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DISCUSSION PAPER SERIES 24/24

Job Search, Unemployment Insurance, and Active Labor Market Policies

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OCTOBER 2024

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Handbook of Labor Economics Chapter: Job Search, Unemployment Insurance, and Active Labor Market Policies

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October 2024

Abstract

This chapter, prepared for the Handbook of Labor Economics, presents a comprehensive overview of how labor economists understand job search among the unemployed and how job search is shaped by unemployment insurance (UI) and active labor market policies (ALMP). It focuses on synthesizing key lessons from the empirical research of the last decade and presents recent novel theoretical developments.

Keywords: job search, unemployment insurance, active labor market policies

Word count: 46930

*Le Barbanchon: Bocconi University, CEPR, IGIER, IZA, J-PAL. Schmieder: Boston University, NBER, CEPR, IZA. Weber: Central European University, CEPR, IZA, RFBerlin.
We thank the editors, Christian Dustmann and Thomas Lemieux, for giving us the opportunity to contribute to the Handbook of Labor Economics. We thank the participants to the Handbook Workshop at RF Berlin for their helpful comments. We thank Francesco Armillei, David Card, Giulia Giupponi, Peter Ganong, Nathan Hendren, Camille Landais, Amelie Schiprowski, and Pauline Leung for providing feedback on early version of our chapter. Savannah Kochinke provided excellent research assistance.

Contents

1 Introduction and Background on UI

Unemployment is a fundamental feature of modern labor markets. It is widely acknowledged that unemployment is caused by search frictions, because it takes time for workers to find jobs and for employers to fill vacancies, as well as structural imbalances in the labor market driven by mismatch of skills or seasonal demand, among others. Figure 1 shows unemployment rates in 2022 among OECD countries. In this year the average unemployment rate in the OECD was 5% but there was large variation in unemployment rates across countries. Even if the unemployment rate was just above 2% in some countries, there is no country with zero unemployment.

In this chapter we present a comprehensive overview of how labor economists understand job search among the unemployed and how job search is shaped by unemployment insurance (UI) and active labor market policies (ALMP). The chapter is laid out in three parts: Section 2 presents the basic search model that has been the foundation of most of the literature on unemployment and job search. The section discusses key features and implications of this model and how they match several stylized facts from the empirical literature. The section focuses in particular on the recent empirical literature shedding new light on the micro-foundations of job search and how insights from this literature have sharpened our understanding of job search and led to refinements of the search model.

We then turn to labor market policies that aim at reducing the negative consequences of unemployment. In section 3 , we discuss how unemployment insurance provides benefits to unemployed workers that cover part of the loss in income. The design of unemployment insurance systems has been the subject of intensive research with many important theoretical and empirical results. We first introduce the Baily-Chetty approach to derive sufficient statistics formulas for welfare effects of altering the generosity of UI benefits, their level and potential duration. We then discuss the key results from the empirical literature that serve as the relevant inputs to these welfare formulas, with a particular emphasis on alternative ways to estimating the social value of UI. Next, we relate the Baily-Chetty formula to the concept of the Marginal Value of Public Funds. Last, we discuss how to incorporate in the design of UI effects on outcomes beyond unemployment duration (wages and separations), and macro effects. We consider a range of other policy-relevant questions related to the design of UI, such as its cyclicality, and its time path.

In many countries, passive unemployment insurance benefit policies are complemented by active labor market policies. These policies aim at activating unemployed workers by making their job search more effective, training them to upgrade their skills, or providing subsidized employment opportunities that integrate unemployed workers in the labor market. Section 4 of the chapter reviews the recent literature evaluating the effectiveness of ALMPs. We highlight exciting new developments in terms of program design, country coverage, target populations, evaluation strategies, and expansions in the comprehensiveness of evaluation studies such as cost benefit analyses and discussions of displacement effects. Overall, we summarize the new findings in this lively literature in 10 main lessons.

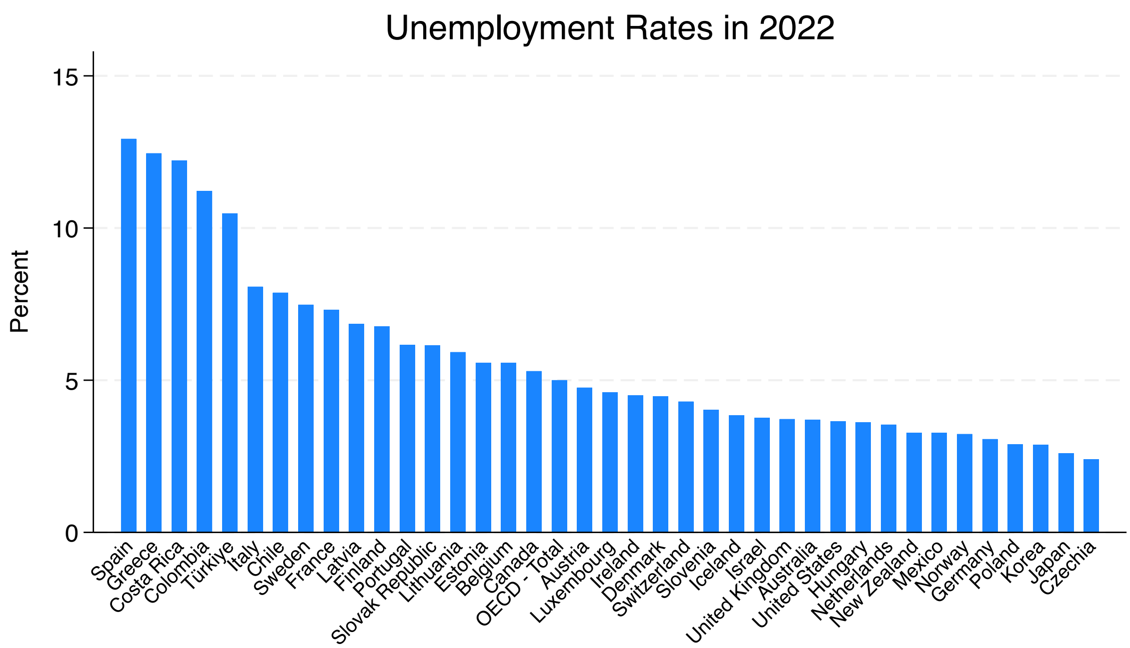
This chapter offers a detailed exploration of the three main topics: job search models, unemployment insurance, and active labor market policies. Each section is designed to stand independently, allowing readers to focus on any one topic in isolation. For a more comprehensive overview across all the three areas, readers may choose to focus on specific subsections in each part: for example, sections 2.2–2.4 in the job search section, sections 3.2–3.5 in the unemployment insurance section, and sections 4.3, 4.5–4.7 in the active labor market policies section.

Before turning to the main topics of this chapter, we provide some context by giving a short historical overview of the development of labor market policies and a cross-country comparison of labor market spending in the next subsection.

1.1 The Origin of Unemployment Insurance and Active Labor Market Policy

The first systems that insured workers against job loss were established with the industrialization in the 18th and 19th century. Insurance was organized at the city level by local guilds or trade unions and coverage was strictly restricted to members. The small scale made the financing of these early insurance systems vulnerable to large crises. In addition, coverage was very limited. Thus, municipalities, provinces, or even national governments increasingly stepped up to subsidize and organise the local systems. The adoption of national unemployment insurance systems was often triggered by large national or international shocks. The United States launched their UI system in 1935 in response to the Great Depression when

Figure 1: Unemployment Rates in 2022



Notes: The figure shows unemployment rates in 2022 for OECD countries. Source: OECD (2024), Unemployment rate (indicator). doi: 10.1787/52570002-en (Accessed on 03 July 2024).

the unemployment rate was above 20%. Canada followed in 1940 and many national systems in European countries go back to the early 20th century as well.

Eventually, the variety of historical insurances developed into specific types of national insurance systems. One important type is a purely tax-financed national UI system; an example is the UK system which was established in 1911. In another type, unemployment insurance features as a component of the contribution-based social insurance system. Within this system Germany first established health insurance in 1883, followed by pension and invalidity insurance in 1889. In 1927 a national unemployment insurance was added to the system. Compared to tax-financed systems, the philosophy of social insurance systems features a stronger connection between contributions and benefits and leaves less room for redistribution. A third group of countries operate a combination of tax and contribution-financed systems, such as the US system. But there are also national systems which are not centrally organized. In four Nordic countries, Denmark, Finland, Iceland, and Sweden, unions operate unemployment insurance through government subsidized UI funds (Holmlund and Lundborg, 1999). In these countries UI membership is voluntary, while centrally organized systems typically mandate membership at least for private sector workers. In some developing countries the insurance component of UI is replaced by a savings based system.

Today unemployment insurance is available in all OECD countries and in a range of other countries. Unemployment insurance is often complemented by a severance pay (SP) system, which provides lump sum payments to displaced workers. Gerard et al. (2024) review UI and SP systems worldwide and report that while higher income countries are more likely to have UI, severance pay systems are more universally available across all types of countries. Savings based systems, in comparison, are rare. Comparisons by program generosity show that UI is more generous in terms of benefit levels and potential benefit durations in high income countries, while SP offers a broader coverage in low income countries. In countries with high levels of informality, coverage with UI is naturally low and accordingly the re-distributive purpose of the programs is limited.

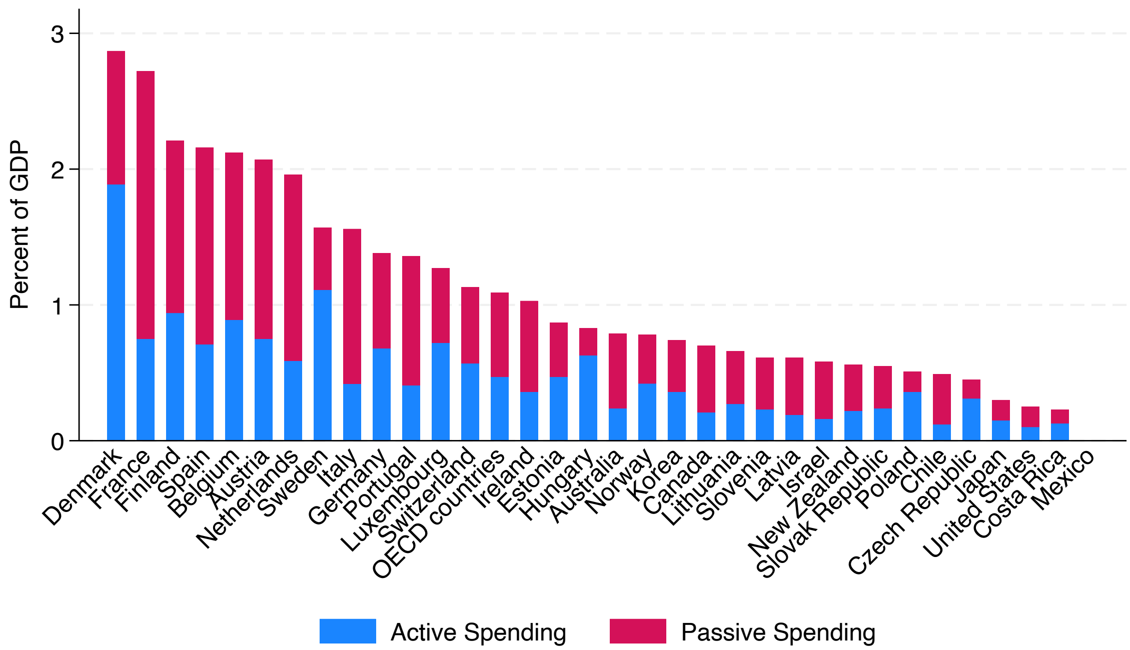
The earliest ALMPs were developed in response to severe market failures. During periods of high and persistent unemployment positions in the public sector were created to employ workers who could not find jobs in the private sector. In a push to modernize the economy, workers with obsolete skills were trained to acquire new skills that were in high demand. Up until the early 1970's most ALMP programs were either training programs or public sector employment programs. At this time the first job search assistance programs emerged as a low-cost alternative with the main aim of moving benefit recipients back to work. In the US public opinion changed from the view that benefit recipients needed to get jobs with the help of public sector employment programs because none were available on the private market, or that they needed to get training because their skills were obsolete to the view that benefit recipients needed to quit stalling and get to work. In the US, this development led to the 1996 Welfare reform along with a major shift in the focus of ALMPs towards job search assistance. European countries followed with some delay. By the early 2000's job search assistance programs were strongly expanded at the cost of training and especially public sector employment programs which were greatly reduced.

1.2 Unemployment Insurance Today

To finance labor market policies governments incur considerable fiscal costs, which are summarized in Figure 2. The figure compares spending on passive and active labor market policies as a percentage of GDP across OECD countries in 2018, the last year with pre-pandemic data. Total spendings on labor market policies range from zero in Mexico to almost 3% of GDP in Denmark and the OECD average is 1.1%. The majority of countries spend a higher share of the budget on passive UI benefit policies than on active labor market policies. But there are some notable exceptions. Denmark, Sweden and Finland, countries with voluntary unemployment insurance systems also have the highest shares of active labor market expenditures, spending about 1% or more of GDP or more on active programs. The expenditures by unemployed worker also vary widely across countries, which can be seen from a comparison of Figures 1 and 2; countries with the highest unemployment rates are not necessarily those with the highest expenditures on labor market policies.

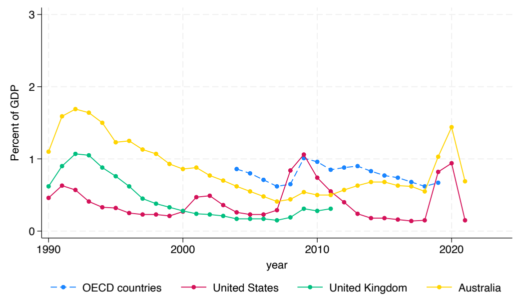
Figure 3 shows the development of active and passive labor market spending as percentage of GDP for selected OECD countries over the last 30 years, split by region. Spending on UI benefits, shown in panels (a) and (b), tend to follow the business cycle, a pattern which is most pronounced in the US where the system features large benefit extensions during times of high unemployment. The spending paths of other countries also indicate policy shifts, for example, passive spending

Figure 2: Labor Market Policy Spending in 2018

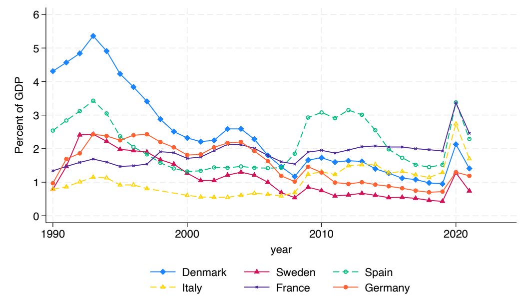


Notes: The figure shows spending on active and passive labor market policies as a share of GDP in 2018 for OECD countries. Source: OECD 2023.

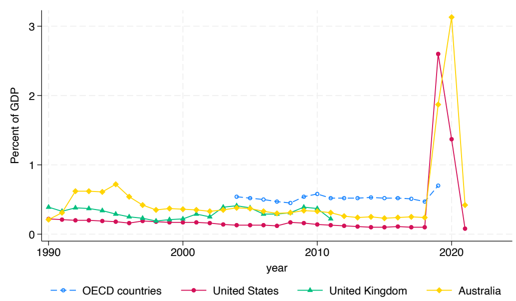
Figure 3: Spending for Passive and Active Labor Market Policy



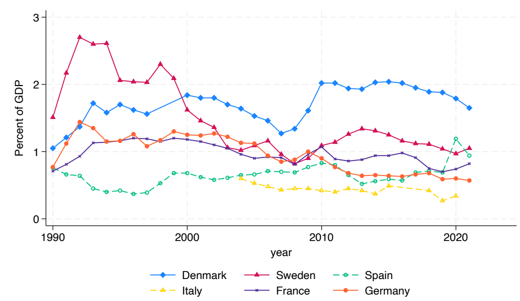
(a) Passive Spending Anglican Countries



(b) Passive Spending EU countries



(c) Active Spending Anglican Countries



(d) Active Spending EU countries

Notes: The figure shows spending on active labor market policies for selected countries. Source OECD 2023.

in Denmark, Sweden, and Germany show declines over the 30 year period. Across European country regions, spending on passive labor market policy seems to have converged to lower levels over time.

Interestingly, spending on active labor market policies shows less cyclicality or variation across countries and over time; it seems to be mostly driven by policy regimes. Generally, spending on active labor market policies is substantially lower in Anglican countries than in Europe, especially Northern and Western Europe. Sweden used to be leading in active spending in the 1990s but has cut spending tremendously over the 2000s. Denmark, on the other hand, has slightly increased their high level of spending and is leading in Europe since the 2010s. The COVID-19 pandemic led to major disruptions in spending patterns which, however, will be temporary.

2 Micro-foundations of Job Search among the Unemployed

This section explores recent developments seeking to understand the micro-foundations of job search. We first lay out the basic partial equilibrium job search model featuring search effort and reservation wages and describe its core predictions. We then present several stylized facts from administrative data on UI, job-finding rates, and reemployment wages that have emerged from the literature. Next, we describe new types of empirical evidence that have emerged in recent years. Based on this new evidence, we then discuss implications for our understanding of job search. We will show what this evidence reveals about the different channels in the basic search model, as well as discuss various refinements to the job search model that have been proposed in the literature and the degree to which the existing evidence supports these refinements.

2.1 A Brief History of Job Search Theory

Early analyses of the labor market relied on static demand and supply models. Labor demand in such models was derived from profit-maximizing firms, while labor supply was derived from individual utility maximization in the presence of a, often nonlinear, budget set. This modeling framework proved powerful for understanding many labor market phenomena. In particular, it provides many insights for understanding the impact of various tax and transfer programs on labor supply. However, these models also feature efficient labor markets and no involuntary unemployment, which seems at odds with obvious frictions in the labor market as well as pervasive unemployment in the actual world.

George Stigler was the first economist to develop a theory for understanding frictions in the search process in 2 seminal papers (Stigler, 1961, 1962). In Stigler's model workers have imperfect knowledge about the available jobs and have to shop around to find the best job. The question then is how many possible jobs the worker should sample given some costly search process. Since workers compete for job offers, some workers may not receive an offer and remain unemployed.

Building on this static formulation of job search several papers developed the first dynamic search models: McCall (1970), Mortensen (1970), and Gronau (1971). In these papers, workers receive job offers randomly drawn from a wage offer distribution. The arrival rate of offers is exogenous, so the only choice for the worker is whether or not to accept a job. These models give rise to a reservation wage, which is defined as the lowest wage a worker is willing to accept. Thus the optimal search strategy of a worker is fully described by her reservation wage at any given point in time.

The 1970s saw many refinements of this basic job search / reservation wage model. A particularly important extension was proposed by Lippman and McCall (1976), who extended the reservation wage model by letting workers choose how much job search effort to exert.

By the time Dale Mortensen wrote the chapter on job search for the first edition of the Handbook of Labor Economics (Mortensen, 1986), the theory of job search was quite developed and well on its way to conquering the hearts and minds of labor and macro-economists.[1]

Indeed, in addition to further developments of the theory, the 1980s and 1990s job search became an important area for empirical analysis. Economists used rich new datasets to study the key predictions of the job search models. Empirically estimating job search models raised many difficult challenges. Economists had to deal with omitted variable bias, incomplete data[2], and identification challenges[3].

The 1980s and 1990s also saw increased interest in analyzing UI policy and some of the very first papers using administrative data in labor economics (Moffitt, 1985; Meyer, 1990; Katz and Meyer, 1990). See Devine and Kiefer (1993) for a good overview of this earlier empirical literature.

While in 1986, Mortensen still stated that "it is too soon for either an 'Oscar' or knighthood", no one was surprised when, in 2010, Dale Mortensen and Peter Diamond were awarded the Nobel Prize in Economics for their contributions to the development of job search theory and its remarkable success in shaping modern economic thinking.

[1]For a review of the huge success of job search theory in macro economics see Rogerson and Shimer (2011).

[2]Data typically includes incomplete unemployment spells, i.e. observations where the start of the unemployment spell is observed but the end date is censored.

[3]Such as how to identify the wage offer distribution given that offers below the reservation wage are typically not observed Flinn and Heckman (1982).

2.2 The Basic Job Search Model

In this subsection, we lay out a general version of the partial equilibrium job search model that has become the workhorse model in the study of unemployment and UI. The model focuses on the job search decision of an unemployed worker, who chooses search effort as well as whether or not to accept a job offer paying a certain wage.[4] The model is quite flexible and allows for a changing environment throughout the unemployment spell. For example, unemployment insurance (UI) benefits may be paid only for a finite period of time or the wage offer distribution may change with unemployment duration, for instance, due to skill depreciation.

The model is set in discrete time and starts when a worker enters unemployment in period $t = 0$. In each period t the worker chooses search effort e_t. The level of effort determines the probability of receiving a job offer s_t via the search production function $f(.)$, such that: $s_t = f(e_t)$. In any given period a worker can receive at most one job offer.[5] The cost of job search is given as $c(e_t)$. We assume that $c(0) = f(0) = 0, c'(e) > 0, f'(e) > 0$ and that $c(f^{-1}(.))$ is convex. If a worker receives a job offer, it comes with a wage w, which is drawn from a wage offer distribution with CDF: $F_t(w)$. When not working, workers receive income (such as UI benefits or home production) b_t. The future is discounted at the discount factor δ.

Flow utility from consumption when unemployed is given as $u(b_t)$, when employed the flow utility is given as $v(w)$. Using different utility functions for employment and unemployment allows for differences in consumption patterns as well as potential psychological costs of unemployment. This notation is particularly common in the optimal UI literature. Both $u(.)$ and $v(.)$ are assumed to be increasing, differentiable and concave.

Once a job is accepted, we assume an individual will keep it forever. The value of accepting a job in period $t + 1$ that pays a wage w is therefore given as:

$$V^E_{t+1}(w) = v(w) + \delta V^E_{t+2}(w)$$

Since the environment is constant at that point $V^E_{t+1}(w) = V^E_{t+2}(w)$ and we can

[4]The earliest model featuring endogenous search intensity and reservation wages is Lippman and McCall (1976).

[5]It is straighforward to model the possibility of multiple job offers per period (Mortensen, 1986) but this comes at the cost of more cumbersome notation.

simplify the value of employment to:

$$V_{t+1}^{E}(w) = \frac{v\,(w)}{1-\delta} \tag{1}$$

The value of unemployment is given as the flow utility from UI benefits minus the cost of search effort, plus the discounted expected value of receiving a job offer:

$$V_t^U = \max_{e_t}\, u\,(b_t) - c(e_t) + \delta \left(f(e_t) \int \max\left(V_{t+1}^E(w), V_{t+1}^U\right) dF_t(w) + (1 - f(e_t)) V_{t+1}^U \right) \tag{2}$$

Given that $s_t = f(e_t)$ is monotonic, we can rewrite this value function in terms of s_t as:

$$V_t^U = \max_{s_t}\, u\,(b_t) - \widetilde{c}(s_t) + \delta \left(s_t \int \max\left(V_{t+1}^E(w), V_{t+1}^U\right) dF_t(w) + (1 - s_t) V_{t+1}^U \right) \tag{3}$$

where $\tilde{c}(s_t)$ is the composite of the actual cost of effort and the inverse of the search production function: $\tilde{c}(s_t) = c(f^{-1}(s_t))$. This reformulation implies that the problem can be solved as if the optimization is with respect to the probability of exiting unemployment s_t. Most of the literature does not rely on data on actual search effort and therefore normalizing search effort to be equal to the job finding probability comes without loss and simplifies the notation. We will use this normalization here as well to derive the key implications of the model, but return to the more general formulation when discussing evidence on actual search effort e_t in Section 2.4 and 2.5.

The problem satisfies the so-called reservation wage property, which simply means that the value of employment is increasing in w and therefore there is a unique wage such that all offers above it are accepted. We call this unique wage the reservation wage ϕ_{t+1} for jobs that start in period $t+1$. The value of unemployment can then be written as:

$$V_t^U = \max_{s_t, \phi_{t+1}}\, u\,(b_t) - \widetilde{c}(s_t) + \delta \left(s_t \int_{\phi_{t+1}}^{\infty} V_{t+1}^E(w) - V_{t+1}^U\, dF_t(w) + V_{t+1}^U \right) \tag{4}$$

Any wage such that $V_{t+1}^E(w) \geq V_{t+1}^U$ is accepted, therefore the reservation wage ϕ_{t+1} is the lowest such wage and has to satisfy $V_{t+1}^E(\phi_{t+1}) = V_{t+1}^U$. Using Equation (1) we get:

$$v(\phi_{t+1}) = (1-\delta)V_{t+1}^{U} \tag{5}$$

First Order Conditions: Given the reservation wage, the first-order condition determining optimal search effort is:

$$\tilde{c}'(s_t^*) = \delta \left(\int_{\phi_{t+1}}^{\infty} V_{t+1}^{E}(w) - V_{t+1}^{U} \, dF_t(w) \right)$$

or

$$s_t^* = \tilde{c}'^{-1} \left(\delta \left(\int_{\phi_{t+1}}^{\infty} V_{t+1}^{E}(w) - V_{t+1}^{U} \; dF_t(w) \right) \right) \tag{6}$$

Using the fact that: $v(w) = (1-\delta)V_{t+1}^{E}$ and $v(\phi_{t+1}) = (1-\delta)V_{t+1}^{U}$ we can write:

$$s_t^* = \tilde{c}'^{-1} \left(\frac{\delta}{1-\delta} \left(\int_{\phi_{t+1}}^{\infty} v(w) - v(\phi_{t+1}) \, dF_t(w) \right) \right) \tag{7}$$

Given the optimal level of search effort in period t this will pin down the reservation wage in t.
Combining (5) and (4) we get an expression for the reservation wage in period t, ϕ_t given optimal search s_t^* in that period and the reservation wage ϕ_{t+1}:

$$v(\phi_t) = (1-\delta) \left(u\left(b_t\right) - \tilde{c}(s_t^*) + \delta \left(s_t^* \int_{\phi_{t+1}}^{\infty} V_{t+1}^{E}(w) - V_{t+1}^{U} \, dF_t(w) + V_{t+1}^{U} \right) \right) \tag{8}$$

Using equation (5) and (1), we get:

$$v(\phi_t) = (1-\delta)\left(u\left(b_t\right) - \tilde{c}(s_t^*)\right) + \delta v(\phi_{t+1}) + \delta \left(s_t \int_{\phi_{t+1}}^{\infty} v(w) - v(\phi_{t+1}) \; dF_t(w) \right) \tag{9}$$

2.2.1 Steady State

Suppose that after some period S we reach a stationary environment, where the wage offer distribution F_S and the benefit level b_S remain constant for all future periods $t \geq S$. In this stationary environment it has to be the case that optimal search s_S^* and the reservation wage $\phi_S = \phi_t = \phi_{t+1}$ is constant. Using this, we can write the first order conditions in the steady state as:

$$s_S^* = \tilde{c}'^{-1}\left(\frac{\delta}{1-\delta}\left(\int_{\phi_S}^{\infty} v(w) - v(\phi_S)\ dF(w)\right)\right) \tag{10}$$

and

$$v(\phi_S) = (1-\delta)\left(u\left(b_S\right) - \tilde{c}(s_S^*)\right) + \delta v(\phi_S) + \delta\left(s_S^* \int_{\phi_S}^{\infty} v(w) - v(\phi_S)\, dF(w)\right)$$

We can rearrange this to:

$$v(\phi_S) = u(b_S) - \tilde{c}(s_S^*) + \frac{\delta}{1-\delta}\left(s_S^* \int_{\phi_S}^{\infty} v(w) - v(\phi_S)\, dF(w)\right) \tag{11}$$

Note that equations (10) and (11) form a system of equations which, given the model parameters, has 2 unkowns: ϕ_S and s_S^*.
To fully solve the system, one first solves the steady state system for ϕ_S and s_S^* and then uses backwards induction to find ϕ_t and s_t^* for all prior periods.

2.2.2 Empirical Moments: Hazard Rate and Reemployment Wage

The hazard rate h_t in period t (that is the number of unemployment spells ending in period t conditional on being unemployed for at least t periods) is given as:

$$h_t = s_t\left(1 - F_t(\phi_{t+1})\right) \tag{12}$$

Denote the expected log reemployment wage of individuals who leave unemployment at the end of period t as $w_t^e \equiv E[\ln w | w \geq \phi_{t+1}]$. Given the model, this is given as:

$$w_t^e = E[\ln w | w \geq \phi_{t+1}] = \frac{\int_{\phi_{t+1}}^{\infty} \ln w\, d\, F_t(w)}{1 - F_t(\phi_{t+1})} \tag{13}$$

Since the hazard rate h_t and the reemployment wage w_t^e are empirically observable, we can compare those to direct estimates from the data. The expected unemployment duration D is

$$D = \sum_t S_t$$

Where S_t is the survival function which is related to the hazard as: $S_t = \prod_{k=1}^{t}(1 - h_k)$.

2.2.3 Search Effort and Reservation Wages throughout the Unemployment Spell

Single Type Consider the first-order condition for search effort in equation (6). How search effort evolves throughout the unemployment spell will depend on how the value of unemployment and the value of employment evolve. As an example, let's consider a case where the only source of non-stationarity is that workers exhaust UI benefits in period P. In this case, V_t^U will fall throughout the unemployment spell until the UI exhaustion point. Equation (6) implies that search effort will therefore increase until benefits are exhausted. Similarly, equation (8) implies that reservation wages decrease throughout the unemployment spell. A falling reservation wage and increasing search effort, both contribute to an exit hazard that is increasing throughout the unemployment spell. Finally, due to falling reservation wages, reemployment wages will fall throughout the unemployment spell.

Multiple Types These predictions of the model are for a single individual, or a homogeneous group (a 'type') of individuals. With heterogeneous individuals ('multiple types'), the aggregate search effort, reservation wage and exit hazard at time t is the average value for those individuals who are still unemployed at time t. Suppose for example that there are N different types j of individuals who have different hazard rates h_{jt} and reservation wages ϕ_{jt} and where the share of type j among all workers entering UI is p_j. In this case, the aggregate hazard is the weighted average of the type-specific hazards where the weights are the survival function multiplied with the type share:

$$h_t^{agg} = \sum_j h_{jt} \frac{S_{jt} p_j}{\sum_j S_{jt} p_j}$$

Similarly, the aggregate reemployment wage is the average reemployment wage among the people who find a job in that period and is given as:

$$w_t^{agg} = \sum_j w_{jt}^e \frac{h_{jt} S_{jt} p_j}{\sum_j h_{jt} S_{jt} p_j}$$

The aggregate hazard/reemployment wage will change throughout the spell, both because the hazard/wage of each type will change with unemployment duration, but also because the fraction of who is still unemployed will change differentially by type.

To illustrate how the aggregation of multiple types can lead to aggregate job finding hazards and reemployment wage paths that are starkly different from the within person paths, we simulate a version of the basic job search model with 2 types. The types differ by 1 parameter: a scaling parameter for the cost of job search.[6] Type 1 has a lower cost of job search than type 2.

Figure 4 a) shows the simulated search effort for the two types in an environment with potential benefit duration of $P = 12$ months. For both types, effort increases up to the exhaustion point and then becomes flat. Type 1 has a lower cost of job search and thus both a higher baseline effort as well as a sharper increase. Panel (b) shows the log reservation wage for the two types. For both types, reservation wages fall for the first 12 months and then stay flat after UI is exhausted. Panel (c) shows the corresponding exit hazard. In this calibration the mean of the wage offer distribution is close to the reservation wage for Type 1, so that type 1 workers reject about half of all job offers and the exit hazard is about half the search effort. By contrast, type 2's reservation wage is much lower and type 2 workers accept almost all job offers. Panel (d) shows the evolution of the log reemployment wage for the 2 types. Since type 1 has a higher reservation wage than type 2, the average accepted wage is also substantially higher. Panel (e) shows the corresponding survival rates for both types. Since type 1 workers have lower search cost, and a higher hazard, they exit the pool of unemployed quickly and the survival rate drops rapidly. Type 2 workers have a much lower exit hazard and thus stay unemployed longer. Correspondingly, Panel (f) shows how the type shares among the still-unemployed change throughout the spell. While both types initially make up 50 percent of the unemployed, the share of type 1 workers falls fast, while type 2 workers make up

[6] We assume a log utility function $u(.) = ln(.)$, a log normal wage offer distribution $ln(w)\ N(\mu_j, \sigma)$ and a cost of job search function $\tilde{c}(s) = k_j \frac{s^{1+\gamma}}{1+\gamma}$.

an ever lasting share of the remaining unemployed. These changes in the type shares are the key driver for the aggregate hazard rate shown in Panel (g): Initially the aggregate hazard rate is pulled up by the high job finding rates of type 1 workers. As these low-cost workers find jobs, the high cost type 2 workers remain and the aggregate hazard approaches the low hazard of type 2. The aggregate hazard also exhibits a small spike at the UI exhaustion point ($P = 12$). The spike is mostly driven by type 1 workers who still make up about 20 percent of the unemployed at the exhaustion point and show a sharply increasing exit hazard leading up to UI exhaustion. Panel (f) shows the aggregate reemployment wage. While reemployment wages fall within both types, the aggregate reemployement wage falls much faster as the type 1 workers, who generate many job offers and only take high paying jobs, exit earlier and the pool of unemployed consists increasingly of the high cost type 2 workers who accept even very low paying jobs.

Note that the aggregate hazard thus shows a very different time path than the type-specific hazard rates. This shows how dynamic selection, that is changes in the composition of workers who remain unemployed throughout the spell, can have a first-order effect on observed hazard rates. Similarly, the expected reemployment wage path of individual worker types may be very different than the aggregate reemployment wage path.

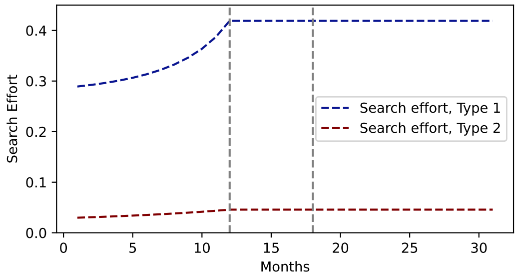
2.2.4 The Effects of UI on Job Finding and Reemployment Wages

Next, we consider what the model predicts for the effects of UI on job search outcomes.

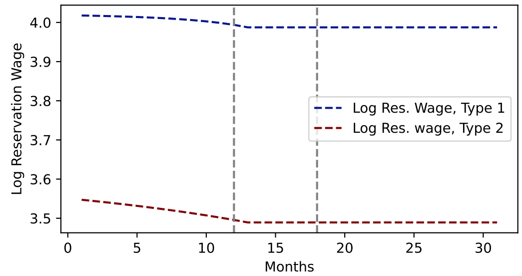
Static Environment: The static search effort and reservation wage are solutions of the system of two equations (10) and (11). First, Equation (10) defines search effort as a function of the reservation wage without any dependence on the UI benefit b. Recall that Equation (10) writes (omitting the S subscript):

$$\tilde{c}'(s^*) = \frac{\delta}{1-\delta}\left(\int_{\phi}^{\infty} v(w) - v(\phi)\ dF(w)\right) \tag{14}$$

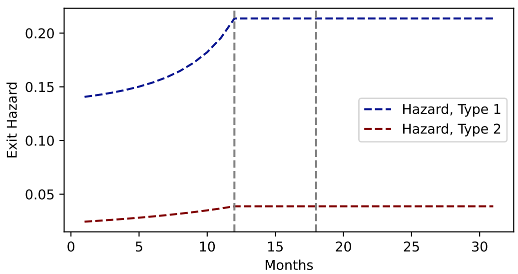
Figure 4: Simulation of Basic Job Search Model with 2 Types



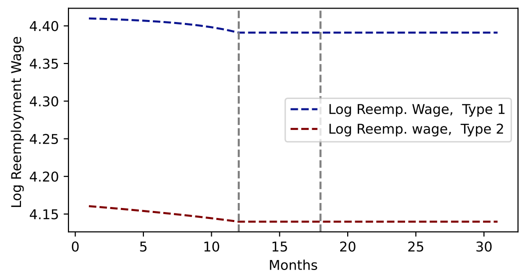
(a) Search effort



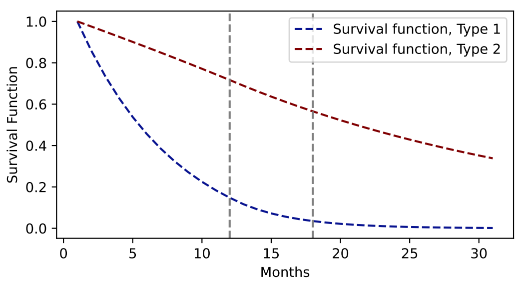
(b) Log Reservation Wage



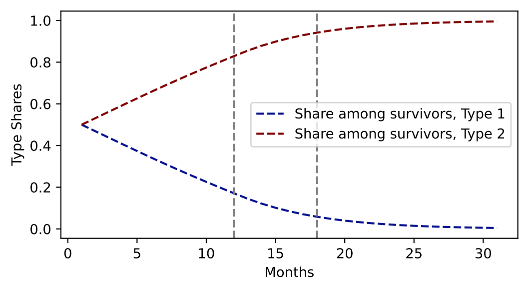
(c) Exit Hazard



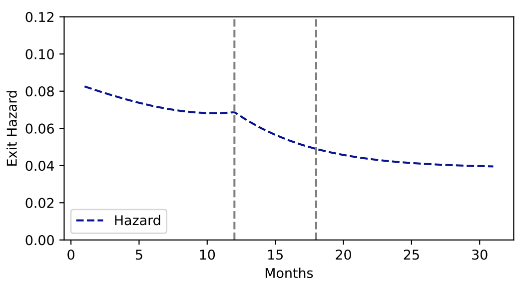
(d) Log Reemployment Wage



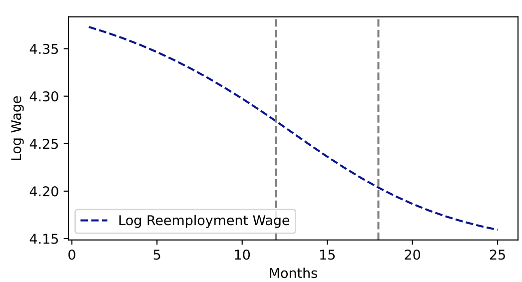
(e) Survival Function



(f) Share of Types



(g) Aggregate Exit Hazard



(h) Agg. Log Reemployment wage

Notes: The figure shows simulations of the basic model with 2 types. The types differ by the cost of job search.

Standard differentiation of the above equation (using Leibniz rule) yields:

$$\begin{aligned}
\frac{\partial s^*}{\partial \phi} &= \frac{1}{\tilde{c}''(s^*)}\frac{\delta}{1-\delta}\int_{\phi}^{\infty}\frac{\partial}{\partial \phi}\left[v(w)-v(\phi)\right]\; dF(w) \\
&= \frac{-v'(\phi)}{\tilde{c}''(s^*)}\frac{\delta}{1-\delta}\left(1-F(\phi)\right) \\
&< 0, \qquad (15)
\end{aligned}$$

where the last inequality stems from the convexity of the composite cost function and and from positive marginal utility.
We can now differentiate the second Equation (11) to obtain the effect of UI benefits on reservation wage. Recall that Equation (11) writes:

$$v(\phi) = u(b) - c(s^*) + \frac{\delta}{1-\delta}\left(s^*\int_{\phi}^{\infty} v(w) - v(\phi)\, dF(w)\right) \qquad (16)$$

The differentiation is simplified as the contribution of changes in search effort (ds) disappears because of the first order condition on search effort. We obtain:

$$v'(\phi)d\phi = u'(b)db - \frac{\delta}{1-\delta}v'(\phi)(1-F(\phi))d\phi$$

After some manipulation, it writes:

$$\frac{\partial \phi}{\partial b} = -\frac{u'(b)}{v'(\phi) + \frac{\delta}{1-\delta}v'(\phi)(1-F(\phi))} > 0$$

Putting the results together we get:

$$\frac{ds^*}{db} = \frac{\partial s^*}{\partial \phi}\frac{\partial \phi}{\partial b} < 0$$

Thus higher UI benefits increase the reservation wage and decrease search effort. Combined with equation (12) we get that

$$\frac{dh}{db} < 0 \qquad (17)$$

In a static environment, the expected duration of an unemployment spell can be written as: $D = \frac{1}{h} = \frac{1}{s^*(1-F(\phi)}$ and therefore $\frac{dD}{db} > 0$, i.e. more generous UI benefits lead to longer unemployment durations.

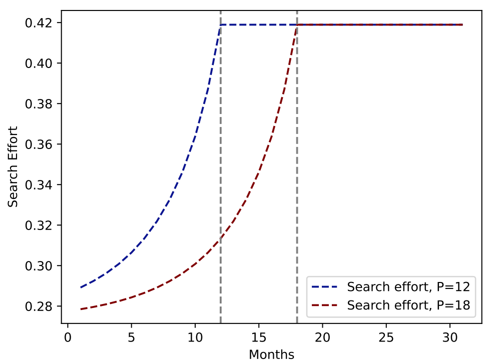
Non-stationary Environment: To derive the comparative statics in the nonstationary environment, consider equation (6). Consider an increase in UI benefits b. Clearly, this does not affect the value of employment but raises the value of unemployment V^U_{t+1}. Furthermore for all t, an increase in V^U_{t+1} will decrease search effort so that $\frac{ds^*_t}{db} < 0$. Similarly equation (4) implies that $\frac{d\phi_t}{db} < 0$. Since both the decrease in search effort and increase in the reservation wage lead to longer unemployment durations we have $\frac{dD}{db} > 0$. Similarly one can derive $\frac{dD}{dP} > 0$.

To illustrate the model predictions for the effects of UI, we simulate an extension of potential benefit durations in the model above. Figure 5 shows how job search responds when PBD is extended from 12 to 18 months. Panel (a) shows how search effort evolves for a single type of worker. Increasing PBD from 12 to 18 months shifts the search effort path to the right, so that search effort is lower at every duration less than 18 months. On the flip side, the reservation wage increases at all durations until 18th months. Both the change in the reservation wage and the search effort lead to a lower hazard rate under the more generous UI regime until benefit exhaustion (Panel c), while the reemployment wage path is shifted upwards (Panel d).

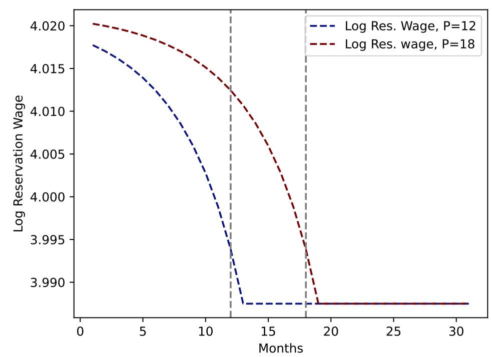
To summarize, the search model makes a few key predictions. Within homogenous types, an increase in the generosity of UI benefits decreases the job finding rate and weakly increases reservation and reemployment wages conditional on unemployment duration.

Importantly while the effect of UI on nonemployment durations is clearly positive, the effect on average reemployment wages is less clear. On the one hand more generous UI leads to higher reservation wages, but on the other hand it leads to, on average, longer unemployment durations and thus workers finding jobs later in the spell, when reemployment wages are lower on average, either due to lower reservation wages or changes in the wage offer distribution throughout the unemployment spell. In the appendix section A we show that if the exhaustion of UI benefits is the only source of non-stationarity, then the positive effect of higher reservation wages dominates the effect of longer unemployment durations and average reemployment wages are strictly increasing in PBD. However, suppose there are other sources of non-stationarity, such as skill depreciation that leads to

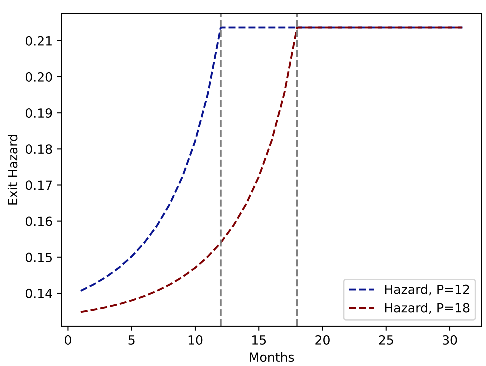
Figure 5: Simulating the Effects of a UI Extension



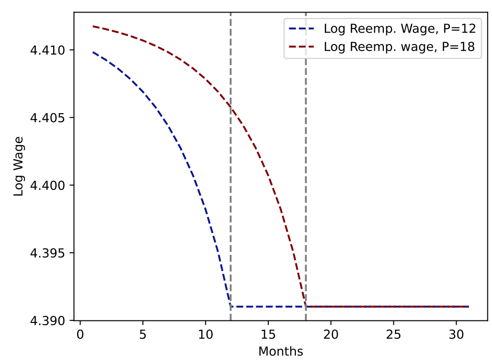
(a) Search effort



(b) Res. Wage



(c) Hazard



(d) Reemp. Wage

Notes: The figure shows simulations of the model in 4 when changing the potential UI benefit duration from 12 to 18 months. Panels (a) to (d) show how search effort, the reservation wage, the exit hazard and the reemployment wage change for Type 1 workers. Panels (e) and (f) show how the aggregate exit hazard and reemployment wage respond to the PBD increase. Panels (e) and (f) also overlay the empirical exit hazards and reemployment wages for Germany from Schmieder et al. (2016), which were used to calibrate the model here.

lower wage offers in later periods. In that case, this latter channel may dominate and lead to a negative effect of PBD on reemployment wages (see, for example, Schmieder et al. 2016).

When aggregating over multiple types, the model predicts that more generous UI increases nonemployment duration, but dynamic selection means that the job finding hazard (reemployment wages) do not necessarily decrease (increase) for all unemployment durations since the type composition is changing.

2.3 Evidence from Job Finding Rates and Re-employment Wages

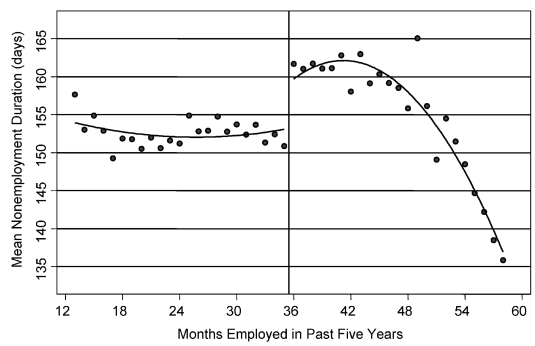
Over the two decades, the literature has documented a set of stylized findings with respect to job search outcomes and how they relate to UI benefits. These findings rely on a combination of high quality administrative datasets, with clean and transparent empirical strategies to estimate the effects of UI on these outcomes. Here we will lay out these stylized findings, highlighting a few selected examples from the literature. The examples focus on regression discontinuity designs, which are straightforward to understand and provide transparent visual evidence, but by no means do they represent and exhaustive list.

We focus here on highlighting some stylized qualitative findings, without discussing the magnitudes of the effects. We will return to the magnitudes and provide a more systematic overview in Section 3, where we discuss the effects of UI policy and their implications for UI policy design.

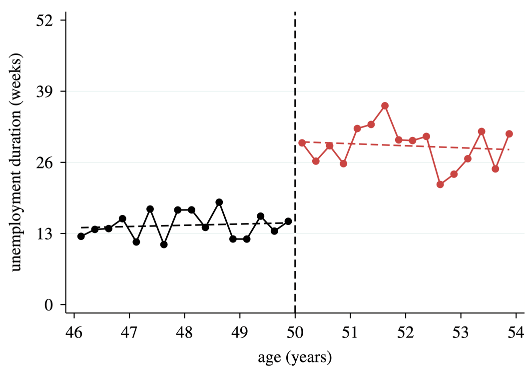
2.3.1 The Effects of UI on Unemployment Duration

As discussed above, the one robust prediction of the basic job search model, even with heterogeneous types is that an increase in UI generosity leads to longer unemployment durations. While this had been tested and confirmed in empirical work since the 1970s, modern evidence based on administrative data and regression discontinuity designs made this point extremely convincingly and clear. Figure 6 shows four examples of papers that estimated the effect of a UI benefit extension on nonemployment durations. Panel (a) and (b) provide evidence from Austria and represent, to our knowledge, the 2 first RD designs in the UI literature. Card et al. (2007) shows the effect of a PBD increase from 20 to 30 weeks at a work history cutoff (number of months employed in previous 5 years) and documents a clear jump in nonemployment duration by about 7 days. Lalive (2008) shows the effect of a particularly large increase in PBD from 39 to 209 weeks at an age cutoff (age 50) and a corresponding doubling of unemployment durations. Panel (c) shows the effect of increasing PBD from 12 to 18 months at an age 42 cutoff and from 18 to 22 months at an age 44 cutoff in Germany (Schmieder et al., 2016) with clear upward jumps in nonemployment duration. Panel (d) shows the effect of being ineligible for UI (i.e. PBD = 0 weeks) to the left of the cutoff vs. being eligible to a PBD of 26 weeks to the right of the cutoff, also showing a clear upward jump in nonemployment durations.

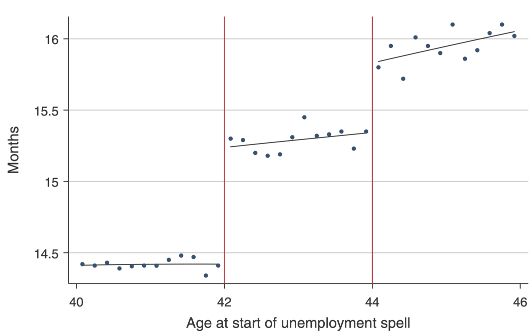
Figure 6: The Effects of UI on Nonemployment Duration: Examples



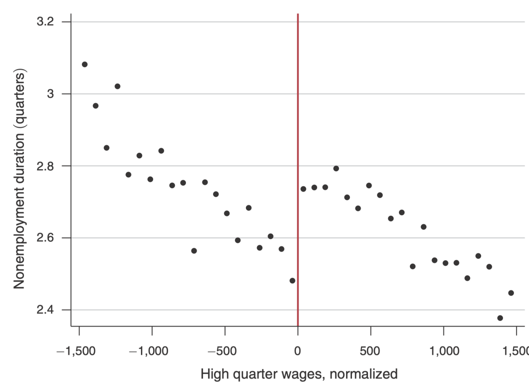
(a) Card, Chetty, and Weber 2007, PBD=20 vs. 30 weeks



(b) Lalive 2008, PBD=39 vs. 209 weeks



(c) Schmieder, von Wachter, and Bender 2016, PBD=12 vs. 18 months



(d) Leung and O'Leary 2020, PBD=0 vs.26 weeks

Notes: Panel a) replicates Figure 8a from Card et al. (2007), Panel b) replicates Figure 2 from Lalive (2008), Panel c) replicates Figure 6 from Leung and O'Leary (2020), and Panel d) replicates Figure 2b from Schmieder et al. (2016).

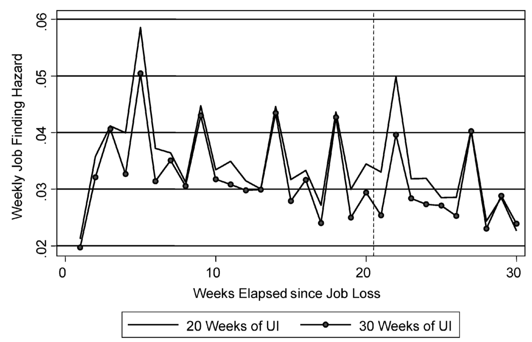
Overall, the evidence that increases in PBDs lead to longer nonemployment durations is extremely strong. A large number of high quality papers across a wide range of countries have produced similar estimates using RD designs (e.g., Centeno and Novo, 2009 for Portugal, Huang and Yang, 2021 for Taiwan, or Gerard and Naritomi, 2021 for Brazil). There are also many papers providing clean evidence using Difference-in-Differences designs or, more recently, Regression Kink Designs (especially for the effects of UI benefit levels).

2.3.2 The Effects of UI the Hazard Rate

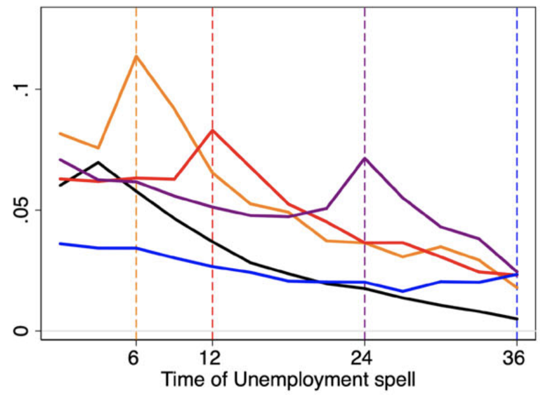
The advent of high frequency administrative data, has allowed economists to obtain non-parametric estimates of the job finding hazards among unemployed individuals. The earliest estimates along those lines were Katz and Meyer (1990) and Meyer (1990). More recently, papers have provided estimates of how hazard rates shift in response to UI PBD changes.

Figure 7 shows several such examples. Panel (a), from Card et al. (2007), presents the first figure in the literature that shows how the weekly job finding hazard shifts when PBD is extended (here from 20 to 30 weeks). The weekly hazard shows spikes every 4 weeks, likely because many jobs end at the end of the month and start on the 1st of the month. Furthermore, the figure clearly shows that for most of the spell the hazard rate is declining with unemployment duration until it increases again leading up to and right after UI exhaustion. Exactly at the exhaustion point for the PBD=20 weeks group the hazard shows a spike in job finding rates. The figure also shows how the PBD extension leads to a lower job finding hazard for most of the spell roughly until the new exahaustion point (PBD=30 weeks). Panel (b), taken from Marinescu and Skandalis (2021), shows a similar figure for France plotting the monthly job finding hazards for 5 groups (PBD = 0, 6, 12, 24 and 36 months). The findings are qualitatively identical: the job finding hazard decreases throughout the spell except for a spike around UI exhaustion. Extending PBD moves the spike and reduces job finding rates up to the new exhaustion point. Panel (c) shows qualitatively the same result for Taiwan (Huang and Yang, 2021) and Panel (d) for Germany (Schmieder et al., 2016).

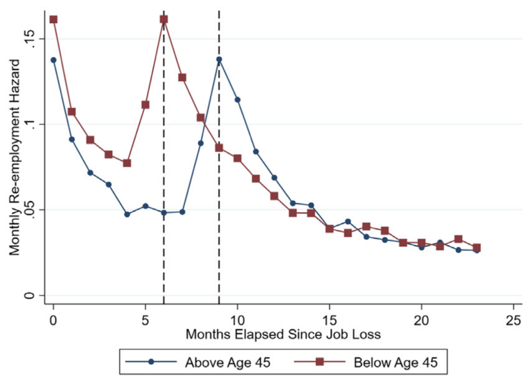
Figure 7: The Effects of UI on the Job Finding Hazard: Examples



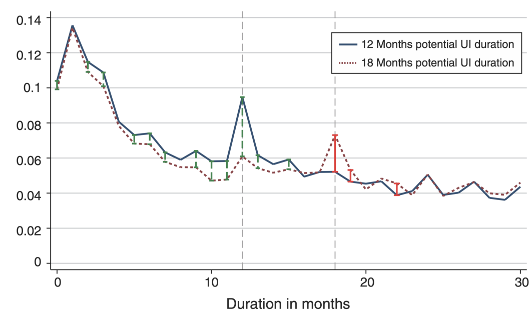
(a) Card, Chetty, and Weber 2007, PBD=20 vs. 30 weeks, Austria



(b) Marinescu and Skandalis 2021, France



(c) Huang and Yang 2021, PBD=6 vs. 9 months, Taiwan



(d) Schmieder, von Wachter and Bender 2016, PBD=12 vs. 18 months, Germany

Notes: The figure shows estimates of the unemployment exit hazard from different papers in the literature. Panel a) replicates Figure 9 from Card et al. (2007), Panel b) replicates Figure 2a from Marinescu and Skandalis (2021), Panel c) replicates Figure 7a from Huang and Yang (2021), and Panel d) replicates Figure 6b from Schmieder et al. (2016).

Overall, the decline in the job finding hazard with unemployment duration in conjunction with a spike in the hazard at UI exhaustion is now well documented.[7]

2.3.3 Reemployment Wages

As discussed above, the basic job search model is ambiguous with respect to how UI extensions affect average reemployment wages if the wage offer distribution

[7]Other examples include Le Barbanchon (2016) for France, DellaVigna et al. (2017) for Hungary, DellaVigna and Paserman (2005) and Ganong and Noel (2019) for the US, Gerard and Gonzaga (2021) for Brazil, Uusitalo and Verho (2010) for Finland, and many more.

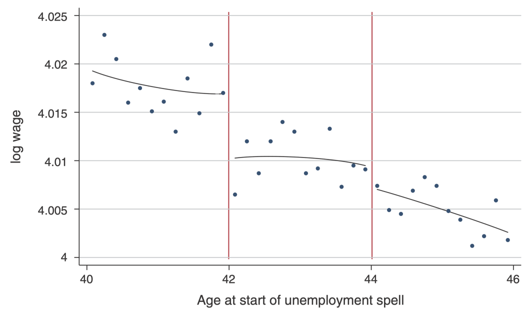
changes with unemployment duration (e.g. due to skill depreciation). Thus it is perhaps not surprising that the literature has found mixed results when estimating this effect. Figure 8 shows several examples from the recent literature. Panel (a), taken from Schmieder et al. (2016), shows that a 6 (and 4) month extension of UI benefits in Germany reduces average reemployment wages by about 0.8 log points. By contrast, Panel (c), from Nekoei and Weber (2017) shows that a 9 week extension in Austria slightly increases average reemployment wages and Panel (e), from Huang and Yang (2021) finds essentially no impact of an extension.

Several papers have analyzed how reemployment wages develop throughout the unemployment spell and how the reemployment wage path shifts in response to a UI extension. Panel (b), from Schmieder et al. (2016), shows that reemployment wages are declining throughout the unemployment spell, by about 25 log points over 1 year. The figure also shows that extending PBD from 12 to 18 months has virtually no impact on the reemployment wage path throughout the unemployment spell. The only exception is that at the UI exhaustion points, the reemployment wage dips down relative to the other group. Panel (d) shows as similar figure from Lalive et al. (2015) for Austria. The finding is qualitatively very similar, with a declining reemployment wage path that is virtually unaffected by a large extension in UI benefits. Finally, Panel (f) shows the reemployment wage path among UI recipients with 6 and 9 months of PBD in Taiwan (based on Huang and Yang, 2021) also finding no shift in the reemployment wage path.[8]

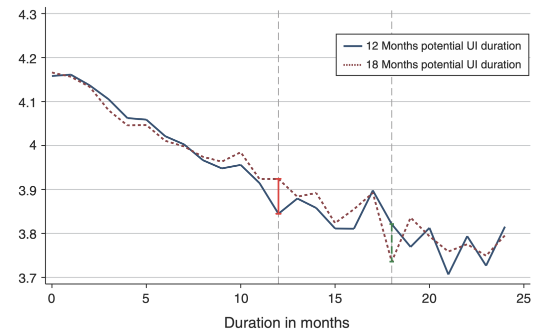
Overall, there seems to be strong evidence that reemployment wages decline with unemployment duration. This is true whether the dependent variable is simply the post-unemployment wage or the difference between post- and pre-unemployment wages. The evidence on the effect of UI on reemployment wages is quite mixed. Many estimates in the literature are close to 0 and when they are statistically significant they are still estimated with sizable standard errors.

[8]Similar declines of the reemployment wage path have been provided by Fallick et al., 2021, though without a comparison group.

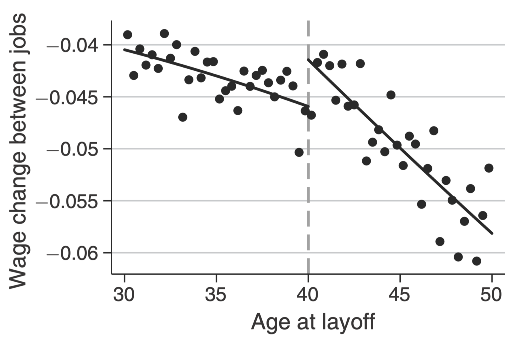
Figure 8: Evidence for Reemployment Wages: Examples



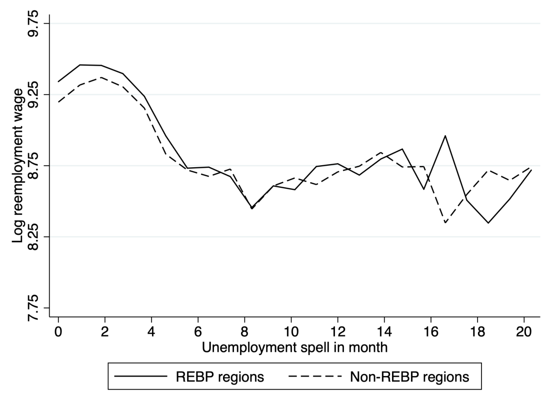
(a) Schmieder, von Wachter and Bender 2016, PBD = 12, 18 and 24 months



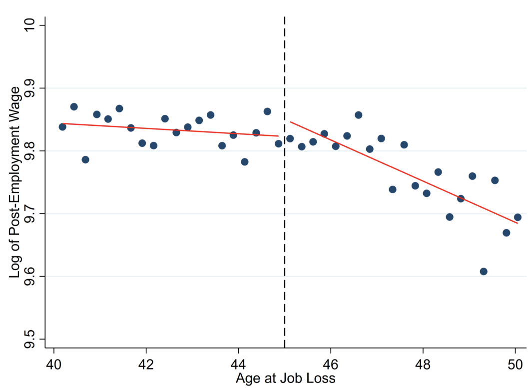
(b) Schmieder, von Wachter and Bender 2016



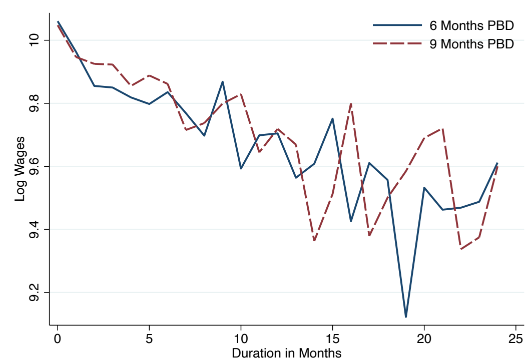
(c) Nekoei and Weber 2017, PBD = 30 vs. 39 weeks



(d) Lalive et al 2015



(e) Huang and Yang 2021, PBD = 6 vs. 9 months



(f) Huang and Yang, PBD = 6 vs. 9 months

Notes: The figure shows a number of examples of the effects of UI on log reemployment wages. Panel (a) and (b) replicate Figure 3a and 6a from Schmieder et al. (2016), Panel (c) replicates Figure 3c from Nekoei and Weber (2017), Panel (d) replicates Appendix Figure 6b from Lalive et al. (2015) and Panel (e) replicates Appendix Figure B1 from Huang and Yang (2021). Panel (f) is based on the same discontinuity and data as Panel (e) and was provided by Huang and Yang for this chapter. It shows RD estimates of log reemployment wages conditional on unemployment duration at the discontinuity for the two PBD groups using the same method as Figure 6a from Schmieder et al. (2016).

2.3.4 Can the Basic Search Model Rationalize the Evidence on Job Finding Rates and Reemployment Wages?

The empirical evidence that we laid out above highlights several stylized facts that appear to hold consistently across time and space: increasing PBD leads to larger nonemployment duration, the job finding hazard is decreasing for most of the spell and exhibit a spike at the exhaustion point, and reemployment wages are decreasing and respond only moderately to changes in PBD.

These broad findings are very consistent with the basic job search model that we developed in this chapter. To illustrate this, we calibrate the model to match the hazard rates and the reemployment wage path in Schmieder et al. (2016) using 4 different types of job seekers that differ by the cost of job search and the mean of the wage offer distribution.

Figure 9 shows the aggregate hazard and the aggregate reemployment wage path for this calibrated model, simulated for $P = 12$ and $P = 18$ months. The figure also shows the corresponding empirical hazard rates from Schmieder et al. (2016). Panel (a) shows that the simulated aggregate hazard matches the main empirical pattern of a declining hazard rate and a spike at the exhaustion point very well. The model also captures the effect of the UI extension to $P = 18$ months: First, the spike in the hazard moves to the new exhaustion point. Second, the hazard rate for $P = 18$ is higher than for $P = 12$ up to the new exhaustion point. Third, the hazard rates eventually cross and subsequently, the hazard rate is slightly higher for the $P = 18$ group. From figure 4 and the previous discussion, we know that within the individual types, the hazard rate is increasing, there is no spike, and the hazard rates for $P = 12$ weakly dominate the $P = 18$ hazard rate. Thus the changes in type composition, i.e. dynamic selection, are the key driver of the empirical hazard rates. In the appendix (section A) we show the corresponding simulations for the 4 individual types, which highlight how the dynamic selection of individual types generates the spike in the exit hazard as well as the decline in hazard and reemployment wages.

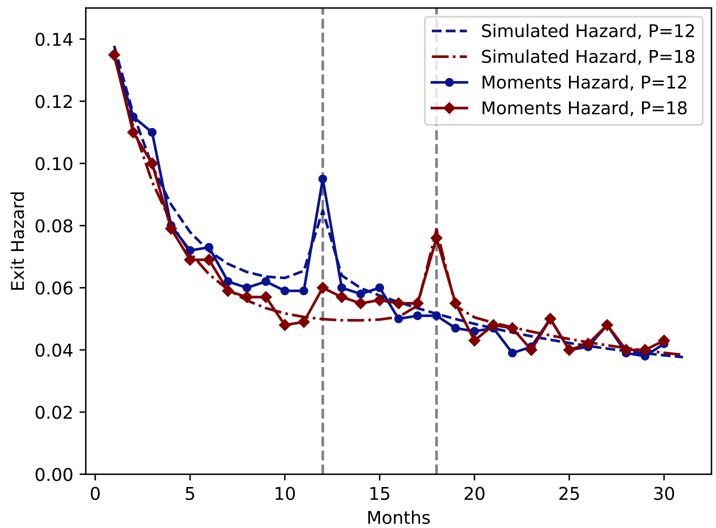
Panel (b) shows the simulated reemployment wage and contrasts it with the model. Here too, the model captures the broad empirical pattern fairly well. The aggregate reemployment wage declines throughout the unemployment spell. There is a small upward shift in the reemployment wage path (driven by the increase in reservation wages).

This highlights that, when allowing for multiple types, the model is very flexible and can fit patterns that are very different from what a single type (homogenous) model would predict. The basic search model with heterogeneity has thus proven a very powerful tool to understand and analyze UI policy, both in papers relying on reduced form empirical methods (e.g., by providing the theoretical basis for welfare analysis, following Baily (1978) and Chetty (2008)), as well as papers that structurally estimate the search model, e.g. for policy predictions. However, this flexibility also has a downside. In practice there are many possibilities for specifying functional forms and/or the exact nature of heterogeneity and the model is often underidentified to distinguish between such choices. For example, it may ex ante not be obvious whether heterogeneity should be in the cost function, the discount parameter, or the utility function and the model may well fit the data quite similarly.[9] Thus one has to be careful not to be lured into blindly trusting an estimated model with good in-sample fit, as the out of sample predictions or welfare implications may not be very robust to alternative model specifications. We argue below that many recent papers in this literature have been aware of these challenges and address them very thoughtfully, by carefully justifying the model choices and showing robustness checks to alternative specifications.

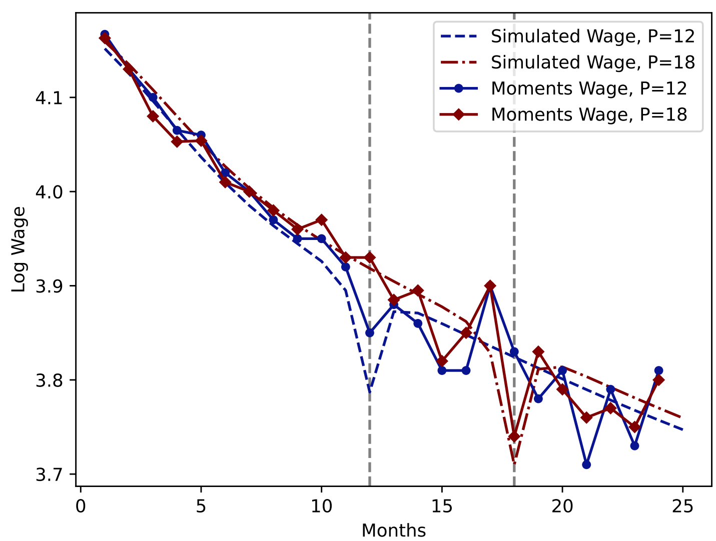
The challenge of under-identification is also particularly acute when trying to distinguish between different microfoundations of the search model. For example, in the model calibration above, the hazard rate falls throughout the unemployment spell entirely due to compositional changes (dynamic selection), while within types the hazard is increasing. However, a small modification of the model would be to assume that the cost of job search increases with unemployment duration. This would lead to declining search effort and job finding rate within a single job seeker type. Since we cannot observe the hazard rate within individuals over time (since each job seeker is only observed exiting the spell once), it is difficult distinguish these two explanations for the declining hazard empirically from each other, if the only data that is available is the typical administrative data with UI receipt, job start and end dates as well as wage information. This has inspired an active literature focused on additional information on job search to complement the evidence from administrative employment and UI records.

[9] Examples of this can be found in DellaVigna et al. (2017) or Gerard and Naritomi (2021), where both papers rely on criteria separate from the empirical moments to distinguish between different forms of heterogeneity

Figure 9: Simulating the Effects of a UI Extension



(a) Agg. Exit Hazard



(b) Agg. Log Reemployment Wage

Notes: The figure shows simulations of the model in 4 when changing the potential UI benefit duration from 12 to 18 months. Panels (a) to (d) show how search effort, the reservation wage, the exit hazard and the reemployment wage change for Type 1 workers.

2.4 New Empirical Moments

Administrative data and the advent of credible research designs with clean, transparent identification has provided a strong basis to better understand job search and how job seekers respond to changes in UI policy. In particular admin data has established several key facts such as a declining exit hazard and reemployment wages throughout the spell as well as that increasing UI generosity leads to longer unemployment durations. As shown above these key findings can be rationalized very well with the basic job search model with multiple types of job seekers. However, administrative data also has important shortcomings. In particular the while the search model can rationalize the main findings the evidence on the model is only indirect. Indeed the two key control variables of the job seeker: search effort and the reservation wage are not observed in typical administrative datasets. Furthermore, since for each worker there is only a single job acceptance event with a single reemployment wage per unemployment spell, it is impossible to estimate empirical hazard rates or reemployment wage paths within individual. This in turn makes it very difficult to differentiate whether the time path in the aggregate hazard and reemployment wage are due to dynamic selection or individual behavior changing.

Motivated by these shortcomings of administrative data, recent years saw a plethora of high quality papers that seek to expand our understanding of job search by bringing new data to the table: high frequency panel survey data on job search, data from online job platforms, consumption data and more.

Here we lay out this new evidence on the key ingredients of the job search model: job search effort, reservation (or target) wages, and consumption. These data have shed new light on the micro foundations of job search. Afterwards in subsection 2.5 we will discuss various refinements and extensions of the basic job search model have been proposed, at least in part, to explain these findings.

2.4.1 Search Effort

Survey Data

One, and perhaps the most straightforward, way to obtain information on search behavior is to ask the unemployed. Such a measure could then be used to test the predictions from the standard job search model regarding the evolution of search through the spell and how it is affected by UI. There was a small literature in the 70 and 80s that relied on small, cross-sectional surveys on job search behavior (see for example Devine and Kiefer (1993); Devine (1991) for a discussion), but the evidence was relatively scarce. The research in the late 90s and 2000s shifted towards policy evaluations and natural experiments relying on the type of administrative data discussed in the previous subsection (2.3).

The early 2010s then saw a revival of interest in shedding light on the microfoundations of job search. A first example was the use of time use diary data, pioneered by Alan Krueger and Andreas Mueller. In Krueger and Mueller (2010), they study job search using the American Time Use Surveys (ATUS) from 2003-2007. By asking respondents to fill out a time diary to carefully account for the time spent during a specific day, time use surveys are likely less are arguably less distorted. For example, in a survey that clearly focuses on job search, respondents may report higher search effort due to social desirability or acquiescence bias. While, in contrast, a time diary is more general and forces a certain degree of consistency on the respondent (the hours of the day have to add up), arguably reducing biases.[10] Krueger and Mueller (2010) show that unemployed workers only spend about 41 minutes on job search related activities on a workday. They also show regression results showing that after controlling for other characteristics, workers who receive higher UI benefits spend less time on job search. Finally, they plot job search effort before and after jobloss and show that search increases up to the exhaustion point and then decreases, thus creating a spike in effort resembling the well known spike in the unemployment exit hazard. In a related paper Krueger and Mueller (2012), analyze time use data from 16 countries in Europe and North America. The paper finds that the average time spent on job search among the unemployed is even lower in Europe, with only about 14-16 minutes on a weekday compared with 38 minutes in Canada and 41 minutes in the US. The paper estimates cross-country regressions for the time spent on search and finds little relation between UI generosity and search effort, but a relative strong correlation between wage inequality and search effort, consistent with the theoretical prediction that higher inequality leads to higher returns to search.

The downside of time use datasets is that they are relatively small when conditioning on unemployment and are cross-sectional with only a single observation

[10] For example, Chou and Shi (2021) discuss how hours worked are overestimated in the CPS and more accurately measured in the ATUS and how this can bias labor supply estimates.

per unemployed worker. This makes it difficult to trace out how search behavior changes over the unemployment spell in a way that is not confounded by dynamic selection. To fully understand the nature of job search, requires data that follows individuals over time throughout their unemployment spell with repeated observations of their job search effort. To fill this gap, Krueger and Mueller (2011) conducted a carefully crafted survey, which combined the strengths of the administrative data process with a high frequency online survey. The Krueger and Mueller (2011) Survey (KM Survey), was based on a complete list of UI recipients in NJ as of September 2009. It then generated a random sample of around 63 thousand individuals, stratified by their unemployment duration at the time. Each individual was then invited via letter to participate in the online survey for a period of 12 weeks with weekly questions related to job search activities. Thanks to the sampling design the different cohorts (a cohort referring to all individuals with a specific unemployment duration at the start of the survey), could then be lined up to trace out job search activities for almost 2 years.

The questionnaire consisted of both a time use diary for a single day, as well as questions on job search for the whole week prior to the survey (recall question). The level of search from the time use diary is around 70 minutes per day, higher than in Krueger and Mueller (2010, 2012), but this is likely explained by the fact that the KM survey focuses on UI recipients who are probably more strongly attached to the labor market than the average unemployed and perhaps because UI recipient are subject to job search requirements to maintain benefit eligibility. The KM survey reveals a curious pattern: within cohorts (or within individuals when controlling for individual fixed effects) search effort is falling rapidly (about 30 min over 12 weeks), while across cohorts search effort is essentially flat. The paper discusses this pattern carefully: first, it could be that effort is truly declining within individuals and this is masked by dynamic selection across cohorts. Second, there could have been secular time trends over the survey duration (since all cohorts were interviewed over the same period duration is colinear with calendar time). Third, there may be reporting bias, e.g. respondents may have stated less effort over time in order to avoid conditional follow-up questions in the questionnaire or because they became more honest as they realized that their responses had no negative consequences. While the paper does not fully resolve these conflicting explanations, the possibility of systematic reporting bias is at least an important caveat for interpreting this key finding.[11] The paper provides some analysis for search effort around UI exhaustion. After controlling for unemployment duration, Search effort appears relatively flat leading up to UI exhaustion and declining moderately afterwards. One possible confounder for the exhaustion analysis is that the survey was conducted at the height of the Great Recession with several UI extensions. While UI benefits were exhausted / lapsed for some recipients, they were subsequently reinstated.

While the analysis of search effort over the spell thus comes with important caveats, the KM survey is a remarkable achievement. It provides a plethora of other information about the job search process. For example, Krueger and Mueller (2011) show that job offers are rare: in a given week, only about 2-4 percent of UI recipients receive a job offer. Furthermore, it seems most job offers are accepted. Krueger and Mueller (2011) also provide fascinating evidence on subjective well-being. The unemployed are quite unsatisfied overall and report having a 'bad mood' or experiencing 'sadness' most of the time. Furthermore, when measured by moment-to-moment measures, workers seem to feel worse over time the longer they are unemployed. From the time diary data, it is also striking that on a given day the unemployed are the least happy, and the most sad and stressed while searching for a job.

DellaVigna et al. (2022) build on the KM survey by collecting data on job seekers in the German UI system. Following a similar overlapping cohort design, they conduct a large, high-frequency survey among UI recipients via text messages (SMS) that follows each cohort of workers over an 18 week period. By focusing on just one or two questions on any given day, the survey collects less information than the KM survey, but has substantially less attrition and may reduce survey response bias caused by interviewee fatigue. Every respondent is asked twice a week how many hours they spend on job search activities on the previous day. The study was conducted over a 2 year horizon with new cohorts starting every month. As a result, calendar time is not perfectly collinear with unemployment duration and the paper can conduct detailed checks for the possibility of reporting bias. Overall the paper finds no evidence for reporting bias and the within and between person estimates of how search effort evolves with unemployment duration are very

[11]The published article was accompanied by a thoughtful discussion by Stephen Davis (Davis, 2011), who also seems to view reporting bias as a likely explanation for at least some of the within-person decline.

similar.

Compared to the KM survey, the setting in DellaVigna et al. (2022) features a very stable economic environment with low unemployment rates and a predictable UI system with a clear UI exhaustion point. An important advantage of the survey is that it samples individuals with different potential benefit durations, which are determined by the contribution history and age of the UI recipient. For these different PBD groups the administrative data shows the typical hazard path of declining hazards early in the spell and a spike in the exit rate at UI exhaustion, comparable to the results in Schmieder et al. (2012) for an earlier time period. Thus the paper can explore how search effort is related to UI exhaustion and PBD across groups and compare the search effort path with the corresponding hazard rates.

The SMS data in DellaVigna et al. (2022) reveals several key findings: First, the level of search effort is with around 83 minutes in a similar ballpark as the recall estimate in the KM survey. Second, within individuals, search effort is essentially flat in the first 6 months after UI entry. Third, Search effort rises by about 8 percent in the 2 months leading up to UI exhaustion and falls by a similar amount afterwards, thus mirroring the spike in the job finding hazard at exhaustion. This suggests that the decline in the aggregate hazard early on in the unemployment spell is not due to declines in search effort. Furthermore, the spike in effort at exhaustion is substantially smaller (around 8 percent) than the spike in the job finding hazard (around 40 percent).

The fact that there is a spike in search effort at the exhaustion point suggests that PBD does impact search effort and in particular that shorter PBD will lead to higher search effort earlier. This is further supported by some RD analysis, reported in the online appendix of the paper, using an age discontinuity determining PBD at age 50. The analysis provides evidence, albeit not very precisely estimated, that PBD extensions reduce search effort earlier in the spell.

Further evidence that UI generosity affects job search effort, is offered by Lichter and Schiprowski (2021), who study how UI generosity affects self reported job search effort in a Difference in Difference designs. The paper exploits a reform in the German UI system in 2008 that raised PBD from 12 to 15 months for workers age 50 to 54. Relying on a survey, called the IZA evaluation dataset, they show that this reform substantially reduced search effort in the affected group.

Table 1, Panel A summarizes some of the stylized findings from the literature on job search based on survey data.

Search Platform and Process Data

Another development in the recent literature was the use of data from online search platforms and other process-generated data to study job search. As the means of job search (vacancy postings, job applications etc.) have shifted from traditional offline to online methods over the past 2 decades, there is in principle a wealth of data captured by various online platforms that facilitate job search.

A first example, relies on the fact that search engines are a simple starting point for job seekers. Baker and Fradkin (2017) uses Google search data to construct a metropolitan area level of job search activity. They first validate this constructed search index by comparing it with estimates of job search from the ATUS job search questions and show that it seems to capture spatial and temporal variation of job search effort very well. They then estimate the effect of PBD extensions during the Great Recession on the search index using a DiD approach as well as an eventstudy design. The DiD analysis shows small, statistically significant, effects: a 10 week extension of PBD reduces search effort by about 1-2 percent. The eventstudy shows statistically insignificant effects, but is underpowered and cannot reject the DiD results.

Also focusing on the PBD extensions during the Great Recession, Marinescu (2017) uses data from the job board Careerbuilder.com, one of (if not the) largest job search platforms in the US at the time. The paper relies on the state and federal UI extensions for 2 identification strategies: an eventstudy design and a fuzzy RD based on UI extensions that were triggered by unemployment rate thresholds. Using the eventstudy design Marinescu finds that a 10 week PBD extension reduces applications in a state by about 4 percent, somewhat larger than the impact in Baker and Fradkin (2017). The RD design leads to very similar results, though somewhat noisier. A key advantage of using job platform data, is that the paper can observe both the supply side (applications) as well as the demand side (posted vacancies). Using the same identification strategies, she shows that there is no effect of the PBD extensions on posted vacancies, and as a result labor market tightness (the difference between log vacancies and log applications) increases in response to UI extensions suggesting that each individual sent application is more likely to be successful. This has important implications for the general equilibrium effects of

UI, which we will discuss in section 3.

While Baker and Fradkin (2017) and Marinescu (2017) focus on the effects of UI on aggregate search effort, Faberman and Kudlyak (2019) leverage data from the SnagAJob job platform to analyze the evolution of search effort within individuals. In the paper, they can follow users of the platform and analyze how search behavior changes over time. A downside of the data is that it does not have information on unemployment. So instead of focusing on unemployment spells, the paper focuses on the period when a worker first submits an application until the first time there is a 5 week break of activity on the platform. The main result of the paper is that it shows that within individuals there is a clear decline in the number of applications sent per week. While worker apply to around 3 applications in the first week, this falls to less than 1 application in the second week and then falls by another 50 percent over the next 10 weeks. The paper also shows that individuals with long spells search more throughout the spell than individuals with short spells. This correlation goes against the notion that higher search effort leads to lower spells, but may be driven by selection. The paper does not observe unemployment or UI receipt, so it cannot speak to how UI generosity or exhaustion affects search.

Marinescu and Skandalis (2021) fill this gap by using from a job search platform that is administered by the French Public Employment Service and linked to administrative data from the UI system. As a result, they can observe when individuals enter unemployment, their UI eligibility, and when they exhaust UI benefits. The setting also features several groups of individuals with distinct PBDs. An important strength of the paper is that it can show job finding rates for several PBD groups and for all groups job finding falls over time but with a spike at the exhaustion point. Using the application data, the paper documents that within individuals search effort increases over time until it peaks at UI exhaustion. After individuals exhaust UI benefits, search effort falls. Thus search effort shows a spike around exhaustion that mimicks the spike in the job finding rate. Overall, it appears that search effort does not explain the initial decline in the job finding rate, but may play an important role in explaining the spike at the exhaustion point. By showing the search effort is clearly affected by PBD, it also provides evidence that UI generosity affects overall search effort (higher generosity leading to lower overall effort).

The paper focuses on similar questions to DellaVigna et al. (2022), but with different strenghts and weaknesses. Due to the administrative nature of the data Marinescu and Skandalis (2021) features a much larger sample (around 450,000 observations) and does not suffer from attrition. Furthermore the applications represent actual job search effort and are not driven by reporting bias. On the other hand, the French job platform only accounts for a small share of all job search (only about 5 percent of jobs are found through the platform) and the reported level is very low (less than 1 application per month), while the survey in DellaVigna et al. (2022) should capture all forms of job search. Overall, the results from the two papers are complementary, with both showing that search effort spikes at the UI exhausion point.

Faberman and Kudlyak (2019) and Marinescu and Skandalis (2021) come to the opposite result, with respect to how search effort varies within individuals over time. While search effort decreases in Faberman and Kudlyak (2019) within individual at the beginning of the unemployment spell, it increases in Marinescu and Skandalis (2021). One possibility might be the different definition of a spell. A spell in Faberman and Kudlyak (2019) starts mechanically with being active on the platform. It may then well be that over time individuals switch to other platforms and search methods thus contributing to the observed decline. On the other hand in Marinescu and Skandalis (2021), a spell starts with unemployment entry and individuals may only gradually begin to use the platform. Of course other differences in sample and context may also affect this comparison.

Finally, Massenkoff (2023) uses unique data from UI claim audits in the US. The Department of Labor conducts random audits among UI claimants via phone, asking questions about reservation wages and job applications and cross-validating the answers with employer reports. The papers has more than a million audit reports from 1987 to 2022. Using information from individuals who were audited more than once in a single UI spell, the paper shows that within a spell job applications are essentially flat. The paper also uses caps imposed on weekly UI benefit levels to estimate the effect of UI benefits on search effort using a Regression Kink Design. The paper finds Essentially no impact of UI on search effort.

Table 1: Evidence on Job Search Effort

Paper	Country	Data	Evolution Through Spell			Effect of UI	
			Level	Initial Evolution	Exhaust. Point	Design	Sign of Effect
Panel A: Survey Data							
Krueger and Mueller 2010	US	ATUS	41 min per day		Spike	OLS/IV	$\frac{de}{db} < 0$
Krueger and Mueller 2012	Europe	Time Use	14-16 min per day			Cross-country	$\frac{de}{db} \approx 0$
Krueger and Mueller 2011	US	NJ Web Survey	70 min per day	Flat / Decreasing	Flat / Decreasing	OLS	$\frac{de}{db} < 0$
DellaVigna et al 2022	Germany	SMS Survey	80 min per day	Flat	Spike	Simple comparison	$\frac{de}{dP} < 0$
Lichter and Schiprowski 2021	Germany	IZA Eval.				DiD	$\frac{de}{dP} < 0$
Panel B: Search Platform and Process Data							
Marinescu 2017	US	Online Search Platform				DiD / RD	$\frac{de}{dP} < 0$
Baker and Fradkin 2017	US	Google Search				DiD	$\frac{de}{dP} < 0$
Faberman and Kudlyak 2019	US	Online Search Platform	1.8 applications per week	Decreasing			
Marinescu and Skandalis 2021	France	Online Search Platform		Increasing	Spike	Simple comparison	$\frac{de}{dP} < 0$
Massenkoff 2023	US	DOL Audits	2 job contacts per week	Flat		RKD	$\frac{de}{db} = 0$

2.4.2 Reservation / Target Wages

Now we turn to what survey and platform data reveal about wage strategies of job seekers. The theoretical job search literature has long featured the reservation wage as a key measure for describing workers search strategies at a given point in time and a key determinant for search outcomes. This is also the approach taken by the basic search model from section 2.2 that features search effort and reservation wages. However, the concept of the reservation wage also faces complications when attempting to empirically measure it. The concept is quite abstract and is not be easy to translate into a survey question. For example, the question ”what is the minimum wage at which you would accept a job offer” may be quite ambiguous without specify exactly what the job is. A job seeker may ask for a certain minimum wage for one job that offers flexible hours, a pleasant work environment, and other benefits, but asks for a higher minimum wage for another job with different attributes. Thus asking for a reservation wage may be meaningless without holding job characteristics constant, which in turn may be hard to do within or across individuals.

Several papers measure target wages instead of reservation wages. A target wage captures the notion that a key decision of workers is which job to apply for and generally speaking workers have certain expectations what a given type of job may pay. A typical way to measure the target wage is to simply ask or observe what the last job application was and how much that type of job typically pays. We will return to the difference between reservation and target wage models in the next section.

The earliest papers that empirically studied the evolution of reservation wages relied mostly on cross-sectional data, e.g. Feldstein and Poterba (1984). The few papers based on panel data had very small samples. For a discussion of this earlier literature see Devine (1991) and the discussion in Krueger and Mueller (2016).

Turning to the modern empirical evidence, we will first discuss studies based on survey data before turning again to papers based on data from search platforms and process data.

Survey Data

The KM Survey asked detailed questions about what type of job workers are looking for (occupation, hours per week) and what is the lowest wage or salary to accept an offer. They use the latter question as a measure of the reservation wage. Since they also ask in subsequent waves about received job offers and what those offers paid and whether the job seeker accepted then it is also possible to estimate whether the reservation wage is predictive of future job acceptance. Krueger and Mueller (2016) provide a detailed analysis of the reservation wage information in the KM data. They use the reservation wage ratio, i.e. the ratio of the reported reservation wage and the pre-unemployment wage as the main outcome variable. The first striking fact is that reservation wage ratio is high, with a mean of around 1. There is also a wide range of reported reservation wages, with a standard deviation of the log reservation wage ratio of around 0.37, which suggests that many job seekers report reservation wages as low as 60 percent of their previous wage but similarly many report reservation wages 40 percent higher than their previous wage. By relying on the nonlinearities in the benefit schedule the paper estimates the effect of weekly UI benefits on the reservation wage controlling for the previous wage and other controls. The implied elasticity of the reservation wage with respect to the benefit level is close to zero, statistically insignificant and somewhat sensitive to controls.

The paper then moves on to explore how reservation wages vary with unemployment duration. Within individuals the reservation wage does not vary with unemployment duration and is essentially flat. Furthermore, it is not affected by UI benefit exhaustion or a UI extension that occurred during the sample period. As additional measures of the job search strategy, the paper also considers whether workers apply to jobs in lower paying occupations or would be willing to accept longer commutes. The paper finds that workers are indeed applying to slightly lower paying occupations and report being willing to accept slightly higher commutes (acceptable commuting time increases by 4.6 minutes over one year). The paper also shows that reservation wages do show a small but significant decline for 2 subgroups: older workers and workers with higher initial savings. Overall, it appears that workers do not change their job acceptance behavior very much over the unemployment spell.

Finally, the paper analyzes whether stated reservation wages are consistent with predictions from a reservation wage model. The basic reservation wage model would imply that all jobs paying less than the reservation wage are rejected and all jobs paying more are accepted. This does not hold in the data: many jobs paying less than the stated reservation wage are still accepted, while many jobs paying more than the reservation wage are rejected. On the other hand, the reservation wage is predictive and job acceptance probabilities rise as the offered wage rises relative to the reservation wage. When job offers are not accepted the data asks why and it seems other job attributes like hours or commuting distance are the main reasons.

DellaVigna et al. (2022) collected information on the target wage of job seekers, by asking participants what the estimated wage was of the last job they applied to. This target wage measure is very flat throughout the spell and then declines slightly after UI exhaustion.

Using panel data from a survey among job seekers in Belgium who are interviewed at UI entry, after 3 and after 6 months, Deschacht and Vansteenkiste (2021) estimate the within person change in reservation wages over time. The paper finds that reservation wages fall by about 0.4 percent per month or about 5 percent over the course of a year.

Lichter and Schiprowski (2021) use the same DiD design decribed above to estimate the effect of a UI extension on reservation wages. The point estimate is positive but small and statistically insignificant.

Search Platform and Process Data

The French UI system collects information on reservation wages from all new UI entrants. Le Barbanchon et al. (2019) use this information to estimate how reservation wages are affected by UI. They exploit a reform in 2009 that altered how PBD is determined for UI claimants. While prior to the reform, PBD was a step function of days worked in the previous year, after the reform this became a continuous function. This leads to differential changes in PBD along the previous days worked variable which the paper uses to construct a Diff-in-Diff design. The paper finds no effect of PBD on stated reservation wages and the estimates are quite precise. The paper also finds no effect on desired hours, type of contract or willingness to commute. The paper validates these results further by exploiting an age discontinuity at age 50, where maximum PBD increases from 24 to 36 months. Using an RD design at this discontinuity leads to very similar results. Overall, the paper finds no evidence that large changes in PBD affect job selectivity.

Using the job platform data described before, Marinescu and Skandalis (2021) can observe what type of jobs workers apply to. By calculating expected wages for these types of jobs, they can thus construct a measure of the target wage over the unemployment spell. The target wage is decreasing but the effect is quite small with a decline of about 1.5 percent over one year. Target wages also decline slightly after UI exhaustion.

Since 2015, UI recipients in Denmark are required to document in each week around 1.5-2 jobs that they applied to in order to remain eligible for UI benefits. Fluchtmann et al. (2023) uses this information to create various characteristics of applied-for-jobs and thus construct target wages, target hours and other measures. A key advantage compared to survey data is the large coverage (all UI recipients in Denmark) as well as the comprehensiveness of the measure, since it is not limited to applied-for-jobs from a single platform. The paper documents that within individuals target wages decline slowly with unemployment duration. Over the duration of a year, mean target wages decrease by around 1 percent when controlling for person fixed effects. There is also a small but precise decrease in the probability of applying for full-time jobs by around 3 percentage points over one year.

Finally leveraging the DOL audit data, Massenkoff (2023) analyses reservation wages. The ratio is somewhat smaller than in the KM data or the French data (Le Barbanchon et al., 2019). The paper finds a significant within-person decline in reservation wages of around 5 percent over one year. Relying on the Regression Kink design to estimate the effects of UI benefit levels on reservation wages, the paper finds no effect.

Table 2: New Evidence on the Reservation/Target Wage

Paper	**Country**	**Data**	**Measure**	**Evolution Through Spell**			**Effect of UI**	
			Reserv. (R) Target (T)	Level	Initial Evolution	Exhaust. Point	Design	Sign of Effect
Panel A: Survey Data								
Krueger and Mueller (2016)	US	NJ Web Survey	R	0.95	Flat / decreasing for some groups	Flat	OLS	$\frac{dw}{db} \approx 0$
DellaVigna et al. (2022)	Germany	SMS Survey	T	1.17	Flat	Decreasing		
Deschacht and Vansteenkiste (2021)	Belgium	Survey	R	0.99	Decreasing, 5% per year			
Lichter and Schiprowski (2021)	Germany	IZA Eval.	R				DiD	$\frac{dw}{dP} = 0$
Panel B: Search Platform and Process Data								
LeBarbachon, Rathelot, and Roulet (2019)	France	Employment Agency	R	0.93			DiD and RD	$\frac{dw}{dP} = 0$
Marinescu and Skandalis (2021)	France	Online Search Platform	T		Decreasing, 1.5% per year	Decreasing		
Fluchtmann et al. (2023)	Denmark	Employment Agency	T		Decreasing, 1% per year			
Massenkoff (2023)	US	DOL Audits	R	0.86	Decreasing, 5% per year		RKD	$\frac{dw}{db} = 0$

2.4.3 Evidence on Consumption during Unemployment

Arguably, the central concern associated with unemployment is its impact on consumption. This is clearly true from the individual perspective, as the painful loss in income and the resulting drop in consumption is a key motivating factor to engage in job search.[12] It is also the case from the perspective of policy makers, who are interested in increasing aggregate welfare through insurance against income shocks and, perhaps, redistribution. This central role of consumption is highlighted in the basic search model where flow utility is defined over consumption and the cost of job search. And similarly, consumption plays a key role in the theoretical (and empirical) analysis of the optimal design of UI policy (see Section 3).

The first systematic analysis of consumption patterns of unemployed workers was conducted by Gruber (1997). The Panel Study of Income Dynamics (PSID) provides longitudinal information on food consumption on an annual level. Using this data Gruber (1997) shows that food consumption drops on average by 6.8 percent at the onset of unemployment. He also shows that the consumption drop is smaller for individuals with higher UI benefits, suggesting that UI does indeed provide some consumption smoothing. [13]

Kolsrud et al. (2018) find similar results using registry data from Sweden. The registry data contains detailed information on consumption on an annual level. Exploiting the timing of the onset of unemployment relative to calendar years the paper can trace out how consumption drops evolve through the unemployment spell. They find that consumption drops around 4.4 percent in the first 20 weeks, but more than doubles, to 9.1 percent for those who are unemployed longer. Since in their sample UI benefits are constant over the spell, this suggests that individuals' ability to smooth consumption through other means is much higher for short unemployment spells. Landais and Spinnewijn (2021) revisit the same data and find a consumption loss of around 13 percent for the unemployed.

The first high-frequency study of consumption at the onset of unemployment as well as at UI exhaustion was conducted by Ganong and Noel (2019). They use

[12]It should be noted that unemployment may be painful for reasons not related to consumption/income. For example, workers may enjoy working or obtain a sense of self-worth from their employment, and play an important social role. Indeed a sizable literature has documented negative effects of unemployment on happiness, social contacts and mental health.

[13]Hendren (2017) revisits the PSID data and shows that UI recipients already show a 2-3% drop in consumption in the year prior to unemployment, a sign that individuals anticipate unemployment risk.

banking data from JP Morgan Chase to obtain household level, monthly data on income and consumption for detailed spending categories. UI spells can be identified off of direct deposits from the UI system. The paper contrasts the household income path with the household consumption path through the UI spell. At the start of unemployment, income drops suddenly by 15 percent, it continues to drop around 1.5 percent per month while on UI and then drops by an additional 40 percent at exhaustion. All together, household income is 66 percent lower after UI exhaustion relative to the pre-unemployment level. Spending on the other hand drops by 6 percent at the beginning of unemployment, then declines by slightly less than 1 percent per month and then drops by another 12 percent at benefit exhaustion. In total, at exhaustion spending is about 25 percent below the pre-unemployment level.

The sudden consumption drop at the onset of unemployment is perhaps easier to explain than at UI exhaustion. Unemployment may come as a surprise to the individual and thus they respond to the sudden information with adjusting spending. By contrast UI exhaustion should not be a surprise. People know in advance that UI benefits are only paid for a finite period (26 weeks for most workers in this context) and that they will face a high probability of reaching the exhaustion point once they are only 2 or 3 months away from it given an average job finding hazard of around 20 percent. But then why do individuals not reduce consumption more prior to exhaustion to save up and to be able to smooth consumption around exhaustion?

Gerard and Naritomi (2021) provide another piece of high-frequency evidence on consumption, this time from Brazil. Their setting differs from Ganong and Noel (2019), in that job losers are eligible for a substantial severance payment (SP). After job loss they are eligible to 5 months of UI. The severance pay is quite large at almost 5 months of income. The paper contrasts workers who are laid off (and eligible to SP), fired (and not eligible to SP), and workers who do not lose their job. The paper finds that job losers with SP have a large increase in spending right after job loss, about 30-40 percent in the first and second month after job loss. Spending then falls rapidly for workers who remain unemployed and is about 20 percent below pre-unemployment levels 1 year after job loss. Given that job losers know that they are experiencing a large negative income shock, it is perhaps surprising that SP is not used for more consumption smoothing. The pattern holds for many types of goods, including nondurables and food, and thus is not driven by large durable purchases (with lasting utility flow). The paper also shows that there is a relative sharp drop in consumption for UI exhaustees of around 15 percent. Finally, the authors provide a weekly spending analysis showing that spending is substantially higher in the week when workers receive their monthly UI payments. Overall, spending appears very responsive to income in the short term, even in the face of what appear to be substantial incentives to smooth consumption.

2.4.4 Other Types of Evidence

In this section, we focus on real-world evidence that relates directly to the individual search behavior of unemployed workers: job selectivity, search intensity, and consumption. There are many other related papers with either a somewhat different focus or different types of data. For example we do not cover the literature focusing on differences by groups (e.g. gender differences as in Le Barbanchon et al. (2021))

One example are audit (or correspondence) studies, where researchers send out fake resumes to potential employers, randomly varying attributes of the resume.[14] By their nature these studies focus on the labor demand side of job search. The researchers measure call-back rates for the resume to see how employer interest is affected by the random attributes. While economists have studied many questions using audit studies (in particular focusing on discrimination), the most pertinent to job search among the unemployed are audit studies that compare callback rates by length of unemployment. Kroft et al. (2013) were the first study focusing on unemployment and found that applicants with long-term unemployment spells were substantially less likely to be called back by employers. Similar results were found by Eriksson and Rooth (2014) in Sweden who also find lower call back rates for the long-term unemployed. By contrast Nunley et al. (2017), for college graduates, and Farber et al. (2019), for somewhat women, do not find evidence of an effect of unemployment duration on call-back rates. Overall the evidence from audit studies is somewhat mixed but at least in some contexts seems to suggest that unemployment duration may have a causal, negative impact on the type of job offers workers receive.

There is also a large experimental literature studying job search in the lab. Re-

[14] See Neumark (2018) for a review.

searchers who rely on a controlled lab environment, can use carefully designed experiments and randomization to obtain estimates of deep parameters and mechanisms that would be very hard to isolate with real world data. For example, Brown et al. (2011) study participants reservation wage in a lab environment and find the reservation wages decline. The paper can distinguish between a number of alternative explanations why reservation wages may decline, e.g. because individuals learn about the optimal reservation wage strategy over time. The challenge with such data is that it is not clear how well the decisions in the lab extrapolate to high stakes real world decisions. We refer to the review article by Cooper and Kuhn (2020) that discusses this literature in some detail.

There is also a substantial literature that uses field experiments to study how various interventions affect job search outcomes. This literature is often interested in active labor market programs (ALMP), which we discuss in section 4. However, some of this work relates to the information environment of the job search process. By altering the type of information available to job seekers these papers show that information is an important constraint for job seekers, e.g. Belot et al. (2019) or Belot et al. (2022b).

2.5 Refining the Search Model

The past years have seen a wealth of new information on the behavior of job seekers. Much of this work was designed to improve our understanding of the mechanisms that underlay the job search process. We now turn to what this information reveals about our theoretical understanding of job search. Starting from the basic job search model from section 2.2, we first discuss what this evidence suggests about the key mechanisms captured in that model, such as the relative importance of search effort and reservation wages. We then discuss modifications and extensions to the job search model that have been proposed in the literature and to what extent the empirical evidence supports these refinements.

We summarize this discussion in Table 3. The table shows for each discussed mechanism or proposed refinement the type of evidence that exists in the literature and the, perhaps tentative, conclusions we would draw from this evidence. The table also provides our assessment for the strength of evidence for this proposed conclusion. We categorized the strength as: ”Unclear” if there is either no clear evidence in any paper or conflicting evidence; ”Suggestive” if there is a single paper with clear evidence in support of the conclusion (and no clear evidence against); "Moderate" if there are 2-3 papers with evidence in support and "Strong" if there are 4 or more papers with evidence in support of the conclusion. This assessment is of course subjective but the intent is to give the reader a sense of conclusions about the search model that have a lot of empirical support vs. other areas that are more speculative and where additional research may be particularly valuable.

Table 3: Modelling Choices and Refinements with Evidence from Literature

Modelling Choices and Refinements	Conclusion from Literature	Strength of Evidence	Type of Evidence
Choice variables that determine UI responses	Search intensity responds to UI generosity; job selectivity does not respond	Strong	See Table 1 and Table 2
Directed Search vs. Reservation Wages	Difficult to empirically separate	Unclear	Survey and platform data
Duration Dependence in Reemp. Wages	Some evidence for duration dependence: skill depreciation / declining reservation and target wages	Moderate	Evidence from reemployment wages, direct measures of skill
Duration Dependence in Job Finding Rates	Dynamic selection accounts for majority of decline in hazard rate, little role for search effort / res. wages	Strong	See Table 1 and Table 2
Present Bias	Clear evidence for present bias	Strong	Spike in hazard, Consumption patterns, Structural estimates
Reference Dependence			
- Search Intensity	Reference dependence partly responsible for spike in hazard	Moderate	Structural estimates / Policy variation
- Wages	Wage offers evaluated relative to previous wages	Unclear	Reduced form
Biased Beliefs			
- Level	Job seekers overestimate job finding probability	Moderate	Comparing subjective and actual job finding probabilities
- Return to search	Not clear whether job seekers over or underestimate returns to search.	Unclear	
Locus of Control	Internal locus of control associate with higher search effort and job finding	Moderate	Reduced form regressions
Employer collusion / Storable Offers			
- Timing of Job Start	Evidence against collusion over job start dates	Suggestive	Survey evidence on job offer and job start dates
- Timing of Recalls	Some evidence that recalls are timed with UI exhaustion	Moderate	
Learning / Information	Some evidence that job seekers learn about stochastic process	Unclear	Reduced form regressions / structural estimates

Notes: For discussion of the evidence see the text. Strength of evidence is subjective, but follows roughly the following key: Unclear - either no clear evidence in any paper or papers with conflicting evidence; Suggestive - a single paper with clear evidence in support; Moderate - 2 to 3 papers with evidence in support; Strong - 4 or more papers with evidence in support.

2.5.1 Channels that determine UI Response

We saw that in the basic job search model UI generosity, say a PBD extension, affects both search intensity e and the reservation wage ϕ which in turn affect the job finding rate: $\frac{de}{dP} < 0$ and $\frac{d\phi}{dP} > 0$.
As we saw in section 2.4 Table 1, there are three papers that have analyzed the

effect of UI extensions on search effort using Diff-in-Diff designs: Marinescu (2017), Baker and Fradkin (2017), and Lichter and Schiprowski (2021). Based on different policy variation (US and Germany, recession and boom) and different measures of search intensity (survey and platform), all three papers find clear evidence that an increase in PBD reduces search effort.

Further evidence comes from studies focusing how search effort evolves around UI exhaustion. Both Marinescu and Skandalis (2021) and DellaVigna et al. (2022) show that search effort increases prior to UI exhaustion and falls afterwards, mimicking the spike in the job finding rate at exhaustion. While the papers do not explicitly estimate the effect of a change in PBD on search effort, they provide comparisons across groups of workers with different PBD and show that the effort spike closely tracks the UI exhaustion point. This also provides strong evidence that PBD has a negative effect on search effort.

Overall, the evidence that $\frac{de}{dP} < 0$ appears quite strong. By contrast, it is less clear whether UI benefit levels affect search effort. Krueger and Mueller (2010) and Krueger and Mueller (2011), find a negative effect of benefit levels on reported time spent on job search, but this is based on cross-sectional regressions of effort on benefit replacement rates with extensive controls and without a design based identification strategy. Krueger and Mueller (2012) find no relationship between benefit generosity and effort in cross country regressions. Finally, Massenkoff (2023) finds no effect of UI benefit levels on search effort in what is arguably the only evidence based on a clear causal identification strategy (RKD).

One important gap in the literature is that there is very little direct evidence on the returns to search effort. One piece of evidence is Arni and Schiprowski (2019) who find that an additional monthly application induced by a higher job search requirement reduces non-employment duration by 4%. Another is the recent working paper by Field et al. (2023), who run an RCT in Pakistan that reduces the psychological cost of job applications to nudge job seekers to send job applications. They find that an additional application increases the probability of a job interview by about 6 percent. They also find evidence for constant returns to search effort (i.e. an elasticity of 1). More indirect evidence on the returns to search, is the structural model in DellaVigna et al. (2022), which involves estimating the search production function $f(e_t)$. Consistent with Field et al. (2023), they find an elasticity of 1 in the model with the best fit again suggesting constant returns to search. However, an important caveat is that the elasticity estimate is sensitive to alternative specifications and the setting is probably not ideal to identify this elasticity.

Turning to reservation wages, Table 2 lists 4 studies that estimated the effect of UI. Two papers find no effect of PBD extensions on reservation wages (Le Barbanchon et al., 2019; Lichter and Schiprowski, 2021) using credible research designs (Diff-in-Diff, RD) while two other papers find no of benefit levels (Krueger and Mueller, 2016; Massenkoff, 2023). While this is not an overwhelming amount of evidence it is fairly consistent: $\frac{d\phi}{dP} = 0$ and $\frac{d\phi}{db} = 0$. To our knowledge, there is not much evidence whether target wages or other measures of job selectivity respond to UI.

In the standard search model the effect of UI on nonemployment durations goes either through effort or reservation wages. As discussed, there is significant evidence that UI affects search effort and some evidence that effort affects job finding rates. By contrast, it is not clear that UI affects reservation wages or other dimensions of job selectivity. This is also consistent with the fact that the literature has not found consistent positive effects of PBD extensions on reemployment wages, unconditionally or conditional on duration (see section 2.3). Therefore this evidence seems to suggest that the effect of UI on nonemployment duration goes mostly through effort and not through job selectivity.

2.5.2 Directed Search vs. Reservation Wages

The earliest search models were reservation wage models since they focused on the job acceptance decision as the key choice variable. These models were then augmented with a search effort decision, as in the model laid out in Section 2.2. Similarly to the extent that surveys elicited information on the job search process they typically asked about reservation wages as well as various measures of search intensity. However, in recent years there has been growing interest in directed job search models (see, for example the review by Wright et al., 2021) in macroeconomics but also in the literature on the micro-foundations of job search. The interest in directed search models was likely in part driven by the fact that observed reservation wages are not straightforward to reconcile with the theoretical notion of reservation wages. For example, the fact that reservation wages are so high and not falling throughout the spell (Feldstein and Poterba, 1984; Krueger and Mueller, 2016) is confusing given that observed reemployement wages are much lower and falling throughout the spell.

From a theoretical perspective, it is also somewhat puzzling that reservation wages do not respond to UI extensions. In the model (equation 5) the reservation wage is directly linked to the value of unemployment $v(\phi_{t+1}) = (1-\delta)V^U_{t+1}$, since it is, by definition, the wage at which an individual is indifferent between remaining unemployed and taking a job.[15] However given that a UI extension typically increases nonemployment duration D, i.e. $dD/dP > 0$ it has to be that either search effort or the reservation wage responds to P. However both are directly linked to the value of unemployment and the model does not allow for de/dP to be positive without $dV^U_{t+1}/dP > 0$, therefore $dD/dP > 0$ implies $dV^U_{t+1}/dP > 0$, which in turn implies $d\phi^U_{t+1}/dP > 0$.

In a directed search model workers choose to apply for a specific job with a fixed wage at every point in time. The probability of actually receiving an offer is then a function of the wage target. See Nekoei and Weber (2017) for an example. In general the target wage should also be linked to the value of unemployment and thus respond whenever effort responds to UI. However if the distribution of offered wages has relatively few discrete mass points (e.g. offered wages for a given occupation are mostly constant) , workers may not change target wages in response to small changes in UI.

An advantage of the directed search model is that the target wage is in principle easier to observe than the reservation wage either directly or in survey data since it is simply the wage of the last applied for job. For example, in papers that rely on platform data can either proxy the target wage as the posted wage of applied for jobs or impute the wage of the applied for job using occupation and other characteristics.

In general, the predictions of a directed search model with target wages and a reservation wage model are very similar, especially in the absence of reservation wage / target wage data. It seems plausible, that in practice both mechanisms play a role in the real world. Some jobs are posted with and some without wage information and workers have imperfect information about what a given job may pay. Thus job search likely involves a directed search component where workers have to actively decide what types of jobs to apply for, but also face some uncertainty about what a job offer may look like and thus may reject offers that offers pay that is too low.

[15] Using this relationship, Shimer and Werning (2007) argued that estimates of reservation wage elasticity can be used to infer the welfare implications of UI changes.

2.5.3 Duration Dependence in Reemployment Wages

An important question is whether unemployment duration in itself has a negative, causal effect on job quality. Such a negative effect could arise for 2 reasons, which can be thought of as a supply side or a demand side effect. On the supply side, workers may become less selective with longer unemployment durations, i.e. lower their reservation wage or target wage. On the demand side, the long-term unemployed may be stigmatized by potential employers or they may lose skills that are valued by the labor market, thus facing lower demand for their labor.
To make this more precise let's frame this in the language of the basis search model from above.[16] Denote the reemployment wage of a worker who takes a job at time t as w_t^e, furthermore, let ϕ_t be the reservation wage in period t and μ_t be the mean of the wage offer distribution ($F_t(w)$). Finally, assume that w_t^e is linear in t and that only the mean of the wage offer distribution changes with t. Consider a small increase in nonemployment duration dt. The effect of this small increase will be through affecting either ϕ_t or μ_t:

$$\frac{dw_t^e}{dt} = \underbrace{\frac{\partial w_t^e}{\partial \phi_t}\frac{\partial \phi_t}{\partial t}}_{\substack{\text{Reservation Wage}\\ \text{Channel}\\ \text{Supply Side}}} + \underbrace{\frac{\partial w_t^e}{\partial \mu_t}\frac{\partial \mu_t}{\partial t}}_{\substack{\text{Skill Depreciation}\\ \text{Channel}\\ \text{Demand Side}}}$$

What do we know about the two channels? On the supply side, Table 2 provides an overview of the evidence that reservation wage or target wages are falling over the spell. The evidence suggests that reservation wages are falling by up to 5 percent per year, though the estimates are not very precise. Note that reservation wages may be falling with duration, i.e. $\frac{\partial \phi_t}{\partial t} < 0$, without impacting reemployment wages. In particular, if the reservation wage is not binding, i.e. below the lowest possible wage offer or $F_t(\phi_t) = 0$, then changing the reservation wage at t does not affect the reemployment wage, i.e. $\frac{\partial w_t^e}{\partial \phi_t} = 0$. In our basic search model the reservation will be not binding if the wage offer distribution is such that there are no wage offers below the reservation wage ($F_t(\phi_t) = 0$). Such a reservation wage implies that all offers are accepted, which is an optimal strategy if there is little variance in wage offers, job offers are rare, the person is very impatient, or it is

[16] The analysis here follows Schmieder et al. (2016). For a similar analysis based on a directed search model see Nekoei and Weber (2017).

relatively easy to move to better jobs once employed. It may well be that this holds for at least some individuals. Also note that the effect of the reservation wage on the reemployment wage, $\frac{\partial w_t^e}{\partial \phi_t}$, is likely less than 1 unless the reservation wage is at a point in the wage offer distribution where the density is large. Therefore we would expect reemployment wages to fall less than reservation wages. This is in fact confirmed by Massenkoff (2023), who provides some helpful evidence by regressing log reemployment wages on log reservation wages and detailed controls. He finds an elasticity is around 0.54, confirming that reemployment wages should fall less than reservation wages.[17]

It therefore makes sense that in Table 2 , target wages seem to fall less than reservation wages, between 0 and 1.5 percent per year. Intuitively, target wages represent the typical job a worker might get, while the reservation wage is only the lower bound. Since the lower bound will fall faster than the average reemployment wage, reservation wages would fall faster.

Turning to the demand side, there is some direct evidence suggesting that wage offers may decline with unemployment duration, while other papers find evidence of no impact. As discussed above, some audit studies found lower callback rates for the longterm unemployed (Kroft et al., 2013; Eriksson and Rooth, 2014), while others found no effect on unemployment durations on callbacks (Nunley et al., 2017; Farber et al., 2019). There are also 2 papers that directly estimate how cognitive skills vary through the unemployment spell. Edin and Gustavsson (2008) uses panel data from Sweden that were part of the International Adult Literacy Survey. Using 2 waves, 1994 and 1998, that were conducted 4 years apart they can observe about 600 workers over time. They find that workers with unemployment spells had losses in literacy scores and that these losses were larger the longer the individual was out of work. The estimates suggest that 1 year of unemployment reduces literacy scores by about 5 percentiles of the skill distribution. By contrast, using higher frequency data from Germany and a broader measure of skills (cognitive and non-cognitive) Cohen et al. (2023) find no evidence of skill depreciation either at the onset or during unemployment.

It is also possible to learn about the relationship between non-employment duration

[17] We know from Krueger and Mueller (2016) and others, that reservation wages are high and workers accept reemployment wages below the stated reservation wage. This goes against the model, so then it is not clear what to expect for the magnitude of $\frac{\partial w_t^e}{\partial \phi_t}$. This makes the evidence in Massenkoff (2023) particularly valuable.

and reemployment wages from UI extensions. Using the basic search model as a framework, Schmieder et al. (2016) show that one can write the effect of a marginal change in P on reemployment wages as:[18]

$$\frac{dE[w_t^e]}{dP} = \underbrace{E\left[\frac{\partial w^e}{\partial \phi_t}\frac{\partial \phi_t}{\partial P}\right]}_{\text{Reservation Wage Shift}} + \underbrace{\left[\frac{\partial w^e}{\partial \phi_t}\frac{\partial \phi_t}{\partial t} + \frac{\partial w^e}{\partial \mu_t}\frac{\partial \mu_t}{\partial t}\right]}_{\text{Effect of Nonemp. Dur on Wage}} \frac{dD}{dP}$$

Thus the effect of a PBD extension on average reemployment wages is the combination of two effects: First, workers become more selective so that the reservation wages increase, which then leads to higher reemployment wages.[19] Second, nonemployment durations increase $\frac{dD}{dP} > 0$ (either due to changes in the reservation wage or search effort), and thus reemployment wages decline due to the duration effect on wages. In the model P always has a weakly positive effect on reservation wages so that: $E\left[\frac{\partial w^e}{\partial \phi_t}\frac{\partial \phi_t}{\partial P}\right] \geq 0$. This implies that $\frac{\frac{dE[w_t^e]}{dP}}{\frac{dD}{dP}}$ provides an upper bound for the duration effect: $\left[\frac{\partial w^e}{\partial \phi_t}\frac{\partial \phi_t}{\partial t} + \frac{\partial w^e}{\partial \mu_t}\frac{\partial \mu_t}{\partial t}\right]$. As discussed before many papers find no effect of PBD on reemployment wages which would suggest that the duration effect is ≤ 0.

Schmieder et al. (2016) finds a negative effect of PBD on reemployment wages (see Figure 8 a) and provide an estimate for $\frac{\frac{dE[w_t^e]}{dP}}{\frac{dD}{dP}}$ of -0.8 percent, which would imply that reemployment wages fall by at least 0.8 percent per month. The paper furthmer shows that reemployment wages conditional on non-employment duration do not seem to be affected, which then implies $E\left[\frac{\partial w^e}{\partial \phi_t}\frac{\partial \phi_t}{\partial P}\right] = 0$, so that the upper bound of the duration effect is in fact the best point estimate. F Furthermore, since the reemployment wage does not shift with respect to P (even though theory implies that $\frac{\partial \phi_t}{\partial P} > 0$), this implies that $\frac{\partial w^e}{\partial \phi_t} = 0$ and therefore $\frac{\partial w^e}{\partial \mu_t}\frac{\partial \mu_t}{\partial t} = \frac{\frac{dE[w_t^e]}{dP}}{\frac{dD}{dP}}$. Thus, this implies that the demand side of the duration effect on reemployment wages induces a -0.8 percent loss in reemployment wages per month of unemployment (or a 10 percent loss over a year). Compared to the estimates of $\frac{dD}{dP}$, estimates of the effect on reemployment wages $\frac{dE[w_t^e]}{dP}$, tend to be less precise due to the large variance in reemployment wages. Thus, several papers found negative point estimates of the effect of UI extensions on wages (Card et al., 2007; van Ours and Vodopivec,

[18] Schmieder et al. (2016) provide a more general decomposition that does not rely on linearity.

[19] Note that the expectations operator is with respect to realizations of nonemployment duration t.

2008; Centeno and Novo, 2009), that are not statistically significant. Using a similar framework, Hernandez Martinez et al. (2023) sets out to estimate $\left[\frac{\partial w^e}{\partial \phi_t}\frac{\partial \phi_t}{\partial t} + \frac{\partial w^e}{\partial \mu_t}\frac{\partial \mu_t}{\partial t}\right]$ when reservation wages are binding by controlling for the reseration wage shift. After this correction, they find a very similar estimate, namely that reemployment wages are falling by about 0.75 percent per month of unemployment.

The clearest evidence for the effect of non-employment duration on wages would be an RCT that randomly varies time out of work, without affecting other channels that may affect wages. As the framework in Schmieder et al. (2016) makes clear, this is hard to do since every instrument that would affect non-emloyment duration by making unemployment more or less attractive will also affect reservation wages. The closest to such an experiment is perhaps Autor et al. (2015) for the related context of disability insurance (DI) applicants in the US. After applying for DI and before they receive a decision whether the application is approved, applicants cannot work without voiding their application. The paper uses exogenous variation in decision times stemming from randomly assigned examiners for applicants who eventually get denied and thus return to the labor force. They find that a one month increase in processing time reduces long-run annual earnings by about 2.4 percent. This is a substantially larger negative effect than the estimates in the UI context imply, which might be due to the special nature of applying for disability or to the specific sample, but it does further underscore the potential negative effect of non-employment on earnings capacity. Overall, the evidence on the effect of non-employment duration on reemployment wages is somewhat mixed. On the supply side the implied estimates range from 0 to -2 percent per year, depending on whether one goes with the evidence from reservation wages, target wages or the evidence from UI extensions. On the demand side the estimates range from 0 to -10 percent per year. Many estimates are not very precise and 95 percent confidence intervals cover a wide range of possible values. Given the importance of the parameter having more and better evidence would be clearly welcome.

2.5.4 Duration Dependence in Job Finding Rates

Policy makers have long been concerned with long-term unemployment, which has long been a challenge in Europe (e.g. Machin and Manning, 1999) and more recently in the US, after the Great Recession (Kroft et al., 2016). A key concern about long unemployment duration is, that being out of work itself has a detrimental effect on the labor market prospects of the unemployed, rendering them increasingly unable to return to work. This would explain the well-documented decline in job-finding rates as time out of work increases. For example the in section 2.3, we saw that job finding rates often decline by around 40-50 percent over the course of a year (see Figure 7). The notion that non-employment duration has a causal effect on job finding is typically referred to as ’true duration dependence’. However, whether there is actually true duration dependence, is not obvious. As we saw in section 2.3.4, the decline in the hazard (and wages) can also be fully explained by heterogeneity between individuals (see Figure 9) where workers with a high cost of effort and low wage offers remain unemployed longer and dominate the pool of the long-term unemployed. It is therefore hard to identify duration dependence from evidence on job finding rates and reemployment wages alone.

Mueller et al. (2021) and Mueller and Spinnewijn (2023) provide a helpful framework for analyzing duration dependence in job finding rates. Let $h_{i,t}$ be the probability of finding a job at duration d for an individual i. The observed duration dependence at duration d is the change in the average job finding probability from d to $d+1$: $E_d(h_{i,t}) - E_{d+1}(h_{i,t+1})$. Observed duration dependence can then be decomposed into the change in the job finding rate within individuals and a dynamic selection component:

$$\underbrace{E_d(h_{i,t}) - E_{d+1}(h_{i,t+1})}_{\text{observed duration dependence}} = \underbrace{E_d\left(h_{i,t} - h_{i,t+1}\right)}_{\text{true duration dependence}} + \underbrace{E_d(h_{i,t+1}) - E_{d+1}(h_{i,t+1})}_{\text{dynamic selection}} \tag{18}$$

This framework suggests two approaches for identifying true duration dependence. Either to directly estimate $E_d\left(h_{i,t} - h_{i,t+1}\right)$ or to estimate the degree of dynamic selection based on the observable worker characteristics.

Regarding the first approach, the main challenge is that one can never observe within-person changes in the job finding rate in the same unemployment spell, since once a job is found the job seeker leaves unemployment. However, recall, that in our basic model the job finding rate can be written as $h_{i,t} = f_t(e_t)(1 - F_t(\phi_{t+1}))$. Thus one can gain some insights from the new evidence on within-person changes in search effort and reservation wages. As we saw in Table 1 and the discussion in section 2.4, search effort appears relatively flat in most studies and thus cannot explain the large observed decline in job finding rates. Similarly Table 2 shows that reservation wages (and target wages) show a only a very small decline, which, as we discussed in section 2.5.3 likely only explains about a 0-2 percent decline in reemployment wages over a year. Thus the behavior of job seekers alone, is unlikely to generate much duration dependence .

Of course, even if behavior does not change much through the spell, duration dependence in the job finding rate could still exist via changes in either the wage offer distribution $F_t(.)$, i.e. due to skill depreciation or stigmatization, or the effectiveness of job search $f_t(.)$. Perhaps the only evidence clearly suggesting a decline in the effectiveness of search comes from the audit studies by Kroft et al. (2013) and Eriksson and Rooth (2014), though as we discussed before the evidence is somewhat mixed. Similarly, the evidence in the previous section on duration dependence in wages is also somewhat mixed. Furthermore, although at least some studies point to substantial negative duration dependence (section 2.5.3) in the order of up to 10 percent wage losses over a year, it is not clear how this duration dependence in wages translates into duration dependence in the job finding rate.

The second approach to learning about duration dependence is to estimate the degree of dynamic selection instead in order to infer true duration dependence from equation (18). Mueller et al. (2021) develop this approach in depth and show that the dynamic selection component is equal to the covariance between job finding rates in the current and next period. With this equation (18) can be written as:

$$\underbrace{E_d(h_{i,t}) - E_{d+1}(h_{i,t+1})}_{\text{observed duration dependence}} = \underbrace{E_d\left(h_{i,t} - h_{i,t+1}\right)}_{\text{true duration dependence}} + \underbrace{\frac{cov_d\left(h_{i,t}, h_{i,t+1}\right)}{1 - E_d(h_{i,t})}}_{\text{dynamic selection}} \tag{19}$$

This covariance between current and future job finding rates in turn consists of two components:

$$\underbrace{cov_d\left(h_{i,t}, h_{i,t+1}\right)}_{\text{Covariance of Job Finding Rates}} = \underbrace{var_d\left(h_{i,t}\right)}_{\substack{\text{Overall Variance of}\\ \text{Job Finding Rates}}} - \underbrace{cov_d\left(h_{i,t}, h_{i,t} - h_{i,t+1}\right)}_{\substack{\text{Transitory Heterogeneity}\\ \text{in Job Finding}}} \tag{20}$$

Thus dynamic selection is a function of the total variance in job finding rate minus the transitory part of this heterogeneity. If the job finding rate is constant within individuals, then the transitory component is zero and dynamic selection is simply a function of the total variance in job finding $cov_d\left(h_{i,t}, h_{i,t+1}\right) = var_d\left(h_{i,t}\right)$. On the other hand if the job finding rate has no constant component and is instead iid, then the variance is equal to the transitory component and there is no dynamic selection.

The indvidual level job finding rate $h_{i,t}$ is of course unobserved. However, we can learn about these components from the extent to which job finding rates are predictable from individual characteristics X_i. Note that any predictable variance $var_d(E_d(h_{i,t}|X_{i,t}$ provides a lower bound for $var_d\left(h_{i,t}\right)$. Based on this, Mueller and Spinnewijn (2023) show how with a prediction model for the job finding rate one can estimate a lower bound for the covariance term $cov_d\left(h_{i,t}, h_{i,t} - h_{i,t+1}\right)$ and therefore an upper bound for true duration dependence. To understand the intuition: imagine there are two types of workers, say high and low skill, and we observe that low skill workers have longer unemployment duration than high skill workers. This can only be explained by between group differences in job finding rates, but if such between group differences exist, then the fact that low skill workers take up a larger share of the long-term unemployed in part explains why job finding rates are lower for long-term unemployed. Overall, the better one can predict job finding rates (or non-employment durations), the higher (the lower bound of) the share of observed duration dependence that is due to dynamic selection.

Mueller et al. (2021) show that with high quality data it is indeed possible to predict job finding rates relatively well. While some of the variation is explained by typical labor market variables, a particularly valuable predictor stems from job seekers information about their job-finding probabilities. They obtain this information from the Survey of Consumer Expectations and the KM survey where workers are asked with what probability they expect to find a job in the future. They show that these elicited beliefs are in fact highly predictive of realized unemployment durations. Using the lower bound implied by equation 19, they show that the lower bound for dynamic selection is 52 percent when using elicitations only and as high as 89 percent when using beliefs and demographics. They also develop a more parametric statistical model, that allows to point-identify the two components of observed duration dependence. They find that heterogeneity explains 84.7 percent of the observed decline, leaving 15.3 percent of the decline to be due to true duration dependence.

Another way to identify heterogeneity in job-finding rates stems from using data where the same individual can be observed over multiple unemployment spells. Alvarez et al. (2023) use this approach to identify duration dependence and dynamic selection using data from Austria. They find little dynamic selection based on observables, but strong evidence for dynamic selection along unobservable dimensions and argue that dynamic selection accounts for a large share of observed duration dependence.

Mueller and Spinnewijn (2023) study duration dependence using detailed data from Sweden. An advantage of the Swedish data is that they can also observe multiple unemployment spells for the same individual. Extending the Mueller et al. (2021) by also looking at heterogeneity based on multiple spells, they find evidence that would suggest that 84 percent of the observed decline in job finding rates is due to dynamic selection, leaving only about 16 percent for true duration dependence.

Overall, this recent literature has made a strong case that dynamic selection is important, in particular along unobserved dimensions.[20] This leaves limited scope for true duration dependence, which in turn is consistent with the limited evidence of changing search effort and reservation wages / target wages over the unemloyment spell.

2.5.5 Present Bias vs. Exponential Discounting

One of the first and consistently documented behavioral biases in economics is the observation that individuals place extra weight on immediate pay-offs relative to the standard exponential discounting applied to a stream of future pay-offs. Building on earlier work by Strotz (1955), Laibson (1997) and O'Donoghue and Rabin (1999) established this notion of 'present bias' in the economics literature.

Laibson (1997) proposes to characterize the subjective present value of a future stream of utility flows as:

$$u_0 + \beta \sum_{t=1}^{T} \delta^t u_t$$

This implies a discount factor between today and the next period of $\beta\delta$, while the discount factor between any two periods in the future is simply $\delta > \beta\delta$. For long-

[20] Earlier papers that only explored a few observables characteristics concluded that the role of dynamic selection is fairly limited (Krueger et al., 2014; Schmieder et al., 2016; DellaVigna et al., 2017).

run decisions individuals are relatively patient, while impatient over short run decisions. Furthermore, preferences are time-inconsistent: when making decisions about future trade-offs they would exhibit more patience than when making the same decision about an immediate trade-off. E.g. consider a person who is deciding how much to save in a retirement account. If the person faces the decision in the form of putting away a certain percentage in a future paycheck (say in period 1, so that it affects consumption utility u_1), the trade-off is guided by exponential discounting and the person may elect to save. On the other hand when making the decision at the point of receiving the payment, thus affecting consumption utility u_0, the trade-off is guided by exponential discounting and the present bias factor β, which reduces the value of saving, potentially drastically.

DellaVigna and Paserman (2005) were first to systematically explore the implications of present bias for job search. They start by integrating present bias in the basic search model from section 2.2. To pin down behavior with present bias requires taking a stance whether individuals anticipate their own present bias for future decisions or not (O'Donoghue and Rabin, 1999). A sophisticated individual anticipates their own present bias for future decisions and as such has rational expectations. A naive agent incorrectly believes that they will behave as an exponential agent in the future.

Focusing on naive present bias, the value of unemployment of an unemployed individual with present bias becomes:

$$V_t^{U,naive} = \max_{s_t,\phi_{t+1}} u\left(b_t\right) - \widetilde{c}(s_t) + \beta\delta\left(s_t \int_{\phi_{t+1}}^{\infty} V_{t+1}^{E}(w) - V_{t+1}^{U}\, dF_t(w) + V_{t+1}^{U}\right), \quad (21)$$

where the future value functions are the value functions of an exponential discounter ($\beta = 1$). A key feature of this problem is that short-run impatience (β) does not affect reservation wages, which are based on the value functions describing future pay-offs, but it does sharply reduce search effort since the pay-off from search is discounted by β, while the search cost is immediate. DellaVigna and Paserman (2005) explore the comparative statics of the search model with respect to patience. With present bias, a more impatient job seeker (smaller β) will search less, the reservation wage is unaffected, and the exit rate will go down. With only exponential discounting, a more impatient job seeker (smaller δ) will search less, but the reservation wage will also go down. The paper argues that under plausible assumptions the reservation wage channel dominates and impatience actually leads to an increase in the job-finding rate. The paper tests the relationship between patience and the job-finding rate using the NLSY and PSID. They use proxies for impatience, such as having a bank account or smoking, and show that those are associated with a lower exit rate, even when controlling for detailed other characteristics. They take this as evidence for present bias.[21]

Further evidence on present bias stems from structural work. Paserman (2008) estimates a structural model in the spirit of DellaVigna and Paserman (2005) using data from the NLSY on unemployment duration and reemployment wages. By treating groups of worker as homogeneous, he can observe the reservation wage as the lowest accepted wage in that group. This indirect information on reservation wages together with job finding rates pin down the discounting parameters. The paper estimates β to be between 0.4 and 0.9, and δ between 0.996 and 1 (with lower wage workers being less patient). Since the paper nests the standard model ($\beta = 1$), the low estimates of β, with relatively tight SEs serve as a rejection of the standard exponential discounting model. Several other recent papers incorporate savings decisions as well as present bias into the search model and provide structural estimates. These recent papers take reemployment wages as fixed so that the choice variables are search intensity s_t and savings, i.e. assets in periot $t+1$: A_{t+1}. The value of unemployment can then be written as:

$$\begin{aligned} V_t^{U,n}(A_t) &= \max_{s_t \in [0,1]; A_{t+1}} u(c_t) - \widetilde{c}(s_t) + \beta\delta \left[s_t V_{t+1}^E(A_{t+1}) + (1-s_t) V_{t+1}^U(A_{t+1}) \right] \\ & \quad \text{subject to: } c_t = A_t + y_t - \frac{A_{t+1}}{1+R}, \end{aligned}$$

where R is the return on savings. The first paper to estimate such a model was DellaVigna et al. (2017) using data from Hungary. The paper focuses on how job finding rates were affected by a unique policy reform that introduced a step function into the UI benefit path. The paper estimates a structural model to explain how the hazard rates and especially the spikes at the exhaustion point respond to the policy reform. A key takeaway is that when endogenous savings decisions are added to the model, then the model can only generate spikes in the hazard

[21]One caveat, is that this argument relies on the reservation wage effect dominating the search effort effect for exponential discounters. However, as discussed above, reservation wages in general are not very responsive and potentially not binding, which may cast some doubt on the strength of this evidence.

rate with a very high degree of impatience. The reason is that patient job seekers would anticipate UI exhaustion and save in advance to smooth the consumption drop, but then, since consumption declines smoothly at exhaustion, there is no sudden increase (spike) in the hazard rate. When estimating the search model with exponential discounting, the obtain $\delta = 0.89$ on a biweekly level, but this would imply an annual discount factor of around 0.06.[22] Such a low annual discount factor is inconsistent with many other estimates from the literature as it would imply that individuals have an extremely short planning horizon and would, e.g., never save for retirement or other long-term goals. By contrast when the paper estimates the model with $\beta\delta$-preferences, the resulting β of 0.58 and δ of 0.995. This implies a yearly discount factor of 0.46 for the first year and 0.88 for subsequent years, which is consistent with other estimates of β the resulting yearly discount factor is high enough to allow for long-term planning. DellaVigna et al. (2022) estimate a similar search model using the results from the SMS survey as moments. Across a number of model specifications they come to the same conclusion. The exponential model results in an implausible low δ and worse fit compared to a model with $\beta\delta$-discounting, which typically results in estimates of β of around 0.4-0.5 with a plausible δ of around 0.995. As in DellaVigna et al. (2017), present biased is identified by the spike in the hazard rate, which the model can only fit if workers are impatient enough not to smooth consumption too much.

Another piece of evidence for present bias comes from Ganong and Noel (2019), who estimate essentially the same model but use the consumption path during unemployment as well as the job finding rate as moments to identify the model parameters. The key empirical pattern the model tries to rationalize is that consumption drops sharply at UI onset and at UI exhaustion but by a lower degree than income. Ganong and Noel (2019) show that this is hard to rationalize even with a multi-type model as long as the types all have the same discount factor. The problem is that if workers are patient, they smooth consumption and consumption does not drop sharply at benefit exhaustion. But if workers are impatient, the drop in consumption at exhaustion is as large as the income drop and too large relative to the data. To solve the puzzle, Ganong and Noel (2019) introduce heterogeneity in

[22] As we discuss in the next subsection 2.5.6, the main innovation of DellaVigna et al. (2017) is to introduce reference-dependence into the search model. In the discussion here, we focus on the version of the model with reference dependence, but the results for the standard model with respect to impatience are qualitatively similar.

the discount factor (either in δ or in β). Allowing for 2 types with different discount factors improves the fit of the exponential model. However, the exponential model would again require implausibly low δ. By contrast the model with $\beta\delta$-preferences fits better with plausible parameter values β between 0.5 and 0.9.

Finally, Gerard and Naritomi (2021) estimate a similar model based on the consumption data from Brazil (see section 2.4 above). A key innovation is that while the previous papers do not seek to distinguish different forms of present bias (usually focusing on naive, with the exception of Paserman (2008)), Gerard and Naritomi (2021) compare three versions: exponential, naive, and sophisticated present bias. Again they find that the exponential model implies implausible levels of impatience ($\delta = 0.69$ on the monthly level). Naive and sophisticated $\beta\delta$ preferences both can generate the broad pattern in their data, but the sophisticated present bias model (with $\beta = 0.7$) obtains a substantially improved fit. They argue that this is because a sophisticated agent anticipates self-control problems in the future, which leads her to save somewhat more in the present.

Overall, there is now very strong evidence for the importance of present bias. Over a wide range of contexts and with different types of empirical moment, models with present bias preferences perform substantially better and with much more plausible parameter estimates than models with only exponential discounting. It is also remarkable, that across all these contexts the estimates for β are quite consistently in a similar ballpark of around 0.5 to 0.7.

2.5.6 Reference Dependence

Since Kahneman and Tversky 1979's seminal 1979 paper a key insight in behavioral economics has been that individuals evaluate payoffs relative to some benchmark or reference point and that they value losses relative to this reference point higher than gains of the same magnitude (a feature known as "loss aversion"). DellaVigna et al. (2017), introduce reference dependence into the standard search model. The paper models the reference point as the average income over the $N \geq 1$ previous periods:

$$r_t = \frac{1}{N} \sum_{k=t-N}^{t-1} y_k.$$

N represents the length of adaption: the higher N, the longer an unemployed worker feels the loss utility from a drop in consumption.[23] Using this reference point, the paper models the utility from consumption as:

$$u\left(c_t|r_t\right) = \begin{cases} v\left(c_t\right) + \eta\left[v\left(c_t\right) - v\left(r_t\right)\right] & \text{if } c_t \geq r_t \\ v\left(c_t\right) + \eta\lambda\left[v\left(c_t\right) - v\left(r_t\right)\right] & \text{if } c_t < r_t \end{cases} \tag{22}$$

The utility functions consists of consumption utility $v\left(c_t\right)$ and gain-loss utility $v\left(c_t\right) - v\left(r_t\right)$. For consumption levels above the reference point ($c_t \geq r_t$), individuals receive gain utility $v\left(c_t\right) - v\left(r_t\right) > 0$, with a weight η. For consumption below the reference point ($c_t < r_t$), the individual suffer loss utility $v\left(c_t\right) - v\left(r_t\right) < 0$, with weight $\lambda\eta$. The parameter η captures the importance of gain-loss utility, while the parameter $\lambda \geq 1$ captures loss aversion. The standard search model is nested in this model for $\eta = 0$.

The unemployed choose search intensity e_t and consumption c_t in each period. The value function of a job seeker can then be expressed as:

$$\begin{aligned} V_t^U(A_t) &= \max_{e_t;A_{t+1}} u\left(c_t|r_t\right) - c\left(e_t\right) + \delta\left[f(e_t)V_{t+1|t+1}^E\left(A_{t+1}\right) + (1 - f(e_t))V_{t+1}^U\left(A_{t+1}\right)\right] \\ &\quad \text{subject to: } c_t = A_t + y_t - \frac{A_{t+1}}{1+R}. \end{aligned}$$

The model makes predictions about the job finding rate that are consistent with the observed patterns in section 2.3: Newly unemployed workers experience strong loss utility and are thus eager to find a job, resulting in a high exit rate. As they stay unemployed, the reference point adapts to the lower income level and workers get accustomed to being unemployed and search effort and the job finding rate decline. As workers approach UI exhaustion point, however, they face a large drop in consumption relative to their reference point and search effort should increase up to the exhaustion point. However after benefits are exhausted, adaption sets in again among individuals who are still unemployed and search effort declines. As a result the time path in search effort should look very similar to the typical hazard rates documented before. However, we saw in section 2.3, that the basic search model with enoguh heterogeneity can also generate an initial decline in the exit rate, followed by a spike at the exhaustion point. In order to test for the empirical

[23]For a discussion of alternative assumptions for the reference point, such as expectations based reference points as in Kőszegi and Rabin (2006), see DellaVigna et al. (2017).

importance of reference dependence, DellaVigna et al. (2017) therefore turn to a unique reform in Hungary in 2005. Prior to the reform UI benefits were constant for 9 months. After the reform UI benefits were increased in the first 3 months and reduced for the following 6 months. From month 10 onwards there was no change. The crucial difference between the standard and reference dependent model is, that the standard model is purely forward looking. Thus conditional on not finding a job in the first 9 months, the standard model predicts the same job finding rate before and after the reform. By contrast in the reference dependent model, the reference point creates a backward looking mechanism. As such, after 9 months a worker in the pre-period would have a higher reference point than a worker in the post period with the same non-employment duration (but lower income in the previous 6 months). DellaVigna et al. (2017) show that this pattern indeed holds in the data. To provide a formal test between the reference dependent and the standard model, they estimate the model structurally with and without reference dependence. They show that the reference dependent model provides a much better fit than the standard model and in particular can explain, quantitatively and qualitatively, the higher hazard rates from 9 months onwards in the post-period, which the standard model fails to match. Another area where the standard and reference dependent model create different predictions is how search effort would evolve within individuals over time. We saw in section 2.2 that if the only source of nonstationarity is UI exhaustion, then the standard model generates search effort that is increasing over time until the exhaustion point and then remains constant. The reference dependent model by contrast predicts a spike in search effort at UI exhaustion. The reference dependent model also predicts a reduction in effort at the beginning of the spell, but this may be dominated by the approaching exhaustion point (and thus the need to search more), so that this prediction is less distinct. As discussed in section 2.4, the two studies that provide clear evidence of the within person evolution of search effort around UI exhaustion Marinescu and Skandalis, 2021; DellaVigna et al., 2022 both found a spike in effort at the exhaustion point consistent with the reference dependent model. It should be noted though that there could be other sources of nonstationarity, for example searching may become more costly over time ($c_t(.)$) or less productive ($f_t(.)$). Furthermore skill depreciation ($F_t(.)$) may lead to lower job quality over time and thus may make search more attractive initially and less attractive later on. These other factors might also explain a decline in search effort over time and lead to a pattern resembling a spike in effort around the exhaustion point. DellaVigna et al. (2022) provide structural estimates of the standard search model without nonstationarity (apart from UI), the referenced dependent model and a model with time-varying search cost (calling it the 'discouragement' model). The estimates suggest that the standard model alone cannot fit the data (in particular the spike in effort), while both the reference dependent model and the discouragement model provide a good fit. The discouragement model can only do so however by also allowing for a very elastic (convex) search production function $f_t(.)$, with an elasticity of close to 2, which may seem implausible. On the other hand, the reference dependent model requires only an elasticity of 1, i.e. job finding is proportional to effort. The best fit is provided by a model with both reference dependence and search effort.

Reference points have also been proposed as a determinant of reservation wages. In principle, if pre-unemployment wages serve as a reference point, this could be an explanation for the relatively high reservation wages (on average close to the pre-unemployment wage) found in the literature. Reference dependence has also been suggested as a way to reconcile the cyclical properties of accepted and reservation wages (Koenig et al., 2016). There is also some experimental work consistent with this (see the discussion in Cooper and Kuhn, 2020). On the other hand, if previous wages would serve as a reference point, one would expect bunching in the post-unemployment wage distribution at the pre-unemployment wage (similar to bunching at round finishing times for marathon runners, see Allen et al., 2017), which to our knowledge has not been found.[24]

2.5.7 Biased Beliefs

A key determinant of how much a job seeker searches for a job is how they perceive how their effort e_t translates into the probability of finding a job $f(e_t)$. It seems plausible that workers have only a vague idea of what $f(e_t)$ actually looks like: job offers are rare and most workers have limited experience with unemployment over their life. It's also likely that the exact shape of $f(.)$ will be very indivdiual specific so that it is limited how much one can learn from other. Let $\hat{f}(e_t)$ be the perceived job search productivity of a worker. The value of unemployment (assuming fixed

[24] We are not aware of published work, but know informally that people have looked for this.

wages) then becomes:

$$V_t^U = \max_{e_t} u(c_t) - c(e_t) + \delta \left[\hat{f}(e_t) V_{t+1}^E) + (1 - \hat{f}(e_t)) V_{t+1}^U \right]$$

This perceived job search production function may be biased relative to the true production function. Spinnewijn (2015) classified such misperceptions into two categories:

Baseline-optimistic (-pessimistic): A baseline optimistic job seeker overestimates the probability of finding a job at a given search effort (or at all levels of search effot: $\hat{f}(e_t) > f(e_t)$. Similarly, a baseline pessimistic person underestimates the probability of finding a job at a given search effort (or at all levels of search effort: $\hat{f}(e_t) < f(e_t)$.

Control-optimistic (-pessimistic): A control optimistic person overestimates the marginal return of effort: $\hat{f}'(e_t) > f'(e_t)$, while a control pessimistic worker underestimates the marginal return: $\hat{f}'(e_t) < f'(e_t)$.

Biased beliefs can lead to search effort being inefficiently too high or too low from the individuals perspective. The first order condition for search effort is:

$$c'(e_t) = \delta f'(e_t) \left[V_{t+1}^E + V_{t+1}^U \right]$$

It is straightforward that a control-optimistic job seeker will search too much, since they overestimate the returns to their effort, while a control-pessimistic job seeker will search too little. Interestingly, a baseline optimistic worker with correct beliefs about the marginal returns to search effort will also search too little. The reason is that the baseline optimistic worker overestimates the probability of finding a job in future periods and thus overestimates V_{t+1}^U, but a higher value of unemployment leads to lower effort. Similarly, a baseline pessimistic worker will search too much. Whether or not workers have biased beliefs has potentially important policy consequences. For example, if job seekers underestimate the true productivity of search effort, interventions that unbias beliefs, such as providing information about job finding prospects could be welfare enhancing and potentially very cheap. Furthemore, Spinnewijn (2015) shows how the typical sufficient-statistics Baily-Chetty approach to calculating the welfare effects of UI reforms has to be modified to account for biases in beliefs.

There is relatively strong evidence that job seekers overestimate the probability of finding a job over a specified time period. For example, Spinnewijn (2015) reports a survey where the average job seeker expected to remain unemployed for an additional 6.8 weeks, while the true duration ended up about 23 weeks. This is clear evidence for baseline-optimism. Similar evidence for baseline-optimism can be seen in Mueller et al. (2021), where workers overestimate the probability of finding a job over the next 3 months and this overestimation actually increases for the long-term unemployed.

By contrast, it is difficult to obtain estimates of control-optimism/pessimism for two reasons: First, it is arguably harder to survey beliefs about marginal returns to effort than it is to ask about levels and second, it is hard to obtain unbiased estimated of the return to effort. Coming up with innovative designs either in the lab or the field could be a fruitful avenue for future work.

2.5.8 Locus of Control

Biased beliefs are closely linked to the psychological concept of locus of control. Locus of control refers to the subjective belief of an individual that their actions determine important life outcomes. Psychologists refer to a person who believes that outcomes are determined by factors outside of their control as having 'external locus of control'. On the other hand a person who believes that they have control over their life is referred to as having an internal locus of control. In the job search context, a person with internal locus of control would have relatively high $\hat{f}'(e_t)$, while a person with external locus of control relatively low $\hat{f}'(e_t)$. Note that this notion of locus of control is independent from whether the perceived differences in locus of control correspond to real differences (i.e. $\hat{f}'(e_t) = f'(e_t)$) or due to biased beliefs (such that an internal locus may correspond to control-optimism, while an external locus to control-pessimism).

There is some evidence that suggest that locus of control is indeed predictive of job search behavior: McGee (2015) uses NLSY data to show that a more internal locus of control is associated with somewhat higher reservation wages and more time spent on search. A similar analysis is providec by Caliendo et al. (2015) using the IZA evaluation dataset of unemployed workers in Germany. They also report that higher internality is associated with higher search effort and higher reservation wages. They also find that job finding rates are indeed higher among workers with a more internal locus of control, suggesting that the search effort effect dominates the reservation wage effect.[25]

2.5.9 Employer Collusion / Storable Offers

In most models of job search, worker search and if they accept a job offer, they start the job in the following period. In practice, this process may not be as simple: negotiating the terms of a contract may take time leading to a delay between a job offer and job acceptance. Furthermore, there may be some flexibility about the job start date, which may be some time after the job is accepted. Boone and van Ours (2012) propose that negotiations over the job start date could explain some of the patterns of the observed exit hazards from unemployment and, in particular, the spike at UI exhaustion. Consider a worker who is relatively content being unemployed, while on UI receives and prefers this to working, while also wanting to avoid being unemployed beyond the UI exhaustion point. Suppose such a workers receives a job offer 2 months before UI exhaustion. In this case they might prefer to 'store the offer', i.e. stay on UI until they exhaust their benefits and start working then. Thus the worker might negotiate a job start date to coincide with benefit exhaustion. In such a world, even if job offers arrive at a constant rate, the unemployment exit rate would be lower prior to UI exhaustion, then spike right at the exhaustion point and fall again afterwards (when all offers lead to job start dates as early as possible).

Boone and van Ours (2012) develop such a 'storable offers' search model where job start dates can be delayed if job seekers and employers agree. Importantly employers are more likely to accept such delays, for job offers with permanent contracts which have higher value to the employer and where the cost of short term delays is relatively less important. The model predicts a spike in unemployment exit at the UI exhaustion point and a larger spike for exits into permanent jobs than for exits into temporary jobs. The paper tests this prediction in Slovenian data and finds a clear spike at 3 different exhaustion points (for 3 different PBD groups) for exits to permanent jobs but a much more muted spike for exits to temporary jobs. This evidence is suggestive of the role of strategic delays. However the probability of accepting a permanent contract might be correlated with other worker characteristics. We saw in section 2.3.4 that the basic search model can

[25]See Cooper and Kuhn (2020) for a longer discussion of Locus of Control and how it relates to other concepts in psychology.

easily generate spikes in the hazard rate with enough heterogeneity. Therefore if worker types are sufficiently flexible and correlated with contract type, the basic model should also be able to generate different spikes for workers with permanent and temporary contracts.

Another way to test for strategic delays in job start dates is to directly measure the time gap between a job offer and a job start date. The storable offer model predicts that this gap should be larger for jobs that start at UI exhaustion, compared to jobs that start before or after. DellaVigna et al. (2022) calculate the job starting gap using the SMS survey data, which asked about job offer, acceptance and start dates. They show that the typical gap between job offer and start is around 30 days. For individuals starting a job in the month of UI exhaustion, the gap is only slightly larger with around 31.5 days, not enough to explain the large spike at the exhaustion point. They also provide an alternative test for the storable offer mechanism. If individuals delay job start dates to start at the UI exhaustion point, then for people exiting at the exhaustion point search effort should be lower in the weeks prior to the job start compared with people exiting after the exhaustion point. While they show that search effort declines about 3-4 weeks prior to a job start, the pattern is virtually identical for job start dates in the exhaustion month as for other job start dates.

Another form of collusion between workers and firms that could explain the spike in the hazard at UI exhaustion could occur in the case of temporary lay-offs that result in recall. For example, Katz and Meyer (1990) find a clear spike in recall rates at UI exhaustion. In principle, recalls can be quantitatively important: Fujita and Moscarini (2017) report a 50 % recall rate for the United States, while Nekoei and Weber (2015) report a 35 % recall rate in Austria (though other papers have found lower recall rates in other countries such as Hungary DellaVigna et al., 2017 or Germany DellaVigna et al., 2022). However, recalls do not explain the spike in the hazard at UI exhaustion by themselves. Katz and Meyer (1990) shows that the spike for unemployment exits to new employers is larger than for recalls and DellaVigna et al. (2017) and DellaVigna et al. (2022) both show that the spike and general pattern of the job finding rate is virtually identical when including or excluding recalls.

Overall, while there is probably some collusion leading to the timing of job start dates, either for jobs with new employers or recalls, the magnitude of this seems very limited and this channel is unlikely to play a large role in either explaining the spike at exhaustion or the disincentive effect of UI. Another form of collusion between workers and employers in the UI context can occur with respect to the timing of job separations to exploit features of the UI system, e.g. to allow a bridge into retirement. We will return to this in section 3.

2.5.10 Learning / Information

The standard search model assumes that workers are fully informed about the job offer arrival rate (or the productivity of job search) and the wage offer distribution. This may well be unrealistic given that these are hard to observe and likely exhibit substantial variation between individuals and across time. It seems possible then that workers have imperfect information about the job search process at the beginning of the unemployment spell and gradually learn throughout the spell as they observe the rate of job offers and their associated wages.

Potter (2021) develops a model where job seekers are imperfectly informed about the stochastic nature of the job search process. He assumes a search production function that takes on a Poisson process:

$$f(e_t) = 1 - exp(-\lambda e_t)$$

which describes the probability of receiving a job for a given time spent on job search e_t, where λ captures the effectiveness of job search. Workers do not know the true value of λ, but instead form beliefs which take on the form of a gamma distribution with two parameters α and β, describing the mean and variance of the true location of λ. While search productivity is thus unknown, workers have perfect information about the wage offer distribution. Workers update their beliefs about search productivity based on how much they are searching and whether or not they receive offers. The longer a worker is searching without receiving an offer the more she updates her belief that the productivity of job search λ is low. If she receives an offer her beliefs about λ increase. Thus as workers remain unemployed and search without offers, they become more pessimistic about the productivity of search effort. This pessimism has two impacts on search effort, it reduces the perceived productivity of search, which leads to lower search effort, but it also reduces the value of unemployment since search in future periods is also less productive.

The paper shows that the first effect typically dominates and that this leads to a reduction in search effort and reservation wages over the course of the unemployment spell. The main piece of empirical evidence provided by the paper is that the model predicts that learning is a function of time spent on job search, not on unemployment duration by itself. Thus of two individuals with the same unemployment duration, the one who has spend more time searching in prior periods without obtaining an offer should exert less effort today. On the other hand individuals who have received (and rejected) job offers in the past positively update their beliefs about the productivity of search and search more today. Potter (2021) tests this prediction in the KM survey and indeed finds that job search in the current period is negatively associated with job search in prior periods and positively associated with previous job offers. The paper also goes on to estimate the model structurally and finds that job seekers at the beginning of the unemployment spell overestimate their job finding prospects by about 60%.

An alternative explanation for a negative effect of past search on present search could be stock-flow matching (Coles and Petrongolo, 2008). Suppose that for a given job seeker there is a finite set of vacancies that match her qualifications or fit her interests. When becoming unemployed a worker can apply to all jobs among this stock of vacancies. However, once the job seeker has applied to the whole stock, going forward she is constraint by the available flow of new vacancies that open up in her field. A worker who searches more in one period may be more likely to exhaust the stock and thus be forced to search less in the next period. The stock-flow model can explain the negative effect of past search on current search, but does not explain why past offers have a positive effect on current search. However having more direct evidence on learning and stock-flow models would be very helpful.

2.6 Discussion

The advent of high-quality administrative data with credible, causal research designs has revealed several clear stylized facts about the job search process. However, such data alone is not sufficient to learn about important underlying mechanisms that drive job search. The arrival of a broad array of data that sheds light on the underlying mechanisms of job search has dramatically deepened our understanding.

Some of the core lessons are that search effort is probably more important in shaping search outcomes than reservation wages. Job seekers likely exhibit present bias that leads to too little search. Dynamic selection is an important driver of changes in aggregate hazards and reemployment wages, limiting the extent to which there is true duration dependence. Several other refinements to the search model have been proposed and found some supportive evidence, such as reference dependence, learning, or biased beliefs about search effort productivity.

Why do job seekers spend so little time on job search? Papers have shown time and again that unemployed workers spend only about 60-90 minutes per day on job search, a tiny fraction of the time they would work at a job. Either the (perceived) returns to searching more must quickly diminish within a day or the marginal cost of search must increase rapidly as workers search even just above an hour. The problem is that low returns to search explanations appear at odds with the fact that stronger incentives via lower UI, clearly affect search outcomes. On the other hand it is not clear why the cost of job search should go up so fast to keep search effort at such a low level. Such a high cost may come from psychological factors. For example, Ahammer and Packham (2023) find big negative effects of unemployment on mental health. Being unemployed and receiving rejections may also impact workers self-esteem, a theory proposed by Kőszegi et al. (2022). More rigorous research on these outcomes and how they shape job search would be very interesting.

3 Design of UI Policy

Unemployment Insurance (UI) provides income replacement to workers who lose their jobs involuntarily. This section discusses the design of Unemployment Insurance. We first present standard frameworks of optimal UI initially introduced by Baily (1978) and extended by Chetty (2006, 2008). Second, the section shows how to quantify the welfare effects of UI, with a specific emphasis on the most recent estimates of the social value of UI. It compares various UI policies using new estimates of the Marginal Value of Public Funds of UI policies in the US and in Europe (MVPFs, see Hendren and Sprung-Keyser, 2020). Third, it discusses how to take into account effects on wages, and on pre-unemployment separation rates when designing UI programs. Fourth, the section asks whether benefits should vary within the unemployment spell or over the business cycle, and discusses how UI interacts with other social policies. Fifth, it discusses macro effects of UI. Overall, we draw seven lessons from the section. Before presenting the Baily-Chetty framework, we recall briefly the main institutional features of UI policies.

3.1 The Structure of Unemployment Insurance Policies

Unemployment Insurance provides income replacement to workers who lose their jobs involuntarily.[26] In general, not all job losers are eligible for UI benefits. To be eligible, workers must have worked for a minimum period or earned a minimum amount of wages (and thus have significantly contributed to the UI fund). For example, in France, the previous work requirement amounts to six months over the two years before separation (2024 rules). In the US, the previous work requirement is known as the monetary requirement. In California, in 2024, workers must have earned at least $1,300 in one of the quarter of the year before losing their job. To be eligible, workers must also satisfy a non-monetary requirement. They must be deprived of work involuntarily, because they have been laid off. Job quitters and workers fired for misconduct are not eligible for UI benefits (in some countries, they may be after a waiting period). They must be searching for jobs actively.

When eligible, and conditional on registering their claim, UI claimants receive weekly or monthly benefits for a fixed period of time. The level of benefits b is usually set as a fraction of previous wages, and subject to a maximum amount. The corresponding replacement rate varies across countries (e.g. around 80% in Sweden vs. around 60% in France) and across workers in the same country. The replacement rate generally decreases with pre-unemployment wages (either by design or because there are caps at maximum benefit level). The fixed period of time during which benefits are paid is known as the Potential Benefit Duration (PBD). The PBD may also vary as a function of pre-unemployment work experience. In some exceptional cases, there is no exhaustion of UI benefits after a fixed period (e.g. in Sweden in the 2000s, or in Belgium). While in simple UI systems, benefits are constant within the claiming period (and until the end of the PBD), some countries implement more complex schedules where benefit levels decrease with unemployment duration (for example in Hungary, Sweden, or Spain).

[26]See Schmieder and von Wachter (2016) for a more detailed description of the structure of UI across various countries.

In this section, we discuss how to choose both benefit levels and PBD. We present next how welfare considerations can guide the policy choice.

3.2 The Welfare Effects of Unemployment Insurance

The design of Unemployment Insurance is guided by its effects on workers' welfare. Unemployment Insurance increases workers' welfare as it provides replacement income and allows workers' to smooth consumption over time. Unemployment Insurance also bears welfare costs. As highlighted in the previous section, generous unemployment insurance slows job finding theoretically and empirically. The workers' behavioral response increases government spending on benefits, which is costly. It requires to increase taxes to satisfy the government budget constraint and in turn, reduces workers' welfare. Unemployment Insurance generates a fiscal externality. Baily (1978) first highlighted those tradeoffs in his seminal work. We describe a simple version of the Baily (1978) approach. We discuss the general application of the Baily approach as advocated by Chetty (2006). We introduce the Baily-Chetty approach in a dynamic environment, which allows to compare the welfare effects of various UI policy parameters (as in Schmieder and von Wachter, 2016).

Baseline Framework The simple model has one single period. At the beginning of the period, workers are unemployed and receive benefits b. They choose search effort s which pins down their job finding rate, and sets the expected length of their unemployment spell $1 - s$. Workers face convex increasing search costs: $\psi(s)$ ($\psi' > 0, \psi'' > 0$). When employed, workers receive a gross wage w and pay taxes τ. In the simple model, we assume that wages are fixed. This is an important departure of the simple Baily framework from the classical job search model of the previous section. We discuss this point later.

Workers derive utility $u(b)$ when unemployed and $v(w - \tau)$ when employed. The utility functions can be different between both states, but they are both increasing and concave in consumption. In the simple model, workers consume their state-specific income and cannot transfer income across states and do not have asset.

Workers then solve the following problem:

$$\max_{s} \,(1-s)\,u(b) + sv(w-\tau) - \psi(s). \tag{23}$$

Workers' optimal behavior is characterized by the first order condition:

$$v(w-\tau) - u(b) = \psi'(s). \tag{24}$$

It states that at the optimum, the marginal cost of search equals the marginal return of switching to employment. It captures the same tradeoff as the more involved dynamic first order condition of the job search model (Equation 6 in Section 2.2). Equation (24) implicitly defines the optimal search effort, and consequently unemployment duration as a function of unemployment benefits: $s(b)$. Differentiating Equation (24) yields that search effort decreases with benefits b $\left(\frac{ds}{db} = -\frac{u'(b)}{\psi''(s)} < 0\right)$. The social planner chooses the level of benefits b and of taxes τ to maximize workers' welfare. The social planner takes as given workers' behavioral reactions to unemployment benefits (she cannot enforce search effort directly). It is subject to a budget constraint, where benefits are financed through taxes: $(1-s(b))b = s(b)\tau$. Formally, the social planner solves the following problem:

$$\max_{b,\tau} \widetilde{W}(b,\tau) = (1-s)\,u(b) + sv(w-\tau) - \psi(s) \tag{25}$$

such that

$$\begin{cases} v(w-\tau) - u(b) & = \psi'(s) \\ (1-s(b))b & = s(b)\tau \end{cases}$$

We note that the budget constraint defines taxes as a function of benefits: $\tau(b) = (1-s(b))b/s(b)$. This allows to write workers' welfare as a function of benefits only and simplifies solving the social planner's problem. Workers' welfare derivative wrt b then writes:

$$\frac{d\widetilde{W}}{db} = (1-s(b))u'(b) - s(b)v'(w-\tau)\frac{d\tau}{db} + \frac{ds}{db}\underbrace{\left(-\psi'(s(b)) + v(w-\tau) - u(b)\right)}_{=0\text{, envelope theorem}} \tag{26}$$

where the last term is zero because of the first order condition of the workers' program (Equation 24). To write $d\tau/db$, we differentiate the government budget constraints. To keep the budget balanced after a db benefit increase, the social planner needs to increase taxes through two channels. First, holding workers search effort constant, taxes have to increase by $(1-s)db/s$. Second, as workers spend more time unemployed, taxes must be further increased by $-bds/s^2$. Replacing this expression of $d\tau/db$ in Equation (26), we obtain:

$$\frac{d\widetilde{W}}{db} = (1-s(b))(u'(b)-v'(w-\tau)) + \frac{v'(w-\tau)b}{s}\frac{ds}{db} \tag{27}$$

$$\frac{d\widetilde{W}}{db} = \underbrace{(1-s(b))(u'(b)-v'(w-\tau))}_{\text{Welfare gain}} - \underbrace{\frac{v'(w-\tau)(1-s)}{s}\eta_{1-s,b}}_{\text{Welfare cost}} \tag{28}$$

where $\eta_{1-s,b}$ is the elasticity of unemployment duration $(1-s)$ with respect to the benefit level b. The first term corresponds to welfare gains. As the marginal utility when unemployed is higher than the marginal utility when employed ($u'(b) > v'(w-\tau)$), transferring income and consumption from the employment state to the unemployment state increases workers' welfare. On the other hand, the second term is negative (recall that $\eta_{1-s,b} > 0$, as $ds/db < 0$). This corresponds to the welfare cost of providing insurance due to the fiscal externality. As unemployment duration increases, extra taxes are levied on wages and workers' welfare when employed decreases. At the optimum, the social planner chooses the benefit level so that marginal welfare gain and cost are equal:

$$\frac{u'(c_u)-v'(c_e)}{v'(c_e)} = \frac{1}{s}\eta_{1-s,b} \tag{29}$$

where c_u (resp. c_e) is the consumption when unemployed (resp. when employed). Equation (29) is known as the Baily-Chetty formula. This formula provides a direct mapping between theoretical welfare effects and empirical counterparts. Many studies estimate the elasticity of unemployment duration wrt benefit generosity. The previous section describes some of them in details, and we discuss the order of magnitude of elasticity estimates from a wider review of the literature in the next section. Estimating the welfare gains involves quantifying the marginal utility change from employment to unemployment. We discuss empirical strategies to identify this change in the next section.

The baseline Baily framework makes strong assumptions on workers' behavior, for example about their access to other consumption smoothing instruments. Following Baily (1978), several studies (Flemming, 1978; Brown and Kaufold, 1988; Lentz, 2009) successfully enriched the underlying job search model showing that the optimal level of benefits depends on various primitives. Those papers eventually quantify the optimal level of UI generosity performing structural estimation of the underlying model. While they provide important and relevant quantifications, including that of deep primitive parameters, Chetty (2006) shows that the reduced-form quantification of the Baily formula is actually sufficient to assess UI optimality. Under a general class of models, the Baily-Chetty formula holds.

To illustrate Chetty (2006) point, we take the example of allowing workers to borrow against their future wages. Suppose that in the augmented model, consumption when unemployed is $c_u = b + a$ with $a > 0$ and consumption when employed is $c_e = w - \tau - a$. Workers now choose both search effort s and borrowings a. This yields an extra first order condition related to borrowing choice $(1-s)u'(c_u) = sv'(c_e)$, while the first order condition related to search effort remains the same. In the augmented model, the social planner also takes into account that workers choose a to smooth consumption over states. However, as workers already optimize over their borrowings, the envelope condition holds and the first order condition of the social planner remains the same as in the baseline model. This implies that the Baily-Chetty formula holds. Intuitively, for any given level of benefits b, the change in marginal utility across states in the augmented model is lower than in the baseline model. Consequently, the optimal level of benefit in the augmented model may be lower than in the simple model. That being said, the optimal level of benefits is such that the Baily-Chetty formula holds. It remains sufficient to identify two statistics in the data - the change in marginal utility across states and the elasticity of unemployment duration wrt benefit level - to test whether unemployment insurance is optimally set.

Dynamic Framework While the static version of the Baily-Chetty formula captures the key trade-off inherent in formulating optimal UI policy, it does not directly speak to the design of Potential Benefit Duration, a key policy variable in practice. For this reason, it is useful to consider a dynamic version. Here, we closely follow the model in Schmieder and von Wachter (2016), which delivers the dynamic Baily-Chetty style formula for both changes in benefit levels (Chetty, 2008) and for PBD extensions (Schmieder et al., 2012).[27] The Unemployment Insurance policy is implemented through two main parameters: benefit level and Potential Benefit Duration. Under standard UI rules, the flow of UI benefits b_t is equal to constant b until unemployment duration reaches the maximum PBD P when it drops to 0. The dynamic framework allows to characterize both the optimal benefit level and optimal PBD.

The model is set in continuous time. We consider workers becoming unemployed at date $t = 0$, and we denote W the value function at the beginning of the spell. At each date t, unemployed workers search with intensity s_t, normalized so that it represents the instantaneous job finding rate. Searching with intensity s_t incurs cost of $\psi_t(s_t)$, which may vary over time. As in the baseline model, wages are fixed and equal to $w - \tau$. Jobs are permanent over the problem time horizon T. As previously, workers are hand-to-mouth. The consumption when unemployed $c_{u,t}$ is equal to their benefits at date t: b_t. The consumption when employed is $c_e = w - \tau$. The value function of the unemployed writes:

$$W = \int_0^T \left[S_t u(c_{u,t}) + (1 - S_t)v(c_e) - S_t\psi_t(s_t)\right] dt \tag{30}$$

where $S_t = \exp\left(-\int_0^t s_u du\right)$ is the survival rate of unemployed workers. Given a sequence of job finding (or exit) rates from date 0 to date t, S_t is the share of the initial unemployed pool still searching for jobs at date t (i.e. the survival rate until time t). Workers choose the search effort sequence s_t to maximize their welfare W. Let us denote $\widetilde{W}(b, P, \tau)$ workers' welfare under the optimal search effort. We rewrite the welfare function to make the dependence in the policy parameters explicit:

$$\widetilde{W}(b, P, \tau) = \int_0^P S_t u(b)dt + \int_P^T S_t u(0)dt + \int_0^T (1 - S_t)v(w - \tau)dt - \int_0^T S_t\psi_t(s_t)dt \tag{31}$$

where s_t and S_t are to be understood as the optimal workers' choice from now on.

As previously, the social planner maximizes workers' welfare under the budget constraint. Let us denote the expected duration of receiving benefits $B = \int_0^P S_t dt$

[27] The key simplification in Schmieder and von Wachter (2016) is to assume hand-to-mouth consumers and to set the model in continuous time. The resulting Baily-Chetty formulas are however virtually identical to the general case with endogenous consumption and capture the same intuition.

and the expected non-employment duration $D = \int_0^T S_t dt$. The budget constraint is $Bb = (T - D)\tau$, which defines τ as a function of b and implicitly of P through both expected duration B and D. The social planner problem writes:

$$\max_{b,P,\tau} \widetilde{W}(b, P, \tau) \tag{32}$$
$$\text{such that } Bb = (T - D)\tau$$

We first differentiate the social planner objective wrt the benefit level. As previously, any endogenous change in search effort has no effect on marginal welfare (envelope theorem). We obtain the following expression for the change in welfare:

$$\frac{d\widetilde{W}}{db} = Bu'(b) - (T - D)v'(w - \tau)\frac{d\tau}{db} \tag{33}$$

From the budget constraint, we have $\frac{d\tau}{db} = \frac{1}{T-D}\left(B + b\frac{dB}{db} + \tau\frac{dD}{db}\right)$. After rearranging and rescaling, the marginal welfare effect of an increase of \$1 in instantaneous benefit (expressed in marginal utility of employed workers) writes:

$$\frac{d\widetilde{W}}{db}\frac{1}{v'(w-\tau)} = \underbrace{B \times \frac{u'(b) - v'(w-\tau)}{v'(w-\tau)}}_{\text{Mechanical transfer to unemployed}} - \underbrace{\left(b\frac{dB}{db} + \tau\frac{dD}{db}\right)}_{\text{Behavioral cost}} \tag{34}$$

Following a \$1 increase in instantaneous benefits, the unemployed receive a total mechanical transfer of B dollars (over the covered unemployment spell without behavioral changes). As usual, transfers related to behavioral changes do not contribute to welfare. The value of the mechanical transfer depends on the difference between the marginal utility of unemployed and employed. In sum, compared to the welfare analysis in the static model, welfare gains are unchanged. On the contrary, the expression of welfare costs now involves two channels. When they slow down job finding, unemployed receive extra benefits that lead the social planner to raise taxes (by $b\frac{dB}{db}$). This channel is similar as in the static model. However, in the dynamic model, there is an extra behavioral cost related to the higher share of unemployed whose benefits exhaust. Even if they do not receive extra benefits after exhaustion, they are not employed and do not pay taxes. Consequently, the social planner further increases taxes on the employed to balance the budget (by $\frac{dD}{db}\tau$).

We now turn to the first order condition related to an increase in Potential Benefit Duration (P). The detailed derivation is reported in the online Appendix. The marginal welfare effect of an increase in PBD (also expressed in marginal utility of employed) writes:

$$\frac{d\widetilde{W}}{dP}\frac{1}{v'(w-\tau)} = \underbrace{S_P b \times \frac{\tilde{u}'(b)-v'(w-\tau)}{v'(w-\tau)}}_{\text{Mechanical transfer to unemployed}} - \underbrace{\left(b\int_0^P \frac{dS_t}{dP}dt + \tau\frac{dD}{dP}\right)}_{\text{Behavioral cost}} \tag{35}$$

where we define $\tilde{u}'(b) = (u(b) - u(0))/b$. When increasing PBD, the social planner transfers income to all workers who would have exhausted their benefits otherwise. As those workers represent a surviving share S_P of the initial pool, the total mechanical transfer has a dollar value equal to $S_P b$. The effect on the exhaustee utility of a \$1 transfer is $(u(b) - u(0))/b$, which corresponds to the average marginal utility between 0 and b. Consequently, $\tilde{u}'(b)$ is comprised between $u'(b)$ and $u'(0)$. The behavioral cost has a new first component compared to the behavioral cost of a db increase. In that case, the first term $b\int_0^P \frac{dS_t}{dP}dt$ represents benefits paid to workers who reach the exhaustion date because they slow down their job finding following the PBD extension.

To compare the welfare effects of both policy changes, we rescale Equation (34) and (35) so that they each represent the effect of a one dollar transfer to the unemployed. This creates the classic dynamic versions of the Baily-Chetty formula for benefit levels and durations:

$$\frac{d\widetilde{W}}{db}\frac{1}{Bv'(c_e)} = \underbrace{\frac{u'(c_{u,t\leq P})-v'(c_e)}{v'(c_e)}}_{\text{Social value of \$1}} - \underbrace{\left(\eta_{B,b} + \eta_{D,b}\frac{D}{B}\frac{\tau}{b}\right)}_{\text{Behavioral cost of \$1}} \tag{36}$$

$$\frac{d\widetilde{W}}{dP}\frac{1}{S_P b v'(c_e)} = \underbrace{\frac{\tilde{u}'(c_{u,t=P})-v'(c_e)}{v'(c_e)}}_{\text{Social value of \$1}} - \underbrace{\frac{1}{S_P}\left(\int_0^P \frac{dS_t}{dP}dt + \frac{dD}{dP}\frac{\tau}{b}\right)}_{\text{Behavioral cost of \$1}} \tag{37}$$

where $\eta_{B,b}$ is the elasticity of expected duration of covered unemployment wrt benefit level and $\eta_{D,b}$ is the elasticity of non-employment duration.

At the social planner optimum, the marginal effect of either policy parameter on workers' welfare is equal to zero. Consequently, to test if the current UI system is optimal, it is sufficient to compute estimates of the consumption-smoothing value and estimates of the behavioral costs, to take the difference between the two estimates and to compare it to zero. We now review estimates of each term.

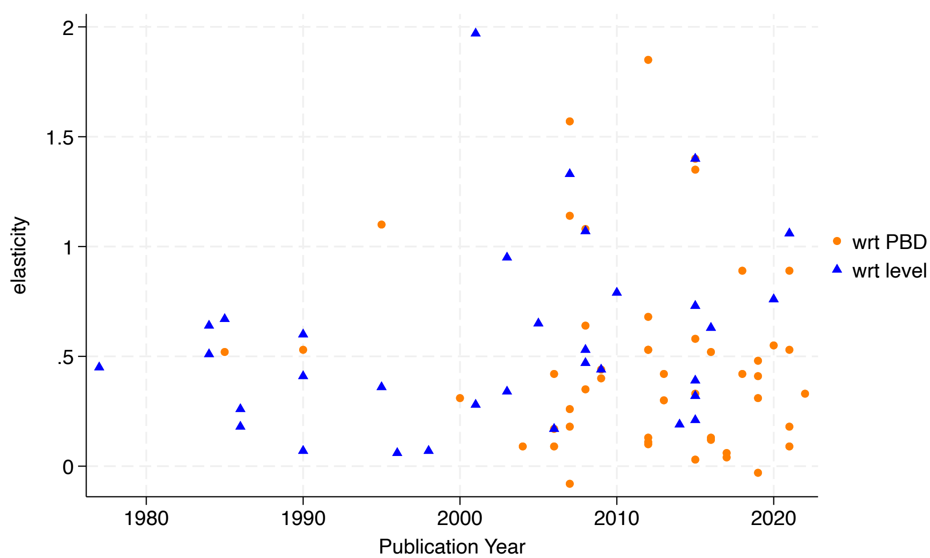
3.3 Quantification of Behavioral Costs

We first review estimates of the behavioral costs and then estimates of the consumption smoothing value.

The behavioral costs of Unemployment Insurance programs depend on their disemployment effects: the elasticity of covered unemployment and non-employment duration wrt benefit level, and the marginal effect of potential benefit duration on the survival curve and on non-employment duration. There is a large literature estimating those UI effects on labor supply. We discuss some recent papers estimating UI effects on labor supply in Section 2.3 and 2.4. The overall evidence is summarized in excellent reviews (Meyer, 2002; Krueger and Meyer, 2002; Schmieder and von Wachter, 2016; Lopes, 2022; Cohen and Ganong, 2024). The recent meta-analysis of Cohen and Ganong (2024) gathers almost 60 UI elasticity estimates from 52 studies published before 2022. Their meta-analysis focuses on the effects of replacement rates and of PBD on either non-employment duration, or covered unemployment duration. Figure 10 plots the estimates collected by Cohen and Ganong (2024) by publication dates. Elasticity estimates are almost all strictly positive implying disemployment effects of both Potential Benefit Duration (circles in orange) and benefit level (triangles in blue). Before interpreting their magnitude, we discuss recent trends in estimation methodology.

Since the mid-1990s, empirical research on UI effects is an important contributor to the credibility revolution. It has developed a series of specific designs to identify causal elasticity estimates. The main identification threat in empirical UI studies is a classical selection issue. Unemployed workers select into unemployment insurance categories based on unobservables. For example, in many countries, the Potential Benefit Duration depends on past work history. High-experience workers are eligible to longer PBD when they become unemployed. Then, comparing workers with long vs. short PBD does not allow to identify the causal effect of PBD as it is confounded by workers' unobserved productivity. In this example, observing past work experience helps to solve the selection issue. However, how much it helps depends critically on the PBD rule itself and on the data quality. When

Figure 10: Elasticity Estimates of Unemployment Duration wrt Potential Benefit Duration or Benefit Level



Notes: This figure presents elasticity estimates gathered in the review by Cohen and Ganong (2024) (Appendix Table B-1 and B-2 from which we exclude six outliers below -1 and above 2). The estimates are for the elasticity of unemployment duration wrt Potential Benefit Duration (PBD) in orange circles and wrt benefit level in blue triangles. The x-axis corresponds to the year of publication of the study.

PBD is a deterministic function of past work experience with some discontinuous jumps, causal effects can be obtained through Regression Discontinuity Designs (Card et al., 2007). One can then compare the unemployment duration of workers in high vs low PBD categories in a neighborhood of the PBD-rule discontinuity cutoff. Such a quasi-experimental design requires high-quality data on previous work experience to correctly identify workers in a neighborhood around the cutoff, and large initial samples so that the final selected sample of local comparison is large enough to detect effects with reasonable statistical power. Because of those data constraints, a significant share of available UI elasticity estimates since the 2000s are from Europe where large administrative datasets are accessible to researchers.

The UI empirical research also features some of the first applications of Regression Kink Designs (Card et al., 2015a,b; Landais, 2015), which leverage discontinuous changes in the slope between the policy variable of interest and a running/selection variable. This design is used to identify the effects of benefit level specifically, as caps on benefit levels generate such kinks in the relationship between previous wages and benefits.

Another widely-used design in empirical UI studies is the difference-in-difference methodology. In the ideal case, the DiD design leverages an exogenous reform in policy parameters that affects only a subpopulation of workers and thus yields a natural control group. This method is less demanding in terms of data quality. It can be implemented either with administrative data or with survey data to the extent that they correctly identify workers impacted by the reform vs the untreated control group. One important identification assumption is that of exogenous reform, which in practice may be violated (Card and Levine, 2000). When labor market conditions worsen, policymakers may increase the generosity of unemployment insurance, as a countercyclical stabilization policy. Then, labor market conditions confound the effect of UI generosity. In some countries, like the US, the UI generosity rules are even countercyclical by design. When state-level unemployment rates reach certain pre-determined thresholds, Potential Benefit Duration is increased through the Extended Benefit and Emergency Unemployment Compensation (EUC) programs (Rothstein, 2011). Such countercyclical rules push US UI research to develop trigger design that control for a flexible (but parametric) function of unemployment when regressing unemployment duration on state-level PBD. Causal identification is then obtained from the discontinuous jump of PBD at unemployment triggers. In recent work, Chodorow-Reich et al. (2019) further leverage ex-post revision in state-level unemployment rate to focus identification on trigger events generated by measurement errors. An alternative strategy is to analyze non-automatic and politically-motivated changes in UI rules (see Card and Levine, 2000; Johnston and Mas, 2018, for such studies in the US). In Europe, few countries have automatic countercyclical rules (except France recently) and many studies implement DiD designs credibly (for example van Ours and Vodopivec, 2008; Le Barbanchon et al., 2019).

In their review of 22 studies, Schmieder and von Wachter (2016) report that the average PBD elasticity is 0.41, and the average elasticity wrt replacement rates is 0.6. Cohen and Ganong (2024) who review twice as many papers find average of published elasticities in the same ballpark: 0.49 wrt PBD and 0.40 wrt replacement rate.

Beyond the average disemployment effects, those studies find interesting heterogeneity between the US and Europe, and over the business cycle. That heterogeneity has implications for targeting UI policies we discuss later.

The elasticity estimates also vary depending on the type of duration outcome considered. Some studies rely on administrative unemployment registers that record covered unemployment only. Such registers measure the duration between the first and last benefit payment in a given claiming spell. The duration outcome then corresponds to the variable B defined in the previous Baily-Chetty formulas. When studies rely on matched unemployment-employment registers (such as social security data), they can record both covered unemployment (B) and nonemployment duration (variable D of the previous Baily-Chetty formula). In survey-based studies, the focus is rather on nonemployment duration, as unemployment receipts variables (when available) typically suffer from measurement error. Schmieder and von Wachter (2016) find that nonemployment duration elasticities are smaller than unemployment duration elasticities.

The Baily-Chetty formula (36) shows that the behavioral cost of UI benefit levels depends on both the elasticity of covered unemployment duration and of nonemployment duration. However, as few studies report both elasticity types, it is useful to make an extra approximation to relate both elasticities and simplify the Baily-Chetty formula before taking it to the data. Under a constant hazard rate assumption s, we have $\frac{dB}{db} = \frac{dD}{db}\xi$ where $\xi = 1 - (1 + Ps)e^{-Ps}$. Consequently, the behavioral cost associated with a \$1 increase in unemployment benefits (and expressed in marginal utility when employed) writes: $\eta_{D,b}\frac{1}{1-S_P}\left(\xi+\frac{\tau}{b}\right)$, where S_P is the survival rate at benefit exhaustion.

Behavioral Cost of UI Benefit Levels Schmieder and von Wachter (2016) report behavioral cost estimates from 18 studies from 5 countries (out of which 11 estimates are from the US). We explain how they measure the different components of the behavioral cost expression. First, they gather the elasticity estimates ($\eta_{D,b}$) from previous studies. Then, for each study, they also gather the other quantities of the behavioral costs: the survival rate at benefit exhaustion S_P, the average hazard rate s (to obtain the factor ξ), and the ratio of taxes over benefit level τ/b. Schmieder and von Wachter (2016) present cost estimates under two scenarios for tax rates. From the perspective of the UI agency, the relevant tax (τ) is the workers' contribution rate for unemployment insurance. On average, in OECD countries, the UI tax rate is of the order of 3%. If we rather assume that the budget of the UI agency is integrated in the general government budget, then the relevant tax rate is higher and amounts to the total tax rate on labor income (around 30%). Using the UI tax rate of 3%, the behavioral cost for each additional \$1 transfer of UI benefits varies between \$0.06 and \$0.95, with a median of \$0.35. Taking the median estimate, for every dollar of mechanical transfer to UI claimants, \$1.35 has to be raised in taxes: \$1 of mechanical transfer and an additional \$0.35 because of the loss of tax revenues due to workers changing their behavior. Using the full labor tax wedge, the median behavioral cost is significantly higher at \$0.81.

Behavioral Cost of UI PBD As before, under the assumption of constant hazard rate, the expression of the behavioral cost of increasing Potential Benefit Duration simplifies. It writes: $\frac{dD}{dP}\frac{1}{S_P}\left(\xi+\frac{\tau}{b}\right)$. Schmieder and von Wachter (2016) report the marginal effects of PBD on nonemployment duration $\frac{dD}{dP}$ for eight European studies and five US studies. In Europe, the median estimate is 0.13: one month increase in PBD translates into a nonemployment duration by around 4 days. In the US, the mean estimated marginal effect is twice as large (0.28). Under the assumption of UI tax rate, the behavioral cost of \$1 transfer to UI exhaustees through PBD extension is between \$0.11 and \$2.13 with a median at \$0.60. As expected, under the assumption of general labor tax wedge, the behavioral cost rises to \$1.78. Overall, the behavioral cost of PBD increase is larger than the behavioral cost of benefit increase.

LESSON 1: The behavioral costs of providing UI are substantial.

3.4 Quantification of the Social Value of UI changes

To assess whether a benefit rise increases welfare, the behavioral costs estimated in the previous section are to be compared to estimates of the social value of more generous UI. Recall from the previous Baily-Chetty formula, that the social value of a $1 transfer from the employed state to the unemployed state is $\frac{u'(c_u)-v'(c_e)}{v'(c_e)}$. The social value depends on the marginal rate of substitution across covered state and contributing state. The empirical literature quantifying the marginal social value is less numerous than the empirical literature estimating disemployment effects, but it is growing fast. After the key seminal contributions of Gruber (1997) and of Chetty (2008), the availability of new types of consumption data (for example Kolsrud et al., 2018; Ganong and Noel, 2019) and the development of new identification methods (for example Landais and Spinnewijn, 2021) provides new insights on the social value of unemployment insurance. We first describe the various quantification methods of the social value of UI and then discuss their applications and corresponding estimates from recent papers.

3.4.1 How to quantify the Social Value of UI?

We review the four main approaches to quantify the social value of UI. We follow their publication order.

The Classical Consumption-Based Approach (Gruber, 1997) The first classical approach to estimate the marginal social value of UI is due to Gruber (1997). The approach rests on important assumptions about the utility function. Gruber (1997) assumes that the utility functions of unemployed and of employed workers are the same ($u(c) = v(c)$). In addition, the approach assumes that the utility function has constant relative risk aversion (CRRA) and can be written as: $u(c) = \frac{1}{1-\gamma}c^{1-\gamma}$. Then the marginal utility writes: $u'(c) = c^{-\gamma}$. Taking a first-order approximation of the marginal utility function, the marginal social value of UI becomes:

$$\frac{u'(c_u) - v'(c_e)}{v'(c_e)} \approx \gamma \frac{c_e - c_u}{c_e} \tag{38}$$

The expression shows that, with data on consumption across states and a CRRA estimate, one can estimate the social value of UI. Gruber (1997) uses consumption data from the Panel Study of Income Dynamics (PSID) in the US. In the PSID survey, food consumption drops by 6.8% when UI eligible workers become unemployed. There is a wide range of estimates for the CRRA parameter. Taking $\gamma = 2$ as a focal CRRA estimate, Gruber (1997) obtains that the marginal social value of \$1 transfer of unemployment benefits is \$0.13. In other words, workers would be willing to pay \$1.13 when they are employed to receive \$1 when they become unemployed. Such a marginal social value is lower than behavioral costs implied by the disemployment effect estimates available in the late 1990s (and still lower than the median of estimates available today). The comparison suggests that the US level of UI benefits is higher than the optimal level from the Baily-Chetty approach, unless the CRRA γ coefficient is significantly larger than 2.

The Liquidity to Moral Hazard Ratio Approach (Chetty, 2008) To address the limitations of Gruber (1997) approach, Chetty (2008) develops a sufficient-statistics approach that does not require consumption data, nor assumptions on the utility function parameters. The key idea is to leverage another type of policy variation in the same context, for example changes in severance payments. Chetty (2008) shows that the various behavioral search responses allow to identify the social value of UI. The underlying job search model is extended to allow unemployed workers to have savings A at the beginning of their spell. Workers' optimal behavior in Equation (24) is marginally modified as:

$$v(A + w - \tau) - u(A + b) = \psi'(s). \tag{39}$$

Let us consider a marginal increase in savings. Savings decrease the gap in consumption across the unemployed and employed states and workers decrease search effort:

$$\frac{\partial s}{\partial A} = \frac{v'(A + w - \tau) - u'(A + b)}{\psi''(s)} < 0 \tag{40}$$

Recall that the marginal effect of a benefit increase on search effort writes: $\partial s / \partial b = -u'(A + b) / \psi''(s)$. Combined with Equation (40), we obtain a new expression for the marginal social value of UI:

$$\frac{u'(c_u) - v'(c_e)}{v'(c_e)} = \frac{-\partial s / \partial A}{\partial s / \partial A - \partial s / \partial b} \tag{41}$$

The above expression shows that the marginal social value of UI is identified as a simple combination of the job search effects of both benefit level and savings. Those two effects are sufficient statistics to assess the Baily-Chetty optimality of current UI levels.[28] The sufficient statistics do not require consumption data.

The approach allows to use quasi-experiments to identify both job search effects, ensuring the credibility of the quantification exercise. Chetty (2008) estimates the effect of severance payments (equivalent to savings in the simple one-period static model). Severance payments are one-time monetary transfers that workers receive from their employers at lay-offs. The minimum amounts of severance payments are mandated by law in many countries and increase with tenure. In some countries, the rule features discontinuities: only workers with a certain job tenure are eligible (Card et al., 2007). Such discontinuities can be leveraged to obtain exogenous variations in severance payments.

Another important insight from Chetty (2008)'s approach is an alternative interpretation of the behavioral search response. Namely, Chetty (2008) highlights that the search response can be decomposed into two channels: substitution and income. On the one hand, higher benefits reduce the net wage ($w - \tau - b$), and lower search effort through a substitution effect. On the other hand, higher benefits also increase liquidity and reduce search through an income effect. The income / liquidity effect corresponds formally to $\partial s/\partial A$. The pure moral hazard cost is then the difference between the total effect on search and the income effect: $\partial s/\partial b - \partial s/\partial A$. Consequently, Equation (41) shows that the value of insurance is the ratio between the liquidity effect and the pure moral hazard effect. Hence, Chetty (2008)'s identification strategy is referred as the *Liquidity to Moral Hazard Ratio* approach. For liquidity constrained workers, we expect large liquidity effect.

Landais (2015) and Huang and Yang (2021) adapt Chetty's approach, when there are no quasi-random variations in severance payment available in the context at hand. Landais (2015) shows that the time profile of benefits can identify the Liquidity-to-Moral-Hazard ratio. Huang and Yang (2021) leverage exogenous variations in reemployment bonus to directly estimate $\partial s/\partial w$. This corresponds to the pure moral hazard effect (note that $\partial s/\partial w = \partial s/\partial A - \partial s/\partial b$). Then, the liquidity effect $\partial s/\partial A$ can be recovered indirectly in difference with the total effect of benefits.

[28]Indeed, the behavioral cost is already identified thanks to the job search effects of benefits.

The Marginal-Propensity-to-Consume Approach (Landais and Spinnewijn, 2021) Recently, Landais and Spinnewijn (2021) propose a third approach to quantify the social value of UI. It combines ingredients from the two previous approaches: consumption data and optimality results from workers behaviors in response to non-UI shocks. Landais and Spinnewijn (2021)'s approach leverages estimates of the marginal propensity to consume (MPC) of both employed and unemployed out of extra income. To present the MPC approach, we consider again the static one-period Baily-Chetty model.[29] We extend the model so that workers can adjust their consumption from their own labor income / benefits (denoted y). They are no longer hand-to-mouth strictly speaking. For example, workers may borrow / save to smooth consumption or their spouses may supply extra labor to generate additional income. Formally, workers undertake actions x_s where s denotes the workers' state (unemployed u or employed e). Action x_s raises consumption at price p_s. Formally, workers solve the following problem:

$$\max_{s,x_e,x_u} (1-s)\, u(c_u, x_u) + sv(c_e, x_e) - \psi(s) \tag{42}$$

$$\text{such that} \quad \begin{cases} c_u &= y_u + \frac{1}{p_u} x_u \\ c_e &= y_e + \frac{1}{p_e} x_e \end{cases}$$

When x_s represents spouses' working hours, p_s is the inverse of the spouses' wage rate. When workers adjust through the financial markets, the shadow price is related to interest rates. The first order conditions wrt the adjustment variables x_u and x_e write:

$$\frac{\partial u(c_u, x_u)}{\partial c} = -p_u \frac{\partial u(c_u, x_u)}{\partial x} \tag{43}$$

$$\frac{\partial v(c_e, x_e)}{\partial c} = -p_e \frac{\partial v(c_e, x_e)}{\partial x} \tag{44}$$

As already noted in Section 3.2, the extra actions available to workers are second-order when assessing the social value of UI (because of the envelope theorem). In the extended model, the social value still amounts to the relative marginal utility

[29] Landais and Spinnewijn (2021) consider both a more complex one-period model and a dynamic model. Our simple model illustrates the method intuition which carries over to general models.

of consumption across states, equal to the marginal rate of substitution ($MRS = \frac{\partial u}{\partial c} / \frac{\partial v}{\partial c}$) minus one. Using the optimality of workers behaviors (above FOCs), the MRS writes:

$$MRS = \frac{\frac{\partial u}{\partial c}}{\frac{\partial v}{\partial c}} = \frac{p_u}{p_e} \frac{\frac{\partial u}{\partial x}}{\frac{\partial v}{\partial x}} \tag{45}$$

The MPC approach quantifies the relative prices of smoothing consumption p_u / p_e and bounds the relative marginal disutility costs $\frac{\partial u}{\partial x} / \frac{\partial v}{\partial x}$. To identify unobserved prices p_s, let us consider an income shock y. After implicit differentiation of the first order condition (43), the consumption response MPC when unemployed writes:

$$\frac{dc_u}{dy_u} = \frac{p_u \frac{\partial^2 u}{\partial x^2} / \frac{\partial u}{\partial x}}{-p_u \frac{\partial^2 u}{\partial c^2} / \frac{\partial u}{\partial c} + p_u \frac{\partial^2 u}{\partial x^2} / \frac{\partial u}{\partial x}} \tag{46}$$

We obtain the consumption response when employed along the same lines. After taking the odds ratio of the MPCs and forming their ratio across states, we obtain:

$$\frac{\frac{dc_u}{dy_u} / \left(1 - \frac{dc_u}{dy_u}\right)}{\frac{dc_e}{dy_e} / \left(1 - \frac{dc_e}{dy_e}\right)} = \frac{p_u}{p_e} \times \frac{\sigma_u^x / \sigma_u^c}{\sigma_e^x / \sigma_e^c} \tag{47}$$

where we denote $\sigma_u^x \equiv -\frac{\partial^2 u}{\partial x^2} / \frac{\partial u}{\partial x}$ and $\sigma_u^c \equiv -\frac{\partial^2 u}{\partial c^2} / \frac{\partial u}{\partial c}$ parameters that capture the curvature of the utility function wrt consumption and wrt action x. Combining Equation (45) and (47), the MRS writes:

$$MRS = \frac{\frac{dc_u}{dy_u} / \left(1 - \frac{dc_u}{dy_u}\right)}{\frac{dc_e}{dy_e} / \left(1 - \frac{dc_e}{dy_e}\right)} \times \underbrace{\frac{\sigma_u^c / \sigma_u^x}{\sigma_e^c / \sigma_e^x}}_{\geq 1} \times \underbrace{\frac{\frac{\partial u}{\partial x}}{\frac{\partial v}{\partial x}}}_{\geq 1} \tag{48}$$

Equation (48) shows that MPCs indeed identify the MRS, but still require assumption on utility functions. Those assumptions are weaker than in the consumption-based approach, as what matters is *relative* marginal utilities or *relative* utility curvature *across states*. If preferences over consumption c and resources x are exponential functions stable across states, then the ratio of utility curvature is equal to one whatever the exact value of risk preferences. For utility functions with decreasing absolute risk-aversion (DARA), the curvature ratio can be bounded above one. This is because we can reasonably assume that unemployed consume less than employed workers ($c_u < c_e$) and devote more resources to support their income ($x_u > x_e$). It is also reasonable that when unemployed devote more resources, their marginal cost is higher than for employed workers, so that the third factor in Equation (48) is also greater than one. The weaker assumptions of the MPC approach come at the cost of point identification. In the end, the MPC approach provides a lower bound on the MRS and on the social value of UI. How informative the lower bound is depends on how its estimate compares to previous estimates obtained from the literature. We discuss estimates in the next section.

In practice, MPC estimates can be identified in quasi-experiments, ensuring the identification credibility of the whole method (as in the Sufficient-Statistics approach). An important requirement though is that the quasi-experiments are homogeneous between employed and unemployed workers. For example, Landais and Spinnewijn (2021) exploit large variations in welfare benefits provided by municipalities across types of households.

The Revealed-Preference Approach The most direct method to identify the social value of UI is to study workers' choices to buy insurance. Building on the MPC-approach model, suppose that workers can get extra UI coverage at rate p_u/p_e. They would buy insurance if and only if $(1-s)\frac{\partial u}{\partial c}\frac{1}{p_u} + s\frac{\partial v}{\partial c}\frac{1}{p_e} \geq 0$. Rearranging the terms, we obtain that workers will buy extra coverage if their MRS is above the expected price:

$$MRS \geq \tilde{p} \equiv \frac{p_u}{p_e} \times \frac{s}{1-s} \tag{49}$$

Note that the relevant price $\tilde{p}$ depends on the individual-specific job finding rates s (or individual unemployment risks). The Revealed-Preference (RP) approach has two important requirements: observing UI coverage choices and precise data on perceived unemployment risks. As UI is a mandatory insurance in many countries, the RP method has not been used in UI studies with the notable exception of Landais and Spinnewijn (2021). Swedish workers have income-related UI benefits (instead of a flat benefit level) if they pay a uniform premium. Iceland, Denmark and Finland are three other countries with voluntary UI schemes.

Other Approaches For the sake of completeness, we briefly discuss two other methods to assess the social value of UI. First, Shimer and Werning (2007) observe that there is a direct relation between the value of unemployment and reservation wages. Recall from Section 2.2 that, in the standard job search model, we have

$v(\phi_0) = (1-\delta)V_0^U$ where ϕ_0 is the reservation wage and V_0^U the expected value of unemployment, both at the beginning of the unemployment spell. δ is the discount factor. Consequently, with data on reservation wages, we can identify the effect of a marginal increase in benefit on workers welfare (see Le Barbanchon et al., 2019).

Second, Hendren (2017) shows how consumption responses *before* job loss to changes in perceived unemployment risk identify the social value of UI. Hendren (2017)'s method builds on the Consumption-Based approach as it requires to observe consumption path before unemployment. In addition, identification requires measuring workers' beliefs about future job loss (available in the Health and Retirement Study for example). Identification rests on the following mechanism. When forward-looking workers learn about future job loss, they decrease consumption all the more that they are willing to increase precautionary savings and transfer income towards the unemployed state. This assumes workers' optimization (as in the MPC approach). Formally, the social value of UI writes:

$$\frac{u'(c_u) - v'(c_e)}{v'(c_e)} \approx -\gamma E\left[\frac{d\log\left(c_{pre}(p)\right)}{dp}\right] \tag{50}$$

where $E\left[\frac{d\log\left(c_{pre}(p)\right)}{dp}\right]$ is the average relationship between consumption when employed (before any job loss) and beliefs about future employment p. The main advantage of Hendren (2017) approach compared to the classical consumption-based approach is that it allows for state-dependent utility function ($u'(c) \neq v'(c)$).

3.4.2 Selected Review of Social Value Estimates

In Table 4, we report estimates of the social value of UI benefit increase.[30] We select a subset of studies representing the four main approaches: Consumption-Based, Liquidity-to-Moral-Hazard ratio, MPC and Revealed Preference approach. Studies using the same approach are grouped into panels. Our objective is not to be exhaustive, but to represent estimates obtained with different approaches and data sources. Table 4 comprises sixteen social value estimates from ten studies.

[30] We select a subset of studies from Table 3 of Schmieder and von Wachter (2016) and update the table with more recent studies. Namely, we do not report every study using the PSID dataset.

Table 4: Estimates of the Social Value of UI benefit increase

Study	Range of Years	Country	Data Source	Key moment	Social value
Panel A: Consumption-Based Approach				**Consumption Loss**	
Gruber (1997)	1968-1987	United States	PSID, food only	at job loss: 6.8%	0.136
Rothstein and Valletta (2017)	2001 panel	United States	SIPP	at job loss: 10.0%	0.2
Rothstein and Valletta (2017)	2008 panel	United States	SIPP	at job loss: 20.0%	0.4
Ganong and Noel (2019)	2012-2015	United States	JPMCI checking account	at job loss: 6.1%	0.122
Landais and Spinnewijn (2021)	2000-2007	Sweden	Tax records	at job loss: 12.9%	0.258
Ganong and Noel (2019)	2012-2015	United States	JPMCI checking account	UI exhaustees: 25%	0.5
Gerard and Naritomi (2021)	2010-2015	Brazil	VAT receipts, RAIS registry	UI exhaustees: 17%	0.34
Hendren (2017)	1992-2013	United States	HRS-PSID	29% after future job loss news	0.58
Panel B: Liquidity to Moral Hazard Approach				**Job Finding Response to**	
Card et al. (2007)	1981-2001	Austria	Social Security Registry	severance pay, RD	1.4
Chetty (2008)	1985-2000	United States	SIPP	severance pay, OLS	1.5
Landais (2015)	1970s-1984	United States	CWBH	time profile of benefits, RKD	0.88
Huang and Yang (2021)	2001-2011	Taiwan	Admin. registers	reemployment bonus, RKD	0.5 - 1.5
Panel C: Marginal Propensity to Consume Approach					
Landais and Spinnewijn (2021)	2000-2007	Sweden	Tax records	Consumption response to welfare benefits	$\geq$0.59
Panel D: Revealed Preference Approach					
Landais and Spinnewijn (2021)	2000-2007	Sweden	Tax records, survey on Unemp risk	Choice of UI scheme	1.13, 2.13

Notes: This table presents estimates of the social value of UI benefit increase across a selected set of studies. The first panel A reports estimates following the Consumption-Based approach introduced by Gruber (1997), and extended to study the social value of PBD extension. To compute the social value from the consumption loss, we set the CRRA parameter to a conservative value of $\gamma = 2$. When studying the social value of PBD extension, we use consumption drop for UI exhaustees. The second panel B reports estimates following the Liquidity-to-Moral-Hazard approach introduced by Chetty (2008). To disentangle moral hazard from liquidity, they use job finding responses to UI and to another policy listed in the column entitled "key moments". Card et al. (2007); Landais (2015); Huang and Yang (2021) use response to PBD extension and Chetty (2008) to benefit level increase. The third and fourth panels C and D report estimates from the MPC and RP approaches respectively (for any unemployed worker, not only UI exhaustees). The MPC approach identifies a lower bound for the social value of UI. Landais and Spinnewijn (2021) do not highlight one specific RP-based estimate, we report here the average social value under both extreme beliefs models of unemployment risk.
Other studies using the same PSID data are Cochrane (1991); Stephens (2001); Chetty and Looney (2006); Kroft and Notowidigdo (2016) and Chetty and Szeidl (2006). Browning and Crossley (2001) also provides Consumption-based evidence for Canada.

The underlying data are drawn from the United States, Sweden, Austria and Brazil. This is already a significant coverage of countries, but there is room for further research to widen the scope of estimates across even more countries. Studies cover estimates from the 1970s to recent years (up to 2015). A noticeable feature of Table 4 is the richness of the data sources mobilized by the empirical UI literature. As observing the consumption path of unemployed workers is key to estimate the social value of UI (in the Consumption-based and MPC approaches), the empirical literature has made recent breakthroughs in data sources. After the seminal use of survey data on consumption by Gruber (1997), the literature flourishes in administrative data: Swedish tax registers in Kolsrud et al. (2018), high-frequency banking transaction data in the US study of Ganong and Noel (2019), and Brazilian VAT receipts matched with employment registers in Gerard and Naritomi (2021). As discussed previously, such granular data allow to observe the precise timing of consumption in relation to job loss and to UI benefit receipts and exhaustion.

Compared to the initial estimate of 6.8% from Gruber (1997), the recent literature find larger consumption drop at job loss reported in Panel A. Rothstein and Valletta (2017) document a 10% drop in consumption in more recent US survey data (SIPP) down to 20% drop during recession times. Using Swedish tax registers, Landais and Spinnewijn (2021) obtain that consumption drops by 12.6% at job loss. Ganong et al. (2022) observe a 6.1% consumption drop at job loss in bank data in the US, which is very close to Gruber (1997) though. We translate the consumption drops at job loss into the social value of UI benefit increase in the last column of Table 4. We assume that the CRRA parameter of the utility function is equal to two. We obtain social value estimates ranging from \$0.12 to \$0.4 (corresponding to the first five estimates in the top panel). They imply that workers would be willing to decrease their consumption when employed by \$1.12 - \$1.4 for an extra dollar of consumption when unemployed. The social value estimates are very sensitive to the CRRA parameter. Assuming that the CRRA is equal to five, the social value estimates would range from \$0.30 to \$1. Such changes are pivotal for optimal UI design. In the Baily-Chetty framework, social value is compared to behavioral cost. When we use consumption drop *at job loss* to compute the social value of UI, the relevant behavioral cost is the one induced by a transfer of benefits through a monthly benefit increase (as opposed to an extension of PBD). In the Baily-Chetty Equation (36), the relevant consumption when unemployed is before UI exhaustion.

From the previous section, the median behavioral cost of monthly benefit increase is \$0.35. Consequently, most social value estimates are lower than the median behavioral cost when we assume $\gamma = 2$, but higher when we assume $\gamma = 5$.

In the last rows of Panel A, Table 4 reports estimates of the drop of consumption for UI exhaustees (compared to their pre job loss levels). These estimates are difficult to compute in survey data as they require very precise measure of benefit receipts and high frequency observations. The new administrative data sources overcome the challenge. We expect the consumption of UI exhaustees to be significantly lower than the consumption of short-term unemployed, as they no longer rely on UI benefits and they may have lower savings after spending some time unemployed. Indeed, Ganong and Noel (2019) find that UI exhaustees have consumption expenditures 25% lower than before job loss (compared to 6.1% just after they become unemployed). Gerard and Naritomi (2021) report a consumption drop for UI exhaustees of 17%, which translates into a social value estimate of \$0.5. The social value estimates are lower than the median estimate of the behavioral costs of benefit increase through PBD extension (the relevant policy instrument for UI exhaustees). In the previous section, the corresponding median behavioral cost is \$0.6 and even higher when computed with the full labor tax wedge. As above, the conclusions drawn from the Baily-Chetty exercise depend heavily on the CRRA assumption. Another consideration is that with such large changes in consumption at exhaustion, the first order approximation underlying Equation (38) may not be valid any more. Taking the estimate from Ganong and Noel (2019), the social value estimate increases from \$0.5 to \$0.77 when we do not linearise the utility function (with $\gamma = 2$).

In the next three panels of Table 4, we report social value estimates from approaches that do not require explicit assumption on the CRRA parameter. Overall, the corresponding studies find higher social value estimates (above \$0.5). In Panel B, we report findings from four studies applying the liquidity to moral hazard ratio approach. Card et al. (2007) and Chetty (2008) both find similar estimates around \$1.5, despite their differences in countries and in empirical designs to estimate the job search response to severance pay. Landais (2015) finds a lower estimate at \$0.89 when he uses variations along the time profile of unemployment benefits to disentangle liquidity and pure moral hazard effects. Huang and Yang (2021) find estimates in the same ballpark \$0.5-1.5, as they use the job finding response to a reemployment bonus to identify the marginal utility when employed (moral hazard denominator).

In the last two panels, we report estimates of the MPC and Revealed-Preference approaches on the same sample of workers from Landais and Spinnewijn (2021). They find that the MPC approach delivers a lower bound for the social value, as high as $0.59. This is higher than the consumption-based estimate that they can compute on the same sample of workers. The Revealed-Preference estimates are even higher and the average estimate ranges from $1.13 to $2.13 depending on how unemployment risks are estimated. When workers are assumed to have correct beliefs about unemployment risks (based on the information available in the register dataset), the mean social value is $2.13. However survey data eliciting subjective beliefs point to important risk misperception. This leads the RP method to overestimate the MRS. After correction, the social value is lower, but still significantly higher than consumption-based estimates. Beyond the average, Landais and Spinnewijn (2021) document significant heterogeneity in social value across groups of workers with a first quartile at $0.8 and a third quartile at $1.73 (estimated under misperceived risks).

While the approaches in Panel B to D do not rely on explicit CRRA parameters, their estimates imply some high values of the CRRA parameters. Chetty (2008) states that his social value estimate could be rationalized in a model with CRRA equal to 5. To match the lower bound of their MPC-based estimate, Landais and Spinnewijn (2021) would have to multiply the observed consumption drop by a CRRA parameter equal to 4. Further research is needed to deliver more estimates through the Liquidity to Moral Hazard ratio approach, the MPC approach and the RP approach. This would help to draw stronger conclusions on whether the consumption-smoothing value of UI is above or below the median behavioral costs (around $0.5).

Further research is also needed on the reasons explaining the differences of estimates across methods. Whether the type of consumption observable in data could drive the difference between Consumption-Based and Liquidity-to-Moral-Hazard ratio approaches is debated. Another important candidate explanation is whether marginal utility depends on state. State-dependent utility leads to severe bias in the Consumption-Based approach. Last, failing to account for behavioral frictions also generate different bias across methods.

LESSON 2: Estimates of the social value of UI differ widely across identification methods. The most recent methods which are robust to risk-aversion assumptions yield significantly higher estimates.

In the previous section, we systematically compare the social value estimates with the behavioral cost estimates. We then follow the traditional Baily-Chetty approach to test whether one single policy instrument, either PBD, or benefit level, is set at its optimum. The Baily-Chetty framework derives the marginal welfare effect of a transfer of $1 to unemployed workers. If the welfare effect of a marginal change in benefits is zero (social value of UI equal behavioral cost), then the social planner already maximizes welfare and no further policy changes are needed. If the welfare effect is positive (social value of UI greater than behavioral cost), then increasing the generosity of UI is the recommended policy. When the welfare effect is negative, the opposite policy recommendation holds. While the traditional Baily-Chetty approach is an established and useful policy assessment tool, it is often implemented for one policy instrument only and independent of changes in other policy instruments. This is an important limitation, as in practice social planners may leverage various policy instruments and policy-makers need empirical guidance on which policy is to be expanded vs another one. The next section presents the unified framework of Hendren and Sprung-Keyser (2020) which addresses this limitation, and allows for policy comparisons, taking into account redistribution effects.

3.5 The Marginal Value of Public Funds

This section discusses how to compare various UI policy changes against one another and against other government policies, within the unified framework of Hendren and Sprung-Keyser (2020). We briefly review how their unified framework yields the Marginal Value of Public Funds (MVPFs) as a key criteria for policy assessment. We then compute UI policy MVPFs using the various previous estimates in Section 3.3 and 3.4. We draw tentative comparisons between policies increasing benefit level vs. PBD, and we discuss how redistribution effects are accounted for in the MVPFs analysis (while absent from the traditional Baily-Chetty approach).

The MVPFs framework The government considers the following social welfare W defined as the weighted sum of individual utilities U_i: $W = \sum_i \psi_i U_i$ where ψ_i is the social welfare weight of individual i. The government evaluates the welfare effects of a policy j with upfront initial spending dp_j. The social welfare change writes:

$$\frac{dW}{dp_j} = \bar{\eta}_j \sum_i WTP_i^j \tag{51}$$

where WTP_i^j is individual i willingness-to-pay for policy j out of her own income. Formally, $WTP_i^j = \frac{dU_i}{dp_j}\frac{1}{\lambda_i}$ with λ_i the marginal utility of individual i. The first factor $\bar{\eta}_j$ is the average social marginal utility of the beneficiaries of the policy.[31]

On the cost side, we denote R the (present discounted) value of government budget and $G_j = \frac{dR}{dp_j}$ the net impact of policy j on government budget. The net impact includes both the initial cost of the program and all other effects of behavioral responses on the government budget. The Marginal Value of Public Funds of policy j is defined as:

$$MVPF_j = \frac{\sum_i WTP_i^j}{G_j} = \frac{WTP^j}{\text{Net Cost}}. \tag{52}$$

The effect of policy j on social welfare per dollar of government expenditure writes: $\bar{\eta}_j MVPF_j$. Introducing the MVPFs conveniently separates the policy effect into two factors. The first factor $\bar{\eta}_j$ captures redistribution effects through social welfare weights. The second factor $MVPF_j$ gives unit weights to all beneficiaries but captures all relevant fiscal externalities.

The MVPFs framework allows to construct hypothetical budget-neutral policy changes. Suppose that the government increases spending on policy A by an amount G_A and reduces spending on policy B by the same amount (to keep the same budget). The policy shift from B to A increases social welfare if and only if:

$$\bar{\eta}_A MVPF_A > \bar{\eta}_B MVPF_B. \tag{53}$$

This clarifies that key inputs for policy choices are estimates of the MVPFs for a large set of policies. We compute the MVPFs estimates for various UI policies. This is useful to compare different types of UI policies one against the other, but also UI policies to policies in other domains.

Before moving to MVPFs estimates, we highlight the conceptual similarities and

[31] The average social marginal utility writes: $\bar{\eta}_j = \sum_i \eta_i \frac{WTP_i^j}{\sum_i WTP_i^j} = \sum_i \psi_i \lambda_i \frac{WTP_i^j}{\sum_i WTP_i^j}$

differences between the MVPFs and Baily-Chetty approach. First, the unified approach of Hendren and Sprung-Keyser (2020) allows for heterogeneity in social marginal utility across policy beneficiaries, while the standard Baily-Chetty approach does not. Second, the MVPFs approach ultimately compares various policies, while the traditional Baily-Chetty approach focuses on the optimality of one policy only. Third, both approaches turn out to be very similar on accounting for government costs. The effects of behavioral responses on the government budget included in the MVPFs denominator are the same as the behavioral costs due to general fiscal externalities in the standard Baily-Chetty formula. The only difference is that the MVPFs computation does not require the government to close the budget constraint.[32]

The MVPFs of UI policies We compute the MVPFs of the two UI policies we considered so far. They increase UI generosity either through an increase in benefit levels (policy b) or through an increase in PBD (policy PBD). Hendren and Sprung-Keyser (2020) compute the MVPFs for those policies using estimates from the US. We follow their UI MVPFs interpretation updating the WTP estimates and expanding the analysis beyond the US. The WTP for \$1 dollar of UI benefits is equal to the MRS between the unemployed and employed states. For policy b, the extra dollar is transferred to unemployed before their benefit exhaustion (with unemployment duration t before PBD P). For policy PBD, it is transferred to UI exhaustees (such that $t > P$). In line with the previous Baily-Chetty formula, we then have:

$$WTP^b = \frac{u'(c_{u,t\leq P})}{v'(c_e)} = 1 + \frac{u'(c_{u,t\leq P}) - v'(c_e)}{v'(c_e)} \tag{54}$$

$$WTP^{PBD} = \frac{u'(c_{u,t>P})}{v'(c_e)} = 1 + \frac{u'(c_{u,t>P}) - v'(c_e)}{v'(c_e)} \tag{55}$$

Consequently, we obtain WTP^b and WTP^{PBD} estimates using the social value estimates in the previous section. For the main analysis, we adopt the Consumption-Based estimates with a CRRA parameter equal to four (instead of two). This makes corresponding Consumption-Based estimates closer to estimates obtained with the three other approaches (Liquidity-to-Moral-Hazard ratio, MPC and RP). For US

[32]Closing the budget constraint may induce extra behavioral costs on government budget as the government raises revenue to fund UI (eg through income effects). They do not appear in the standard Baily-Chetty approach though.

WTP estimates, we use as consumption drop estimates: 9% at job loss (Hendren and Sprung-Keyser, 2020) and 25% for UI exhaustees (Ganong et al., 2022).[33] For Europe, the only available consumption drop estimates in Table 4 are from Sweden: 4.4% at job loss and 9.1% for long-term unemployed (Kolsrud et al., 2018). This is not ideal, as Sweden has a relatively high replacement rate and no UI exhaustion over the period of estimation. Consequently, one should take our European MVPFs with a grain of salt.

The denominators of the MVPFs are simply the \$1 spent on UI benefit increase augmented with the effect on government budget due to behavioral reactions. Assuming that behavioral reactions are related to search effort only, the second term corresponds to the behavioral costs of the Baily-Chetty formulas (36) and (37). The behavioral reactions generate negative fiscal externality increasing the net cost of the UI policy. Formally, we have:

$$G_b = 1 + \text{Behavioral Costs}_b \tag{56}$$

$$G_{PBD} = 1 + \text{Behavioral Costs}_{PBD} \tag{57}$$

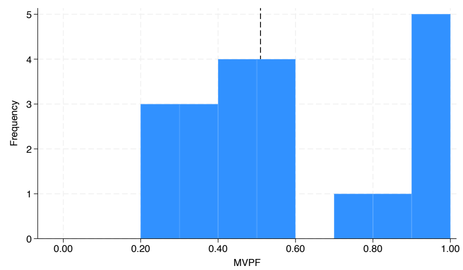
We use the behavioral cost estimates analyzed in Schmieder and von Wachter (2016). As the MVPFs framework adopts the point of view of the general government (not of the UI agency), we choose the full labor wedge as relevant tax for fiscal externality. Appendix Tables B1 and B2 report 34 MVPFs estimates for European and US policies resp.

Figure 11 plots the distributions of the MVPFs by continent in Panel 11a and 11b. European MVPFs vary between .24 and .99. The median of the 21 EU estimates is .51. All estimates are below the reference value of one, which corresponds to simple nondistortionary transfers from the government to an individual. In Panel 11b, the 13 US estimates vary between .51 and 1.18 with a median at .78. Four US estimates are above the reference value of one. Of course, such a conclusion depends highly on the CRRA choice and on the tax definition.

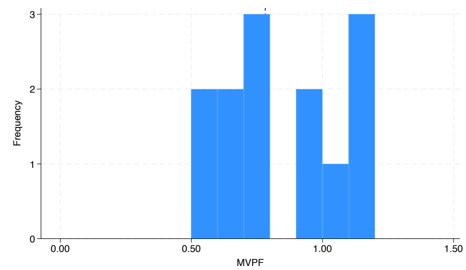
In Panel 11c, we take the perspective of either the US federal government or a European government whose budget is consolidated across countries. We ask whether

[33] For the consumption drop at job loss in the US, the 9% estimate corresponds to the 6% estimates from Ganong et al. (2022) and Gruber (1997) corrected for pre-job loss drop in consumption documented in Hendren (2017). It is close to the 10% estimate of Rothstein and Valletta (2017) from the 2001 PSID panel.

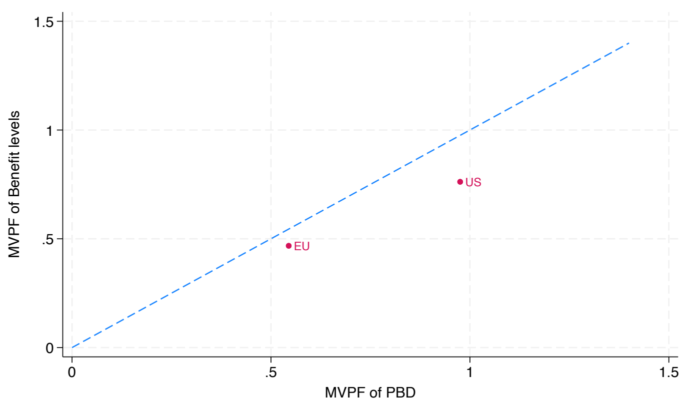
Figure 11: The Marginal Value of Public Funds



(a) EU



(b) US



(c) Comparing PBD and Benefit level policies

Notes: The figure reports estimates of the Marginal Value of Public Funds for UI policies. To compute the MVPFs, we use WTP estimates from Table 4 (Consumption-based approach) and the behavioral costs estimates from Schmieder and von Wachter (2016) (with some updates). See main text for details. Panel 11a shows the MVPFs distribution in Europe. The dashed vertical line corresponds to the median value. Panel 11b shows the MVPFs distribution in the US. For each continent, we compute the median MVPFs for each UI policy (PBD vs benefit level) and we plot $MVPF^b$ against $MVPF^{PBD}$ in Panel 11c. The dashed blue line corresponds to the 45 degree line.

such a government should increase benefit levels while reducing PBD, or the opposite. We compute the median $MVPF^{b}_{EU}$ and $MVPF^{PBD}_{EU}$ within Europe and the corresponding median estimates in the US separately. In Panel 11c, we plot the $MVPF^{b}$ median estimates vs the $MVPF^{PBD}$ median estimates, together with the 45 degree line. We find that the EU is located almost on the 45 degree line. Consequently if the beneficiaries of the two policies have equal social marginal utility, the government will be indifferent between spending on either policies. There is no welfare gain for changing the policy-mix. On the contrary, the US is located below the 45 degree line. Under the same assumption of equal social marginal utility across each policy beneficiaries, the US government can increase welfare by increasing spending on PBD and decreasing spending on benefit level by the same amount ($MVPF^{PBD}_{US} > MVPF^{b}_{US}$). This policy recommendation holds as long as the relative social marginal utility of PBD- vs benefits-increase beneficiaries $\bar{\eta}^{PBD}_{US}/\bar{\eta}^{b}_{US}$ is above $MVPF^{b}_{US}/MVPF^{PBD}_{US} = 0.78$. Of course, such US policy recommendation depends heavily on PBD elasticity estimates, which are only three in our sample. In addition, we do not account for uncertainty in those estimates. Further research is needed to alleviate those two weaknesses. The above analysis rather shows the flexibility of the MVPFs framework.

One important question remains though. What would be a reasonable quantification for the relative social marginal utility between the PBD-extension and the benefit-increase beneficiaries ($\bar{\eta}^{PBD}/\bar{\eta}^{b}$)? In principle, the average social marginal utility depends first on government social welfare weights of beneficiaries, and second on their marginal utility when employed.[34] Let us consider that the government puts the same social welfare weights on all job losers. Then, differences in social marginal utility are driven by differences in average individual marginal utility when employed. UI exhaustees who benefit from PBD extension are generally negatively selected on potential wages compared to the average pool of unemployed (see negative duration dependence of wages in Section 2). Their marginal utility when employed is thus greater than that of average UI claimants. It is then reasonable to consider $\bar{\eta}^{PBD}_{US}/\bar{\eta}^{b}_{US} > 1$. Consequently, a policy-mix with some redistribution objective would tilt towards PBD-extension rather than benefit-increase even more than the relative $MVPF$s suggest.

[34] For the sake of simplicity, we assume that there is no heterogeneity in the individual WTP within each beneficiary group. Otherwise, a third component depending on the product between individual social marginal utility and WTP matters.

The previous MVPFs estimates are useful to compare UI policies to any other policy (e.g. training policies or other educational policies). For example, Hendren and Sprung-Keyser (2020) shows how UI policies have lower MVPFs than educational policies in the US. Further research is needed to confirm this finding in Europe. Comparing UI policies and educational policies also require to compute their respective average social marginal utility. For UI policies, this amounts to the average social marginal utility of employed workers at risk of becoming unemployed, which further research would need to quantify.

3.6 UI effects beyond unemployment duration

The design of UI policy requires a priori to also consider effects beyond job finding rates, such as effects on reemployment wages, and on job separation. In this section, we provide some informal insights on the channels through which such other effects impact UI design, and some empirical evidence on their magnitude.

3.6.1 UI effects on wages

To analyze wage effects, the UI literature modifies the underlying job search model underlying the Baily-Chetty framework in Section 3.2. Whether the model assumes random search or directed search, the introduction of wages does not change the social value part of the Baily-Chetty formula (Chetty, 2006, 2008; Nekoei and Weber, 2017).[35] As explained in Section 3.2, as long as wages are a choice variable for workers, direct UI effects on wages do not contribute to the social value (because of the envelope theorem). Outside of the Baily-Chetty framework, direct wage effects may matter. For example, when the social planner maximizes welfare beyond the private value of UI for unemployed, UI externalities through wages may enter in the social value of UI (see Section 3.8 for example).

On the contrary, UI effects on wages may contribute to the social cost of the Baily-Chetty formula. When taxes are proportional, wage increases due to UI loosen the government budget constraint. This triggers a positive fiscal externality that counteracts the standard negative fiscal externality related to disemployment effects. Assuming that search is directed as in Nekoei and Weber (2017), we derive the UI

[35] For a detail proof in the random search model, see model extension 2 in Chetty (2008). Nekoei and Weber (2017) discusses the wage channel in the directed search model. We adopt their model in the online appendix.

welfare effect in the one-period static model (see online appendix for computation details). The marginal welfare effect of UI benefit increase writes:

$$\frac{d\widetilde{W}}{db}\frac{1}{(1-s)v'(c_e)} = \frac{u'(b) - v'(w(1-t))}{v'(w(1-t))} - \frac{1}{s}\eta_{1-s,b} + \eta_{w,b} \tag{58}$$

where all notations are previously defined, except the proportional tax rate t and the elasticity of wages wrt benefit generosity $\eta_{w,b}$. Compared to the fixed-wage Baily-Chetty formula (26), the only difference is the wage elasticity term ($\eta_{w,b}$). In Section 3.3, we report an average estimate of 0.6 for the elasticity of unemployment duration ($\eta_{1-s,b}$). This is a lower bound for the fixed-wage behavioral cost of the one-period model.[36] How does this compare to available estimates of the wage elasticity to benefits?

The quasi-experimental literature on wage effects of UI finds (if anything) modest effects on reemployment wages. We analyze twelve elasticity estimates from eight studies (Card et al., 2007; Lalive, 2007; van Ours and Vodopivec, 2008; Centeno and Novo, 2009; Le Barbanchon, 2016; Schmieder et al., 2016; Nekoei and Weber, 2017; Johnston and Mas, 2018). The average elasticity amounts to -.028 and estimates range from -.16 to .017.[37] All studies, except Nekoei and Weber (2017), report that more generous UI *decreases* reemployment wages, even though most estimates are not statistically significant at standard levels. Among the studies implying a negative elasticity, Schmieder et al. (2016) has the most precise estimate, which turns out to be statistically significant. Their estimate of the wage elasticity (wrt PBD) is -.014. Doubling the UI generosity decreases wages by 1.4%. Nekoei and Weber (2017) is the only study finding a statistically significant positive effect. Their estimate of the wage elasticity (wrt PBD) is .017. The two wage elasticity estimates imply a fiscal externality following a \$1 UI transfer ranging from -\$.014 to \$.017. Compared to the median estimate of fiscal externality due to disemployment effects (\$.34), this is one order of magnitude lower. However, the average disemployment estimate may not be the relevant comparison point. For example, Nekoei and Weber (2017) find that the elasticity of nonemployment is also low in their context, so that UI effects on wages can partly compensate for the negative fiscal externality

[36] The fixed-wage behavioral cost ($\frac{1}{s}\eta_{1-s,b}$) depends on the fraction of periods spent employed s. As $s \in (0,1)$, the behavioral cost is larger than the elasticity estimate.

[37] The -.16 estimate from Johnston and Mas (2018) is an outlier, as the second lowest elasticity equals -.06.

due to disemployment. Their quantification relies on long job duration (high s in the Baily-Chetty formula), and on reemployment wage effects persisting all over the job spell. Two aspects that deserve further research.

Jäger et al. (2020) study the effects of UI replacement rates on *average economy-wide wages*. They implement difference-in-difference designs around four major UI reforms changing benefit levels in Austria. They find that $1 increase in UI benefits leads to $.01 dollars increase in wages. The wage effect rises to around $.11 for job movers, although the estimate is not statistically significant (see Table IV in Jäger et al., 2020). Again, this suggests that wage effects are a second-order term of UI behavioral costs.[38] Further research on wage effects due to benefit level increases would be helpful to confirm this lesson.

LESSON 3: In the majority of recent studies, more generous UI policy decreases wages imposing further (second-order) behavioral cost to provide UI.

3.6.2 UI effects on job separation

Beyond the effects on unemployed job seekers, UI generosity may affect the behavior of employed workers. In theory, when employed workers become eligible to more generous UI in case of job loss, the value of their outside option increases, and job surplus decreases, which can trigger higher separations. In the Baily-Chetty framework, such effects do not contribute to the social value of UI to the extent that separations are the outcome of an optimizing behavior of employed workers.[39] Of course, in a general equilibrium framework, there could be extra cost for firms of excess turnover induced by UI rules. Actually, the early literature analyzing UI effects on job separations takes the firms' perspective and asks whether experience-rating in UI contribution rates makes firms internalize the social cost of

[38] Lindner and Reizer (2020) is a notable exception where both fiscal externalities of disemployment and of wage effects are of similar order of magnitude. They analyze a benefit front-loading reform in Hungary. It changes the time path of benefits, but keeps constant the overall amount of benefits paid over the potential benefit duration. Accounting for the time path of job finding rate, "the new benefit mechanically increased government spending by around US$119 (SE 0.8) per unemployed worker" p.142. Shorter unemployment spells generate positive fiscal externality of $77 ($119 \times 65\%$). Higher reemployment wages increase government budget by $194.

[39] To account for separation effects, one possibility is to recast the static Baily-Chetty framework such that employment duration is the sum of pre-unemployment and post-unemployment spells. This formally groups the two margins of adjustment into one.

job loss in their firing decisions (Feldstein, 1976; Topel, 1983, 1984; Anderson and Meyer, 1993).

In this section, we report empirical evidence on the effects of more generous UI on workers separation rates. In the Baily-Chetty framework, separation effects induce fiscal externalities to be accounted for. We do not formalize here the general expression of the fiscal externality and leave it for future research. In a nutshell, when workers react to more generous UI by separating from their firms and claiming benefits, this generates supplementary UI spending to the marginal unemployed workers and tax loss (as employment duration decreases). In practice, the moral hazard cost at the separation margin depends on the precise UI eligibility rules. In many countries, eligibility UI rules require unemployed workers to be involuntary deprived from work, which makes job quitters ineligible to UI.[40] This limits workers' moral hazard, as employers may be reluctant to pay the extra costs associated to layoffs (minimum severance payments, red-tape, and litigation costs when workers contest dismissals in labor courts). That being said, workers and firms may collude and bargain on the layoff costs internalizing the workers eligibility for benefits.

The early empirical evidence of UI effects on separations comes from Canada with a series of four papers using the same data from the 1980s (Christofides and McKenna, 1996; Green and Riddell, 1997; Green and Sargent, 1998; Baker and Rea, 1998). To qualify for UI benefits, Canadian workers must work at least 14 weeks over the year before their dismissals. The work experience requirement varies across regions as a function of local unemployment rate. Christofides and McKenna (1996) and Green and Sargent (1998) document spikes in the weekly exit rate from *employment* at the regional experience requirement. This first empirical evidence mirrors spikes found in the exit rate out of unemployment. While visually appealing, Christofides and McKenna (1996) and Green and Sargent (1998) cannot fully rule out confounding shocks at the regional level (as the source of variation in requirement is driven by local unemployment rate). Green and Riddell (1997) and Baker and Rea (1998) leverage a politically-motivated change in the eligibility rule in 1990 that sets all regions at the maximum 14 week criteria. As there are very few regions at the maximum in 1989, they cannot be used as a precise control group. However, they convincingly show that the spikes follow the requirement change in

[40] In practice, job quitters may become eligible after some waiting period.

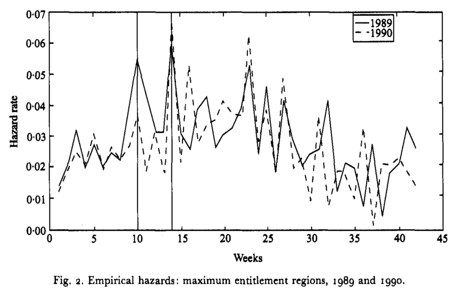
affected regions (see Panel (a) in Figure 12 which reproduces Figure 2 from Green and Riddell (1997)).

In Figure 13, we plot quasi-experimental estimates of separation effects published from the 2000s onwards.[41] We report effects on separation rates of three types of UI variation. In red circles, we plot effects comparing eligible workers vs. ineligible workers for whom any UI claims would be denied (Albanese et al., 2020; Leung and O'Leary, 2020; Van Doornik et al., 2023). They rely on discontinuity in the UI eligibility rules as a function of previous work requirements, eventually coupled with reforms as in Leung and O'Leary (2020). The second and third types of UI variations are at the intensive margin among eligible workers only. Blue squares in Figure 13 correspond to studies that contrast workers with long vs short Potential Benefit Duration. The green triangle corresponds to the study by Jäger et al. (2020) comparing workers with high vs low replacement rates (had they claimed). Overall, the selected recent quasi-experimental estimates confirm the existence of moderate UI effects that increase separations.

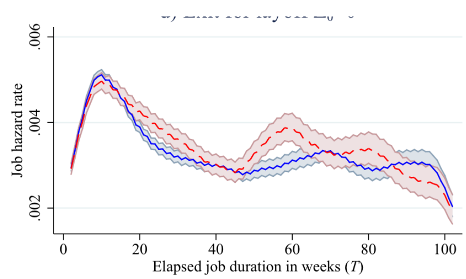
Among prime-age workers (below 50 years old), three studies find statistically significant positive effects (Albanese et al., 2020; Van Doornik et al., 2023; Jessen et al., 2023), and two studies do not reject zero effects (Leung and O'Leary, 2020; Jäger et al., 2020). Albanese et al. (2020) leverages a design with strong credibility. In Italy, workers with above 52 weeks of work experience over the two years before separation are UI eligible if they satisfy a second criteria: they have also worked one extra day *before* the two-year qualification period. Panel (b) in Figure 12 shows that the layoff hazard rate has spikes at the 52-week threshold for treated workers (satisfying the second criteria), but not for control workers. Studying a reform lowering the minimum work requirement in Brazil, Van Doornik et al. (2023) find strong and precisely estimated effects on formal employment (with some evidence of substitution towards informal employment). Jessen et al. (2023) study the Polish rules that extend PBD for 6 months in counties with high unemployment rates (compared to the national level). In a RDD design, they estimate that UI eligible workers are 6%

[41] We find two papers providing early evidence in the US: Jurajda (2002) and Light and Omori (2004). While suggestive, they do not leverage quasi-experiments to identify separation effects. Jurajda (2002) analyses past employment duration of a sample of unemployed workers using duration models with unobserved heterogeneity. Light and Omori (2004) leverage variation across states and across year in the overall generosity of benefits, but do not focus on politically-motivated changes. Rebollo-Sanz (2012) confirms the existence of spikes in employment separation at eligibility cutoff in Spain, but does not cast the analysis within a quasi-experimental setting.

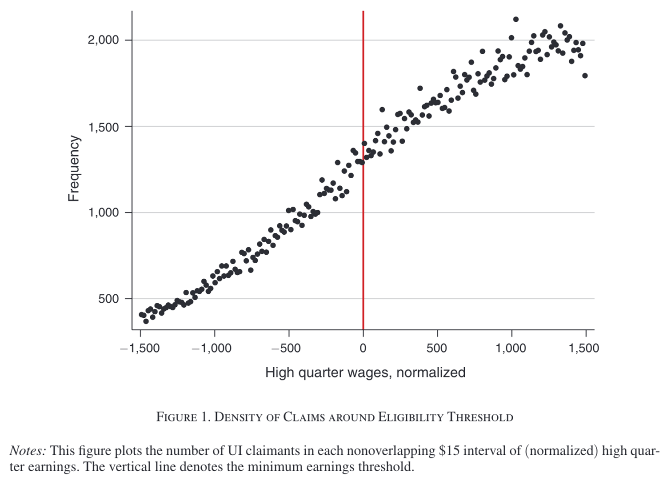
Figure 12: UI effects on job separation



(a) Green and Riddell (1997)



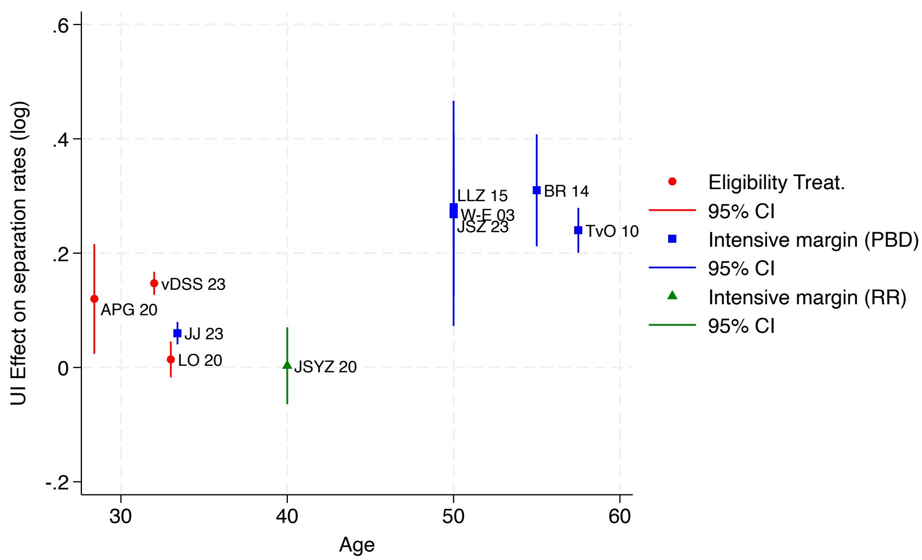
(b) Albanese et al. (2020)



(c) Leung and O'Leary (2020)

Notes: The figure reports graphical results from three studies analyzing UI effects on job separation. Panel (a) is Figure 2 from Green and Riddell (1997). It illustrates the spikes of exit rate from employment at the work experience requirement (10 weeks in 1989 and 14 weeks in 1990). Panel (b) is Figure 4d from Albanese et al. (2020). It plots the layoff rate as function of job tenure in Italy for two groups of workers. Treated workers (dashed red line) become eligible for UI when they reach the 52 weeks threshold, while control workers are not eligible at any job tenure. Panel (c) is Figure 1 from Leung and O'Leary (2020). It shows the distribution of high quarter wages for new claimants in the US. On the right-hand side of the vertical red line, claimants are eligible for UI benefits, while on the left-hand side, they are not.

Figure 13: Recent quasi-experimental estimates of UI effects on workers separation



Notes: This figure reports UI effects on separation rates for ten studies (Winter-Ebmer, 2003; Tuit and van Ours, 2010; Baguelin and Remillon, 2014; Lalive et al., 2015; Albanese et al., 2020; Leung and O'Leary, 2020; Jäger et al., 2020; Van Doornik et al., 2023; Jäger et al., 2023; Jessen et al., 2023). We plot the main study estimate against the average age in the sample (or the age cutoff for RDD estimates). We distinguish three types of estimates. When the design allows to compare UI eligible workers vs. ineligible workers, estimates are in red (circle markers). When the design compares workers with more or less generous UI benefits (among eligible workers only), we use blue square markers in the case of Potential Benefit Duration contrasts and green triangle markers in the case of Replacement Rate contrasts. Vertical lines correspond to 95% confidence interval. For Leung and O'Leary (2020) and Baguelin and Remillon (2014), we assume that effects on unemployment inflow rates translate into separation rate effects. The estimates of Winter-Ebmer (2003), Landais (2015) and Jäger et al. (2023) overlap as they use the same DiD design in Austria (REBP).

more likely to separate from their employers.[42] While this effect is smaller than the 12-13% effect in Albanese et al. (2020) and Van Doornik et al. (2023), it is sizeable for a change in benefit generosity at the intensive margin.[43] The three previous positive estimates are to be weighed against Leung and O'Leary (2020) who fail to detect manipulation in the US at the past earnings cutoff determining UI eligibility (see Panel (c) in Figure 12 reproducing their Figure 1). Note that Leung and O'Leary (2020) analyze unemployment inflow rates rather than separation rates. Interpreting their results as evidence on separation rates implicitly assumes that take-up behaviors do not offset an initial discontinuity in the separation rate at the qualifying cutoff. This assumption requires further research.[44] However, the same argument that the absence of manipulation in McCrary (2008) test is implicitly a test of no UI effects on job separation applies beyond the context of Leung and O'Leary (2020) to any RDD studies analyzing UI effects of unemployment duration. We count seven of them in the review by Schmieder and von Wachter (2016) discussed in Section 3.3. This is numerous evidence against UI effects on separations, especially when the underlying UI variation is at the intensive margin. Jäger et al. (2020) also reports a small and insignificant effect of replacement rates on average separation rates in Austria leveraging four large reforms in DiD designs.

The literature is still silent about the sources of the difference in results across the previous studies on middle-age workers. The lack of precise information about UI rules may explain why in some contexts no manipulation is detected, while in other contexts salient reforms make employed workers correctly evaluate their outside options. Beyond information imperfection, the strictness of employment protection and the firms' separation costs are probably important determinants of the separation elasticity to workers' outside option. Further research is needed on those explanations.

The UI literature finds stronger separation effects among senior workers (above 50

[42] Note that the 6% estimate in Jessen et al. (2023) does not capture intertemporal substitution effect around the date when PBD extension is triggered.

[43] Hartung et al. (2024) is another study that finds lower transition rates from employment to registered unemployment after the decrease in UI generosity of the Hartz reforms in Germany. As the Hartz reforms are extensive, the authors implement an heterogeneous treatment design around the reform date, which requires stronger identification assumption than the other quasi-experiments in Figure 13.

[44] Anderson and Meyer (1997) report that claiming behavior or take-up depend positively on UI generosity. Note that to falsify the above assumption, one would need strong discontinuity in take up *negatively* correlated with UI eligibility.

years old). The evidence comes from large variations in Potential Benefit Duration at the intensive margin (blue squares in Figure 13). Winter-Ebmer (2003), Landais (2015) and Jäger et al. (2023) all analyze the same 1989 Regional Extended Benefit Period (REBP) reform that extends PBD from 30 to 209 weeks for workers above 50.[45] Their DiD estimates amount to a separation increase by around 25%. Tuit and van Ours (2010) and Baguelin and Remillon (2014) find a similar order of magnitude in response to PBD increase from 324 to 405 weeks in the Netherlands and from 200 to 270 weeks in France. One interpretation is that a large PBD increase allows senior workers to bridge the period between job separation and retirement. In such contexts, UI programs implicitly act as early-retirement schemes.[46] Clear evidence that UI affects job separations via this bridge-to-retirement channel has been found. Recent examples include Inderbitzin et al. (2016), Kyyrä and Pesola (2020), Riphahn and Schrader (2023), and Gudgeon et al. (2023).

LESSON 4: The empirical literature of the effects of UI eligibility on job separations is still thin, reporting both positive and zero effects. Separation effects of UI generosity are small for middle-age workers, but can be large for senior workers.

The previous studies do not precisely quantify the implied fiscal externality of separation effects. As an illustration, we describe this exercise from Khoury (2023). In France, laid-off workers in firms justifying economic difficulties receive higher benefits (by around 10%) if their job tenure at separation is above 2 years before 2011 and above 1 year after 2011. Khoury (2023) documents that the 2011 reform does not significantly change the overall number of layoffs, but leads workers and firms to retime layoffs. Namely, bunching estimates show that 10% of workers who would have been laid off in the month before reaching the one-year cutoff are actually laid off just after the cutoff because of the reform. Khoury (2023) studies the fiscal externality induced by the reform and quantifies two terms. The first term accounts for the behavioral job finding response of laid-off workers who have higher benefits because of their tenure being between one and two years (classical moral hazard of unemployed). The second term captures the extra UI spending on bunchers at the one-year cutoff. Overall, the second term due to separation retiming is marginal compared to the first classical moral hazard term. Further research

[45] Lalive (2007) and Lalive (2008) analyze the reform effects on unemployed behaviors.

[46] For example, in France, quarters on covered UI count towards pension eligibility requirements.

turning the previous separation effects estimates in Figure 13 into fiscal externality quantities would be helpful to assess the importance of separation effects for UI design.

In addition to separation effects, standard economic theory predicts that more generous UI would lead workers to exert less effort on the job. The effort reaction is expected to be all the larger that wages do not adjust upwards to support the net value of employment (which seems to be the case given the modest wage effects in the previous section). Empirical research on workers' effort on the job (or productivity) faces a measurement challenge. Workers' effort is not observable in standard dataset (independently of wages). Two recent papers overcome the difficulty by using scanner data from a retail company (Lusher et al., 2022) or by using sick leave data (Jäger et al., 2020). Lusher et al. (2022) find that an 18-week PBD extension during the Great Recession in the US leads to a 2% decrease in cashier scanning speed. Jäger et al. (2020) find with strong statistical power that changes in replacement rates in Austria do not affect time spent on sick leaves of employees. To sum up, the few available estimates point towards modest UI effects on effort on the job.

3.7 Other UI Design Questions

In this section, we discuss answers to other classical questions on UI design of particular interest for policy-makers. Should replacement rates decrease, increase or stay flat over the unemployment spell? Should UI generosity vary over the business cycle? How does UI interact with other social programs?

3.7.1 Should benefits decrease / increase over the unemployment spell?

In the dynamic Baily-Chetty framework analyzed in Section 3.2, we restrict the policy space to constant benefit levels b until an exhaustion date P when they drop to zero. In practice, we observe more elaborate designs with time-varying benefits b_t that differ across countries. While Belgium and Sweden used to have a constant benefit level that never expires, France implemented in 2021 a decreasing path of benefits where high-wage workers lose 30% of their unemployment benefits after seven months of unemployment. Should benefits decrease over the unemployment spell?

In their seminal contribution, Shavell and Weiss (1979) give theoretical insights to define the optimal path of UI benefits. They show that declining benefit levels provide powerful incentives for hand-to-mouth unemployed to search for jobs, which significantly reduces moral hazard cost of providing insurance. The intuition follows the job search model already analyzed in Section 2.2. Forward-looking job seekers increase search intensity as the future value of unemployment decreases. Holding constant overall spending on UI (absent any behavioral reactions), front-loading benefits improves workers' welfare by reducing moral hazard costs. However, there are also counteracting arguments supporting an increasing profile. If workers are not consuming hand-to-mouth and have initial wealth at job loss, the insurance agency should delay benefit payments to later periods when the unemployed workers have depleted their wealth. For these workers, marginal utility is higher later in the spell, and a benefit transfer has then higher consumption smoothing value. Shavell and Weiss (1979) conclude that the optimal path of benefits depends on the relative strength of the two channels, and could even result in non-monotonic path (first increasing and then decreasing) as the relative strength may vary over the spell. Following Shavell and Weiss (1979), a large theoretical optimal-contracting literature enriches the environment with more elaborate sources of non stationarity (Shimer and Werning, 2006, 2008) and broader sets of policy instruments (such as duration dependent wage tax or reemployment bonus in Hopenhayn and Nicolini (1997); Pavoni (2009)). They reach various conclusions from decreasing benefit profiles in Hopenhayn and Nicolini (1997) to slightly increasing profiles in Shimer and Werning (2008). Overall, the theoretical literature emphasizes the importance to introduce an endogenous wedge between consumption and benefits. A direction followed by the recent empirical literature assessing the local optimality of current benefit profiles.

Along those lines, Kolsrud et al. (2018) show that Baily-Chetty formulae apply at each date of the unemployment spell. They consider a small variation of benefits at date t: db_t. The social value depends on the average marginal utility of unemployed survivors at date t and the corresponding consumption smoothing value. The behavioral costs are due to implied changes in job finding rates over the entire spell. For example, higher benefits later in the spell slow down job finding even of short-term unemployed. Kolsrud et al. (2018) compute the empirical counterparts of the social value and behavioral costs of UI for every month after unemployment entry in Swedish data. They adopt the Consumption-Based approach to estimate the social value of UI along the spell. To identify the elasticity of the exit rate along the spell, they rely on duration-dependent caps on benefit levels in a RKD design, caps exogeneously shocked by a reform. The rich empirical design allows to estimate the incentive costs of changes in benefits both early versus late in the unemployment spell. As expected, the drop in consumption along the spell translates into higher social value of late benefits. The RKD estimates reveal that incentives costs are larger early in the spell. Consequently, both terms in the Baily-Chetty formulae suggest that increasing locally the benefit profile would increase welfare in Sweden. The fact that the moral hazard channel also leads to benefit back-loading contrasts with the incentive channel emphasized initially in Shavell and Weiss (1979). Surprisingly, estimated behavioral responses to benefit changes early in the spell are more costly than responses to late benefit changes (even though late changes are also found to trigger job finding response early in the spell).

The back-loading recommendation in Kolsrud et al. (2018) contrasts with the positive evaluation of a front-loading reform in Hungary by Lindner and Reizer (2020). The reform changes the time path of benefits, but keeps constant the overall amount of benefits paid over the potential benefit duration. Comparing job finding just before and after the reform date, Lindner and Reizer (2020) find positive effects on exit rates early in the spell. The behavioral reaction generates positive fiscal externality that amounts to 65% of the mechanical cost of the reform. Positive wage effects imply significant extra positive fiscal externality, even though not statistically significant. Without data on consumption, Lindner and Reizer (2020) cannot assess the social value of the reform.

LESSON 5: The recent empirical evidence challenges recommending benefit schedules with decreasing unemployment benefits over the unemployment spell.

3.7.2 Should benefits vary over the Business Cycle?

Should benefits vary over the Business Cycle? In the United States, federal or state programs provide PBD extension when local unemployment rates cross predefined levels. The US UI system is de facto countercyclical. Its generosity increases in recessions: PBD reached up to 99 weeks during the Great Recession. France is another example with countercyclical UI: since 2022, PBD decreases by 25% when unemployment rate is below 9%. On the other hand, many other countries have UI rules without automatic adjustments to business conditions (even though in practice policy-makers may decide discretionary changes in the UI rules depending on labor market conditions). The Baily-Chetty framework allows to assess the pros and cons of cyclical UI rules.

Both the social value and the behavioral costs of UI may a priori vary with business conditions. During recessions, job losers may have lower private savings and thus lower ability to smooth consumption. This would lead to higher insurance value in recessions than in booms. On the behavioral cost side of the Baily-Chetty formula, different theories lead to procyclical or countercyclical elasticities of unemployment duration wrt UI benefits (Landais et al., 2018a). One argument for procyclical elasticities is that during recessions vacant jobs are so scarce that job search effort does not significantly improve reemployment prospects. Then unemployment duration elasticities are smaller during downturns. Beyond duration elasticities, the behavioral cost of PBD extensions is determined by the inverse of the share of UI exhaustees (see factor $1/S_P$ in Equation (37)). When labor markets are slack, job findings rates are lower and the share of exhaustees increases mechanically. This second effect of labor market conditions induces procyclical behavioural costs and justifies to increase PBD during downturns. While the theoretical arguments for countercyclical UI are well understood, it is only recently that the UI literature quantifies the cyclical variation in the marginal welfare of benefit increase.

The first important contribution is Schmieder et al. (2012) who find support for countercyclical UI in the data. In Germany, job losers above certain age thresholds have higher PBD, which allows to identify the marginal PBD effect on nonemployment duration in RDDs. As the age-specific UI rule remains essentially invariant from the mid-80s to 2005, Schmieder et al. (2012) estimate yearly PBD effects in a consistent way. They find overall low cyclicality of PBD effects, and, if anything, procyclicality. Combined with the strong countercyclicality of UI exhaustees in Germany, Schmieder et al. (2012) conclude that the behavioral cost of UI is procyclical. While they do not have data on the social value of UI to assess its cyclicality, their empirical evidence points towards countercyclical PBD.

Second, Kroft and Notowidigdo (2016) offer complementary empirical evidence supporting countercyclical UI rules in the US. Kroft and Notowidigdo (2016) analyze consumption data from the PSID as in Gruber (1997). Focusing on within-state time variation in local unemployment rate, they document larger consumption drops when unemployment is higher: -8% in high-unemployment years ($u = 8.5\%$) vs. -5% in low-unemployment years ($u = 4.9\%$). Unfortunately, statistical power is low, and such variations are not statistically significant. Other estimates of the social value cyclicality would be helpful to make progress. In addition, Kroft and Notowidigdo (2016) estimate the unemployment duration elasticity wrt benefit levels (instead of PBD). While PBD changes are correlated with labor market conditions (by US UI design rules), Kroft and Notowidigdo (2016) argue that changes in benefit levels are exogenous and they report that their variations within states over time are not correlated with local unemployment rates. Using within-state time variation, they find that an increase in unemployment by 2.3 percentage points (from 6.2%) attenuates the duration elasticity by 50% (from 0.6 to 0.3). This is a large procyclicality of duration elasticity.

LESSON 6: The available micro empirical evidence suggests that behavioral costs of UI are lower during recessions.

The Baily-Chetty approach is in partial equilibrium and assumes away externalities and spillovers on uninsured job seekers or on firms and their employees. This assumption may be strong when analyzing UI rules in different labor market conditions. We discuss further macro effects of UI in Section 3.8.

3.7.3 Substitution with other programs from the safety net

After they exhaust their UI benefits, unemployed workers may claim welfare benefits or enter into disability programs. Similarly, job losers who fail to meet the qualification criteria to be eligible for UI benefits may rely on other programs from the safety net. Consequently, changes in UI rules lead to potential substitution effects with other social programs, and the design of UI should take them into account. As emphasized in Hendren and Sprung-Keyser (2020), the UI spillover effects on other programs contribute to the behavioral costs (or savings) part of the UI welfare analysis. If extra spending on UI reduces disability applications, the fiscal externality is positive as the government saves on disability spending. The empirical literature finds suggestive evidence that more generous UI benefit levels decrease applications to Disability Insurance (DI), but effects are imprecisely estimated (Lindner, 2016). Mueller et al. (2016) do not find meaningful effects of PBD extensions on DI applications during the Great Recession. Using estimates from Lindner (2016), Hendren and Sprung-Keyser (2020) compute that one extra dollar of UI transfer saves $0.33 in DI spending. The DI spillover fiscal externality is large (about half the median fiscal externality without spillovers reported above). As the initial substitution estimate is imprecisely estimated, there are also large standard errors around the DI fiscal externality and no strong conclusions should be drawn. However, Hendren and Sprung-Keyser (2020) computation illustrates how important spillovers to other programs may be. Leung and O'Leary (2020) studies substitution at the initial eligibility margin. They find that becoming UI eligible at job loss reduces welfare (TANF) receipts by half among low-earnings UI applicants. However overall TANF participation of UI applicants is low, so that transferring one dollar to the unemployed through a softer eligibility requirement saves only $0.04 in TANF spending.

Finally, the recent empirical UI literature leverages new large and detailed datasets to identify effects on outcomes outside of the labor market, such as health or crime (e.g. Britto et al., 2022). As providing unemployment insurance typically reduces the occurrence of bad health or crime after job loss, governments may be able to reduce spending in public health programs or in the judicial system. Further research is definitely welcome to better account for those positive spillovers in UI design.

3.8 Micro and Macro effects of UI programs

In this section, we discuss effects of UI programs beyond the micro effects on UI beneficiaries. When UI claimants compete for jobs with non beneficiaries, any changes in their job search effort impact the job finding rate of uncovered job seekers. In parallel, UI programs may affect job creation, as they make recruitment more costly for firms or push up equilibrium wages and lower expected profits. The externalities of UI programs on uncovered job seekers and on firms are additional channels affecting the labor market equilibrium. This section discusses how to account for equilibrium effects when assessing the effects of UI programs on aggregate social welfare and provides recent empirical evidence on their magnitude.

3.8.1 Welfare Effects of UI programs in Equilibrium

The underlying job search model of the Baily-Chetty approach is in partial equilibrium. It assumes that job finding depends on individual search effort only. To account for externalities and equilibrium effects, we allow individual job finding rates to also depend on aggregate labor market conditions through tightness. To fix ideas, we write the individual job finding rate h_i as:

$$h_i = e_i f(\theta) \tag{59}$$

where e_i is the individual job search effort and $f(\theta)$ is the job finding rate per unit of search effort, which depends positively on the equilibrium labor market tightness (θ). Tightness is defined as the ratio of vacancies posted by firms V over aggregate search effort $\bar{e}U$. In equilibrium, tightness is determined by aggregate labor supply and labor demand. The key idea of the equilibrium analysis is that UI programs affect labor market tightness through the reactions of claimants and of firms. In such random matching models, the decentralized equilibrium is generally inefficient (unless the Hosios condition is satisfied when wages are bargained under Nash, see Pissarides 2000). Consequently, more generous UI programs increase social welfare if they push tightness towards its efficient level (Landais et al., 2018a).

More precisely, Landais et al. (2018a) develop a theory of optimal UI in matching models. They assume random search. Firms post vacancies to recruit, which costs the wages of the employees that they allocate to recruitment activities. Firms have decreasing marginal returns to labor. Those assumptions generate downward sloping labor demand curves in the tightness - employment plane, even when wages are fixed. On the supply side of the market, workers choose search effort. The model has a general mechanism for wage setting (reduced-form, not micro founded). It assumes that wages depend on tightness and the net value of employment for workers. This allows for rigid wages or Nash bargaining models. In such a framework, Landais et al. (2018a) derive the following formula for the optimal UI replacement rate (R):

$$R = \underbrace{\frac{l}{\epsilon^m} \frac{u(c_e) - u(c_u)}{w} \left[\frac{1}{u'(c_e)} - \frac{1}{u'(c_u)} \right]}_{\text{Baily-Chetty Replacement Rate}} + \underbrace{\left[1 - \frac{\epsilon^M}{\epsilon^m} \right] g(\theta, \theta^*)}_{\text{Equilibrium Correction}} \tag{60}$$

where ϵ^m and ϵ^M are the micro and macro elasticities of unemployment wrt UI and $g(\theta, \theta^*)$ is an efficiency term measuring how far the current equilibrium tightness θ is from the efficient level θ^*. The macro elasticity ϵ^M measures the total response of unemployment to a change in UI, when all endogenous reactions are allowed. It takes into account labor demand reactions, and wage adjustment. The micro elasticity ϵ^m measures the change in unemployment due to the reaction of unemployed search effort, holding tightness constant. For the sake of space, we do not detail here the expression of the efficiency term $g()$, but we note that it is zero when the equilibrium is already efficient ($g(\theta = \theta^*, \theta^*) = 0$).

The first implication of the optimal replacement rate formula is that spillovers matter to the extent that they create a wedge between the micro and macro elasticities. If tightness is not affected by UI generosity, then the micro and macro elasticities are equal. The equilibrium correction disappears from the formula and only the first term remains. It corresponds to the optimal replacement rate from the partial-equilibrium Baily-Chetty formula. It is then sufficient to trade off the consumption smoothing benefits of UI with the incentive cost due to lower search intensity.

When the macro and micro elasticities differ ($\epsilon^M \neq \epsilon^m$), the equilibrium correction matters to the extent that $\theta \neq \theta^*$. For example, if wages are bargained under Nash with bargaining power equal to the elasticity of the matching function (Hosios condition), then the decentralized equilibrium is efficient and the marginal effect of tightness on social welfare is zero. When the baseline tightness is away from θ^*, the efficiency term $g(\theta, \theta^*)$ can be positive or negative depending on whether a tightness increase pushes towards θ^* or resp. away from θ^*. Following up on the same example, under Nash bargaining, the decentralized tightness may be lower or higher than θ^*, depending on the workers bargaining power.

In the end, whether the equilibrium correction increases or decreases welfare depends on the product of both the signs of the efficiency term and of the elasticity wedge factor. When the macro elasticity is greater than the micro elasticity ($\epsilon^M > \epsilon^m$), spillovers reduce tightness following a UI benefit increase. If baseline tightness is inefficiently low ($g > 0$), macro effects generate a social welfare cost, and the macro optimal replacement rate is lower than the Baily-Chetty replacement rates. On the contrary, if tightness is inefficiently high ($g < 0$), macro effects increase social welfare, pushing upwards the macro optimal replacement rate.[47]

[47] In random search models, tightness is inefficiently high, when workers bargaining power is low.

Formula (60) highlights the critical role of the macro-micro elasticity wedge. Following Landais et al. (2018a), a recent but growing empirical UI literature seeks to identify this key parameter. Before reviewing it in the next section, this raises one last theoretical question. The standard search and matching model with Nash bargaining unambiguously predicts that UI lowers labor market tightness (through wage pressures). Which matching models predict the opposite?

Landais et al. (2018a) show that the job rationing model of Michaillat (2012) features lower macro elasticity than micro elasticity. This is due to a *rat-race* channel that limits macro UI effects on unemployment. The intuition can be better conveyed by assuming rigid wages. Let us also assume that wages are relatively high, so that jobs are rationed.[48] For the sake of the illustration, let us take the extra assumption that the number of jobs is fixed. When an individual unemployed reduces job search effort, this mechanically increases the employment opportunities of competing job seekers. Low-search-effort unemployed go down the queue in front of the jobs, shifting up other candidates in the queue. Consequently, the job finding rate per unit of search increases and so does tightness. Going back to the job rationing model with endogenous job creation, the rat-race effect is stronger when labor demand is less elastic (in the tightness - employment plane).

3.8.2 Empirical Evidence on Spillovers and Macro Effects

Following the Great Recession and the theoretical contribution of Landais et al. (2018a), a fast growing UI literature seeks to produce empirical evidence on spillovers and macro effects. The objective is quite different from the micro empirical strand that we reviewed extensively in Section 3.3. Seeking clean identification, the micro literature generally compares treated and untreated unemployed who are as close as possible from one another. Mimicking the experimental paradigm, it looks for individual-level exogenous variations that randomize unemployed in treatment groups with higher UI benefits. One important requirement is then to compare treated and untreated unemployed who search for jobs in the same labor markets in order to hold labor market conditions constant. Consequently, by design, the micro literature does not identify UI effects on aggregate labor market tightness. In addition, while refining the individual-level comparison, the micro method tends

[48] With high wages, decreasing marginal returns would prevent firms to hire all workers even if tightness is zero.

to select samples of untreated unemployed who are the most likely to compete with the treated unemployed. As a consequence, the micro elasticity estimates may capture large rat-race effects that only a macro approach would allow to properly account for when concluding about welfare effects. This identification issue is a classical trade off between the CIA and the SUTVA assumptions in the design of credible evaluation.

To quantify the importance of macro effects, the empirical literature develops designs that mimick randomization at the market level. Market-level quasi-experiments identify the macro elasticity of unemployment needed in Formula (60). They also document the nature of spillovers by identifying market-level changes in vacancies and aggregate wages and by identifying UI effects on the job search of non UI claimants in treated markets.

Lalive et al. (2015) find a significant *rat-race* channel in Austria in the late 1980s - early 1990s. They leverage the REBP policy shock that extended PBD by almost 3 years for senior unemployed in 28 of the 100 regions in Austria (micro UI effects of the REBP are previously documented in Winter-Ebmer, 2003; Lalive, 2007, 2008). The setting is particularly well suited for identifying macro effects, as some regions experience an increase in UI generosity while others are left untreated. Even if treated regions are selected based on their industry composition, the DiD assumptions are reasonably met. The setting is even better suited for documenting market externalities, as some unemployed in treated markets are not eligible for PBD extension because of their exact age or work history. DiD estimates show that ineligible unemployed have lower unemployment duration in treated markets than in non-treated markets (2 to 8 weeks depending on the non-eligible group considered). The spillovers on ineligible workers (and the absence of wage effects) imply that the macro elasticity is 20% lower than the micro elasticity.

Five studies provide macro UI effect estimates for the US during the Great Recession (Hagedorn et al., 2013; Marinescu, 2017; Johnston and Mas, 2018; Chodorow-Reich et al., 2019; Boone et al., 2021).

Marinescu (2017) analyzes a state-level dataset on job applications and vacancies from CareerBuilder.com, a very large American online job board, and uses as source of UI variation the PBD extension from the federal EUC and state-level programs. While aggregate job applications decrease by 1% when PBD increase by 10%, there is no robust effect on vacancies. The empirical evidence is consistent with the job rationing model with rigid wages, where labor demand is inelastic. The macro-micro wedge amounts to 39%.

Johnston and Mas (2018) study a large PBD cut in Missouri in 2011 (unanticipated and politically motivated). To obtain the micro elasticity, they contrast the unemployment duration of UI claimants who claim just before or just after the reform date (RDD), which holds tightness constant. They estimate the macro elasticity comparing the evolution of the aggregate unemployment in Missouri to a synthetic control made up of several other US states. Their micro and macro estimates are consistent with the absence of spillovers on unemployed who are not directly affected by the reform (zero macro-micro wedge).[49]

Chodorow-Reich et al. (2019) analyze PBD extension state-level events that occurred because of measurement error in real-time unemployment statistics (observed ex-post thanks to revised unemployment series). This allows to overcome the mechanical link between unemployment and benefit extension in the US during the Great Recession. In their baseline specification, they find that increasing PBD by one month generates at most a 0.02 percentage point increase in unemployment rate. Similarly, they find small insignificant effects on state-level vacancies and worker earnings. Chodorow-Reich et al. (2019) find smaller macro effects than Johnston and Mas (2018). Assuming that the micro elasticity of Johnston and Mas (2018) applies to the context in Chodorow-Reich et al. (2019), their result implies significant spillovers during the Great Recession.

Boone et al. (2021) implement another strategy to circumvent the mechanical link between unemployment and benefit extension during the Great Recession in the US: a *border design*. The design compares two adjacent counties in neighboring states that experience different benefit extensions. Boone et al. (2021) rule out that one month increase in PBD decreases the employment to population ratio by more than 0.02. Their result is of similar magnitude as Chodorow-Reich et al. (2019), but much less negative than the first border-design estimates by Hagedorn et al. (2013).[50]

Overall, the US evidence from the Great Recession suggests a limited role for UI

[49] In a recent study, Jessen et al. (2023) also find that macro and micro elasticity are equal in Poland. They further show the absence of spillovers on untreated unemployed and of effects on tightness.

[50] Differences come from the use of different data sources and Boone et al. (2021) estimating more flexible econometric models.

extension in the aggregate unemployment increase and the subsequent slow recovery. Except in Johnston and Mas (2018), macro effects are smaller than micro effects because of spillovers on uncovered unemployed. An alternative explanation lies in the stabilization role of UI expansions as they support aggregate demand (McKay and Reis, 2021). Further research is needed especially on quantifying the aggregate demand externality.

While all the previous studies allow to estimate the micro-macro wedge, they do not inform on the efficiency term and more importantly on its sign. Landais et al. (2018b) develop an identification strategy for the efficiency term based on proxies for recruitment costs (namely the share of workforce dedicated to recruiting).[51] They find that tightness is inefficiently low in recessions and inefficiently high in good times. Together with a macro-micro wedge lower than one, the efficiency term estimates suggest countercyclical UI even when accounting for market-level externalities.

To summarize, our review of the recent literature allows us to draw the following lesson.[52]

LESSON 7: Recent empirical evidence from seven studies suggests that macro elasticities are not larger than micro elasticities and possibly smaller.

3.9 Discussion

To conclude the section on UI policy design, we put together the different lessons that we draw for UI policies, and discuss other avenues for future research.

After presenting the Baily-Chetty framework and its extensions, we reviewed the recent empirical assessment of the behavioral costs and social value of UI and drew the following lessons:

1. **LESSON 1: The behavioral costs of providing UI are substantial.**
2. **LESSON 2: Estimates of the social value of UI differ widely across identification methods. The most recent methods which are robust to risk-aversion assumptions yield significantly higher estimates.**

[51] The efficiency terms also depend on marginal social cost of unemployment: fiscal cost and non-pecuniary cost on unemployed.

[52] See Cohen and Ganong (2024) a recent review of micro-macro elasticity wedge.

3. **LESSON 3: In the majority of recent studies, more generous UI policy decreases wages imposing further (second-order) behavioral cost to provide UI.**

4. **LESSON 4: The empirical literature of the effects of UI eligibility on job separations is still thin, reporting both positive and zero effects. Separation effects of UI generosity are small for middle-age workers, but can be large for senior workers.**

5. **LESSON 5: The recent empirical evidence challenges recommending benefit schedules with decreasing unemployment benefits over the unemployment spell.**

6. **LESSON 6: The available micro empirical evidence suggests that behavioral costs of UI are lower during recessions.**

7. **LESSON 7: Recent empirical evidence from seven studies suggests that macro elasticities are not larger than micro elasticities and possibly smaller.**

While the Baily-Chetty model and its extensions to market-externalities provide a powerful framework to discuss the design of UI, it relies on optimizing neoclassical agents. In Section 2, we reviewed recent empirical evidence that is more in line with behavioral models with agents whose preferences exhibit reference dependence, present bias, or incorrect beliefs. Welfare effects with this type of agents can differ substantially from the predictions of neoclassical models (for example, Spinnewijn, 2015; Mueller et al., 2021, discuss how to account for biased beliefs). Further research would be helpful to cast the optimal UI analysis further into behavioral models.

At least two other strands of the literature on optimal UI make recent advances and are only touched upon in Section 3. First, new data allow to study UI designs in the developing world. This literature discusses classical issues related to informality from a new perspective (Gerard and Gonzaga, 2021; Van Doornik et al., 2023; Liepmann and Pignatti, 2024). It also delivers first rate empirical evidence about UI effects on crime and domestic violence (Britto et al., 2022; Britto, 2022). The UI literature in developing countries is also interested in the joint design of severance pay and UI (Gerard et al., 2024), and in the design of alternative insurance policies such as individual saving accounts (Hartley et al., 2011).

Second, in the wake of the COVID pandemics (2020-2022), there is a regained interest in exceptional crisis UI policies (Ganong et al., 2022) and how short-time work policies complement or substitute UI policies (see the corresponding chapter by Cahuc in the same handbook). Short-time work (STW) policies allow firms to reduce working hours of their employees on a temporary basis, while they receive public subsidies to replace their wages. Workers under the STW scheme are in the gray area in-between employment and unemployment. In the US, workers in the gray area in-between employment and unemployment are targeted by another program: partial UI. Partial UI allows UI claimants to work in low-earnings jobs and still receive some UI benefits. Recently, Lee et al. (2021) and Le Barbanchon (2020) study the design of partial UI.[53] Further analysis on those "grey-area" programs would be particularly helpful for UI design.

Another first-order design question is how to complement UI policy with Active Labor Market Policies. We provide answers of how ALMPs work in the next section.

4 Active labor market policies

We have seen in the previous sections that job search and especially the choice of search effort are important parameters determining labor market outcomes. UI programs provide income support that allows unemployed workers to smooth consumption after an income shock but also triggers behavioral responses due to moral hazard. Active labor market policies (ALMPs) have been designed to complement passive benefit programs and address labor market frictions. The policies have three broad strategies to achieve their goals. First, ALMPs aim at reducing moral hazard from benefit receipt by imposing search requirements and monitoring search efforts. Second, they aim at improving the efficiency of individual job search and speeding up the return to employment of unemployed job seekers by providing counseling. Third, they aim at reducing skill mismatch in the labor market by providing skill training to low wage and unemployed workers, allowing them to access better paid jobs and thus improving their labor market outcomes. Following these goals, a large number of different ALMPs have been developed around the

[53] see McCall (1996) for an early analysis of employment effects of partial UI in the US

world. These programs differ widely depending on the policy objective, the program content, the target population, and in terms of program costs. From policy design, it is extremely important to understand if these programs work and how they achieve their results. Do ALMPs help integrating job seekers in the labor market or improving labor market outcomes of participants? Under which conditions are the programs cost effective? Do programs have unintended externalities for example due to displacement of non-participating job seekers?

In this section, we first review existing meta-analyses and summaries of the ALMP evaluation literature which comprehensively cover studies written until 2014. Then we turn to the most recent contributions that were written over the last 10 years, a period over which we observe a massive expansion of the use of and interest in ALMPs worldwide. We review the recent literature with the aim of highlighting the areas where it has advanced. We organize the review in 10 main lessons drawn from studies that make significant contributions that go beyond standard program evaluation exercises. Tables 6 and 7 provide a list of 37 studies included in our review along with their main features.

We see the review of the ALMP literature in this chapter as a complement to a systematic meta-analysis. The meta-analysis would cover a much larger range of studies to allow a systematic review of program effects found in the literature. But it would have to restrict the focus of studies to a set that fits into a common scheme, for example in terms of program types and outcome variables. Here, we are not so much interested the systematic review of program effects on certain outcomes, as in pointing out novel and promising approaches in program design, research questions regarding the economic and welfare impacts surrounding ALMPs, and contributions in evaluation strategies.[54] To visualize the overlap between the two approaches, column (1) in Tables 6 and 7 shows an indicator whether the study fits in the ALMP meta-analysis scheme adopted by Card et al. (2018).

4.1 Meta Analysis Studies

The earliest program evaluation studies of ALMPs go back to the 1970's and 80' in the US (Ashenfelter, 1987). In Europe interest in the effectiveness of ALMP pro-

[54] In fact, our focus on promising program designs potentially introduces a bias towards examples of successful ALMPs. Our question is not so much what works on average? It rather is, what can work?

grams sparked in the mid 1990's at a time when problems with unemployment were high on the political agenda and detailed micro data on program participants and their labor market outcomes increasingly became available. Since then the number of ALMP evaluation studies has been exploding around the world. Card et al. (2010) and Card et al. (2018) performed meta-analyses of studies written from 1995 until 2007 and 2014, respectively. They focus on evaluations of active programs that are targeted at individuals who participate in the program for a limited time. Further, they restrict the analysis to cover studies based on micro data which apply a treatment and control group design with some form of selection correction. The second meta-analysis relies on a cumulative number of 207 studies. Card et al. (2018) categorize programs into five main types. *Job search assistance* (JSA) programs, which provide counseling to job seekers and monitor the search effort of benefit recipients, either in personal meetings with case workers or in group workshops. *Training* programs focus on human capital enhancement either in classroom training or a combination of on-the-job and off-the job training. *Employment subsidies in private sector jobs* aim at bringing job seekers into employment and rely fully on on-the-job human capital enhancement. *Public sector employment programs* create special jobs in the public sector which employ unemployed workers in sectors where they are not competition with the private market. The fifth program type includes programs which combine features of different types, for example training programs with a placement assistance.

To construct the meta-data, the authors extract program effect estimates for 5 main program types and different participant groups over three time horizons, estimating short-run effects (in the first year after program completion), medium run (1-2 years after the program), or long run (more than 2 years after the program. In total Card et al. (2018) collect a sample of 857 separate program estimates from all studies. The main outcome variable considered in about 40% of the collected estimates is the employment rate. Other outcomes are earnings or exit rates from unemployment or into employment. The sample of estimates covers a wide range of countries worldwide. But the majority are from Europe and the US, such that Germany, Denmark, France, and the US make up more than 50% of the sample. In terms of program types, about 50% of the estimates evaluate impacts of training programs.

Card et al. (2018) report four main findings from their meta-analysis. First, short run program effects tend to be small but average program effects improve in the medium and longer run. Second, the time profile of program effects varies by program type. While job search assistance programs have relatively stable effects over different horizons, the effects of programs with a human capital component improve strongly over time. Third, there is some evidence of heterogeneity of program effects for different participant groups and of potential gains from matching specific participants to specific program types. But due to small sample sizes these results were not stable. Fourth, program effects vary with cyclical conditions and the authors find that ALMPs have larger impacts in times of low GDP growth or high unemployment.

On the methodological side, Card et al. (2018) find that average effects reported by experimental designs – about 30% of the sample – are not systematically different from non-experimental estimates. Neither do average effects differ systematically between published and non-published studies. The meta-analysis does not give evidence of other forms of "publication bias" either, which is fairly uncommon in the meta-analysis literature. It appears that researchers or referees did not have a strong preconception that ALMPs necessarily have positive effects or that non-positive findings are uninteresting. Furthermore, many studies were conducted in close collaboration with the administration operating programs which increased the exposure of many different types of findings. Another remarkable insight from the meta-analysis of program estimates is a large dispersion in program effects even among estimates with high levels of precision. Typically, we would assume that as sample sizes increase and estimates become more precise the range of estimates should get closer to the "true" program effect. In the ALMP literature it seems to be hard, however, to nail down a "true" effect. Even within relatively narrow categories of program types there appears to be a large degree of unobserved heterogeneity in program effects that cannot be explained by sampling error. The origin of this heterogeneity is probably due to differences in institutional environments, participant groups, or program implementations.

Limitations of the major part of ALMP evaluation studies written by 2014 were twofold. First, few studies provided a detailed cost-benefit analysis or precise measures of program costs which made it impossible to assess the cost efficiency of different programs. Second, the evaluation studies focused on partial equilibrium effects comparing the mean outcome in the treatment group with the mean out-

come in the control group. At the time, there were little concerns about general equilibrium effects or potential spillover and displacement effects (except Crépon et al., 2013). This reduces the external validity of the findings and leaves many questions regarding program scalability unanswered.
Recent reviews of the literature include Crépon and van den Berg (2016) who review evidence from experimental studies and discuss policy relevant and methodological questions that advance the literature. McCall et al. (2016) provide a long run review of the literature on government sponsored vocational education in the US and 5 European countries (UK, Denmark, Sweden, France, Germany) with a focus on training provided via ALMPs. McKenzie (2017) reviews evidence from ALMP evaluations in developing countries.

4.2 Lesson 1: New insights in the role of caseworkers

At the employment office job-seekers interact with caseworkers who provide job search assistance and counseling, monitor search activity, and impose benefit sanctions for non-compliance with search requirements. In addition, caseworkers refer job seekers to ALMP programs. This means that caseworkers potentially have an important role in shaping search success and longer-run outcomes of job-seekers. A number of recent studies provide valuable insights about the role of caseworkers. This literature has overcome two important empirical challenges. Firstly, by accessing novel administrative data that contain information on individual matches between job-seekers and caseworkers. Secondly, by exploiting random assignments of job-seekers to caseworkers either via an RCT design or by exploiting quasi-random assignments in the institutional setting.

Evidence fom the US: The use of caseworker resources varies widely across countries. In particular, they are less systematically used in the U.S than in Europe. Rising unemployment rates at the start of the Great Recession led to increased funding and renewed the interest in so-called *re-employment programs* in the US. The main purpose of this JSA program was to assess benefit eligibility and monitor search effort of new benefit recipients. In addition, some programs also provided job search counseling to unemployed workers with positive eligibility reviews. Michaelides and Mueser (2020) evaluate four programs in three US states that were implemented in an experimental design. Potential participants were randomly assigned

to receive letters inviting them to meetings with a caseworker. At the meetings an attendant's eligibility status was assessed and non-eligible benefit recipients were disqualified. If the program had a service component the remaining attendants received counseling services, information about other job search services, and referrals to training programs. Outcome data from administrative records of the employment office reveal that all programs substantially shortened participants' unemployment benefit receipt durations either because individuals did not attend the meeting or because they were disqualified after the assessment. While programs focused on eligibility monitoring only had short-lived effects, programs with a counseling component also succeeded in increasing employment and earnings of participants over the first year after program assignment.
An evaluation of longer run effects of the most successful re-employment program implemented in Nevada, shows that these employment and earnings effects even persisted over a longer horizon of up to 8 years (Manoli et al., 2018). Re-employment programs were first expanded during the Great Recession but in many locations they also continued during the post-recessionary period which allows an examination of the effects under different labor market conditions. Michaelides and Mueser (2023) exploit the fact that the program design as well as the random assignment mechanism of the Neveada REA/RES program was more or less unchanged for a nine year period. They show that the beneficial employment and earnings effects can also be found in a tighter labor market. Similarly, McConnell et al. (2021) report positive employment and earnings effects of intensive counseling services in the Adult and Dislocated Worker Programs implemented in the post-recessionary period.

Evidence fom the Europe: Many European countries rely more heavily on caseworker meetings and enforce stricter search monitoring of unemployment benefit recipients than the United States. In these systems benefit recipients are required to attend regular meetings with case workers and to keep detailed records of their search activity which is monitored by the caseworker (Maibom et al., 2023). Schiprowski (2020) examines the effect of caseworker meetings on the exit rate from unemployment. She exploits random variation in caseworker absences in Switzerland and finds that a cancelled meeting leads to a 5% increase in unemployment duration. The impact of a caseworker absence is positively related to caseworker productivity. Missing a meeting with a below median productive caseworker has no impact on unemployment duration but missing a meeting with a highly productive caseworker is strongly detrimental. Another piece of evidence on the effectiveness of caseworker meetings is provided by Schiprowski et al. (2024), who use the SMS data from DellaVigna et al. (2022) to study the dynamics of job search effort around caseworker meetings. They find that search effort increases modestly just before a caseworker meeting and falls back afterwards. Caseworker meetings that are accompanied by a formal agreement on search effort between workers and caseworker are more effective, as are vacancy referrals by caseworkers.

Cederlöf et al. (2021) systematically estimate the value added of caseworkers in Sweden with respect to job finding and job quality. They exploit quasi-random assignment of job seekers to caseworkers by the date of birth within local employment offices. It turns out that caseworker value added has substantial impacts on search outcomes along both outcome dimensions. Replacing the lowest quartile of caseworkers with an average caseworker would shorten unemployment durations by about 10%. High value added caseworkers also affect earnings and employment outcomes of job-seekers in the long run. Consistent with the literature on teachers, the Swedish results imply that caseworker value added is multi-dimensional. Different types of caseworkers reduce unemployment durations or increase employment and earnings outcomes. A caseworker characteristic that is strongly correlated with value added is caseworker experience. There are also potential gains from matching caseworkers and job seekers who are similar in terms of labor market experience.

A further strand of the literature investigates the timing and frequency of caseworker meetings. Meetings early in the unemployment spell might have a threat effect of pushing reluctant job seekers off the benefit roll but they are also more costly as they involve a larger number of job seekers some of whom might find jobs without external help. Homrighausen and Oberfichtner (2024) investigate a program in Germany which randomly assigns offers of caseworker meetings to pre-registered job-seekers who have not lost their jobs yet.[55] While the early meeting invitations increase the number of meetings attended during the first months of the unemployment spell, early meetings are not successful in reducing inflow into unemployment nor do they speed up re-employment.

[55] In Germany workers have to register at the employment office as soon as they receive a layoff notice from their employer or at least 3 months before the termination of a temporary job.

Maibom et al. (2017) explore the effects of a set of Danish programs which vary the timing and frequency of caseworker meetings in an experimental design. They find that early individual caseworker meetings significantly improve employment outcomes of job seekers. But programs designed to generate a threat effect do not have a significant impact. Böheim et al. (2022) evaluate an experiment which exogenously varies caseworker caseloads at the employment office. They find that caseworkers with lower caseloads schedule more meetings per job seeker and the additional meetings result in small positive employment effects over the next 2 years.

Evidence from programs that are especially designed for disadvantaged workers shows that these workers may benefit from caseworker assistance not only during job search but also from job coaching and career advice *after* a job has been found. In the next section we will discuss how so-called *wraparound support services* which include coaching and follow-up services after program completion lead to more favourable job changes (Bobonis et al., 2022; Katz et al., 2022).

4.3 Lesson 2: More focus on programs for special groups

Most of the ALMP programs surveyed in the meta-analysis by Card et al. (2018) were avaliable to broadly defined intake groups, such as unemployed, UI benefit recipients, or low income workers. Evaluation studies often estimated treatment effects for different groups to investigate effect heterogeneity by gender, age and other characteristics. But the needs of individual job seekers are potentially very different and there are limits of one-fits-all programs targeted to broad intake groups. The recent literature presents several studies evaluating programs that were specifically designed for particular target groups. Here we discuss examples of programs for disadvantaged workers, immigrants, and youths.

Disadvantaged workers are individuals with low labor market attachment, low income and low education. Card et al. (2018) find that ALMPs tend to be more successful for participants from disadvantaged groups. In particular, they benefit more JSA programs than registered unemployed. The task of ALMP is to integrate disadvantaged groups in the labor market and to help them find and keep good jobs with career opportunities. Mostly this is achieved by increasing their occupational skills. But in line with the success of search assistance for disadvantaged workers suggested by the meta-analysis, these workers might also benefit from training in soft skills helping them to overcome psychological barriers and improving their work attitudes, behavior and decision making. The literature discusses several approaches to addressing the lack of participants' non-cognitive or cognitive skills. They consist of intensive counseling services, non-cognitive skill training, combinations of skills training and counseling, and wage subsidies.

Bobonis et al. (2022) evaluate the Self-Sufficiency Project (SSP) Plus program that was implemented in a randomized control trial in Canada in the 1990s. The main branch of the program, SSP Regular, offered time limited financial incentives to take up employment in the form of earnings supplements to single parents who were long-term income assistance recipients. In addition to earnings supplements, SSP Plus offered intensive employment support services during and after job search. In particular, program participants were matched with individual caseworkers who pro-actively offered counseling and advice for job search and career advancement over a period of up to four years. Take-up of this service was voluntary for program participants.

Bobonis et al. (2022) evaluate long-run effects of SSP Plus relative to SSP Regular over a 20 year horizon. Their results show that the intensive support services led to substantial and long-lasting earnings gains compared to the regular SSP program where earnings gains faded quickly after the earnings supplements had expired. SSP Plus participants experienced an increase in full-time employment and a decrease in receipt of welfare benefits. Looking into the mechanisms driving these effects, the authors find that the support service helped participants to move up the career ladder towards better paid and more stable jobs. Survey evidence also shows an improvement in non-cognitive skills and measures of grit.

Sectoral employment programs in the US studied by Katz et al. (2022) target young low-wage workers with less than high school education. These programs offer a package of measures combining soft-skills training, occupational skill training and career support services which start with the job search period and extend to the post-employment period where they provide retention and advancement services. Similar to the Canadian SSP Plus program, sectoral employment programs succeed in persistently raising participant's earnings and job quality in the medium run. While it is challenging to disentangle the contribution of the different program components, the available evidence suggests that wraparound services are essential complements to occupational skill training. Career counseling, in particular, helps job seekers with non-traditional careers get access to jobs in high paying firms.

Schlosser and Shanan (2022) present an experimental evaluation of *employment circles* in Israel, a program that offers training in soft and occupational skills along with frequent caseworker meetings. The program is targeted at income support recipients a group with low earning and employment prospects, who are, however, required to search for jobs in order to keep their benefits. The authors argue that employment circles can have two potential effects on participants, first a threat effect due to enforcement of program requirements that will mainly reduce welfare recipiency and second, a skill enhancement effect that should lead to increases in employment and labor earnings. The study compares effects among two participant groups, new entrants into income support receipt and long-term support recipients. Evaluation results show that the program increases measures of soft skills such as grit and the motivation to search for jobs and to work in the group of long-term support recipients. This group also gains from the program in terms of employment and labor earnings along with reduced income support receipt. Among new benefit entrants the threat effect of the program dominates, however. While there are only small gains in employment among new entrants assigned to the intensive program, they are more likely to leave the benefit system.

Kasy and Lehner (2023) study a public sector employment subsidy in Austria with the aim of ”eradicating long term unemployment”. The program offers 3 years of subsidized employment to long-term unemployed workers. The wage in subsidized jobs is determined by collective bargaining agreements which raise wage earnings of participants above the UI benefit level. Eligible workers were randomly assigned to enter the program in two cohorts which allows evaluating short run program effects. Due to the wage incentives, program take-up is extremely high which in turn implies positive short run employment and earnings effects. Beside labor market outcomes, the focus of the study is on health, job satisfaction and societal well-being of participants. All these outcomes are positively affected by the wage subsidy.

Immigrants and Refugees In many countries immigrants have substantially lower employment rates than natives even many years after arrival (Brell et al., 2020). Especially low-skilled immigrants from low-income countries or refugee immigrants who relocate involuntarily and have to cope with traumatic experiences face substantial problems with labor market integration. A small literature that is mostly focused on Northern European countries investigates whether and how these problems can be overcome by specially designed ALMPs. Arendt et al. (2022) and Foged et al. (2024) review the effects of different types of welfare and integration policies, which include ALMP, on the labor market performance of refugees in Denmark. Denmark has admitted refugees over a long period during which the policy environment changed multiple times. Together with detailed longitudinal data this provides an ideal setting of studying these policies.

Generally, the literature finds only moderate effects of ALMP participation on immigrants' or refugees' labor market outcomes. Explanations for the moderate effects relate to the multi-dimensionality of problems faced by participants and to conflicting incentives within the institutional setting (Arendt et al., 2022). For example, immigrants who are uncertain about their residency status face low incentives to search for jobs or trainees may not be able to fully benefit from training programs because of language problems. The most promising programs are those that closely target program content and the sequence of measures to individual needs. A novel Finnish program combines individualized training plans with intensive language courses for immigrants. Sarvimäki and Hämäläinen (2016) exploit a discontinuity in program eligibility during the program roll-out when eligibility depended on the date of entry into the population register. Based on this design, the authors find that individualized plans are more successful in improving immigrants' earnings in the long run than traditional ALMPs. Another successful approach relies in specialized programs that focus on training newly arrived refugees in target occupations with labor shortages in the local labor market (Dahlberg et al., 2024; Foged et al., 2022b), which we will discuss in the next section.

The literature is more encouraging, however, on the high value of language courses for the economic integration of immigrants. Generally, selection issues make it difficult to evaluate the effect of language skills on labor market outcomes. But several recent well-identified studies relying on discontinuity designs or randomized assignments document credible and positive effects of intensive language training. In Denmark, Foged et al. (2022a) find that long-run earnings of immigrants increase after participation in intensive language training. These programs also have positive spillover effects on the educational success of the participants' children (Foged et al., 2023). In France, Lochmann et al. (2019) find positive employment effects of mandatory language courses for immigrants and Heller and Mumma (2023) find long-run earnings gains of language training in the US.

Youths High youth unemployment is a major policy concern in many countries which raises the demand for ALMPs supporting young workers. Effective programs should lead young workers towards successful career tracks especially during in the sensitive transition period between education and labor market entry and thereby avoid long run negative outcomes. However, the evidence regarding youth programs is not particularly encouraging. Based on the meta-analysis results, Card et al. (2018) conclude that programs for youths are less likely to yield positive impacts. The evidence in the recent literature is mixed regarding the success of programs with the aim of speeding up the school work transition.

Gelber et al. (2016) evaluate the New York City Summer Youth Employment Program which offers short-term public sector jobs to young people aged 14 - 21 during the summer months. Because the program received more applications than there were available slots, those were randomly assigned via a lottery. Combining application data with tax records, the authors show that the program increases earnings in the short run. But the extra labor market experience has no impact on the probability of college enrollment or on longer run earnings outcomes. However, participation in a summer job is successful in keeping young people out of trouble as it significantly reduces incarceration and mortality.

Le Barbanchon et al. (2023b) exploit a similar lottery design in Uruguay where students between age 16 and 20 apply to a work-study program and get randomly assigned to available seats. The program finances short-term, part-time jobs in public sector enterprises which come with the obligation to remain enrolled in school. To foster attachment to education, program rules also prevent firms from keeping participants in the same job after the program ends. Evaluation results show that the program increases employment and earnings of participants in the first years after the end of the subsidized job. It also succeeds in increasing attachment to education, as participants are more likely to remain in school and perform well in terms of their grades. Not surprisingly, the effect on education is mainly driven by participants from poor households.

Crépon et al. (2013) evaluate a job search assistance program with intensive counselling that starts during job search and continues once a job is found and which is available for recent university graduates who could not find jobs and had been unemployed for at least 6 months. The results reveal low program take-up rates among individuals randomly assigned to program offers and a small positive employment effect in the short run which, however, fades quickly. Taking into account spillovers to the control group who are not offered the program, the net program effect is negative with less jobs generated than in the absence of the program, see also Section 4.7.
ALMPs in developing countries are typically available for youth who suffer from extremely high rates of joblessness. We discuss available evidence in Section 4.5.

4.4 Lesson 3: Program design takes demand side into account

Traditional ALMP policies focus on workers on the supply side of the market and programs are designed to directly increase job finding rates or to facilitate the workers' access to stable and well-paid jobs. Recent policy approaches increasingly take the demand side into account either by involving local employers actively in the design of training programs or by implementing programs that directly address employers.

Training program design involving employers A comprehensive approach in the design of training programs involves the potential employers of training participants. Surveying local labor demand will allow targeting training efforts towards occupations that are in high demand, offer high starting wages and opportunities for career advancement. Alternatively, employer involvement can be fostered by creating subsidized on-the-job training positions in firms. Evidence on successful training programs with employer involvement comes from programs that are organized locally at the community level and in close cooperation with potential employers. In Latin America, the World Bank and the Inter American Development Bank pushed programs based on the "Chilean model" which is focused on integrating disadvantaged workers in the labor market by designing programs that integrate employer needs and activate employers who provide training jobs (Ibarrarán and Rosas Shady, 2009).

Exploiting the spatial roll-out over time, Foged et al. (2022b) evaluate *industry packages* an innovative integration program for refugees in Denmark. The program targets training efforts in low-wage occupations with high numbers of unfilled vacancies in the local labor market. After a short training course in one of the target occupations, trainees are matched to jobs with local employers. Compared to standard training programs for immigrants, industry packages result in higher employment rates of participants in the first years after immigration. Dahlberg et al. (2024) evaluate a program with a strong employer involvement that was implemented in a randomized control trial in the labor market of Gothenburg in Sweden. The evaluation finds large positive effects doubling employment rates of participants relative to the control group in the first year after the program.

Sectoral employment programs in the US, surveyed by Katz et al. (2022), are also designed in close collaboration with potential employers. The programs offer short courses with occupational and soft-skills training to low educated and disadvantaged workers. Evaluations across four US sites demonstrate large and persistent gains in employment and earnings over 2 to 6 years after program participation. Overall, the program effects of sectoral employment programs are far more promising than the modest effects that are found in evaluations of traditional US training programs.

While the idea of involving employers more strongly in the design of ALMPs is appealing, there are also a number of concerns and open questions. First, programs which are too closely aligned to specific employers raise concerns of spillover effects as employers receive subsidies for training efforts they would have financed themselves. Second, while these programs speed up employment transitions of participants in the short run they might reduce their job flexibility in the longer run, if they mostly train firm specific skills and make it costly for participants to signal their skills to alternative employers (Hanushek et al., 2017). In addition, employers might be able to extract rents from trainees and pay lower wages (Naidu and Sojourner, 2020).

Programs addressed at firms Turning the idea of job search assistance for job-seekers around, the French employment service introduced a free recruitment service for small and medium sized firms. The program offered assistance with vacancy posting, candidate selection, and referrals of job seekers to participating firms. It was implemented in a slack labor market in 2015 at the end of the Great Recession. Algan et al. (2020) evaluate the program effects from a large scale RCT where a set of randomly assigned firms got access to the program. Compared to non-treated control firms, the experimental firms posted more vacancies and increased hiring, which led to a positive gain in net employment at the firm level. The additional hires were mostly low skilled workers in permanent jobs. The interpretation of these findings is that in a slack labor market firms reduce demand for low-skilled positions because of high recruitment costs. The program offers an effective screening service to deal with large number of potential applicants for each vacancy and firms value the high quality information from applicant referrals.

Instead of addressing recruitment costs, hiring credits aim at reducing labor costs to increase labor demand. Cahuc et al. (2019) evaluate a temporary hiring credit that was non-anticipated and targeted towards small firms at the onset of the Great Recession in France in 2009. The program reduced employer social security contributions of new hires with wages close to the minimum wage over a relatively short time period. Exploiting quasi-experimental designs the authors show that the policy succeeded in raising labor demand. The hiring credit substantially increased employment growth and total hours worked in eligible firms. Results from a structural model that allows the comparison of alternative policy scenarios highlight that the success of hiring credits requires a careful policy design. The French scenario in 2009 fulfilled a series of criteria that are crucial for a successful hiring credit. Namely, it involved a temporary subsidy that was not anticipated by firms, was only available during a limited period of time of high unemployment, was targeted towards a small set of firms, and implemented in market with rigid wages due to binding minimum wage floors. Given these criteria, the policy paid for itself, as we will discuss in section 4.8.

Bertrand and Crépon (2021) investigate a firm side policy in South Africa with the aim of understanding why small firms are not growing in a labor market with high unemployment. Their intervention provides a random sample of small firms with free access to a website with information on labor laws governing hiring and firing regulations. They find that the informational intervention substantially increases employment growth in treated firms and provide evidence that limited knowledge of labor laws can significantly constrain hiring in small firms.

4.5 Lesson 4: Internationalization of ALMP use and evaluations

Developing countries are characterized by high rates of non-employment especially among youths and even among highly educated workers. Firms in these countries are small to medium sized and they are often reluctant to hire workers. Labor frictions on both sides of the market appear to hinder a more efficient allocation of firms and workers. On the supply side, skill shortages, credit constraints for investments in training, high costs of job search and of signalling skills to employers play an important role. On the demand side, inefficient labor regulation, high costs of training workers on the job, and high costs of screening job applicants and assessing worker skills restrict firm growth. These frictions can potentially be addressed by ALMPs.

Card et al. (2018) include studies from 47 countries world-wide in their meta analysis where the majority of studies and estimates are from Europe and North-America. Studies from low-income countries were mostly from Latin America. The vast majority of studies from low-income countries evaluated training programs which were offered to youth. The program effects were estimated over the short to medium run (i.e. the first 2 years after participation) and the share of evaluations based on experimental designs was lower than in typical high-income countries. In line with Card et al. (2018), a survey by McKenzie (2017) is not very enthusiastic about program impacts in evaluation studies of early ALMPs in developing countries. As the programs tended to be costly and had only moderate effects, McKenzie concludes that ALMPs in developing countries are unlikely to be very effective.

Over the last decade the number of studies from developing countries has exploded in line with the general ALMP literature. We see a significant increase in the quality of evaluations, mostly in well-designed experimental settings with a focus on longer run-outcomes and an increase in the variety of different programs that aim at addressing multiple labor market frictions that are specific to low-income countries. Here, we list some examples of promising approaches:

Alfonsi et al. (2020) implement a two-sided experiment in Uganda, a country with a young population and extremely low levels of youth employment. In the experiment workers are randomized into two treatment groups where they are either offered a seat in a vocational training course or the option to receive an apprenticeship training with a firm. On the other side of the market firms are randomly matched to job applicants with or without training or to applicants with a wage subsidy for apprenticeship training. Experimental participants' labor market outcomes are followed over a four year period. The evaluation results show that take-up is high among workers who are assigned to vocational training courses. Compared with the control group their employment and earnings outcomes improve substantially over the long run. Firms, on the other hand, are very reluctant to hire trained or untrained applicants to whom they are matched. The take-up rate is somewhat higher for applicants with a wage subsidy for apprenticeship training. Compared to the conntrol group workers receiving firm training do better especially in the short run. But in the longer run these employment and earnings advantages fade. Alfonsi et al. (2020) set up a job-ladder model to interpret the findings and evaluate general equilibrium effects. They conclude that in this market vocational training of young workers is more effective than policies offering incentives for firms to train workers. An important determinant of the labor market success of workers receiving vocational training is a skill certificate that is recognized by employers. This allows workers to effectively signal their skills and facilitates career moves of trained workers.[56]

Bassi and Nansamba (2022) study the effects of signalling non-cognitive skills on labor market outcomes of workers who come out of vocational training. They show that certified skills affect the job matches and labor market outcomes of applicants. At the end of their training program workers are assigned to job interviews with firms. In half of the interviews a certificate of worker non-cognitive skills is randomly revealed to both the applicant and the employer. In this population workers volunteer to participate in vocational training and they are thus positively selected in terms of non-cognitive skills. The authors first evaluate the effect of revealing the signal on worker and firm expectations and find that firm revise their expectations upward while worker expectations are unchanged, which is consistent with positive selection. In terms of employment outcomes, the signal does not increase employment probability relative to the control group. But the signal leads to positive assortative matching of workers with high skills and employers with higher skill demand. In line with this sorting, workers' wages are higher if the signal is revealed and they increase in their revealed skills.

[56] In a companion paper Rasul et al. (2023) study the effects of the experimental interventions on job search and expectations of young workers. This is one of the few studies explicitly examining how ALMP affects job search.

Carneiro et al. (2020) study the effect of a wage subsidy program with training component on labor market outcomes of unemployed workers in Macedonia. Macedonia is one of the poorest countries in Europe with extremely high unemployment and low youth employment rates. For the wage subsidy program, the Macedonian employment office collected job applications from workers and vacancy postings from employers. The officials matched applicants and vacancies and randomly invited half of the applicants assigned to each vacancy to a job interview for a subsidized position. The control group did not have access to one of these jobs. Results show that access to an interview increased the employment probability in the treatment group in the short run and the effects only declined moderately over the 3.5 year horizon of the follow up study. Average treatment effect estimates show that individuals who get access to training in a subsidized job due to the interview have a very high employment probability at the end of the observation period. Survey evidence further shows that both work-related as well as non-cognitive skills increased among treated applicants.

Abebe et al. (2021) study two randomized interventions that aim at helping young unemployed workers finding good jobs in Ethopia. The first intervention is a transport subsidy reducing search cost by offering bus tickets to the city center where a large job board is located. The second intervention is a job search assistance workshop which provides certificates from general skill tests and allows applicants to signal their skills. In the short run both interventions increase the probability of finding stable jobs in the formal sector. But the effect only persists for the job search workshop in the longer run. After 4 years, workshop participants have similar employment rates but substantially higher earnings than control group members. The authors explain this result by an increase in match quality between firms and workers. But the interventions do not create additional jobs.

Muralidharan et al. (2023) evaluate the impact of the National Rural Employment Guarantee Scheme (NREGS) in India, the world's largest scale public employment program. For identification they exploit the spatial roll-out of a high quality payment system which strongly increased access to the program and reduced corruption. To estimate the program effect they compare outcomes in households in regions that implemented the high quality payment system early with households in regions which implemented it 2 years later. They control for potential spatial spillovers from neighboring regions that were treated early. Estimation results show that improving the quality of NREGS implementation reduced poverty and increased income among the rural poor. Thereby, income gains stemmed mainly from increases in employment and private market wages. These results are in line with general equilibrium effects at the level of treated regions where the NREGS created an outside option for poor workers. In a monopsonistic labor market this result induces private sector employers to increase wages and thus leads to a more efficient allocation of labor. Further evidence in support of the interpretation based on imperfect labor markets are rising reservation wages and declining farm earnings and land prices, while production increases. Although NREGS covers only a small share of 4% of rural employment it thus has a large impact on market wages.

4.6 Lesson 5: Advances in labor market design on online search platforms

Over the last two decades when high speed internet access became widely available, online job boards have substantially transformed formal job search and replaced traditional search channels. By now most vacancies are posted on large job boards and most job seekers search online. For research, search platforms provide a wealth of novel data allowing researchers to closely track agents over time. Linking search platform data to employment registers further allows to observe search outcomes, i.e. which jobs and workers get finally selected. These opportunities have fundamentally transformed research of the search and matching process which used to be treated as a black box for a long time. In addition, online job platforms offer novel opportunities for the design of ALMPs that aim at improving the match between job seekers and vacancies. The idea is that algorithms can partly substitute caseworker tasks and recommend suitable vacancies to job seekers, which can either speed up job search or broaden search by uncovering job opportunities that the job seeker would have otherwise missed. A clear advantage is that algorithms, once implemented, have negligible marginal costs and offer fascinating prospects of overcoming search and matching frictions. However, new opportunities for market design also raise a series of questions which we will discuss below. For a comprehensive summary of the literature see Kircher (2022).

In a seminal paper Belot et al. (2019) follow a group of job seekers searching on the online platform of the Scottish employment office. The system allows job seekers to enter keywords indicating their desired occupations and job locations. After observing job search choices for a few weeks the researchers randomly selected a treatment group of job seekers who received automated recommendations to search in occupations that are related to their prior choices. The occupation recommendations are generated by a prediction algorithm based on the frequency of occupational transitions observed in the labor market and on similarities in skill requirements between occupations. The objective of the recommendation algorithm is to point out relevant job opportunities that had been overlooked. Compared to a control group of searchers for whom the search interface remained unchanged, jobs seekers receiving automated recommendations started to search more broadly and also applied to vacancies in a wider range of occupations, especially if they had searched narrowly prior to the intervention. Broader search among the formerly narrow job searchers also led to a significant increase in invitations to job interviews. Interestingly, job seekers transferred the information from online recommendations to their job search activities outside the platform. They received more interview invitations overall, not only from vacancies for which they had applied via the platform. The study was run at a small scale with only 300 participants, which limits the power of finding out about the ultimate job search success and whether broader search also leads to faster transitions into employment and higher quality jobs.

In a follow-up study with a slightly larger sample of long-term unemployed job seekers in England, Belot et al. (2022a) implement the automated personalized occupational advice program in a setting where they can observe employment outcomes in administrative data. The English program has similar effects on the breadth of search and on job applications as the Scottish one. But the experiment also reveals positive effects program effects on the probability that long-term unemployed workers find stable jobs and reach a certain earnings limit. These jobs are generated from searching in a broader set of occupations rather than from increased search effort or search in a larger geographical area.

Both studies treat a relatively small part of the labor market with automated personalized occupational advice, which reduces concerns of displacement effects as it unlikely that treated job seekers get jobs that would have otherwise gone to workers in the comparison group. However, it remains unknown what would happen once the program is scaled up. Evaluating spillover effects of online job recommendations on other job seekers is the objective of the most recent contributions in the literature, which we will discuss in the next section.

Le Barbanchon et al. (2023a) develop a job recommender system based on a machine learning tool that can be rolled out on the full search platform of the Swedish public employment service. This system generates a personalized short list of most relevant vacancies for each job seeker based on past vacancy views of job seekers with similar search preferences. In contrast to automated occupation recommendations which focus on information that might be overlooked the job recommender diffuses information among similar job seekers. The authors show that that recommender generated vacancies increase the geographical and occupational breadth of job opportunities and have a strong focus on vacancies that are less popular by other job seekers. To assess displacement and congestion effects, Le Barbanchon et al. (2023a) use a clustered 2-sided randomization design, where job seekers as well as vacancies are randomly assigned to treatment and control groups. Treated job seekers receive vacancy recommendations and treated vacancies are shown in the recommendations. In addition, spatial variation is generated by randomly assigning a subgroup of local labor markets to a non-treated super control region. This design allows to study very flexibly the effects of the job recommender on job search and job finding outcomes of various subgroups of job seekers as well es hiring outcomes of vacancies. In terms of job search, findings from the country-wide experiment in 2021-2022 confirm the previous literature. Treated job seekers are more likely to follow the recommendations and to apply to recommended vacancies than to non-recommended ones. In addition, employment rates of treated job seekers increase slightly.

An analysis at vacancy-unemployed pair-level, reveals important reallocation effects of the recommender system. In particular, there is little evidence that job seekers in the control group are crowded out of employment in vacancies that were recommended to treated seekers. Neither is the recommender system driving up competition for recommended vacancies among treated job seekers. The finding of small congestion effects in Sweden is in contrast to results by Altmann et al. (2022), which will be discussed in more detail in the next section.

In the spirit of traditional job search assistance programs, the main aim of automatised recommendations for job seekers is overcoming labor market frictions by improving the rate at which job seekers find jobs and at which vacancies get filled. Ideally, the recommendations should also lead to a more efficient allocation of workers to jobs and improve match quality. Advanced machine learning technologies offer ample opportunities for the development of job recommendation systems and further research will be necessary to investigate the variability of recommendations generated by different systems, their alignment with job seekers' goals, and their impacts on labor market outcomes (Bied et al., 2023; Behaghel et al., 2024).

4.7 Lesson 6: Growing awareness of spillover or displacement effects

For a long time the comparison of mean outcomes in the treatment group and the control group in a controlled environment with randomized assignment was regarded as the gold standard of ALMP program evaluation. However, results can be misleading if there are spillovers from the treatment to the control group. Early concerns of spillover effects were raised in the context of public sector employment programs which risk subsidizing jobs that would also be created in absence of the program. See Johnson and Tomola (1977), who document that the Public Sector Employment service in the US replaced other jobs that would have filled by local governments.

Regarding training programs, most economists think that spillover effects are relatively limited. In an economy with restricted supply of human capital, trained individuals will compete with workers higher up in the job ladder who have more outside options, which in turn limits the risk of displacements (Katz et al., 2022). However, search assistance programs or automated job referral programs might create large spillovers by privileging the access to jobs for one group at the cost of the others. This is easily seen in the case of referrals to a specific job vacancy. If the treated job seeker gets the job, the vacancy is no longer available to the job seeker in the control group and the recommendation program just re-orders the job queue.

The first study that addressed this problem seriously was by Crépon et al. (2013). The authors evaluated a job search assistance program for young unemployed university graduates in France which was implemented across multiple local labor markets. The evaluation design is based on a double randomization strategy. In a first step treatment intensities determining the share of treated job seekers, were randomly assigned across local labor markets. In the second steps eligible job seekers were randomly assigned to the JSA program according to the local treatment intensities. This design allows comparisons of treated and control individual within each region, but also across regions. If individuals in the control group in regions with high treatment intensity have systematically different outcomes than control individuals in regions where the program is not implemented this is indicative of spillover effects.

In their evaluation, Crépon et al. (2013) find evidence of substantial spillover effects which lead to displacement of workers in the control group. While a comparison of treated and control group outcomes within regions points to a positive employment effect of the JSA program on participants, the comparison of non-treated individuals across regions indicates even stronger displacement effects. The authors conclude that overall more jobs were lost than found. This estimate is compatible with a labor market where vacancies do not adjust to the increase in job search activity induced by the JSA program, at least in the short run. The evidence from France indicates that displacement effects are especially strong in weak labor markets with few vacancies, which further supports this model.

Cheung et al. (2023) replicate the French experiment and confirm the importance of spillover effects. They investigate a JSA program in Sweden which intensifies the frequency of caseworker meetings and job search workshops to which newly unemployed workers are assigned. The experiment is rolled out across 72 employment offices. In a random group of active offices half of the eligible job seekers are assigned to programs and in the comparison group of inactive offices no job seeker is assigned. The experimental results show that in the Swedish case, the net effect of program participation on the probability of leaving unemployment in the first three months of unemployment is positive. But this net effect, i.e. the employment gain of treated job seekers compared to those in inactive regions, is about the same magnitude as the displacement effect on non-treated job seekers. An important mechanism driving the displacement effect are vacancy referrals that are shown earlier to program participants who have more frequent meetings with caseworkers. This indicates that the program increased competition for a fixed number of vacancies and created congestion.

Altmann et al. (2022) implement a large randomized control trial among unemployment benefit recipients in Denmark. Danish job seekers are mandated to set up a search profile specifying their target occupations on the centralized job search platform. UI benefit recipients have to visit this website regularly and keep a record of the jobs they apply for. Based on this information the authors implement an information intervention with three different treatments in a double randomization design. In the first stage municipalities were randomly assigned a treatment intensity. In control regions no job seeker received information treatment and in the remaining regions 60% and 90% of job seekers were treated, respectively. In the second stage job-seekers in each region were randomly assigned to a *vacancy treatment* informing them about quantities of vacancies in each of the occupations on their profiles, a *occupational recommendation treatment* referring them to suitable alternative occupations similar to Belot et al. (2019), and a *joint treatment* consisting of both components. This strategy creates a continuous measure of treatment intensity at the level of local labor markets, which takes into account that individuals do not only search and work in their own municipality but may commute to neighboring places.

Regarding search strategies the results by Altmann et al. (2022) confirm Belot et al. (2019) and show that job seekers receiving occupational recommendations broadened their search and job applications. The information on open positions per occupation, in contrast, led to a narrower focus on core occupations, and combining both sources of information canceled out changes in job search such that the joint treatment had no effect on job search. In terms of employment and earnings effects there is large heterogeneity across local labor markets with different treatment intensities. In markets with low treatment intensity both the vacancy and the occupation recommendation treatment have positive effects on hours worked and earnings within 12 months of treatment, while the joint treatment has smaller effects. In municipalities with high treatment intensity, however, the effects on hours and earnings are small and insignificant. This indicates substantial negative spillover effects of the change in search strategies by treated individuals. Comparing outcomes among untreated and treated individuals across regions shows little evidence that job seekers in the control group are affected by spillovers. But workers receiving the information treatment increasingly compete for the same types of vacancies as treatment intensity goes up. This means that treatment creates congestion effects as all search effort is concentrated on a smaller set of vacancies. These results highlight the importance of carefully designing automated recommendations at a large scale.

4.8 Lesson 7: Discussion of cost effectiveness

To evaluate the overall effectiveness of ALMPs it is essential to compare the gains and the cost of the program. The early ALMP evaluation literature was mostly silent about this comparison and with few exceptions, studies did not report program costs. In their meta-analyses Card et al. (2010) and Card et al. (2018) use information on program duration to proxy for program costs in meta-regressions.
The recent literature is more concerned about the overall welfare implications of ALMPs and by now many studies contain detailed cost benefit analyses. Table 5 summarizes the information on program costs and gains from the studies included in our review. When we compare reported program costs across studies it becomes apparent that there is a large variation as is shown in column (2). Program costs range from a few dollars or euros for short-term job search assistance programs to multiple thousands for long-term training and wage subsidy programs. The variation makes it obvious why a careful analysis of the monetary program gains is important to justify high cost programs.
Studies use a variety of different concepts to evaluate program gains which may be explained by the policy context or by data availability, see column (5). Many analyses consider a cumulative measure of gains over several years ranging from 1 to over 20 years, see column (4). The main differences in the benefit concepts is that some studies consider returns to an individual investment comparing the upfront program participation cost to a measure of total individual earnings gains. Other studies consider returns to a public investment and compare participation costs to measures of social returns in terms of tax revenues and foregone benefit payments. Interestingly, with the only exception of Crépon et al. (2013) who report a net loss in jobs in regions where the program is implemented, all studies with detailed cost benefit data in Table 5 report that programs either break even or are financially advantageous for participants or for society as a whole. The examples in our comparison demonstrate that not only low cost programs can be run cost effectively, but also significant investments in human capital or labor market attachment can pay off for individuals or society in the longer run, which is an encouraging message for ALMP. Whether the studies shown in Table 5 are positively selected or whether authors choose to only report favourable cost benefit measures is a question which

will have to be resolved in a meta-analysis of the full literature.

A unified analysis that allows a comparison across multiple policies is the marginal value of public funds (MVPF) suggested by Hendren and Sprung-Keyser (2020). Computed in the context of ALMPs, the MVPF compares a measure of the willingness to pay – e.g. the cumulative discounted present value of the net of tax earning gain of participants – to a cost measure given by the upfront program cost plus the fiscal externality for the government. For an example, see the calculation of the MVPFs of Job Corps in Hendren and Sprung-Keyser (2020).

Table 5: Cost and Benefit Analyses in Recent ALMP Studies

Study	Currency	Cost per Participant	Cumulative Gain	Years of Sustained Gain	Benefit Concept
	(1)	(2)	(3)	(4)	(5)
Panel A: Training					
Alfonsi et al. (2020)	USD	510	1,246	15	NPV of change in steady state individual earnings
Heller and Mumma (2023)	USD	4,492	4,374	27	increased tax revenue
Sarvimäki and Hämäläinen (2016)	Euro	15,000	28,000	10	cumulative gross income plus foregone benefit payments
Schlosser and Shanan (2022)	NIS	1,400	3,886	1	income gain and benefit reduction
Katz et al. (2022)	USD	4,459 23,135	28,662 38,484	5 5	cumulative earnings gains societal net benefit from participant earnings gains
Hyman (2018)	USD	40,362	43,398	10	NPV of change in individual earnings
Panel B: JSA					
Michaelides and Mueser (2020)	USD	201	775	1	saving in unemployment benefits
McConnell et al. (2021)	USD	692	6,630 2,187	2.5	net income gain of participants increase in tax revenue
Maibom et al. (2017)	Euro	903	2003	4	discounted net government gain
Bobonis et al. (2022)	CAND	4,804	5,900	20	real annual earnings gain
Abebe et al. (2021)	USD	18.2 9	299 31	1 1	earnings gain in year 4 earnings gain in year 4
Böheim et al. (2022)	Euro	390	1,075	2	saving in benefits plus gain in taxes and SS contributions
Crépon et al. (2013)	Euro	1,600	-12		jobs created in active regions
Panel C: Public Sector Employment Program					
Kasy and Lehner (2023)	Euro	90,000			cost of 3 year subsidized job
Panel D: Firm Side Intervention					
Algan et al. (2020)	Euro	145	1,277	1	income in minimum wage job
Bertrand and Crépon (2021)	USD	200	20		cost per job created
Cahuc et al. (2019)	Euro	700	700	1	saving in unemployment benefits
Cheung et al. (2023)	Euro	99	0.25		increase in job finding rate across all job seekers in active offices

Notes: This table presents cost benefit calculations from several studies evaluating ALMP policies. Benefit Concept refers to the definition of program gains applied in the study.

4.9 Lesson 8: Wide range of outcome variables

As can be seen from Tables 6 and 7, significant progress has been made in the ALMP evaluation literature regarding the set of outcome variables that are included in recent studies. A wider set of different outcomes give a more comprehensive picture of the program impacts. Card et al. (2018) restricted the meta-analysis to estimates of program impacts on the probability of employment, because this was the most commonly reported outcome measure. While several studies evaluated effects on exit rates from unemployment only, few included earnings as an outcome measure. This has changed dramatically in studies written over the last 10 years. With very few exceptions, all recent studies report program impacts on employment and earnings. In addition, program effects on measures of job match quality such as occupation and firm type are available in some studies. As we can also see from the tables, a substantial share of studies report program effects on outcomes observed over the longer-run of three years or more after program participation. This is not only the case for studies evaluating effects on training programs, which naturally target long-run outcomes. But also studies evaluating JSA programs are increasingly concerned with longer-run outcomes.

Measures of earnings and other outcomes observed in administrative register data may be imperfect summaries of the value of specific jobs. It is therefore desirable to supplement them with more comprehensive measures from survey data. Several studies provide program effects on measures of cognitive and non-cognitive skills (Schlosser and Shanan, 2022; Katz et al., 2022), information on health, mortality, well-being, life and job satisfaction and measures of social well-being (Kasy and Lehner, 2023). Another novelty is the elicitation of measures of job search strategies and search effort. Search outcomes are implicitly observed for programs generating automated job search advice on large search platforms. Other studies elicit information from surveys (**?**).

4.10 Lesson 9: What are the mechanisms explaining program effects

Why do programs work? Besides reporting program effects, studies are increasingly interested in this question which certainly is of high relevance for program design. Delving into mechanisms allows us to understand why certain groups can benefit from a program while others cannot or why a program works in one labor market and not in another.

A first approach towards understanding mechanisms driving program effects is an analysis of program effect heterogeneity. Especially heterogeneity over the business cycle or by labor market tightness across local labor markets reveals how the program design interacts with labor market conditions. For example, vacancy referrals and recruitment assistance might be more valuable for job seekers and firms in weak labor markets (Cheung et al., 2023; Crépon et al., 2013; Algan et al., 2020; Belot et al., 2019; Altmann et al., 2022) and training might be more effective if it is targeted towards local demand (Katz et al., 2022; Foged et al., 2022b).

Specific program features can also be relevant for the program success. Several studies highlight that certificates issued by ALMP programs allow workers to signal their skills to employers, which can be crucial for job finding success (Katz et al., 2022; Bassi and Nansamba, 2022; Alfonsi et al., 2020). Another feature that has received attention in the literature is the timing of counseling services, whether they should be available early in the unemployment spell or later (Maibom et al., 2017), whether it is beneficial to continue career counseling services once a job is found (Katz et al., 2022; Bobonis et al., 2022) or whether counseling should already start on the job prior to the transition into unemployment (Homrighausen and Oberfichtner, 2024).

Another determinant of program success may be non-cognitive skills. They are hard to observe and even harder to train. But available evidence indicates that especially disadvantaged workers with long absences from the labor market benefit from programs that promote their non-cognitive skills (Schlosser and Shanan, 2022; Bobonis et al., 2022).

Several recent ALMP evaluation studies successfully introduce structural models that can be used to understand mechanisms driving evaluation outcomes (Crépon et al., 2013; Cheung et al., 2023), to simulate alternative policy scenarios (Cahuc et al., 2019) or to evaluate potential general equilibrium effects (Alfonsi et al., 2020; Muralidharan et al., 2023).

4.11 Lesson 10: Novel identification strategies

The overview of identification strategies in Tables 6 and 7 shows that RCT has become the standard evaluation methodology of ALMPs, at least among studies written over the last 10 years and reviewed in this chapter. Moreover, important methodological innovations were made in the design of the RCTs. In this chapter, we have discussed experimental designs that randomize at the worker-firm match level (Abebe et al., 2021; Carneiro et al., 2020) and designs that randomize both sides of the market (Alfonsi et al., 2020; Le Barbanchon et al., 2023a) in Section 4.5, as well as designs based on double clustered randomization that allows uncovering spillover effects (Crépon et al., 2013; Cheung et al., 2023; Le Barbanchon et al., 2023a; Altmann et al., 2022) in Section 4.7.

Progress was also made with adaptive targeted treatment assignments in field experiments. Caria et al. (2024) evaluate three different ALMP programs with the aim of integrating Syrian refugees in the Jordanian labor market. They stratify the intake population in 16 strata in order to find out which program works best for which group of participants. To maximizing the precision of treatment effect estimation as well as the welfare of experimental participants an adaptive targeted treatment assignment algorithm is chosen. Outcomes are measured over time such that employment and earnings trajectories are observed for each participant and newly eligible individuals can be included in the randomization over time. Treatment shares of newly entering participants are determined by observed outcomes of earlier cohorts based a Tempered Thompson Algorithm with hierarchical Bayesian updating. Results confirm that while the programs only slightly increase overall employment, the adaptive assignment strategy can improve employment outcomes among certain groups of participants.

Innovations in non-experimental identification involve the transfer of the judge leniency design to variation in caseworker propensity of assigning unemployed workers to ALMP programs. Humlum et al. (2023) show that a large group of benefit recipients in Sweden are as good as randomly assigned to caseworkers based on their day of birth within local employment offices. After isolating quasi-randomly assigned groups the study exploits variation in the propensity of program assignments among caseworkers as an instrumental variable to identify the effect of program participation on employment and earnings outcomes. This identification strategy finds positive employment and earnings effects of training participation in the medium run, which are significantly larger than the effects of wage subsidy programs. Interestingly, OLS estimates of the program effects show the opposite sign, which indicates large negative selection bias as individuals with negative unobserved characteristics are assigned to training programs. Positive program effects are due to individuals who complete training and switch occupations especially into sectors with low levels of offshoring.

An earlier study using a similar IV design to evaluate the earnings effect of Trade Adjustment Assistance programs in the US is Hyman (2018). In this program training courses are available for workers who were displaced by firms suffering from increased import competition or outsourcing. Displaced workers had to apply for program access and the applications were reviewed by case investigators who assess the employer's exposure to import competition. Applications were assigned to case investigators based on caseloads which generates quasi-random variation. The IV strategy thus exploits variation in approval shares across caseworkers. The estimates indicate large cumulative gains in long-term earnings from retraining displaced workers.[57]

[57]Part of the earnings gains might be due to UI benefit extensions avialable to program participants. The decomposition of the overall effects into contributions from benefits versus training is the subject of ongoing work.

Table 6: Characteristics ALMP Studies: Training, Employment Subsidies, Firm Programs

Study	Standard ALMP	Country	Outcome variables	Design	Intake Period	Intake Group	Long Run	Cost Benefit	Spillover Effects
Panel A: Training									
Alfonsi et al. (2020)	yes	UG	employment, earnings, skills	RCT	2013	youth training applicants	yes	yes	yes
Rasul et al. (2023)	yes	UG	job search, expectations, call backs	RCT	2013	youth training applicants	yes		
Dahlberg et al. (2024)	yes	SE	employment, integration, program participation	RCT	2016-20	newly arrived refugees			
Foged et al. (2022b)	yes	DK	employment, earnings	DD	2008 -19	newly arrived refugees			
Foged et al. (2023)	yes	DK	education in second generation	RD		newly arrived refugees			
Foged et al. (2022a)	yes	DK	employment, earnings, occupation, education, criminal outcomes	RD	1996 - 2003	refugees	yes		
Heller and Mumma (2023)	yes	US	earnings, voter registration	RCT	2008-16	immigrants	yes	yes	
Katz et al. (2022)	yes	US	earnings, employment in high wage occupations or industries	RCT	2011-13	youth, low income	yes	yes	
Lochmann et al. (2019)	yes	FR	labor force participation, employment, household income	RD	2010	immigrants			
Sarvimäki and Hämäläinen (2016)	yes	FI	earnings, benefit receipt, employment, occupational quality, training participation	RD	1990-99	immigrants	yes	yes	
Schlosser and Shanan (2022)	yes	IL	employment, benefit recipiency, non-cognitive skills	RCT	2014	unemployed		yes	
Panel B: Employment Subsidies									
Carneiro et al. (2020)	yes	MK	employment, cognitive and non-cognitive skills	RCT		program applicants	yes		
Gelber et al. (2016)	yes	US	employment, earnings, college enrollment, incarceration, mortality	RCT	2005-2008	applicants age 14-21	yes	yes	
Kasy and Lehner (2023)	yes	AT	employment, worker well-being, health	RCT	2020	long term unemployed		yes	yes
Le Barbanchon et al. (2023b)	yes	UY	employment, earnings, school enrollment, grades, time use, soft skills	RCT	2012-14	students aged 16-20			
Muralidharan et al. (2023)	yes	IN	earnings, poverty, wages, employer market power	DD	2010-12	poor households		yes	yes
Panel C: Firm Programs									
Algan et al. (2020)	no	FR	vacancy posting, hiring, match quality	RCT	2015	small firms	yes	yes	
Bertrand and Crépon (2021)	no	ZA	firm level employment, hiring	RCT	2013	firms		yes	
Cahuc et al. (2019)	no	FR	hiring rate, separation rate, employment growth, hours growth, av. wage	DD, IV	2009	small firms		yes	

Notes: This Table lists the studies evaluating ALMPs included in this review. Standard ALMP refers to the definition by Card et al. (2018), Long run refers to program effects reported for three or more years after program participation, Cost Benefit refers to whether the study includes a cost benefit analysis or information about program costs. Spillovers refers to whether the study addresses spillover or displacement effects.

Table 7: Characteristics ALMP studies: Job Search Assistance, Automated Advice

Study	Standard ALMP	Country	Outcome variables	Design	Intake Period	Intake Group	Long Run	Cost Benefit	Spillover Effects
Panel D: Job Search Assistance									
Abebe et al. (2021)	yes	KE	job finding, employment, earnings, job satisfaction, search effort	RCT	2014	youth	yes	yes	yes
Bassi and Nansamba (2022)	no	UG	employment, matching assortativeness, skills, expectations	RCT	2014	trainees			
Bobonis et al. (2022)	yes	CA	employment, earnings, job quality, non- cognitive skills	RCT	1994-95	single mothers	yes	yes	
Böheim et al. (2022)	no	AT	number of meetings, job offers, other ALMPs, sanctions, job finding, labor market exit, unemployment exit	RCT	2015	registered unemployed		yes	yes
Cederlöf et al. (2021)	no	SE	job finding	RCT	2003-10	registered unemployed			
Cheung et al. (2023)	yes	SE	unemployment	RCT	2015	newly unemployed		yes	yes
Crépon et al. (2013)	yes	FR	job finding	RCT	2007	youth long term unemployed		yes	yes
Homrighausen and Oberfichtner (2024)	no	DE	employment, earnings	RCT	2018	registered unemployed			
Maibom et al. (2017)	yes	DK	employment	RCT	2008	newly unemployed	yes	yes	
Manoli et al. (2018)	yes	US	benefit receipt, employment, earnings, home-ownership, DI receipt	RCT	2009	benefit claimants	yes		
McConnell et al. (2021)	yes	US	employment, earnings	RCT	2011 -13	job losers, low income adults		yes	
Michaelides and Mueser (2020)	yes	US	benefit receipt, employment, earnings	RCT	2009	benefit claimants			
Michaelides and Mueser (2023)	yes	US	benefit receipt, employment, earnings, UI exit hazard	RCT	2009, 2015	benefit claimants		yes	yes
Schiprowski (2020)	no	CH	job finding	DD	2010-12	registered unemployed			
Panel E: Automated Advice									
Altmann et al. (2022)	no	DK	job search, employment, earnings	RCT	2019	benefit claimants			yes
Belot et al. (2022a)	no	UK	job search, applications, employment, earnings	RCT	2019	long term unemployed			
Belot et al. (2022b)	no	UK	job search,applications, interview invitations	RCT	2013-14	unemployed			
Le Barbanchon et al. (2023a)	no	SWE	job search, applications, employment	RCT	2021-22	registered unemployed			yes

Notes: See notes for Table 6.

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Online Appendix
Handbook of Labor Economics: Job Search, UI, and ALMP

Thomas Le Barbanchon, Johannes Schmieder, Andrea Weber

This online Appendix presents extra material of the chapter of the Handbook of Labor Economics on Job Search, Unemployment Insurance, and Active Labor Market Policies.

A Search model: Effects of UI extensions and calibration details

Proof that UI Extensions Increase Reemployment Wages if UI is only source of non-stationarity

Denote the expected log reemployment wage of individuals with PBD=P who leave unemployment at the end of period t as:

$$w_t^e(P) = E[\ln w | w \geq \phi_{t+1}, P] = \frac{\int_{\phi_{t+1}}^{\infty} \ln w \, d\, F_t(w)}{1 - F_t(\phi_{t+1})}$$

The probability of finding a job in t conditional on being unemployed is:

$$g(t) = \left(\prod_{j=0}^{t-1} 1 - h_j \right) h_t$$

The expected reemployment wage conditional on entering unemployment is:

$$\begin{aligned} E\left[w_t^e(P)\right] &= \sum_0^{\infty} w_t^e(P)\, g(t) \\ &= \sum_{t=0}^{\infty} w_t^e(P) \left(\prod_{j=0}^{t-1} 1 - h_j^P \right) h_t^P \end{aligned}$$

Suppose that the only source of non-stationarity is the finite duration of P. In that case for all $t > 0$:

$$w_t^e(P+1) = w_{t-1}^e(P)$$

and

$$h_t^{P+1} = h_{t-1}^P$$

Also note that the reemployment wage is falling since reservation wages are falling throughout the spell:

$$w_t^e(P) > w_{t+1}^e(P)$$

Proof. The expected reemployment wage conditional on entering unemployment with PBD = P+1 is:

$$
\begin{aligned}
E\left[w_t^e(P+1)\right] &= \sum_{t=0}^{\infty} w_t^e(P+1) \left(\prod_{j=0}^{t-1} 1-h_j^{P+1}\right) h_t^{P+1} \\
&= h_0^{P+1}\, w_0^e(P+1) + \sum_{t=1}^{\infty} w_t^e(P+1) \left(\prod_{j=0}^{t-1} 1-h_j^{P+1}\right) h_t^{P+1} \\
&= h_0^{P+1}\, w_0^e(P+1) + (1-h_0^{P+1}) \sum_{t=1}^{\infty} w_t^e(P+1) \left(\prod_{j=1}^{t-1} 1-h_j^{P+1}\right) h_t^{P+1} \\
&= h_0^{P+1}\, w_0^e(P+1) + (1-h_0^{P+1}) \sum_{t=1}^{\infty} w_{t-1}^e(P) \left(\prod_{j=1}^{t-1} 1-h_{j-1}^{P}\right) h_{t-1}^{P} \\
&= h_0^{P+1}\, w_0^e(P+1) + (1-h_0^{P+1}) \sum_{t=1}^{\infty} w_{t-1}^e(P) \left(\prod_{j=1}^{t-2} 1-h_j^{P}\right) h_{t-1}^{P} \\
&= h_0^{P+1}\, w_0^e(P+1) + (1-h_0^{P+1}) \sum_{t=0}^{\infty} w_t^e(P) \left(\prod_{j=0}^{t-1} 1-h_j^{P}\right) h_t^{P} \\
&= h_0^{P+1}\, w_0^e(P+1) + (1-h_0^{P+1}) E\left[w_t^e(P)\right]
\end{aligned}
$$

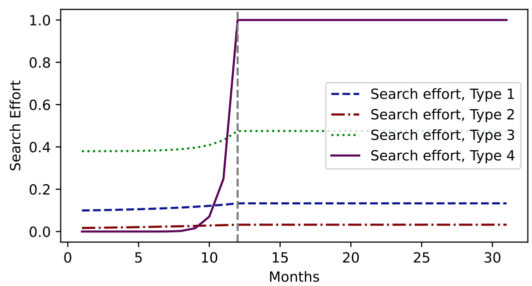
Since the reservation wage is falling throughout the spell, $w_0^e(P+1) > E\left[w_t^e(P)\right]$. This in turn implies:

$$
E\left[w_t^e(P+1)\right] > E\left[w_t^e(P)\right] \qquad \square
$$

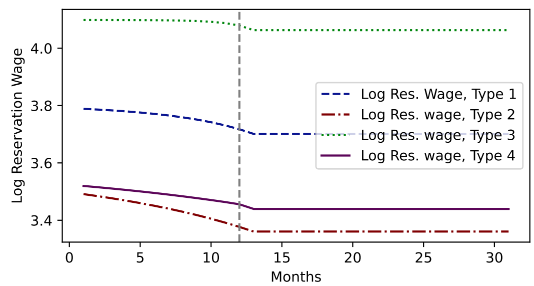
Model calibration for individual types

The following figure shows the simulation of the calibrated model in section 2.3.4 for the 4 individual types.

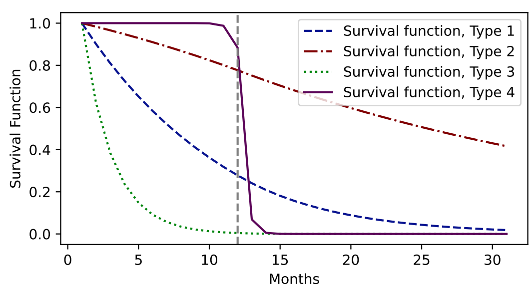
Figure A1: Search Model Calibration with 4 Types



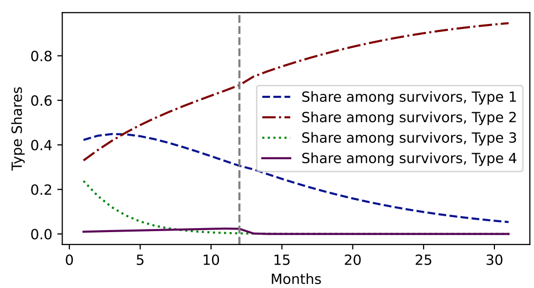
(a) Search Effort



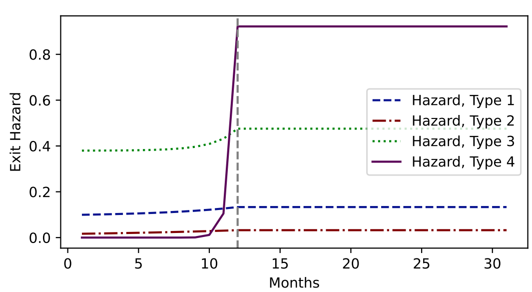
(b) Reservation Wage



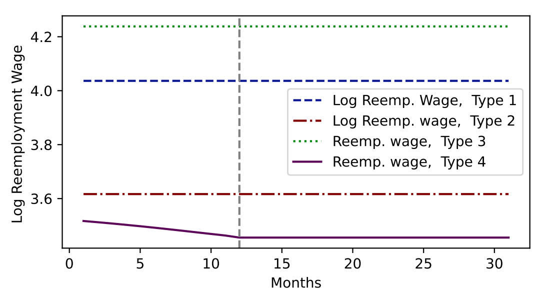
(c) Survival Function



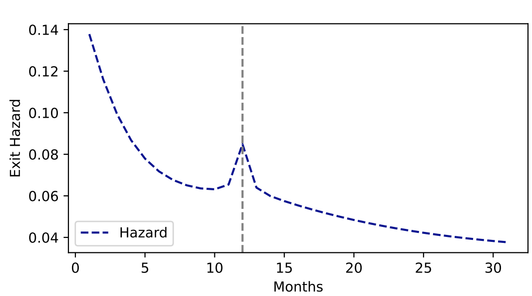
(d) Share among Survivors



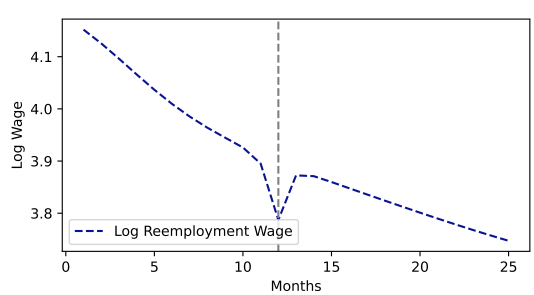
(e) Hazard



(f) Log Reemployment Wage



(g) Agg. Hazard



(h) Agg Log Reemployment Wage

Notes: The figure shows the model calibration underlying Figure 9 in the main text by individual types for $P = 12$.

B Derivation of the Baily-Chetty Formula

Baily-Chetty Formula for PBD Extension

As previously, any endogenous change in search effort has no effect on marginal welfare (envelope theorem). We obtain the following first order condition:

$$\frac{d\widetilde{W}}{dP} = S_P u(b) - S_p u(0) - (T-D)v'(w-\tau)\frac{d\tau}{dP} \tag{61}$$

From the budget constraint, we have $\frac{d\tau}{dP} = \frac{1}{T-D}\left(b\frac{dB}{dP} + \tau\frac{dD}{dP}\right)$. After rearranging, the first order condition becomes:

$$\frac{d\widetilde{W}}{dP} = S_P(u(b) - u(0)) - v'(w-\tau)\left(b\frac{dB}{dP} + \tau\frac{dD}{dP}\right) \tag{62}$$

We further note that $\frac{dB}{dP} = \frac{d}{dP}\left[\int_0^P S_t dt\right] = S_P + \int_0^P \frac{dS_t}{dP}dt$. Rearranging terms, we obtain:

$$\frac{d\widetilde{W}}{dP} = S_P b v'(w-\tau)\left(\frac{(u(b)-u(0))/b}{v'(w-\tau)} - \frac{1}{S_P b}\left(bS_P + b\int_0^P \frac{dS_t}{dP}dt + \tau\frac{dD}{dP}\right)\right) \tag{63}$$

Baily-Chetty Formula with Wage Effects

We follow the directed search approach in Nekoei and Weber (2017). The model is static with only one period and workers are initially unemployed. On top of choosing search intensity s at cost $\psi(s)$, job seekers target jobs with wage w. Their unemployment duration is defined as $1-\lambda(s,w)$, where $\lambda(s,w)$ can be understood as their job finding rates. When unemployed, workers derive utility $u(b)$ from receiving benefits b. When employed, their utility is $v(w(1-t))$ with wages w and proportional taxes t.Job finding increases with search intensity, but decreases with wages. High-wage jobs are harder to get. Workers maximize the following welfare objective:

$$\max_{s,w} W(s,w) = (1-\lambda(s,w))\,u(b) + \lambda(s,w)v\,(w(1-t)) - \psi(s) \tag{64}$$

The first order conditions write:

$$\frac{\partial \lambda}{\partial s} \times (v - u) = \psi'(s) \tag{65}$$

$$\frac{\partial \lambda}{\partial w} \times (v - u) = -\lambda(1-t) \times v'\left(w(1-t)\right) \tag{66}$$

The Social Planner maximizes workers welfare under the budget constraint:

$$\max_{b,t} \widetilde{W}(b,t) = \left(1 - \lambda(s,w)\right) u(b) + \lambda(s,w) v\left(w(1-t)\right) - \psi(s) \tag{67}$$

such that

$$\begin{cases} \frac{\partial \lambda}{\partial s} \times (v-u) & = \psi'(s) \\ \frac{\partial \lambda}{\partial w} \times (v-u) & = -\lambda(1-t) \times v'\left(w(1-t)\right) \\ (1-\lambda)b & = \lambda(t \times w) \end{cases}$$

Let us differentiate the workers welfare and after simplifying thanks to the envelope theorem (first order conditions above), we have:

$$d\tilde{W} = (1-\lambda)u' \times db - \lambda w v' \times dt \tag{68}$$

We then differentiate the budget constraint: $t = \frac{b}{w}\frac{1-\lambda}{\lambda}$. It yields:

$$dt = \frac{db}{w}\frac{1-\lambda}{\lambda} - \frac{b}{w}\frac{1-\lambda}{\lambda}\frac{dw}{w} - \frac{b}{w}\frac{d\lambda}{\lambda^2} \tag{69}$$

$$dt = t\frac{db}{b} - t\frac{dw}{w} - t\frac{d\lambda}{\lambda(1-\lambda)} \tag{70}$$

Replacing the expression of dt in the marginal welfare equation, we have:

$$d\tilde{W} = (1-\lambda)u' \times db - \lambda w v' \times \left(\frac{db}{w}\frac{1-\lambda}{\lambda} - t\frac{dw}{w} - t\frac{d\lambda}{\lambda(1-\lambda)}\right) \tag{71}$$

$$d\tilde{W} = (1-\lambda)(u' - v') \times db + \lambda w v' \times \left(t\frac{dw}{w} + t\frac{d\lambda}{\lambda(1-\lambda)}\right) \tag{72}$$

$$d\tilde{W} = (1-\lambda)(u' - v') \times db + \lambda v' t dw + w v' t \frac{d\lambda}{1-\lambda} \tag{73}$$

We renormalize the marginal welfare by the marginal utility when employed and express it in response to a one-dollar transfer.

$$\frac{d\widetilde{W}}{db}\frac{1}{(1-s)v'(c_e)} = \frac{u'(b)-v'(w(1-t))}{v'(w(1-t))}+\frac{\lambda}{1-\lambda}t\frac{dw}{db}-\frac{wt}{1-\lambda}\frac{d(1-\lambda)}{(1-\lambda)db} \tag{74}$$

$$\frac{d\widetilde{W}}{db}\frac{1}{(1-s)v'(c_e)} = \frac{u'(b)-v'(w(1-t))}{v'(w(1-t))}+\frac{\lambda wt}{(1-\lambda)b}\frac{bdw}{wdb}-\frac{wt}{(1-\lambda)b}\frac{bd(1-\lambda)}{(1-\lambda)db} \tag{75}$$

$$\frac{d\widetilde{W}}{db}\frac{1}{(1-s)v'(c_e)} = \frac{u'(b)-v'(w(1-t))}{v'(w(1-t))}+\eta_{w,b}-\frac{1}{\lambda}\eta_{1-\lambda,b} \tag{76}$$

where $\eta_{w,b}$ is the elasticity of wages wrt benefit generosity and $\eta_{1-\lambda,b}$ the elasticity of unemployment duration. For the sake of simple notations, we keep s as the job finding rate in the main text. Here is the final expression:

$$\frac{d\widetilde{W}}{db}\frac{1}{(1-s)v'(c_e)} = \frac{u'(b)-v'(w(1-t))}{v'(w(1-t))}-\frac{1}{s}\eta_{1-s,b}+\eta_{w,b} \tag{77}$$

where all notations are previously defined, except the proportional tax rate t and the elasticity of wages wrt benefit generosity $\eta_{w,b}$.

C Extra Tables

Table B1: Marginal Value of Public Funds Estimates in Europe

Country	Study	Design	Policy	$\frac{dD}{dP}\frac{P}{D}$	$\frac{dB}{dP}\frac{P}{B}$	Behavioral costs UI	Behavioral costs full	Cons. drop	Social Value	MVPF
Austria	Lalive, van Ours, Zweimueller, 2006	DiD	PBD	0.10		0.24	0.55	0.09	0.36	0.88
Austria	Lalive, van Ours, Zweimueller, 2006		PBD	0.21		0.58	1.29	0.09	0.36	0.59
Austria	Lalive, 2007		PBD	0.73		1.17	3.05	0.09	0.36	0.34
Austria	Lalive, 2007		PBD	0.98		1.52	4.04	0.09	0.36	0.27
Austria	Card, Chetty, Weber, 2007	RD	PBD	0.11		0.11	0.37	0.09	0.36	0.99
Austria	Lalive, 2008		PBD	0.56		2.13	4.58	0.09	0.36	0.24
Slovakia	van Ours and Vodopivec, 2008	DiD	PBD	0.63		0.94	2.36	0.09	0.36	0.41
Slovakia	van Ours and Vodopivec, 2008		PBD	0.43		0.67	1.67	0.09	0.36	0.51
Slovakia	van Ours and Vodopivec, 2008		PBD	0.72		1.54	3.44	0.09	0.36	0.31
Portugal	Centeno and Novo, 2009	RD	PBD	0.45		1.15	2.16	0.09	0.36	0.43
Germany	Schmieder, von Wachter, Bender, 2012	RD	PBD	0.14	0.58	0.12	0.41	0.09	0.36	0.96
Germany	Schmieder, von Wachter, Bender, 2012		PBD	0.12	0.54	0.13	0.38	0.09	0.36	0.98
Germany	Schmieder, von Wachter, Bender, 2012		PBD	0.13	0.67	0.14	0.42	0.09	0.36	0.96
France	Le Barbanchon, 2015	RD	PBD	0.40		0.52	1.35	0.09	0.36	0.58
Sweden	Carling et al, 2001	DiD	Benefit	1.60		1.33	2.24	0.04	0.18	0.36
Norway	Roed and Zhang, 2003		Benefit	0.95		1.00	1.51	0.04	0.18	0.47
Norway	Roed and Zhang, 2003		Benefit	0.35		0.37	0.56	0.04	0.18	0.75
Austria	Lalive et al., 2006	DiD	Benefit	0.15		0.11	0.29	0.04	0.18	0.92
Spain	Arraz et al, 2008	Pre-Post	Benefit	0.80		0.65	1.12	0.04	0.18	0.55
Austria	Card, Lee, Pei, Weber, 2015	RKD	Benefit	2.00		1.38	3.44	0.04	0.18	0.27
Austria	Card, Lee, Pei, Weber, 2015		Benefit	1.00		0.69	1.72	0.04	0.18	0.43

This Table reports updated elasticity and behavioral costs estimates for the European studies listed in Table 1 and 2 of Schmieder et al (2016), excluding the three outliers. D stands for non-employment duration and B covered unemployment duration. Elasticities are either wrt PBD or benefit levels. Behavioral costs are computed either using the UI contribution rate of 3% or the full labor wedge. We add consumption drop estimates and compute the corresponding social value assuming a CRRA parameter equal to 4. We compute the corresponding MVPFs.

Table B2: Marginal Value of Public Funds Estimates in the US

States	Study	Design	Policy	$\frac{dD}{dP}\frac{P}{D}$	$\frac{dB}{dP}\frac{P}{B}$	Behav. costs UI	full	Cons. drop	Social Value	MVPF	MVPF HS-K
CWBH, all states	Katz and Meyer, 1990		PBD	0.41	0.52	1.05	1.89	0.25	1.00	0.69	0.45
New Jersey	Card and Levine, 2000	DiD	PBD	0.45	0.74	0.39	1.05	0.25	1.00	0.98	
Missouri	Johnston and Mas, 2015	Temporal RD	PBD			0.36	0.69	0.25	1.00	1.18	0.83
US – Georgia	Solon, 1985	DiD	Benefit	0.10	0.07	0.08	0.14	0.09	0.34	1.18	1.03
CWBH - all states	Katz and Meyer, 1990	State-by-year	Benefit	0.80		0.52	1.07	0.09	0.34	0.65	0.43
US - New York	Meyer and Mok, 2007	Pre-post	Benefit	0.60	0.30	0.41	0.81	0.09	0.34	0.74	
US - New York	Meyer and Mok, 2007		Benefit	0.12	0.30	0.08	0.16	0.09	0.34	1.15	
US - New York	Meyer and Mok, 2007		Benefit	0.23	0.30	0.16	0.31	0.09	0.34	1.02	0.89
US	Chetty, 2008	DiD	Benefit	0.53		0.36	0.71	0.09	0.34	0.78	0.68
US, 5 states	Landais, 2015	RKD	Benefit	0.29	0.73	0.14	0.40	0.09	0.34	0.96	0.84
US	Kroft and Notowidigdo, 2015	DiD	Benefit	0.63		0.39	0.84	0.09	0.34	0.73	0.48
US - Missouri	Card, Johnston, Leung, Mas, Pei, 2015	RKD	Benefit	0.78	0.77	0.82	1.24	0.09	0.34	0.60	0.44
US - Missouri	Card, Johnston, Leung, Mas, Pei, 2015		Benefit	1.21	0.35	0.64	1.63	0.09	0.34	0.51	0.74

This Table reports updated elasticity and behavioral costs estimates for the US studies listed in Table 1 and 2 of Schmieder et al (2016), excluding the three outliers. D stands for non-employment duration and B covered unemployment duration. Elasticities are either wrt PBD or benefit levels. Behavioral costs are computed either using the UI contribution rate of 3% or the full labor wedge. We add consumption drop estimates and compute the corresponding social value assuming a CRRA parameter equal to 4. We compute the corresponding MVPFs and report in the last column the MVPFs computed in Hendren and Sprung-Keyser (2020).