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

September 15, 2023

Abstract

Wage insurance provides income support to displaced workers who find reemployment at a lower wage. We study 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. The program includes an age-based eligibility cutoff, allowing us to use a regression discontinuity design to estimate earnings and employment outcomes for workers whose age at separation make them eligible or ineligible for the program. We find that wage insurance eligibility increases short-run employment probabilities and leads to higher cumulative earnings in the long run. Using a search model and earnings decomposition to clarify which mechanisms underlie these results, we find that shorter unemployment durations largely drive increased long-term earnings among workers eligible for wage insurance. The net costs to the government are negative even under conservative assumptions.

*We thank Theodore Naff and Kelsey Pukelis for outstanding research assistance. We thank Richard Audoly, Hilary Hoynes, Felix Koenig, Pawel Krolikowski, Dorothy Kronick, David Molitor, Jesse Rothstein, Johannes Schmieder, Lowell Taylor, and seminar participants at Boston University, Carnegie Mellon University, the Federal Reserve Bank of New York, University of Georgia, University of Nottingham, Penn State, Princeton NLS-E, UC-Berkeley, 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 also acknowledges support from the UVA Bankard Fund for Political Economy. 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, or the Federal Reserve System. Any views expressed are those of the authors and not those of 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).

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 unemployment (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 US 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.

Our regression discontinuity 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. 2019; 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 merge administrative data on TAA petitions with the US 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. 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 over 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 $83,850 among marginally ineligible workers. These earnings effects are largely driven by shorter unemployment durations among wage insurance eligible workers; wage insurance eligibility reduces the initial unemployment spell duration by approximately 1 calendar quarter and reduces the

[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 of relaxed eligibility for disability insurance at age 55 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, as in Jones (2015). See Section 5 for details.

total time out of employment across all non-employment spells by 1.26 quarters over 4 years.

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. 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 for workers 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 unanticipated 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.

We interpret our empirical findings through the lens of a standard partial equilibrium search model with endogenous search effort. In this model, workers receive a wage insurance subsidy if they obtain reemployment at a wage below their pre-displacement wage. This 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 unemployment durations, helping workers avoid the potentially negative effects of duration-dependent wage offers.

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

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 only two prior evaluations of wage 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

[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 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."

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 inconsistent estimates of the program's impact on employment outcomes.

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.[11] 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.[12]

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

[11] 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.

[12] In Appendix F, we compare the effects of wage insurance to those of a fixed subsidy payment with the same expected cost, finding that wage insurance drives a larger increase in employment probability. Also, 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.

might be less effective and more socially costly (US Trade Deficit Review Commission 2000). Workers eligible for wage insurance under TAA are heavily concentrated in locations facing large trade shocks (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. We present a standard partial equilibrium random search model that incorporates wage insurance in Section 3. 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 examine mechanisms in Section 7, and evaluate benefits and costs in Section 8 based on the marginal value of public funds. Section 9 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 US 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."[13] 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.[14] 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 US; (3) being an

[13] See https://www.dol.gov/general/topic/training/tradeact (accessed March 27, 2023) for details.
[14] See Appendix A for a discussion of additional benefits available under TAA.

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% of petitions have been certified, with higher rates in more recent years.[15] 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.

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.[16]

2.2 Wage Insurance

The wage insurance 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.[17]

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

[16] See footnote 9.

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

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.[18]

The benefit eligibility period lasts for two years, starting with the earlier of either reemployment or the exhaustion of state-funded 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 state-funded UI has a shorter benefit eligibility window.[19] 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 accommodates this rule using a one-sided "donut RD" approach or a regression-kink design.

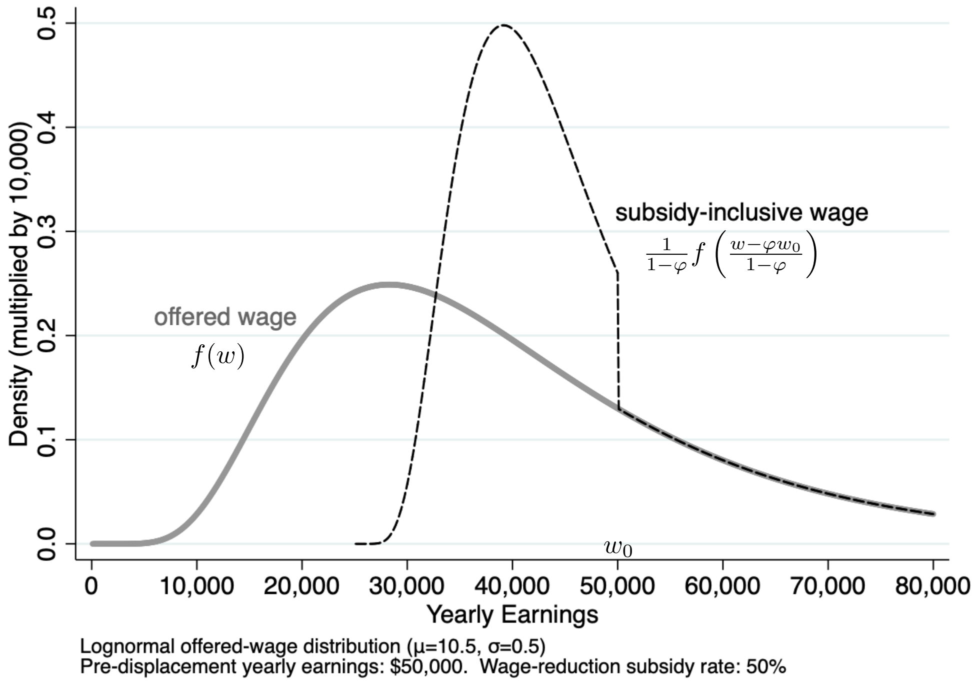
As an example, consider a worker initially earning $50,000 per year and who finds reemployment in a job paying $40,000 per year. The yearly wage subsidy represents half of the gap between the old and new earnings, i.e. $5,000 per year. Figure 1 shows how this benefit structure affects the worker's perception of potential wage offers, ignoring benefit caps for simplicity (Section 3 defines the notation in Figure 1 and derives the subsidy-inclusive wage distribution).[20]

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

[19] 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).

[20] 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.

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



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 unemployment durations.

Note that Figure 1 assumes the wages offered by firms are not affected by the presence 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 9.

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 Search Model

To help gain intuition for how wage insurance eligibility affects an unemployed worker's incentives and search behavior, this section introduces wage insurance into a standard partial-equilibrium job search model (McCall 1970) with endogenous search effort. Our goal is to characterize how wage insurance eligibility influences a worker's reservation wage and optimal search effort. For simplicity and to permit a graphical representation of optimal search behavior, we assume a stationary setting in which employment and wage insurance eligibility are permanent and payments are uncapped.[21]

Setup

Time is discrete, the worker is displaced at $t = 0$ after earning w_0 in the previous job, potentially eligible for wage insurance in all subsequent periods, and lives forever. The subsidy rate is φ, and we define the worker's 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 unemployment. 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 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.[22]

[21] In practice, 85% of payment recipients do not hit programs caps.

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

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

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, 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 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 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 $V^u = V^e(\overline{w})$. Standard manipulations to equation (4) then imply

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

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.

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

Effect of Wage Insurance Eligibility on Search Behavior

In Appendix E, we show that wage insurance eligibility reduces the reservation wage and increases the optimal search effort relative to an otherwise identical situation without wage insurance eligibility. Here, we present a graphical analysis that yields the same conclusion and facilitates intuition.

The left side of equation (5) reflects the cost of turning down a wage offer $\overline{w}$ to continue searching. This cost includes the discounted value of employment at that wage minus the benefits lost when employed plus the cost of searching with the optimal effort. The right side of equation (5) reflects 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. Equating the cost and benefit yields the reservation wage $\overline{w}$.

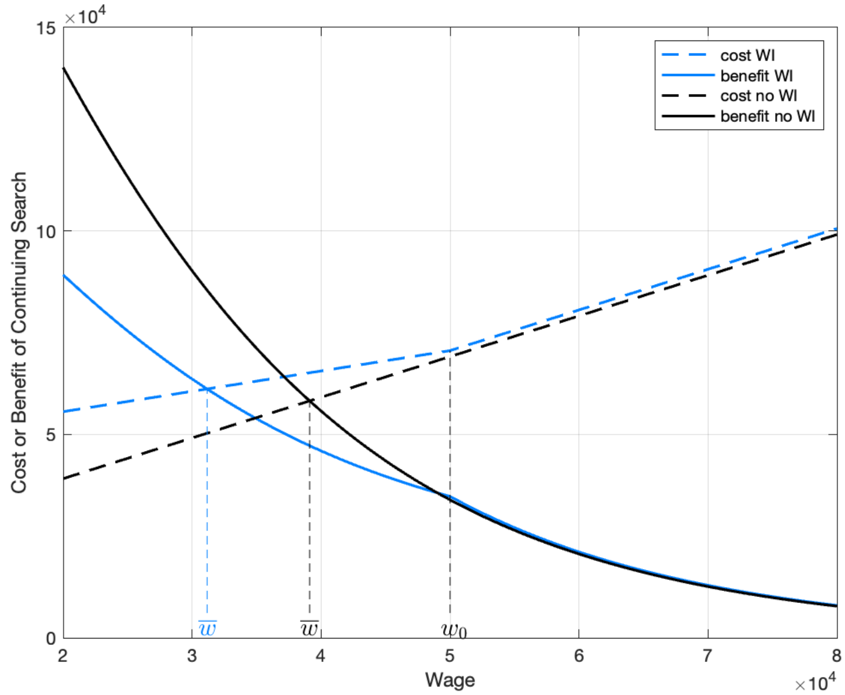
Figure 2 plots the reservation wage condition in equation (5). First, consider a worker without wage insurance eligibility, plotted in black. For this worker, the cost of continued search on the left side of equation (5) is an increasing straight line, reflecting the increased cost of turning down higher wage offers. It is straightforward to show that the benefit of continued search on the right side of equation (5) is decreasing and convex in $\overline{w}$, as shown in the figure. The intersection of these two profiles along the x-axis yields the worker's reservation wage.

Now consider the profiles for a worker eligible for wage insurance, shown in blue in Figure 2. For wage offers above w_0 (in this example \$50,000), the profiles are identical to those for an ineligible worker, conditional on the value of λ^*. For offers below w_0, both profiles change. The cost of turning down a wage offer is now higher, because the worker loses the subsidy as well when turning down the wage. Because wage insurance subsidies are larger for lower wages (recall equation (1)) the slope of the cost profile is less negative to the left of w_0. The benefit of continued search 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 the Figure, by tilting the cost and benefit profiles below w_0, wage insurance eligibility lowers the reservation wage.[23] It also increases the optimal search

[23]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). In Appendix F we present a reservation wage figure paralleling that in Figure 2 for a constant reemployment subsidy and show that wage insurance increases employment probabilities more than a reemployment subsidy with the same expected cost.

Figure 2 – Optimal Reservation Wage With and Without Wage Insurance



Notes: Figure plots the optimal reservation wage condition in equation (5) with wage insurance (in blue) and without (in black). See text for discussion. Illustrative simulation uses a lognormal wage distribution $F(w)$ with $\mu = 10.5$ and $\sigma = 0.5$; convex search cost function $c(\lambda) = \kappa \cdot \lambda^{1+\gamma}/(1+\gamma)$ with $\kappa = 500{,}000$ and γ=1; β=0.95; b=10,000; and w_0=50,000.

effort by increasing the value of receiving a wage offer, which appears on the right side of equation (6). Under the assumption of convex search cost, this in turn implies an increase in the optimal search effort λ^*. This increase accounts for the slight differences in the cost and benefit profiles in Figure 2 for wage values above w_0.

Although quite simple, this 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 should lead to shorter unemployment durations. To the extent that the reservation wage is binding, eligible workers will exhibit lower reemployment wages, all else equal. However, if 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).[24]

[24]In the context of more complex search models incorporating non-stationary search behavior, 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.

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. We then present descriptive statistics.

4.1 TAA Petition and Worker Data

We use the universe of TAA petitions (1974-2016), acquired through Freedom of Information Act (FOIA) requests at the US Department of Labor (see Hyman 2018). This dataset contains an observation for each petition (roughly 84,000 in total), including two critical pieces of information for all approved and denied petitions. First, each petition contains the plant (establishment) name and address, which we match to Census Bureau establishments and the workers who separate from those establishments. Second, each petition contains a series of dates, including 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.

The petition database additionally includes information on petitioner filer types (company, union, worker-group, or state career center), DOL-assigned 4-digit Standard Industrial Classification (SIC) codes, and the company's main product or service, allowing us to observe what industries were most influenced by the program. Finally, each petition 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. These data include anonymized records of all individuals receiving TAA-related benefits, and indicate which individuals participated in the RTAA wage insurance program. This 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 but is not sufficiently detailed to match LEHD workers directly.[25]

[25]These data were obtained through two separate FOIA requests at the Department of Labor, which

4.2 Census Data

We merge the TAA petition data to the US Census Bureau's LEHD administrative files by first following the procedure discussed in Hyman (2018). The LEHD files allow for the construction of a detailed person-level panel dataset that tracks workers' quarterly employment status, earnings, and educational status 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 US states and covers over 90 percent of US workers, for this project, 24 states and the District of Columbia approved data access.[26] Our main sample uses quarterly earnings from 2007 to 2014 for these 24 states from the 2014 LEHD Snapshot. We also observe an indicator for UI-covered employment in any state.[27]

Using each worker's (de-identified) social security number, we also merge in worker date of birth, gender, and race from the Social Security Administration Numident file (available in the LEHD Individual Characteristics File). 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). Additionally, we incorporate firm age and firm size variables at the employer level. 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.

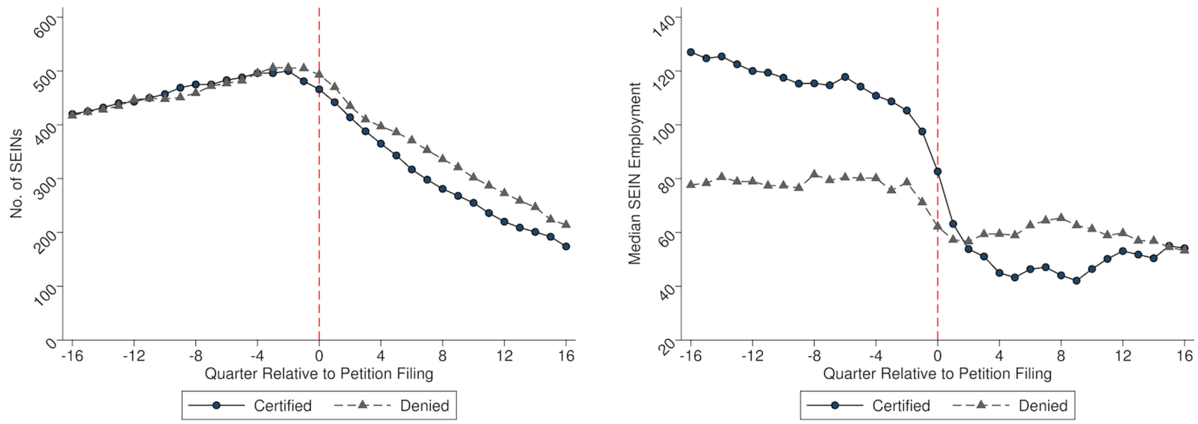
feature in Park (2012) and Reynolds and Palatucci (2012).

[26]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).

[27]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.

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 a commensurate increase in 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 start with 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. 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 C 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."

We supplement this sample of TAA-certified petitions with a sample of petitions denied by the Department of Labor. Results from 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. 2019; 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). A second relevant policy is that work requirements for childless adults enrolled in the Supplemental Nutrition Assistance Program (SNAP or "food stamps") stop at age 50 (Gray et al. 2023). As a result of these changes in disability insurance and SNAP, 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.

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 unemployment after displacement. This restriction ensures 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 unemployment and obtain qualitatively similar estimates, suggesting this restriction does not meaningfully change our findings.

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. In addition, Appendix Table D.1 shows 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		
	Mean (1)	SD (2)	# Workers (3)	Mean (4)	SD (5)	# Workers (6)
Age at separation	45.38	[10.47]	28,000	43.38	[10.91]	48,500
Less Than High School	0.11	[0.32]	28,000	0.13	[0.33]	48,500
High School	0.39	[0.49]	28,000	0.32	[0.47]	48,500
Some College	0.33	[0.47]	28,000	0.33	[0.47]	48,500
College or higher	0.17	[0.38]	28,000	0.22	[0.42]	48,500
Female	0.35	[0.48]	28,000	0.39	[0.49]	48,500
Black	0.12	[0.32]	28,000	0.14	[0.35]	48,500
White	0.82	[0.38]	28,000	0.77	[0.42]	48,500
Other Race	0.06	[0.24]	28,000	0.09	[0.29]	48,500
Hispanic	0.07	[0.25]	28,000	0.12	[0.32]	48,500
Prior Earnings in -8Q to -5Q	45,160	[21,900]	28,000	48,620	[25,700]	48,500
Δ Prior Earnings from -8Q to -5Q	17.84	[3,623]	28,000	-8.05	[4,091]	48,500
Overall Tenure (years)	14.69	[4.56]	28,000	14.42	[5.15]	48,500
Petitioning-Firm Tenure (years)	7.24	[5.39]	28,000	6.14	[5.13]	48,500
Firm Age (years)	29.65	[9.89]	28,000	31.85	[8.63]	48,500
Log Firm Size	7.85	[1.88]	28,000	9.50	[2.15]	48,500

Notes: 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 are rounded due to Census disclosure rules, where $N = 76,500$ pooling both 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.

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.[28]

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. 2021a). Figure 4 presents descriptive event study plots of employment and earnings replacement rates for workers aged 47–53.[29]

About 60% of workers are reemployed after four years. Workers replace slightly over half of their pre-separation earnings (excluding any subsidies) by this time, indicating 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

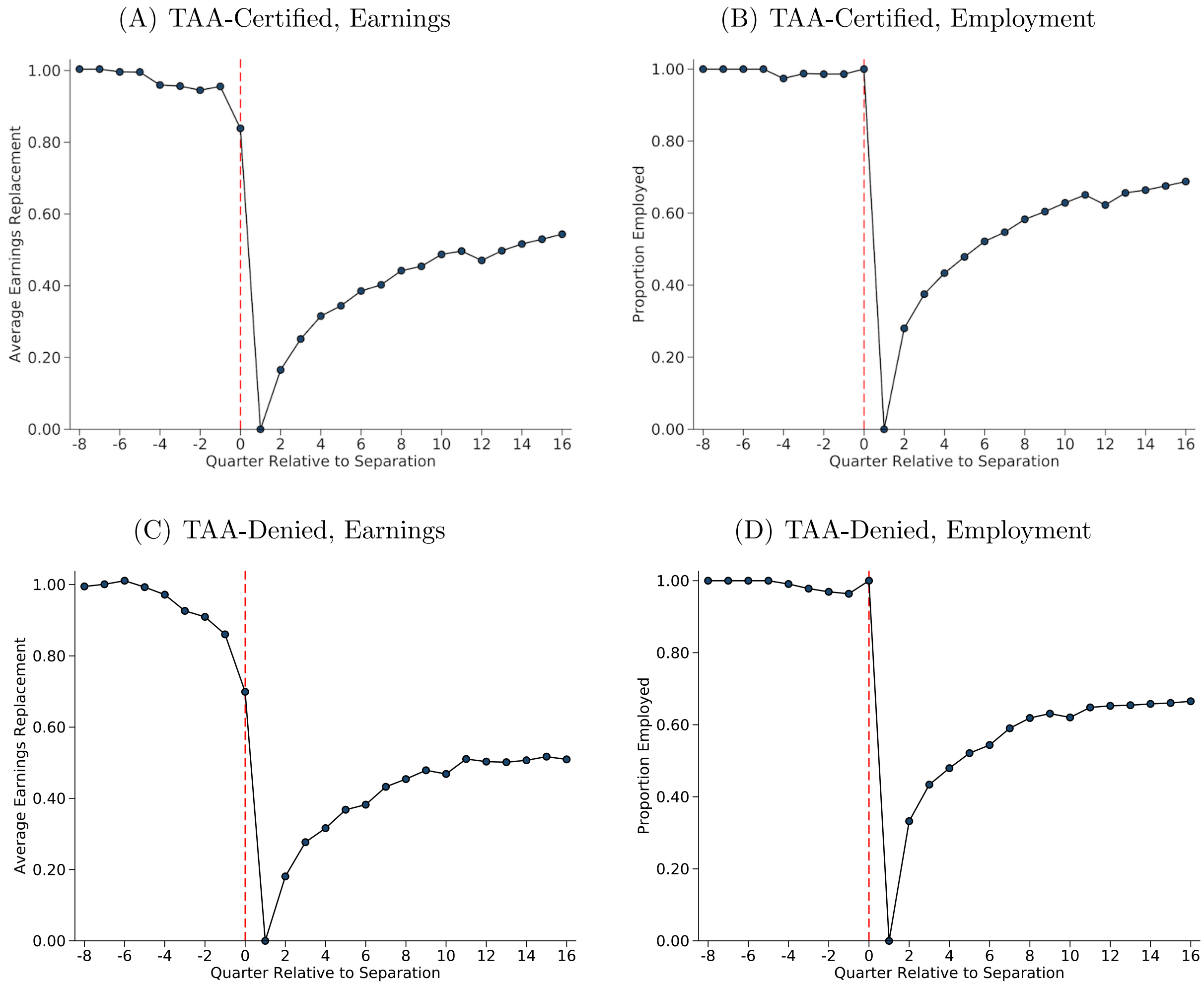
To estimate the causal effect of wage insurance, we leverage the requirement that workers must be age 50 or older when reemployed to be eligible. After the TAA petition relating to a given displacement episode is certified by Department of Labor investigators, the associated workers qualify for the baseline TAA benefits of training and extended UI payments described above. Those aged 50 or older have the option of receiving standard TAA benefits and/or wage insurance, while younger workers only qualify for standard TAA.[30] The relevant dates determining eligibility are defined at the petition level and do not vary across workers covered by the same petition, so an individual worker is unable to manipulate their displacement date relative to their birth 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 the intent-to-treat (ITT) effect of wage insurance on worker outcomes.

[28] These worker characteristics are consistent with the industrial composition of the analysis sample, the majority of whom separate from Manufacturing, Textile, and Assembly (NAICS=33) plants.

[29] Appendix Figure D.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.

[30] Less than 25% of TAA recipients receive both wage insurance payments and enroll in training programs.

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.

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. Looked at 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.[31] In the context of an instrumental-variables analysis seeking to estimate the effect of receiving wage insurance payments, 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.

Our preferred RD specification is a local linear model, 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 choose a local linear model following Gelman and Imbens (2017), who recommend using low-order polynomial specifications.

We first estimate this equation for both the certified and denied samples separately. We then pool the 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 outcomes at the discontinuity between the certified and denied samples. The terms in the second and third

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

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.[33] 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.

5.1 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.[34] 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 D.2 and Appendix Table D.3 show outcomes are nearly always balanced in both the certified and denied samples. Appendix Table D.4 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

[32] 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.

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

[34] 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 D.5).

separation are remarkably small, at less than 0.5% of the control mean.[35] 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 at the discontinuity.

Second, we verify that the density of the age distribution is smooth at the discontinuity. Appendix Figure D.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 research design.

6 Results

6.1 Earnings Replacement Rates and Employment

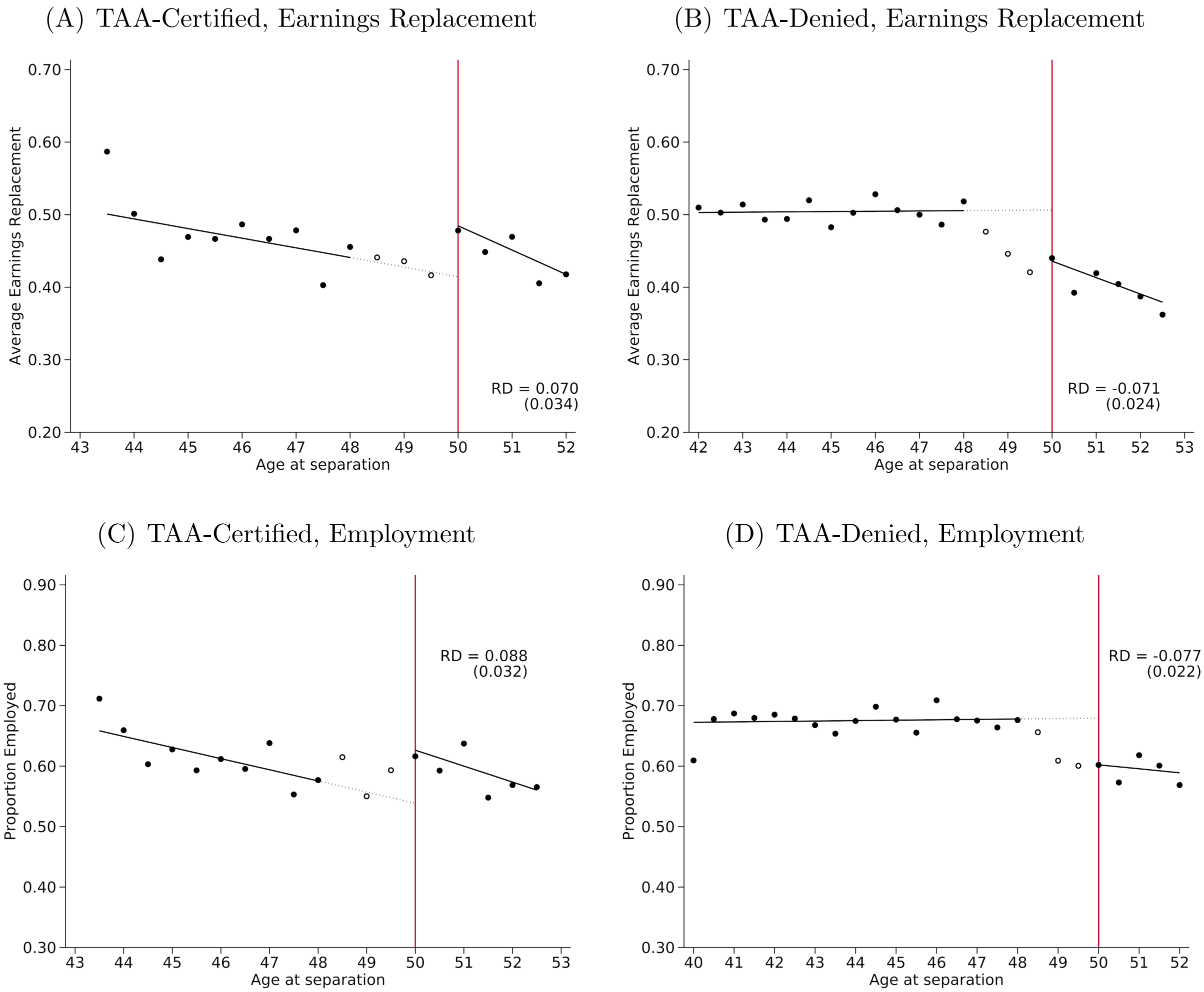
To illustrate the variation identifying our estimates, we first present scatterplots from estimating equation (7) at 8 quarters following separation, and then subsequently show the RD estimates for all other quarters. Figure 5 shows results for earnings replacement and employment in the certified and 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).[36] These estimated

[35] 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.

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

Figure 5 – RD Scatterplots, 8 Quarters since Separation



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.

changes are again large relative to their respective control means.[37]

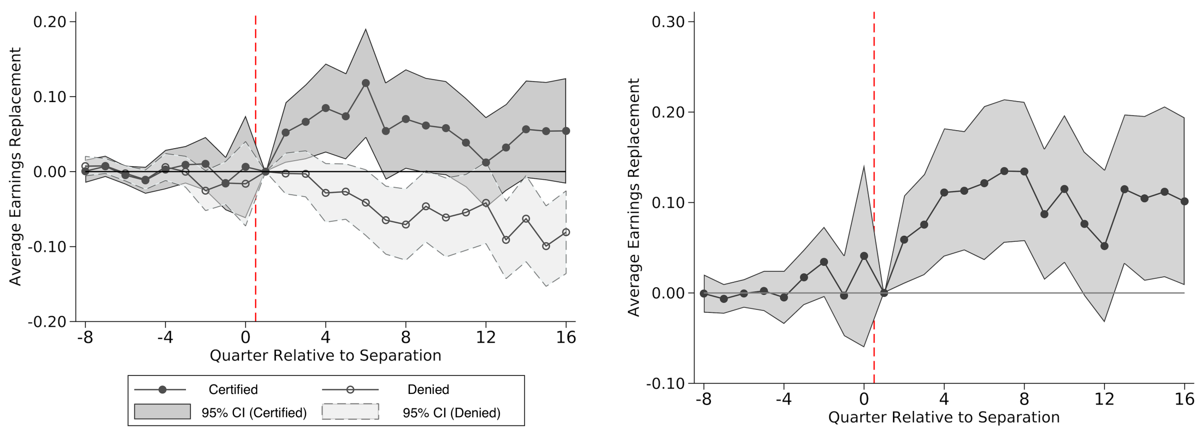
workers are particularly likely to receive disability insurance.

[37] In an exploratory analysis, Hyman et al. (2021b) 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.

We generate RD estimates as in Figure 5 for earnings replacement and employment 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 from estimating equation (8) in Panel B.[38]

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

[38]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.

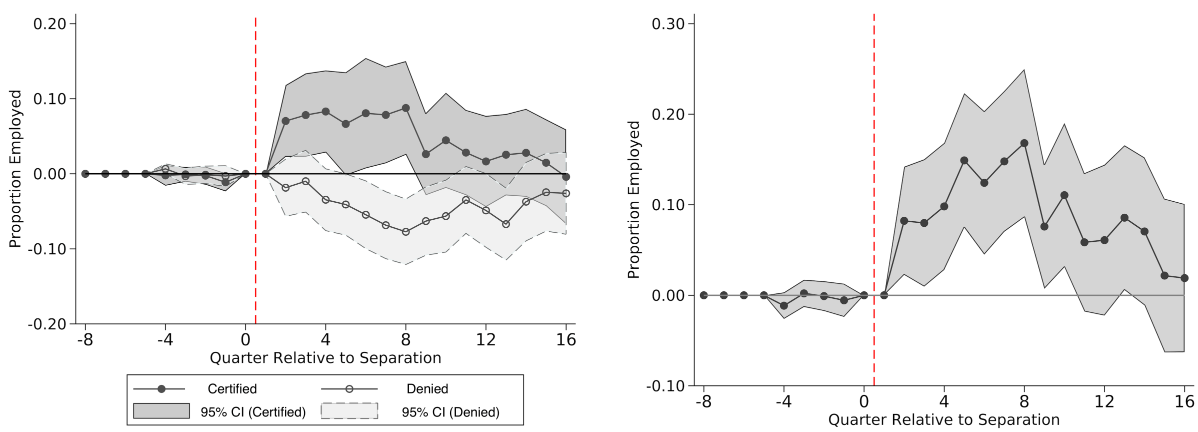
in the denied sample falls shortly after displacement and remains below zero. Taking the difference in these two discontinuities, the D-RD (Panel B) estimate is large during most quarters post-separation and eventually declines to a small and statistically insignificant increase by the end of four years.

It is natural to consider potential heterogeneity in these results. We fail to find evidence of heterogeneity in the D-RD estimates for employment or earnings replacement by whether the worker's county of residence is above or below the median unemployment rate in the calendar quarter of petition. We also fail to find differences in outcomes by gender, race, or education.

Figure 7 – Employment Results

(A) RD Estimates

(B) D-RD Estimates



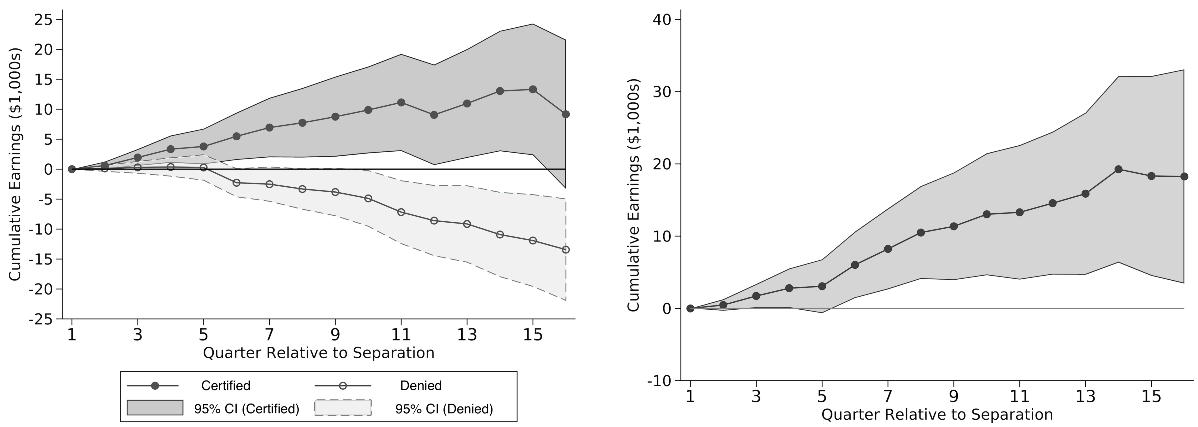
Notes: Panel A plots RD estimates of employment for TAA-certified and TAA-denied samples from estimating equation (7) from 8 quarters pre-separation to 16 quarters post-separation. Panel B plots D-RD estimates from estimating equation (8) over the same interval. Shaded areas denote 95% confidence intervals. 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.

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.[39] By the end of four years, wage insurance eligibility increases cumulative earnings by \$18,260, with a 95% confidence interval of \$3,436 to \$33,090. This represents a 21.8% increase relative to the average 4-year cumulative earnings of \$83,850 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. We obtain similar results when using a second-order polynomial, a triangular kernel, including baseline covariates, or clustering standard errors by petition.[40] The main results are also robust to using alternative bandwidths, including a smaller bandwidth that is half the size of the 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

[39] 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.

[40] Due to rules and considerations related to Census disclosure procedures, we have chosen to report qualitative findings in this paragraph that have been reviewed by Census officials, but not to release individual figures or tables, which are available upon request.

using the smaller bandwidth are less precise but still marginally significant.

Our results do not hinge on how we use variation in partial eligibility within the small window below age 50 (i.e. within the donut). The main results are qualitatively robust to a specification that does not exclude these workers to predict the regression function to the left of the discontinuity. As expected, effects are attenuated due to partial treatment among those just left of the discontinuity, but the overall pattern of the estimates remains the same and, for the most part, is statistically significant at the 10% level or lower (Appendix Figure D.4). Moreover, the estimates are quantitatively similar to our main results when we estimate a regression kink design that includes variation within the donut (Appendix Figure D.5).

Our main results are also robust to relaxing each of the sample restrictions one at a time. We obtain similar results when also including workers who switch to another firm without a full quarter of unemployment along with our main sample of displaced workers. The former may have voluntarily switched rather than being involuntarily displaced. Next, 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 D.6). Finally, the main D-RD results are 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, in fact, larger than our reported OLS D-RD estimates. However, the 2SLS estimates are imprecise and not statistically different from OLS or zero.[41]

Finally, we re-estimate the RDs using age 55 as the cutoff, rather than age 50, as a falsification test. 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 wage insurance and TAA training on both sides of the age-55 cutoff. Therefore, we should not expect an increase in employment or earnings replacement as observed in Figure 6 and Figure 7 when using the age-55 cutoff. If anything, we should see a deterioration in labor market outcomes, reflecting increased access to disability insurance. As shown in Appendix Figure D.7, we observe large and statistically significant decreases in employment and earnings replacement for the certified

[41] The second stage in examiner designs is known to carry wide standard errors (Angrist et al. 1999; Hull 2017). Given we are greatly restricting the data in Hyman (2018) to a subset of petitions and workers within age bandwidths, the imprecision of these estimates is not surprising.

sample at age 55. We observe a decrease or no change in outcomes for the denied sample, depending on the years considered. When estimating the D-RD at age 55, instead of age 50, 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 unemployment duration. We find less 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. It is straightforward to calculate the share of the overall effect on cumulative earnings driven by increased employment probability in any given quarter. Collectively, these quarterly effects sum to the cumulative effect shown in Figure 8.[42] We find that 65.5 percent of the 4-year cumulative earnings effect is explained by an increased probability of employment.[43]

[42]Let $earn_{it}$ be worker i's earnings in period t relative to displacement and D_i be an indicator for wage 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 D presents additional details of this decomposition and notes how each of these terms maps to an estimate from one of the D-RDs.

[43]This result is corroborated by Appendix Figure D.8, which estimates earnings replacement rates by period

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.

Table 2 – Mechanisms: D-RD Estimates

	Estimate	S.E.	Control Mean	*N*
Panel A. Employment				
Ever reemployed	0.008	0.029	0.826	76,500
Unemployment duration	-1.002	0.381	4.199	76,500
Total quarters not employed	-1.259	0.426	5.776	76,500
Earnings replacement rate | reemployed, first full quarter	0.053	0.040	0.615	56,000
Earnings | reemployed, first full quarter ($)	338.2	628.8	7,680	56,000
Earnings | reemployed, last full quarter ($)	332.5	637.7	9,754	56,000
Panel B. Job Quality				
Employment duration of 1st job post-separation (Q)	1.043	0.435	6.872	56,000
Firm age (years) of first job post-separation	0.734	0.650	31.45	56,000
Log firm size of first job post-separation	0.336	0.308	7.706	56,000
Earnings growth rate (percentage points)	-0.195	1.798	3.119	56,000
Predicted quarterly earnings of 1st job post-separation, logs	-0.001	0.015	9.212	56,000
Predicted quarterly earnings of 1st job post-separation, ($)	13.99	103.9	11,530	56,000
Panel C. Job Laddering and Mobility				
Number of unique firms	-0.041	0.090	1.787	56,000
Switched industries (3-digit) by Q12	-0.009	0.042	0.569	56,000
Switched industries (3-digit) by Q16	-0.037	0.041	0.589	56,000
Switched county of employment by Q12	0.018	0.040	0.519	56,000
Switched county of employment by Q16	0.010	0.040	0.554	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.

Consistent with the employment results in Figure 7, the average unemployment

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.

duration following displacement among eligible workers is shorter by 1 quarter, and these workers spend 1.26 fewer quarters out of employment across all non-employment spells. Yet we do not find differences in whether eligible 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. Our point estimate is close to zero, and the upper bound of the 95% confidence interval rules out increases of 6.4 percentage points in the probability of ever finding reemployment, equal to 7.7 percent of the control mean. These results are consistent with the model predictions in Section 3, that eligible workers pursue employment more intensively than ineligible workers by increasing search intensity and/or lowering reservation wages.

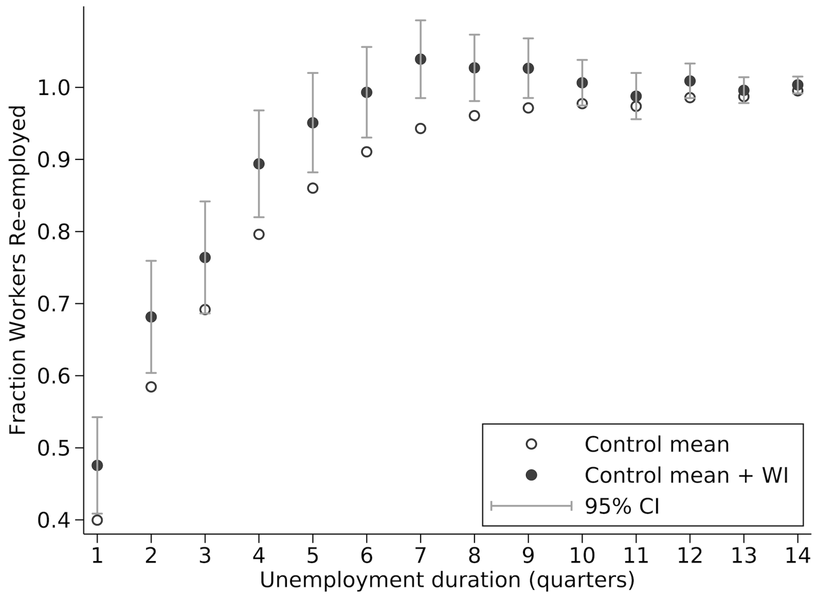
We find positive and statistically insignificant effects on earnings replacement and earnings levels in the first full quarter of employment after the initial displacement.[44] This result is unlikely to be confounded by compositional differences because ever being reemployed is not affected by wage insurance eligibility (Table 2). 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; Bentolila and Jansen 2017). Since eligible workers have substantially shorter unemployment 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* unemployment duration shorter for eligible workers, but the entire distribution of durations shifts left for eligible workers. This figure plots results from separate D-RDs in which the dependent variable is an indicator for whether the worker had an unemployment duration of a given length or shorter. The regression is restricted to those finding reemployment. At each quarter of unemployment duration through 9 quarters, a larger fraction of workers eligible for wage insurance has found reemployment compared to those not eligible. Note that concerns regarding dynamic selection along the duration distribution are partly mitigated by our focus on earnings replacement rates rather than raw earnings levels.

Figure 10 shows evidence of 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 unemployment duration. The downward sloping duration dependence curve in reemployment earnings is robust to controlling for education status (high school, some college, college plus), demographics (gender, ethnicity), worker overall

[44]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).

Figure 9 – Differences in Unemployment Durations



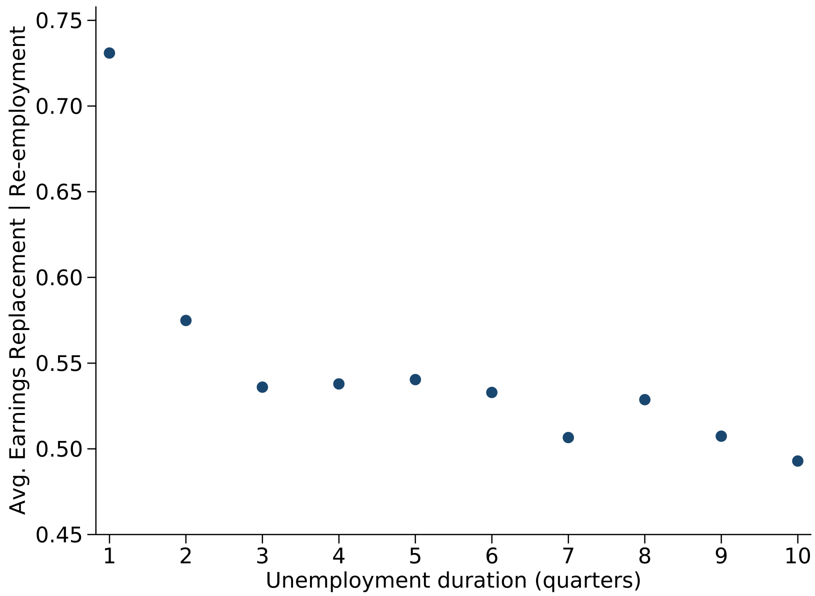
Notes: Figure plots predicted values from D-RDs of unemployment durations among those who become reemployed after displacement. The dependent variable in each regression is an indicator of whether the worker had an unemployment duration of a given length or shorter, as displayed on the x-axis. Since all workers who become reemployed during our sample period do so between 2 and 16 quarters since displacement, we consider unemployment durations from 1 quarter (corresponding to relative quarter 2 in Figure 8) to 14 quarters (corresponding to relative quarter 15 in Figure 8). 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 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. Estimates may exceed 1 because of variability in regression estimates.

tenure, and firm age and size at the worker's separation firm.[45]

Wage insurance-eligible 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 (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 employment duration of their first job, it is

[45] We fail to find 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.

Figure 10 – Earnings Replacement by Unemployment Duration



Notes: Figure plots mean earnings replacement rates against unemployment durations among displaced workers who become reemployed. The figure pools workers aged 45–54 at separation.

largely mechanical due to censoring of our data and the shorter unemployment durations referenced above. The increase is of a similar magnitude to the reduction in the unemployment duration, which 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 a more precise 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% 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 large decreases (or increases) in firm quality; the lower bounds of the 95% confidence interval rule out decreases of 3% in earnings when measured in logs and 1.5% when measured in levels.

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. 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. 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, 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.

8 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 subsection 2.2). If the program were more broadly available, the behavior of firms and workers would likely change, with implications for wages and unemployment 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 based on the tax rate τ. We conservatively assume a low combined tax rate of $\tau = 18.9\%$, coming from 15% federal income taxes, 1% state and local taxes, and 2.9% Medicare taxes.[46] We exclude Social Security taxes (12.4% split between employee and employer) because we assume future Social Security benefits will approximately offset these taxes for this population. Cumulative earnings after 16 quarters ($T = 16$) are discounted at quarterly rate r, which we set to 0.0074 (equal to a 3% annual rate). Denoting wage insurance subsidy payments per eligible worker as s, private benefits are calculated as:

$$\Delta W = \left(s + z \times \frac{\hat{\gamma}_3^{T,earnings}}{(1+r)^{T-1}} + b \times \hat{\gamma}_3^{unemployment} \right) (1 - \tau), \tag{10}$$

where $\hat{\gamma}_3^{T,earnings}$ denotes the D-RD estimate of earnings for relative quarter $T = 16$ shown in Figure 8. The parameter z lowers the valuation of labor earnings from reduced leisure, which Mas and Pallais (2019) estimate to equal 0.6 for unemployed workers. UI payments equal the average quarterly benefit b multiplied by the change in unemployment duration $\hat{\gamma}_3^{unemployment}$ from Table 2. We set $b = \$3,783$, based on a \$291 average weekly UI payment.[47] 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 $\hat{\gamma}_3^{unemployment} < 0$, 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 \$150 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. Omitting reductions in TAA training payments, DI benefits, health insurance tax credits, and other transfers would likely make these savings to the government larger. The savings to the government equal:

[46] While some in our sample reside in states without state income taxes, we view a 1% average state tax on these earnings to be conservative.

[47] We calculate this weekly average as the mean of weekly UI benefits as reported by the Bureau of Labor Statistics between 2009q3 and 2014q4, deflated to 2010q1 to match the earnings results.

[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% of all eligible workers are estimated to receive any TAA service (D'Amico and Schochet 2012). Third, at most 25% of 50-year olds who receive any TAA services receive wage insurance during our sample period (Appendix Figure D.3). Multiplying these yields an estimate of \$149 per eligible worker.

$$\Delta C = \tau \times \frac{\hat{\gamma}_3^{T,earnings}}{(1+r)^{T-1}} + b \times \hat{\gamma}_3^{unemployment} \tag{11}$$

We calculate that $\Delta C = \$10,945$ per eligible worker. The majority of savings are from reduced UI payments. Since we have assumed a relatively low tax rate, the tax receipts on increased earnings are smaller by comparison.

Using these formulas, Appendix Figure D.9 illustrates the MVPF under a range of assumptions about subsidies per eligible worker. We find that the savings to government budgets generally exceed program costs ($\Delta E < \Delta C$). We view this calculation as conservative because other fiscal externalities such as reduced DI benefits and other transfers would only further reduce government spending (as indirectly suggested by evidence on employment (Figure 7) and earnings (Table 2) in the TAA-denied sample). Appendix Figure D.9 also investigates sensitivity of the MVPF to using lower bounds of the confidence intervals for our D-RD estimates instead of the point estimates. Under any plausible value of WI payment per eligible worker, the fiscal externality exceeds program costs and therefore produces net savings to the government (Appendix Figure D.9). In this case, the MVPF's denominator is negative and the program "pays for itself."[49]

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; Johnston and Mas 2018).[50] 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.

9 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

[49] Hendren and Sprung-Keyser (2020) label the MVPF to be "infinite" in this situation.
[50] See policyimpacts.org for descriptions of how the MVPF is calculated from each study.

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. In our context, wage insurance is a highly cost-effective policy; 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 unemployment 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% 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 may have broader implications for workers who lose their jobs due to automation or other competitive forces that characterize the contemporary economy. Automation is widely seen to be an important force in affecting labor markets over the long-term (Abraham and Kearney 2020), 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 should extend these results in a number of different directions. First, research could 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, research 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.

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, David Dorn, Gordon Hanson, 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.

Autor, David H, David Dorn, and Gordon H 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.

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 XXI Century," *Annual Review of Economics*, forthcoming.

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.

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 D. 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 D., 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.

Couch, Kenneth A and Dana W 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.

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.

_ , _ , **and Yalun Su**, "Disability and Distress: The Effect of Disability Programs on Financial Outcomes," 2019.

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.

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.

Hyman, Benjamin G., Brian K. Kovak, Adam Leive, and Theodore Naff, "Wage Insurance and Labor Market Trajectories," *AEA Papers and Proceedings*, May 2021, *111*, 491–95.

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.

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.

_ , Notowidigdo Matthew Lange Fabian, 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 Pierce Schott, "The Surprisingly Swift Decline of US Manufacturing Employment," *American Economic Review*, 2016, *106* (7), 1632–1662.

Pierce, Justin R. and Peter K. Schott, "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.

Samuel, García-Pérez Ignacio Bentolila and Marcel Jansen, "Are the Spanish long-term unemployed unemployable?," *SERIEs*, 2017, *8*, 1–41.

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.

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

Schmieder, Johannes, Till von Wachter, 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.

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.

Schochet, Peter Z., 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.

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

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.

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.[51] 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.[52] 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).[53]

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).[54] 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).

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

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

[53] 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.

[54] 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 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.

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.[55] 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.[56] 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 than through communication with the employer. Subsidy payments are made directly to

[55]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.

[56]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."

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.[57] 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.[58] 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.[59]

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.[60] 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.[61] 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 Unemployment Insurance claims in each fiscal year using weekly claims data provided by the U.S. Department of Labor.[62] 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 subcomponents 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.

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

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

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

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

[61] 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.

[62] Data available here: https://oui.doleta.gov/unemploy/claims.asp.

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

(A) Total spending - US

(B) Total spending - LEHD states

(C) Total participants - US

(D) Total participants - LEHD states

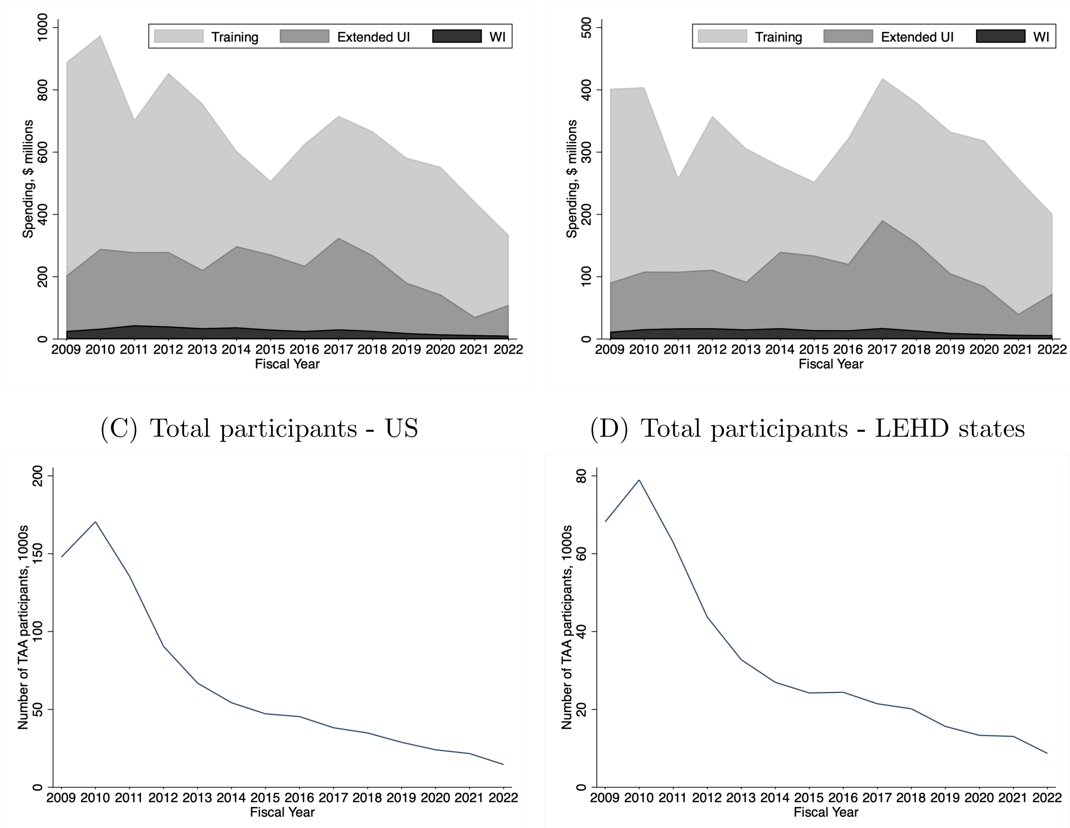
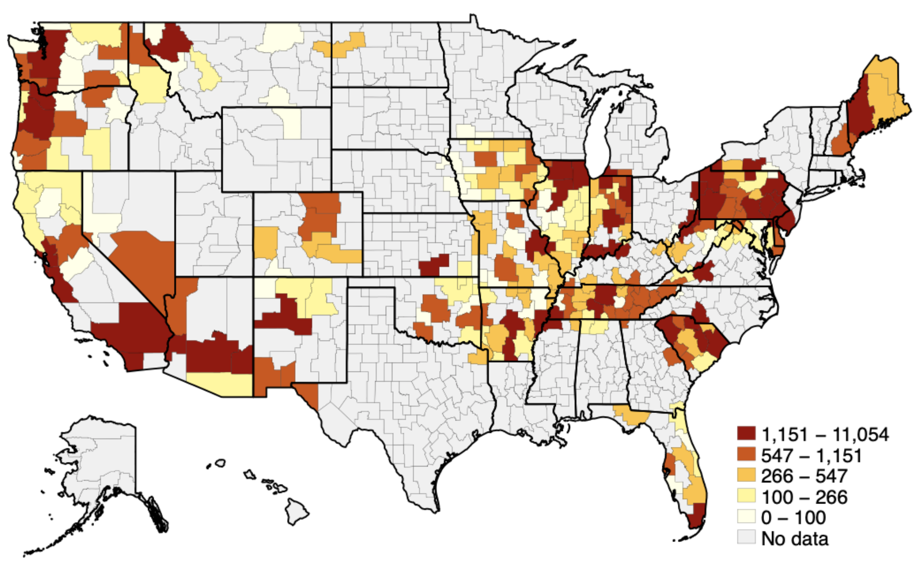


Figure A.2 – No. TAA Petitioning Workers by Commuting Zone, LEHD states 2009 - 2014



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.[63] 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.[64] 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

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

[64] 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 24 states that approved use of the data in our Census proposal.[65] 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.[66]

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 24 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

[65] 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.

[66] 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 24 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 24 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 24 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).[67] 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

[67] 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.[68] 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.[69] Otherwise, the SEINUNIT with the most earnings in the quarter is the primary employer.[70]

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).[71] 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.[72] 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

[68] 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.

[69] 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.

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

[71] 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.

[72] 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 Predicted High-Takeup Sample Using Machine Learning

C.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.

C.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."

C.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.[73] 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.

[73]Our main analysis uses a target recall cutoff of 0.7, but we investigate the implications of varying that cutoff in Figure C.3 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
    - $\mathcal{N}_{\text{Train}} \longleftarrow \mathcal{N} \backslash \mathcal{N}_{-i}$
    - $\mathcal{N}_{\text{Valid}} \longleftarrow \mathcal{N}_{-i}$
    - $\text{model}(h).fit(\mathcal{N}_{\text{Train}})$
    - $p \longleftarrow \text{model}.predict\_proba(\mathcal{N}_{\text{Valid}})$
    - Max Precision Scores$[i] \longleftarrow max_precision_for_target_recall(\text{data} = \mathcal{N}_{\text{Valid}}, \text{predicted_prob} = p, \text{recall_cutoff} = k)$
  - **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, $k^* \longleftarrow max\_precision\_for\_target\_recall(\text{data} = \mathcal{N}^0, \text{predicted\_prob} = p, \text{recall\_cutoff} = k)$
- $model(h^*).fit(\mathcal{N} \cup \mathcal{N}^0)$
- $p \longleftarrow model.predict\_proba(\mathcal{N} \cup \mathcal{N}^0 \cup \mathcal{M})$
- $Y_{\text{pred}} \longleftarrow (p > k^*)$

end

C.4 Feature List

We consider the following features in classifying petitions as high-takeup or low-takeup.[74] 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. Accounts for potential differences across states in providing information regarding wage insurance eligibility.
  - `sic4`:
    4-Digit SIC Code (highest granularity).
  - `occ_codedetailed`:
    Primary Detailed OCC Code (highest granularity).
  - `workergroup`:
    Whether the petition included production workers, service workers, or both.
  - `pet_type`:
    Whether the petition was filed by unions, company, state career centers, or workers, since this may influence workers' knowledge of the program.
  - `determcode`:
    Determination code covering nature of certification, which may be based on direct import competition, shifts in production to other countries, competition in upstream or downstream industries, or may be a 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, which captures the speed of investigations and potentially the generosity of certification determinations.
  - `certofficer`:
    Name of DOL officer who certified the petition. Note that the certification officer makes the final decision on whether the petition is certified for ATAA in the pre-2009 regime.
  - `Multi_State`:
    Indicator for whether petition covers multiple states.

[74] Note that 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.

  - `Multi_Estab_Ind`:

    Indicator for whether the petition covers multiple establishments.

  - `submission_wait`:

    Number of weeks between submission date and determination date for the petition. Included to account for petitions where workers had little or no time to act upon ATAA eligibility due to a slow determination.

- State-level characteristics

  - `ATAA_Alloc`:

    ATAA funds allocated to the state in fiscal year of petition determination, which reflects the available ATAA resources in the liable state.

  - `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. Included to account for observed differences in A/RTAA takeup across states.

  - `take_state_JSA_TAA`:

    Aggregate Job Search Allowance/TAA exiters ratio in state of petition. Included to account for local TAA workers engaged in job search (since work required for A/RTAA but not for training).

  - `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.

C.5 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 C.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 C.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.

C.6 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

C.7 Results

Figures C.1 and C.2 present standard performance metrics for the models' ability to categorize observations in the testing set (i.e. out of sample).

In Figure C.1, 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.

Figure C.2 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 C.1 – ROC Curve

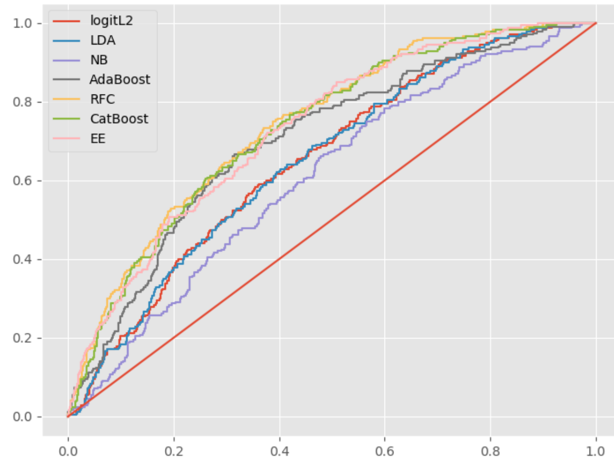


Figure C.2 – Precision-Recall Curve

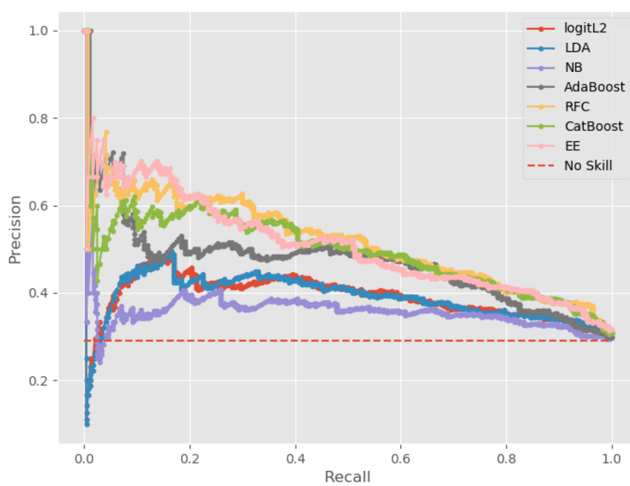
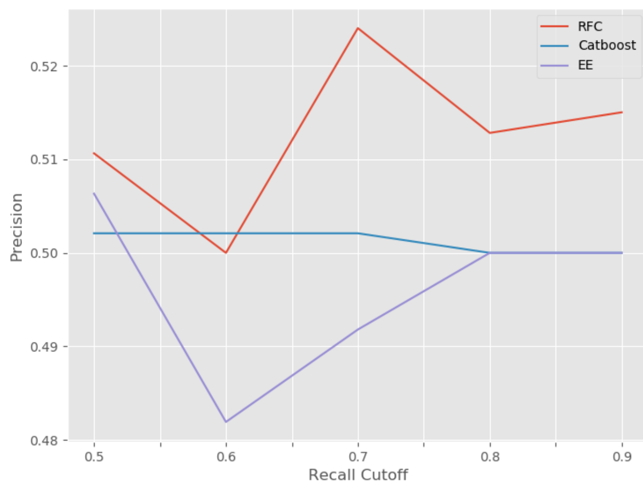


Figure C.3 shows each model's predictive precision in the testing set (out of sample) when using hyperparameters selected subject to different recall cutoff values, ranging from 0.5 to 0.9. Although there appears to be a lot of variation, note that the y-axis scale is quite fine; the precision values 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 C.3 – 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.

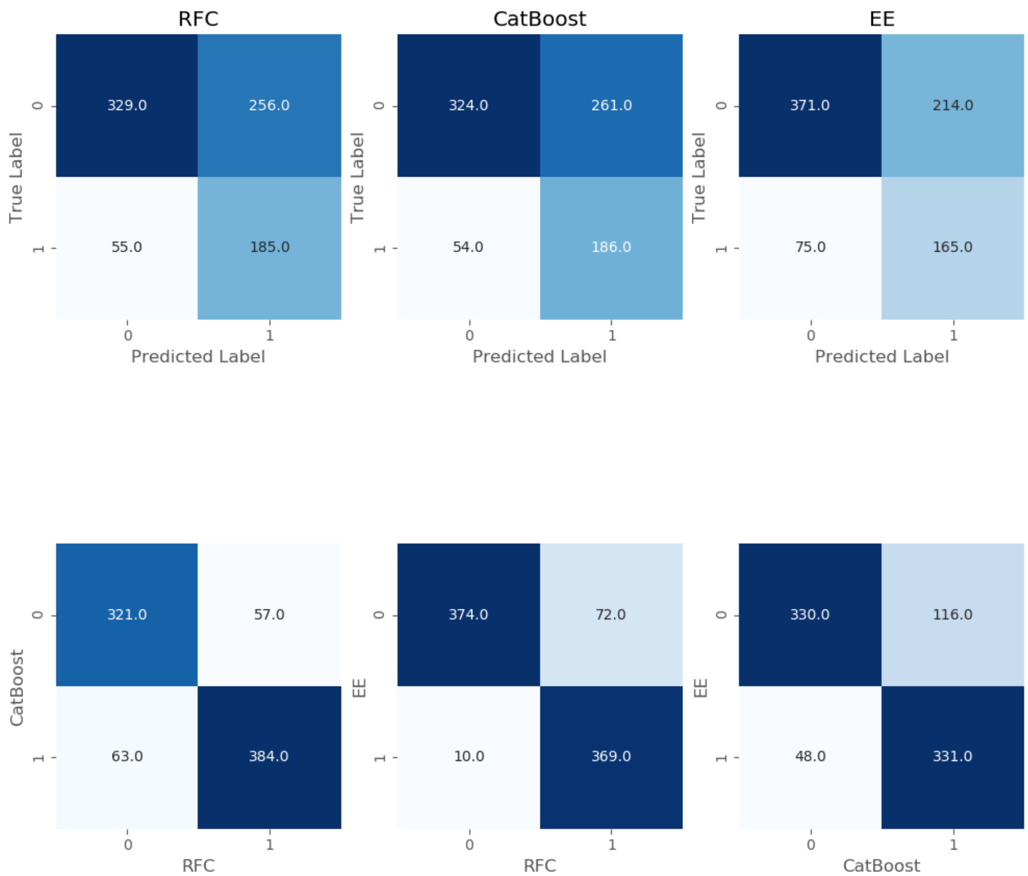
Figure C.5 presents confusion matrices showing the relationship between each model's classification and the true labels. The number of true positives is in the lower right, true negatives in the upper left, false positives in the upper right, and false negatives in the lower left. The three models all perform very similarly.

Figure C.6 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. Figures C.7 and C.8 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

Figure C.6 – Pairwise Confusion Matrix Between Models



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

Classifier (RFC).

Figure C.7 – Feature Importance - RFC

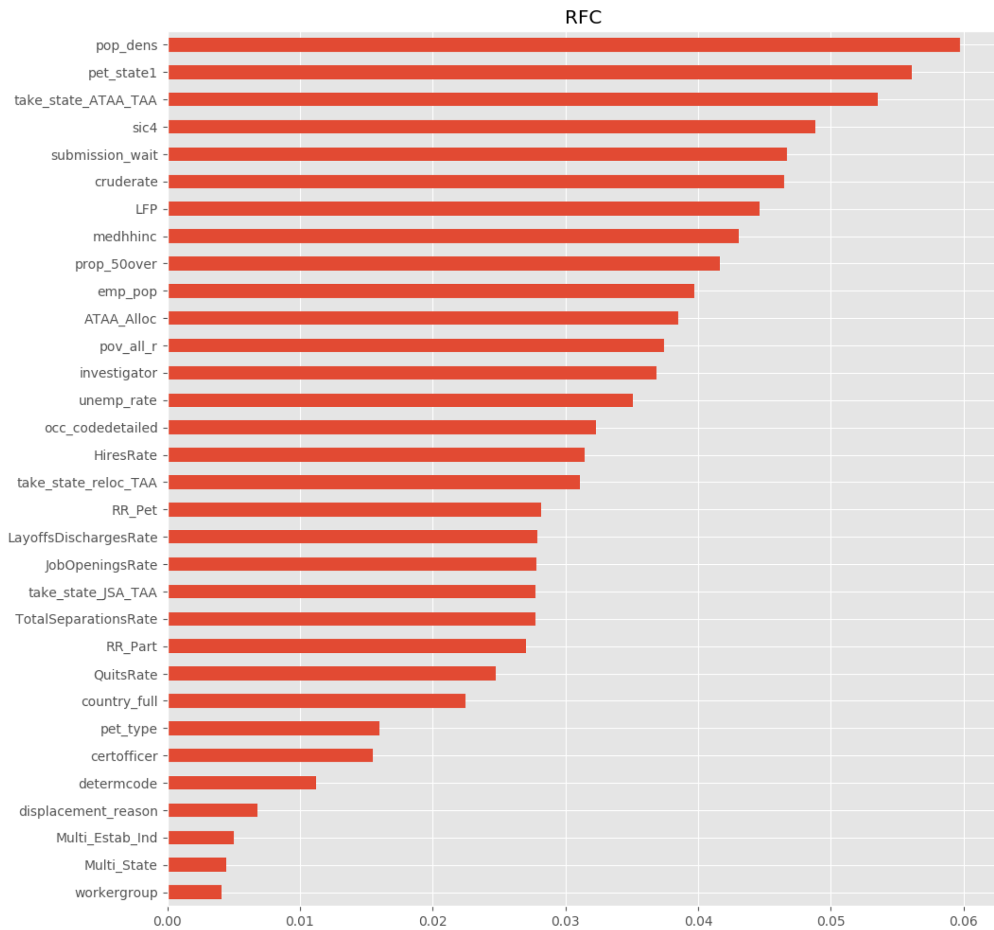
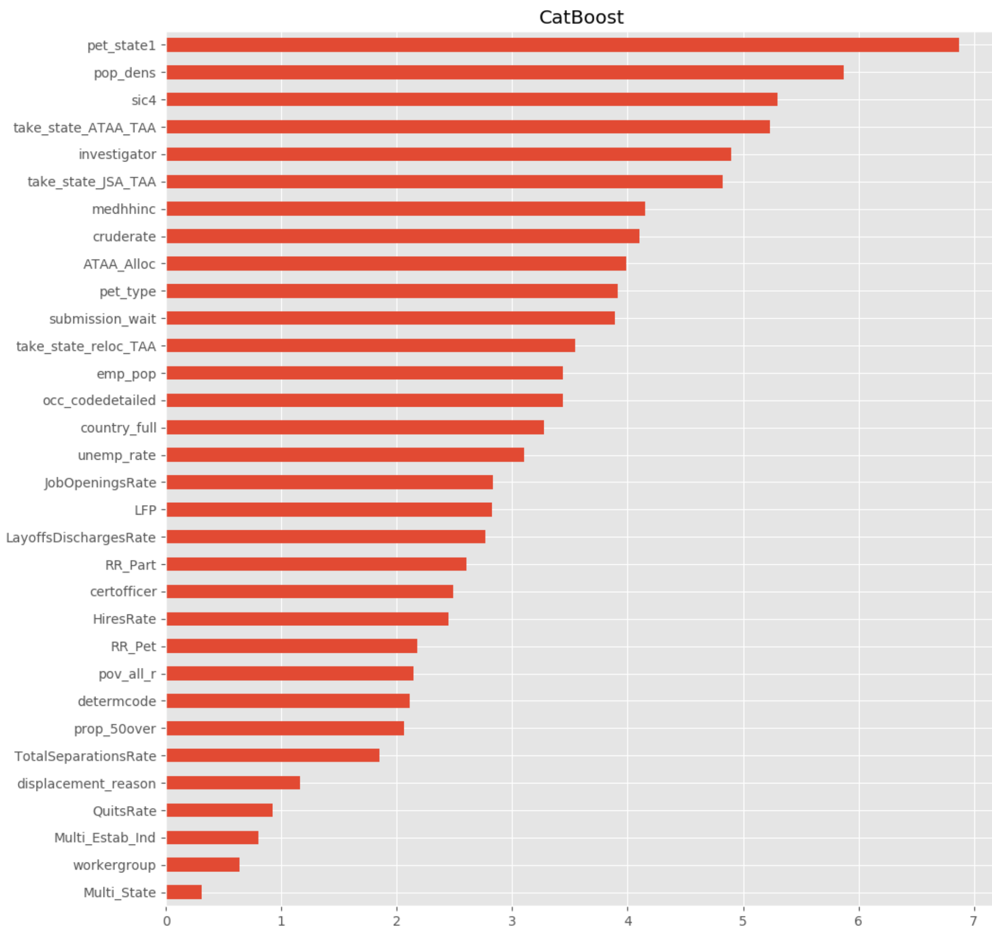


Figure C.8 – Feature Importance - CatBoost



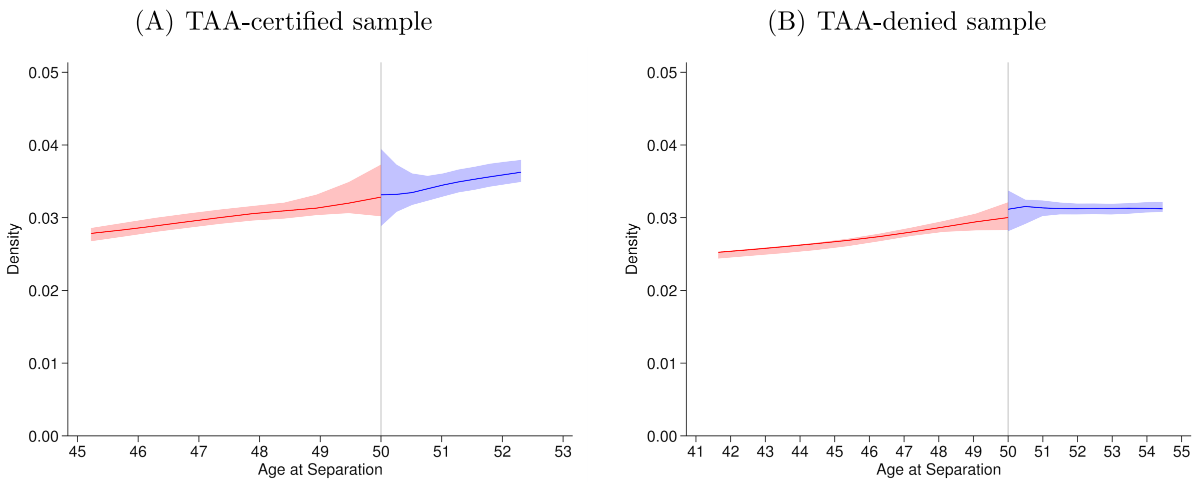
D Additional Results

Table D.1 – Descriptive Statistics for Displaced Workers, 2010-2014 CPS DWS

	Mean	SD	# Obs
Age at separation	39.56	[10.85]	9,688
Less Than High School	0.11	[0.31]	9,688
High School	0.29	[0.46]	9,688
Some College	0.32	[0.47]	9,688
College or higher	0.28	[0.45]	9,688
Female	0.42	[0.49]	9,688
Black	0.13	[0.33]	9,688
White	0.80	[0.40]	9,688
Other Race	0.07	[0.26]	9,688
Hispanic	0.18	[0.38]	9,688
Prior Yearly Earnings	40,927	[40,491]	9,688
Prior-Firm Tenure (years)	4.86	[5.84]	9,604

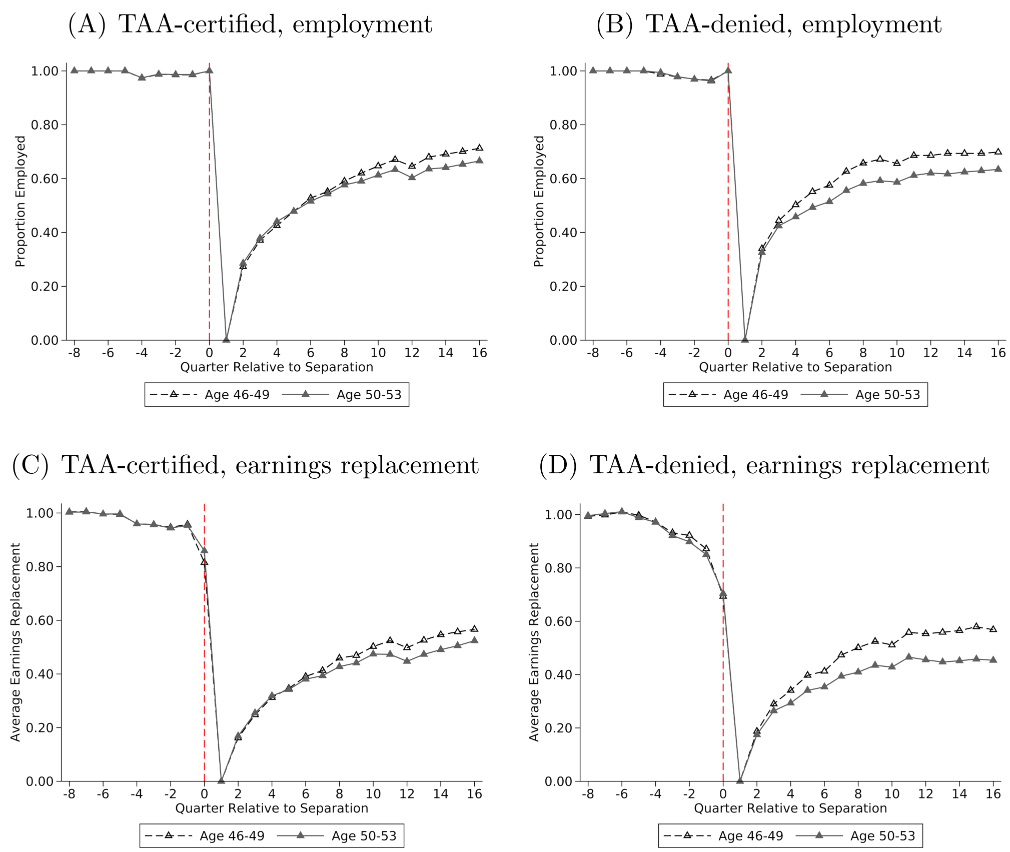
Notes: Authors' calculations using the 2010, 2012, and 2014 Displaced Workers Supplement of the Current Population Survey, obtained from IPUMS-CPS (Flood et al., 2022). We include workers age 22-60 at displacement for consistency with the sample definition in the main analysis. Averages and standard 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, and are deflated to 2010Q1.

Figure D.1 – Density of Age at Separation



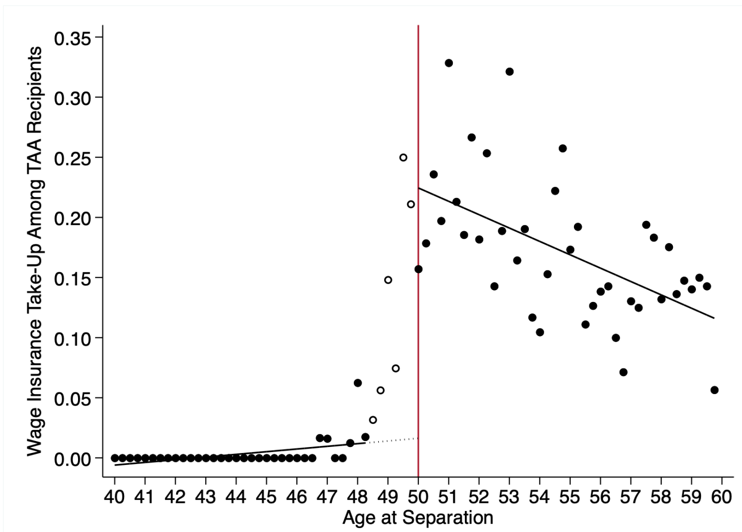
Notes: Panels A and B plot distribution of age at separation for the TAA-certified and TAA-denied samples, respectively. Samples are restricted to high labor force attachment, defined as earning at least $3,000 in each quarter from 8 to 5 quarters prior to separation. Graphs plot densities estimated 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 D.2 – Descriptive Event Studies of Earnings Replacement and Employment by Age



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 D.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. 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 D.2 – Covariate Balance in RD, TAA-certified sample

	Discontinuity	S.E.	Control Mean	% diff	N
Predicted Prior Earnings in -8Q to -5Q	1,329	816.4	47,680	2.8	28,000
Prior Earnings in -1Q	254.3	280.5	11,700	2.2	28,000
Prior Earnings in -5Q	100.5	288.8	11,900	0.8	28,000
Prior Earnings in -6Q	267.1	275.9	11,810	2.3	28,000
Prior Earnings in -7Q	408.8	293.5	11,920	3.4	28,000
Prior Earnings in -8Q	281.3	298	11,910	2.4	28,000
Prior Earnings in -8Q to -5Q	1,019	1,082	47,630	2.1	28,000
Δ Prior Earnings from -8Q to -5Q	296.3	212.7	43.68	678.3	28,000
Less Than High School	0.028	0.017	0.097	28.9	28,000
High School	-0.077	0.026	0.457	-16.8	28,000
Some College	0.013	0.024	0.303	4.3	28,000
College or Higher	0.033	0.019	0.152	21.7	28,000
Female	-0.038	0.022	0.367	-10.4	28,000
Black	-0.007	0.014	0.094	-7.4	28,000
White	0.002	0.017	0.858	0.2	28,000
Other Race	0.005	0.010	0.049	10.2	28,000
Hispanic	-0.022	0.011	0.050	-44.0	28,000
Overall Tenure (quarters)	-0.259	0.820	63.96	-0.4	28,000
Petitioning-Firm Tenure (quarters)	-0.097	1.009	33.57	-0.3	28,000
Firm Age (years)	0.505	0.495	29.22	1.7	28,000
Log Firm Size	-0.023	0.079	7.731	-0.3	28,000
Year of filing	0.085	0.068	2010	0.0	28,000

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 1.5 year donut for each outcome, and a uniform kernel to weight observations.. 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 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 D.3 – Covariate Balance in RD, TAA-denied sample

	Discontinuity	S.E.	Control Mean	% diff	N
Predicted Prior Earnings in -8Q to -5Q	416.7	853.3	51,020	0.8	48,500
Prior Earnings in -1Q	322.0	312.6	12,210	2.6	48,500
Prior Earnings in -5Q	159.9	297.2	12510	1.3	48,500
Prior Earnings in -6Q	300.2	291	12660	2.4	48,500
Prior Earnings in -7Q	447.5	304.6	12480	3.6	48,500
Prior Earnings in -8Q	507.9	331.6	12550	4.0	48,500
Prior Earnings in -8Q to -5Q	1362	1152	50260	2.7	48,500
Δ Prior Earnings from -8Q to -5Q	238.9	162.7	92.33	258.7	48,500
Less Than High School	0.009	0.013	0.106	8.5	48,500
High School	-0.008	0.021	0.328	-2.4	48,500
Some College	-0.011	0.016	0.33	-3.3	48,500
College or Higher	-0.007	0.018	0.238	-2.9	48,500
Female	0.017	0.017	0.379	4.5	48,500
Black	-0.006	0.013	0.146	-4.1	48,500
White	-0.008	0.014	0.784	-1.0	48,500
Other Race	0.021	0.011	0.07	30.0	48,500
Hispanic	0.024	0.01	0.08	30.0	48,500
Overall Tenure (quarters)	-0.79	0.744	65.06	-1.2	48,500
Petitioning-Firm Tenure (quarters)	-0.104	0.764	27.95	-0.4	48,500
Firm Age (years)	-0.079	0.319	31.56	-0.3	48,500
Log Firm Size	0.15	0.082	9.236	1.6	48,500
Year of filing	-0.016	0.063	2011	0.0	48,500

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 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.

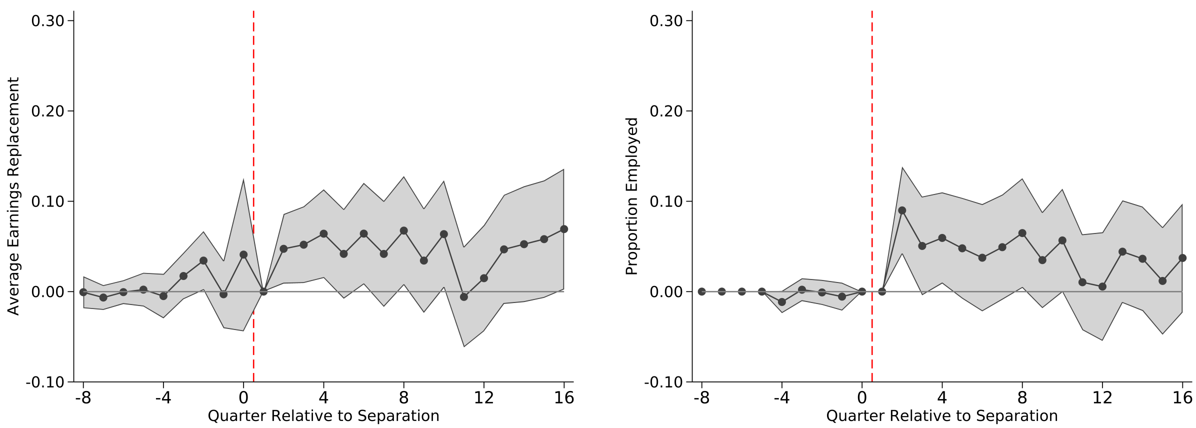
Table D.4 – Covariate Balance in D-RD

	Estimate	S.E.	Control Mean	% diff	N
Predicted Prior Earnings in -8Q to -5Q	582	984	50,420	1.2	76,500
Prior Earnings in -1Q	-174.3	487.3	10,960	-1.6	76,500
Prior Earnings in -5Q	-34.92	415.7	12,530	-0.3	76,500
Prior Earnings in -6Q	41.86	426.5	12,660	0.3	76,500
Prior Earnings in -7Q	-41.35	453.6	12,500	-0.3	76,500
Prior Earnings in -8Q	-81.61	473	12,540	-0.7	76,500
Prior Earnings in -8Q to-5Q	245.5	1,658	50,180	0.5	76,500
Δ Prior Earnings from -8Q to -5Q	-78.59	281.9	27.17	-289.3	76,500
Less Than High School	0.031	0.022	0.107	29.0	76,500
High School	-0.052	0.030	0.341	-15.2	76,500
Some College	0.008	0.027	0.323	2.5	76,500
College or Higher	0.026	0.026	0.232	11.2	76,500
Female	-0.049	0.028	0.382	-12.8	76,500
Black	0.000	0.020	0.147	0.0	76,500
White	0.003	0.021	0.783	0.4	76,500
Other Race	-0.011	0.015	0.070	-15.7	76,500
Hispanic	-0.033	0.016	0.080	-41.3	76,500
Overall Tenure (quarters)	0.443	1.160	65.36	0.7	76,500
Petitioning-Firm Tenure (quarters)	0.514	1.265	27.99	1.8	76,500
Firm Age (years)	0.667	0.558	31.47	2.1	76,500
Log Firm Size	-0.184	0.123	9.247	-2.0	76,500
Year of filing	0.050	0.094	2011	0.0	76,500

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

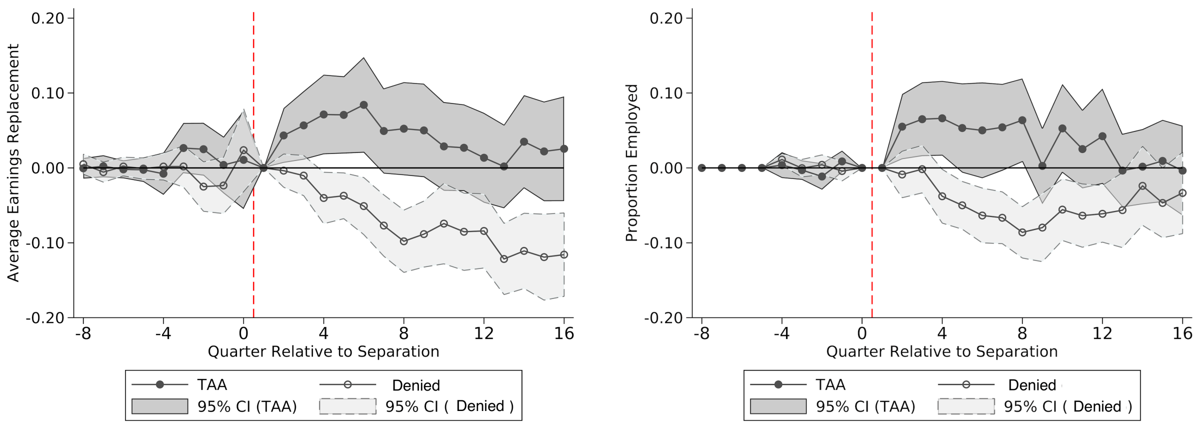
(A) Earnings replacement (B) Employment



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.

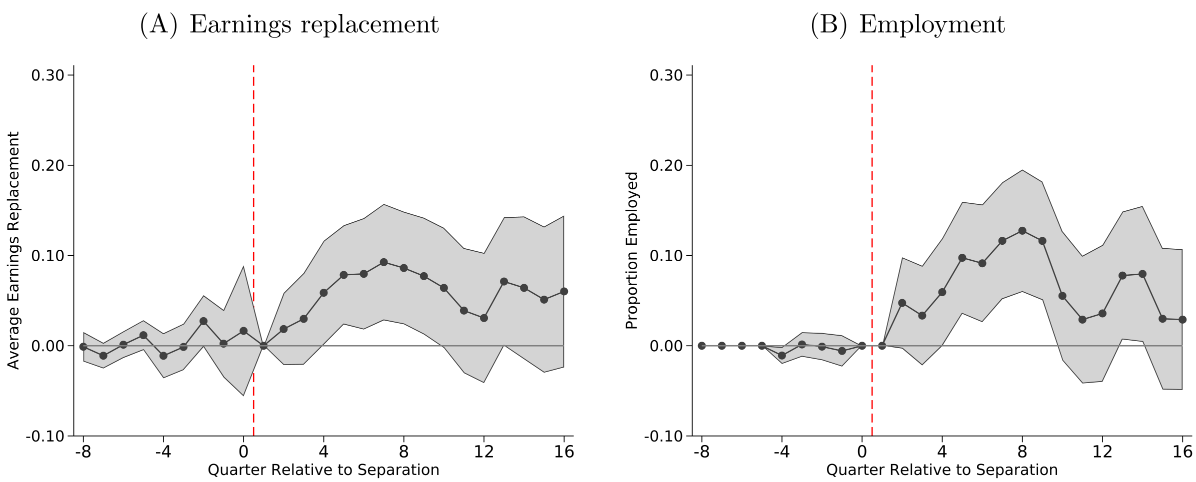
Figure D.5 – Robustness: Regression Kink (RK) Estimates

(A) Earnings replacement (B) Employment



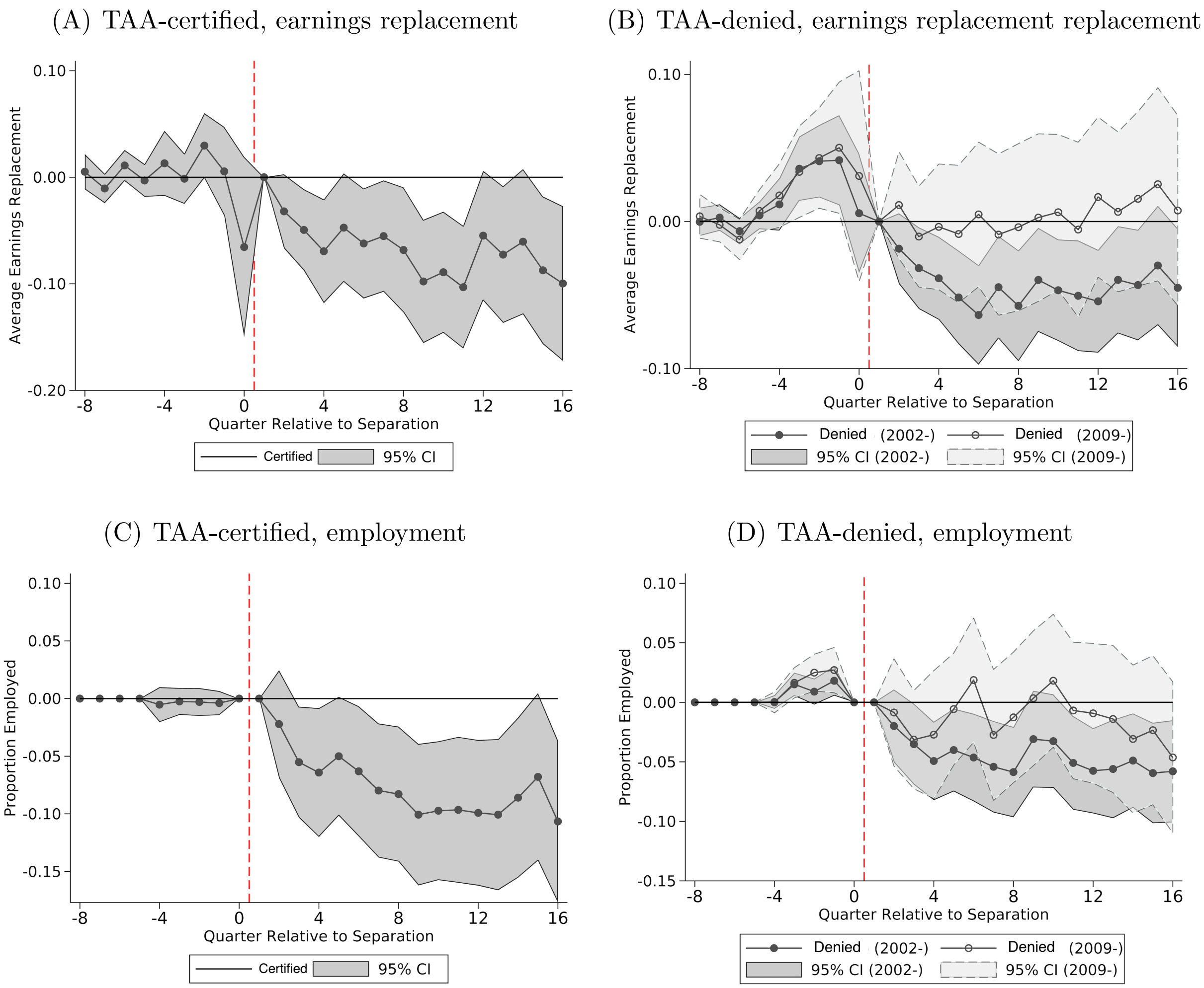
Notes: Panels plot estimates for earnings replacement (Panel A) and employment (Panel B) using a regression kink (RK) design. The eligibility rule for wage insurance introduces two kinks, which correspond to the edges of the donut used in the main analysis. The position of the lower kink varies with 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 β 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 D.6 – Robustness: D-RD Estimates, All Petitions without ML Restriction



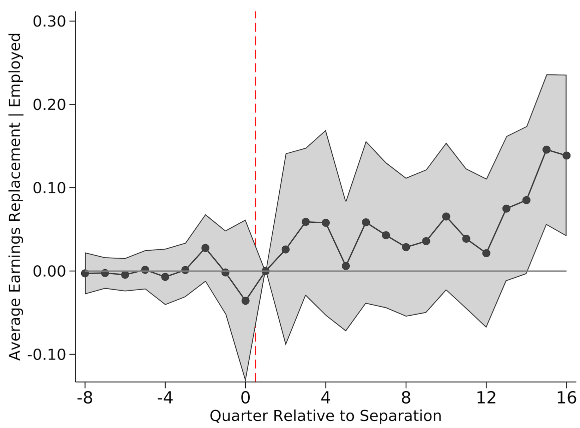
Notes: Panels plot D-RD estimates for earnings replacement (Panel A) and employment (Panel B) for all petitions, including those excluded from the ML sample. Shaded areas denote 95% confidence intervals. Samples are restricted to high labor force attachment in the second year before displacement, defined 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 D.7 – Falsification test: RD Results using Age 55 Cutoff



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.

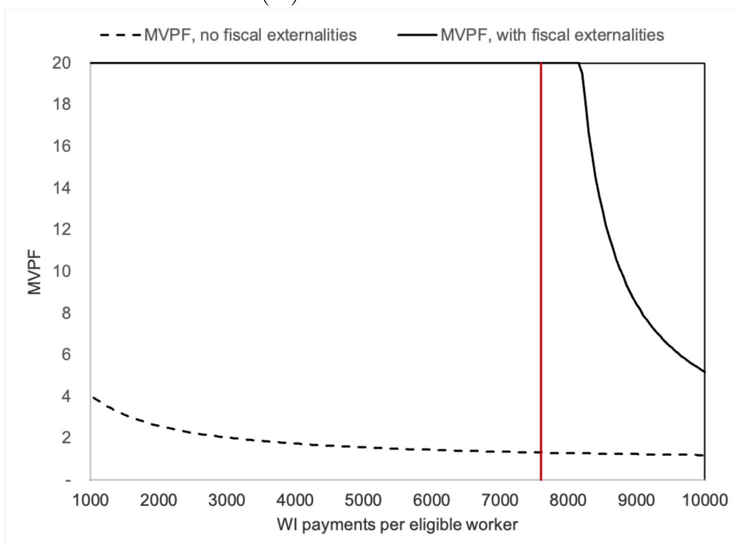
Figure D.8 – Earnings Replacement Conditional on Employment



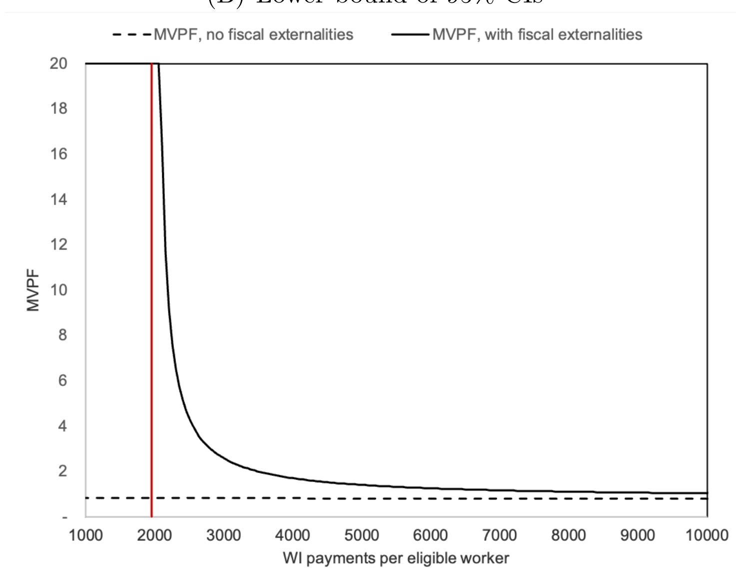
Notes: Figure plots D-RD results of earnings replacement rates conditional on employment from estimating equation (8) from 8 quarters pre-separation to 16 quarters post-separation. Earnings replacement rates are calculated as earnings relative to the average from the second year before displacement, and are 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 D.9 – MVPF vs. Wage Insurance Payments per Eligible Worker

(A) Point estimates



(B) Lower bound of 95% CIs



Notes: Figures plot the MVPFs vs. wage insurance (WI) payments per eligible worker using the point estimates for cumulative earnings and unemployment durations (Panel A) or the lower bounds of the 95% CIs (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 20 from above and indicate with red vertical lines the subsidy value at which fiscal externalities exceed program costs.

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:

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

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.

E 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.

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

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

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

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 $d\overline{w}/d\varphi < 0$.

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

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

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

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 Comparison to Constant Reemployment Subsidy

Here we compare wage insurance subsidies against a constant reemployment subsidy that does not depend upon the worker's pre-displacement or reemployment wages. The setup is identical to that in Section 3 and Appendix E with the exception that reemployed workers receive a fixed subsidy, given by ς, in any new job, irrespective of whether it pays less than the pre-displacement job.

Using the same approach as the analysis in Appendix E, one can show that increasing the value of the reemployment subsidy lowers the reservation wage.

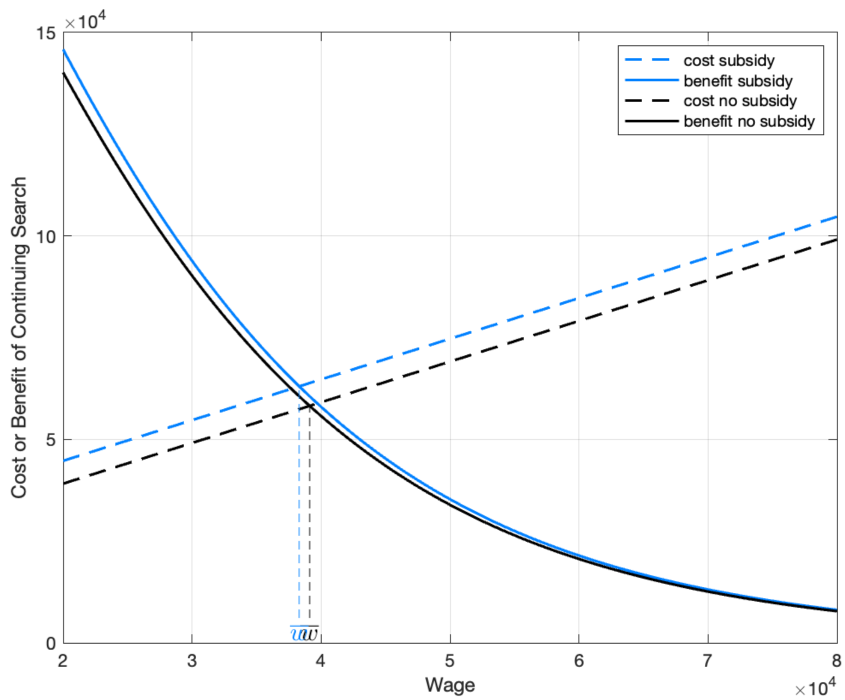
$$\frac{d\overline{w}}{d\varsigma} = -\frac{1}{1 + \lambda^* \frac{\beta}{1-\beta}(1 - F(\overline{w}))} < 0 \tag{15}$$

Similarly, search effort increases with an increase in the value of the reemployment subsidy.

$$\frac{d\lambda^*}{d\varsigma} = -\frac{\beta}{1-\beta} \frac{(1 - F(\overline{w}))}{c''(\lambda^*)} \frac{d\overline{w}}{d\varsigma} > 0 \tag{16}$$

We now compare the effects on workers' search behavior of wage insurance vs. a constant reemployment subsidy. To ensure a fair comparison, we set the magnitude of the reemployment subsidy such that the expected subsidy payments are identical under both policies. We use the simulation in Figure 2 as the baseline for wage insurance. Figure F.1 shows the equivalent diagram for the reemployment subsidy with equal expected subsidy payment. Because the subsidy amount is of equal value for all values of the reemployment wage, the costs and benefits of search no longer exhibit the kinks seen in Figure 2. As a result, the reservation wage falls much less with the constant subsidy than with wage insurance. In contrast, the search effort increases a bit more under the constant subsidy than with wage insurance. To summarize the implications of these changes in search behavior, we calculate impact of each policy on the worker's employment probability after one period. The employment probability increases by 6.6 percentage points for wage insurance while it increases by only 1.2 percentage points for the constant reemployment subsidy.

Figure F.1 – Optimal Reservation Wage With and Without Constant Reemployment Bonus



Notes: Figure plots the optimal reservation wage condition in equation (5) with a reemployment subsidy (in blue) and without (in black). See text for discussion. Illustrative simulation uses a lognormal wage distribution $F(w)$ with $\mu = 10.5$ and $\sigma = 0.5$; convex search cost function $c(\lambda) = \kappa \cdot \lambda^{1+\gamma}/(1+\gamma)$ with $\kappa =$ 500,000 and γ=1; β=0.95; and b=10,000. The subsidy value ς is chosen to yield the same expected subsidy payment to that under wage insurance in Figure 2.