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Stepping-stone effect of atypical jobs: Could the least employable reap the most benefits?

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Stepping-stone effect of atypical jobs: Could the least employable reap the most benefits?*

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Abstract

This article estimates the causal impact of atypical work on the probability of finding regular, durable employment and on wage gains. Using a novel administrative dataset on the employment and unemployment history of $1/25^{\text{th}}$ of French workers and the timing-of-events approach, we find a robust stepping-stone effect and no evidence of a lock-in effect. Starting atypical work during unemployment raises the likelihood of finding regular work by 87% in the following months, and has no effect on wage growth. Interestingly, this effect is stronger for workers with weaker ties with the labor market, such as those unemployed for a long period, older individuals or those who worked fewer hours in the year prior to the start of the spell.

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1 Introduction

With the parallel rise in part-time jobs, temporary contracts, and agency work, there is considerable interest in understanding how such atypical work arrangements affect the careers of workers seek regular, full-time work. After ample research, the question remains open, partly because of varying methodologies, contexts and data quality. This paper innovates by estimating the causal impact of atypical work on the transition to regular, full-time work using a novel French administrative dataset describing in detail the timing of workers' employment and unemployment history. We are able to restrict our analysis to individuals who explicitly declare seeking a permanent position, to focus on specific subgroups and to disentangle between lock-in effects, stepping-stone effects and wage growth effects. We define atypical work as a less desirable and hopefully temporary work situation such as part-time or temporary work with fewer monthly hours than full-time work. We use the exact timing and duration of labor contracts to identify when an individual returns to full-time, durable work.

For job seekers lacking the right skill set or experience, atypical work may help develop expertise, meet potential new employers, and eventually succeed in having a temporary position converted into a permanent contract. Agency work can also play a similar role (see Neugart and Storrie (2006) and Houseman et al. (2003)), as well as subsidized jobs (see Gerfin et al. (2005)). This is often referred to as the "stepping stone" effect of atypical work. However, atypical work may reduce the time available or the incentive to search for a more desirable job, the so-called "lock-in" effect. In many countries, the possibility of combining atypical work with partial unemployment benefits also raises the same moral hazard concerns as regular unemployment benefits do (see McCall (1996) and Ek and Holmlund (2011), for instance). Over time, these effects could trap workers in a string of atypical jobs and repeated unemployment spells and worsen labor market dualism.

All of these channels can exist simultaneously, but it is unlikely that they would impact all workers uniformly. Workers with lower job prospects are probably the ones most likely to benefit from stepping stones while for more skilled or experienced workers, atypical work could provide little benefits, or even prove detrimental. Little attention has been paid to this potential heterogeneity in previous works, a neglect which could lead to poorly targeted labor policies.

A final source of ambiguity is the variety of reservation wages which directly affects the length of unemployment spells. To shed light on the potential trade-off between speed and wage gain and capture the total utility gain from doing atypical work, we jointly model the impact of atypical work on the log hourly wage gain from one regular job to the next.

Our empirical approach is based on the well-known timing-of-events (TOE) approach (Abbring and Van den Berg, 2003) which captures the causal impact of atypical work by exploiting the randomness in the arrival of job offers. We were able to obtain privileged access to an administrative dataset of $1/25^{\text{th}}$ of the French population covering a 7-year period. This data contains detailed information on the duration and motives for which workers registered at *Pôle emploi*, the French employment agency, administrative records used to compute unemployment benefits, and detailed information on all their employers, including their working hours, annual earnings, and firm-specific identifiers. This allowed us to define unemployment spells with great precision and identify when workers returned to stable full-time work without suffering from the censoring bias often encountered in the empirical literature.

We find a surprisingly strong stepping stone effect, and no evidence of lock-in effects. Having done atypical work previously in an unemployment spell raises the monthly probability of finding regular work by 87% on average. We find larger effects for long-term unemployed workers, older workers, and those who work fewer hours in the year prior to the start of the spell. We find no effect of atypical work on subsequent hourly wages, suggesting that the benefits to workers may accrue entirely from reduced unemployed time.

The rest of the paper is organized as follows. Section 2 presents the empirical model and its assumptions and is followed by Section 3 which discusses several studies that have used similar approaches. Section 4, presents the data and details the construction of our variables of interest. Section 5 presents the results and Section 6 concludes.

2 The empirical approach

The main challenge in estimating the causal impact of atypical work on the rate at which workers find regular jobs is finding a proper counterfactual. A correlation between search effort for atypical work and for full-time work unexplained by observable factors could be spuriously interpreted as the effect of atypical work itself. More motivated job seekers may find both types of jobs more easily, leading to a positive correlation. In contrast, workers with ample savings or with highly sought-after skills may search only for permanent positions, while those who have been employed for a long period may become open to any type of work, giving rise to a negative correlation. For these reasons, workers entering atypical jobs may not be comparable to workers who do not. The measured effect could also be biased due to dynamic sorting since workers with a higher probability of finding regular work will tend to exit unemployment without having done atypical work.

Two main strategies have been proposed to address the risk of spurious correlation. One involves creating a synthetic comparison group by matching on observable criteria. The second, used here, is often referred to as the "timing-of-events" approach. Pioneered by Abbring and Van Den Berg (2003), it exploits the randomness of the timing of entry into a treatment to calculate its causal impact while controlling for unobservable stable individual characteristics. It is especially useful for applications involving administrative datasets which often lack important demographic information. For instance, household savings are key to explaining an individual's job search behavior (see Bloemen (2002)), but this variable is seldom available. This approach also dispenses with the need for exclusionary restrictions in the form of covariates affecting the relevant outcome only through treatment assignment such as in IV approaches. Because entering into atypical work and into regular work are similar events, finding an instrument that directly influences one but not the other may be challenging.

2.1 Timing of events

The fundamental assumption of the TOE approach is that events can be modelled as dynamic processes in which subjects don't know in advance the exact moment at which a treatment will begin, or when the outcome of interest will occur. This makes it well suited for the study of labor markets. From a worker's point of view, entry into treatment signifies starting a new atypical job. As in standard Diamond-Mortensen-Pissarides search and matching models, workers are presumed to search continuously and not to know in advance when their search efforts will result in a new match. There may be heterogeneity in terms of search effort or employability across workers and over time, which varies the underlying risk of finding work. But workers may not know in advance the exact moment at which an employer will hire them.

Finding regular work can be represented by a mixed proportional hazard model

$$\theta_R\left(t \mid x_t, a\left(t\right), V_R\right) = \lambda_R\left(t\right) \exp\left(x_t \beta_R + a\left(t\right) \gamma_R + V_R\right)$$

where θ_R is the risk of finding regular work and t is the time elapsed since the start of the unemployment spell. We discretize the time intervals by month, an obvious choice given that many datasets already record information on part-time work on a monthly basis.

The baseline risk of finding work as function of time $\lambda_R\left(t\right)$ is independent of other covariates and will be modelled as piecewise constants for 10 time intervals (choosing different time intervals did not affect the results significantly).

Control variables in the vector x_t can vary with time and their impact is captured by β_R. All of them are measured just before the start of the unemployment spell, except the local unemployment rate which is measured at the beginning of the new regular job. The variable of interest is $a(t)$, which equals 0 if a worker has never done atypical work during the spell (either worked part-time or accepting a temporary contract), and equals 1 as soon as atypical work occurred once during the spell. Since stopping atypical work is an endogenous decision, we will not differentiate the impact of doing atypical work currently and having done it previously in the spell in most specifications. The impact of atypical work is captured by γ_R and assumed to be effective only after atypical work has occurred. This "non-anticipation" assumption is crucial for identification since it is the source of randomness in starting atypical work.[1] Its impact, captured by γ_R, is not allowed to vary during and after atypical work; it is not heterogeneous in the benchmark specification.

The unobserved heterogeneity term V_R captures stable workers characteristics influencing the risk of finding regular work that are not included in x_t. As discussed earlier, V_R could be correlated with $a(t)$. Without V_R, the model is not identified since γ_R might spuriously capture the correlation between the probability of finding atypical work and the probability of finding regular work. To deal with this selection effect, Abbring and Van Den Berg (2003) propose to compute the correlation between treatment assignment and outcome using information contained in the data itself. The TOE approach models jointly the outcome and the treatment assignment, allowing for unobserved hetero-

[1] One could challenge this assumption since some workers might be informed a few months ahead of time of the start of an atypical job. In all likelihood, workers could reduce their search activity in the months preceding the start of their atypical jobs, leading to a slightly overestimated positive impact. However, since atypical jobs tend to start relatively quickly and since γ measures the difference in job finding rates for the whole period preceding and following the start of atypical work, such bias is probably small.

Alternatively, we may consider that the treatment starts at the moment at which the worker is informed that he will start a new job. This would be considered an information shock, as discussed by Abbring and Van Den Berg (2003). Unfortunately, this information was not available in our data.

Finally, note that if some workers are indeed informed in advance of the start of an atypical job, they could potentially reduce their search effort in the meantime. However, the bias to the coefficient of interest should be mild since it compares the whole period before starting atypical work with the whole period following it.

geneity in both processes. The instantaneous hazard rates are denoted θ_R for exiting unemployment and θ_A for entering treatment:

$$\theta_R\left(t \mid x_t, a\left(t\right), V_R\right) = \lambda_R\left(t\right) \exp\left(x_t \beta_R + a\left(t\right) \gamma + V_R\right)$$

$$\theta_A\left(t \mid x_t, V_A\right) = \lambda_A\left(t\right) \exp\left(x_t \beta_A + V_A\right).$$

The unobserved heterogeneity terms V_R and V_A capture other worker characteristics influencing the risk of finding regular work and the risk of entering atypical work.[2] They are allowed to be correlated, which captures the potential endogeneity of the outcome with the probability of starting atypical work.[3]

Part of the positive impact of atypical work may also be in terms of wage gains in the next regular job. To isolate the specific impact of atypical work on wages, we follow the approach of Caliendo et al. (2016), although we model log wages in first-difference rather than in levels. By introducing wages in levels, Caliendo et al. (2016) allow unobserved heterogeneity to control for the correlation between average earning potential and the likelihood of entering atypical work. This is useful given that atypical work or *mini jobs* could be filled by unskilled workers who would not earn substantially more in a regular job. But first differencing wages as we do has the advantage of removing all individuals' unobserved stable earning potential and allows unobserved heterogeneity to capture unobserved correlation between earning trends and the probability of entering atypical work. Such correlation could go either way. More motivated individuals could both achieve better wage gains and find atypical work more easily. On the contrary, liquidity-constrained individuals could

[2] A common and equivalent formulation replaces V_R and V_A with $\ln\left(V_R\right)$ and $\ln\left(V_A\right)$.

[3] As discussed in Cockx et al. (2013), echoing Chamberlain (1980) and Wooldridge (2002) (p. 488), one consequence of this heterogeneity is its potential correlation with the explanatory variables. In that case, these variables would not carry a structural interpretation. Nevertheless, identification of the treatment effect remains unaffected.

be more willing to take temporary atypical work and have a lower reservation wage for their next permanent job.

Both atypical work $a(t)\gamma_W$ and the unobserved heterogeneity V_W are assumed to influence log hourly wage gain linearly:

$$d\ln w = x_t\beta_W + a(t)\gamma_W + V_W + \varepsilon_W.$$

The impact of the controls are captured by β_W.

The model is estimated by maximum likelihood. The likelihood contribution of a single spell for an individual can be expressed as the product of the three processes:

$$L(V_R, V_A, V_W) = \begin{array}{c} \left(\theta_R\left(\tau_R \mid x_{\tau_R}, a(\tau_R), V_R\right)\right)^{I(\tau_R \leq 36)} \exp\left[-\int_0^{\min(\tau_R,36)} \theta_R\left(t \mid x_t, a(t), V_R\right) dt\right] \\ \times\theta_A\left(\tau_A \mid x_{\tau_A}, V_A\right)^{I(\tau_A \leq \tau_R \cap \tau_A \leq 36)} \exp\left[-\int_0^{\min(\tau_A,\tau_R,36)} \theta_A\left(t \mid x_A, V_A\right) dt\right] \\ \times\left(\frac{1}{\sqrt{2\pi\sigma^2}} \exp\left(-\frac{\hat{\varepsilon}_W^2}{2\sigma^2}\right)\right)^{I(\tau_R \leq 36)}, \end{array}$$

where $\tau_R \in (0, \infty)$ denotes the time at which a spell is completed and $\tau_A \in (0, \infty)$ is the time at which an individual enters atypical work. All spells are censored after 36 months. In the wage equation, $\hat{\varepsilon}_W = d\ln w - x_t\hat{\beta}_W + a(t)\hat{\gamma}_W + \hat{V}_W$ is the difference between the observed and predicted wage increase when a regular job is found within 36 months of losing the previous job.

Many individuals in our data experience multiple spells. In the benchmark specification, V_R and V_A do not vary between spells, which make several assumptions concerning the mixed proportional hazard model no longer necessary. Abbring and Van Den Berg (2003) show that if V_R and V_A are stable for the same individual over different spells and if spells are independent of other spells given x, the proportionality assumption becomes less critical. The lack of independence between x, V_R, and V_A is also less problematic (van den Berg, 2001). We assume stability for the main specification, but let V_R, and V_A be spell-specific as robustness check.

The unconditional likelihood contribution of an individual with m unemployment spells is obtained by integrating $L(V_R, V_A)$ over V_R and V_A:

$$L = \int \int \int \left(\prod_{i=1}^{m} L_i \left(V_R, V_A, V_W \right) \right) dG \left(V_R, V_A, V_W \right).$$

It is possible to be flexible in the specification of the unobserved heterogeneity. We specify the distribution as bivariate discrete (see Lindsay (1983), Heckman and Singer (1984), Aitkin and Rubin (1985)). Because G is a finite mixture model with discrete support points, we can rewrite L as

$$L = \sum_{j=1}^{J} \sum_{k=1}^{K} \sum_{q=1}^{Q} \left(\pi_{j,k} \prod_{i=1}^{m} L_i \left(v_{Rj}, v_{Ak}, v_{Wq} \right) \right)$$

where $\pi_{j,k,q}$ is the probability of the individual having the vector of heterogeneity values (v_{Rj}, v_{Ak}, v_{Wq}) ,an occurrence of the random vector (V_R, V_A, V_W). The number of classes for the hazard heterogeneity of exiting unemployment, starting atypical work and wage gain are J, K and Q, respectively. Note that v_{R1}, v_{A1} v_{W1} are the constant terms of the reference group, and v_{Rj}, v_{Ak} and v_{Wq}, for $j, k, q \neq 1$, capture the heterogeneous risk of classes j, k, q in comparison with the reference group.

Because the weights $\pi_{j,k,q}$ are estimated as part of the likelihood, they are specified as logit to ensure that each probability is bounded between zero and one and that $\sum_{j=1}^{J} \sum_{k=1}^{K} \sum_{q=1}^{Q} \pi_{j,k,q} = 1$. Hence,

$$\pi_{j,k,q} = \frac{\exp \left(p_{j,k,q} \right)}{\sum_{j=1}^{J} \sum_{k=1}^{K} \sum_{q=1}^{Q} \exp \left(p_{j,k,q} \right)}.$$

One of the probabilities $\pi_{j,k,q}$ is residual with $p_{j,k,q} = 0$.

Montecarlo studies by Gaure et al. (2007) have shown that the TOE approach is very robust in computing causal effects. This procedure is designed to control for selection bias arising from stable individual characteristics. However, in addition to V_R and V_A, there could also be unobserved, time-varying factors co–influencing the risk of finding atypical work and regular work. As a concrete example, imagine that after attending a job fair, a worker accepts an offer for a part-time job. Soon after, the worker also obtains an offer for regular work. If the econometrician cannot observe the job fair, its effect on the probability of finding regular work will be spuriously attributed to the temporary part-time job. Unfortunately, this eventuality is impossible to detect in an administrative dataset containing little information about search efforts. Therefore, a necessary exclusionary restriction is the absence of time varying unobserved heterogeneity at the individual level. A similar mechanism may be at play if, for instance, macroeconomic conditions influence the probability of finding both types of jobs. To account for this effect, we controlled for time- and region-specific unemployment rates.

An important concern is identifying the proper number of support points of the distribution of V_R, V_A and V_W. Considering the nonparametric nature of both heterogeneity and baseline time dependence, Baker and Melino (2000) have indicated a risk of bias for the duration dependence and the coefficients of unobserved heterogeneity. To identify the number of support points, we first adopted an approach commonly used in the literature, adding support points based on the improvement of model fit (based on the Akaike information criterion, as recommended by Gaure et al. (2007), or on the Bayesian information criterion).[4] The presence of time-varying covariates is also helpful in identifying heterogeneity in the data (see Brinch, 2011). Multiple spells play a similar role (see Gaure, 2007). As is discussed in the results section, even with a large sample, there is ultimately limited heterogeneity in the data, and at most two support points per equation could be added, for a maximum of 2*2*2=8 total potential support points. However, most of the associated weights converged to zero. To make sure that the main results are robust, we generated 10

[4] For specifications with spell-specific heterogeneity specifications, we follow Li and Smith (2015).

different sets of starting weights and kept the estimates with the highest likelihood. Most converged to the same values for the weights and all showed quasi-identical point estimates for γ_R, giving high confidence in the robustness of our results.

3 Timing-of-events and time-varying treatment effects in the literature

The impacts of atypical work and partial unemployment insurance or similar employment-conditional benefit programs have been studied in a growing number of countries. We review the main contributions to this literature.

Using Finnish data, Kyyrä (2010) finds that workers benefitting from partial unemployment benefits have a greater likelihood of entering full-time work after the program, and that the likelihood of finding work during the program is not reduced. Using Danish data, Kyyrä et al. (2013) find that exit rates from unemployment are reduced for workers receiving partial benefits, but slightly increased post treatment. A similar pattern is found by Fremigacci and Terracol (2013) in France, who also measure a reduced probability of finding regular work for workers currently receiving partial unemployment benefits, but markedly increased afterwards.

Differentiating between an in-treatment effect and a post-treatment effect is not trivial from a methodological point of view. As Cockx et al. (2013) point out, if the entry into treatment can lead to selection biases, so can exit from treatment. For instance, if there is heterogeneity in the chance of finding regular employment, workers still unemployed after exiting a temporary job will not have the same composition as those at the start of the job. The post-treatment effect will be affected by dynamic sorting. Fremigacci and Terracol (2013) address this issue by modelling both time-to-treatment and time-in-treatment. However, to be identified properly, the time-in-treatment model must also rely on the same assumptions required for the validity of the TOE approach. Crucially, a worker must not anticipate the end of a temporary contract, a strong assumption given that some workers may choose to quit voluntarily at a certain date in the future and that a substantial number of part-time jobs may have a predefined duration. This is especially true for France, which has stringent rules governing the maximum duration of temporary contracts.

To circumvent the possible endogeneity of exiting treatment, Cockx et al. (2013) do not differentiate between in- and post-treatment effects, but seek to identify a potential lock-in effect indirectly by allowing the treatment effect to vary over time. Using Belgian data, they find a positive impact of the program and no evidence of a lock-in effect. Richardson and van den Berg (2013) point out that, just like the baseline risk, an apparent variation in the treatment effect over time could be the result of compositional effects. If a program has a lock-in effect on some workers and a stepping stone effect on other workers, the composition of the workers participating in the program will change over time and the perceived treatment effect will inevitably decline as the group exhibiting the stepping stone effect exits to employment more quickly (Cockx et al. (2013) measure a negative but insignificant time dependence of the treatment effect). To distinguish between a time-varying and a heterogeneous treatment effect, Richardson and van den Berg (2013) prove identification of a time-dependent treatment effect and an unobserved heterogeneity in the treatment effect. In addition to Richardson and van den Berg (2013)'s methodology for identifying varying treatment effects over time, we estimate a specification in which the treatment effect depends on the time at which an individual entered treatment. This is potentially very helpful from a policy perspective in order to know whether active labor market policy should or should not target recipients that may have been unemployed for a long time.

4 The data

An important contribution of our work is in terms of data quality. The FH-DADS dataset is a combination of three matched French administrative data sources: the FH, the D3, and the DADS, tracking employment and unemployment history of $1/25^{\text{th}}$ of the workforce from 1996 to 2004. Details about the advantages and limitations associated with the three are described in Appendix B. The following section provides exact definitions of unemployment spells and of atypical work.

4.1 Unemployment spells

A spell starts when an individual who has not been in a spell during the previous month begins a new job-seeking process with the employment agency.[5] We are interested in regular and stable employment. A worker is considered to have returned to work when the following three conditions are met:

1. From the DADS, we observe a month during which the worker has worked at least 140 hours.[6]

2. For the following six consecutive months, the worker works at least an average of 100 hours per month.[7]

3. The worker is no longer registered at the employment agency at the end of the six-month

[5] Working with the FH dataset, we have noticed that some spells start the day after a previous spell ends. These 'new' registrations seem to serve purely administrative purposes since only one parameter usually changes from one spell to the next. Thus, a spell that started immediately after a previous spell was considered the continuation of the previous one. Consequently, when many spells were merged together, the values of the variables at the start of a spell refer to the first of these 'sub-spells'.

[6] Because the DADS includes the beginning date and the end date of each job accepted over the course of the year and the total hours worked, we estimate the work hours in every month employed based on the total hours divided by the number of months worked. Total hours worked for all jobs during a month are simply the sum of the hours of every job.

[7] The worker is allowed to have several jobs or change employers during the period.

This ex-post way of observing a return to regular employment is similar to Cockx and Picchio (2012). It is an imperfect metric because even a seemingly stable job could nonetheless end abruptly, or a precarious job could last for a long time. This could be interpreted as a mismeasurement of the true duration, which would lead to conservative biases of hazard ratios (Meier et al., 2003). Nevertheless, we argue that the labor market information available for our investigation makes it possible to measure spells with greater precision than previously done in the literature.

period.[8]

We tested different definitions for ending of unemployment spells, some of which are shown as robustness checks, but the results were not affected in notable ways. This definition of unemployment spells results in longer average spell length than is typically reported in the literature. This is due in part to our emphasis on stable jobs with sufficient average work hours, contrary to other authors who often cannot observe work hours in the new jobs, or do not observe how long the new job lasts. It is also partly due to the comparatively less dynamic French labor market with longer unemployment spells.

All censoring is non-informative. Censored spells are those ending after December 2004 or lasting longer than 36 months, the maximum length considered. There is thus no correlation between the length of a spell and the moment of censoring.

4.2 Atypical work

As explained in the previous section, our benchmark specification identifies a job seeker as "treated" if at least one month of atypical work has been done previously in the spell. An intuitive definition of atypical work would be any work (either part-time or temporary) that does not satisfy the definition of regular, full-time employment. Our main treatment variables will be atypical work as defined in the FH dataset on job seekers registered at *Pôle emploi,* the French employment agency. They are required to declare all professional activity on a monthly basis, regardless of whether they are eligible for unemployment benefits. Even after finding part-time or temporary work, they typically remain registered with the agency because it offers several advantages such as counselling, training, and internships, and could accelerate the process of qualifying for UI benefits in the future.

[8] We only require deregistration at the end of the period because it may take some time for a worker to gain confidence in the stability of the new job and end his interaction with the agency, or for the agency to recognize that a worker is no longer using its services.

An alternative definition of atypical work will also be used as a robustness test. The D3 dataset of UI benefits recipients also provides information on monthly work activity. The program called *Activité réduite* allows unemployed workers to collect partial unemployment benefits while working less than full-time, up to 136 hours in the month or 70% of the previous wage. A worker is considered "treated" as soon as a positive amount of UI benefits has been received while working a positive number of hours during the month. We exclude individuals reported as having worked with zero hours, and those having worked long enough or earned enough income to have their UI benefits suspended completely.[9] Contrary to our preferred treatment variable, non-UI recipients or workers whose UI benefits have expired cannot be observed, which makes this variable less attractive. Results obtained using this variable were nevertheless similar to those with our preferred treatment variable.

Figure 1 shows the empirical hazard rates into atypical work, the hazard rate out of unemployment after atypical work has occurred in the spell, and the hazard rate out of employment if no atypical work has been done according to the number of months since the start of the spell. For clarity, only the 95% confidence interval is shown. The figure is created from the entire sample.

All risks were fairly high in the first months of the spell and decreased substantially over time. Of course, we cannot know at this stage whether this is due to a decrease in search intensity or to a composition effect because highly active job seekers left the sample sooner. Workers who have already done atypical work show a higher probability of exiting unemployment, suggesting a positive impact of accepting atypical work on the probability of finding permanent work. This difference may be driven by selection effects and cannot be interpreted as causal.

Figure 2 shows the resulting stocks for workers who start atypical work (are treated) and those who do not during the spell. The complete dataset contained 320,206 spells. After 36 months, $12.9\% + 6.5\% + 24.2\% = 43.6\%$ of all spells had included at least one month of atypical work, and

[9] To be eligible for partial unemployment insurance during the studied period, a job seeker must not have worked more than 136 hours per month or have earned more than 70% of the reference income.

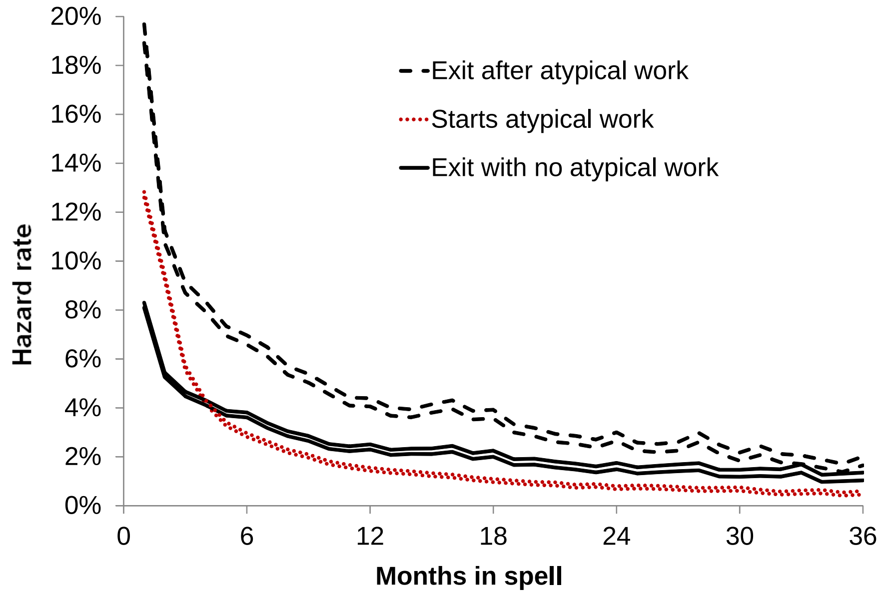

Figure 1: Probabilities of starting atypical work and finding regular work

Note: Only 95% confidence intervals are shown.

55.6% of the workers had exited to full-time employment without being censored. In total, only 24.2% + 29.2% = 53.5% of all entrants had found a job without being censored after three years. At that time, the total fraction of censored spells was 12.9% + 21.6% = 34.5%. Note that those who eventually returned to unemployment after exiting were not recounted as unemployed.

4.3 Wage and control variables

The wage variable is the change in average after tax hourly wage in the six months preceding the spell and six months after the spell. Control variables include an interaction of sex and marital status, considering that the impact of marital status on the labor market probably differs by sex. We also include 8 age dummy variables, 10 qualification dummy variables, the number of children, a dummy variable for non-French European citizens, and a dummy variable for citizens of non-European countries. As in Kyyrä et al. (2013), we also include information on work history during the year prior to the start of an unemployment spell because it might influence a worker's eligibility for receiving

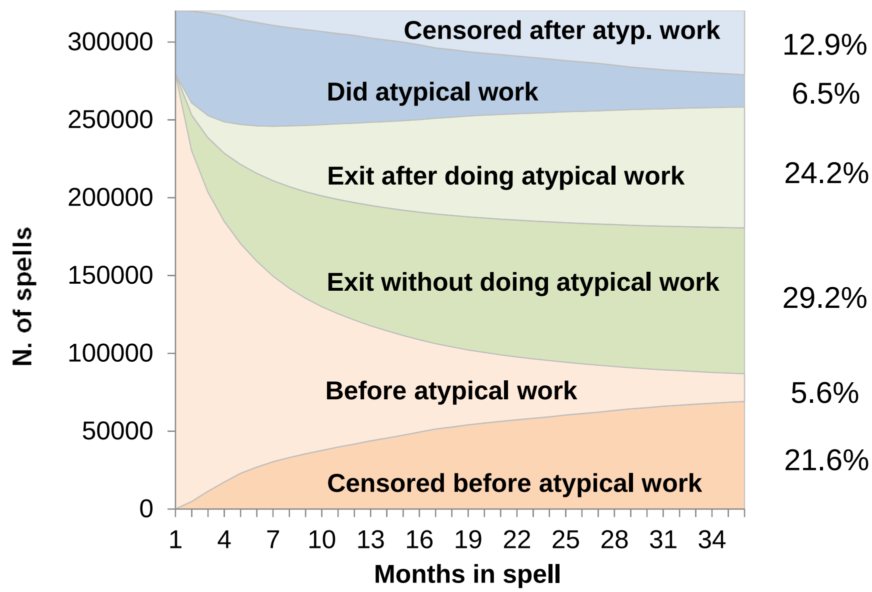

Figure 2: Number of unemployed workers according to time in spell

benefits and his motivation to start atypical work. Specifically, we include the total hours worked, the number of months during which benefits were collected, and the number of months registered at the employment agency. We also include the quarterly unemployment rate at the local level (over 300 employment zones), a variable that changes during the course of a spell.

We keep only individuals who are immediately available for work, who state that they are looking for a full-time job. We exclude special categories such as show business workers with intermittent career paths. We retain spells with complete records only.

Table 6 (see Appendix A) shows descriptive statistics for the entire final sample. As high as 44% of unemployed workers eventually do atypical work at some point in their unemployment spell. Men and unmarried workers are overrepresented. Half of our sample is between 20 and 30 years old. In the year preceding the start of the spell, job seekers had worked on average 1083 hours, had been collecting UI benefits for 0.38 months, and had been registered at the employment agency for 1.44 months.

4.4 Alternative specifications, robustness checks and subgroups

As robustness check, we show a specification without individual controls and one without unobserved heterogeneity. We also divide atypical work between those who worked 78 or fewer hours during the month (short) and those that worked more (long) and should presumably expect better labor market outcomes.

Since atypical work may be beneficial in the long run while having a lock-in effect in the short run, we also include a specification with a dummy indicating that atypical work is done during the current month. We are mindful of the potential caveats of measuring a causal impact with the presence of duration dependence, as discussed in Section 3. To avoid bias due to dynamic sorting, we present estimates where the time elapsed since the entry into atypical work is decomposed into a series of lags, regardless of whether a worker is still doing atypical work or not. We show a specification in which atypical work is interacted with unobserved heterogeneity, and one with a full set of interactions with all control variables, as suggested by Richardson and van den Berg (2013), to identify the dynamic treatment effect. Finally, we consider a specification in which we let the effect of atypical work vary as a function of the time elapsed since the start of the spell. Presumably, atypical work could be especially beneficial for workers who have spent a long time unemployed and may be viewed as less employable by potential employers. If so, the long-term unemployed could represent a good target for active labor market policy.

As additional robustness checks, the model is estimated using the second variable for atypical work, joint with UI, extracted from the D3 dataset. We also experiment with two alternative specifications for the definition of a new stable job. Our second – less stringent – specification requires a worker to work at least 100 hours per month in the three following months instead of six. The third – more stringent – requires 140 hours per month for the six following months to be considered as permanently employed with a stable job.

The FH-DADS has the advantage of providing a large volume of data that makes it possible to focus our attention on various subgroups and various treatment variables while enjoying appreciable sample sizes. This could facilitate fine-tuning active labor market programs and target job seekers who are most likely to benefit from atypical jobs. We also consider specific age groups and occupation categories (according to French administrative classification), which may have varying difficulty in finding new work and, thus, benefit more from a stepping stone effect of atypical work.

We also split the sample in terms of the number of hours worked, the number of months registered at the employment agency, and the number of months unemployment benefits were collected in the year prior to the start of the spell, since this may affect eligibility to UI benefits and may predict the ability of a worker to find work quickly.

5 Results

Table 1 shows results for the benchmark specification, with coefficients displayed as hazard ratios for the probability of finding regular work in column 1 and the probability of finding atypical work in column 2. The wage gain results are displayed in column 3. Coefficients in bold are significant at the 5% level and their standard errors are displayed on the right.

Atypical work increases job finding rates by 87%, a large impact in light of the literature and one that clearly supports a stepping stone effect rather than a lock-in effect.

Doing atypical work does not affect starting wage in the next regular job. The benefits of atypical work appear to be mainly in terms of probability of finding work. Similar results were found by Fontaine and Rochut (2014) using dynamic matching on the same dataset, and by Godøy and Roed (2016). In contrast, Booth et al. (2002) and Caliendo et al. (2016) did find a negative impact for the starting wage.

Table 1: Benchmark results for a_{Aty}

Process	Finding regular work 1		Entering atypical work 2		Wage gain in regular work 3	
	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
Did atypical work	**1.87**	0.042			-0.003	0.004
Spell dur. 1-2 mos	**2.66**	0.211	**3.61**	0.295		
Spell dur. 3-4 mos	**1.93**	0.151	**2.42**	0.198		
Spell dur. 5-6 mos	**1.76**	0.137	**1.96**	0.162		
Spell dur. 7-8 mos	**1.49**	0.117	**1.8**	0.15		
Spell dur. 9-11 mos	**1.4**	0.107	**1.34**	0.112		
Spell dur. 12-14 mos	**1.33**	0.102	1.14	0.098		
Spell dur. 15-17 mos	**1.34**	0.104	**1.26**	0.109		
Spell dur. 18-21 mos	**1.18**	0.092	1.09	0.096		
Spell dur. 22-25 mos	1.11	0.089	1.04	0.096		
Spell dur. 26-30 mos	**1.38**	0.113	1.14	0.11		
Unempl. rate (reg.×yr.)	1.01	0.005	**1.03**	0.004	-0.001	0.001
Under 20	**3.04**	0.693	**4.5**	1.086	**0.229**	0.069
Aged 20 to 29	**3.97**	0.882	**4.38**	1.038	0.084	0.067
Aged 30 to 39	**3.09**	0.686	**3.58**	0.849	0.044	0.067
Aged 40 to 49	**2.48**	0.55	**3.28**	0.778	0.033	0.067
Aged 50 to 59	1.37	0.306	**1.88**	0.446	0.032	0.067
Single man	**0.75**	0.02	**0.86**	0.023	**0.035**	0.007
Single woman	**0.66**	0.019	**1.11**	0.03	**0.034**	0.008
Divorced man	**0.84**	0.037	**0.71**	0.033	0.008	0.013
Divorced woman	**0.59**	0.027	1.03	0.039	**0.046**	0.013
Married woman	**0.59**	0.016	**1.08**	0.027	**0.027**	0.008
Hours wrk p.y.*1e-4	**1.07**	0.005	**1.02**	0.005	**0.002**	0.001
Months UI p.y.	1	0.008	**0.98**	0.006	0.002	0.002
Months Agcy p.y.	1.01	0.005	**1.07**	0.004	0	0.001

Number of children	**0.97**	0.009	0.98	0.009	0.003	0.003
European (non-Fr)	**0.89**	0.054	**0.87**	0.05	0.01	0.017
Non-European	**0.77**	0.028	**0.73**	0.025	0.011	0.01
Qualification not specified	**1.72**	0.07	**0.52**	0.031	-0.017	0.011
Routine tasks	**0.53**	0.034	1.09	0.062	-0.029	0.018
Specialized worker	**0.68**	0.027	**1.28**	0.048	0.012	0.011
Qualified workers	**0.86**	0.03	**1.49**	0.053	-0.009	0.01
Highly qualified workers	1.07	0.048	**1.39**	0.065	-0.024	0.012
Non-qualified employees	**0.63**	0.021	**1.1**	0.036	-0.006	0.009
Qualified employees	**0.85**	0.024	**1.27**	0.037	**-0.023**	0.008
Technicians	**1.08**	0.039	**1.5**	0.055	-0.018	0.01
Administrators	1.04	0.049	**1.23**	0.061	-0.019	0.014
v	**0.01**	0.002	**0**	0.001	-0.055	0.068
$\ln(\sigma)$					**-1.24**	0.006
$v_2 - v_1$	**-2.06**	0.041	**0.67**	0.046	**-1.18**	0.028

	Coef.	S. E.		Coef.	S. E.
$\pi_{1,1,1}$	**0.808**	0.031	$\pi_{2,1,1}$	0	0
$\pi_{1,1,2}$	0	0	$\pi_{2,1,2}$	0	0
$\pi_{1,2,1}$	0	0	$\pi_{2,2,1}$	**0.122**	0.051
$\pi_{1,2,2}$	0	0	$\pi_{2,2,2}$	0.07	0.023

					Coef.	S. E.
Nb Observ.	668,133	Log Likelihood	-168,284.97	$\mathrm{Corr}(V_R, V_A)$	0.564	0.147
Nb Indiv.	50,000	A. I. C.	336,678.93	$\mathrm{Corr}(V_R, V_W)$	0.564	0.147
Nb Spells	53,234	B. I. C.	338,031.87	$\mathrm{Corr}(V_A, V_W)$	1	0

Note: Col. 1 and 2 show hazard ratios (except for $\pi_{i,j,k}$). Standard errors next to coefficients. Coefficients sig. at the 5% level in bold.

The estimates for the control variables have intuitive interpretations. Younger workers, especially those between 20 and 29 years old, have a higher probability of exiting unemployment as well as a higher likelihood of finding atypical work, probably reflecting both higher levels of education, general skills and less experience, which makes them more mobile and opens more opportunities. Fewer savings could also motivate them to find a job more quickly.

Men, especially married (the reference group), find regular work more quickly. On the other hand, women, especially single, find atypical work more rapidly than men do.

Workers who have worked more hours in the year preceding the start of the spell find a new job more easily, especially regular ones. They may be more engaged with the labor market and are potentially recalled more often to their old job.

Non-French and especially non-European job seekers find regular and atypical work with lower probability.

An interesting pattern emerges when looking at qualifications. Compared to executives, the reference group, many sets of qualifications give workers a lower probability of finding regular employment, but a higher probability of finding atypical work.

Even with a simple 2*2*2 specification of unobserved heterogeneity, most support points converge to zero, reflecting mild unobserved heterogeneity in the data. There is a weak positive correlation between V_R and V_A and between V_R and V_W, suggesting individuals who find regular jobs faster may also find atypical jobs faster and gain wage on their next employment.

5.1 Robustness checks

Individual controls are removed in columns 1 and 2 of Table 2, keeping as control only local unemployment rate. The magnitude of the main effect is slightly reduced, but the unobserved heterogeneity

Table 2: Impact of atypical work, robustness tests

	No controls				No unobs. heterog.			
	Finding regular work		Wage gain in regular work		Finding regular work		Wage gain in regular work	
	1		2		3		4	
	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
Did atyp. work	**1.61**	0.044	-0.00665	0.005	**2.00**	0.031	-0.00649	0.005
$v_2 - v_1$	0.12	0	**-1.14**	0.008				
Controls	No, except unemp rate.				Yes			
		Coef.	S. E.					
π_{111}		**0.557**	0.0174					
π_{112}		0.063	0.00198					
π_{222}		**0.379**	0.0194					
Other $\pi_{i,j,k}$		0	0					
N. obs.	668,133				668,133			
Log Likelihood	-172,041.35				-169,172.57			
A. I. C.	344,114.71				338,449			
B. I. C.	344,511.9				339,740			

Notes: Estimates show hazard ratios. Standard errors next to coefficients. Coefficients sig. at the 5% level in bold. Same set of controls as the benchmark specification, including duration dependence dummies. Duration to finding atypical work is still modeled, but not shown.

does not become richer. In fact, for finding regular work, the coefficient of unobserved heterogeneity $v_2 - v_1$ is not significantly different from zero.

Columns 3 and 4 confirm the mild impact of unobserved heterogeneity on the main result. Even when removed completely, the measured impact of atypical work stays similar. The Akaike and the Bayesian information criteria significantly select the benchmark specification over these alternative specifications.

Table 3 splits atypical work between fewer than 78 hours or more than 78 hours in a single month.

As before, this 'treatment intensity' only reflects total hours and does not distinguish between jobs with longer daily hours or temporary jobs with more days during the month. For atypical work performed in the past, the variable "Did atypical work long" takes value 1 if the spell contains more months of atypical work with more than 78 hours than months with fewer than 78 hours and the reverse is true for "Did atypical work short". As could be expected from a stronger treatment, column 1 show that individuals benefit twice as much from working more hours during the month. Interestingly, column 2 suggests that atypical work of short duration may actually have a small negative impact on the starting wage in the next regular work. Column 3 and 4 show the impact of having done atypical work and still doing it right now. Contrary to a lock-in narrative, columns 3 and 4 suggest that the positive impact of doing atypical work is actually stronger in the short term. The total effect is multiplicative, so the point estimate hazard ratio of doing atypical work of longer than 78 hours in the current month is $1.67 * 1.29 - 1 = 115.43\%$, while that of fewer than 78 hours is $1.21 * 1.44 - 1 = 74\%$. Not only does atypical work with fewer hours have a smaller immediate impact, but it vanishes more quickly.[10]

5.2 Time-varying impact of atypical work

Table 4 shows results for specifications allowing the effect of atypical work to vary over time. Column 1 allows the impact to vary since the start of atypical work and includes an interaction term of atypical work with the unobserved heterogeneity. Column 2 also interacts atypical work with all other explanatory variables as specified by Richardson and van den Berg (2013). Note that for columns 1 and 2, all coefficients are multiplicative. In column 1 for instance, for a worker who has done atypical work 4 months ago in the baseline group, atypical work boosts the probability of finding

[10] Note that the modeling of the time-varying impact of atypical work in columns 3 and 4 is not immune to dynamic sorting effects, which we address in the next table.

Table 3: Impact of atypical work, shorter or longer than 78 hours/month

	Length of aty. work				Length aty. work × doing now			
	Finding regular work		Wage gain in regular work		Finding regular work		Wage gain in regular work	
	1		2		3		4	
	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
Did atyp. work short	**1.45**	0.037	**-0.0119**	0.006	**1.21**	0.036	-0.00597	0.008
Did atyp. work long	**1.96**	0.05	-0.000741	0.006	**1.67**	0.05	-0.00371	0.007
Doing atyp. work short					**1.44**	0.045	-0.0124	0.009
Doing atyp. work long					**1.29**	0.039	0.00726	0.009
Controls	Yes				Yes			
π_{111}		**0.17**	0.0114			**0.17**	0.114	
π_{222}		**0.83**	0.0114			**0.83**	0.114	
Other $\pi_{i,j,k}$		0	0			0	0	
N. obs.	668133				668133			
Log Likelihood	-168553.06				-168463.66			
A. I. C.	337216.11				337041.31			
B. I. C.	338581.46				338456.31			

Notes: Estimates show hazard ratios. Standard errors next to coefficients. Coefficients sig. at the 5% level in bold. Same set of controls as the benchmark specification, including duration dependence dummies. Short/long mean 78 or fewer/79 or more hours worked during the month. Note: duration to finding atypical work is still modeled, but not shown.

regular work by $2.022 * 0.707 * 0.852 * 0.829 - 1 = 0.9\%$. For column 2, the baseline effect of atypical work cannot be interpreted directly due to the interaction terms and must be estimated. At mean value of all controls, the average probability of finding regular work without having done atypical work is 1.57% and it is 3.58% after having done atypical work. This 128% increase is slightly larger than in the benchmark specification. Interestingly, even though only the specification of column 2 properly accounts for dynamic sorting, both columns 1 and 2 convey a similar message, echoing columns 3 and 4 of Table 3: the positive impact of atypical work tends to fade away as time passes.

Column 3 shows the impact of atypical work as function of the time since job loss. The impact is high from the start, as was already suggested by Figure 1, but the impact clearly increases as the spell gets longer, up to 113% for spells of 31-36 months. Considering that long-term unemployed workers have a very low job finding rate, it is not surprising to see them benefit from a stronger stepping stone effect. These results echo the literature on long-term unemployment and job finding. For instance, in the field experiment by Eriksson and Rooth (2014), workers unemployed for more than 9 months face a negative stigma from potential employers, but this stigma goes away as soon as the spell ends, regardless of the skill level of the job.

5.3 Alternative specifications and subgroups

Table 5 presents the results for various robustness checks and specific subgroups. Line 1 shows the result of using the alternative atypical work variable, which by construction only includes work while receiving partial unemployment insurance. The impact is still positive and significant, but smaller in magnitude. This may reflect the fact that UI claimants have higher reservation wages than non-claimants. Lines 2 and 3 show that alternative definitions of regular employment didn't change markedly the effect of atypical work. Line 4 also shows no great difference when the unobserved

Table 4: Impact of atypical work on finding regular work, time-varying impact of atypical work

	Lags		Lags + interact.			
	1		2		3	
	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
Did atyp. work	**2.022**	0.05	0.996	-1.046		
Had done atyp. wrk. 2 mths ago	**0.707**	0.024	**0.697**	0.024		
Had done atyp. wrk. 3 mths ago	**0.852**	0.028	**0.832**	0.028		
Had done atyp. wrk. 4 mths ago	**0.829**	0.035	**0.797**	0.034		
Started atyp. wrk. at mth 1-2					**1.735**	0.048
Started atyp. wrk. at mth 3-4					**1.978**	0.069
Started atyp. wrk. at mth 5-6					**1.87**	0.079
Started atyp. wrk. at mth 7-8					**1.927**	0.093
Started atyp. wrk. at mth 9-11					**1.952**	0.103
Started atyp. wrk. at mth 12-14					**1.837**	0.122
Started atyp. wrk. at mth 15-17					**1.978**	0.148
Started atyp. wrk. at mth 18-21					**1.745**	0.159
Started atyp. wrk. at mth 22-25					**1.837**	0.231
Started atyp. wrk. at mth 26-30					**1.461**	0.245
Started atyp. wrk. at mth 31-36					**2.13**	0.603
Did atyp. work / heterogeneity	**2.063**	0.396	**0.459**	0.091		
Controls	Yes		Yes		Yes	
Did atyp. work × controls	No		Yes		No	
$\pi_{1,1,1}$	0.854	0.013	0.139	0.0131	0.243	0.022
$\pi_{2,2,2}$	0.146	0.013	0.861	0.0131	0.757	0.022
Other $\pi_{i,j,k}$	0	0	0	0	0	0
N. obs.	668,133		668,133		668,133	
Log Likelihood	-168,218.22		-168,145.19		-169,094.35	
A. I. C.	336,552.45		336,458.37		338,305.69	
B. I. C.	337,992.27		338,543.63		339,757.92	

Notes: Estimates show hazard ratios. Standard errors next to coefficients. Same set of controls as the benchmark specification, including duration dependence dummies. Coefficients sig. at the 5% level in bold.

heterogeneity is allowed to change between spells.

Lines 5-11 show the impact of workers experience on the job market before their registration at the unemployment agency. Notably, workers who received benefits and those who were registered at the agency in the year prior to the current spell benefit less from atypical work. This could reflect subpopulations in less stable careers for whom exiting a string of precarious jobs is more difficult. Individuals who worked fewer hours in the year preceding the unemployment spell also benefit more from atypical work.

Focusing on specific age groups (lines 12-17) also yields insightful results. Atypical work mildly helps workers under 30 years old to find regular work while older workers, especially over 60, benefit strongly. In terms of tasks (lines 18-27), those who benefit the most are highly qualified workers, although by a narrow margin compared to others.

These results may carry different implications for individual workers and for policymakers. Workers who benefit most from atypical work tend to be those who had a very low baseline job finding rate. But if the goal is to increase the flow of unemployed workers into regular work by making atypical work more attractive, they may not be the preferred target. For instance, at regressors mean, workers under the age of 20 who find atypical work go from a regular job finding rate of 4.2% to 7.2%, a 71% increase, but also a 3 percentage points gain. For a worker between the ages of 50 and 60, the rate would go from 0.18% to 0.33%, an impressive 80% increase, but a modest 0.15 percentage-point gain. If the goal is to maximize the flow of unemployed workers into regular employment, maximizing the percentage-point increase by helping young workers start a first job would seem the most beneficial. If the goal is to help the least employable, older workers have the most to gain. This pattern is present for many subgroups considered. The least employable seem to have the most to gain from atypical work.

Table 5: Impact of atypical work, alternative specifications / subgroups

		Coef.	S. E.	N. of obs.	Log likelihood
1	Atypical work and UI	**1.525**	0.029	668,133	-139,327.61
2	2^{nd} def. of reg. employment	**1.761**	0.024	814,530	-235,897
3	3^{rd} def. of reg. employment	**1.721**	0.023	810,661	-234,630
4	Spell specific heterogeneity	**1.994**	0.031	668,133	-168,325.93
5	Worked >= 1000 hours year before spell	**1.906**	0.028	616,220	-178,758.38
6	Worked < 1000 hours year before spell	**1.863**	0.03	668,362	-166,612.4
7	Did not work year before spell	**2.171**	0.046	690,721	-119,217.18
8	Was registered at emp. agency year before spell	**1.749**	0.038	297,357	-101,236.18
9	Not registered at emp. agency year before spell	**2.065**	0.033	668,276	-164,449.61
10	Received benefits year before spell	**1.642**	0.042	167,615	-58,359.019
11	Did not receive benefits year before spell	**2.026**	0.032	663,750	-166,340.54
12	Under 20	**1.714**	0.104	37,512	-15,174.909
13	20 to 29	**1.723**	0.025	555,523	-182,392.34
14	30 to 39	**1.964**	0.045	651,433	-168,653.73
15	40 to 49	**1.815**	0.052	533,269	-122,095.81
16	50 to 59	**1.992**	0.11	376,275	-55,293.641
17	over 60	**3.013**	0.898	7,436	-882.62
18	Qualification Not Specified	**1.445**	0.057	76,635	-26,755.69
19	Qualified, Routine task	**1.723**	0.164	74,334	-15,359.546
20	Qualified, Specialized worker	**1.844**	0.077	245,409	-55,959.953
21	Qual. Wrk w resp.	**1.917**	0.059	282,761	-79,019.784
22	Highly qualified. worker	**2.104**	0.07	124,189	-33,126.892
23	Non-qualified employee	**1.797**	0.058	533,353	-117,029.49
24	Qualified employee	**1.861**	0.039	672,067	-173,587.35
25	Technicians	**1.837**	0.043	200,548	-65,156.184
26	Administrators	**1.933**	0.079	100,879	-27,522.513
27	Executive	**1.893**	0.044	324,609	-80,582.366

Note: Each line shows the impact of atypical work on the probability of finding regular work for different models based on different parameters or subsamples. All models contain the same set of controls as the benchmark specification, including duration dependence dummies. Estimates show hazard ratios. Standard errors next to coefficients. Coefficients sig. at the 5% level in bold.

6 Conclusion

We have shown that atypical work significantly increases the probability that a French job seeker will find regular employment later on in his unemployment spell. In the benchmark specification, starting atypical work results in a 87% increase in the monthly probability of exiting unemployment completely in the following months. Unobserved heterogeneity is present, but weak. We measured a positive correlation between the likelihood of finding regular work and that of finding atypical work.

Overall, the stepping stone effect is strong and relatively homogeneous for most subgroups of workers considered and for various time-varying versions of the impact of atypical work. There is an obvious inverse relationship between the likelihood of finding work and the impact of atypical work on job-finding rates. Job seekers who are older, who have not worked in the year prior to their spell, and those who have been unemployed for a long time all start with a lower hazard rate into regular, full-time work. Yet for them, the stepping stone effect is stronger in proportion. However, a policy targeting groups who already have a higher chance of finding regular work would be better at increasing the total flow of workers out of unemployment. Hence, there is a tension between the objective of providing a stepping stone to the least employable and increasing equilibrium employment levels.

Our results suggest that partial unemployment programs, specifically the French *activité réduite* program, could help workers find regular work. At the individual level, entry into atypical work increases future career stability. However, partial unemployment programs do not encourage all types of contracts equally. It is an indirect subsidy for part-time or temporary jobs and its net impact on the composition of contracts in the labor market is unclear because firms might tend to decrease their use of permanent contracts in response. Because of the obvious risk of spillover effects on other job seekers, a meaningful cost–benefit analysis would require studying a real reform to the program or at least, modelling the entire labor market.

In addition, it is difficult to determine the specific impact of the *activité réduite* program in its current form. Its monetary incentives vary from person to person according to the individual situation, but the program is available universally to all workers. Since the level of benefits received is the direct result of the number of hours worked during a month, the impact of partial unemployment insurance programs cannot be disentangled from the impact of atypical work without an identification strategy based on variation in legislation, or a structural approach in a general equilibrium setting. Finally, the program could be analyzed within the theoretical framework of optimal progressive income taxation.

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Appendix

A Descriptive statistics

Table 6: Descriptive statistics (spell level)

Variable	Mean	S. D.	Variable	Mean	S. D.
Will do atypical work	0.436	0.496	UI benefits/1000	4.02	27.89
Single male	0.336	0.472	Hours wrk p.y.	1083.3	735.99
Single female	0.24	0.427	Months UI p.y.	0.384	1.382
Div./sep./wid. male	0.037	0.188	Months Agcy p.y.	1.437	2.864
Div./sep./wid. female	0.038	0.19	Spell length 1-2 months	0.154	0.361
Married male	0.195	0.396	Spell length 3-4 months	0.085	0.279
Married female	0.155	0.362	Spell length 5-6 months	0.062	0.241
Qual. N.S.	0.082	0.274	Spell length 7-8 months	0.046	0.209
Qual. Routine tasks	0.019	0.136	Spell length 9-11 months	0.048	0.214
Qual. Spec. wrk	0.063	0.242	Spell length 12-14 months	0.036	0.186
Qual. Wrk w resp.	0.097	0.296	Spell length 15-17 months	0.03	0.17
Highly qual. wrk	0.046	0.209	Spell length 18-21 months	0.027	0.162
Non-qual. empl.	0.131	0.338	Spell length 22-25 months	0.019	0.135
Qual. empl.	0.359	0.48	Spell length 26-30 months	0.017	0.13
Technicians	0.076	0.265	Spell length 30-36 months	0.013	0.113
Administrator	0.033	0.179	Spell was censored	0.463	0.499
Executive	0.095	0.294	Under 20 y.o.	0.045	0.208
French origin	0.929	0.257	20 to 29 y.o.	0.492	0.5
European (non-French)	0.02	0.14	30 to 39 y.o.	0.239	0.427
Non-European origin	0.051	0.22	40 to 49 y.o.	0.139	0.346
Number of children	0.549	1.009	50 to 59 y.o.	0.081	0.272
			Over 60 y.o.	0.004	0.064

B The FH-DADS dataset

The FH (historical file) contains information on an individual's history of interaction with the government employment agency (*Pôle emploi*). Unemployed or employed individuals can register with the agency to obtain job-finding assistance. The large majority of unemployed workers, defined according to the International Labour Organization, choose to do so. The FH contains information on the type of work sought, previous work experience, and socio-demographic background variables such as age, sex, marital status, and children. Crucially, the date of registration with the agency is used to determine the start of an unemployment spell. The agency also records a job seeker's hours worked per month for the duration of their registration.

The D3 is an extract from the national beneficiary file on workers receiving unemployment benefits. It contains detailed data on past wages used to calculate benefits. It is also a second source of information on atypical work, given that benefit collectors must declare current wage income to compute the net monthly UI benefits that they are allowed to receive.

Finally, the DADS (annual declaration of social data) is a matched worker-firm dataset derived from the administrative declaration all French companies are required to file for fiscal purposes. It details the wages and hours worked for all their employees. In the version of the DADS matched with the FH and the D3, it provides for each worker the starting date of the first contract of the year and the ending date of the last contract of the year (January 1^{st} and December 31^{st} if the contract is ongoing) for all employers during the year. Data on the firms include three different sector classifications and an identification number that allows each firm to be tracked and could be used to match a firm with outside information. The main use of the DADS is to identify when a worker has returned to stable, full-time employment.

The information in the FH-DADS allows tracking the employment and unemployment history of individual workers from 1996 to 2004, although early years up to 1998 can only be used for the work history in the DADS data. It therefore permits a very precise definition of the start of unemployment spells, the end of unemployment spells, and several definitions of atypical work and partial unemployment benefits. The size of the dataset permits an examination of various subgroups according to sector, employment history, age, etc.

A great advantage of the DADS data is that we can avoid the issue of non-random right censoring, an almost ubiquitous concern when using administrative data. Unemployment spells are often defined by participation in a government program, and spells are considered censored when a job seeker exits the program without further information.[11] Without data on future employment trajectories, there is no way of knowing when a new full-time open-ended job has been found. Workers who transit from a state of unemployment to another situation such as education, training, job placement programs, or nonparticipation in the labor force are typically recorded as censored. This censoring is, of course, non-random and will bias the estimates if it is not taken into account. Workers who perceive that they have a poor chance of finding full-time work are the ones most likely to exit the labor force for non-activity or education. Fremigacci and Terracol (2013) are the only authors who explicitly model this type of censoring by treating it as another random process.[12] With information on jobs contained in the DADS, it is possible to identify the exact moment at which a worker finds full-time work. Since we restrict our sample to workers who explicitly want regular work, decisions to eventually leave the labor force are not treated as censoring, but as part of the spell. Reducing search effort is simply considered an endogenous decision by the worker that enters the underlying risk dynamically. This is captured by the piecewise-constant baseline risk.

Unfortunately, observing only the first and last day of employment and the total hours worked during the year does not allow us to determine the exact number of hours worked each month, especially for part-time jobs. If monthly hours vary throughout the year or if there are periods of unemployment in-between two periods of employment, then these variations in work hours are necessarily averaged out. Also, if there is a change in the terms of a contract or in the wage level, it is impossible to determine during which month the change has occurred. Consequently, monthly hours worked derived from DADS data represent the average hours worked during the entire employment

[11] Kyyrä (2010) and Kyyrä et al. (2013) use periods of unemployment-related transfers as basis for spells. Fremigacci and Terracol (2013) use registration with employment centers as proxy for spells.

[12] For this treatment to be valid, censoring has to occur through a random process and be non-anticipated by the worker prior to its occurrence over and above the underlying risk.

period over the course of a year.

In terms of scope, certain individuals are excluded from the DADS, notably government workers, public servants, those working for an individual private employer (15%, as estimated by Le Barbanchon and Vicard (2009)), and the self-employed. By restricting our sample to individuals already present in the DADS, these groups are excluded from the start. However, an individual previously in the DADS who finds work in a sector not covered by the DADS is considered still unemployed. Our estimates of interest would most likely be affected if the probability of moving to a sector not covered by the DADS differed between individuals who did atypical work and those who did not. Although this is possible, we believe that the resulting number of misclassifications should be relatively low.

Aside from these limitations, the FH-DADS offers numerous advantages, including its large data volume and long panel. It provides a 1/25th sample of the entire French population. Selecting for individuals present in both the FH and the DADS, the FH-DADS covers more than 250 000 individuals, each of whom experienced at least one unemployment spell. Due to computing limitations, for the benchmark specification, a representative sample of 50 000 individuals was selected from the total sample.

Administrative data often have the drawback of offering fewer and less useful variables for researchers. This is not the case with the FH-DADS. Thanks to the combination of the three data sources, we were able to reconstruct the complete career history of a worker over many years. This makes it possible to precisely create each labor market variable. Notably, the DADS contains the complete employment history of workers after their return to work, allowing us to specify precisely what constitutes a true return to full-time stable employment.