Essays on Unemployment

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Abstract

This thesis examines the experiences of unemployed workers. The chapters use administrative, survey, and self-collected data to better understand how unemployment and the government insurance that covers it impact workers. I argue that unemployment insurance increases subsequent unemployment less than previously thought, and many earnings-relevant skills would likely hold steady during the additional unemployment. Collectively, the chapters show that the costs of unemployment insurance expansions are smaller than the existing consensus suggests.

The first chapter, written jointly with Geoffrey Schnorr, identifies the employment effects of approving additional unemployment insurance claims whose eligibility is in doubt. To be eligible for unemployment insurance in the United States and most other countries, workers must have lost their job through no fault of their own. We use administrative data on the universe of unemployment insurance claimants in California since 2002 to estimate how adjusting the leniency of these criteria would affect claimants' subsequent employment. By comparing claimants whose applications are as-good-as randomly assigned to relatively strict government employees with those assigned to relatively lenient government employees, we isolate the causal effect of approving the marginal cases. Empirically, we find that unemployment duration increases by approximately two weeks. Based on a simple theoretical model of optimal UI, we map this employment effect to the efficiency cost of unemployment insurance. We find that the efficiency costs of approving these marginal cases are much lower than the efficiency costs for other types of UI benefit expansions.

The second chapter, written jointly with Andrew Johnston and Attila Lindner, documents how survey-based skills evolve as workers remain unemployed. In addition to the psychological and financial costs that unemployment imposes on workers themselves, a central concern to policymakers is that unemployment may also harm long-term productivity if workers lose skills while unemployed. We provide direct evidence that this does not appear to be the case using a linked panel of skill elicitations and administrative employment records among newly unemployed German workers in the late-2000s. We validate that the skills are meaningful: baseline measurements predict prior earnings and panel measurements change around certain significant life events. Despite large falls in both subjective well-being and reemployment earnings as workers remain unemployed, we find that all of the earnings-relevant cognitive and noncognitive skills in the survey remain constant. We provide evidence that the lack of observed skill depreciation cannot be explained by various types of measurement error.

The third chapter, written jointly with Peter Ganong, aggregates the existing academic literature measuring the effect of UI benefit generosity on unemployment duration. Building on the first chapter, we hand-collect a comprehensive dataset of the 52 published studies that estimate this effect. While the theoretical and empirical consensus is that UI benefit generosity increases unemployment duration, we argue the magnitude is overstated due to publication bias: statistically insignificant or negatively-signed estimates are much less likely to be published. Correcting for publication bias decreases the overall average elasticity of unemployment duration with respect to UI benefit generosity by one-fifth to one-half. Aggregating evidence across studies, the predicted elasticity is 0.27 with respect to the replacement rate and 0.35 with respect to potential benefit duration in a policy regime similar to the typical U.S. state. However, the elasticity is larger by a factor of two or more under typical potential benefit duration extensions that occur during recessions.

JEL Classification: J24, J64, J65

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Chapter 1

No-Fault Job Loss? Less Moral Hazard: The Employment Effects of Unemployment Insurance for Marginally Eligible Claimants

Unemployment insurance (UI) eligibility requires a claimant to have lost their job through no fault of their own. Approximately 10% of claims are deemed ineligible solely based on the job separation reason. Using the systematic variation in separation-based eligibility approval rates across UI claim processing offices and examiners in California from 2002 to 2019, we show that granting UI eligibility causes approximately 2 additional weeks of nonemployment. These effects are the smallest for lower-income claimants. By replicating existing research designs for other UI policy margins within the California data, we conclude the efficiency costs of UI benefit expansions through separation-based eligibility criteria are lower compared to those of expansions through monetary eligibility, weekly benefit amount, or potential benefit duration.

1.1 Introduction

Unemployment insurance (UI) mitigates the financial consequences of involuntary job loss. However, at any given point in time over the past several decades, nearly two-thirds of the unemployed in the United States do not receive UI benefits. Table 1 decomposes the proximate reason for UI non-receipt among all unemployed people at a given point in time and among those who applied for UI over the course of their unemployment spell. One reason is the *intensive* margin of UI benefit generosity: there is a maximum limit on benefit duration for those who ever receive benefits. But another significant reason—larger by a factor of 9 when looking at the unemployed at a given point in time—is the *extensive* margin of UI benefit generosity: one needs sufficient prior earnings and a qualifying job loss reason in order to ever receive benefits. Ineligible job loss reasons drive almost two-thirds of this UI non-receipt.

The extensive margin of UI benefit generosity—also referred to as benefit eligibility criteria—affects many people, and its leniency is a policy choice. This is clear for the objective formula applied to administrative earnings records to determine monetary eligibility. But it also matters for the subjective determination of whether the claimant lost their job through no fault of their own, or what we refer to as separation-based eligibility. States explicitly vary in whether certain circumstances—such as quitting for family care responsibilities or getting fired for substance use—qualify as eligible job loss. For example, for each initial claim filed in the respective states with varying criteria, South Carolina has over three times as many of these eligibility denials as Hawaii. The federal government made this particularly salient during the Covid-19 pandemic, as the CARES Act temporarily extended pandemic-related UI benefits to anyone who quit as a "direct result of Covid-19" (Department of Labor, 2020).

Despite its policy relevance and the vast UI literature, there is scant evidence on the causal employment effects of UI extensive margin eligibility. There is robust evidence that intensive margin benefit amounts—either potential benefit duration (PBD) or weekly benefit amount (WBA)—increase unemployment duration, an effect often referred to as moral hazard (Schmieder and Von Wachter, 2016). It may be natural to think that the extensive margin would have qualitatively similar employment effects, but each margin's relative efficiency is unclear.

In this paper, we document the types of UI claimants affected by extensive margin eligibility criteria, identify the employment consequences of any UI benefit receipt, and derive the relative efficiency of various intensive and extensive margins of UI benefit generosity. We obtain causal estimates of separation-based UI eligibility on employment using a leniency-based research design with the universe of California UI records from 2002 to 2019. We show that socioeconomically disadvantaged groups are likelier to have benefits denied due to separation-based eligibility. Separation-based eligibility causally increases the duration of nonemployment by approximately two weeks. In comparison to the causal employment effects of other margins of UI benefit generosity that we also estimate in our data, we show the lowest UI benefit expansion efficiency costs correspond to separation-based eligibility benefit expansions.

For the descriptive analysis, our microdata allow us to show that disadvantaged claimants are likelier both to be investigated and to be denied due to separation-based eligibility. This is in line with a recent investigation of separation-based eligibility denials in Georgia during the first year of the Covid-19 pandemic (Donnan et al., 2021). And while Lachowska et al. (2021) show with coarser data from Washington State that employers are likelier to dispute claims for Black and less educated claimants, our ability to directly observe each step along the eligibility process allows us to demonstrate that issues and denials due to the claimant's self-report on their UI claim are just as common, particularly among socioeconomically disadvantaged groups.

For the causal analysis, understanding how claimants are affected by benefit eligibility is more demanding than simply comparing the characteristics of different claimant groups. A central concern is that there are unobserved differences affecting both eligibility approval and subsequent employment. For concreteness, suppose an employer initiates a job separation, but it is in dispute whether the worker who claims UI was laid off for lack of work or fired for misconduct. Prior misconduct could directly hurt the worker's future employment prospects through word-of-mouth, and the types of workers who engage in misconduct egregious enough to be denied UI are likely less attractive job candidates.

Our identification strategy leverages variation across UI claim processing offices in their propensity to approve separation-based eligibility issues. Because claims with separation-based eligibility issues are quasi-randomly assigned to these offices for adjudication, any office-level differences in approval rates can be attributed to the causal effect of assignment to the office itself. After confirming the existence of these office-level differences and their ability to recover the local average treatment effect (LATE) of UI eligibility in an instrumental variables framework, we estimate a statistically significant 2-week increase in nonemployment duration and an imprecise null effect on subsequent earnings. Our results are similar in a complementary research design based on the quasi-random assignment of claims with separation-based eligibility issues to individual government examiners within offices.

To situate these effects in the existing UI literature, we first interpret separation-based eligibility as changing the total weeks of PBD (from 0 to the claimant's PBD). By doing so, we can easily compare our effect on nonemployment both to the one existing extensive margin study on monetary eligibility and the many existing intensive margin studies on PBD. In this light, our nonemployment duration effect is half the existing one for monetary eligibility and around the 25$^{\text{th}}$ percentile of PBD estimates.

We contextualize these employment effect magnitudes as relatively small with an apples-to-apples comparison between all margins of UI generosity. Informally, we measure the per-dollar cost to the government of different types of UI benefit expansions. Formally, the object of interest is the ratio of behavioral costs to the government's budget compared to the transfer absent any behavioral response. This has been used in previous work to compare different intensive margins of UI generosity (Schmieder and Von Wachter, 2016), and we generalize its empirical implementation to study the extensive margin of benefit eligibility. Projecting employment effects from our main IV estimates onto the tax code delivers the behavioral costs, while projecting unemployment potential outcomes from an instrumental variables compliers analysis onto the tax code delivers the mechanical transfer. By directly estimating the quantities of interest—as opposed to routing auxiliary estimates through a behavioral model—we take an approach similar to Lee et al. (2021).

We highlight implications for UI policy using a simple Baily-Chetty optimal UI framework. In lieu of observing consumption-smoothing benefits to estimate an optimal extensive margin UI eligibility threshold, we make a bounding argument. Under standard regularity conditions, those on the margin of extensive margin eligibility have larger marginal utility decreases when denied due to their lower earnings and larger percentage income drops. On efficiency grounds, the estimated lower disincentive costs and these larger consumption-smoothing benefits point to the desirability of reallocating UI benefit generosity away from the intensive margin towards the extensive margin.[1]

This paper's primary contribution is estimating the employment effects of a previously unstudied UI policy. In contrast to the large literature on the employment effects of intensive margin UI benefit generosity, Leung and O'Leary (2020) is the only existing paper on the extensive margin.[2] Their monetary eligibility variation due to the minimum earnings threshold is local to workers with very low earnings and labor force attachment. Because this monetary eligibility variation applies to a very specific population, it is *a priori* unclear how it would extrapolate to the much larger and more economically diverse population at risk of separation-based eligibility denial.

In providing a theoretically-motivated benchmark for the effects of separation-based eligibility relative to several other UI policy margins, we also provide the most comprehensive comparison of UI policy margins in a given institutional context. The most closely related paper in this respect is Lee et al. (2021), which estimates the per-dollar transfer costs of increasing WBA and decreasing partial benefit marginal taxes based on variation within Washington State in 1995.[3] One other paper that studies the employment effects of multiple UI margins in the same setting is Landais (2015a), doing so for WBA and PBD within 5 US states in the late 1970s. We make a unified comparison across the two most commonly studied UI benefit intensive margins—WBA and PBD—in addition to two UI

[1]In practice, however, there is a mix of other costs that such a reallocation would potentially incur, such as an increase in job separations or a decrease in administrative costs (Lusher et al., 2022; Ragan, 1984; Solon, 1984). We discuss this and other welfare-relevant considerations in Section 1.5.2.

[2]A recent systematic review of causal estimates of UI disincentive effects contained 39 intensive margin estimates: 18 pertaining to weekly benefit amount and 21 pertaining to potential benefit duration (Schmieder and Von Wachter, 2016).

[3]Two other related papers estimate the per-dollar transfer costs of disability insurance policies (Haller et al., 2020) and pension policies (Haller, 2022). This quantity is also the fiscal externality term in the denominator of the Marginal Value of Public Funds (MVPF), which Hendren and Sprung-Keyser (2020) evaluate for a wide range of government transfer programs in the United States.

benefit extensive margins of eligibility that have received little attention in the literature. In doing so, we can more plausibly abstract from other institutional factors in reconciling effect size differences. Pertinent to policy, we provide suggestive evidence on a welfare-enhancing budget-neutral reform and highlight the additional causal effects necessary to understand its net impact.

This paper is the first application of a leniency design to UI benefit receipt. The most closely related leniency applications include disability insurance (Autor et al., 2019; Maestas et al., 2013; French and Song, 2014); retraining benefits (Hyman, 2018); and job search assistance (Schiprowski, 2020; Schmieder and Trenkle, 2020). A recent survey of the applied microeconomics literature finds over 70 studies employing a judges design, where the application span criminal justice; finance; health; patents; and various government programs at the federal, state, and local level (Chyn et al., 2022).

A final contribution based on our identification approach new to the UI literature is estimating treatment effect heterogeneity of UI benefits across a wide range of observable characteristics. Our UI benefit individual-level variation is vast and does not directly depend on prior income or age. With this power, we find that responses to UI are the lowest for lowest-income claimants; if anything, their unemployment duration decreases. We also find small effects for the top and bottom quartile age groups. In contrast, the existing UI literature relies on either interacting state-level panel variation in UI generosity with observable demographics (e.g., Chetty, 2008a), calibrating a lifecycle labor supply model (e.g., Michelacci and Ruffo, 2015), or estimating regression discontinuity designs at separate policy cutoffs across an age range of several years (e.g., Schmieder et al., 2012a).

1.2 Institutional Details

In this section, we highlight general UI eligibility rules. First, we discuss separation-based eligibility adjudication, which provides identification for our causal research design in Section 1.4. We also outline general UI benefit parameters, or the monetary implications of benefit denial. We focus on California because that is the setting of our causal analysis, but we conclude by comparing California's rules to those in other states. There is significant across-state variation in separation-based eligibility policy design, but California's rules are broadly similar to those in most states.

1.2.1 Overall Eligibility Criteria for UI

Eligibility criteria fall into two broad categories: monetary eligibility and nonmonetary eligibility.[4] Within each of these two criteria categories, there are conditions related to the prior employment spell and conditions related to the claimant's current actions. In order to receive any UI benefits, workers must establish initial eligibility. After doing so, they must continue certifying their eligibility each week in order to receive that week's benefit payment.

Monetary eligibility criteria pertain to prior or current income, and they are relatively easy for the state to verify using administrative earnings records. In order to receive any benefits at all, the worker must have accrued a minimum amount of earnings during approximately the year preceding the claim. The dollar requirement is low enough that nearly all newly unemployed formal workers meet it.[5] After the claimant establishes initial eligibility, any income the claimant receives can reduce that benefit week's payment, potentially to $0.

Nonmonetary eligibility encompasses all other eligibility criteria, and it includes the focus of our project: separation-based eligibility. Separation-based eligibility criteria require that the worker lost their job through no fault of their own. Unlike monetary eligibility criteria, which are objectively verifiable based on administrative records, separation-based eligibility criteria hinge on a subjective investigation into claimant and employer reports. And unlike other nonmonetary eligibility criteria—such as availability to start work, active job searching, and truthful reporting—that relate to the claimant's current actions, separation-based eligibility is exclusively retrospective.

[4] See the Department of Labor's Comparison of State UI Laws for a thorough discussion of UI policy parameters.

[5] For more detail on the minimum earnings requirement, see Section 1.11.2 where we use it in a separate research design.

1.2.2 Separation-based Eligibility Criteria and Detection

Rather than denying benefits to all workers who quit or were fired, separation-based eligibility criteria deny quits without "good cause" and firings with "misconduct". The California Employment Development Department (EDD) maintains a list of circumstances surrounding job loss that is intended to be exhaustive. These are broadly separated into quits and firings, and each detailed subcategory delineates how circumstantial details should affect the UI eligibility determination.[6] These detailed subcategories pertain both to work conduct and personal conduct.

Whether a given quit or firing is deemed no-fault is subjective, creating scope for claimants with similar circumstances to receive different eligibility determinations. The subjective criteria for firings hinge on the type, intention, and degree of employee misconduct, while those for quits highlight how "involuntary" the employee's decision was. EDD's UI Benefit Determination Guide contains a section titled "Weighing the Facts" that instructs adjudicating examiners to "imagine the Scales of Justice with both sides equally weighted" and consider whether the claimant's or employer's evidence carries more weight (California Employment Development Department, 2023). One instructive example is the guideline for adjudicating quits due to commute difficulty. The guidelines state that "because travel time is subjective, depending upon the claimant's situation and labor market area, there is no hard-and-fast answer".

Due to this subjectivity, states detect separation-based eligibility issues using attestations from the claimant and the previous employer. Figure 1-1 summarizes the eligibility issue detection process. All claimants select a separation reason upon claim filing; if they contend the reason was a layoff, the previous employer can dispute that contention. This process is starkly different from verifying monetary eligibility, which the state automatically checks upon claim filing using administrative earnings records.

Subject to some federal regulation, each state has the discretion to decide separation-

[6] The quit subcategories are attendance at school or training, conscientious objection, voluntary leaving, travel difficulty, domestic circumstances, health and safety considerations, the irresistible compulsion to use intoxicants, leaves of absence, personal affairs, leaving for other work, union relations, and wages and time. The firing subcategories are attendance, attitude toward the employer, dishonesty, health or physical condition, insubordination, use of intoxicants and drug testing, manner of performing work, neglect of duty, off-the-job-conduct, relations with coworkers and customers, union relations, and violation of employer rules.

based eligibility parameters. This is clearest for voluntary quits, as state law can explicitly designate qualifying circumstances. For example, in terms of provisions granting eligibility for specific circumstances, 27 states lack any related to joining the military, 9 lack any related to family member illness, 6 states lack any related to domestic violence, and 2 states lack any related to sexual harassment at work.[7] Apart from the presence of these general provisions, different states can require different types of documentation or apply a different burden of proof.[8]

1.2.3 Quasi-Random Assignment of Eligibility Issues to Offices and Examiners

A key feature in our California data that influences the eligibility determination is the quasi-random assignment of claims for examination. This occurs in two stages: first to an office and then to an individual examiner who generally works within the office. Our research designs use this assignment mechanism to isolate quasi-random variation in initial eligibility denials and benefit receipt, and our primary research design focuses on office assignment.

Once a claimant's self-report or employer's dispute triggers an eligibility investigation, it is assigned to a single office almost entirely based on the last two digits of the claimant's Social Security Number (SSN). The limited exceptions include claimants with special communication needs or employment from another state. Importantly for our causal research design, the last two SSN digits are quasi-randomly assigned by the federal government (Social Security Administration).

The mapping of the one-hundred final SSN digits to offices changes over time for two workload management reasons. First, the number of offices gradually increases from nine to fourteen. Second, even when the number of offices remains the same, EDD periodically reassigns SSN groupings from relatively understaffed offices to relatively overstaffed offices. Taken together, there are 19 distinct assignment regimes throughout our entire

[7] California has provisions for all of these circumstances except for joining the military. See Chapter V of the Comparison of State UI Laws.

[8] These rules also vary internationally. For example, many countries allow UI claimants to retain eligibility even after a quit or a firing but impose lower benefit levels, as in Thailand, or a several-month-long waiting period, as in Germany and Japan (Schmieder and Von Wachter, 2016).

sample period.

After being assigned to an office, claims are then assigned to a single examiner for investigation and determination. While all claims are quasi-randomly assigned to offices, only a subset of claims is also *a priori* quasi-randomly assigned to examiners. Quasi-random assignment to examiners arises from a scheduling queue that sequentially matches pending claims to available examiners in the assigned office who speak the claimant's language. Around 40% of claims are assigned to examiners in this manner. These assignments are useful for our supplementary examiner research design, as unobserved examiner characteristics are conditionally independent of unobserved claim characteristics. The other 60% of claims are taken up while awaiting automatic scheduling on an ad hoc basis by an examiner with unexpected availability.

1.2.4 Monetary Implications of UI Eligibility

Initial eligibility approval makes you likelier—but not perfectly so—to replace lost wages with UI. Broadly speaking, there are three reasons why some ineligible claimants do not receive UI and some eligible claimants do: incomplete take-up, delayed eligibility issue detection, and subsequent appeals.

First, claimants with eligible initial determinations may never receive benefits if they do not subsequently submit a weekly certification. Examples include becoming quickly reemployed, not satisfying ongoing nonmonetary eligibility because of insufficient job search activities, or simply failing to complete the certification form. This is common for claims both with and without any separation-based eligibility issues, occurring 17% and 23% of the time, respectively.

Second, claimants may receive some UI benefits before a separation-based eligibility issue arises. This could be triggered either by periodic government audits or delayed employer disputes. Because initial claimants must satisfy a 1-week waiting period before they can receive benefits and employers must dispute a claim within 10 days of receiving government notice about the claim, this is uncommon.

Third, successful appeals to Administrative Law Judges in the California Unemployment Insurance Appeals Board can reverse the initial eligibility determination. Claimants who successfully appeal an ineligible determination can receive backdated benefits as long as they certify continuing unemployment for those weeks. Conversely, employers who successfully appeal eligible determinations prevent any further benefit payments; the government may claw back previously paid benefits occurs if the initial determination was due to claimant misreporting.

We combine Department of Labor aggregate data on appeal propensities and success rates with our microdata that indicate other reasons for benefit receipt to demonstrate the quantitative importance of each channel. Figure 1-2 illustrates the mapping of eligibility approval to UI benefit receipt by reason for those with separation-based eligibility issues in California, where bar widths are proportional to sample counts. Claimant appeals are both common and often successful, while employer appeals are both rare and often unsuccessful. Take-up is the most common reason for benefit nonreceipt among eligible claimants.

The benefits that eligible claimants can receive as they remain unemployed are economically meaningful. State benefits replace approximately 50% of prior weekly earnings and usually have a maximum duration of 26 weeks. Replacement rates are relatively lower for workers with higher and more stable earnings, as the weekly benefit amount (WBA) is a function of the highest quarterly earnings during the base period and is capped at $450 per week for most of our sample period. Potential benefit duration (PBD) increases during recessions through federally-funded extensions, reaching 52 weeks in 2002 and 99 weeks from 2009 to 2012. The PBD can also vary across claimants, as those with variable quarterly earnings can have lower PBD.[9]

[9] California's rules are comparable to those in other states. Its $450 maximum WBA is approximately the median amount across states during 2019, where the lowest is $221 in Louisiana and the highest is $844 in Washington State. Nearly every state's usual PBD is 26, though there is some variation from 14 weeks in Alabama to 30 weeks in Massachusetts. For additional detail about how WBA and PBD are calculated in California, see Section 1.11.2.

1.3 Data and Descriptive Analysis: Who is Affected by Separation-Based UI Eligibility?

This section describes the administrative UI data from California. In addition to commonly used linked employer-employee quarterly earnings and paid UI benefits, we observe information on how the state adjudicates eligibility issues. Using this, we show that claimants with eligibility issues and denials are disproportionately from lower socioeconomic groups.

1.3.1 Data Sources

Our primary analysis links individual-level administrative datasets on UI claims and quarterly earnings from 2002 through 2019. Both datasets are maintained by the State of California's Employment Development Department (EDD): UI claims microdata since 2000 and quarterly earnings records since 1995.[10] We exclude years before 2002 due to missing data on processing office assignments and years after 2019 due to a vastly different policy regime during the COVID-19 pandemic.

UI claims microdata contain nearly all of the information that EDD collects from claimants or produces itself while administering claims. The claims data comprise three categories: claimant self-reports at initial filing, benefit payments throughout the claim, and agency documentation of how the claim was processed. The claimant's self-reports constitute most of the demographics we use. The benefit payments are primarily used as a measure for receiving any UI. Still, we can also observe detailed timing using each individual weekly payment's dollar amount, payment disbursal date, and compensated unemployment week. Finally, the agency documentation includes any eligibility issues that were detected along with the timing and outcome of the resulting investigations. Importantly for our research design leveraging leniency variation, we observe the processing office and examiner that handled the eligibility determination.

We measure employment using the near-universe of California quarterly earnings records. These linked employer-employee data include all UI-covered earnings, which does not cap-

[10] Subsets of these data have been used in a series of policy briefs on UI in California during the pandemic (Bell et al., 2022a).

ture some government, nonprofit, and informal employment. We use these data for two purposes. Earnings measures following the claim are our primary outcomes of interest. Our definition of nonemployment duration is consecutive quarters without any earnings, starting with the quarter following the initial claim.[11] This necessarily undercounts nonemployment duration by ignoring any within-quarter periods of nonemployment immediately following filing and immediately preceding reemployment. While this could plausibly bias our estimated nonemployment duration response downwards, we show in Section Section 1.5.2 that it also generates an upward bias for our welfare-relevant measure of eligibility expansion efficiency costs. Reassuringly, conclusions drawn from both estimates are consistent with each other.

Detailed UI eligibility determination data is a unique aspect of our UI records. For all determinations, we observe the separation-based eligibility issue type (e.g., quit or firing), de-identified but time-invariant identifiers for the processing office and examiner handling the determination, the determination date, and the initial eligibility determination. Additionally, since 2009, we observe the claimant's self-reported separation reason: quit, firing, or layoff.

Apart from the detailed eligibility data and realized processing office assignment we observe for all determinations, we observe data pertinent to the assignment of claims to examiners since 2017. The primary variable is the claimant's spoken language determines which examiners a claim *could* be sent to. Because the examiner interview is conducted verbally over the phone, the assigned examiner must be able to communicate with the claimant. Additionally, we can observe an indicator of whether the interview was scheduled algorithmically via the queue or via the ad hoc scheduling process. Claims scheduled through the queue do not involve any manager discretion, so the only claimant characteristic affecting assignment is spoken language.

[11] We use quarterly nonemployment duration rather than weekly insured unemployment duration because the latter is by definition unobserved for unpaid claims. For the purpose of defining unemployment duration, we right-censor at 8 quarters following the quarter of the initial claim. Both of these choices accord with the extant study on UI eligibility based on monetary eligibility criteria (Leung and O'Leary, 2020).

1.3.2 Claimants with Separation-Based Eligibility Issues are Lower-Income

Table 1.2 shows that disadvantaged groups are disproportionately likely to have a separation-based eligibility issue and denial. Each column contains sample sizes and average characteristics for a given subset of claimants, where each column is a strict subset of the adjacent column to the left. Among monetarily eligible claims, nearly one-third had at least one separation-based eligibility determination and 40% of these were denied. Comparing average characteristics across these groups of claims demonstrates that younger, non-white, female, lower-income, and higher state disability insurance-usage claimants are likelier to have a separation-based eligibility issue and for that issue to result in benefit denial.

Even when claims are eventually paid, a separation-based eligibility investigation or initial denial delays payments. Theoretically, there are two primary channels why such payment delays could affect a claimant's job search: decreased liquidity and increased benefit receipt uncertainty. Even conditional on the eventual receipt of benefit payments, both channels plausibly decrease unemployment duration. This is relevant to interpret the initial eligibility determination as a single policy lever that can impact claimants in multiple ways.

Empirically, eligibility investigations and denials disproportionately impact lower-income claimants. As a reference point, Column 1 of Table 1.3 shows the average prior quarterly earnings, fraction receiving any UI payments, and typical payment delays for claimants without eligibility issues. The mandatory 1-week waiting period means that the first covered week of unemployment usually occurs 10 days after claim filing, and processing logistics mean that claimants usually wait another week until receiving the payment itself. Because eligibility investigations require interviewing the claimant and former employer, Column 2 shows that the time to first payment is slightly delayed for claimants who are investigated but ultimately approved.[12] Because this delay is usually only 1 week, it likely does not have a meaningful impact on claimants outcomes. Finally, Column 3 shows that a successfully appealed separation-based eligibility denial delays payment receipt by over

[12]Both of these delay measures are well under the Department of Labor's definitions of UI agency timeliness.

two months as the case proceeds through the California Unemployment Insurance Appeals Board.

While the above patterns are evidence of disparate impact, it is important to note that we cannot determine the underlying cause. For example, when it comes to selection into having an eligibility dispute, it is possible this is because socioeconomically disadvantaged people work in jobs with a higher rate of quits and firings compared to layoffs. On the other hand, it is possible that employers are likelier to dispute the claims filed by socioeconomically disadvantaged people.[13] Similarly, when it comes to selection into an eligibility denial conditional on an eligibility issue, we cannot say whether lower-income claimants are likelier to have had separation circumstances befitting denial or are likelier to be denied conditional on their separation circumstances.

1.4 Causal Results: What are the Employment Impacts of Separation-Based UI Eligibility?

In this section, we present our main result that UI benefit eligibility has only modest effects on a claimant's subsequent employment outcomes. We focus on a design leveraging variation in the processing office to which claimants from 2002 to 2019 were assigned, and Section 1.11.1 shows the results are similar using a more granular source of variation over a shorter time period.

1.4.1 Design: Processing Office Assignment as UI Policy Variation

Our research design leverages the fact, as discussed in Section 1.2.3, that claims with separation-based eligibility issues are quasi-randomly to processing offices for adjudication. By comparing subsequent employment outcomes of claimants assigned to relatively lenient offices and those assigned to relatively strict offices, we aim to approximate an experiment that approves the marginal claimants with separation-based eligibility issues who

[13] Lachowska et al. (2021) employ a movers design with workers who file UI with different employers in different years and find a blend of these explanations: firms that pay all of their workers lower wage premia also are likelier to dispute UI claim eligibility.

were barely denied (or vice versa).

Treating the initial eligibility determination as the endogenous treatment of interest—and estimating its effect on marginally eligible claimants—corresponds to a policy reform that increases leniency when adjudicating separation-based eligibility issues. We argue this is more policy-relevant than the actual receipt of payments, as the initial eligibility determination is the policy lever at the UI agency's disposal. Nevertheless, we sometimes scale by measures capturing the monetary consequence of benefit eligibility, such as eligibility's effect on benefit receipt or the total potential benefit duration.

1.4.2 Implementation: Econometric Framework and Identification Assumptions

We first lay out the main estimating equation for the design based on assignment to processing offices. We present and validate the identifying assumptions necessary to interpret the coefficient of interest as a causal effect of UI eligibility.

Estimating Equation

We construct our instruments to isolate the quasi-random assignment of a given eligibility determination to a processing office. We ensure that identifying variation in eligibility approval stems from the SSN-based assignment regime, and we allow eligibility approval leniency within a given processing office to vary across quits vs. firings.

We directly observe the SSN-based assignment regime of claims to processing offices for only 11 years, so we infer this from the data. To do so, we define the intended processing office for a given combination of the final 2 SSN digits and claim filing month as the office that processed at least 95% of those SSN digits' claims in the month. This measure is comprehensive: 97% of digit-month combinations have a defined intended office, and it is missing mostly when the SSN-based assignment regime changes in the middle of a month. It is also externally validated: for the 11 years with written agency documentation of the SSN-based assignment regime, it agrees for more than 99% of digit-months.

At a given point in time, the mapping of claims to intended offices is quasi-random.

However, because both office assignment propensities and potential outcomes change over time, we additionally include time controls. Office assignment propensities change over time as office openings shift the number of SSN digits for which each office is responsible. Potential outcomes change with macroeconomic conditions that affect the composition of UI claimants and their job search prospects.

Our final instruments additionally interact the processing offices dummies implied by the SSN-based assignment regime with the separation-based issue type (i.e., discharge for misconduct vs. voluntary quit). Importantly, we include issue type as a control to partial out level differences in eligibility across issue types. Allowing for flexibility in leniency appears to be empirically relevant, as Figure 1-7 shows that office-level leniency is uncorrelated across issue types, even though there is statistically and economically significant variation across offices within each issue type.[14]

This strategy hinges on the issue type itself being a characteristic unaffected by the processing office. It would be a problem if there is often ambiguity in the proximate job separation reason and processing offices differ in classification tendencies.[15] In practice, this is rare. Only 7% of cases have claimant-reported separation reasons that do not match the final determination's issue type, and many of these are plausibly due to claimant transcription error during claim filing. Figure 1-8 shows that the minimal variation (1.3 percentage points) in the composition of separation-based eligibility issue types across offices is uncorrelated with issue-specific eligibility approval rates.

Consider the following system of equations for claimant i who files an initial claim in month t:

$$Y_{it} = \beta D_{it} + \mathbf{X}'_{it}\psi + e_{it} \tag{1.1}$$

$$D_{it} = \mathbf{Z}'_{it}\gamma + \mathbf{X}'_{it}\mu + \varepsilon_{it} \tag{1.2}$$

[14] One potential explanation—that we cannot empirically verify or rule out—is that offices differ in both their propensity to approve claimants vs. employers and their propensity to rule in favor of the party with vs. without the burden of proof. The variation in propensities would have to be similar in magnitude, but they could have an arbitrary correlation.

[15] Existing UI case law does contain a handful of precedents where this was the case, such as withdrawn resignations, mutual misunderstandings, and pressured resignations. See California Unemployment Insurance Appeals Board precedent summaries at `https://cuiab.ca.gov/precedent-decisions-a-d/`.

where Y_{it} is an outcome of interest (e.g., UI payment amount, subsequent earnings, nonemployment duration, etc.), D_{it} is an indicator for eligibility approval at the initial separation-based eligibility determination, $\mathbf{Z}_{it}$ is a vector of indicator variables corresponding to the interaction of assigned processing office and separation-based eligibility issue type, and $\mathbf{X}_{it}$ is the fully interacted set of claiming filing month-by-issue type dummies that serve as control variables. The coefficient of interest is β, and the equation is overidentified because the excluded instrument $\mathbf{Z}_{it}$ is a vector.

Our preferred estimator for Equations 1.2 and 1.1 is the two-step estimator unbiased jackknife IV (UJIVE) (Kolesár et al., 2013). Intuitively, this estimator extends the logic of jacknife IV (JIVE) to accommodate covariates (Angrist et al., 1999). To do so, it leaves out the own observation in the first-stage given by Equation 1.2 both when partialling out the covariate fixed effects and when projecting the vector of office-by-issue instruments onto the endogenous treatment. Results are qualitatively similar when we employ alternative estimators like 2SLS and LIML.

Following a design-based approach to inference due to our individual-level variation, all specifications cluster standard errors by claimant (Abadie et al., 2022). We do so because SSN-based randomization is persistent for individual i making multiple claims in time t and t'. There are indeed individuals with multiple separation-based eligible issues in the sample period: our sample includes 6.9 million initial claims with eligibility issues from 5.5 million unique individuals.

Validating the Instrument

The coefficient β in Equation 1.1 identifies a local average treatment effect as long as four broadly defined conditions hold: first-stage relevance, independence, exclusion, and monotonicity. We present supporting evidence of each in the form of testable implications and institutional details.

First-stage relevance. The first identification assumption—first-stage relevance—requires that office assignment be predictive of separation-based eligibility. We directly test this by estimating the first-stage regression equation Equation 1.2 and testing the joint significance of the office-by-issue dummies.[16] Column 1 of Table 1.4 shows the first-stage F-statistic for this regression is 405, which is well above the threshold of 104.7 that ensures 95% coverage without any adjustment to standard errors (Lee et al., 2022).[17] Figure 1-10 plots the coefficients from the first-stage regression; variation in eligibility approval propensity is evenly spread across office-by-issue pairs.

Independence. The second identification assumption—independence—is bolstered in part by the SSN-based assignment of claims to processing offices, and we present evidence in favor of it in the third through sixth columns of Table 1.4. As before, we test the joint significance of the office-by-issue instruments in Equation 1.2. In each regression, the outcome variable is a pre-existing claimant characteristic. For several different claimant characteristics, the range across office-by-issue pairs is small we do not find statistically significant differences across office-by-issue pairs.

Excludability The third identification assumption—excludability—requires that the only effect processing offices have on the endogenous outcome Y is through the initial eligibility determination. The primary excludability concerns relate to the other administrative processing duties that offices handle, as there are economically small but statistically significant differences in these measures. The last two columns of Table 1.4 show that the expediency of making an eligibility determination and the propensity to find continuing claim issues differs across offices.[18] Both of the F-statistics are statistically significant, but the range across office-by-issue pairs is only 4 days and 3 percentage points, respectively. Reassuringly, Figure 1-9 shows that these minor differences are uncorrelated with eligibility approval propensity. Accordingly, in Section 1.11.1, we show that results for the design based on examiner assignment are similar when including or excluding office-level variation.

While processing offices handle several administrative duties, it is important to note that

[16]Testing the joint significance of the office-by-issue dummies $\mathbf{Z}_{it}$ rather than a constructed scalar leniency correctly accounts for the degrees of freedom in the overidentified setup (Hull, 2017).

[17]When the first-stage F-statistic falls below this level in specifications with limited subsamples, we inflate standard errors following the tF-adjustment of Lee et al. (2022).

[18]Continuing claim issues include failure to engage in work search, failure to be available for work, and irregular reporting. These most often results in denying benefits for only the relevant week.

they are not responsible for job search assistance or training. Job postings are provided on a centralized online board, CalJOBS[SM], for which all UI claimants are required to register. Similarly, personalized workforce services are accessible based on geographic proximity—rather than assigned based on SSN digits—through American Jobs Centers of California[SM].

Our results primarily focus on eligibility approval as the endogenous treatment of interest, as it is a clear policy level of the government. However, we also contextualize magnitudes using other measures of UI benefit receipt, such as by scaling the effect of eligibility on receiving UI payments. When doing so, positive bias from plausible exclusion restriction violation makes our main findings of small employment responses even starker. The likely source of an exclusion restriction violation is UI benefit timing. Conditional on receiving UI benefits, a claimant assigned to a relatively lenient office is likelier to receive those benefits through an initial eligible determination rather than a successful appeal following an initial ineligible determination. Based on the third row of Table 1.3, this means the claimant waits, on average, over 2 fewer months to receive the first payment. If anything, the increased liquidity afforded by more expedient payments leads to a longer unemployment duration.

Monotonicity. Independence, excludability, and first-stage relevance are sufficient in the constant effects setup in Equation 1.1 and Equation 1.2. However, if we allow for heterogeneous treatment effects so that the parameter of interest in Equation 1.1 is β_i, then we require some form of a monotonicity assumption. Monotonicity assumptions place restrictions on the first-stage relationship in Equation 1.1, and they ensure that the resulting estimand of interest in Equation 1.1 can be interpreted as a local average treatment effect (LATE) by weighting individual treatment effects with weakly positive weights that sum to 1 (Imbens and Angrist, 1994).

Broadly speaking, there are two types of monotonicity concerns stemming from Equation 1.1: the assumed homogeneity of γ and the included control variables $\mathbf{X}_{it(s)}$. The first type of concern yields testable implications, and we provide evidence in favor of it. The second type of concern motivates specific robustness checks, which we explore in Table 1.8.

The first type of monotonicity concern is that compared to another office-by-issue pair, a given office-by-issue pair may be relatively more lenient with one type of claimant but relatively less lenient with another type. This applies even under unconditional random assignment; for simplicity, suppose that is the case so that Equation 1.2 omits the fixed effect controls. A sufficient monotonicity condition to recover a LATE would then be *average monotonicity* (Frandsen et al., 2023; Chan et al., 2022). Informally, this requires that office-by-issue-specific eligibility approval and overall eligibility approval are positively correlated for each claimant. Formally, the average monotonicity condition is:

$$\sum_{j \in J} \lambda_j \left(p_j - p\right)\left(D_i(j) - \overline{D}_i\right) \geq 0 \quad \forall i \tag{1.3}$$

where J is the set of office-by-issue pairs, J_i is the office-by-issue pair corresponding to claimant i, $\lambda_j := Pr(J_i = j)$ is the probability a claim is assigned to j, p_j is the approval probability for claims in office-by-issue pair j, $p := \sum_{j \in J} \lambda_j p_j$ is the average approval probability across all offices and issues, $D_i(j)$ is the counterfactual determination if claimant i were assigned to j, and $\overline{D}_i := \frac{1}{|J|} \sum_{j \in J}$ is claimant i's overall expected approval probability.

A test to assuage the first type of monotonicity concern is showing that the same overall first-stage relationship holds within various subsamples (Frandsen et al., 2023). A useful auxiliary object for this test is the predicted first-stage from Equation 1.2. To calculate it, we manually implement the residualization and leave-one-out procedure. Specifically, let $A_{it(os)}$ be an indicator eligibility approval of claimant i's claim filed in month t with issue types s and an SSN that implies they should be assigned to office o. We residualize this by fully-interacted office-by-issue controls in Equation 1.2:

$$A^*_{it(os)} = A_{it(os)} - \mathbf{X}'_{it(s)}\mu \tag{1.4}$$

We then calculate the scalar leave-one-out mean of this residualized leniency measure at the office-by-issue level, which is indexed by os:

$$\tilde{Z}^{os}_{it} = \left(\frac{1}{n_{os} - 1}\right)\left(\sum_{k(os)} \sum_{t(oj)} A^*_{kt(os)} - A^*_{it(os)}\right) \tag{1.5}$$

where n_{os} denotes the total number of algorithmically scheduled separation-based eligibility issues of type s assigned to office o.

In support of the monotonicity assumption, Figure 1-3 shows that it is positively correlated with the claimant's own eligibility approval decision within various subsamples. This holds for claimant demographics and prior employment. Along these lines, Table 1.8 shows that even when we allow leniency to vary at various socioeconomic levels or over time, these group-specific or period-specific approval probabilities are highly correlated for a given office-by-issue pair.

The second type of monotonicity concern is related to the *conditional* quasi-random assignment that motivates including fixed effects. There are two conceptually distinct issues that stem from including controls in the specification, but both can be addressed in a specification that fully saturates Equation 1.1 and Equation 1.2 with interactions between the office-by-issue instruments and the fixed effect controls.[19]

We confirm that our results are not sensitive to the second type of monotonicity concern. In particular, we run separate regressions in each of the 480 month-by-issue cells and manually aggregate the estimates in proportion to cell size. Each separate regression is within a given month-by-issue cell, so it does not require additional controls. This permits flexibility in the first and second-stage equations for each month-by-issue pair, so none of them is susceptible to the second type of monotonicity concern. Reassuringly, as shown in Table 1.8, all of the manually reweighted estimates fall within the confidence intervals from our main specification.

1.4.3 Results: Eligibility Affects UI Receipt More than Employment

Unsurprisingly, initial eligibility approval increases UI benefit receipt. The top panel of Columns 1 through 3 of Table 1.5 show estimates of Equations 1.1 and 1.2 using UJIVE, where the endogenous treatment D is eligibility approval and the outcome Y is some mea-

[19]One possible issue is if unmodeled heterogeneity in the first-stage effects γ in Equation 1.2 is correlated with the included fixed effects. Goldsmith-Pinkham et al. (2022) show this would induce *contamination bias*, meaning that the estimated office-by-issue effect in one entry of γ would depend on the true office-by-issue effect in another entry of γ. Another concern stems from treatment effect heterogeneity in the second-stage equation: β in Equation 1.1. In this case, Blandhol et al. (2022) show it is possible for a specification that does not include full interactions between the instruments and covariates to deliver negative weights.

sure of benefit receipt. The bottom panel estimates the average value of the outcome Y among those who were on the margin of eligibility approval but received an ineligible determination.[20]

For those on the margin of eligibility approval, eligibility approval doubles various measures of benefit receipt. For example, Column 1 shows the probability of receiving any UI payments increases from 31% to 63%. Panel (a) of Figure 1-12 illustrates the doubling of payment receipt by presenting the “visual IV", where the causal effect is represented by the positive relationship between the office-by-issue-level reduced-form differences in payment receipt vs. first-stage differences in eligibility approval Angrist (1990). Interestingly, while the 31% receipt rate for marginally denied claimants roughly coincides with the 26% average benefit receipt rate among all separation-based eligibility denials given in the final column of Table 1.2, the 63% receipt rate for marginally approved claimants is somewhat lower than the receipt rates among either all claims without eligibility issues (77%) or all claims with separation-based that were nevertheless approved (83%), as shown in Table 1.3.

Our main result is that, despite doubling UI benefit receipt, separation-based eligibility approval has virtually no effect on subsequent earnings and only modest effects on subsequent unemployment. Columns 4 through 6 have the same structure as Columns 1 through 3, except the outcome Y is some measure of subsequent employment. Column 4 reports the near-zero, statistically insignificant impact on average subsequent quarterly earnings over the 7 quarters following the initial claim. The point estimate is 17, indicating that UI eligibility *raises* subsequent total earnings by just over \$100. However, uncertainty is large: the standard error is an order of magnitude larger than the point estimate.

An indicator for any employment in a quarter is much more precise outcome than total earnings in the quarter, and examining this outcome reveals decreases caused by UI eligibility. Column 5 shows that approved claimants are 4 percentage points less likely to have any employment in the quarter following the initial claim. This difference is just under one-

[20]Following Frandsen et al. (2023), we estimate the untreated complier mean Y^0 by interacting Y with an indicator for eligibility *denial* $(1 - D)$ in Equation 1.1, replacing the indicator for eligibility approval D with an indicator for eligibility denial $1 - D$, and estimating the system. We can also estimate the treated complier mean by interacting Y with an indicator for eligibility *approval* D in Equation 1.1.

tenth the untreated complier mean employment rate in that quarter (0.49), and it is highly statistically significant.

We also find increases in nonemployment duration when we translate the lack of employment in a given quarter into a nonemployment duration. Column 6 shows that eligibility lengthens nonemployment by 0.14 additional quarters, and the effect is marginally statistically significant. This less than 2-week increase in nonemployment duration is small relative to the more than 10-week increase in unemployment benefit receipt. However, because our quarterly measure is an underestimate of the underlying weekly measure as discussed in Section 1.3.1, the estimated treatment effect will be an underestimate in absolute value.

Dynamics. We next show the dynamics behind our main results in Table 1.5. To do so, we run separate regressions of the form in Equation 1.1 and Equation 1.2, where the outcome $Y(k)$ is now a benefit or employment outcome measure k time periods relative to the inital claim filing. Figure 1-4 displays these dynamic treatment effects, and Figure 1-13 further decomposes those treatment effects into potential outcomes for marginally approved and denied claimants.

Eligibility approval has the largest impact on payment receipt approximately 1-3 months after claim filing, but its effect on cumulative benefit receipt continues to grow throughout the subsequent year. Panel (a) of Figure 1-4 shows the dynamic treatment effects of eligibility approval on both paid and covered weeks, where the former is the calendar week the UI benefit is paid and the latter is the week of unemployment for which the payment compensated. The moderate gap between paid and covered week treatment effects during the first month reflects retroactive benefit payments made to cover certified weeks while the eligibility determination process was ongoing.

We do not find any evidence of employment hysteresis, as any impacts on contemporaneous employment or earnings are statistically insignificant and roughly zero 8 quarters after the initial claim. Panels (b) and (c) of Figure 1-4 plot treatment effects for employment and earnings, respectively, in a 4-year window centered around claim filing. Eligibility approval estimates using employment outcomes are consistently statistically insignificant prior to claim filing, which further bolsters the independence assumption of our research design. There is an immediate decrease in the probability of any employment, but the effect diminishes over time and disappears at the end of the window. In contrast, there is never a statistically significant effect of eligibility approval on average quarterly earnings. A majority of point estimates following claim filing are actually positive, and all are quite close to 0.

Figure 1-13 contextualizes the magnitude of treatment effects relative to baseline outcome outcomes by separately plotting potential outcomes for marginally approved and denied claimants. The dissipation in treatment effects on employment probability is driven by an increase in employment among the approved rather than a decrease in employment among the denied. For both marginally approved and marginally denied claimants, there is a persistent decrease in any employment and total earnings of around one-third. This is similar to unemployment earnings losses documented in previous work (Davis and von Wachter, 2011; Jacobson et al., 1993). This is evidence against an interpretation that separation-based eligibility's relatively small effect is driven by labor force exit irrespective of UI eligibility.

Heterogeneity. Individual-level variation with a very strong first-stage allows us to show that employment effects are the smallest for those with the lowest prior earnings. Table 1.6 separately re-estimates Equation 1.1 and Equation 1.2 within specific subsamples. One notable source of heterogeneity is that the point estimate for any earnings in the quarter following the UI claim is 0 among claimants in the lowest quartile of prior earnings. Pairwise differences across earnings groups are marginally statistically insignificant, though the negative effects on employment are consistent for the other earnings groups.

Another suggestive pattern of heterogeneity is the lack of response to eligibility for the youngest and oldest workers. While we cannot observe schooling or retirement decisions, it is plausible that these other labor force categories particularly relevant to these groups are mediating factors. These findings are independently pertinent to the academic literature. First, they support findings from theoretical lifecycle models that moral hazard responses should be lowest for the youngest workers (Michelacci and Ruffo, 2015). Second, they suggest one should use caution when extrapolating from a UI policy local average treatment effects (LATEs) based on an age cutoff (Schmieder et al., 2012a; Lalive, 2008; Centeno and Novo, 2009; Caliendo et al., 2013).

While there are relatively few claimants who were on state disability insurance prior to filing for UI, the quantitatively largest degree of heterogeneity is along this margin. In particular, UI eligibility *decreases* nonemployment by 5 weeks for those who recently claimed disability insurance but increases it by 3 weeks for those who did not. One explanation is that the work search requirements embedded in UI shorten unemployment durations for those who can otherwise plausibly substitute to other margins of social insurance.

We do not find any patterns in heterogeneity by time period, gender, or race in Table 1.6. Notably, even though the maximum potential benefit duration is much higher during and soon after the Great Recession, we find the employment effects of eligibility are smallest during and soon after this period.

To test whether heterogeneity in the effect of UI receipt is a likely mediator of heterogeneity in employment responses, Table 1.9 runs the subsample regressions with the outcome of receiving any UI payments. Indeed, the patterns of heterogeneity in employment responses across income groups and time periods is mirrored by heterogeneous effects on UI receipt. For example, eligibility increases UI receipt by 28 percentage points for the lowest-earners but 39 percentage points for the highest earners. Nevertheless, the heterogeneity in payment effects across prior income cannot explain the heterogeneity in employment effects for two reasons: the former increase monotonically while the latter is different for only the lowest earners. However, heterogeneity in payment effects does seem to explain much of the heterogeneity in employment effects across time periods. This complements Bell et al. (2022b), who find that the elasticity of employment with respect to WBA rises during recessions due to longer PBDs and baseline unemployment durations during those time periods.

The one subsample without an effect of initial eligibility approval on UI receipt are those who self-report a layoff. This is the set of claimants whose eligibility investigation is triggered by an employer dispute. The lack of an effect on payments among this group explains the null employment response to eligibility found in Table 1.6. One explanation for why there is no effect of initial eligibility on payments is that the marginal denials stemming from employer disputes are particularly likely to be overturned upon later appeal.

1.5 Policy Implications: How Do the Effects of Separation-based Eligibility Compare to Other UI Policies?

This section contextualizes the employment effects estimated in Section 1.4 to show that the employment effects of separation-based eligibility imply lower efficiency costs than those for other UI policy margins. The other UI policy margins we consider are extensive margin monetary eligibility and intensive margin benefit amounts through potential benefit duration (PBD) and weekly benefit amount (WBA). In addition to drawing on existing estimates from the literature for the other UI policy margins, we replicate those designs in our California data to make consistent comparisons within the same institutional context and dataset. Section 1.11.2 further details the sample construction, identification assumptions, and estimation procedures underlying these replication estimates.

1.5.1 Comparing Nonemployment Responses to Benefit Weeks for Some UI Policy Margins

We begin by showing that, in terms of additional weeks of nonemployment per week of benefit eligibility, our separation-based eligibility estimates based on office assignment are small relative to the existing literature. While we will show in Section 1.5.2 that this is not the welfare-relevant metric for measuring efficiency costs—as it does not scale by the mechanical transfer to claimants—it is a helpful benchmark for understanding the absolute magnitude of the behavioral response to UI benefits.

PBD shifts benefit weeks on the intensive margin, while monetary and separation-based eligibility do so on the extensive margin. For PBD, we draw on the 21 available estimates in Schmieder and Von Wachter (2016) Table 1 Column 5; for monetary eligibility, we use the single existing estimate in Leung and O'Leary (2020); and for separation-based eligibility, we show our main estimate based on office assignment in Section 1.4.

We employ two scaling procedures to make the extensive margin directly comparable to the intensive margin. First, we account for the fact that extensive margin eligibility only partially translates to benefit receipt, as discussed in Section 1.2.4. We scale the causal effect of eligibility on consecutive quarters of nonemployment by the causal effect of eligibility on UI benefit receipt.[21] Second, we translate quarters of nonemployment into weeks of unemployment and UI eligibility into benefit weeks. The former is a factor of 13, and the latter is a factor of total PBD including extended benefits. Table 1.12 shows that the average total PBD is 45 weeks for the office-assignment design's sample from 2002 to 2019 that includes the Great Recession, while Table 1 Column 2 in Leung and O'Leary (2020) shows this is 32.1 weeks for their analysis sample.

Figure 1-5 plots the effects of an additional week of potential benefits on weeks of consecutive nonemployment. All of the extensive margin estimates are within the interquartile range of the intensive margin estimates, but our separation-based eligibility estimate is around the literature's 25$^{\text{th}}$ percentile estimate for PBD and below the literature's existing estimate for monetary eligibility.

There are two drawbacks to this comparison of nonemployment responses: comprehensiveness and welfare-relevance. First, it does not facilitate comparisons with WBA expansions. Second, as we demonstrate in the next section, welfare-relevant costs of benefit expansions require additionally (i) translating this behavioral response to a fiscal externality on the government budget and (ii) scaling by the mechanical benefit transfer to claimants if their behavior were held fixed.

1.5.2 Comparing Per-Dollar Transfer Costs for All UI Policy Margins

Our preferred metric for comparing different UI policies is the per-dollar cost to the government of a given UI benefit expansion, or the ratio of behavioral costs to mechanical costs (BCMC ratio). We proceed by first deriving the welfare-relevance of this quantity in a stylized optimal UI framework, which highlights its theoretical advantages relative to the

[21]For monetary eligibility, this is the fuzzy RDD estimate reported in Table A1 Column 2 in Leung and O'Leary (2020): receiving UI benefits increases nonemployment quarters by 0.571. For separation-based eligibility, this is the ratio of two overidentified instrumental variables estimates; we obtain similar results directly using UI benefit receipt as the endogenous treatment in the instrumental variables design.

quantity in the previous section. We then map it to our main results on the employment effects of separation-based eligibility. Finally, we compare it to estimates corresponding to other UI policy, both from the existing academic and from replications in our California data. We find the BCMC ratio is lowest for separation-based eligibility.

Theoretical Motivation

Before demonstrating the BCMC ratio's theoretical relevance, we first define it formally.[22] Consider a vector of UI policy rules θ (e.g., separation-based eligibility approval probability, monetary earnings threshold, WBA, or PBD), a scalar earnings tax rate τ, and an optimizing representative worker's resulting set of choices $\mathbf{Y}(\theta,\tau)$ (e.g., search effort while unemployed, diligence while employed, etc.). Given government policy, these choices imply UI benefits $B(\mathbf{Y}(\theta,\tau);\theta)$ and taxes paid $T(\mathbf{Y}(\theta,\tau);\tau)$. The government's budget G is net transfers $B(\mathbf{Y}(\theta,\tau);\theta) - T(\mathbf{Y}(\theta,\tau);\tau)$. Consequently, the total government of a UI policy change $\mathrm{d}\theta_j$ along a policy margin j can be decomposed into an indirect effect due to behavioral responses and a direct effect due to the mechanical benefit transfer:

$$\underbrace{\frac{\mathrm{d}G}{\mathrm{d}\theta_j}}_{\text{total}} = \frac{\mathrm{d}B(\mathbf{Y}(\theta,\tau);\theta) - T(\mathbf{Y}(\theta,\tau);\theta,\tau)}{\mathrm{d}\theta_j} = \underbrace{\left(\frac{\partial B}{\partial \mathbf{Y}} - \frac{\partial T}{\partial \mathbf{Y}}\right)\cdot\frac{\partial \mathbf{Y}}{\partial \theta_j}}_{\text{behavioral}} + \underbrace{\frac{\partial B}{\partial \theta_j}}_{\text{mechanical}} \tag{1.6}$$

The ratio of the second terms and third terms in Equation 1.6 is the BCMC ratio, and we next show it features prominently in the welfare effects of UI policy reforms. For the worker, let $U(\cdot,\cdot,\cdot)$ be the representative worker's utility function with arguments B (i.e., consumption while unemployed), T (i.e., consumption while employed), and $\mathbf{Y}$ (i.e., other choices). The worker treats government policy θ and τ as fixed. Denote the worker's resulting indirect utility function by $V(\theta,\tau)$. For the government, its balanced budget constraint $G(B,T) = 0$ implicitly defines a function $\tau(\theta)$:

$$\frac{\mathrm{d}\tau}{\mathrm{d}\theta_j} = -\frac{\frac{\partial G(\mathbf{Y}(\theta,\tau);\theta,\tau)}{\partial\theta_j}}{\frac{\partial G(\mathbf{Y}(\theta,\tau);\theta,\tau)}{\partial\tau}} = -\frac{\frac{\mathrm{d}G}{\mathrm{d}\theta}}{\frac{\mathrm{d}G}{\mathrm{d}\tau}} \tag{1.7}$$

[22] The following discussion closely follows the derivations in Schmieder and von Wachter (2017) and Lee et al. (2021).

The welfare change due to a UI policy reform $\mathrm{d}\theta_j$ in utility terms is:

$$\begin{aligned}\frac{\mathrm{d}V}{\mathrm{d}\theta_j} &= U_1 \cdot \frac{\partial B}{\partial \theta_j} + U_2 \cdot \frac{\partial T}{\partial \tau} \cdot \frac{\mathrm{d}\tau}{\mathrm{d}\theta_j} \\ &= U_1 \cdot \frac{\partial B}{\partial \theta_j} - U_2 \cdot \phi \cdot \frac{\mathrm{d}G}{\mathrm{d}\theta_j}\end{aligned} \tag{1.8}$$

where the first line comes from applying the envelope theorem to the worker's problem while respecting the government budget constraint while the second line comes from substituting in Equation 1.7 and denoting $\phi := \frac{\partial T}{\partial \tau} \cdot \frac{\mathrm{d}G}{\mathrm{d}\tau}$ as the mechanical share of total deficit reduction following a tax increase. Intuitively, first-order welfare changes come only from changes to the tax and transfer system. The mechanical UI transfer $\frac{\partial B}{\partial \theta_j}$ matters in proportion to the marginal utility while unemployed U_1. The resulting total effect on the government budget $\frac{\mathrm{d}G}{\mathrm{d}\theta_j}$ requires changing taxes while employed. ϕ is the share of this tax change that has a first-order welfare impact, and this matters in proportion to the marginal utility while employed U_2.

To aid interpretation of Equation 1.8, we substitute in the total government cost decomposition from Equation 1.6 and normalize the entire equation. The normalization divides through by $U_2 \cdot \phi \cdot \frac{\partial B}{\partial \theta_j}$. The U_2 rescaling translates the utility welfare change into a money-metric, the $\frac{\partial B}{\partial \theta_j}$ rescaling translates UI policies of different magnitudes into a common unit of "welfare gain per dollar that provides first-order welfare gain", and rescaling by ϕ focuses attention on the welfare costs depending on the choice of UI policy θ. The resulting equation is:

$$\frac{\mathrm{d}W}{\mathrm{d}\theta_j} = \frac{U_1}{U_2 \cdot \phi} - 1 - \frac{\left(\frac{\partial B}{\partial \mathbf{Y}} - \frac{\partial T}{\partial \mathbf{Y}}\right) \cdot \frac{\partial \mathbf{Y}}{\partial \theta}}{\frac{\partial B}{\partial \theta_j}} \tag{1.9}$$

where $\frac{\mathrm{d}W}{\mathrm{d}\theta_j} := \frac{\frac{\mathrm{d}V}{\mathrm{d}\theta_j}}{U_2 \cdot \phi \cdot \frac{\partial B}{\partial \theta_j}}$ is the money-metric unit welfare change.

In the parlance of the Baily-Chetty formula for optimal UI, $\left(\frac{U_1}{U_2 \cdot \phi} - 1\right)$ is the "benefit-side" and $\frac{\left(\frac{\partial B}{\partial \mathbf{Y}} - \frac{\partial T}{\partial \mathbf{Y}}\right) \cdot \frac{\partial \mathbf{Y}}{\partial \theta}}{\frac{\partial B}{\partial \theta}}$ is the "cost-side" (Baily, 1978; Chetty, 2006). For a UI benefit expansion, the former captures the (first-order) welfare gain from transferring consumption across employment states, while the latter represents the (first-order) welfare loss from raising additional revenue beyond the mechanical transfer of money across employment states.

In this sense, the BCMC ratio is a sufficient statistic for the efficiency costs of different UI benefit reforms $\mathrm{d}\theta_j$: for a given gap in marginal utilities across employment states, a lower BCMC ratio indicates that a UI benefit reform is likelier to be welfare-increasing.[23]

While the above model is quite general, one plausible generalization worth noting is allowing for effects of UI policy reforms $\mathrm{d}\theta_j$ independent of effects on taxes and transfers. For the worker, administrative hassles could directly affect utility. If so, the right-hand side of Equation 1.9 would have an additional summand capturing the direct effect of the reform ($\propto \frac{\partial U}{\partial \theta_j}$). For the government, administrative costs could directly affect the government budget independent of net transfers $B - T$. If so, the numerator of the BCMC ratio would have an additional term capturing the UI policy reform's direct effect on administrative costs ($\frac{\partial G}{\partial \theta_j}$).

The representative worker framework generates immediate policy implications based on the BCMC ratio. For example, suppose $\mathrm{BCMC}_j < \mathrm{BCMC}_k$ for different UI policies θ_j and θ_k. The first term $\left(\frac{U_1}{U_2 \cdot \phi} - 1\right)$ depends on the final transfer of consumption across states rather than the source of this transfer, so the money-metric unit welfare change is higher for the UI policy margin j than k ($\frac{\mathrm{d}W}{\mathrm{d}\theta_j} > \frac{\mathrm{d}W}{\mathrm{d}\theta_k}$). Consequently, a budget balanced policy reallocation away from k towards j ($\mathrm{d}\theta_k < 0 < \mathrm{d}\theta_j$) raises total welfare.

Different UI policy margins tend to apply to different types of workers, and this can have important implications for making welfare comparisons across policy reforms. Without a representative consumer, social welfare is an aggregation of individual utilities, and the aggregation weights may depend on individual characteristics or marginal utility itself (Saez and Stantcheva, 2016). As a concrete example, the monetary eligibility minimum earnings threshold θ_j affects UI benefits for relatively low-earning claimants who either receive some UI benefits or none at all, while the maximum WBA threshold θ_k affects UI benefits for relatively high-earning claimants who are already eligible for UI benefits. The relative gap in marginal utilities across employment states $\left(\frac{U_1}{U_2 \cdot \phi} - 1\right)$ is plausibly larger for the monetary eligibility earnings threshold θ_j than the maximum WBA threshold θ_k, as di-

[23]The general Baily-Chetty optimal UI framework can be seen as a specific application of the MVPF framework (Hendren and Sprung-Keyser, 2020). Through the lens of the MVPF, the Baily-Chetty framework essentially separately calculates (1) the MVPF of a UI benefit increase *without* a tax adjustment and (2) the MVPF of the tax increase necessary to finance such a UI benefit increase. The BCMC ratio is the fiscal externality term corresponding to the MVPF of a UI benefit increase.

minishing marginal utility implies that the first dollar of insurance is more valuable than additional dollars of insurance. Moreover, utilitarian preferences for redistribution would imply that those affected by monetary eligibility have higher generalized social marginal welfare weights, as they have higher marginal utility due to lower baseline income. While a balanced budget policy reform affecting different types of workers would no longer be a Pareto improvement due to distributional consequences, it could in theory be combined with reforms along existing non-UI policy dimensions to generate one (Hendren, 2020).

Empirical Implementation

Our mapping of the theoretical framework to empirical estimates for each UI policy θ_j focuses on estimating BCMC_j. For simplicity, we assume as in the baseline setup that the UI policy reform $d\theta_j$ affects the government budget only through net transfers (i.e., $\frac{\partial G}{\partial \theta_j} = 0$). Section 1.5.2 explores plausible generalizations.

Behavioral costs are estimated as causal effects on government expenditures, while the mechanical transfer is estimated by applying the policy reform to counterfactual behavior in the absence of the reform. For measuring government expenditure, we directly observe only UI benefits and UI-covered labor income. We consider 8 quarters of earnings starting with the quarter of UI claim filing. To maintain comparability with Schmieder and Von Wachter (2016), we apply a 31.47% tax rate to translate labor income to taxes paid.[24] Our definition of government expenditures as UI benefits minus implied tax revenue thus does not account for effects on other government transfers (Leung and O'Leary, 2020). For studying policy reforms, our exogenous variation is individual-level rather than aggregate. This individual-level variation depends on features of UI claim applications, so we always condition on the realized sample of UI claimants. This ignores any effect a UI policy reform could have on the composition of UI claimants as outlined in Figure 1-1: either selection into unemployment, selection into claim filing conditional on unemployment, or, for the case of separation-based eligibility, selection into an eligibility determination.

We next outline the general strategies for estimating the BCMC for each margin. The

[24]This is the US total tax wedge in 2015 according to the OECD. It is similar to the 34.3% marginal tax rate for federal, state, and payroll taxes estimated by NBER TAXSIM for a single California worker in 2015 with our separation-based analysis sample's average income.

total cost is always the causal effect of policy variation on net transfers, while the estimation strategy for isolating the mechanical transfer depends on the specific UI policy margin. Section 1.11.2 details sample construction, estimating equations, and identification assumptions. It also provides graphical intuition for these calculations based on hypothetical employment responses to different types of UI benefit expansions.

Separation-based eligibility. Available policy variation for the extensive margin of separation-based eligibility is the probability of eligibility approval for a given UI claimant. Holding fixed the composition of UI claimants with separation-based eligibility issues, the total government cost of approving the marginal claimant is estimable using our instrumental variables (IV) research design.

To build intuition for the mechanical transfer, suppose that claimants receive UI benefits if and only if they are deemed eligible. The total government cost causal effect is estimated off of marginal claimants, so the mechanical transfer is the counterfactual benefit amount implied by a marginally denied claimant's monetary entitlement and subsequent nonemployment duration. This is an untreated potential outcome for compliers, which our IV design recovers.

To measure counterfactual benefits, we proportionately allocate benefits starting in the quarter following the claim. For example, counterfactual benefits in the quarter immediately following the initial claim are 0 if the claimant had any employment in that quarter and $WBA \cdot \min\{PBD, 13\}$ if they did not. More generally, counterfactual benefits k quarters following the initial claim quantity are $WBA \cdot \min\{\max\{PBD - 13k, 0\}, 13\}$.

Finally, we account for the fact that eligibility approval does not map one-to-one to benefit receipt. To do so, we scale down our counterfactual benefits by the causal effect of eligibility approval on the probability of receiving any UI benefits. Intuitively, the total causal effect on UI benefit dollars we estimate includes the imperfect mapping between eligibility approval and benefit receipt; subtracting off the mechanical transfer to recover the behavioral cost requires us to do the same.

Our strategy for inferring counterfactual benefits implies two sources of measurement error. Inferring nonemployment duration based on the absence of any employment in the entire quarter will understate the degree of actual nonemployment, which generates a negative bias in the mechanical transfer (and thus a positive bias in the BCMC). Moreover, if there is a nonzero correlation between heterogeneity in the degree of nonemployment and heterogeneity in effect of eligibility on nonemployment duration, then scaling by the average effect of eligibility on benefit receipt will introduce bias. A positive correlation generates a positive bias, while a negative correlation generates a negative bias.

Monetary eligibility. Available policy variation for the extensive margin of monetary eligibility is the minimum earnings threshold. Because this criterion also determines whether claimants receive any benefits or none at all, our measurement approach mirrors that for recovering the separation-based eligibility's BCMC.

The primary distinction in estimation is that our identifying variation comes from a minimum earnings threshold—specifically, the level of high-quarter wages—that facilitates a regression discontinuity design (RDD). The same intuition for identifying causal effects carries through, as a local RD can be interpreted as an IV with monetary eligibility as the endogenous treatment and (recentered) high-quarter wages. In terms of treatment effects, the total cost to the government of monetary eligibility is the RDD estimate on net government transfers. This is the size of the discontinuity in net transfers across the monetary eligibility threshold. In terms of counterfactual outcomes, the mechanical transfer is counterfactual benefit receipt for marginally denied claimants. This is the level of counterfactual benefits for those who fall just below the minimum earnings threshold.

Finally, is worth noting that, by definition, claimants on the margin of monetary eligibility have very low formal earnings. Table 1.12 shows the average prior quarterly earnings for those around the threshold is only $1,267, which is less than one-fifth the value for the sample of claimants with separation-based eligibility issues.

Weekly benefit amount. Available policy variation for the intensive margin of WBA is the kink in the mapping of prior earnings—specifically, the level of high-quarter wages (HQW)—to WBA around the maximum WBA. Our causal research design is therefore a regression kink design (RKD).[25]

[25]This strategy follows Bell et al. (2022b), who estimate and decompose the heterogeneous impacts of

The total government cost estimate follows the same IV intuition as above, where the endogenous treatment is WBA. In words, just as the above monetary eligibility RDD scales the reduced-form discontinuity in net transfer *levels* by the first-stage discontinuity in eligibility *levels*, the WBA RKD scales the reduced-form discontinuity in net transfer *slopes* with respect to HQW by the first-stage discontinuity in WBA *slopes* with respect to HQW.

The mechanical transfer estimate is derived similarly to the one for monetary eligibility: an outcome level for those at the HQW threshold. It is the total number of UI benefit weeks claimed among claimants at the kink. Intuitively, the mechanical transfer of a \$1 WBA increase is simply the number of times the claimant receives a benefit payment.

Potential benefit duration Available policy variation for the intensive margin of PBD is the kink in the mapping of prior earnings—specifically the ratio of high-quarter wages to base period wages ($\frac{HQW}{BPW}$)—to PBD around the maximum (regular) PBD. Our causal research design is therefore once again a regression kink design (RKD).

The total government cost is estimated exactly as above with net transfers as the outcome, except with (total) PBD as the endogenous treatment and $\frac{HQW}{BPW}$ as the running variable. The mechanical transfer estimate is the fraction of claimants at the kink exhausting benefits scaled by their WBA. In words, it is the additional benefit week only for claimants who otherwise remain unemployed through benefit expiry.

Results

Table 1.7 summarizes the cost decomposition for each type of UI benefit expansion. For each benefit expansion type, the unit of individual-level treatment is in parentheses adjacent to the benefit expansion type. Column 1 reports the treatment effects for each benefit expansion type on net government transfers, which we measure UI benefit dollars paid minus tax revenues collected. Because each benefit expansion type has different units, treatment effects are not directly comparable with each other. All of the point estimates are positive and highly statistically significant, indicating that no type of benefit expansion is self-financing.

WBA expansions over different time periods in the same dataset that we use.

This treatment effect on benefits paid to claimants minus taxes paid by the claimants can be further decomposed into treatment effects on each component, which is presented in Columns 2 and 3. Monetary eligibility, PBD, and WBA all decrease tax revenues, as the positive coefficients indicate an increase in tax costs to the government; these effects are all highly statistically significant. The striking difference is that separation-based eligibility does not decrease tax revenues. This mirrors our earlier finding in 1.5 on average quarterly earnings following the claim, where the magnitude differences in Table 1.7 are due to including earnings in the quarter of claim filing, applying a tax rate, and summing over quarters rather than averaging.

The decomposition relevant to efficiency costs, however, is separating the total government cost into (i) benefits mechanically provided to claimants in the absence of behavioral responses and (ii) benefits paid and taxes lost due to claimants changing their behavior. Column 4 isolates the mechanical transfer of UI benefits, which we estimate in different regression specifications depending on the benefit expansion type. By definition, expanding benefit generosity mechanically increases benefits paid to claimants. Accordingly, all of the coefficients are positive and highly statistically significant.

The share of an increase in UI benefits mechanically due to program rule changes can be calculated as the ratio of the coefficient in Column 4 over the coefficient in Column 3. In theory, as long as there is no offsetting behavioral response that is a force towards decreased benefit receipt—such as a decrease in unemployment duration or takeup in response to a benefit expansion—100% is an upper bound for this share. In practice, the mechanical benefit transfer we estimate for both extensive margins of eligibility exceeds its margin's total effect on benefits received. Because we do not find any direct evidence of offsetting behavior, it is likely that this discrepancy is driven by the previously discussed measurement error in inferring counterfactual benefits for eligibility margins.[26] Nevertheless, we interpret this as evidence that the vast majority of the increase in UI benefits due to eligibility is mechanical. On the other hand, for the intensive margins of WBA and PBD, we find that only approximately two-thirds of the benefit increase is mechanical.

[26] In line with the tax cost of monetary eligibility shown in Column 2 Row 2 of Table 1.7, the RDD estimate of monetary eligibility on any employment in the subsequent quarter is -0.03 (SE=0.008) and on quarters of nonemployment duration is 0.20 (SE=0.05).

Column 5 puts the previous pieces together to calculate the welfare-relevant object of interest: the ratio of behavioral costs to mechanical costs. The mechanical cost is the coefficient in Column 4 and the behavioral cost is the increase in total government costs after accounting for the mechanical transfer (i.e., the coefficient in Column 1 minus the coefficient in Column 4). Taking our estimates at face value, we find that the behavioral response to separation-based eligibility *increases* tax revenue, which leads to a *negative* behavioral costs and thus a negative BCMC ratio. However, given the previously discussed measurement concerns and our focus on making relative comparisons policy margins, we direct to the robustness of the relatively low separation-based eligibility BCMC. For example, even if we take the upper bound of the revenue change confidence interval (so that there is $641 revenue decrease rather than a $78 revenue increase) and assume the mechanical transfer is only 90% of the total benefits increase (which is well outside the confidence interval), we still find a separation-based eligibility's BCMC ratio of 0.39. This is similar to monetary eligibility's BCMC ratio of 0.32 and still well WBA's of 0.57 and PBD's of 0.88. Moreover, if we apply the same robustness procedure to monetary eligibility's BCMC ratio, its BCMC ratio increases to 1.36.

Finally, we situate these California-specific BCMC estimates within the wider UI literature. Analogous to Figure 1-5, Figure 1-6 plots our BCMC estimates against those from papers surveyed by Schmieder and Von Wachter (2016). Just as we find a larger BCMC for PBD than WBA in California, the literature's median BCMC estimate for PBD is larger than its median BCMC estimate for WBA. Our WBA and PBD estimates both lie within the interquartile ranges of corresponding literature estimates, indicating that our institutional context is comparable to others around the world.

The key takeaway from Figure 1-6 is that the separation-based eligibility's BCMC is well below those for other margins in various contexts. Even the aforementioned "robustness" value of 0.39 for separation-based eligibility's BCMC would be the fourth-lowest BCMC (i.e., below the 25$^{\text{th}}$ percentile) within either the set of WBA estimates or PBD estimates from the literature.

Other Considerations

While we show in Section 1.5.2 that the BCMC is a sufficient statistic for the efficiency cost of a UI benefit expansion and in Section 1.5.2 that the BCMC for separation-based eligibility is lower than those for other UI policy margins, it is important to revisit the caveats mentioned in those sections when considering the policy implications of our findings. The following brief discussion focuses on consumption-smoothing benefits, administrative costs, social welfare weights, and other behavioral responses we cannot observe in our data. We believe the first two reasons are likely to increase the total welfare effect of separation-based benefit expansions, while the last reason is likely to decrease it.

First, unobserved consumption-smoothing benefits are likely to increase the welfare benefits of separation-based eligibility expansions for two reasons. As Equation 1.9 demonstrates, the gap in marginal utilities across employment statuses captures the consumption-smoothing benefits of the benefit expansion. One reason, as shown in Tables 1.2 and 1.12, is that claimants on the margin of separation-based or monetary eligibility have relatively lower incomes than other UI claimants. With diminishing marginal utility of consumption, this suggests a given consumption drop due to unemployment implies a larger gap in marginal utilities. Another reason is that those impacted by intensive margin benefit expansions were already receiving inframarginal UI dollars, while those impacted by extensive margin benefit expansions were not receiving any UI. Again, with diminishing marginal utility of consumption, the first dollar of insurance provides the greatest consumption-smoothing value.

Second, the separation-based eligibility determination process involves both monetary and utility costs, and eligibility expansions could avoid those. Investigating cases has fixed and variable costs in the form of technological infrastructure and examiner wages, respectively. As mentioned in the discussion of Figure 1-1, the rate of claimant appeals following an initial denial is much higher than the rate of employer appeals following an initial approval. Therefore it is likely that increasing the approval rate will decrease the total number of appeals of an initial determination.

Third, different welfare frameworks likely imply different impacts of incorporating generalized marginal social welfare weights. Related to utilitarian considerations, claimants on the separation-based eligibility margin are relatively low-income. Preferences for redistribution provide an independent reason for transfers to this group, though it is unclear whether this is desirable beyond standard redistribution through the tax code (Atkinson and Stiglitz, 1976; Akerlof, 1978). Non-utilitarian considerations could include the relative undesirability of “false positives" (i.e., approving a claimant who technically does not satisfy eligibility) vs. “false negatives" (i.e., denying a claimant who technically does satisfy eligibility). One estimate of the current levels of “ground-truth" eligibility comes from periodic Department of Labor audits through the Benefit Accuracy Measure (BAM) Program. These aggregate data estimate that approximately one-fourth of separation-based eligibility denials should’ve been approved while one-twentieth of approved claims should have been denied on separation-based eligibility grounds.

Finally, an expansion in separation-based eligibility could induce other general equilibrium behavioral responses not captured by our partial equilibrium approach. One plausible channel is an increase in job separations through increased employee quits.[27] While quasi-experimental work documents that quits increase in response to benefit extensions, panel variation in separation-based eligibility criteria across states finds mixed evidence on quits (Jäger et al., 2022; Ragan, 1984; Solon, 1984). Another plausible channel is a change in the composition of employers’ offered jobs and desired candidates. For example, employers may be less willing to employ workers they deem to be at risk of quitting due to personal circumstances or committing misconduct. We believe either of these general equilibrium responses is likelier to be important if the policy change is non-marginal and clear to outside parties, such as adding an entire category of UI-eligible quits. On the other hand, they are less likely to be important if it is a marginal shift in the probability of eligibility approval.

[27]However, experience-rated employers may be less likely to fire workers if they are likelier to receive UI benefits.

1.6 Conclusion

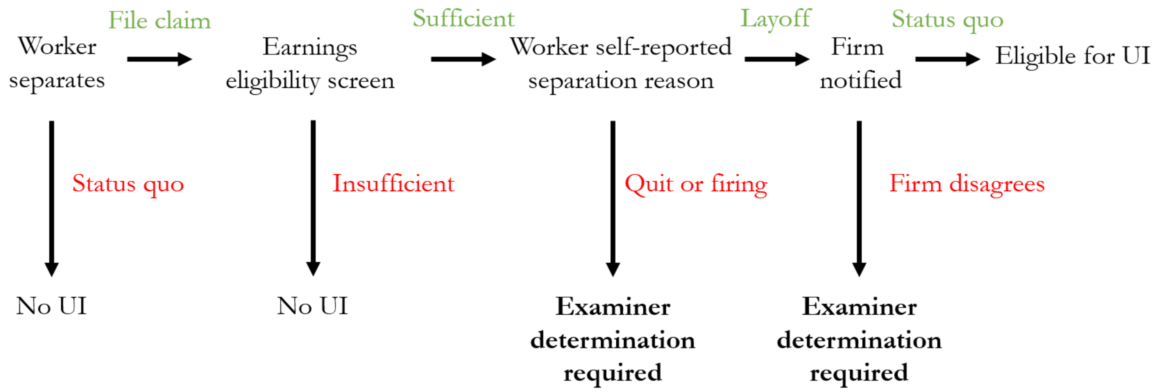
Whether a UI claim is initially approved or denied on separation-based eligibility grounds greatly matters for subsequent UI benefit receipt but much less so for subsequent employment. We provide the first evidence of these causal effects using a design leveraging variation in eligibility approval rates across offices to which claimants are randomly assigned. Our individual-level variation using data on the universe of UI claims in California for almost two decades provides significant power, and we use this to show that any decreases in employment due to separation-based eligibility are the smallest for low-income claimants.

We show these employment responses are particularly small in the context of a theoretically-motivated measure of the efficiency costs of benefit expansions. This holds both relative to estimates from the existing UI literature and relative to replications of those research designs within our own data.

Does this mean UI agencies should relax their separation-based eligibility criteria, or at least reallocate benefit generosity? Our empirical findings provide evidence that such policy reform is welfare-improving. Nevertheless, our theoretical framework highlights the relevant normative considerations policymakers should account for and unmeasured empirical objects that future research should estimate.

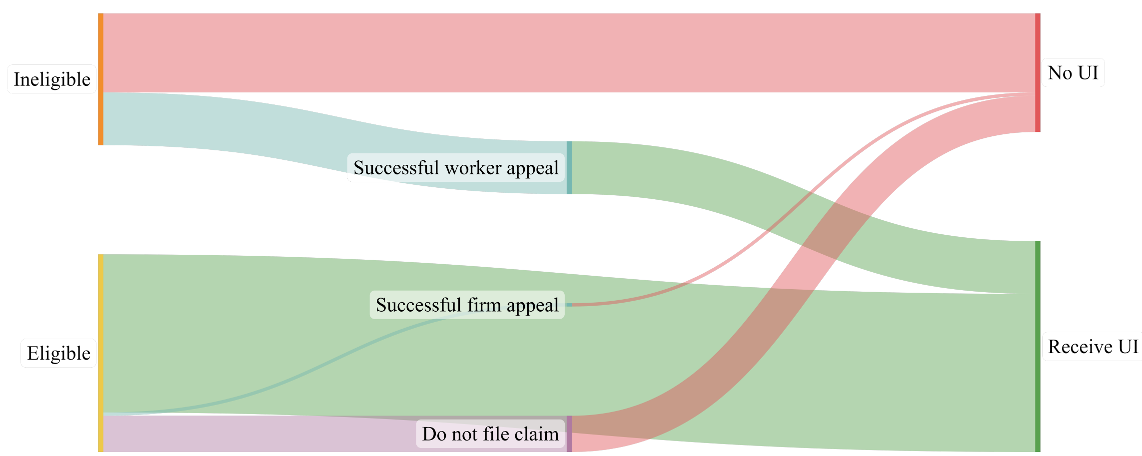
1.7 Main Figures

Figure 1-1: Selection Into Separation-Based Eligibility Determinations



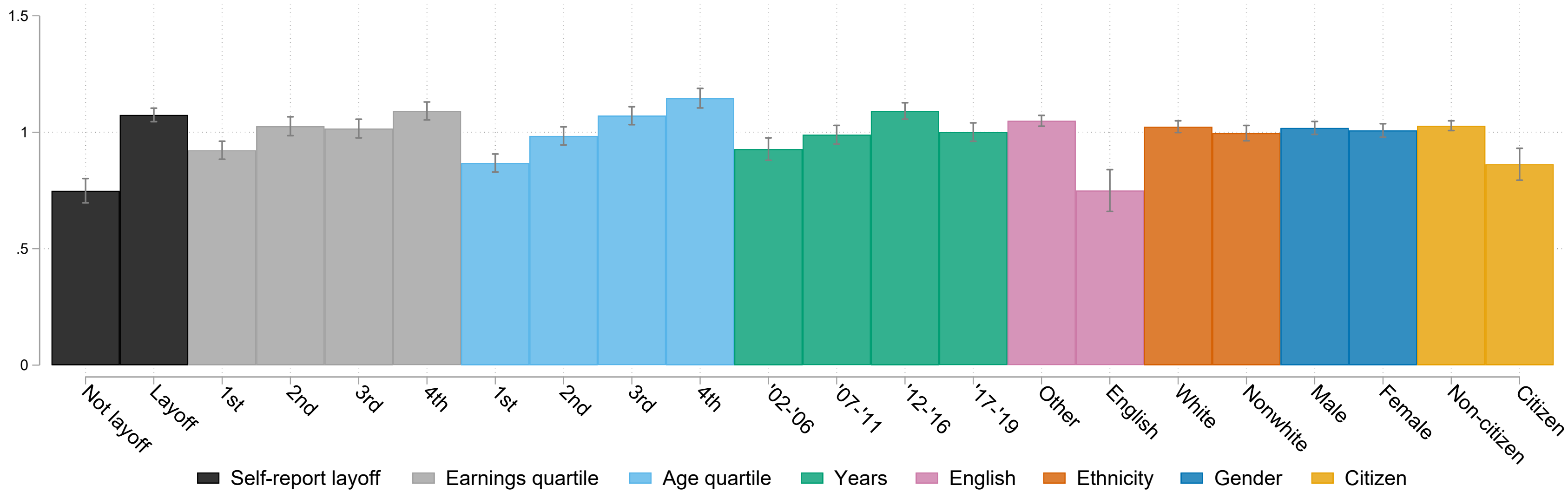
Notes: This chronological flowchart describes how unemployed workers can end up in our separation-based eligibility sample. Arrows in blue indicate steps leading to UI benefit receipt, while arrows in red indicate steps that put UI benefit receipt in jeopardy.

Figure 1-2: Reasons for UI Benefit (Non-)Receipt by Eligibility Approval Status



Notes: This Sankey diagram describes the mapping between separation-based eligibility and UI payment receipt in our sample of separation-based eligibility issues from 2002 to 2019. Bar thickness is proportional to the relevant percentage of claims. The rates of successful worker and firm appeals come from aggregate statistics in DOL ETA 5130 Benefit Appeals Report. All other quantities come from our EDD microdata.

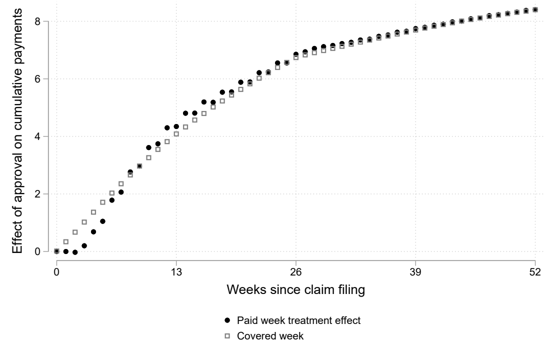
Figure 1-3: Consistency of Office-by-Issue Leniency Measures



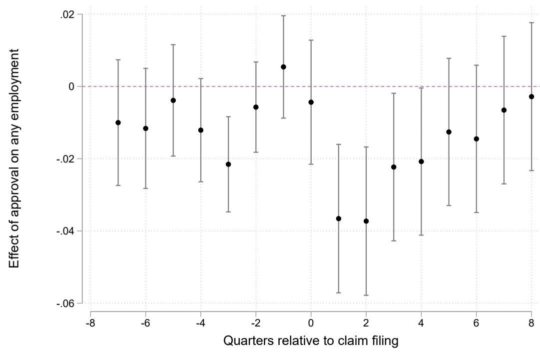
Notes: Each bar represents a separate regression of the claimant's own eligibility decision D_{it} on their assigned office-by-issue's overall leave-one-out residualized eligibility leniency $\tilde{Z}_{it}^{j}$ within a given subsample. Each color represents a different categorical variable, and separate bars refer to separate levels of that categorical variable. Lower quartiles correspond to lower levels of the variable. Robust standard errors are clustered by claimant and error bars provide 95% confidence intervals.

Figure 1-4: Dynamic Impacts of Eligibility Approval

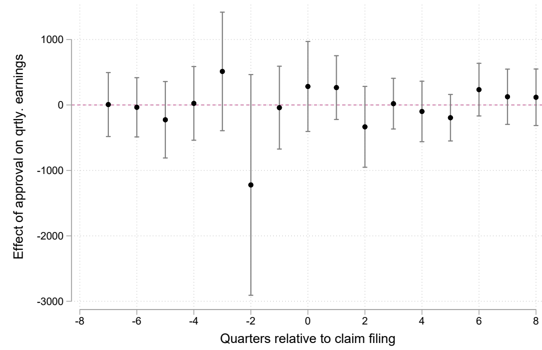
(a) Paid and Covered Weeks



(b) Any Earnings

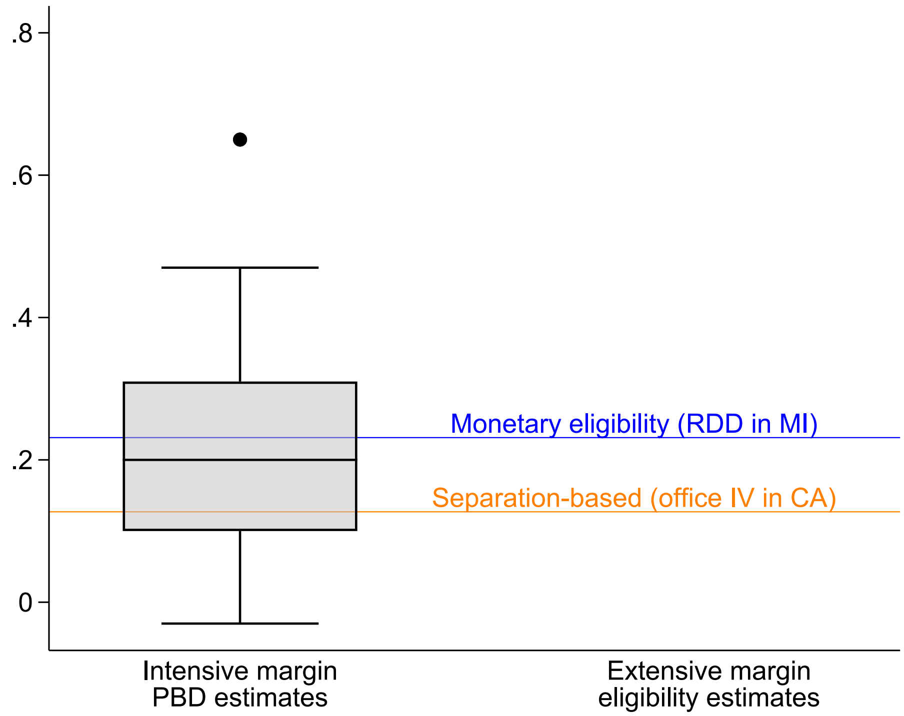


(c) Avg. Earnings Incl. 0's



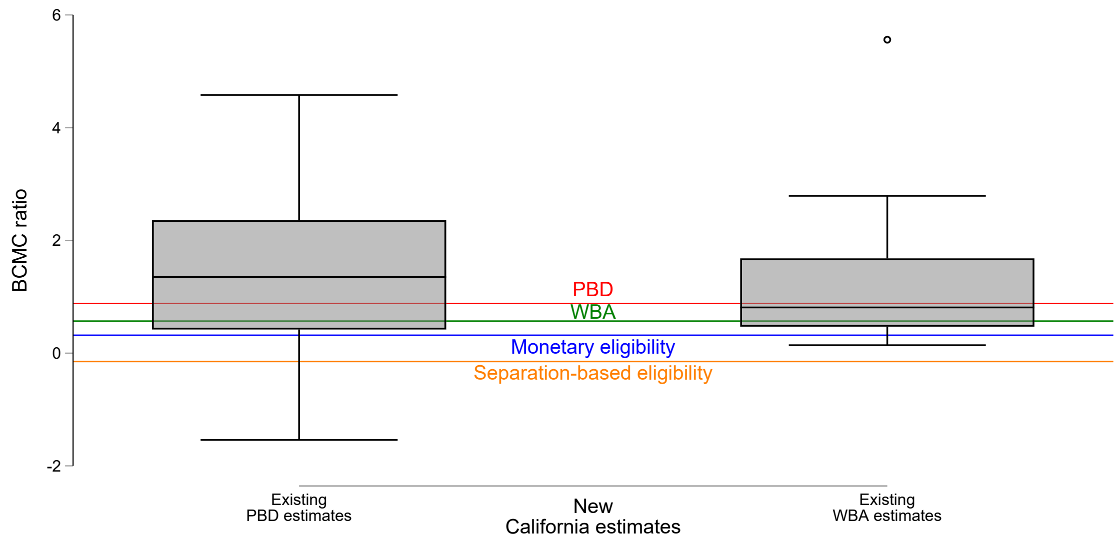
Notes: Each panel displays coefficients from separate regressions of the form in Equation 1.1 and Equation 1.2. Outcomes in panel (a) are cumulative measurements of payments as of each of the 52 weeks following claim filing. A paid week refers to the calendar week the payment is made, while a covered week refers to the week of unemployment to which that payment pertains. Outcomes in Panels (b) and (c) are contemporaneous measures for the quarters before, during, and after claim filing. Panels (b) and (c) include 95% confidence intervals while Panel (a) omits them. The sample is of all regular initial claims with separation-based eligibility issues between 2002 to 2019.

Figure 1-5: Nonemployment Duration Effects per Week of Potential Benefit Duration



Notes: The graph plots values of the partial effect of an additional week of UI benefits on nonemployment duration. The box-and-whisker chart is from 21 benefit duration estimates surveyed in Schmieder and Von Wachter (2016). The separation-based eligibility estimate using our main office-assignment-based IV design in California from 2002 to 2019 is in orange and the monetary eligibility estimate from Leung and O'Leary (2020) using a minimum earnings threshold-based RD design in Michigan from 2005 to 2010 is in blue. Eligibility estimates are derived from the causal effects of eligibility on consecutive quarters of subsequent unemployment. These causal effects are scaled by (1) the effect of eligibility on benefit receipt, (2) the average number of total potential benefit weeks for the relevant sample, and (3) the thirteen weeks in a quarter of nonemployment.

Figure 1-6: Comparison of Estimated BCMC Ratios with the Literature



Notes: The graph plots values of the BCMC ratio, which is defined as the ratio of behavioral costs to mechanical costs for a UI benefit expansion. The vertical coordinate of each horizontal line corresponds to the BCMC ratio of the adjacent UI policy margin written in the same color as the horizontal line. The left-hand side box-and-whisker chart represents 17 BCMC estimates from Schmieder and Von Wachter (2016), and the right-hand side box-and-whisker chart represents 17 BCMC estimates from the same review. We exclude the two BCMC estimates for PBD from Schmieder and Von Wachter (2016) that are multiple orders of magnitude larger than those from the other studies.

1.8 Main Tables

Table 1.1: UI Receipt and Ineligibility Among the Unemployed

	(1)	(2)	(3)	(4)
	Receive UI	**No UI Receipt due to Insufficient Prior Earnings**	**No UI Receipt due to Ineligible Job Loss Reason**	**Received UI but Exhausted Benefits**
Share of the unemployed	35%	38%	10%	6%
Share of filed UI applications	63%	13%	8%	7%

Notes: The first row pertains to all unemployed individuals at a given point in time and breaks down reasons for currently (not) receiving UI benefits. It reproduces from Table 1 in Auray et al. (2019), and the population is the entire US from 1989 to 2012. The authors infer receipt and ineligibility reasons using the March Supplement of the Current Population Survey. Ineligibility due to insufficient earnings is inferred from prior earnings applied to state eligibility rules. Ineligibility due to job loss is limited to self-reported quits. The implied residual percentage is due to incomplete take-up among eligible claimants. The second row pertains to initial UI claims and breaks down reasons for ever (not) receiving UI benefits. The population is initial UI claims from 2002 to 2019, which we measure using our administrative data. Ineligibility is directly observed from agency records and excludes initially ineligible claimants who received UI following successful appeals.

Table 1.2: UI Receipt and Ineligibility Among the Unemployed

	(1)	(2)	(3)	(4)	(5)
	All Claims	Monetarily Eligible	+ Separation Issue	+ Initial Denial	+ No Payments
N (millions)	26.8	22.9	6.9	2.9	2.1
Share of previous column		86%	30%	42%	74%
Avg. prior quarterly earnings ($)	6,960	7,881	6,912	5,615	5,241
Weekly benefit amount ($)		288	274	251	239
Age	39	40	36	34	33
Female	0.45	0.45	0.49	0.50	0.51
Nonwhite	0.65	0.65	0.63	0.67	0.67
English-speaking	0.88	0.87	0.94	0.94	0.94
Any SDI, yr before UI claim	0.08	0.09	0.14	0.12	0.11
Claimant reports layoff	0.72	0.73	0.23	0.21	0.11
Share misconduct (vs. quit)			0.62	0.44	0.42
UE duration (qtrs)	2.2	1.9	2.4	2.3	2.4

Notes: Column 1 includes all regular UI initial claims filed in California from 2002 to 2019. Each subsequent column adds an additional restriction to those applied in the previous columns. Column 2 restricts to initial claims satisfying the minimum earnings eligibility threshold. Column 3 further restricts to initial claims with any investigation of a separation-based eligibility issue. Column 4 restricts to separation-based eligibility issues whose initial determination is a denial. Column 5 restricts to separation-based eligibility denials that have no evidence of a successful appeal in the form of claimed benefit payments. SDI refers to California's state-administered disability insurance program.

Table 1.4: Validating Instrumental Variables Assumptions in the Office Research Design

	Relevance		Independence				Exclusion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Eligibility	Any payments	Prior avg. qtrly. earn.	Age	Female	Nonwhite	Days to det.	Other issue if paid
Range	0.09	0.05	176	0.14	0.01	0.01	3.51	0.03
Mean	0.59	0.59	6,911	36	0.49	0.63	28.94	0.17
Joint F-statistic	405	77	1	1	1	1	35	24
Joint F p-value	0.00	0.00	0.30	0.21	0.11	0.42	0.00	0.00

Notes: Statistics in each column come from first-stage coefficients γ in Equation 1.2 using all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. For each variable listed at the top of the column, the table reports the range of first-stage coefficients when that variable is the outcome, the overall sample mean, and the joint F-statistic and corresponding p-value when testing all first-stage coefficients. Columns 1 and 2 test the first-stage relevance assumption using endogenous UI treatments, Columns 3 through 6 test the independence assumption using claimant demographics, and Columns 7 and 8 test the exclusion restriction using other claim processing outcomes.

Table 1.3: Payment Timing Implications of Eligibility Investigations and Appeals

	(1)	(2)	(3)
	No eligibility issues	Separation issue but approved	Separation denial but paid
Avg. prior quarterly earnings	8,327	7,837	6,708
Any payments	0.77	0.83	1.00
Median days to 1st payment	16	21	93
Median days to 1st covered week	10	10	12
N (millions)	16.0	4.0	0.8

Notes: Columns represent mutually exclusive groups of regular UI initial claims filed in California from 2002 to 2019. Column 1 includes only claims without an initial eligibility issue. Column 2 includes only claims with separation-based eligibility issues that were initially approved. Column 3 includes only paid claims with separation-based eligibility issues that were initially deemed disqualifying, which is evidence of a successful claimant appeal.

Table 1.5: Effects of Initial Eligibility Approval

	UI Benefits			Subsequent Employment		
	(1)	(2)	(3)	(4)	(5)	(6)
	Any payments	Payments (weeks)	Payments ($)	Avg. $ in qtrly. earnings (w/ 0's)	Any earnings 1 qtr. after	Consecutive qtrs. w/o earnings
IV	0.32	10.3	2,547	17	-0.04	0.14
SE	(0.01)	(0.43)	(146)	(142)	(0.01)	(0.07)
tf SE	[0.01]	[0.43]	[146]	[142]	[0.01]	[0.07]
OLS	0.55	16.9	4,935	979	[-0.06]	0.30
SE	(0.00)	(0.02)	(5.37)	(5)	(0.00)	0.00
Y^0	0.31	6.9	2,323	3,530	0.49	2.37
F	405					
Unique N	5.5m					

Notes: The instrumental variables estimate is β in Equation 1.1 where the endogenous treatment D is initial eligibility approval. The sample includes all separation-based eligibility issues from regular initial claims between 2002 and 2019. Y^0 is the untreated complier mean estimated following Frandsen et al. (2023). The tF adjustment does not change standard errors because the F-statistic is above 104.7 Lee et al. (2022). All robust standard errors are at the 95% confidence level and are clustered by claimant.

Table 1.6: Heterogeneous Effects of Initial Eligibility Approval on Nonemployment Duration

	Age Quartile				Gender		Race	
	1st	2nd	3rd	4th	Male	Female	White	Nonwhite
IV	0.04	0.22	0.32	-0.02	0.16	0.13	0.05	0.16
SE	(0.11)	(0.11)	(0.13)	(0.17)	(0.09)	(0.09)	(0.11)	(0.08)
tf SE	[0.11]	[0.11]	[0.13]	[0.17]	[0.09]	[0.09]	[0.11]	[0.08]
F	117	121	102	86	196	214	151	261
N	1.51m	1.86m	1.81m	1.73m	3.49m	3.41m	2.54m	4.38m
	Prior Earnings Quartile				Employed Qtr. Before Claim		SDI → UI	
	1st	2nd	3rd	4th	No	Yes	No	Yes
IV	-0.09	0.24	0.20	0.20	-0.18	0.19	0.25	-0.38
SE	(0.12)	(0.13)	(0.13)	(0.14)	(0.20)	(0.07)	(0.07)	(0.20)
tf SE	[0.12]	[0.13]	[0.13]	[0.14]	[0.22]	[0.07]	[0.07]	[0.22]
F	127	100	101	103	50	362	371	51
N	1.73m	1.73m	1.73m	1.73m	0.88m	6.04m	6.08m	0.83m
	Time Period				Issue		Self-Reported Layoff	
	2002-6	2007-11	2012-16	2017-19	Misconduct	Quit	No	Yes
IV	0.27	0.25	0.08	0.02	0.11	0.17	0.11	-0.05
SE	(0.11)	(0.11)	(0.12)	(0.14)	(0.09)	(0.09)	(0.09)	(0.17)
tf SE	[0.11]	[0.11]	[0.12]	[0.14]	[0.09]	[0.09]	[0.09]	[0.19]
F	241	244	118	92	415	385	231	48
N	1.77m	2.17m	1.87m	1.10m	4.28m	2.64m	3.25m	0.99m

Notes: The overall sample includes all separation-based eligibility issues from regular initial claims between 2002 and 2019. The IV results are estimated from Equations 1.1 and 1.2 using UJIVE where the endogenous treatment D is initial eligibility approval and the outcome Y is consecutive quarters without earnings following the initial claim. Each column represents a separate model estimated on a given subsample. Earnings quartiles are constructed in the entire sample based on the average quarterly earnings in 7 quarters prior to the claim. Lower quartiles correspond to lower levels of the variable. The 1st time period is 2002-2006, the 2nd is 2007-2011, the 3rd is 2012-2016, and the 4th is 2017-2019. The tF adjustment affects subsamples with an F-statistic below 104.7 and uses a linear interpolation between Table 3A values in Lee et al. (2022). All robust standard errors are at the 95% confidence level and are clustered by claimant.

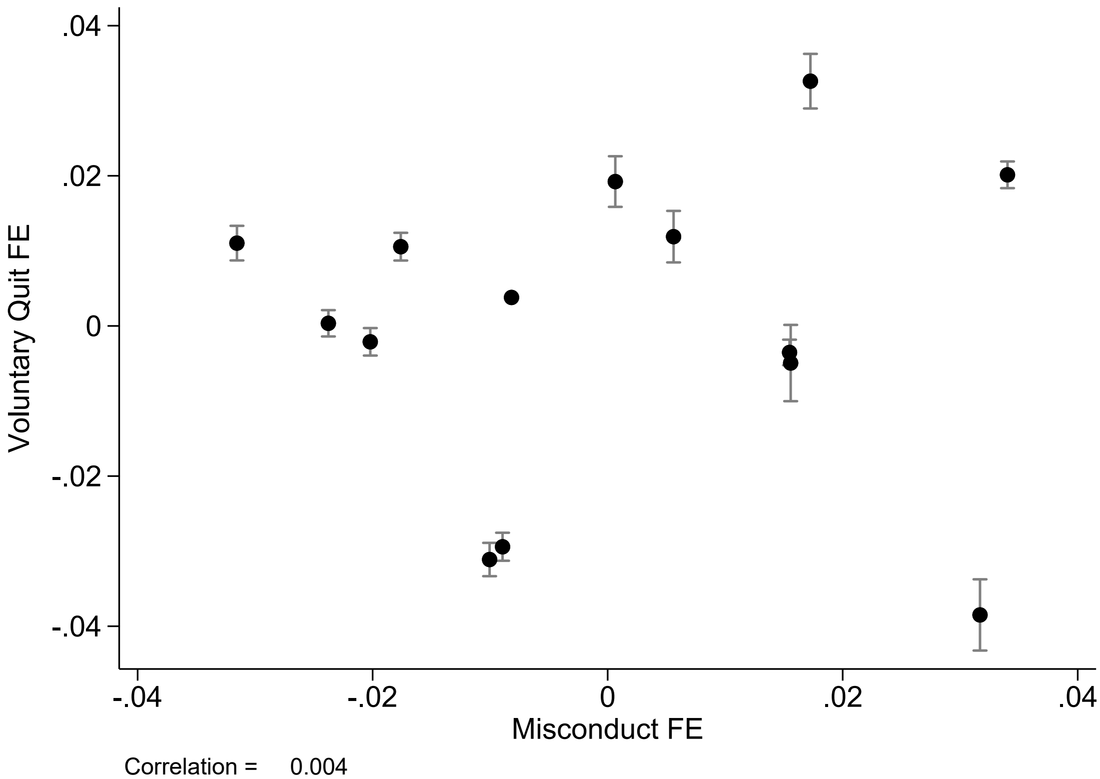
Table 1.7: Decomposition of Benefit Expansion Costs for Different UI Policy Margins

	(1)	(2)	(3)	(4)	(5)
	Total cost	Total tax cost	Total benefit cost	Mechanical cost	$\frac{\text{Behavioral cost}}{\text{Mechanical cost}}$
Separation-based (eligibility)	2468	-78	2546	2902	-0.15
	(400)	(367)	(145)	(92)	
Monetary eligibility (eligibility)	591	294	297	449	0.32
	(21)	(21)	(3)	(2)	
Weekly benefit amount ($)	36	6	30	23	0.57
	(2)	(2)	(< 1)	(< 1)	
Potential benefit duration (weeks)	66	16	50	35	0.88
	(6)	(5)	(3)	(< 1)	

Notes: Each row is a different UI policy treatment margin estimated within our California data. The coefficient in Column 1 is the causal effect of treatment on net government transfers. It is the sum of those in Columns 2 and 3, which are causal effects on total UI benefit dollars and total income taxes, respectively. The mechanical cost is the counterfactual benefit dollars untreated claimants would receive were they to receive treatment but not change their behavior. Column 5 is the BCMC ratio, which is calculated by subtracting the coefficient in Column 3 from the coefficient in Column 1 and dividing this difference by the coefficient in Column 3. Robust standard errors are in parentheses. Further details on sample construction and estimation are in Section 1.11.2.

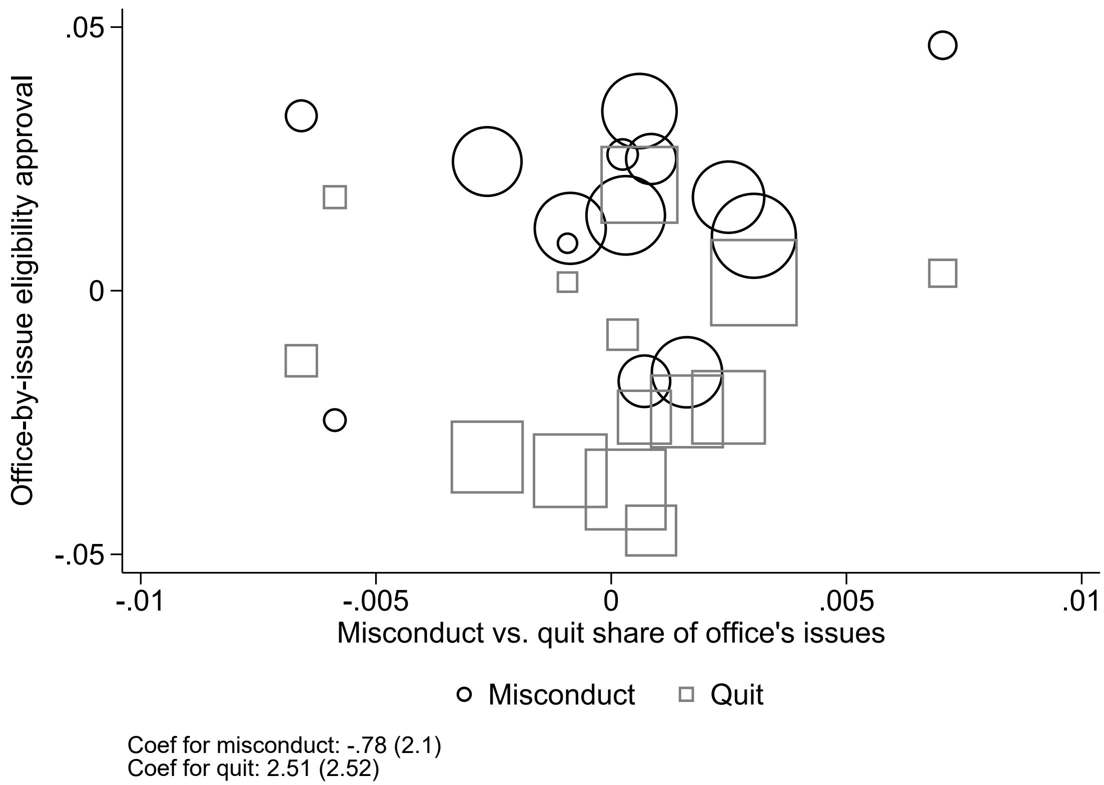
1.9 Appendix Figures

Figure 1-7: Office-Level Leniency is Uncorrelated Across Issue Type



Notes: The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. Each marker is a processing office. The coordinates are office-by-issue coefficients γ estimated from Equation 1.2 where the outcome D is eligibility approval; coefficients are normalized so that the across-office average fixed effect for each issue type is 0. The figure's regression coefficient and robust standard error are from a weighted OLS regression of the discharge fixed effects on quit fixed effects at the office level, where each observation is weighted by the total number of separation-based eligibility issues adjudicated by that office.

Figure 1-8: Minimal Variation in Office-level Issue Type Share is Uncorrelated with Issue-specific Approval Rates

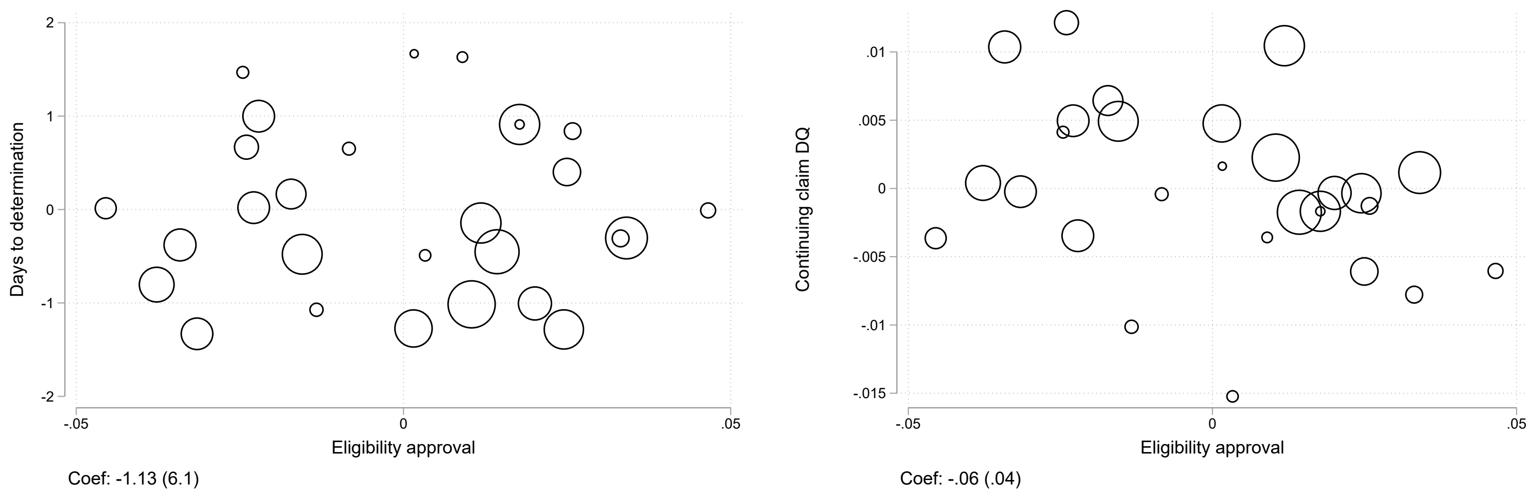


Notes: The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. Each marker is an office-by-issue pair. The *y*-coordinates are office-by-issue coefficients γ estimated from Equation 1.2 where the outcome D is eligibility approval; coefficients are normalized so that the across-office average fixed effect for each issue type is 0. The *x*-coordinates are analogous coefficients on the overall offices estimated from a form of Equation 1.2 where the outcome D is the misconduct share at the office and $\mathbf{Z}$ omits the issue interactions. The figure's regression coefficient and robust standard error are from a weighted OLS regression of the office-by-issue eligibility fixed effects on the office issue type fixed effects, where each observation is weighted by the total number of separation-based eligibility issues adjudicated by that office.

Figure 1-9: Other Office-level Processing Differences are Unrelated to Eligibility Approval Propensity

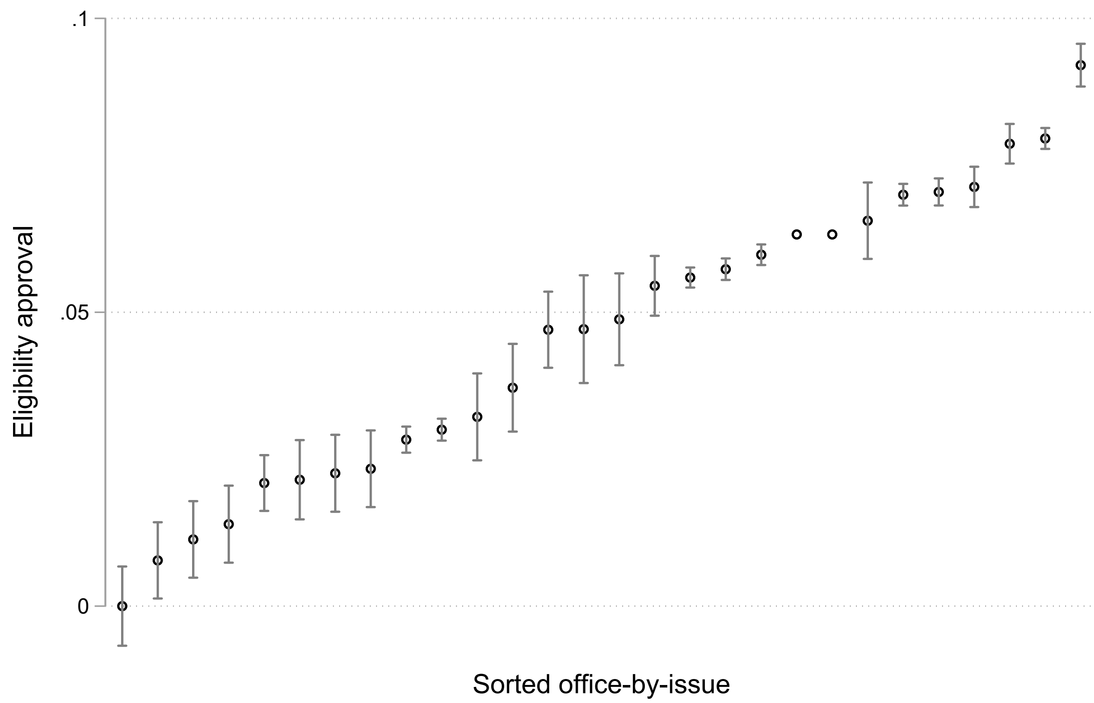
(a) Initial Claim Adjudication

(b) Continuing Claim Adjudication



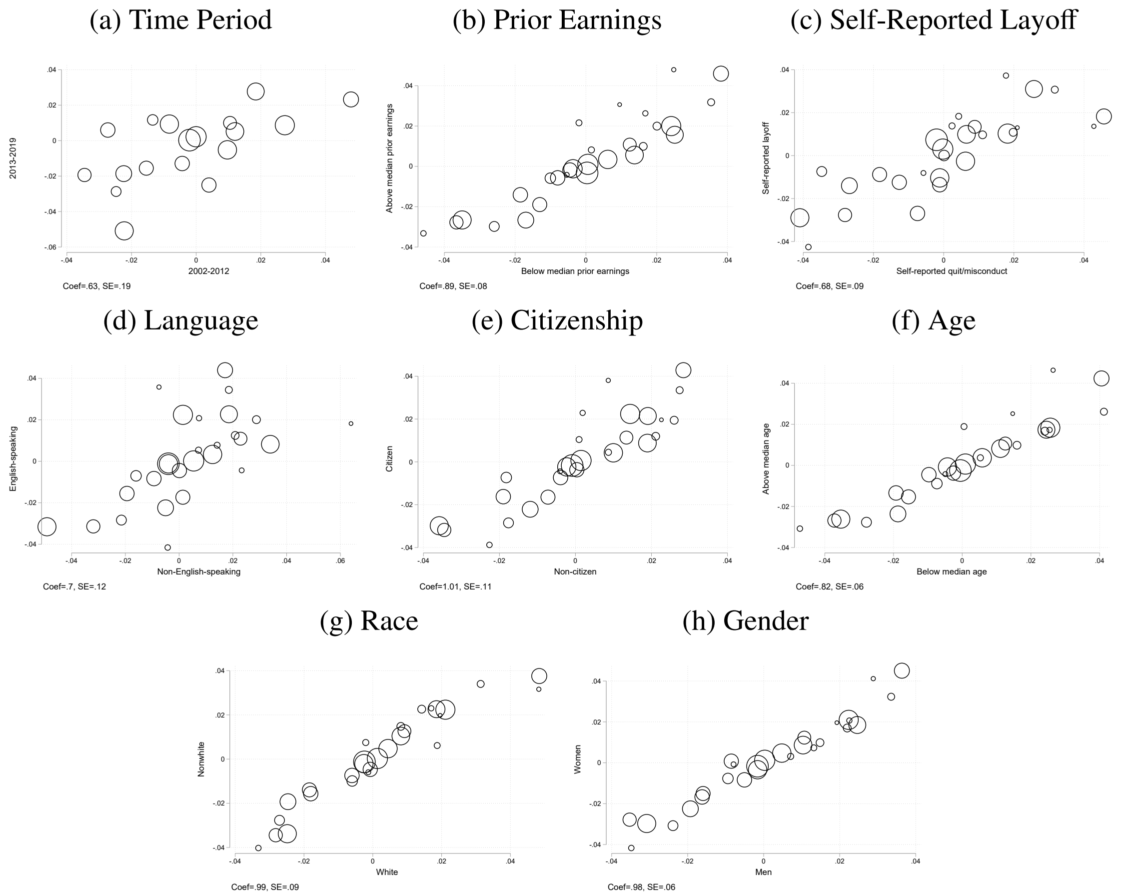
Notes: The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. Each bubble is an office-by-issue pair. Bubble size is proportional to the number of eligibility issues the office-by-issue pair adjudicates. The x-axis indexes office-by-issue coefficients in γ estimated in Equation 1.2 where the outcome D is eligibility approval; coefficients are normalized so that each within-issue across-office average fixed effect is 0. The y-axis in the left panel is the number of days between the claim filing date and the recorded eligibility determination date. The outcome in the right panel is an indicator for any disqualification related to continuing claims. The figure's regression coefficient and robust standard error come from a weighted OLS regression of the outcome fixed effects on eligibility approval fixed effects at the office-by-issue level, where each observation is weighted by the number of separation-based eligibility issues in that office during the sample period.

Figure 1-10: Variation in Eligibility Approval Propensities Across Office-by-Issue Pairs



Notes: The figure plots office-by-issue coefficients γ estimated from Equation 1.2 where the outcome D is eligibility approval. Coefficients are sorted along the x-axis in ascending order and normalized so that the smallest fixed effect is 0. The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019.

Figure 1-11: Consistency of Approval Rates Across Demographics and Within Office-by-Issue

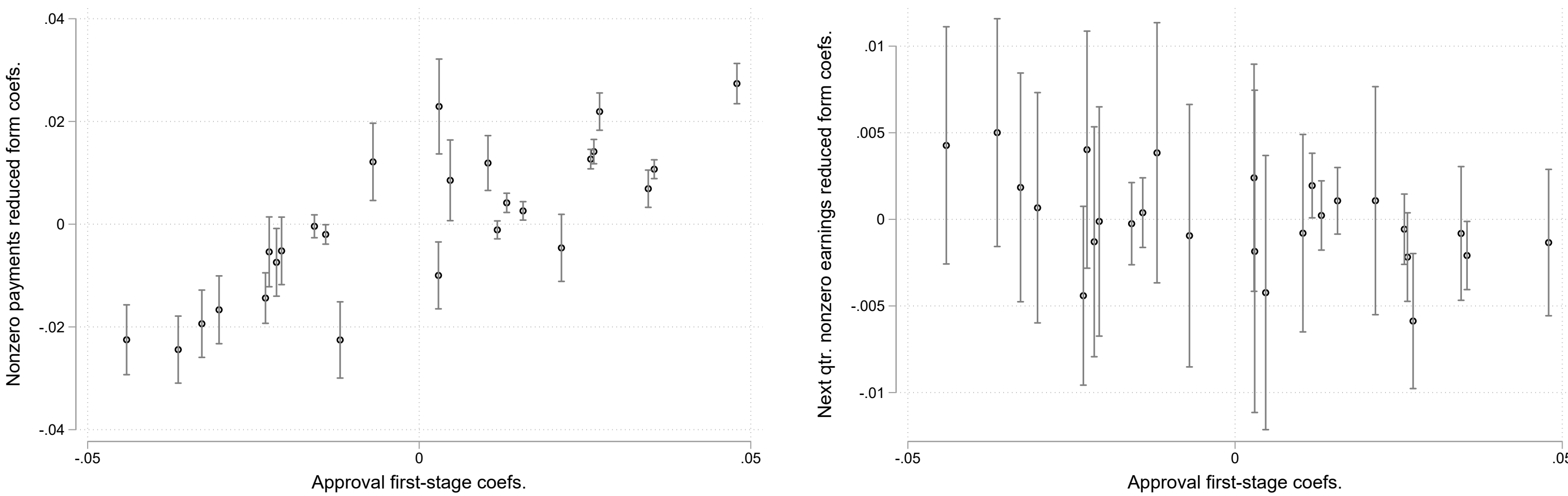


Notes: Each panel displays group-specific separation-based eligibility approval propensities at the office-by-issue level. Each bubble is an office-by-issue pair. Bubble size is proportional to the number of eligibility issues the office-by-issue pair adjudicates and the coordinates are eligibility approval rates for groups of claimants labeled on the axes. The coefficient is from a regression of the approval rate for the *y*-axis group on the approval rate for the *x*-axis group at the office-by-issue level weighted by the number of eligibility issues in the office-by-issue pair. The sample includes all separation-based eligibility issues from regular initial claims between 2002 and 2019.

Figure 1-12: Visual IV Representation of First-Stage and Reduced-Form Effects

(a) Any payments

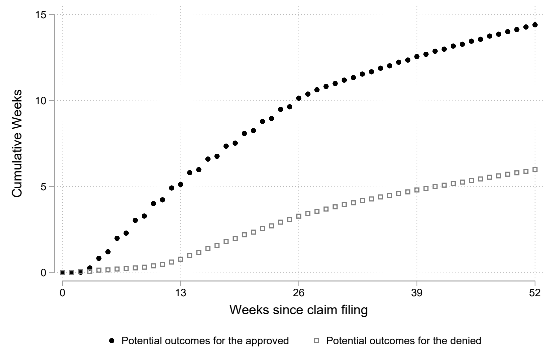
(b) Any earnings 1 qtr. after



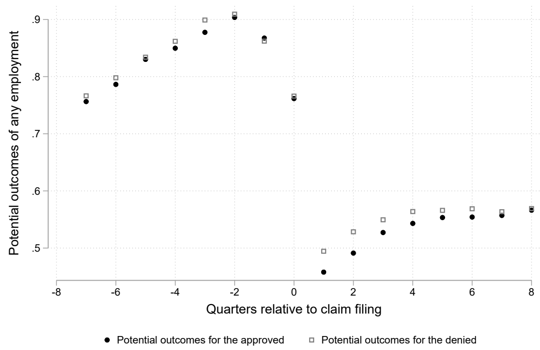
Notes: Each panel presents reduced-form coefficients for office-by-issue pairs on the *y*-axis and first-stage coefficients for office-by-issue pairs on the *x*-axis. Both sets of coefficients come from Equation 1.2, where the outcome is eligibility approval for the first-stage and the variable in the graph title for the reduced-form. The sample is of all regular initial claims with separation-based eligibility issues between 2002 to 2019.

Figure 1-13: Dynamic Potential Outcomes by Treatment Status

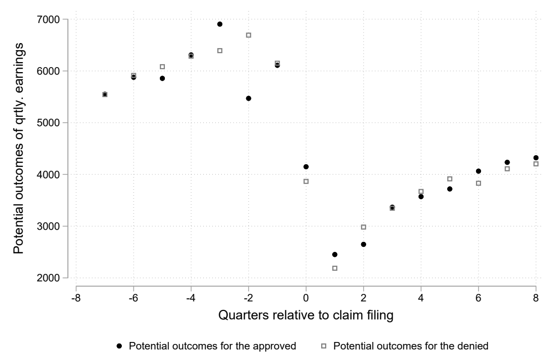
(a) Paid Week



(b) Any Earnings



(c) Avg. Earnings Incl. 0's



Notes: Each panel decomposes the treatment effects in Figure 1-4 into complier potential outcomes for approved and denied claimants. Treated potential outcomes are estimated by interacting Y with an indicator for eligibility *approval* D in Equation 1.1. Untreated potential outcomes are estimated by interacting Y with an indicator for eligibility *denial* $(1 - D)$, replacing the indicator for eligibility approval D with an indicator for eligibility denial $1 - D$, and estimating the system. Panels (b) and (c) include 95% confidence intervals while Panel (a) omits them. The sample is of all regular initial claims with separation-based eligibility issues between 2002 to 2019.

1.10 Appendix Tables

Table 1.8: Robustness of Main Results to a Fully Saturated Specification

	(1)	(2)	(3)	(4)
	Any Payments	Avg. $ in qtrly. earnings (w/ 0's)	Any earnings 1 qtr. after	Consecutive qtrs w/o earnings
Baseline IV	0.32	17	-0.04	0.14
SE	(0.01)	(142)	(0.01)	(0.07)
Saturate and Weight IV	0.37	-14	-0.05	0.05
Untreated complier mean	0.26	3,530	0.49	2.37
First-stage F	405			
Unique N	5.5m			

Notes: The first two rows and the last three rows replicate results in Table 1.5. The sample includes all separation-based eligibility issues from regular initial claims between 2002 and 2019. The baseline IV results are estimated from Equations 1.1 and 1.2 using UJIVE where the endogenous treatment D is initial eligibility approval. These include fixed effects for month and issue type. The third row labeled "Saturate and Weight" separately estimates the office assignment IV design within the 480 month-by-issue cells. Each specification, therefore, does not include controls. We aggregate these estimates proportional to the sample size within each month-by-issue cell.

Table 1.9: Heterogeneous Effects of Initial Eligibility Approval on UI Receipt

	Age Quartile				Gender		Race	
	1st	2nd	3rd	4th	Male	Female	White	Nonwhite
IV	0.33	0.37	0.35	0.26	0.32	0.33	0.32	0.33
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
tf SE	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]	[0.01]	[0.01]	[0.01]
F	112	121	102	86	196	214	151	261
N	1.51m	1.86m	1.81m	1.73m	3.50m	3.42m	2.55m	4.39m
	Prior Earnings Quartile				Employed Qtr. Before Claim		SDI $\rightarrow$ UI	
	1st	2nd	3rd	4th	No	Yes	No	Yes
IV	0.28	0.33	0.36	0.39	0.18	0.35	0.34	0.27
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.03)
tf SE	[0.02]	[0.02]	[0.02]	[0.02]	[0.03]	[0.01]	[0.01]	[0.03]
F	127	100	101	103	50	362	371	51
N	1.73m	1.73m	1.73m	1.73m	0.88m	6.06m	6.11m	0.83m
	Time Period				Issue		Self-Reported Layoff	
	2002-6	2007-11	2012-16	2017-19	Misconduct	Quit	No	Yes
IV	0.41	0.30	0.34	0.43	0.33	0.32	0.40	-0.03
SE	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.03)
tf SE	[0.01]	[0.01]	[0.02]	[0.02]	[0.01]	[0.01]	[0.01]	[0.03]
F	241	243	118	92	417	383	231	48
N	1.77m	2.17m	1.87m	1.10m	4.29m	2.65m	3.25m	0.99m

Notes: The overall sample includes all separation-based eligibility issues from regular initial claims between 2002 and 2019. The IV results are estimated from Equations 1.1 and 1.2 using UJIVE where the endogenous treatment D is initial eligibility approval and the outcome Y is an indicator for receipt of any UI payments following the initial claim. Each column represents a separate model estimated on a given subsample. Earnings quartiles are constructed in the entire sample based on the average quarterly earnings in 7 quarters prior to the claim. Lower quartiles correspond to lower levels of the variable. The 1st time period is 2002-2006, the 2nd is 2007-2011, the 3rd is 2012-2016, and the 4th is 2017-2019. The tF adjustment affects subsamples with an F-statistic below 104.7 and uses a linear interpolation between Table 3A values in Lee et al. (2022). All robust standard errors are at the 95% confidence level and are clustered by claimant.

1.11 Appendix Analyses

1.11.1 Separation-Based Eligibility Effects using Examiner Assignment

This section supplements our primary instrumental variables research design based on processing office assignment during 2002-2019 with a complementary instrument variables research design based on examiner assignment during 2017-2019. Its structure of it mirrors that of Section 1.4. The primary advantage of the design based on examiner assignment is that the more granular source of variation addresses potential monotonicity and excludability concerns. The primary disadvantage is that data availability limits the time period, which complicates making comparisons across policies, weakens the first-stage relationship, and precludes heterogeneity analyses.

Estimating Equation

Consider the following system of equations for claimant i's initial claim filed in month t:

$$Y_{it} = \beta D_{it} + \mathbf{X}_{it}'\psi + e_{it} \tag{1.10}$$

$$D_{it} = \mathbf{Z}_{it}'\gamma + \mathbf{X}_{it}'\mu + \varepsilon_{it} \tag{1.11}$$

where Y_{it} is the endogenous outcome of interest; D_{it} is the endogenous UI treatment of interest; $\mathbf{Z}_{it}$ is a vector of indicator variables corresponding to a full interaction between the assigned examiner and separation-based issue type; and $\mathbf{X}_{it}$ is a vector of control variables (i.e., fully-interacted dummies for language, assigned office, separation-based issue type, and claim filing month). The equation is overidentified because the excluded instrument $\mathbf{Z}_{it}$ of assigned examiners is a vector.

The sample used to estimate Equations 1.11 and 1.10 consists of only algorithmically scheduled claims between 2017 to 2019 in order to isolate the quasi-random assignment of examiners. Following the existing literature, we mitigate weak instruments concerns by limiting to examiners who handled a sufficient number of claims (Bhuller et al., 2020). We choose 200 as the threshold, as it retains approximately 90% of the sample.

Including language, office, issue, and time fixed effects is motivated by the assignment mechanism discussed in Section 1.2.3. First, while language fixed effects are most important to our identification strategy, they are unlikely to be quantitatively important. In particular, even though language certainly affects both a claimant's examiner assignment probabilities and plausibly affects subsequent employment outcomes, Table 1.2 shows that 94% of the sample speaks English. Second, while the assigned office is quasi-randomly assigned based on SSN, we include assigned office fixed effects to strengthen the exclusion restriction. This controls for any potential effect the office can have on claimants apart from the eligibility decision. Third, issue type fixed effects are necessary due to the instrument including an interaction with issue type. Finally, month-of-claim fixed effects are included due to changing macroeconomic conditions. California's unemployment rate gradually fell from 5% to 4% between 2017 and 2019, but claimants later on in the sample period faced the severe Covid-19 recession several quarters after their claim. If the pool of examiners remained fixed during this time period, then increased precision would be the sole benefit of these time fixed effects. However, due to some examiner hiring and attrition, these also address a potential confound due to changes in examiner composition over time.

Validating the Instrument

Like the research design based on examiner assignment, the identification assumptions to interpret β in Equation 1.1 as a partial equilibrium LATE are independence, excludability, first-stage relevance, and monotonicity.

A useful auxiliary object for testing these assumptions is the predicted first-stage from Equation 1.11. To calculate it, we manually implement the residualization and leave-one-out procedure. Specifically, let $A_{it(jlos)}$ be an indicator eligibility approval of claimant i's claim filed in month t that is assigned to examiner j and residualize this by the fully-interacted language l, office o, issue type s, time t fixed effects in Equation 1.11:

$$A^*_{it(jlos)} = A_{it(jlos)} - \mathbf{X}'_{it(los)}\mu - \eta_t \tag{1.12}$$

We then calculate the scalar leave-one-out mean of this residualized leniency measure at the examiner-level:

$$\tilde{Z}_{it}^{js} = \left(\frac{1}{n_{js} - 1}\right)\left(\sum_{k(js)} \sum_{t(js)} A^*_{kt(jlos)} - A^*_{it(jlos)}\right) \tag{1.13}$$

where n_{js} denotes the total number of algorithmically scheduled separation-based eligibility issues of type s handled by examiner j.

The first identification assumption—independence—requires that examiners be independent of potential outcomes, and it is conceptually supported by the quasi-random assignment of examiners among examiners assigned algorithmically. Table 1.10 empirically supports. Specifically, we calculate the residualized leave-one-out approval rate $\tilde{Z}_{it}^{j}$ from Equation 1.13 among the sample of algorithmically scheduled claims and split claims by those with above vs. below median values of $\tilde{Z}_{it}^{j}$. Reassuringly, these two groups of claimants have strikingly similar pre-existing characteristics. The only statistically significant difference across the two groups—self-reported layoff and eligibility issue type—are economically small in magnitude.

The second identification assumption—excludability—requires that the only effect that examiners have on the endogenous outcome Y is through the endogenous treatment D. This is the chief benefit of the design based on examiner assignment. The design based on examiner assigned includes office fixed effects, so it leverages only *within-office* variation in eligibility approval propensities. In other words, in addition to the case for exclusion in the office-based design, the examiners-based design account for any other administrative effects of the office. The only role of the examiner is to make the eligibility determination. They do not handle other administrative duties related to the claim.

The third identification assumption—first-stage relevance—requires that the examiner assignment be predictive of the endogenous treatment. We directly test this by estimating the first-stage regression equation Equation 1.11 and testing the joint significance of the examiner dummies. The first-stage F-statistic of 8 for the binary endogenous treatment of initial eligibility approval is just above conventional thresholds for instrument relevance (Stock and Yogo, 2005). To ensure proper 95% coverage, we employ the tF confidence interval adjustment suggested by Lee et al. (2022). Given our first-stage F-statistics of 8 for the endogenous treatment of eligibility approval (any UI payment receipt), this inflates the second-stage confidence interval by 94%.

The final identification assumption—monotonicity—requires that an examiner-by-issue pair that is relatively more lenient with one type of claimant cannot be relatively less lenient with another type of claimant. Following the test implemented in Section 1.4, Figure 1-14a confirms the first-stage relationship is positive within various claimant subsamples, which is a testable implication of average monotonicity.

Results

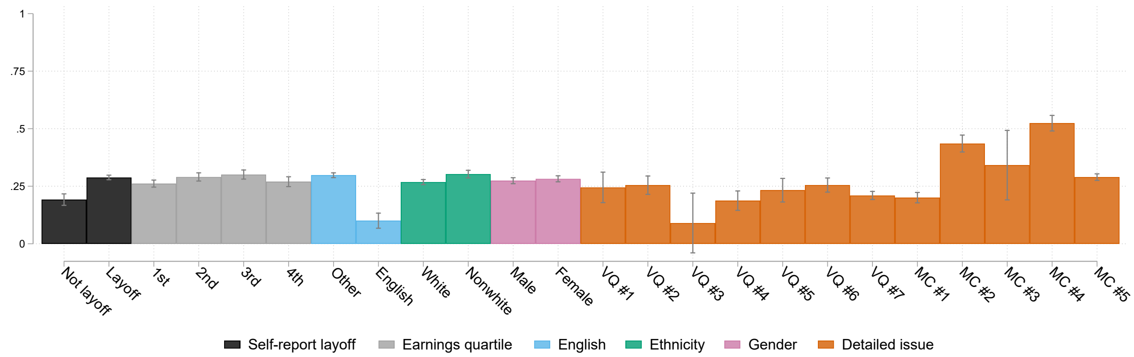
As in the office-based research design, the examiners-based research design finds that meaningfully increases benefit receipt. The decrease in any employment the quarter following the claim is larger by approximately 3 percentage points, and there is also a statistically significant decrease in earnings. Table 1.11 shows the treatment effect on receiving any payments, having any employment in the subsequent quarter, and average earnings in the subsequent quarter. Due to the weaker first-stage relationship, the tF adjustment is now nontrivial; this almost doubles the confidence intervals. Even so, the eligibility effects are highly statistically significant.

The dynamic effects of payment receipt mirror those in the office-based design, as Figure 1-15 replicates Figure 1-11 using examiner assignment as eligibility approval variation. The cumulative effect of eligibility approval on payment receipt is just over 10 weeks of benefit payments.

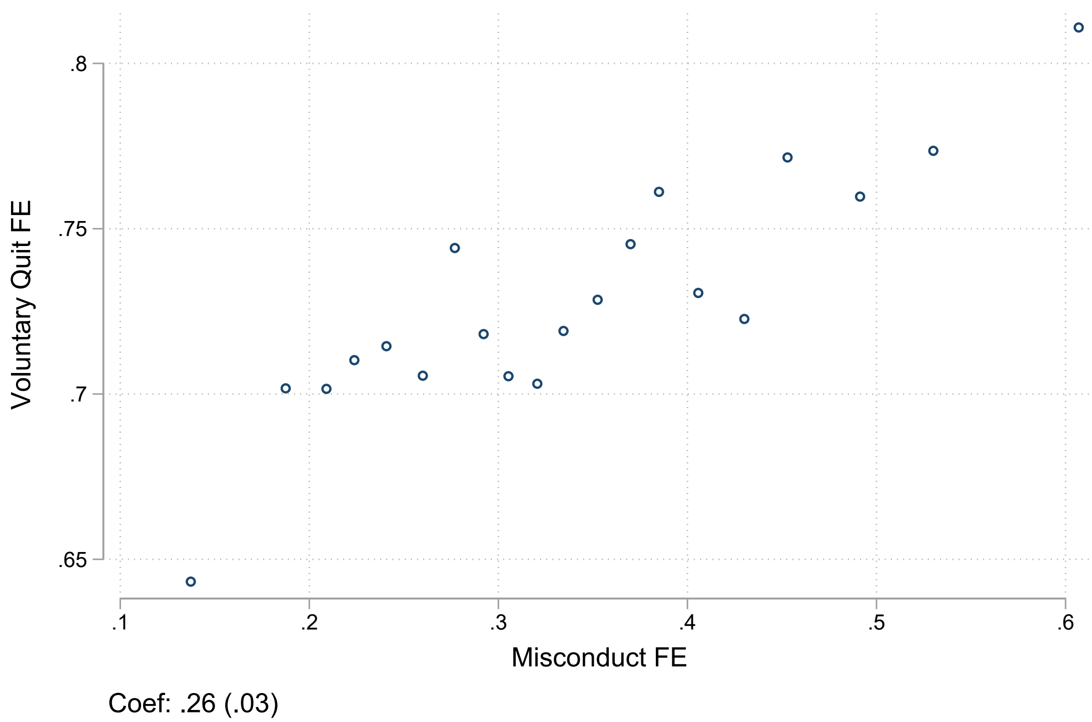
Figure 1-16 displays the dynamic employment effects of eligibility. The patterns are consistent with those in Figure 1-4, as any negative employment effect dissipates two years after the claim. The primary difference is that the point estimates for impacts on average total earnings are negative and only marginally insignificant. Within each panel, there are two series. One series includes office fixed effects while the other excludes office fixed effects. The similarity between the two series suggests that the downstream impacts of office-level eligibility variation are consistent with those of examiner-level eligibility variation, which assuages excludability concerns in the design based on office assignment.

Figures and Tables for Separation-Based Eligibility Examiner Research Design

Figure 1-14: Consistency of Examiner Leniency Measures



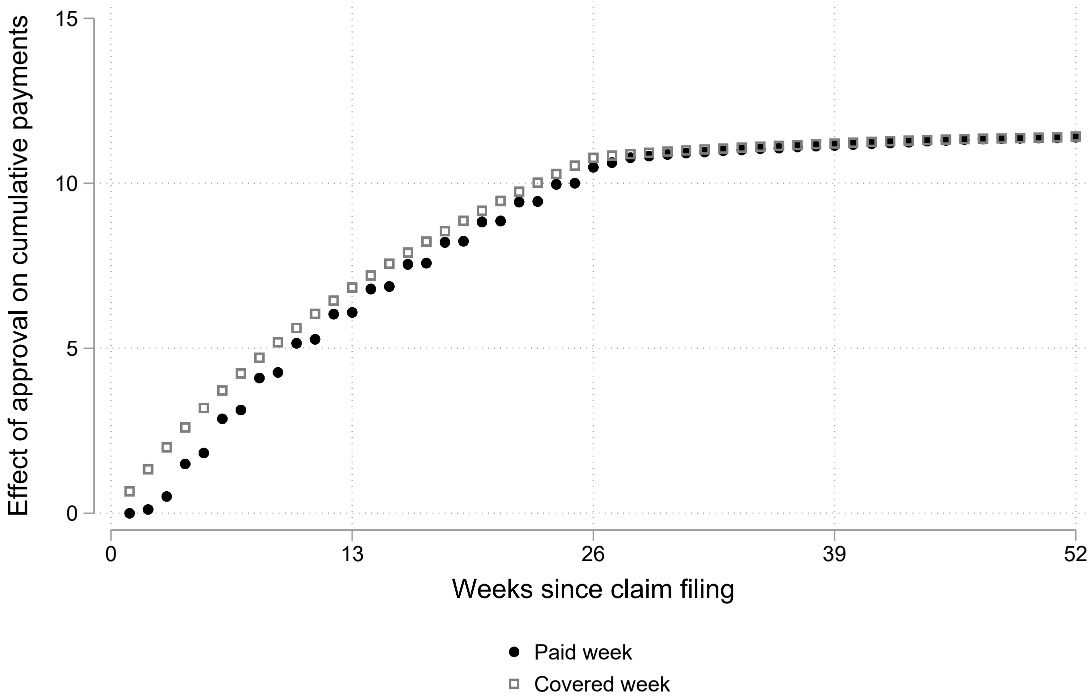
(a) Positive First-Stage within Claimant Subsamples



(b) Positive Correlation between Issue-Specific Leniency

Notes: In the top panel, each bar represents a separate regression of the claimant's own eligibility decision D_{it} on their assigned examiner's overall leave-one-out residualized eligibility leniency $\tilde{Z}_{it}^{j}$ within a given subsample. Each color represents a different categorical variable, and separate bars refer to separate levels of that categorical variable. Lower quartiles correspond to lower levels of the variable. Detailed issue types refer to subcategories within misconduct (MC) and voluntary quit (VQ). Robust standard errors are clustered by claimant and error bars provide 95% confidence intervals. The bottom panel is a binned scatterplot of the average quit-specific examiner-level leniency by ventiles of misconduct-specific examiner-level leniency. Each ventile contains approximately 50 unique examiners.

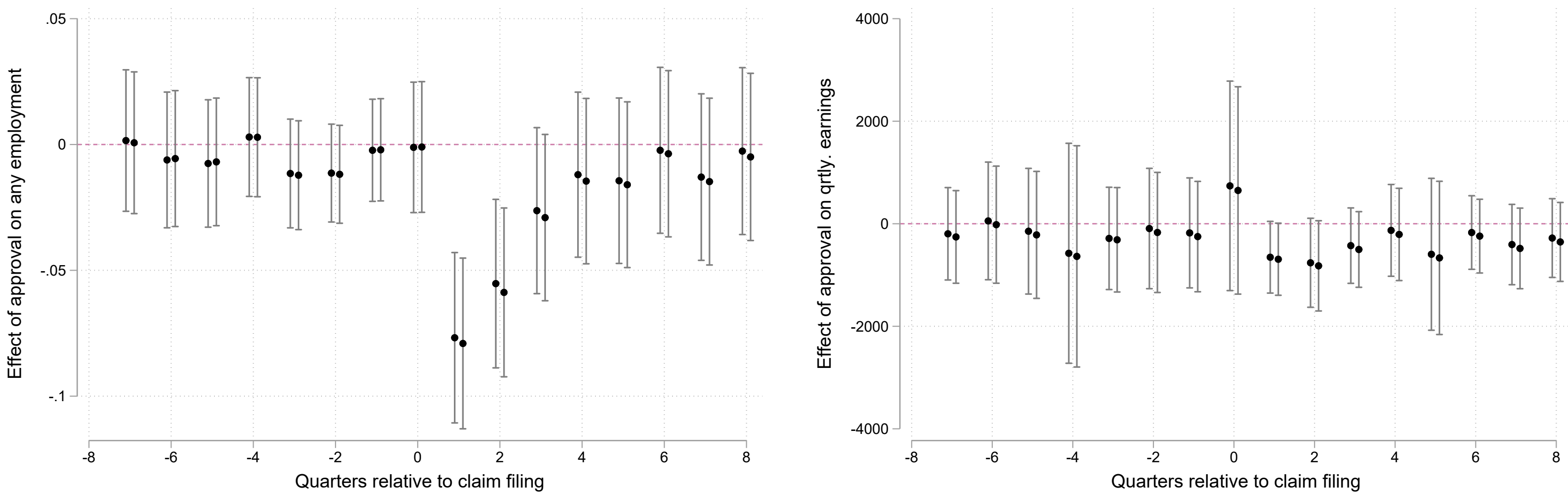
Figure 1-15: Dynamic Impacts of Eligibility Approval on Cumulative Benefit Receipt



Notes: This figure displays coefficients from separate regressions of the form in Equation 1.10 and Equation 1.11, where the outcome is a measure of cumulative payments as of that week. A paid week refers to the calendar week the payment is made, while a covered week refers to the week of unemployment to which that payment pertains.

Figure 1-16: Dynamic Impacts of Eligibility Approval on Employment, Both Including and Excluding Office Fixed Effects

(a) Any Employment (b) Avg. Qtrly Earnings



Notes: Both panels display coefficients from separate UJIVE IV regressions of the form in Equation 1.10 and Equation 1.11, where the outcome Y is a measure of contemporaneous quarterly employment and the endogenous treatment D is initial eligibility approval. Within each panel, one series includes office fixed effects while the other excludes office fixed effects. The sample includes all separation-based eligibility issues from regular initial claims between 2017 and 2019 that were assigned to examiners through the scheduled queue. The tF adjustment uses our first-stage F of 8.42 and conservatively derives the inflation factor of 1.944 using a linear interpolation between Table 3A values in Lee et al. (2022). All robust standard errors are at the 95% confidence level and are clustered by claimant.

Table 1.10: Claimant Balance Across Examiner Leniency

	(1)	(2)	(3)	(4)
	Ad hoc scheduling	Algorithmic scheduling		
		Above-median leniency	Below-median leniency	p-value of diff.
Age	37.5	37.5	37.5	0.60
Nonwhite	0.696	0.675	0.673	0.39
Prior earnings	8,371	8,913	8,921	0.85
Days to decision	27.1	19.4	19.4	0.61
Self-reported layoff	0.270	0.115	0.110	0.00
Share misconduct	0.609	0.715	0.707	0.00
Initially eligible	0.601	0.663	0.574	0.00
Any payments	0.574	0.605	0.553	0.00
N	658,020	222,579	222,579	

Notes: The overall sample consists of all regular UI initial claimants between 2017 and 2019 with a separation-based eligibility issue. The sample of algorithmically scheduled claims limits to examiners who handled at least 200 such claims. Column 1 reports claimant demographics at the time of claim filing and claim outcomes. Column 2 reports averages for claimants assigned ad hoc to examiners, which is not the analysis sample of interest. Columns 3 and 4 report averages of claimants assigned to examiners with above and below-median leniency, respectively, as measured by the leave-one-out residualized eligibility approval rate. Column 5 reports p-values calculated from separate regressions of each row variable on an indicator for being assigned to an examiner with above median leniency. These regressions also include language-by-assigned office fixed effects and standard errors are clustered by claimant.

Table 1.11: Effects of Initial Eligibility Approval

	(1)	(2)	(3)	(4)
	Any Payments	Earnings 1 qtr. after	Emp. 1 qtr. after	Consecutive qtrs w/o earnings
IV	0.55	-653	-0.08	0.25
SE	(0.01)	(183)	(0.01)	(0.05)
tf-adjusted SE	[0.01]	[357]	[128]	[0.10]
OLS	0.02	973	-0.06	0.20
SE	(0.00)	(30)	(0.00)	(0.01)
Untreated complier mean	0.26	3,293	0.56	1.94
First-stage F	8			
Unique N	396k			

Notes: The sample includes all separation-based eligibility issues from regular initial claims between 2017 and 2019 that were assigned to examiners through the scheduled queue. The IV results are estimated from Equations 1.10 and 1.11 using UJIVE where the endogenous treatment D is initial eligibility approval. The tF adjustment uses our first-stage F of 8.42 and conservatively derives the inflation factor of 1.944 using a linear interpolation between Table 3A values in Lee et al. (2022). All robust standard errors are at the 95% confidence level and are clustered by claimant.

1.11.2 Replicating Other UI Research Designs in California Data

This section contains supporting details regarding the institutional details and necessary sample restrictions for each of the non-separation-based UI policies we estimate in the California data. The research design for monetary eligibility (weekly benefit amount, potential benefit duration) uses a discontinuous change in the level (slope) in the policy at a cutoff for some continuous measure of prior earnings; the difference in levels (slopes) of an outcome across the cutoff is the reduced-form effect, the difference in levels (slopes) of the UI policy across the cutoff is the first-stage effect; and the ratio of the reduced-form effect to first-stage effect is the local average treatment effect of the UI policy for those around the cutoff (Cattaneo et al., 2020).

The primary threat to identification for all the RD/RK designs is manipulation that leads claimants locally above the cutoff to be different than those locally below the cutoff for reasons other than the UI policy treatment of interest. This could be due to strategic filing behavior by the claimant (e.g., knowing one's own earnings history along with UI eligibility rules and declining to file a claim if monetarily eligible) or incomplete record-keeping (e.g., some records of claims denied on monetary eligibility grounds are not retained). We test for this in two ways. First, we implement two distinct tests for discontinuities in the sample density around the cutoff (McCrary, 2008; Cattaneo et al., 2020). Second, we examine the conditional expectation of an auxiliary measure of earnings that is not directly related to the earnings measure used to determine UI benefits.

Monetary Eligibility

Institutional Details The base period is defined as the set of quarters used to determine monetary entitlement. The standard base period is defined as the earliest 4 of the 5 completed quarters preceding the UI claim, and the alternative base period is defined as the 4 completed quarters preceding the UI claim. Base period wages are defined as the sum of all earnings during the base period, and high quarter wages are defined as the highest quarter of earnings during the base period.

There are two ways claimants can be monetarily eligible: (i) high-quarter wages at least \$1,300 or (ii) high-quarter wages at least \$900 and a ratio of base period wages to high-quarter wages of at 1.25. Monetary entitlement for claims filed prior to April 2012 is determined solely by the standard base period. For claims filed after April 2012, if the claimant is monetarily ineligible using the standard base period, monetary entitlement is then checked using the alternative base period. In all time periods, claimants can appeal a monetary eligibility denial by producing evidence of UI-covered earnings that were erroneously excluded from administrative records.

Sample Restrictions We restrict to claims filed prior to April 2012, as the alternative base period qualification almost entirely removes the discontinuity in monetary eligibility at $HQW^* = 0$.

We define recentered high-quarter wages HQW^* as follows:

$$HQW^*(BPW) = \begin{cases} HQW - 900 & \text{if } BPW < 1.25 \\ HQW - 1300 & \text{if } BPW \geq 1.25 \end{cases} \tag{1.14}$$

We also consider only claims with $HQW^* \in (-900, 2500)$.

Estimating Equation We estimate 2SLS linear regressions. The reduced-form and first-stage equations are:

$$Y_i = \beta_0 + \tau_y T_i + \beta_1 HQW_i^* + \beta_2 HQW_i^* \cdot T_i + \varepsilon_i \tag{1.15}$$

$$D_i = \alpha_0 + \tau_d T_i + \alpha_1 HQW_i^* + \alpha_2 HQW_i^* \cdot T_i + u_i \tag{1.16}$$

where Y_i is an outcome measure (e.g., total UI benefits received); D_i is an indicator for being recorded as monetarily eligible; and $T_i = \mathbf{1}\left[HQW_i^* \geq 0\right]$. The fuzzy RD estimator for the causal effect of monetary eligibility, which we use for calculating the total behavioral cost, is the ratio of the reduced-form and first-stage coefficients on T_i, $\hat{\tau} = \hat{\tau}_y / \hat{\tau}_d$. We use the constant β_0 to calculate the mechanical transfer with Y_i as counterfactual benefits.

Manipulation and First-Stage Tests Panel (a) of Figure 1-18 shows a clear discontinuity in the sample density across the monetary eligibility cutoff. The density is approximately

10% lower just below the cutoff, and this missing mass is consistent with either strategic claim filing or incomplete record-keeping. Panel (b) shows that average prior earnings are very slightly but discontinuously lower just above the cutoff. Finally, Panel (c) shows a clear discontinuous increase in UI benefit receipt across the cutoff.

Weekly Benefit Amount

Institutional Details Within a given year, WBA is determined solely by high-quarter wages. The maximum WBA in California has been \$450 since January 2005 and increased four times during 2000: from \$230 to \$370 in January 2002, to \$410 in January 2003, and to \$450 in January 2005. The target replacement rate was 0.39 prior to 2002, 0.45 in 2003, and 0.5 thereafter. Therefore WBA is:

$$WBA(t) = \min\left(\frac{HQW}{13} \cdot RR(t), WBA^{max}(t)\right) \tag{1.17}$$

where $RR(t)$ is the target replacement rate and $WBA^{max}(t)$ the maximum WBA at time t.

There are two temporary WBA supplements during our sample period. First, the Federal Additional Compensation program added \$25 to all WBAs from February 2009 through December 2010 due to the Great Recession. Second, the Federal Pandemic Unemployment Compensation program added \$600 to all WBAs between April and June 2020 due to the Covid-19 pandemic.

Sample Restrictions As we will expand on in the following discussion of potential benefit duration program rules, there is an offsetting kink in WBA among claimants with $PBD < 26$. Therefore, following Bell et al. (2022b), we restrict to claimants with the full regular potential benefit duration of 26 weeks. This excludes those with especially variable earnings across quarters.

We define recentered high-quarter wages HQW^* as follows:

$$HQW^*(t) = HQW - \frac{13}{RR(t)} \cdot WBA^{max}(t) \tag{1.18}$$

where $WBA^{max}(t)$ is the maximum WBA at time t. We restrict to claims with $HQW^* \in$

$(-5000, 5000)$. *Estimating Equation* We estimate 2SLS linear regressions. The reduced-form and first-stage equations are:

$$Y_i = \beta_0 + \tau_y T_i + \beta_1 HQW_i^* + \beta_2 HQW_i^* \cdot T_i + \varepsilon_i \tag{1.19}$$

$$D_i = \alpha_0 + \tau_d T_i + \alpha_1 HQW_i^* + \alpha_2 HQW_i^* \cdot T_i + u_i \tag{1.20}$$

where Y_i is an outcome measure (e.g., total UI benefits received); D_i is the recorded WBA; and $T_i = \mathbf{1}\left[HQW_i^* \geq 0\right]$. The fuzzy RKD estimator for the causal effect of an additional \$1 of WBA, which we use for calculating the total behavioral cost, is the ratio of the reduced-form and first-stage coefficients on the interaction term $HQW_i^* \cdot T_i$, $\hat{\beta} = \hat{\beta}_2/\hat{\beta}_2$. It is a fuzzy RKD because the actual WBA awarded to claimants may differ if they appeal with non-recorded UI-eligible wages. We use the constant β_0 to calculate the mechanical transfer with Y_i as the number of paid weeks.

Manipulation and First-Stage Tests Panel (a) of Figure 1-19 shows a visually smooth sample density across the WBA kink. Though the density appears to be mostly smooth across the entire support in Panel (a), both density manipulation tests reject smoothness across the kink. As discussed in Bell et al. (2022b), this is due to the bunching of high-quarter wages in even increments of \$1,000. This is not a problem as long as the “bunchers" are not unobservably different than nearby “non-bunchers" (Barreca et al., 2016). Reassuringly, Panel (b) shows average prior quarterly earnings are smooth through the kink. Finally, Panel (c) shows a clear discontinuous change in WBA slope across the kink in line with program rules.

Potential Benefit Duration

Institutional Details Once WBA is determined as described above, a formula determines the regular maximum benefit amount (MBA). In words, aims for no more than a 50% replacement based both on WBA and total base period wages. Formally, it is:

$$MBA = \min\left(\frac{1}{2} \cdot BPW, WBA \cdot 26\right) \tag{1.21}$$

Regular PBD is then defined as the number of weeks a claimant can receive their WBA before exhausting their regular MBA. Rearranging Equation 1.21:

$$PBD = \begin{cases} 26 & \text{if } WBA \cdot 26 \leq \frac{1}{2} BPW \\ \frac{\frac{1}{2} \cdot BPW}{WBA} & \text{if } WBA \cdot 26 > \frac{1}{2} \cdot BPW \end{cases} \tag{1.22}$$

Substituting in the case when $WBA < WBA^{max}$ from Equation 1.17 into Equation 1.22:

$$PBD = \begin{cases} 26 & \text{if } 4 \cdot RR(t) \leq \frac{BPW}{HQW} \\ \frac{13}{2 \cdot RR(t)} \cdot \frac{BPW}{HQW} & \text{if } 4 \cdot RR(t) > \frac{BPW}{HQW} \end{cases} \tag{1.23}$$

Equation 1.23 demonstrates the kink in the regular PBD formula with respect to $\frac{BPW}{HQW}$ we exploit.

There are several benefit extensions during our sample period. These benefit extensions increase the total PBD at a given point in calendar time in proportion to the regular PBD.[28] We define total PBD as the total number of continuous weeks of full regular and extended benefits the claimant could receive if they remained continuously unemployed.[29]

Sample Restrictions To avoid the offsetting kink due to WBA^{max} kink, we exclude claims with $WBA = WBA^{max}$. Additionally, since the ratio $\frac{BPW}{HQW}$ is by definition bounded above by 2—which is usually the location of the PBD kink—we further restrict to claims with earnings in every quarter of the base period.

We define the recentered ratio of base period wages to high-quarter wages $\frac{BPW}{HQW}^*$ as follows:

$$\frac{BPW}{HQW}^*(t) = HQW - \frac{BPW}{HQW} \cdot 4RR(t) \tag{1.24}$$

We further restrict to claims with $\frac{BPW}{HQW}^*(t) \in (-0.5, 0.5)$.

Estimating Equation We estimate 2SLS linear regressions. The reduced-form and first-

[28]Temporary Extended Unemployment Compensation increased the maximum PBD from March 2002 through December 2003 by at least an additional 13 weeks, Emergency Unemployment Compensation increased the maximum PBD from July 2008 through December 2013 by up to an additional 73 weeks, and Pandemic Emergency Unemployment Compensation increased the maximum PBD by up to an additional 73 weeks starting in March 2020. See Bell et al. (2022b) and Chodorow-Reich et al. (2019) for more detail.

[29]We ignore several instances of 1-week gaps when extended benefits temporarily expired.

stage equations are:

$$Y_i = \beta_0 + \tau_y T_i + \beta_1 HQW_i^* + \beta_2 HQW_i^* \cdot T_i + \varepsilon_i \tag{1.25}$$

$$D_i = \alpha_0 + \tau_d T_i + \alpha_1 HQW_i^* + \alpha_2 HQW_i^* \cdot T_i + u_i \tag{1.26}$$

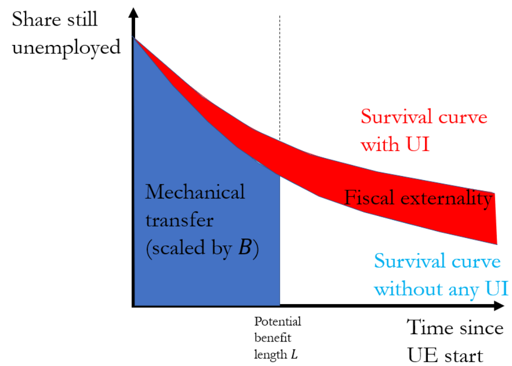
where Y_i is an outcome measure (e.g., total UI benefits received); D_i is the total PBD; and $T_i = \mathbf{1}\left[HQW_i^* \geq 0\right]$. The fuzzy RKD estimator for the causal effect of an additional week of benefits, which we use for calculating the total behavioral cost, is the ratio of the reduced-form and first-stage coefficients on the interaction term $HQW_i^* \cdot T_i, \hat{\beta} = \hat{\beta}_2/\hat{\beta}_2$. It is a fuzzy RKD due to measurement error in apportioning extended benefits in addition to the aforementioned possibility of benefit recomputation. We use the constant β_0 to calculate the mechanical transfer with Y_i as the product of WBA and an indicator for exhausting total benefits.

Manipulation and First-Stage Tests Panel (a) of Figure 1-20 shows a visually smooth sample density across the PBD kink. The density is smooth across the entire support in Panel (a), and both manipulation tests do not come close to rejecting the null of density continuity across the kink. Additionally, Panel (b) shows average prior quarterly earnings are smooth through the cutoff. Finally, Panel (c) shows a noisy but clear discontinuous change in PBD slope across the kink in line with program rules.

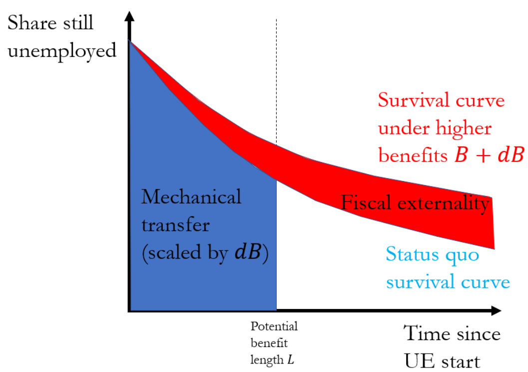
Figures and Tables for Other UI Research Designs

Figure 1-17: Graphical Illustration of the BCMC Ratio

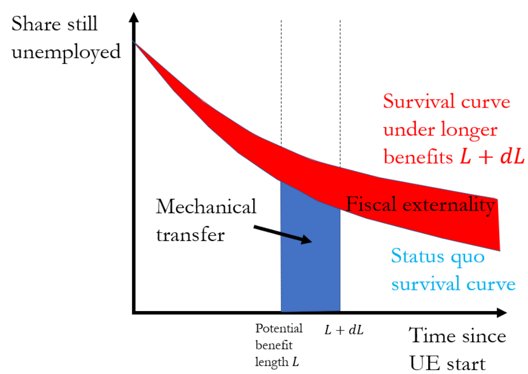
(a) Separation-based/Monetary Eligibility Expansion



(b) Weekly Benefit Amount Expansion

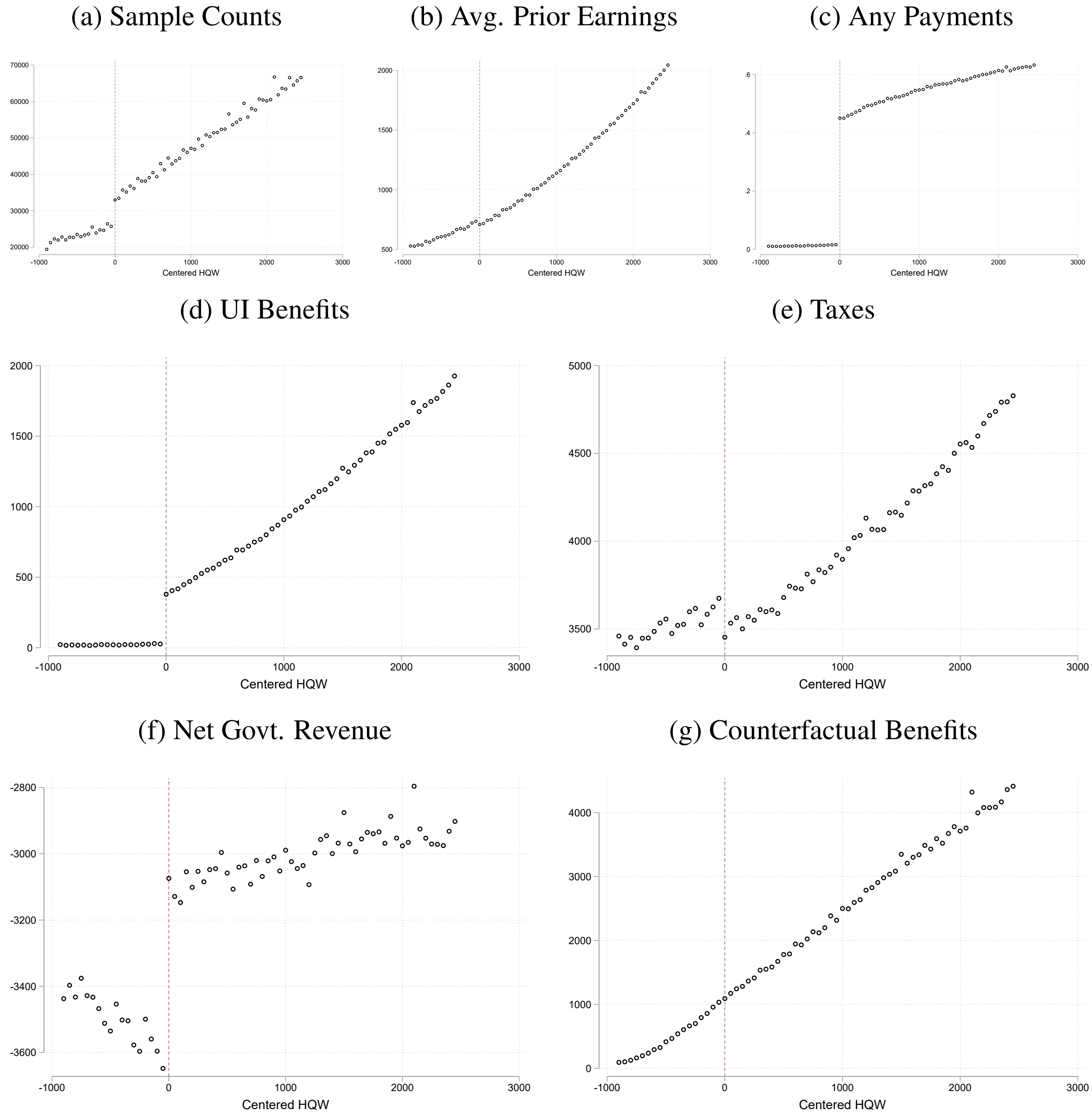


(c) Potential Benefit Duration Expansion



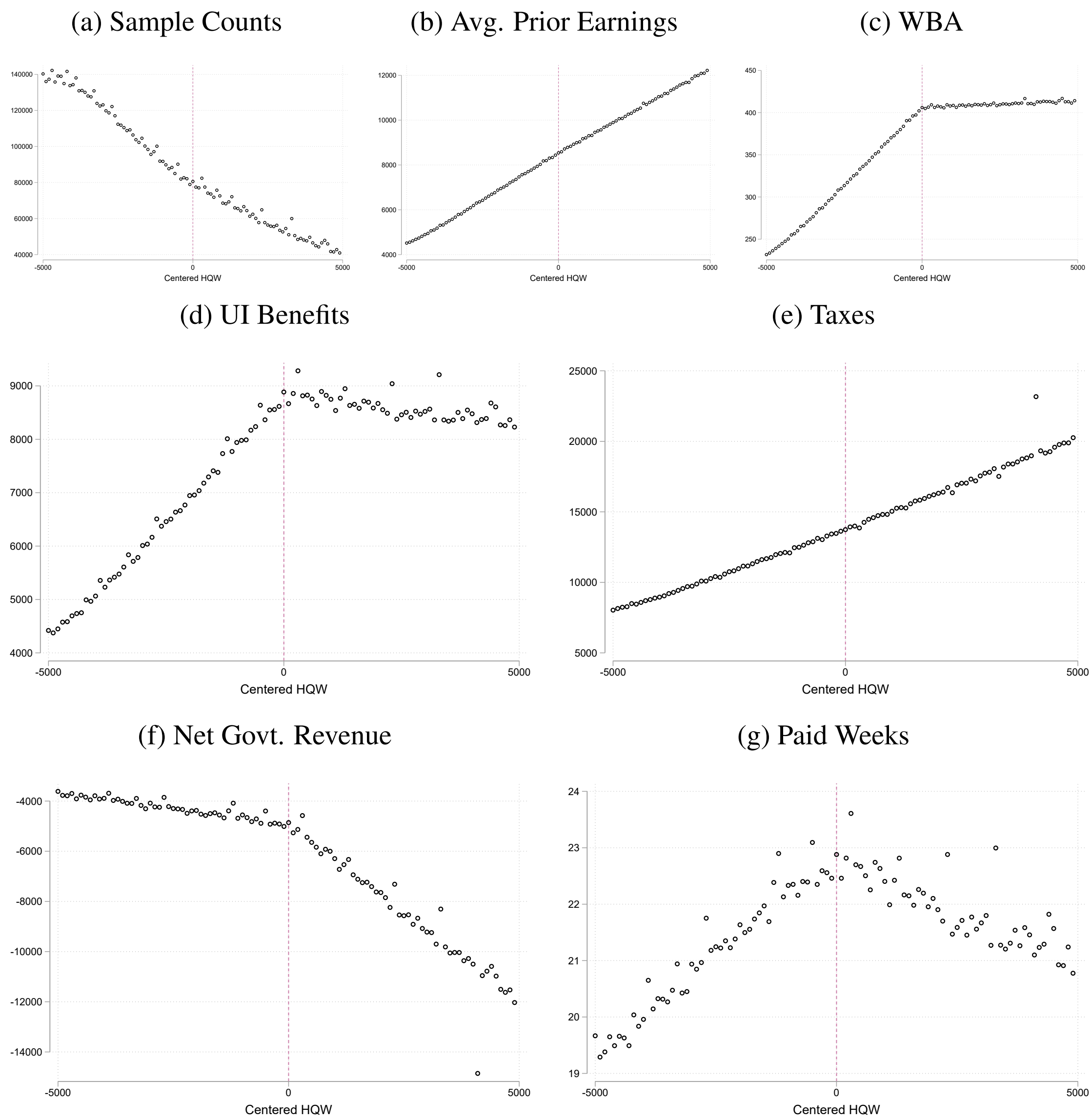
Notes: The panels illustrate calculating behavioral and mechanical costs to the government of different types of UI benefit expansions. Each panel displays hypothetical survivor curves with and without the benefit expansion for a homogeneous population, where the vertical distance between survivor curves represents an increase in nonemployment duration in response to benefit expansions. Under the status quo policy, the weekly benefit amount is B and the potential benefit duration is L. The mechanical cost is the portion of benefit expansions claimants received if nonemployment duration were held fixed. The behavioral cost is the sum of additional benefits and foregone tax revenues due to the increase in nonemployment duration.

Figure 1-18: Monetary Eligibility Regression Discontinuity Design



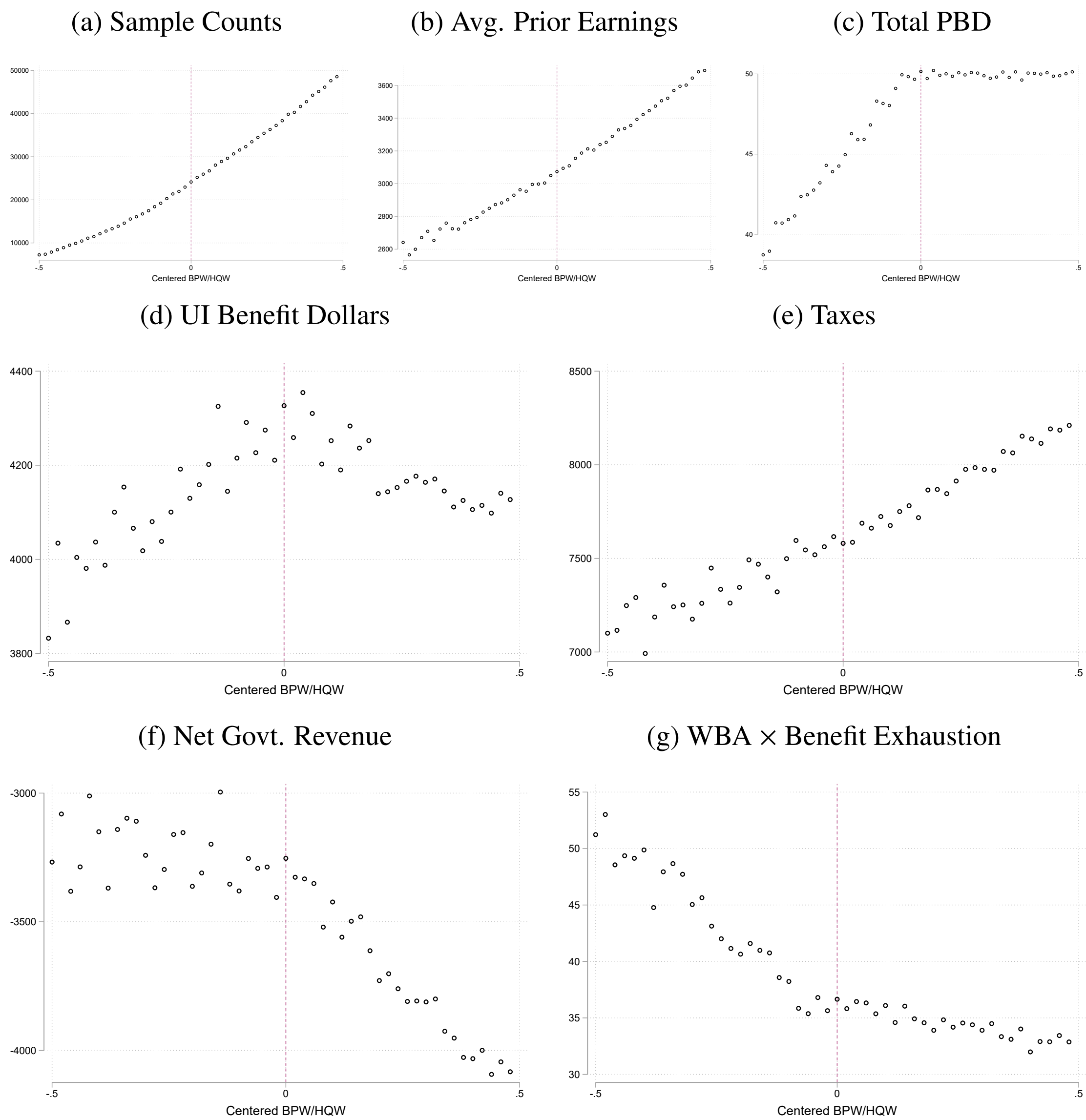
Notes: Each panel is a binned scatterplot of the monetary eligibility analysis sample described in Table 1.12. The running variable is high-quarter wages relative to the year-specific cutoff and the bin width is \$50. Panel (a) are counts in the sample. *p*-values for the McCrary (2008) test and Cattaneo et al. (2020) test of a discontinuity in density are both 0. Panel (b) is a placebo outcome of average quarterly earnings in the 7 quarters prior to the initial claim. Panel (c) is an indicator for receiving any UI benefit payments. Panel (f) is taxes paid in Panel (e) minus benefits received in Panel (d). Panel (g) is unscaled counterfactual benefits, whose cutoff value we scale by monetary eligibility's effect on payment receipt (0.52) to calculate the mechanical transfer.

Figure 1-19: Weekly Benefit Amount Regression Kink Design



Notes: Each panel is a binned scatterplot of the weekly benefit amount analysis sample described in Table 1.12. The running variable is high-quarter wages relative to the year-specific kink and the bin width is \$100. Panel (a) are counts in the sample. p-values for the McCrary (2008) test and Cattaneo et al. (2020) test of a discontinuity in density are 0 and 0.02, respectively. Panel (b) is a placebo outcome of average quarterly earnings in the 7 quarters prior to the initial claim. Panel (c) is the actual WBA awarded to the claimant. Panel (f) is taxes paid in Panel (e) minus benefits received in Panel (d). Panel (g) is the number of paid benefit weeks, whose value at the kink is the mechanical transfer.

Figure 1-20: Potential Benefit Duration Regression Kink Design



Notes: Each panel is a binned scatterplot of the potential benefit duration analysis sample described in Table 1.12. The running variable is the ratio of base period wages to high-quarter wages relative to the year-specific kink and the bin width is 0.02. Panel (a) are counts in the sample. *p*-values for both the McCrary (2008) test and Cattaneo et al. (2020) test of a discontinuity in density are 0.14 and 0.79, respectively. Panel (b) is a placebo outcome of average quarterly earnings in the 7 quarters prior to the initial claim. Panel (c) is the total PBD awarded to the claimant, which is the sum of regular PBD and any benefit extensions. Panel (f) is taxes paid in Panel (e) minus benefits received in Panel (d). Panel (g) is the number of paid benefit weeks, whose value at the kink is the mechanical transfer.

Table 1.12: Summary Statistics of Each UI Policy Margin's Analysis Sample

	(1)	(2)	(3)	(4)
UI Policy	**Separation eligibility**	**Monetary eligibility**	**Weekly benefit amount**	**Potential benefit duration**
Research Design	**IV**	**RDD**	**RKD**	**RKD**
Avg. prior qtrly earnings ($)	6,893	1,267	7,565	3,258
WBA ($)	274	76	341	221
Regular PBD (weeks)	24.4	20.0	26	25.3
Total PBD (weeks)	45.0	45.1	48.5	48.8
Any UI benefit receipt	0.59	0.49	0.78	0.72
P(Exhaust UI | any UI)	0.18	0.10	0.13	0.10
Quarters of UE	2.4	2.4	1.8	1.5
Years	'02-'19	'02-'12	'00-'19	'00-'19
N	7.1m	2.9m	8.7m	1.2m

Notes: This table contains average characteristics for the relevant samples in each research design using the California microdata. For separation-based eligibility, the office-based IV sample is our main analysis sample of all separation-based eligibility issues from regular initial claims between 2002 and 2019. For monetary eligibility, the RDD sample restricts to those with high quarter wages fewer than $900 below the threshold and or $2,500 above the threshold, where the threshold is usually $900, during the pre-2012 time period without alternative base period qualification. For weekly benefit amount, the RKD sample restricts to those with high quarter wages within $5,000 of the kink and at the maximum potential benefit duration. For potential benefit duration, the RKD sample restricts to those with a ratio of base period wages relative to high quarter wages within 50 percentage points of the kink and below the maximum weekly benefit amount.

Chapter 2

Skill Depreciation during Unemployment: Evidence from Panel Data

We use a panel of survey responses linked to administrative data in Germany to measure the depreciation of skills while workers are unemployed. Both the reemployment hazard rate and reemployment earnings steadily fall with unemployment duration, and indicators of depression and loneliness rise substantially. Despite this, we find no decline in a wide range of cognitive and noncognitive skills while workers remain unemployed. We find the same pattern in a panel of American workers. The results imply that skill depreciation in general human capital is unlikely to be a major explanation for duration dependence.

2.1 Introduction

As workers remain unemployed, they see their prospects for earnings and reemployment steadily fall—a tendency that economists call *duration dependence* (Van den Berg and Van Ours, 1996; Kroft et al., 2013). Figure 2-1 demonstrates this relationship using a panel of German workers. Panel (a) shows the steep decline in job finding as workers remain unemployed. The odds of finding a job are five times higher right after job loss than they are after two years of unemployment. The bottom figure shows that reemployment wages also fall over time as workers remain unemployed. The long-term unemployed earn 20 percent less than the short-term unemployed when they return to work conditional on prior earnings.

One explanation for these patterns is that human capital *depreciates* while workers are unemployed (Jarosch, 2021; Schmieder et al., 2016; Aaronson et al., 2010). In the presence of skill depreciation, even temporary shocks causing unemployment can have long-lasting effects (Pissarides, 1992; Rothstein, 2020), which could explain chronically high unemployment in some Western European countries and so-called "scarring effects" after recessions (Acemoglu, 1995; Ljungqvist and Sargent, 2008). The serious concern about skill loss during unemployment is not only the concern of academics, but also of prominent policymakers. A great example of that is Ben Bernanke's Monetary Policy Report to Congress in 2013:[1]

> *"One concern we do have, of course, is the fact that more than 40 percent of the unemployed have been unemployed for six months or more. Those folks are either leaving the labor force or having their skills eroded. Although we haven't seen much sign of it yet, if that situation persists for much longer then that will reduce the human capital that is part of our growth process going forward."* -Ben Bernanke, Federal Reserve Chairman, 2013

Understanding the role of skills in declining outcomes clarifies which interventions are likely to be successful. Programs that attempt to maintain skills are most likely to be effective *if* skill declines actually explain the fall in reemployment prospects. If employment

[1] https://www.federalreserve.gov/newsevents/testimony/bernanke20130226a.htm

prospects decline for some other reason, then it is less clear that such interventions will be fruitful. We have surprisingly scarce evidence on the evolution of skills during unemployment, as pointed out by Machin and Manning (1999) in the Handbook of Labor Economics. Direct eliciations of skills are restricted to a handful of surveys with few unemployed respondents. Further, a naïve comparison of the skills of people unemployed for different periods is unlikely to reveal a causal relationship of unemployment on skills because of selection: those with weaker employable skills may take longer to find work.

To address the core challenge, we employ novel *panel* data to track the evolution of an individual's skills as he or she remains unemployed. Our data measure skills for a large sample of German workers at the onset of unemployment around 2007 and three additional times over the following three years. The panel dimension addresses traditional selection problems by making within-worker comparisons over time, implicitly controlling for unobserved factors that differ across workers.

A unique feature of our data is that it measures a wide range of cognitive and noncognitive general skills. The cognitive measurements include math skills, verbal fluency, immediate memory, and recall memory.[2] The survey also includes standard measures of so-called noncognitive skills including conscientiousness, locus-of-control, patience, reciprocity, risk tolerance, and social trust (Almlund et al., 2011; Weiss et al., 2013; Kautz et al., 2014). The cognitive and noncognitive skills measured in the survey are meaningful components of human capital. Skills measured at the onset of unemployment meaningfully predict prior wages measured in administrative data, with out-of-sample R^2 values of 5–8 percent.

We find no evidence that any of the measured skills decline during an unemployment spell. This is true when we focus on within-person changes over the unemployment spell and if we compare the skill evolution of the unemployed to those of quickly reemployed in a difference-in-differences style analysis. Cognitive skills like mathematics, verbal fluency, and memory show no significant changes, and if anything, modestly improve. Noncognitive skills like locus-of-control and trust remain unaffected as workers remain unem-

[2]The verbal fluency question comes from a Weschler Adult Intelligence score module, while memory questions come from the Rey Auditory Verbal Learning Test (Groth-Marnat, 2003; Rey, 1964).

ployed. These patterns hold for workers potentially most seriously affected by unemployment: full-time workers with high labor-force attachment and those who were involuntarily unemployed. The only skills that show any evidence of decline are noncognitive skills like conscientiousness, risk tolerance, trust, patience, and reciprocity, though these skills do not appear to be earnings-relevant as they are essentially uncorrelated with earnings. We find little indication of dynamic selection based on cognitive and noncognitive skills over the unemployment spell. This is in line with previous evidence in the literature highlighting little selection based on demographic characteristics (see e.g. Schmieder et al., 2016).[3]

We provide two quantitative benchmarks to show the near-zero, statistically insignificant changes in skills among the continually unemployed which can explain only a small part of the fall in reemployment wages. First, we collapse skills into an index of predicted earnings and rule out reductions associated with larger than 3 percent (1 percent) earnings decline after 6 months (12 months). This is small relative to the 10–30 percent fall in reemployment wages among those with unemployment durations of at least 6 months. Second, focusing on the group of people who become reemployed soon after a skill survey, the point estimates suggest that skill depreciation does not explain any change in reemployment earnings, and the confidence intervals rule out explaining a share of earning declines larger than 28% after 6 months and 6% after 12 months.

We complement our analysis of newly unemployed German workers by studying a representative panel of older American workers. We use data from the Health and Retirement Study (HRS) that measures cognitive skills—including simple mathematics, memory, and fine-motor skills—for those at least 50 years old in the United States. The broad skill items we focus on predict earnings and explain about 2 percent of overall earnings variation. While the survey provides a more limited array of skill measures relative to the German data, we observe individuals prior to unemployment and over a longer period of time. We implement an event-study framework where we study the evolution of skills before and after unemployment onset.

[3]The lack of dynamic selection based on skills does not rule out dynamic selection based on other factors. A recent working paper by (Mueller and Spinnewijn, 2023) demonstrates the predictability of unemployment duration based on rich and detailed information on unemployed workers. (Mueller and Spinnewijn, 2023) also show that skills like IQ cannot predict unemployment duration (see Panel C in Table 3), which is consistent with our findings.

Our three main findings from the American data are highly complementary to our findings from the German data. First, we confirm the core findings from the German data: skills do not appear to be affected by unemployment duration, and general measures of life satisfaction are affected. Second, we demonstrate that there are no trends in survey measurements prior to the unemployment onset. Third, we validate the plasticity of our survey measure of skills by showing that skills do respond to certain shocks. In particular, we document that there is a significant reduction in skills following retirement. While the decline of cognitive skills after retirement is established in the previous literature before (see e.g. Rohwedder and Willis, 2010), these findings highlight that the stability of skills following unemployment is not merely the artifact of the skill measures used here.

These results paint a subtle picture of unemployed workers' experiences. In multiple countries across different worker populations, general elicited skills are stable over unemployment while general life satisfaction falls. The magnitudes of the impacts on life satisfaction are quantitatively meaningful: there is a 0.2–0.3 standard deviation increase in depression, loneliness, and life dissatisfaction in both surveys.

We contribute to the examination of skill depreciation and duration dependence. Most studies focus on changes in reemployment wages without directly observing the change in skills (see e.g. Kroft et al., 2013; Jarosch, 2021; Schmieder et al., 2016; Centeno and Novo, 2009; Jacobson et al., 1993). Notable exceptions are Arellano-Bover (2022) and Dinerstein et al. (2022), who document skill depreciation for young workers. Arellano-Bover (2022) shows that early-career unemployment shocks have negative effects on measured cognitive skills several decades later. Dinerstein et al. (2022) exploit quasi-experimental variation in unemployment at the beginning of Greek teacher's career and show negative effects of the length of unemployment on teachers' performance measured by students' test scores. Both of these papers find clear indications of skill depreciation coming from unemployment at the beginning of individuals' careers, which underscores the concerns of policy-makers as highlighted by Ben Bernanke. Nevertheless, contrary to this evidence, we find no indication of a decline in skills over the unemployment spell in the overall population in Germany and for the older workers in the United States. This suggests that the negative consequences of unemployment might be a more relevant concern at younger ages.[4] This finding could improve the targeting of programs designed to elevate skills over the unemployment spell.

The only other paper, to our knowledge, that longitudinally measures cognitive skills following unemployment is Edin and Gustavsson (2008). They document a decrease in literacy scores using surveys of several hundred workers surveyed four years apart. We improve on this evidence by applying broader measures of skills and using more granular data with detailed information on skills evolution around unemployment onset. Other closely related work examines the plasticity of personality measures around significant life events, though the results are mixed. Some papers find these measures are stable over time (Cobb-Clark and Schurer, 2012; Cobb-Clark and Schurer, 2013; Anger et al., 2017), while others find significant decreases (Preuss and Hennecke, 2018; Stillman and Velamuri, 2020). We extend previous work in three key dimensions: we link survey responses to administrative records on earnings and employment which provides more reliable measures of earnings and duration; we provide measures of a broader array of cognitive and noncognitive skills; and we use much larger samples with skill measurement at multiple points of the unemployment spell.

Finally, we align with a lengthy literature finding decreases in life satisfaction during unemployment. The most closely related work in this respect is Krueger and Mueller (2011), which elicits subjective well-being among a panel of newly unemployed workers at a weekly frequency. Most of the other work uses lower-frequency, general population surveys (Kettlewell et al., 2020; Powdthavee, 2012; Winkelmann and Winkelmann, 1998).

2.2 Data on Newly Unemployed German Workers

This section describes the content and context of our primary data source: the IZA/IAB Linked Evaluation Dataset from Germany. It is a representative sample of newly unemployed workers around 2007 with a panel survey of elicited skills and an administrative panel of employment outcomes.

[4]Skills tend to grow until age 30 and then fall (Edin and Gustavsson, 2008). As a result, the previous literature's finding of a negative effect of unemployment on the young could be due to a change in the growth rate rather than the level.

2.2.1 Panel Survey of Skills and Employment

The IZA Evaluation Dataset is a panel of four survey waves over three years composed of a representative sample of newly unemployed workers in Germany.[5] The German Federal Employment Agency randomly sampled prime-age individuals who filed for unemployment between June 2007 and May 2008. 17,396 individuals consented to the study (76 percent of invitees). The initial survey was completed within two months following entry into unemployment, and subsequent surveys were administered to participants who responded to all previous surveys. These occurred at twelve and thirty-six months following entry into unemployment. The initial survey screened out individuals who already had a job lined up upon entering unemployment, but subsequent waves surveyed all remaining participants regardless of labor market status. For the June, October, and February cohorts, an additional survey was administered six months after entry into unemployment. All surveys were administered by telephone.

Our primary outcomes of interest are the survey's objective cognitive assessments and subjective noncognitive self-assessments, both of which we refer to as skills. All survey waves include at least some of these questions. Table 2.3 details some of the individual survey skill items, Figure 2-4 summarizes which of these skills were elicited from each of the survey cohorts over time, and Figure 2-5 shows the number of respondents in each wave by question type.[6] Notably, we have close to 700 unemployed respondents around 6 months after onset for each question type, around 300 unemployed 12 months after, and around 150 unemployed 36 months after. This sample is an order of magnitude larger than other surveys on the skills of the unemployed. The initial survey also collects detailed demographics, and all waves include self-assessed life satisfaction and recent labor market experiences.

The personality assessments we use to measure noncognitive skills are Likert-scale responses, while the cognitive assessments measure objective performance.[7] Most questions

[5]IZA maintains a complete list of publications and working papers using the Evaluation Dataset: https://ed.iza.org/files/documentation/publications.pdf. This dataset has not yet been used to study skill depreciation.

[6]See Arni et al. (2014) for a detailed discussion of the survey content, questionnaire administration, and sample composition.

[7]The cognitive questions are nontrivial. For example, Table 2.5 shows that, at baseline, the three math

remained the same across survey waves.

2.2.2 Defining Earnings and Unemployment Duration

Administrative data from Germany's Federal Employment Agency comprise our primary source of labor market outcomes. Employment data is available for the 88% of survey respondents who consented. Prior to the unemployment spell, we observe the average daily wage, hours, and separation reason for the most recent employment spell along with annual employment and earnings for the ten previous years. For each of the thirty months following entry into unemployment, we observe benefit receipt, average daily earnings, and employment contract type. Because the final survey occurred thirty-six months after unemployment onset while the administrative data continues only thirty months after unemployment onset, we define labor market status at thirty-six months using the observed labor market status at thirty months. Average daily earnings during an employment spell is top-coded at 149€, and we refer to this as the wage throughout our analyses.[8]

In order to view unemployment as a discrete shock, our conception of employment focuses on relatively stable working arrangements. In this regard, a relevant feature of German labor markets is the tax-advantaged marginal employment contracts.[9] The most common arrangement is often referred to as a "mini job" (*geringfügige Beschäftigung*), which limits those without other employment to earning at most 400€ per month during our sample period. Another arrangement is often referred to as "short-term employment" (*kurzfristige Beschäftigung*), which limits participants to working at most 70 days in the position each year. These arrangements are quite common. Table 2.5 shows that slightly more than one-fifth of our sample of newly unemployed workers previously had this type of arrangement as their only employment, and we do not include these individuals in our

questions are answered correctly by 97%, 60%, and 21%; of the 10 listed words, the average immediate recall amount is 6.6 and the average subsequent recall amount is 5.1; and the median number of animals listed in a minute is 23.

[8]This comes from the limitation of the German Social Security data. Taking into account the full universe of German workers top coding affects around 10% of men and 1% of women and it is not binding for most people in our sample. Imputation methods have been developed to deal with this issue (see e.g. Card, Heining, and Kline, 2013). Nevertheless, we do not apply those to follow the existing literature on reemployment wages as close as possible (see e.g. Schmieder et al. (2016)).

[9]See Ebbinghaus and Eichhorst (2009) for a comprehensive discussion of German labor market institutions around this time period.

analysis sample.

Our definition of employment as non-marginal work recorded in the administrative data motivates our definitions of unemployment duration and reemployment wages. In particular, our primary definition of unemployment duration is the number of months the individual spent without any non-marginal employment following their initial entry into unemployment. The reemployment wage is then the wage in the first month that they first gained non-marginal employment.

As discussed in the Introduction, Figure 2-1 contains the changes in reemployment wages and hazard rates using the above definitions of employment. Panel (b) shows the change in reemployment wage relative to the prior spell. To control for macroeconomic trends, it also differences out wage growth of the quickly reemployed. We separately plot this double difference for each realized duration of unemployment, which reveals a noisy but consistent fall in reemployment wages with unemployment duration. Figure 2-6 confirms that the pattern is not driven by differencing out macroeconomic trends or changes in hours upon reemployment, and Figure 2-7 shows that the fall in reemployment wages with duration consistently holds across all prior earnings levels.

A unique feature of our linked administrative-survey data is that the survey includes typically unobserved labor market activities, such as self-employment or informal arrangements. This is particularly relevant for probing the robustness of our main result that skills do not change during unemployment. A potential concern is that the null result is driven by measurement error, as those who lack employment in the administrative data for long stretches are actually employed through alternative means. However, we find no indication that such type of mismeasurement can explain our findings. 2.10 details the relationship between administrative and survey-based measures of unemployment and shows that our results are robust to a narrower definition of unemployment based on the absence of any survey-based or administrative record of employment or training.

2.2.3 Defining Skill Indices

Because many skills are measured using multiple items, we reduce the questions for each skill group to a standardized unidimensional index. For each survey item, we construct a question-specific z-score, where we standardize using the mean and standard deviation of initial wave responses. We construct a skill-specific index by taking an equal-weighted average of the questions pertaining to that skill[10]. Finally, we standardize these indices—again using the initial survey's mean and standard deviation—to aid comparison across skills.

Recent work has highlighted potential limitations of interpreting psychometric responses cardinally (Bond and Lang, 2019; Nielsen, 2023), so we also follow Nielsen (2019) and index elicited skills to an economic outcome with a clear cardinal interpretation: earnings. In particular, we define summary skill indices for groups of skills. For each skill group—cognitive skills (math, verbal fluency, and memory); one set of noncognitive skills (the Big-5 and locus of control, which were elicited from all respondents); another set of noncognitive skills (risk tolerance, trust, patience, and reciprocity); and all skills together—we predict wages immediately prior to the unemployment spell with the baseline elicitations. In accordance with our primary definitions of employment and unemployment, we do so only for those with non-marginal employment immediately prior to the spell who have not yet been reemployed in non-marginal employment by the first survey. To avoid measurement error due to differences in hours worked, we also restrict the prediction exercise to those whose previous job was full-time. With each mapping of a group of skills to predicted earnings, we define summary skill indices in subsequent survey waves.

2.2.4 Predictive Content of Skills

The summary indices mapping skill groups to earnings are useful only insofar as the skills are predictive of earnings. We validate this in two ways. We first show that baseline skills are meaningfully correlated with prior earnings. Given this correlation and the variation in baseline skills, we then show that the elicited general skills explain a nontrivial amount of

[10]This aggregation also increases statistical power for detecting effects that operate in the same direction for a given skill (Kling et al., 2007).

the variation in prior earnings.

For the first part—that baseline skills are predictive of prior earnings—Figure 2-8 displays coefficients on each skill from separate univariate regressions with prior earnings as the dependent variable. We restrict to those in non-marginal employment for comparability with our analysis sample, and we restrict to those in full-time employment to avoid measurement error stemming from hours differences. As shown in panel (a), math has the most statistically and economically significant relationship with prior earnings; a 1 standard deviation increase in math performance is correlated with approximately 12% higher earnings. Locus of control and each of the Big-5 also have statistically significant relationships with prior earnings; a 1 standard deviation increase for each one is correlated with approximately 3% higher earnings. We confirm in panel (b) that these correlations hold even conditional on detailed demographics.

For the second part—that variation in baseline skills explains a meaningful portion of the variation in prior earnings—Table 2.4 shows that the composite skill index explains 5–8% of the *out-of-sample* variation in baseline earnings.[11]

Due to its interpretability and usefulness in subsequent decompositions, our preferred index is based on an OLS prediction whose inputs are z-scores of the individual skill groups. This implicitly treats ordinal responses as cardinal, so we also explore more flexible prediction methods that accommodate nonlinearities. Out-of-sample R^2 slightly increases when we use fully saturated levels of the cumulative distribution function for Likert-scale responses as inputs into a regularized prediction procedure. We show our results are robust when using these more flexible indices (see Figure 2-10).

Another benefit of a linear mapping from skill z-scores to predicted earnings is that we can easily demonstrate predicted earnings losses for decreases in skills. Figure 2-9 plots the earnings changes implied by our baseline indices mapping. A “loss" in a skill is defined as a decrease (increase) in the survey item z-score for an item with a positive (negative) coefficient in the multivariate regression of prior earnings on survey items at baseline. With this definition, a 0.5 standard deviation decrease in *all* skills is correlated

[11]For model evaluation, we use the out-of-sample R^2 with 10-fold cross-validation. This randomly splits the baseline data into 10 groups, trains the given model on 9 groups of the 10 groups, evaluates that model fit on the 1 hold-out group, and averages over the 10 possible iterations.

with 30% lower predicted earnings. We view this as additional evidence that our earnings-based skill indices are well-powered to detect changes in skills.

2.2.5 Summary Statistics at Baseline

Table 2.1 presents demographics and baseline survey responses by eventual unemployment duration. Column (1) describes our analysis sample that limits the employment to those whose prior employment spell was a non-marginal job.[12] Workers earned on average 58€ each day before job loss, which is approximately the average wage in Social Security data among all employed workers over this time period (Card et al., 2013). They are relatively high-attachment workers, as over four-fifths were previously full-time and the average duration of the previous employment spell is almost 10 years.

One potential concern with our main result showing no skill depreciation during unemployment is that skill depreciation for the long-term unemployed already occurred by the first survey, which was administered in the first two months of unemployment. The baseline survey levels provide suggestive evidence against that explanation. In particular, the baseline skill elicitations are, if anything, higher for those with eventually long unemployment durations. This is not dispositive, though, as those with longer eventual unemployment durations also differ along other dimensions: they are older, more educated, more likely to be female, and less likely to have involuntarily lost their job.[13]

While skills elicited in the first survey are not differentiated by reemployment status, self-reported life satisfaction clearly is. There is an approximately 0.3 standard deviation gap between those who are reemployed and all others who are not yet reemployed.

[12] Table 2.5 shows the difference between the analysis sample and all respondents. The primary difference between these two groups is that almost by definition, our analysis sample is less likely to have any marginal employment (and thus more likely to have a full-time with higher earnings). Still, the demographic characteristics and the measured skills in the two samples are very similar, which provides evidence in favor of our finding's generalizability.

[13] One concern is that the workers who never become reemployed appear observably different. Our results in the 6-month and 12-month survey waves are robust to excluding those who never become reemployed.

2.3 Evolution of Skills among Unemployed German Workers

This section discusses the evolution of skills over the unemployment spell for German workers. We show that skills do not decline during unemployment. Our upper bound on the contribution of changes in skills to falling reemployment wages observed in the data rules out contributions of more than 28%. These results are robust to various measurement concerns.

2.3.1 Empirical Measurement of Skill Evolution

We begin showing the evolution of skills over the unemployment spell. In our benchmark specification, we assess the skills of the unemployed in comparison to those who have been employed within 2 months of job loss and stayed employed afterward. We apply this comparison to control for potential elicitation bias of skills, as similar questions are used to measure skills at each subsequent wave.

By choosing workers who are employed within 2 months as a reference group, we implicitly assume that the elicitation error is the same for them and for the unemployed. If anything, this biases toward finding skill depreciation among the unemployed. Specifically, to the extent there is additionally skill *appreciation* during employment, we would overestimate the extent of skill *depreciation* among the unemployed.[14]

To assess the change in skill over the unemployment spell, we run the following regression:

$$Skill_{it} = \sum_{\tau \in 2,6,12,30} (\alpha_\tau \mathbb{I}[t=\tau] + \beta_\tau unemp_{it} \times \mathbb{I}[t=\tau]) + \epsilon_{it} \tag{2.1}$$

where $Skill_{it}$ measures the individual i skill measured in wave t, and $unemp_{it}$ measures if someone is unemployed through wave t (vs. reemployed before wave 2 and continuously

[14] The choice of the reference group does not drive the lack of skill depreciation among the unemployed. Online Appendix Figures 2-11 and 2-12 show that measured skills in the reference group are close to zero in each wave, while the measured skills of the unemployed slightly increase over time.

employed through wave t)[15]. The panel has waves $t \in \{2, 6, 12, 36\}$, and the corresponding β_τ coefficients reflect the average skills of the unemployed relative to the reference group. Notice that in this regression we do not control for individual effects, so β_τ reflects both the selection (i.e. the long-term unemployed might have different skills at the baseline) and the depreciation of skills over the unemployment spell.

We report the β_τ coefficients in panel (a) of Figure 2-2 using various skill measures as an outcome.[16] The figure shows the average change in the composite skill index (black dots) for the unemployed at 2, 6, 12, and 36 months after unemployment onset. This skill index is the log of the predicted daily wage based on all available skills. To contextualize the confidence interval magnitudes, we set the absolute value of the upper and lower bounds of the y-axis to correspond to 1 standard deviation in the composite skill index at baseline.

Average skills among the still unemployed are only slightly lower than those for the reemployed 2 months after unemployment. The marginally significant point estimate indicates that earnings predicted by all skills are 4 percentage points lower for the unemployed. Separating by cognitive and noncognitive skills, we find smaller differences that are not statistically significant. The fact that the unemployed and those who found a job within two months are very similar in the baseline survey justifies our choice of using the latter as a reference group.

Panel (a) of Figure 2-2 also highlights that the average unemployed's skills do not fall over the unemployment spell. If anything, there is a slight increase in the measured skill of the unemployed, though any differences are generally statistically insignificant. The lack of change in average skills among the unemployed could reflect the combination of two things: 1) the long-term unemployed are not negatively selected; 2) the skills of the unemployed are not depreciated over the unemployment spell.

We isolate the contribution of skill depreciation by looking at the within-person evolution of skills. In particular, we estimate the following regression:

[15] As of each follow-up survey, approximately two-thirds of those who were reemployed before the wave 2 were not continuously employed through wave t. Including these individuals in the reference group does not affect our results.

[16] We report α_τ in the Online Appendix Figures 2-11 (blue diamonds), which correspond to the evolution of the skills for the reference group of those who found a job within 2 months and remained continuously employed. For the reference group, the change in cognitive skills is generally positive and the change in noncognitive skills is generally zero.

$$Skill_{it} = \gamma_i + \sum_{\tau \in 6,12,30} (\alpha_\tau \mathbb{I}[t = \tau] + \beta_\tau unemp_{it} \times \mathbb{I}[t = \tau]) + \epsilon_{it} \tag{2.2}$$

Notice that this is the same regression as before (equation 2.1) but with individual fixed effects γ_i and excluding wave 1 (month 2) from the summation index τ. The coefficient of interest is again β_τ, which now shows the average within-person change relative to the baseline skill measured two months after onset.

We plot the estimated β_τ in Panel (b) of Figure 2-2. We do not find any indication of average earnings-relevant skills falling within-person over the unemployment spell. The confidence intervals include 0, and the point estimates, if anything, are positive. For every skill group at every point in time, we can rule out relative decreases larger than only a few points in log predicted earnings. This is in stark contrast to the observed changes in wages upon reemployment relative to prior wages shown in Figure 2-1 and Figure 2-7, where the change in earnings for longer unemployment spells is approximately 20 log points.

Figure 2-13 shows that the lack of relative changes in skills mostly holds for each of the individual skill items. None of the point estimates is significant. For the cognitive skills that have the greatest association with prior earnings, the relative changes are, if anything, positive.

To the extent that any skills depreciate during unemployment, they are noncognitive skills. Conscientiousness, risk tolerance, trust, patience, and reciprocity all decline by 0.2-0.6 standard deviations. While these point estimates are all negative, they do not drive the earnings-based skill indices because these self-assessed personality traits have much weaker associations with prior earnings. Moreover, noncognitive skills that have stronger associations with prior earnings—like locus of control and stability—do not decrease with longer unemployment durations.

It is also worth highlighting that Panel (a)—showing the average change of the unemployed's skill—and Panel (b)—showing the within-person skill change—of Figure 2-2 are very similar. Therefore, the change of skills over the unemployment spell primarily reflects the within-person change in skills, while the dynamic selection of individuals based on their baseline skills plays little role. The lack of dynamic selection might not be surprising given the limited dynamic selection observed in past wages (see the row labeled “prior wage" in Table 2.1). Still, we are not aware of any paper documenting this fact by directly assessing the overall evolution of skills over unemployment durations.

2.3.2 Quantifying the Contribution of Skill Changes to the Fall in Reemployment Wages

We have so far demonstrated that reemployment wages fall considerably during unemployment (see Figure 2-1) while earnings-relevant skills do not (see Panel (b) of Figure 2-2). One complication in interpreting the relative magnitudes of these findings is that they apply to slightly different samples: reemployment wages are by definition observed only for those who find a job within our survey panel.

To quantify the contribution of skill changes to the fall in reemployment wages, we study subsamples of survey respondents whose reemployment we observe. We compare the change in skills to the change in log wages—the log reemployment wage minus the log wage in the job prior to unemployment onset—for those respondents who are continually unemployed through a given survey wave but become reemployed soon afterward. In particular, we take respondents whose unemployment spell lasts more than 6 (12) months but fewer than 12 (30) months. We compare these respondents’ change in log wages to the within-person skill change measured at months 6 (12).

We estimate the within-person change in skills by applying the same regression specification as before, shown in equation 2.2, except that we restrict the sample to respondents who find a job between 6 (12) and 12 (30) months and to those who are in the reference group. For the change in log earnings, we calculate the log wage change relative to the wage change in the reference group. We apply the comparison to the reference group to control for macroeconomic trends and to make the empirical designs estimating wage changes and skill changes comparable.

Formally, we estimate the change in log wages by applying the following regression specification on the same sample of workers (reemployed between a certain period and the reference group):

$$\log(wage_{it}) - \log(wage_{i0}) = \theta_t + \beta reemp_{it} + \epsilon_{it} \tag{2.3}$$

where $\log(wage_{it}) - \log(wage_{i0})$ is the difference in log wages at time t and the log wages at the previous job and $reemp_{it}$ equals to one if the individuals are reemployed between 6 (12) and 12 (30) months. We include each reemployed individual only once in the sample (in the month when the individual is reemployed), while the reference group is included in all months. Therefore, θ_t shows the change in log wage between time t and in the previous job for the reference group (reemployed within 2 months and stayed employed afterward), while β shows the wage changes for those reemployed in month t relative to the reference group wage change.

Table 2.2 shows the main decomposition results. Panel A corresponds to the unemployed who were reemployed between 6 and 12 months after job loss. The first row shows substantial wage losses (16.8%, s.e. 4.5%) in line with existing estimates in the literature (see e.g. Schmieder et al. (2016)). The remaining rows show within-person skill changes in terms of log predicted wages. These skill indices remained the same between months 2 (wave 1) and months 6 (wave 2). The wage change that can be attributed to the within-person skill change (0.2%, s.e. 2.5%) is small, statistically insignificant, and has a sign opposite to the one implied by skill depreciation. The confidence intervals in Column (2) suggest that we can rule out a larger than 5% fall in wages due to skill depreciation at the conventional significance levels. In Columns (3) and (4) we also calculate the potential contribution of skill changes to the fall in reemployment wages by taking the ratio of the change in skill to the change in wages. The point estimate suggests that the contribution is negative and close to zero (0.8%). The 95$^{\text{th}}$ percentile confidence intervals reported in Column (4) suggest that at most 28% of the fall in wage can be attributed to the change in skills.

We see a similar pattern if we look separately at different components of the skill measure. The change in the cognitive index can explain wage changes between -4% and 3%. Therefore, at the conventional significance level, it can explain at most 21% of the wage fall upon reemployment. The change in noncognitive skills alone explains even less. We can rule out a contribution that is larger than 6% (5%) for the primary (secondary) noncognitive skill index.

Panel B of Table 2.2 focuses on those reemployed between months 12 and 30 months.[17] The long-term unemployed experience an even larger wage loss (28.1%, s.e. 3%) than those who found a job between 6 and 12 months. At the same time, the within-person change in skills between months 2 (wave 1) and 12 (wave 3) is again small, statistically insignificant, and “wrong-signed". The point estimates indicate that skills increase for the unemployed (relative to the reference group). Even with modest sample sizes, the 95% confidence intervals rule out skill changes contributing to more than 6% of the fall in reemployment wages.

2.3.3 Interpretation and Alternative Explanations

Our finding that many skills do not depreciate during unemployment leverages a unique panel survey including a wide range of skills for a large sample of long-term unemployed individuals. In this section, we discuss some of our data's limitations and how to interpret our results in light of these limitations.

Included skills. The survey elicits cognitive and noncognitive skills that are significantly predictive of earnings. We interpret these as general skills, and they do not measure every dimension of earnings-relevant skills. Nevertheless, they are used extensively in the academic literature, and they are also very similar to those used by policymakers for understanding skills specifically for the unemployed. Unemployment insurance agencies administer these types of survey-based general skills assessments to identify appropriate job-search plans and training programs. For example, Florida used to mandate all unemployment insurance claimants complete a general skills review at the onset of unemployment[18]. In a recent review of American Job Centers across the United States, Fortson et al. (2017) find that all surveyed centers offer these types of assessments—usually, the Test of Adult

[17] The relatively small share of respondents is due to earlier reemployment, survey attrition, and limited question elicitation. See Figure 2-5 for further details on the first two reasons and Figure 2-4 for further details on the third reason.

[18] This applied from 2011 through 2013. Chapter 443 of the Florida Statutes outlines this requirement.

Basic Education (TABE)—for these purposes, and almost one-third deem them as core to their training targeting.

Elicitation bias. One set of concerns relates to the fact that the skills are elicited through a survey. First, like most measures of noncognitive skills, ours are self-assessed. Reassuringly, respondents have no incentive to dissemble, and the self-assessments used in the survey are standard procedures used in the psychology literature.

Second, even for objective skills like answering math questions correctly, the skill elicitation is a function of both ability and effort. The effect of continued unemployment on survey effort is theoretically ambiguous: the opportunity cost of effort may be lower while unemployed due to greater free time, but the psychological costs may be higher while unemployed from discouragement. We view measuring the net effect of skills and effort as a feature rather than a bug, as "marshalling effort" is plausibly related to job search efficiency and employee productivity.

Third, the questions are repeated across waves. The effect of this on our results is hard to characterize: survey learning would generate positive drift in measured skill changes while survey fatigue would generate negative drift. We account for these effects, whatever they may be, by using a group of workers who are reemployed by the baseline survey and who remain continually employed thereafter. This comparison group differences out common influences of learning or fatigue specific to the survey. To the extent that skills are accumulated during employment, this differencing would bias us towards finding skill depreciation. Therefore, if anything, that would lead to attributing a bigger role of skill changes explaining the fall in reemployment wages.

In addition to that, we do not find evidence of differential elicitation bias driving our main findings. If elicitation bias increases over time, then both the standard deviation of skills and their predictive power for explaining reemployment wages should fall over time. Table 2.6 shows that the standard deviation of the different skills is relatively stable across the waves, while Table 2.7 documents that the predictive power of skills for explaining reemployment wage differences is similar to the baseline explanatory power of skills. These findings underscore that elicitation bias does not play a major role in our context.

Survey timing. Another set of concerns relates to the survey's timing. First, the initial survey is administered up to two months *after* the onset of unemployment. It is possible that skill depreciation takes place immediately upon unemployment onset. However, those who are quickly reemployed do not experience any decline in reemployment wages, which suggests that their skills do not deteriorate. Furthermore, there are no meaningful differences in baseline skills between those quickly reemployed and those remaining unemployed, suggesting that the skills of the long-term unemployed also did not depreciate considerably in the first two months.[19]

Selective Attrition. While we do not find evidence of skill depreciation in the sample we observe, one concern is that we fail to observe the evolution of skills for those who cease responding to the survey. To test this, we compare prior trends for those who keep responding to the survey in the future with those who stop responding to the survey.

We find essentially no evidence of negative differential attrition. Table 2.8 displays coefficients from separate regressions of the change since baseline as of survey wave t on an indicator for failing to respond to the following survey wave t', an indicator for remaining continually unemployed through survey, and interaction term between the two, and a constant. The sample includes only those who are continually unemployed through t' or reemployed by month 2 and continually employed through t', so the constant represents the average change by t for the continually reemployed who also respond at t'. The primary coefficients of interest correspond to the interaction terms, indicating whether the unemployed who attrit are on different trends. Without correcting for multiple hypothesis testing, nearly all of the interaction term coefficients are statistically insignificant and positive. This implies that the unemployed who attrit are, if anything, on *upward* skill trajectories. The only statistically significant trend is for the secondary noncognitive index for those who attrit by month 12, though we can rule out negative trends greater than 1.2 log points of prior wages.

We also find little evidence that survey attrition is correlated with employment status. Our population of interest is those who remain continually unemployed, and our analysis

[19] We find no indication of depreciation for the unemployed before the unemployment onset among older Americans (see Section 4).

sample compares their skill trajectories to trajectories for those who are reemployed by the baseline survey and remain continually employed. Table 2.9 shows that the attrition probability for the continually reemployed is not statistically significantly different as of month 6 and month 30, while the continually reemployed are 4 percentage points likelier to attrit by the month 12 survey.

Measurement error in unemployment. While our main result defines unemployment as the absence of non-marginal employment in administrative records, we also explore a more granular definition of unemployment that additionally restricts to those without any marginal employment in administrative records and without any self-reported employment in survey records. Using the survey data we can also exclude unemployed workers with training in the analysis. Figure 2-22 and Figure 2-23 show that the same patterns hold in this more limited sample. While the decreased sample size widens the confidence intervals, it is still the case that the composite skill index change point estimate is positive and statistically insignificant.

Alternative skill indices Our baseline specifications treat any ordinal skill items as cardinal, but we show that more flexible, high-dimensional specifications produce similar results. We apply the prediction model using fully saturated ordinal responses as described in Table 2.4. The LASSO prediction has much higher out-of-sample predictive power than OLS, but its shrinkage properties bias us against finding skill changes. On the other hand, the poor out-of-sample predictive power of OLS is due to being on the other side of the bias-variance tradeoff. Figure 2-10 shows that the main results hold for both alternative indices. All the point estimates of within-person changes are positive, and we reject any decrease in skill indices greater than 5 percentage points despite the increased noise.

Floor effects. The lack of decrease in skills could be explained by having many respondents near the lowest values of skill elicitations at baseline. In Table 2.5 we demonstrate that this is not the case in our survey by showing the raw values of the skill elicitations. None of the skill item averages are near their minimum levels.

Involuntary job loss. We confirm our null results hold within subgroups that are more plausibly susceptible to skill depreciation: those who were previously employed full-time but involuntarily lost their job. Figure 2-15 and 2-16 shows that the point estimates remain largely unchanged but the confidence intervals are modestly larger if we restrict the analysis to that subgroup of workers.

2.3.4 Change in Life Satisfaction

The survey item that exhibits the clearest divergence over the unemployment spell is not skill-based. We study the change in self-assessed life satisfaction in Figure 2-14. The figure shows that there is a meaningful selection in baseline levels and falls since baseline for the unemployed. Panel (a) shows life satisfaction at baseline is almost half a standard deviation lower for the unemployed relative to the reemployed and Panel (b) shows this falls by approximately 0.1 standard deviations for the continually unemployed and rises by approximately 0.1 standard deviations for the reemployed. As a result, we see a 0.2-0.3 standard deviation decline in life satisfaction for the unemployed relative to the control group.

2.4 Evolution of Skills for Older American Unemployed

To explore the generalizability of these results, we examine a panel of older American workers. A key advantage of the survey data used here is that we can measure skills *before* the unemployment onset. The primary deficiencies relative to the German data are that skill elicitations are less detailed and that the skills are measured less frequently.

2.4.1 Data and Methodology

We use survey responses from the Health and Retirement Study (HRS), which is a panel of approximately 20,000 Americans over the age of 50 each year spanning 1992 through 2018. The survey includes questions on employment status—allowing us to identify unemployment spells for each respondent that has one—and asks questions that measure a variety of cognitive skills.

Upon unemployment, we document a similar decline in re-employment hazards and re-employment earnings as in Germany (see Figure 2-17). Reemployment hazards fall from 23 percent two years after we initially observe unemployment to 7 percent two years later. Reemployment earnings two years after we initially observe unemployment are almost 40 percent lower than they were before unemployment and are nearly 60 percent lower four years after unemployment.

To study skill depreciation, we use the full panel of available surveys and elicited skills. The skills are summary measures that aggregate information across many different survey items. The primary measures that have significant coverage are the Telephone Interview for Cognitive Status (TICS), a Cognitive Score (COGTOT), Mental Status Summary Score (MSTOT), and Fine Motor Skills (FINEA).[20]

We form event studies of skills around job-loss events. For workers reporting unemployment at some point, we identify the first observed unemployment spell and treat that as the event of interest. The data included in the event-study figures and corresponding regression estimates use all the pre-unemployment data in which the respondent was employed up to ten years before their event, and all the post-unemployment data in which the respondent was unemployed up to ten years after their event. Our panels, therefore, are not balanced, but they maximize the available data for each event study. We use an event-study specification to measure changes in outcomes each year around layoffs:

$$Skill_{it} = \alpha_i + \alpha_t + \beta' X_{it} + \sum_{\substack{j=-10 \\ j\neq -1}}^{j=10} \pi_j \mathbb{1}\{t - t_i^* = j\} + \varepsilon_{it} \tag{2.4}$$

Here, $Skill_{it}$ denotes a skills measure for worker i in year t. α_i and α_t are worker and year fixed effects and X_{it} contains age-specific fixed effects that vary for an individual respondent over time. The function $\mathbb{1}\{t - t_i^* = j\}$ represents event-study dummies that equal one if an observation is exactly j years from individual i's unemployment-onset date, and zero otherwise. The π_j coefficients capture the dynamics of the skills measures before and after the unemployment onset. To make sure that the post-unemployment effects reflect the

[20]Some skills, like vocabulary, are measured more sparingly. We include estimates of the effect of unemployment on these outcomes in Table 2.10.

skills of those who are continuously unemployed, we exclude observations for a worker after he or she is employed. Identification comes from comparing workers that became unemployed in different years.

We measure $Skill_{it}$ by applying the same procedure as in Germany. First, we predict log earnings with elicited skills measures. Specifically, we regress contemporaneous log earnings on skill measures among workers with positive earnings before their first unemployment event. In the German data we used (daily) wages; in HRS, we use earnings because the coverage is much broader for earnings. The best predictor of log earnings is the cognitive score (t-statistic: 30.5) and the second one is fine motor skills (t-statistic: 13.5). We then predict log earnings using the model for each individual throughout the panel, generating a summary skill index, as we did for German workers. It bears mentioning that the skill index is scaled in log earnings which generates an easy interpretation: a coefficient captures the percent change in earnings predicted by changes in skills.

2.4.2 Results

Panel (a) of Figure 2-3 shows our main result. The skill index does not change around unemployment onset. With the additional pre-unemployment data, we see no pre-trends in skills leading up to the unemployment spell. This holds for the 10 years following the onset of the unemployment spell as well. The construction of the skill index does not drive this: individual event studies for memory, cognitive function, vocabulary, fine motor skills, and simple math show no systematic changes following the unemployment event (see Figure 2-18).

To increase the power of detecting declines in skills, we pool all years in the event study together. Specifically, we adapt Equation 2.4 by replacing event-time dummies with a simple indicator for post-job-loss. The coefficient on this indicator reflects the average change in outcomes associated with unemployment while accounting for differences in age, time, and persistent individual differences:

$$Skill_{it} = \alpha_i + \alpha_t + \beta' X_{it} + \pi \mathbb{1}\{t \geq t_i^*\} + \varepsilon_{it} \tag{2.5}$$

Like before, we account for differences across individuals (α_i), secular trends (α_t), and differences occurring systematically with aging (X_{it}). The coefficient on $\not{\Vdash}\{t \geq t_i^*\}$ captures the change in $Skill_{it}$ a worker experiences when he or she becomes unemployed, holding year and age constant. In this pooled specification, the post-unemployment dummies corresponding to cognitive measures are all statistically insignificant (see Table 2.10). The confidence intervals rule out earnings declines greater than 2.2%, 3.2%, and 0.9%, respectively for TICS, COGTOT, and MSTOT. Similarly, the estimates for memory, numeracy, vocabulary, and fine motor skills rule out declines greater than 1.3%, 0.4%, 0.7%, and 1.8%, respectively.

Skill depreciation at retirement. Because the HRS is not only limited to the newly unemployed, we can validate the plasticity of skill measures by studying changes around other significant events. In particular, we study changes in skills around retirement by estimating equation 2.4 but defining $\not{\Vdash}\{t - t_c^* = j\}$ based on the first year of retirement (and not unemployment onset). Panel (b) of Figure 2-3 plots estimates. The skill index is flat leading up to retirement but falls immediately after retirement. The post-unemployment point estimates correspond to a significant 10 log point decrease in skill-predicted earnings and are all statistically significant.

Alternative event-study specification. Recent literature highlights potential biases with the staggered event study implemented in equation 2.4 in the presence of treatment effect heterogeneity. We thus also implement an event-study estimator robust to these concerns (Borusyak et al., 2021). Figure 2-20 produces similar patterns and magnitudes. In the case of retirement, the alternative estimation method has slight positive pre-trends prior to unemployment, suggesting that the actual drop in skills could be even larger following retirement.

Change in life satisfaction at unemployment. Finally, we study the change in life satisfaction around unemployment events. Similar to our results from Germany, we find that life satisfaction significantly drops around unemployment. Figure 2-19 shows that self-assessed measures of negative mood—depression, feeling alone, and feeling unmotivated— are flat leading up to unemployment but spike following unemployment. When pooling the post-unemployment periods, we find that unemployment coincides with a 0.21σ increase in depression, 0.16σ increase in loneliness, and a 0.16σ increase in feeling a lack of motivation. Each of these estimates is significant at the 0.001 level (see Table 2.10, Panel B).

2.5 Conclusion

We provide direct evidence of a lack of skill depreciation among the unemployed in two different contexts: newly unemployed workers in Germany and the general population of older age workers in the United States. These skills are survey-based and include both objective cognitive performance and self-assessed noncognitive traits. In both contexts, panel data accounts for potentially time-invariant skill differences between workers with different lengths of unemployment.

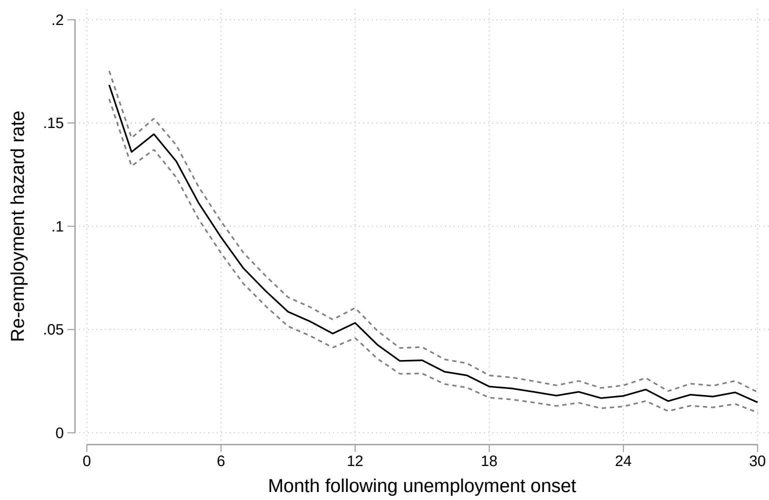
Despite (i) substantial correlations between skills and earnings while employed and (ii) substantial earnings declines following unemployment, we find (iii) little evidence of skills declines during unemployment in our data. In the German data, we rule out changes in the general skills explaining a share larger than 28% after 6 months of unemployment and 6% after 12 months of unemployment. The lack of fall in skills during unemployment is not due to the immutability of the skills we observe: in the United States data, skills meaningfully decline following retirement.

While our measures of earnings-relevant skills do not decline during unemployment, both surveys reveal significant nonpecuniary costs of unemployment. In both contexts, various measures of life satisfaction decrease upon unemployment.

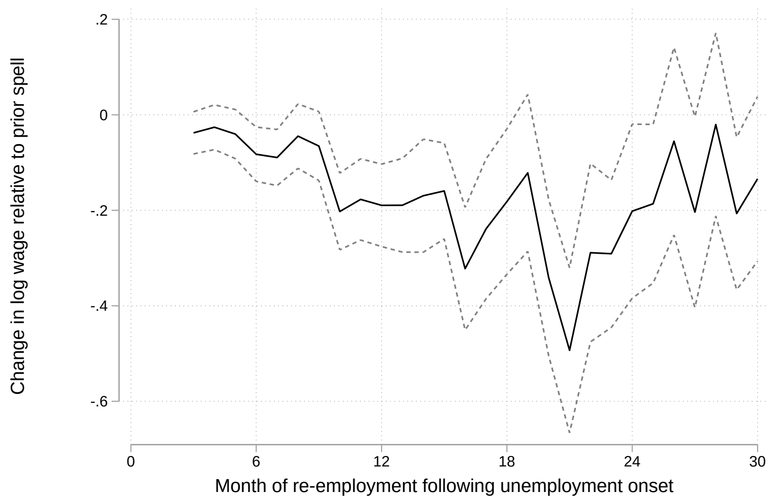
Taken together, while we confirm results from prior work showing persistently large pecuniary and nonpecuniary costs of unemployment, our evidence is inconsistent with a decline in general skills driving this. This does not necessarily mean general skills training is ineffective for the unemployed, but it casts doubt on the motivation that these people are likeliest to benefit due to recent skill declines.

2.6 Main Figures

Figure 2-1: Reemployment Hazards and Reemployment Wages over Unemployment



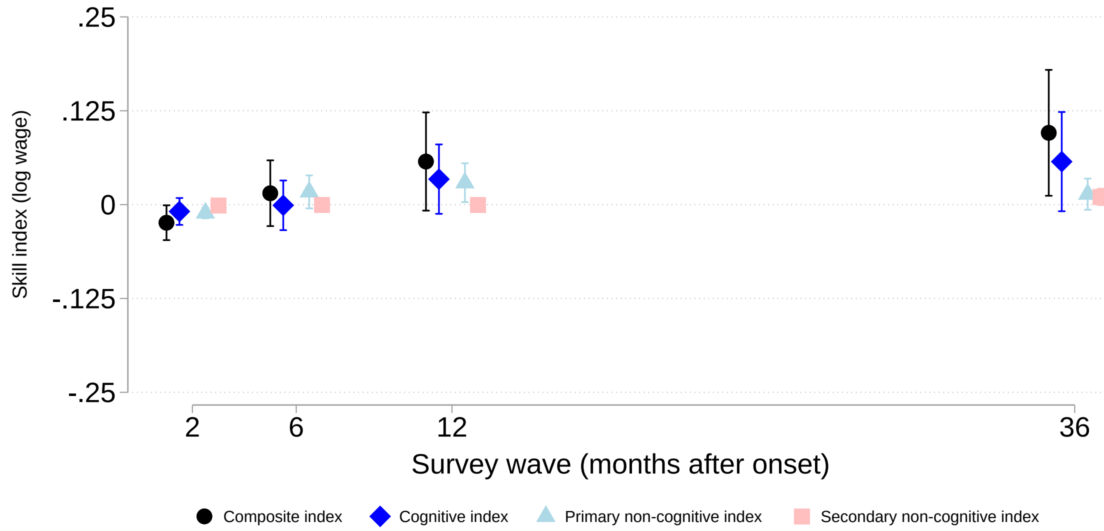
(a) Reemployment Hazard Rates



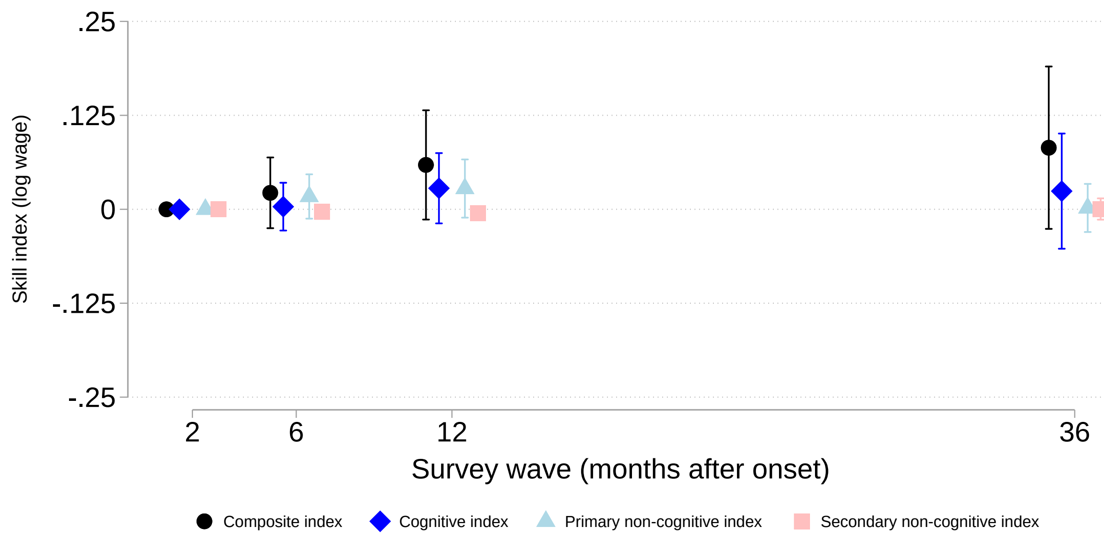
(b) Reemployment Wages

Notes: Panel (a) plots the reemployment hazard rates – the probability of finding a job conditional on being unemployed in the previous month. Panel (b) plots the reemployment wages – the difference between log wages upon reemployment (conditional on finding a job in that month) and log wages in the previous employment spell. To control for macroeconomic trends we adjust the series with the wage growth of workers always employed since the first survey. Unadjusted wage growth is shown in Panel (a) of Appendix Figure 2-6. Wage is calculated as the employee's gross daily wage. In addition, Panel (b) of Figure 2-6 shows reemployment wages when the employee's gross hourly wage is used. The dashed lines show the 95% confidence intervals around the estimates.

Figure 2-2: Evolution of Skills Over the Unemployment Spell



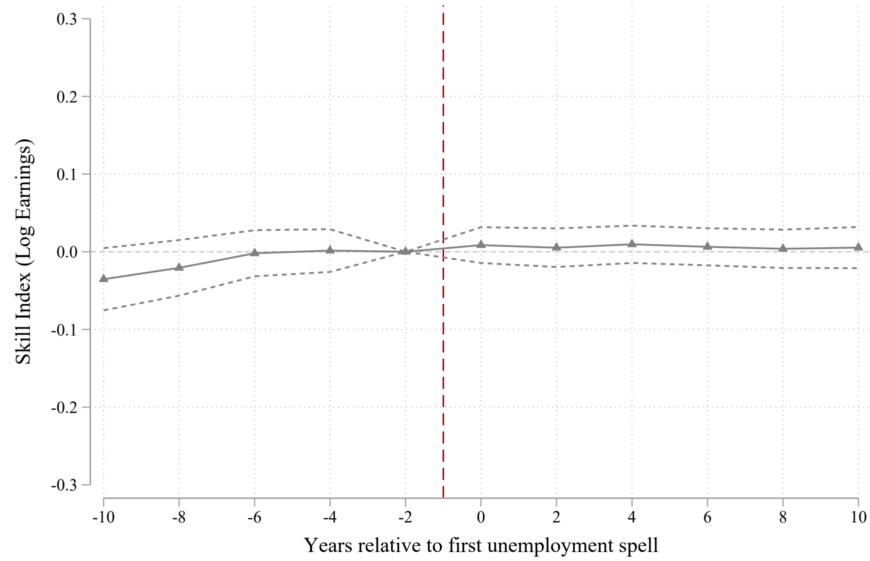
(a) Average Skill of the Unemployed over the Unemployment Spell



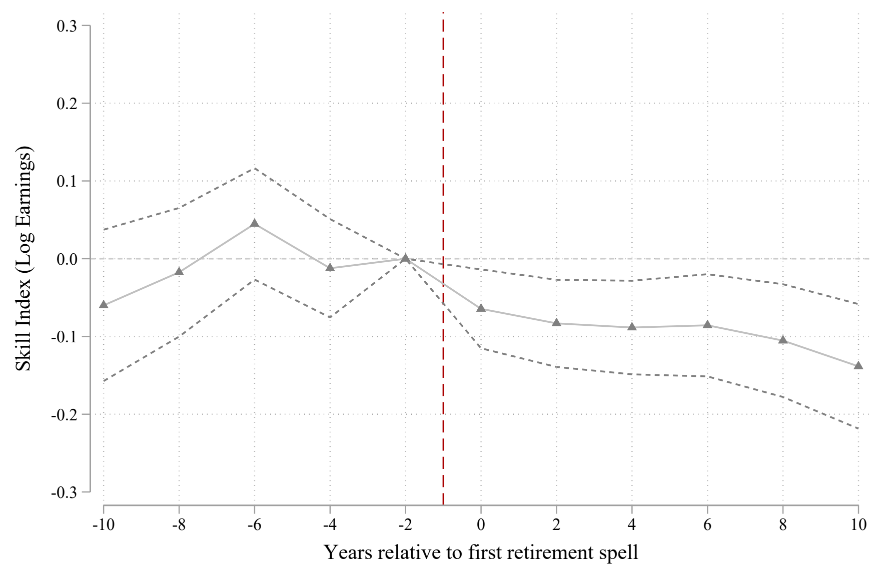
(b) Within-person Skill Changes over the Unemployment Spell

Notes: Both panels plot the change in skill indices of the unemployed relative to the reference group. Panel (a) reports the β_τ coefficients (along with the 95$^{\text{th}}$ percentile confidence intervals) from equation 2.1, where the skills of the unemployed at each wave are compared to those who found a job within 2 months. Panel (b) reports estimates with within-person fixed effects (see equation 2.2). The skill index is formed by predicting the prior employment spell's wages using OLS and treating survey responses as cardinal. The primary noncognitive index includes only the Big-5 and locus of control questions, the secondary noncognitive index includes the personality traits, and the composite index includes all cognitive and noncognitive questions. The y-axis scale represents approximately $\pm 1\sigma$ of the log predicted wages using the composite skills index as measured at baseline, which is 0.22.

Figure 2-3: Within-Person Skills Changes around Unemployment and Retirement Among Older American Workers



(a) Within-person Skill Changes around Unemployment



(b) Within-person Skill Changes around Retirement

Notes: This figure shows the within-person change in skills around unemployment (Panel (a)) and retirement (Panel (b)) events estimated using equation 2.4. Event time zero shows the first transition from employment to unemployment (retirement) for each worker in the survey (HRS). In Panel (a), we exclude observations after unemployment in which the worker regains employment to make sure that the post-unemployment effects reflect the skills of those who are continuously unemployed. In the regression, we control for worker age (fully saturated), person effects, and time effects. The skill index is formed by predicting the employed worker's earnings using OLS. The y-axis scale represents approximately $\pm 1\sigma$ of the skills index (log predicted earnings), which is 0.31.

2.7 Main Tables

Table 2.1: Demographic Characteristics and Skills by Eventual Unemployment Duration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Sample	**By realized duration (months)**					**Correlation**
		0-2	2-6	6-12	12-30	30+	ρ
Demographics							
Female	0.44	0.42	0.40	0.47	0.48	0.48	0.06
Age at UE	34.57	32.82	34.62	35.47	34.64	36.37	0.09
University degree	0.25	0.24	0.22	0.24	0.26	0.33	0.09
Immigrant	0.20	0.19	0.20	0.20	0.24	0.20	0.01
Previous Emp. Spell							
Prior wage (€)	57.57	56.39	57.38	56.69	54.97	61.45	0.05
Full-time	0.84	0.86	0.86	0.84	0.80	0.80	-0.07
Duration (years)	9.31	9.00	9.59	9.51	9.01	9.36	-0.00
Involuntary Unemp.	0.45	0.43	0.49	0.47	0.41	0.41	-0.04
Baseline Survey							
Life satisfaction	-0.01	0.16	-0.08	-0.18	-0.11	0.01	-0.02
Composite skill index (€)	60.49	60.87	59.73	60.11	58.74	62.11	0.03
Math	0.00	0.00	-0.03	0.01	0.02	0.03	0.01
Locus of control	-0.00	0.01	-0.05	-0.01	0.00	0.06	0.03
Extravert	-0.00	-0.02	-0.02	0.04	0.02	0.00	0.01
Stable	0.02	0.03	0.00	0.06	0.03	0.01	-0.00
Open	0.00	0.02	-0.05	0.00	0.02	0.07	0.03
Conscientious	0.02	-0.04	0.09	0.10	-0.04	-0.02	-0.02
Observations	11684	3264	3437	1568	1141	2274	11684

Notes: All columns except for the final one report the average value of the row variable within the column group. The final column reports the correlation coefficient between the variable and the realized months of unemployment. Wage is calculated as the employee's gross daily wage. The composite skill index predicts prior log wages using all cognitive and noncognitive items at baseline and so it is scaled in log wages. All baseline survey questions are expressed as z-scores.

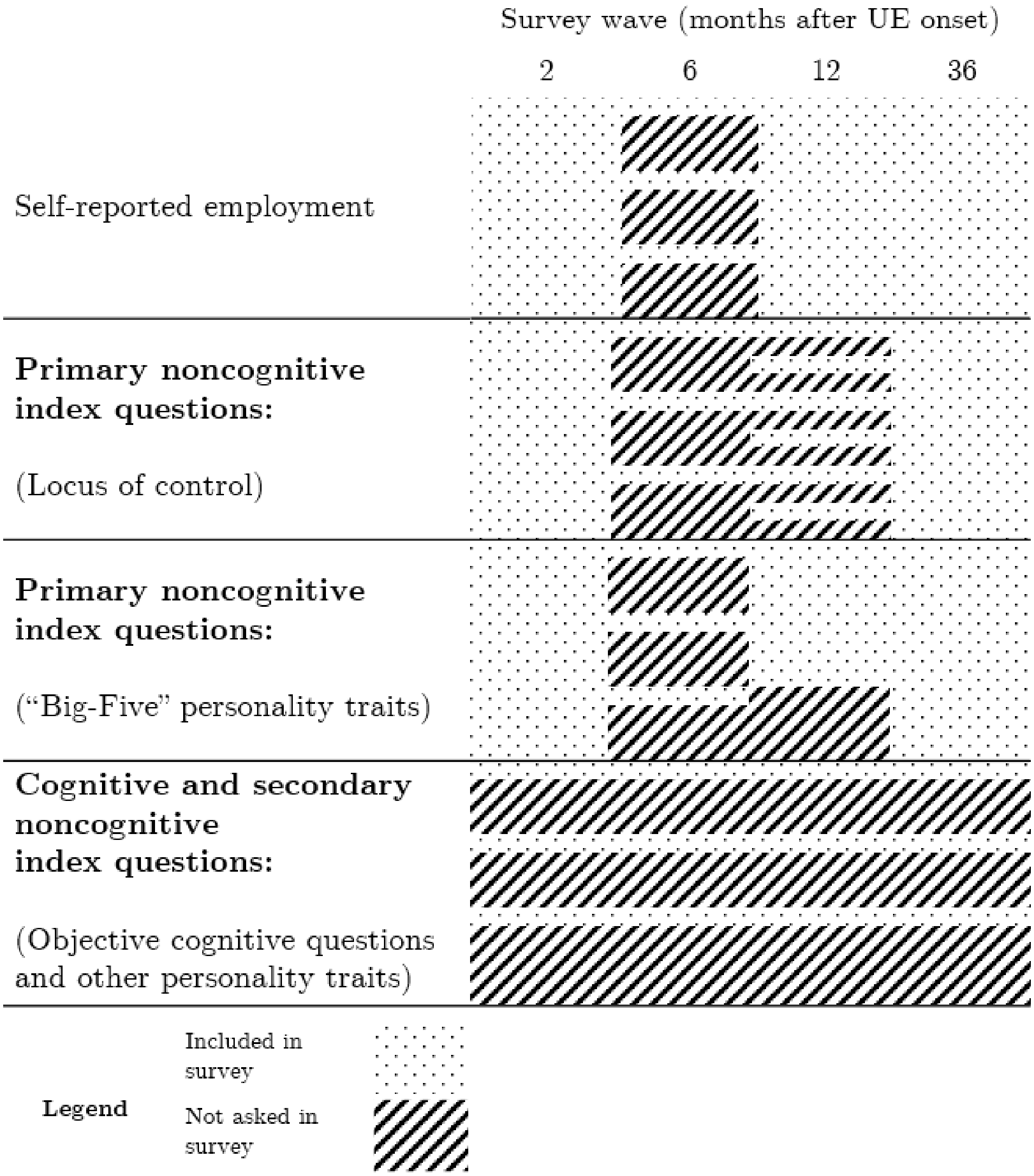
Table 2.2: Contribution of Skill Changes to the Fall in Reemployment Wages

	(1)	(2)	(3)	(4)
	Change since baseline		**Contribution of skill (%)**	
	coeff	range	coeff	range
Panel A: Reemployed between 6–12 months				
Log wage	-.168	[-.26,-.08]		
N=252	(.045)			
Composite index (Wave 2)	.002	[-.05,.05]	-0.8	[-29,28]
N=210	(.025)		(14.6)	
Cognitive index (Wave 2)	-.004	[-.04,.03]	2.6	[-16,21]
N=217	(.016)		(9.6)	
Primary non-cognitive index (Wave 2)	.014	[-.01,.04]	-8.2	[-23,6]
N=242	(.012)		(7.3)	
Secondary non-cognitive index (Wave 2)	-.002	[-.01, .04]	1.2	[-2,5]
N=222	(.003)		(1.8)	
Panel B: Reemployed between 12–30 months				
Log wage	-.281	[-.34,-.22]		
N=635	(.03)			
Composite index (Wave 3)	.058	[-.01,.13]	-20.6	[-47,6]
N=57	(.036)		(13.0)	
Cognitive index (Wave 3)	.024	[-.02,.07]	-8.6	[-25,8]
N=100	(.024)		(8.6)	
Primary non-cognitive index (Wave 3)	.027	[-.002,.06]	-9.7	[-21,1]
N=179	(.015)		(5.5)	
Secondary non-cognitive index (Wave 3)	-.005	[-.01,.003]	1.7	[-1,5]
N=100	(.004)		(1.5)	

Notes: The table assesses the contribution of changing skills to the fall in reemployment wages. We report the change in wages and the within-person skill change for the unemployed finding a job between months 6 and 12 in Panel A, and between 12 and 30 months in Panel B. In both panels, we report changes in log wages in the top row of Column (1). We report β coefficient estimated based on equation 2.3. The remaining rows in Column (1) report the change in within-person skill estimated based on equation 2.2, but restricting the sample to those reemployed between 6 (12) and 12 (30) months in Panel A (B). Skill indices are formed by predicting the prior employment spell's daily wages using OLS and treating survey responses as cardinal. The primary noncognitive index includes only the Big-5 and locus of control questions asked to all respondents. The secondary noncognitive index includes only the personality traits asked to a subset of cohorts. The cognitive index measures math, memory and verbal fluency. The composite index includes all cognitive and noncognitive questions. All skill indices are scaled in log wages. We report point estimates and standard errors in parentheses below. In Column (2) we report the corresponding confidence intervals at the 95$^{\text{th}}$ percentile. Column (3) calculates the ratio between the estimated change in skill index and the observed change in log wages (shown in row (1)) and converts it to percent by multiplying by 100. Standard errors are calculated using the delta method. Column (4) reports the corresponding confidence intervals at the 95$^{\text{th}}$ percentile. We report the number of observations (N) below each outcome variable.

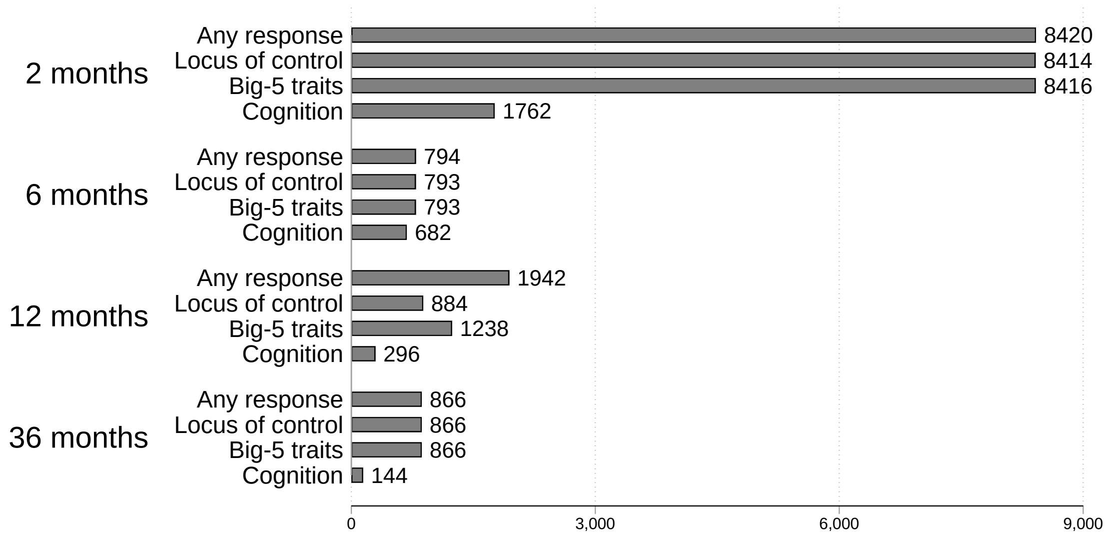
2.8 Appendix Figures

Figure 2-4: Included Questions by Survey Wave and Cohort

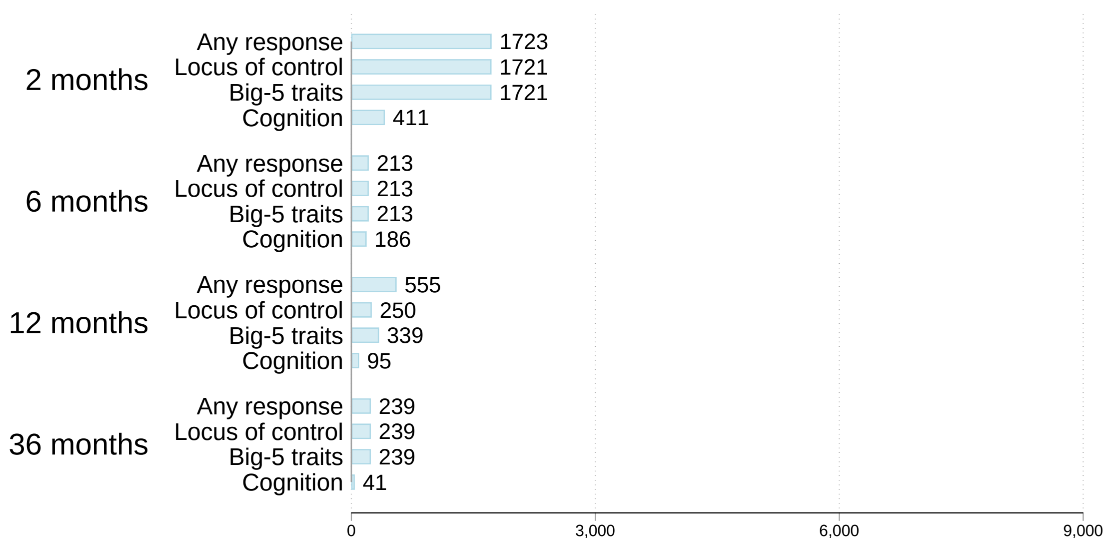


Notes: This figure indicates available data for the twelve cohorts over time for different question topic groups. Within each question topic group, the first row corresponds to the June 2007 cohort and the last row corresponds to the May 2008 cohort. Dots indicate that relevant questions in the topic group were solicited from that cohort at the given point in time, while diagonal lines indicate that they were not. For example, the June 2007, October 2007, and February 2008 cohorts were always asked cognitive and secondary noncognitive questions; no other cohorts were ever asked these questions.

Figure 2-5: Sample Sizes by Survey Wave and Included Questions



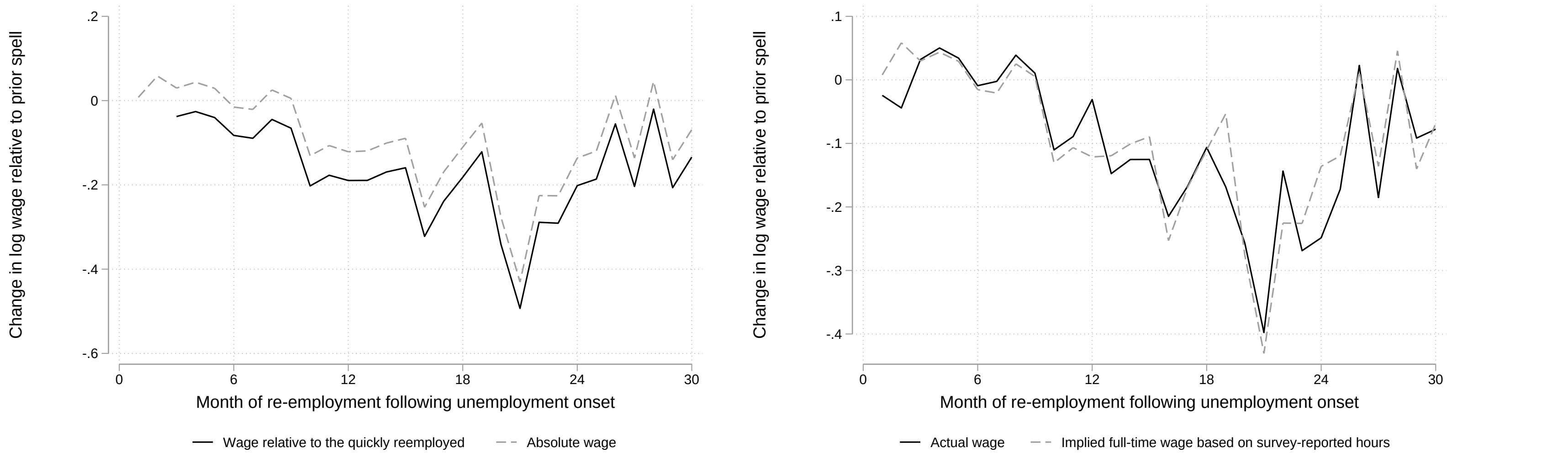
(a) Continually Unemployed Since Unemployment Entry



(b) Continually Employed Since Survey Two Months After Entry

Notes: This figure shows the number of observations for each question type for wave 1 (2 months), wave 2 (6 months), wave 3 (12 months) and wave 4 (36 months). Bars represent survey respondents in the analysis sample for each wave. Panel (a) restricts to respondents without any form of employment since unemployment entry, and Panel (b) restricts to respondents who were reemployed by the wave 1 survey and continually employed since then. Employment is defined as non-marginal employment in the administrative data.

Figure 2-6: Evolution of Reemployment Wages over the Unemployment Spell: Robustness

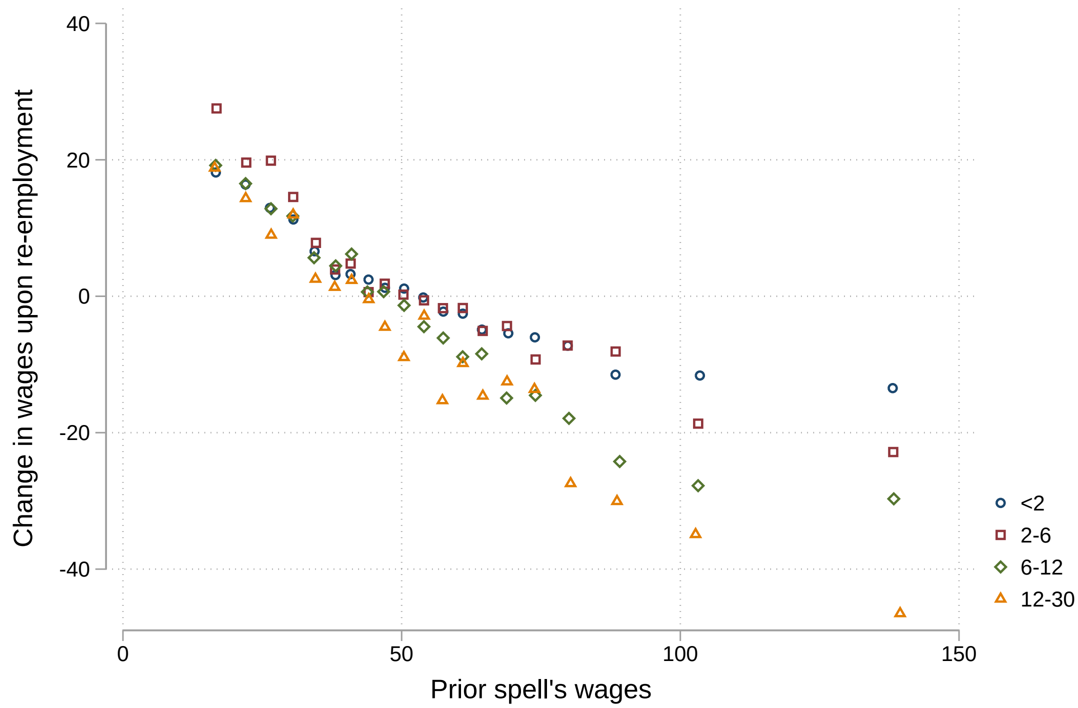


(a) Raw vs. Adjusted for Macroeconomic Trends

(b) Daily vs. Hourly Wages

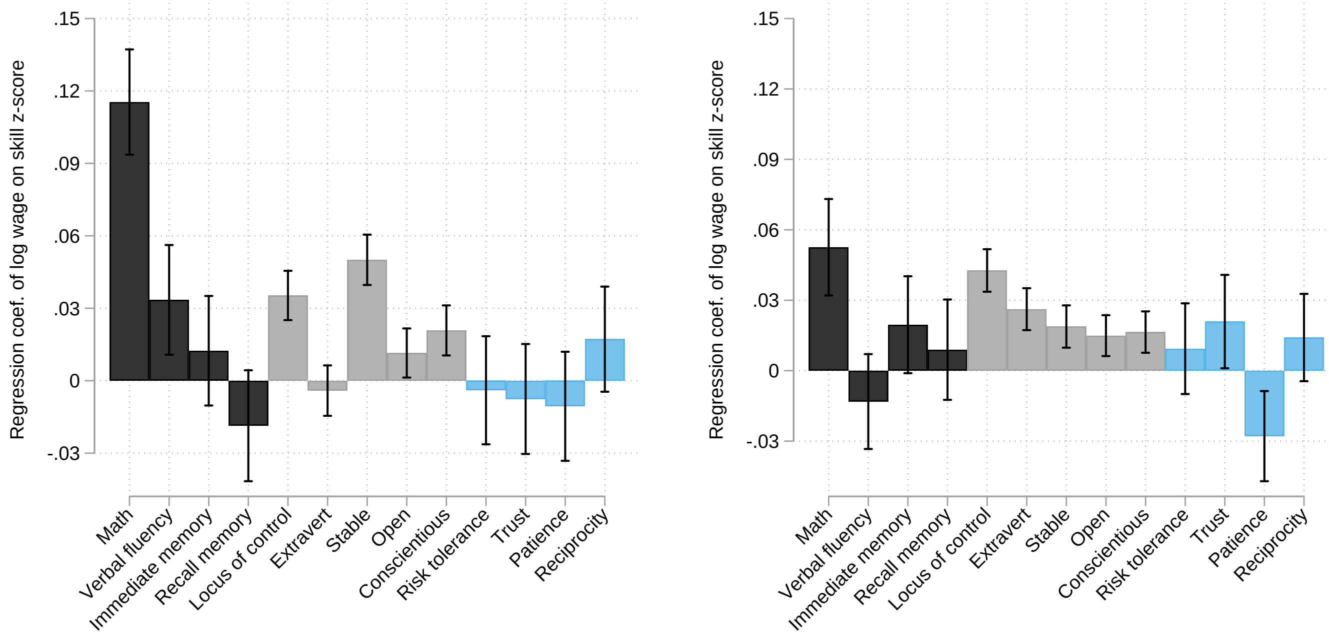
Notes: Both panels plot administrative wages upon reemployment by the month of reemployment as in Panel (b) of Figure 2-1. Reemployment wages are calculated as the within-worker difference between the wages upon reemployment relative to wages prior to unemployment. In Panel (a), the gray line shows the “raw" reemployment wages. The solid black line reproduces our main specification where we control for macroeconomic trends as well by comparing the wage changes relative to the wages of those who were reemployed quickly (within 2 months) and stayed employed after that. In Panel (b) reemployment wages are calculated as the employee’s gross *daily* wage (our benchmark definition, solid line) or as the employee’s gross *hourly* wage (dashed line). The latter is calculated using the self-reported weekly hours measured in our survey.

Figure 2-7: Relationship between Reemployment Wage Change and Prior Wages by Unemployment Duration



Notes: We report the non-parametric binscattered relationship between reemployment wages and prior wages by unemployment duration. Reemployment wages are calculated as the within-worker difference between the wages upon reemployment and the wages prior to unemployment. Wages are calculated as the employee's gross *daily* wage measured in €.

Figure 2-8: Relationship Between Previous Wages and Baseline Skills

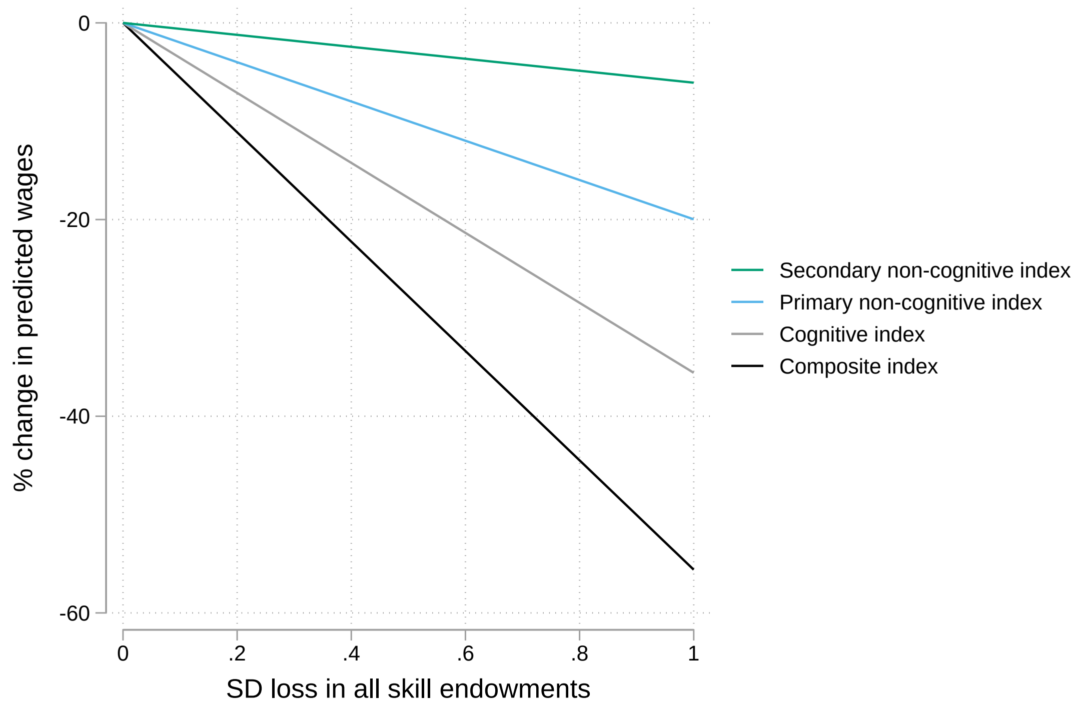


(a) Raw Relationship

(b) Relationship Conditional on Demographics

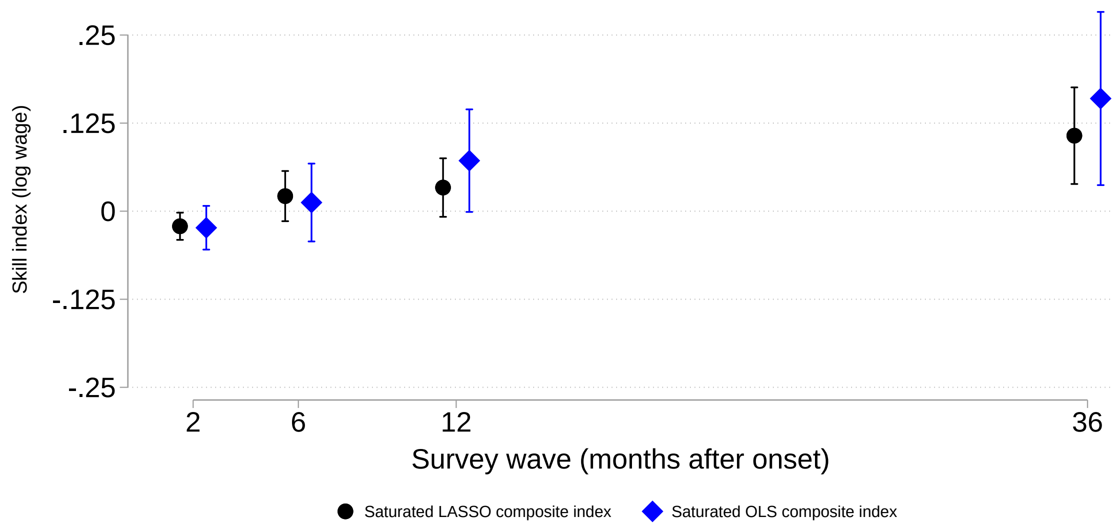
Notes: The figures show the relationship between wages in prior jobs and skills measured at the baseline wave (month 2). We report the coefficients and 95% confidence intervals from separate regressions of the previous employment spell's wages on each baseline survey measurement. Surveyed skills are measured as z-scores, so each coefficient can be interpreted as the predicted change in log wages for a one standard deviation change in the surveyed skill. Panel (a) shows the raw relationship, while Panel (b) shows the relationship conditional on worker demographics. Demographic controls include an age quadratic term, gender, migrant status, and categories for education and professional certifications. The bar colors correspond to different skill groups: cognitive (black), primary noncognitive (gray), and secondary noncognitive (blue).

Figure 2-9: Relationship Between Baseline Skills and Prior Wages

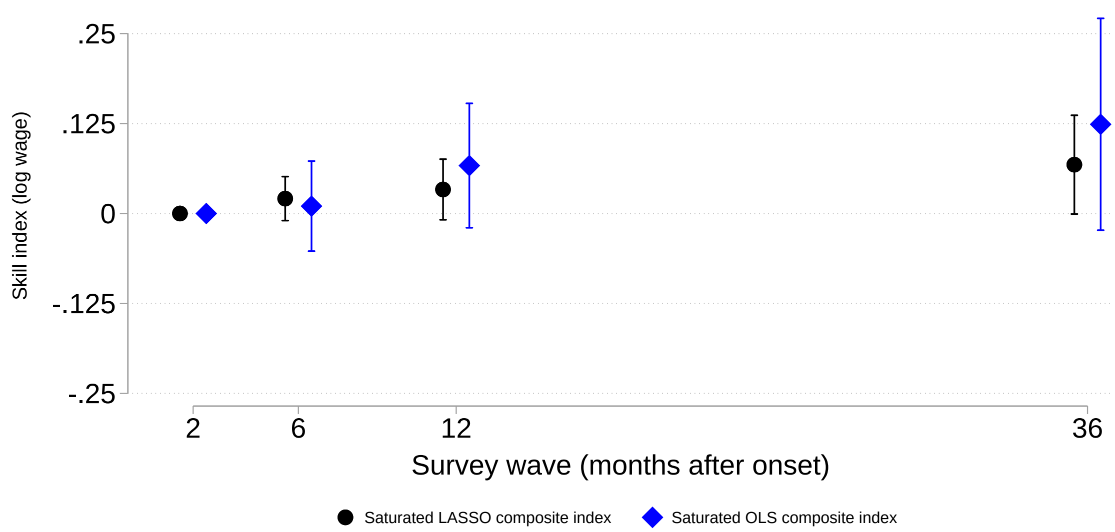


Notes: This figure plots the implied decrease in wages in response to the depreciation of the underlying skills. We apply our preferred prediction model to create skill indices. We apply OLS regression of prior wages on each individual baseline skill question, where Likert scale questions are treated as cardinal. Moving along the x-axis from 0 to 1 corresponds to a 1 standard deviation depreciation of skills in every underlying question in that skill category. Depreciation is defined as a change that is associated with lower prior wages in the prediction model. In particular, we assume that items with positive (negative) coefficients in the prediction model are decreased (increased) by one standard deviation.

Figure 2-10: Evolution of Skills Over the Unemployment Spell: Fully Saturated Indices



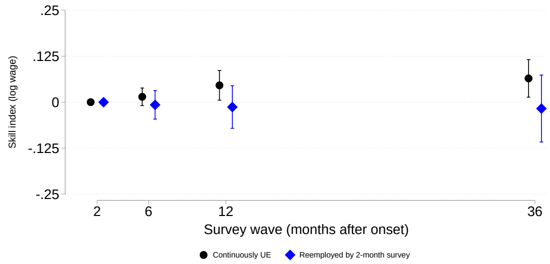
(a) Average Skill of the Unemployed over the Unemployment Spell: Fully Saturated Indices



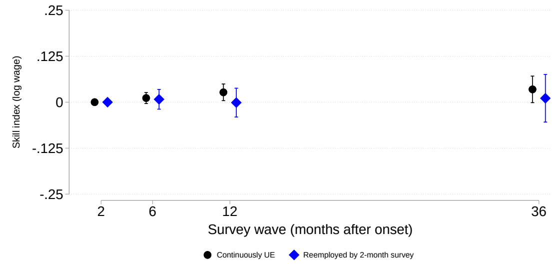
(b) Within-person Skill Changes over the Unemployment Spell

Notes: This figure explores whether the pattern shown in Figure 2-2 is robust to applying alternative methods for constructing the composite skill index. Both panels plot the change in skill indices of the unemployed relative to the reference group. Panel (a) reports the β_τ coefficients (along with the 95$^{\text{th}}$ percentile confidence intervals) from equation 2.1, where the skills of the unemployed at each wave are compared to those who found a job within 2 months. Panel (b) reports estimates including within-person fixed effects (see equation 2.2). The skill index is formed by predicting the prior employment spell's wages using either OLS (blue diamond) or adaptive LASSO (black dots). Both skill indices use all available skill items. The predictors are all binary. We convert any ordinal skill item, such as a Likert scale response, into a fully saturated set of indicators. The y-axis scale represents approximately $\pm 1\sigma$ of the log predicted earnings using the composite skills index as measured at baseline, which is 0.22.

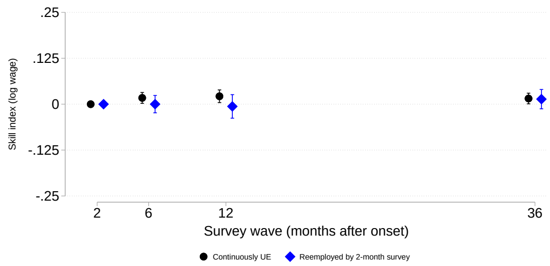
Figure 2-11: Evolution of Skill Index Over Time for the Reference Group and for the Unemployed



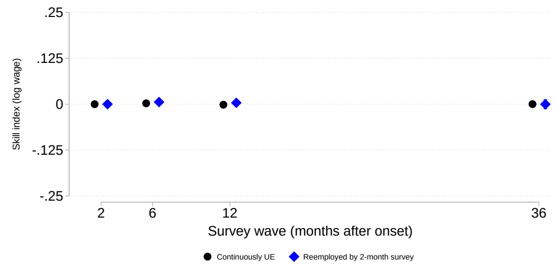
(a) Composite Skill Index



(b) Cognitive Skill Index



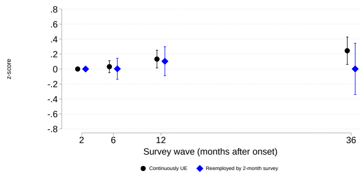
(c) 1st Noncognitive Skill Index



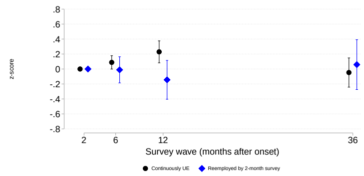
(d) 2nd Noncognitive Skill Index

Notes: This figure shows the within-person skill index change separately for the reference group (reemployed within two months and continuously employed afterward) and for the unemployed (we report the result for each individual skill items separately in Figure 2-12). In all panels, we report estimates based on equation 2.2. The blue diamonds represent changes since the baseline for the reference group (α_τ in equation 2.2), while the black dots represent changes for the continuously unemployed ($\alpha_\tau + \beta_\tau$ in equation 2.2). The skill index is formed by predicting the prior employment spell's wages using OLS and treating survey responses as cardinal. The primary noncognitive (Panel (c)) index includes only the Big-5 and locus of control questions, the secondary noncognitive index includes the personality traits (Panel (d)), the cognitive skill index (Panel (b)) includes fluency, maths, and short-term recall, and the composite skill index (Panel (a)) includes all cognitive and noncognitive questions.

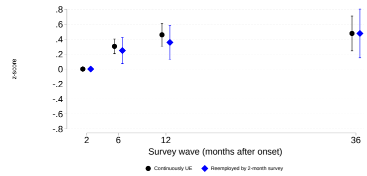
Figure 2-12: Evolution of Individual Skill Items Over Time for the Reference Group and for the Unemployed



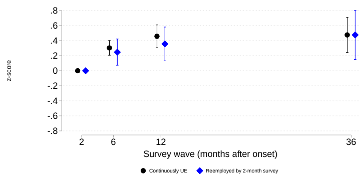
(a) Math



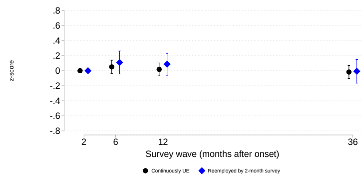
(b) Verbal fluency



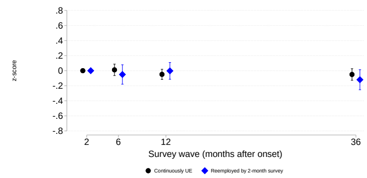
(c) Immediate memory



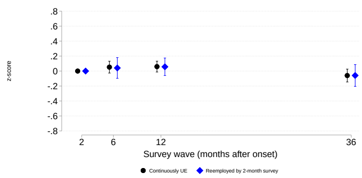
(d) Recall memory



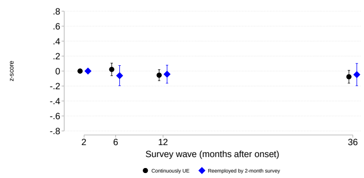
(e) Locus of control



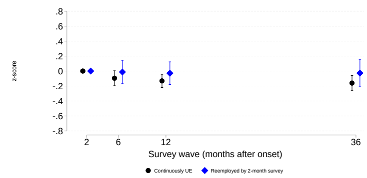
(f) Extravert



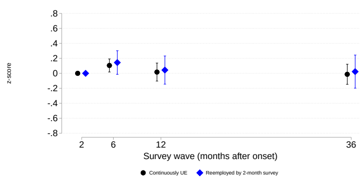
(g) Stable



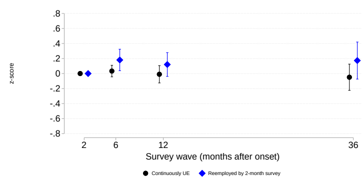
(h) Open



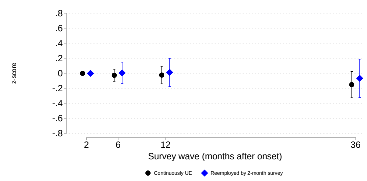
(i) Conscientious



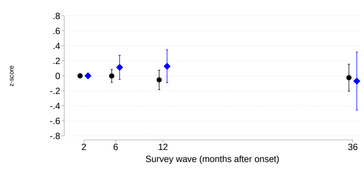
(j) Risk tolerance



(k) Trust



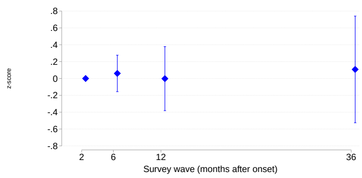
(l) Patience



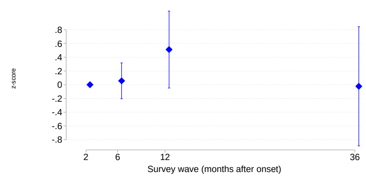
(m) Reciprocity

Notes: This figure shows the within person-change for the reference group (reemployed within two months and continuously employed afterward) and for the unemployed for each individual skill items separately (we report the result for skill indices in Figure 2-11). Responses are treated as cardinal and signed appropriately. When a category has multiple underlying questions, each question is first converted to a z-score and then those z-scores are averaged together. The z-score standardized is based only on the initial survey and then applied to all surveys.

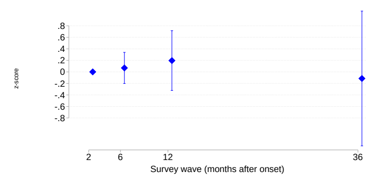
Figure 2-13: Evolution of Individual Skill Items Over the Unemployment Spell



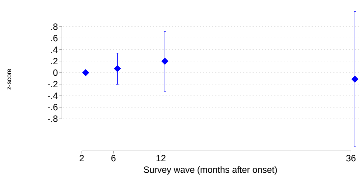
(a) Math



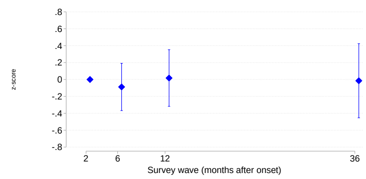
(b) Verbal fluency



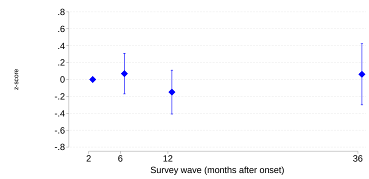
(c) Immediate memory



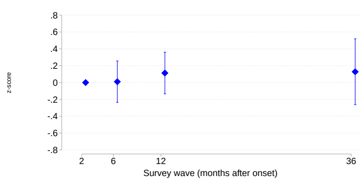
(d) Recall memory



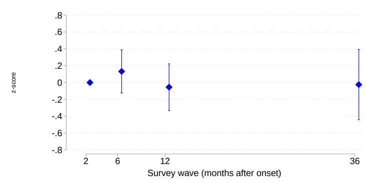
(e) Locus of control



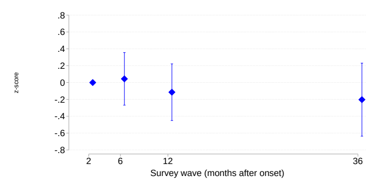
(f) Extravert



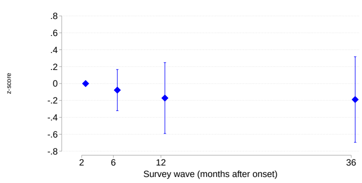
(g) Stable



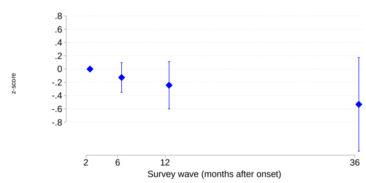
(h) Open



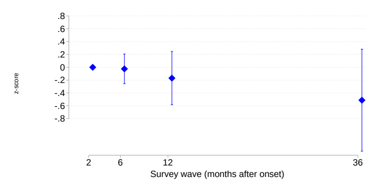
(i) Conscientious



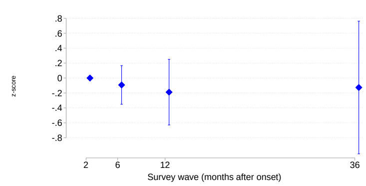
(j) Risk tolerance



(k) Trust



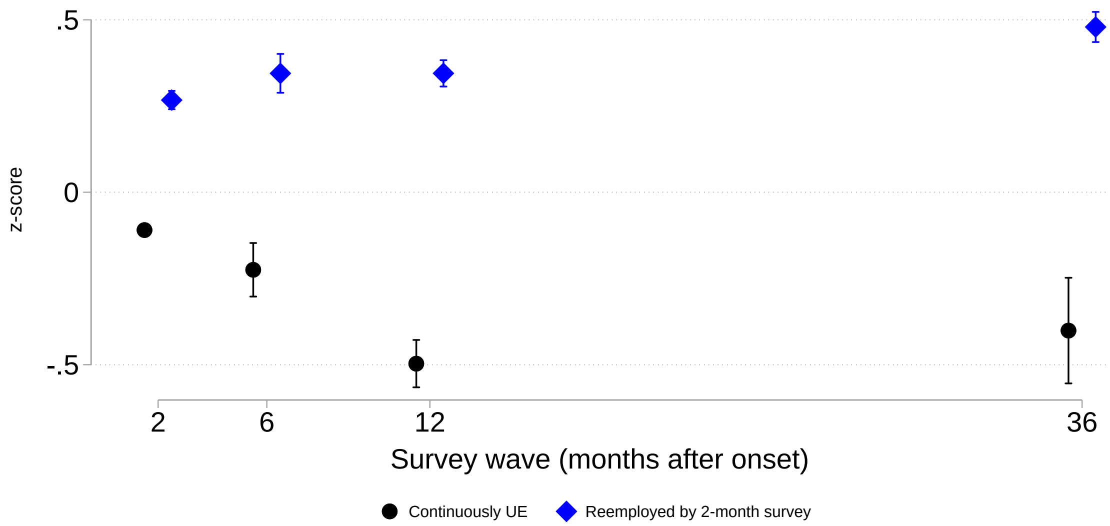
(l) Patience



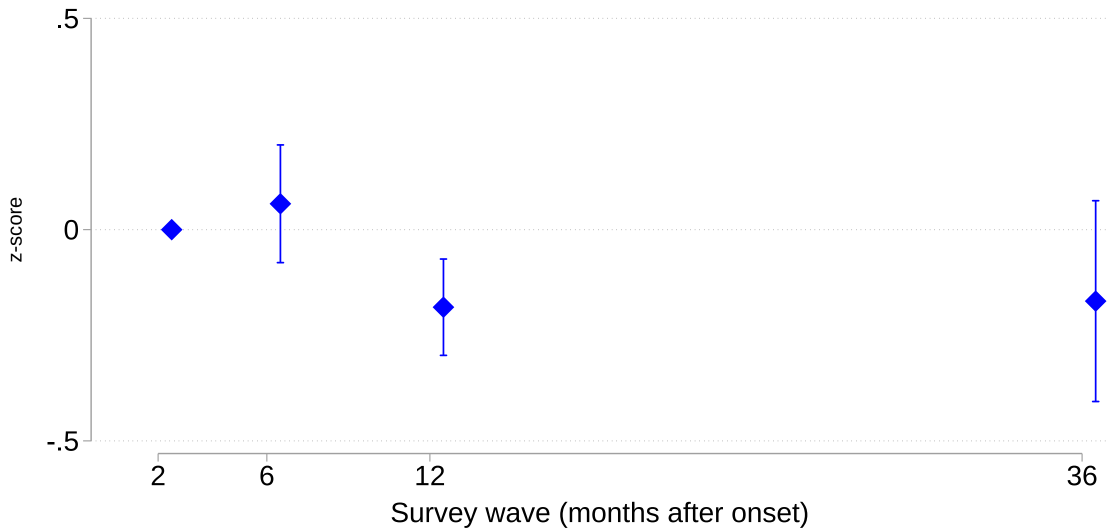
(m) Reciprocity

Notes: This figure recreates Panel (b) of Figure 2-2 for each individual skill items separately. We report the β_τ coefficients (along with the 95$^{\text{th}}$ percentile confidence intervals) from equation 2.2. Responses are treated as cardinal and signed appropriately. When a category has multiple underlying questions, each question is first converted to a z-score and then those z-scores are averaged together. The z-score standardized is based only on the initial survey and then applied to all surveys.

Figure 2-14: Evolution of Life Satisfaction over Time



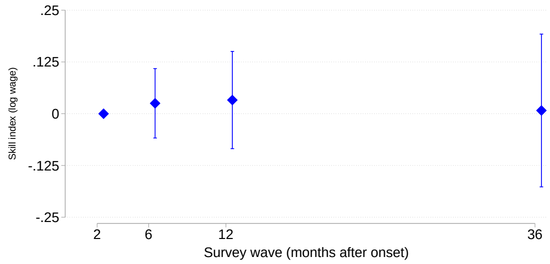
(a) Life Satisfaction over Time for the Unemployed and for the Reference Group



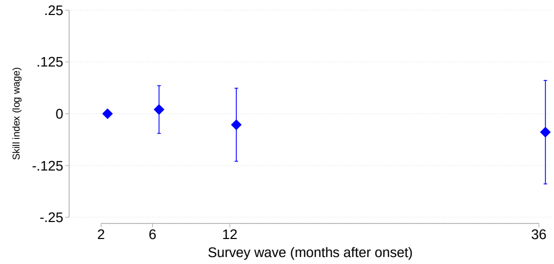
(b) Within-person Change in Life Satisfaction for the Unemployed (relative to the Reference Group)

Notes: This figure shows the evolution of self-reported life satisfaction over time. Panel (a) shows life satisfaction separately for the reference group (reemployed within two months and continuously employed afterward) and for the unemployed. We report the average level of life satisfaction at each survey wave by estimating equation 2.1. The blue diamonds represent changes since the baseline for the reference group (α_τ in equation 2.1), while the black dots represent changes for the continuously unemployed ($\alpha_\tau + \beta_\tau$ in equation 2.1). In Panel (b) we show the within-person change in life satisfaction of the unemployed relative to the employed by estimating equation 2.2. The blue diamonds show the estimated β_τ in equation 2.2. We standardize the self-reported life satisfaction based on responses to the initial survey. Due to panel response availability, unemployment duration is defined using survey responses for life satisfaction.

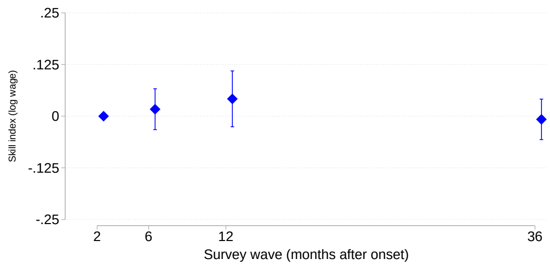
Figure 2-15: Evolution of Individual Skill Items Over the Unemployment Spell: Restricting to Involuntary Losers of Full-time Employment



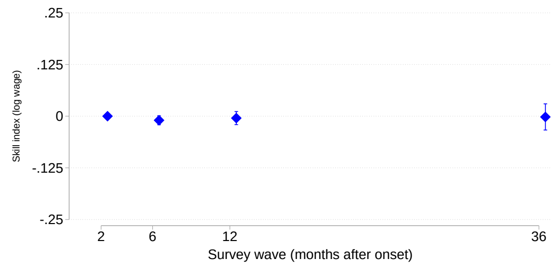
(a) Composite Skill Index



(b) Cognitive Skill Index



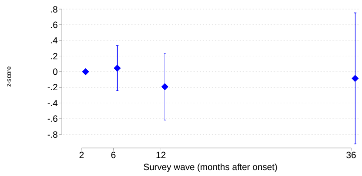
(c) 1st Noncognitive Skill Index



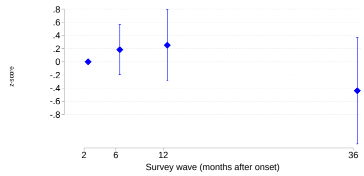
(d) 2nd Noncognitive Skill Index

Notes: This figure recreates Panel (b) of Figure 2-2 but restricts respondents who involuntarily lost a full-time job. In all panels, we report estimates with within-person fixed effects (the β_τ coefficient from equation 2.2). The skill index is formed by predicting the prior employment spell's wages using OLS and treating survey responses as cardinal. The primary noncognitive (Panel (c)) index includes only the Big-5 and locus of control questions, the secondary noncognitive index includes the personality traits (Panel (d)), the cognitive skill index (Panel (b)) includes fluency, maths, and short-term recall, and the composite skill index (Panel (a)) includes all cognitive and noncognitive questions. The y-axis scale represents approximately $\pm 1\sigma$ of the log predicted wages using the composite skills index as measured at baseline, which is 0.22.

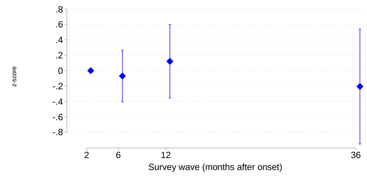
Figure 2-16: Evolution of Individual Skill Items Over the Unemployment Spell: Restricting to Involuntary Losers of Full-time Employment



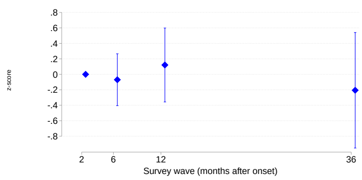
(a) Math



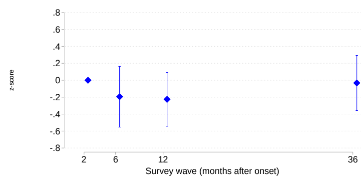
(b) Verbal fluency



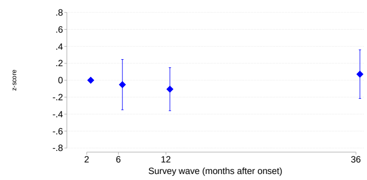
(c) Immediate memory



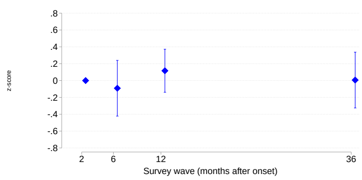
(d) Recall memory



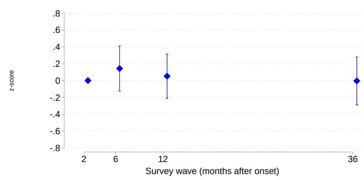
(e) Locus of control



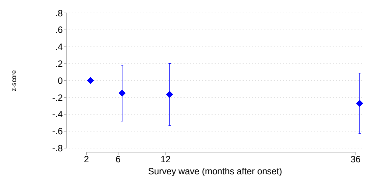
(f) Extravert



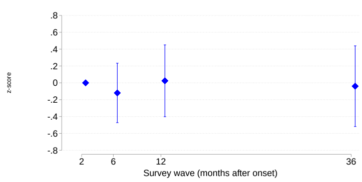
(g) Stable



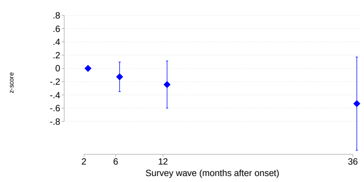
(h) Open



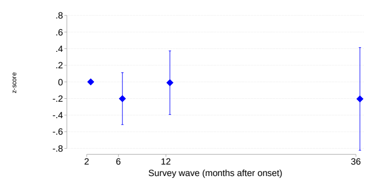
(i) Conscientious



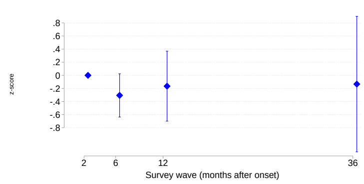
(j) Risk tolerance



(k) Trust



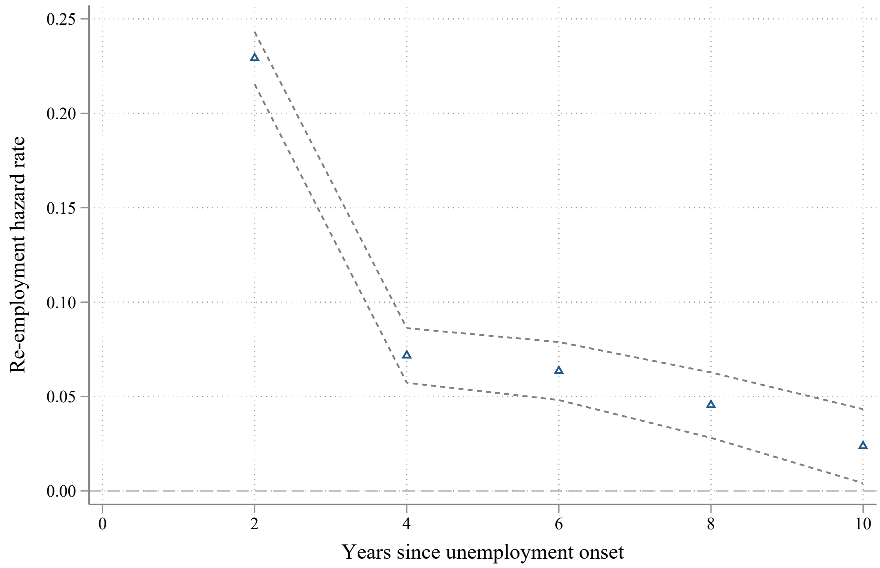
(l) Patience



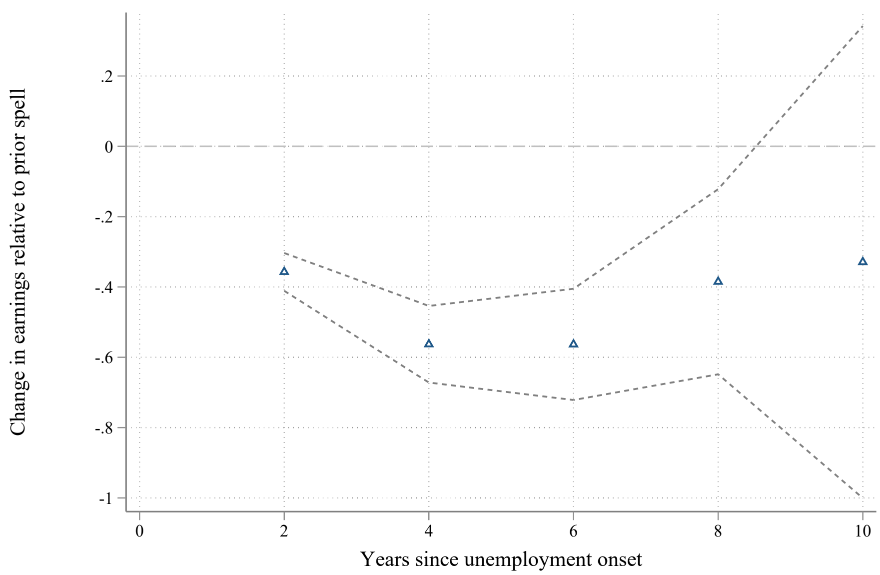
(m) Reciprocity

Notes: This figure recreates Panel (b) of Figure 2-2, restricting to respondents who involuntarily lost a full-time job. We report the result for each individual skill item separately (for skill indices see Figure 2-16). In all panels, we report estimates with within-person fixed effects (the β_τ coefficient from equation 2.2). Responses are treated as cardinal and signed appropriately. When a category has multiple underlying questions, each question is first converted to a z-score and then those z-scores are averaged together. The z-score standardized is based only on the initial survey and then applied to all surveys.

Figure 2-17: Reemployment Hazards and Reemployment Wages over the Unemployment Spell Among Older American Unemployed



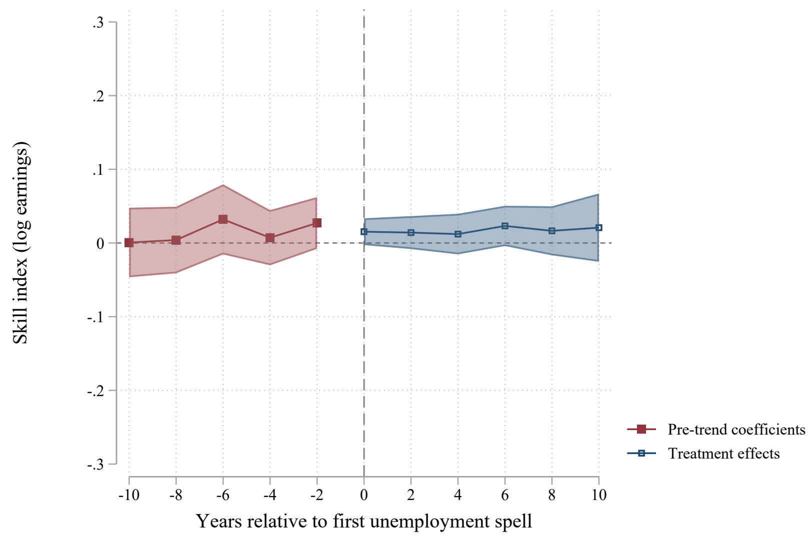
(a) Reemployment Hazard Rates



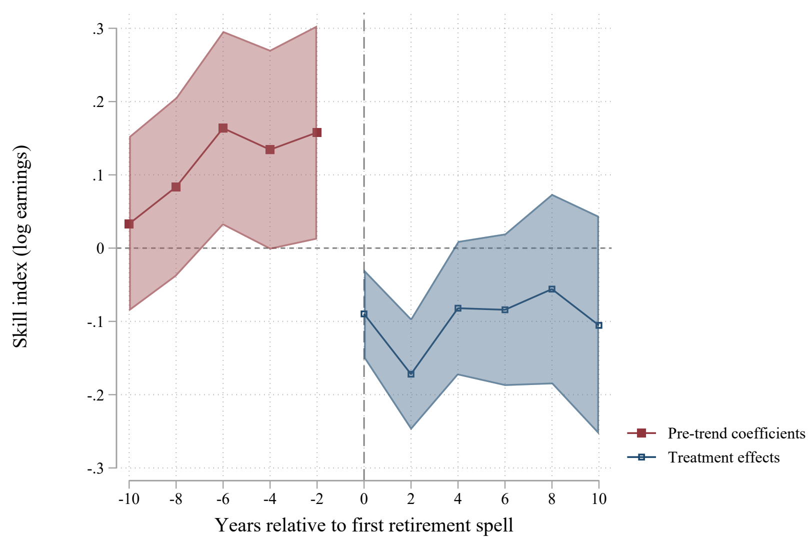
(b) Reemployment Earnings

Notes: Panel (a) plots the reemployment hazard rates – the probability of finding a job conditional on being unemployed two years before. Panel (b) plots the reemployment earnings – the share difference between earnings upon reemployment (conditional on finding a job) and earnings in the previous employment spell. In both panels, we use the HRS. The dashed lines shows the 95% confidence intervals around the estimates.

Figure 2-20: Within-Person Skills Changes around Unemployment and Retirement Among Older American Workers: Applying Borusyak et al. (2021)



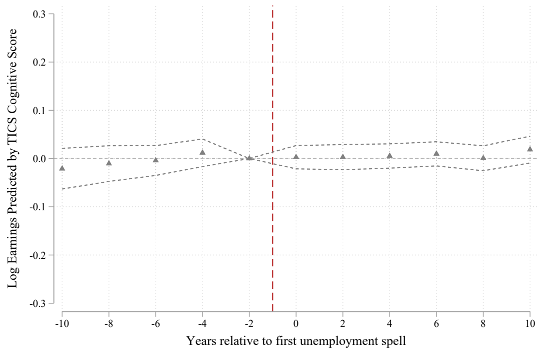
(a) Unemployment Events



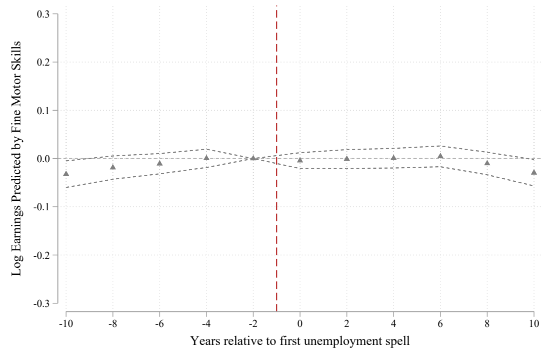
(b) Retirement Events

Notes: This figure reproduces Figure 2-3 using the event study specification from Borusyak et al. (2021). Event time zero shows the first transition from employment to unemployment (retirement) for each worker in the survey (HRS). In Panel (a), we exclude observations after unemployment in which the worker regains employment to make sure that the post-unemployment effects reflect the skills of those who are continuously unemployed. In the regression, we control for worker age (fully saturated), person effects, and time effects. The skill index is formed by predicting the employed worker's earnings using OLS.

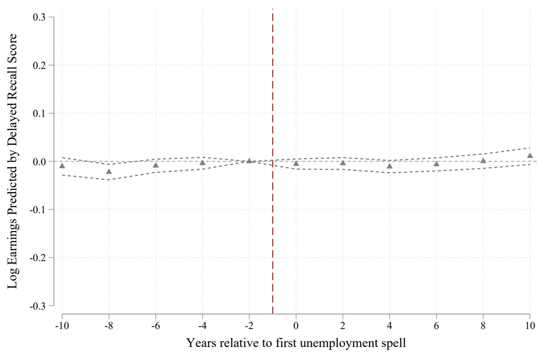
Figure 2-18: Within-Person Skill Changes around Unemployment Among Older American Workers: Individual Skill Items Measured as Predicted Log Earnings



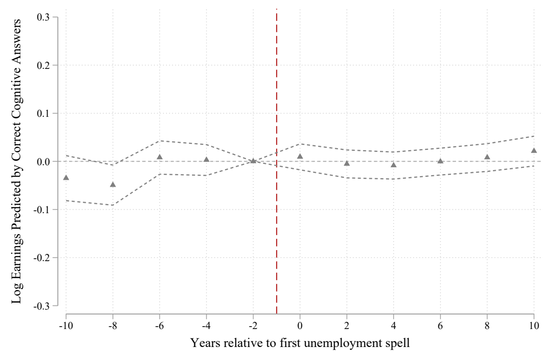
(a) Interview for Cognitive Status



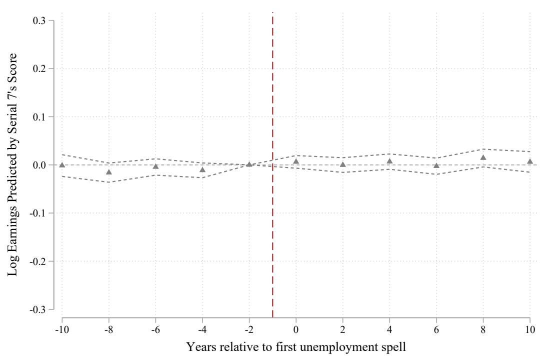
(b) Fine Motor Skills



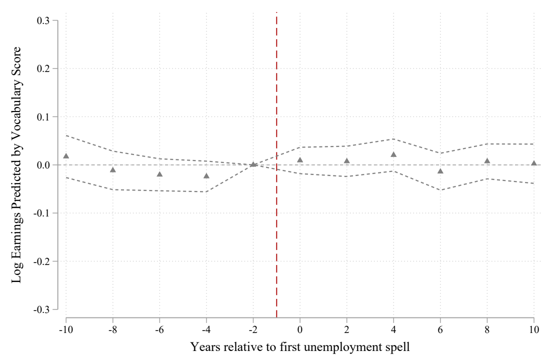
(c) Memory Recall



(d) Cognitive Total



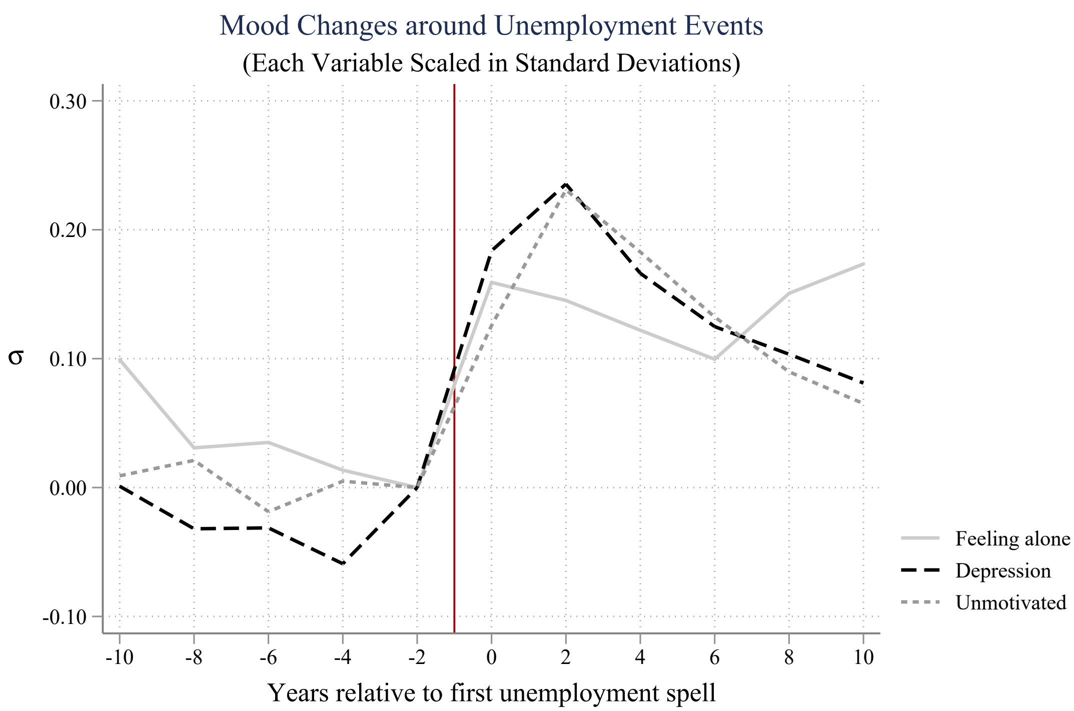
(e) Simple Math



(f) Vocabulary

Notes: This figure shows the within-person change in skills around unemployment separately for individual skill items (see Figure 2-3 for the composite skill index). Event time zero shows the first transition from employment to unemployment for each worker in the survey (HRS). We exclude observations after unemployment in which the worker regains employment to make sure that the post-unemployment effects reflect the skills of those who are continuously unemployed. In the regression, we control for worker age (fully saturated) and person effects. Skills are scaled by their predictive power of pre-unemployment log earnings, where is OLS used as a prediction model.

Figure 2-19: Within-Person Mood Changes around Unemployment Among Older American Workers



Notes: This figure shows the within-person change in mood among older American workers. Event time zero shows the first transition from employment to unemployment for each worker in the survey (HRS). We exclude observations after unemployment in which the worker regains employment to make sure that the post-unemployment effects reflect the skills of those who are continuously unemployed. In the regression, we control for worker age (fully saturated), person effects, and time effects. Each well-being variable measured in z-scores. A positive increase in mood (feeling alone, unmotivated, depression) is associated with a decline in well-being.

2.9 Appendix Tables

Table 2.3: Skill and Life Satisfaction Survey Content

Domain	Detail	Question Type
Cognitive Skills		
Math	3 free-response questions	easy, medium, hard
Short-term recall	Recall 10 words	Immediately after hearing + later during survey
Verbal fluency		List as many animals as possible in 1 minute
Primary non-cognitive		
Locus of Control	Agreement with statements about control over one's outcomes	Likert scale (1-7 agreement) with 10 questions
(4 of the) Big-5 Traits	Subjective evaluation of openness, conscientiousness, extraversion, and stability	Likert scale (1-7 agreement) with 3 questions for each trait
Secondary non-cognitive		
Other Personality Traits	Subjective evaluation of trust in others, patience, reciprocity, and risk tolerance	Likert scale (1-7 agreement) with 1 question each
Life Satisfaction		
Life Satisfaction	Subjective self-assessment	Cardinal assessment of life satisfaction on a 1-10 scale

Notes: This table shows the main contents of our survey on skills and life satisfaction. See Arni et al. (2014) for a detailed discussion of the survey content, questionnaire administration, and sample composition.

Table 2.4: Out-of-Sample R^2 for Predicting Prior Wages with Baseline Skills

	OLS		LASSO	
	Linear in cardinal responses	Fully saturated CDFs	Linear in cardinal responses	**Fully saturated CDFs**
All skills	0.047	-0.032	0.082	0.010
Cognitive skills	0.051		0.067	
Primary noncognitive skills	0.031	0.032	0.033	0.049
Secondary noncognitive skills	-0.01	-0.02	-0.00	0.01

Notes: This table compares the out-of-sample performance of various prediction models. Each cell is the average out-of-sample R^2 with 10-fold cross-validation when predicting prior earnings with baseline surveyed skills. The prediction includes only previously full-time workers who were not yet re-employed by the initial survey. Each row represents the skills used in prediction: all, only cognitive, only primary noncognitive (the Big-5 and locus of control), and secondary noncognitive index (other personality traits). Each column represents the estimator used. The LASSO penalty is selected using a 3-step adaptive Lasso. Our main results use the OLS estimator linear in cardinal responses. Figure 2-10 shows the estimates when a Lasso with fully saturated CDFs model is applied.

Table 2.5: Summary Statistics of All Respondents vs. the Analysis Sample

	(1) **All**	(2) **Analysis Sample**
Demographics		
Female	0.47	0.44
	(0.50)	(0.50)
Age at Unemployment	33.76	34.57
	(10.78)	(10.67)
University Degree	0.26	0.25
	(0.44)	(0.43)
Immigrant	0.20	0.20
	(0.40)	(0.40)
Previous Emp. Spell		
Prior wage (€)	47.00	57.57
	(34.39)	(30.73)
Full-time	0.74	0.84
	(0.44)	(0.37)
Duration (years)	9.02	9.31
	(4.60)	(4.46)
Involuntary Unemp.	0.40	0.45
	(0.49)	(0.50)
Baseline Survey		
Life satisfaction (out of 7)	6.60	6.58
	(2.10)	(2.11)
Composite skill index (€)	57.53	57.68
	(10.57)	(10.56)
Correct math (out of 3)	1.77	1.77
	(0.75)	(0.75)
Listed words (1 minute)	23.56	23.33
	(7.02)	(7.00)
Immediate memory (out of 10)	6.59	6.58
	(1.67)	(1.66)
Recall memory (out of 10)	5.19	5.13
	(1.96)	(1.96)
Locus of control (out of 7)	4.77	4.77
	(0.82)	(0.82)
Extravert (out of 7)	5.17	5.16
	(1.13)	(1.12)
Stable (out of 7)	4.22	4.25
	(1.20)	(1.20)
Open (out of 7)	5.05	5.05
	(1.21)	(1.21)
Conscientious (out of 7)	6.20	6.22
	(0.89)	(0.88)
Observations	15173	11684

Notes: This table replicates the summary statistics shown in Table 2.1 separately for all respondents and the analysis sample. The analysis sample restricts to those with non-marginal employment immediately prior to the unemployment spell. Unlike Table 2.1, we report the raw values of survey responses rather than the z-scores.

Table 2.6: Standard Deviation of Responses by Survey Waves

	(1) 2-month	(2) 6-month	(3) 12-month	(4) 36-month
Panel A: Skills indices				
Composite	13.34	14.13	14.36	14.88
Cognitive	10.16	10.85	11.40	11.53
Primary noncognitive	8.23	8.59	8.32	8.25
Secondary noncognitive	2.02	1.83	1.85	1.78
Panel B: Cognitive				
Math	0.99	1.01	1.01	1.03
Verbal fluency	1.00	1.05	1.15	1.01
Immediate memory	0.99	1.05	1.02	1.00
Recall memory	1.00	1.03	0.99	0.95
Panel C: Primary noncognitive				
Locus of control	1.00	1.00	0.97	0.95
Extravert	0.99	1.02	0.98	0.96
Open	1.00	0.97	0.98	0.96
Conscientious	0.99	0.91	0.95	0.95
Panel D: Secondary noncognitive				
Risk tolerance	1.01	0.90	1.04	1.00
Stable	1.00	0.99	0.97	0.98
Trust	1.01	0.90	0.90	0.88
Patience	1.00	0.94	0.89	0.91
Reciprocity	1.00	0.90	0.89	0.89

Notes: This table reports the standard deviation of given skills at each survey wave. Column (1) reports the standard deviation in wave 1 (month 2), Column (2) in wave 2 (month 6), column 3 in wave 3 (month 12), Column (4) in wave 4 (month 36). The rows in the table represent the relevant skills. Panel A reports skill indices, which are formed by predicting the prior employment spell's earnings using OLS and treating survey responses as cardinal. Panels B, C and D show the standard deviation of the individual cognitive, primary noncognitive, and secondary noncognitive skill items, respectively. In panels B through D, we report the standard deviation of the skill items standardized by the wave 1 (month 2) standard deviation.

Table 2.7: Predictive Power of Skills Explaining Reemployment Wages

		(1) R^2	(2) N	(3) β	(4) SE(β)
Panel A: Reemployed at 6-12 months					
Composite	Baseline skills vs. prior wages	0.201	210	1.06	0.14
	6-month skills vs. reemployment wages	0.130	210	0.78	0.16
Cognitive	Baseline skills vs. prior wages	0.121	217	1.15	0.21
	6-month skills vs. reemployment wages	0.116	217	1.00	0.22
Primary noncognitive	Baseline skills vs. prior wages	0.075	242	1.22	0.27
	6-month skills vs. reemployment wages	0.080	242	1.02	0.26
Secondary noncognitive	Baseline skills vs. prior wages	0.004	222	1.08	1.17
	6-month skills vs. reemployment wages	0.021	222	2.46	1.06
Panel B: Reemployed at 12-30 months					
Composite	Baseline skills vs. prior wages	0.084	57	0.81	0.44
	12-month skills vs. reemployment wages	0.087	57	0.68	0.38
Cognitive	Baseline skills vs. prior wages	0.051	100	0.69	0.36
	12-month skills vs. reemployment wages	0.043	100	0.51	0.30
Primary noncognitive	Baseline skills vs. prior wages	0.053	179	0.90	0.29
	12-month skills vs. reemployment wages	0.012	179	0.36	0.25
Secondary noncognitive	Baseline skills vs. prior wages	0.019	100	2.36	1.73
	12-month skills vs. reemployment wages	0.014	100	1.75	1.65

Notes: This table studies the predictive power of skill indices explaining reemployment wages in different samples. Panel A focuses on those who become reemployed between the month 6 and 12, while Panel B on those who reemployed between the month 12 and 30. Column (1) in Panel A (B) reports the R-squared from a regression of 6 (12) month skills measured in wave 2 (3) and *wages*. In rows labeled "6-month (12-month) skills vs. prior wages" we use *prior wages* in the regression. Note that skill indices are trained to explain prior wages and so these rows serve as a benchmark. Rows labeled "6-month (12-month) skills vs. reemployment wages" use *reemployment wages* in the regression. Column (2) shows the sample size in the regression. Column (3) shows the regression coefficients of the regression, while Column (4) reports the standard errors. For both panels we report results using composite skill index, cognitive skill index, primary noncognitive (the Big-5 and locus of control), and secondary noncognitive index (other personality traits).

Table 2.8: Change in Skills by Unemployment and Survey Attrition

	(1) Composite	(2) Cognitive	(3) Primary noncognitive	(4) Secondary noncogntive
Panel A: Change from 2-month survey to 6-month survey				
UE spell > 12 months	.027	.014	.017	.001
	(.027)	(.019)	(.015)	(.004)
Attrit at 12 months	-.038	-.003	-.022	.006
	(.036)	(.023)	(.018)	(.005)
Interaction term	.013	.000	.016	-.010
	(.043)	(.028)	(.022)	(.006)
Constant	.006	.000	.008	.002
	(.023)	(.016)	(.013)	(.003)
N	598	619	701	640
Panel B: Change from 2-month survey to 12-month survey				
UE spell > 30 months	.040	.027	.016	-.001
	(.051)	(.037)	(.020)	(.005)
Attrit at 30 months	-.025	-.005	.008	.005
	(.058)	(.044)	(.028)	(.006)
Interaction term	.029	.012	.024	-.001
	(.070)	(.050)	(.032)	(.008)
Constant	-.001	.006	-.006	-.000
	(.043)	(.034)	(.018)	(.004)
N	159	254	445	258

Notes: This table shows the within-person skill change since the 2-month baseline survey among those who are either continually re-employed or continually unemployed in the administrative data by the subsequent survey wave. Panel A is a regression of the skill change as of the 6-month survey on an indicator for being continually unemployed in the administrative data as of month 12, an indicator for not responding to the 12-month survey, and an interaction term. Panel B is a regression of the skill change as of month 12 on an indicator for being continually unemployed in the administrative data as of the 12-month survey, an indicator for not responding to the 30-month survey, and an interaction term. Each column within each panel represents a separate regression of a different skill index. Robust standard errors are shown in parentheses.

Table 2.9: Survey Attrition by Employment Status

	Survey Wave		
	Month 6	**Month 12**	**Month 30**
Continually employed since 2-month survey	.018	0.039	0.028
	(.029)	(.018)	(.021)
Reemployed but not continually since 2-month survey	.056	0.032	0.028
	(.019)	(.010)	(.011)
Constant	.375	.427	.619
	(.013)	(.008)	(.010)

Notes: This table shows differences in attrition probability by employment status in the administrative data. Each column is a separate regression where the outcome is an indicator for attriting from the analysis sample by that survey wave. The independent variables are (1) an indicator for remaining continually employed since the baseline survey (the group we refer as the "reference group" in our main analysis) and (2) either becoming reemployed after the baseline survey but before the current survey or becoming unemployed after reemployment. The omitted category represented by the constant is remaining continually unemployed by that survey wave. Robust standard errors are shown in parentheses. Asterisks correspond to two-sided tests for differences relative to those who remain continually unemployed.

Table 2.10: Change in Skills and Well-being Following Unemployment among Older American Workers

	(1) $\mathbb{I}\{t \geq t_i^*\}$ coeff	(2) 95% CI range	(3) Within R-squared	(4) N
Panel A: Skills (Scaled in log earnings)				
COGTOT (cognitive score 1)	-0.0087	[-.032,.014]	0.0003	2,700
	(0.0118)			
TICS (cognitive score 2)	-0.0047	[-.022,.012]	0.0001	2,727
	(0.0087)			
MSTOT (cognitive score 3)	0.011	[-.009,.031]	0.0006	2,700
	(0.0102)			
TR20 (simple math 1)	-0.0050	[-.014,.004]	0.0002	8,395
	(0.0048)			
SER7 (simple math 2)	0.006	[-.004,.016]	0.0002	8,471
	(0.0053)			
IMRC (immediate recall)	-0.0039	[-.014,.006]	0.0001	8,395
	(0.0051)			
DLRC (delay recall)	-0.0046	[-.013,.004]	0.0002	8,395
	(0.0044)			
VOCAB (vocabulary)	0.0222	[-.007,.051]	0.0044	979
	(0.0148)			
FINEA (fine motor skills)	-0.0091	[-.018,.000]	0.0005	9,150
	(0.0044)			
Composite Index	0.0102	[-.008,.029]	0.0006	2,695
	(0.0094)			
Panel B: Well-being (Z-Score)				
CESD (depression)	0.2121	[-.151,.273]	0.0076	8,833
	(0.0309)			
FLONE (loneliness)	0.1594	[.094,.225]	0.0034	8,824
	(0.0336)			
GOING (unmotivated)	0.1587	[.092,.226]	0.0031	8,808
	(0.0342)			
BMI (body-mass index)	0.0086	[-.018,.035]	0.0001	9,265
	(0.0133)			

Notes: This table shows the within-person skill change following unemployment for older American workers. Column (1) reports the coefficients (with the corresponding standard errors) of $\mathbb{1}\{t \geq t_i^*\}$ estimated based on equation 2.5 for each skill and well-being measure separately. In the regression, we control for worker age (fully saturated), person effects, and time effects. We also exclude observations after unemployment in which the worker regains employment to make sure that the post-unemployment effects reflect the skills of those who are continuously unemployed. In Column (2) we report the corresponding 95% confidence intervals. Column (3) reports the within-person R-squared from the regression, while Column (4) shows the number of observations in each regression. Panel A shows the skill measures, where the skills are scaled by their predicted pre-unemployment log earnings, and OLS is used as a prediction model. Panel B reports the change in well-being measured in z-scores. A positive increase in mood (loneliness, unmotivated, depression) is associated with a decline in well-being. In both panels, the acronyms come from the HRS survey.

2.10 Validating Administrative Unemployment Duration with Survey-Reported Employment

A novel feature of our data is that we observe both self-reported employment status and employment status in the social security records. In some cases, there is a discrepancy between the two measures. We demonstrate this in Figure 2-21, which shows the self-reported employment status of individuals who are unemployed (or not marginally employed) according to the administrative data in the month 2, 6 and 12 survey waves.[21]. A majority—but not all—of these individuals identify as unemployed or marginally employed. This is more relevant later on in the unemployment spell. By month 12, 55% of unemployed or marginally employed individuals in the administrative data self-report the same status. The rest are a mix of activities: almost 20% are self-employed, 10% are in training, 5% are in regular activities and 10% are in other-category (family care, homemaking, illness/handicap, extended holiday).

We confirm the robustness of our main results using a narrower definition of unemployment based on the combination of survey and administrative data. Rather than viewing unemployment as the absence of non-marginal employment, we define it as the absence of all forms of employment and training in the survey or administrative data. This conservatively zooms in on those who are plausibly at the highest risk of skill depreciation while unemployed. When controlling for survey wave effects, however, we maintain the reference group of the quickly reemployed based on non-marginal employment in the survey data. This conservatively compares the unemployed to those who are most likely to be building skills during employment.

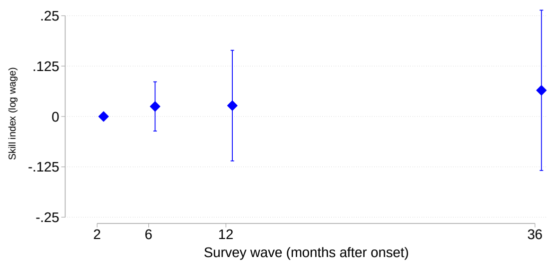
In Figure 2-22 and in Figure 2-23 we replicate our main findings in this more restricted sample. The estimated changes in skill throughout the unemployment spell are almost identical though the estimates are somewhat noisier. These findings highlight that our main conclusions about the lack of skill depreciation over the unemployment fact are not driven by measurement errors in reemployment status.

[21]We observe administrative records for only 30 months after unemployment onset, so we are not able to complete this exercise for the survey 36 months after unemployment onset. In the main analysis, as we explained in Section 2.2, we define labor market status at thirty-six months using the observed labor market status at thirty months.

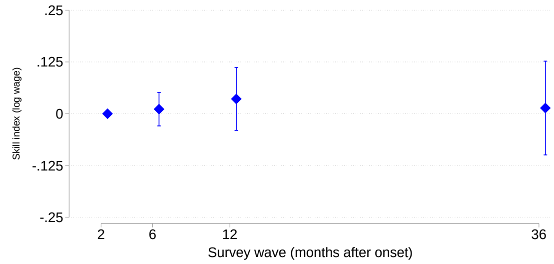
Figure 2-21: Distribution of Self-Reported Status Among Unemployed in the Administrative Data

Notes: The sample at each survey wave corresponds to those in the administrative data without contemporaneous non-marginal employment, which is our benchmark definition of unemployment. Each stacked bar is the share of respondents reporting being engaged in the labeled labor force activity. The “other" category aggregates family care, homemaking, illness/handicap, extended holiday, and other reasons. Respondents could report multiple activities in 2-month (wave 1) and 12-month (wave 3) surveys.

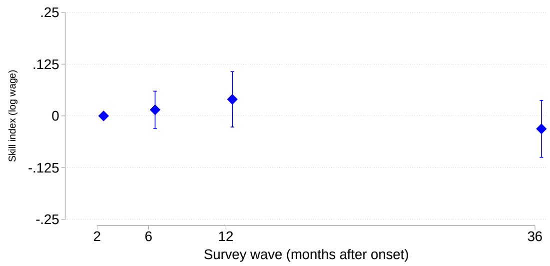
Figure 2-22: Relative Changes in Skill Indices Over the Unemployment Spell: Combined Survey and Administrative Definition of Unemployment



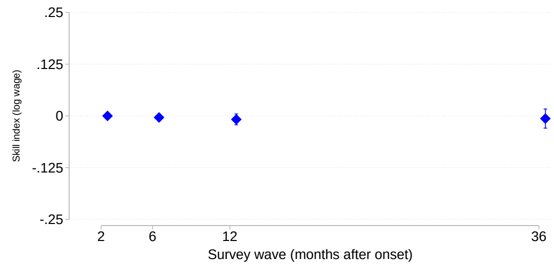
(a) Composite Skill Index



(b) Cognitive Skill Index



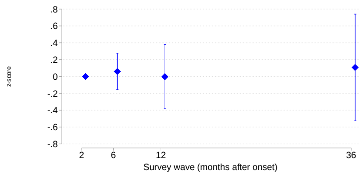
(c) 1st Noncognitive Skill Index



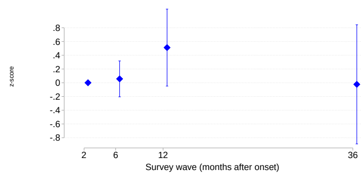
(d) 2nd Noncognitive Skill Index

Notes: This figure recreates Panel (b) of Figure 2-2 but restricts to the definition of unemployment as the absence of all types of employment and training in both survey and administrative data. In all panels, we report estimates with within-person fixed effects (the β_τ coefficient from equation 2.2). The skill index is formed by predicting the prior employment spell's wages using OLS and treating survey responses as cardinal. The primary noncognitive (Panel (c)) index includes only the Big-5 and locus of control questions, the secondary noncognitive index includes the personality traits (Panel (d)), the cognitive skill index (Panel (b)) includes fluency, maths, and short-term recall, and the composite skill index (Panel (a)) includes all cognitive and noncognitive questions. The y-axis scale represents approximately $\pm 1\sigma$ of the log predicted wages using the composite skills index as measured at baseline, which is 0.22.

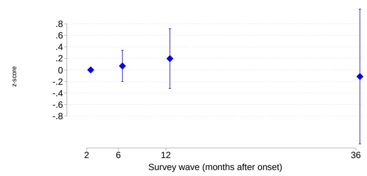
Figure 2-23: Evolution of Individual Skill Items Over the Unemployment Spell: Combined Survey and Administrative Definition of Unemployment



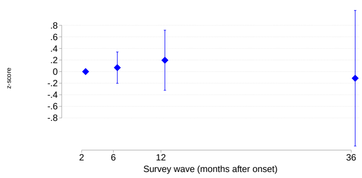
(a) Math



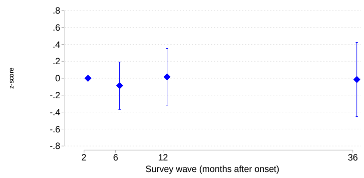
(b) Verbal fluency



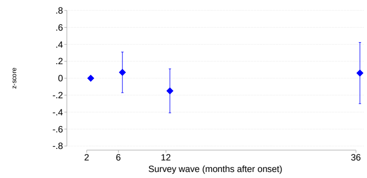
(c) Immediate memory



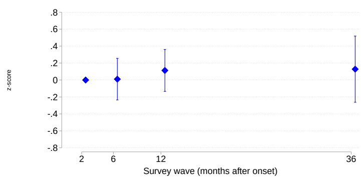
(d) Recall memory



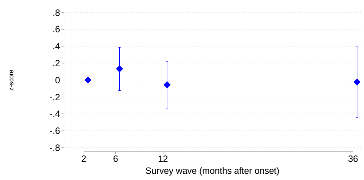
(e) Locus of control



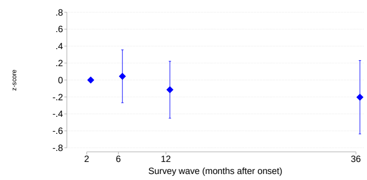
(f) Extravert



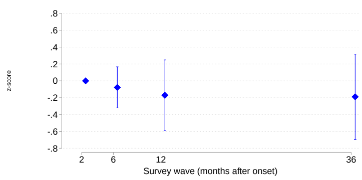
(g) Stable



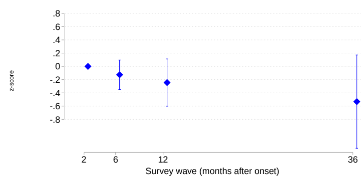
(h) Open



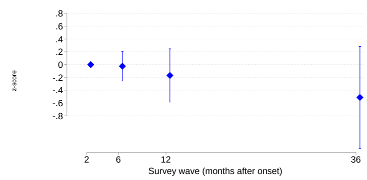
(i) Conscientious



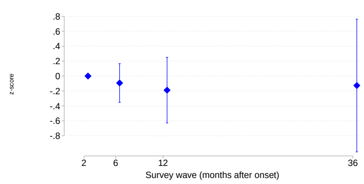
(j) Risk tolerance



(k) Trust



(l) Patience



(m) Reciprocity

Notes: This figure recreates Panel (b) of Figure 2-2 and Figure 2-13 for each individual skill items separately, restricting to the definition of unemployment as the absence of all types of employment and training in both survey and administrative data. We report the β_τ coefficients (along with the 95th percentile confidence intervals) from equation 2.2. Responses are treated as cardinal and signed appropriately. When a category has multiple underlying questions, each question is first converted to a z-score and then those z-scores are averaged together. The standardized z-score is based only on the initial survey and then applied to all surveys.

Chapter 3

Disemployment Effects of Unemployment Insurance: A Meta-Analysis

The empirical literature on unemployment insurance robustly finds significant effects of UI benefit generosity on unemployment duration. We systematically collect estimates and contextual factors from 52 studies, documenting publication bias favoring larger disemployment effects. Accounting for publication bias reduces the elasticity of durations with respect to the replacement rate by one-half and the elasticity of durations with respect to potential benefit duration by one-fifth. Aggregating evidence across studies, the predicted elasticity is 0.27 with respect to the replacement rate and 0.35 with respect to potential benefit duration in an environment whose policy regime is similar to the typical U.S. state (e.g. benefits last 26 weeks). However, in an environment where potential benefit duration is 99 weeks, the elasticity is two to three times larger.

3.1 Introduction

Economic theory predicts expanding a given individual's unemployment insurance (UI) benefit level or duration should increase their unemployment duration. The income and substitution effects—often referred to as the liquidity and moral hazard effects, respectively, in the literature—operate in the same direction (Chetty, 2008b). In theory, aggregate benefit expansions could have countervailing general equilibrium effects, but the unambiguous partial equilibrium response would still apply (Lalive et al., 2015).

It is therefore unsurprising that various reviews of the empirical UI literature find that increases in UI benefits increase unemployment duration (Krueger and Meyer, 2002; Meyer, 2002; Schmieder and von Wachter, 2016; Lopes, 2022). These have focused on only the older literature prior to the proliferation of quasi-experimental methods (Krueger and Meyer, 2002; Meyer, 2002), only the first wave of papers to use quasi-experimental methods (Schmieder and von Wachter, 2016), or only the employment effect of potential benefit duration (Lopes, 2022). Some of these reviews have correlated effects with certain study characteristics, though the limited number of included studies limits the conclusions that can be drawn.

Moreover, given the strong prior that benefit expansions should increase unemployment duration, it is likely that researchers and journals apply more scrutiny to statistically insignificant findings or findings of decreased unemployment duration (Ioannidis, 2005). Such a tendency to censor certain noisy realizations or specification-search to attain a desired result is often referred to as "publication bias", and it has been shown to have significant impacts in other areas (Andrews and Kasy, 2019).

In this paper, we comprehensively survey the extant empirical literature on the employment effects of UI replacement rates and UI potential benefit durations. The estimands we focus on are the elasticity of unemployment duration with respect to replacement rate and the elasticity of unemployment duration with respect to potential benefit duration; hereafter, we refer to them as the RR elasticity and PBD elasticity, respectively. After collecting the most comprehensive list of studies and study characteristics to date on the employment effects of UI benefits, we document the presence of publication bias. We estimate that statistically insignificant disemployment effects are around three-quarters less likely to be published. Explicitly accounting for this propensity following Andrews and Kasy (2019) decreases the mean disemployment elasticity by 17-49%.

After showing that publication bias is a driver of heterogeneity in the literature, we next uncover other key determinants of heterogeneity. To do so while controlling for publication bias, we follow the recent meta-analysis literature and implement Bayesian model averaging (Havranek et al., 2022; Gechert et al., 2022; Zigraiova et al., 2021; Bajzik et al., 2020). This method finds study characteristics that have large partial correlations with the published disemployment elasticity.

The key source of heterogeneity in the elasticity is the baseline PBD. We find contexts with higher baseline PBD are likelier to have greater disemployment elasticities, and this is particularly true for RR increases. To a lesser extent, we also find that periods with elevated unemployment have larger disemployment elasticities.

Section 3.2 contains the study collection procedure. We follow the meta-analysis best practices laid out in Havránek et al. (2020). Section 3.3 presents evidence of publication bias and two different types of corrections. Section 3.4 documents heterogeneity in elasticities based on study characteristics. Section 3.5 concludes.

3.2 Data

We aim to survey all papers that estimate the causal effect of unemployment insurance potential benefit duration or replacement rate on unemployment duration, and we facilitate comparisons across studies by converting effect sizes to elasticities. We additionally collect standard errors as an input for tests of potential publication bias and other study characteristics as predictors of effect size heterogeneity.

3.2.1 Calculating the Elasticity as a Common Measure Across Studies

The elasticity is our effect size of interest, as it can be readily calculated from regressions with either realized duration or hazard rate as the outcome. In capturing policy treatments, we distinguish between different types of UI policy margins: potential benefit duration

(PBD) and replacement rate (RR).[1] In capturing unemployment duration, we distinguish between two measures of unemployment: weeks with any employment (total nonemployment) and weeks claiming UI benefits (covered unemployment).

Constructing an elasticity measure from studies with unemployment duration as the outcome is straightforward. For simplicity, suppose the benefit generosity parameter is specified in logarithm terms. When the regression specification has the logarithm of unemployment duration as the outcome, we obtain the elasticity by exponentiating the treatment coefficient and subtracting 1. When the specification instead has the level of unemployment duration as the outcome, we convert the semi-elasticity corresponding to the treatment coefficient to an elasticity by dividing it by the average level of unemployment duration in the sample.[2] More generally, when the RR or PBD is specified in level terms, we follow the aforementioned procedure to obtain a semi-elasticity and convert that to an elasticity by multiplying by the baseline average RR or PBD.

Many studies instead rely on hazard models because unemployment duration is right-censored. In some of these cases, authors simulate the estimated hazard model under counterfactual benefit generosity to directly report a duration elasticity. When they do not do so, we make an additional assumption to obtain an unemployment duration elasticity. Because the outcome in hazard specifications is the logarithm of the unemployment exit hazard rate, exactly following our procedure for translating duration regression coefficients to elasticities delivers the elasticity of *exit hazard* with respect to RR or PBD. In the special case of a constant hazard rate, the duration elasticity is the additive inverse of the hazard elasticity. Accordingly, we calculate the duration elasticity assuming a constant hazard rate.

The elasticity's standard error is another outcome of interest due to its usefulness in detecting and correcting publication bias. Whenever we translate a reported coefficient to an elasticity, we obtain the elasticity's standard error by ensuring the elasticity's t-statistic is the same as the coefficient's t-statistic. In the event that the authors directly report a sim-

[1] The potential benefit duration is the maximum number of benefit weeks a claimant can receive, and the replacement rate is the fraction of prior weekly earnings a claimant receives as benefits. Some studies exploit variation in the weekly benefit amount rather than the replacement rate, and we categorize those estimates as RR elasticities.

[2] For causal research designs, we scale by the control group's average level, when available. For regression discontinuity designs, in particular, we divide by the level for control units immediately next to the policy threshold.

ulated duration elasticity from a hazard model, we use that as the elasticity point estimate and obtain the relevant t-statistic from the hazard model's UI generosity coefficient.

3.2.2 Study Collection Procedure

We collect journal publications from Google Scholar following the guidelines compiled by the Meta-Analysis in Economics Research Network (Havránek et al., 2020). We limit to papers by the time the search was conducted on August 15, 2022, and we limit to the first 1,000 results that Google Scholar returns.[3] Only 407 of the 1,000 total manuscripts we collect from Google Scholar are journal publications. Of these 407 publications, 88 contain any estimate of the effect of RR or PBD on unemployment duration. Finally, 52 papers contain enough information to calculate a UI duration elasticity and standard error: 30 for PBD elasticities and 27 for RR elasticities.

Papers often include several robustness checks and heterogeneity analyses, but we restrict attention to the author's preferred specification. For a given type of unemployment outcome and UI policy margin, we collect a single elasticity estimate. We rely on the paper's discussion to identify what constitutes the main estimate. In the absence of a discussion highlighting a single main estimate, we choose the estimate in the earliest table with the maximal set of controls.

Some papers do not include an overall elasticity, instead displaying only group-specific elasticities.[4] Because this disaggregation choice is potentially germane to publication bias, we collect each group-specific main estimate from these papers.

In addition to the main duration elasticities and their standard errors, we collect several other study characteristics. There are two broad categories of variables: economic characteristics and methodological characteristics. economic characteristics are those that could drive the theoretical causal parameter of interest. These include features of the macroe-

[3] Specifically, we use the software Publish or Perish with the following query: `duration "standard error" OR "standard errors" OR PBD OR "benefit duration" OR WBA OR "weekly benefit amount" OR "replacement rate" "unemployment insurance"`. In words, this requires the paper's text to contain the word "duration", the exact phrase "unemployment insurance", and at least one of the other phrases.

[4] For example, all statistical tests in Bennmarker et al. (2007) are gender-specific. The paper's elasticities for men are consistently positive and statistically significant while elasticities for women are consistently negative of the same absolute magnitude and marginally statistically significant.

conomic environment—such as the contemporaneous national unemployment rate—and features of the policy environment—such as the baseline RR or PBD. On the other hand, methodological characteristics are those that could plausibly drive a difference between the theoretical causal parameter of interest and the study's estimate. For example, this includes the dataset type and identification strategy. Table 3.2 contains the full set of variables and their sources.

3.2.3 Summary Statistics of Included Studies

Table 3.6 and Table 3.7 contain the full list of included estimates and their sources for PBD elasticities RR elasticities, respectively. The unadjusted average PBD elasticity is 0.48, while the unadjusted average RR elasticity is 0.37. A review with approximately half of the studies we capture finds a slightly lower average PBD elasticity (0.43) and a higher average RR elasticity (0.60) (Schmieder and von Wachter, 2016).

In terms of economic characteristics, the vast majority of studies pertain to either Europe or the United States. Just under one-third (17 of 52) pertain to the United States, while only three of the rest pertain to non-European countries (Brazil, Canada, Turkey). The most commonly represented European countries are Austria (8), Germany (7), Norway (4), and Sweden (3); Finland, France, the Netherlands, Portugal, Slovenia, Spain, and Switzerland are other European countries with 1 or 2 papers.[5]

There is substantial variation across studies in the baseline level of PBD. The maximum number of benefit weeks claimants in the "control group" can receive across studies ranges from 16 to 186 with an interquartile range of 25.

In terms of methodological characteristics, two-thirds of the main estimates are identified using a quasi-experimental research design. Table 3.4 shows this is more common for PBD elasticities (90%) compared to RR elasticities (41%).[6] Figure 3-4 shows it is

[5]Some papers share the same type of benefit variation in the same region. We refer to these as "contexts", and allow for correlation between estimates within the same context in Section 3.3.

[6]RR formulas typically depend on prior earnings with a minimum and maximum amount. Prior to the proliferation of regression kink designs, cross-sectional identification strategies for the RR elasticity would include parametric controls for the benefit formula's running variable. For example, as Katz and Meyer (1990) write, "*It is hard to make a plausible case for endogeneity of these sources of variation given that we are controlling for the previous wage, as well as state characteristics through state fixed effects*".

much more common for recently published papers to use quasi-experimental identification strategies. While the nine-tenths of the studies published prior to 1990 relies on selection on observables assumptions using cross-sectional variation, the one-fifth of the studies published since 2017 uses quasi-experimental identification strategies.

The sample comprises estimates published in influential economics journals. The 25th percentile and median impact factors correspond to Labour Economics and the Journal of Public Economics, respectively. Half of the estimates are from such field journals, one-fifth of the estimates are from one of the "Top-5" general interest journals, one-tenth are from econometric methods journals, and the rest are from other general interest journals.[7]

Finally, we note that two useful methodological practices that are not fully adopted in our sample. First, among the papers whose main specification is a hazard model, only one-fifth simulate the model-implied duration elasticity. Approximating a duration elasticity using the hazard model coefficient requires assuming a constant hazard rate, as discussed in Section 3.2.1. However, this does not appear to hold in the data; among papers that graphically report hazard rates by unemployment duration, there is strong evidence of duration dependence. Specifically, the typical range in weekly hazard rates (8 percentage points) is slightly larger than the average weekly hazard rate (7%); nearly every paper finds that it falls with unemployment duration. Second, four papers overstate the reported elasticity by using the coefficient in a log-log model rather than exponentiating the coefficient and subtracting 1, as discussed in Section 3.2.1. For example, one paper refers to an elasticity of unity in the abstract, while applying the correct transformation shows the elasticity is less than 0.7.

[7] *Field journals*: American Economic Journal: Economic Policy, ILR Review, Journal of Human Resources, Journal of Labor Economics, Journal of Public Economics, LABOUR, Labour Economics, National Tax Journal, The Review of Economics and Statistics

"Top-5" journals: American Economic Review, Journal of Political Economy, The Quarterly Journal of Economics, The Review of Economic Studies Econometric methods journals: Journal of Applied Econometrics and Journal of Econometrics

Other general interest journals: American Economic Association Papers and Proceedings, Bulletin of Economic Research, Economics Letters, European Economic Review, Journal of The European Economic Association, Moneda Y Crédito, Oxford Bulletin of Economics and Statistics, Oxford Economic Papers, Portuguese Economic Journal, Swiss Journal of Economics and Statistics, and The Economic Journal

3.3 Publication Bias Evidence and Corrections

This section provides evidence of publication bias in favor of statistically significant disemployment effects. The visual evidence is many t-statistics that just reach conventional levels of statistical significance but few t-statistics that do not. A model of publication bias deflates the mean elasticity by 17%-49%, which produces an estimate that coincides with the average among a subset of studies apparently not subject to publication bias.

3.3.1 Graphical Evidence of Publication Bias

Publication bias generates a correlation between published elasticities and their standard errors by truncating the distribution of latent effects. If positive estimates are likelier to be published, the distribution of elasticities is truncated at 0. If statistically significant estimates are likelier to be published, then the distribution of t-statistics—the ratio of elasticity to its standard error—is truncated at 1.96.

Figure 3-1 provides evidence consistent with publication bias in favor of statistically significant positive estimates for both the RR and PBD elasticity. First, in terms of the sign, 95% of elasticities are positive. This is despite the fact that Panel (a) shows many small elasticities close to 0. Second, in terms of statistical significance, 83% are significant at conventional levels. Estimates below the red line in Panel (a) are statistically significant, and there are many studies just on the statistically significant side but few just on the statistically insignificant side. Panel (b) emphasizes this by plotting the t-statistic, or the ratio between the elasticity (x-coordinate) and standard error (y-coordinate) from Panel (a). We include all main estimates in this figure, but Figure 3-5 shows the same pattern of bunching when excluding studies with multiple main estimates.

It is worth noting that a positive correlation between elasticities and their standard errors is possible in theory. In particular, it is theoretically possible that there is a positive correlation between elasticities and their standard errors across studies. For example, smaller states could happen to have larger disemployment effects. Nevertheless, it is difficult to believe that such a correlation would deliver a ratio coinciding with the conventional statistical significance threshold.

While t-statistics pertaining to both RR and PBD bunch around 1.96, the bunching is much more pronounced for RR. A plausible channel for which we find evidence is a difference in identification strategy usage. Figure 3-2 shows that the spike in t-statistic density just above statistical significance is most pronounced for cross-sectional variation and regression kink design, while it is least pronounced for regression discontinuity designs. The only research design without clear visual evidence of t-statistic bunching around 1.96 is regression discontinuity, which is used for half of the PBD elasticities but none of the RR elasticities. Conversely, the research designs with the clearest bunching are cross-sectional variation and regression kink design, the union of which is used for three-quarters of the PBD elasticities. The unique lack of t-statistic bunching for regression discontinuity designs among non-experimental methods is in line with a review of the broader empirical economics literature (Brodeur et al., 2020).[8]

Another plausible channel is the difference in plausible sources of endogeneity. Policy endogeneity is the clearest difference-in-difference designs identifying the PBD elasticity and cross-sectional designs identifying the RR elasticity. PBD extensions typically arise in weak macroeconomic environments. To the extent there is additional PBD variation amenable to a difference-in-differences design, it is often targeted to vulnerable groups who are plausibly hit harder by the macroeconomic shock. On the other hand, the weekly benefit amount in RR formulas is typically an increasing function of prior earnings, as the idea of UI is that it replaces prior earnings. Therefore, in the absence of publication bias, the plausible direction of selection bias from policy endogeneity is positive for PBD elasticities but negative for RR elasticities.[9] In turn, publication bias that favors positive disemployment effects would be likelier to censor RR elasticities than PBD elasticities.

[8]The prior literature finds that t-statistic bunching is most pronounced for instrumental variables. This suggests why we see pronounced bunching for regression kink designs, as all such specifications instrument for change in unemployment slopes across policy kinks with the change in benefit slopes.

[9]Landais (2015b) compares the convention in cross-sectional designs to control for a polynomial of prior earnings with more flexible specifications local to benefit formula kinks as in a regression discontinuity design. The elasticities from a regression kink design (≈ 0.3) are nearly half that of the elasticity from a cross-sectional design (0.56).

3.3.2 Simple Bias Correction: Averaging only "High-Quality" Estimates

Our first publication bias correction is to simply focus on a subset of estimates not subject to publication bias. As discussed above, Figure 3-2 suggests this is the case for regression discontinuity designs, which all estimate PBD elasticities (see Table 3.4). This pattern is in line with prior work and is plausibly due to the perceived high degree of internal validity in these research designs (Brodeur et al., 2020). In turn, focusing on these estimates mitigates concerns about selection bias in additional to publication bias.

Different aggregation approaches suggest that the average PBD elasticity is around 0.35, which is approximately 25% smaller than the overall average unadjusted PBD elasticity (0.48). The averaging approaches slightly differ based on treating estimates within a given policy context. The top row of Table 3.5 first obtains the unweighted average elasticity within each context and then takes the average of main estimates across contexts. The second row makes the parametric assumption that elasticities are normally distributed across contexts and possibly correlated within contexts. The estimated average elasticity is essentially unchanged, and the across-context variance in elasticities (0.08) is small relative to the average elasticity.

3.3.3 Structural Bias Correction: Andrews and Kasy (2019) using All Estimates

We next back out the latent distribution of elasticities absent publication bias following Andrews and Kasy (2019). Relative to the previous approach, we can include a much wider range of studies, particularly those that estimate RR elasticities. Additionally, rather than assume away publication bias, we explicitly model it. The primary drawback is that the structural model requires a stronger set of assumptions.

The main idea of the approach is to jointly model the latent distribution of elasticities and the publication probability for different realizations. Latent estimates are defined as realizations of the estimated elasticity prior to the publication process. We make parametric assumptions about the publication probability as a function of t-statistic but leave the functional form of the latent distribution of elasticities unrestricted by using the paper's Generalized Method of Moments (GMM) approach. The key identification assumption for this approach is that the latent distribution of elasticities is independent of the latent distribution of standard errors of the elasticity.

Following the setup in Section I of Andrews and Kasy (2019), define the distribution of latent elasticities and their standard errors to be $(\Theta, \Sigma) \sim \mu_{\Theta,\Sigma}$. This distribution describes heterogeneity across studies and the noisiness of estimates. For a given study with distribution parameters (Θ, Σ), a noisy realization of the latent elasticity X is drawn: $X \mid \Theta, \Sigma \sim N(\Theta, \Sigma^2)$.

Motivated by the lack of t-statistics just below 0 and the excess mass of t-statistics just above 1.96, we assume the following publication probabilities for t-statistics relative to t-statistics above 1.96:

$$p(t) = \begin{cases} \beta_{p,1} & \text{if } t < 0 \\ \beta_{p,2} & \text{if } t \in [0, 1.96) \end{cases} \tag{3.1}$$

Normalizing the publication probability for t-statistics above 1.96 is without loss of generality, as that pins down the total number of published estimates. However, assuming a constant publication probability within each region is a strong parametric assumption.

Table 3.1 shows significantly lower publication probabilities for elasticities that are not positive and statistically significant, which in turn implies that the mean of the latent distribution of elasticities is lower than the average observed elasticity. The debiased mean elasticities are 0.4 for PBD and 0.19 for RR (Column 3), which are 17-49% lower than the unadjusted elasticities (Column 1). The debiased mean PBD elasticity is nearly the same as the average PBD elasticity among regression discontinuity designs (Column 2).

Even though the relative publication probabilities for elasticities that are not statistically significant and positive elasticities are approximately 25% for both PBD and RR (Columns 4 and 5), the debiasing has a larger effect for RR. This is because the estimated standard error of its latent distribution is more than twice as large as that for PBD (Column 3). Intuitively, a latent distribution that tends to produce smaller standard errors decreases the likelihood of noisy realizations that would be more likely to be censored.

As a point of comparison, the degree of publication bias in favor of UI disemployment effects is comparable to the degree of publication bias in favor of minimum wage disemployment effects that Andrews and Kasy (2019) find. In their application, the publication probability for statistically insignificant results is between one-quarter and one-third. Similarly, the mean of the latent distribution is half the average observed estimate.

One difficulty in comparing the means of the latent distributions of RR and PBD elasticities is that they come from different studies with study characteristics. In the next section, we account for the correlation between study characteristics and estimated elasticities to facilitate a consistent comparison across PBD and RR. An additional feature of this approach is that it documents heterogeneity across economic features.

3.4 Predictors of Heterogeneity in Elasticity

This section predicts heterogeneity in elasticity estimates by study characteristics. We describe the advantages of a meta-analysis compared to existing heterogeneity analyses within individual studies, and we argue features of the existing UI policy environment are of particular interest. Elasticities are larger for a longer baseline PBD but do not vary with the baseline RR.

3.4.1 Motivation for Included Predictors

The purpose of predicting the elasticity with study characteristics is to provide suggestive evidence on causal mechanisms relevant to policymakers. As discussed in Section 3.2, the collected study characteristics for predicting the elasticity can be separated into two categories: methodological characteristics and economic characteristics. Methodological characteristics should be interpreted as auxiliary controls. We include them to account for any estimation choices that could affect the estimated elasticity. In contrast, economic characteristics should be interpreted as the key dimensions of heterogeneity. These are factors that policymakers can account for when prospectively setting UI benefit parameters in their own economic context.

We believe meta-analysis's comparative advantage when applied to UI benefits is in documenting heterogeneity by baseline benefit parameters. Estimating an elasticity requires variation in the benefit parameter itself, but this typically comes from a single policy discontinuity or policy reform that in turn has a single baseline PBD or RR. A PBD elasticity that increases with the baseline PBD is a prediction of the Shavell and Weiss (1979) model of job search under UI that has not been directed tested empirically. The closest existing evidence comes from two papers with multiple discontinuities in PBD at different ages or years of tenure (van Ours and Vodopivec, 2008; Schmieder et al., 2012b). These papers do find significant heterogeneity, but this could be explained by demographic differences that determined the baseline benefit differences. When including the baseline PBD (in weeks) and baseline RR (fraction) as predictors, we additionally interact them with the policy margin. This is because a given increase in benefit PBD increases total benefit entitlement more with a higher RR and vice versa.

One economic dimension that has received attention in the literature is whether labor supply elasticities vary with the business cycle, as theory is ambiguous but policy-relevance is clear Schmieder and von Wachter (2016). This type of heterogeneity analysis is feasible in existing studies, as authors can implement the same research design at different points of the business cycle throughout the sample time period. The existing literature has found a mix of positive (Bell et al., 2022b), negative (Kroft and Notowidigdo, 2016; Landais, 2015b), and insignificant (Schmieder et al., 2012b) correlations between the elasticity and unemployment rate. We include deviations of the contemporaneous unemployment from the country's average to provide across-study evidence on this topic.

One other economic dimension of theoretical interest is whether the benefit variation affects many people or only a limited set of people. This is the distinction between the "micro" elasticities from changing a given claimant's benefits *ceteris paribus* and the "macro" elasticity from changing benefit rules that affect all claimants (Landais et al., 2018). All but one of the included studies leverages person-level benefit variation with individual unemployed workers as the unit of observation, so we subjectively categorize sources of benefit variation based on whether a significant fraction of the overall labor market is affected. Congestion externalities in job search would be a force towards finding smaller elasticities when many workers are affected, which Lalive et al. (2015) finds but Johnston and Mas (2018) does not. On the other hand, general equilibrium effects on firm-side vacancy creation would be a force towards finding larger elasticities (Karahan et al., 2019).

We primarily view the remaining economic variables as additional controls. For example, the labor tax wedge variable is intended to account for the possibility that more countries with more generous transfer systems may also have more stringent tax systems. Similarly, the United States indicator coarsely captures other institutional differences between American job seekers and those in the rest of the predominantly European sample.

Most of the methodological characteristics are proxies for study quality: data source, research design, and publication bias susceptibility. First, administrative data is less prone to measurement error. Second, quasi-experimental research designs—particularly regression discontinuity designs—are less prone to selection bias due to policy endogeneity. Third, noisier estimates are more prone to publication bias. Following the meta-regression literature, we include the standard error as a control (Stanley, 2008). Furthermore, motivated by findings in Section 3.3 that publication bias is more severe for certain research designs, we interact standard errors with each research design.

Other methodological characteristics are less clearly related to study quality but are included to increase comparability across estimates. For example, it is unclear whether elasticities derived from hazard models are higher quality than those derived from duration regression. The former requires a constant hazard assumption despite an empirical regularity that hazard rates fall with unemployment duration, while the latter requires truncating the observation duration for censored spells. Additionally, elasticities with the outcome as either total nonemployment duration or covered unemployment are related but conceptually distinct estimands.

3.4.2 Documenting Heterogeneity Using Bayesian Model Selection

When projecting elasticities onto study characteristics, the power for detecting the importance of a given predictor diminishes as the number of predictors increases. In particular, as more predictors are added, the probability that a given variable is highly correlated with another will increase. While the augmented model will better predict the final outcome, it becomes more difficult to interpret which factors drove the prediction.

Following recent developments in the meta-analysis literature, we use Bayesian model averaging (BMA) to highlight heterogeneity correlates (Havranek et al., 2022; Gechert et al., 2022; Zigraiova et al., 2021; Bajzik et al., 2020). The frequentist analog to BMA is as follows: run separate regressions predicting the elasticity using all possible subsets of the study characteristics and average coefficients across regressions. BMA instead imposes priors over included covariates and their coefficients, iteratively updates those priors based on the likelihood, and outputs Bayesian analogs to the regression coefficient (posterior mean) and p-value (posterior inclusion probability). We use priors common in the BMA literature and estimate the model using the `bms` package in R (Eicher et al., 2011; Zeugner and Feldkircher, 2015).[10]

Table 3.3 displays coefficients summarizing the strength of each predictor's relationship with the elasticity. Due to unavailable data on certain covariates for some studies, the sample includes only 35 estimates. The first two panels include economic characteristics of primary interest, the third panel includes auxiliary economic characteristics, and the final three panels include methodological characteristics. Columns 1 and 2 are the primary measures of interest. The posterior inclusion probability in Column 1 captures a Bayesian notion of statistical significance, and the posterior mean in Column 2 captures the coefficient magnitude of the relationship in the linear model.

The main source of heterogeneity we find with respect to any economic characteristic is that elasticities are larger with longer baseline PBD. These characteristics have a 73%-91% posterior inclusion probability, while no other economic characteristic's inclusion probability is above 24%. This interaction effect holds both for PBD and RR elasticities. For example, when the baseline PBD is 10 weeks longer, the coefficients in Table 3.3 imply that both types of elasticities will be larger by 0.03-0.04. This is partly mechanical for the case of the PBD elasticity, as the baseline PBD appears in the numerator.[11] On the other hand, the economic intuition for the case of the RR elasticity is clear: a given RR increase provides a greater total benefit entitlement when the PBD is longer.

While there is a positive relationship between elasticities and baseline PBD, there is

[10] In particular, we use Zellner's g-prior for the regression coefficients and a uniform model prior.

[11] The PBD elasticity is the percent change in unemployment duration with respect to the percent change in PBD: $\epsilon = \frac{\mathrm{d}UE}{\mathrm{d}PBD} \cdot \frac{\overline{PBD}}{\overline{UE}}$.

not a relationship between elasticities and the baseline RR. RR is measured as a fraction of prior earnings, so the magnitudes are quantitatively small. In particular, the estimates imply that increasing the RR by 100% of prior earnings corresponds to a decrease in the RR elasticity of 0.03 and a decrease in the PBD elasticity of 0.02.

We do not find other that any other economic characteristics are meaningfully correlated with elasticities. If anything, aggregate reforms and periods with high unemployment rates appear to have higher elasticities. The magnitudes for both of these findings are small, however, and the post-inclusion probabilities below one-half suggest that they are more often than not discarded from the model as predictors.

The two key methodological characteristics are standard errors and the definition of unemployment duration. First, in line with Figure 3-2, the positive relationship between elasticities and their standard errors is most pronounced for difference-in-differences designs. Second, we find that measuring unemployment duration as total nonemployment delivers a much lower elasticity. This pattern holds even within studies, which implicitly controls for all contextual characteristics. Among the 15 studies with both types of outcome elasticities, the difference (-0.290) is nearly the same as the posterior mean estimated by BMA (-0.242). There are two mechanical reasons for this. First, the level of total nonemployment is by definition greater than the level of covered unemployment for a claimant who takes up benefits due to the time-limited nature of benefits. This level term enters the denominator of the elasticity. Second, for any claimant whose unemployment spell in the absence of treatment exceeds the eventual benefit extension, the benefits extension mechanically increases the covered duration one-for-one but changes the total nonemployment duration only through income effects.

Taking all of these characteristics into account, we generate summary measures of predicted elasticities in Figure 3-3. We highlight baseline benefit parameters as the economic characteristics of interest and set the other predictors to levels conventional in the present-day United States. The x-axis range for Panel (a) corresponds to the historical range of PBD in the United States: as little as 12 weeks in present-day Florida and as high as 99 weeks during the Great Recession. Similarly, the x-axis range for Panel (b) corresponds to the typical 50% RR claimants receive in the United States. We separately plot these series for PBD elasticities and RR elasticities due to the included interaction terms.

The first takeaway from these figures is a visual summary of the strong heterogeneity by baseline PBD shown in Table 3.3. Policymakers adjusting benefits under extended benefits that range up to 99 weeks can expect a PBD elasticity of approximately 0.9 and a RR elasticity of approximately 0.6. These are more than twice times as large as the corresponding values predicted at the standard PBD of 26 weeks. In contrast, the degree of heterogeneity by baseline RR is quantitatively very small. Interestingly, we show the strong, positive relationship between elasticity and the baseline PBD holds without adjusting for any other study characteristics in Figure 3-6.

The second takeaway from these figures is that, after accounting for all other study characteristics, the PBD elasticity is larger than the RR elasticity. This holds across various policy environments and is in line with our publication-bias corrected estimates from Table 3.1 that do not account for study characteristics. For typical features of the United States, the RR elasticity with respect to covered unemployment duration is 0.27, and the corresponding PBD elasticity is 0.35. The PBD elasticity is nearly identical to both of the publication-bias-corrected PBD elasticities derived in Section 3.3, though it is worth noting that this section's elasticity would decrease by approximately 0.1 if we instead average over both total nonemployment duration and covered unemployment duration as outcomes.

An advantage of the meta-analysis framework in comparing the average PBD elasticity and average RR elasticity is that it controls for any characteristics that systematically vary across the two study types. Our finding of relatively lower RR elasticities compared to PBD elasticities is different than what one would infer from taking the average across studies in (Schmieder and von Wachter, 2016). Nevertheless, that review finds the efficiency costs of RR increases are approximately half of the efficiency cost of PBD increases due to differences in the benefit dollars mechanically transferred to claimants. Adjusting RR elasticities downward would likely strengthen the finding of relatively lower efficiency costs for that type of benefit expansion.

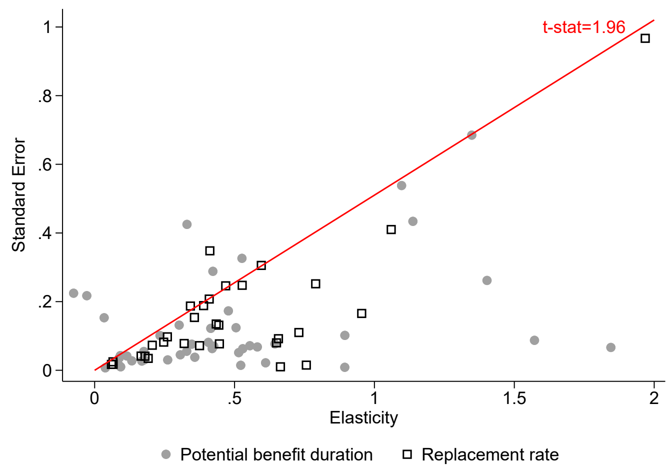
3.5 Conclusion

We comprehensively survey the literature and confirm prior reviews finding that unemployment insurance benefit expansions increase unemployment. Prior surveys likely overstate the magnitude of this effect due to publication bias. Correcting for publication bias decreases the average disemployment elasticity by 20-50%.

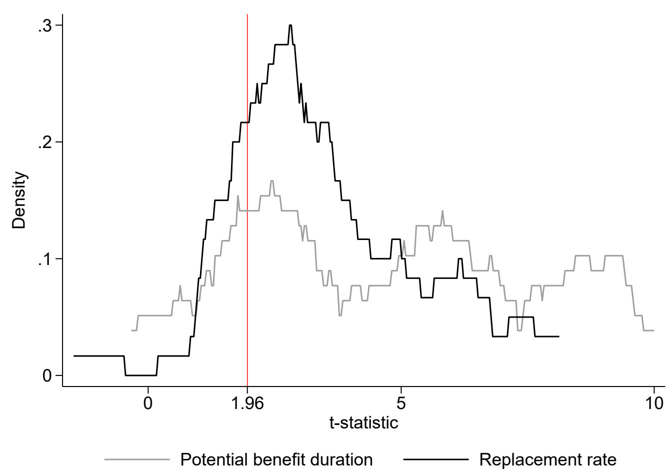
After accounting for publication bias and study characteristics, a typical RR elasticity in terms of covered unemployment is 0.27, and a typical PBD elasticity in terms of covered unemployment duration is 0.35. Elasticities are larger for longer baseline PBD, but they do not significantly vary with other economic characteristics.

3.6 Main Figures

Figure 3-1: Descriptive Evidence of Publication Bias by UI Benefit Expansion



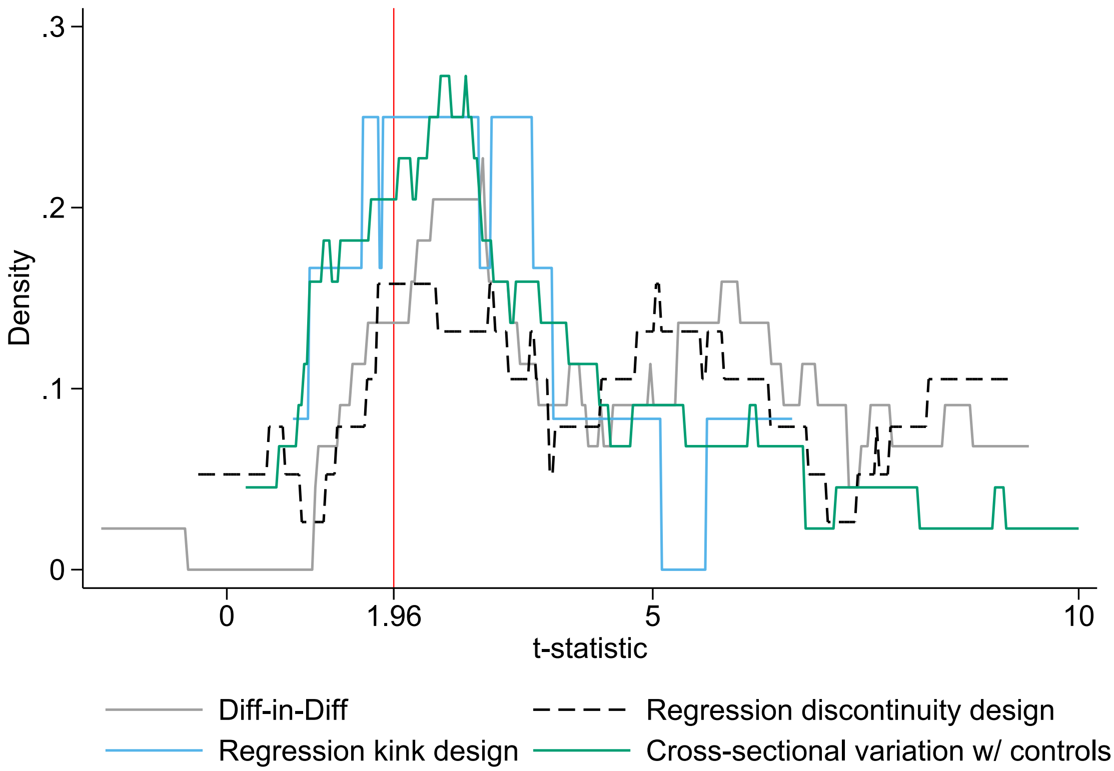
(a) Funnel Plot Asymmetry



(b) *t*-statistic bunching

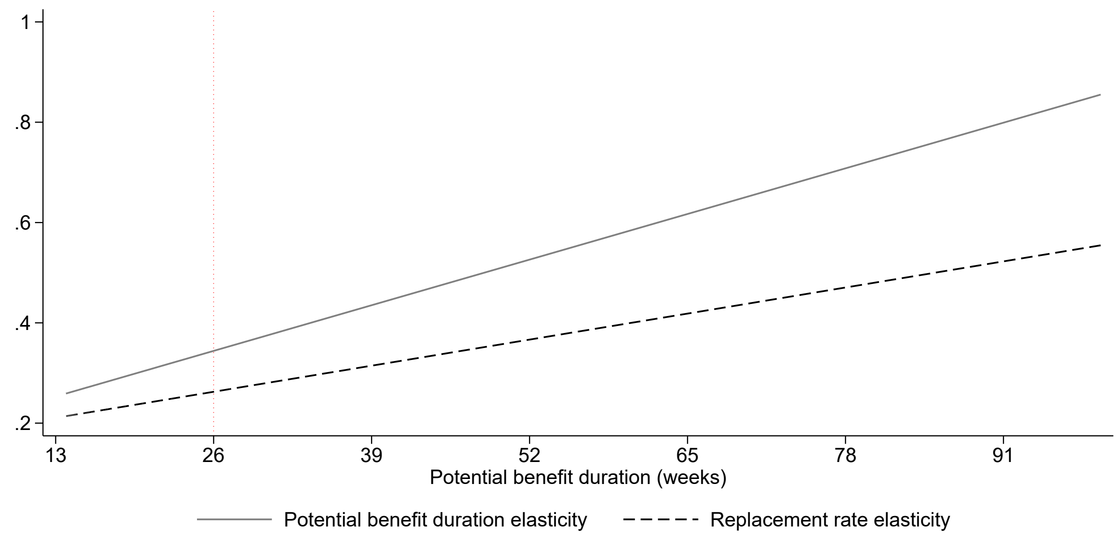
Notes: Each panel includes all main estimates. In Panel (a), each dot is a separate main estimate. The red line demarcates statistical significance at the 95% confidence level, where estimates below the red line are statistically significant. For visual clarity, a replacement rate elasticity of -3.32 and standard error of 2.25 from Hunt (1995) is excluded. Panel (b) is the density of *t*-statistics, or the ratios of the elasticity to its standard error. For visual clarity, it excludes *t*-statistics above 10 from Gerard and Gonzaga (2021); Rebollo-Sanz and Rodríguez-Planas (2020); Schmieder and von Wachter (2016); Schmieder et al. (2012b); Røed and Westlie (2012); Lalive (2007); Moffitt (1985).

Figure 3-2: *t*-statistic bunching by research design

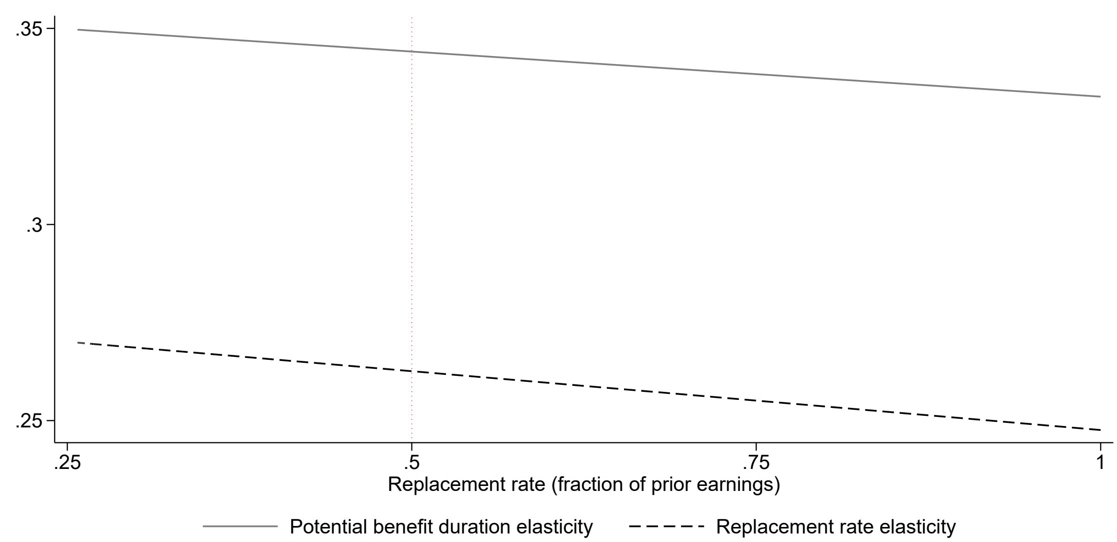


Notes: The density plot includes all main estimates. The *t*-statistics is the ratio of the elasticity to its standard error. For visual clarity, the density plot excludes *t*-statistics above 10 from Gerard and Gonzaga (2021); Rebollo-Sanz and Rodríguez-Planas (2020); Schmieder and von Wachter (2016); Schmieder et al. (2012b); Røed and Westlie (2012); Lalive (2007); Moffitt (1985).

Figure 3-3: Predicted Elasticities from Bayesian Model Averaging



(a) Heterogeneity by Baseline Potential Benefit Duration



(b) Heterogeneity by Baseline Replacement Rate

Notes: This figure summarizes the Bayesian Model Averaging output from Table 3.3. All series assume the setting is the present-day United States, the estimand is the covered duration elasticity, the policy variation affects all people, the dataset is administrative, and the research design is a regression discontinuity. Panel (a) plots heterogeneity by the baseline potential benefit duration assuming a replacement rate of 0.5. Panel (b) plots heterogeneity by the replacement rate assuming a potential benefit duration of 26. These assumed values correspond to typical levels in the United States and are indicated by the vertical dashed red lines. The scale of the *x*-axes roughly indicate historical ranges of these policy variables.

3.7 Main Tables

Table 3.1: Publication Bias Corrections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\bar{\hat{\epsilon}}$	$\bar{\hat{\epsilon}}_{RDD}$	Θ	Σ	$\beta_{p,1}$	$\beta_{p,2}$	N_{total}	N_{clusters}
Potential benefit duration	0.48	0.36	0.40	0.33	0.25	0.22	44	24
	(0.05)	(0.06)	(0.05)	(0.07)	(0.19)	(0.14)		
Replacement rate	0.37		0.19	0.80	0.24	0.17	32	21
	(0.14)		(0.08)	(0.07)	(0.27)	(0.12)		

Notes: The table reports statistics on the observed and latent distribution of elasticities for potential benefit duration and replacement rate. $\bar{\hat{\epsilon}}$ is the sample mean of realized elasticities. $\bar{\hat{\epsilon}}_{RDD}$ is the sample mean of realized elasticities in regression discontinuity designs, which apply only to 23 PBD elasticity estimates from 11 contexts in the sample. The remaining columns report estimated parameters from the Andrews and Kasy (2019) GMM-based publication bias correction. Θ and Σ are the means of the latent distributions of elasticities and standard errors, respectively. $\beta_{p,1}$ and $\beta_{p,2}$ are the publication probabilities for negative t-statistics and positive t-statistics below 1.96, respectively. N_{total} is the total number of main estimates and N_{clusters} is the number of unique policy contexts in the overall sample for that policy. Standard errors for each parameter estimate are below in parentheses. All standard errors allow for correlation within a given policy context.

Table 3.2: Study Characteristics for Predicting the Elasticity

Category	Variables
Economic characteristics	
Policy	Potential benefit duration (vs. replacement rate), All affected by variation (vs. targeted variation)
Environment	Baseline potential benefit duration, Sample year, Relative unemployment rate, United States, Labor tax wedge
Methodological characteristics	
Data	Administrative data (vs. survey), total nonemployment (vs. covered unemployment) as the outcome
Estimation technique	RDD/RKD, DiD, hazard model
Publication	Journal impact factor (z-score)

Notes: Dummy variables are coded up taking the value 0 with the category in parentheses.
Economic characteristics definitions: The baseline potential benefit duration is defined as the amount for the control group in quasi-experimental designs or the average sample amount in cross-sectional designs. The sample year is the year of the initial policy reform or the average sample year; it is coded up relative to the present-day (2023) such that all values are positive. The relative unemployment rate measures the time-specific macroeconomic environment. It comes from the World Bank's World Development Indicators database that is available since 1991. The difference comes from subtracting the average across all available years from the value in the sample year. The labor tax wedge summarizes the country's tax code. It is defined as the ratio between the amount of taxes paid by an average single worker without children and the corresponding total labor cost for the employer. It is the latest available value from the OECD: 2019 for Brazil and 2021 for all other countries. The estimation technique variables are all dummy variables. Regression discontinuity designs (RDD) and regression kink designs (RKD) are pooled together, difference-in-differences (DiD) is its own category, and the omitted category is selection on observables designs relying on cross-sectional variation. The journal impact factor is the IDEAS/RePEc Simple Impact Factor as of April 10, 2023.

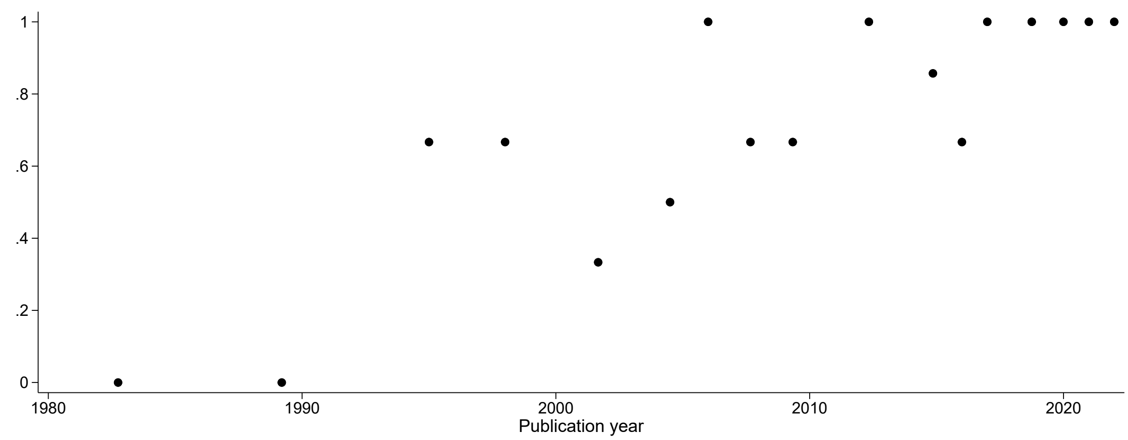
Table 3.3: Study Characteristics Correlated with Elasticities

	(1)	(2)
	Posterior Inclusion Probability	Posterior Mean
(Intercept)	1.000	0.221
Policy variation		
PBD elasticity (vs. RR elasticity)	0.261	0.016
Aggregate variation	0.145	0.007
Baseline benefits		
Baseline PBD (weeks)	0.905	0.004
Baseline PBD x PBD elasticity	0.728	0.003
Baseline RR (fraction)	0.149	-0.030
Baseline RR x PBD elasticity	0.199	0.007
Study context		
Sample year (2023 = 0)	0.181	-0.001
Relative unemployment (pp)	0.239	0.007
Labor tax wedge (pp)	0.159	-0.001
United States dummy	0.166	-0.018
Data and estimation		
Administrative data	0.128	0.003
Nonemployment as outcome	0.874	-0.242
Hazard model	0.146	0.006
DID or RKD	0.152	0.007
RDD	0.159	-0.011
Standard errors		
SE	0.181	0.089
SE x (DID or RKD)	0.951	1.748
SE x RDD	0.142	-0.005
Journal		
Impact Factor (z-score)	0.152	0.003

Notes: This table contains model output from Bayesian model averaging to predict all elasticity main estimates with study characteristics. The first column is the posterior inclusion probability, the second column is the posterior mean, and the third column is the posterior standard deviation. Variables are described in the notes to Table 3.2.

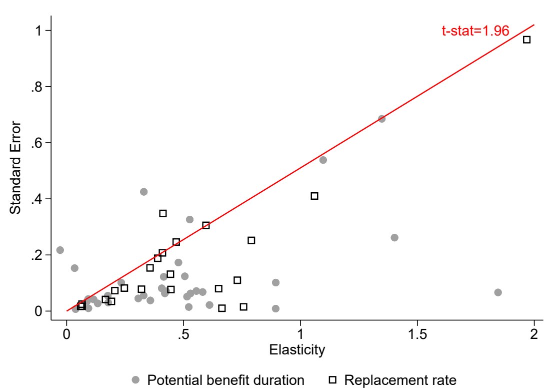
3.8 Appendix Figures

Figure 3-4: Quasi-Experimental Share of Studies by Publication Year

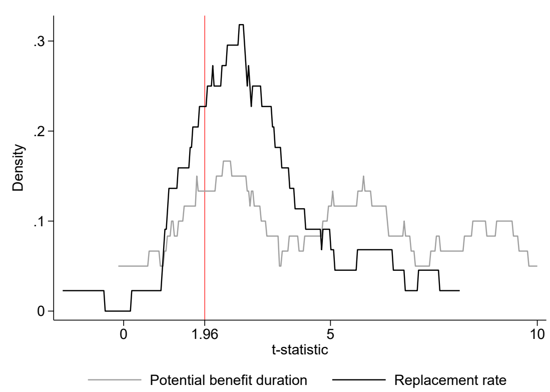


Notes: The figure is a binned scatterplot of the conditional mean by ventiles of year. Quasi-experimental studies are defined as those that identify the elasticity using a difference-in-differences design, regression discontinuity design, or regression kink design. The only other type of study in the sample is cross-sectional variation that uses a selection on observables assumption for identification.

Figure 3-5: Descriptive Evidence of Publication Bias by UI Benefit Expansion (1 estimate per study)



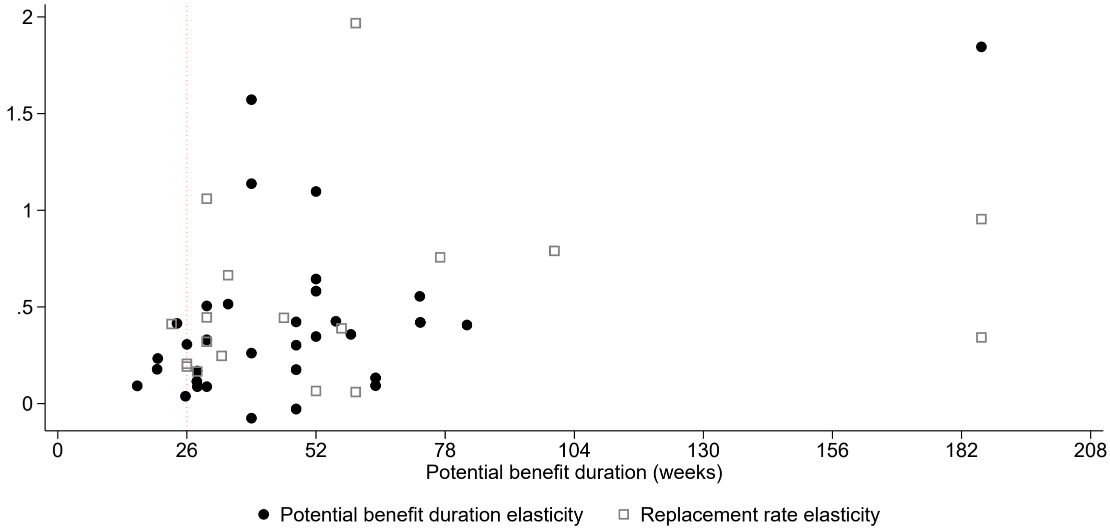
(a) Funnel Plot Asymmetry



(b) *t*-statistic bunching

Notes: This figure reproduces Figure 3-1 but additionally excludes any paper with multiple main estimates. In Panel (a), each dot is a separate main estimate. Panel (b) is the density of *t*-statistics, or the ratios of the elasticity to its standard error.

Figure 3-6: Heterogeneity by Baseline PBD Holds With Unadjusted Estimates



Notes: The figure plots all unadjusted main elasticity estimates by the study's baseline weeks of potential benefit duration. For visual clarity, we omit a replacement rate elasticity of -3.32 and baseline PBD of 52 from Hunt (1995).

3.9 Appendix Tables

Table 3.4: Distribution of Research Design by Policy Margin Among Included Studies

	DID	RDD	RKD	Other	Total
Potential benefit duration	13	13	1	3	30
	(43%)	(43%)	(3%)	(10%)	(100%)
Replacement Rate	8	0	3	16	27
	(30%)	(0%)	(11%)	(59%)	(100%)
Total	21	13	4	19	57
	(37%)	(23%)	(7%)	(33%)	(100%)

Notes: The table includes 57 total observations from the 52 included papers because 5 papers estimate both an elasticity with respect to potential benefit duration and an elasticity with respect to replacement rate. The rows split policy parameters by whether the elasticity is with respect to potential benefit duration or replacement rate. The columns correspond to mutually exclusive research designs. The first three columns are quasi-experimental designs: DID is difference-in-differences, RDD is regression discontinuity design, and RKD is regression kink design. Other refers to papers using only cross-sectional variation, which implicitly relies on a selection on obervables assumption for identification. The numbers correspond to the total number of estimates in that cell, and the percentages in parentheses refer to the fraction of the row's observations in that cell. Percentages may not add up to 100% due to rounding.

Table 3.5: Aggregating PBD Elasticities from Regression Discontinuity Designs

	(1)	(2)	(3)	(4)	(5)
	Avg. elasticity	Std. err. of avg.	Across-context variance	N_{total}	N_{clusters}
OLS	.36	(.06)	.04	11	11
Random Effects	.35	(.06)	.08	23	11

Notes: The only included main estimates are elasticities with respect to potential benefit duration estimated using regression discontinuity designs. N_{total} is the total number of main estimates and N_{clusters} is the number of unique policy contexts. The first row collapses the dataset to a single main estimate per study by taking a simple average. The average and standard error correspond to the constant in an OLS regression. The second row maintains all main estimates and estimates a random effects model that assumes elasticities are normally distributed across contexts and correlated within contexts.

Table 3.6: Included Studies: Disemployment Elasticities with Respect to Potential Benefit Duration

Notes: Each row is a separate main estimate. The first column is the authors, publication year, and journal abbreviation. The second column indicates that all of the estimates correspond to the elasticity with respect to potential benefit duration. The third column contains mutually exhaustive categories for regression discontinuity design (RDD), regression kink design (RKD), difference-in-differences (DID), and cross-sectional variation with controls (cross-sectional). The fourth column describes whether the unemployment outcome is total nonemployment or covered unemployment. The fifth column is the elasticity of the unemployment duration outcome with respect to the benefit generosity parameter and the sixth column is its standard error. The seventh describes the calculation sources from the published paper.

Paper	**Policy**	**Design**	**UE meaure**	**Elasticity**	**SE**	**Source**
Addison & Portugal ’08 EL	PBD	DID	Total	0.23	0.1	Table 2
Caliendo & Tatsiramos ’13 JAE	PBD	RDD	Total	0.42	0.29	Text for elasticities (pg 624) and Table V for SEs
Caliendo & Tatsiramos ’13 JAE	PBD	RDD	Total	0.3	0.13	Text for elasticities (pg 624) and Table V for SEs
Card & Levine ’00 JPubEc	PBD	DID	Covered	0.31	0.05	Table 6 Column 2 Row 2
Card et al ’07 QJE	PBD	RDD	Total	0.18	0.03	Table II Column 1 Row 2
Centeno & Novo ’09 PEJ	PBD	RDD	Total	0.43	0.07	Figure 1, Table 2 Columns 2 and 4
De Groot et al ’19 Labour Econ.	PBD	DID	Total	0.41	0.08	Text (pg 207) and Table 2
Fackler et al ’19 Labour Econ.	PBD	RDD	Covered	0.48	0.17	Table 1 Column 2
Fackler et al ’19 Labour Econ.	PBD	RDD	Total	-0.03	0.22	Table 1 Column 2
Farber et al ’15 AEAPP	PBD	Cross-sectional	Total	0.03	0.15	Figure 2 and Column 2 Row 2 Table 1
Filiz ’17 Labour	PBD	RDD	Covered	0.04	0.01	Table 3 Column 1
Gerad & Gonzaga ’21 AER	PBD	RDD	Covered	0.89	0.01	Figure 5 Panel (a) and Text (pg 192)

Gerad & Gonzaga ’21 AER	PBD	RDD	Total	0.09	0.04	Figure 5 Panel (a) and Text (pg 192)
Hunt ’85 JOLE	PBD	DID	Total	1.1	0.54	Table 2, Table 5 Columns 3 and 5, Table 7 Column 2, Text (pg 91-92)
Johnston & Mas ’18 JPE	PBD	RDD	Total	0.42	0.06	Text (pg 2500)
Johnston & Mas ’18 JPE	PBD	RDD	Covered	0.89	0.1	Text (pg 2500)
Katz & Meyer ’90 JPubEc	PBD	Cross-sectional	Covered	0.53	0.33	Table 3 Columns 1 and 2, Rows 2 and 5
Kyyra & Pesola ’20 Labour Econ.	PBD	DID	Total	0.55	0.07	Table 2 Column 4 and text (footnote 12)
Lalive & Zweimuller ’04 JPubEc	PBD	DID	Total	0.09	0.04	Table 4 bottom row and Table 3
Lalive ’07 AEAPP	PBD	RDD	Total	0.26	0.03	Table 1 Row 1
Lalive ’07 AEAPP	PBD	RDD	Total	1.57	0.09	Table 1 Row 1
Lalive ’07 AEAPP	PBD	RDD	Total	-0.08	0.22	Table 1 Row 1
Lalive ’07 AEAPP	PBD	RDD	Total	1.14	0.43	Table 1 Row 1
Lalive ’08 JOE	PBD	RDD	Total	0.35	0.08	Table 2 Column 2 Panel B and Figures 3 and 8
Lalive ’08 JOE	PBD	RDD	Total	0.64	0.08	Table 2 Column 2 Panel B and Figures 3 and 8
Lalive et al ’06 RESTAT	PBD	DID	Total	0.17	0.03	Table 5 Rows 2-4 and Table 4 Rows 1-2
Lalive et al ’06 RESTAT	PBD	DID	Total	0.09	0.01	Table 5 Rows 2-4 and Table 4 Rows 1-2
Lalive et al ’15 AER	PBD	DID	Covered	1.4	0.26	Table 1 Columb 2 and Table 2 Columns 3 and 4

Lalive et al ’15 AER	PBD	DID	Total	0.58	0.07	Table 1 Columb 2 and Table 2 Columns 3 and 4
Landais ’15 AEJ:EP	PBD	RKD	Covered	1.35	0.68	Table 4 Column 2
Landais ’15 AEJ:EP	PBD	RKD	Total	0.33	0.43	Table 4 Column 2
Le Barbanchon ’16 Labour Econ.	PBD	RDD	Total	0.12	0.04	Table 7 Column 2 Rows 5
Le Barbanchon et al ’19 JPubEc	PBD	DID	Covered	0.36	0.04	Table 4 Column 4 Row 2, Table A1
Lichter & Schiprowski ’21 JPubEc	PBD	DID	Covered	0.53	0.06	Table 2 Column 6
Lichter & Schiprowski ’21 JPubEc	PBD	DID	Total	0.18	0.05	Table 2 Column 6
Moffitt ’85 JOE	PBD	Cross-sectional	Total	0.51	0.05	Table 3 Column 4
Nekoei & Weber ’17 AER	PBD	RDD	Total	0.51	0.12	Table 2 Column 1
Petrunyk & Pfeifer ’22 BER	PBD	DID	Total	0.33	0.06	Tabe 3 Column 4 bottom panel and Table 1 Column 2
Roed & Westlie ’12 JEEA	PBD	DID	Total	1.85	0.07	Table 4 Column 2
Schmieder et al ’12 QJE	PBD	RDD	Covered	0.61	0.02	Table W2 Column 1, Table 3 Column 1, Table W-1, Figure 1
Schmieder et al ’12 QJE	PBD	RDD	Total	0.09	0.01	Table W2 Column 1, Table 3 Column 1, Table W-1, Figure 1
Schmieder et al ’16 AER	PBD	RDD	Covered	0.52	0.01	Table 1 Columns 1 and 2
Schmieder et al ’16 AER	PBD	RDD	Total	0.13	0.03	Table 1 Columns 1 and 2
Van Ours & Vodopivec ’06 JOLE	PBD	DID	Total	0.42	0.12	Table 5 Row 1

Table 3.7: Included Studies: Disemployment Elasticities with Respect to Replacement Rate

Notes: Each row is a separate main estimate. The first column is the authors, publication year, and journal abbreviation. The second column indicates that all of the main estimates correspond to the elasticity with respect to the replacement rate. The third column contains mutually exhaustive categories for regression discontinuity design (RDD), regression kink design (RKD), difference-in-differences (DID), and cross-sectional variation with controls (cross-sectional). The fourth column describes whether the unemployment outcome is total nonemployment or covered unemployment. The fifth column is the elasticity of the unemployment duration outcome with respect to the benefit generosity parameter and the sixth column is its standard error. The seventh describes the calculation sources from the published paper.

Paper	**Policy**	**Design**	**UE meaure**	**Elasticity**	**SE**	**Source**
Arranz et al ’09 MyC	RR	Cross-sectional	Covered	0.44	0.13	Table 7 Column 4
Belzil ’01 JAE	RR	Cross-sectional	Total	0.25	0.08	Text (pg 634)
Blau & Robins ’86 JPubEc	RR	Cross-sectional	Total	0.18	0.04	Table 4 Column 1 and text (pg 188)
Blau & Robins ’86 JPubEc	RR	Cross-sectional	Total	0.26	0.1	Table 4 Column 1 and text (pg 188)
Card et al ’15 AEAPP	RR	RKD	Covered	0.21	0.07	Table 1 Column 1 Row 2 of NBER WP
Carling et al ’01 EJ	RR	DID	Total	1.97	0.97	Table 4 Column 4 Row DPOL and text (footnote 19)
Carling et al ’96 JPubEc	RR	Cross-sectional	Total	0.06	0.02	Text (pg 327) for elasticity, Table 3 Column 1 row for SE
Chetty ’08 JPE	RR	Cross-sectional	Total	0.41	0.21	Table 2 Column 1
Classen ’77 ILRR	RR	Cross-sectional	Covered	0.45	0.08	Table 2 Column 1
Eugter ’15 SJES	RR	DID	Total	0.39	0.19	Table 4 Column 1
Hunt ’85 JOLE	RR	DID	Total	-3.32	2.25	Table 2, Table 5 Columns 3 and 5, Table 7 Column 2, Text (pg 91-92)

Katz & Meyer ’90 JPubEc	RR	Cross-sectional	Covered	0.07	0.02	Table 3 Columns 1 and 2, Rows 2 and 5
Katz & Meyer ’90 QJE	RR	Cross-sectional	Covered	0.41	0.35	Table 4 Column 1 Row 3 and Table 1
Kroft & Notowidigdo ’16 RESTUD	RR	Cross-sectional	Total	0.47	0.25	Table 2 Column 1 Row 1
Lalive et al ’06 RESTAT	RR	DID	Total	0.17	0.04	Table 5 Rows 2-4 and Table 4 Rows 1-2
Landais ’15 AEJ:EP	RR	RKD	Covered	0.73	0.11	Table 4 Column 1
Landais ’15 AEJ:EP	RR	RKD	Total	0.32	0.08	Table 4 Column 1
Lee et al ’21 JOLE	RR	RKD	Covered	1.06	0.41	Table 4 Column 5
Meyer & Mok ’14 NTJ	RR	DID	Total	0.19	0.03	Table 3 Column 1
Moffitt ’85 JOE	RR	Cross-sectional	Total	0.66	0.01	Table 3 Column 4
Portugal & Addison ’90 ILRR	RR	Cross-sectional	Total	0.6	0.31	Table 3 Column 3 and Table 1
Poterba & Summers ’95 RESTAT	RR	Cross-sectional	Total	0.36	0.15	Table 4 for elasticity Table 3 Column 2 Row 4 for SE
Rebollo-Sanz ’20 JHR	RR	DID	Total	0.76	0.02	Table 3 Column 5, corrected simulation from text (pg 149)
Roed & Zhang ’03 EJ	RR	Cross-sectional	Total	0.95	0.17	Table 4 Column 1 Row 1
Roed & Zhang ’03 EJ	RR	Cross-sectional	Total	0.34	0.19	Table 4 Column 1 Row 1
Roed & Zhang ’05 EER	RR	Cross-sectional	Total	0.65	0.08	Table 3 Column 1 and Text (pg 1823)
Roed et al ’08 OEP	RR	Cross-sectional	Total	0.66	0.09	Table 2 Column 1
Roed et al ’08 OEP	RR	Cross-sectional	Total	0.37	0.07	Table 2 Column 1

Topel ’84 JOLE	RR	Cross-sectional	Total	0.53	0.25	Table 3 Column 3 Row 6 and Table 2
Topel ’84 JOLE	RR	Cross-sectional	Total	0.43	0.13	Table 3 Column 3 Row 6 and Table 2
Uusitalo & Verho ’10 Labour Econ.	RR	DID	Total	0.79	0.25	Table 3 Column 1 Row 1 and Text (pg 650)
Winter-Ebmer ’98 OBES	RR	DID	Total	0.07	0.02	Table 1

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