The Impact of the Federal Pandemic Unemployment Compensation on Job Search*

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Abstract

During the COVID-19 pandemic, the Federal Pandemic Unemployment Compensation (FPUC) program increased US unemployment benefits by $600 a week, which increased replacement rates to over 100% for many workers. What was the impact on the labor market? We collect data on vacancy posting and job applications on the online jobs platform Glassdoor. In the first part of the paper, we estimate the effect of FPUC on applications and vacancies, by exploiting large variation in the proportional increase in benefits, in a panel at the week and local labor market level. To isolate the effect of FPUC from the contemporaneous effects of the COVID-19 crisis, we allow for different trends in local labor markets differentially exposed to unemployment at the onset of the COVID-19 crisis, before FPUC. We verify that trends in outcomes prior to FPUC do not correlate with *future* increases in benefits, which supports the validity of our identification strategy. We find that a 10% increase in benefits lead to a 3.6% decline in applications, no change in vacancies, and a 3.3% increase in labor market market tightness. In the second part of the paper, we then describe the context in which FPUC was implemented, as it is also crucial to assess the impact of FPUC on unemployment and welfare. We document that labor market tightness was particularly low during FPUC, suggesting that each application had a low chance of resulting in a job. Overall, our findings suggest that even if FPUC decreased search effort as predicted by search theory, low returns to search can explain the limited effect of FPUC on unemployment found in prior literature.

Keywords: unemployment insurance, job vacancies, job applications, COVID-19
JEL Codes: J63, J64, J65

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1 Introduction

The coronavirus COVID-19 has had a dramatic impact on the US economy, with many business closures and the unemployment rate skyrocketing to 14.7% in April 2020. In response to the crisis, policy makers passed the CARES Act, which increased the level of unemployment benefits by $600 a week (the Federal Pandemic Unemployment Compensation (FPUC)) for both those ordinarily eligible to unemployment insurance, and for workers newly made eligible, including gig workers. Ganong et al. (2020) find that as many as two thirds of unemployed workers have a replacement rate above 100%, i.e. they collect more in unemployment benefits than their typical wage. As economic theory predicts that more generous benefits decrease job search effort, this policy might have increased unemployment and delayed the recovery of the economy. However, all the early evidence suggests that there were at most very limited effects on employment (Bartik et al. (2020); Altonji et al. (2020), Dube (2020)). In this paper, we examine this apparent paradox. We show that the FPUC did have a negative effect on job search effort consistent with theoretical predictions, and we provide explanations for the limited effect on employment. The FPUC had a large negative effect on job applications but a limited impact on employment due to congestion, as the number of applications per job was unusually high. i.e. labor market tightness was unusually low.

To understand the empirical approach we take in this paper, it is important to have in mind that there are substantial challenges to identify the effect of the FPUC. In principle, one can estimate the effect of FPUC by exploiting the large disparities in the increase in generosity of unemployment benefits for different workers: for workers who would have been eligible to lower weekly benefits levels before the CARES Act, the additional $600 per week represents a larger relative increase. However, the passage of the FPUC coincided with exceptionally large and brutal changes in the labor market: because of changes in economic activities in response to pandemic, labor demand collapsed in March. It was visible before the implementation of FPUC, as the number of job listings fell (Kahn et al., 2020), while the number of unemployed workers skyrocketed. What is more, workers in low-paying jobs were most likely to lose their jobs, and those workers are precisely the ones who experienced the largest increase in their potential replacement rate with the FPUC. These shocks could represent severe confounding factors for the identification of the effect of the FPUC on the labor market. In this paper, we take several steps to address this identification challenge. First, instead of taking employment as an outcome, we focus on the effect of FPUC on search effort and job vacancies— which do not reflect as directly these labor demand shocks. Second, we look at the evolution of outcomes at a granular level, which allows us to include various controls to credibly neutralize the effect of the covid-19 crisis or other confounders.

We analyze high frequency data on job applications and job postings from Glass-

door.com, for January 2018 to July 2020. We combine them with administrative data to measure UI generosity and search behavior each week, in each local labor market — defined as the interaction of state, occupations and industries and wage deciles. To compute potential replacement rates for Glassdoor job seekers, we exploit the rich information self-reported by Glassdoor users on their prior earnings. We then use the benefits calculator created by Ganong et al. (2020), which computes unemployment benefits based on individuals' state of residence and pre-displacement quarterly earnings. To ensure the external validity of our analysis, we re-weigh observations such that each State×industry×occupation in the CPS reflects the proportion of US workers in that State×industry×occupation in the CPS in the Current Population Survey for 2018-2020.

In the first part of the paper, we estimate the impact of FPUC on search effort (job applications), job vacancies and labor market tightness in March-July 2020. Since the FPUC increased benefits differently based on prior earnings and state of residence, we exploit these differences across local labor markets. We account for seasonality using data from prior years. To account for the many abrupt changes to the labor market that happened around the beginning of the FPUC, we control for industry by week fixed effects, which accounts in particular for the differential impact of the crisis by industry, as some industries involving customer contact were much more severely affected. State by week fixed effects control for differential policies across states, and other factors that change at the state level. Finally, we also include a control for the probability of people being unemployed *prior* to the FPUC in January-March 2020 depending on prior earnings and industry. This control helps to further account for the ways different local labor markets were hit by COVID-19, independently of the FPUC. Our identifying assumption is that, in the absence of the policy change and conditional on all the controls, outcome variables of interest would have evolved similarly in local labor markets with different increases in UI. We show that, conditional on all our controls, future increases in FPUC do not correlate with trends in job applications, job vacancies or labor market tightness in the weeks prior to the enactment of the FPUC. This supports our identification strategy.

We find that a 10% increase in the benefit replacement rate due to the FPUC leads to a 3.6% decline in job applications, consistent with predictions from search models. At the same time, the FPUC had no effect on job vacancies. Therefore, a 10% in the benefit replacement rate increased labor market tightness by 3.3%. Therefore, because of the FPUC, labor market tightness was higher than it would otherwise would have been. To confirm our main results, we use an alternative identification strategy: we focus on the construction sector, where job loss before FPUC was not systematically correlated with earnings and hence identification challenges are less severe. While the estimates are naturally less precise than in the full data, we see very similar qualitative patterns: FPUC decreased applications, had no effect on vacancies, and therefore ultimately increased labor market tightness.

In the second part of the paper, we then describe the context in which FPUC was implemented, as it is also crucial to assess the impact of FPUC on unemployment and welfare. We document the evolution of applications, vacancies, and labor market tightness before and during the time of the FPUC. It turns out that seasonally adjusted aggregate applications slightly increased by 4.4%, despite the large decrease in search effort induced by the FPUC. In the same time, job vacancies declined by 26%. As a result, labor market tightness decreased by 31%, implying that on average employers got more applicants for their vacancies, and recruitment difficulties did not increase. We decompose the effects into the beginning of the COVID-19 crisis (March-May 2020) and the reopening phase (June-July 2020). While seasonally adjusted tightness was exceptionally low during the first phase, it came back roughly to its pre-COVID level in June-July 2020.

We finally discuss how our results contribute to the assessment of the welfare impact of the FPUC during the 19-COVID crisis. Our results speak to aspects of the impact of FPUC on welfare that have to to with labor market tightness. Landais et al. (2018) formalize that the welfare impact of unemployment insurance depends on its impact on labor market tightness and of the impact of tightness on welfare. Our paper shows that there was positive effect of FPUC on labor market tightness. Moreover, we also show that labor market tightness was particularly low when FPUC was implemented, which suggests that the labor market was in a region where increases in tightness were welfare improving. Taken together, our results hence indicate that providing more generous unemployment insurance was desirable when FPUC was implemented, as it contributed to bringing labor market tightness back towards a more efficient level. We note that for a comprehensive welfare analysis of FPUC, one should also take into account elements beyond labor market tightness, such as the welfare gains associated with consumption smoothing for UI recipients, and the costs of unemployment during a pandemic. Although our findings do not speak to these elements, we note that they would likely re-inforce our conclusion regarding the desirability of high unemployment insurance in the context of the COVID-19 crisis. First, it is very likely that UI recipients were very liquidity constrained, and that FPUC was hence crucial to sustain their consumption. Second, having lower employment was probably not very costly if not desirable, considering that employed workers were more at risk of getting infected with COVID-19 and infecting others.

Our paper contributes to the body of work analyzing the effects the CARES Act on employment. Several contemporaneous studies show no association between the extra $600 a week unemployment insurance (FPUC) and employment, either using the Homebase dataset of hourly employees in small businesses (Bartik et al., 2020; Altonji et al., 2020; Finamor and Scott, 2021) or the Census Household Pulse Survey (Dube, 2020). Relative to this literature, we provide two major contributions. First, our data allow us to improve the identification strategy. Measuring search effort and vacancy creation instead of employment, and at a very granular level allows us to control away variations that are due to

the COVID-19 crisis more convincingly. Second, we provide an explanation for the limited effect of FPUC on employment: there was a large effect on search behavior but a limited impact on employment due to congestion effects, i.e. the low level of labor market tightness.

We also contribute to the empirical literature on the impact of unemployment benefit levels on job search. The increase in the level of benefits during the COVID-19 recession is larger than most of what the literature has been looking at in the past (Schmieder and von Wachter, 2016). Our findings are consistent with the estimated effect of unemployment insurance on search effort in prior literature (Krueger and Mueller, 2010; Fradkin and Baker, 2017; Marinescu, 2017; Lichter and Schiprowski, 2020).

Last, our paper contributes more generally to the study of the labor market during COVID-19 (Bartik et al., 2020; Gupta et al., 2020; Cheng et al., 2020; Fairlie et al., 2020; Montenovo et al., 2020), focusing on job postings and applications. We add to work investigating trends in the applications and job postings during COVID in Sweden (Hensvik et al., 2020), and job postings in the US (Kahn et al., 2020; Campello et al., 2020; Forsythe et al., 2020).

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2 Data

We combine several datasets from Glassdoor, one of the world's largest jobs and career sites. We use job vacancy listings and job applications from Glassdoor's online jobs platform April 2018 to December 2020 for the U.S. We also exploit information on self-reported users' salaries to measure the replacement rates they could be eligible for under the various unemployment insurance programs.

2.1 Data on job listings and job applications

Job listings Glassdoor collects job openings from companies posting jobs directly to the site, crawls of company career sites, direct feeds from applicant tracking systems and from third-party job boards. Glassdoor then ties job listings data to specific industries from employer self-reports and third-party data, and groups them into canonical occupations that can be mapped into O*NET-SOC codes based on the job titles. In our analysis, we focus on new job listings posted each week rather than the total stock of active job listings. The flow of new job listings is more responsive to sharp changes in labor market conditions and policy and captures a disproportionate share of application activity.

Glassdoor's job listings capture a large portion of online U.S. job listings, which themselves comprise a substantial percentage of all job openings. But job listings at very small businesses or managed by unions that are typically not online may be underrepresented (Chamberlain and Zhao, 2019). We hence use weights in all our analysis, based on the

average number workers in the labor force in the industry, occupation and state from the Current Population Survey for 2018-2020. To test the validity of our data, we compare our measure of job listings from Glassdoor with the measure from JOLTS in 2018-2020 in Figure **??**. For both, we present the relative variation in the count of new job listing at the monthly level, relative to the average in January-February 2020. We see that the two measures move very closely, especially after 2019 (the correlation is 0.6 for 2019-2020). In particular, the magnitude of the decrease in job listings in April and May is the same in the two data sources.

Job applications To measure search effort, we take the count of applications, defined as a user starting an application by clicking on the "Apply Now" button. Because clicking on the link takes users to other websites for some job listings, we do not directly observe completed applications, but these actions should be a strong proxy for applying. Applications provide a proxy of search effort, under the assumption that the probability of applying on Glassdoor's website rather than use other search channels is relatively stable over time. We allocate applications into an industry and occupation category based on jobs characteristics, and into a state based on applicants' IP address.

We note that applications on Glassdoor don't come only from unemployment benefits recipients, therefore our estimates at the local labor market level also capture the search effort of job seekers who do not receive UI. This is the right measure for our purpose as it does not make a difference from the perspective of employers that applicants receive UI or not. But we note that, as job seekers who do not receive UI should react less to changes in the generosity unemployment insurance, these market level estimates should be smaller than estimates of the microeconomic impact of the CARES Act on the search of UI recipients.

2.2 Data on earnings and unemployment benefits

To study the role of UI generosity, we need to determine the "potential replacement rates" for job seekers in our sample, i.e. the replacement rates that those eligible for unemployment insurance would receive at each point in time. The replacement rate level in normal times depends on rules specific to each state. We use the benefits calculator created by Ganong et al. (2020), which computes unemployment benefits based on individualsâ state of residence and pre-displacement quarterly earnings based on UI guidance provided by the Department of Labor. Once the standard replacement rate is determined, we just need to add \$600 lump-sum payments to standard weekly benefits levels to compute the replacement rate with the FPUC.

As explained in Ganong et al. (2020), we need two key pieces of information to compute the standard weekly benefits level: the average weekly earnings before unemployment and the number of day worked for each quarter in the year before unemployment. In our

preferred wage measure, we use earnings self-reported by Glassdoor users. Although it is not required to search for jobs on the platform, some Glassdoor users report information on their prior jobs, including the job title, employer, location and year associated with their wage. The wage information is very detailed, and includes base salary and additional compensation like tips and cash bonuses. This data hence provides rich granular information on earnings for a population relatively close to the overall sample of applicants on Glassdoor's search platform.[1] We take the average prior earnings for each state-occupation, using a very dis-aggregated occupation definition (O*Net 6-digit codes, 416 categories).[2] The second piece of information we need to infer workers' potential replacement rate is the number of days worked in each quarter of the year before unemployment. Based on the average number of days worked reported in the 2019 Annual Social and Economic Supplement (ASEC) of the CPS, most individuals eligible for unemployment insurance worked continuously during the four quarters preceding unemployment. Therefore, we assume that individuals worked continuously during the four quarters preceding unemployment.

2.3 Panel at the week and local labor market level

We arrange our data as a panel, at the calendar week and local labor market level. We define a local labor market as the interaction of state, 2-digit occupations, 2-digit industries and wage deciles. We keep local labor markets for which we observe at least 1000 applications in 2018-2020, such that we are able to detect variation in weekly applications count. As mentioned in section 2.1, to ensure the external validity of our analysis, we re-weigh observations such that each industry×state×occupation in our sample reflects the share of that industry×state×occupation in the labor force in the Current Population Survey for 2018-2020. We present descriptive statistics for the main variables in Table A.1.

3 Institutional background and empirical strategy

3.1 Institutional background

The Federal Pandemic Unemployment Compensation (FPUC) The CARES Act was signed into law on March 27 2020. It included a number of measures, including a boost to unemployment insurance benefits, amounting to a budget item of 260 billion dollars. The Federal Pandemic Unemployment Compensation (FPUC) was an additional $600 a week of unemployment benefits for all workers eligible to unemployment insurance. The FPUC was also available to workers who are not normally eligible for benefits, but

[1] Karabarbounis and Pinto (2018) show that the average and variance of the distribution of salaries within geographical region and within industry are fairly representative when compared against the Quarterly Census of Employment and Wages and Panel Study of Income Dynamics.

[2] We do not use industry for prior earnings because the cells become very small and most of the variation in wages is at the occupation rather than the industry level (Marinescu and Wolthoff, 2019).

were eligible for Pandemic Emergency Unemployment Compensation (PEUC) benefits or Pandemic Unemployment Assistance (PUA) benefits. PEUC provided an additional 13 weeks of benefits for those whose regular unemployment benefits have been exhausted. PUA provided payment to workers not traditionally eligible for unemployment benefits (self-employed, independent contractors, workers with limited work history, and others) who were unable to work as a direct result of the coronavirus public health emergency.

The $600 FPUC amount was chosen to raise the UI replacement rate to around 100% for the average U.S. worker. Because of technical issues in state unemployment insurance systems, it was not possible to tailor FPUC to each worker's prior earnings. The increase in UI replacement rates associated with FPUC hence varied widely for two reasons. First, a fixed $600 increase in benefits generated a larger increase in replacement rate for occupations with lower wages. Second, there were differences in the generosity of statesâ pre-existing unemployment insurance benefit formulas. The date when the CARES Act was voted, on March 27, corresponds to the moment when the amount of additional benefits that would be provided to help unemployed workers during the pandemic became known. From that date, unemployed workers could hence anticipate their upcoming replacement rate, and potentially adjust their behavior accordingly. But the actual payments only started in the following weeks (Bartik et al., 2020). The last week that FPUC was paid was the week of July 20, 2020.[3]

Labor market shocks during the COVID-19 pandemic The enactment of FPUC coincided with an exceptionally large and abrupt negative labor market shock, which any analysis of FPUC must account for. In March, employment decreased precipitously and unemployment skyrocketed (appendix Figure A.1). This shock was due to changes in economic activities in response to the pandemic—either spontaneous or imposed by lockdown measures. Importantly, this negative labor market shock disproportionately hit low-paying jobs, and hence workers who experienced the largest increase in their potential replacement rate due to FPUC. Therefore, failing to account for this shock would likely bias estimates of the causal effect of FPUC on aggregate labor market outcomes such as employment or labor market tightness.

3.2 Empirical strategy

Although the CARES Act increased the generosity of unemployment benefits for all workers at the same time (up to some minor delays in implementation), there were large disparities in the magnitude of this increase. For each local labor market, we compute a measure of the relative increase in the benefits associated with FPUC: $ln(\frac{UI_{FPUC}}{UI_{Standard}})_{s,o,i,w}$. We estimate

[3]According to the U.S. Department of Labor, "FPUC is not payable for any week of unemployment ending after July 31, 2020". In all states the week of unemployment ends on a Saturday or a Sunday, so the last week of FPUC was the week of July 20.

the association between the evolution of outcome variables relative to the week before the CARES Act and the increase in benefits generosity. We first explain the approaches we take to ensure that the estimates we obtain can be interpreted as the causal effect of FPUC, and then present our empirical models.

Identification strategy We can identify the effect of the increase of FPUC under the assumption that, in the absence of a change in unemployment insurance, the outcome variable would have evolved similarly in local labor markets with different increases in UI. The identification of the effect of FPUC on the outcome variables is threatened by any factor that affects the outcomes around the date of the CARES Act and correlate with the size of the increase in generosity in UI. These confounding factors are a major concern as the COVID-19 epidemic caused large job destruction around that time, and this destruction correlated with worker's wages. We address this identification challenge in several ways.

First, we control for various observable variables to capture the effect of the COVID-19 crisis. As the COVID-19 crisis hit different states and industries in different ways, we successively include time×state fixed effects, and time×industry fixed effects. We then go one step further and directly detect empirically the local labor markets that were hit the most by the COVID-19 crisis, to allow for different trends in these markets: we predict the probability of becoming unemployed *prior to the CARES Act*. We use the monthly CPS for January to March 2020, matched with the Outgoing Rotation Group 2019 (ORG) (Flood et al. (2020)). We predict the probability of being unemployed in a logit model including industry fixed effects, the interactions of prior wage and industry fixed effects, and state fixed effects. Consistent with descriptive statistics presented in Figure A.1, we find that there is a strong negative relationship between prior earnings and the risk of unemployment (coefficients are reported in Table A.2). We use the estimated coefficients from the logit regression of unemployment of the interaction of wage deciles and industries fixed effects to predict the probability of being unemployed in our study sample. We use the deciles of this predicted probability to proxy for the exposure to the impact of the COVID-19 crisis on each local labor market. We include time×Exposure decile fixed effects to control away trends associated with the impact of the COVID-19 crisis, rather than the effect of FPUC.

Second, we use this analysis of the correlation between prior earnings and the risk of unemployment in different sectors, to focus on sectors where identification problems appear less severe. In Table A.2, we see that this correlation is small and insignificant in the Construction sector (in the Mining sector as well, but there is virtually no applications for this sector on Glassdoor). We hence implement a case study in the Construction sector.[4]

[4]There is substantial variation in earnings between the most frequent occupations in the construction sector. Prior weekly earnings in CPS-ORG range around $1410 for property, real estate, and community association managers ; $1020 for electricians, construction equipment operators ; $960 for construction laborers, carpenters and masons.

Compared to the first approach, the advantage of this case study is that we don't impose any functional form to the confounding effect of the COVID-19 crisis on outcomes. However, it restricts the sample drastically, and we lose in precision and external validity. As the effect of the FPUC might not be completely homogeneous across sectors, we should not expect the estimates to be exactly identical, but it is a useful robustness check for our qualitative conclusions.

Third, for all specifications, we test the plausibility of our identifying assumption by analyzing the correlation between the evolution of outcomes before the CARES Act, and the magnitude of the benefits increase induced by FPUC. If our identification strategy is valid, our pre-CARES ACt estimates should not be significant. This is equivalent to the test of the parallel trend assumption in a differences-in-differences setup. We will show that when we implement the various strategies to neutralize potential confounding factors, these estimates are close to zero and insignificant. Our strategies hence seem appropriate to identify the effect of FPUC.

Estimation models We estimate the following model:

$$ln(Y_{t,s,o,i,w}) = \sum_{\tau \neq -1} \beta_\tau \cdot \mathbb{1}[t = FPUC + \tau] \cdot ln(\frac{UI_{FPUC}}{UI_{Standard}})_{s,o,i,w} + \delta_{s,o,i,w} + \lambda_{week(t),s,o,i,w} + \rho_{s,t} + \phi_{i,t} + \psi_{t,Intensity(s,i)} + \varepsilon_{t,s,o,i,w} \quad (1)$$

$ln(Y_{t,s,o,i,w})$ denotes the logged outcome variable in calendar week t, state s, occupation o, industry i and wage decile w. The term $\mathbb{1}[t = FPUC + \tau]$ indicates that the calendar week t is τ weeks before the start of FPUC (for $\tau < 0$), or after (for $\tau \geq 0$). The β_τ are the coefficients of interest: for $\tau < 0$, they help test the identifying assumption. For $\tau \geq 0$, the coefficients represent the effect of the relative increase in weekly benefits induced by FPUC on the outcome variable if the identification strategy is valid, and can be interpreted as an elasticity. $\delta_{s,o,i,w}$ are local labor market fixed effects, $\varepsilon_{t,s,o,i,w}$ is the error term.

Our favorite specification includes various other control variables. We include week-of-year×local labor market fixed effect, $\lambda_{week(t),s,o,i,w}$, to control away seasonal variations. To isolate the effect of FPUC form the effect of the COVID-19 crisis (and potentially other confounders), we let time t fixed effects vary by state ($\rho_{s,t}$), industry ($\phi_{i,t}$) and Exposure decile ($\psi_{t,Intensity(s,i)}$).

We present coefficients β_τ week by week graphically. In tables, we present the coefficients associated with the whole period between the vote of FPUC and its last week of implementation ($\mathbb{1}[t \in$FPUC period]). It is equivalent to estimating the elasticity in a regression of the logged outcome on the logged replacement rate, with similar controls. We systematically estimate coefficients for the interaction of benefits increases and all the periods covered by our data except for the week before the vote of FPUC, such that the

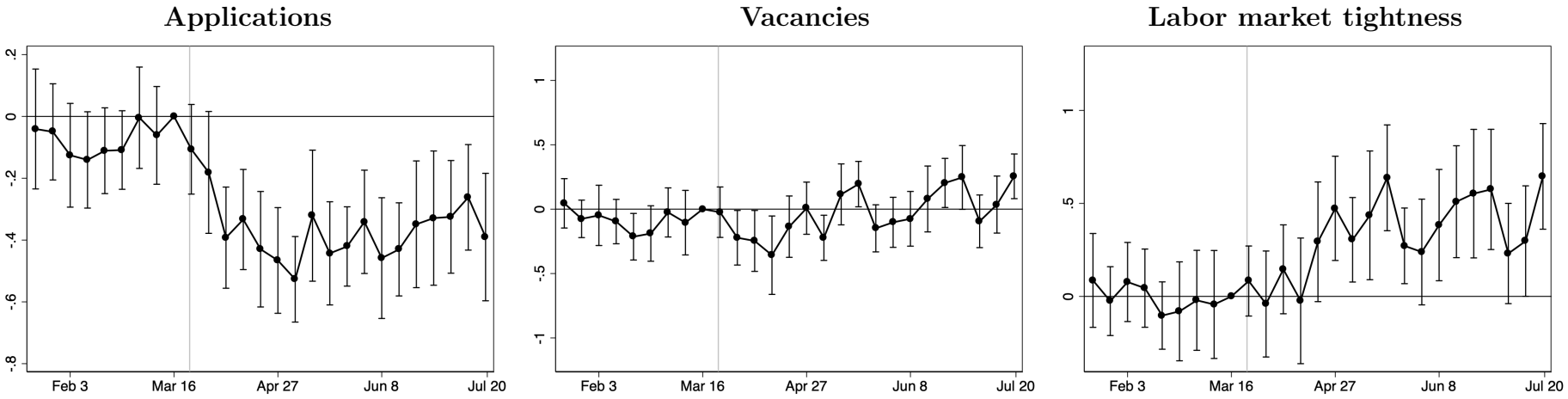
week before the vote of FPUC always serves as the reference period. We cluster standard errors at the state level.

4 The impact of FPUC on the labor market

4.1 Main results

Pre-FPUC trends Figure 1 presents the correlation between various labor market outcomes and the increase in UI generosity induced by FPUC, week by week. We present our favorite specification, including all the controls for potential confounding factors: time×state fixed effects, time×industry fixed effects and time×Exposure decile fixed effects. In weeks prior to the adoption of FPUC, there is no significant correlation between applications and *future* benefits increases. This supports the credibility of our identifying assumption that there would be no difference in the evolution of applications across markets with different increases in benefits in the absence of FPUC, conditional on the control variables included.

Figure 1: The impact of FPUC on applications, vacancies and tightness, week by week



Notes: This Figure reports the estimates from regressions of the logged count of applications, the logged count of vacancies, and the logged ratio of vacancies over applications on the interaction of each week around the enactment of the FPUC and the potential increase in UI generosity $ln(\frac{UI_{FPUC}}{UI_{Standard}})$: the coefficients before the enactment of the FPUC help test our identification assumption; the coefficients after represent the elasticity of search with respect to benefits levels. We use a panel at the calendar week and local labor market level—where local labor markets are defined as as the interaction of state, 2-digit occupations and 2-digit industries and wage deciles. We include calendar week and local labor market fixed effects. We control for seasonality using week-of-year×local labor market fixed effects. This Figure presents our favorite specification, with the full set of controls for potential confounders: time×state fixed effects, time×industry fixed effects and time×Exposure deciles fixed effects. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS. Thin lines denote 95% confidence intervals, based on robust SE clustered at the state level.

Applications Immediately after the passage of the CARES Act, which establishes FPUC, we see a significant decrease in applications, which stabilizes after three weeks and until FPUC expires (Figure 1). The evolution of search effort after FPUC is consistent with the theoretical prediction that a higher benefit replacement rate decreases search effort.

Vacancies In theory, higher unemployment benefits could decrease job creation and thereby decrease tightness—if the "job creation" channel dominates", or leave job creation unchanged and increase tightness—if the "rat race" mechanism dominates (Landais et al., 2018). Prior literature found that the number of vacancies was unaffected by the increase in potential benefit duration during the Great Recession (Marinescu, 2017). In our data, there is no effect of FPUC on job vacancies[5] (Figure 1).

Labor market tightness Since FPUC decrease applications and had no effect on vacancies, it increases labor market tightness (Figure 1, third panel).

Elasticity Table 1 presents the corresponding elasticities. The elasticity of applications with respect to the benefit replacement rate is -0.361 , (Table 1, col. 1): a 10% increase in the replacement rate leads to a 3.61% decline in job applications. The elasticity of vacancies with respect to the replacement rate is close to zero and not statistically significant. The elasticity of tightness (vacancies/applications) is therefore close to the opposite of the elasticity of applications: a 10% increase in the benefit replacement rate leads to a 3.34% increase in labor market tightness.

4.2 Robustness checks

Other specifications To illustrate the role of the controls we include, we present the estimates when we add them one by one for applications in Figure A.3, for vacancies in Figure A.4 and for applications in Figure A.5. Controlling for seasonality using labor market by week of the year fixed effects is particularly important for applications: without them, it looks like the effect of FPUC is transitory. Controlling for industry by week fixed effects is particularly important for vacancies: without them, it looks like the effect of FPUC on vacancies is positive. This suggests that industries were differentially affected by the COVID-19 crisis, and not accounting for them would bias the estimate of the impact of FPUC.

Alternative identification strategy To confirm our main results, we use an alternative identification strategy: we focus on the construction sector, where job loss before FPUC was not systematically correlated with earnings. While the estimates are naturally less

[5]FPUC seems to decrease the number of vacancies immediately after it was voted, in week 4, but this effect is transitory.

Table 1: Elasticity of search with respect to benefits levels

	Applications (ln)	Vacancies (ln)	Tightness (ln)
	(1)	(2)	(3)
Replacement rate (ln)	-0.361***	-0.027	0.334***
	(0.071)	(0.069)	(0.106)
Exposure decile×Time FE	✓	✓	✓
Industry×Time FE	✓	✓	✓
State×Time FE	✓	✓	✓
Labor market×Week of year FE	✓	✓	✓
Labor market FE	✓	✓	✓
No. of Obs	1,523,252	1,523,252	1,523,252

Notes: This Table reports the estimates from the regression of logged labor market outcomes on the logged replacement rate, in a panel at the calendar week and local labor market level—where local labor markets are defined as as the interaction of state, 2-digit occupations and 2-digit industries and wage deciles. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS. Robust standard errors clustered at the state level are in parenthesis (* p< 0.10, ** p< 0.05, *** p< 0.010).

precise than in the full data, we see very similar qualitative patterns: FPUC decreased applications, had no effect on vacancies, and therefore ultimately increased labor market tightness (Figure A.6). These results are reassuring: our key results hold in a segment of the economy where there are no substantial factors confounding the identification of the effect of FPUC.

5 Labor market tightness during the FPUC

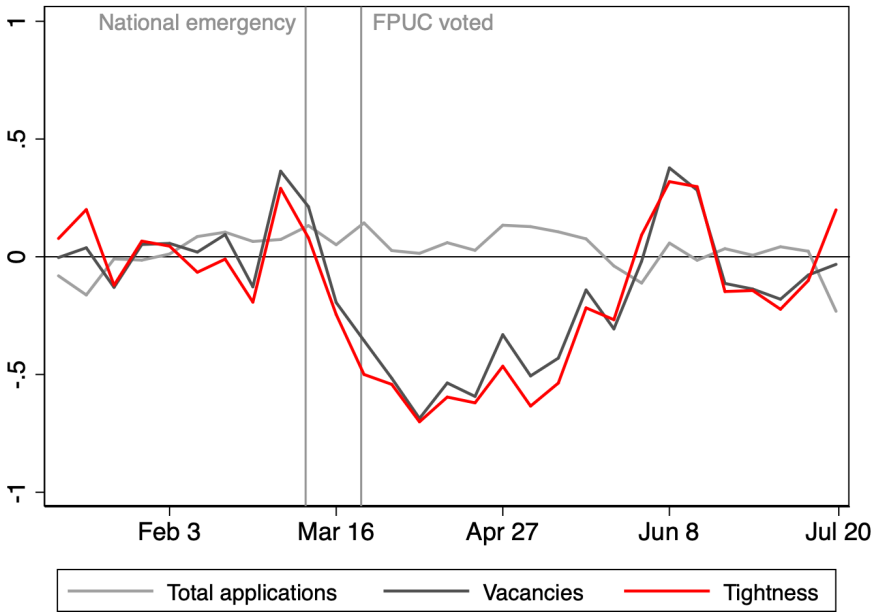
What was the labor market situation when FPUC was introduced? This is important to understand the role of FPUC: we have shown that FPUC decreases applications, but was this from a low or a high level of applications? To appreciate whether applications are low or high, we have to compare them to the number of job vacancies. Thus, we document the evolution of the market-level number of applications and new vacancy postings during the COVID-19 crisis. This allows us to describe the evolution of labor market tightness (vacancies/applications), which determines workers' ability to find jobs, and employers' ability to fill their vacancies.

5.1 The evolution of labor market tightness relative to pre-covid

We present the evolution of seasonally adjusted applications, new vacancy postings, and labor market tightness relative to their baseline levels in Jan-Feb 2020 in Figure 2. Despite the negative effect of FPUC on job applications, there is no decline in aggregate job applications. In the non seasonally adjusted series (Figure A.7, left panel), applications decline

a bit after FPUC, but this pattern occurs every year (appendix Figure A.8). In contrast, the number of vacancies strongly declined both before and right after FPUC. Since applications stayed constant, labor market tightness (vacancies/applications) declined in lock step with vacancies.

Figure 2: Changes in applications, vacancies, and labor market tightness relative to Jan-Feb 2020 (seasonally adjusted)



Notes: The Figure presents the changes in the total weekly count of applications, the weekly count of new posted vacancies, and labor market tightness (ratio of vacancies over total applications), relative to their baseline levels in Jan-Feb 2020. We regress the logged variables on the time coefficients, and then subtract the average of the estimates obtained for the period Jan-Feb 2020, such that each coefficients represents the relative variation with respect to the average level in Jan-Feb 2020. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS.

Quantitatively, seasonally adjusted labor market tightness declined by 30.6% during the FPUC period relative to January-February 2020 (Table 2, col. 3, upper panel), reflecting a labor market more favorable to recruiting firms, despite the large increase in unemployment benefits. Applications slightly increased (col. 1, upper panel) during the FPUC period, while vacancies declined by 26.2% (col. 2). During the reopening phase, in June-July 2020, seasonally adjusted labor market tightness was similar to its level in Jan.-Feb. 2020 (Table 2, col. 3, lower panel). Applications (col. 1) and vacancies (col. 2) were also roughly at the same level as in Jan.-Feb. 2020.

Table 2: Changes in labor market tightness during the period of the FPUC, relative to Jan-Feb 2020

Sample:	All			Potential increase in UI Q1	Q2	Q3	Q4
Outcome:	Total Applications	Vacancies	Tightness	Tightness	Tightness	Tightness	Tightness
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FPUC period	0.044**	-0.262***	-0.306***	-0.413***	-0.334***	-0.287***	-0.190***
	(0.018)	(0.022)	(0.021)	(0.023)	(0.024)	(0.046)	(0.032)
Week of year FE	✓	✓	✓	✓	✓	✓	✓
No. of Obs	1,540,598	1,540,598	1,540,598	613,586	412,050	297,346	217,616
FPUC, Mar-May	0.076***	-0.448***	-0.524***	-0.547***	-0.558***	-0.547***	-0.444***
	(0.021)	(0.023)	(0.021)	(0.023)	(0.021)	(0.048)	(0.035)
FPUC, Jun-Jul	0.004	-0.030	-0.034	-0.246***	-0.055	0.039	0.126***
	(0.019)	(0.022)	(0.025)	(0.028)	(0.040)	(0.054)	(0.037)
Week of year FE	✓	✓	✓	✓	✓	✓	✓
No. of Obs	1,540,598	1,540,598	1,540,598	613,586	412,050	297,346	217,616

Notes: This Table shows the levels of the total weekly count of applications, the weekly count of new posted vacancies, and labor market tightness during the period of the FPUC, relative to their baseline levels in Jan-Feb 2020. In the upper panel, we present levels during the total period of FPUC, while in the lower panel, we divide the FPUC period in two sub-periods. The estimates are obtained by regressing each logged outcome on a dummy for the FPUC period, and a dummy for all calendar weeks outside of the baseline period of January-February 2020 (such that the calendar weeks during January-February 2020 are the omitted) in a panel at the calendar week and local labor market level, including week-of-year fixed effects to control for seasonal variation. The estimates are obtained in the full sample in col (1)-(3), and separately for local labor markets in each quartile of the distribution of potential UI increase in col (4)-(7). We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS. Robust standard errors clustered at the state level are in parenthesis (* p< 0.10, ** p< 0.05, *** p< 0.010).

5.2 Heterogeneity

Even though seasonally labor market tightness remained relatively depressed throughout the FPUC period, one could be concerned that recruitment difficulties increased in the local labor markets most affected by FPUC. However, even in the markets most affected by FPUC (4th quarter of the increase in replacement rate due to FPUC), tightness during FPUC was 19% lower than in January-February 2020 (Table **??**, col. 7, upper panel). During the re-opening phase in June-July 2020, tightness was higher than during the depth of the COVID crisis in March-May 2020, and markets with a higher increase in the replacement rate (higher quartiles) saw a stronger rebound in tightness in June-July 2020 (Table 2, lower panel col 4-7, and Figure A.9). For the fourth quartile, which saw the highest increase in the replacement rate, seasonally adjusted tightness in June-July 2020

was slightly higher (12.6%) than it was at the beginning of 2020 (Table 2, lower panel, col. 8). For the three lower quartiles (col. 4-6), tightness during reopening remains at or below the levels reached in January-February 2020.

Finally, Figure A.10 and Figure A.11 show that the evolution of applications, vacancies and labor market tightness are qualitatively similar across sectors. We find that tightness tends to decline at the beginning of the FPUC period, and climbs somewhat in June-July 2020. The magnitudes of the fluctuations differ by sector in expected ways: for example, during the lockdown, vacancies in sectors that were directly affected like leisure & hospitality and transport & logistics (appendix Figure A.11) understandably decreased more than in other sectors. In most sectors, the decline in tightness was essentially the same as the decline in vacancies, but in leisure & hospitality, tightness declined less than vacancies because applications also declined during the FPUC period (appendix Figure A.11). Workers in this sector might have decreased their search more due to infection risk. In construction, which we analyzed above, the evolution of tightness is similar to that in other sectors: even though FPUC clearly increased tightness, tightness during the FPUC period was mostly below tightness in January-February 2020. This indicates that, in the absence of FPUC, tightness in the construction sector would have been significantly above its pre-crisis level, leading to a rat race for jobs.

6 Discussion

We discuss how our results contribute to the welfare analysis of FPUC.

6.1 The impact of FPUC on welfare

Our results speak to aspects of the impact of FPUC on welfare that have to to with labor market tightness. Landais et al. (2018) formalize that the welfare impact of unemployment insurance depends on three elements: first, its impact on labor market tightness; second, the impact of tightness on welfare, and third, the trade-off between consumption smoothing and moral hazard. Our paper provides evidence on the first two. First, our results show a positive effect of FPUC on labor market tightness. Second, we have shown that labor market tightness was particularly low when FPUC was implemented. This means that tightness would have been even lower in the absence of FPUC. Landais et al. (2018) show that it is efficient to increase tightness when it is particularly low: it strongly decreases the cost of unemployment to job seekers without a commensurate increase in recruitment difficulties for employers. Overall, our results hence indicate that providing more generous unemployment insurance was desirable when FPUC was implemented, as it contributed to bringing labor market tightness back towards a more efficient level.

While our analysis focused on labor market tightness, one should also consider other

important elements to determine the welfare impact of FPUC. First, it is very likely that UI recipients were liquidity constrained during the crisis, and that FPUC was hence crucial to sustain their consumption. Second, during the COVID-19 crisis, the sanitary situation made unemployment much more desirable than in normal times: to the extent that unemployed workers were less likely to get infected with COVID-19 and infect others, unemployment was associated with welfare gains (Fang et al., 2020). Overall, these elements all point towards the desirability of high unemployment insurance benefits during this crisis.

7 Conclusion

During the COVID-19 crisis, a 10% increase in unemployment benefits due to the Federal Unemployment Pandemic Assistance (FPUC) led to a 3.6% decline in Glassdoor job applications. Since the FPUC had no effect on vacancies, its effect on applications led to a commensurate 3.3% increase in labor market tightness (vacancies/applications). However, despite the negative effect of FPUC on applications, seasonally adjusted aggregate job applications remained stable during the FPUC period. With a 4% increase in aggregate applications and a steep 26% decline in job vacancies relative to January-February 2020, seasonally adjusted labor market tightness declined by 31% during the FPUC period. Even in the top 25% of labor markets with the highest increase in the level of unemployment benefits, tightness still declined by 19%. By decreasing applications, FPUC has thus contributed to lifting up a depressed labor market tightness, likely increasing welfare. Our results can help explain why higher unemployment benefits had no effect on employment after the CARES Act (Bartik et al., 2020; Altonji et al., 2020; Dube, 2020). In a context of excessive competition for jobs among workers, more generous unemployment insurance reduces wasteful applications and has little effect on employment.

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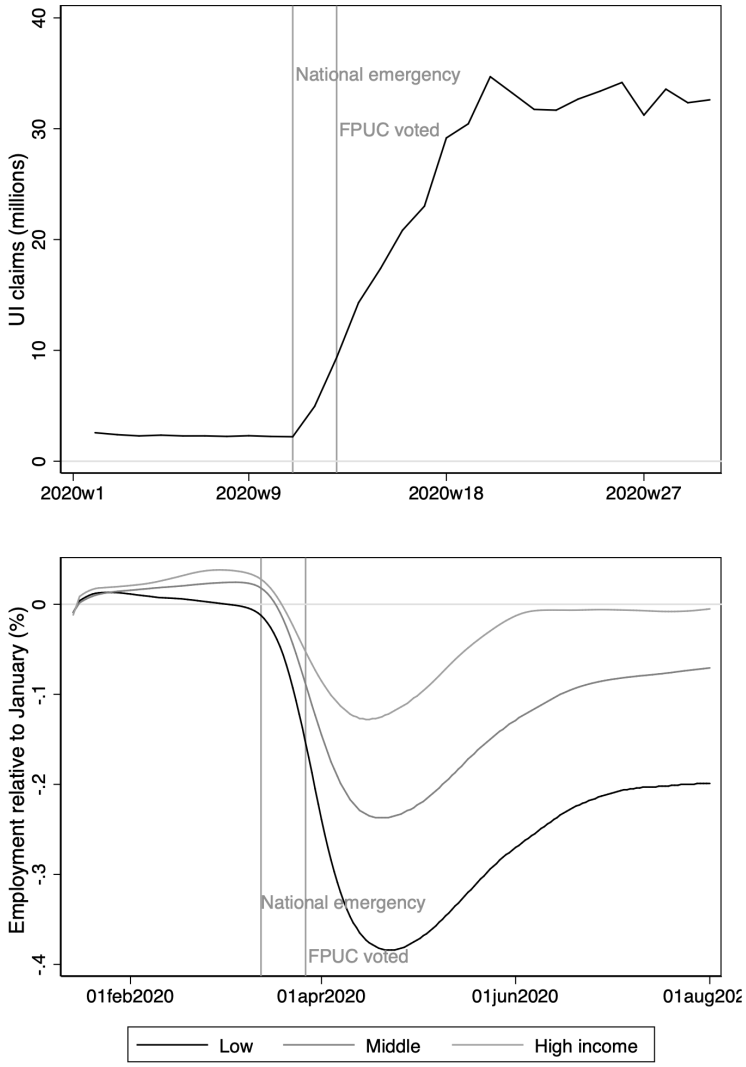
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Appendix

Figure A.1: Descriptive statistics on the state of the labor market when FPUC was voted



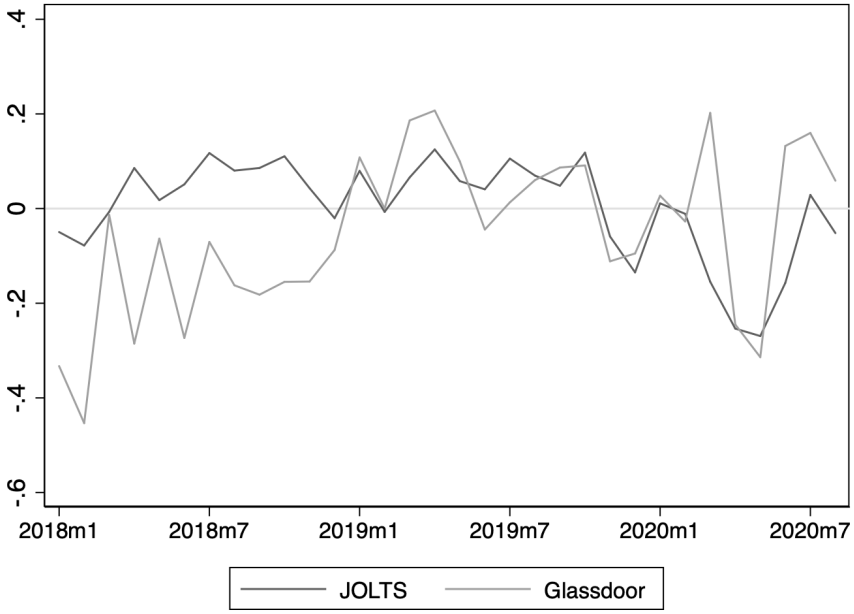
Notes: This Figure shows the evolution of the number of UI claims (new and continuing) in the left panel. In the right panel, it shows the evolution of employment rates among workers at different part of the wage distribution (low wage, middle wage or high wage), relative to their levels in January 2020. The data are described in Chetty et al. (2020), and made publicly available on the website $https://tracktherecovery.org$.

Table A.1: Descriptive statistics in our study sample

	Mean	SD	p25	p50	p75
	(1)	(2)	(3)	(4)	(5)
Count of applications, each week in each local labor market	82.2	204.3	12.0	26.0	70.0
Count of vacancies, each week in each local labor market	189.6	659.1	16.0	44.0	126.0
Tightness (vacancies / applications)	4.7	22.6	0.7	1.4	3.4
Replacement rate without FPUC	43.6	8.4	38.2	45.4	49.2
Replacement rate with FPUC	146.1	47.9	107.3	143.5	173.8

Notes: This Table presents descriptive statistics in our study sample. All variables are computed for each calendar week from January 2018 to July 2020, in each local labor market. We use weights reflecting the size of each State×industry×occupation in the labor force in the CPS.

Figure A.2: Comparison of the measure of new vacancies from JOLTS and Glassdoor



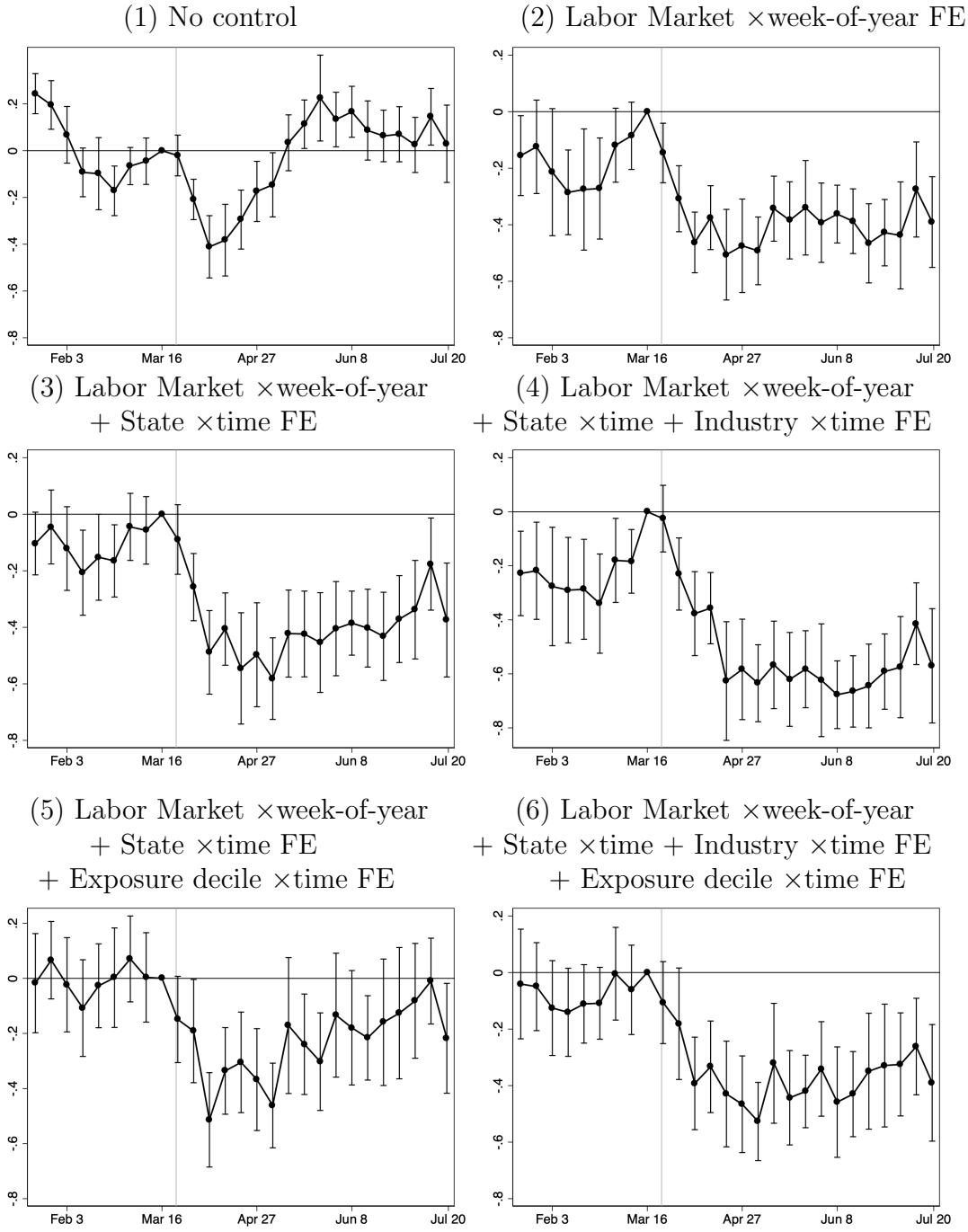
*Notes:*The Figure presents the variation in the count of new job listing at the monthly level, relative to the average level in January-February 2020. We compare the measure of new vacancies from JOLTS, to the measure from Glassdoor data—which we use in the rest of the paper. For the measure from Glassdoor, we present the weighted sum of job listings, using weights reflecting the size of each State×industry×occupation in the labor force in the CPS.

Table A.2: The probability of unemployment in January-March 2020 depending on past earnings, by industry

	Pba Unemployed		
	(1)	(2)	(3)
ln(prior earnings)	-0.372***		
	(0.024)		
Agriculture × ln(prior earnings)		-0.525***	-0.541***
		(0.198)	(0.208)
Mining × ln(prior earnings)		-0.061	-0.047
		(0.393)	(0.395)
Construction × ln(prior earnings)		-0.096	-0.097
		(0.088)	(0.089)
Manufacturing × ln(prior earnings)		-0.531***	-0.553***
		(0.097)	(0.096)
Retail × ln(prior earnings)		-0.433***	-0.449***
		(0.081)	(0.081)
Transportation × ln(prior earnings)		-0.521***	-0.539***
		(0.111)	(0.110)
Information × ln(prior earnings)		-0.248*	-0.236*
		(0.130)	(0.134)
Finance × ln(prior earnings)		-0.266***	-0.277***
		(0.057)	(0.059)
Business services × ln(prior earnings)		-0.388***	-0.403***
		(0.066)	(0.067)
Education & health × ln(prior earnings)		-0.360***	-0.370***
		(0.043)	(0.045)
Leisure & hospitality × ln(prior earnings)		-0.262***	-0.264***
		(0.087)	(0.088)
Consumer services × ln(prior earnings)		-0.657***	-0.663***
		(0.132)	(0.137)
Government × ln(prior earnings)		-0.603***	-0.575***
		(0.136)	(0.137)
Industry FE	-	✓	✓
State FE	-	-	✓
No. of Obs	54,023	54,017	54,017

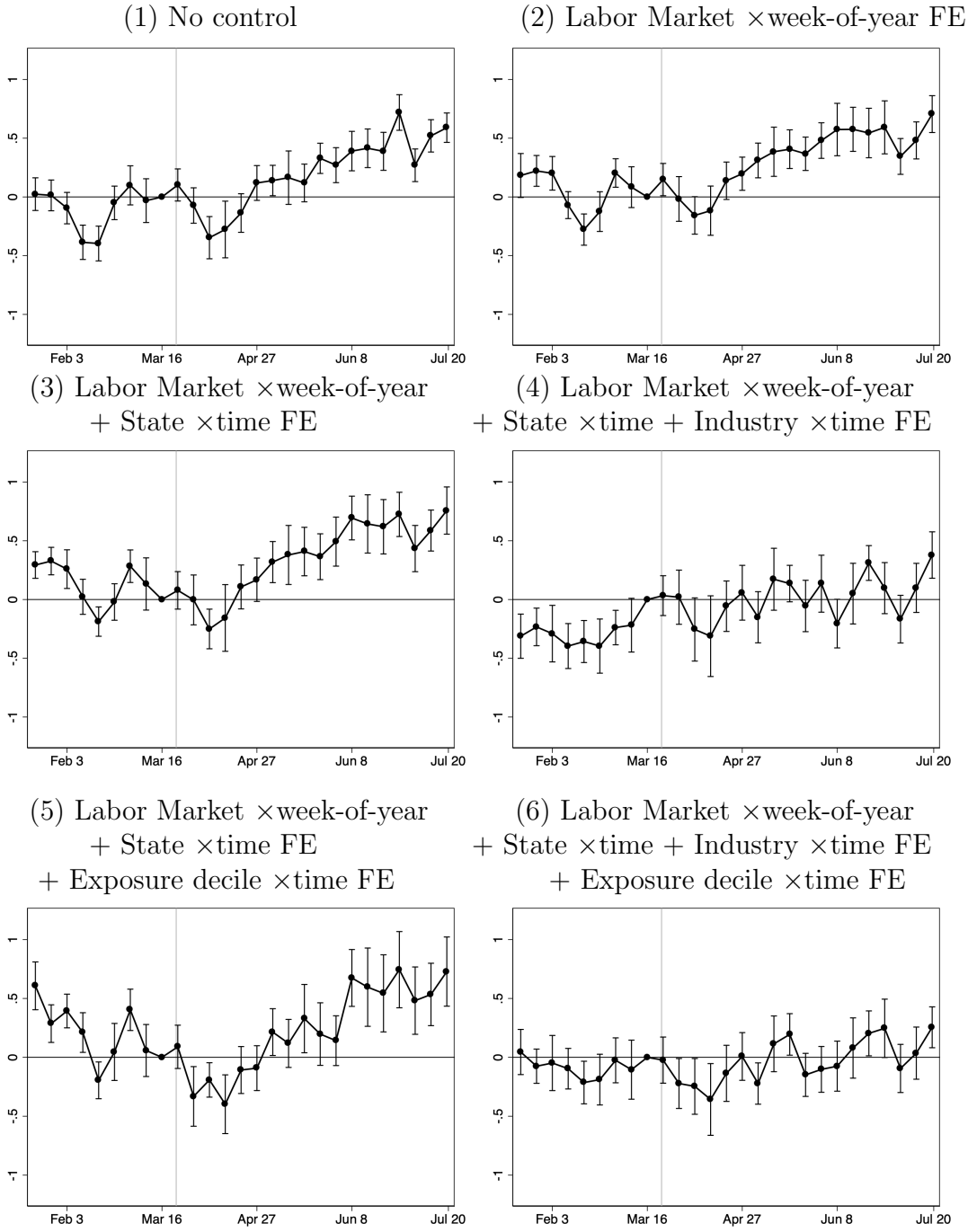
Notes: This table reports the estimates of the regression of a dummy for unemployment on prior earnings, two-digit industries and states, in a logit model. The variables on labor market status come from the monthly CPS data for January-March 2020, and the variables on prior earnings come from the Outgoing Rotation Group data for 2019. The two datasets are matched at the individual level. We use individual survey weights. Robust standard errors in parenthesis (* p< 0.10, ** p< 0.05, *** p< 0.010).

Figure A.3: The impact of FPUC on applications week by week



Notes: This Figure reports the estimates from regressions of logged count of applications on the interaction of each week around the enactment of the FPUC and the potential increase in UI generosity $ln(\frac{UI_{FPUC}}{UI_{Standard}})$. We use a panel at the calendar week and local labor market level, and all models include calendar week and local labor market fixed effects. We also add various controls in different panels. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS. Thin lines denote 95% confidence intervals, based on robust SE clustered at the state level.

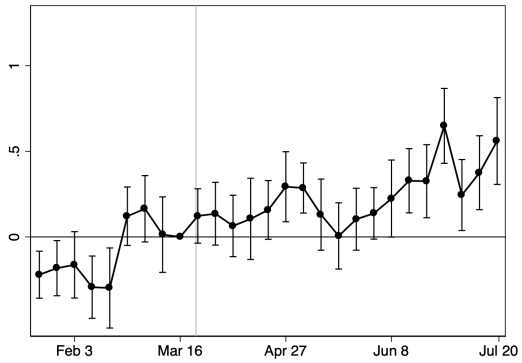
Figure A.4: The impact of FPUC on new vacancies week by week



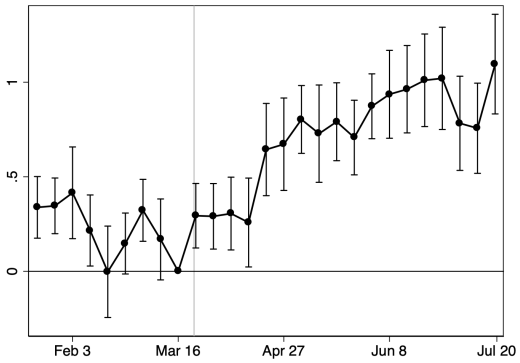
Notes: This Figure reports the estimates from regressions of logged count of new vacancies on the interaction of each week around the enactment of the FPUC and the potential increase in UI generosity $ln(\frac{UI_{FPUC}}{UI_{Standard}})$. We use a panel at the calendar week and local labor market level, and all models include calendar week and local labor market fixed effects. We also add various controls in different panels. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS. Thin lines denote 95% confidence intervals, based on robust SE clustered at the state level.

Figure A.5: The impact of FPUC on labor market tightness, week by week

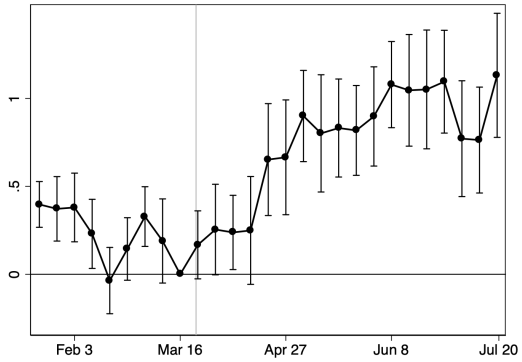
(1) No control



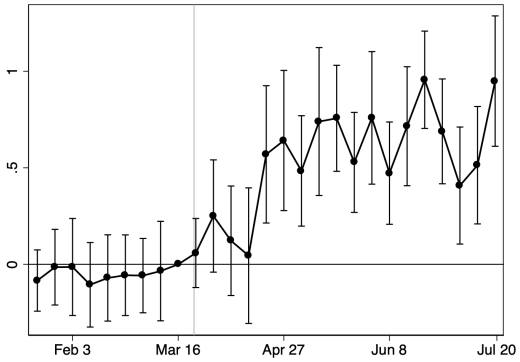
(2) Labor Market ×week-of-year FE



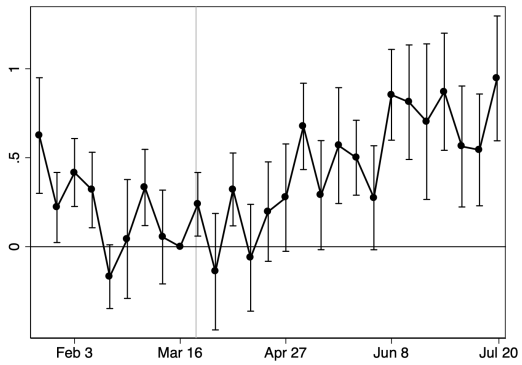
(3) Labor Market ×week-of-year + State ×time FE



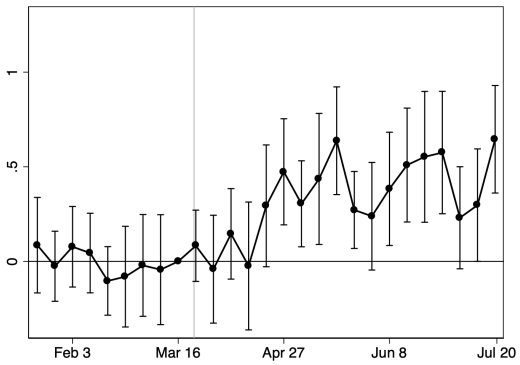
(4) Labor Market ×week-of-year + State ×time + Industry ×time FE



(5) Labor Market ×week-of-year + State ×time FE + Exposure decile ×time FE

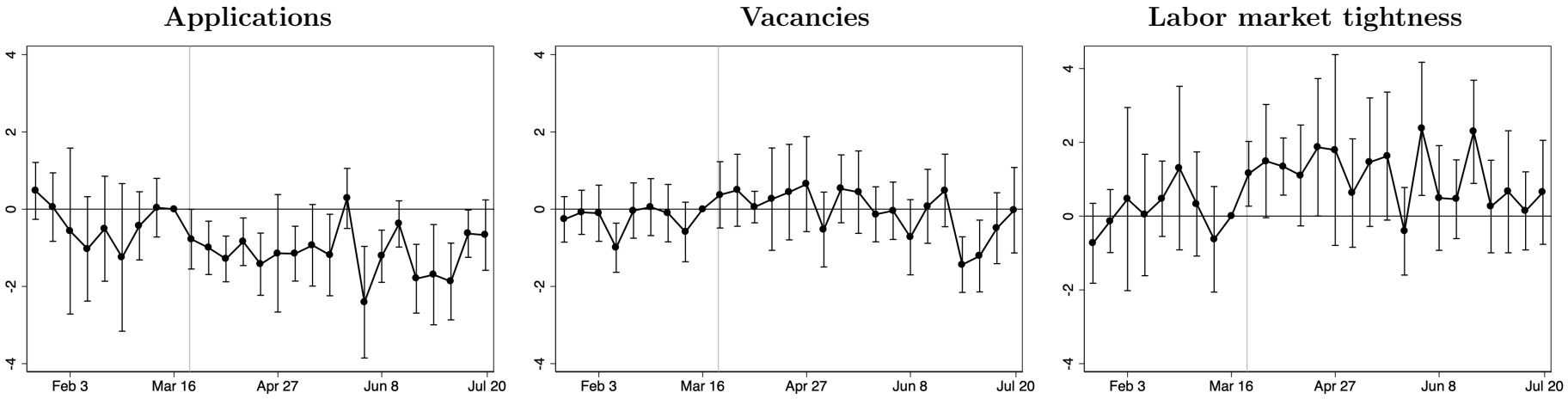


(6) Labor Market ×week-of-year + State ×time + Industry ×time FE + Exposure decile ×time FE



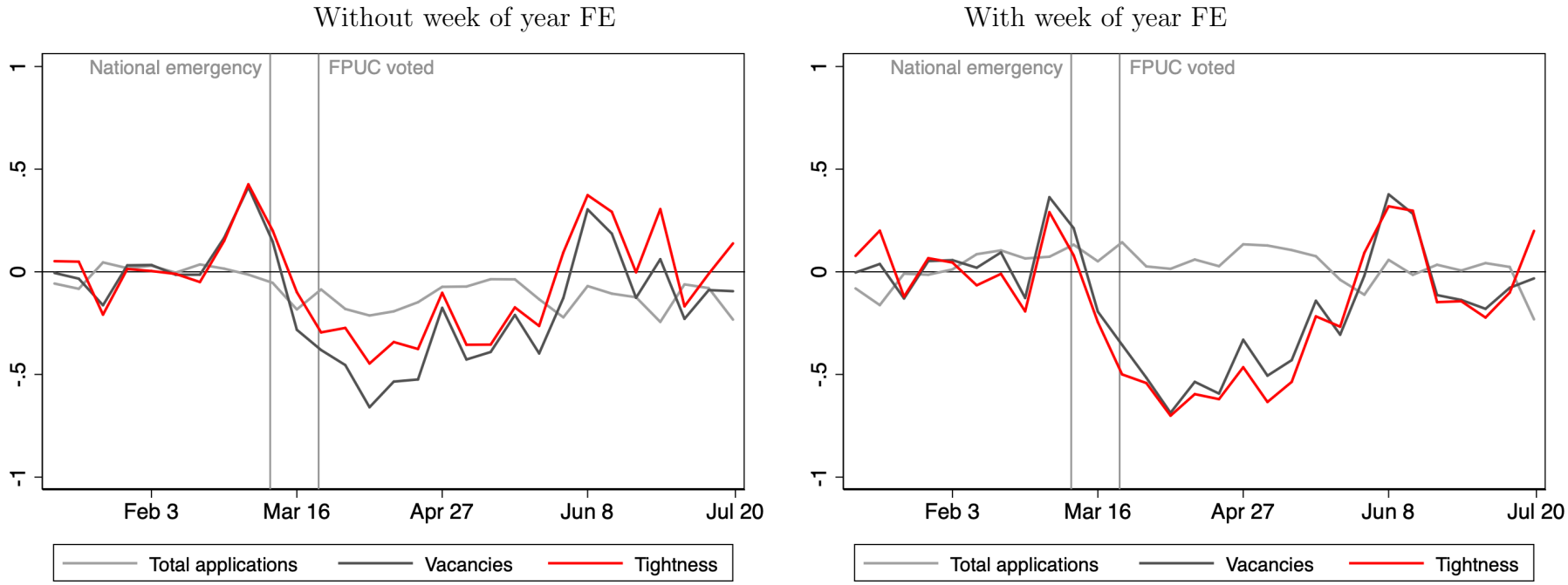
Notes: This Figure reports the estimates from regressions of logged labor market tightness (vacancies/applications) on the interaction of each week around the enactment of the FPUC and the potential increase in UI generosity $ln(\frac{UI_{FPUC}}{UI_{Standard}})$. We use a panel at the calendar week and local labor market level, and all models include calendar week and local labor market fixed effects. We also add various controls in different panels. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS. Thin lines denote 95% confidence intervals, based on robust SE clustered at the state level.

Figure A.6: The impact of FPUC on applications, vacancies and tightness in the construction sector, week by week



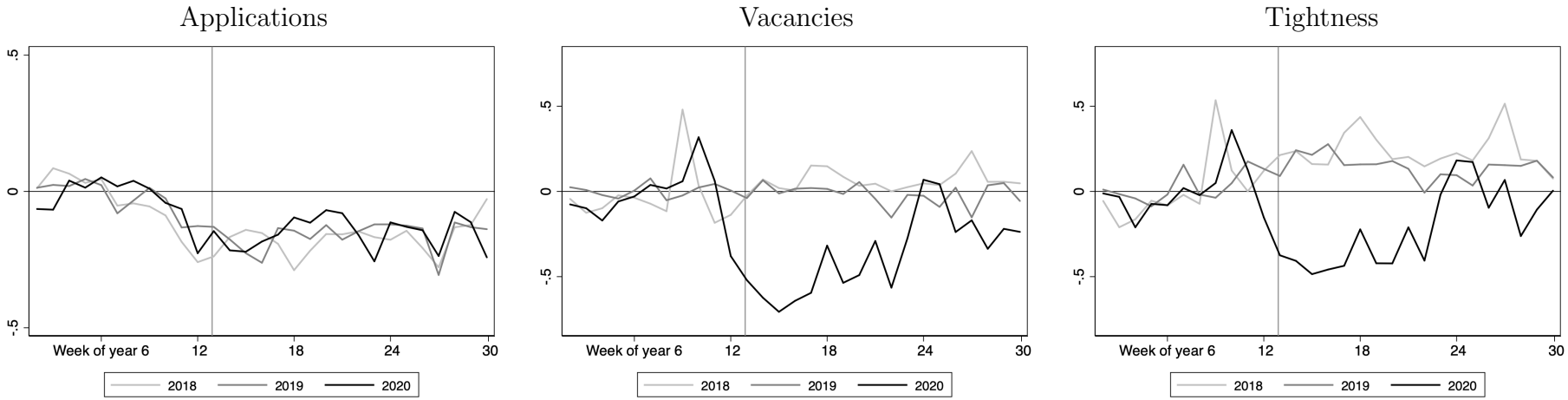
Notes: This Figure reports the estimates from regressions of logged search effort on the interaction of each week around the enactment of the FPUC and the potential increase in UI generosity $ln(\frac{UI_{FPUC}}{UI_{Standard}})$ in the construction sector: the coefficients before the enactment of the FPUC help test our identification assumption; the coefficients after represent the elasticity of search with respect to benefits levels. We use a panel at the calendar week and local labor market level, and all models include calendar week, local labor market, and local labor market×week-of-year fixed effects. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS. Thin lines denote 95% confidence intervals, based on robust SE clustered at the state level.

Figure A.7: Changes in applications, vacancies, and labor market tightness relative to Jan-Feb 2020



*Notes:*The Figure presents the changes in tightness (ratio of the weekly count of new posted vacancies over weekly count of applications), relative to their baseline levels in Jan-Feb 2020. We regress the logged tightness on the time coefficients, and then subtract the average of the estimates obtained for the period Jan-Feb 2020, such that each coefficients represents the relative variation with respect to the average level in Jan-Feb 2020. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS.

Figure A.8: Weekly changes in applications, vacancies and tightness relative to Jan-Feb, each year



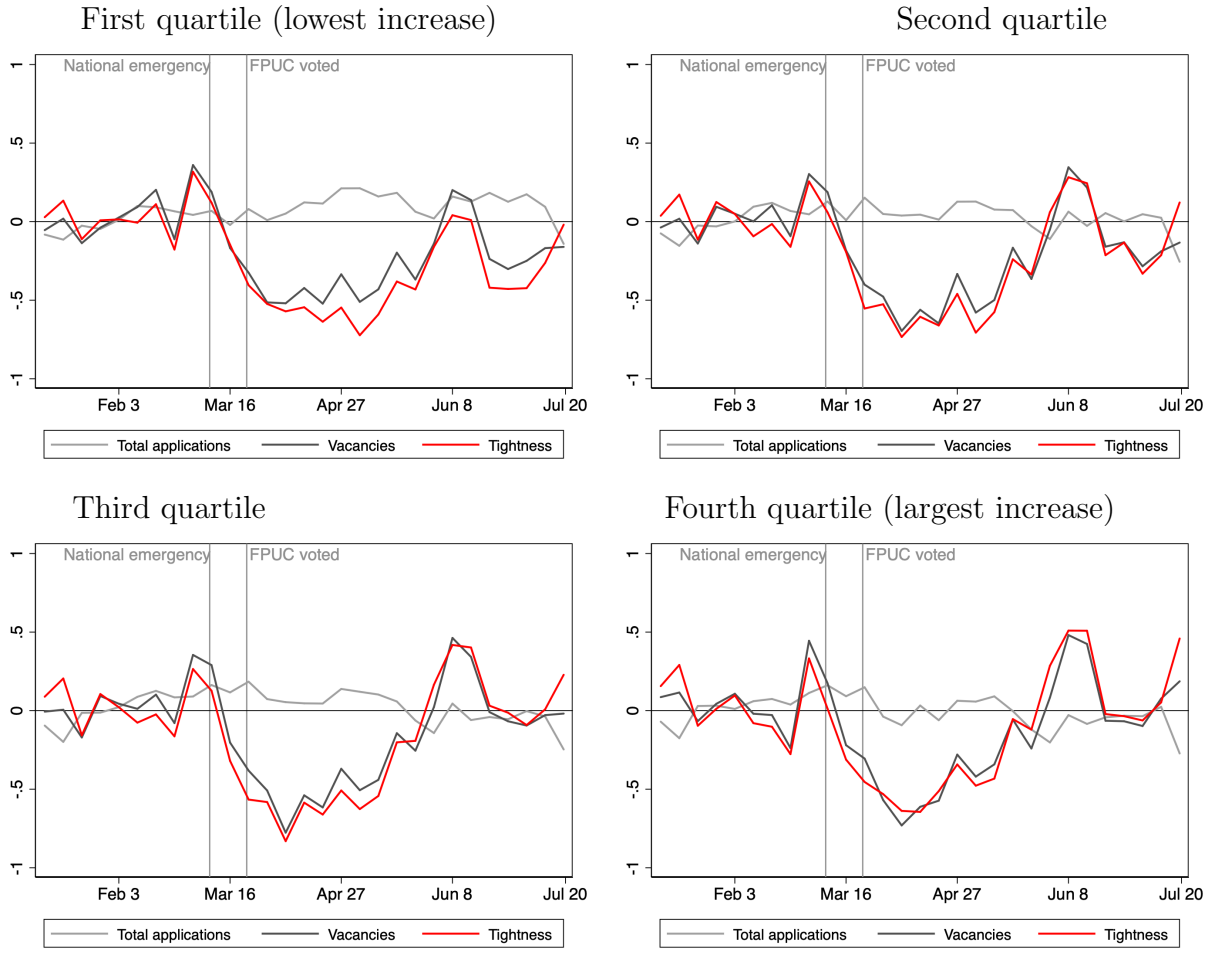
Notes: The Figure presents the weekly changes in the count of applications, of new posted vacancies and of labor market tightness, relative to their baseline levels in Jan-Feb, such that each coefficients represents the relative variation with respect to the average level in Jan-Feb. We present them separately for the years 2018, 2019 and 2020. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS.

Table A.3: Changes in labor market tightness during the period of the FPUC relative to Jan-Feb 2020, without accounting for seasonal variation

Sample:	All			Potential increase in UI Q1	Q2	Q3	Q4
Outcome:	Total Applications	Vacancies	Tightness	Tightness	Tightness	Tightness	Tightness
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FPUC period	-0.128***	-0.237***	-0.109***	-0.221***	-0.127***	-0.091*	0.004
	(0.021)	(0.024)	(0.024)	(0.022)	(0.032)	(0.046)	(0.042)
Week of year FE	No	No	No	No	No	No	No
No. of Obs	1,540,598	1,540,598	1,540,598	613,586	412,050	297,346	217,616
FPUC, Mar-May	-0.117***	-0.415***	-0.298***	-0.356***	-0.325***	-0.310***	-0.201***
	(0.022)	(0.023)	(0.023)	(0.023)	(0.024)	(0.043)	(0.043)
FPUC, Jun-Jul	-0.142***	-0.014	0.128***	-0.052**	0.121**	0.183***	0.260***
	(0.022)	(0.027)	(0.029)	(0.026)	(0.046)	(0.056)	(0.045)
Week of year FE	No	No	No	No	No	No	No
No. of Obs	1,540,598	1,540,598	1,540,598	613,586	412,050	297,346	217,616

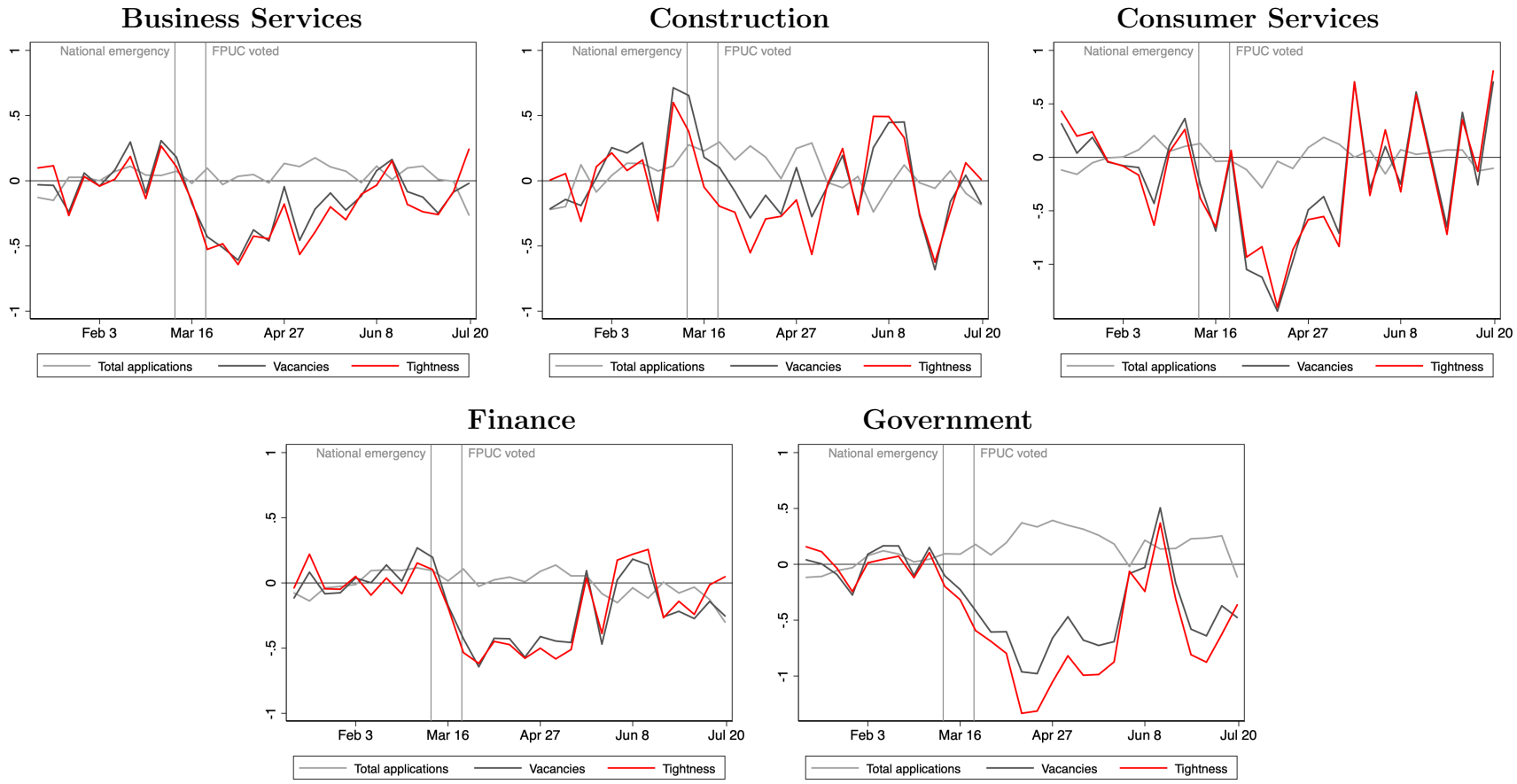
Notes: This Table presents the same estimates as Table 2, except that we do not control for seasonal variation, using week of year fixed effects. The table shows the levels of the total weekly count of applications, the weekly count of new posted vacancies, and labor market tightness during the period of the FPUC, relative to their baseline levels in Jan-Feb 2020. In the upper panel, we present levels during the total period of FPUC, while in the lower panel, we devide the FPUC period in two sub-periods. The estimates are obtained by regressing each logged outcome on a dummy for the FPUC period, and a dummy for all calendar weeks outside of the baseline period of January-February 2020 (such that the calendar weeks during January-February 2020 are the omitted) in a panel at the calendar week and local labor market level. The estimates are obtained in the full sample in col (1)-(3), and separately for local labor markets in each quartile of the distribution of potential UI increase in col (4)-(7). We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS. Robust standard errors clustered at the state level are in parenthesis (* p< 0.10, ** p< 0.05, *** p< 0.010).

Figure A.9: Changes in applications, vacancies and labor market tightness relative to Jan-Feb 2020, by UI increase quartile



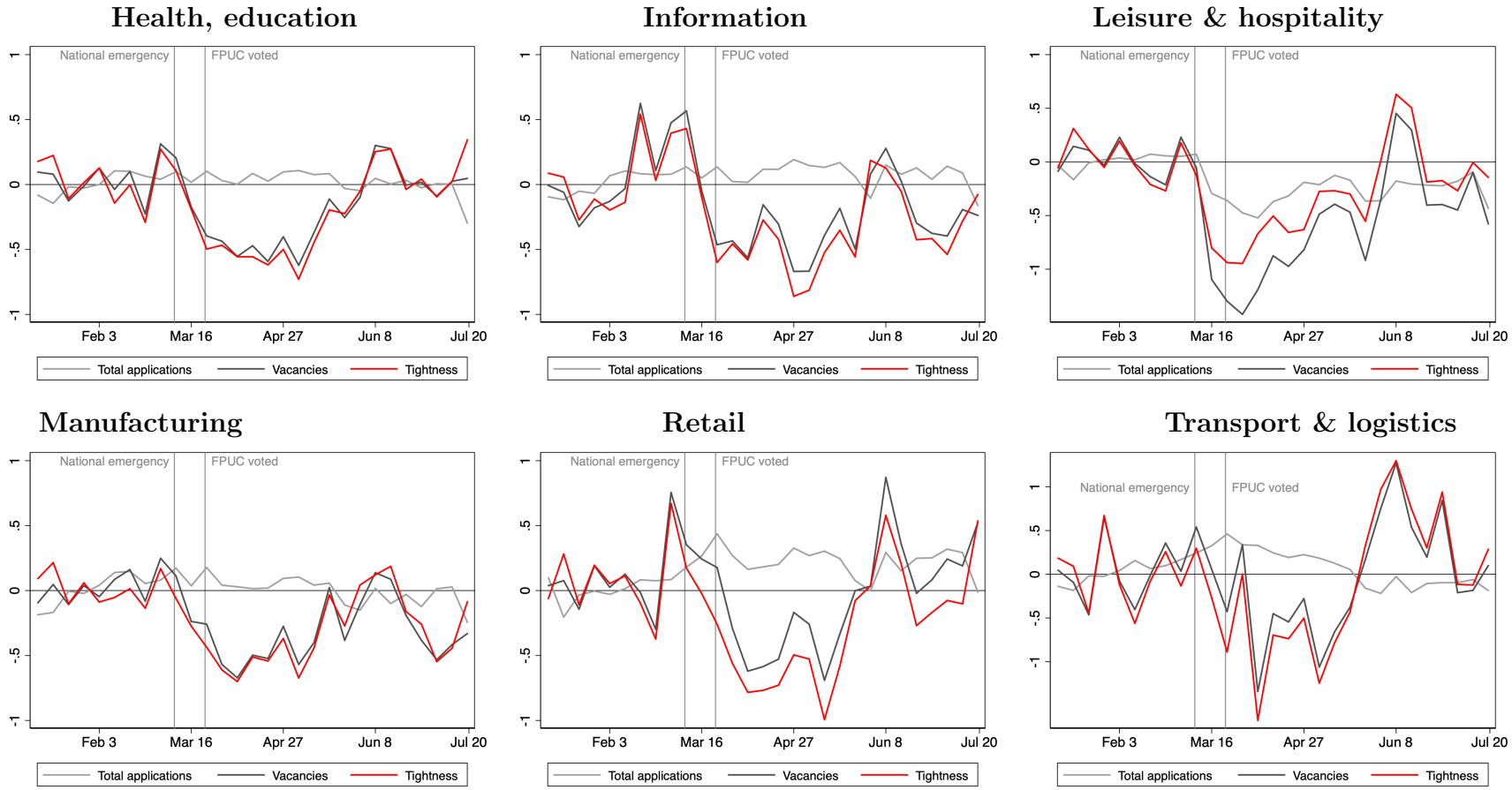
*Notes:*The Figure presents the changes in tightness (ratio of the weekly count of new posted vacancies over weekly count of applications), relative to their baseline levels in Jan-Feb 2020. We regress the logged tightness on the time coefficients, and then subtract the average of the estimates obtained for the period Jan-Feb 2020, such that each coefficients represents the relative variation with respect to the average level in Jan-Feb 2020. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS.

Figure A.10: The evolution of job applications & job postings in specific sectors:



Notes: The Figure presents separately for each sector the changes in the weekly count of applications and of new posted vacancies, relative to their baseline levels in Jan-Feb 2020: the first panel present separately the evolution of logged applications and vacancies, while the second panel present the evolution of the difference in these logged variables. Specifically, we smooth each count variable by computing the moving average in a centered 5-weeks window, and we take the log +1. We estimate the time coefficients, and then subtract the average of the estimates obtained for the period Jan-Feb 2020, such that each coefficients represents the relative variation with respect to the average level in Jan-Feb 2020. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS.

Figure A.11: The evolution of job applications & job postings in specific sectors:



Notes: The Figure presents separately for each sector the changes in the weekly count of applications and of new posted vacancies, relative to their baseline levels in Jan-Feb 2020: the first panel present separately the evolution of logged applications and vacancies, while the second panel present the evolution of the difference in these logged variables. Specifically, we smooth each count variable by computing the moving average in a centered 5-weeks window, and we take the log +1. We estimate the time coefficients, and then subtract the average of the estimates obtained for the period Jan-Feb 2020, such that each coefficients represents the relative variation with respect to the average level in Jan-Feb 2020. We use weights to reflect the proportion of the labor force in each State×industry×occupation in the CPS.