwidth from the IMSE-optimal bandwidth, and accounting for aging into eligibility using a regression-kink design rather than the donut approach just discussed. As described in Section 6.3, our results are robust to these choices and do not vary meaningfully from our main specification.

Identification Assumptions: The key identifying assumption of the RD is that the potential outcomes are smooth at the age-50 cutoff in the absence of the treatment.[35] We perform several checks to validate the research design. First, we test for balance in baseline covariates at the discontinuity by replacing y_{it} in equations (7) and (8) with each of our demographic controls and employment characteristics at baseline. Appendix Table C.1 and Appendix Table C.2 show outcomes are nearly always balanced in both the certified and denied samples. Appendix Table C.3 demonstrates balance in the D-RD: out of 22 covariates, one is statistically significant at the 5 percent level, as expected by chance. The magnitudes of earnings differences prior to separation are remarkably small, at less than

[33]See Grembi et al. (2016) for a formal analysis of D-RD models and Deshpande (2016), Malamud et al. (2023), and Masuda and Shigeoka (2023) for recent examples.

[34]Analysis of TAPR data indicates very few workers younger than 48.5 at separation ever take-up wage insurance. See Appendix Figure C.3.

[35]For the donut RD, the assumption is that the potential outcomes would have evolved smoothly through the excluded donut in the absence of WI eligibility. The RD estimate therefore compares the jump between the projected estimate immediately to the left of the discontinuity to the regression function immediately to the right. As robustness to using a donut, we also estimate a regression kink design that includes variation within the donut and finds nearly identical results (Appendix Figure C.5).

0.5 percent of the control mean.[36] As a summary measure of balance, we predict average earnings 5 to 8 quarters prior to separation from a regression using the full set of controls—firm tenure, log firm size, firm age, year of filing, and fixed effects for education, race, state, and 3-digit industry—and find no evidence of differences in predicted earnings at the discontinuity.

Second, we verify that the density of the age distribution is smooth at the discontinuity. Appendix Figure C.1 shows no evidence of bunching near the cutoff. In both samples, we fail to reject the null hypothesis of a continuous density at age 50, using the manipulation test for a continuous running variable from Cattaneo et al. (2018). These checks support the identifying assumptions required for the validity of the RD and D-RD research designs.

6 Results

6.1 Earnings Replacement Rates and Employment

To illustrate the variation identifying our estimates, we first present scatterplots from estimating equation (7) at 8 quarters following separation, and then subsequently show the RD estimates for all other quarters relative to separation. Figure 5 shows results for earnings replacement and employment in the certified and denied samples. Earnings replacement is defined as quarterly earnings (inclusive of zeros) divided by the same worker's average quarterly earnings in the second year before displacement (quarters -8 to -5). Wage insurance payments are excluded from all measures of earnings, as LEHD earnings are derived from payroll tax forms (ES-202) (see Appendix B). To improve visual clarity of the graphs, we collapse outcomes to 6-month age bins, but the fitted regression lines and estimates are constructed using age based on precise date of birth.

There is an estimated 7.0 percentage point increase in earnings replacement at the discontinuity for the certified sample (Panel A). This effect is large relative to the control mean of just above 40 percent (the predicted regression line immediately to the left of the discontinuity). In contrast, there is a 7.1 percentage point decrease in earnings replacement at the discontinuity for the denied sample (Panel B), consistent with the expected negative effect of relaxed eligibility requirements for disability insurance. The corresponding effects on employment are similar, with an estimated increase of 8.8 pp for the certified sample (Panel C) compared to a 7.7 pp decrease for the denied sample (Panel D).[37] These estimated

[36]As described in Appendix A, severance and bonuses are excluded from annual earnings calculations, so there is little possibility for workers expecting to be displaced to increase earnings immediately prior to separation in anticipation of receiving a higher wage insurance payment. The lack of imbalance in earnings one quarter prior to separation provides support that such "gaming" does not occur.

[37]While these disemployment effects are relatively large, Autor et al. (2014) find that trade-displaced

changes are again large relative to their respective control means.[38]

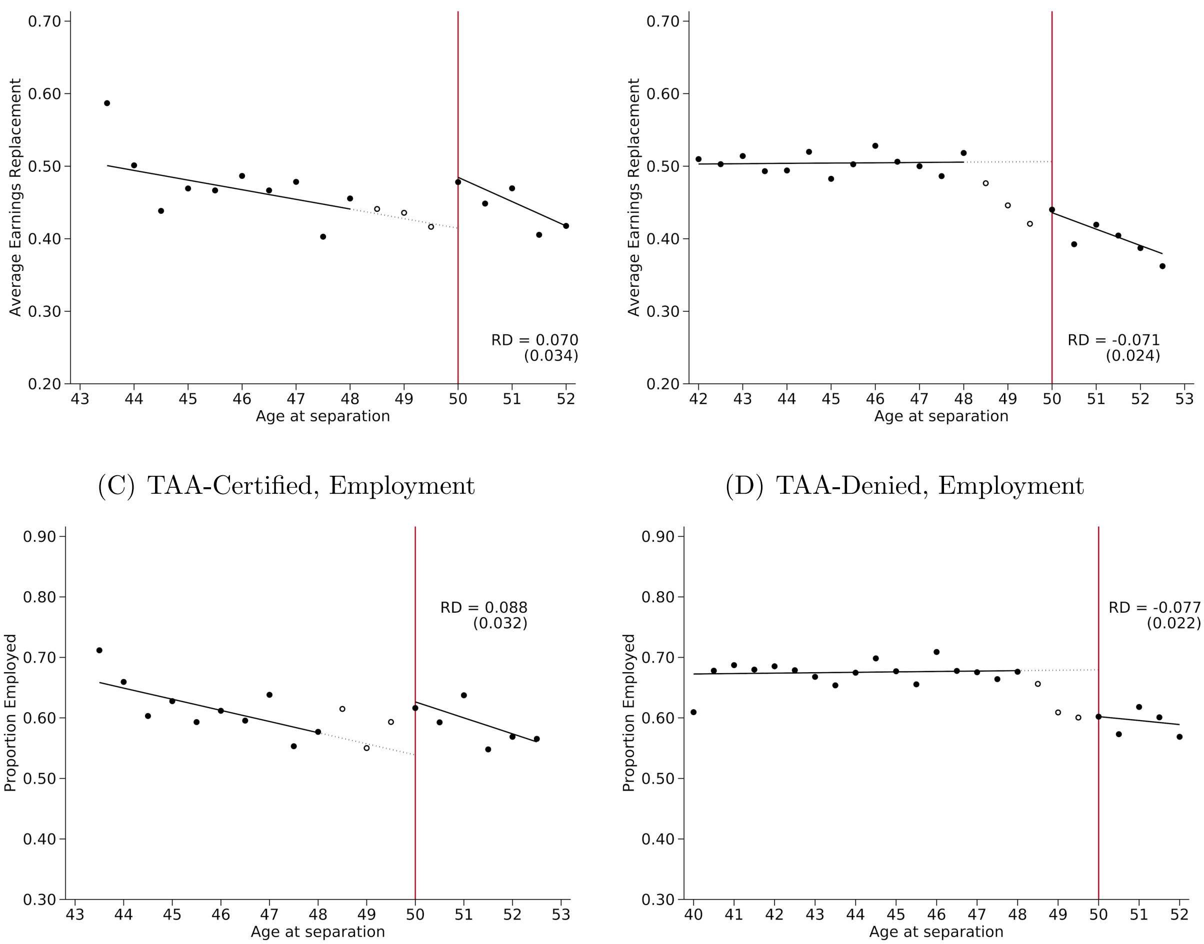
Figure 5 – RD Scatterplots, 8 Quarters since Separation

(A) TAA-Certified, Earnings Replacement

(B) TAA-Denied, Earnings Replacement

(C) TAA-Certified, Employment

(D) TAA-Denied, Employment



Notes: Panel A visually displays the RD results for earnings replacement in the certified sample at 8 quarters after separation and Panel B shows the corresponding results for the denied sample. Earnings replacement is defined as quarterly earnings (inclusive of zeros) divided by the average quarterly earnings in the second year before displacement. Panels C and D show the RD results for employment at 8 quarters after separation for the certified and denied samples, respectively. Hollow dots denote observations in the donut that are excluded from estimation. All samples are restricted to high labor force attachment in the second year before displacement, defined as 4 quarters with UI-covered earnings each exceeding $3,000. Each regression estimate uses MSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a uniform kernel to weight observations. Standard errors of the RD estimates in parentheses.

We generate RD estimates as in Figure 5 for earnings replacement and employment

workers are particularly likely to receive disability insurance.

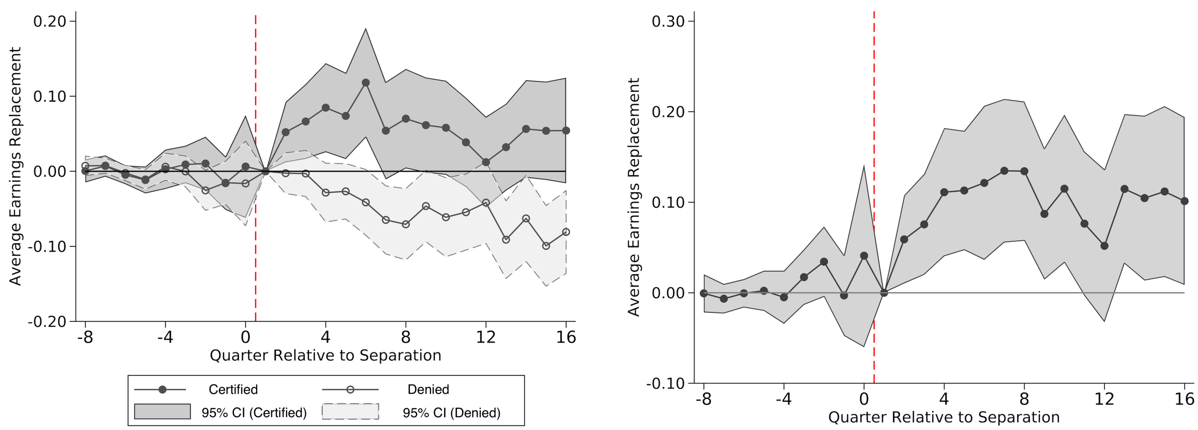
[38]In an exploratory analysis, Hyman et al. (2021) use a difference-in-differences design comparing outcomes for workers aged 50-54 at displacement to those aged 45-49, finding small effects. Figure 5 Panels (A) and (C) make clear that this approach will yield estimates that are biased downward in the certified sample because earnings and employment outcomes decline with age, masking the jump at age 50.

in each quarter relative to displacement, ranging from 8 quarters before to 16 quarters afterwards. Figure 6 plots these RD results for earnings replacement, with the estimates at quarter 8 corresponding to the RDs in Figure 5. We overlay the results from estimating equation (7) separately on the certified and denied samples in Panel (A) and present the D-RD estimates from equation (8) in Panel (B).[39]

Figure 6 – Earnings Replacement Results

(A) RD Estimates

(B) D-RD Estimates



Notes: Panel A plots RD estimates of earnings replacement rates (including zeros) for TAA-certified and TAA-denied samples from estimating equation (7) from 8 quarters pre-separation to 16 quarters post-separation. Panel B plots D-RD estimates from estimating equation (8) over the same interval. Shaded areas denote 95% confidence intervals. Samples are restricted to high labor force attachment in the second year before displacement, defined as 4 quarters with UI-covered earnings each exceeding $3,000. Each regression estimate uses MSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a uniform kernel to weight observations.

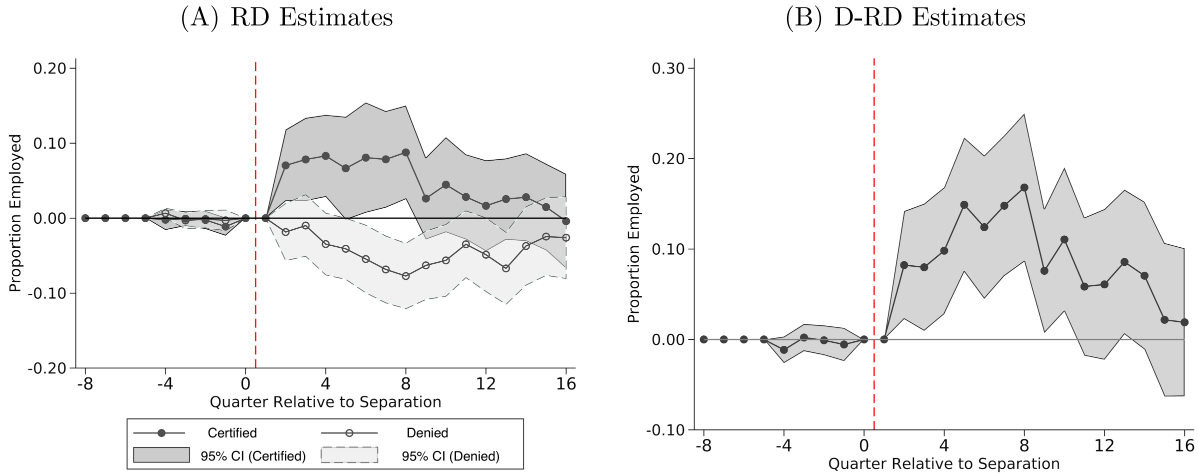
Similar to the results at 8 quarters following displacement shown in Figure 5, we find large increases in earnings replacement for much of the first three years. While the effect declines over time, it remains large and statistically significant even at the end of 4 years. Wage insurance eligibility increases earnings replacement rates by about 10 percentage points during much of the sample period.

Figure 7 presents the corresponding figures for employment. Employment increases in the certified sample during the first 8 quarters and then declines to zero after three years, after the expiry of both wage insurance eligibility and TAA training. By contrast, employment in the denied sample falls shortly after displacement and remains below zero. Taking the difference in these two discontinuities, the D-RD (Panel B) estimate is large during most

[39] These results are not driven by differences in the types of workers who become reemployed during this period. We find no evidence that the composition of reemployed workers systematically differs at the age 50 discontinuity, as measured by their predicted baseline earnings.

quarters post-separation and eventually declines to a small and statistically insignificant increase by the end of four years.

Figure 7 – Employment Results



Notes: Panel A plots RD estimates of employment for TAA-certified and TAA-denied samples from estimating equation (7) from 8 quarters pre-separation to 16 quarters post-separation. Panel B plots D-RD estimates from estimating equation (8) over the same interval. Shaded areas denote 95% confidence intervals. Samples are restricted to high labor force attachment in the second year before displacement, defined as 4 quarters with UI-covered earnings each exceeding $3,000. Each regression estimate uses MSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a uniform kernel to weight observations.

We considered a variety of dimensions of potential heterogeneity in these D-RD estimates, including by gender, race, education level, and whether the worker's county of residence was above or below the median unemployment. In each case, we found no statistically significant differences in the effects of wage insurance eligibility. In the latter case, the lack of heterogeneity suggests we are not simply finding large favorable effects of wage insurance because displaced workers in our sample are in particularly distressed labor markets.

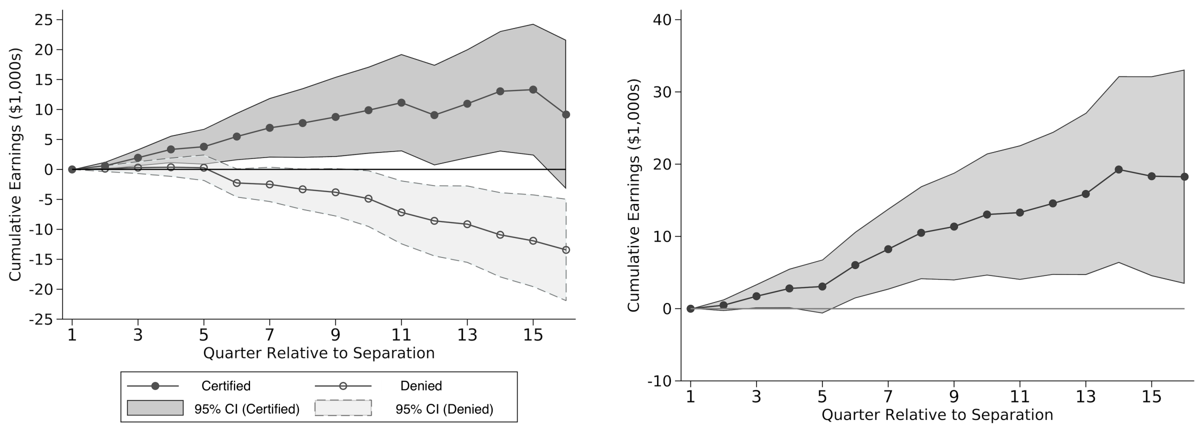
6.2 Cumulative earnings

To summarize the overall impact of wage insurance eligibility on workers' employment outcomes, Figure 8 plots RD and D-RD estimates for cumulative earnings, defined as the sum of earnings during the first t quarters following displacement (in dollars deflated to 2010Q1). Cumulative earnings effects increase steadily for the certified sample, while they fall for the denied sample starting in the fifth quarter (Panel A). The D-RD estimate—our preferred estimate of the causal effect of wage insurance—shows a steady rise in cumulative earnings that continues throughout the four years of our sample.[40] By the end of four years, wage insurance eligibility increases cumulative earnings by $18,260. This represents a 26.6 percent increase relative to the average 4-year cumulative earnings of $68,630 among ineligible workers.

Figure 8 – Cumulative Earnings Results

(A) RD Estimates

(B) D-RD Estimates



Notes: Panel A plots RD estimates of cumulative earnings for TAA-certified and TAA-denied samples from estimating equation (7) using cumulative earnings during the first t quarters following separation. Panel B plots D-RD estimates from estimating equation (8) over the same interval. Earnings have been deflated to 2010Q1 dollars. Shaded areas denote 95% confidence intervals. Samples are restricted to high labor force attachment in the second year before displacement, defined as 4 quarters with UI-covered earnings each exceeding $3,000. Each regression estimate uses MSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a uniform kernel to weight observations.

6.3 Robustness

Our main findings are robust to a range of alternative regression specifications and sample definitions.[41]

Sensitivity to Kernel, Bandwidth, and Polynomial Choice: We obtain similar results when using a second-order polynomial, a triangular kernel, including baseline covariates, or clustering standard errors by petition. The main results are also robust to using alternative bandwidths, including a smaller bandwidth that is half the size of the

[40] The plateau in relative quarter 14 is due to compositional changes in the sample owing to the censoring of long-run outcomes for workers displaced after 2011. If we run this regression on a balanced panel of workers displaced in 2009, we find sustained and monotonic increases in cumulative earnings.

[41] Due to Census disclosure limitations, we report some of the findings in the following paragraphs as qualitative descriptions that have been reviewed by Census officials rather than as an individual figure or table. Full results are available upon request.

MSE-optimal bandwidth on both sides of the cutoff or a larger bandwidth that is 50 percent wider than the MSE-optimal bandwidth. Not surprisingly, the estimates using the smaller bandwidth are less precise but still marginally significant.

Treatment of Partially Eligible Workers and Regression Kink Design: Our results do not hinge on how we treat partially-eligible workers within the small window below age 50 (i.e. within the donut). When including these partially-treated workers in predicting the regression function to the left of the discontinuity, the effects are attenuated as expected. However, the pattern of the estimates is robust and generally statistically significant at the 10 percent level or lower (Appendix Figure C.4). Moreover, the estimates are quantitatively similar to our main results when we estimate a regression kink design that includes the partially-eligible workers (Appendix Figure C.5).

Job-to-Job Transitions, Expanded Time Horizon, and Examiner Design: Our main results are also robust to relaxing each of the sample restrictions one at a time. We obtain similar results when including workers who switch to another firm without a full quarter of non-employment along with our main sample of displaced workers. The former may have voluntarily switched rather than being involuntarily displaced. The main D-RD results are also robust to using denied TAA petitions since 2002 (instead of 2009) as the denied sample. While we prefer to keep the time periods of the certified and denied petitions aligned, this analysis suggests the choice of denied petitions does not drive the D-RD results. We also instrument for certification status in our D-RD equation using the petition-level investigator leniency IV from Hyman (2018). We find that 2SLS D-RD point estimates for earnings replacement and employment are larger than our baseline OLS D-RD estimates. However, the 2SLS estimates are imprecise and not statistically different from OLS or zero.[42]

Relaxing Predicted Take-Up Restriction: The results are robust to including displaced workers from all petitions rather than only the sample of petitions with high predicted take-up (Appendix Figure C.6). In Appendix Figure C.7 we probe the sensitivity of our cumulative earnings results by varying the stringency of the high-takeup restriction. The D-RD estimates for cumulative earnings range from about $12,000 to $22,000, with our preferred estimate of $18,260 falling just above the midpoint. Results are very similar across three distinct prediction models.

[42]The second stage in examiner designs is known to carry wide standard errors (Angrist et al. 1999; Hull 2017). Given that we are restricting the data in Hyman (2018) to a subset of post-Great Recession petitions in which a larger share of workers were certified, there is little first stage variation from which to precisely estimate effects using the 2SLS design.

Falsification Test using Age 55 Disability Insurance Cutoff: We implement a falsification test to assess the assumption underlying our D-RD analysis, that the denied sample accurately captures the effects of relaxed disability insurance eligibility rules for workers in the certified sample. The eligibility criteria for disability insurance further relax at age 55, while wage insurance eligibility does not change at this age. Displaced workers covered by certified petitions have access to both wage insurance and standard TAA benefits on both sides of the age-55 cutoff, so we should not observe an increase in employment or earnings replacement at age 55 (unlike at age 50). Instead, we should see a deterioration in labor market outcomes, reflecting increased access to disability insurance. As shown in Appendix Figure C.8, we observe large and statistically significant decreases in employment and earnings replacement for the certified sample at age 55, with magnitudes similar to those in the denied sample at age 50. When estimating the D-RD at age 55, we find reductions in employment and earnings replacement that are always negative post-separation and generally not statistically different from zero. The fact that we only detect increases in earnings replacement and employment for the age 50 discontinuity in the TAA-certified sample provides further confidence that our results capture the causal effect of wage insurance eligibility.

7 Mechanisms

In this section, we investigate the mechanisms through which wage insurance eligibility increased displaced workers' subsequent earnings; recall that Figure 5 and Figure 6 show a roughly 14 percentage point increase in earnings replacement 8 quarters following displacement. Our analysis in this section suggests that wage insurance eligibility increases worker earnings primarily through increased employment probability and reduced non-employment duration. We find minimal support for other mechanisms involving job quality, worker skills, match quality, or industry switching.

We first perform a statistical decomposition of our main result. The effects on cumulative earnings in Figure 8 potentially reflect both the increased probability of employment shown in Figure 7 and an increase in earnings conditional on employment. We calculate the share of the overall effect on cumulative earnings in a given quarter driven by increased employment probability vs. increased earnings conditional on employment, such that these quarterly effects sum to the cumulative effect shown in Figure 8.[43] We find that 65.5 percent of the 4-year cumulative earnings effect is explained by an increased probability of employment, and the remaining contribution from earnings conditional on

[43] Let $earn_{it}$ be worker i's earnings in period t relative to displacement and D_i be an indicator for wage

employment emerges primarily in the fourth year following separation.[44]

In Table 2, we investigate a range of related outcomes to better understand how these differences in employment and earnings emerge between eligible and ineligible workers. In all cases, the table presents D-RD estimates with each row denoting a separate regression. The outcomes are either invariant to quarter following displacement or explicitly list the applicable quarter.

Consistent with the employment results in Figure 7, the average non-employment duration following displacement is shorter by 1 calendar quarter among eligible workers, and these workers spend 1.26 fewer quarters out of employment across all non-employment spells. These results are consistent with the theoretical prediction that eligible workers pursue employment more intensively than ineligible workers by increasing search intensity and/or lowering reservation wages. As in the long-run employment effects in Figure 7, wage insurance eligibility does not impact whether workers are more likely to ever find reemployment, at least through four years following displacement. We are able to rule out moderately-sized increases in this outcome; the point estimate is close to zero, and the upper bound of the 95-percent confidence interval rules out increases of 6.4 percentage points in the reemployment probability, equal to 7.7 percent of the control mean.

We find positive but small and statistically insignificant effects on earnings replacement and earnings levels in the first full quarter of employment after the initial displacement.[45] This result is unlikely to be confounded by compositional differences because, as just discussed, ever being reemployed is not affected by wage insurance

insurance eligibility. The effect of wage insurance eligibility on cumulative earnings can be written as follows.

$$\mathbb{E}\left[\sum_t earn_{it}|D_i = 1\right] - \mathbb{E}\left[\sum_t earn_{it}|D_i = 0\right] = \sum_t \left(\mathbb{E}\left[earn_{it}|D_i = 1\right] - \mathbb{E}\left[earn_{it}|D_i = 0\right]\right)$$

Then for each period, the effect of eligibility on earnings can be decomposed into terms capturing the effect on the probability of employment and the effect on earnings given employment. Using the Law of Total Expectation, the effect of interest can be written as:

$$\begin{aligned}
&\mathbb{E}\left[earn_{it}|D_i = 1\right] - \mathbb{E}\left[earn_{it}|D_i = 0\right] = \\
&\qquad \mathbb{E}[earn_{it}|D_i = 1, emp_{it} = 1] \times (\mathbb{P}(emp_{it} = 1|D_i = 1) - \mathbb{P}(emp_{it} = 1|D_i = 0)) \\
&\qquad +(\mathbb{E}[earn_{it}|D_i = 1, emp_{it} = 1] - \mathbb{E}[earn_{it}|D_i = 0, emp_{it} = 1]) \times \mathbb{P}(emp_{it} = 1|D_i = 0)
\end{aligned}$$

where emp_{it} is an indicator for worker i's employment status in period t. Appendix C presents additional details of this decomposition and notes how each of these terms maps to an estimate from one of the D-RDs.

[44]See Appendix Figure C.9, which estimates earnings replacement rates by period but omits observations with zero earnings (i.e. quarters in which the worker is not employed). Consistent with our main results in Figure 6 and the decomposition presented here, we find increased earnings for eligible workers who are employed in later periods following displacement.

[45]We fail to find evidence of bunching in reemployment earnings around the maximum salary for wage insurance eligibility based on density test of Cattaneo et al. (2018).

Table 2 – Mechanisms: D-RD Estimates

	Estimate	S.E.	Control Mean	N
Panel A. Employment				
Ever reemployed	0.008	0.029	0.799	76,500
Non-employment duration (quarters)	-1.002	0.381	5.939	76,500
Total quarters not employed	-1.259	0.426	7.163	76,500
Earnings replacement rate | reemployed, first full quarter	0.053	0.040	0.616	56,000
Earnings | reemployed, first full quarter ($)	338.2	628.8	7,929	56,000
Earnings | reemployed, last full quarter ($)	332.5	637.7	9,910	56,000
Panel B. Job Quality				
Employment duration of 1st job post-separation (Q)	1.043	0.435	7.195	56,000
Firm age (years) of first job post-separation	0.734	0.650	29.08	56,000
Log firm size of first job post-separation	0.336	0.308	6.684	56,000
Earnings growth rate (percentage points)	-0.195	1.798	2.937	56,000
Predicted quarterly earnings of 1st job post-separation, logs	-0.001	0.015	9.156	56,000
Predicted quarterly earnings of 1st job post-separation, ($)	13.99	103.9	11,200	56,000
Panel C. Job Laddering and Mobility				
Number of unique firms	-0.041	0.090	1.919	56,000
Switched industries (3-digit) by Q12	-0.009	0.042	0.627	56,000
Switched industries (3-digit) by Q16	-0.037	0.041	0.691	56,000
Switched county of employment by Q12	0.018	0.040	0.513	56,000
Switched county of employment by Q16	0.010	0.040	0.583	56,000

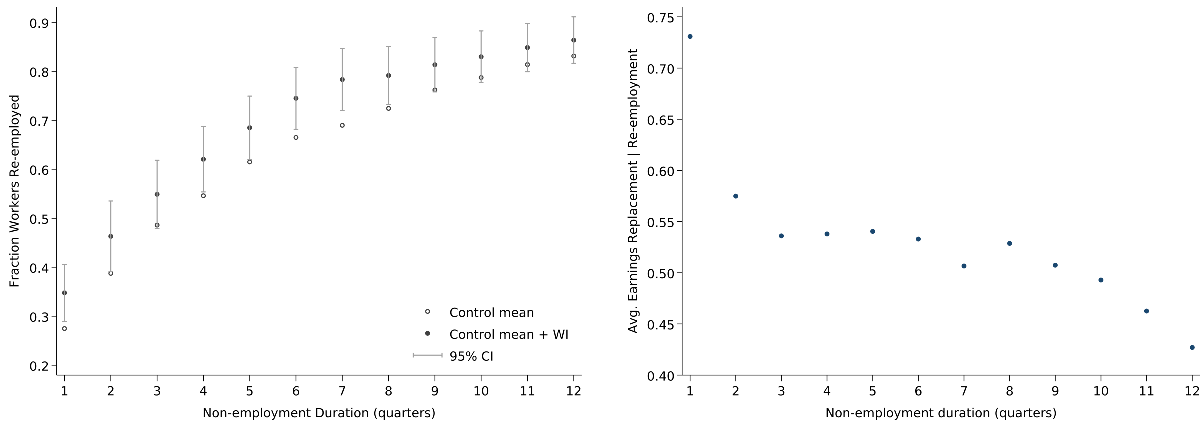
Notes: Table presents D-RD results for estimating equation (8) for different outcomes. Each row corresponds to a separate regression. The difference in discontinuities measures the jump in the regression function at age 50 for the TAA-certified sample relative to the TAA-denied sample. The Control Mean denotes the projected estimate immediately to the left of age 50 for the TAA-certified sample. Each regression uses MSE-optimal bandwidths calculated separately for each side of the 1.5 year donut for each outcome, and a uniform kernel to weight observations. Predicted earnings in Panel B are constructed by regressing average firm-level earnings against log firm age, log firm size, and 6-digit industry codes. Sample sizes for each regression vary depending on the bandwidth used. We report full sample sizes prior to bandwidth selection due to Census disclosure rules, where N=56,000 corresponds to the sample of reemployed workers.

eligibility. The lack of reduction in reemployment earnings might seem to contradict the prediction of reduced reservation wages. However, a large literature finds substantial duration dependence in reemployment wages, with workers who experience longer unemployment durations earning lower reemployment wages (Kroft et al. 2013, 2019; Schmieder et al. 2016; Nekoei and Weber 2017). Since eligible workers have substantially shorter non-employment durations on average, their reemployment wages will tend to be higher, all else equal, offsetting reductions in the reservation wage.

Figure 9 emphasizes this point by showing that not only is the *average* non-employment duration shorter for eligible workers, but the entire distribution of durations shifts left for eligible workers. Panel (A) plots results from separate D-RDs in which the dependent variable

Figure 9 – Non-Employment Durations and Duration Dependence

(A) Distribution of Non-employment Durations (B) Earnings Replacement by Non-empl. Duration



Notes: Panel (A) plots predicted values from D-RDs of unemployment durations. The dependent variable in each regression is an indicator of whether the worker had an unemployment duration of a given length or shorter, as displayed on the x-axis. We consider unemployment durations from 1 quarter (corresponding to relative quarter 2 in Figure 8) to 12 quarters (corresponding to relative quarter 13 in Figure 8). Panel (A) regressions include workers that never become reemployed within our sample period (up to four years since separation). The Control mean (hollow circles) denotes the mean of that outcome immediately to the left of age 50 for the TAA-certified sample. The Control mean + wage insurance (WI, shaded circles) adds the D-RD estimate (γ_3^t from equation (8)) and shows its 95% confidence interval. Samples are restricted to high labor force attachment, defined as earning at least \$3,000 in each quarter from 8 to 5 quarters prior to separation. Panel (B) plots mean earnings replacement rates against unemployment durations among displaced workers who become reemployed. The figure pools workers aged 45–54 at separation.

is an indicator for whether the worker had a non-employment duration of a given length or shorter. At each quarter of non-employment duration through 9 quarters, a larger fraction of workers eligible for wage insurance has found reemployment compared to those not eligible. This shift is driven by strong reemployment effects after 1 quarter of non-employment; the hazard analysis in Appendix Figure C.10 confirms this finding.

Panel (B) of Figure 9 shows evidence of strong negative duration dependence within our sample of displaced workers. The graph plots average earnings replacement rates among those who become reemployed as a function of their non-employment duration. The possibility of compositional differences by quarter of re-employment (dynamic selection) precludes us from causally interpreting the duration dependence slope in our setting. Selection concerns are partly mitigated by our focus on earnings replacement rates rather than raw earnings levels, and the observed magnitude of duration dependence is robust to controlling for education status (high school, some college, college or higher), demographics (gender, ethnicity), worker overall tenure, and firm age and size at the worker's separation firm.[46] This evidence motivates our choice to include duration dependence in our quantification exercise in Section 8, but due to remaining identification concerns regarding its magnitude, we rely on external estimates of duration dependence from Schmieder et al. (2016) and examine robustness to varying the magnitude.

Together, the panels of Figure 9 show how the reduction in non-employment duration driven by wage insurance eligibility can offset declines in reservation wages in the presence of negative duration dependence. Consider the large drop in reemployment earnings for workers with one vs. two quarters of non-employment duration in Panel (B). Since in Panel (A), wage insurance eligibility substantially increases the probability of becoming reemployed after a single quarter, wage insurance-eligible workers are more likely to find a better-paying job by virtue of their shorter durations.

When eligible for wage insurance, workers may be willing to accept lower-quality jobs, planning to exhaust the subsidy before moving to another job. We study various measures of reemployment job quality commonly used in the UI literature (e.g. Nekoei and Weber 2017) in Table 2 Panel (B), including the duration of the first post-separation job and the age and size of that employing firm. In all cases, we find small and statistically insignificant effects, suggesting that the job quality margin is not substantially affected by wage insurance eligibility. While there is an increase in the estimated employment duration of the first job, this effect is largely mechanical due to right-censoring of observed employment spells. Since eligible workers have non-employment durations that are shorter by a calendar quarter, their observed employment spells are longer by a quarter as well. Still, this finding at least provides no evidence for reductions in job quality. We also examine the number of unique firms employing the worker following displacement. Again, we find a small and statistically insignificant effect, suggesting similar job quality and similar match quality for workers with and without wage insurance eligibility.

As an alternative measure of job quality, we construct predicted earnings by regressing average quarterly earnings at the firm level against log firm age, log firm size, and fixed effects for 6-digit industry codes. These variables alone explain 45 percent of the variation in average firm-level earnings. As shown in the final two rows of Panel (B), we fail to find differences in predicted earnings measured in either logs or levels and can rule out even small decreases (or increases) in firm quality; the lower bounds of the 95-percents confidence interval rule out decreases of 3 percent in earnings when measured in logs and 1.5 percent when measured in levels.

[46] We find no evidence that the profile of downward sloping duration dependence in reemployment earnings is different between workers who displace between age 45 and 49, versus workers who displace between age 50 and 54, when controlling for a linear slope in age-at-separation on either side of the age 50 cutoff. We therefore focus on duration dependence pooling ages 45–54.

The final mechanism we consider in Table 2 relates to a stated goal of wage insurance when it was initially proposed and introduced as a demonstration project in TAA. Proponents of wage insurance hoped that it might encourage workers to leave declining industries and shift into expanding industries, with the wage insurance subsidy facilitating this transition while workers accumulate experience and human capital in the new industry (U.S. Trade Deficit Review Commission 2000; Kletzer and Litan 2001; U.S. White House 2016). In Table 2 Panel (C), we test whether workers switched industries compared to their pre-displacement job, using a change in the first 3 digits of their job's NAICS code to classify a switch. We do not find evidence in support of this mechanism in this context; all of the industry transition effects are small and statistically insignificant (although the baseline rate of switching is high). We also fail to detect evidence of switching using more aggregated (2-digit) measures of industry switching. While this set of findings suggests that wage insurance eligibility does not lead to much industry switching in the RTAA context, it is possible that similar policies targeting younger workers, who have more working years left to amortize an investment in new industry-specific skills, may respond more strongly. The last set of rows shows that wage insurance does not increase geographic mobility, as measured by obtaining employment in a different county.

The conclusions from Table 2 are robust to a variety of alternative approaches. First, we reproduce the table using an RD in the TAA-certified sample to verify that our qualitative results do not depend on differencing out the RD effects in the TAA-denied sample. We obtain qualitatively similar estimates to Table 2 for all outcomes except log(firm size); while its effect is of similar magnitude to the D-RD estimates, it is statistically significant in the TAA-certified RD. We also explore sensitivity to relaxing the sample restriction to include predicted high-takeup petitions. Our D-RD estimates remain qualitatively similar when we include all petitions; we continue to find statistical nulls for all outcomes except non-employment duration and total quarters not employed, and these point estimates are only slightly attenuated relative to Table 2.

8 Quantitative Analysis

This section shows that the large re-employment effects we estimate despite negligible effects on re-employment wages are consistent with a standard partial-equilibrium search model. While the objective of the model in Section 3 was to guide intuition about how wage insurance qualitatively influences worker behavior, in this section, we add more realistic features to facilitate quantification. The quantitative model incorporates non-stationary aspects of the RTAA program and setting, including finite program eligibility periods and duration dependence in wage offers. We calibrate the model's parameters to reflect the policy context and to target empirical moments from our D-RD results. After showing that our empirical results are consistent with model-predicted responses to wage insurance, we consider questions related to external validity and mechanisms. In the main text, we summarize the key features and results, and provide details in Appendix F.

8.1 Overview and Parametrization

The quantitative model builds upon the simple framework developed in Section 3, incorporating wage insurance into the non-stationary partial equilibrium setup in Schmieder et al. (2016). In this setting, workers optimally choose their search effort and reservation wage in each period, and these choices may evolve over the non-employment spell. This non-stationary behavior results from (1) finite eligibility periods for wage insurance and UI benefits and (2) wage offers' dependence on the non-employment duration ("duration dependence"). After program eligibility expires and the wage offer distribution becomes stationary, the worker's search behavior is constant over time. In Appendix F.1 we solve for optimal search behavior in this stationary period and then derive optimal behavior in the preceding periods by backward induction, so the worker is forward-looking. We then simulate the expected re-employment wage and non-employment duration given the optimal search behavior with and without wage-insurance eligibility.

To operationalize this approach, we assume the worker draws wage offers from an exogenous, log-normal distribution. To capture the negative duration dependence identified in the prior literature and in Figure 9, the distribution shifts left over the course of the non-employment spell. As in Schmieder et al. (2016), we assume that the evolution of the wage offer distribution is fully captured by changes in the location parameter, μ_t. The offer distribution is stationary from period T onward, such that $\mu_t = \mu_T \ \forall t \geq T$. In prior periods, μ_t falls by δ in each period, such that $\mu_{t+1} = \mu_t - \delta \ \forall t < T$ with $\delta > 0$.

When unemployed, the worker is eligible for a UI payment b_{UI} for T_{UI} quarters, after which they receive $b < b_{\text{UI}}$. Wage insurance is available to employed workers for T_{WI} quarters following displacement. For simplicity, there is no on-the-job search, employment is an absorbing state, and workers are risk-neutral. Workers face an income tax rate τ^{INC} on earnings and benefits and an additional payroll tax rate τ^{PAY} on earnings. Search costs take the form $c(\lambda_t) \equiv k\lambda_t^{1+\gamma}/(1+\gamma)$, where $\lambda \in [0, 1]$ is the probability of receiving an offer in period t, $k > 0$ is a scale parameter, and $\gamma > 0$ is a curvature parameter. Other parameters have the same interpretation as in Section 3.

Table 3 displays the parameter values we employ in our simulations. Details appear in Appendix F.3, but a few choices warrant discussion here. When possible we externally calibrate parameters based on the policy environment or applicable values from the literature. Exceptions are the wage offer distribution parameters, μ ($\equiv \mu_1$) and σ, and the level parameter k in the search cost function. We choose these parameters to target four empirical moments via minimum distance: the mean non-employment duration and reemployment wage for wage insurance-ineligible workers and the effect of wage insurance eligibility on these outcomes, all reported in Table 2. When simulating search behavior, we assume that 53 percent of workers are aware of the wage insurance program, following survey evidence on TAA-eligible workers from Dolfin and Berk (2010). The curvature of the cost function is from DellaVigna et al. (2017), aggregated to quarterly frequency (see Appendix F.3 for details). We take our baseline duration-dependence parameter $\delta^{mid} = 0.024$ from Schmieder et al. (2016), adjusted for quarterly frequency. For robustness, we also consider cases with half ($\delta^{low} = 0.012$) and double ($\delta^{high} = 0.048$) the baseline amount of duration dependence.

Table 3 – Model Parameters

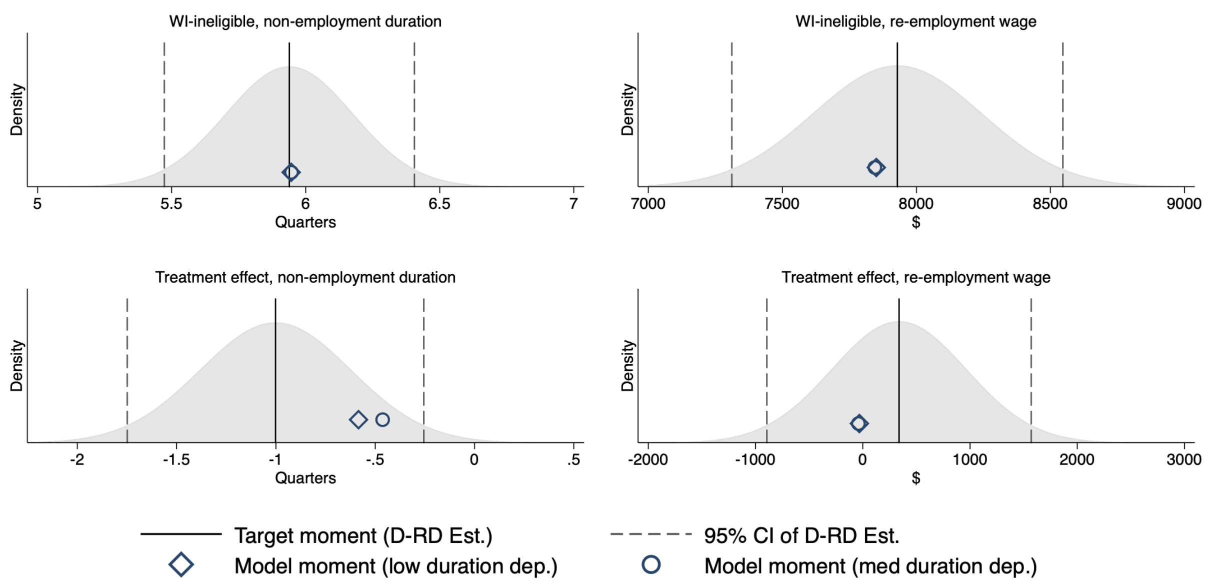
Parameter	Value	Note
Panel A. Externally-calibrated		
β, Discount factor (quarterly)	0.9879	Yearly discount factor = 0.95
γ, Cost function - curvature	3.974	DellaVigna et al. (2017)
δ^{mid}, Duration Dependence	0.024	Schmieder et al. (2016)
δ^{low}, Low Duration Dependence	0.012	$0.5 \times \delta^{mid}$
δ^{high}, High Duration Dependence	0.048	$2 \times \delta^{mid}$
w_0, Mean of prior earnings	\$11,908	Appendix Table C.1
b_{UI}, Average quarterly UI payment	\$5,031	Kovalski and Sheiner (2020)
b, Average payment after UI expiry	\$4,427	$0.88 \times b_{\text{UI}}$, Ganong and Noel (2019)
τ^{INC}, Income tax rate (federal + state)	0.193	NBER TAXSIM
τ^{PAY}, Payroll tax rate (employee share)	0.0765	NBER TAXSIM
φ, Subsidy rate of WI	0.5	Policy rule
T_{WI}, Quarters of WI eligibility	10	Policy rule
T_{UI}, Quarters of UI/TRA eligibility	10	Policy rule
T, Quarters in model	16	Sample period
Panel B. Internally-calibrated		
μ, Mean of wage offer distribution	8.736	
σ, S.D. of wage offer distribution	0.200	
k, Cost function - level	13,052	

Notes: Table presents values for the model's parameters that are externally calibrated (Panel A) and internally calibrated (Panel B). The internally-calibrated parameters are calculated via minimum distance estimation. γ is aggregated to the quarterly level from the reported parameter 0.81 from DellaVigna et al. (2017). See Appendix F for details.

8.2 Comparison of Model Results to Empirical Results

Figure 10 shows that our empirical results are consistent with the simulated outcomes from the model. For each of the four moments, the figure displays the sampling distribution (gray area) and 95 percent confidence interval (dashed vertical lines) associated with our D-RD estimates in Table 2. We show the same moments implied by the model under low and medium levels of duration dependence (denoted by diamonds and circles, respectively), omitting those for high duration dependence because they are visually indistinguishable from the medium duration dependence case (see Appendix Table F.2). In all cases, the model moments fall well within the 95-percent confidence intervals of the D-RD estimates.

Figure 10 – Comparison of Model Results to D-RD Results



Notes: Figure displays model moments relative to the target moments, which are the D-RD estimates from Table 2. Each plot corresponds to a different moment, with the D-RD estimate displayed as a vertical line, its sampling distribution shown in the shaded gray area, and the 95% confidence interval shown as dashed vertical lines. The model moments are plotted for the case of medium duration dependence corresponding to $\delta_{\mu}^{mid} = 0.024$ (circles) and low duration dependence (diamonds), corresponding to half this level.

The model fits the mean non-employment duration and reemployment earnings of wage insurance-ineligible workers extremely well. It also captures much of the effect of wage insurance on non-employment duration, with effects ranging from -0.46 to -0.58 quarters. The effect on reemployment earnings is consistently small (ranging from -$31 to -$61) and well within the confidence interval of the empirical estimate. Collectively, the quantitative predictions of the model are broadly consistent with the magnitudes of our empirical results.

8.3 Wage Insurance and Duration Dependence

In Appendix F.6, we show that the quantitative effects of wage insurance depend upon the presence of negative duration dependence in wage offers. We simulate worker search behavior and outcomes using our baseline parameters, but assuming that there is no duration dependence, i.e. $\delta = 0$. In Appendix Table F.3, we find that the effect of wage insurance eligibility on the non-employment duration is about 40 percent smaller compared to the baseline model with duration dependence. Appendix Figure F.1 clarifies the intuition behind this result by plotting the optimal search behavior over time for workers with and without wage insurance and for settings with and without duration dependence. In the presence of negative duration dependence, the expected wage draw falls over time, and therefore the value of increased search effort falls. Because wage insurance delivers larger subsidies when reemployment wages are lower, it partly counteracts the incentive to reduce search effort over time as wage offers deteriorate. This leads to larger reductions in non-employment duration, suggesting that wage insurance is likely to be particularly effective in settings with negative duration dependence.

8.4 Robustness to Outside Option

Our empirical setting raises a potential external-validity concern because the workers in our TAA-certified sample have access to very generous unemployment insurance benefits, which may amplify the effects of wage insurance eligibility on non-employment durations. To assess this concern, we simulate worker search behavior and outcomes using our baseline parameters, but assuming that unemployment insurance eligibility lasts only for the standard 26 weeks (2 quarters) rather than 10 quarters under TAA. The results, reported in Appendix Table F.4, find that reducing UI benefit duration does reduce the magnitude of wage insurance's effects on non-employment duration, but the reduction is very small: the predicted effect of wage insurance on non-employment durations is at most 6.7 percent smaller when UI generosity falls from 10 quarters to 2 quarters. The similarity of the simulated treatment effects suggests that our empirical results are not driven by the particularly generous outside option facing TAA-eligibile workers.

9 Marginal Value of Public Funds

We now evaluate the cost-effectiveness of the RTAA wage insurance program. Using our D-RD estimates, we calculate the marginal value of public funds (MVPF) as developed by Hendren (2016) and Hendren and Sprung-Keyser (2020). Partial-equilibrium calculations are applicable in our context since the program is small (see Section 2.2). If the program were more broadly available, the behavior of firms and workers would likely change, with implications for wages and non-employment durations. We also do not consider spillovers between households or across jurisdictions (Agarwal et al. 2023).

The MVPF is the ratio of the private willingness to pay for benefits (ΔW) to net government costs, defined as program costs (ΔE) less savings to government budgets (ΔC):

$$MVPF = \frac{WTP}{Net\ Govt\ Cost} = \frac{WTP}{Program\ Costs - Govt\ Savings} = \frac{\Delta W}{\Delta E - \Delta C} \tag{9}$$

The numerator of the MVPF reflects the willingness of workers to pay for wage insurance benefits. This includes both direct transfers—wage insurance payments to program participants plus changes in UI payments, valued at their monetary amounts—and any expected changes in earnings, which we value using our D-RD estimates. This linear valuation abstracts from any consumption smoothing benefits for risk averse households, which would only increase the numerator and lead to an even higher MVPF. In the denominator, we consider program costs as the sum of wage insurance payments and administrative costs. These costs may be offset by increased tax receipts on higher earnings and reduced UI payments, as well as other fiscal externalities.

The private benefits ΔW are the sum of wage insurance payments and increased earnings from Figure 8, less the change in UI payments received by workers. Each of these terms is converted into after-tax dollars. As described in Section 8, we use NBER TAXSIM to calculate a combined marginal income tax rate of $\tau^{\text{INC}} = 19.3$ percent (coming from 15 percent federal income taxes and 4.3 percent state taxes) that applies to earnings and benefits.[47] Earnings are also subject to the employee payroll tax rate $\tau^{\text{PAY}} = 7.65$ percent. We sum quarterly earnings effects through 16 quarters ($T = 16$), discounted at quarterly gross rate $1 + r$, which we set to 1.0122 to match the discount factor used in Section 8. Denoting wage insurance subsidy payments per eligible worker as s, private benefits are calculated as:

$$\Delta W = \left(s + b_{\text{UI}} \times \hat{\gamma}_3^{non-employment}\right)(1 - \tau^{\text{INC}}) + \left(z \times \sum_{t=1}^{T=16} \frac{\tilde{\gamma}_3^{t,earnings}}{(1+r)^{t-1}}\right)(1 - \tau^{\text{INC}} - \tau^{\text{PAY}}) \tag{10}$$

where $\tilde{\gamma}_3^{t,earnings}$ denotes changes in each D-RD cumulative earnings estimate ($\hat{\gamma}_3^{t,earnings}$) for each relative quarter through $T = 16$, as shown in Figure 8. The parameter z lowers the

[47] We assume a $40,000 salary for a married couple filing jointly with no dependents. The 4.3 percent state tax rate is calculated as the simple average of state tax rates for states in our sample.

valuation of labor earnings from reduced leisure, which Mas and Pallais (2019) estimate to equal 0.6 (60 percent of nominal earnings) for unemployed workers. UI payments equal the average quarterly benefit b_{UI} multiplied by the change in non-employment duration $\hat{\gamma}_3^{non-employment}$ from Table 2. We set $b_{\text{UI}} = \$5,031$ based on Kovalski and Sheiner (2020). Since our estimates correspond to ITT effects, we consider a range of assumptions about the average wage insurance payment per eligible worker s, rather than assuming a particular take-up rate and benefit amount. Because $\hat{\gamma}_3^{non-employment} < 0$, workers effectively subtract foregone unemployment insurance payments from becoming employed more quickly when valuing their willingness to pay for wage insurance.

Program costs ΔE are the sum of (1) wage insurance payments and (2) administrative costs per eligible worker. Based on estimates from D'Amico and Schochet (2012), we calculate that the administrative costs of WI are approximately \$149 per eligible worker.[48] In considering fiscal externalities ΔC, our baseline calculation conservatively only includes the amount of tax revenues collected on the increased earnings and reductions in UI benefits. Including reductions in TAA training payments, DI benefits, health insurance tax credits, and other transfers would make these savings to the government even larger. The savings to the government equal:

$$\Delta C = \tau^{\text{INC}} \times \sum_{t=1}^{T=16} \frac{\tilde{\gamma}_3^{t,earnings}}{(1+r)^{t-1}} - b_{\text{UI}} \times \hat{\gamma}_3^{non-employment} \tag{11}$$

We calculate that $\Delta C = \$10,818$ per eligible worker. Slightly over half of these savings are from reduced UI payments.

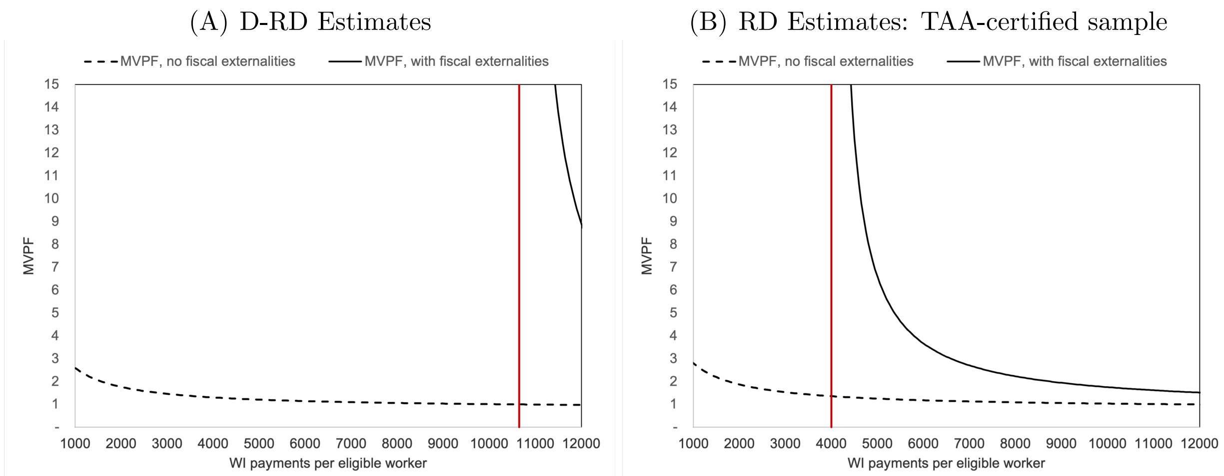
Using these formulas, Figure 11 illustrates the MVPF under a range of assumptions about effective subsidies paid per eligible worker (where s scales up both ΔW and ΔC). We find that the savings to government budgets generally exceed program costs ($\Delta E < \Delta C$), no matter the value of s. This is especially true for the most likely value of s, which is approximately \$2,970, which we calculate as mean payments among wage insurance recipients (\$5,600) times the fraction of surveyed workers aware of the program (53.1 percent according to Dolfin and Berk (2010)). This calculation of the MVPF denominator is conservative for two reasons. First, other fiscal externalities such as reduced DI benefits and other transfers would only further reduce government spending (as indirectly suggested by evidence on

[48] We calculate the administrative costs per TAA recipient as the product of three terms. First, we take the administrative costs of \$1,105 per TAA recipient in 2006 dollars and inflate to 2010Q1 dollars. Second, approximately 50 percent of all eligible workers are estimated to receive any TAA service (D'Amico and Schochet 2012). Third, at most 25 percent of 50-year olds who receive any TAA services receive wage insurance during our sample period (Appendix Figure C.3). Multiplying these yields an estimate of \$149 per eligible worker.

employment (Figure 7) and earnings (Figure 8) in the TAA-denied sample). Second, ΔC only includes tax revenue levied on cumulative earnings effects through 16 quarters (which we can identify empirically). If positive earnings effects persist beyond this period, both the numerator of the MVPF calculation and ΔC would be understated, which would understate the MVPF.

Figure 11 also illustrates the MVPF remains high when using only RD estimates from the TAA-certified sample. Under any plausible value of WI payment per eligible worker, the fiscal externality exceeds program costs and therefore produces net savings to the government (Figure 11). In this case, the MVPF's denominator is negative and the program "pays for itself."[49] In Appendix Figure C.11, we also investigate sensitivity of the MVPF to using lower bounds of the confidence intervals for our D-RD estimates instead of the point estimates. The program remains highly cost-effective.

Figure 11 – MVPF vs. Wage Insurance Payments per Eligible Worker



Notes: Figures plot the MVPFs vs. wage insurance (WI) payments per eligible worker using the point estimates for cumulative earnings and unemployment durations from the D-RD estimates (Panel A) or RD estimates of the TAA-certified sample only (Panel B). To illustrate the importance of fiscal externalities, dashed lines show the MVPFs excluding tax receipts on increased earnings and reduced UI payments and solid lines show the MVPF including fiscal externalities. For visual clarity, we truncate the MVPFs at 15 from above and indicate with red vertical lines the subsidy value at which fiscal externalities exceed program costs (i.e. where $MVPF = \infty$).

Wage insurance is thus an extremely cost-effective policy in the population of trade-displaced workers. This result stands in contrast to cost-effectiveness estimates of most other social insurance and training policies targeting adults. The range of MVPFs for unemployment insurance policies generally falls between 0.4 and 1 (Solon 1985; Katz and Meyer 1990; Chetty 2008; Landais 2015, Card et al. 2015; Kroft and Notowidigdo 2016;

[49] Hendren and Sprung-Keyser (2020) label the MVPF to be "infinite" in this situation.

Johnston and Mas 2018). Studies of adult job training also often find modest benefits relative to costs (Hollister et al. 1984; Couch 1992; Cave et al. 1993; Schochet et al. 2008; Schochet 2018). Hyman (2018) shows TAA training is cost-effective compared to other adult training programs, with an MVPF of 2.7. However, similarly to our finding for RTAA wage insurance, Kostøl and Mogstad (2014) find that allowing DI recipients to keep some benefits while working generates tax revenues that more than offset the DI payments.

10 Conclusion

The severe consequences of worker displacement, despite existing safety net policies, motivate the consideration of new social insurance programs. We analyze the effects of the wage insurance provisions of the U.S. Trade Adjustment Assistance (TAA) program using employer-employee data from the Census Bureau's LEHD dataset linked to establishment-level petitions for TAA benefits. Wage insurance eligibility increases short-run employment probabilities and leads to higher cumulative earnings in the long run. These empirical results are quantitatively consistent with a standard non-stationary partial equilibrium search model with endogenous search effort and duration dependence. Wage insurance is a highly cost-effective policy in this context; the tax receipts on increased earnings and reduced UI payments fully offset the costs of the program. The program's effectiveness primarily results from shorter non-employment spells, which allows workers to avoid the negative consequences of duration-dependent wage offers.

The labor market effects we estimate are considerably more favorable than those found in other wage insurance programs (Cahuc 2018). One reason for this difference might be that wage insurance under TAA had less stringent eligibility requirements compared to other programs, which required a 26-week reemployment deadline (Bloom et al. 2001) or program application prior to taking up a new job (Stephan et al. 2016). Neither requirement applied in our setting, and over 30 percent of wage insurance-eligible workers who become reemployed took longer than 26 weeks to do so (Figure 9).

Although the wage insurance program we study here is available only to workers affected by trade, our findings have implications for a broad set of workers. Both upstream suppliers and downstream clients of firms facing trade competition are eligible for TAA, so the program's reach extends beyond narrow industries and geographic areas, and includes the service sector. Additionally, wage insurance may be relevant for workers who lose their jobs due to automation or other competitive forces that characterize today's economy. Automation is widely seen to be an important force in affecting labor markets over the long-term (Abraham and Kearney 2020; Autor et al. 2022), with economically large impacts on wages and employment (Acemoglu and Restrepo 2020). Various wage insurance schemes have been proposed as potential alternatives or complements to the current UI program, but these proposals have been hampered by a lack of evidence on how a large-scale wage insurance program would function in the U.S. context. Our results help inform researchers and policymakers as they pursue novel ways to address the challenges faced by displaced workers in the coming years.

Future research could extend these results in a number of different directions. First, research can estimate the effects of wage insurance on other important outcomes like mortality, which has been shown to increase after job loss (Sullivan and Von Wachter 2009). Second, future work might explore the determinants of wage insurance take-up, drawing on insights from incomplete participation in other social insurance programs and means-tested benefits (Ko and Moffitt 2022). Finally, the wage insurance program we study is relatively small and its institutional features preclude the ability of firms to adjust wages in response to worker eligibility. Analyzing the general equilibrium effects of a national wage insurance policy and implications for optimal policy design would inform efforts to scale up wage insurance.

References

Abowd, John, Bryce Stephens, Lars Vilhuber, Fredrik Andersson, Kevin McKinney, and Marc Roemer, "The LEHD Infrastructure Files and the Creation of the Quarterly Workforce Indicators," in Timothy Dunne, J. Bradford Jensen, and Mark Roberts, eds., *Producer Dynamics: New Evidence from Micro Data*, University of Chicago Press, 2009, pp. 149–230.

Abraham, Katherine and Melissa Kearney, "Explaining the Decline in the US Employment-to-Population Ratio: A Review of the Evidence," *Journal of Economic Literature*, 2020, *58* (3), 585–643.

Acemoglu, Daron and Pascual Restrepo, "Robots and Jobs: Evidence from US Labor Markets," *Journal of Political Economy*, 2020, *128* (6), 2188–2244.

Agarwal, David, William Hoyt, and Tidiane Ly, "A New Approach to Evaluating the Welfare Effects of Decentralized Policies," *Working Paper*, 2023.

Akerlof, George, "The Economics of 'Tagging' as Applied to the Optimal Income Tax, Welfare Programs, and Manpower Planning," *American Economic Review*, 1978, *68* (1), 8–19.

Alcott, Hunt, Christopher Knittel, and Dmitry Taubinsky, "Tagging and Targeting of Energy Efficiency Subsidies," *American Economic Review*, 2015, *105*, 187–191.

Ammermüller, Andreas, Bernhard Boockmann, Michael Maier, and Thomas Zwick, "Eingliederungszuschüsse und Entgeltsicherung für Ältere–Analysen auf Basis natürlicher Experimente," *Vierteljahrshefte zur Wirtschaftsforschung*, 2006, *75* (3), 49–66.

Angrist, Joshua D, Guido W Imbens, and Alan B Krueger, "Jackknife Instrumental Variables Estimation," *Journal of Applied Econometrics*, 1999, *14* (1), 57–67.

Armour, Philip, Richard V. Burkhauser, and Jeff Larrimore, "Using the Pareto Distribution to Improve Estimates of Topcoded Earnings," *Economic Inquiry*, 2016, *54* (2), 1263–1273.

Autor, David, Caroline Chin, Anna M Salomons, and Bryan Seegmiller, "New frontiers: The origins and content of new work, 1940–2018," Technical Report, National Bureau of Economic Research 2022.

__ , David Dorn, and Gordon Hanson, "The China Syndrome: Local Labor Market Effects of Import Competition in the United States," *American Economic Review*, 2013, *103* (6), 2121–68.

__ , __ , and __ , "The China Shock: Learning from Labor-Market Adjustment to Changes in Trade," *Annual Review of Economics*, 2016, *8*, 205–40.

__ , __ , __ , and Kaveh Majlesi, "Importing Political Polarization? The Electoral Consequences of Rising Trade Exposure," *American Economic Review*, 2020, *110* (10), 3139–3183.

__ , __ , __ , George A Song Jae, Andrew K Rose, Janet L Yellen, Helga Hessenius, Rudiger Dornbusch, and Manuel Guitian, "Trade Adjustment: Worker-level Evidence," *Quarterly Journal of Economics*, 2014, *129* (4), 1799–1860.

Baicker, Katherine and Marit Rehavi, "Policy Watch: Trade Adjustment Assistance," *Journal of Economic Perspectives*, 2004, *18* (2), 239–255.

Bartik, Timothy J, "Using Place-based Jobs Policies to Help Distressed Communities," *Journal of Economic Perspectives*, 2020, *34* (3), 99–127.

Bloom, Howard S, Saul Schwartz, Susanna Lui-Gurr, Suk-Won Lee, Jason Peng, and Wendy Bancroft, "Testing a Financial Incentive to Promote Re-employment among Displaced Workers: The Canadian Earnings Supplement Project (ESP)," *Journal of Policy Analysis and Management*, 2001, *20* (3), 505–523.

Boeri, Tito and Pierre Cahuc, "Labor Market Insurance Policies in the Twenty-First Century," *Annual Review of Economics*, 2023, *15*, 1–22.

Bound, John, "The Health and Earnings of Rejected Disability Insurance Applicants," *American Economic Review*, 1989, *79* (3), 482–503.

Brainard, Lael, Nicholas Warren, and Robert Litan, "Insuring America's Workers in a New Era of Offshoring," 2005.

Brussig, Martin, Sarah Bernhard, Ursula Jaenichen, and Thomas Zwick, "Zielstellung, Förderstrukturen und Effekte der "Entgeltsicherung": Erfahrungen mit einem Kombilohn für ältere Arbeitnehmerinnen und Arbeitnehmer," *Zeitschrift für ArbeitsmarktForschung - Journal for Labour Market Research*, 2006, *39* (3/4), 491–504.

Burkhalter, Kyle and Chris Chaplain, "Replacement Rates for Hypothetical Retired Workers," *Social Security Administration Actuarial Note*, 2023.

Burtless, Gary, "Income Supports for Workers and Their Families," in Harry Holzer and Demetra Nightingale, eds., *Reshaping the American Workforce in A Changing Economy*, Urban Institute, 2007, pp. 239–271.

Cahuc, Pierre, "Wage Insurance, Part-Time Unemployment Insurance and Short-Time Work in the XXI Century," *IZA Discussion Paper*, 2018, (12045).

Calonico, Sebastian, Matias Cattaneo, and Rocio Titiunik, "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs," *Econometrica*, 2014, *82* (6), 2295–2326.

Card, David, Andrew Johnston, Pauline Leung, Alexandre Mas, and Zhuan Pei, "Work Incentive Effects of Taxing Unemployment Benefits," *American Economic Review*, 2015, *105*, 126–130.

__ , Jochen Kluve, and Andrea Weber, "What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations," *Journal of the European Economic Association*, 2018, *16*, 894–931.

Carey, Colleen, Nolan Miller, and David Molitor, "Why Does Disability Insurance

Enrollment Increase during Recessions? Evidence from Medicare," *NBER Working Paper 29988*, 2022.

Cattaneo, Matias, Michael Jansson, and Xinwei Ma, "Manipulation Testing Based on Density Discontinuity," *The Stata Journal*, 2018, *18* (1), 234–261.

Cave, George, Hans Bos, Fred Doolittle, and Cyril Toussaint, "Jobstart: Final Report on a Program for School Dropouts," Technical Report, Manpower Demonstration Research Corporation 1993.

Chan, Sewin and Ann Stevens, "Job Loss and Employment Patterns of Older Workers," *Journal of Labor Economics*, 2001, *19*, 484–521.

Chen, Susan and Wilbert van der Klaauw, "The Work Disincentive Effects of the Disability Insurance Program in the 1990s," *Journal of Econometrics*, 2008, *142* (2), 757–784.

Chetty, Raj, "Moral Hazard versus Liquidity and Optimal Unemployment Insurance," *Journal of Political Economy*, 2008, *116*, 173–234.

Corson, Walter, *New Jersey Unemployment Insurance Reemployment Demonstration Project: Final Evaluation Report* number 3, US Department of Labor, Employment and Training Administration, Unemployment . . . , 1989.

Couch, Kenneth, "New Evidence on the Long-Term Effects of Employment Training Programs," *Journal of Labor Economics*, 1992, *10*, 380–388.

_ **and Dana Placzek**, "Earnings Losses of Displaced Workers Revisited," *American Economic Review*, 2010, *100* (1), 572–89.

Crépon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora, "Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment," *The Quarterly Journal of Economics*, 2013, *128* (2), 531–580.

Currie, Janet and Firouz Gahvari, "Transfers in Cash and In-Kind: Theory Meets the Data," *Journal of Economic Literature*, 2008, *46*, 333–383.

D'Amico, Ronald and Peter Schochet, "The Evaluation of the Trade Adjustment Assistance Program: A Synthesis of Major Findings," Technical Report, Mathematica Policy Research 2012.

Davidson, Carl and Stephen A Woodbury, "Wage-rate Subsidies for Dislocated Workers," 1995.

Davis, Steven and Till von Wachter, "Recessions and the Costs of Job Loss," *Brookings Papers on Economic Activity*, 2011, pp. 1–72.

Decker, Paul and Christopher O'Leary, "Evaluating Pooled Evidence from the Reemployment Bonus Experiments.," *The Journal of Human Resources*, 1995, *30* (3), 534–550.

DellaVigna, Stefano, Attila Lindner, Balázs Reizer, and Johannes Schmieder, "Reference-Dependent Job Search: Evidence from Hungary," *Quarterly Journal of Economics*, 2017, *132* (4), 1969–2018.

Deshpande, Manasi, "Does Welfare Inhibit Success? The Long-Term Effects of Removing Low-Income Youth from the Disability Rolls," *American Economic Review*, 2016, *106* (11), 3300–3330.

_ **, Tal Gross, , and Yalun Su**, "Disability and Distress: The Effect of Disability Programs on Financial Outcomes," *American Economic Journal: Applied Economics*, 2021, *13* (2), 151–178.

Dolfin, Sarah and Jillian Berk, "National Evaluation of the Trade Adjustment Assistance Program: Characteristics of Workers Eligible Under the 2002 TAA Program and Their Early Program Experiences," Technical Report, Mathematica Policy Research 2010.

Flaaen, Aaron, Matthew Shapiro, and Isaac Sorkin, "Reconsidering the Consequences of Worker Displacements: Survey versus Administrative Measurements," *American Economic Journal: Macroeconomics*, 2019, *11* (2), 193–227.

Flood, Sarah, Miriam King, Renae Rodgers, Steven Ruggles, J. Robert Warren, and

Michael Westberry, "Integrated Public Use Microdata Series, Current Population Survey: Version 10.0 [dataset]," Technical Report, IPUMS 2022.

French, Eric and Jae Song, "The Effect of Disability Insurance Receipt on Labor Supply," *American Economic Journal: Economic Policy*, 2014, *6* (2), 291–337.

Ganong, Peter and Pascal Noel, "Consumer Spending during Unemployment: Positive and Normative Implications," *American Economic Review*, 2019, *109* (7), 2383–2424.

Gelber, Alex, Timothy Moore, and Alexander Strand, "The Effect of Disability Insurance Payments on Beneficiaries' Earnings," *American Economic Journal: Economic Policy*, 2017, *9* (3), 229–261.

Gelman, Andrew and Guido Imbens, "Why High-order Polynomials Should Not Be Used in Regression Discontinuity Designs," *Journal of Business & Economic Statistics*, 2017, *0* (0), 0–0.

Gravoueille, Maxime, "Wage and Employment Effects of Wage Subsidies," Technical Report, Working Paper 2022.

Gray, Colin, Adam Leive, Elena Prager, Kelsey Pukelis, and Mary Zaki, "Employed in a SNAP? The Impact of Work Requirements on Program Participation and Labor Supply," *American Economic Journal: Economic Policy*, 2023, *15* (1), 306–341.

Grembi, Veronica, Tommaso Nannicini, and Ugo Troiano, "Do Fiscal Rules Matter?," *American Economic Journal: Applied Economics*, 2016, *8* (3), 1–30.

Haughwout, Andrew, Benjamin Hyman, and Or Shachar, "The Option Value of Municipal Liquidity," 2022.

Heckman, James J, Lance J Lochner, and Petra E Todd, "Earnings Functions, Rates of Return and Treatment Effects: The Mincer Equation and Beyond," *Handbook of the Economics of Education*, 2006, *1*, 307–458.

Hendren, Nathan, "The Policy Elasticity," *Tax Policy and the Economy*, 2016, *30*, 51–89.

—, "Knowledge of Future Job Loss and Implications for Unemployment Insurance," *American Economic Review*, 2017, *107* (7), 1778–1823.

— **and Ben Sprung-Keyser**, "A Unified Welfare Analysis of Government Policies," *Quarterly Journal of Economics*, 2020, *135* (3), 1209–1318.

Hollister, Robinson, Peter Kemper, and Rebecca Maynard, "The National Supported Work Demonstration," Technical Report, Manpower Demonstration Research Corporation 1984.

Hull, Peter, "Examiner Designs and First-stage F Statistics: A Caution," *Unpublished Working Paper*, 2017, p. 5.

Hyman, Benjamin, "Can Displaced Labor Be Retrained? Evidence from Quasi-Random Assignment to Trade Adjustment Assistance," 2018.

—**, Brian Kovak, Adam Leive, and Theodore Naff**, "Wage Insurance and Labor Market Trajectories," *AEA Papers and Proceedings*, 2021, *111* (May), 491–495.

Jacobson, Louis S, Robert J LaLonde, and Daniel G Sullivan, "Earnings losses of displaced workers," *The American Economic Review*, 1993, pp. 685–709.

Johnston, Andrew and Alexandre Mas, "Potential Unemployment Insurance Duration and Labor Supply: The Individual and Market-Level Response to a Benefit Cut," *Journal of Political Economy*, 2018, *126*, 2480–2522.

Jones, Damon, "The Economics of Exclusion Restrictions in IV Models," Technical Report, National Bureau of Economic Research 2015.

Katz, Lawrence, "Wage Subsidies for the Disadvantaged," in Freeman Richard and Gottschalk Peter, eds., *Generating Jobs*, Russell Sage Foundation, 1998, pp. 21–53.

— **and Bruce Meyer**, "The Impact of the Potential Duration of Unemployment Benefits on the Duration of Unemployment," *Journal of Public Economics*, 1990, *41*, 45–72.

Kletzer, Lori, "Job Displacement," *Journal of Economic Perspectives*, 1998, *12* (1), 115–136.

— **and Robert Litan**, "A Prescription to Relieve Worker Anxiety," Technical Report, The Brookings Institution 2001.

Ko, Wonsik and Robert Moffitt, "Take-Up of Social Benefits," *NBER Working Paper 30148*, 2022.

Kondo, Illenin, "Trade-induced displacements and local labor market adjustments in the U.S," *Journal of International Economics*, 2018, *114* (C), 180–202.

Kostøl, Andreas Ravndal and Magne Mogstad, "How Financial Incentives Induce Disability Insurance Recipients to Return to Work," *American Economic Review*, 2014, *104* (2), 624–655.

Kovalski, Manuel Alcala and Louise Sheiner, "How does unemployment insurance work? And how is it changing during the coronavirus pandemic?," 2020.

Kroft, Kory and Matthew Notowidigdo, "Should Unemployment Insurance Vary with the Unemployment Rate? Theory and Evidence," *Review of Economic Studies*, 2016, *83* (3), 1092–1124.

— **, Fabian Lange, and Matthew Notowidigdo**, "Duration Dependence and Labor Market Conditions: Evidence from a Field Experiment," *Quarterly Journal of Economics*, 2013, *128* (3), 1123–1167.

— **,** — **,** — **, and Matthew Tudball**, "Long Time Out: Unemployment and Joblessness in Canada and the United States," *Journal of Labor Economics*, 2019, *37* (S2), 355–397.

Krueger, Alan and Andreas Mueller, "Job Search, Emotional Well-Being and Job Finding in a Period of Mass Unemployment: Evidence from High-Frequency Longitudinal Data," *Brookings Papers on Economic Activity*, 2011, *42* (1), 1–81.

Lachowska, Marta, Alexandre Mas, and Stephen Woodbury, "Sources of Displaced Workers' Long-Term Earnings Losses," *American Economic Review*, 2020, *110* (10), 3231–3266.

LaLonde, Robert John, *The Case for Wage Insurance*, Council on Foreign Relations New York, 2007.

Landais, Camille, "Assessing the Welfare Effects of Unemployment Benefits Using the Regression Kink Design," *American Economic Journal: Economic Policy*, 2015, *7*, 243–278.

Lawrence, Robert Z. and Robert E. Litan, *Saving Free Trade: A Pragmatic Approach*, Washington, D.C.: The Brookings Institution, 1986.

— **, Lawrence B. Krause, Robert H. Meyer, and Linda Cohen**, "Adjusting to Economic Change," in Alice M. Rivlin, ed., *Economic Choices 1984*, Washington, D.C.: The Brookings Institution, 1984, chapter 6, pp. 119–156.

Lieber, Ethan and Lee Lockwood, "Targeting with In-Kind Transfers: Evidence from Medicaid Home Care," *American Economic Review*, 2019, *109*, 1461–1485.

Lise, Jeremy, Shannon Seitz, and Jeffrey Smith, "Equilibrium Policy Experiments and the Evaluation of Social Programs," Working Paper 10283, National Bureau of Economic Research February 2004.

Litan, Robert, "Wage Insurance: A Potentially Bipartisan Way to Help the Middle Class," Technical Report, The Brookings Institution 2015.

Low, Hamish and Luigi Pistaferri, "Disability Insurance: Theoretical Trade-Offs and Empirical Evidence," *Fiscal Studies*, 2020, *41* (1), 129–164.

Maestas, Nicole, Kathleen Mullen, and Alexander Strand, "Does Disability Insurance Receipt Discourage Work? Using Examiner Assignment to Estimate Causal Effects of SSDI Receipt," *American Economic Review*, 2013, *103* (5), 1797–1829.

Magee, Christopher, "Endogenous Tariffs and Trade Adjustment Assistance," *Journal of International Economics*, 2003, *60* (1), 203–222. Emperical Exchange Rate Models.

Malamud, Ofer, Andreea Mitrut, and Cristian Pop-Eleches, "The Effect of Education on Mortality and Health: Evidence from a Schooling Expansion in Romania," *Journal of Human Resources*, 2023, *58*, 561–592.

Mas, Alexandre and Amanda Pallais, "Labor Supply and the Value of Non-Work Time: Experimental Estimates from the Field," *American Economic Review: Insights*, 2019, *1*, 111–126.

Masuda, Kazuya and Hitoshi Shigeoka, "Education and Late-Life Mortality: Evidence from a School Reform in Japan," *NBER Working Paper 31472*, 2023.

McCall, J. J., "Economics of Information and Job Search," *Quarterly Journal of Economics*, 1970, *84* (1), 113–126.

Meyer, Bruce D, "Lessons from the US Unemployment Insurance Experiments," *Journal of Economic Literature*, 1995, *33* (1), 91–131.

Monarch, Ryan, Jooyoun Park, and Jagadeesh Sivadasan, "Domestic Gains from Offshoring? Evidence from TAA-Linked U.S. Microdata," *Journal of International Economics*, 2017, *105* (C), 150–173.

Nekoei, Arash and Andrea Weber, "Does Extending Unemployment Benefits Improve Job Quality?," *American Economic Review*, 2017, *107* (2), 527–561.

O'Leary, Christopher J, Paul T Decker, and Stephen A Wandner, "Cost-effectiveness of targeted reemployment bonuses," *Journal of Human Resources*, 2005, *40* (1), 270–279.

Oreopoulos, Philip, Marianne Page, and Ann Stevens, "The Intergenerational Effects of Worker Displacement," *Journal of Labor Economics*, 2008, *26*, 455–483.

Park, Jooyoun, "Does occupational training by the trade adjustment assistance program really help reemployment? Success measured as occupation matching," *Review of International Economics*, 2012, *20* (5), 999–1016.

Pierce, Justin and Peter Schott, "The Surprisingly Swift Decline of US Manufacturing Employment," *American Economic Review*, 2016, *106* (7), 1632–1662.

_ and _ , "Trade Liberalization and Mortality: Evidence from US Counties," *American Economic Review: Insights*, March 2020, *2* (1), 47–64.

Rege, Mari, Kjetil Telle, and Mark Voturba, "Parental Job Loss and Children's School Performance," *Review of Economic Studies*, 2011, *78*, 1462–1489.

Reynolds, Kara and John Palatucci, "Does Trade Adjustment Assistance Make a Difference?," *Contemporary Economic Policy*, 2012, *30* (1), 43–59.

Ruhm, Christopher, "Are Workers Permanently Scarred by Job Displacements?," *American Economic Review*, 1991, *81*, 319–324.

Schmieder, Johannes and Till von Wachter, "Does Wage Persistence Matter for Employment Fluctuations? Evidence from Displaced Workers," *American Economic Journal: Applied Economics*, 2010, *2*, 1–21.

_ , _ , and Jörg Heining, "The Costs of Job Displacement over the Business Cycle and Its Sources: Evidence from Germany," *American Economic Review*, 2023, *113*, 1208–54.

_ , _ , and Stefan Bender, "The Effect of Unemployment Benefits and Nonemployment Durations on Wages," *American Economic Review*, March 2016, *106* (3), 739–77.

Schochet, Peter, "National Job Corps Study: 20-Year Follow-Up Study Using Tax Data," Technical Report, Mathematica Policy Research 2018.

_ , John Burghardt, and Sheena McConnell, "Does Job Corps Work? Impact Findings from the National Job Corps Study," *American Economic Review*, 2008, *98*, 1864–1886.

_ , Ronald D'Amico, Jillian Berk, Sarah Dolfin, and Nathan Wozny, "Estimated Impacts for Participants in the Trade Adjustment Assistance (TAA) Program Under the 2002

Amendments," Technical Report, Mathematica Policy Research 2012.

Solon, Gary, "Work Incentive Effects of Taxing Unemployment Benefits," *Econometrica*, 1985, *53*, 295–306.

Stephan, Gesine, Gerard van den Berg, and Pia Homrighausen, "Randomizing Information on a Targeted Wage Support Program for Older Workers: A Field Experiment," 2016.

Stevens, Ann, "Persistent Effects of Job Displacement: The Importance of Multiple Job Losses," *Journal of Labor Economics*, 1997, *15*, 165–88.

_ **and Jeremy Moulton**, "Effects of Late-Life Job Loss on Wealth and Labor Supply," in Kenneth Couch, Mary Daly, and Julie Zissimopoulos, eds., *Lifecycle Events and Their Consequences: Job Loss, Family Change, and Declines in Health*, Stanford University Press, 2013, pp. 281–315.

_ **and Jessamyn Schaller**, "Short-run Effects of Parental Job Loss on Children's Academic Achievement," *Economics of Education Review*, 2011, *30*, 289–299.

Sullivan, Daniel and Till Von Wachter, "Job displacement and Mortality: An Analysis using Administrative Data," *The Quarterly Journal of Economics*, 2009, *124* (3), 1265–1306.

U.S. Department of Labor, "Trade Act Programs," 2023.

U.S. Trade Deficit Review Commission, *The US Trade Deficit: Causes, Consequences, and Recommendations for Action*, The Commission. Washington, DC, 2000.

U.S. White House, "FACT SHEET: Improving Economic Security by Strengthening and Modernizing the Unemployment Insurance System," Technical Report, Office of Press Secretary. President Obama 2016.

Vilhuber, Lars and Kevin McKinney, "LEHD Infrastructure Files in the Census RDC-Overview," Technical Report, U.S. Census Bureau 2009.

von Wachter, Till, Jae Song, and Joyce Manchester, "Trends in Employment and Earnings of Allowed and Rejected Applicants to the Social Security Disability Insurance Program," *American Economic Review*, 2011, *101* (7), 3308–3329.

Wandner, Stephen A., "Wage Insurance as a Policy Option in the United States," *Upjohn Institute Working paper*, 2016, (16-250).

Woodbury, Stephen A and Robert G Spiegelman, "Bonuses to Workers and Employers to Reduce Unemployment: Randomized Trials in Illinois," *The American Economic Review*, 1987, pp. 513–530.

Zwick, Thomas, *Evaluation der Maßnahmen zur Umsetzung der Vorschläge der Hartz-Kommission: Arbeitspaket 1, Wirksamkeit der Instrumente; Modul 1d, Eingliederungszuschüsse und Entgeltsicherung ; Endbericht durch den Forschungsverbund*, Vol. F356d of *Forschungsbericht / Bundesministerium für Arbeit und Soziales*, Mannheim u.a.: Zentrum für Europäische Wirtschaftsforschung (ZEW) GmbH, 2006.

Online Appendices [Not for Publication]

A Institutional Details of Trade Adjustment Assistance and Wage Insurance

A.1 Trade Adjustment Assistance

The Wage Insurance program that we study is part of the broader federal Trade Adjustment Assistance (TAA) program, which provides assistance to workers adversely affected by international trade.[50] Specifically, the program serves "workers who lose their jobs or whose hours of work and wages are reduced as a result of increased imports" (U.S. Department of Labor, 2023). The program's main benefits are funding for up to three years of approved job training and extended unemployment insurance (UI) payments provided to workers during training. TAA-eligible workers may also receive modest reimbursements for job-search and relocation expenses and are eligible for the Health Coverage Tax Credit.

To be eligible for TAA benefits, a worker must be part of a group of adversely affected workers that has successfully petitioned the Department of Labor for TAA certification. From 2009 onward, eligible workers may have produced goods or services prior to displacement.[51] A TAA petition may be filed by the workers themselves, their firm, or their union or other representative. U.S. Department of Labor investigators are tasked with determining whether applicants were laid off by companies whose decline in sales was due to increased imports or outsourcing, and have subpoena power to request confidential information from any given firm or plant. The investigator seeks to verify the petition eligibility criteria, mainly a substantial decline in employment and a decline in sales coincident with increased imports or offshoring or production (19 U.S.C. §2272).[52]

Once investigators certify a petition associated with a given plant, all workers displaced from that plant within the year preceding and two years following the petition filing date may qualify for TAA, irrespective of who filed the associated petition (19 U.S.C. §2273,2291). In addition to being part of a certified displacement, in order to receive TAA benefits, a worker must have had at least 26 weeks of employment at $30 or more per week during the year preceding displacement and sufficient prior earnings or employment to qualify for UI benefits under state regulations (19 U.S.C. §2291).

Upon a petition's approval, notice is published in local newspapers along with a description of potential benefits, and likely-eligible workers receive written notice through their state's Department of Labor (or other cooperating state agency).[53] In addition, workers receive advance notification of plant closings and mass layoffs under the Worker Adjustment and Retraining Notification (WARN) Act, which in most states triggers an information session with State officials who provide details on available benefits (e.g. in Pennsylvania, this is known as a "Benefits Rights Interview", and includes information on RTAA).

[50] See Hyman (2018) for additional detail on the main provisions of TAA.

[51] Training and Employment Guidance Letter No. 22-08, May 15, 2009.

[52] A substantial decline in employment is defined as "the lesser of 50 workers or 5 percent of the workers within a firm" or 2 or more workers for firms with 50 or fewer workers (20 CFR 618.110). However, as shown in Figure 3, the vast majority of firms with certified layoffs experience much larger contractions or shut down entirely.

[53] See 19 U.S.C. §2275 and Training and Employment Guidance Letter No. 22-08, May 15, 2009, p.A-31.

A.2 Wage Insurance

Beginning in 2002, the broader Trade Adjustment Assistance program included a pilot wage insurance program known as "Alternative Trade Adjustment Assistance." Our analysis focuses on the permanent version, known as "Reemployment Trade Adjustment Assistance" (RTAA), which went into effect in 2009. When an eligible worker finds reemployment at a wage below their pre-displacement wage, they receive a subsidy covering up to 50 percent of the gap between their old and new wages for up to two years.

To be eligible for wage insurance subsidy payments a worker must be eligible for the broader TAA program, must find work at a different firm earning a lower wage than in their pre-displacement job, and, critically for our research design, must be age 50 or over at reemployment (19 U.S.C. §2318). This structure means that younger TAA-certified displaced workers are eligible for standard TAA benefits, while older TAA-certified workers have access to both standard TAA benefits and wage insurance. The wage insurance eligibility period lasts two years, starting from the earlier of i) the date of reemployment or ii) the exhaustion of state-funded UI benefits (26 weeks in most states, in the absence of federal extensions for periods of high unemployment). This rule implies that, for example, if an unemployed worker exhausts 26 weeks of state-funded UI and remains unemployed for an additional 6 months before finding a job paying less than their pre-displacement job, they can receive only up to 1.5 years of wage insurance payments. Total subsidy payments per worker were also capped at $12,000 in 2009-2011 and $10,000 thereafter, and workers were ineligible to receive subsidy payments if their yearly earnings at reemployment exceeded $55,000 in 2009-2011 and $50,000 thereafter. In practice, the average wage insurance payment was about $5,600 per recipient.

Determining Subsidy Amounts

The subsidy amount is defined as 50% of the difference between annualized pre-tax wages prior to separation and annualized reemployment wages.[54] Annualized pre-displacement wages are calculated as the product of the hourly wage rate in the last full week of employment, the number of hours worked in that week, and 52. This calculation omits overtime wages and hours, bonuses, and severance payments, which limits workers' ability to distort pre-displacement earnings in an effort to increase the wage insurance payment. Annualized post-displacement wages are calculated similarly, but for the first full week of employment in the new job. Recalls at pre-displacement employers are precluded from wage insurance.[55] Subsidy payments are made on a weekly, biweekly, or monthly basis, and the responsible state agency reviews the worker's wages on a monthly basis to adjust the subsidy amount and ensure that the worker remains eligible given the benefit and yearly earnings caps mentioned above.

Weekly earnings are generally calculated based on pay stubs submitted by the worker to the responsible state agency and verified using administrative earnings records, rather

[54] The information in this paragraph is from Section H.7 of Training and Employment Guidance Letter No. 22-08, May 15, 2009 unless otherwise noted.

[55] TEGL 05-15 Attachment A, section H.3.3, p. A-72: "To determine that a worker is eligible for RTAA, the CSA must make a finding that the employment obtained by the worker is not at the "firm" from which the worker was separated, that is, the "firm" identified in the certification."

than through communication with the employer. Subsidy payments are made directly to the worker, generally through direct deposit into a personal bank account. In fact, based on conversations with officials in state workforce departments, employers generally do not know if one of their workers is receiving wage insurance payments.[56] This limits the employer's ability to capture the subsidy, particularly given the small size of the program.

Workers may receive wage insurance when employed part-time, at least 20 hours per week, if they are simultaneously enrolled in a TAA-approved training program. In this case, subsidy payments are rescaled to reflect the number of hours employed.[57] A person who becomes self-employed after displacement may receive wage insurance, in which case the responsible state agency calculates an approximate hourly wage in self-employment to determine the subsidy amount.[58]

Because wage-insurance eligible workers are also eligible for standard TAA benefits, the program includes various rules regarding how the programs interact. Once a worker receives their first wage insurance payment, they are no longer eligible for extended unemployment insurance payments under standard TAA.[59] In contrast, a worker may receive wage insurance after receiving extended UI payments under TAA, but with their wage-insurance benefit period reduced in proportion to the amount of extended UI payments received.[60] As mentioned in the prior paragraph, part-time workers may receive wage insurance benefits when simultaneously enrolled in TAA-approved training.

A.3 Program Generosity

The RTAA wage insurance program covers a very small share of unemployed workers in the U.S. TAA Annual Reports provide the estimated number of workers covered by approved petitions in each fiscal year. Starting in 2013, the reports additionally provide the median age of program participants, which is age 50 or above in all years. Therefore, 0.5 times the number of petition-covered workers is an upper bound on the number of newly RTAA-eligible workers in that fiscal year. We compare this estimate to the number of new UI claims in each fiscal year using weekly claims data provided by the U.S. Department of Labor. This comparison implies that, during our sample period, less than 0.3 percent of those filing new UI claims were eligible for wage insurance.

In Appendix Figure A.1, we further plot fiscal year expenditures for the various sub-components of TAA. These include training and extended UI components of TAA, as well as total spending on wage insurance (RTAA). We show these expenditures nationally, as well as separately for the LEHD-covered set of 24 states and the District of Columbia. In Appendix Figure A.2 below, we map the petition-estimated number of workers applying for TAA at a finer geography level (commuting zones) for our analysis sample of the LEHD states from 2009 to 2014, to demonstrate our geographic coverage.

[56] This structure contrasts with wage subsidies provided directly to the employer. See Katz (1998).

[57] Training and Employment Guidance Letter No. 22-08, May 15, 2009, Section H.7 provides the relevant formula.

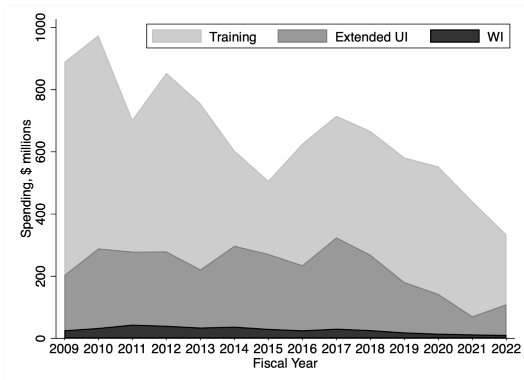
[58] Training and Employment Guidance Letter No. 02-03, August 6, 2003, FAQ numbers 14 and 15.

[59] Training and Employment Guidance Letter No. 22-08, May 15, 2009, p.A-63.

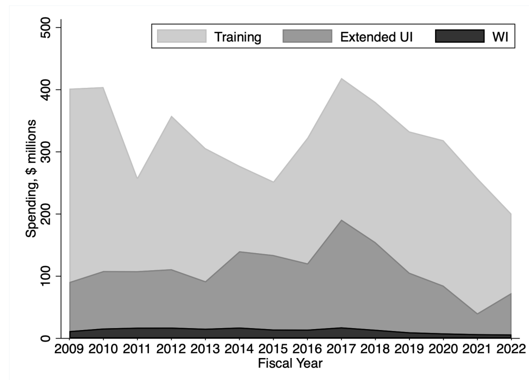
[60] See Training and Employment Guidance Letter No. 22-08, May 15, 2009, p.A-57 and the updated version in Training and Employment Guidance Letter No. 22-08 Change 1, November 20, 2009, which corrects a typo in the benefits formula.

Figure A.1 – Trends in TAA spending and participation, FY2009-FY2022

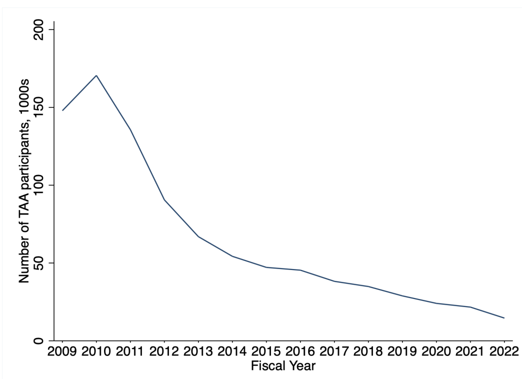
(C) Total spending - US



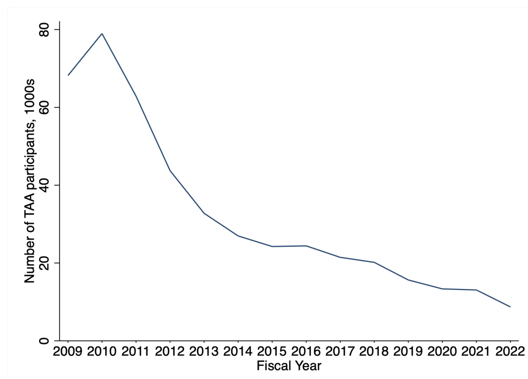
(D) Total spending - LEHD states



(E) Total participants - US

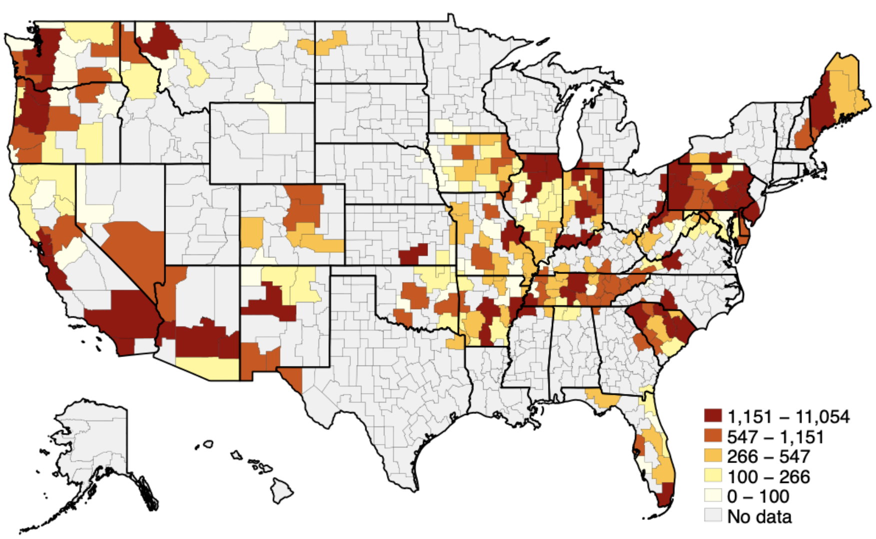


(F) Total participants - LEHD states



Notes: TAA spending data from Department of Labor (OTAA Dashboard), available here: https://www.dol.gov/agencies/eta/tradeact/data; UI spending data available here: https://oui.doleta.gov/unemploy/claims.asp. Data accessed February 24, 2023.

Figure A.2 – No. TAA Petitioning Workers by Commuting Zone, LEHD states 2009 - 2014



Notes: Data from Hyman (2018).

B Data Appendix

This appendix describes our process to identify TAA-eligible workers in the LEHD data, for whom age at displacement determines eligibility for wage insurance under RTAA. To focus on this group of workers, we prioritize including workers who we can confidently identify as TAA-eligible, while omitting others.

B.1 Identify TAA petitions in the LEHD

Step 1: Identify Petition Firm (EIN)

To identify workers involved in TAA-petitioning establishments, we match TAA petitions to firms, and then identify workers at the appropriate establishment within the firm using the 2014 LEHD snapshot. First, we follow Hyman (2018) and match the establishment address and firm name reported in the petition data to the relevant firm's Employer Identification Number (EIN) using the Standard Statistical Establishment List / Business Register (SSEL/BR) assembled between the Census and IRS. We implement separate matches by address and by firm name within the petitioning firm's state. If the name and address matches yield EINs for different firms, we leave the petition unmatched and return to it later. If within a state, both the address and the firm name match to the same firm's EIN, or if we can only match either the firm name or address to an EIN, we keep the matched firm's EIN. To ensure that we can match potentially shuttered plants to firm EINs, we perform this exercise in both the calendar year of the petition (based on the year US DOL received the petition, called the "institution date"), and the year preceding the petition. If the two yield different EINs across years (but have unique EINs within years), we assign the petition the matched EIN in the year preceding the petition. To check the petition-firm match process up to this point, we take 3 random samples of 100 petitions, and manually verify that petition names and addresses match SSEL/BR primary and secondary company names and addresses (SSEL/BR usually provides 2 names, including one for the parent).

To ensure that we incorporate all firms whose workers took up wage insurance, we manually check the petition-firm match for all petitions with at least one wage-insurance taker. We identify these petitions using the Trade Act Participant Report (TAPR) data, which provides information on workers who took up any TAA-related services. This manual check compares the firm name, address, and supplementary "second" firm name (which typically refers to any relevant parent/subsidiary distinction when reported) in the petition against the same information in the SSEL/BR data. We check petitions that were matched using the firm name and address procedure described in the preceding paragraph, updating the EIN match as needed, and when possible match petitions that were unmatched using the procedure in the preceding paragraph. We do so by first manually searching for the petition address in the SSEL/BR. If the addresses match, subject to minor spelling discrepancies, we confirm that the company name (including potential parents/subsidiaries) matches, and then add that EIN to the list of petition matches. If we cannot find an address match, we manually search for the petition company name in the SSEL/BR. If the company names match (again subject to minor spelling discrepancies), we add that EIN to the list of matched petitions. If we are still unable to find a match, we use additional public data from Google searches that link the name of the parent company and any subsidiaries. We then search for these names in SSEL/BR, restricting the search to firms sharing the same locality (city or town) as the petition address. In all such cases, we trace the EIN from the firm-level ECF to the worker-level EHF and confirm that the EIN suffers a decline in workers of a similar order of magnitude as that estimated in the petition data.[61] For the sample of certified petitions, we further confirm the quality of the match at this point by manually checking for matching sequences of three consecutive quarters of earnings in TAPR with the EHF earnings data and requiring that there be at least one individual-level match in TAPR to confirm the EIN assignment at the worker level.[62] If an EIN assignment meets these conditions, we add the EIN to our list of matched petitions.

Step 2: Identify Petition Establishment (SEINUNIT)

Since TAA certifications apply to workers at a given plant rather than an entire firm, we keep petitions that can be mapped to a unique LEHD establishment. Because the LEHD is based on state UI records, which only provide the worker's firm of employment (SEIN), not their establishment, we implement this mapping using the following process. The LEHD's Employer Characteristics File (ECF) provides the list of establishments (SEINUNIT) associated with each firm (EIN) by state and year. For cases where the petition matches to a firm with a single establishment within a state-year pair, we immediately have a unique establishment match, so we keep this petition and add it to the analysis sample.

For cases where the petition matches to a firm with multiple establishments, or multiple state firm identifiers (SEINs), we utilize additional information in an attempt to identify a unique establishment associated with the petition. First, we use geocoded petition addresses from Hyman (2018) to attain a unique county for each petition. If there is a unique establishment in that county and EIN, we keep that establishment and add that petition to the analysis sample. For remaining unmatched petitions, we identify the county or counties associated with the city and state of the petition address using a crosswalk between 2019 Census places (cities, villages, towns, townships) and counties from Haughwout et al. (2022). If the petition city and state map to a unique county containing a single matched establishment in the firm, we keep that establishment and add that petition to the analysis sample.

For remaining unmatched petitions, we use a combination of a 2010 HUD mapping and a similar mapping from Kondo (2018) which assigns counties to petition zip codes based on the "majority of addresses within a zip code." If the petition city and state map to a unique county containing a single matched establishment in the firm, we keep that establishment and add that petition to the analysis sample. We drop any remaining petitions that map to multiple establishments within the same county, as our prior steps are unable to uniquely identify the petitioning establishments

[61] We do this by checking all SEINs in the cases in which the EIN maps to multiple SEINs in the ECF.

[62] We are able to do this because TAPR data reports quarterly earnings prior to participating in a Trade Act training program, often taken directly from state ES-202 data.

B.2 Identify Workers Displaced from TAA-eligible Establishments in the LEHD

After establishments (SEINUNITS) with TAA-certified displacements have been identified, our next goal is to identify workers who had a TAA-eligible unemployment spell. To do so, we must identify displaced workers and determine whether the timing of their displacement falls within the TAA eligibility window.

We observe employment spells using the LEHD Employee History File (EHF), which contains quarterly worker earnings histories associated with each worker's SEINUNIT at which they are employed. The worker identifier is the personal identifier key (PIK), and the establishment identifier is the SEINUNIT; we therefore have a dataset at the PIK-SEINUNIT-QuarterYear level for the set of 25 states that approved use of the data in our Census proposal.[63] The LEHD provides the employing firm of these matched participating workers within the state but does not specify their establishment within the firm. To assign workers to establishments, we use the Unit-to-Worker (U2W) imputation file, which imputes each worker's establishment within a multi-establishment firm using information on establishment size as well as the establishment and worker addresses. It does so 10 times using a probabilistic Bayesian assignment method (note that the same establishment may be drawn multiple times for each worker). We then assign each worker to the single establishment with the most imputations for that worker (when two or more establishments are tied for the most imputations, that worker is omitted from the remainder of the process). We now have assigned all workers at petitioning firms to unique LEHD establishments.

We also have an indicator variable at the PIK-QuarterYear level, which indicates whether a worker was employed in any US state (the time coverage for this indicator varies by state, but is available for about two-thirds of observations). This information helps correct any spuriously tagged layoff events that may instead reflect continuous employment in another state.[64]

Displaced workers are identified in two ways, keeping in mind that the LEHD only reports earnings at the quarterly level. First, when we observe a full quarter of non-employment (i.e. zero quarterly earnings in our 25 states and not employed in other states), we have high confidence that the worker was displaced. Such workers comprise our main sample. However, a displaced worker may transition to a new employer (an SEIN distinct from the petitioning establishment) within two quarters, such that they do not spend a full quarter unemployed and do not exhibit a quarter with zero earnings. In this case, it is difficult to distinguish between displaced workers and those who voluntarily switched employers. We refer to these workers as "switchers" and include them in a supplementary "switcher-inclusive" sample.

Before including workers in the switcher-inclusive sample, we must avoid situations in

[63]These include AR, AZ, CA, CO, DC, DE, FL, IA, ID, IL, IN, KS, MD, ME, MO, MT, NM, NV, OK, OR, PA, SC, TN, WA, WV.

[64]These are the EHF "US Indicators" data, which, when available, record with 100% certainty whether a worker was employed at a UI-covered firm in the US in a given quarter. The US Indicators, however, tell us nothing about earnings outside of the set of 25 states. While this may cause minor concern for our effects on total earnings if wage insurance disproportionately induces workers to earn outside of the set of 25 states via increased mobility, we do not observe a missing mass of workers on either side of the eligibility cutoff for wage insurance that would emerge if this state selection issue were present.

which workers appear to switch establishments based on a change in SEINUNIT resulting from reorganizations that do not change the worker's physical workplace. We use the LEHD Successor-Predecessor File (SPF) to remove such workers from the switcher-inclusive sample. When switching occurs within the set of 25 states in which we observe the SPF, we remove workers from this sample if: (1) the firm reports workers were involved in a reorganization ("ES-identified" in the SPF); or (2) 5 or more workers are observed transitioning in UI data ("UI-identified" in the SPF) and the percent of workers at the predecessor firm transitioning to the successor firm or from the predecessor firm is greater than 25%. When transitions are into states where we do not have the SPF (but know their switching status from the US Indicators file), we remove any switchers when 5 or more workers are observed transitioning out of the 24-state set, as these may reflect relocations as well.

For each displacement event for a worker initially employed at a TAA-certified establishment, we must determine whether the worker's displacement falls within the three-year "TAA eligibility window" around the petition determination date (notification of petition approval or rejection).[65] The first and last calendar quarters of this eligibility window will likely include both workers who separate within the eligibility window (and are thus eligible) and workers who separate outside the window in the same calendar quarter (and are ineligible). We therefore apply a conservative sample restriction to avoid including non-eligible workers: we drop workers who separate in the first quarter of the eligibility window for the petition, or the last quarter of the eligibility window. While dropping these workers avoids including those who separate outside the eligibility window, and so who are not eligible, we also drop some eligible workers in the process. In the case that multiple overlapping petitions are filed over several years and a worker has displacement events that may apply to either petition, we assign the worker to the earlier petition. When a petition at a given SEINUNIT is certified and it has an overlapping denied petition, we keep workers who are displaced when the certified petition does not overlap the denied petition.This procedure allows workers to have multiple TAA-eligible displacement events as long as eligibility windows are non-overlapping. In these rare cases, the data may contain copies of worker histories, but these histories are indexed to different quarters of separation such that a worker's earnings information is never duplicated within a calendar quarter.

B.3 Pull Employment and Earnings Histories of Displaced Workers

Once eligible displaced workers have been identified, we calculate full earnings histories by summing each worker's quarterly earnings across all employers in the 24 LEHD states. Earnings are set to zero even if the worker is employed outside of the 24-state set, and therefore earnings are interpreted as specific to this set. By contrast, employment histories are computed from both the 24 LEHD states and the US Indicators file covering remaining states. When employment status is unknown in the US Indicators file due to lack of coverage, and the worker does not have positive earnings in the 24-state set, we record employment as missing. For each quarter, we record the "number of jobs" by counting the number of different SEINUNITs at which the worker is employed. While we do not observe hours

[65] As explained in Hyman (2018), workers who can demonstrate a layoff event up to one year prior and two years after the petition date qualify for TAA provisions, including wage insurance.

worked in the LEHD, the number of jobs may provide a proxy for part-time work within the 24-state set.[66] To study SEINUNIT transitions, we define a primary employer for each worker in a given quarter. If the worker has positive earnings from the TAA-petitioning firm, that is their primary employer.[67] Otherwise, the SEINUNIT with the most earnings in the quarter is the primary employer.[68]

As our primary objective is to study non-employment rather than unemployment (including distinctions regarding disillusionment), we choose to code earnings in panel edges (i.e. strings of zero quarterly earnings at the beginning or end of a worker's panel) as zeros rather than missing, except when a worker has zero earnings but is employed in a non-LEHD state, in which case we treat earnings as missing. Alternatively, one could code earnings in these quarters as missing, but doing so would condition on the endogenous employment outcome. We define highly attached workers as workers who have quarterly earnings of at least $3,000, 8 to 5 quarters prior to separation. We merge individual demographic variables from the LEHD ICF file (gender, age, race, ethnicity), and firm-level variables from the ECF (firm size in number of workers, firm age in years, and firm NAICS code (concorded over time and cross-checked within the ECF) at the SEIN level).[69] We calculate employee tenure as the number of consecutive months of overall employment, as well as firm-specific tenure as the number of consecutive months a worker is employed at a given SEIN.

B.4 TAA-Denied Sample

Our main sample described above uses TAA petitions that were approved by the US Department of Labor (USDOL) during the RTAA eligibility period (i.e. all approved petition numbers greater than number 70,000—the first petition eligible under RTAA in 2009). However, TAA-petitioning establishments that are denied under the RTAA regime provide an additional placebo group to understand the evolution of labor market behavior of similar workers absent wage insurance eligibility.[70] To identify petitioning establishments whose workers are denied benefits under the RTAA regime, we repeat the steps above, with a handful of refinements to account for the context of denied petitions.

First, when selecting the petitioning establishment among multi-establishment firms, the TAPR dataset is unhelpful as it only contains information on approved petitions. It is also the case that we are unable to create an analog of potentially high take-up denied firms as we do in the main sample. Second, when refining our sample using manual lookups, instead of cross-checking all petitions with at least one wage-insurance taker, we must use information on the estimated number of eligible workers on the petition. To reduce this set computationally, we cross-check all petitions for which there is a sizable estimated number of

[66] Although hours are reported on UI-records in Minnesota and Washington, the Census requires three states for any disclosed LEHD estimate, and our data does not include Minnesota.

[67] Workers at contracting employers may exhibit an "Ashenfelter" dip, which could result in it no longer being the majority employer from which the worker garnishes wages.

[68] We use this same definition when defining the "switcher-inclusive" sample.

[69] We also merge in ICF data on education, however this is imputed for % of the sample, unless the worker reported education level in the decennial Census or was part of the annual ACS sub-sample.

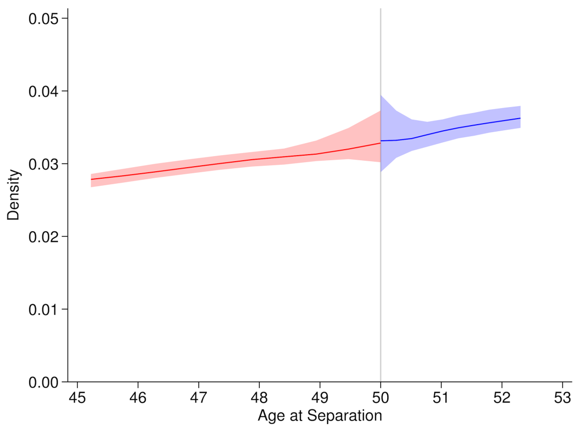
[70] Hyman (2018) shows that an important portion of the variation in TAA (and therefore RTAA) eligibility is due to whether the petition is randomly assigned a lenient versus strict USDOL investigator.

workers, as these are most likely to be multi-establishment firms. We manually cross-check all petitions with at least 100 estimated workers reported on the petition. Lastly, to ensure a sufficiently large sample during the RTAA period, we do not make any further restrictions on geography when attempting to identify the petitioning denied plant. Finally, with respect to overlapping petitions, our denied sample is similarly defined as including separating workers in all quarters in which there are no overlapping approved petitions, excluding workers who separate in the first quarter of the eligibility window for the petition, or the last quarter of the eligibility window.

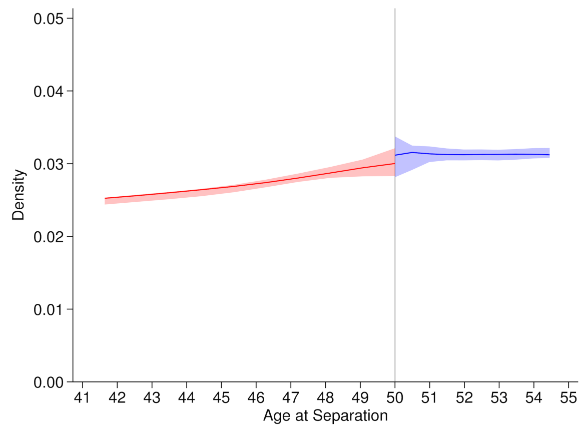
C Additional Results

Figure C.1 – Density of Age at Separation

(A) TAA-certified sample



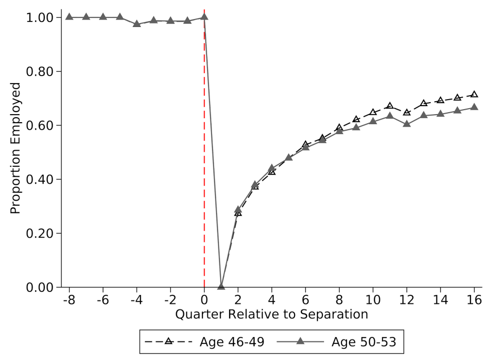
(B) TAA-denied sample



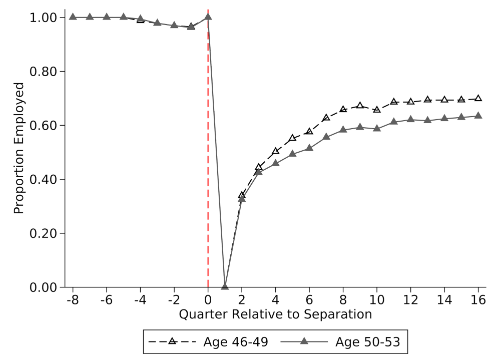
Notes: Panels A and B plot distribution of age at separation for the TAA-certified and TAA-denied samples, respectively. Samples are restricted to high labor force attachment, defined as earning at least $3,000 in each quarter from 8 to 5 quarters prior to separation. Graphs plot densities estimated separately on each side of the cutoff using the methods in Cattaneo et al. (2018). Census disclosure rules prevent showing histograms. There is no evidence of manipulation in either sample.

Figure C.2 – Descriptive Event Studies of Earnings Replacement and Employment by Age

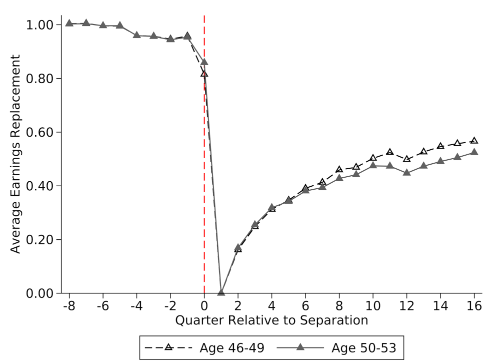
(A) TAA-certified, employment



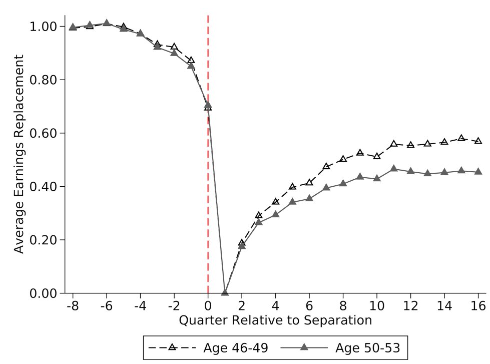
(B) TAA-denied, employment



(C) TAA-certified, earnings replacement

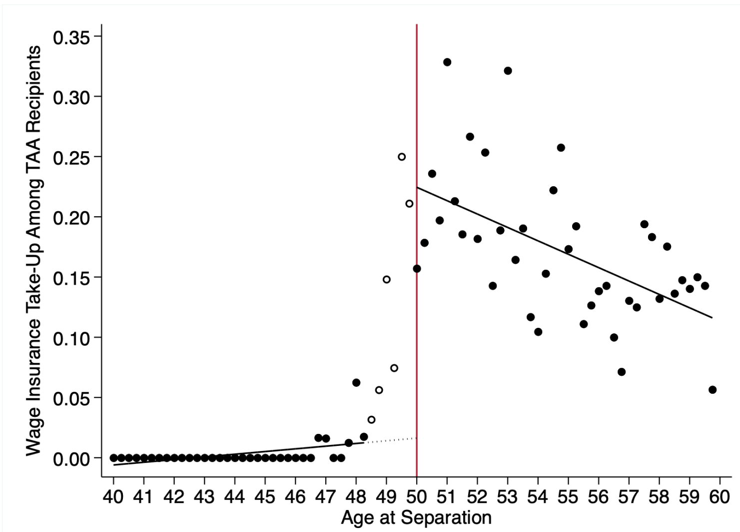


(D) TAA-denied, earnings replacement



Notes: Panels A and B plot employment rates for the certified and denied samples, respectively. Panels C and D plot earnings replacement rates for the certified and denied samples, respectively. Samples are restricted to high labor force attachment in the second year before displacement, defined as 4 quarters with UI-covered earnings each exceeding $3,000.

Figure C.3 – Wage Insurance Receipt among Workers Receiving any TAA Benefit



Notes: Figure plots the proportion of workers receiving any TAA benefits who ever receive wage insurance payments using data from Trade Act Participant Reports. Means are calculated within quarterly age bins, with age measured at separation. The solid lines show linear polynomials fit on the raw data using age measured in days, with separate polynomials above and below age 50. Workers displaced between ages [48.5,50) are excluded from the polynomial fit below age 50 as denoted by the hollow circles for those ages.

Table C.1 – Covariate Balance in RD, TAA-certified sample

	Discontinuity	S.E.	Control Mean	% diff
Predicted Prior Earnings in -8Q to -5Q	1,329	816.4	47,680	2.8
Prior Earnings in -1Q	104.8	314.2	11,310	0.9
Prior Earnings in -5Q	100.5	288.8	11,900	0.8
Prior Earnings in -6Q	267.1	275.9	11,810	2.3
Prior Earnings in -7Q	408.8	293.5	11,920	3.4
Prior Earnings in -8Q	281.3	298	11,910	2.4
Prior Earnings in -8Q to -5Q	1,019	1,082	47,630	2.1
Δ Prior Earnings from -8Q to -5Q	296.3	212.7	43.68	678.3
Less Than High School	0.028	0.017	0.097	28.9
High School	-0.077	0.026	0.457	-16.8
Some College	0.013	0.024	0.303	4.3
College or Higher	0.033	0.019	0.152	21.7
Female	-0.038	0.022	0.367	-10.4
Black	-0.007	0.014	0.094	-7.4
White	0.002	0.017	0.858	0.2
Other Race	0.005	0.010	0.049	10.2
Hispanic	-0.022	0.011	0.050	-44.0
Overall Tenure (quarters)	-0.259	0.820	63.96	-0.4
Petitioning-Firm Tenure (quarters)	-0.097	1.009	33.57	-0.3
Firm Age (years)	0.505	0.495	29.22	1.7
Log Firm Size	-0.023	0.079	7.731	-0.3
Year of filing	0.085	0.068	2010	0.0

Notes: Table presents balance tests of estimating equation (7) on baseline covariates and pre-separation outcomes. The discontinuity measures the jump in the regression function at age 50. The Control Mean denotes the regression estimate immediately to the left of age 50 for the TAA-certified sample. Each regression uses MSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a uniform kernel to weight observations. Sample sizes for each regression vary depending on the bandwidth used. We report full sample size prior to bandwidth selection ($N = 28,000$) due to Census disclosure rules. Predicted earnings are calculated from a regression of earnings 8Q-5Q before separation against firm tenure, log firm size, firm age, year of filing, and fixed effects for education, race, state, and 3-digit industry.

Table C.2 – Covariate Balance in RD, TAA-denied sample

	Discontinuity	S.E.	Control Mean	% diff
Predicted Prior Earnings in -8Q to -Q	416.7	853.3	50,120	0.8
Prior Earnings in -1Q	76.4	338.2	10,950	0.7
Prior Earnings in -5Q	159.9	297.2	12,510	1.3
Prior Earnings in -6Q	300.2	291	12,660	2.4
Prior Earnings in -7Q	447.5	304.6	12,480	3.6
Prior Earnings in -8Q	507.9	331.6	12,550	4.0
Prior Earnings in -8Q to -5Q	1362	1,152	50,260	2.7
Δ Prior Earnings from -8Q to -5Q	238.9	162.7	92.33	258.7
Less Than High School	0.009	0.013	0.106	8.5
High School	-0.008	0.021	0.328	-2.4
Some College	-0.011	0.016	0.33	-3.3
College or Higher	-0.007	0.018	0.238	-2.9
Female	0.017	0.017	0.379	4.5
Black	-0.006	0.013	0.146	-4.1
White	-0.008	0.014	0.784	-1.0
Other Race	0.021	0.011	0.07	30.0
Hispanic	0.024	0.01	0.08	30.0
Overall Tenure (quarters)	-0.79	0.744	65.06	-1.2
Petitioning-Firm Tenure (quarters)	-0.104	0.764	27.95	-0.4
Firm Age (years)	-0.079	0.319	31.56	-0.3
Log Firm Size	0.15	0.082	9.236	1.6
Year of filing	-0.016	0.063	2011	0.0

Notes: Table presents balance tests of estimating equation (7) on baseline covariates and pre-separation outcomes. The discontinuity measures the jump in the regression function at age 50. The Control Mean denotes the regression estimate immediately to the left of age 50 for the TAA-denied sample. Each regression uses MSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a uniform kernel to weight observations. Sample sizes for each regression vary depending on the bandwidth used. We report full sample sizes prior to bandwidth selection ($N = 48,500$) due to Census disclosure rules. Predicted earnings are calculated from a regression of earnings 8Q-5Q before separation against firm tenure, log firm size, firm age, year of filing, and fixed effects for education, race, state, and 3-digit industry.

Table C.3 – Covariate Balance in D-RD

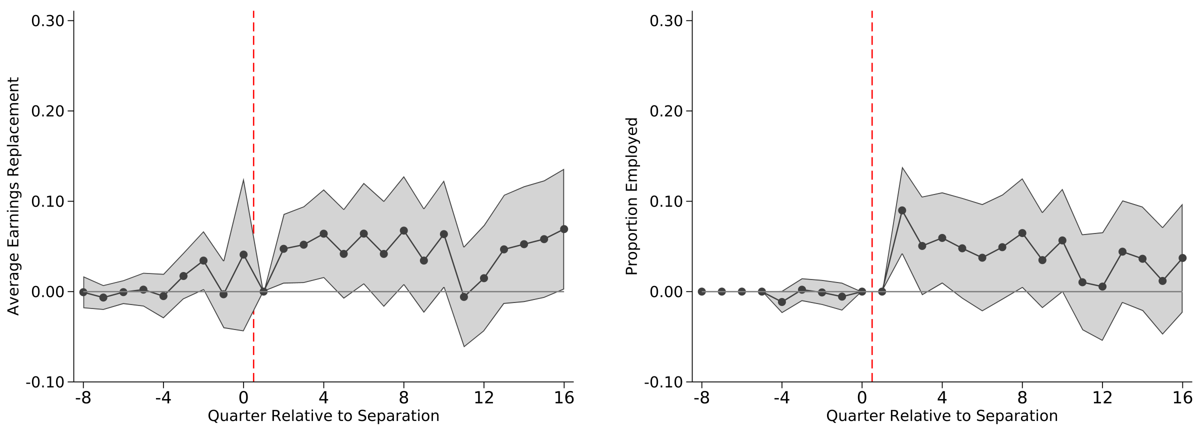
	Estimate	S.E.	Control Mean	% diff
Predicted Prior Earnings in -8Q to -5Q	582	984	46,780	1.2
Prior Earnings in -1Q	-174.3	487.3	11,300	-1.5
Prior Earnings in -5Q	-34.9	415.7	11,860	0.3
Prior Earnings in -6Q	41.86	426.5	11,800	0.4
Prior Earnings in -7Q	-41.35	453.6	11,940	-0.3
Prior Earnings in -8Q	-81.61	473.0	11,880	-0.7
Prior Earnings in -8Q to-5Q	245.5	1,658	47,480	0.5
Δ Prior Earnings from -8Q to -5Q	-78.6	281.9	41.0	-191.8
Less Than High School	0.031	0.022	0.088	35.2
High School	-0.052	0.030	0.455	-11.4
Some College	0.008	0.027	0.300	2.7
College or Higher	0.026	0.026	0.154	16.9
Female	-0.049	0.028	0.367	-13.4
Black	0.000	0.020	0.147	0.0
White	0.003	0.021	0.856	0.4
Other Race	-0.011	0.015	0.047	-23.4
Hispanic	-0.033	0.016	0.053	-62.3
Overall Tenure (quarters)	0.443	1.160	64.28	0.7
Petitioning-Firm Tenure (quarters)	0.514	1.265	33.22	1.5
Firm Age (years)	0.667	0.558	29.16	2.3
Log Firm Size	-0.184	0.123	7.731	-2.4
Year of filing	0.050	0.094	2010	0.0

Notes: Table presents balance tests of estimating equation (8) on baseline covariates and pre-separation outcomes. Each row corresponds to a separate regression. The difference in discontinuities measures the jump in the regression function at age 50 for the TAA-certified sample relative to the TAA-denied sample. The Control Mean denotes the regression estimate of that outcome immediately to the left of age 50 for the TAA-certified sample. Each regression uses MSE-optimal bandwidths calculated separately for each side of the age-50 discontinuity for each outcome, and a uniform kernel to weight observations. Sample sizes for each regression vary depending on the bandwidth used. We report full sample sizes prior to bandwidth selection ($N = 76,500$) due to Census disclosure rules. Predicted earnings are calculated from a regression of earnings 8Q-5Q before separation against firm tenure, log firm size, firm age, year of filing, and fixed effects for education, race, state, and 3-digit industry.

Figure C.4 – Robustness: D-RD Estimates without 1-Sided Donut

(A) Earnings replacement

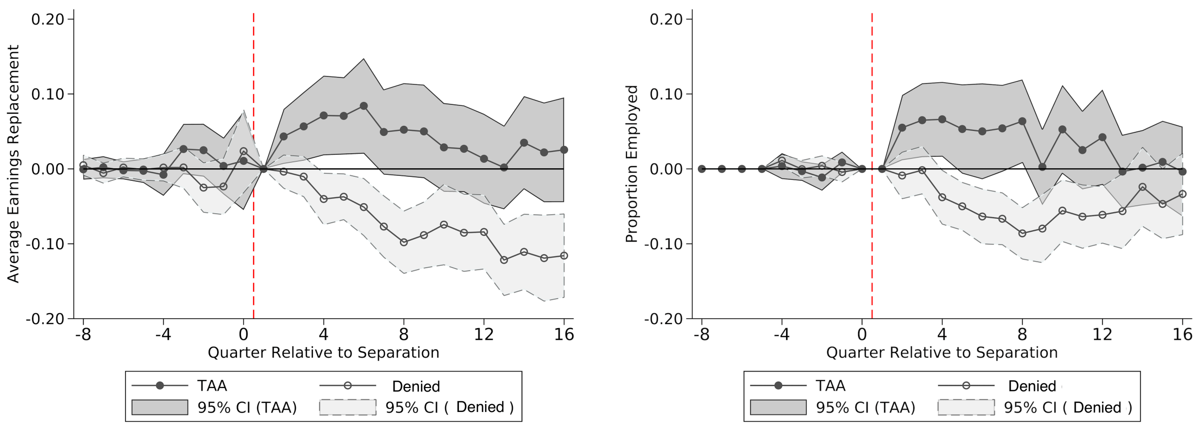
(B) Employment



Notes: Panels plot D-RD estimates for earnings replacement (Panel A) and employment (Panel B) for regressions that do not include the one-sided donut. Shaded areas denote 90% confidence intervals. Samples are restricted to high labor force attachment in the second year before displacement, defined as 4 quarters with UI-covered earnings each exceeding $3,000. Each regression uses MSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a uniform kernel to weight observations.

Figure C.5 – Robustness: Regression Kink (RK) Estimates

(A) Earnings replacement (B) Employment

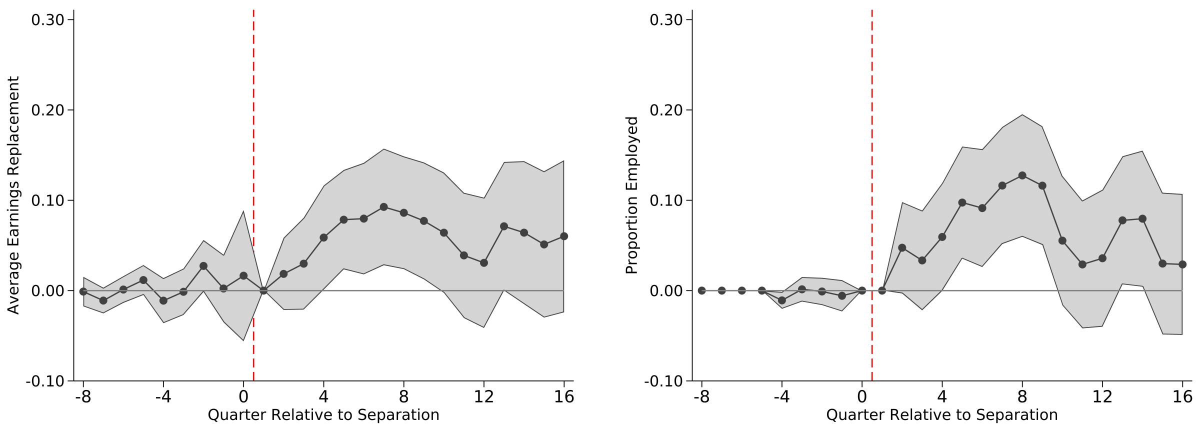


Notes: Panels plot estimates for earnings replacement (Panel A) and employment (Panel B) using a regression kink (RK) design. The eligibility rule for wage insurance introduces two kinks, which correspond to the edges of the donut used in the main analysis. The position of the lower kink varies with the quarter relative to separation, while the higher kink is anchored at age 50. Specifically, the position of the lower kink (k_1) in relative quarter t is equal to $\max\{50 - \frac{\min(t,6)}{4}, 48.5\}$. The RK incorporates variation "inside" the donut hole by estimating the *change* in the slope of the outcome as a function of how much time workers are eligible: workers are always eligible above age 50, they are never eligible below k_1, and the fraction of time they are eligible scales linearly between age 50 and k_1. We estimate a single "joint" RK design using both kinks together, assuming no jumps at the kink points and equal slopes of the outcome variable with respect to age to the left of the lower kink and to the right of the higher kink. This yields the following constrained regression equation $y_i = \alpha + \beta(age_i - k_1) + \gamma \cdot B_i \cdot (age_i - k_1) + \delta \cdot B_i + \varepsilon_i$ where B_i is an indicator for ages between the kinks, and δ is constrained to be equal to $(k_1 - 50) \cdot \gamma$. Equivalently, one can estimate $y_i = \alpha + \beta(age_i - k_1) + \gamma \cdot \max\{0, \min\{50 - k_1, age_i - k_1\}\} + \varepsilon_i$ without constraints. Figures plot estimates of γ from these regressions in each relative quarter of separation. Shaded areas denote 95% confidence intervals. As in the main results, samples are restricted to high labor force attachment in the second year before displacement, defined as 4 quarters with UI-covered earnings each exceeding $3,000. Each regression uses MSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome (the same as those used in the main results), and a uniform kernel to weight observations.

Figure C.6 – Robustness: D-RD Estimates, All Petitions without ML Restriction

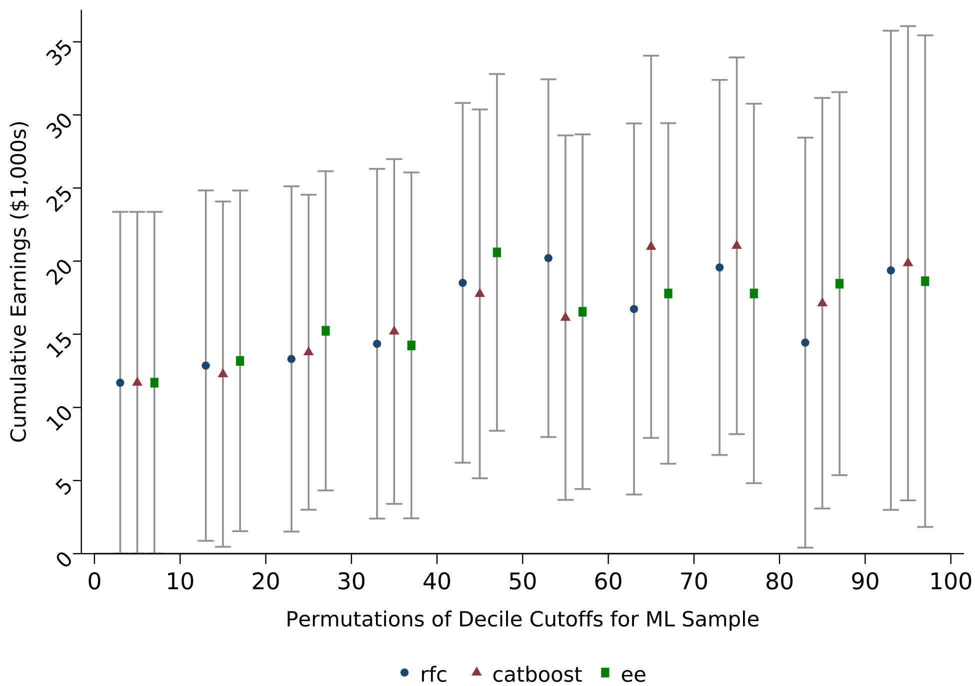
(A) Earnings replacement

(B) Employment



Notes: Panels plot D-RD estimates for earnings replacement (Panel A) and employment (Panel B) for all petitions, including those excluded from the ML sample. Shaded areas denote 95% confidence intervals. Samples are restricted to high labor force attachment in the second year before displacement, defined as 4 quarters with UI-covered earnings each exceeding $3,000. Each regression uses MSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a uniform kernel to weight observations.

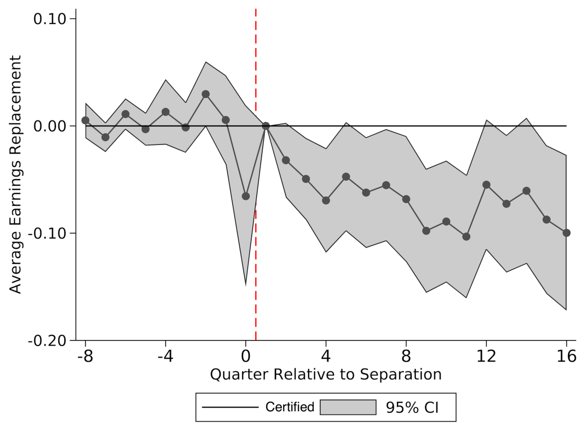
Figure C.7 – Sensitivity of ITT Estimates to Predicted Probability Cutoffs for High Take-Up



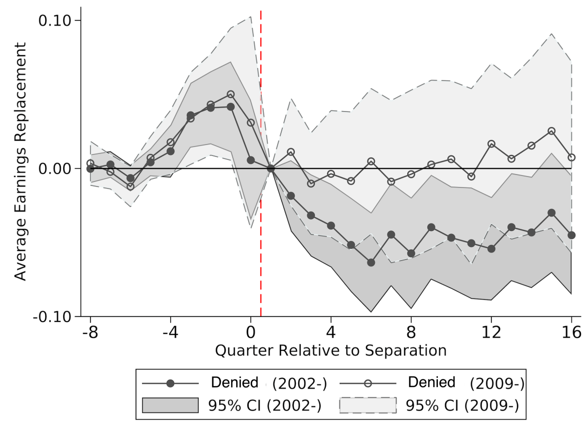
Notes: Figure plots D-RD estimates for cumulative earnings 16 quarters after displacement, for 10 deciles of ML predicted probability cutoffs used to define high take-up in the "certified" sample. In the first decile (least restrictive), all petitions are included in the ML sample, and estimates are thus identical across all three ML classifiers. In the tenth decile (most conservative), only the top 10% of predicted probabilities are included in the ML sample. Across all classifiers, estimates are generally increasing in decile cutoffs, with D-RD effect sizes ranging from about $12,000 to $22,000, and with greatest precision for ML high-take-up cutoffs between the 40th and 70th percentiles of the predicted probability distribution. Our preferred estimate of $18,260 reported in Figure 8 corresponds to the random forest classifier (rfc) predicted probability cutoff of 0.2858 to be identified as high take-up (see Appendix D for further details).

Figure C.8 – Falsification test: RD Results using Age 55 Cutoff

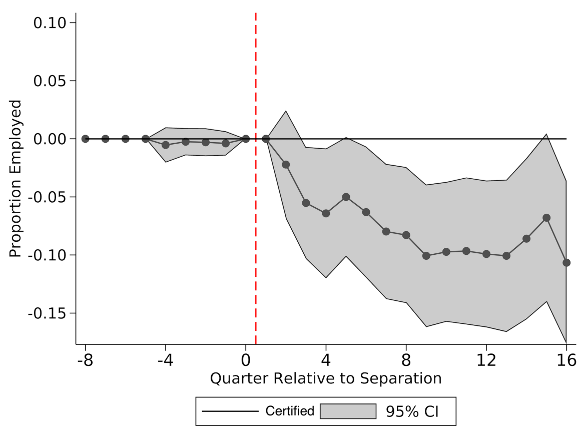
(A) TAA-certified, earnings replacement



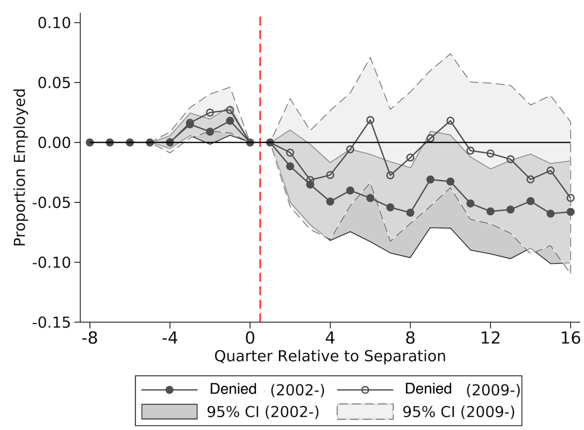
(B) TAA-denied, earnings replacement replacement



(C) TAA-certified, employment

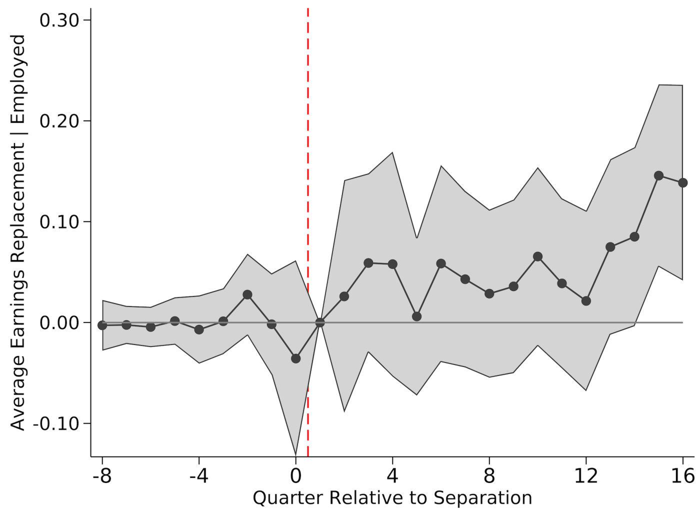


(D) TAA-denied, employment



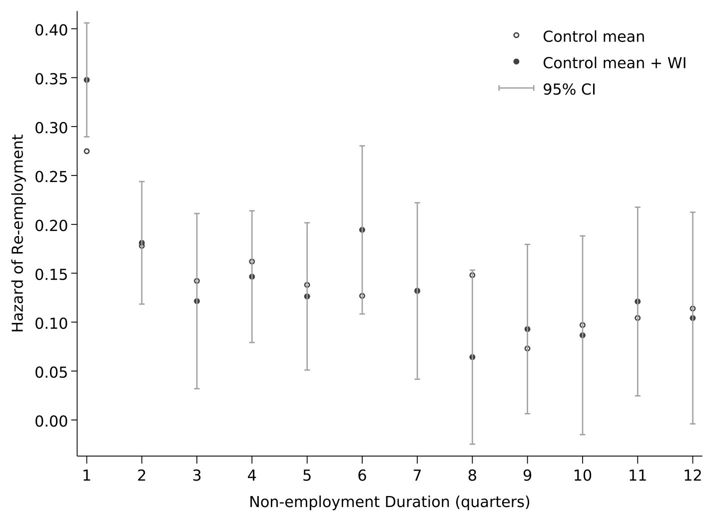
Notes: Panels A and B plot earnings replacement rates for the TAA-certified and TAA-denied samples, respectively, using age 55 as the discontinuity. Panels C and D plot corresponding figures for employment rates, again using the age 55 discontinuity. Shaded areas denote 95% confidence intervals. Samples are restricted to high labor force attachment in the second year before displacement, defined as 4 quarters with UI-covered earnings each exceeding $3,000. Each regression uses MSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a uniform kernel to weight observations.

Figure C.9 – Earnings Replacement Conditional on Employment



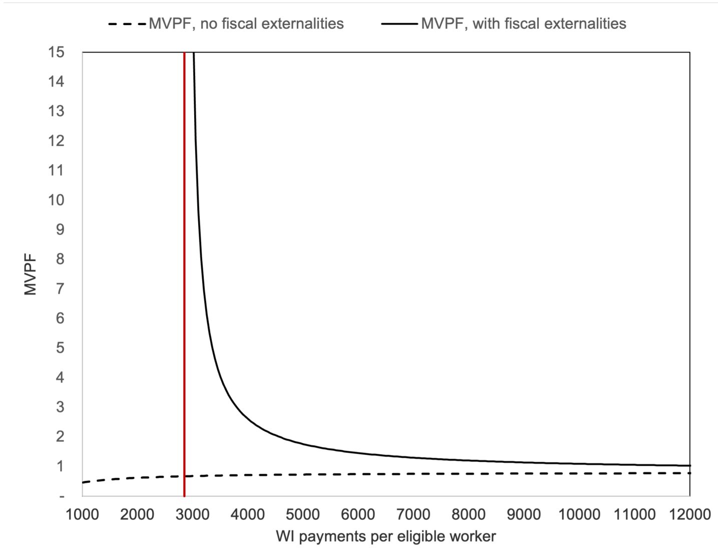
Notes: Figure plots D-RD results of earnings replacement rates conditional on employment from estimating equation (8) from 8 quarters pre-separation to 16 quarters post-separation. Earnings replacement rates are calculated as earnings relative to the average from the second year before displacement, deflated to 2018Q1 dollars prior to calculating the replacement rate. Shaded areas denote 95% confidence intervals. Sample is restricted to high labor force attachment in the second year before displacement, defined as 4 quarters with UI-covered earnings each exceeding $3,000. Each regression uses MSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a uniform kernel to weight observations.

Figure C.10 – Hazard of Reemployment



Notes: Figure plots D-RD estimates using equation (8) from 1 quarter to 12 quarters since separation, where dependent variable is the hazard rate of employment. Notably, statistically significant effects of wage insurance eligibility on the hazard of reemployment are only detected 1 quarter from separation. Each regression uses MSE-optimal bandwidths calculated separately for each side of the cutoff, and a uniform kernel to weight reservations

Figure C.11 – MVPF using lower bound of 95% CIs



Notes: Figure plots MVPFs vs. wage insurance (WI) payments per eligible worker using the lower bounds of the 95% confidence intervals for cumulative earnings and unemployment durations from the D-RD estimates. To illustrate the importance of fiscal externalities, dashed lines show the MVPFs excluding tax receipts on increased earnings and reduced UI payments and solid lines show the MVPF including fiscal externalities. For visual clarity, we truncate the MVPFs at 15 from above and indicate with red vertical lines the subsidy value at which fiscal externalities exceed program costs (i.e. where $MVPF = \infty$).

Additional Details of Earnings Decomposition: Each term in the statistical decomposition of cumulative earnings maps to a D-RD estimate. Recall that the effect of wage insurance eligibility on cumulative earnings can be written as follows.

$$\mathbb{E}\left[\sum_t earn_{it}|D_i = 1\right] - \mathbb{E}\left[\sum_t earn_{it}|D_i = 0\right] = \sum_t \left(\mathbb{E}\left[earn_{it}|D_i = 1\right] - \mathbb{E}\left[earn_{it}|D_i = 0\right]\right)$$

The decomposition is:

$$\begin{aligned}
\mathbb{E}\left[earn_{it}|D_i = 1\right] - \mathbb{E}\left[earn_{it}|D_i = 0\right] = \\
\mathbb{E}[earn_{it}|D_i = 1, emp_{it} = 1] \times (\mathbb{P}(emp_{it} = 1|D_i = 1) - \mathbb{P}(emp_{it} = 1|D_i = 0)) \\
+(\mathbb{E}[earn_{it}|D_i = 1, emp_{it} = 1] - \mathbb{E}[earn_{it}|D_i = 0, emp_{it} = 1]) \times \mathbb{P}(emp_{it} = 1|D_i = 0)
\end{aligned}$$

To implement the decomposition, we must separately calculate the values of the second and third lines and then sum them across years to calculate the overall contribution of each component. We can do so by running our standard RD analysis for earnings on a sample of employed workers in each period, where:

- $\mathbb{E}[earn_{it}|D_i = 1, emp_{it} = 1]$ is the earnings replacement among employed workers who are treated. It is estimated as $\gamma_0^t + \gamma_1^t$ from equation (8) where the dependent variable is earnings conditional on employment.
- $(\mathbb{P}(emp_{it} = 1|D_i = 1) - \mathbb{P}(emp_{it} = 1|D_i = 0))$ is the change in employment probability due to treatment. It is estimated as γ_3^t from equation (8) where the dependent variable is employment.
- $(\mathbb{E}[earn_{it}|D_i = 1, emp_{it} = 1] - \mathbb{E}[earn_{it}|D_i = 0, emp_{it} = 1])$ is the change in earnings (in levels) from treatment among those employed. It is estimated as γ_3^t from equation (8) where the dependent variable is earnings conditional on employment.
- $\mathbb{P}(emp_{it} = 1|D_i = 0)$ is the probability of employment among those who are not treated. It is estimated as γ_0^t from equation (8) where the dependent variable is employment.

D Predicted High-Takeup Sample Using Machine Learning

D.1 Objective and Summary

Because we study the effects of wage insurance *eligibility* on worker outcomes, our analysis may struggle to identify any effects of the program if takeup is very low. Program reports and discussions with administrators raise concerns that many eligible workers were not aware of the A/RTAA wage insurance program, potentially explaining low takeup rates, particularly early in the program's implementation. To address this issue, we identify the types of petitions in which wage insurance takeup was historically high. We do so using data from the Trade Act Participant Reports (TAPR) from 2005 to 2011, which record the number of wage insurance participants associated with each approved TAA petition. We use data from this period to train a machine learning (ML) classifier that identifies high-takeup petitions based on their observable characteristics and use this model to predict which petitions are likely to have high takeup in the post-2011 data, where the realized takeup rate is not observable.

We implement this classification nonparametrically using a standard ML classification process that takes the following steps: The labeled data (covering 2005 to 2011) are split into training and testing samples. The training sample is used to fit a given model, and the testing sample is used to determine the model's accuracy in predicting the classification out of sample. The training process involves choosing a set of "hyperparameters" determining various aspects of the model's structure, for example the maximum depth in tree-based models. This choice of hyperparameters is analogous to using fitting rules that restrict the number of higher order polynomials or that bound the set of all possible interaction terms in a regression. These hyperparameters are chosen through cross-validation, in which the training sample is split into equally sized portions, the model is fit on all the training data except one held-out portion, and its predictive accuracy is evaluated on the held-out portion. The process is then repeated for each of the hold-out samples. We choose optimal hyperparameters as those maximizing predictive precision conditional on achieving at least a target level of recall (our preferred metric of predictive accuracy, discussed in detail below). Once optimal hyperparameters are chosen in the training sample, we fit the model on all of the training data and verify the quality of the classification model out-of-sample by testing our predictions in the testing sample. Given favorable results in the testing sample, we use the model to classify all petitions as high or low takeup, including those in the post-2011 data without observable takeup. We then generate a subsample of workers associated with petitions predicted to have high takeup.

D.2 High-Takeup Definition

We calculate the takeup rate as the number of observed wage-insurance recipients falling under a given TAA petition in the TAPR data divided by the number of estimated TAA-eligible workers reported for the associated petition. We define "high takeup" petitions as those with an observed takeup rate of at least 2%. While this cutoff may seem low, it likely reflects a much higher takeup rate among wage-insurance-eligible displaced workers, for two reasons. First, the petition data tend to overestimate the number of eligible workers by a factor of 2 to 3, so the denominator in the observed takeup rate is quite a bit larger than the true number of TAA-eligible workers. Second, because only those age 50 or over at displacement are eligible for wage insurance, the denominator in the observed takeup rate is again too large, since it includes both older and younger workers. Among relevant petitions in the 2005 to 2011 range, 13% satisfy our definition of "high takeup."

D.3 Approach

Our objective is to generate a sample consisting primarily of high-takeup petitions (true positives) while avoiding missing many high-takeup petitions (false negatives). With that goal in mind, we employ a 10-fold cross validation procedure to select hyperparameters specific to each model. The predictive accuracy metric that we target in cross validation is maximum precision conditional on achieving at least a specified level of recall. This metric allows us to maximize the share of true high-takeup firms among those chosen (max precision) while setting a cap on the share of true high-takeup firms not chosen (target recall). Algorithm 1 describes the calculation of this predictive accuracy metric in detail. For a given classification model, we assign a prediction of 1 to observations whose posterior probabilities (scores) exceed a given cutoff. We choose the cutoff that maximizes precision among all those that satisfy the recall target. The resulting value of precision is our predictive accuracy metric for that model.

Algorithm 1: Max Precision for Target Recall

Data: True Classification (Y_{True}), Predicted Probabilities from Model ($p = \mathbb{P}(Y_{\text{Model}} == 1)$), and Target Recall Cutoff (k).
Result: Maximum precision given target recall cutoff $k \equiv Obj$

begin
  Recall Target$[\cdot] \longleftarrow \emptyset$
  Precision Target Scores$[\cdot] \longleftarrow \emptyset$
  $i \longleftarrow 0$
  for $c \in [\min(p), \max(p)]$ **do**
    $Y_{\text{pred}} \longleftarrow (p > c)$
    Recall Target$[i] \longleftarrow (\text{recall}(Y_{\text{True}}, Y_{\text{pred}}) > k)$
    Precision Target Scores$[i] \longleftarrow \text{precision}(Y_{\text{True}}, Y_{\text{pred}})$
    i++
  end
  if $\max(\textit{Recall Target}) = 0$ **then**
    $Obj \longleftarrow 0$
  else
    $Obj \longleftarrow \max_j(\text{Precision Target Scores}[j] \mid \text{Recall Target}[j] == 1)$
  end
end

An additional issue in our context stems from the fact that our outcome variable is imbalanced, with only 13% of the petitions in our training data classified as high takeup. This poses a challenge for predictive modeling, as most machine learning algorithms used for classification assume an equal number of examples for each class. This results in models that have poor predictive performance, specifically for the minority class, which in our case is the set of high-takeup firms. We address this issue as follows. Certain models (e.g. EasyEnsemble) balance the data internally as part of the algorithm. Otherwise, we oversample the minority class (high-takeup firms) to compensate for the imbalance. While we could also undersample the majority class to address this concern, given our relatively small sample size (in machine learning terms), we have opted to risk overfitting in the training sample rather than risk losing information that might be critical to our classification out-of-sample.

Algorithm 2 describes our main classification algorithm. As already mentioned, the approach it describes is entirely standard. We present it here simply for clarity and completeness. We first randomly split the sample of labeled data into training ($\mathcal{N}$) and testing ($\mathcal{N}^0$) sets, corresponding to 90% and 10% of the sample, respectively. We choose hyperparameters h using 10-fold cross-validation within the 90% training sample, targeting a maximum precision given recall of at least 0.7.[71] For each fold, we fit the model to the training data omitting the cross-validation hold-out set ($\mathcal{N}_{-i}$), generate posterior probabilities in the hold-out set, and calculate the max precision given target recall in the hold-out set. We store these max precision values for each fold and average them across folds to calculate the average precision score for a given vector of hyperparameters.

We then select the hyperparameter vector h^* that maximizes this average precision given target recall metric. Using the optimal hyperparameters, we fit the model to the entire training sample and use the fitted model to predict takeup in the testing set. We record the max precision at target recall metric in the testing set to evaluate the model's out-of-sample performance and record the associated posterior probability cutoff, k^*. Finally, we fit the model to the entire sample of labeled data ($\mathcal{N} \cup \mathcal{N}^0$) and use it to predict takeup for the entire dataset ($\mathcal{N} \cup \mathcal{N}^0 \cup \mathcal{M}$), including observations in the *unlabeled* data ($\mathcal{M}$), post 2011. Observations with posterior probability greater than k^* are classified as high takeup.

[71] Our main analysis uses a target recall cutoff of 0.7, but we investigate the implications of varying that cutoff in Figure D.2 below.

Algorithm 2: Predicting High Takeup Petitions

Input:

model: The classification model, *model*(h) represents the model endowed with hyperparameter h

$\mathcal{H}$: Hyperparameter space

k: Target recall cutoff

K: Number of folds in cross validation

$\mathcal{N}$: Set of training observations, balanced with under-sampling

$\mathcal{N}_{-i} \in \mathcal{N}$: the hold-out subset during the i^{th} cross validation

$\mathcal{N}^0$: Set of testing observations

$\mathcal{M}$: Set of unlabeled observations

Data:

max_precision_for_target_recall: a function that computes the maximum precision given a target recall cutoff, described in Algorithm 1

Result:

h^*: The optimal hyperparameter vector searched via cross validation

p^*: The optimal posterior probability cutoff

Y_{pred}: Predicted classfication based on *model*(h^*)

begin
    Avg Precision Scores$[\cdot] \longleftarrow \emptyset$
    for $h \in \mathcal{H}$ **do**
        Max Precision Scores$[\cdot] \longleftarrow \emptyset$
        for $i \in 1 : K$ **do**
            $\mathcal{N}_{-i} \longleftarrow$ the hold out set of observations
            $\mathcal{N}_{\text{Train}} \longleftarrow \mathcal{N} \backslash \mathcal{N}_{-i}$
            $\mathcal{N}_{\text{Valid}} \longleftarrow \mathcal{N}_{-i}$
            model$(h).fit(\mathcal{N}_{\text{Train}})$
            $p \longleftarrow$ model.*predict_proba*$(\mathcal{N}_{\text{Valid}})$
            Max Precision Scores$[i] \longleftarrow$ *max_precision_for_target_recall*(data $= \mathcal{N}_{\text{Valid}}$, predicted_prob $= p$, recall_cutoff $= k$)
        end
        Avg Precision Scores$[h] \longleftarrow$ *mean*(Max Precision Scores)
    end
    $h^* \longleftarrow$ *argmax*(Avg Precision Scores$[\cdot]$)

    model$(h^*).fit(\mathcal{N})$
    $p \longleftarrow$ *model.predict_proba*$(\mathcal{N}^0)$
    Model Max Precision, $k^* \longleftarrow$ *max_precision_for_target_recall*(data $= \mathcal{N}^0$, predicted_prob $= p$, recall_cutoff $= k$)

    model$(h^*).fit(\mathcal{N} \cup \mathcal{N}^0)$
    $p \longleftarrow$ *model.predict_proba*$(\mathcal{N} \cup \mathcal{N}^0 \cup \mathcal{M})$
    $Y_{\text{pred}} \longleftarrow (p > k^*)$
end

D.4 Model Selection

We consider the following models in classifying petitions:

- **Logit l2:** Logistic Regression with $l2$ norm penalty.
- **LDA:** Linear discriminant analysis using MLE.
- **NB:** Naive Bayesian Model.
- **RFC:** Random Forest Classifier.
- **AdaBoost:** AdaBoost Classifier with decision tree estimator as base.
- **CatBoost:** CatBoost Classifier with decision tree estimator as base.
- **EE:** Easy Ensemble using AdaBoost Classifier with decision tree estimator as base.

Table D.1 records the performance of each model in our testing sample using the optimal parameters for each model selected using cross validation. Given this out-of-sample performance, we focus the remainder of our analysis on RFC, Catboost, and EE in more detail as potential candidates for our final classification model.

Table D.1 – Out-of-Sample (Testing Set) Performance, 2005Q1 - 2010Q4

	Accuracy	F1	Precision	Recall	Geometric Mean
Logit l2	0.597	0.669	0.567	0.817	0.307
LDA	0.593	0.653	0.569	0.766	0.322
NB	0.587	0.647	0.565	0.756	0.316
RFC	0.654	0.675	0.636	0.721	0.423
Adaboost	0.570	0.664	0.545	0.850	0.247
Catboost	0.671	0.710	0.634	0.807	0.432
EE	0.663	0.676	0.651	0.703	0.438

Notes: The table displays our measures of out-of-sample performance in the testing set for different classification models. Accuracy is a weighted arithmetic mean of Precision and Inverse Precision (weighted by Bias) as well as a weighted arithmetic mean of Recall and Inverse Recall (weighted by Prevalence). Precision is the fraction of relevant instances among the retrieved instances. Recall is the fraction of relevant instances that were retrieved. The F1 score is the harmonic mean of the precision and recall. The geometric mean is the root of the product of class-wise sensitivity. This measure seeks to maximize the accuracy on each of the classes while keeping these accuracies balanced. The sample consists of participants in TAA from the TAPR data and estimates of participants from the TAA Petition data from 2005Q1 through 2010Q4. Takeup rates are winsorized to the 1st and 99th percentiles.

D.5 Hyperparameters

During cross validation, we consider the following hyperparameters for each of the three model candidates (RFC, Catboost, and EE). These parameters were chosen to avoid overfitting to the training data, enhancing model's performance out of sample. The numbers in parenthesis report the optimal hyperparameter values (h^*).

- **RFC**
  - `n_estimators (300)`
    Number of base estimators (decision trees) in the forest
  - `min_sample_split (2)`
    The minimum number of observations required at a node to be considered for further split
  - `min_sample_leaf (5)`
    The minimum number of observations required to be considered feasible for constructing a new node from splitting
  - `max_depth (16)`
    The maximum depth of each base estimator
- **Catboost**
  - `n_estimators (110)`
    Number of base estimators (decision trees) in the forest
  - `subsample (0.6)`
    Proportion of sample for bagging
  - `learning_rate (0.1)`
    Step size for moving along the gradient's direction
  - `min_data_in_leaf (2)`
    The minimum number of observations required to be considered feasible for constructing a new node from splitting
  - `max_depth (10)`
    The maximum depth of each base estimator
- **EE(Choose Adaboost as base classifier)**
  - `n_estimators(EE) (80)`
    Number of Adaboost in the EE model
  - `n_estimators(Adaboost) (80)`
    Number of base estimators (decision trees) in each Adaboost model
  - `max_depth (15)`
    The maximum depth of each decision tree
  - `learning_rate (0.2)`
    Step size for moving along the gradient's direction

D.6 Results

Figure D.1 Panels (A) and (B) present standard performance metrics for the models' ability to categorize observations in the testing set (i.e. out of sample).

In Panel (A), the ROC curve plots the true positive rate (true positives over all relevant elements; equivalent to recall) on the y-axis and the false positive rate (false positives over all non-relevant elements) on the x-axis. The plot is generated by varying the cutoff probability in the posterior probability distribution for each model. As a higher true positive rate and a lower false positive rate is preferred, curves lying to the northwest indicate superior performance. In this case, the top three models perform very similarly on this metric.

Panel (B) presents a similar figure showing precision (true positives over all selected) on the y-axis and recall (true positives over all relevant elements) on the x-axis. Again, this plot is generated by varying the cutoff probability in the posterior probability distribution for each model. As precision and recall are both desirable, curves lying to the northeast indicate superior performance. As with ROC, the top three models perform very similarly on this metric.

As discussed above, we use the prediction accuracy metric of max precision given target recall in selecting optimal hyperparameters through cross validation. Our main analysis uses a target recall cutoff of 0.7, but we investigated the implications of varying that cutoff.

Figure D.1 – ROC and Precision-Recall

(A) ROC Curve

(B) Precision-Recall Curve

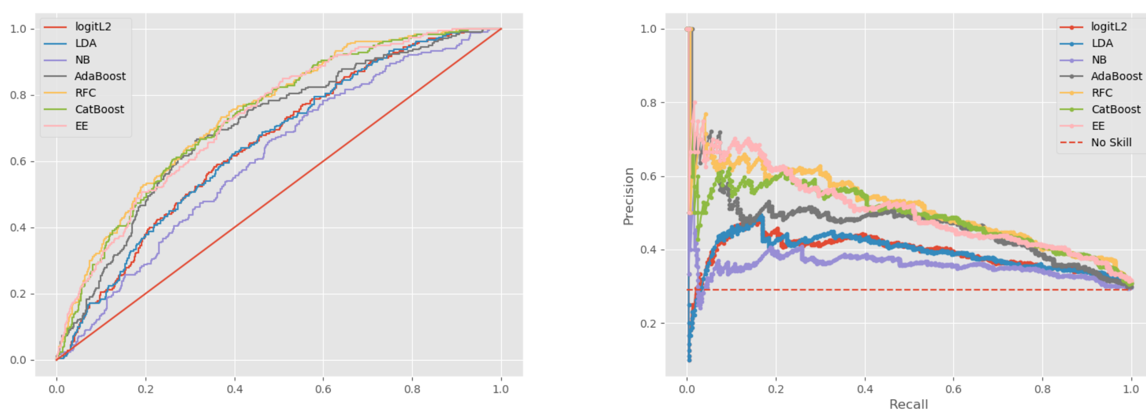
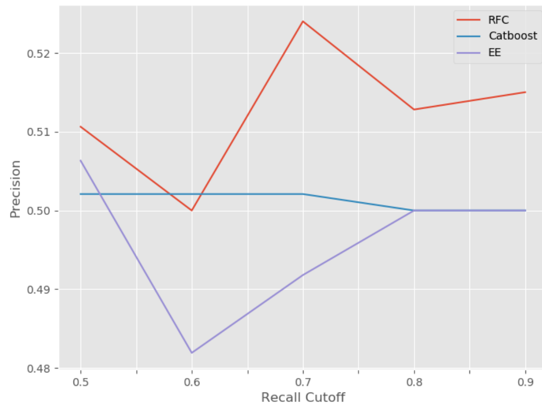


Figure D.2 shows each model's predictive precision in the testing set (out of sample) when using hyperparameters selected subject to different recall cutoff values, ranging from 0.5 to 0.9. Although there appears to be a lot of variation, note that the y-axis scale is quite fine; the precision values are quite similar across models and do not vary much with the recall cutoff. This is consistent with our observation that the optimal hyperparameters do not change substantially when varying the recall cutoff. This suggests that, at least in the 0.5-0.9 range, varying the recall cutoff does not meaningfully affect our findings. Note that because each point on these curves represents a non-parametric model with potentially different hyperparameters, the curves need not be downward sloping.

Figure D.2 – Max Precision Given Target Recall



Notes: For each recall cutoff, we cross validate over the hyperparameter space, and train the model with the optimal hyperparameters, then plot each model's out-of-sample precision in the testing set for the relevant recall cutoff.

Figure D.3 Panel (A) presents confusion matrices showing the relationship between each model's classification and the true labels. The number of true positives is in the lower right, true negatives in the upper left, false positives in the upper right, and false negatives in the lower left. The three models all perform very similarly.

Figure D.3 Panel (B) examines the extent to which the three models select the same petitions. There is very strong agreement across the three models, with particularly small differences between RFC and CatBoost. These results are quite encouraging, as they suggest that the three models will yield similar high-takeup subsets of the data.

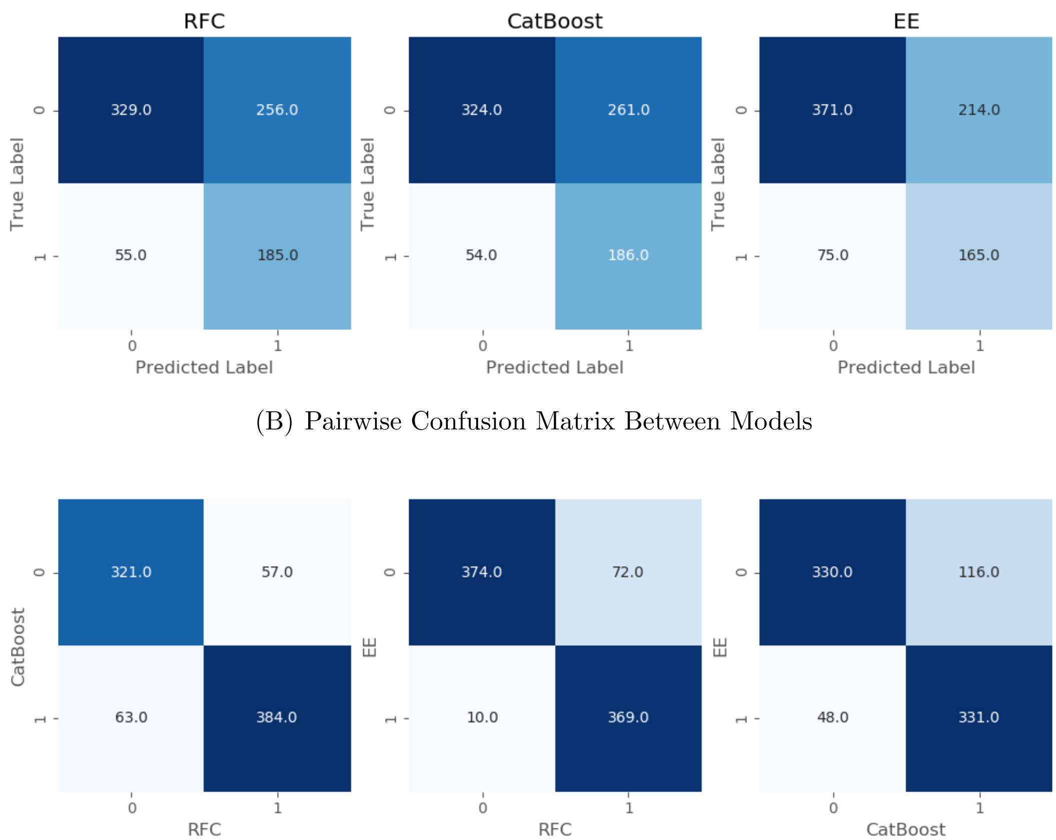
Because our identification comes from differences in eligibility across workers of different ages within a displacing firm, our claims of internal validity will be unaffected by focusing on a subset of high-takeup firms. However, in order to consider external validity questions, it is helpful to report which features are of particular importance in driving high levels of takeup. Figure D.4 Panels (A) and (B) report feature importance for the RFC and CatBoost models (a similar figure is difficult to generate for ensemble models like EE). The values on the x-axis report the impurity-based feature importance, which increases with more nodes splitting based on the relevant feature and larger differences in labels across the two split groups, all else equal. The two models have quite similar rankings across features, with petition state, county population density, the state-level A/RTAA takeup estimate, and 4-digit SIC industry as the top 4 features in both models.

Given the similarity in performance across the three models and the fact that they yield very similar sets of predicted high-takeup petitions, the analysis of the high-takeup sample in the main text simply presents results based on the well-known Random Forest Classifier (RFC).

Figure D.3 – Confusion Matrices

(A) Confusion Matrix Against True Labels

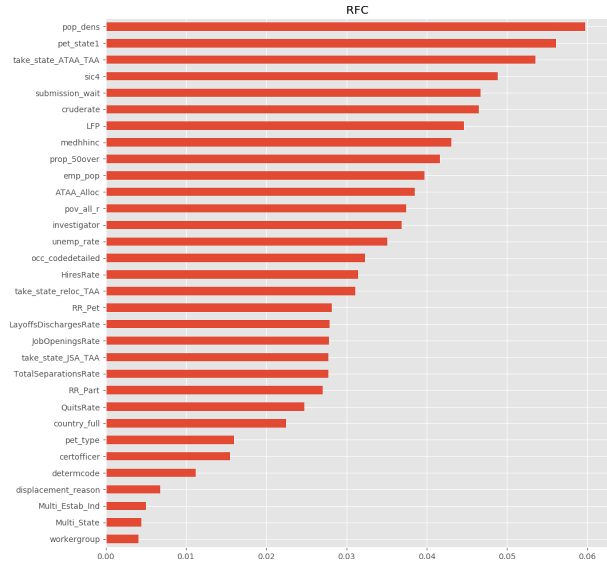
(B) Pairwise Confusion Matrix Between Models



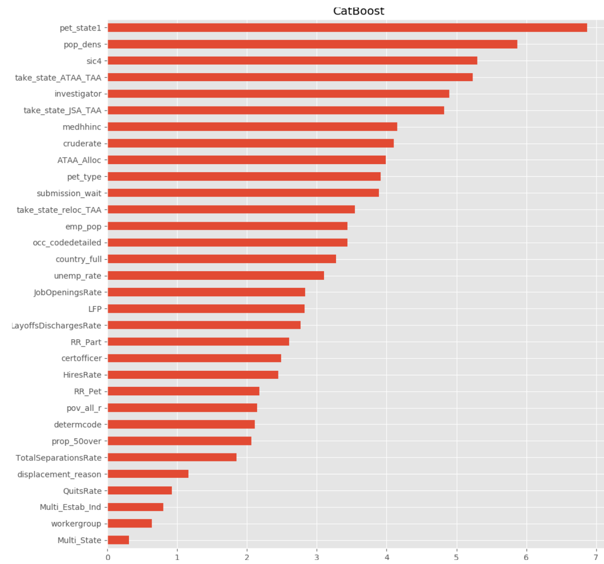
Notes: Panel (A) shows how each model performs against the true labels. The pairwise confusion matrix in Panel (B) demonstrates to what extent each pair of models agrees.

Figure D.4 – Feature Importance

(A) RFC



(B) CatBoost



Notes: We consider the following features in classifying petitions as high- or low-takeup. We include petitions with zero takeup in our model provided they have at least one observed participant in the TAPR. Petitions without any observed participants in TAPR are dropped. For continuous variables, we impute missing values to the mean. For categorical variables, we impute missing values to the most frequent category.

Petition-specific characteristics: *pet_state1*: Primary state for petition; *sic4*: 4-Digit SIC Code (highest granularity); *occ_codedetailed*: Primary Detailed OCC Code (highest granularity); *workergroup*: Whether petition included production workers, service workers, or both; *pet_type*: Whether petition was filed by unions, company, state career centers, or workers; *determcode*: Nature of certification, including direct import competition, shifts in production to other countries, competition in upstream or downstream industries, or partial certification; *displacement_reason*: Reason for displacement in the petition - includes import competition, offshoring/outsourcing, or other; *country_full*: Source country for trade shock that justified certification; *investigator*: Name of DOL officer who conducted investigation into petition; *certofficer*: Name of DOL officer who certified the petition; *Multi_State*: Indicator for whether petition covers multiple states; *Multi_Estab_Ind*: Indicator for whether the petition covers multiple establishments; *submission_wait*: Weeks between submission date and determination date for the petition.

State-level characteristics: *ATAA_Alloc*: ATAA funds allocated to the state in fiscal year of petition determination; *JobOpeningsRate*: Job openings rate in state of petition; *HiresRate*: Hiring rate in state of petition; *QuitsRate*: Quits rate in state of petition; *LayoffsDischargesRate*: Layoff rate in state of petition; *TotalSeparationsRate*: Total separations rate in state of petition; *take_state_ATAA_TAA*: Aggregate ATAA/TAA exiters ratio in state of petition; *take_state_JSA_TAA*: Aggregate Job Search Allowance/TAA exiters ratio in state of petition; *take_state_reloc_TAA*: Aggregate Relocation Allowance/TAA exiters ratio in state of petition; *RR_Part*: Percent of participants with Rapid Response in state of petition, included to account for local awareness of A/RTAA; *RR_Pet*: Percent of petitions with Rapid Response in state of petition, included to account for local awareness of A/RTAA.

County-level characteristics: *unemp_rate*: Unemployment rate in county of petition; *emp_pop*: Employment-population ratio in county of petition; *cruderate*: Deaths per 1,000 people in county of petition, included to account for social welfare factors in local community; *prop_50over*: Proportion of working age population over 50 and under 65 in county of petition, included to account for size of the potentially ATAA-eligible population in the county; *pov_all_r*: Poverty rate in county of petition; *medhhinc*: Median household income in county of petition; *pop_dens*: Population density in county of petition; *LFP*: Labor force participation rate in county of petition.

E Pedagogical Search Model Proofs

Here we provide proofs of the assertions in Section 3 that wage insurance lowers the reservation wage and increases search effort.

Effect of Wage Insurance on the Reservation Wage

To examine the effect of wage insurance on the reservation wage, begin with equation (5) and differentiate with respect to the wage insurance subsidy rate φ, holding λ^* fixed at its optimal value by the envelope theorem. Note that the term associated with the changing lower bound of integration in Leibnitz rule equals zero.

$$(1-\beta)\frac{dV^e(\overline{w})}{d\varphi} = \lambda^*\beta \int_{\overline{w}}^{\infty} \left(\frac{dV^e(w)}{d\varphi} - \frac{dV^e(\overline{w})}{d\varphi}\right) dF(w) \tag{12}$$

$$(1-\beta)\frac{dV^e(\overline{w})}{d\varphi} = \lambda^*\beta \int_{\overline{w}}^{\infty} \frac{dV^e(w)}{d\varphi} dF(w) - \lambda^*\beta(1-F(\overline{w}))\frac{dV^e(\overline{w})}{d\varphi}$$

$$\left[(1-\beta)+\lambda^*\beta(1-F(\overline{w}))\right]\frac{dV^e(\overline{w})}{d\varphi} = \lambda^*\beta \int_{\overline{w}}^{w_0} \frac{dV^e(w)}{d\varphi} dF(w) + \lambda^*\beta \int_{w_0}^{\infty} \frac{dV^e(w)}{d\varphi} dF(w)$$

The derivatives of the value of employment are as follows when $w < w_0$.

$$\frac{dV^e(w)}{d\varphi} = \frac{\partial V^e(w;\varphi)}{\partial \varphi} = \frac{w_0 - w}{1-\beta}$$

$$\frac{dV^e(\overline{w})}{d\varphi} = \frac{\partial V^e(\overline{w};\varphi)}{\partial \varphi} + \frac{\partial V^e(\overline{w};\varphi)}{\partial \overline{w}}\frac{d\overline{w}}{d\varphi} = \frac{w_0 - \overline{w}}{1-\beta} + \left(\frac{1-\varphi}{1-\beta}\right)\frac{d\overline{w}}{d\varphi}$$

When $w \geq w_0$, $dV^e(w)/\varphi = 0$. Plugging these into the above expression yields the following, which implies that $d\overline{w}/d\varphi < 0$.

$$\left[(1-\beta)+\lambda^*\beta(1-F(\overline{w}))\right]\left((w_0-\overline{w}) + (1-\varphi)\frac{d\overline{w}}{d\varphi}\right) = \lambda^*\beta \int_{\overline{w}}^{w_0} (w_0 - w) dF(w).$$

$$\begin{aligned}
\left[(1-\beta)+\lambda^*\beta(1-F(\overline{w}))\right](1-\varphi)\frac{d\overline{w}}{d\varphi} &= \lambda^*\beta \int_{\overline{w}}^{w_0} (w_0 - w) dF(w) - \lambda^*\beta \int_{\overline{w}}^{\infty} (w_0 - \overline{w}) dF(w) - (1-\beta)(w_0-\overline{w}) \\
&= \lambda^*\beta \int_{\overline{w}}^{w_0} \left[(w_0-\overline{w}) + (\overline{w} - w)\right] dF(w) - \lambda^*\beta \int_{\overline{w}}^{\infty} (w_0 - \overline{w}) dF(w) \\
&\quad - (1-\beta)(w_0-\overline{w}) \\
&= -\lambda^*\beta \int_{\overline{w}}^{w_0} (w - \overline{w}) dF(w) - \lambda^*\beta \int_{w_0}^{\infty} (w_0 - \overline{w}) dF(w) - (1-\beta)(w_0-\overline{w}) \\
&= -\lambda^*\beta \int_{\overline{w}}^{w_0} (w - \overline{w}) dF(w) - (w_0-\overline{w})\left[(1-\beta)+\lambda^*\beta(1-F(w_0))\right]
\end{aligned}$$

$$\frac{d\overline{w}}{d\varphi} = -\frac{\lambda^*\beta \int_{\overline{w}}^{w_0} (w-\overline{w}) dF(w) + (w_0-\overline{w})\left[(1-\beta)+\lambda^*\beta(1-F(w_0))\right]}{\left[(1-\beta)+\lambda^*\beta(1-F(\overline{w}))\right](1-\varphi)} < 0$$

Effect of Wage Insurance on Search Effort

Begin by taking the derivative of the first-order condition for search effort in equation (6), noting that the term associated with the changing lower bound of integration in Leibnitz rule equals zero.

$$c''(\lambda^*)\frac{d\lambda^*}{d\varphi} = \underbrace{\beta\int_{\overline{w}}^{\infty}\left(\frac{dV^e(w)}{d\varphi} - \frac{dV^e(\overline{w})}{d\varphi}\right)dF(w)}_{\equiv\mathbb{A}} \tag{13}$$

Note that the right side of this expression appears in equation (12) as well; refer to it as $\mathbb{A}$. Because search effort costs are convex, $c'' > 0$, the sign of $d\lambda^*/d\varphi$ is determined by the sign of $\mathbb{A}$. Going back to (12),

$$\begin{aligned}
\lambda^*\mathbb{A} &= (1-\beta)\frac{dV^e(\overline{w})}{d\varphi} \\
&= (w_0 - \overline{w}) + (1-\varphi)\frac{d\overline{w}}{d\varphi} \\
&= (w_0 - \overline{w}) - \frac{\lambda^*\beta\int_{\overline{w}}^{w_0}(w-\overline{w})dF(w) + (w_0-\overline{w})\left[(1-\beta)+\lambda^*\beta(1-F(w_0))\right]}{(1-\beta)+\lambda^*\beta(1-F(\overline{w}))} \\
&= \frac{(w_0-\overline{w})\left[(1-\beta)+\lambda^*\beta(1-F(\overline{w}))\right] - \lambda^*\beta\int_{\overline{w}}^{w_0}(w-\overline{w})dF(w) - (w_0-\overline{w})\left[(1-\beta)+\lambda^*\beta(1-F(w_0))\right]}{\left[(1-\beta)+\lambda^*\beta(1-F(\overline{w}))\right]}
\end{aligned} \tag{14}$$

Since the denominator of this expression is positive, focus on the numerator

$$\begin{aligned}
\text{numerator} &= (w_0-\overline{w})\lambda^*\beta(F(w_0)-F(\overline{w})) - \lambda^*\beta\int_{\overline{w}}^{w_0}(w-\overline{w})dF(w) \\
&= \lambda^*\beta\left[(w_0-\overline{w})(F(w_0)-F(\overline{w})) - \int_{\overline{w}}^{w_0}(w-\overline{w})dF(w)\right] \\
&= \lambda^*\beta\left[\int_{\overline{w}}^{w_0}(w_0-\overline{w})dF(w) - \int_{\overline{w}}^{w_0}(w-\overline{w})dF(w)\right] \\
&= \lambda^*\beta\int_{\overline{w}}^{w_0}(w_0-w)dF(w) > 0.
\end{aligned}$$

Therefore, the numerator in (14) is positive, which implies that $\mathbb{A} > 0$, which from equation (13) implies that $d\lambda^*/d\varphi > 0$, i.e. search effort increases with wage insurance.

F Quantitative Analysis Details

This appendix presents details of the model and quantification exercises outlined in Section 8. We first describe the model's structure and calibration. We then describe the solution methods and quantitative results.

F.1 Model Setup

We model the search behavior of an individual risk-neutral worker who is displaced at $t = 0$ (time is discrete) from a job earning a wage w_0. The worker is forward looking with a discount factor $\beta \in (0, 1)$. When non-employed, the worker receives a payment b_t, which may depend upon the non-employment duration t. In each period, the worker expends a search effort λ, which comes at a convex cost $c(\lambda)$ where $c(\lambda), c'(\lambda), c''(\lambda) > 0 \ \forall \lambda \in [0, 1]$. The search effort is scaled such that λ is the probability of receiving an offer. Wage offers are drawn from an exogenous distribution $F_t(w)$, which may change over the course of the non-employment spell to reflect duration dependence in wage offers. For simplicity, there is no on-the-job search and employment is an absorbing state, such that an accepted job offer is maintained in perpetuity. Given risk neutrality, we also rule out saving and borrowing, so consumption equals income in each period.

Wage insurance-eligible workers who find reemployment at a wage below w_0 receive a subsidy covering a fraction $\varphi \in (0, 1)$ of the difference between their old and new wage for a finite number of periods T_{WI}. The subsidy-inclusive wage $\widetilde{w}(w)$ is then

$$\widetilde{w}(w) = \begin{cases} w + \varphi(w_0 - w) & \text{if } w < w_0 \text{ and } t \leq T_{\text{WI}} \\ w & \text{if } w \geq w_0 \text{ or } t > T_{\text{WI}} \end{cases} \tag{F.1}$$

For a worker without wage insurance eligibility, $\varphi = 0$. We do not incorporate wage-insurance benefit or eligibility caps in the model because the vast majority of participants do not hit these caps (see footnote 22). Below, we will introduce income and payroll taxes, but we omit them from the derivations for clarity. We assume there exists a stationary period $t = T$ such that $T_{\text{WI}} < T$, $b_t = b_{t+1} \ \forall t \geq T$, and $F_t(w) = F_{t+1}(w) \ \forall t \geq T$.

F.2 Optimal Search Behavior

Value of Employment: In period t, the indirect utility of employment at wage w is

$$V_t^e(w) = \widetilde{w}(w) + \beta V_{t+1}^e(w). \tag{F.2}$$

Since employment is an absorbing state and there is no on-the-job search, the value of employment at a given wage is deterministic, so there is no expectation in the continuation value. If $w \geq w_0$, then the worker receives no subsidy and earns w in all periods, so the setting is stationary and $V_t^e(w) = V_{t+1}^e(w) \ \forall t$. If $t > T_{\text{WI}}$, then the benefit period has expired, the worker receives no subsidy, and again the setting is stationary. Therefore,

$$V_t^e(w) = \frac{w}{1 - \beta} \quad \text{if } w \geq w_0 \text{ or } t > T_{\text{WI}}. \tag{F.3}$$

Given $V_{t+1}^e(w)$, we can use (F.2) to solve for $V_t^e(w)$ by backward induction. Doing so for $w < w_0$ and $t \leq T_{\text{WI}}$ yields the following expression.

$$(1-\beta)V_t^e(w) = \eta_t w_0 + (1-\eta_t)w \qquad \text{if } w < w_0 \text{ and } t \leq T_{\text{WI}} \tag{F.4}$$

$$\text{where} \quad \eta_t \equiv \varphi(1-\beta^{T_{\text{WI}}+1-t})$$

This expression shows that, for a forward-looking worker, the value of employment with a subsidized wage falls toward the value of the unsubsidized wage as the wage-insurance benefit expiry date approaches. When the benefit expiry date is far in the future, $\eta_t \approx \varphi$ and the value of employment at w approximately equals the discounted value of the subsidized wage $\widetilde{w} = \varphi w_0 + (1-\varphi)w$. As the benefit expiry date approaches (i.e. as t approaches $T_{\text{WI}}+1$), η_t approaches zero, and the value of employment at w approaches the discounted value of the unsubsidized wage w. Combining the preceding results yields a closed-form expression for $V_t^e(w)$:

$$V_t^e(w) = \begin{cases} \frac{w}{1-\beta} & \text{if } w \geq w_0 \text{ or } t > T_{\text{WI}} \\ \frac{\eta_t w_0 + (1-\eta_t)w}{1-\beta} & \text{if } w < w_0 \text{ and } t \leq T_{\text{WI}} \end{cases} \tag{F.5}$$

Value of Unemployment and Optimal Reservation Wage: The indirect utility of unemployment in period t is

$$\begin{aligned} V_t^u &= b_t + \max_{\lambda_t}\left[-c(\lambda_t) + (1-\lambda_t)\beta V_{t+1}^u + \lambda_t\beta\int_0^\infty \max\{V_{t+1}^e(w), V_{t+1}^u\}dF_t(w)\right] \\ &= b_t + \max_{\lambda_t}\left[-c(\lambda_t) + \beta V_{t+1}^u + \lambda_t\beta\int_{\overline{w}_t}^\infty \left(V_{t+1}^e(w) - V_{t+1}^u\right)dF_t(w)\right] \end{aligned} \tag{F.6}$$

where $\overline{w}_t$ is the reservation wage, such that $V_{t+1}^u = V_{t+1}^e(\overline{w}_t)$. We can rewrite this expression in terms of the known function V_t^e by plugging in the optimal search effort λ_t^* and using $V_{t+1}^u = V_{t+1}^e(\overline{w}_t)$.

$$V_t^e(\overline{w}_{t-1}) = b_t - c(\lambda_t^*) + \beta V_{t+1}^e(\overline{w}_t) + \lambda_t^*\beta\int_{\overline{w}_t}^\infty \left(V_{t+1}^e(w) - V_{t+1}^e(\overline{w}_t)\right)dF_t(w) \tag{F.7}$$

Given values for $\overline{w}_t$ and λ_t^*, this expression implicitly defines $\overline{w}_{t-1}$ in terms of known functions.

Optimal Search Effort: From (F.6), we can write the first order condition for optimal search effort in t.

$$c'(\lambda_t^*) = \beta\int_{\overline{w}_t}^\infty \left(V_{t+1}^e(w) - V_{t+1}^u\right)dF_t(w) = \beta\int_{\overline{w}_t}^\infty \left(V_{t+1}^e(w) - V_{t+1}^e(\overline{w}_t)\right)dF_t(w) \tag{F.8}$$

This expression implicitly defines the optimal search effort in t, λ_t^*, given the same period's reservation wage, $\overline{w}_t$.

Integral Terms: Both equations (F.7) and (F.8) include the integral term

$$h_t(\overline{w}_t) \equiv \int_{\overline{w}_t}^{\infty} \left(V^e_{t+1}(w) - V^e_{t+1}(\overline{w}_t)\right) dF_t(w). \tag{F.9}$$

Given the parameterization discussed in the next section, we can solve this integral in closed form using properties of the lognormal distribution. Appendix Section F.7 shows the solution, which allows us to operationalize the preceding expressions.

Stationary Search Behavior and Backward Induction For $t \geq T$, the setting is stationary and the value functions and worker search behavior are constant over time. This implies that $V^u_T = V^u_{T+1}$ and $V^e_T(w) = V^e_{T+1}(w)$, which in turn imply that $V^e_T(\overline{w}_{T-1}) = V^e_{T+1}(\overline{w}_T) = V^e_T(\overline{w}_T)$. Therefore (F.7) for $t = T$ is

$$V^e_T(\overline{w}_T) = b_T - c(\lambda^*_T) + \beta V^e_{T+1}(\overline{w}_T) + \lambda^*_T \beta h_T(\overline{w}_T). \tag{F.10}$$

Equation (F.8) for period T is

$$c'(\lambda^*_T) = \beta h_T(\overline{w}_T). \tag{F.11}$$

Together (F.10) and (F.11), jointly define the optimal search behavior in the stationary period. Given $\overline{w}_T$ and λ^*_T, equation (F.7) yields $\overline{w}_{T-1}$ and equation (F.8) then yields λ^*_{T-1}. Backward induction can then proceed to $t = 1$, yielding the profile of optimal search behavior over time.

F.3 Parameterization

We assume the wage offer distribution $F_t(w)$ is lognormal, with location parameter μ_t and dispersion parameter σ. We allow μ_t to fall over the course of the unemployment spell to capture the possibility of negative duration dependence in wage offers. Following Schmieder et al. (2016), we assume that μ_t falls by a fixed amount $\delta > 0$ in each period $t < T$, after which it remains constant, i.e.

$$\mu_t = \begin{cases} \mu_1 - (t-1)\delta & 1 \leq t \leq T \\ \mu_1 - (T-1)\delta & t > T. \end{cases}$$

Note that the evolution of μ_t is entirely determined by the initial location parameter value μ_1, the duration dependence parameter δ, and the stationary time period T. The lognormal assumption allows us to solve the integral term $h_t(\overline{w})$ in equation (F.9) in closed form. See Appendix Section F.7 for the relevant derivations.

We assume the unemployment payment takes on a value b_{UI} representing UI payments (and/or TRA payments under TAA) during the first $T_{\text{UI}} < T$ periods and a lower value $b < b_{\text{UI}}$ thereafter:

$$b_t = \begin{cases} b_{\text{UI}} & t \leq T_{\text{UI}} \\ b & t > T_{\text{UI}}. \end{cases}$$

The cost of search effort takes the following form:

$$c(\lambda_t) \equiv k \cdot \frac{\lambda_t^{1+\gamma}}{1+\gamma},$$

where $k, \gamma > 0$, k sets the level of search costs, and γ is the effort elasticity of marginal cost.

F.4 Simulation

For a given set of parameters, we solve for the worker's optimal search behavior by solving for the reservation wage and optimal search effort in the stationary period and then using backward induction to solve for optimal search behavior in preceding periods, as described in Appendix Section F.2. Given that optimal search behavior, we use the model to calculate expected wages and non-employment duration as follows.

Define emp_t as an indicator equal to 1 if employed in period t and zero otherwise. Note that all time periods referenced here are shifted by one quarter relative to the "Quarter Relative to Separation" in Figure 6 and related figures. Here, the full quarter of non-employment following displacement is $t = 0$ rather than $t = 1$.

The probability of becoming employed in period t conditional on not being employed previously is $\lambda_t(1 - F_t(\overline{w}_t))$, and the expected wage for those newly employed in period t is

$$w_t^e \equiv \frac{\int_{\overline{w}_t}^{\infty} w dF_t(w)}{1 - F_t(\overline{w}_t)}.$$

The expected non-employment duration is then

$$\sum_{t=1}^{16} \Pr(\text{emp}_t = 1 | \text{emp}_{t-1} = 0) \cdot \Pr(\text{emp}_{t-1} = 0) \cdot t.$$

Because everyone is non-employed leaving $t = 0$, $\Pr(\text{emp}_0) = 0$. We also construct our non-employment duration data as if everyone leaving $t = 15$ non-employed is reemployed in $t = 16$, so $\Pr(\text{emp}_{16} = 1 | \text{emp}_{15} = 0) = 1$. Given these restrictions, the expected non-employment duration is

$$\lambda_1(1 - F_1(\overline{w}_1)) \cdot 1 \cdot 1 + \sum_{t=2}^{15} \lambda_t(1 - F_t(\overline{w}_t)) \cdot \prod_{\tau=1}^{t-1} (1 - \lambda_\tau(1 - F_\tau(\overline{w}_\tau))) \cdot t \tag{F.12}$$

$$+ 1 \cdot \prod_{\tau=1}^{15} (1 - \lambda_\tau(1 - F_\tau(\overline{w}_\tau))) \cdot 16. \tag{F.13}$$

The expected reemployment wage among those who are reemployed is then

$$\frac{\sum_{t=1}^{16} \Pr(\text{emp}_t = 1 | \text{emp}_t = 0) \cdot \Pr(\text{emp}_{t-1} = 0) \cdot w_t^e}{\sum_{t=1}^{16} \Pr(\text{emp}_t = 1 | \text{emp}_t = 0) \cdot \Pr(\text{emp}_{t-1} = 0)}.$$

As in the prior expression, $\Pr(\text{emp}_0) = 0$. In contrast, we cannot assign a wage to those leaving $t = 15$ without employment, so we need to normalize by the share of people ever reemployed. The expected reemployment wage is then

$$\frac{\lambda_1(1-F_1(\overline{w}_1)) \cdot 1 \cdot w_1^e + \sum_{t=2}^{16} \lambda_t(1-F_t(\overline{w}_t)) \cdot \prod_{\tau=1}^{t-1}(1-\lambda_\tau(1-F_\tau(\overline{w}_\tau))) \cdot w_t^e}{\lambda_1(1-F_1(\overline{w}_1)) \cdot 1 + \sum_{t=2}^{16} \lambda_t(1-F_t(\overline{w}_t)) \cdot \prod_{\tau=1}^{t-1}(1-\lambda_\tau(1-F_\tau(\overline{w}_\tau)))}. \tag{F.14}$$

For a given set of parameters, we simulate the expected non-employment duration in (F.12) and the expected reemployment earnings in (F.14), first assuming the worker is eligible for wage insurance and then assuming they are ineligible. This allows us to observe the non-employment duration and reemployment earnings for ineligible workers and to calculate the effect of wage insurance eligibility on both of these outcomes. To maintain realism in comparing these simulated effects against those we estimate in Table 2, we account for the fact that many workers were unaware of the RTAA wage insurance program during our sample period by assuming that 53 percent of workers are aware of the wage insurance program, following survey evidence on TAA-eligible workers from Dolfin and Berk (2010). This assumption reduces the magnitude of the effect of wage eligibility by 47 percent, presuming that a worker who is eligible for wage insurance but unaware of it does not change their search behavior or outcomes.

F.5 Calibration

When possible we externally calibrate parameters based on the policy environment or applicable values from the literature. The exceptions are the parameters of the wage offer distribution, μ ($\equiv \mu_1$) and σ, and the cost function level parameter k. As discussed below, we choose these parameters to target empirical moments via minimum distance. To match the granularity of our data, we define a time period as a calendar quarter.

The worker has a quarterly discount factor of $\beta = 0.9879$, which corresponds to a yearly discount factor of 0.95. The pre-displacement wage $w_0 = \$11,908$, which is the observed mean quarterly earnings 8 to 5 quarters prior to displacement in the TAA-certified sample (Appendix Table C.1; $\$11,908 = \$47,630/4$). We set the quarterly UI payment $b_{\text{UI}} = \$5,031$ based on averages reported in Kovalski and Sheiner (2020). Since the worker is risk-neutral and does not save, we set $b = 0.88 \cdot b_{\text{UI}}$ to match the decline in consumption at the exhaustion of UI benefits estimated in Ganong and Noel (2019). To capture the extended unemployment insurance payments under TAA, b_{UI} is available for $T_{\text{UI}} = 10$ quarters. The worker is eligible for wage insurance for $T_{\text{WI}} = 10$ quarters at a subsidy rate $\varphi = 0.5$. Because we observe 16 quarters of post-displacement outcomes, we set the stationary period to $T = 16$.

Although we omitted the relevant terms from the derivations above for clarity, it is straightforward to incorporate relevant taxes. The worker pays an income tax rate τ^{INC} on earnings, subsidy payments, and UI benefits and pays an additional payroll tax rate τ^{PAY} on earnings. Based on NBER TAXSIM, we set $\tau^{\text{INC}} = 0.193$ as the sum of 15% federal income tax and 4.3% state income tax, where the latter is calculated as the mean state income tax for a worker with income w_0 in our sample of LEHD states (see footnote 27). We set $\tau^{\text{PAY}} = 0.0765$ to correspond to the employee's share of payroll taxes. One might argue for a lower payroll tax rate because Social Security benefits are linked to taxes. However, the link is much less than one to one, even at this income level (Burkhalter and Chaplain 2023). As a robustness test, we set payroll taxes to zero and find the results are little changed.

We choose the cost function curvature parameter γ to match the estimate from DellaVigna et al. (2017). We take the estimates from their model with geometric time discounting and a single worker type (single cost function level parameter) in their Table 1 column (2), as this model most closely matches our setting. Because their estimates use 15-day bins rather than quarters, we must aggregate their estimates to the quarterly level. We first derive the cost of achieving a given offer probability at the quarterly level given the 15-day cost function estimates $\hat{\gamma}$ and $\hat{k}$,

$$c(\lambda_t) = \rho \hat{k} \frac{\left[1 - (1 - \lambda_t)^{1/\rho}\right]^{1+\hat{\gamma}}}{1 + \hat{\gamma}},$$

where $\rho = 6.09$ is the number of 15-day bins per quarter. We then plot this aggregated cost across the range of values of λ_t and then fit our quarterly cost function in (F.12) using non-linear least squares. This process yields an estimate of $\gamma = 3.974$.

Given the well-known difficulty in estimating duration dependence separately from differential selection by unemployment duration, we calibrate δ using a value from the literature and consider wide-ranging alternative values for robustness. Our preferred value $\delta^{mid} = 0.024$ is based on the estimate of Schmieder et al. (2016) in their Table 4 column (1) after converting their estimates from monthly to quarterly frequency ($0.0240 = 0.008 \cdot 3$). For robustness, we also consider values that double ($\delta^{high} = 0.048$) and halve ($\delta^{low} = 0.012$) this baseline value.

The remaining parameters, μ ($\equiv$ μ_1), σ, and k are internally calibrated using minimum distance. Specifically, we choose these parameter values by matching the following four moments predicted by the model to their empirical moments from Table 2: (1) the non-employment duration without wage insurance; (2) the treatment effect of WI on non-employment duration; (3) re-employment earnings without wage insurance; (4) the treatment effect of WI on re-employment earnings. Specifically, we use the control means shown in Table 2 for moments (1) and (3) that correspond to wage insurance-ineligible workers' outcomes and the D-RD point estimates for moments (2) and (4) that correspond to the treatment effects.

We choose parameters that minimize the sum of squared errors across the four moments, weighting by the inverse standard error of the empirical estimate associated with each moment in Table 2. For each model, we use 100 starting values and select the parameters that yield the the minimum objective criterion. In practice, the parameters nearly always converge to the same values regardless of the starting values and produce the same objective criterion. The parameter values also lie in the interior of the choice set over which the optimizer is permitted to search.

F.6 Results

The chosen parameters appear in Appendix Table F.1 and the associated simulated outcomes are reported in Appendix Table F.2. In both cases, we present a different row for each model using a different value of the duration dependence parameter δ. The results demonstrate that the simulated outcomes are not particularly sensitive to the duration dependence parameter.

Table F.1 – Internally Calibrated Parameter Values

δ	μ	σ	k
$\delta^{low} = 0.012$	8.812	0.131	8,128
$\delta^{mid} = 0.024$	8.736	0.200	13,052
$\delta^{high} = 0.048$	8.690	0.258	21,731

Table F.2 – Empirical Estimates and Simulated Outcomes

	WI-ineligible		Effect of WI	
	non-employment duration	reemployment earnings	non-employment duration	reemployment earnings
Empirical estimates:	5.939	7,929	-1.002	338.2
Model outcomes:				
$\delta^{low} = 0.012$	5.946	7,850	-0.584	-31.86
$\delta^{mid} = 0.024$	5.947	7,845	-0.463	-40.78
$\delta^{high} = 0.048$	5.945	7,813	-0.464	-61.25

Notes: In the main text we compare data and model moments using a widely cited measure of duration dependence corresponding to δ^{mid}; here, we show robustness of our matches across various choices of δ.

Wage Insurance and Duration Dependence

In this section, we show that the quantitative effects of wage insurance depend upon the presence of negative duration dependence in wage offers. To do so, we take the baseline parameters in Table 3 and turn off duration dependence by setting $\delta = 0$. We then simulate worker search behavior and expected outcomes. The results appear in the second row of Table F.3, with the first row replicating the simulated outcomes from the baseline model (Table F.2, δ^{mid}) for comparison.

Table F.3 – Simulated Outcomes from Relaxing Duration Dependence

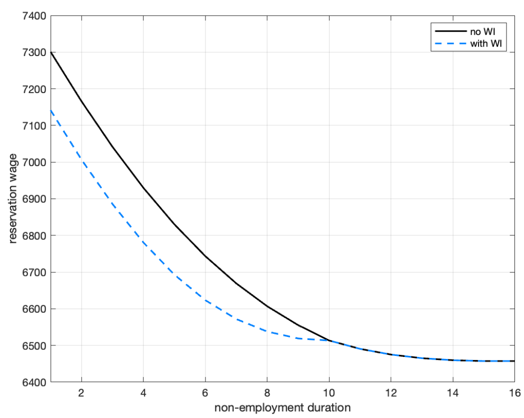
	WI-ineligible		Effect of WI	
Model	non-employment duration	reemployment earnings	non-employment duration	reemployment earnings
$\delta^{mid} = 0.024$	5.947	7,845	-0.463	-40.78
$\delta = 0$	11.76	9,255	-0.275	-36.43

Because wage offers remain much more favorable without negative duration dependence than with it, the average non-employment duration and reemployment earnings are substantially higher. In contrast, the effect of wage insurance eligibility on non-employment duration is substantially smaller without duration dependence; the magnitude falls by 40.6 percent compared to the baseline model. The effect on reemployment earnings also falls in magnitude by 10.7 percent, but the effect was already very small compared to the level for ineligible workers.

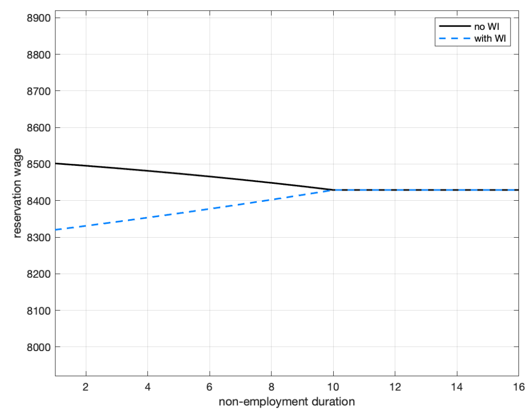
To help understand why duration dependence moderates the effect of wage insurance eligibility, Appendix Figure F.1 plots the optimal search behavior over time for workers with and without wage insurance and for settings with and without duration dependence. Panels (A) and (B) show the evolution of the reservation wage with baseline duration dependence and without duration dependence, respectively. Panels (C) and (D) plot the optimal search effort. For both pairs of figures, the y-axis is set to the same scale, so differences are comparable. The black solid lines show the search behavior for a worker without wage insurance eligibility and the dashed blue lines are for an eligible worker.

Figure F.1 – Search Behavior With and Without Duration Dependence

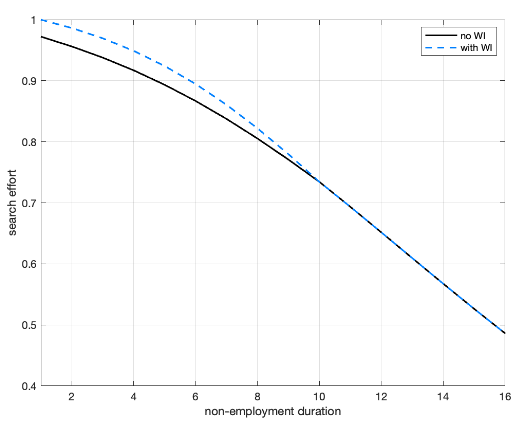
(A) Res. Wage with Duration Dependence



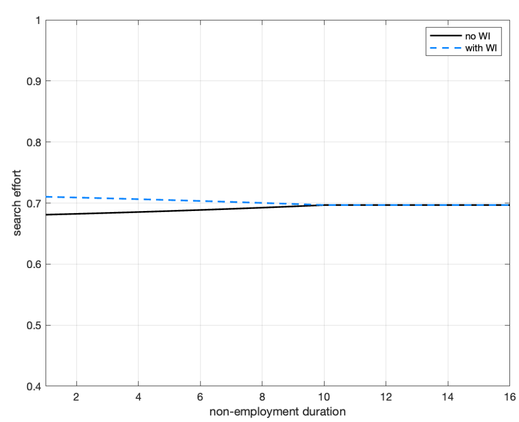
(B) Res. Wage without Duration Dependence



(C) Search Effort with Duration Dependence



(D) Search Effort without Duration Dependence



In the presence of negative duration dependence, the expected wage draw falls over time, so both the reservation wage and optimal search effort profiles are decreasing. For both values of duration dependence, wage insurance lowers the reservation wage during the subsidy eligibility period. Similarly, wage insurance increases search effort during the eligibility period. In both cases, the effect of wage insurance is largest when there are many periods of eligibility remaining and smaller as the eligibility window closes.

Wage insurance distorts search behavior more in the presence of negative duration dependence because the subsidy amount increases when reemployment wages are lower. This increase partly counteracts the incentive to reduce search effort over time as wage offers deteriorate, leading to larger reductions in non-employment duration. Together, these results suggest that wage insurance is likely to be particularly effective in settings with negative duration dependence.

Robustness to Outside Option

In Appendix Table F.4, we hold fixed the calibrated parameters for each varint of the model, with the exception of the maximum unemployment insurance duration, T_{WI}. Rather than the baseline value of 10 quarters, which reflects the extended benefits available through TAA, we set $T_{\text{WI}} = 2$ to reflect the 26-week benefit eligibility under standard unemployment insurance. As discussed in the main text, in all cases, the effect of wage insurance eligibility on the non-employment duration is smaller when the UI benefit duration is shorter. However, these differences are very small, with effects ranging from 3.7 to 6.7 percent smaller in magnitude across the models. The similarity of the simulated treatment effects suggests that our empirical results are not driven by the particularly generous outside option facing TAA-eligibile workers.

Table F.4 – Simulated Outcomes for 2 Quarters of UI

Model	WI-ineligible non-employment duration	WI-ineligible reemployment earnings	Effect of WI non-employment duration	Effect of WI reemployment earnings
$\delta^{low} = 0.012$	5.602	7,835	-0.545	-33.79
$\delta^{mid} = 0.024$	5.695	7,826	-0.444	-41.96
$\delta^{high} = 0.048$	5.712	7,786	-0.447	-61.31

F.7 Closed-Form Integrals

Consider the integral $h_t(\overline{w}_t)$ defined in (F.9) and use properties of the lognormal distribution to solve the relevant integral. There are two cases to consider, where $\overline{w}_t < w_0$ and $t+1 \leq T_{\text{WI}}$ and where $\overline{w}_t \geq w_0$ or $t+1 > T_{\text{WI}}$.

Case 1: $\overline{w}_t < w_0$ and $t+1 \leq T_{\text{WI}}$: Employment with wage $\overline{w}_t$ will be subsidy-eligible in period $t+1$, so $(1-\beta)V^e_{t+1}(\overline{w}_t) = \eta_{t+1}w_0 + (1-\eta_{t+1})\overline{w}_t$. The wage draw w is only subsidized if it is below w_0. Therefore,

$$\begin{aligned}(1-\beta)h_t(\overline{w}_t) &= (1-\eta_{t+1})\int_{\overline{w}_t}^{w_0}(w-\overline{w}_t)dF_t(w) + \int_{w_0}^{\infty}\left[w-(\eta_{t+1}w_0+(1-\eta_{t+1})\overline{w}_t)\right]dF_t(w)\\ &= (1-\eta_{t+1})\int_{\overline{w}_t}^{w_0} wdF_t(w) - (1-\eta_{t+1})\overline{w}_t(F_t(w_0)-F_t(\overline{w}_t))\\ &\quad + \int_{w_0}^{\infty} wdF_t(w) - \left[\eta_{t+1}w_0 + (1-\eta_{t+1})\overline{w}_t\right](1-F_t(w_0))\end{aligned}$$

The remaining integrals in this expression are simple truncated lognormals. We show the derivations for closed-form values in (F.15) after presenting the second case.

Case 2: $\overline{w}_t \geq w_0$ **or** $t+1 > T_{\mathrm{WI}}$: In this case, $(1-\beta)V^e_{t+1}(\overline{w}_t) = \overline{w}_t$. As in the prior case, the wage draw w is only subsidized if it is below w_0, but these draws are rejected because they are less than $\overline{w}_t$, so no relevant wage draws are subsidized. Therefore,

$$\begin{aligned}(1-\beta)h_t(\overline{w}_t) &= \int_{\overline{w}_t}^{\infty}(w-\overline{w}_t)dF_t(w)\\ &= \int_{\overline{w}_t}^{\infty} wdF_t(w) - \overline{w}_t(1-F_t(\overline{w}_t)).\end{aligned}$$

Combining the preceding two cases yields a piecewise definition of $h_t(\overline{w}_t)$.

Truncated Lognormal Solution: The object of interest is

$$\int_{a_1}^{a_2} wdF(w)$$

where $F(w)$ is a lognormal distribution with parameters μ and σ. By the definition of the lognormal distribution

$$\int_{a_1}^{a_2} wdF(w) = \int_{a_1}^{a_2} w\frac{1}{w\sigma\sqrt{2\pi}}\exp\left(-\frac{(\ln w-\mu)^2}{2\sigma^2}\right)dw.$$

Simplify and implement a change of variables such that $x=\ln w$, $e^x = w$, and $dw = e^x dx$.

$$\begin{aligned}\int_{a_1}^{a_2} wdF(w) &= \int_{\ln a_1}^{\ln a_2}\frac{1}{\sigma\sqrt{2\pi}}\exp\left(-\frac{1}{2}\left(\frac{x-\mu}{\sigma}\right)^2\right)e^x dx\\ &= \int_{\ln a_1}^{\ln a_2}\frac{1}{\sigma\sqrt{2\pi}}\exp\left(-\frac{1}{2}\left(\frac{x-\mu}{\sigma}\right)^2 + x\right)dx\end{aligned}$$

Manipulating the term inside the exponential yields:

$$\begin{aligned}
-\frac{1}{2}\left(\frac{x-\mu}{\sigma}\right)^2 + x &= \frac{2\sigma^2 x - (x^2 - 2x\mu + \mu^2)}{2\sigma^2} \\
&= \frac{-x^2 + 2x(\mu+\sigma^2) - \mu^2}{2\sigma^2} \\
&= \frac{-x^2 + 2x(\mu+\sigma^2) - (\mu+\sigma^2)^2 - \mu^2 + (\mu+\sigma^2)^2}{2\sigma^2} \\
&= -\frac{\left(x-(\mu+\sigma^2)\right)^2}{2\sigma^2} + \frac{-\mu^2 + \left(\mu^2 + 2\mu\sigma^2 + (\sigma^2)^2\right)}{2\sigma^2} \\
&= -\frac{\left(x-(\mu+\sigma^2)\right)^2}{2\sigma^2} + \mu + \frac{\sigma^2}{2}
\end{aligned}$$

Plug this in above and let $\mu^* \equiv \mu + \sigma^2$ to yield the value of the integral in terms of the normal CDF with mean $\mu^*$and standard deviation σ.

$$\begin{aligned}
\int_{a_1}^{a_2} w dF(w) = \int_{\ln a_1}^{\ln a_2} \frac{1}{\sigma\sqrt{2\pi}} \exp\left(-\frac{(x-\mu^*)^2}{2\sigma^2} + \mu + \frac{\sigma^2}{2}\right) dx \\
\exp\left(\mu + \frac{\sigma^2}{2}\right) \int_{\ln a_1}^{\ln a_2} \frac{1}{\sigma\sqrt{2\pi}} \exp\left(-\frac{1}{2}\left(\frac{x-\mu^*}{\sigma}\right)^2\right) dx \\
\exp\left(\mu + \frac{\sigma^2}{2}\right) \left[\Phi_{\mu^*,\sigma}(\ln a_2) - \Phi_{\mu^*,\sigma}(\ln a_1)\right] \qquad \text{(F.15)}
\end{aligned}$$