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June 2020

CREPE DISCUSSION PAPER NO. 73

CENTER FOR RESEARCH AND EDUCATION FOR POLICY EVALUATION (CREPE)

THE UNIVERSITY OF TOKYO

http://www.crepe.e.u-tokyo.ac.jp/

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June 15, 2020

Abstract

In response to the COVID-19 shock, the number of new job vacancies declined by 30% in April 2020, with no signs of recovery in May 2020. We present five facts behind the collapse in job creation using newly available microdata on vacancy postings that underly official vacancy statistics. First, the number of vacancy postings has a strong negative correlation with the tendency to stay at home across regions, but not with the number of COVID-19 confirmed cases or business suspension requests by local governors. Second, 80% of the collapse in job creation has been due to the hiring freezes at the firm level. Larger firms and older firms froze hiring more. Third, occupations with high share of working-from-home time, measured using a household survey, saw a smaller decline in vacancy postings. Fourth, posted wages barely responded to the COVID-19 shock at the aggregate-, the regional-, and the job-level. Fifth, the monthly probability of posted wage adjustment has been stable at 10% during the pandemic, with more rigidity downward, and the wage growth conditional on adjustment during the pandemic is around 1% lower than the previous year.

*We thank Daiji Kawaguchi, Sagiri Kitao, Satoshi Tanaka, and Kazuhiro Teramoto for their helpful comments. A part of the analysis uses Japanese Panel Study of Employment Dynamics (JPSED), which was provided by the Social Science Japan Data Archive, Center for Social Research and Data Archives, Institute of Social Science, The University of Tokyo. Fukui was approved to access JPSED data and is responsible for the analysis that involves this data.

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1 Introduction

The COVID-19 shock has brought significant impacts to the labor market all over the world, and Japan is not an exception. While the Japanese labor market did not present a hike in unemployment rate (2.9% in April 2020) compared to the United States, the number of new job vacancy postings declined by 24% from the previous month in April 2020, as shown in Figure 1. This is the largest decline as a monthly change since 1993. Compared to the same month of the previous year, the number of new vacancies is more than 30% lower. While the official statistics for May are not yet available, as of June 13, our data suggests that there was no sign of recovery in May (see Figure 2).

Number of vacancy postings is a forward looking indicator, as it is a main determinant of how many unemployed workers will be able to find a job. To put it differently, even with the stable unemployment rate, the current declining trend in vacancy creation may increase unemployment in the near future. Therefore, it is important to understand why job creation has collapsed and to assess how much recovery we would expect.

In this paper, we provide five facts behind the collapse in job creation during the pandemic in Japan, using a newly available microdata on online vacancy postings that underly official vacancy statistics. The data provides granular information at the job-level, which allows us to make a step forward to understanding the evolution of the Japanese labor market in response to the pandemic.

First, the the number of job postings is strongly negatively correlated with the tendency to stay at home both at the aggregate and the prefecture levels. In contrast, the number of confirmed COVID-19 cases or the the business suspension requests by local governors are uncorrelated with vacancy postings. While we do not claim any causality, this suggests that what is behind the decline in vacancy postings is not the pandemic itself nor the enforced social distancing, but rather the voluntary social distancing.

Second, the collapse in job creation is mainly driven by hiring freezes at the firm level. Our accounting exercise shows that 80% of the decline is due to the fact that many firms halted new hirings altogether. Moreover, larger firms and older firms froze hiring more than others. The results hold both at the aggregate level and the prefecture level. This might be surprising because one usually expects larger firms are better able to absorb the COVID-19 shock. Indeed, Campello et al. (2020) document that in the US, it was mainly the smaller firms that halted new hiring.

Third, occupations with higher shares of working time from home saw a smaller decline in vacancy postings during the pandemic. As Dingel and Neiman (2020) point out, whether a job can be done at home is a crucial determinant in assessing the economic impacts of social distancing. While the existing works (e.g., Mongey et al., 2020) assess whether the ability to work from home mitigates the damage on the existing workforce, we complement this literature by documenting the differential impact on job creation. Differently from Dingel and Neiman (2020), who predict the ability to work from home based on job characteristics using O*Net, we construct a measure of work-from-home using a household survey that asks how many hours workers *actually* worked from home.

Fourth, posted wages have barely responded to the COVID-19 shock, despite the fact that job creation declined by 30%. The results hold at the aggregate-, the regional-, and the job-level. Aggregate posted wages did not see any significant responses during the pandemic. Regions that are hard-hit by the pandemic did not see any differential wage growth. Moreover, holding the job fixed, changes in posted wages during the pandemic are no different from the ones in previous year of the same month. Many recent papers incorporate nominal wage rigidity in assessing the macroeconomic impact of the pandemic (e.g., Farhi and Baqaee, 2020; Guerrieri et al., 2020). Consistent with these models, our tentative results indicate that indeed, wages did not respond or at least are very slow to respond to the pandemic shock.

Fifth, to understand why wages did not respond, we look at the frequency of wage adjustment and its response to the pandemic. The frequency of wage adjustment is the sole determinant of nominal wage rigidity in the Calvo-style model, and the stability of the frequency is the key feature. We document that only 10% of newly posted jobs post wages that are different from the previous post, and this number has been stable during the pandemic. Moreover, when they adjusted, wages are two to five times more likely to rise than to fall both before and during the pandemic, suggesting the presence of downward nominal wage rigidity. These facts are broadly consistent with the existing evidence in the US from before the pandemic (Hazell and Taska, 2019). Given that only 10% of jobs change wages, it is not surprising that the wages saw little responses to the pandemic. But did wages respond once they have chance to adjust? We document that the wage growth conditional on adjustment in the period of April-May 2020 is 1% lower than the previous year. Therefore, wage response to the pandemic is proceeding, at best, very slowly.

All together, our results provide important moments of the data that would be useful for building theoretical models to understand the labor market consequence of the pandemic in Japan. While some of our evidence confirms already documented facts in the US, some other findings are either new or in contrasts to them. Understanding the source of difference is the next step that we are aiming for.

Our paper is most closely related to the recent work that also used online job vacancy data to understand the labor market effect of the pandemic (Kahn et al., 2020; Campello et al., 2020; Hensvik et al., 2020). We not only complement this literature by documenting the similar facts in Japan, but also present different results on vacancy creation by firm size and some new results on wage adjustment.

We are not the first to use the microdata that underlies official vacancy statistics in Japan. Kawata (2019) used the confidential data to document labor market mismatch. Using the publicly available data, Kawata (2020) also studied the vacancy posting during COVID-19 in Japan.

This paper also relates to the literature that studies the economic impact of the pandemic in Japan. These include the impact of the pandemic on the existing workforce (Kikuchi et al., 2020), consumption response (Watanabe and Omori, 2020), firm default (Miyakawa, 2020), the effect on mental health (Yamamura and Tsutsui, 2020), and uncertainty faced by the small firms (Kawaguchi et al., 2020). We particularly follow the lead by Miyakawa (2020) in using the Google Mobility report as capturing the geographic variation in the pandemic "shock."

2 Data

Main data: HRog data We obtained data on job vacancy postings from HRog data, provided by Goalist. Goalist retrieves information on vacancy postings from more than 100 websites, including those posted on "Hello-work" (or Public Employment Security Office). The vacancy postings on Hello-work underly the official vacancy statistics, Employment Referrals for General Workers (ERGW). In this paper, we focus on the vacancy postings on Hello-work. The sample period starts runs from January 2019 to May 2020.

The data for each posting contains rich information, such as job posting dates, job title, firm and

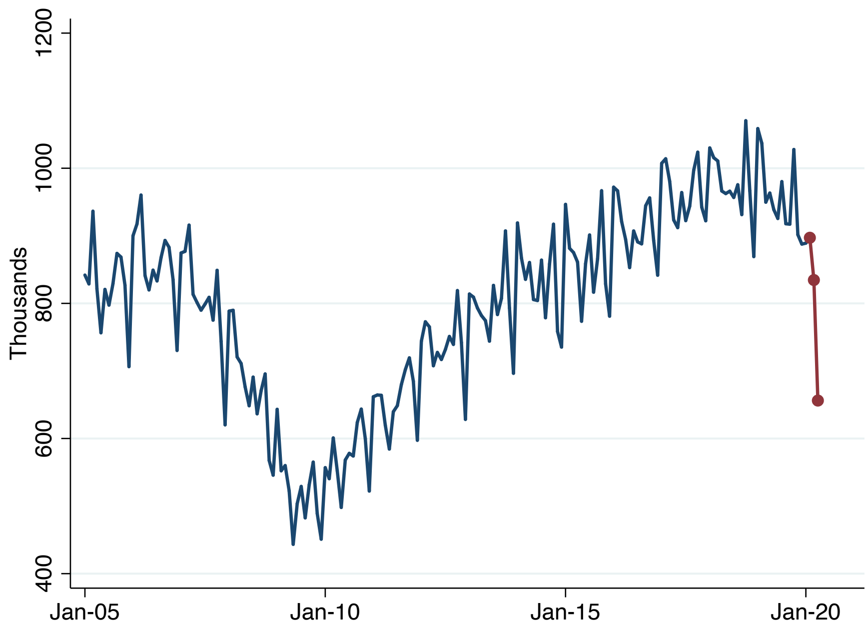


Figure 1: Aggregate new vacancy postings

Note: Figure 1 shows the number of new vacancy postings at the monthly frequency. The red line describes the data during March-April 2020. The data is from Employment Referrals for General Workers by the Ministry of Health, Labour and Welfare.

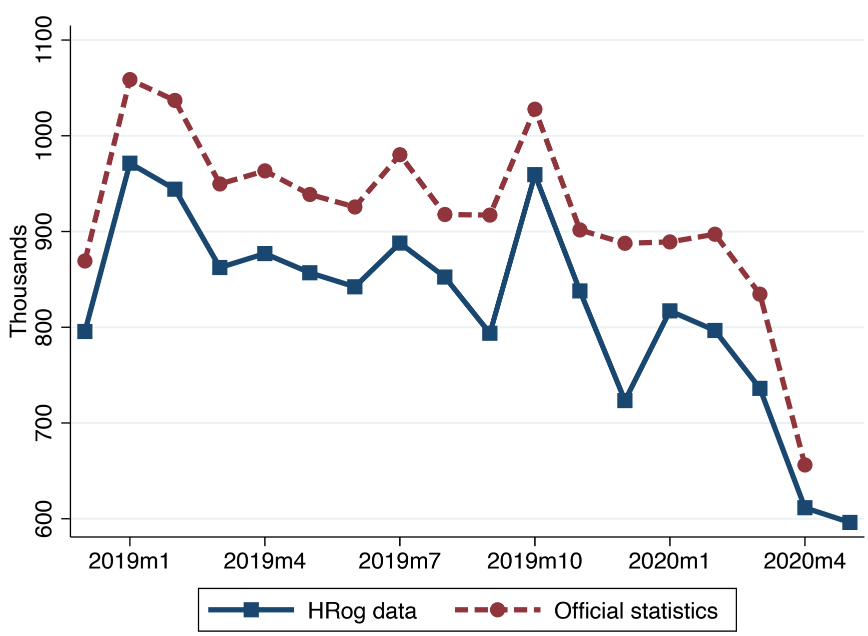


Figure 2: Comparing the number of vacancy postings of Hrog data and official statistics

Note: This figure compares the number of new vacancy postings at the monthly frequency in HRog data to that in official statistics. The blue solid line denotes HRog data, and the red dashed line denotes official statistics. Data in May 2020 is only available for HRog data as of now.

establishment information, industry, posted wages, and skill requirements. There are two caveats, however. First, unfortunately, the occupation classification is missing in our dataset. Second, Goalist scrapes in the beginning of every month. This means that we miss job postings that are retracted within a month.

Other data source We supplement HRog data with a few other data sources. We utilize the official vacancy statistics, ERGW, for the validation exercise. We also use this data when we analyze by occupation, as occupation classification is missing in our dataset. In measuring the hours spent working from home by occupation, we use the Japanese Panel Study of Employment Dynamics (JPSED), which was provided by the Social Science Japan Data Archive. We also use the number of COVID-19 cases by prefecture provided by Toyo Keizai,[1] the mobility data by prefecture from Google mobility report,[2] and the implementations of business suspension requests by local governors from prefecture websites.

First look at HRog data Figure 2 shows the number of new vacancy postings in ERGW (official statistics) and HRog data from January 2019 to May 2020. Although HRog data consistently miss around 10% of vacancy postings, the difference is stable over time. The official statistics for May 2020 are not available yet, as of June 13, 2020. In HRog data, the number of new vacancy postings sees no recovery.

Definition of a "job" When we conduct job-level analysis, we need to define a job. We define two posts as being for the same job if they (i) have exactly the same establishment name, (ii) have exactly the same job title, and (iii) their workplaces are in the same prefecture. In some cases, a firm name appears in the place of an establishment name, and thus, this procedure sometimes does not identify a job. To alleviate this concern, we drop jobs that appear more than twice in a given month. This drops around 6% of the observations.

[1] https://toyokeizai.net/sp/visual/tko/covid19/

[2] https://www.google.com/covid19/mobility/

3 Five facts about vacancy postings during the pandemic in Japan

We present five facts behind the collapse in job creation to help understand its cause and assess the prospect of recovery.

Fact 1: Vacancy posting is strongly negatively correlated with tendency to stay at home, but not with the number of COVID-19 confirmed cases nor the business suspension requests by the local governors.

Voluntary social distancing is considered to be a leading cause of the economic crisis during the pandemic. Indeed, Figure 3 shows that, at the aggregate level, when the number of vacancy postings declined by 30%, the mobility to residential area has increased by 15% according to Google mobility report. Figure 4 shows that the same correlation holds at the prefecture level. It says that the prefecture with larger increase in the tendency to stay at home also experienced larger decline in job creation.

We formally explore this relationship by exploiting the geographic variation and high-frequency nature of our data. We also examine two other hypotheses in explaining the collapse in job creation: (i) the number of newly confirmed COVID-19 cases, and (ii) the business suspension request by the local governors. Note that the timing and the duration of business suspension requests had substantial geographic variation. For example, Tokyo started to request on April 11, and ends in May 31st, while Shimane have never requested.

We consider the following regression specification:

$$\ln v_{pt} = \beta_1 \ln(\text{stay at home})_{pt} + \beta_2 \Delta \ln(1 + \text{COVID-19 cases})_{pt} + \beta_3 S_{pt} + \text{FEs} + \epsilon_{it}, \tag{3.1}$$

where $\ln v_{pt}$ denotes log vacancy in week t in prefecture p, and $\ln(\text{stay at home})_{pt}$ is the log of the measure of how much people stayed at home, provided Google mobility report, $\Delta \ln(1 + \text{COVID-19 cases})_{pt}$ is the log-changes in (one plus) the cumulative number of confirmed cases,[3] $S_{pt} \in [0, 1]$ is the fraction of days during week t in prefecture p that business suspension was requested, and FEs are prefecture and time fixed effects. The sample period is from February 2020 to May 2020.

Table 1 shows the results. In column (1), we confirm that there is a strong negative correlation between the number of vacancy posting and the tendency to stay-at-home. The second column shows that the vacancy posting is almost unrelated to the number of COVID-19 cases. The third column shows that the business suspension request by local governors is also not correlated with vacancy posting. In the forth column, we jointly include all the three variables and find that the tendency to stay at home is a sole strong predictor of vacancy posting. The magnitude is large: 1% increase in a tendency to stay at home is associated with 1.7-2% decreases in the vacancy postings. Given that the aggregate tendency to stay at home has increased by 15% during April, in a pure accounting sense, this factor explains almost all the collapse in vacancy posting.

We, by no means, claim any causality. However, this is suggestive that the voluntary social distancing, rather than enforced social distancing or the pandemic itself, may have been an important factor.

[3] We add one in order to deal with the issue of zero.

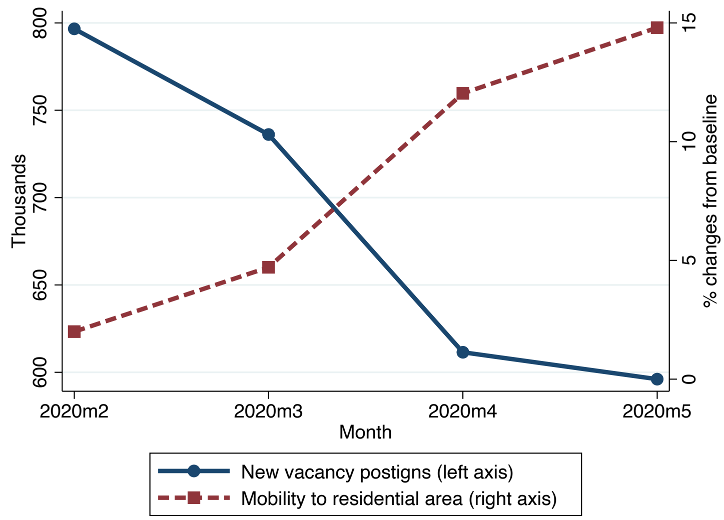


Figure 3: Vacancy postings and stay-at-home at the aggregate level

Note: Figure 3 plots the number of new vacancy postings (in solid line) and the mobility to residential area (in dashed line). The mobility data is from a Google mobility report.

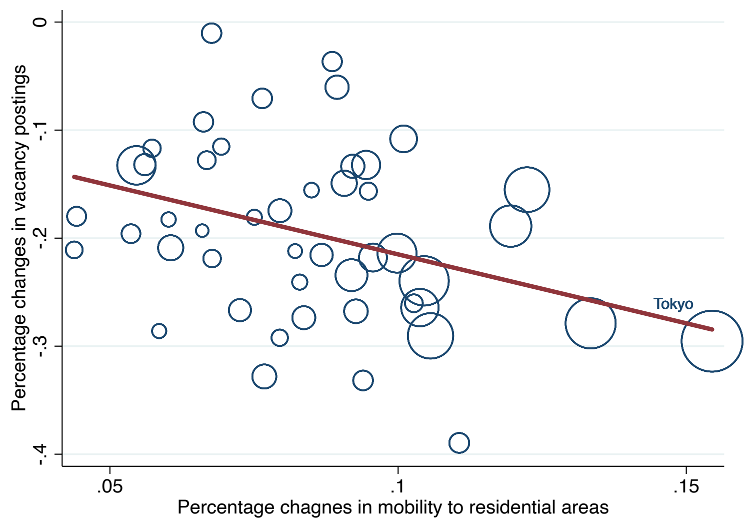


Figure 4: Vacancy postings and stay at home at the prefecture level

Note: Figure 4 plots changes in the number of new vacancy postings from March 2020 to May 2020 on the vertical axis and the changes in mobility to residential area on the horizontal axis at the prefecture level. The size of the circle represents the population size. The red solid line is the linear regression using population as weights. The mobility data is from Google mobility report.

	Dependent variable: log vacancy			
	(1)	(2)	(3)	(4)
log (stay at home)	-1.728			-2.110
	(0.649)			(0.767)
Δ log (1+COVID-19 cases)		0.028		0.021
		(0.013)		(0.013)
Business suspenstion requests			-0.029	0.082
			(0.036)	(0.052)
Time FE	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓
Observations	705	705	705	705
Adjusted R^2	0.951	0.950	0.949	0.951

Table 1: Vacancy posting and stay-at-home

Note: Table 1 shows the coefficients from regression equation (3.1). The dependent variable is log of vacancy in all specifications. All the regressions are weighted by prefecture population size. Standard errors in parenthesis are clustered at the prefecture level.

Fact 2: Eighty percent of the decline in vacancy postings is due to the hiring freeze at the firm level. Larger firms, older firms, and firms with greater capital froze hiring relatively more.

Why did job creation collapse by 30%? Did 30% of firms completely stop hiring (extensive margin), or did all firms reduce hirings equally by 30% (intensive margin)? We answer the question first by considering the accounting identity. Let $\Delta v_{T,t_0}$ denote the decline in vacancy posting from month t_0 to T. Let π_t denote the fraction of firms that post a positive amount of vacancies in a given month t, and μ_t denote the average amount of vacancies across firms conditional on posting in month t. Then we can decompose the decline in vacancy posting as

$$\Delta v_{T,t_0} = \underbrace{\Delta \pi_{T,t} \bar{\mu}_{T,t}}_{\text{extensive margin}} + \underbrace{\bar{\pi}_{T,t} \Delta \mu_{T,t}}_{\text{intensive margin}}, \tag{3.2}$$

where $\bar{x}_{T,t} \equiv \frac{1}{2}(x_T + x_t)$, and $\Delta x_{T,t} \equiv x_T - x_t$ for any x. The extensive margin accounts for the decline in vacancy postings due the decline in the fraction of firms that post jobs. The intensive margin accounts for the decline in vacancy postings due the decline in the average number of jobs among the firms posting jobs.

Figure 5 shows the decomposition. It shows that the 22 percentage points decline is accounted by extensive margin, and the remaining 6 percentage points decline is accounted by intensive margin. Therefore, 80% ($\approx$ 22/28) of the decline in vacancy posting is due to the hiring freezes at the firm-level.

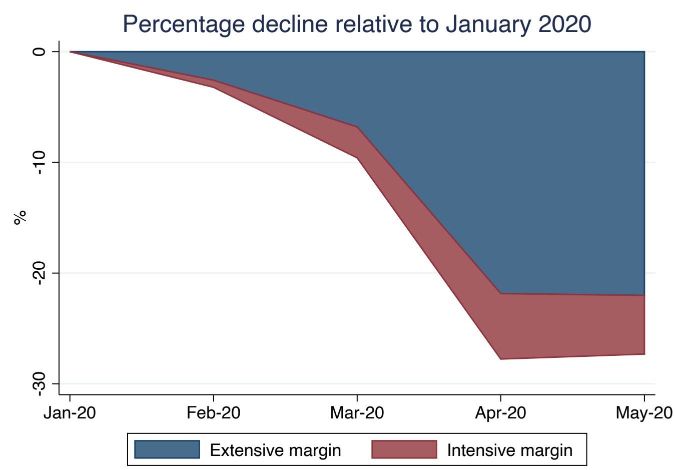


Figure 5: Decomposition of the decline in vacancy creation

Notes: Figure 5 shows the decompositon of percent decline in the number of new vacancy postings relative to Jannuary 2020, from (3.2).

It is then natural to ask what type of firms froze hiring. Figure 6 shows the fraction of firms with positive vacancy postings in each month by quintiles of firm characteristics measured in January 2020. Figure 6a plots the changes from January 2020 by firm employment size. While the fraction declined by only 2% for smallest firms, it declined by 10% for largest firms. Similarly, Figure 6b similarly shows that the firms with more capital froze hiring 5% more than the firms with little capital, and Figure 6c says older firms were 4% more likely to freeze hiring relative to the youngest group of firms.

Exploiting regional variation. One obvious concern is that larger firms might have stopped hiring for other reasons. For example, staring in April 2020, the "equal pay for equal value of work" law was imposed only on large firms. This law effectively prohibits discrimination between full-time tenured employees and other employees (e.g., part-time workers). This national policy might have differentially discouraged large firms from hiring, independent of the COVID-19 shock.

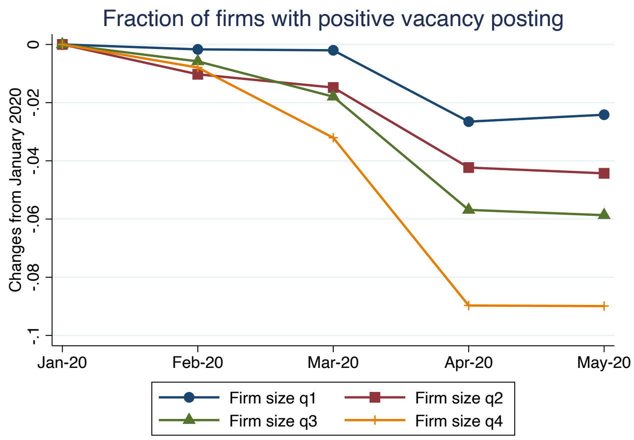
In order to alleviate this concern, we exploit regional variations in exposures to the pandemic to difference out any national shocks. Building on Fact 1, we use the tendency to stay-at-home as the regional pandemic shock. We consider the following regression:

$$\ln(1+v_{fpt}) = \sum_{q=1}^{4} \beta_q \cdot \mathbb{I}(f\text{'s quintile is } q) \cdot \ln(\text{stay at home})_{pt} + \text{FEs} + \epsilon_{fpt}, \tag{3.3}$$

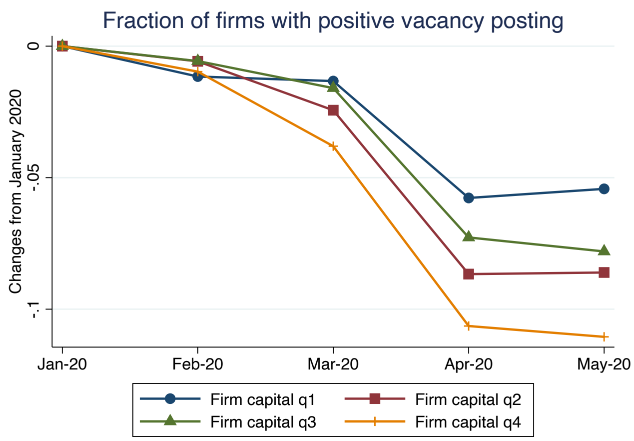
where v_{fpt} is the number of new vacancy postings by firm f in prefecture p during month t, $\ln(\text{stay at home})_{pt}$ is log of the mobility to residential area in p during t, $\mathbb{I}(f\text{'s quintile is } q)$ is the indicator function, which takes one if the firm's characteristics (employment, capital, or age) is in quintile q, and FEs are fixed effects. Importantly, we include quantile $\times$ time fixed effect. This absorbs any national shocks that are common to firms in quantile q. Therefore, by differencing out national policies such as "equal pay for equal value of work," we focus on how large firms located in a prefecture that is hard-hit by the pandemic create jobs differently from the large firms in a less-hit prefecture. The sample period is from February 2020 to May 2020 at the monthly frequency.

Table 2 shows the results. Column (1), (2), and (3) consider quintiles in terms of size, capital, and age, respectively. Across all specifications, we find the same relationships as in the time-series data: the larger firms saw a greater decline in vacancy postings in response to the pandemic shock. We therefore are fairly comfortable in concluding that the fact presented here is a consequence of the pandemic, and not a spurious correlation caused by any national shocks that occurred at the same time as the pandemic.

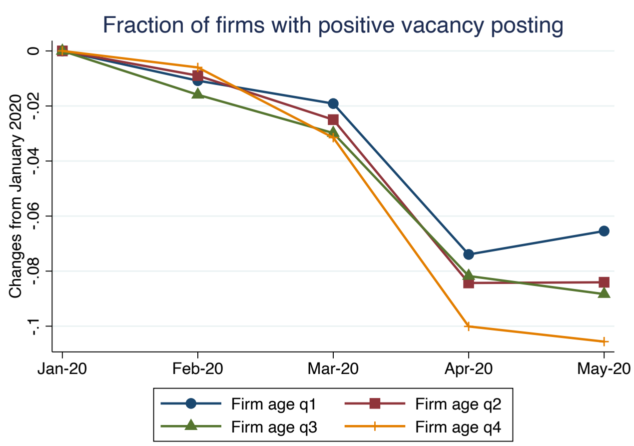
These results are in a sharp contrast to the results from Campello et al. (2020) in the context of the US. They document that the small firms froze hiring relatively more than the large firms.



(a) By firm employment



(b) By firm capital



(c) By firm age

Figure 6: Fraction of firms with positive vacancy posting by firm characteristic

Notes: Figure 6 shows the fraction of firms with zero vacancy posting stratified by firm employment size, capital, and age.

	Employment size	Capital	Age
	(1)	(2)	(3)
ln(stay at home) × Quintile 1	-0.631	-0.767	-0.891
	(0.160)	(0.274)	(0.238)
ln(stay at home) × Quintile 2	-0.804	-0.985	-1.129
	(0.208)	(0.220)	(0.275)
ln(stay at home) × Quintile 3	-1.047	-1.070	-1.141
	(0.289)	(0.265)	(0.283)
ln(stay at home) × Quintile 4	-1.217	-1.026	-1.089
	(0.353)	(0.225)	(0.278)
Firm × Prefecture FE	✓	✓	✓
Quintile × Time FE	✓	✓	✓
Observations	1439412	1272736	1425592
Adjusted R^2	0.227	0.213	0.226

Table 2: Regional variations in stay-at-home and vacancy postings by firm size

Note: Table 2 shows the coefficients from regression equation (3.3). The dependent variables in all columns are log of one plus number of vacancy at the firm level. Columns (1), (2), and (3) consider quintile in terms of employment size, capital, and age, respectively. Standard errors in parenthesis are clustered at the prefecture level.

Fact 3: Occupations with high shares of time working from home had a smaller decline in vacancy postings.

In evaluating the economic impact of “social distancing,” the feasibility of jobs being done at home caught great attention (Dingel and Neiman, 2020). We ask whether jobs that can be done at home saw any differential pattern from other jobs in job creation during the pandemic.

In measuring whether a job can be done at home, we first constructed pre-pandemic measures of how many hours workers spent working from home for each occupation. JPSED data asks workers, ”How many hours per week did you spend working at places other than work-place, such as home, cafés, or restaurants?” We divided this measure by the total hours worked per week to construct share of hours spent working at home. We then computed the average for each occupation, using the cross-sectional weights. We manually constructed occupational cross-walk between JPSED data and occupational classification in vacancy statistics, and ended up with 22 classifications. Note that, because occupational classification is missing in our dataset, we relied on official vacancy statistics for this analysis.

Therefore, differently from Dingel and Neiman (2020), we use a measure of how much workers *actually* worked from home. We make this choice for two reasons. First, this measure is potentially more reliable than the prediction based on job characteristics. Second, although Japan O*Net provides numerous job characteristics by occupations, it contains less information that are useful to construct work-from-home measures than the US O*Net. A potential drawback is that our measure will not capture jobs that can be easily done at home, but have not been done so before the pandemic.

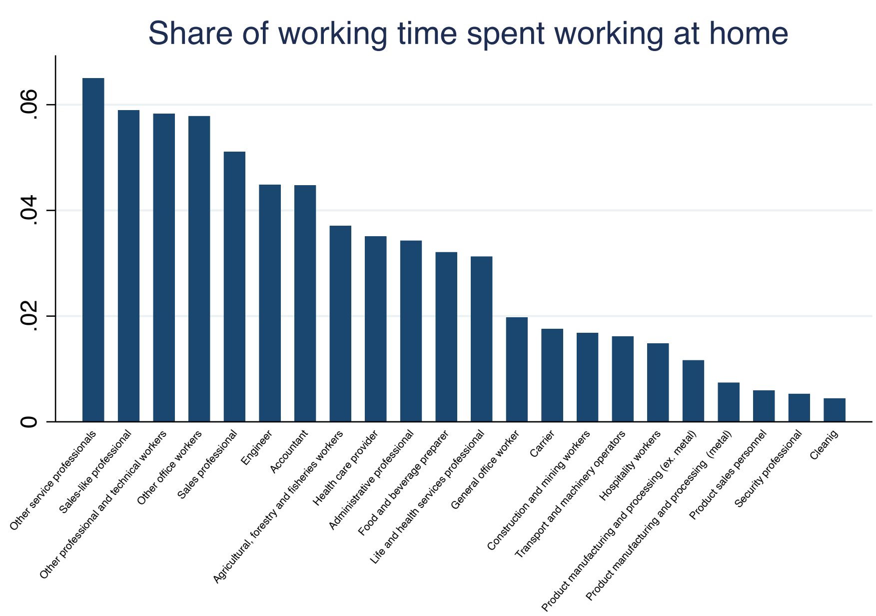


Figure 7: Share of working time spent working at home across occupations

Note: Figure 7 plots the average share of working time spent working at home, constructed from JPSED data. We first compute the share at the worker-level and then take average across occupations using the cross-sectional weights provided in JPSED data.

Figure 7 visualizes our work-from-home measures. Not surprisingly, professional service occupations come in at the top, and production workers or cleaning occupations tend to fall at the bottom. These patterns are broadly consistent with Dingel and Neiman (2020).

Figure 8 then plots the log-changes in vacancy in March-April 2020 from the previous year with the same months on the vertical axis and our work-from-home measures on the horizontal axis. We can see that there is a positive relationship between the two, suggesting that jobs that can be done from home saw a milder collapse in job creation. The point estimates of the slope is 2.3, and it is statistically significant at 5%. That is, occupations with 1% higher share of work-from-home saw 2.3% less decline in vacancy postings.

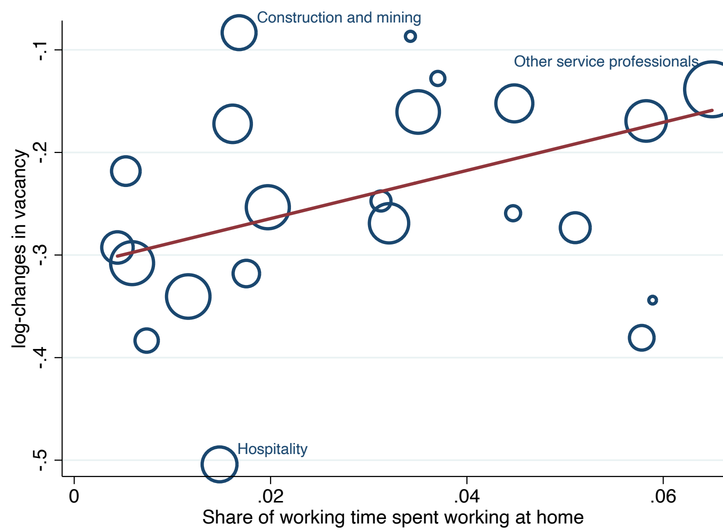


Figure 8: Changes in vacancy during the pandemic and work-from-home measures

Note: Figure 8 plots the log-changes in vacancy postings from March-April 2019 to March-April 2020 on the vertical axis and our work-from-home measures on the horizontal axis. The size of each circle represents initial amount of vacancies. The red line is the linear regression using the initial vacancy as weight.

Fact 4: Posted wages have barely responded to the pandemic shock, at the aggregate-, regional- and the job-level.

The wage adjustment is the key determinant of the incentives to create jobs. We explore whether the posted wages have responded to the COVID-19 shocks so far. Figure 9 shows the mean of the lower- and the upper-bounds of posted wages in a given week. Figure 10 shows the average of the job-level wage growth. Although the minimum wage increase in October 2019 makes the figure harder to interpret, both figures show that there is no significant response of posted wages. This is true especially if we compare April-May 2020 to the previous year with the same months. One might argue there seems to be some difference, but they are anyway very small relative to the fact that the vacancy creation collapsed by 30%.

We can also exploit regional variation to examine the wage response at the local labor market level. It has been pointed out that during the financial crisis, the nominal wage growth was very different at the regional level from the national level (Beraja et al., 2019). Figure 11 shows the mean nominal wage response against the changes in stay-at-home at the prefecture level. The left-panel shows the one for full-time jobs, and the right-panel shows the one for part-time jobs. There is essentially no relationship between the two for the full-time jobs. There is a weak negative relationship for the part-time jobs, but the magnitude is small: 1% increase in stay-at-home reduces the prefecture-level wage growth by 0.08%.

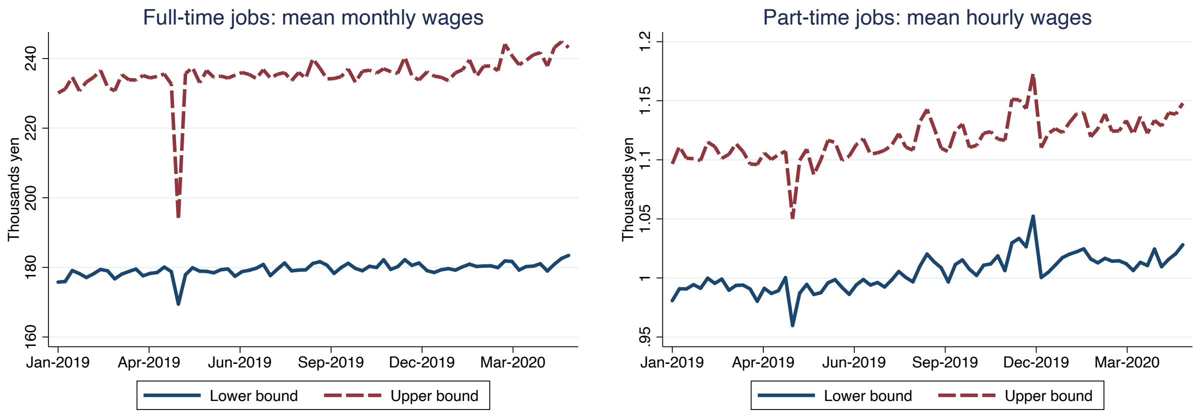
However, these results does not necessarily imply that the wages at the job-level did not respond because we did not adjust for compositional changes. It could be the case that the wages at the job-level declined, but more high-wage jobs have posted, leaving the aggregate mean wages unchanged. Such compositional changes have been emphasized in the US (Cajner et al., 2020). To explore this possibility, we run the following regression at the job-level:

$$\Delta \ln w_{jt} = \beta \times (\text{Post COVID})_t + \text{FEs} + \epsilon_{jt}, \tag{3.4}$$

where $\Delta \ln w_{jt}$ is the wage growth in week t of job j since the last post, $(\text{Post COVID})_t \in \{0, 1\}$ is an indicator which takes one from April to May, 2020, and FEs is a set of fixed effects. Here β captures the wage response at the job-level during the pandemic conditional on the set of fixed effects. We only present the results for the lower-bound, but the results for the upper-bound is nearly identical. To minimize the effect of minimum wage increased, which was implemented in October 2019, we exclude sample periods from September to November in 2019. The sample period starts in April 2019.

Table 3 shows the results. The first three columns show the results for full-time jobs, and the last three columns show the results for part-time jobs. Within each type of jobs, we first only control for monthly dummies to capture any seasonality, then add prefecture and 3-digit industry effects, and finally control for job fixed effects. Across all specifications, the estimates are within a range of $\pm 0.1\%$, and they are precisely estimated. These results suggest that not only at the aggregate level, but also at the job-level, there has been little wage response.

In summary, we conclude that the nominal wages have not yet adjusted to the pandemic shock so far, at any level of aggregation.

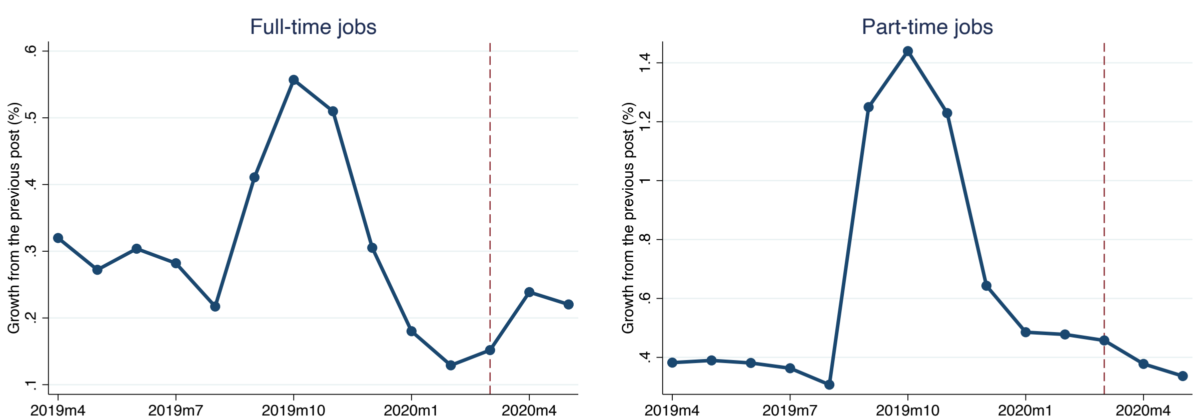


(a) Full-time jobs

(b) Part-time jobs

Figure 9: Mean posted wages

Note: Figure 9 plots the mean of the lower- and the upper-bounds of posted wages at the weekly frequency. The negative spike in the end of April 2019 corresponds to national holidays.

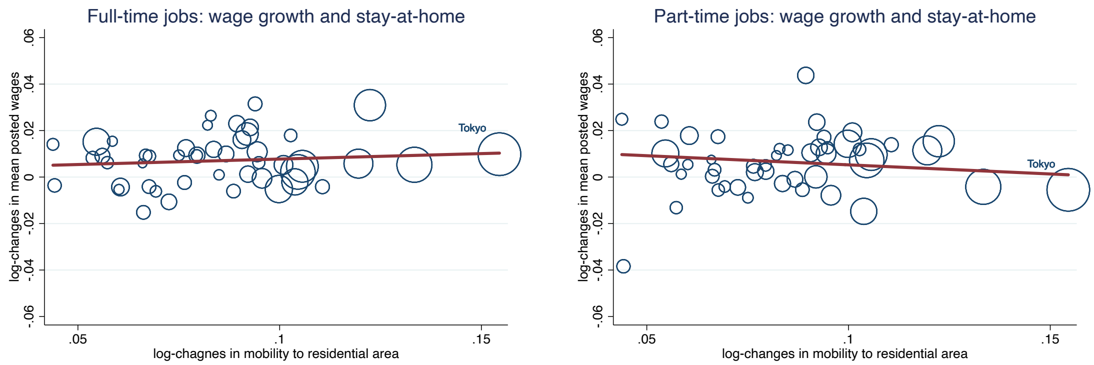


(a) Full-time jobs

(b) Part-time jobs

Figure 10: Mean posted wages growth

Note: Figure 10 plots the mean of posted wage growth for each month. We first compute the wage growth at the job-level from the previous post and then take the average for each month. We take the lower bound of the range as the posted wage. The dash red line indicates March 2020. The spike around October 2019 corresponds to the minimum wage increase.



(a) Full-time jobs

(b) Part-time jobs

Figure 11: Mean posted wage growth and stay at home at the prefecture level

Note: Figure 11 plots changes in the mean posted wage from March 2020 to May 2020 on the vertical axis and the changes in mobility to residential area on the horizontal axis at the prefecture level. The size of the circle represents the population size. The red solid line is the linear regression using population as weights. The mobility data is from Google mobility report.

	Full-time			Part-time		
	(1)	(2)	(3)	(4)	(5)	(6)
Post COVID (in %)	-0.067	-0.075	-0.107	-0.028	-0.014	-0.055
	(0.007)	(0.009)	(0.029)	(0.012)	(0.011)	(0.017)
Monthly dummy	✓	✓	✓	✓	✓	✓
3-digit industry FE		✓			✓	
Prefecture FE		✓			✓	
Job FE			✓			✓
Observations	1701928	1632379	1268155	928895	874623	659905
Adjusted R^2	0.000	0.002	-0.059	0.001	0.004	-0.079

Note: The table shows the coefficients from regression equation (3.4). Standard errors in parenthesis are two-way clustered at the job and the month level.

Table 3: Wage response at the job-level to COVID-19 shock

Fact 5: The monthly probability of posted wage adjustment has been stable at 10% during the pandemic with more rigidity downward. Conditional on adjustment, the wage growth during the pandemic is 1% lower than the previous year.

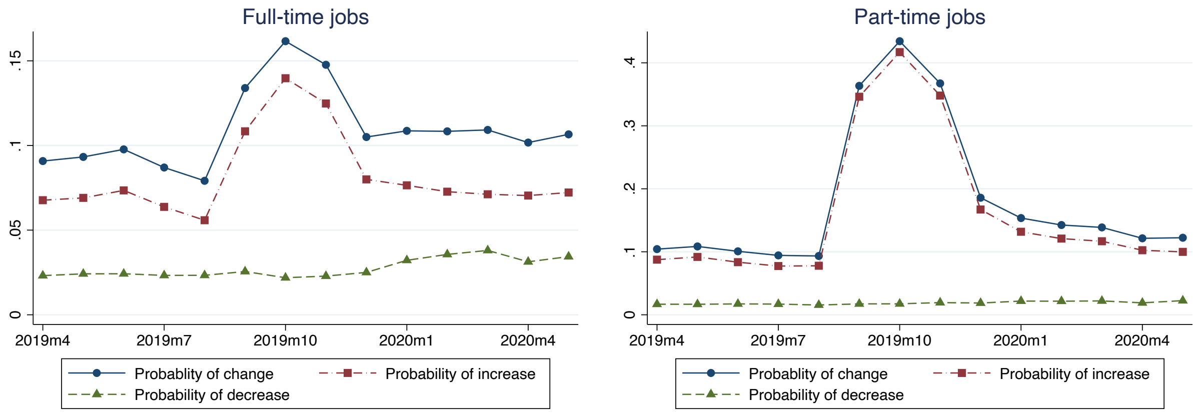
Why did the posted wage not respond to the COVID-19 shock? We explore an explanation based on nominal wage rigidity. We first look at the frequency of nominal posted wage adjustment, as it is a key parameter in Calvo-style models.

Figure 12 shows the probability of wage changes, wage increases, and wage decreases for full-time jobs (left panel) and part-time jobs (right panel). The probability of wage changes is measured as the fraction of jobs in a given month that post wages that are different from the previously posted wages. The upward spike around October 2019 is due to the minimum wage increase, so we ignore these periods.

First, only around 10% of jobs change wages every month. Therefore wage adjustment is infrequent. Second, wages are two and five times more likely to rise than to fall for full-time jobs and part-time jobs, respectively. These results are consistent with the notion of downward nominal wage rigidity, especially for part-time jobs. Overall, the results confirm the evidence presented in Hazell and Taska (2019) in the context of the US.

Importantly, these probabilities have been remarkably stable during the pandemic. Given that only 10% adjust every month, it is then not surprising that the aggregate nominal wage did not respond to the pandemic shock. But are firms responding to the pandemic shock conditional on adjustment? Figure 13 shows the histogram of non-zero wage changes for April-May 2020 and we compare it to the one for April-May 2019. For full-time jobs, the distribution of wage changes during the pandemic is somewhat skewed toward the left compared to the previous year. The mean wage growth conditional on adjustment during the pandemic is 2%, while it is 3.1% in the previous year. For part-time jobs, it seems that wage changes are more concentrated around zero during the pandemic. In fact, the mean wage growth conditional on adjustment is 2.8% in April-May 2020, but it is 3.8% in the previous year. A caution is that these responses might be due to other factors. For example, both the minimum wage and the consumption tax has been increased in October 2020. These policies might have depressed labor demand independent from the pandemic shock. Therefore we view 1% lower wage growth as the upper-bound on the absolute magnitude of response.

In summary, it seems the wage adjustment is proceeding, at best, very slowly, with only 10% of jobs lowering wage growth by 1% every month.

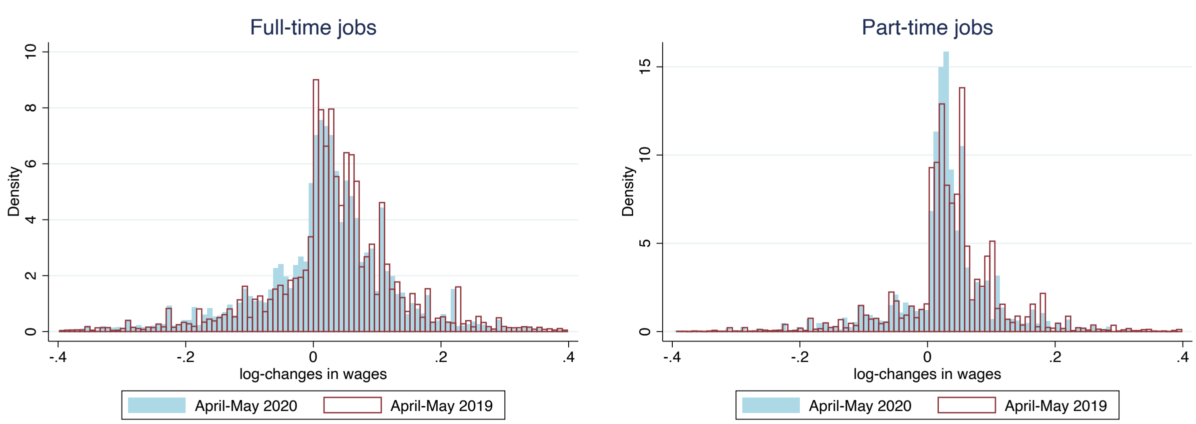


(a) Full-time jobs

(b) Part-time jobs

Figure 12: Probability of wage changes

Note: Figure 12 plots probability of changes in posted wages from the previous post. The left panel is for full-time jobs, and the right panel is for part-time jobs. The upward spike around October 2019 is due to the minimum wage increase.



(a) Full-time jobs

(b) Part-time jobs

Figure 13: Distribution of wage changes conditional on adjustment

Note: Figure 13 show the histogram of log-change in wages for vacancies posted in April 2020 (in blue) and April 2019 (in red) for full-time (left panel) and part-time (right panel) jobs. Zero growth are excluded.

4 Conclusion

In this paper, we presented five facts behind the collapse in job creation during the pandemic in Japan, using a newly available microdata. Our main goal was to provide descriptive facts. We thus deliberately refrained from drawing normative conclusions. A natural next step is to build a theoretical framework to explain thse empirical findings and to assess policy responses.

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Appendix

Correction to the previous version of the paper

In the previous version of our paper, we stated "there is little support for downward nominal wage rigidity," but we do find some support for the notion of nominal wage rigidity in the current version of the paper. The discrepancy between the current and the previous version comes from how we define wage growth. In this section, we first provide definitions of wage growth in the previous and current version. Then, we discuss how these two different definitions may lead different vies on the notion of nominal wage rigidity.

In the previous version, we defined wage growth as changes in posted wages for the same job, whose vacancies were also posted in the previous month. For example, when we showed wage growth in April 2020, we restricted samples to the jobs, whose vacancies were posted in March 2020. Using this definition, we found little support for downward nominal wage rigidity. For example, comparing the same jobs posted on March 2020 and April 2020, on average, 52% of the jobs are posted with the same wages, 23.8% of the jobs are posting with lower wages, and 24% of the jobs are posted with higher wages. In addition, there is no asymmetry in whether to increase or decrease wages, and these numbers are roughly the same.

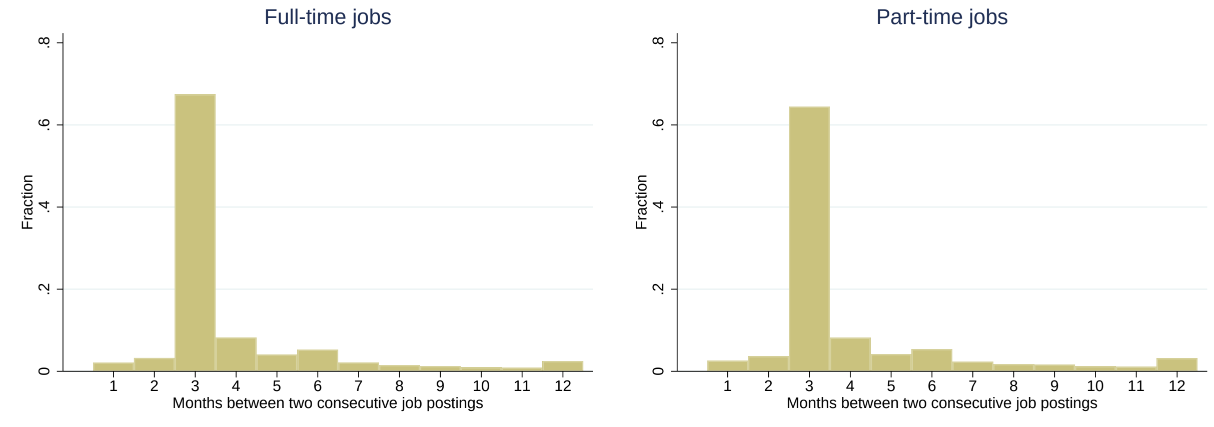
In the current version, we define wage growth as growth in wages between two consecutive vacancies posted by the same job. This sample restriction on jobs is broader than the one in the previous version, since we now see not only job vacancies posted in the previous month, but also the ones posted more than a month ago. For example, when we see wage growth in April 2020, we do not restrict samples to the jobs, whose vacancies were posted in March 2020 and include the jobs, whose consecutive vacancies were not posted in March 2020 but earlier than March 2020.[4] This distinction turns out to be substantial for two aspects as follows.

First, most of the jobs are posted in every three months, and the fraction of the jobs posted in two consecutive months is small. Figure 14 shows the distribution of months between two consecutive postings. For both full-time and part-time time jobs posted in 2020, over 70% had been posted exactly three months before. In addition, it is rare to have two consecutive postings in two consecutive months (about 3% for both full-time and part-time jobs). This bunching in three months is partially due to an institutional reason: Once a vacancy is posted, it is automatically active for three months.

Second, wage growth patterns are heterogeneous across wage change frequencies. Also, while there is less support for downward nominal wage rigidity for the jobs which post more frequently than every three months, there is some support for the jobs which post less frequently or equal to three months. Figure 15 shows the probability of changes in posted wages from the previous post by months between two consecutive postings. First, for the jobs whose vacancies are posted in the previous month as well, the probability of increase and that of decrease are roughly equal. This is consistent with our previous finding when we restricted samples to these jobs. Second, for the jobs whose consecutive vacancies are posted three months before, the probability of change is the smallest. Third, for the jobs whose consecutive vacancies are posted more than three months before, the probability of change becomes larger as the interval becomes longer, but that increase is

[4] Since we have data only after January 2019, we need to omit jobs, whose vacancies have not been posted more than once since Jan 2019. For that reason, we restrict our samples to the jobs, which have posted their vacancies at least once after January 2020. This may decrease the bias from data omission as we see in Figure 14, most jobs post vacancies in every three months, and it is rare to have a break longer than 12 months between two consecutive vacancies.

Figure 14: Distribution of months between two consecutive postings



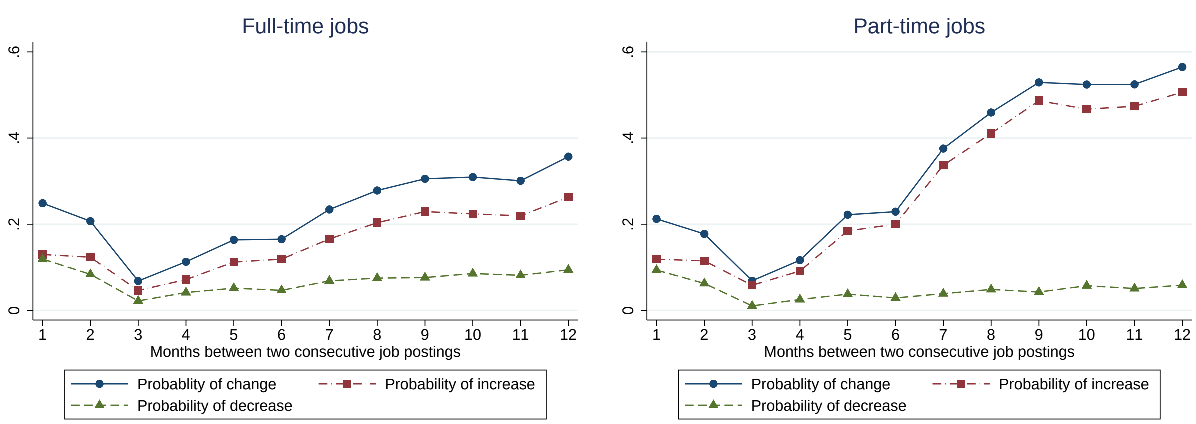
(a) Full-time jobs (b) Part-time jobs

Note: The figures show the histogram for the fraction of months between two consecutive job postings for full-time jobs (left panel) and part-time jobs (right panel), respectively. Samples are restricted to the jobs, which posted their vacancies at least one after January 2020. The job postings whose last consecutive vacancies were posted more than 12 months ago are added up to the ones of 12 months.

mostly from wage increase and little from wage decrease. This supports the notion of downward nominal wage rigidity in the longer horizons.

In summary, when we change the definition of wage changes by expanding the samples from job vacancies with monthly postings to those with less frequent postings, we find some support for the notion of downward nominal wage rigidity. This difference may come from heterogeneity in wage changes behavior across job postings frequencies. Then what would be a rationale for this relationship between wage changes and posting frequencies? We plan to keep exploring in an ongoing work, but one possibility is selection. As discussed above, once posted, job vacancies are automatically active for three months in the website. However some firms re-post jobs despite this automatically active status of job postings. This may be because these firms are hit by some shocks and need to adjust wages more often.

Figure 15: Probability of wage changes by months between two consecutive postings



(a) Full-time jobs

(b) Part-time jobs

Note: The figure plots probability of changes in posted wages from the previous post. x-axis is the month between two consecutive postings. Samples are restricted to the jobs, which posted their vacancies at least one after January 2020. The job postings whose last consecutive vacancies were posted more than 12 months ago are added up to the ones of 12 months.