Effects of UI generosity on unemployment

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Abstract

I use job openings data from Burning Glass Technologies (BGT) in an extended Diamond-Mortensen-Pissarides (DMP) model of job search, first developed in Gallant et al., 2020, to study the effects of making unemployment insurance (UI) more generous on job search behavior. I find that states that terminated the generous Federal UI schemes briefly saw a rise in aggregate search effort; aggregate search effort is measured as a weighted sum of the number of non-employed workers with different durations of unemployment spells.

1 Introduction

The 2019 novel coronavirus disease, dubbed COVID-19, kept millions of workers at home, and away from their jobs in order to prevent the spread of the virus. In the wake of the pandemic, the Federal government took on an unprecedented expansion of unemployment benefits, and in this paper I examine the effects of this expansion on labour market recovery. My results suggest that at the state level, when the generous UI was cut-off there was an increase in the job-finding rate of the non-employed workers.

For background, the time-line of the COVID-19 response may be summarized as follows: on 27 March 2020, President Trump signed the CARES act into law which expanded UI by

- adding $600 to UI checks via the Federal Unemployment Compensation Program (FPUC)
- making UI available for 13 extra weeks for those who had exhausted their benefits
- making previously ineligible workers eligible, e.g. self-employed workers were allowed to claim UI under the Pandemic Unemployment Assistance program (PUA);

The CARES expired on 21 December 2020; the Consolidated Appropriations Act was passed by the Trump administration on 27 December 2020 (this plan added $300 to UI payments, made $600 stimulus payments to households earnings less than \$75,000, and kept many of the programs initiated under the CARES act from expiring); finally, the American Rescue Plan Act passed on 11 March 2021 by the Biden administration, extending the enhanced UI till 6 September 2021.

Citing labour shortages, a number of states chose to end their participation in the enhanced unemployment insurance programs before September 2021[1]- giving rise to a natural experiment that allows me to compare the labour market outcomes between the states that did, and did not opt out of the enhanced UI. Thus the sudden termination of UI benefits in some states allows me to decouple UI generosity from the effects of general equilibrium.

In particular, I focus on the effect of UI on aggregate search effort as defined in Gallant et al., 2020, which is a weighted sum of the different components of the non-employed. I use aggregate search effort (i.e. effective number of job-seekers) for two reasons. First, the total search effort is not observed directly in the data; I rely on the variation in the stocks and flows of the labour market to deduce it. In particular, I use an extended Diamond-Mortensen-Pissarides model first developed in Gallant et al., 2020 which returns the total search effort. Secondly, since labour market conditions and UI generosity are correlated, I use the Gallant et al., 2020 to decouple the labour market from the business cycle.

Furthermore, the pandemic induced a number of novel movements in labor market stocks, which obscures their empirical relationship with UI generosity. For example: the Beveridge curve, plotted in Figure 1 (after normalizing the mass of the unemployed by the population), the November 2019–June 2020 Beveridge curve moves rightwards. This shift in the Beveridge curve reflects the fact that the standard measures of unemployment in the data[2] are ill-suited to accommodate the unprecedented movements in labour stocks that occurred as a result of the pandemic. On the one hand, the number of unemployed kept rising throughout much of 2020, while on the other hand, vacancies recovered relatively quickly (see Section 2.2 for a more detailed discussion, aided with data from BGT) causing the Beveridge curve to shift outward. In particular, these movements highlight the need to account for the composition of the unemployed, as there is substantial heterogeneity in the job-finding rates of the different types of non-employed.

On the other hand, accounting for the heterogeneity of job-search intensity amongst different parts of the non-employed population is able to restore the Beveridge curve. We can see as much in Figure 2, which plots the total search effort normalized by the population in the X-axis, instead of the unemployed-as-fraction-of-population. Intuitively, the number of unemployed kept increasing throughout the pandemic, but not all workers were contributing to labour market congestion equally. By accounting for the variation in the contribution to labour market congestion, we are able to restore the Beveridge curve from its rightward shift.

[1] Some as early as June, 19, 2020- three months before the benefits expired

[2] The unemployment data comes from the CPS, see the data section.

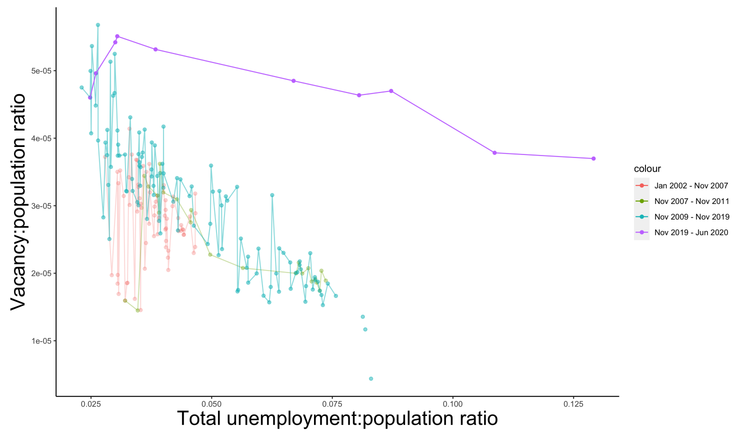

Figure 1: The above plot shows the Beveridge Curve for the US economy since 2000, scaling the number of unemployed by the number of prime-age adults (between 25 and 54 years of age) in the US. The 2019 Beveridge curve is clearly shifted outward due to the sudden rise in unemployment as a result of the pandemic.

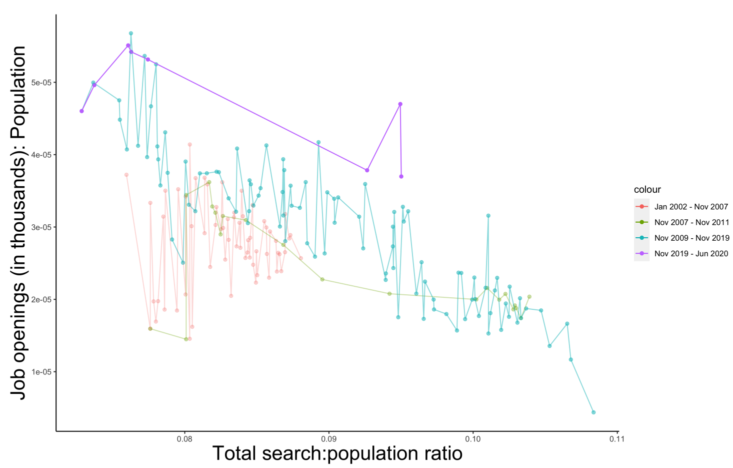

Figure 2: As shown in Gallant et al., 2020, accounting for the differences in job-search intensity amongst the non-employed allows us to recover the Beveridge curve from its rightward shift.

The Federal government expanded UI by authorising funds to state governments to then disburse UI claims; citing labour shortages, 21 states chose to terminate the expanded UI programs three months early. This introduced variation in UI at the state level; using vacancy data from Burning Glass Technologies (BGT) and labour market data from the CPS, I estimate the Gallant et al., 2020 model to measure total search effort at the state level. I calibrate this model using pre-pandemic data and then compare the effects of UI on total search effort in states that terminated the expanded UI programs early with the states that didn't. I find that states that terminated the extended UI had a lower aggregate search effort than the states that didn't. These states also appear to have a higher job-finding rate as well. Unfortunately, neither effect is large enough to appear significant at the 95% level of confidence.

1.1 Literature review

This paper extends the work of Gallant et al., 2020, and although thematically similar (both look at labour market dynamics during the pandemic recession), there are at least three notable differences: First, I study the effects of unemployment insurance on job-finding rates (Gallant et al., 2020 do not look UI at all). Second, I use a DMP model at the state level to estimate job-finding rates (Gallant et al., 2020 look at the data at the national level only), and I use BGT data to study vacancies which is more granular than the Job Openings and Labour Turnover Survey data (JOLTS) used by Gallant et al., 2020; e.g. BGT can give me state level information on vacancies, which isn't available in JOLTS.

This paper contributes to the rapidly evolving literature looking at the effect of making UI more generous on the labour market. Some notable examples include: Elsby et al., 2010, Elsby et al., 2011, Ernst, 2015, Nekoei and Weber, 2017, and Farber and Valletta, 2015. While restricting our attention to the labour economics literature there are many papers more relevant to the Covid-19 pandemic recession: Finamor and Scott, 2021 and Scott and Finamor, 2020 show a negative relationship between UI generosity and employment using high frequency data from Homebase[3]; in particular, there is an employment gap, with workers receiving more UI working less than their peers receiving relatively less UI. However, the authors note that, this gap was established before the enhanced UI went into effect. The authors conclude that the employment gap was more likely driven by the pandemic than by the FPUC.

Although the Federal government had increased and expanded UI in response to the pandemic-recession, the states were left in charge of implementing the UI programs. Citing labour shortages, many states opted to cut-off UI before the extensions were set to expire. Dube, 2021 uses an event-study design with high frequency data from the Census Household Pulse Survey to conclude the benefits reduction had little impact on job gains. In particular, upon focussing on groups

[3]Homebase is a software company providing scheduling and time clock services to small business

most likely to receive UI (non-college graduates, and people not from high-income households), the job gains were minimal, and his diff-in-diff estimates ruled out much of the micro UI duration elasticities recorded in the existing literature.

Looking towards more macro-flavoured articles there appears to be broad agreement in the policy literature on using fiscal policy to help job-losers weather the pandemic-recession; Mitman and Rabinovich, 2021 investigate the optimal policy response of UI to shocks. The authors find that with commitment, the optimal policy accounts for changes in job-search behavior as a result of future UI benefits changes. Such a policy front-loads UI-unlike the optimal discretionary policy. The authors conclude that in response to a shock like the pandemic-recession, a large and transitory increase in UI is optimal; a policy rule contingent on change in unemployment (rather than its level) does a good job of approximating the optimal policy response.

In Furman et al., 2020, the authors make four policy recommendations to help economic recovery in the wake of the pandemic recession; provide income support for the unemployed, underemployed, and most vulnerable by extending the generous UI; use temporary work subsidies to encourage job-seekers to find work; support small and medium business by extending loans to them and; provide block grants to state and local governments. In a policy working paper for the World Bank, Moorty et al., 2020 recommend providing sick leave allowances, make cash transfers available to households, and expand and increase unemployment insurance. They too recommend making employment subsidies available to firms to mitigate job-losses.

The rest of this paper is organized into data, model, results, and ends with the conclusion.

2 Data

2.1 Current population survey

Monthly CPS data is available via IPUMS and the US Census between January 2001 to August 2020, and samples are restricted to individuals aged 25 to 55.

2.1.1 Census CPS

The US Census data can be found:

[https://www.census.gov/data/datasets/time-series/demo/cps/cps-basic.html].

I pull data on workers' employment status and recall using the basic CPS from the Census; in particular, the CPS asks the unemployed if their employer gave them a date to return to work, and about their job search behavior. Furthermore, I use the cross-sectional data in the census CPS to measure monthly stocks of each labour market state. The IPUMS CPS doesn't record the variables necessary for classifying the workers into different types of non-employment.

Following Gallant et al., 2020 workers are allowed to transition between 4 states (E, T, P, N), denoting *employment*, *temporarily unemployed*, *permanently unemployed*, and *Not-in-labour-force* respectively. CPS also records the spell duration for both types of unemployed. Employment and NILF may be understood as usual, i.e. an employed worker is one who reports having worked for pay or profit during the survey week; a respondent who is NILF is someone who reports neither having a job, nor looking for one. A permanently unemployed worker is one who doesn't have a job, and is looking for one. Finally, a temporarily unemployed worker is comparable to someone who has been furloughed.

In Table 1 we see the number of temporarily unemployed i.e. furloughed workers, as a fraction of the population shoots up by an order of magnitude in April 2020. This stands in contrast to recessions of the past, wherein, it was the number of permanently unemployed, i.e. workers that either quit or were fired, that rose (discussed in Elsby et al., 2010).

	Date	$\frac{E}{Pop}$	$\frac{T}{Pop}$	$\frac{P}{Pop}$	$\frac{NILF}{Pop}$	$\Delta\frac{E}{Pop}$	$\Delta\frac{T}{Pop}$	$\Delta\frac{P}{Pop}$	$\Delta\frac{NILF}{Pop}$
1	2020-01-01	0.80	0.01	0.02	0.17				
2	2020-02-01	0.80	0.01	0.02	0.17	-0.00	-0.00	-0.02	0.01
3	2020-03-01	0.79	0.02	0.02	0.17	-0.02	1.16	-0.02	0.02
4	2020-04-01	0.67	0.11	0.02	0.20	-0.15	5.56	-0.03	0.16
5	2020-05-01	0.70	0.09	0.02	0.20	0.04	-0.20	0.06	-0.03
6	2020-06-01	0.72	0.06	0.03	0.19	0.04	-0.33	0.32	-0.04
7	2020-07-01	0.73	0.05	0.03	0.19	0.01	-0.12	0.01	0.00
8	2020-08-01	0.75	0.04	0.03	0.19	0.02	-0.30	0.05	-0.01
9	2020-09-01	0.75	0.03	0.03	0.19	0.01	-0.26	0.09	0.01
10	2020-10-01	0.76	0.02	0.03	0.18	0.02	-0.31	0.01	-0.03
11	2020-11-01	0.76	0.02	0.03	0.19	-0.00	-0.18	0.01	0.02
12	2020-12-01	0.76	0.02	0.03	0.19	-0.00	0.28	-0.08	0.00
13	2021-01-01	0.76	0.02	0.04	0.19	-0.01	0.05	0.09	0.01
14	2021-02-01	0.76	0.02	0.04	0.19	0.00	-0.17	0.05	-0.00
15	2021-03-01	0.76	0.02	0.04	0.19	0.00	-0.05	-0.02	-0.01
16	2021-04-01	0.76	0.01	0.03	0.19	0.00	-0.14	-0.08	0.02
17	2021-05-01	0.77	0.01	0.03	0.19	0.00	-0.19	0.00	-0.01
18	2021-06-01	0.76	0.01	0.04	0.19	-0.00	0.06	0.09	-0.01
19	2021-07-01	0.77	0.01	0.03	0.19	0.01	-0.06	-0.06	-0.01
20	2021-08-01	0.77	0.01	0.03	0.18	0.01	-0.04	-0.04	-0.01
21	2021-09-01	0.78	0.01	0.03	0.18	0.01	-0.34	-0.09	-0.01
22	2021-10-01	0.78	0.01	0.03	0.18	0.00	-0.15	-0.05	-0.00

Table 1: The above table shows labor market stocks as fractions of population, with the data coming from the CPS. The last four colums shows the relative change in proportion of the population in employment, temporary unemployment, permanent unemployment, and Not-in-labour-force, respectively.

I follow Forsythe et al., 2020 to sort individuals in the CPS into the four labour market states; the authors of that paper propose using the more detailed questions on labour market participation in the CPS to classify respondents into whether they are waiting to be re-employed, or if they are searching for a job. Amongst active job seekers, those on temporary layoff have a higher likelihood of returning to their former industry of employment (see Forsythe et al., 2020 for more details).

Following Gallant et al., 2020, I find the share of workers (as fraction of the population) who report being employed but absent from work rises to 2.4% in February-April 2020, from its usual level at 0.003%. On the other hand, Forsythe et al., 2020 find workers absent without pay are more likely to return to the same industry when they are go back to work than the permanently unemployed. The BLS has reported that "analysis of the underlying data suggests that this group includes workers affected by the pandemic who should be classified as unemployed on temporary layoff" (BLS, 2020). Forsythe et al., 2020 show that 19 out of every 20 worker who are employed but absent from work return to their former industry. This suggests that labour force participants from this group are very likely to return to their former employer, and as such should be counted among the temporarily unemployed.

It is worth noting here that the temporarily unemployed always tend to exit unemployment faster than their permanently unemployed peers, recession or no; see Figure 3, which plots the job-finding rate for the different types of unemployed[4]. What makes the labour market during the pandemic special is the rise in the share of unemployed who are on temporary layoff, i.e. there is a composition effect at play. We can see this in Figure 4. Figure 5 shows the increase in unemployment during the 2019–2020 period can almost entirely be explained by the rise in temporary unemployment.

Furthermore, similar to Gallant et al., 2020, I note a rise in the share of the population that is neither retired nor disabled, but isn't participating in the labour force either: it rises from 13.2% to 16.5%. It's not obvious where/how to assign this group: If we consider the 16.5% of the population that isn't retired or disabled but isn't participating in the labour force either as if they were temporarily unemployed during the pandemic recession, then we must consider the 13.2% of the population that is absent from work during non-recession times to be temporarily unemployed as well. Doing so will cause the Gallant et al., 2020 model to overestimate the share of temporarily unemployed during normal times. Thus, I keep this group in NILF, but flag that in April and May 2020, this group, to an unusually large extent, consists of people who will return to work.

[4]See Fujita and Moscarini, 2017, which uses Survey of Income and Program Participation data from 1990 to 2013 to find a large share of workers return to their former employers.

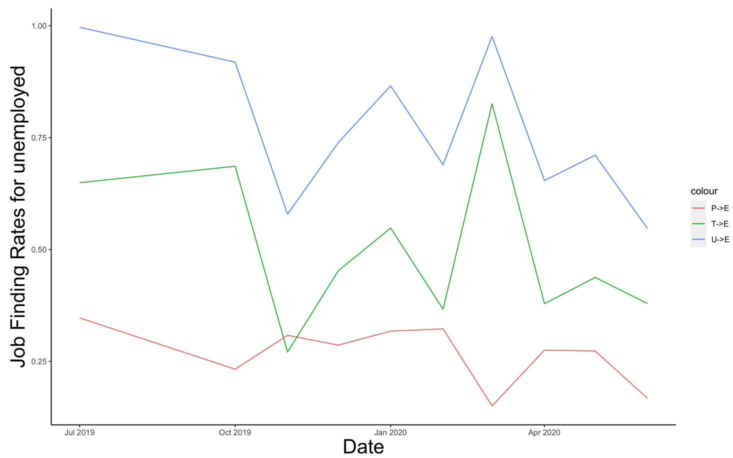

Figure 3: The above figure plots the job-finding rate for the different types of unemployed. Throughout the sample, the temporarily unemployed exit unemployment at a higher rate than the permanently unemployed.

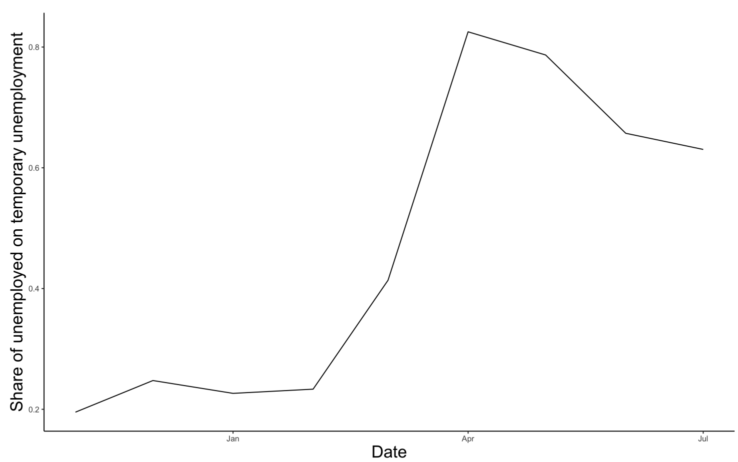

Figure 4: Share of unemployed on temporary unemployment, from 2019 to 2020

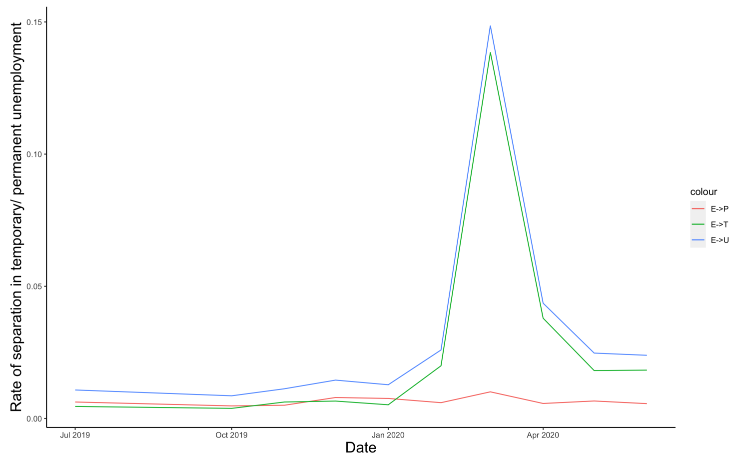

Figure 5: This plot shows the separation rates by unemployment type using adjusted transitions measured using CPS data. Each month reports the transition rate from the employed population in the previous month. The definition of temporary unemployment includes workers who report being on temporary layoff, as well those 'absent from work for other reasons' without pay.

2.1.2 IPUMS CPS

The IPUMS CPS data can be found:

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

Integrated Public Use Microdata Series, Current Population Survey: Version 9.0 [dataset].

Minneapolis, MN: IPUMS, 2021.

https://doi.org/10.18128/D030.V9.0

The panel aspect of the IPUMS CPS gives me demographic information, and allows me to calculate the monthly transition rate to and from each labour market state.

In particular, I retain age, sex, race, employment status, and panel weights to be able to control for person-effects and time-trends. I am able to link the IPUMS CPS with the Census CPS using the CPSID.

2.2 Burning Glass Technologies

Burning Glass Technologies is a software company headquartered in Boston, MA, providing "real-time data on job growth, skill demand, and labour market trends" (BGT, 2021). I use their vacancy data, which details the date of a job's posting, as well as its location.

BGT uses a web-spider to scan the known web and tracks about 3.4 million unique, and active openings, making their data more up-to-date than JOLTS. My sample consists of job openings data from January 2007 to October 2021, with the exception of 2008 and 2009. I am able to construct a time series by aggregating the number of openings at the level of the (geographic) state, for each month from January 2007 on. In Figure 7, I show the full sample of vacancy

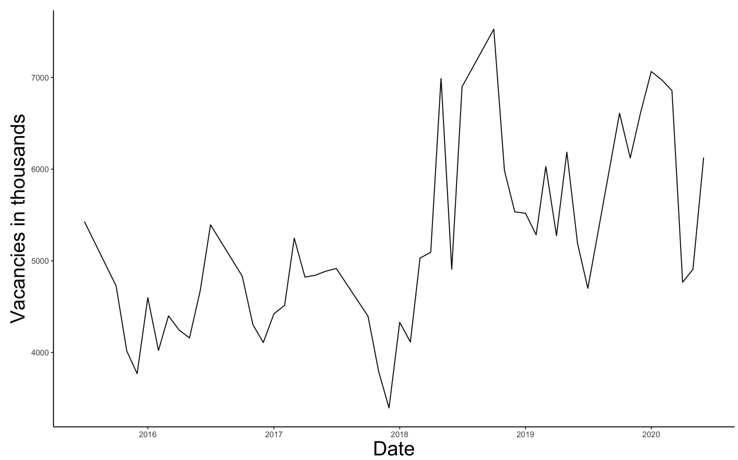

Figure 6: Vacancies over time, seasonally adjusted, with data from BGT. It is worth noting here that the peak in the number of jobs in 2015 (considered to a year with a very good labour market) is *lower* than the trough in number of jobs in 2020.

numbers from BGT, disaggregated to the state level.

3 Search and matching model

The search and matching model returns total search effort endogenously. Total search can be thought of as a weighted sum of the different types of unemployed; the Gallant et al., 2020 model exploits the fact that the temporarily and permanently unemployed find jobs at different rates, and thus contribute to labour market tightness differently.

Definition 1. *Total search effort can be defined as a weighted sum of the different components of the non-employed. Using variation in the stocks and flows of*

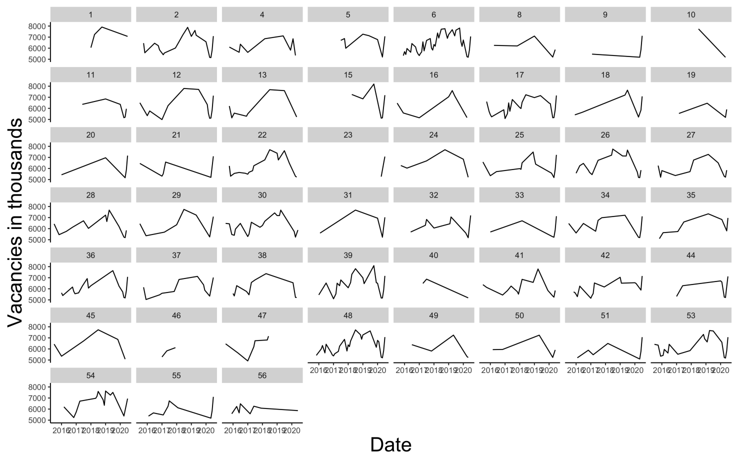

Figure 7: Vacancies over time in thousands, seasonally adjusted with data from BGT. Each of the above facet corresponds to one of 50 states plus D.C.

the labour market, we can deduce the total search effort of those on temporary unemployment, permanent unemployment, and NILF. One unit of total search effort is equivalent to the search effort of a permanently unemployed worker who just entered unemployment.

The search and matching model developed in Gallant et al., 2020 accomplishes this by extending the classic DMP framework to accommodate heterogeneity in job-search effort/job-search cost; the number of unemployed can be recovered from the Gallant et al., 2020 framework as the unweighted sum of temporarily and permanently unemployed:

$$U = T + P.$$

We can now calculate the unemployment rate as

$$u = \frac{U}{U + E}.$$

Indeed, we can recover the classic DMP model by simply using total unemployment instead of total search effort.

Workers are modelled as transitioning between four labour market states that show persistence:

- Employed (E)

  Definition 2. *This group consists of persons who did any work for pay or profit during the survey reference week; persons who did at least 15 hours of unpaid work in a family-operated enterprise; and persons who* *were temporarily absent from their regular jobs because of illness, vacation, bad weather, industrial dispute, or various personal reasons.*

- Permanently unemployed (P)

  Definition 3. *Persons are classified as unemployed if they do not have a job, have actively looked for work in the prior 4 weeks, and are currently available for work.*

- Temporarily unemployed (T)

  Definition 4. *Temporarily unemployment is a state a worker finds themselves in upon being furloughed rather than dismissed by their employer. In the CPS interview, respondents are asked if their employer has given them a date to return to work and/or if they have been given any indication that they will be recalled to work in 6 months.*

- NILF (N)

  Definition 5. *Persons who are neither employed nor unemployed are not in the labor force. This category includes retired persons, students, those taking care of children or other family members, and others who are neither working nor seeking work.*

3.1 Actively job seeking vs awaiting recall during temporary unemployment

Following Gallant et al., 2020, the temporary unemployment state is itself sub-divided into two transitory states, to highlight the distinction between temporarily unemployed workers who are actively seeking work (denoted by T^A), and those who are waiting to be recalled (denoted T^W). Thus,

$$T = T^A + T^W.$$

These states are transitory in the sense that temporarily unemployed workers are modelled as being assigned to either type of temporary unemployment with probability q at the start of each period. This setup obviates the need to follow the duration dynamics for each type of temporarily unemployed separately. Thus at the start of date t, a temporarily unemployed worker actively seeks work with probability:

$$q_t \equiv \Pr(T^A|T). \tag{1}$$

This probability can be measured directly from the CPS as the share of temporarily unemployed who say they are actively seeking work. Both types of temporarily unemployed are modelled as transitioning to N or P, and from $(E, N.P)$ at the same rate. The advantage of this setup is that I can leverage the much bigger sample of temporarily unemployed from before the pandemic recession to get more precise, and consistent estimates of transition rates into, and out of temporary unemployment. Furthermore, following Gallant et al., 2020

I calibrate this model using pre-pandemic data when the number of temporarily unemployed was much smaller; this choice further necessitates transience in T^A and T^W.

Let d denote the duration of unemployment spell; $T(d), P(d)$ are indexed by duration as d affects their transition rate via duration dependence in job-finding rates and changes in the composition of the unemployed (workers are allowed to transition to and from T and P over the course of their unemployment spell).

Having defined the different types of non-employed, I can now specify total search effort as

$$S_t = \underbrace{\overline{P}_t}_{\substack{\text{Search effort}\\ \text{from permanently}\\ \text{unemployed}}} + \underbrace{sN_t}_{\substack{\text{Search effort from}\\ \text{NILF}}} + \underbrace{(1-\pi\lambda_t^{T^W\to E})\overline{T}_t^A}_{\substack{\text{Search effort from}\\ \text{temporarily unemployed}}} \tag{2}$$

I go over each term in the right hand side of Equation 2 in turn.

3.2 Search effort of permanently unemployed

Let $\lambda^{(X)\to(Y)}$ denote the transition rate from state $X \in \{E, P(d), T^W, T^A(d), N\}$ to state $Y \in \{E, P(d), T^W, T^A(d), N\}$. The job-finding rate on date t for a permanently unemployed worker who has a spell duration of d is given by

$$\lambda_t^{P(d)\to E} \equiv \Pr(E_t|P_{t-1}(d)) = A(d)m_0 x_t^{1-\alpha} \tag{3}$$

where $A(d)$ is monotonically decreasing function, $A(d) > 0 \forall d, A(0) = 1$. It captures duration dependence of unemployment spells, and reports the probability of a given worker exiting unemployment relative to an unemployed worker (permanently or temporarily unemployed) who just entered unemployment. Intuitively, this reports the baseline hazard of transitioning out an unemployment spell, normalized by the hazard rate of an unemployed worker who just entered unemployment.

As in Krueger et al., 2014 unemployed individuals with different durations contribute to aggregate search effort in proportion to their job-finding rates. Define $\overline{P}_t$ as the weighted average of the permanent unemployed in time t:

$$\overline{P}_t = \sum_{d=1}^{D} A(d)P_t(d).$$

The above expression accounts for differences in spell duration distribution on total search effort; if the permanently unemployed are mostly composed of the long term unemployed, their job-finding rate will be low, and will thus contribute to labour market congestion more. Conversely, if the permanently unemployed are more similar to the temporarily unemployed, they'll have a higher job-finding rate, and will thereby congest the labour market less. This expression constitutes the first term of Equation 2.

3.3 Search effort of those not in labour force

Those not in the labour force search with intensity $s \in [0, 1]$, resulting in a job finding probability of:

$$\lambda_t^{N \to E} \equiv \Pr(E_t | N_{t-1}) = s m_0 x_t^{1-\alpha} \tag{4}$$

where s is a term to be calibrated with the data. Using s and the mass of NILF on date t, N_t, we can recover the second term in Equation 2.

3.4 Search effort of temporarily unemployed

A fraction q_t of the date t temporary unemployed are modelled as actively looking for work. Therefore, $q_t \cdot T_t(d) \equiv T_t^A(d)$, and $(1 - q_t) \cdot T_t(d) \equiv T_t^W(d)$. Since the CPS asks respondents if they are looking for work, and if their employer gave them any indication that they'd be called back to work, I can estimate q_t from the data directly using the relationship specified in Equation 1. To highlight the contrast between these two types of temporarily unemployed, consider the position a chef at a restaurant might find themselves in compared to that of a busboy at the same restaurant during the pandemic induced lockdown. The former can be reasonably confident that their employer will honour their word when they are told they'll be called back to work at a future date. The latter on the other hand can't be as sure since the employer might find it more cost-effective to hire a new busboy or just leave the vacancy unfulfilled.

The temporarily unemployed workers who are not searching for work do not congest the labour market at all, and their job-finding rate is given by the exogenous variable: $\lambda_t^{T^W \to E}$. This term reflects the rate at which they get recalled by their employers.

On the other hand, the temporarily unemployed workers who are searching for work do contribute to labour market congestion. It is assumed that these workers are less likely to be recalled by their former employers[5], compared to their peers who choose to wait to be recalled. At the same time, I also assume their search effort leads them to being matched with a job at the same rate as the permanently unemployed. Let $0 < \pi < 1$ denote how much less likely an actively job seeking, temporarily unemployed worker is be recalled than a peer who is waiting for recall. We can now write $\lambda_t^{T^A \to E}$ as:

$$\lambda_t^{T^A(d) \to E} = \pi \lambda_t^{T^W \to E} + (1 - \pi \lambda_t^{T^W \to E}) \lambda_t^{P(d) \to E}. \tag{5}$$

The value of π is implied by equation (5), and is calculated using the monthly

[5]Indeed, it is impossible to look at the data and tell how likely it is that we see a worker being recalled. But the workers themselves would know how likely they are to be recalled by their employers, and thus would decide to look for work, or wait to be called back on the basis of their private information.

job-finding rates. In expectation, π can be estimated as

$$\hat{\pi} = \frac{1}{T}\sum_{d=0}^{24} \frac{\lambda_t^{T^A(d)\to E} - \lambda_t^{P(d)\to E}}{\lambda_t^{T^W\to E}(1-\lambda_t^{P(d)\to E})}. \tag{6}$$

Furthermore, since the temporarily unemployed, actively searching find jobs at the same rate as the permanently unemployed, the weighted average for their search effort can be calculated as

$$\overline{T}_t^A = \sum_{d=1}^{24} A(d) T_t^A(d).$$

Thus, using the above expression, the expected value of π in Equation 6, and the exogenous recall rate $\lambda^{T^W\to E}$ we can derive the final term in the expression returning total search effort, in Equation 2.

3.5 Job-finding rates

The job-finding rates are determined by market tightness (the ratio of vacancies to aggregate search effort exerted by non-employed individuals). Total search effort on date t is denoted by S_t, and the number of vacancies is denoted by V_t. Thus the matching function can be written as

$$\frac{M(S_t, V_t)}{S_t} = m_0(S_t^{\alpha} V_t^{1-\alpha})\frac{1}{S_t} = m_0 x_t^{1-\alpha} \tag{7}$$

where $x_t = \frac{V_t}{S_t}$ denotes labour market tightness, and m_0 and α are terms to be calibrated with the data. We can think of S_t as U_t re-weighted to account for the composition of the unemployed- in particular, to account for differences in separation and job-finding rates of the different types of job seekers. It incorporates the contribution of $P(d)$, $T(d)$, and N_t towards labour market congestion.

3.6 Model calibration and estimation procedure

3.6.1 Transitions

The transition rates directly estimated using the CPS panel are not consistent with the time surveys of labour forces states obtained from the monthly cross-sections. In particular, denote by $\hat{\Pi}$ the distribution across the four labour force states: E, P, T, and, $NILF$, as measured in the cross-section on date t, and let $\hat{\Lambda}$ the 4×4 transition matrix between dates $t, t+1$. I expect the following relation to hold:

$$\hat{\Pi}_{t+1} = \hat{\Lambda}_t \hat{\Pi}_t.$$

Unfortunately, this identity is not satisfied as respondents are more likely to report not being in the labour force due to the rotation group bias[6].

[6] see Krueger et al., 2017 and Bailar, 1975 for more details

The issue is that the CPS gathers data from survey respondents for four months, allows the respondents to exit the sample for eight months, and then interviews them again for four months, after which the respondents are never included in the CPS again. In principle, CPS respondents should be observed for a calendar year, but over the course of the year, people stop responding to survey requests[7] to avoid answering questions, or move away.

Furthermore, due to the novel movements in the data (discussed in the Sections 1 and 2.1.1) modelling attrition is likely to lead to biased estimates when fitting the model to the during-pandemic data.

Following Gallant et al., 2020 I assume the distribution as measured in Π_t coincides with the true distribution in the population and re-estimate $\hat{\Lambda}_t$ to restore consistency.

This involves estimating 16 transition rates; with the constraint that the rows of the transition matrix must sum to 1, I am left with 12 terms to estimate. I adopt the approach developed in Kroft et al., 2016: First, I normalize the population for each month to 1, which provides 3 more restrictions, and the unemployed with spell duration of zero give rise to 2 more restrictions. This leaves 7 more restrictions to be imposed. The date t transition rate from state X to state Y can be written as $\lambda_t^{X \to Y}$. I calculate the following ratios:

$$\frac{\widehat{\lambda^{P\to T}}}{\widehat{\lambda^{N\to T}}}, \frac{\widehat{\lambda^{T\to P}}}{\widehat{\lambda^{T\to N}}}, \frac{\widehat{\lambda^{T\to E}}}{\widehat{\lambda^{T\to N}}}, \frac{\widehat{\lambda^{N\to P}}}{\widehat{\lambda^{N\to E}}}, \frac{\widehat{\lambda^{E\to N}}}{\widehat{\lambda^{E\to P}}}, \frac{\widehat{\lambda^{P\to N}}}{\widehat{\lambda^{P\to E}}}, \frac{\widehat{\lambda^{E\to T}}}{\widehat{\lambda^{N\to T}}} \tag{8}$$

and assume that these ratios of estimated transition rates are valid estimates of the ratio of true transition rates $\frac{\lambda^{X\to Y}}{\lambda^{X\to Y}}$: $X, Y \in \{E, P, T, N\}$. As noted in Gallant et al., 2020, "the key assumption is that the biases in the estimated transition rates need to 'cancel out' when [I] take ratios (i.e., that the biases that cause the inconsistency are proportional across each of the airs of labor market states above)." The resultant transition rates impose consistency in labour market stocks (i.e., the number of people in a given labour market state) from month to month.

3.6.2 Calibration

1. Using CPS monthly data, I estimate stocks and transition rates of the labour market states, assigning flows from NILF to unemployment using pre-2020 data on NILF transitions.

2. The $A(d)$ function is estimated using non-linear least squares, and is specified as

$$A(d) = (1 - \alpha_1 - \alpha_2) + \alpha_1 \exp(\beta_1 \times d) + \alpha_2 \exp(\beta_2 \times d), \tag{9}$$

   with the sample consisting of the actively job seeking temporarily unemployed, and permanently unemployed.

[7] CPS is a voluntary survey, with a *monthly* response rate of about 90%, after excluding empty housing units/ houses with only ineligible members

3. The remaining parameters are estimated by minimising the distance between the observed job-finding rate of the unemployed and the model-predicted job-finding rate over the pre-2020 sample period. These estimates, along with the duration parameter estimates (i.e. the NLS coefficients) are reported in Table 2.
4. At the national level, the above three steps suffice to calibrate the model; at the state level, I first cluster the CPS monthly data at the (geographic) state level and repeat step 1.
5. I estimate $A(d)$ at the state level using a third degree polynomial regression (more details in the next subsection); as in step 2, the sample consists of actively job seeking temporarily unemployed, and permanently unemployed. However, since the data has been clustered at the state level, this procedure runs the linear regression 50 times- once for each state.
6. Similar to step 3, I use a minimum distance algorithm to estimate the remaining model parameters, by minimising the distance between observed and predicted job-finding rates at the state level.

Table 2: This table reports parameter values estimated from each step of the model calibration, using 2000–2019 data at the national level.

	Estimate	Std. Error	t value	$\Pr(>\lvert t\rvert)$
α_1	0.265	0.025	10.694	0.000
α_2	0.450	0.043	10.360	0.000
β_1	1.791	0.480	3.731	0.001
β_2	0.068	0.019	3.480	0.002
π	0.3743			
a	0.708			
s	0.342			
m_0	0.459			

3.7 Model fit

The fit of the model may be assessed by comparing the job-finding rates observed in the data with those predicted by the model. Consider Figures 8 - 11 which show the observed and predicted rates at which the different types of job-seekers enter employment.

Taking the model to the data at the state level, we see that the Gallant et al., 2020 specification of $A(d)$ doesn't lead to the best fit. Refer to Figure 12 wherein I plot $A(d)$ as a function of duration using labour market data at the national level. As noted before, $A(d)$ can be measured from the CPS as the probability of a worker with spell duration d transitioning out of an unemployment spell,

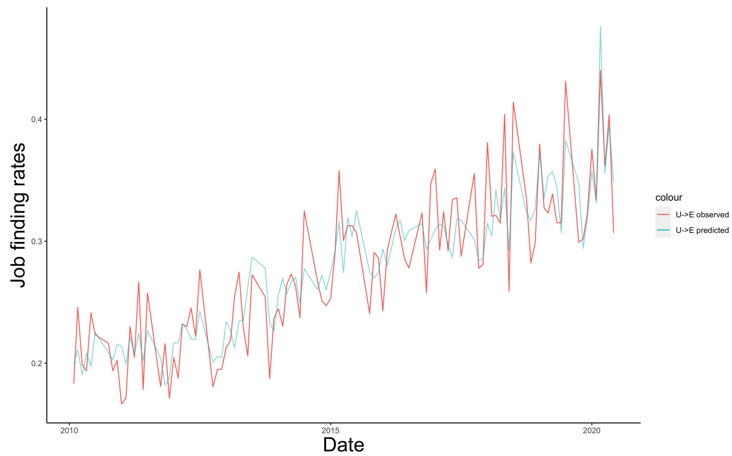

Figure 8: Model predicted job-finding rates, and observed job-finding rates for unemployed workers from 2010 to 2020.

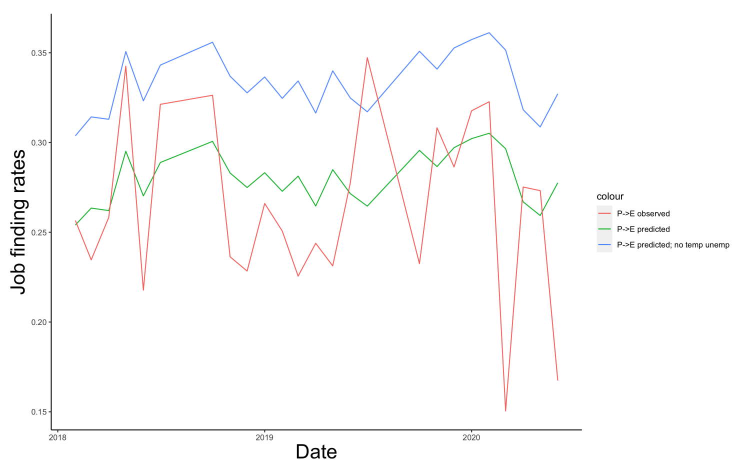

Figure 9: Model predicted job-finding rates, and observed job-finding rates for permanently unemployed workers. The overall job-finding rate of the unemployed is calculated by taking a weighted average of the job-finding rates of the temporary and permanent unemployed. A model without temporary unemployment overestimates the job-finding rate of the permanently unemployed when looking at the out-of-sample fit with the data.

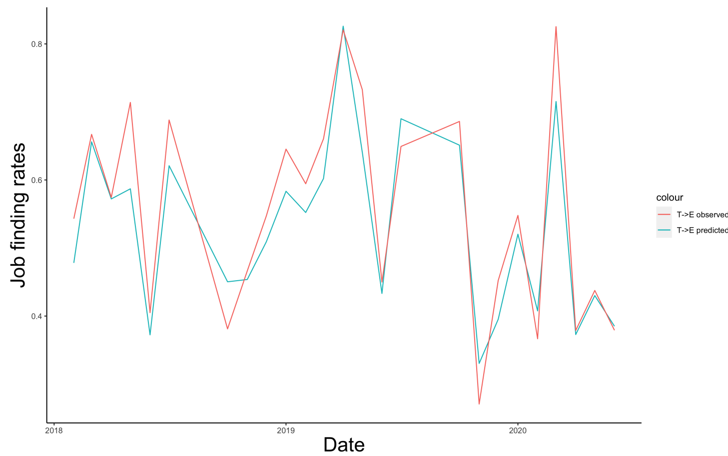

Figure 10: Model predicted job-finding rates, and observed job-finding rates for temporarily unemployed workers. A model that doesn't account for temporarily unemployed worker completely fails to predict their job-finding rates.

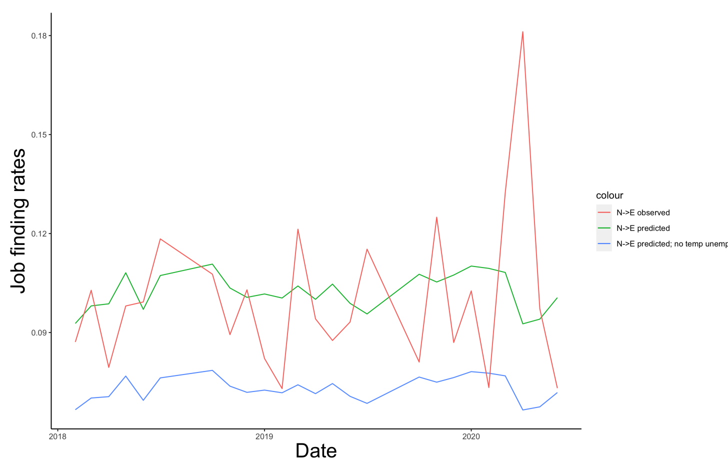

Figure 11: Model predicted job-finding rates, and observed job-finding rates for those NILF workers. Once more, the model without temporary unemployment has a much worse out-of-sample fit with the data.

relative to the probability of a worker with spell duration 1 making the same transition. To compute it,

1. I restrict my sample from the CPS to prime age workers (i.e. 24 -55 year olds)
2. I construct an indicator $\mathbb{1}_{\{U_t, E_{t+1}\}}$ which equals one if I observe a worker being unemployed on date t and transitioning to employment on date $t+1$
3. After clustering the observations on spell duration, I use a GLS model, regressing $\mathbb{1}_{\{U_t, E_{t+1}\}}$ on spell duration $d \in \{1, \ldots, 24\}$ while controlling for demographic characteristics, and year and month effects
4. Using the intercept and residuals of the above regression, I can construct the baseline transition rate: $\lambda^{U \to E}(d)$
5. Now I can derive $A(d)$ as the baseline transition rate with duration d normalized by the baseline transition with duration 1, i.e.

$$A(d) = \frac{\lambda^{U \to E}(d)}{\lambda^{U \to E}(1)}.$$

6. At the national level I can use the non-linear specification in Equation 9 to estimate duration parameters
7. At the state level I eschew Equation 9 in favour of a third degree polynomial regression as in Equation 10:

$$A(d) = \beta_0 + \beta_1 \times d + \beta_2 \times d^2 + \beta_3 \times d^3 \tag{10}$$

8. $A(0) = 1$ for both estimations, at the state and national levels.

Using a non-linear least squares regression of $A(d)$ on spell duration in `R` with the same model specification as in Gallant et al., 2020 causes an error `Error in nls(...) // Singular gradient`, likely due to poor starting values and/or over-parameterization[8]. In Figure 13, I plot the data points $A(d)$ against spell duration at the state level. Compared to Figure 12, we can see that $A(d)$ at the national level doesn't look like $A(d)$ at the state level for any of the states.

4 Effect of UI generosity on job search

Since March 2020, when the CARES act was passed, many unemployed workers became eligible for enhanced UI benefits till late 2020, and then many provisions of the CARES act were extended by the Biden administration in January 2021 (see Section 1 for a summary of the Federal government's response to the pandemic, pertaining to UI expansion). Briefly, the most important aspects of the enhanced UI consists of the Federal Pandemic Unemployment Compensation,

[8]See https://stat.ethz.ch/pipermail/r-help/2008-March/158329.html

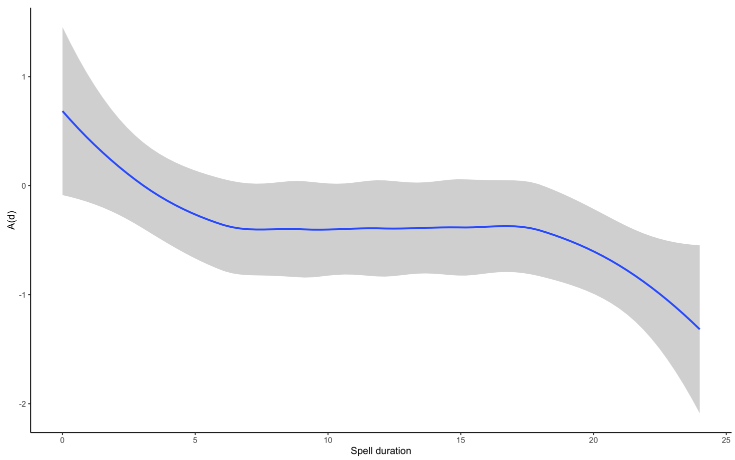

Figure 12: The above plot reports the variation in the baseline hazard with spell duration. The plot has been rendered by locally fitting $A(d)$ on spell duration

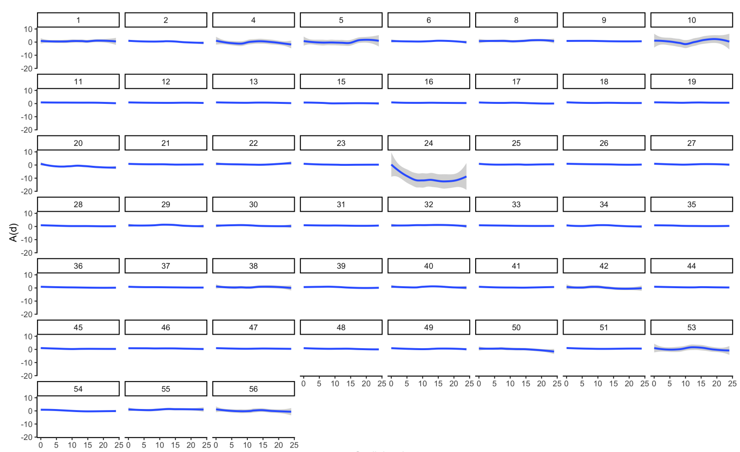

Figure 13: The above plot reports the variation in the baseline hazard with spell duration. The plot has been rendered by locally fitting $A(d)$ on spell duration

and the Pandemic Unemployment Compensation. While the latter broadened the UI eligibility rules, the former added a flat \$600 to all UI claimants' checks. The idea was that by adding \$600 to the states' UI checks, the median American worker who has been laid-off would be compensated with 100% of the wages he would have earned, had he/she/they not been fired. However, the probability of being laid-off was much higher for workers in the bottom wage quartile as noted in Cortes and Forsythe, 2020, and as a result, the median UI claimant received around 45% more money from UI, than he/she/they would have from work (see Ganong et al., 2020 for more details).

Theory suggests that this enhanced UI would serve as a disincentive to finding work- or at least make workers want to wait longer for a better job offer than they would have, had the UI benefits not been enhanced. The intuition is that the enhanced UI raises the reservation wage of unemployed workers, since no worker would accept a job that pays a lower wage than what UI pays him. Alternatively, we can simply argue that the enhanced UI reduces the search effort.

De-jure the Federal government enhanced UI till September 2021, states governments were responsible for the disbursement of UI claims. Citing labour shortages and blaming the generous UI, 26 states opted to cut off the enhanced UI early. In particular, 21 states had cut off the extended UI by July 2021. I exploit the variation in the timing of the generous UI termination to test the theoretical predictions of doing so on job-finding rates.

4.1 T-test of difference in search effort

In order to test this hypothesis I make use of state level UI disbursement laws, along with the Gallant et al., 2020 search-and-matching model to estimate

$$\mathbb{E}(S_t|UI > 0) - \mathbb{E}(S_t|UI = 0).$$

I first normalize the search efforts of both UI eligible and ineligible groups by the population. The expected aggregate search effort by enhanced UI eligible workers (that is, the aggregated search effort from 2019 to July 2020, which is when the CARES act expired) equals 975. The same figure for enhanced UI ineligible workers is 793 (i.e., the aggregate search effort from July 2020 to October 2021, the period after the CARES act expire) suggesting there was a lower aggregate search effort in states following the termination of the enhanced UI. The results of the Two Sample T-test are tabulated in Table 3.

4.2 Diff-in-diff in matching and search

I am able to use the variation in the timing of the states' termination of UI to run a diff-in-diff regression to test the effects of terminating UI early on search effort and match rate. As before I first normalize the variable of interest by the state population. Thus the diff-in-diff design may be specified as

$$S_t = \beta_0 + \beta_2 treated + \beta_3 treatment \times treated \tag{11}$$

Table 3: T test for equality of aggregate search by UI eligibility

T statistic	4.6,
degrees of freedom	1173
p-value	5e-06
95 percent confidence interval	104.1, 260.0
mean search effort in UI eligible group	975
mean search effort in UI ineligible group	793

In Table 4 I report the results of a diff-in-diff looking at the difference in job-finding rates between states that terminated UI early and the ones that didn't.

Table 4: Treatment is the early termination of UI; the treated group consists of states that terminated the enhanced UI in July. The American Rescue Plan expired on September 2021, though some states terminated the enhanced UI in July 2021. The following table reports results of the diff-in-diff design in Equation 11

	Estimate	Std. Error	t value	$\Pr(>\|t\|)$
(Intercept)	-0.9180	0.0246	-37.29	0.0000
treatment	0.2738	0.3575	0.77	0.4438
treated	-0.3278	0.0348	-9.41	0.0000
treatment:treated	-0.0544	0.5134	-0.11	0.9156

I also use a diff-in-diff design to study the effect of terminating UI early on search effort normalized matching rate; the diff-in-diff design may be specified as

$$\begin{aligned} \log\left(\frac{M(S_t, V_t)}{S_t}\right) = \log M(x_t) = \beta_0 + \beta_1 treatment + \beta_2 treated \\ + \beta_3 treatment \times treated \end{aligned} \tag{12}$$

In Table 5, I report the results of the diff-in-diff specified in Equation 12

The results in Tables 4 and 5 confirm the theoretical prediction i.e. the states that cut off the UI early saw their non-employed exit non-employment at a higher rate than the states that didn't, however, neither result is significant at the 95% level of confidence.

5 Conclusion

This paper aims to make a couple of contributions to the literature: first, using an extended DMP model of search-and-matching, it suggests that the termination

Table 5: The following table shows the results of the diff-in-diff research design of regressing search effort normalized job-finding rate on cutting off UI early. Here, the treatment is the termination of the Federal UI enhancement program; treatment group consists of 21 states that chose to terminate generous UI early. The design specification is reported in Equation 12.

	Estimate	Std. Error	t value	$\Pr(>\lvert t \rvert)$
(Intercept)	-22.2654	0.0634	-351.26	0.0000
treatment	-1.1059	0.9206	-1.20	0.2297
treated	0.9448	0.0897	10.53	0.0000
treatment:treated	0.6040	1.3221	0.46	0.6478

of the federal UI expansion was followed by a higher job-finding rate in the states that terminated early. While this lends support to theoretical predictions, unfortunately the results fail to be significant at the 95% confidence level. Second, it introduces data from Burning Glass Technologies to the model, which is more current than JOLTS[9].

While the results of this paper suggest that the Federal pandemic response (as far as UI is concerned) was followed by a stint of lower job-findng rates, it remains silent on general equilibrium effects of the enhanced UI, which was accompanied by a number of other measures that affected different parts of the economy. A possible avenue to expand on the conclusions of this paper would be to use a heterogeneous agents model (e.g. a neo classical growth model, or a neo Keynesian one).

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[9] In the process, this paper also translates the estimation and calibration code of Gallant et al., 2020 from `Stata` to `R`

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The authors use micro data on earnings together with each state's UI system to compute the distribution of UI after the uniform FPUC supplement was implemented by the CARES Act. In particular, the $600 figure chosen so as to replace 100% of the mean US wage with compared with mean state UI benefits. However, using state-specif eligibility data, and micro-data on earnings and labor force status from CPS, Ganong et al find that there is substantial variation in the effects of the CARES Act: a median laid-off retain worker can collect 166% of their prior wage in UI. On the other hand, a still-employed grocery worker may not see any pay raise. Furthermore, PUC reverses income patterns by income level and sector which would have otherwise resulted due to the pandemic. E.g. expected income for median workers in the bottom 20% of wage earners would have fallen by 9.3% but with the $600 PUC, it rose by 19.5%.

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This paper investigates the optimal response of unemployment insurance to economic shocks, with and without commitment. Using a simple dynamic framework, the authors model risk-averse workers searching for jobs at a cost, facing potentially stochastic search efficiency, and separation rates. This analysis leads to three insights about optimal UI: first, the moral hazard cost depends most directly on search efficiency rather than the unemployment rate- meaning it optimal to index UI to the primitive shock, and not unemployment. Second, the dynamic nature of the environment complicates the standard Baily-Chetty formula, since future unemployment benefits distort current search efforts. Third, unemployment matters over time, e.g: if unemployment is expected to fall in the future, UI has a low consumption smoothing benefit, which accrues to the small number of future unemployed, but it distorts the behavior of all currently unemployed. These results suggest the optimal policy intertemporally substitutes UI from periods with low unemployment to periods with high unemployment.

Moorty, L., Mahajan, S., Andronova Vincelette, G., & Izvorski, I. (2020). A policy framework for mitigating the impact of the covid-19 crisis.

Nekoei, A., & Weber, A. (2017). Does extending unemployment benefits improve job quality? *American Economic Review*, *107*(2), 527–61
A search model incorporating duration dependence predicts two countervailing forces: UI induces workers to seek higher-wage jobs, but reduces wages by lengthening unemployment. Empirically UI raises wages by improving reemployment firm quality and attenuating wage drops. The results are derived using a RD on samples over and under 40 years of age, from the Austrian Social Security Database.

Scott, D., & Finamor, L. (2020). Employment effects of unemployment insurance generosity during the pandemic
uses data from Homebase, a private firm providing scheduling and time clock software to small businesses. The high-frequency, longitudinal data allows the authors to estimate UI benefits for each worker, and follow their labour market status through early 2020. The authors employ an event study design to test the effects of higher replacement rates on employment or hours of work, and find that workers with more generous UI benefits did not experience differential declines in employment after the CARES Act was passed. The authors also replicate their main results using the CPS. "All of the additional tests support the same conclusion: the negative labor market effects associated with replacement rates are

attributable to changes in mid-March; we do not observe negative effects after the passage of the CARES Act."