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Spreading Out the Payment of Unemployment Benefits: The Impact on Transitions to Employment

Maxime Le Bihan* and Brice Fabre*

Abstract – In October 2021, a reform of the method used to calculate unemployment benefits had the effect of simultaneously reducing the daily allowance while increasing the potential duration of eligibility for unemployment benefits for those with discontinuous employment histories. In this article, we demonstrate that this change of system has been followed by an acceleration in the return to employment of the benefit claimants affected: the proportion of their number finding a new job within six months of the end of their previous contract is now between 3.2 and 6.8 percentage points higher than it was before the reform. This trend has been driven by a rise in temporary contracts lasting less than six months. The more discontinuous an individual's career before losing their latest job, the more substantial the effect. Additional survey data make it possible to profile the population affected by this change, revealing that they face greater financial pressure and adopt more wide-ranging job search strategies than the unaffected population.

JEL: J08, J64, J65

Keywords: Unemployment insurance, employment histories, quality of work

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In 2019, the French government decided to make adjustments to four key aspects of the unemployment insurance regulations: increasing the number of working hours required to become eligible for unemployment benefits, adjustments to the formulae used to calculate unemployment benefits, gradually decreasing benefits for high-earning individuals, and adjustments to the contributions paid by employers based on the extent to which they use short-term contracts (bonus-malus). After several years of delays and modifications, primarily due to COVID, the measures, which were originally scheduled for implementation in 2019, finally came into force in 2021.

The purpose of the present study is to assess the impact of the changes made to the benefit calculation method, often referred to as the “daily reference wage reform” (DRW – *Salairé journalier de référence* in French), where the DRW represents the salary used to calculate the value of benefits received by a jobseeker eligible for unemployment insurance. Before the reform, the DRW corresponded to the average daily wage the recipient earned on the days they worked over the preceding twelve months.¹ Recipients were eligible to claim benefits, calculated on the basis of this DRW, for a period corresponding to the number of days worked during a given reference period. Since the reform, the DRW now corresponds to the average daily wage earned over all calendar days in the reference period, whether or not the recipient worked on those days. In return, the benefits calculated using this new DRW can now be claimed for a number of days equivalent to the total number of calendar days in the aforementioned reference period. As such, both the DRW and the duration of time for which jobseekers are eligible to claim benefits have shifted from equations based on number of days worked to equations based on calendar days, meaning that recipients who have alternated between periods in work and periods out of work see their daily benefits reduced but the potential duration of their eligibility for benefits increased.

The aim of our study is to assess the consequences of these changes in terms of the career paths of benefit claimants. It is interesting to note that, as far as we are aware, no article yet published has dealt with a reform that simultaneously affects the monthly value and the total duration of unemployment benefits. Indeed, such reforms are rare. In this respect, the present study is unprecedented.

For the purposes of our analyses, we took “discontinuous employment history” to mean anybody who, at least once in the period used to calculate their unemployment benefits, has experienced a period out of work sandwiched between two periods in work. We conducted two types of analyses: firstly, an analysis of the causal effects of the reform on employment trajectories, using administrative data on jobseekers; secondly, an analysis of survey data for a specific period following the application of the reform, allowing us to profile those benefit claimants with discontinuous employment histories more accurately and offer more informed interpretations. For the purposes of our causal analyses, we adopted two complementary identification strategies. The first is what we call “difference in discontinuities”, looking at those who became eligible for benefits when their employment contracts ended on or around 1st October in the year in which the reform was applied, and comparing them with the year preceding the reform. The second is a “triple difference” strategy, making use of a regulatory quirk which meant that, of those eligible for unemployment benefits as of 1st October, some were affected by the reform while others were not.

Our results for employment trajectories show that individuals with discontinuous employment histories returned to work more rapidly after becoming eligible for unemployment benefits. The proportion of individuals in this category finding a new job within six months of their last contract ending was between 3.2 and 6.8 percentage points higher, depending on the specifications used, increasing from around 55% to 60% as a result of the reform. Nevertheless, this increase was driven by an increase in temporary contracts lasting less than six months, and individuals with a chequered employment record were not any faster when it came to finding a temporary job on a longer contract, or a permanent employment contract (CDI). These results have been shown to be robust in response to various tests (alternative empirical specification, placebos). They are, however, limited to a six-month period. Our results do not allow us to reliably monitor the progress of individuals beyond the limit of six months after they become eligible for unemployment benefits. Nonetheless, we demonstrate that the effects we have identified are even more significant for individuals affected by the recent reform,

1. The number of days worked here corresponds to the number of calendar days spanned by the employment contract. In actual fact, it corresponds to 5 days per calendar week during the period covered by the contract, multiplied by 1.4.

i.e. those with long breaks in their employment histories.

The results of the ‘SEJOUR’ survey, meanwhile, suggest that financial pressures are felt more keenly by individuals with discontinuous employment records than by others. On average, individuals with discontinuous employment records also report themselves to be willing to make more concessions to find work, compared with other individuals. Such concessions are often closely correlated with other factors influencing individual income, such as the value and duration of benefits, eligibility for other social benefits, and the amount of time spent on unemployment benefits. Although it is difficult to draw a definitive connection between these results and the reform – since our study relates to a specific period subsequent to the reform, without comparison to the pre-reform situation – it nonetheless seems plausible to draw connections between faster returns to work, even on short-term contracts, and the declarations of individuals with discontinuous careers regarding the financial pressures and concessions they have experienced, in so far as the reform has had the effect of reducing the amount of benefits paid to those with discontinuous employment histories.

It should be noted that the purpose of this study is to analyse the specific effects of the reform on individuals with discontinuous employment histories; as such, we do not seek to ascertain the effects of the reform on the labour market as a whole (it does not, for example, measure the extent to which the aforementioned effects impact individuals beyond the scope of our analysis).

Our evaluation is structured as follows: Section 1 describes in detail the content of the reform; Section 2 comprises a review of the existing literature; Section 3 describes our data, our analytical framework and the definitions we used, and Section 4 defines the population impacted by the reform; Section 5 presents our methodology and the result of our causal analysis of the effects of the reform on career trajectories, while in Section 6 we analyse the survey data before offering some conclusions.

1. Description of the Reform

The reform to the method used to calculate unemployment benefits under consideration has changed both the daily reference wage (DRW) and the potential duration of eligibility for benefits. The DRW is calculated by dividing the total wages earned during a given period by

the number of days in that period. It is used to calculate the daily allowance (DA – *Allocation journalière* in French). The potential duration of eligibility for benefits corresponds to the maximum number of days for which the DA will be paid. The reform of the calculation method considered in this paper essentially represents a switch from the old system, whereby the reference period used to calculate the DRW and the potential duration of eligibility for benefits was defined by the number of days worked, to a new system where that period is defined by the number of calendar days. In other words, non-working days are now included in the denominator of the DRW (pushing it downwards) and when calculating the potential duration of eligibility for benefits (pushing this figure upwards).

In detail, before the reform:

The DRW corresponded to the wages earned over the last 12 months, divided by the number of days worked during the last 12 months. These last 12 months corresponded to the period referred to as the “calculation reference period” (CRP).

The potential duration of eligibility for benefits corresponded to the number of days worked during the last 24 months (for those under the age of 53).² These last 24 months corresponded to the period referred to as the “eligibility reference period” (ERP).

Now, since the reform:³

The DRW corresponds to the wages earned during the ERP (last 24 months for claimants under the age of 53) divided by the number of calendar days between the first day and last day that they worked within this ERP (this number of days is capped at a certain level).⁴

The potential duration of eligibility for benefits corresponds to the new denominator of the DRW, namely the (capped) number of calendar days between the first day and last day that claimants worked during the ERP.

2. This period was extended to 30 months for those aged 53 or 54, and 36 months for those aged 55 and over.

3. It is worth noting that the system described here is no longer in place at time of writing, in the wake of a 2023 reform which modified the potential duration of eligibility with reference to the economic outlook, and the Unemployment Insurance Agreement finalised on 15 November 2024.

4. The number of calendar days is capped at the number of days worked during the ERP multiplied by 1.4 (to convert it to the equivalent of a seven-day week) then multiplied by 1.75. In other words, the number of days out of work cannot exceed 75% of the number of days worked. This cap was introduced following the ruling issued by the Conseil d’État on 25 November 2020, which found that the absence of such a measure would lead to excessive treatment differences affecting the DRW and potential eligibility period of people who had actually worked the same number of hours.

The reform took effect for benefit entitlements as of 1st October 2021, and for contract terminations making claimants eligible for benefits since 1st October 2021. In light of this calendar, an adjustment mechanism was introduced for unworked days within the COVID-19 lockdown periods.⁵

The reform thus ushered in three changes:

Spreading out the benefits payable to those with “discontinuous employment histories”, i.e. those whose periods of reference contain spells out of work between spells in employment: for this population, the DRW has fallen while the potential duration of eligibility for benefits has increased.

The CRP is now aligned with the ERP: henceforth, the DRW is calculated using the same reference period used to calculate the potential duration of eligibility for benefits.

The reference period for past wages has been extended (from 12 to 24 months), which means that the value of benefits depends on wages which were earned further in the past.

The present study focuses on the first of these three changes. Our goal is to ascertain the extent to which the decrease in the daily value of benefits received by those with discontinuous career histories (accompanied by an increase in the potential duration of their eligibility for these benefits) has affected the employment trajectories of individuals in this category.

The path to implementation for this reform was long and relatively complex, which means that it was possible to plan ahead. However, doubts persisted until the very end with regard to the precise content of the reform, as well as its implementation date.⁶ Our goal is to ascertain the extent to which the decrease in the daily value of benefits received by those with discontinuous career histories (accompanied by an increase in the potential duration of their eligibility for these benefits) has affected the employment trajectories of individuals in this category.

2. Literature Review and Scope of Analysis

2.1. Literature Review

There is an abundant existing literature exploring variations in the monetary value and duration of eligibility for benefits, and how they affect the rate of people returning to work (see the reviews provided by Schmieder & von Wachter, 2016; Lopes, 2022, and Cohen & Ganong,

2024). The general consensus is that reducing the monetary value of benefits has a positive effect on getting people back to work, while extending the potential duration of benefits has a negative effect. The present article represents a contribution to this literature, since the reform in question has modified both the value and the potential duration of unemployment benefits in France. Nevertheless, this reform is somewhat peculiar in that it modifies these two parameters in opposing directions, while the total capital available for benefits remains unchanged (or almost unchanged). What has changed is the duration over which payments are spread, which accounts for the originality of the present study, and other efforts to assess the same reform (Fontaine *et al.*, 2025; Issehnane *et al.*, 2025). Previous studies have also sought to simultaneously gauge the effects of the value and duration of benefit payments, albeit in different contexts: looking at cases where both the value and potential duration of benefits were increased (e.g. in Austria, see Lalive *et al.*, 2006), or else making use of the threshold effects induced by unemployment insurance regulations (e.g. in Poland, Jessen *et al.*, 2025). The only article of which we are aware which focuses on a change to the temporal distribution of benefit capital is that published by Lindner & Reizer (2020), looking at a Hungarian reform whereby benefit payments were weighted more towards the beginning of the eligibility period.

Their results indicate that this had a positive impact in terms of both getting people back into work and the wages they earned in their new jobs. The impact of the 2021 DRW reform will depend, in part, on the elasticity of employment in relation to both benefit levels and their potential duration, with each of these elasticities having opposing effects in the case of this reform. This raises the question of the relative value of these two forms of elasticity. The existing literature suggests that elasticities pertaining to the monetary value of benefits are, on the whole, stronger than those pertaining to

5. In order to ensure that periods out of work associated with the COVID lockdowns were not prejudicial to the calculation of unemployment benefits, days not spent working between 1st March 2020 and 31st May 2020, and between 30 October 2020 and 30 June 2021, were not included in the total number of days out of work.

6. The reform was initially supposed to take effect on 1st April 2020 (Decree 2019-797 dated 26 July 2019), but due to the pandemic it was pushed back to 1st September 2020 (Decree 2020-361 dated 27 March 2020), then to 1st January 2021 (Decree 2020-929 dated 29 July 2020). The ruling of the Conseil d'État dated 25 November 2020 then rendered the reform null and void. The reform was thus adapted with the addition of new texts (Decree 2021-346 dated 30 March 2021 and Decree 2021-730 dated 8 June 2021). The interim decision issued by the Conseil d'État on 22 June 2021 further delayed implementation of the reform, on account of the economic context which remained unstable as the pandemic dragged on. A new application date was finally set in Decree 2021-1251 dated 29 September 2021.

potential duration. To be more precise, studies of the impact of changing the potential duration of benefits have found estimated elasticities for duration of unemployment ranging from 0.1 to 1, with a median of 0.4 (Schmieder & von Wachter, 2016; Lopes, 2022; Cohen & Ganong, 2024). In these studies, the marginal effects associated with these elasticities suggest that a variation of one month in the potential duration of eligibility for benefits would lead to an increase in the duration of unemployment of anywhere between 1.5 and 20 days. Elasticities relating to changes in the value of benefits, meanwhile, have been less thoroughly documented than those pertaining to duration. Nevertheless, estimates range from 0.12 to 2 with a median of 0.53 (Schmieder & von Wachter, 2016; Cohen & Ganong, 2024). Per certain hypotheses, this median elasticity implies that a €1 reduction in the value of daily benefit payments can reduce the duration of unemployment by between 1.3 and 21 days.⁷ In the next sub-section, we offer further details of our analytical framework with regard to these two elasticities.

In addition to the impact on getting people back to work, numerous previous studies have looked at the effect of unemployment benefits on the quality of jobs to which beneficiaries return, measured in terms of their starting salary or job stability (cf. Le Barbanchon *et al.* (2024) for a literature review). By looking at the types of contracts which jobseekers sign when returning to work, our study represents a contribution to this literature, whose findings have varied considerably. Belzil (2001) finds that unemployment benefits have relatively little effect on the quality of the jobs that jobseekers find, while Lalive (2007), Le Barbanchon (2016) and Huang & Yang (2021) all concur that there is no impact.⁸ Nekoei & Weber (2017), meanwhile, finds that unemployment benefits have a positive effect on wages, while Caliendo *et al.* (2013) detect a positive effect on both wages and duration of employment, and Tatsiramos (2009) finds a positive impact on duration of employment. Finally, Schmieder *et al.* (2016) find that unemployment benefits have a negative effect on wages. The diversity of these findings can be partly attributed to the fact that unemployment benefits may buoy salary demands, while also extending the duration of unemployment which has the opposite effect and pushes wages downwards.

Our work also sits within a research trend focusing more specifically on the French unemployment benefits system. In this vein, Le Barbanchon (2016) finds that benefits have

a significant effect on the amount of time people spend in unemployment, but no effect on the quality of the jobs to which they return. Le Barbanchon *et al.* (2019) report that unemployment benefits have no impact on reservation wage, while both Prieto (2000) and Marinescu & Skandalis (2021) have studied the behavioural aspects of job searches while receiving unemployment benefits.

Finally, the various measures contained in the reform of 2019–2021 have been subjected to various assessments coordinated by an evaluation committee (the Committee for the Evaluation of the Unemployment Insurance Reform 2019–2021 [2025]). Among these studies, Fontaine *et al.* (2025) and Issehnane *et al.* (2025) have also looked at the reform of the DRW. Fontaine *et al.* (2025) report a short-term spike in the number of days worked but no effect on the quality of jobs, whereas Issehnane *et al.* (2025) also report more people returning to employment, albeit with a greater proportion of unstable, short-term contracts.

2.2. Analytical Framework

The reform which is the object of this study may be regarded as a new opportunity to directly compare the effects of adjusting the monetary value of unemployment benefits and the effects of modifying the potential duration of eligibility for those benefits. Using ε_1 and ε_2 to represent the elasticity, in absolute value terms, of the probability of returning to work within a given timeframe, with reference to the potential duration of eligibility for benefits and the monetary value of those benefits, when all other things are equal, we have a situation whereby:

$$\frac{\Delta y}{y} = \varepsilon_1 \frac{\Delta D}{D} + \varepsilon_2 \frac{\Delta A}{A}$$

where y is the indicator for return to employment, D is the potential duration of eligibility for benefits, and A the daily allowance. SR denotes the reference salary used to calculate the DRW (supposing that SR does not change with the reform), D_1 and D_2 represent the denominator of the DRW respectively before and after the reform, also supposed to be the potential duration of eligibility for benefits, and β is the relative variation in potential duration of eligibility for benefits on account of the reform

7. This calculation assumes an average daily allowance of €34 (corresponding to the daily allowance before the reform, in this case), with the change uniformly affecting all individuals with discontinuous employment records. It also assumes an average period of one year spent in unemployment.

8. On wages for Lalive (2007) and Huang & Yang (2021), on wages and time spent in employment for Le Barbanchon (2016).

$(\frac{\Delta D}{D} = \frac{D_2 - D_1}{D_1})^9$. As such, if the total capital $A \times D$ remains constant following the reform, we have: $1 + \frac{\Delta A}{A} = -\frac{1}{1 + \beta}$ or $\frac{\Delta A}{A} = -\frac{\beta}{1 + \beta}$, which allows us to state that:

$$\frac{\Delta y}{y} = \varepsilon_1 \beta - \varepsilon_2 \frac{\beta}{1 + \beta}.$$

Analysing this term with reference to β with given values for ε_1 and ε_2 (and where $\beta > 0$, given that the reform increases the potential duration of eligibility for benefits), we can note that if $\varepsilon_1 \geq \varepsilon_2$, then $\Delta \ln y$, which represents the effect of the reform on the return to employment, is always negative. If, however, $\varepsilon_1 < \varepsilon_2$, then $\Delta \ln y$ increases up to the point where $\beta = \sqrt{\frac{\varepsilon_2}{\varepsilon_1}} - 1$, then decreases thereafter to become negative from $\beta = \sqrt{\frac{\varepsilon_2}{\varepsilon_1}} - 1$ onwards.¹⁰

In other words, for a relative variation in the potential duration β , the resulting relative variation in the daily allowance is less than β , so that if we model the potential duration and monetary value of the daily allowance at constant elasticity, the effect on potential duration is greater in most configurations, suggesting that the reform has had a negative impact on return to employment. In order for the daily allowance effect to be greater, a necessary condition is that $\varepsilon_1 < \varepsilon_2$, which is not sufficient in so far as the overall effect becomes negative once β exceeds a certain level. The empirical literature suggests that, on the whole, $\varepsilon_1 < \varepsilon_2$. Nonetheless, the theoretical effect of the reform remains ambiguous, even in this scenario. Moreover, if we define β as the treatment intensity for a benefit recipient, the treatment effect does not always work in the same direction, and becomes more pronounced when the treatment intensity is greater.

3. Data, Scope of Analysis and Definitions

Our work makes use of two types of data. On the one hand, we draw upon administrative data relating to jobseekers, allowing us to analyse the causal effects of the reform on the employment trajectories of these jobseekers in the labour market. On the other hand, we make use of a survey conducted in the summer of 2023, around a year and a half after the reform came into effect, allowing for comparisons between a population whose rights were modified by the reform and a population whose rights were unaffected.

3.1. Administrative Data

For the purposes of this study, we used the national register of beneficiaries (FNA) produced

by France Travail and adjusted by Unédic, which contains information on the benefits received by recipients, their employment trajectories and their socio-demographic characteristics. This allowed us to identify those careers affected by the reform, observing the days worked and days without work during the reference periods.

We also used the “France Travail nominative social declarations” (DSN-FT), corresponding to the nominative social declarations for all individuals registered with France Travail (the public employment service formerly known as Pôle Emploi, including during the period covered by our analysis), which can be matched with the FNA by means of shared individual identifier codes. These data provide information regarding the employment contracts held by individuals registered with France Travail in the period beginning in the month of their registration and ending six months after their removal from the agency’s lists, thus allowing us to measure rates of return to employment. The task of matching the DSN to the FNA is handled by France Travail. The DSN are compulsory monthly declarations submitted by employers in France, used to calculate social security contributions and pass on data concerning employment contracts to the various administrative entities who make use of it. In fact, these declarations cover all contracts from the private sector and a certain proportion of public-sector contracts since 2018.

3.2. Scope of Analysis of Administrative Data

The new rules for calculating the DRW are applicable to all individuals becoming eligible for benefits since 1st October 2021, on the condition that their most recent employment contract (making them eligible for these benefits) concluded before that date. In order to obtain periods both before and after the reform came

9. These equations focus on making the transition from a counting system based on days actually worked to a system based on calendar days, and thus overlook the extension from 12 to 24 months of the reference period used to calculate the maximum eligibility period. For simplicity’s sake, they also assume that the denominator of the DRW is equal, both before and after the reform, to the potential duration of eligibility.

10. We assume that the total benefit capital remains constant, although it may actually vary slightly due to the complexity of the unemployment insurance rules. If the CRP is not extended, since the daily allowance formula dampens variations in the DRW, the total capital (daily allowance $\times$ potential duration) should increase slightly. Nonetheless, the extension of the CRP (from 12 to 24 months) implies that, when wages are going up, the capital $\text{DRW} \times \text{potential duration}$ should decrease slightly, making the total variation in the capital (daily allowance $\times$ potential duration) ambiguous. Figure S-V (see the Online Appendix) confirms that these variations do indeed remain small: the DRW capital falls by between €2,500 and €3,900 depending on the specification used, while the daily allowance capital falls by between €0 and €1,155, and is not significant for one of the specifications (see Section 5.1 for details of the specifications). These results confirm the hypothesis that the reform, on the whole, maintains the total benefit capital unchanged.

into force, we chose to focus on those becoming eligible for unemployment benefits between early September and late November 2021. We chose this relatively small time window because specific unemployment insurance measures connected with the pandemic came to an end in July 2021, with a potential contamination effect in August and, further downstream, new eligibility conditions came into force on 1st December. To allow for comparisons between different periods of time, we adopted the same field for the years 2017, 2018 and 2022, excluding 2019 and 2020 on account of the pandemic.¹¹

The scope of our analysis encompasses all individuals becoming eligible for the ARE benefit (*Aide au retour à l'emploi*, or “back to work”), and thus excludes other forms of benefits governed by their own specific regulations: intermittent workers in the entertainment sector (Appendices 8 and 10 of the unemployment insurance regulations), professional stability contracts (CSP) and benefits intended to help people remain in work. Furthermore, since the DSN only partially covers the public sector pre-2022, we chose to disregard public-sector contracts and focus on employment not including the public sector.¹² The number of eligible beneficiaries stood at 3,446,675 before these restrictions were applied, then 3,112,222 when we excluded intermittent workers, CSPs and in-work benefits.

It is nonetheless worth noting that these data, derived from the administrative management of unemployment insurance, only cover those “claiming” unemployment benefits, i.e. those individuals who chose to register with France Travail. We thus did not observe either unemployed people not registered with France Travail, or those who did not spend time in unemployment after losing their job, which leads to a certain implied selection of the target population with regard to our variable of interest, i.e. the return to employment of those who find themselves unintentionally out of work.

3.3. Definition – Individuals with Discontinuous Employment Records

The ensuing analyses are all informed by our definition of what it means to have a discontinuous employment record. For an individual's employment history to be regarded as discontinuous, they must have been out of work for at least 1% of the calendar days in their eligibility reference period (ERP). In order to identify these individuals, we used the ratio of days worked to

total calendar days in the ERP. The FNA allows us to identify the ERP start and end dates, as defined by unemployment insurance regulations, as well as the start and end dates of all employment contracts falling within the ERP and used in the calculation of benefits due. Using these data sources, we calculated the number of days in the numerator and denominator of our ratio, and thus select our sample.

We were thus able to zoom in on the effects of the reform on individuals with discontinuous employment histories, of particular relevance because discouraging people from alternating between periods in work and periods out of work was purportedly the primary objective of this reform. We therefore did not seek to analyse the behavioural effects associated with other mechanical effects of the reform, specifically those linked to the extension of the reference period from 12 to 24 months when calculating the DRW (aligning the CRP with the ERP).

The decision to focus exclusively on individuals with “discontinuous employment records”, as defined above, reduced the size of our sample from 3,112,222 to 1,008,264. Figure S-II in the Online Appendix (link to the Online Appendix at the end of the article) shows the distribution of the ratio between number of days worked and number of calendar days, for individuals with discontinuous employment records (where the ratio is less than or equal to 99%). The size of the cohort associated with each ratio bracket gets bigger as the ratio rises.

3.4. The SEJOUR Survey

Unédic was responsible for conducting the SEJOUR survey, with field work carried out by *La Voix Du Client* (LVDC), quizzing jobseekers on their current circumstances, and particularly the state of their job search. Conducted one year after the reform took effect, the resulting data allow us to distinguish between those individuals who had experienced discontinuous careers before claiming unemployment insurance, and those who had not. A discontinuous employment record, as defined in detail above, implies that individuals were out of work for at least 1% of the days contained in the eligibility reference period (survey respondents were, generally, also present in the administrative data, to which this definition was applied).

11. It is not possible to use eligibility figures for 2019 because the corresponding six-month employment trajectories were affected by the COVID pandemic.

12. Our definition of the public sector corresponds to the following provisions in the legal categorisation used by the Sirene classification (Level II): 41, 71, 72, 73, 74.

The survey sample comprised 41,700 individuals, selected at random from among the 670,000 people becoming eligible for benefits between 1st October 2022 and 31 January 2023. They were interviewed in July 2023, an average of six months after becoming eligible. Weighting coefficients were constructed in order to split the groups evenly on the basis of their employment histories (discontinuous or not) and various demographic characteristics (gender, age group, gender and age group combined).

Data collection was conducted in three different ways: 4% of the sample (1,600 individuals) were contacted by telephone, 24% (10,000 individuals) by email with follow-up phone calls, and 72% by email only. The total response rate was 17%, with 50%, 22% and 13% respectively for these three collection methods. Weighting was then conducted to correct for non-response disparities between the collection methods, and the potential influence of the survey method on the responses received.

4. Profile of Individuals with Discontinuous Employment Records

In this section we seek to describe the changing characteristics of individuals with discontinuous employment records before and after 1st October 2021, in order to more accurately visualise the challenges and factors to be taken into consideration in our identification strategies (see Table S-1 in the Online Appendix). Econometric analysis of the administrative data reveals that, compared with individuals becoming eligible for benefits before the reform (September 2021), those becoming eligible after the reform (October and November 2021) presented noticeably different characteristics: their average age was higher, the proportion of women was lower, there were fewer fixed-term contracts and more temporary contracts in the period preceding eligibility, and a shorter average duration of the most recent employment contract preceding unemployment. These trends were also observed in previous years (2017, 2018), suggesting that there are seasonal dynamics at play rather than the effects of the reform to the DRW calculation method.

In terms of the unemployment benefits paid out, the period obviously reflected the effects of the reform. Figure I shows that the average DRW fell by €14, a decrease of 25% on its pre-reform level (from €56 to €42). Breaking down DRW into a numerator (which mechanically increased after the reform since the period for which wages were taken into account increased by a whole

year) and a denominator (which changed for the same reason, as well as the fact that days out of work within this period are now taken into account) allows us to better grasp the dynamics of this development. Between the two periods, the numerator of the DRW was multiplied by 2.3, while the denominator was multiplied by 2.8. By way of a comparison, in the two preceding years (2017, 2018) the numerator and denominator grew by an average of 8% and 1% respectively between September and October-November, leading to a slight increase in the DRW. We must control for this seasonal effect so as not to underestimate the drop in the DRW caused by the reform.

Meanwhile, the daily allowance was cut by €6, equivalent to 17.6% of its pre-reform value (from €34 to €28). The potential duration of eligibility for benefits, i.e. the maximum number of days for which benefits can be claimed, increased from 417 to 519 days, a rise of 24%. These changes are direct consequences of the reform, and as such are only observed in this period.

5. Causal Effects of the Reform on Employment Trajectories

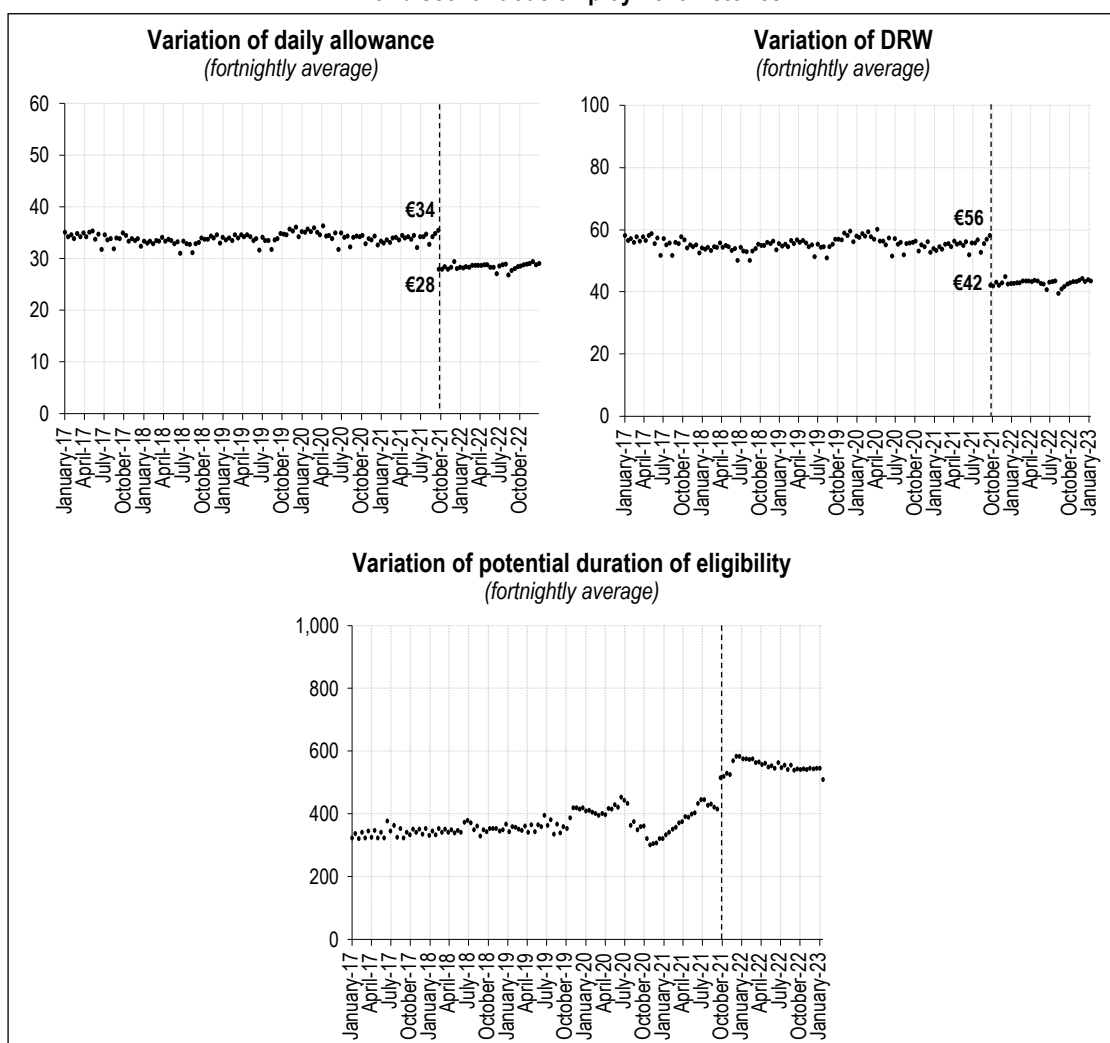
5.1. Empirical Strategies

In order to isolate the causal effects of the reform on employment trajectories, we made use of two identification strategies: one based on “difference in discontinuities” and another based on the triple difference method.

5.1.1. Strategy 1: Difference in Discontinuities

The reform studied here came into effect for unemployment benefit claimants whose most recent contract of employment ended after 1st October 2021. A discontinuity regression (Imbens & Lemieux, 2008; Lee & Lemieux, 2010) might thus consist of comparing the trajectories of individuals eligible for benefits due to contracts ending just after 1st October 2021 with the trajectories of individuals eligible for benefits due to contracts ending just before that date. Such a strategy would be partly dependent on the hypothesis that unobservable factors affecting the variable of interest would not be affected by the contract end date, as well as the hypothesis that these contract end dates were not in any way manipulated (for example, an employee might have sought to ensure that their contract ended before 1st October 2021, so that their unemployment benefits would not be affected by the reform).

Figure I – Variation in the daily allowance, DRW and potential duration of eligibility for beneficiaries with discontinuous employment histories



Note: Each point represents the average daily allowance / DRW / potential duration of benefit eligibility for individuals with discontinuous employment records over a two-week period. The date shown is that on which their most recent employment contract ended, entitling them to claim unemployment benefits.

Field: All individuals with discontinuous employment records

Source: FNA, France Travail, Unédic.

Nevertheless, the seasonality of contract terminations makes it difficult to maintain the hypothesis that unobservable factors affecting the variable of interest would not be affected by the contract end date, given the high frequency of contracts ending at the end of the month (Fontaine *et al.*, 2025). We thus sought to isolate the effect of the reform by looking at “differences in discontinuities”, i.e. the difference in the discontinuity effect observed in 2021 and that observed in 2018 (2019 and 2020 are excluded from the sample, as explained in Section 3). This is equivalent to adopting the hypothesis that, without the reform, variations in the variable of interest around the 1st October threshold date would have been similar in 2021 and 2018. In other words, factors associated with the seasonality of the variable of interest

around this date would play a similar role in 2021 and in 2018, leading to similar variations in the variable of interest. In short, for each year (2018 and 2021) we compare the different trajectories of individuals whose contracts ended just before and just after the 1st October cut-off data, and then compare the differences between 2018 and 2021.

We also simultaneously estimate the effect of discontinuity in 2017 and in 2022, for the purpose of comparing the results with 2018, which constitutes a placebo test for an earlier and later year. Ultimately, our empirical specifications can be expressed as follows:

$$Y_{if} = X_{if}\beta + \sum_{a \in \{2017, 2021, 2022\}} \mathbf{1}_{a=annee_f}(X_{if}\beta_a) + Z_{if}\theta + \varepsilon_{if}(1)$$

where

$$\begin{aligned}
X_{if}\beta &= \beta_0 + \beta_1 \mathbb{1}_{\text{jour}_f \geq j_0} + \beta_2 (\text{jour}_f - j_0) \\
&\quad + \beta_3 \mathbb{1}_{\text{jour}_f \geq j_0} (\text{jour}_f - j_0) \\
X_{if}\beta_a &= \beta_{0,a} + \beta_{1,a} \mathbb{1}_{\text{jour}_f \geq j_0} + \beta_{2,a} (\text{jour}_f - j_0) \\
&\quad + \beta_{3,a} \mathbb{1}_{\text{jour}_f \geq j_0} (\text{jour}_f - j_0)
\end{aligned}$$

Y_{if} represents the dependent variable associated with an individual i whose contract ended at date f . annee_f and jour_f denote the year and day of date f . j_0 corresponds to 1st October. The coefficient of interest is the coefficient $\beta_{1,2021}$. $\beta_{1,2017}$ and $\beta_{1,2022}$ are placebo coefficients. Z is a vector of the control variables: they include the variables contained in Table S-1 in the Online Appendix) as well as the appropriate codes from the ROME classification of professions (14 categories).¹³ In light of the potential differences between contracts ending at the end of the month and those ending at the start of the month, our principal results encompass a two-week period before and after 1st October. Robustness tests for this time window are presented in Section 5.3.^{14,15} As can be seen from the equations above, we also used a linear specification for days. By means of a robustness test, our results are also presented in quadratic form (see Section 5.3).¹⁶

5.1.2. Strategy 2: Triple Difference

Nonetheless, the difference in discontinuities method has the disadvantage of being restricted to the estimation of local effects (for all individuals becoming eligible for benefits in a given period around 1st October). In order to estimate the impact on a larger population (1 month before and after the reform), and control more effectively for the influence of demographic variables, we made use of a second strategy (the triple difference method) to further isolate the impact of the reform on getting people back into work.

This strategy takes as its starting point a difference-in-differences, making use of variations in treatment status observed on a single eligibility date. As it happens, the applicability of this reform depends on the date on which the claimant's last contract ended, as well as the date at which they become eligible for benefits. To be more precise, the reform applies to those becoming eligible for benefits as of 1st October 2021 and whose previous contract ended after that date. As such, those becoming eligible for benefits as of 1st October 2021, but on account of a contract which concluded before that date, were not affected by the reform.

Making use of this double condition enabled us to estimate the difference-in-differences. We thus retained as our time variable the month in which

individuals became eligible for benefits, and not the month in which their contract ended. For each of these months, we distinguished between those becoming eligible because their contract ended in that same month, and those becoming eligible because of a contract which ended in a previous month. For individuals becoming eligible in September, these two sub-groups were not treated, whereas for October the first sub-group was treated while the other was not (we did not consider those becoming eligible for benefits in November, because too few observations from that month were untreated).^{17,18}

However, because the potential presence of seasonal effects makes it difficult to state with any certainty that those becoming eligible for benefits in October and September would have experienced similar employment trajectories if the reform had not happened, we preferred to estimate the treatment effect using a triple difference approach, i.e. estimating the difference between the aforementioned difference-in-differences for 2021 and for 2018.

This strategy rests upon two hypotheses: firstly, that, depending on the date of eligibility, the fact that the contract end date falls in the same month or the preceding month is not dependent on unobservable variables associated with return to employment (hypothesis associated with the difference-in-differences), and, secondly, that if the reform had not been introduced, the

13. This restriction, and the restriction which only retains observations with no missing values for the control variables, reduces the number of new beneficiaries from 365,366 to 36,519 before 1st October, and from 642,898 to 177,242 after 1st October.

14. The optimal window test proposed by Calonico et al. (2020) suggests a 3-day window, which is unrealistic given the context surrounding this particular discontinuity (differences between contracts ending at the start and the end of the month). We thus opted for a 15-day window, which we feel is relatively small, while remaining realistic and even faintly conservative (Figure S-IV shows that, broadly speaking, expanding the window increases the treatment effect).

15. The optimal window test proposed by Calonico et al. (2020) suggests a 3-day window, which is unrealistic given the context surrounding this particular discontinuity (differences between contracts ending at the start and the end of the month). We thus opted for a 15-day window, which we feel is relatively small, while remaining realistic and even faintly conservative (Figure S-IV shows that, broadly speaking, expanding the window increases the treatment effect).

16. We only tested 1st and 2nd degree polynomials, following the recommendations of Gelman & Imbens (2019). The AIC and BIC criteria for this window were 275,305 and 275,714 respectively for the linear model, and 273,988 and 274,560 for the quadratic model. Given the proximity of these values, we opted for the most parsimonious option – namely, the linear model. Nonetheless, Figure S-IV presents the values for the effects of these two polynomials using different window sizes, showing that the results are stable.

17. Given the possibility of differing benefits for up to six months, the month in which the contract ends and the month in which benefit payments begin may, in some cases, be separated by quite some time. We capped at two months the period of time between eligibility beginning and the most recent contract ending.

18. This restriction, and the restriction which only retains observations with no missing values for the control variables, reduces the number of new beneficiaries from 365,366 to 288,538 before 1st October, and from 642,898 to 269,160 after 1st October.

differences in “back to work” figures for those becoming eligible for benefits in September and those becoming eligible in October would have been similar in the years 2018 and 2021 (hypothesis associated with the third difference).

All of which leaves us with the following equation:

$$Y_{ifo} = X_{ifo}\gamma + \sum_{a \in \{2017, 2021, 2022\}} \mathbb{1}_{a=annee_f}(X_{ifo}\gamma_a) + Z_{ifo}\eta + \vartheta_{ifo} \quad (2)$$

where

$$X_{ifo}\gamma = \gamma_0 + \gamma_1 \cdot \mathbb{1}_{mois_f=mois_o} + \gamma_2 \cdot \mathbb{1}_{mois_o=octobre} + \gamma_3 \cdot \mathbb{1}_{mois_f=mois_o} \cdot \mathbb{1}_{mois_o=octobre}$$

$$X_{ifo}\gamma_a = \gamma_{0,a} + \gamma_{1a} \cdot \mathbb{1}_{mois_f=mois_o} + \gamma_{2,a} \cdot \mathbb{1}_{mois_o=octobre} + \gamma_{3,a} \cdot \mathbb{1}_{mois_f=mois_o} \cdot \mathbb{1}_{mois_o=octobre}$$

Y_{ifo} represents the dependent variable associated with an individual i , whose contract ended on date f and who became eligible for benefits at date o . $mois_f$ and $mois_o$ represents the month in which the contract ended and the month i became eligible for benefits, respectively. The coefficient of interest is $\gamma_{3,2021}$, $\gamma_{3,2017}$ and $\gamma_{3,2022}$ are the placebo coefficients.

Putting this strategy into practice required us to conduct a series of validity tests in advance. With regard to the absence of manipulation effects affecting contract end dates, this hypothesis would usually be tested by means of a regression discontinuity exercise looking at the distribution density of the variable in question around the threshold value (e.g. the test proposed by McCrary, 2008). Such discontinuity might nonetheless exist in this case, due to the seasonality of contracts ending, hence the advantage of adopting the difference in discontinuities method. One way of testing this hypothesis is to check that the number of contracts ending between late September and early October was not substantially different between 2021 and 2018.¹⁹ We also conducted a second series of tests, which consisted of verifying that the effect of the reform was, on the whole, not significant when we calculated the specification equation using individual characteristics (such as sector of activity or level of education) as dependent variables. This test allows us to ensure that we are comparing similar groups, which is an implication of the fact that we have continuous unobservables relating to contract end dates, and similar seasonal effects in 2018 as in 2021. Table S-2 in the Online Appendix shows the results of these two sets of tests. In addition, we also conducted the placebo tests detailed hereunder. For each variable, Table S-2 shows the difference-in-discontinuities coefficient and the standard deviation. For the first set of tests, we can see that the coefficient associated with number of contracts ending is clearly not

significant at the usual thresholds. In the interests of robustness, the second line shows a similar test for the number of individuals becoming eligible for unemployment benefits, with the same result. The subsequent lines correspond to the second set of tests, for variables such as sector of activity and level of educational qualification. Most of these coefficients were not significant, although there were fewer individuals with qualifications inferior to the high school diploma after 1st October.

5.1.3. Variables of Interest

We applied both of these strategies to dependent variables for the time elapsing between the end of the contract entitling recipients to claim benefits and their next employment contract. To be more precise, we constructed two indicators which were equal to 1 if the individual found a new job within no more than three months (or six months) of their previous contract coming to an end. These served as our indicators for the variables “return to employment within three months” (or “return to employment within six months”). Our data do not contain details of the career trajectories of individuals more than six months after the end of their registration with France Travail. Beyond six months after the end of a previous contract, the rate of attrition is differentiated.

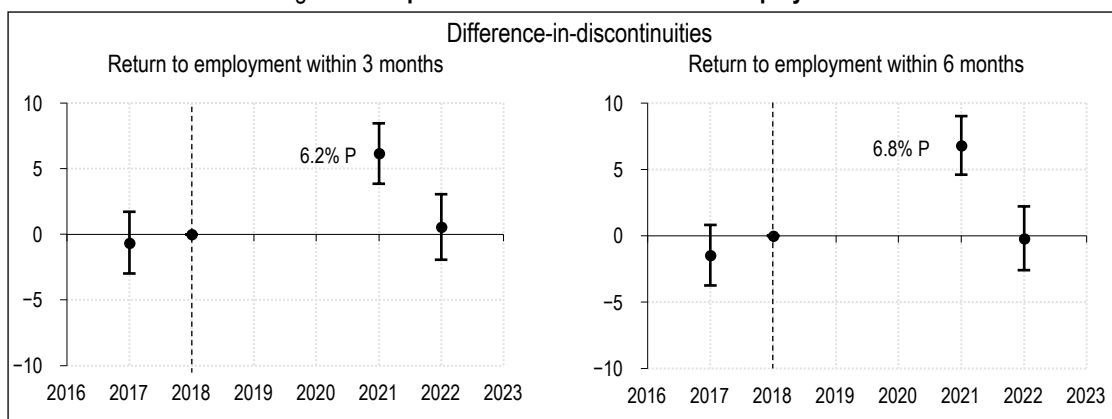
We also established “return to employment” indicators for different types of contracts (fixed-term contract, CDI, etc.). For example, the variable denoting return to employment on a fixed-term contract within three months was equal to 1 if the individual signed a new fixed-term contract within three months of the end of the contract which entitled them to claim unemployment insurance.

5.2. Results

Figure II shows the principal results of our regressions, for both the difference in discontinuities and triple difference approaches (full details of these coefficients can be found in Tables A1-1 and A1-2 of the Appendix). Our results show that the rate of return to employment three or six months after the end of a previous contract increased by 6.2 and 6.8 percentage points under the difference in discontinuities regression, and by 2.4 and 3.2 points using the triple difference method, bearing in mind that the respective rates of return to employment before the reform were

19. The idea is to calculate the same equation used for the specification (2), with an observation for contract end date, and thus without the vector control variables Z .

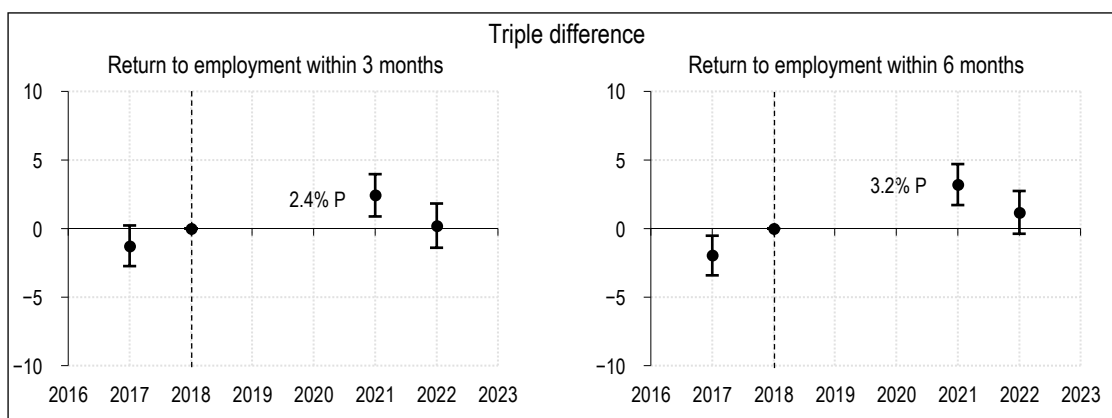
Figure II – Impact of the reform on return to employment



Note: These graphs represent treatment effects for a given year, with a confidence interval of 95%. The coefficients of interest are detailed in Table A1-1 in the Appendix.

Field: All new benefit claimants (BC) with discontinuous employment records whose most recent contract ended (CE) within a 15-day window either side of 1st October.

Source: FNA, France Travail, Unédic; DSN-FT, France Travail.



Note: These graphs represent treatment effects for a given year, with a confidence interval of 95%. Detailed regression tables provided in the Appendix: Table A1-2.

Field: All new benefit claims (BC) from individuals with discontinuous employment records in September-October 2017–2022.

Source: FNA, France Travail, Unédic; DSN-FT, France Travail.

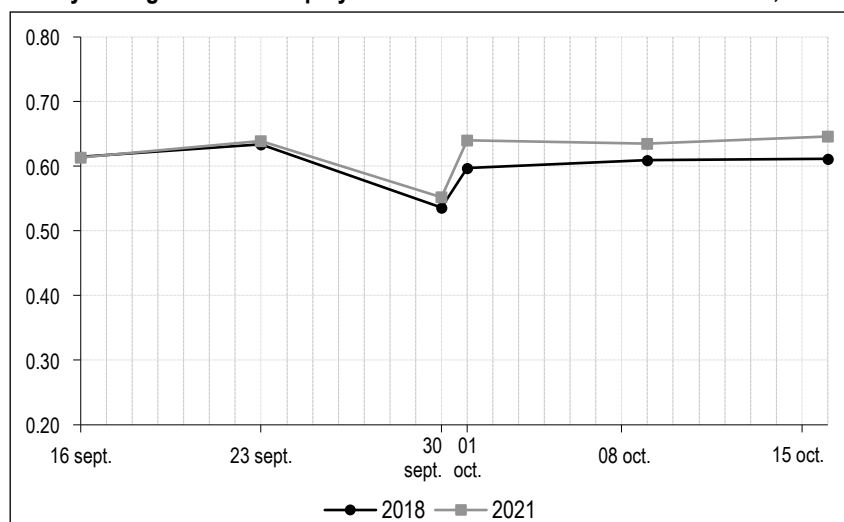
36% and 55%.²⁰ These results indicate that the reform is pushing jobseekers with discontinuous employment records to return to work more rapidly. Figure III is a visual representation of the effects associated with the difference in discontinuities model. It shows the weekly average rate of return to employments after six months, for a one-month period centred upon 1st October, in the years 2021 and 2018. We should note that the gap between the curve for the year 2018 and the curve for the year 2021 increases in what appears to be a discontinuous manner between the last week of September and the first week of October, due to a more substantial increase in people returning to work between these two weeks in 2021, compared with 2018.

This positive effect on return to employment was not visible in the preceding year (2017), nor the ensuing year (2022), where the coefficient was

either not significantly different from zero, or else significant but negative.

We then repeated this analysis for different types of contracts: fixed-term or temporary contracts of under six months, fixed-term or temporary contracts of more than six months, and permanent contracts. For each of these three categories, Figure IV shows the rate of return to employment within six months. Using both of our identification strategies, we found that the effects were concentrated primarily on fixed-term or temporary contracts of under six months. For these short contracts, the impact was estimated at 6.7 percentage points by the difference in discontinuities regression, and 3.5 percentage points using triple difference.

20. These percentages correspond to the average rate of return to employment for individuals with discontinuous employment records in September 2021.

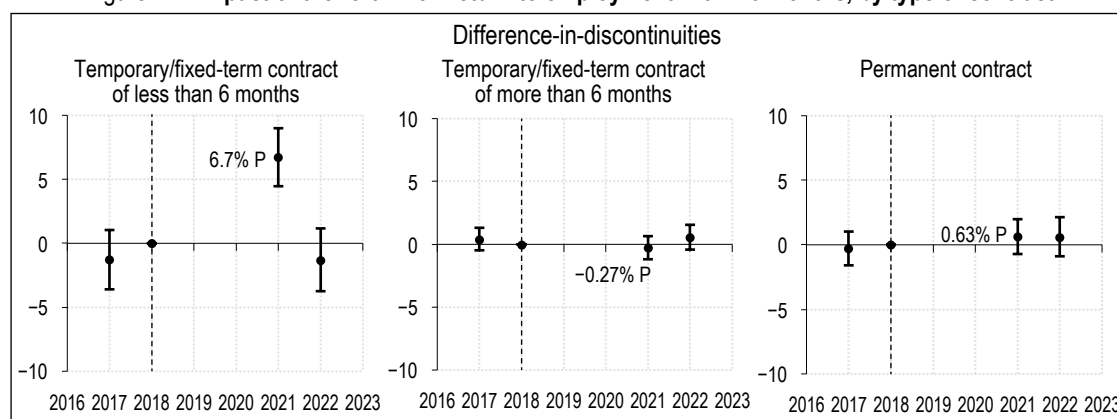
Figure III – Weekly average return to employment within 6 months around 1st October, for 2018 and 2021


Note: This graph shows the weekly average rate of return to employment for individuals whose most recent contracts ended between the date indicated and 7 days before that date and before 1st October, and 7 days after that date and after 1st October.

Field: All new benefit claimants (BC) with discontinuous employment records whose DRW falls within a 15-day window either side of 1st October.

Source: FNA, France Travail, Unédic; DSN-FT, France Travail.

Figure IV – Impact of the reform on return to employment within 6 months, by type of contract

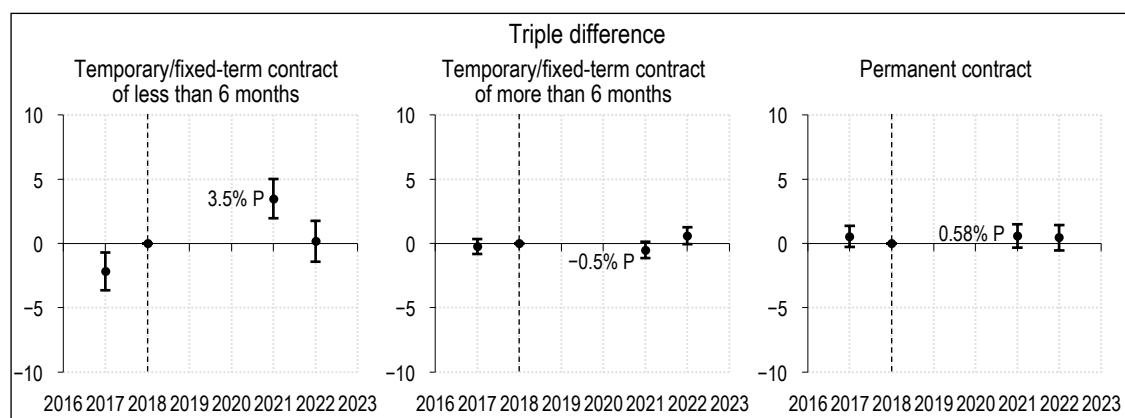


Note: These graphs represent treatment effects for a given year, with a confidence interval of 95%.

Detailed regression tables provided in the Appendix: Table A1-3.

Field: All new benefit claimants (BC) with discontinuous employment records whose DRW falls within a 15-day window either side of 1st October.

Source: FNA, France Travail, Unédic; DSN-FT, France Travail.



Note: These graphs represent treatment effects for a given year, with a confidence interval of 95%.

Detailed regression tables provided in the Appendix: Table A1-4.

Field: All new benefit claims (BC) from individuals with discontinuous employment records in September-October 2017–2022.

Source: FNA, France Travail, Unédic; DSN-FT, France Travail.

Finally, we sought to determine the extent to which measurements of the impact on return to employment varied with treatment intensity. To do so, we made use of the conceptual framework set out in Section 2.2, analysing variations in the treatment effect with reference to β (relative variation in potential duration of eligibility for benefits, estimated as the ratio between number of days out of work and number of days worked in the ERP period). The fact that the average effect of the reform on return to employment was positive suggests that, on the whole, the elasticity for probability of returning to work is more sensitive to the monetary value of benefits than it is to the potential duration of eligibility for said benefits ($\varepsilon_1 < \varepsilon_2$). Nevertheless, Section 2.2 suggests that even if this is the case, the effect at first grows as β increases, but later drops off and even becomes negative. For each of our specifications, we divided the sample into four equal parts corresponding to quartiles of β , (four “quarters” of the population) and estimated the effect for each of these quarters. The key here was to estimate the relative effects, since each quarter was characterised by a different initial level of access to employment. To do so, we re-estimated each specification in logit form, enabling us to calculate the average probability of an individual returning to employment with and without the reform, for each quarter, and then to calculate the ratio between the difference between these two probabilities and the probability of returning to employment had the reform never occurred. Table S-3 in the Online Appendix shows the results in full for both of our specifications. We found that, on the whole, the treatment effect became more substantial as the treatment intensity was increased.

These results suggest that, in keeping with the equations given in Section 2.2, the elasticity of the daily allowance was greater than the elasticity of duration, to such an extent that it compensated for the fact that, for a given variation in time, the proportional variation in the daily allowance was mechanically obliged to be smaller. This result echoes the findings of Fontaine *et al.* (2025) who, after conducting a similar exercise looking at this same reform, found elasticity values for the daily allowance of between 0.5 and 0.9, while the elasticities for maximum duration of benefits were lower, ranging from 0.06 to 0.22.

In summary, our results show that individuals with discontinuous employment histories more rapidly returned to work on short contracts, but were not any faster when it came to finding long-term contracts (fixed-term or temporary

contract of more than six months, or permanent contract). These effects were felt even more keenly by individuals who saw the nature of their benefits change as a result of the recent reform. In summary, our results show that individuals with discontinuous employment histories more rapidly returned to work on short contracts, but were not any faster when it came to finding long-term contracts (fixed-term or temporary contract of more than six months, or permanent contract). Our research suggests that the reform has inspired a short-term move towards more temporary jobs, with no perceptible short-term increase in the number of stable jobs.

5.3. Additional Robustness Tests

Figure II and Figure IV in Section 5.2 already contain robustness tests, consisting of estimating placebo coefficients for the years 2017 and 2022. As noted above, the results of these tests appear to confirm the validity of our methodology.

We conducted another placebo analysis for each of our two identification strategies, by setting 1st September 2021 as the fictitious implementation date for the reform, instead of 1st October 2021. The results are shown in Figure S-III of the Online Appendix. The triple difference placebo revealed no significant effect, confirming the robustness of the methodology. The difference in discontinuities placebo test did reveal an effect which was positive and significantly non-0 for all these years, which is likely to be indicative of an effect specific to September 2018, compared with September in other years, something which was not observed in the difference in discontinuities exercise for the 1st October cut-off date.

Finally, we used the difference in discontinuities method to estimate treatment effects for different numbers of days, for the specification described in Section 5.1 and for a quadratic specification, consisting just in adding an effect associated with the square of the difference between the number of days and the threshold on each side of the threshold. These results are shown in Figure S-IV in the Online Appendix, and show that, on the whole, the effects remained comparable in scale up to one month before and after the threshold date.

6. Using the SEJOUR Survey to Cast Further Light on the Effects of the Reform

In the preceding section, we spotlighted the effects of the reform in terms of getting people back into employment. Nevertheless, several questions remain unanswered, particularly with

regard to the mechanisms by which the effects of this reform are felt: through what channel does the decrease in benefits affect return to employment? We turned to the SEJOUR survey in search of potential answers to this question. The survey allowed us to compare, for a period subsequent to the reform, a sample of individuals with discontinuous employment records and a sample of individuals with unbroken employment records, looking at other characteristics than those already available in the administrative data we used. However, it should be noted that this is not a causal analysis (which would require us to have the same information for the pre-reform period), but rather an attempt to tease out post-reform differences in the characteristics of benefit recipients with discontinuous employment records and those with unbroken employment records, cross-comparing these findings with our observations from the administrative data with a view to facilitating interpretation of the results of the causal analysis.

First of all, among the individuals questioned for this survey, those with discontinuous employment records were more likely than other recipients to earn a net monthly wage of less than €2,000 (76% compared with 65%), and might thus be subject to greater financial constraints. The survey contains a number of variables pertaining to the degree of financial pressure experienced, which were of use to our analysis. We used three questions in particular (see Table S-4 in the Online Appendix). Firstly, the proportion of respondents estimating that the financial difficulties they would encounter if they were unable to find a new source of income would be “severe” (9 or 10 on a scale of 0 to 10) was greater among those with discontinuous careers than it was among those with unbroken employment records. Moreover, and with specific regard to individuals receiving benefits at the time of completing the survey, those with discontinuous records report having a harder time covering their basic needs with unemployment benefits alone (28.6% compared with 37.5%), while their savings are also relatively small (63.2% of individuals with patchy employment records would not be able to get by financially for more than two months on their personal savings, compared to 53.4% of people with continuous employment histories). We also observed that individuals with discontinuous employment records face tighter financial constraints than those with unbroken career histories. Since the survey was conducted after the reform, and does not allow for comparison

with the pre-reform period, it is not possible to determine whether the differences observed between the groups can be attributed to the reform. However, in light of the negative impact of the reform on the monetary value of benefits, it seems clear that the gap between the groups has not improved in the wake of the reform.

Furthermore, jobseekers interviewed for the SEJOUR survey were asked about what concessions they would be prepared to make in order to find a new job: a longer commute, a lower salary, less leisure and family time, a job which they perceive to be of lower quality, or with diminished working conditions, or a change in the type of work required by the job. Figure S-IV in the Online Appendix shows that individuals with discontinuous employment histories were more likely to be willing to make concessions in four of these six areas. The gap was particularly noticeable when it came to accepting less leisure time or a less qualified position (these two gaps were also significantly non-null at a 1% threshold).

Although descriptive in nature, these statistics suggest that, compared with those with continuous employment records, individuals with discontinuous employment histories earn lower wages, feel greater financial pressure and are more likely to be willing to make concessions. This chimes with our analysis of the administrative data, which suggests that the reform has sped up the process of getting people back to work – principally in temporary jobs – by means of a mechanical effect which has caused benefits to fall more sharply for individuals with discontinuous employment records. This resonates with previous studies, which have found that reactions to changes in benefits depend on people’s capacity to absorb disruption to their income. Reducing the benefits received by people already facing financial constraints may hurry them back into work, but not necessarily into quality jobs (Chetty, 2008; Landais, 2015).

* *
*

The reform to unemployment insurance initiated by the French government in 2019, implemented progressively between 2019 and 2021, contains various measures intended to provide a greater incentive to find lasting employment. Among these measures, the reform of the method used to calculate the DRW consisted of including days out of work between two periods of employment

when calculating both the average wage used to determine the value of benefit payments, and also the maximum possible duration of eligibility for benefit payments. This reform thus spreads out the benefits payable to individuals with discontinuous employment records, by reducing the daily value of these benefits while increasing the amount of time over which they can be paid.

Our analyses show that the reform has encouraged individuals with gaps in their employment history to return to work more rapidly, primarily by means of temporary contracts. To be more precise, the proportion of individuals with discontinuous employment records finding new jobs no more than six months after the end of the contract which saw them register with France Travail has increased by between 3.2 and 6.8 percentage points as a result of the reform, depending on the empirical specification we use to measure this rate. Overall, it has risen from 55% to around 60%. This effect has been driven by short-term contracts (less than six months), and the reform has done nothing to speed up the process of finding a temporary contract of more than six months, or a permanent contract. These effects are even more substantial for individuals affected by the reform, i.e. those whose

previous employment records contain one or more lengthy periods out of work.

Furthermore, the SEJOUR survey shows that, since the reform, individuals with discontinuous employment records have experienced greater financial pressures than others, and are more willing to make concessions when looking for work. Although it is not possible to directly attribute these results to the reform, since the new legislation has reduced the financial resources available to benefit recipients with discontinuous employment records, it seems plausible to draw a connection between their willingness to make concessions and the fact that more people in this category have returned to work rapidly in temporary posts since the reform took effect.

Finally, these effects were only estimated for a period of six months following jobseekers' registration with France Travail (due to data constraints), and we cannot exclude the possibility that there may be some delayed effects on long-term jobs. Moreover, this study focuses only on benefit recipients with discontinuous employment histories, and not the indirect effects on other populations (other jobseekers, unregistered individuals, etc.), making it impossible to draw any conclusions about net job creations resulting from this reform. □

Link to Online Appendix:

www.insee.fr/en/statistiques/fichier/9066696/ES550_LeBihan-Fabre_Online-Appendix.pdf

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APPENDIX

Table A1-1 – Difference-in-discontinuities: return to employment within different time frames

Return to employment within:	3 months	6 months
Date > 1 st October	0.0765*** (0.0087)	0.0641*** (0.0085)
2017× Date > 1 st October	-0.0063 (0.0120)	-0.0146 (0.0116)
2021× Date > 1 st October	0.0616*** (0.0117)	0.0681*** (0.0113)
2022× Date > 1 st October	0.0056 (0.0127)	-0.0019 (0.0123)
Number of days before 1 st October	-0.0094*** (0.0005)	-0.0069*** (0.0005)
2017× Number of days before 1 st October	-0.0010 (0.0008)	-0.0008 (0.0007)
2021× Number of days before 1 st October	-0.0039*** (0.0007)	-0.0027*** (0.0007)
2022× Number of days before 1 st October	0.0009 (0.0008)	0.0015** (0.0007)
Date > 1 st October × Number of days before 1 st October	0.0101*** (0.0011)	0.0082*** (0.0010)
2017× Date> 1 st October × Number of days before 1 st October	0.0038*** (0.0015)	0.0036** (0.0014)
2021× Date> 1 st October × Number of days before 1 st October	0.0046*** (0.0014)	0.0024* (0.0014)
2022× 1 st October × Number of days before 1 st October	0.0003 (0.0015)	0.0001 (0.0015)
Constant	0.4963*** (0.0895)	0.6058*** (0.0890)
R ²	0.0447	0.0359
Adjusted R ²	0.0445	0.0357
Number of observations	204,936	204,936

Note: Ordinary least squares estimates. p-values below 0.10 (*), 0.05 (**) and 0.01 (***). Standard deviation robust against heteroscedasticity shown in parentheses. Full results can be found in Table S-5 in the Online Appendix.

Field: All new benefit claimants (BC) with discontinuous employment records whose DRW falls within a 15-day window either side of 1st October.

Source: FNA. France Travail. Unédic; DSN-FT. France Travail.

Table A1-2 – Triple difference: return to employment within different time frames

Return to employment within:	3 months	6 months
Date > 1 st October	-0.0056* (0.0031)	0.0057* (0.0031)
2017× Date > 1 st October	0.0076* (0.0046)	0.0145*** (0.0047)
2021× Date > 1 st October	-0.0121*** (0.0044)	-0.0164*** (0.0045)
2022× Date > 1 st October	-0.0098** (0.0047)	-0.0142*** (0.0047)
Month of eligibility / DRW identical	0.0899*** (0.0038)	0.0747*** (0.0037)
2017× Month of eligibility / DRW identical	0.0157*** (0.0052)	0.0171*** (0.0051)
2021× Month of eligibility / DRW identical	0.0067 (0.0054)	0.0080 (0.0052)
2022× Month of eligibility / DRW identical	-0.0069 (0.0057)	-0.0123** (0.0055)
Date > 1 st October × Month of eligibility / DRW identical	0.0155*** (0.0054)	0.0216*** (0.0053)
2017× Date> 1 st October × Month of eligibility / DRW identical	-0.0126* (0.0076)	-0.0197*** (0.0074)
2021× Date> 1 st October × Month of eligibility / DRW identical	0.0242*** (0.0079)	0.0320*** (0.0076)
2022× Date> 1 st October × Month of eligibility / DRW identical	0.0021 (0.0082)	0.0118 (0.0080)
R ²	0.0546	0.0398
Adjusted R ²	0.0546	0.0398
Number of observations	533,561	533,561

Note: Ordinary least squares estimates. p-values below 0.10 (*), 0.05 (**) and 0.01 (***). Standard deviation robust against heteroscedasticity shown in parentheses. Full results can be found in Table S-6 in the Online Appendix.

Field: All individuals with discontinuous employment records becoming eligible for unemployment benefits.

Source: FNA. France Travail. Unédic; DSN-FT. France Travail.

Table A1-3 – Difference-in-discontinuities: return to employment within 6 months by type of contract

Type of contract	temporary or fixed-term	temporary or fixed-term of more than 6 months	permanent contract
Return to employment within:	(6 months)	(6 months)	(6 months)
Date > 1 st October	0.0796*** (0.0086)	-0.0092*** (0.0033)	-0.0076 (0.0050)
2017× Date > 1 st October	-0.0128 (0.0118)	0.0042 (0.0046)	-0.0028 (0.0067)
2021× Date > 1 st October	0.0673*** (0.0116)	-0.0027 (0.0047)	0.0064 (0.0069)
2022× Date > 1 st October	-0.0129 (0.0125)	0.0057 (0.0050)	0.0063 (0.0077)
Number of days before 1 st October	-0.0080*** (0.0005)	0.0005** (0.0002)	0.0004 (0.0003)
2017× Number of days before 1 st October	-0.0014* (0.0008)	-0.0001 (0.0003)	0.0005 (0.0004)
2021× Number of days before 1 st October	-0.0035*** (0.0007)	0.0007** (0.0003)	0.0001 (0.0004)
2022× Number of days before 1 st October	0.0017** (0.0008)	0.0001 (0.0003)	-0.0002 (0.0005)
Date > 1 st October × Number of days before 1 st October	0.0089*** (0.0010)	0.0007* (0.0004)	-0.0007 (0.0006)
2017× Date > 1 st October × Number of days before 1 st October	0.0046*** (0.0014)	-0.0005 (0.0006)	-0.0003 (0.0008)
2021× Date > 1 st October × Number of days before 1 st October	0.0026* (0.0014)	-0.0011* (0.0006)	0.0006 (0.0008)
2022× 1 st October × Number of days before 1 st October	-0.0003 (0.0015)	-0.0002 (0.0006)	0.0006 (0.0009)
Constant	0.6136*** (0.0918)	-0.0262*** (0.0068)	-0.0905** (0.0418)
R ²	0.0661	0.0071	0.0321
Adjusted R ²	0.0659	0.0069	0.0319
Number of observations	204,936	204,936	204,936

Note: Ordinary least squares estimates. p-values below 0.10 (*), 0.05 (**) and 0.01 (***). Standard deviation robust against heteroscedasticity shown in parentheses. Full results can be found in Table S-7 in the Online Appendix.

Field: All new benefit claimants (BC) with discontinuous employment records whose DRW falls within a 15-day window either side of 1st October.

Source: FNA. France Travail. Unédic; DSN-FT. France Travail.

Table A1-4 – Triple difference: return to employment within 6 months by type of contract

Type of contract	temporary or fixed-term	temporary or fixed-term of more than 6 months	permanent contract
Return to employment within:	(6 months)	(6 months)	(6 months)
Date > 1 st October	0.0092*** (0.0031)	-0.0020* (0.0012)	0.0036** (0.0017)
2017× Date > 1 st October	0.0082* (0.0046)	0.0017 (0.0018)	0.0009 (0.0025)
2021× Date > 1 st October	-0.0142*** (0.0044)	0.0004 (0.0018)	-0.0011 (0.0025)
2022× Date > 1 st October	-0.0032 (0.0047)	-0.0043** (0.0019)	-0.0041 (0.0028)
Month of eligibility / DRW identical	0.0800*** (0.0038)	-0.0020 (0.0015)	0.0028 (0.0022)
2017× Month of eligibility / DRW identical	0.0291*** (0.0052)	-0.0031 (0.0020)	-0.0093*** (0.0029)
2021× Month of eligibility / DRW identical	0.0038 (0.0053)	-0.0016 (0.0022)	0.0052 (0.0032)
2022× Month of eligibility / DRW identical	-0.0175*** (0.0056)	-0.0027 (0.0023)	0.0079** (0.0035)
Date > 1 st October × Month of eligibility / DRW identical	0.0239*** (0.0054)	0.0063*** (0.0021)	-0.0109*** (0.0031)
2017× Date > 1 st October × Month of eligibility / DRW identical	-0.0216*** (0.0075)	-0.0023 (0.0030)	0.0055 (0.0042)
2021× Date > 1 st October × Month of eligibility / DRW identical	0.0349*** (0.0078)	-0.0050 (0.0032)	0.0059 (0.0046)
2022× Date > 1 st October × Month of eligibility / DRW identical	0.0017 (0.0081)	0.0061* (0.0034)	0.0045 (0.0050)
Constant	0.5981*** (0.0609)	0.0232 (0.0191)	-0.0720*** (0.0271)
R ²	0.0683	0.0096	0.0284
Adjusted R ²	0.0683	0.0096	0.0283
Number of observations	533,561	533,561	533,561

Note: Ordinary least squares estimates. p-values below 0.1 (*), 0.05 (**) and 0.01 (***). Standard deviation robust against heteroscedasticity shown in parentheses. Full results can be found in Table S-8 in the Online Appendix.

Field: All individuals with discontinuous employment records becoming eligible for unemployment benefits.

Source: FNA. France Travail. Unédic; DSN-FT. France Travail.

Are We Measuring Working Hours Correctly? The Impact of Question Ordering in Surveys

Mikael Beatriz* and Louis-Alexandre Erb**

Abstract – A randomized controlled trial conducted as part of TraCov 2 (a survey investigating people's experiences of work since the COVID pandemic) allows us to estimate the impact of the order in which questions are asked on respondents' estimates of their working hours. It appears that respondents report working an average of 23 minutes more per week when they are asked to describe the nature of their working hours (night work, weekend work, work outside normal hours, etc.) *before* specifying the number of hours they work, instead of the other way around. Furthermore, this gap is most pronounced among higher- and intermediate-level occupations, categories which generally involve long, flexible working hours and frequent remote working. These results suggest that the nature of employees' working schedules has an influence on their perception of the amount of time they spend working. This opens up new possibilities, not only in terms of improving survey questionnaires, but also for further research into how working hours are measured.

JEL: C42, C93, J20, J81

Keywords: Working time, reporting bias, working conditions, experimentation

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In 2023, payroll employees reported that they worked an average of 36 hours per week, according to data derived from the TraCov 2 survey (survey on people's experiences of work since the COVID pandemic) conducted by DARES. This estimate is based upon the responses received to the question: "on average, how many hours do you work each week at your main job?" As a key indicator of transformations in the labour market, the amount of time that people spend working casts light upon production dynamics, work organisation strategies, and their consequences for working conditions and employees' health. Quantifying working hours is not easy, as demonstrated by the discrepancies between the figures from the employee surveys and the administrative data derived from employer declarations.

Even within the context of a household survey, estimates of working time may vary depending on the context, the wording of the question, and the way it is interpreted. Much like approaches based on legal norms – for example, the working hours stipulated in employment contracts – this approach raises questions regarding the social construction of working time indicators, not least their capacity to reflect the diversity of professional situations accurately.

In the TraCov 2 survey, the question was formulated in a "spontaneous" way, which is to say that its wording "gave respondents the freedom to define their own practices." (Héran, 1984) Nonetheless, the responses given by survey respondents are neither random nor devoid of meaning. Although, in the absence of an explicit definition, the term "work" could potentially be interpreted in several different ways in the context of the question "how many hours do you work?" ("work corresponds to my job", "work means when I am on the premises of my employer", "the number of hours I am obliged to be present", etc.), the responses obtained by this and other surveys reveal considerable statistical regularity for different groups (women, higher-level occupations, young people, etc.). In other words, as Gollac would have it (1997, p. 10), "supposedly spontaneous answers rarely are, which is precisely why they are meaningful." Spontaneous answers thus comply with underlying conventions and schemas, which may be more or less informed by legal categorisations (Demazière & Dubar, 1997; Melot & Péglise, 2008) and more or less shared by different groups. These factors must be analysed when interpreting responses. Two parameters in particular may explain the differences in estimated working times reported by

different surveys: firstly, the definition contained in the question, and, secondly, the design of the survey itself.

Under French law, the working hours of employees correspond to the amount of time they actually work, which is to say the period of time during which an employee is "at the disposal of their employer and complying with their directives, without the ability to freely attend to personal matters."¹ This definition does not dispel a certain ambiguity as to the distinction between work and non-work (Barrois & Devetter, 2017), an ambiguity which may be alleviated or exacerbated by historical developments in the organisation of working time. The introduction of per-day contracts, tax exemptions on overtime, relaxation of the rules on Sunday working and, more broadly, the increased flexibility of working hours have all contributed to a diversification of working schedules and, in the process, shifted perceptions of work depending on one's employment status and sector (Supiot, 1995; Lokiec, 2009; Miné, 2017). Furthermore, the perception of working time varies depending on the nature of working hours and the professional constraints involved, which are far from homogenous and universal across the workforce (Chenu, 2002; Bué & Coutrot, 2009). The increasing fragmentation of working hours (Bouffartigue & Bouteiller, 2002) has led to a proliferation of different contractual arrangements governing working time, while also clouding our perception and critical awareness of this time, making quantification all the more challenging (Pronovost, 2013).

The way in which the organisation of employees' working hours shapes their perception of work and its boundaries naturally has an influence on the way in which respondents formulate their answers to survey questions. In a survey dealing with the organisation of work, thinking about the internal logic and contextual positioning of the questions is every bit as important as the choice of sample or the degree of statistical precision. A quantification operation, in this case via a questionnaire, is never a neutral exercise, and this can have an impact on the information collected. On the one hand, it may be influenced by the interaction between the interviewer and the respondent – when an interviewer is present – as well as other factors such as language barriers, lexical interpretations, the context in which the survey is conducted and even the role played by the organisation commissioning the survey (Bessière *et al.*, 1997). On the other hand, the

1. Article L3121-1 of the Code du travail (Labour Code).

wording, order and presentation of the questions can also directly influence the responses received (Rossi *et al.*, 2013). For example, the distribution of responses is sensitive to question wording, positioning, format, visual presentation and response methods (Tourangeau *et al.*, 2004). Question ordering constitutes a “context effect”, a well-documented phenomenon (Höhne & Goerres, 2023). In France, the 2013 overhaul of the questionnaire used in INSEE’s Labour Force Survey illustrates this effect. That year, the question on the number of hours worked per week was repositioned just ahead of a series of questions dealing with weekend, evening and night work. Furthermore, a note was added to the question, encouraging respondents to include any overtime. These modifications prompted respondents to estimate their average weekly working hours to be 20 minutes shorter than the estimates obtained using the previous questionnaire (INSEE, 2013). This example is testament to the influence of survey design on the measurement of indicators, even those we would consider to be stable.

All of which raises an important question, namely: how does survey design influence measurements of declared working hours? Based on the experience garnered from the Labour Force Survey as discussed above, we can identify a number of possibilities. For example, omitting to describe working hours beforehand could lead respondents to underestimate their working hours, particularly those tasks which they take home with them – often “overlooked” by executives, for example – or hours worked outside usual working schedules, due to remote working, among other things (Metzger & Cléach, 2004; Genin, 2017). Likewise, explicitly mentioning overtime in the definition of working hours, or omitting to specify which hours should be declared, might steer interpretation of the question towards “official” overtime (as defined in the French Labour Code), i.e. leading respondents to underdeclare certain hours which exceed the normal or legal limits, particularly if they are neither paid nor compensated for this time (with days off in lieu, for example), which is in fact not even recognised as work. For example, periods of “effective non-work” may be considered as working hours by some segments of the population, such as service personnel, domestic employees or home help (Barrois & Devetter, 2017). Similarly, travel time or “free time which is actually occupied by work” – for example, when the cognitive burden of work exceeds the time actually spent on work activities because employees are

“preoccupied beyond the time spent performing their duties” – are often passed over in silence, particularly in sectors such as teaching, the arts and management (Supiot, 1995). At a more basic level, we must also remember the importance of undeclared overtime, believed to account for approximately 60% of all undeclared activity in Europe (AitBihiOuali & Bargain, 2021).

In order to examine how the questioning context influences declarations of working hours, the TraCov 2 survey deployed an original experimental protocol. The question pertaining to weekly working hours was positioned, at random, either before or after a series of questions on the nature of those working hours. This randomised controlled trial made it possible to draw comparisons between two independent groups of respondents: those who encountered the “working time question” before the series of questions digging deeper into those hours, and those who encountered it after. It thus becomes possible to estimate the gap between the numbers of working hours declared in both of these situations, since the individual characteristics of respondents likely to influence their declarations should be controlled by the structure of the experiment. If a significant gap is observed, we can thus attribute it to the ordering of the questions. This protocol was tested on three different sub-samples, each corresponding to a different mode of data collection, enabling us to estimate differences in declared working hours with different question orders for our three collection modes (telephone, internet, sequence of telephone/internet).

The contribution of this study is threefold. Firstly, it serves to deepen our understanding of survey design. Secondly, it casts light on other contemporary issues in which the quantification of working time is a key variable. These include the slowdown in the apparent productivity of labour, which certain studies have sought to explain by examining quantification tools, as well as disparities in the hourly wages earned by women and men, making it essential to take full consideration of differences in working hours. Finally, at a more general level, this study enriches the existing literature on working time and recent transformations in employment, particularly in light of evolving regulatory and organisational frameworks.

Following a brief review of the existing literature on the effects of survey protocol on the quantification of working hours, the second section of this article sets out our survey protocol and methodology. In the third section, we present

our results and analyse them with reference to the characteristics of the employees. In the concluding section, we discuss the limitations and replication potential of this experiment, and the implications of our results for survey design in general, and the quantification of working hours in particular.

1. Factors Which Might Potentially Influence Measurements of Working Time in Surveys

1.1. Type of Survey

Several studies, predominantly in English-speaking countries, have examined estimates of working time in surveys. The majority of these studies compared different measurements of working time.

An initial series of studies compared the working hours declared by payroll employees with those declared by their employers. Mellow & Sider (1983) thus carried out a parallel survey of the employers of those employees surveyed in the Current Population Survey (CPS; the American equivalent of the European and French Labour Force Surveys). Their results indicated that the number of hours declared by employees was, on average, greater than the number of hours reported by their employers, and that this gap was bigger for executives and managers. This might be attributed to the fact that employers generally only have access to information pertaining to contractual working hours or – more broadly – to the hours for which they must pay (or otherwise compensate) their employees, which may lead them to omit to declare some of the hours these employees work (overtime, for example). However, other studies, such as that conducted by Rodgers *et al.* (1993), found no such gap between these two declarative sources, although it should be noted that the latter study focused on a single company, and as such is not representative of the workforce as a whole. It remains interesting nonetheless, as the company in question has a high rate of union membership among employees, who are paid by the hour, two factors which could account for the alignment of the two estimates.

A second approach consists of utilising time use surveys, in which respondents are asked to describe their activities over the course of a given day or week, breaking them down into very short blocks of time (generally around ten minutes). This detailed information makes it possible to calculate the amount of time that respondents actually worked, and compare the

resulting figures to the responses given to direct questions about their working hours. Based on time use surveys conducted in the USA in 1965, 1975 and 1985, Robinson & Bostrom (1994) found that the number of working hours declared spontaneously by respondents was greater than that derived from the detailed descriptions they gave of their working days. Their results also indicated that this phenomenon was more common among people working more than 50 hours per week, and among women. More recently, and using comparable methods, similar findings have been yielded by studies looking at Belgium, France and Canada (Robinson *et al.*, 2011; Pronovost, 2013). Finally, Frazis & Stewart (2004) compared the estimates derived from the Current Population Survey (CPS) and the American Time Use Survey (ATUS, the equivalent of the French *Emploi du temps* survey). The latter is a complementary survey to the CPS, making it possible to compare the responses given by the same respondents to the two surveys. The authors found that working hours were slightly overdeclared in the CPS. The gap was more pronounced among women and highly-qualified individuals.

Finally, a third method was proposed by Jacobs (1998) using the National Survey of the Changing Workforce published in the USA in 1992. Jacobs used this survey to compare spontaneous responses to a question about weekly working hours with an estimate calculated using declared work start and finish times. In order to avoid the risk of memory bias, the author thought it better to ask respondents what time they left for work and what time they returned home, rather than ask what time they started and finished work. He then subtracted their commuting time to obtain an estimate of the duration of their working day. Unlike the methods described above, this approach revealed no statistically significant disparity between the two measurement methods.

These results indicate that asking respondents to spontaneously quantify the number of hours they work generally leads to a higher figure than that obtained using more formal quantification methods. This gap appears to be bigger for certain types of employees (such as higher-level occupations, women, and the most highly qualified workers).

Nonetheless, it is difficult to state with any certainty that spontaneous responses systematically lead to “overdeclaration” as Robinson & Bostrom (1994) and Pronovost (2013) have suggested, and thus to argue that time use

tables are the best way to proceed. In truth, other methods of quantifying working hours are not exempt from bias. The underdeclaration of certain hours, as mentioned in the introduction, is a striking example of this. More broadly, and quite aside from criticising the activity classification schemes employed in time use surveys, which, like all classification schemes, are influenced by social representations and paradigms (Desrosières, 1989), we must engage more effectively with a structural difficulty: quantification through a description of the working day implies a hierarchical prioritisation of activities within specific periods. For example, an employee commuting via public transport and using this time to write work emails or finalise presentations might see this activity categorised principally as “travel between home and work” and “normal professional work” as a secondary activity, or vice versa. But that activity could just as well be categorised as “surfing the internet” or “other activities on computer”, presenting further difficulties of interpretation. Once again, prevailing representations of paid work influence not only the design of the survey itself, but also the ways in which people respond to it. Finally, the research cited above uses a time use grid which spans 24 hours, not a whole week, which may prevent us from extrapolating their results to represent an entire working week.

1.2. Questionnaire Structure

The way in which surveys are designed may influence the responses they receive. Körner & Wolff (2016) have compared the working hours declared in the Labour Force Surveys (LFS) for France and Germany. By means of a methodical analysis of the breakdown of the working population in each country and of the quantification methods used in their respective surveys, they sought to identify the origin of the apparent disparity in working times between these two nations. Among the explanations they proposed, question positioning and accompanying instructions play a key role. In the German LFS, the question regarding working hours is prefaced with a paragraph explaining what is to be included in the definition of working time. This paragraph does not explain that respondents should only count their primary job, nor does it specify whether any or all breaks should be counted. Moreover, as the authors note, the length of this introductory passage means that interviewers are unlikely to read it in full. The French LFS also contains an introductory paragraph before the working hours question, but the wording is different: it specifies which

activities should not be counted as working time, and makes clear that the question concerns only the respondent’s main job. Moreover, the question is positioned at the tail end of a long series of related questions concerning working time: sick leave, time off for training, overtime etc. This is not the case in the German LFS survey. The progressive ordering of the French questionnaire prepares respondents – most likely acting as a cognitive facilitator – warming them up and prompting them to think about the different component parts of their working week before giving an overall estimate of their hours. Although it is not possible to put a precise figure on these intuitions, in the absence of a robust experimental protocol, the article nonetheless highlights the importance of survey structure when it comes to estimating working time.

At a more general level, Rossi *et al.* (2013) have identified five separate effects which could potentially influence responses to survey questions: (1) the revelation effect: preceding questions may, through a process of conscientisation, alter the response that an individual would spontaneously have given without those questions; (2) the redundancy effect: questions on the same subject scattered throughout the questionnaire may cause respondents to wonder whether or not they have understood the questions correctly, and thus potentially to change the way they answer; (3) the consistency effect: respondents may seek to maintain a degree of consistency in their answers to questions addressing the same subject; (4) the fatigue effect: after being asked a large number of questions on similar subjects, the precision of answers may wane or partial non-responses may begin to appear; and (5) the confidence effect: essentially the opposite of the previous problem, whereby respondents gain in confidence and become more precise in their responses as the questionnaire progresses. Any and all of these factors could be in play in the TraCov 2 survey, analysed in the subsequent sections of this article.

2. Data and Methodology

2.1. About the TraCov 2 Survey

The TraCov 2 survey is the second edition of the TraCov survey conducted by DARES (the statistical department of the Ministry of Labour). Its goal was to assess the working conditions and health status of workers three years after the onset of the COVID-19 public health crisis.

Data gathering began on 12 January 2023 and concluded on 10 April 2023, addressing a

random sample of 63,579 individuals between the ages of 18 and 64 and residing in France (excluding Mayotte), chosen from among all individuals living in ordinary housing and present in INSEE's Fidéli register (*Fichiers démographiques sur les logements et les individus* – Housing and individual demographic files). The survey was conducted using a hybrid protocol: the sample was split into three separate 'subsets' before data collection commenced. Individuals assigned to the first subset were invited to take the survey online, those in the second subset were contacted by telephone, while those in the third subset were contacted sequentially: they were first invited to complete the online questionnaire and, if they had not responded within a few weeks, they were given a second chance to take the survey over the telephone. The survey protocol and polling strategy are described in Beatriz (2025).

A total of 38,155 individuals responded to the survey, a response rate of 60% (57% for the first subset, 61% for subset 2 and 71% for subset 3). Of these respondents, 28,122 were in employment, of whom 24,660 were payroll employees. The scope of our analysis is limited to payroll employees, excluding those employed directly by individuals, who were not asked to respond to all of the questions regarding the organisation of their working hours. This left us with a final sample of 22,630 individuals, 22,617 of whom provided an estimate of the number of hours they worked each week. In order to limit the influence of extreme values or outliers, and ensure the robustness of our estimates, working time data were winsorized, which is to say that all observations falling below the 1st distribution centile and beyond the 99th distribution centile were pegged to the values of the 1st and 99th centiles respectively (this operation is discussed in further detail in Section 3).

2.2. Measuring Working Hours, and the Randomized Controlled Trial

The amount of time worked by employees can be defined and measured in different ways (Létraublon & Zilloniz, 2016). One option is to measure this time on a *legal* basis. The legal duration of the working week is set out in the Labour Code (35 hours, equivalent to 1,607 hours per annum for a full-time employee), and deviations from this legal duration can be used to determine overtime and the like. Another solution is to measure working time on a *collective* basis, i.e. measuring the hours worked by a group of full-time employees and displayed in their place of work, or set *individually* for part-time

employees. With the collective measurement, the result may deviate from the legal standard when measured over a seven-day period. A third option is known as *usual* working hours, measuring the hours worked in a standard week without any exceptional events such as leave days or public holidays, and including all of the hours spent working. Finally, working time can also be calculated on an *effective* basis. This is measured annually, but can be extrapolated to give a figure for a standard week by including all hours worked, with or without payment, and excluding all hours which respondents did not work (holiday, public holiday, illness etc.).² Each of these measurement approaches has its advantages and disadvantages, and the choice of which method to use will depend on one's goals: legislative purposes, collective bargaining, statistics (e.g. calculating hourly wages), analyses of professional relationships, studies of lived experience of work, etc. The TraCov 2 survey measures the average number of weekly working hours declared by respondents, in keeping with the objectives of the survey (cf. Section 2.1). This measurement is close to the *usual* working hours method.

The random aspect of this experiment concerns those questions pertaining to working hours. The (average) time that respondents spend working each week was measured by means of an open question with no specific instructions. Once the sample had been drawn and for each individual in the sample, a random draw was performed following a Bernoulli distribution with a probability of 0.5 in order to assign them randomly to one of two groups. This outcome of this draw determined the positioning of the question regarding working hours: either before the questions regarding the nature of those working hours (*group A*), or else after (*group B*) (see Box).

The section dealing with the nature of working hours includes questions on atypical working patterns (evening, night, early morning or weekend work) and whether the time worked is calculated in days and not hours (so called *forfait en jours*). Depending on the response to the latter question, a follow-up question explores "extra" work (i.e. work done outside the usual hours, or during free time, by employees whose time worked is calculated in days). For those respondents who reported engaging in such extra

2. Using data from the Labour Force Survey, annual figures are derived from the reference week by multiplying the latter by the number of weeks in a year. This makes it possible to account for variations in activity over the course of the year without bias (for example, working less in August or late December), in so far as reference weeks are evenly spread across the year for the sample used in the Labour Force Survey.

Box – Details of Questions Pertaining to Working Time in the Tracov 2 Survey

The question allowing us to quantify working time was worded as follows:

Question 1: “On average, how many hours do you work each week at your main job?”

Respondents answered in full hours, with no decimals. There were no additional instructions. This question was placed either before (group A) or after (group B) the following questions:

Question 2: Do you work evenings, nights or early mornings (staggered hours)?

- Every day
- Often
- Sometimes
- Never

Question 3: Do you work weekends?

- Regularly
- Occasionally
- Never

Question 4: Is your time worked calculated in days (as opposed to in hours)?

- Yes
- No

Depending on the answer to Question 4, the question regarding extra time differed slightly

Respondents answering “Yes” to Question 4 then received the following question:

Question 5a: Do you sometimes work during your free time, on account of your workload?

- Always
- Often
- Sometimes
- Never

Or:

Question 5b: Do you sometimes work outside your usual hours, on account of your workload?

- Always
- Often
- Sometimes
- Never

If the respondent reported working overtime (Always, Often or Sometimes to Question 5a or Question 5b), they then faced the following question:

Question 6: On average, how many extra hours do you work each week?

- Less than 1 hour per week
- Between 1 and 3 hours per week
- Between 3 and 7 hours per week
- More than 7 hours per week

work, a final question asked them to estimate the excess time they spent working each week.

The response rates for groups A and B were the same. From a socio-demographic perspective (Table 1), as with employment conditions (Table 2), the composition of the two groups was broadly similar. The breakdown of respondents between groups A and B was also balanced in each of the three subsets which make up the sample, and subset 3 was balanced in terms of the way in which respondents actually completed the survey – online or on the telephone (see Table A in the Appendix).

Partial non-response, i.e. cases in which respondents completed the survey but did not answer certain questions, was permitted by this survey protocol but remained rare. For example, thirteen respondents failed to answer the principal question regarding their usual working hours (11 in group A and 2 in group B). These respondents were excluded from the study. Once these non-respondents had been set aside, there were no more non-responses to the questions regarding work on weekends or at night. The absence of partial non-responses can most likely be attributed to the relative clarity of the questions, although we cannot rule out the possibility

Table 1 – Socio-demographic characteristics of respondents in the groups (in %)

		Overall	Question asked first (Group A)	Question asked afterwards (Group B)
		(100%)	(49.5%)	(50.5%)
Gender	Women	50	49	50
	Men	50	51	50
Age	Under 30	21	20	21
	Age 30–39	23	24	23
	Age 40–49	25	25	25
	50 and over	31	31	30
Level of qualification	Below high school diploma	29	30	28
	High school diploma (<i>baccalauréat</i>)	21	21	21
	2 to 4 years of higher education	31	30	31
	5 or more years of higher education	20	20	20
Family circumstances	No children	49	49	49
	Child(ren) of age 3 or over	41	41	41
	At least one child under the age of 3	10	10	10
In a relationship	No	36	36	36
	Yes	64	64	64

Interpretation: 50% of respondents to the survey were women, with 49% in group A and 50% in group B.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

Table 2 – Employment circumstances of respondents in the groups (in %)

		Overall	Question asked first (Group A)	Question asked afterwards (Group B)
		(100%)	(49.5%)	(50.5%)
Socio-professional category	Higher-level occupations	25	25	25
	Intermediate-level occupations	24	25	24
	White-collar workers	29	28	29
	Blue-collar workers	22	22	22
Employer	Private sector ⁽¹⁾	75	76	74
	Civil Service	25	24	26
Contract	Fixed-term ⁽²⁾	15	14	15
	Permanent or civil service	85	86	85
Part-time	No	85	85	85
	Yes	15	15	15
Seniority	Less than 1 year	17	17	18
	Between 1 and 5 years	29	29	30
	Over 5 years	53	54	52
Size of organisation	Fewer than 10 employees	23	22	23
	10–49 employees	30	29	30
	50–499 employees	32	32	32
	500 employees or more	16	16	16
Possibility of remote working	Remote working possible ⁽³⁾ but not practiced	15	15	14
	Remote working possible and practiced occasionally (max. 1 day per week)	13	13	13
	Remote working possible and practiced regularly (more than 1 day per week)	13	13	13
	Remote working not possible	60	59	60

⁽¹⁾ Including associations and public companies.

⁽²⁾ Fixed-term contracts, temp workers, apprentices, interns.

⁽³⁾ When asked “Do you want to work remotely”, employees who responded “Not applicable, my tasks cannot be performed remotely” were regarded as holding jobs where “remote working is impossible. All other respondents were thus considered to hold jobs where remote work would be possible. See Beatriz & Erb (2024a) and Beatriz & Erb (2024b) for a detailed description of this population.

Interpretation: 25% of respondents to this survey were employed in executive positions.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

that some respondents indulged in *satisficing* – i.e. completing the survey with the minimum possible effort. However, non-responses were recorded for the doubtless more complex questions regarding per-day calculation of time worked (1.44% partial non-responses among all respondents, i.e. 325 respondents; 1.41% and 1.47% in groups A and B respectively) and extra hours (0.39% for question 5a; 0.11% for 5b; 0.15% for 6). Partial non-response rates were similar for groups A and B, which suggests that question ordering does not influence this phenomenon. We dealt with these issues as follows: non-responses to the question regarding per-day calculation of time worked were marked as “No”, while non-responses to the other questions were aligned with the modal value of all responses. Given the very low rate of partial non-responses, this phenomenon is not liable to influence subsequent results and will not be discussed any further.

Finally, we sought to ascertain whether or not the answers to the series of questions regarding the nature of working hours were affected by the positioning of these questions within the questionnaire, i.e. before or after the question on number of hours worked (Table 3). Certain declaration disparities were observed. Slightly fewer individuals in group B reported working nights, evenings or early mornings, while more of their number reported a calculation of their time worked in days (not in hours). While the

proportion of employees sometimes working outside their usual hours did not vary between the two groups, the number of hours worked outside usual hours did: when the question about the number of hours worked per week was asked after the other questions (group B), the proportion of respondents declaring more than 3 hours of extra work was 2.5 percent higher.

3. Results

3.1. Descriptive Statistics

On average, the employees in group A declared that they usually worked 2,183 minutes per week (36 hours and 23 minutes), compared with 2,209 minutes for those in group B (36 hours and 49 minutes). The gap between these two groups thus stands at 25 minutes,³ a difference which is significant at the usual thresholds. In other words, employees who answered the questions about the nature of their working hours *before* the question on their average weekly working hours reported working longer than those in the other group. In the rest of this article, we will use the term *excess time* to refer to the working hours of group B minus those of group A.

This excess time does not appear to vary much in response to individual characteristics (Table 4).

3. Any disparities observed between the different working times declared by groups A and B (3rd – 2nd columns) and the gap indicated in the 4th column are due to rounding up/down.

Table 3 – Responses to questions on the nature of working hours, by group

		Question asked first (Group A)	Question asked afterwards (Group B)	Gap (Group B) – (Group A)
Working evenings, nights or early mornings (question 2)	Every day/Often/Sometimes	30.4% (0.4)	29.0% (0.4)	-1.4** (0.6)
Working on weekends (question 3)	Regularly/Occasionally	24.2% (0.4)	23.2% (0.4)	-0.9* (0.6)
Calculation of time worked in days – <i>forfait-jour</i> (question 4)	Yes	30.6% (0.4)	32.7% (0.4)	2.1*** (0.6)
Overtime (questions 5a et 5b)	Always/Often/Sometimes	59.7% (0.5)	60.8% (0.5)	1.0 (0.7)
Average number of overtime hours per week ⁽¹⁾ (question 6)	Less than 1 hour	23.5% (0.5)	21.8% (0.5)	-1.7** (0.7)
	Between 1 and 3 hours	46.1% (0.6)	45.3% (0.6)	-0.8 (0.9)
	3 hours or more	30.4% (0.6)	32.9% (0.6)	2.5*** (0.8)

⁽¹⁾ For those employees who reported working overtime.

Note: Significance thresholds of 10% (*), 5% (**) and 1% (***). Standard errors are in parentheses.

Reading: On average, 30.4% of employees in group A work evenings, nights or early mornings, compared with 29.0% in group B, a gap of -1.4 percentage points.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

It is slightly higher for employees with intermediate qualifications (from high school leavers to those with 4 years in higher education), and those between the ages of 30 and 39 with at least one child under the age of 3. For the latter category, having children is associated with changes to working hours, a higher likelihood of working unusual hours, or changes to working patterns (Erb *et al.*, 2022). These descriptive results might thus suggest that some of their work is being shifted outside the usual hours, leading to a higher frequency of hours worked outside the standard schedule, for example when some parents leave work early to take care of their children, then continue working later in the

evening, a practice which could make it more difficult to estimate average working hours.

However, excess working time does appear to vary more substantially between different employment conditions (Table 5). Those in intermediate-level occupations and higher-level occupations tended to have more excess working time (43 and 41 minutes respectively) than white-collar workers (27 minutes), and especially blue-collar workers, for whom no excess working time was observed.⁴ This gradient is

4. Higher-level occupations correspond to the French socio-professional category cadres, intermediate-level occupations to professions intermédiaires, white-collar workers to employés, and blue-collar workers to ouvriers.

Table 4 – Weekly working hours declared with reference to the positioning of the question and the socio-demographic characteristics of the respondents

	Question asked first (Group A)	Question asked afterwards (Group B)	Gap (in minutes) (Group B) – (Group A)
Overall	36 h 23	36 h 49	25.1*** (7.0)
Gender	Women	34 h 45	24.1*** (9.8)
	Men	37 h 35	28.9*** (9.5)
Age	Under 30	35 h 16	19.1 (18.8)
	Age 30–39	36 h 43	36.8*** (13.0)
	Age 40–49	37 h 03	14.5 (12.8)
	50 and over	36 h 09	27.9** (12.8)
Level of qualification	Below high school diploma	35 h 18	–9.4 (14.1)
	High school diploma (<i>baccalauréat</i>)	35 h 25	41.1*** (14.5)
	2 to 4 years of higher education	36 h 24	41.4*** (11.1)
	5 or more years of higher education	39 h 04	27.9** (16.8)
Family circumstances	No children	36 h 01	30.2*** (10.7)
	Child(ren) of age 3 or over	36 h 47	15.8* (10.2)
	At least one child under the age of 3	36 h 20	44.4** (21.3)
In a relationship	No	35 h 22	20.7* (13.0)
	Yes	36 h 52	26.7*** (8.2)

Note: Significance thresholds of 10% (*), 5% (**) and 1% (***). Standard errors are in parentheses.

Reading: Employees in group A reported working an average of 36 hours and 23 minutes per week, while those in group B declared 36 hours and 49 minutes, i.e. 25.1 minutes more.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

Table 5 – Weekly working hours declared with reference to the positioning of the question and the employment conditions of respondents

		Question asked first (Group A)	Question asked afterwards (Group B)	Gap (in minutes) (Group B) – (Group A)
	Overall	36 h 23	36 h 49	25.1*** (7.0)
Employer	Private sector ⁽¹⁾	36 h 32	37 h 01	29.3*** (7.9)
	Civil Service	36 h 00	36 h 16	15.2 (14.6)
Socio- professional category	Higher-level occupations	39 h 48	40 h 28	40.5*** (14.6)
	Intermediate-level occupations	36 h 06	36 h 48	42.8*** (11.3)
	White-collar workers	33 h 40	34 h 07	27.4** (13.6)
	Blue-collar workers	36 h 24	36 h 03	-21.4 (15.1)
Seniority	Less than 1 year	34 h 31	35 h 19	47.4*** (19.2)
	Between 1 and 5 years	36 h 33	36 h 41	8.2 (13.6)
	Over 5 years	36 h 50	37 h 17	27.9*** (8.9)
Contract	Fixed-term ⁽²⁾	33 h 08	33 h 36	27.1 (22.2)
	Permanent or civil service	36 h 54	37 h 20	25.3*** (7.2)
Part-time	No	38 h 16	38 h 33	17.4*** (6.3)
	Yes	25 h 49	26 h 37	48.2*** (19.1)
Size of organisation	Fewer than 10 employees	34 h 53	34 h 57	3.8 (15.8)
	10–49 employees	36 h 12	36 h 33	21.8** (12.6)
	50–499 employees	36 h 47	37 h 27	40.1*** (11.7)
	500 employees or more	38 h 01	38 h 33	32.3** (17.3)
Possibility of remote working	Remote working possible ⁽³⁾ but not practiced	36 h 50	37 h 35	44.8*** (17.7)
	Remote working possible and practiced occasionally (max. 1 day per week)	38 h 29	39 h 34	65.3*** (17.1)
	Remote working possible and practiced regularly (more than 1 day per week)	38 h 27	39 h 21	54.2*** (16.6)
	Remote working not possible	35 h 19	35 h 24	4.6 (9.5)

⁽¹⁾ Including associations and public companies.

⁽²⁾ Fixed-term contracts, temp workers, apprentices, interns.

⁽³⁾ When asked “Do you want to work remotely”, employees who responded “Not applicable, my tasks cannot be performed remotely” were regarded as holding jobs where “remote working is impossible”. All other respondents were thus considered to hold jobs where remote work would be possible.

Note: Significance thresholds of 10% (*), 5% (**) and 1% (***). Standard errors are in parentheses.

Reading: On average, higher-level occupations in group A reported working 39 hours and 48 minutes per week, while those in group B declared 40 hours and 28 minutes, i.e. 40.5 minutes more.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

broadly consistent with other observations about qualification levels. A similar trend can be observed with regard to remote working: for those in jobs where remote working is impossible (Beatriz & Erb, 2024a; 2024b),⁵ no excess time was observed. Excess time was observed in the other categories, however, especially among those who do effectively work remotely (between 54 and 65 minutes, compared with 45 minutes for employees who could work remotely but do not). Excess time also varies depending on the type of job: it was significantly non-null for employees in the private sector (29 minutes) and those working for organisations with more than 10 employees.

The distribution of working hours also tells us something about those work patterns most likely to lead to excess working time (Table 6). Up to and including 35 hours/week, the proportion of employees in each hourly category shows little

variation between groups A and B. However, the variation becomes very clear among those individuals working the legally stipulated number of weekly working hours (35 in France), where the proportion of respondents in group B is 4.6 points lower than in group A. Above 35 hours, and in virtually all categories, respondents in group B were systematically more numerous than their counterparts in group A.

3.2. Econometric Analysis

In order to confirm these initial results, we estimated linear regression models where the dependent variable was the declared number of working hours and the principal explanatory

5. When asked "Do you want to work remotely", employees who responded "Not applicable, my tasks cannot be performed remotely" were regarded as holding jobs where "remote working is impossible". All other respondents were thus considered to hold jobs where remote work would be possible.

Table 6 – Distribution of usual number of working hours depending on the positioning of the question (in %)

	Overall	Question asked first (Group A)	Question asked afterwards (Group B)	Gap (in percentage points) (Group B) – (Group A)
Less than 8 hours	1.4	1.3	1.5	0.1
From 8 h to less than 10 h	1.2	1.2	1.2	0.1
From 10 h to less than 15 h	1.0	1.0	1.0	-0.1
From 15 h to less than 20 h	1.7	1.7	1.7	0.0
From 20 h to less than 25 h	3.3	3.3	3.2	0.0
From 25 h to less than 30 h	3.2	3.4	3.1	-0.3
From 30 h to less than 35 h	5.0	5.1	5.0	-0.1
35 h	24.7	26.9	22.4	-4.6
36 h	3.0	2.8	3.2	0.4
37 h	6.5	6.3	6.6	0.4
38 h	5.7	5.4	6.0	0.6
39 h	10.7	11.0	10.4	-0.6
40 h	13.8	13.8	13.8	0.1
41 h	1.2	0.9	1.5	0.6
42 h	3.1	2.6	3.6	1.0
43 h	1.1	0.9	1.4	0.5
44 h	0.6	0.4	0.7	0.3
45 h	5.0	4.7	5.3	0.6
From 46 h to less than 50 h	1.5	1.2	1.7	0.6
From 50 h to less than 55 h	3.9	3.8	4.0	0.2
From 55 h to less than 60 h	0.8	0.7	0.9	0.2
More than 60 hours	1.7	1.6	1.8	0.2

Note: These gaps may not correspond precisely to the difference between the two proportions, due to rounding up.
Reading: On average, 1.4% of survey respondents worked fewer than 8 hours per week. The proportion was 1.3% in group A and 1.5% in group B, a gap of 0.1 percentage points.
Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

Table 7 – Differences in declared weekly working hours depending on the positioning of the question

	Without control variables	With controls	With controls for employment conditions	With controls for socio-demographic and employment conditions
	(1)	(2)	(3)	(4)
Group B (Ref.: Group A)	+25.1*** (7.0)	+25.9*** (6.6)	+22.7*** (5.9)	+22.5*** (5.9)
R ²	0.001	0.097	0.288	0.296
Observations	22,617	22,617	22,617	22,617

Note: Linear regression coefficients with significance thresholds of 10% (*) , 5% (**) and 1% (***). The standard errors in parentheses are corrected for heteroskedasticity. The socio-demographic control variables are: age, gender, level of education, family circumstances and relationship status (see Table 1); the control variables for employment conditions are socio-professional category (Level 1 in PCS 2020), nature of employer, type of contract, full/part-time work, time in post, size of organisation and possibility of remote working (see Table 2).

Reading: On average, socio-demographic characteristics and employment conditions being equal, employees in group B report working 22.5 minutes longer each week than those in group A.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

variable was the group to which the employee was assigned (A or B). The estimated models were then enriched with variables for socio-demographic characteristics, working conditions or both (Table 7).

Including all control variables (socio-demographic factors and employment conditions) did not alter the estimated excess working time. This result is not surprising, since respondents

were assigned to the two groups randomly for the purposes of the controlled trial.

3.3. Heterogeneity Analyses

In order to build upon our analysis of excess working time and identify the groups of employees most affected by this phenomenon, we conducted the heterogeneity analyses outlined below.

Table 8 – Differences in declared weekly working hours depending on the positioning of the question and socio-professional category

		(1)	(2)
Group (Ref.: Group A)	B	-21.3 (15.1)	-16.1 (13.0)
Socio-professional category (Ref.: Blue-collar workers)			
	Higher-level occupations	204.3*** (14.4)	136.1*** (15.7)
	Intermediate-level occupations	-18.5 (13.0)	6.6 (12.4)
	White-collar workers	-164.3*** (14.1)	-16.3 (12.67)
Group B x Socio-professional category (Ref.: Group B x Blue-collar workers)			
	Higher-level occupations	61.9*** (20.9)	63.5*** (18.7)
	Intermediate-level occupations	64.1*** (18.8)	49.5*** (16.4)
	White-collar workers	48.8*** (20.3)	31.2* (17.0)
Controls		None	SD, CE
R ²		0.067	0.296
Observations		22,617	22,617

Note: Linear regression coefficients with significance thresholds of 10% (*) , 5% (**) and 1% (***). The standard errors in parentheses are corrected for heteroskedasticity. The socio-demographic control variables (SD) are: age, gender, level of education, family circumstances and relationship status (see Table 1); the control variables for employment conditions (CE) are socio-professional category (Level 1 in PCS 2020), nature of employer, type of contract, full/part-time work, time in post, size of organisation and possibility of remote working (see Table 2).

Reading: On average, socio-demographic characteristics and employment conditions being equal, higher-level occupations report 63.5 minutes more excess working time than blue-collar workers.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

3.3.1. By Socio-Professional Category, Remote Working Practice and Full-Time or Part-Time Work

Table 8, (9 and 10, respectively) represents an estimate of a linear model to which we added, as an explanatory variable, a variable combining a dummy for belonging to group B and the respondent's socio-professional category (or situation regarding remote working in Table 9, or full-time work dummy in Table 10). The

descriptive results are broadly corroborated, with some nuances.

Only higher-level occupations, intermediate-level occupations and white-collar workers had excess working time. This result is consistent with the result regarding remote working, for two reasons (Table 9). Firstly, because only posts compatible with remote working were found to be associated with significant excess working time, and such posts are almost entirely the

Table 9 – Declared weekly working hours depending on the positioning of the question and relationship to remote working

	(1)	(2)
Group (Ref.: Group A) B	4.6 (9.5)	6.8 (8.0)
Possibility of remote working (Ref.: Remote working not possible)		
Remote working possible but not practiced	90.9*** (13.2)	27.4** (11.9)
Remote working possible and practiced occasionally (max. 1 day per week)	189.1*** (14.0)	43.1*** (13.2)
Remote working possible and practiced regularly (more than 1 day per week)	187.4*** (13.5)	-19.3 (13.9)
Group B x Possibility of remote working (Ref.: Group B x Remote working not possible)		
Remote working possible but not practiced	40.2** (19.2)	19.0 (16.7)
Remote working possible and practiced occasionally (max. 1 day per week)	60.7*** (19.6)	48.8*** (16.8)
Remote working possible and practiced regularly (more than 1 day per week)	49.6*** (19.1)	49.8*** (16.9)
Controls	None	SD, CE
R ²	0.033	0.298
Observations	22,617	22,617

Note: See Table 8.

Reading: On average, socio-demographic characteristics and employment conditions being equal, employees in positions where remote working is possible, and those who do work remotely occasionally, report 48.8 minutes more excess working time than those in positions where remote working is not possible.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

Table 10 – Differences in declared weekly working hours depending on the positioning of the question and full-time versus part-time work

	(1)	(2)
Group (Ref.: A) B	17.5*** (6.3)	18.0*** (6.1)
Type of duration of work (Ref.: Full-time) Part-time	-746.3*** (13.9)	-677.2*** (13.8)
Group B x type of duration of work (Ref.: Group B x full-time)	30.7 (20.1)	30.9 (19.4)
Controls	None	SD, CE
R ²	0.245	0.296
Observations	22,617	22,617

Note: See Table 8.

Reading: On average, socio-demographic characteristics and employment conditions being equal, the excess working time declared by part-time employees was 30.9 minutes greater than that declared by full-time employees, and this value is not significantly different from 0.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

preserve of higher- and intermediate-level occupations (see Beatriz & Erb, 2024a). Secondly, because the actual practice of remote working, which is even more socially situated, increases the estimated amount of excess time. Finally, working part-time rather than full-time did not lead to significant differences in excess time (Table 10). Nevertheless, it is difficult to state with any certainty that those in part-time do not work excess hours, as the estimates for this category were somewhat imprecise on account of their smaller numbers.

3.3.2. By Mode of Data Collection

Since both groups were randomly divided into three data collection subsets, it is possible to explore the possibility that the disparity in working time measured between groups A and B might vary depending on the survey mode of collection.

The existing literature has demonstrated that individuals respond differently to questionnaires when they are administered in different ways (Beck *et al.*, 2022). Being asked a question by an interviewer (over the telephone, for example) may induce a greater social desirability bias – respondents tend to give responses cleaving closely to acceptable social norms, even if their personal circumstances are quite different – than with self-administered questionnaires (in this case, filling in an online form without the presence of an interviewer). This phenomenon has a greater impact on questions of a subjective or sensitive nature, which may be the case here (for example, the temptation to declare a higher number of working hours). Such differences can indeed be observed, since in both group A and group B, the declared working hours were slightly higher in subset 2 (telephone interviews) than in subset 1 (online only) (Table 11).

Estimates of excess time for each separate subset are naturally less precise, but they remain significant at a 10% threshold for all three subsets. Table 7 was thus recalculated, adding an indicator for subset number and how this interacted with group Affiliation (Table 12). Responding online rather than over the telephone yielded a significantly lower estimate of working hours, which may indicate that the social desirability bias was felt more keenly over the telephone. Furthermore, the interaction term was not significant at a threshold of 5%, which suggests that the disparity between groups A and B did not differ statistically between these data collection modes. When adopting a 10% threshold, however, excess time does appear to be greater among respondents in the second subset (interviewed on the telephone), which might be attributed to the fact that it is easier to scroll back up the page on the online questionnaire, and thus to modify one's previous response to the question regarding average working hours. Notwithstanding the smaller sample sizes which somewhat limit the precision of the estimate, these results show that this excess time was significant in all the subsets.

3.4. Robustness

The results detailed above could be affected by the winsorization operation performed to eliminate outliers (see Section 2.1).

Responses for average duration of working time were winsorized in order to peg outlying values to the values of the 1st and 99th percentiles. Respondents with extreme working hours were distributed with equal probability between groups A (95 respondents, 0.84% of the sample, below the 1st percentile; 113 respondents, or 1.0%, beyond the 99th) and B (80 respondents,

Table 11 – Declared weekly working hours depending on the positioning of the question and survey mode of collection

	Question asked first (Group A)	Question asked afterwards (Group B)	Gap (Group B) – (Group A)
Overall	36 h 23 (4.9)	36 h 49 (5.0)	25*** (7.0)
Subset 1 Internet	36 h 23 (6.3)	36 h 40 (6.5)	17* (9.1)
Subset 2 Telephone	36 h 47 (10.5)	37 h 16 (10.5)	28* (14.8)
Subset 3 Internet/telephone	36 h 05 (10.8)	36 h 53 (11.3)	47.1*** (15.6)

Note: Significance thresholds of 10% (*), 5% (**) and 1% (***). Standard errors are in parentheses.

Reading: On average, in the subset 2, respondents in group A reported working 36 hours and 47 minutes, while those in group B declared 37 hours and 16 minutes, i.e. 28 minutes more.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

Table 12 – Differences in declared working hours by data collection mode

		(1)	(2)
Group (Ref.: Group A)	B	16.7* (9.1)	13.4* (8.4)
Subset (Ref.: Subset 1)	Subset 2 (telephone)	24.2** (12.2)	25.7*** (9.7)
	Subset 3 (internet/telephone)	-17.4 (12.5)	-5.0 (10.4)
Group B x Subset (Ref.: Subset 1)	Subset 2 (telephone)	11.7 (17.4)	26.8* (13.9)
	Subset 3 (internet/telephone)	30.5* (18.1)	19.8 (15.3)
Controls		None	SD, CE
R ²		0.012	0.297
Observations		22,617	22,617

Note: See Table 8.

Reading: On average, socio-demographic characteristics and employment conditions being equal, respondents in subset 2 of group B declared working significantly more hours each week than their counterparts in subset 1 of group B (25.7 minutes more).

The difference in excess working time between respondents in subset 2 and subset 1 is not significant at the 5% threshold.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

Table 13 – Declared weekly working hours depending on the positioning of the question and the way outlying values were handled

	Question asked first (Group A)	Question asked afterwards (Group B)	Gap (in minutes) (Group B) – (Group A)
Winsorization at 1 st and 99 th percentiles (method used in this article)	36 h 23	36 h 49	25.1*** (7.0)
Trimming at 1 st and 99 th percentiles	36 h 29	36 h 54	25.5*** (6.5)
Outlying values left unaltered	36 h 30	37 h 04	34.6*** (8.0)

Note: Linear regression coefficients with significance thresholds of 10% (*) , 5% (**) and 1% (***). Standard error values given in brackets.

Reading: On average, after winsorization of the values, employees in group A reported working 36 hours and 23 minutes, while those in group B declared 36 hours and 49 minutes, a gap of 25.1 minutes.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

or 0.70%, below the 1st percentile; 103, or 0.9%, beyond the 99th). In order to ensure that the estimate was not unduly influenced by this corrective measure, we recalculated in two different ways: firstly, without winsorization, i.e. retaining the outlying values, and secondly by simply trimming off those outlying values. The results are presented in Table 13.

Winsorization and trimming yielded broadly similar estimates of excess working time, whereas omitting the outlying values pushed the estimate of excess time to 35 minutes. This is not surprising, since these operations are designed to reduce the distribution amplitude by targeting the biggest values. We believe that correcting these extreme values is a pertinent choice, however, not least because some values are very clearly the results of typos or respondents

misunderstanding the question (for example, 17 individuals reported that they worked, on average, between 150 and 168 hours per week). Moreover, as the variance in the average number of working hours without correcting for these extreme values is greater (standard error of 8, compared with 7 for winsorization or trimming), the estimation of excess time is less precise.

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Within the confines of a randomized controlled experiment, our work demonstrates the presence of a revelation effect affecting declarations of average working hours, an effect provoked by the ordering of the questions concerning the

nature of these hours. The existing literature had already observed the existence of significant differences depending on the method used to estimate working hours (declared by the employee or the employer) and the tools involved (direct question, time use schedule, logbook, etc.). Several studies have also mooted the possibility that survey design could play a role, without evaluating any potential causality, while others have used experimental approaches to demonstrate the significant influence of contextual factors on declarative responses in general, without considering the specific applicability of this phenomenon to the measurement of working hours. This article seeks to fill that gap.

On a more general level, this study casts light on the way we quantify the average amount of time people spend working each week. Our results contain two main lessons. Firstly, when employees describe the nature of their working hours (working nights, Saturdays, Sundays, outside standard working hours, etc.) before declaring the total duration of their working week, rather than afterwards, they declare an average of 23 extra minutes. Secondly, this “excess time”, as we have called it, is primarily reported by employees holding higher- and intermediate-level occupations, or those who may or do work remotely, i.e. employees whose effective working hours may differ from the hours stated in their contract, or may not be subject to strict controls, making them more difficult for respondents to estimate.

Although the extent of this excess time may seem relatively modest at the individual level, it is far from negligible in aggregate terms. Extrapolated over a whole year, for an employee working 46 weeks per annum, this excess time amounts to around 18 hours of extra work. Considering all employees (with the exception of those employed by private individuals) in France excluding Mayotte, this would amount to around 405 million working hours. By way of a comparison, that would be broadly equivalent to the total number of hours worked each year by employees in the agricultural sector.

Moreover, the wording of the question regarding average number of hours worked has been identical in the INSEE’s Labour Force Survey questionnaire since 2023, and is harmonised with European standards as per the new regulations governing integrated social statistics (*Integrated European Social Statistics*, IESS, 2021). The positioning of this question, and the ordering of the preceding or subsequent questions, and the

presence or absence of further contextual explanations, remain within the margin of adaptation afforded to each country. However, the results presented here demonstrate that such operational decisions are not without consequences, and can influence the manner in which respondents assess and declare their working hours.

Our experimental protocol unambiguously demonstrates the effect of question ordering, because the only difference between our two versions of the questionnaire was the positioning of the working time question in relation to the series of questions on the nature of working hours. Nonetheless, interpretations of this effect may vary. An initial way of reading this difference, which would appear to be the most consistent with the results observed, would be to attribute it to a revelation effect: quizzing employees in advance and in detail on the organisation of their working hours thus encourages them to more systematically include all periods of time during which they actually do work. The excess time reported is thus the result of respondents counting their working hours more thoroughly than they would have done had the working time question been posed without warning at the outset. In this view, positioning the detailed questions about working hours before the question on total working time allows us to account more effectively for the hours that people actually work.

Other mechanisms could be at work, but their compatibility with the results appears less convincing. A fatigue effect (Rossi *et al.*, 2013) linked to the later positioning of the question on working time in the questionnaire could potentially lead employees to adjust their responses unintentionally. But this effect seems unlikely to account for the declaration of longer working hours.

The estimated effect varies considerably depending on the population surveyed: one population appeared to be entirely unaffected, while another witnessed a significant effect. Higher- and intermediate-level occupations are the only socio-professional categories for whom the effect was significant. We also noted that the effect was only felt by those in posts compatible with remote working. Unlike white-collar and blue-collar workers, higher- and intermediate-level occupations are more likely to work long, variable or excess hours (Bué & Coutrot, 2009). Night and weekend work (which is more likely to involve blue- and white-collar workers; Nguyen, 2022) takes place within a clearer, more standardised institutional

framework, which means that it more closely resembles usual working time. These hours are also frequently associated with compensation incentives such as additional pay or time off in lieu (Baudelot & Gollac, 1993). In other words, excess time is recorded when such deviation from standard hours is generalised, hard to pin down or vague, to borrow Barrois & Devetter's terminology (2017), for example when working remotely or bringing work home at the end of the day. Far from being mere artefacts, these differences illustrate the invisible mechanisms which regulate work, particularly when the boundaries between work time and personal time are unclear. By playing with question ordering, this experiment succeeds in revealing some of these working time "grey areas", which more traditional measurement tools struggle to capture.

It is important to situate the transformation of working hours within their historical perspective. It is part and parcel of the structural transformations that have reshaped payroll employment since the 1980s, characterised by the increased flexibility of jobs and organisations: a substantial increase in temporary employment contracts, a boom in part-time work (Marchand & Minni, 2019) and, since the 1990s, a general intensification of work (Askenazy, 2004), followed by the massive expansion of remote working in the 2020s (Beatriz & Erb, 2024a). These developments have contributed to the crumbling of the temporal boundaries between personal and professional lives. It is a phenomenon which

illustrates the creeping presence of work beyond its formal time and place, embodied in recent times by the rise of remote working.

This study represents a new addition to the literature on measuring working time and, more broadly, examining the dynamics of payroll employment. Any methodological approach to measuring working hours is necessarily a construct, whose results will depend upon the survey protocol. There are several potential avenues for building upon the present study. A protocol allowing for comparison between objective measurements (administrative data, for example, or data from time clocks) and self-declared estimates would allow us to better grasp the variation associated with data collection modes. In parallel to such efforts, qualitative monitoring of actual working activity, sensitive to the specificities of different professions, would cast further light on forms of work which are otherwise unseen or unrecognised. Finally, it would be pertinent to repeat this experiment with a larger sample, more detailed investigation of working hour constraints (shift work, on-call work, breaking down weekend work to look separately at Saturdays and Sundays, etc.), or by increasing the degree of precision by asking respondents to estimate their weekly working time in hours and minutes. Further research in this vein would contribute to a more nuanced understanding of the differences between statistical sources and, beyond that, prompt us to think more carefully about the boundaries of work itself. □

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APPENDIX

Table A – Distribution of survey respondents by wave and data collection mode

(in %)		Question asked first (Group A)	Question asked afterwards (Group B)
Overall		49.5	50.5
Subset 1	Internet	49.7	50.3
Subset 2	Telephone	48.2	51.8
Subset 3	Overall	49.7	50.3
	Internet	50.1	49.9
	Telephone	49.5	50.5

Reading: 49.7% of people in subset 1 belong to Group A and 50.3% to Group B.

Source and field: DARES, TraCov 2 survey, employees excluding those employed by private individuals in France (exc. Mayotte).

Will Labour Shortages and Skills Mismatches Throw Sand in the Gears of the Green Transition in Belgium?

Mikkel Barslund*, Wouter Gelade** and Geoffrey Minne***

Abstract –This article aims to identify green shortage occupations, to characterise people who transition into these occupations, and to explore the factors that facilitate or hinder transitions into green occupations, focusing on the skills required to exercise these occupations. By linking Belgian Labour Force Survey data with international (O*NET) green job definitions and regional lists of occupations in shortage, we examine green shortage occupations, the workers who transition into them, and the role of skills and skill distances in facilitating or hindering these transitions. We find that half of green employment falls into shortage occupations. Technical and resource management skills are important drivers of transitions into green shortage jobs. Skill mismatches for green shortage jobs are large, yet workers who move into green shortage jobs experience less skill disruption than expected.

JEL: J23, J24, J62, Q52

Keywords: green jobs, labour shortage, skills

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The green transition is underpinned by a global political commitment to mitigate the environmental externalities of economic activities, which is expected to spur a rapidly growing demand for green skills. This growth should be particularly pronounced in Europe, driven by initiatives such as the European Green Deal (EGD), which aims to make the EU climate-neutral by 2050. The European Centre for the Development of Vocational Training (Cedefop, 2021) projects that between 2020 and 2030, achieving the EGD targets could lead to the creation of about 2.5 million additional jobs. Beyond job creation, achieving a clean energy transition is expected, above all, to require substantial qualitative changes in the labour market, including the tasks performed, the technologies used, and the skills required.

In parallel, many advanced economies, including Belgium, are experiencing significant labour and skills shortages. Seven out of ten Belgian companies identify the scarcity of skilled workers as a long-term impediment to investment (EIB, 2024). Even if this difficulty extends beyond environmental sectors, the demand for green skills, in particular, is expected to surpass the available supply of appropriately skilled workers. The feasibility of the green transition is closely tied to the alleviation of these shortages, as ongoing technological advances and regulatory shifts necessitate the development of new skills and the reconfiguration of existing tasks within the workforce (Dierdorff *et al.*, 2011; Vona *et al.*, 2015). Without sufficient workers taking up jobs we classify as green shortage jobs, the transition to a decarbonised economy may be hindered.

The aim of this article is threefold: we characterise green shortage jobs; we characterise the individuals who transition into green shortage jobs, including in terms of skills; and we explore the extent to which skills mismatches impact these transitions. Skills are central to this discussion, particularly given the role of innovation and the implementation of new technologies in sectors such as renewable energy and energy-efficient construction. As Cedefop (2023) aptly notes, “[t]he green transition calls for an inclusive skilling revolution involving all workers [...]”. Our study seeks to identify the factors that facilitate or hinder transitions into green jobs, with a particular emphasis on the alignment between workers’ skills, and on workers today employed in polluting industries. Labour and skills shortages are likely to go hand in hand with fewer job-to-job transitions into green jobs,

potentially exacerbating existing labour market mismatches.

We use longitudinal data from the Belgian Labour Force Survey (LFS, Eurostat (2017–2024)), supplemented by information on the green nature of jobs taken from Occupational Information Network (O*NET) and regional lists of occupations facing recruitment challenges. Belgian regional public employment services identify occupations in shortage by combining a statistical analysis of the data and both internal and external expert judgement. This allows us to trace labour transitions into and out of green and non-green jobs, as well as into and out of shortage and non-shortage jobs over the 2017–2024 period (excluding COVID years). Our analysis also draws on other variables from O*NET classifications including skills requirements, and working conditions and LFS detailed information on workers, including demographics, educational attainment, and industry.

These data allow us first to establish that approximately one third of Belgian workers are employed in occupations that contribute to the transition towards a decarbonised economy (green jobs), and for about half of them these occupations experience labour shortages (green shortage jobs). Green shortage jobs are predominantly occupied by men, as are to a lesser extent green non-shortage jobs. Green shortage jobs are less demanding in qualifications than other green jobs: 31% of the workers in green shortage jobs have a high level of educational attainment (tertiary education), compared to 62% in green non-shortage jobs. Conditional on educational attainment and other observed characteristics, green shortage jobs offer wages on average about 3 percent higher than those of green non-shortage jobs and around 5–7 percent higher than those of non-green jobs, with a premium especially strong among workers with lower educational attainment.

From one year to the next, most workers remain within their occupational categories (green shortage, green non-shortage, non-green shortage, non-green non-shortage), with only 4–7% changing category. Transitions to green shortage jobs are predominantly undertaken by young workers, by males, and by workers living outside Brussels. They are more frequent among those initially employed in construction and industry. For such transitions, technical skills are essential, as well as resource management skills. Focusing on workers in brown jobs—i.e., those that harm the environment, a sub-group of non-green jobs—we find that this

small subsample shares some characteristics with green shortage workers, such as lower educational attainment and better employment conditions than workers in other occupational categories.

Transitions into green shortage occupations are likely to involve significant changes in skills needed. We find that the mismatch between the skills possessed by workers and those required for their closest green shortage jobs (*potential* job changes) is larger than the mismatch with respect to their closest non-green or non-shortage jobs. Workers in brown jobs are distinctive in this respect, as their skill distance to the closest green shortage jobs is substantially lower. However, among *observed* job changes, transitions to green shortage jobs are those whose skill distance between the initial and the subsequent occupation is the lowest. Green shortage jobs typically require more experience and longer in-plant training. This may help explain why observed transitions into these occupations tend to occur between jobs with relatively similar skill profiles.

Focusing on the case of Belgium, our article explores the factors affecting labour mobility into green shortage occupations, bridging the literature on green jobs and that on shortage jobs. Extensive research has focused on the estimation of green job numbers (Vona *et al.*, 2019; Bowen *et al.*, 2018; Bowen & Hancke, 2019; Valero *et al.*, 2021; OECD, 2023), and highlighted the prevalence of technical jobs held by highly educated workers. Our study moves beyond the static description of green occupations and focuses on the factors that facilitate or hinder transitions into these occupations. This approach aligns with research on occupational mobility (Bachmann *et al.*, 2020 and recent studies on transitions to green jobs (EBRD, 2023; Causa *et al.*, 2024; Curtis *et al.*, 2024; see Apostel & Barslund, 2024 for a literature review). Our article also contributes to the literature on labour shortages and their economic impacts (e.g. Le Barbanchon *et al.*, 2023; Frohm, 2021; Groiss & Sondermann, 2025). Most papers highlight the significant effect of labour shortages on wages and employment dynamics. Although demand for skilled labour in the environmental sector is acknowledged, studies linking labour shortages to green jobs are limited (Horbach, 2014; Sato *et al.*, 2023). Belgium is an interesting case study due to the existence of structural labour shortages and the EU's ambitious climate targets which are leading to the implementation of strong measures to reduce emissions (EIB, 2024).

This topic is also relevant to policy as demand for green jobs is expected to grow as decarbonisation intensifies, while labour supply may lag. Survey evidence indicates that skilled worker shortages are already constraining private investment in green technologies (EIB, 2023), underscoring the need to promote green job transitions and address occupational shortages. In Belgium, the slow pace of transitions into green shortage jobs can be attributed to both limited occupational mobility and a deficiency in critical technical and resource management skills. Addressing these challenges would require a concerted policy effort to promote job-to-job transitions, facilitate skills acquisition, and adapt the training infrastructure to the higher demand for technical skills.

The article is structured as follows. In Section 1, we describe the dataset, and the way occupations and skills are classified. Section 2 provides some descriptive statistics. Section 3 presents our results on occupational transitions. Section 4 discusses our findings, then we conclude.

1. Data

Our analysis draws on the Belgian Labour Force Survey (LFS), a quarterly household sample survey conducted among individuals aged 15–89 residing in Belgium. It is carried out by the Belgian Statistical Office (Statbel) and is based on a sample of approximately 110,000 individuals per year. It contains information on their labour market status and the occupation of those concerned. Since 2017, the Belgian LFS is a rotating panel, in which individuals participate four times. They take part in the survey for two consecutive quarters (t and $t+1$), do not participate for the two following quarters, and participate again for another two consecutive quarters ($t+4$ and $t+5$). We will focus on yearly transitions by comparing people's labour market situation at time t and $t+1$ with that at time $t+4$ and $t+5$, respectively. We use data from 2017–2024, retaining only the 2017–2018, 2018–2019, 2022–2023 and 2023–2024 transitions, as survey administration and labour market functioning were partly disrupted during the COVID period. We use weights from Statbel to ensure representativeness.¹

1. The weights provided by Statbel consider both unequal selection probabilities and non-response bias, including attrition between waves. Since 2017, a three-step weighting procedure has been applied to calculate these weights: (1) calculation of design weights based on sampling probabilities, (2) correction for non-response and panel attrition using mixed-effects logistic regression models, and (3) post-stratification to align the weighted sample with known characteristics of the population of private households. This procedure improves the representativeness of the sample by correcting for non-response and panel attrition.

The matching of Belgian LFS observations to the classifications of shortage occupations, green/non-green jobs, brown jobs and skills is done at the four-digit ISCO level, the most detailed level. This is an international hierarchically structured classification operated by the ILO (International Labour Office), that includes 433 occupations in the non-military sector. Some earlier contributions to the literature have used LFS data with a less detailed occupational classification. Such aggregated approaches risk overlooking heterogeneity in both the green and shortage dimensions of occupations. For example, the three-digit category “Process control technicians” consists of occupations (in the four-digit level) differing across these dimensions. The sub-category “Incinerator and water treatment plant operators” is classified as green, while “Metal production process controllers” is not. In turn, the latter is in shortage, whereas another non-green occupation, “Petroleum and natural gas refining plant operators”, is not.

1.1. Green and Brown Jobs

Green jobs are defined as those intrinsically connected to activities or tasks aimed at mitigating the impact of humans on the climate, such as lowering greenhouse gas emissions, enhancing energy efficiency, recycling materials, and adopting renewable energy sources (Dierdorff *et al.*, 2011). We derive a green jobs indicator using the 24.1 version of the O*NET classification of green occupations. Our approach builds upon the methodology in Consoli *et al.* (2016), that relies on the task-based O*NET taxonomy.² It categorises green jobs into three distinct groups, namely, “Green New and Emerging (GNE)”, “Green Enhanced Skills (GES)”, and “Green Increased Demand (GID)”. However, for the main analysis, we consolidate these into one group to focus on the issue of labour shortages. This aggregated group includes, for example, energy auditors, wind energy engineers, and recycling workers (GNE); atmospheric scientists, urban planners, and power plant operators (GES); and chemists, electricians, insulation workers, and bus drivers (GID). Because our analysis ultimately relies on ISCO occupations, some aggregated occupations can be linked to more than one O*NET green subcategory.

The classification of green jobs is anchored in the US Standard Occupational Classification (SOC) codes and follows the O*NET’s Green Economy programme. The 905 occupations at the SOC eight-digit level have been assessed via a combination of surveys, case studies and expert judgement to determine the extent to

which green activities and technologies increase demand for each occupation (Dierdorff *et al.*, 2011). Each LFS respondent’s occupation being measured in the ISCO nomenclature, we proceed in a two-step mapping procedure: (i) aggregating SOC codes from the eight- to the six-digit level, and (ii) crosswalking six-digit SOC to four-digit ISCO. In the first step, we construct a binary indicator classifying six-digit occupations according to whether they include any green subcomponent at the eight-digit level.³ In the second step, we link the six-digit level SOC codes to the corresponding ISCO four-digit codes using a conversion table provided by the United States Bureau of Labor Statistics (BLS, 2015). Our baseline indicator is a binary variable equal to one if at least one SOC six-digit code mapped to a given ISCO four-digit occupation is classified as green, and zero otherwise. The most prevalent green occupations are listed in the Appendix A2.

We adopt a broad definition of green jobs, mainly because our analysis focuses on occupational mobility and its connection with shortage indicators. As in Consoli *et al.* (2016), our approach can inflate green job counts compared with studies using a continuous score based on the share of green skills in the occupations and/or stricter thresholds (e.g. Vona *et al.*, 2019; OECD, 2023).⁴ Using a green job indicator weighted by SOC six-digit composition, Valero *et al.* (2021) estimated green employment share in Belgium at around 20%.

Brown occupations, in contrast, are defined as those that harm the environment and typically occur in highly polluting industries. In practice, we rely on the list defined by Vona *et al.* (2018), which identifies the most represented jobs in

2. Using a product-based approach, such as Eurostat’s EGSS accounts, would require additional assumptions to map into LFS data and could misclassify workers when the focus is on occupations—e.g. including an accountant in a sewage-treatment plant or excluding a sustainability manager in a bank. Under this approach, green sectors represent about 2% of domestic employment in Belgium.

3. We follow the approach of Vona *et al.* (2019) who aggregated the O*NET’s eight-digit SOC occupations to the six-digit level assuming employees are uniformly distributed across eight-digit occupations within each six-digit SOC. No employment data at such detail are available, and according to Vona *et al.*, most green skill variation occurs at the six-digit level.

4. For example, the ISCO four-digit category “Building frame and related trades workers, not elsewhere classified” (ISCO 7119) is mapped from five six-digit SOC occupations: solar photovoltaic installers (SOC 47-2231), hazardous materials removal workers (SOC 47-4041), construction and related workers (SOC 47-4099), fence erectors (SOC 47-4031), and manufactured building and mobile home installers (SOC 49-9095). The first three are labelled green, as O*NET links them to tasks such as “Identify or separate waste products or materials for recycling or reuse” or “Install active solar systems.” Consequently, the “any” binary indicator classifies this ISCO code as green, and the frequency-weighted indicator is 0.6. The employment-weighted indicator—based on US and Belgian employment—is 0.74. Robustness tests with frequency- and employment-weighting are presented in the Appendix A1.

industries responsible for a large share of emissions of six air pollutants. Their binary indicator is defined at the six-digit SOC level, which we converted to ISCO using the same method as for green jobs. Finally, we excluded occupations labelled as green from the initial list of brown jobs. Most brown jobs are concentrated among the ISCO major groups of “craft and related trades workers” and “plant and machine operators and assemblers”.

1.2. Occupational Shortages

An occupational shortage is “[a] sustained disequilibrium in which the demand for workers exceeds the supply of workers available and willing to work at a given wage and under given working conditions, in a particular place and at a particular point in time” (Barnow *et al.*, 2013). Labour shortages are traditionally measured through job vacancies or a ratio between unemployment and job vacancies. Occupations may, however, face recruitment difficulties because specific skill profiles are scarce, even when labour shortages are not particularly large in quantitative terms. Such skill mismatches can complicate empirical analysis. And even when traditional shortage indicators point to labour shortages, they do not necessarily identify the factors underlying recruitment difficulties, such as skills mismatches, working conditions, or geographical constraints.

Our labour shortage indicator identifies occupations where filling vacancies is difficult, and recruitment takes longer than the average across occupations. These are identified annually as “critical functions” by Belgium’s three regions (Flanders, Wallonia, Brussels), using local expertise from regional public employment services. The regions have a legal duty to establish these lists, which are used to guide training policies and to identify educational pathways needed to alleviate labour shortages. Unemployed jobseekers can, under certain conditions, follow a training course targeted at one of these professions while retaining part of their unemployment benefits. Although each region follows its own methodology, in practice, there is clear overlap, with the three lists resulting from a comparable three-step procedure.

The first step is a statistical analysis of jobseekers and job vacancies. Employment services receive notifications of vacancies from recruiters and registrations from jobseekers in need of income support, job-search assistance or training. These inputs are classified according to occupation to derive a ratio between the number

of unemployed and the number of vacancies by occupation, which can be used to assess labour shortages. For some occupations, administrative data on vacancies and jobseekers is incomplete, and survey-based information is used instead.

The second step is the internal expert judgement. Staff members of the employment services responsible for employer relations, sectoral expertise, and training provide an assessment of the recruitment difficulties identified. They handle the job vacancies and maintain regular contact with employers, enabling them to identify the causes of recruitment difficulties.

The last step is the external expert judgment. The public employment services submit their lists to representatives of sectoral training funds and/or of employers’ federations, whose knowledge and experience enable them to complete, qualify or enrich the lists.

We rely on the regional lists of occupations in shortage established in 2023 that we mapped to the ISCO classification (4-digit level) by using a broad definition of shortage: an ISCO occupation is considered in shortage in a region if it matches at least one occupation on the regional list. Following the ISCO classification, 149 occupations are affected by shortages in Flanders, 105 in Wallonia, and 73 in Brussels.⁵ In general, the reasons behind shortages are multifaceted, can be mutually reinforcing and are often difficult to identify (Conseil Supérieur de l’Emploi, 2024). Shortages can be related to, among other things, difficult working conditions, stringent requirements concerning education and experience, or low remuneration. The decarbonisation of the economy meanwhile may generate shortages as it requires new skills and increases demand for a number of jobs.

Between 2013 and 2023, labour market tensions increased in Belgium and Europe. Many sectors experienced labour shortages after the COVID-19 crisis as the economic recovery was associated with a rapid and temporary expansion of aggregate demand (Coppens *et al.*, 2021). However, most shortages are structural (Conseil Supérieur de l’Emploi, 2024), with many occupations recurring annually on regional shortage lists. In 2023, Belgium ranked among the top three European countries with the highest number of shortage occupations (European Labour Authority, 2024).

5. Following their own classifications, the number of critical occupations or jobs in short supply is 241 in Flanders, 162 in Wallonia, and 102 in Brussels.

1.3. Skills and Skill Distance

In our analysis, skills are measured at the occupational level and sourced from O*NET data. They refer to developed capacities that either facilitate learning and the rapid acquisition of knowledge or enhance the performance of tasks across various jobs. There are seven distinct categories of skills: content, process, social, complex problem-solving, technical, systems, and resource management skills (Table 1). Each category comprises several dimensions, and in total, there are 35 sub-categories of skills; technical skills, for example, include installation, equipment maintenance, and quality control analysis, among others.⁶

For each occupation, we use the “importance score” assigned to each skill category required to perform the job. The importance score for a sub-category of skills is measured on a five-point Likert scale ranging from “not important” (1) to “extremely important” (5).

We then measure the skill distance between two occupations using an approach derived from the one used by the OECD (2024). The skill distance between occupations is defined as the distance between the skills required in occupation i and those required in occupation j , as follows:

$$Skill_distance_{i,j} = \frac{1}{7} \sum_{c=1}^7 skills_gap_cat_{c,i,j}$$

$$\text{with } skills_gap_cat_{c,i,j} = \frac{1}{n_c} \sum_{k=1}^{n_c} |S_{k,i} - S_{k,j}|,$$

and where $skills_gap_cat_{c,i,j}$ is the skills gap between occupations i and j for skill category c . It is calculated as the average gap in the importance of the sub-skills within this category. The importance of sub-skill k within occupation i is denoted $S_{k,i}$ and each skill category c contains n_c sub-skills. The skill distance between occupations i and j is then simply the average of all skill distances across all categories. Since the importance of each sub-skill is measured on a scale ranging from one to five, the skill distance ranges from zero to four.

2. Descriptive Statistics

2.1. Green Jobs, Brown Jobs and Occupational Shortages

Using our definitions of green jobs and jobs in shortage, we can allocate each worker to one of four categories: green shortage, green non-shortage, non-green shortage and non-green non-shortage. In Belgium, 53% of workers are employed in occupations in shortage, and 34% of all workers are in green occupations (Figure I). This share of 34% is partly driven

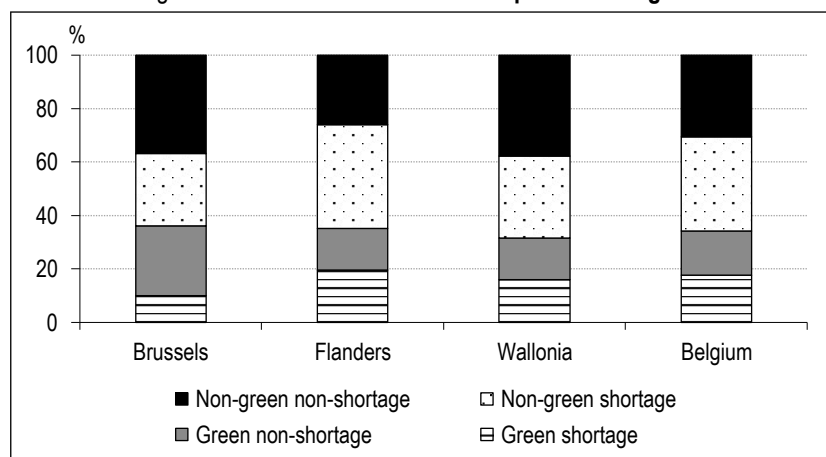
6. Further details on the subcategories are provided in the Online Appendix S1.

Table 1 – Definitions of skill categories

	Description	Examples of sub-skills
Content skills	Basic foundation needed to work with and acquire more specific skills in a variety of different domains	Reading; active listening; mathematics; science
Process skills	Knowledge of procedures that contribute to the more rapid acquisition of knowledge and skills across a variety of domains	Critical thinking; active learning; learning strategies; monitoring
Social skills	Developed capacities used to work with people to achieve goals	Social perceptiveness; coordination; negotiation; instructing
Complex problem-solving skills	Developed capacities used to solve novel, ill-defined problems in complex, real-world settings	Complex problem-solving
Technical skills	Developed capacities used to design, set-up, operate, and correct malfunctions involving the application of machines or technological systems	Technology design; installation; programming; operation monitoring; equipment maintenance; equipment selection; repairing; quality control analysis
Systems skills	Developed capacities used to understand, monitor, and improve socio-technical systems	Judgment and decision-making; systems analysis and evaluation
Resource management skills	Developed capacities used to allocate resources efficiently	Time management; management of financial, material and personnel resources

Source: O*NET.

Figure I – Share of the different occupational categories



Note: The chart distinguishing brown jobs from other non-green jobs is reproduced in the Online Appendix (see Figure S2-I)
 Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS 2017–2019 and 2022–2024.

by the definition of green jobs and falls to 15% under our most stringent alternative definition.⁷

Shortages are present across all sub-categories of green jobs—“green new and emerging,” “green enhanced skills,” and “green increasing demand”—but are particularly pronounced in the latter, where 70% of workers are in an occupation that faces shortages, compared with 51% and 45% in the first two, respectively. The share of people in green occupations varies little regionally, with 36% of green jobs in Brussels, 35% in Flanders, and 32% in Wallonia. The composition of labour shortages in Brussels is more concentrated in non-green occupations, consistent with its urban economic structure.

Slightly more than half of all green employment is in shortage occupations (18% of total employment). Shortage occupations account for nearly the same share of green and non-green jobs (54% in the latter, 51% in the former).⁸ Labour shortages in green jobs can stem from multiple factors, including demanding working conditions or stringent job requirements.⁹

The demographic breakdown of green and non-green jobs, distinguishing those in shortage reveals strong differences between categories (Table 2). Green jobs are mostly male, especially in shortage occupations, where 86% of workers are male, compared to 70% in green non-shortage and around 40% in non-green jobs. Age distribution is relatively similar across categories, with workers of prime working age (30–49) making up the majority. Educational attainment in contrast differs across categories: over half of green shortage workers have medium-level education (ISCED 3–4), compared with 30% in green non-shortage jobs, where

most hold higher education (ISCED 5+). Brown jobs, which form a subset of non-green jobs, represent 3% of the sample. They share several characteristics with green shortage jobs: they are also male-dominated (84%) and disproportionately held by medium- and low-educated workers.

Employment conditions are generally better in green shortage jobs. These positions are mostly full-time and feature a higher share of permanent contracts, although tenure is close to the average. Wages tend to be higher for green jobs compared to non-green jobs, particularly for non-shortage occupations. For instance, the average gross monthly wage for full-time workers in green non-shortage jobs is €4,770, which is significantly higher than the €3,990 earned by workers in non-green non-shortage jobs. The relationship between wages and job category also varies with educational attainment. While wages increase with education in all occupation categories, the differences between green shortage and green non-shortage jobs become

7. Using frequency-weighted or employment-weighted indicators yields a share of 15% (frequency-weighted index with a threshold of more than three quarters being green jobs) to 32% (frequency-weighted index with a threshold of more than one quarter being green jobs) of total employment. See Appendix A1 for details.

8. Occupations following a more stringent definition of green jobs are equally likely to be in shortage: the share of jobs in shortage ranges from 50% to 60%. Across green subcategories, the ‘increasing demand’ group exhibits a higher share of shortage jobs (72%), and the ‘enhanced skills’ group a lower one (46%).

9. Our data highlight the diversity of the potential causes of labour shortages, with significant differences between green and non-green shortages. An extension of the baseline empirical analysis, which examines labour shortages across several occupational characteristics, such as working conditions, prestige, and experience requirements, is presented in the Online Appendix S3. Green shortage occupations are more often associated with demanding working conditions and experience requirements, while low pay and low prestige are more typical of non-green shortages.

Table 2 – Demographic composition by occupational category – green/non-green, shortage/non-shortage, and brown occupations

	(1) Full sample	(2) Green shortage	(3) Green non-shortage	(4) Non-green shortage	(5) Non-green non-shortage	(6) Brown
Male	0.53	0.86	0.70	0.37	0.43	0.84
Age (in years)	41.4	41.3	42.0	40.8	41.6	41.0
Age 29 or less	0.19	0.19	0.15	0.21	0.18	0.22
Age 30–49	0.52	0.53	0.56	0.51	0.53	0.49
Age 50 or more	0.29	0.29	0.29	0.28	0.29	0.29
Low education	0.12	0.16	0.11	0.14	0.10	0.34
Medium education	0.38	0.53	0.28	0.37	0.35	0.61
High education	0.50	0.31	0.62	0.49	0.56	0.06
Born in Belgium	0.81	0.83	0.77	0.81	0.83	0.78
Resident in						
Brussels	0.10	0.06	0.16	0.08	0.12	0.05
Flanders	0.61	0.68	0.57	0.67	0.52	0.64
Wallonia	0.29	0.26	0.27	0.25	0.36	0.31
Working full-time	0.76	0.91	0.89	0.65	0.74	0.92
Seniority (in years)	10.76	10.57	10.68	10.45	11.27	11.01
Permanent contract	0.79	0.84	0.72	0.79	0.78	0.81
Gross monthly wage (in €)	3,982	4,029	4,767	3,490	3,987	3,022
Received formal training in the past four weeks	0.03	0.01	0.02	0.05	0.04	0.01
Number of observations	86,086	14,906	13,833	30,318	27,029	2,752

Note: Brown jobs are a subset of non-green jobs; due to their small sample size, shortage subdivision is not considered. Gross monthly wages are restricted to full-time employees. Unless otherwise specified, figures are shares. All variables are measured in the first quarter of the survey. Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS (2017–2024 excluding 2020–2021, except for the wage which is based on the 2022–2023 sample).

less favourable to green shortage occupations at higher education levels.¹⁰

However, these average wage differences may be partly due to compositional effects (OECD, 2024). After controlling for a range of individual characteristics (in a log-linear regression model), including age, region of residence, gender, educational level, sectoral dummies and occupational group dummies, we obtain a significant wage premium for green shortage jobs: 3.4% relative to green non-shortage jobs, 6.9% relative to non-green shortage jobs, and 5.3% relative to non-green non-shortage jobs (Figure II-A). Despite differences in methodology and data, these findings align with recent papers reporting a “green wage premium” of 4–7% (Vona *et al.*, 2019; EBRD, 2023; Kuai *et al.*, 2025). The green wage premium is stronger among workers with low or medium education, consistent with Vona *et al.* (2019) (Figure II-B, C, D). Our definition of green jobs differs from studies such as Popp *et al.* (2024), which documented a negative wage premium for a small group of high-skilled workers.

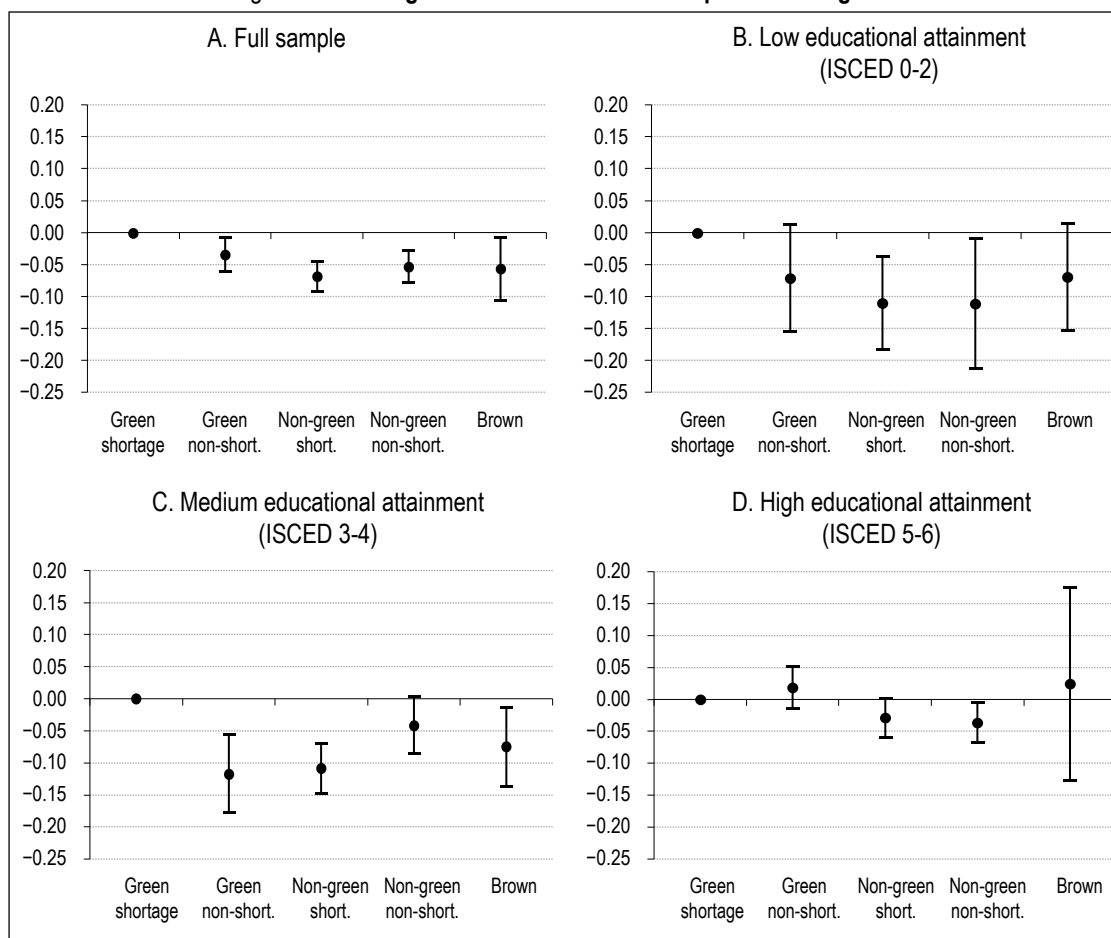
Brown jobs are concentrated in occupational categories where earnings are generally below the overall average. After controlling for individual characteristics, the earnings premium for green shortage jobs relative to brown jobs reaches 5.7%, which would indicate that, *ceteris paribus*, there remains a wage incentive to move from a brown job to a green-shortage job even within the same occupational class.

2.2. Occupational Transitions

A successful green transition depends largely on the ability of the workforce to move into green shortage jobs. In order to assess this ability, we first look at the actual transitions between job categories in the recent past. We analyse job-to-job transitions by comparing the occupation a given quarter with the one a year later, irrespective of whether they occur within the same company or sector. Changes within an occupation, such as task modifications, are not

10. We analyse monthly gross wages using available data for 2022–23 and restrict to full-time employees to ensure meaningful comparisons of wage levels. Detailed wage statistics by education attainment and occupation category are reported in the Online Appendix (Table S2-1).

Figure II – Earnings differences between occupational categories



Note: The figure reports coefficients (log points) and 95% confidence intervals from a regression of the logarithm of gross monthly wages on job-category dummy variables (baseline: "green shortage"). The job categories dummies are mutually exclusive (meaning that Non-green shortage and Non-green non-shortage categories exclude the Brown category). Additional controls include age categories, gender dummy, education categories (Figure II-A), tenure with current employer, regional dummies, fulltime and permanent contract dummies, an indicator for being born in Belgium, sectoral dummies (5 categories) and occupation classes dummies (ISCO 1-digit level).
Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS 2022–2023.

considered a job-to-job transition. For instance, an architect shifting from working on industrial projects to residential projects does not change occupation according to our definition. The shares of workers transitioning between the four different occupational categories over a one-year period are presented in Table 3-A. It shows that most workers remain within the same category, with 90% not even changing occupation.

Transitions to green shortage jobs are less common than transitions to green non-shortage jobs, even though there are a similar number of such jobs. Workers with a non-green shortage or non-green non-shortage job have a 1.1% and 2.0% probability, respectively, to transition to a green non-shortage job, but only a 0.8% and 1.1% probability, respectively, to move to a green shortage job. Those with a green non-shortage job have a 1.9% probability of making this transition.

Some recurrent pathways can be observed among workers moving into green shortage jobs. Examples include taxi drivers becoming lorry, bus or tram drivers, secretaries moving into managerial roles or specialised positions as industry or transport clerks, and shop salespersons becoming technicians in industry or construction. Other common cases include shifts between different engineering occupations or between managerial positions that vary in their green or shortage status. Those pathways only account for one-sixth of transitions into green shortage jobs, highlighting the diversity of occupational mobility.

Among workers who change occupation from one year to the next, the tendency to stay within green or within non-green occupations is strong (Table 3-B); only around 30% of non-green workers move to a green occupation, and shortage occupations are underrepresented. Moreover, green-to-green transitions are more

Table 3 – Transitions between occupational categories

A. All workers (113,409 observations)

		Occupational category one year later				
		Green shortage	Green non-shortage	Non-green shortage	Non-green non-shortage	Total
Occupational category in reference quarter	Green shortage (%)	94.6	2.2	1.4	1.7	100
	Green non-shortage (%)	1.9	93.5	1.8	2.8	100
	Non-green shortage (%)	0.8	1.1	95.7	2.3	100
	Non-green non-shortage (%)	1.1	2.0	2.7	94.2	100

B. Only workers who change occupation (11,137 observations)

		Occupational category one year later				
		Green shortage	Green non-shortage	Non-green shortage	Non-green non-shortage	Total
Occupational category in reference quarter	Green shortage (%)	34.9	27.0	16.9	21.2	100
	Green non-shortage (%)	19.2	34.6	18.4	27.8	100
	Non-green shortage (%)	10.7	14.5	43.7	31.1	100
	Non-green non-shortage (%)	11.9	20.8	29.0	38.2	100

Note: Panel A includes only workers who were employed both in the reference quarter and one year later, whereas Panel B includes only workers who changed occupation between the reference quarter and one year later.

Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS (2017–2024 excluding 2020–2021).

likely across a wide variety of demographic groups (gender, education, region and age groups). Note that green-to-green transitions also can help achieve a low-emission economy since switching to a green shortage occupation can relieve short-term constraints linked to shortages. However, because demand is expected to increase for many green jobs, reallocating workers within the green economy offers only a partial solution. Green-to-green transitions may reduce shortages in some occupations while creating labour needs in others.

By contrast, a relatively large share of the workers in brown jobs transition into green shortage jobs: when changing occupations, 37% move into a green shortage job, against 23% into a green non-shortage job and only 15% into another brown job. These findings however concern a small share of the workforce (2.1%), which suggests that counting on transitions from brown jobs alone cannot meet the growing demand for green jobs. Nonetheless, these findings show that green shortage jobs may offer a viable pathway for career change for these workers under pressure because of the transition.

2.3. Skill Distance

Compared to the three other occupational categories, green shortage jobs exhibit a higher

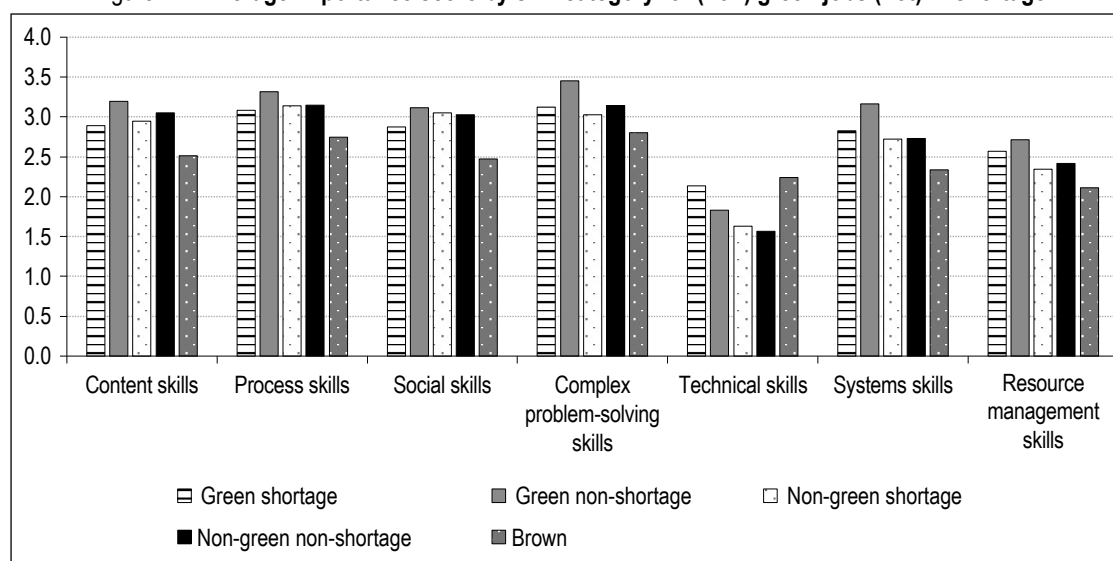
importance score on technical skills (Figure III), consistent with Consoli *et al.* (2016), who emphasise the important role of manual and technical occupations in the green transition. From a policy perspective, it is important to note that brown jobs show a similar pattern, albeit driven by different subskills (e.g., “repairing” rather than “technological design” skills). Both job categories score lower on content, process, and social skills relative to other occupations. These results should be interpreted with caution, as educational attainment influences both workers’ skill profiles and their representation across occupational categories, making it difficult to disentangle the two effects.

2.3.1. Skill Distance of Potential Transitions

We now propose a measure of skill mismatch between the current working population and green shortage occupations. For each worker, the measure is based on the ten “closest” green shortage occupations, i.e. the ten occupations with the lowest skill distance, from which we calculate the average.¹¹ The green shortage occupations that are most accessible to the current workforce not yet in green shortage occupations can be found in all three subcategories of green

11. Choosing one or five of the closest jobs instead of ten as the threshold does not alter the results

Figure III – Average importance score by skill category for (non) green jobs (not) in shortage



Note: The score varies from 1 = Not important, to 5 = Extremely important.

Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS (2017–2024 excluding 2020–2021).

occupations. On average, out of the 10 closest green shortage occupations, 6.5 are classified as “green increased demand”, 5 as “green enhanced skills” and 3.1 as “green new and emerging” occupations, reflecting differences in their shares in the current workforce.¹²

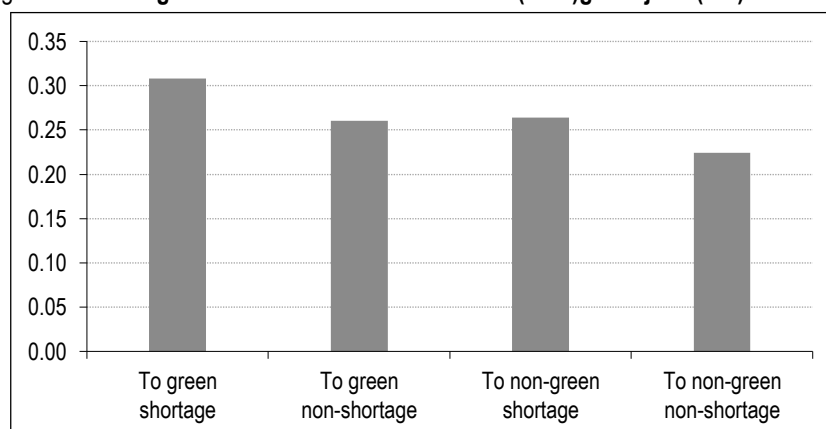
On average, for workers not already in a green shortage job, the skill distance to the ten closest green shortage jobs is 0.31 (Figure IV). Applying the same analysis to green non-shortage, non-green shortage, non-green non-shortage occupations, we find that skill distance is larger for jobs in shortage, regardless of whether they are green or not. Imbalances between

the supply and demand of skills, though not the only cause of recruitment difficulties for a company, are potentially a key driver of the increase in the number of hard-to-fill vacancies (Cedefop, 2024).

Finally, the skill distance is the largest for green jobs in shortage, and the lowest for non-green jobs not in shortage (0.22). All differences between categories are significant. The skill distance to categories other than green shortage jobs and non-green jobs not in shortage is 0.26.

12. This sums up to more than 10 because after mapping O*NET occupations into ISCO occupations, a given ISCO occupation may be associated with more than one green subcategory.

Figure IV – Average skill distance to the ten closest (non-)green jobs (not) in shortage



Reading note: The graph shows the average skill distance with respect to the ten closest (non-)green (non-)shortage jobs, averaged over all workers not already in a (non-)green (non-)shortage job. Confidence intervals are not shown because they are small (e.g. a standard error of the order of 0.0005).

Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS (2017–2024 excluding 2020–2021).

This would be compatible with about a third of skills having a skill distance of one point (for example the difference between “important” and “very important”) for green shortage jobs, against about a fourth for green non-shortage jobs, the importance of the other skills being the same.

To assess the robustness of these results, we build a second measure of the skill distance between the working population and (non-) green jobs (not) in shortage.¹³ For each worker, we calculate the share of (non-) green (not in) shortage occupations within a limited skill distance, defined as those with a distance lower than the median skill distance across all observed transitions. We then average these shares within each occupational category. The main finding—that the skill distance of the workforce to green shortage occupations is the largest—holds true with this second measure.

A limited skill distance does not necessarily imply that a transition is easy because additional, external factors could be influential (e.g. accredited degrees) or because one missing skill is critical. For example, the skill distance between an ICT installer and an electrical engineering technician is low as they have similar requirements for technical skills. However, they differ in their mathematics and science skill requirements. A transition would thus require either existing skills in mathematics and science or a substantial amount of training to acquire these skills.

Workers in brown occupations appear to be better positioned, from a skills perspective, to move into green shortage occupations. The skill distance of brown occupation workers to green shortage occupations is substantially smaller than the distance of the rest of the workforce to green shortage occupations (Table 4). Most brown jobs, characterized by their prevalence in industry, share significant skill overlap with many green shortage occupations. For instance, machine operators in food manufacturing or metal processing—classified as brown due to

their sector’s high emissions—possess technical competencies closely aligned with green shortage jobs in construction and industry, such as building trades workers, plumbing technicians, or manufacturing supervisors. This proximity is marked in technical skills like equipment maintenance and selection.

2.3.2. Skill Distance of Observed Transitions

The skill distance of the current Belgian working population to green shortage occupations tends to be larger than the distance to the other categories of occupations. But is skill distance for *observed* annual occupational transitions into green shortage occupations larger as well? To answer this question, we analyse the skill distance between the initial job and the subsequent job for transitions that took place between 2017 and 2024, using the same measure of skill distance.

The observed job-to-job transitions towards green shortage jobs are characterised by a lower skill distance between the initial and subsequent jobs. These transitions have an average distance of 0.40, while for transitions to other job categories, this ranges from 0.41 to 0.45 (Figure V).¹⁴ The distinction between occupations in shortage and those not in shortage reveals differences in transition patterns that are not apparent when considering green occupations and non-green occupations as respectively single groups.

When breaking down skill distance by skills category, all show a lower score in transitions to green jobs in shortage except for technical skills. Average distances in terms of technical skills are similar for transitions into the different job categories, suggesting that technical skills are proportionally more important for moving to a green job in shortage.

13. See the Online Appendix (Figures S2-II and S2-III).

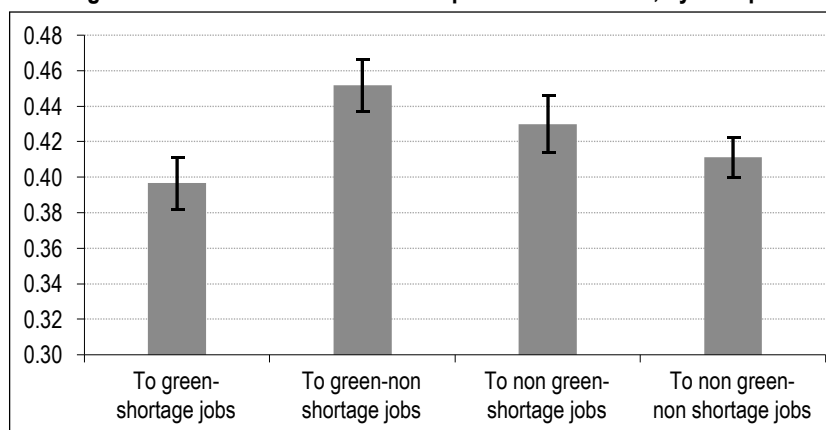
14. These are simple (unconditional) averages. However, correcting for demographic differences between groups would only affect the results at the margin. For the green shortage job category, it would decrease from 0.393 to 0.390.

Table 4 – Average skill distance to the closest green shortage occupations by initial occupational category

	To the 10 closest green shortage occupations
From a green non-shortage occupation	0.27 (0.0006)
From a non-green shortage occupation	0.31 (0.0004)
From a non-green non shortage occupation	0.32 (0.0004)
From a brown occupation	0.18 (0.0008)
All (from any occupation other than green shortage)	0.31 (0.0003)

Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS (2017–2024 excluding 2020–2021). Standard errors (clustered at the individual level) in brackets.

Figure V – Average skill distance for observed occupational transitions, by occupational category



Note: The graph shows that the average skill distance between the initial and destination occupations is 0.40 for workers who leave an occupation that isn't both green and in shortage for a green shortage one. Data are weighted, and 95% confidence intervals are added.

Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS (2017–2024 excluding 2020–2021).

Our results suggest that the shift to a decarbonised economy would require a substantial labour reallocation and the development of specific skills,¹⁵ as skill distances for potential transitions are larger for green shortage jobs. However, the opposite prevails in descriptive statistics related to observed transitions. The difference between potential and observed transitions is compatible with a tendency to favour transitions towards green shortage jobs that can be easily completed at the expense of those requiring the acquisition of entirely new skills or a longer process of acquiring skills. A possible explanation for this is the higher cost, in terms of training, associated with acquiring skills required for green shortage jobs, and working conditions.¹⁶ On the one hand, these jobs tend to require more practical experience and longer on-the-job training. On the other hand, they often involve more arduous working conditions, such as greater exposure to contaminants or hazardous environments, which could discourage training even if it leads to a higher wage. Whatever the explanation of this puzzle, it implies that from a labour market perspective, the green transition could be slowed down by both qualitative and quantitative elements.

3. Transitions to Green Shortage Occupations and the Importance of Skill Distance

3.1. Transitions to Green Shortage Occupations

Various factors may influence the likelihood of occupational mobility, and we use a regression framework to identify factors driving workers into green shortage occupations. To do so, we

focus on changes of occupational categories. We run the following regression:

$$\text{Change_to_green_shortage_occ} = \alpha + \beta \text{skills} + \gamma X + \epsilon,$$

where *Change to green shortage occ* equals 1 if the person moved to a green shortage occupation from another occupational category, and 0 if she remained in another occupational category. The explanatory variables include the skills required by the initial occupation (*skills*) and a set of control variables *X* including age, education, gender, region of residence, nationality, household size, whether the contract is permanent, whether the contract is full-time, professional status, whether the worker undertook training in the four weeks preceding the survey, the industry as per five broad groups (primary/utilities, manufacturing, construction, business services, and non-market services) and year fixed effects. These variables describe the situation in the initial quarter and are thus observed prior to the transition. Overall, these results should be taken as associational rather than causal, albeit conditional on a rich set of control variables.

We run the same type of regression for transitions into other categories of occupations (green non-shortage, non-green shortage, and non-green non-shortage) with the dependent variable equalling 1 if the worker from another category of occupation transitions to the occupation category (Table 5). The population of interest is all workers not already having this

15. On the sectoral dimension job-to-job transitions to green shortage jobs do involve bigger changes. About 32% of transitions to a green shortage job involve sectoral changes, compared to 27% for green jobs that are not in shortage and 27% for non-green jobs.

16. See Table S2-7 in the Online Appendix.

category of occupation in the initial quarter. We also split the same regression for the transition to green shortage occupations by level of education (Table 6). Finally, few people transition to a new kind of job and for this reason, as recommended by Cameron & Trivedi (2005), we use a complementary log-log regression (Cloglog) to address the imbalance in the number of 0- and 1-outcomes.¹⁷

Results regarding demographic characteristics are as expected. There are no significant differences in the probability to switch to green shortage occupations with the level of education. However, focusing on the transitions to green shortages in physically demanding jobs or on the transitions to those with higher work pressure, lower educated workers become significantly more likely to switch to each of them than more educated ones (see the analysis by type of green shortage occupations in the Online Appendix S3 – link at the end of the article). Low- and medium-educated workers have proportionally more often a job in shortage, whether green or non-green, and they seem to transition more to such jobs especially non-green ones. Transitions to green non-shortage jobs are more likely to involve highly educated workers. Furthermore, we observe that some categories of skills (content skills, technical skills and resource management skills) are strongly associated with transitioning to green shortage jobs, though the strength of the associations may vary with the level of education.

Men are overrepresented in green occupations and are also more likely to transition to them, whether these jobs are in shortage or not. Younger workers are not overrepresented in green shortage jobs, but they are more likely to transition to them. This is due to the fact that early-career job changes are more common in general. When we run the same regression on the sub-sample of workers who change occupation, young workers are not more likely to transition to green shortage jobs. Nonetheless, the higher tendency of young people to switch occupations makes them a significant reservoir of candidates to fill green shortage jobs. Workers with temporary contracts and wage earners also switch jobs more often, but these factors have little impact on transitions to green shortage jobs.¹⁸

Technical skills seem important to filling green shortage jobs. One-point higher technical skills on the five-point Likert scale are associated with a 0.6 percentage point higher probability of switching to a green shortage job, which is a 46% higher probability than the average one

(1.3%). In contrast, it is not the case for green non-shortage jobs. Technical skills are known to be important for green jobs (Vona *et al.*, 2015) and manual occupations are overrepresented in green jobs (Consoli *et al.*, 2016).¹⁹ Looking at concrete examples of destination occupations, for low-educated workers, having sufficient technical skills increases the likelihood of transitioning to carpenter, electrician, or logistics-related occupations whereas for highly educated workers it increases the likelihood of transitioning into occupations such as industrial and production engineers, construction supervisors or resource management positions.

Higher resource management skills also appear to lead to higher transitions to green shortage jobs, and mostly so for high educational level workers. Consistent with this result, destination occupations commonly observed among these transitions include roles in generic managerial positions²⁰ (e.g. trade manager or managing director) and occupations in the construction sector (e.g. building architect or construction manager).

Content skills encompass a broad range of foundational skills for acquiring domain-specific expertise—reading, speaking, mathematics, and science. The higher these skills are, the greater the likelihood to transition into a green shortage job. This effect is the largest for high-educated workers.

People in jobs requiring systems skills—i.e., capacities to understand, monitor, and improve the functioning of complex organisations and processes—have a lower significant negative probability to transition to green shortage jobs. These skills, which are important for decision-making and systems evaluation, are more common in roles such as human resource manager or judge and are less relevant to green jobs.

17. Probit regressions produce almost identical results (results available upon request).

18. Focusing on workers who experienced occupational mobility helps account for the higher propensity of certain categories of workers to switch jobs. The probability of transitioning to a green shortage job is the lowest: 14.3% of transitions from non-green and/or non-shortage jobs lead to green shortage jobs, compared to 19.5% to 29.3% for other transition categories (see Table S2-6 in Online Appendix).

19. Additional regressions based on the 35 O*NET skill subcategories (available upon request) indicate that repairing, troubleshooting and equipment maintenance are the most beneficial technical sub-skills for transitioning to green shortage jobs, while among highly educated workers, technology design and operations analysis also play an important role. Those regressions are similar to those in Table 5, containing all 35 sub-categories of skills instead of the seven large categories of skills.

20. Generic managerial occupations appear in the green classification because some of the underlying O*NET occupations are associated with environmental or sustainability-related activities and are aggregated into broader managerial categories in the ISCO classification.

Table 5 – Probability of transitioning to a (non-)green (non-)shortage occupation one year later

	To a green shortage occupation		To a green non-shor- tage occupation		To a non-green shor- tage occupation		To a non-green non- shortage occupation	
Level of education (ref.: high):								
Low	0.002	(0.002)	−0.007*** (0.002)		0.012** (0.005)		−0.004	(0.004)
Medium	0.002	(0.001)	−0.010*** (0.002)		0.003 (0.003)		−0.002	(0.003)
Content skills	0.011*** (0.004)		0.008* (0.005)		−0.021*** (0.007)		0.010	(0.006)
Process skills	−0.003 (0.005)		−0.009 (0.008)		0.005 (0.009)		−0.011	(0.009)
Social skills	−0.002 (0.003)		−0.004 (0.004)		0.003 (0.007)		0.002	(0.005)
Complex problem-solving skills	−0.002 (0.004)		0.001 (0.005)		0.000 (0.007)		−0.006	(0.007)
Technical skills	0.006*** (0.002)		−0.001 (0.002)		−0.013*** (0.004)		−0.009**	(0.004)
Systems skills	−0.010** (0.004)		0.001 (0.006)		0.007 (0.007)		0.007	(0.007)
Resource management skills	0.010*** (0.002)		0.011*** (0.003)		0.002 (0.004)		0.005	(0.004)
Age category (ref.: 18–29)								
30–49	−0.004*** (0.002)		−0.005* (0.002)		−0.019*** (0.004)		−0.016***	(0.003)
50–64	−0.009*** (0.002)		−0.013*** (0.002)		−0.027*** (0.004)		−0.023***	(0.003)
Male	0.012*** (0.001)		0.006*** (0.002)		−0.007*** (0.002)		−0.007***	(0.002)
Household size (ref.: 1)								
2	0.000 (0.002)		0.000 (0.002)		−0.003 (0.003)		−0.005	(0.003)
3	0.000 (0.002)		−0.001 (0.002)		−0.002 (0.003)		−0.006*	(0.003)
4 or more	0.000 (0.002)		−0.003 (0.002)		0.001 (0.003)		−0.006**	(0.003)
Nationality (ref.: Belgian)								
EU non-Belgian	0.001 (0.002)		−0.001 (0.003)		0.001 (0.004)		−0.002	(0.004)
Non-EU	0.003* (0.004)		0.005 (0.005)		0.008 (0.006)		−0.008*	(0.004)
Region (ref.: Wallonia)								
Brussels	−0.006*** (0.001)		0.001 (0.002)		0.002 (0.003)		0.008**	(0.004)
Flanders	0.003** (0.001)		0.002 (0.002)		0.009*** (0.002)		0.002	(0.002)
Permanent contract	−0.006** (0.002)		−0.003 (0.003)		−0.017*** (0.004)		−0.014***	(0.004)
Working full-time	−0.001 (0.002)		0.007** (0.003)		−0.007** (0.003)		−0.005*	(0.003)
Professional status (ref.: employee)								
Civil servant	−0.004 (0.002)		−0.001 (0.003)		−0.009*** (0.003)		0.001	(0.003)
Self-employed	−0.009*** (0.001)		−0.007*** (0.002)		−0.015*** (0.003)		−0.018***	(0.003)
Other	−0.002 (0.006)		0.009 (0.011)		−0.004 (0.009)		0.014	(0.017)
Training in the past four weeks	0.005* (0.002)		0.004* (0.002)		0.010*** (0.003)		0.011***	(0.003)
Initial sector of activity (ref.: non-market services)								
Primary & utilities	0.007*** (0.004)		0.013** (0.005)		−0.006 (0.007)		0.005	(0.006)
Manufacturing	0.017*** (0.003)		0.018*** (0.004)		−0.009*** (0.003)		0.008**	(0.004)
Construction	0.009*** (0.003)		0.005 (0.003)		−0.008** (0.004)		−0.005	(0.004)
Business services	0.009*** (0.001)		0.012*** (0.002)		0.003 (0.003)		0.010***	(0.003)
Average probability of transitioning to this job type (%)	1.3		1.8		2.4		2.6	
Year dummies	Yes		Yes		Yes		Yes	
Observations	96,003		96,952		75,703		79,639	

Note: The sample includes people aged 18–64 in employment, excluding those already employed in that job category. Job-related characteristics (sector, type of contract, skills, training, professional status) and individual characteristics (household size, region of residence, age) are based on the job and the situation held before the transition. Average marginal effects based on a complementary log-log regression, with standard errors (clustered at the individual level) in bracket. Sample weights are used *** p<.01, ** p<.05, * p<.1.

Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS (2017–2024 excluding 2020–2021).

Table 6 – Probability of transitioning to a green shortage occupation one year later – by level of education

	All levels of education	Low-educated	Medium-educated	High-educated
Level of education (ref.: high):				
Low	0.002 (0.002)			
Medium	0.002 (0.001)			
Content skills	0.011** (0.004)	0.006 (0.021)	0.005 (0.008)	0.011** (0.005)
Process skills	-0.003 (0.005)	-0.022 (0.018)	0.004 (0.011)	0.002 (0.007)
Social skills	-0.002 (0.003)	0.034*** (0.012)	0.000 (0.006)	-0.008** (0.004)
Complex problem-solving skills	-0.002 (0.004)	-0.006 (0.022)	-0.002 (0.007)	-0.002 (0.005)
Technical skills	0.006*** (0.002)	0.018** (0.009)	0.006 (0.004)	0.003* (0.002)
Systems skills	-0.010** (0.004)	0.019 (0.014)	-0.014* (0.008)	-0.010* (0.005)
Resource management skills	0.010*** (0.002)	-0.015* (0.009)	0.008 (0.005)	0.012*** (0.003)
Probability of transitioning to green shortage job (%)	1.3	1.7	1.6	1.0
Demographic and professional controls (as in Table 5)	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Observations	96,003	11,125	33,573	51,305

Note: The sample contains all people aged 18–64 in employment, excluding workers already employed in that job category. Job-related characteristics (sector, type of contract, skills, training, professional status) and individual characteristics (household size, region of residence, age) are based on the job and the situation held before the transition. Average marginal effects based on a complementary log-log regression, with standard errors (clustered at the individual level) in brackets. Full regression results are provided in the Online Appendix (Table S2-2). Sample weights are used. *** p<0.01, ** p<0.05, * p<0.1.

Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS (2017–2024 excluding 2020–2021).

Having attended a training course in the past four weeks is significantly associated with a transition to all job categories, although the coefficient is smallest for transitions to green jobs. However, we should be careful in interpreting these findings, not least because of potential reverse causality.

Workers in non-market services are least likely to transition to green shortage jobs, which is not surprising given the low share of green jobs within this sector. Workers in business services are significantly more likely to make such transitions, although the strongest associations are found for workers initially employed in manufacturing and construction. As most Belgian workers are employed in services, while many green shortage jobs are found in non-service sectors, these transitions nevertheless account for a significant proportion of new entries into green shortage jobs.

Wages are a variable omitted from the baseline specification due to numerous missing values. Its inclusion would reduce the sample by 18% and risk introducing sectoral selection bias. That being said, on the sub-sample selected this way, wages do not correlate significantly with transitions to green shortage jobs nor alter the other coefficients in the regression.²¹

3.2. Skill Distance With Respect to Green Shortage Jobs

Occupational mobility decisions are influenced by the nature of the change and the costs of a transition, notably with respect to skills acquisition. Transitions to jobs requiring similar skills are generally easier and quicker, whereas those involving different skills are more challenging. To test whether skill distance hinders transitions to green shortage jobs, we add our skill distance variables to the regression on transitions to (non-)green jobs (not) in shortage (Table 7). Unsurprisingly, transitions to green shortage jobs are more likely for workers for whom the skill distance to these jobs is lower. For example, a skill distance to the closest green shortage jobs of less than half a point (on the 4-point scale) is associated with a 1.85 percentage points higher probability to transition to such an occupation. This would more than double the baseline probability of such a transition, which is 1.3%.²²

Including the skill distance variables has a minimal impact on the magnitude and significance

21. Results including wage-quartile variables and varying sets of control variables are given in Table S2-3 of the Online Appendix.

22. This result also holds for the other job categories and is robust to using our second measure on skill distance (the share of jobs for which skill distances are limited – see Table S2-5 in the Online Appendix).

Table 7 – Probability of transitioning to a (non-)green occupation (not) in shortage one year later – regression with additional explanatory variables on skill distances

	To a green shortage job	To a green non-shortage job	To a non-green shortage job	To a non-green non-shortage job
Average skill distance to ten closest green shortage jobs	-0.037* (0.019)	-0.003 (0.025)	0.088*** (0.028)	-0.071** (0.030)
Average skill distance to ten closest green non-shortage jobs	-0.032 (0.021)	-0.060** (0.028)	0.127*** (0.039)	-0.055* (0.030)
Average skill distance to ten closest non-green shortage jobs	0.003 (0.023)	0.003 (0.025)	-0.188*** (0.033)	0.165*** (0.031)
Average skill distance to ten closest non-green non-shortage jobs	0.065** (0.030)	0.054* (0.033)	-0.011 (0.043)	-0.051 (0.040)
Baseline probability of transitioning to this job type (%)	1.3	1.8	2.4	2.6
Demographic and professional controls (as in Table 5)	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Observations	96,003	96,952	75,703	79,639

Note: See Table 5.

Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS (2017–2024 excluding 2020–2021).

of most other explanatory variables—such as skills, age, and sector of activity—suggesting that these factors are associated with transitions into green shortage occupations even among individuals with comparable skill distance to these jobs.²³ Technical skills are a notable exception, their coefficient becomes small and insignificant once skill distance is controlled for. This indicates that low technical skills often translate into a greater skill distance from green shortage jobs, but conditional on a given skill distance, technical skills do not create a greater barrier to transition than other skill categories. Nevertheless, because low technical skills contribute substantially to higher skill distance, technical competencies remain an important factor for transitions into green shortage occupations.

4. Discussion

Our analysis has focused on the role of skills in transitions to green shortage jobs. While we control for many factors, unobserved contextual, individual or job-related characteristics may still be influential. This section discusses additional factors that may help explain transitions into green shortage occupations.

Firstly, unobserved individual factors may influence the decision to transition to a green shortage occupation. For instance, individual awareness of environmental issues, or job-related stereotypes could potentially influence such decisions. Unfortunately, the LFS does

not measure respondents' perceptions of these factors. The financial dimension may also be a factor of mobility, and it is difficult to measure. Although we have introduced wages as a control variable in some specifications, tackling this subject is complex since financial wealth and wage expectations can have a layered influence.

Secondly, job-related factors—other than skills requirements—may influence occupational mobility. Labour shortages differ markedly across firms (Groiss & Sondermann, 2025), while the green transition may drive substantial labour reallocation towards green jobs (Vandeplas *et al.*, 2022). In this context, job characteristics such as working conditions and experience requirements may play an important role. Our data suggests that green shortage occupations tend to differ in terms of working conditions and the professional experience they require. Even if on average, job strain is not particularly higher for green jobs, certain growing green sectors—especially waste disposal and recycling—offer comparatively low pay, limited job security (OECD, 2024), and higher health risks (Conseil Supérieur de l'Emploi, 2025) which reduce their attractiveness to workers, even when they demand little or no training. The category “green increasing demand”, which is found to be relatively more affected by labour shortages in our data, tend to be more exposed to arduous

23. The complete set of regression coefficients is presented in Table S2-4 of the Online Appendix.

working conditions (Eurofound, 2022). A causal relationship between these factors and occupational mobility is, however, difficult to establish, as the absence of a transition may reflect either limited opportunities to move or a voluntary decision to remain in the current occupation. Our data shows that green shortage occupations tend to require longer on-the-job training and are associated with greater exposure to hazardous conditions or contaminants (see Table S2-7 in the Online Appendix). In general, green occupations require more technical skills and longer on-the-job training. From a policy standpoint, this suggests that favouring on-the-job training on specific equipment could help to address the shortage of green jobs.

Thirdly, the inactive and unemployed constitute a large potential pool of workers who could potentially take up green shortage jobs and indeed represent almost half of those who start such jobs. Further empirical analysis based on the Belgian LFS focusing on jobless persons who had held an occupation in recent years reveals that their skills generally do not match with those required in green shortage occupation, and in particular that many of them lack the necessary technical skills (see Online Appendix S4).

Finally, several sources of uncertainty may affect workers' mobility. Uncertainties about the evolution of the economy and society, the political environment, and the pace of development of green activities may influence both the demand for green jobs and workers' propensity to move into them. They also include macroeconomic uncertainties relating to future developments in the labour force and the working-age population. According to Cedefop (2023) projections, employment in Belgium is expected to increase by 8.3% between 2022 and 2035, with one-third of this increase in green jobs. The working-age population is expected to grow by 1.2% over the same period (Federal Planning Bureau, 2025). Given these projections, meeting future labour demand, including for green occupations, may require both an increase in the labour force participation and a reskilling of part of the inactive population.

* *
*

What are the characteristics of the individuals who transition into green shortage occupations and do skills play a role in this process?

Answering these questions is of importance from both policy and research perspectives, as reallocating labour towards green jobs is a key prerequisite for the transition to a decarbonised economy. The demand for green skills is expected to rise as the reduction of emissions increasingly becomes a priority. However, labour supply is rather rigid in the short term, and the green transition is likely to encounter challenges related to occupational shortages and skills mismatch.

Our article shows that green shortage occupations are a distinct category; both as regards their composition and the transitions leading to them. In Belgium, all other things being equal, green shortage jobs tend to be slightly better paid, other characteristics being equal and are more likely to be occupied by individuals with low or medium levels of education than other categories of occupations. The “green increasing demand” sub-category of green occupations, covering jobs whose employment grows without major task changes—such as electricians or train drivers—, is more affected by shortages. Transitions into green shortage jobs depend crucially on skill categories, with technical and resource management skills playing a central role. We find that skills mismatches for *potential* transitions to green shortage jobs are large, except notably for workers in a brown job. Yet *observed* transitions into green shortage jobs tend to occur between occupations with relatively similar skill profiles, suggesting that workers favour transitions requiring limited changes in skill requirements.

As the green transition ranks high among policy objectives of EU, training programmes should prioritise green shortage occupations. Recent evidence from Frick *et al.* (2025) shows that, for jobseekers, training largely operates through transitions into occupations that are more distant in terms of required skills, suggesting a strong potential for labour reallocation; fostering training, particularly for the acquisition of technical skills, could therefore significantly support the development of green activities. On-the-job training could also be an appropriate stepping stone to facilitate the workforce reallocation towards green jobs.

However, it is important to acknowledge some limitations in our analysis. The definition of green shortage jobs and the use of a crosswalk to merge international classifications of occupations and skill categories with survey data introduce some uncertainty. Developing occupation classifications tailored to European labour

markets and, for future cross-country analyses, a harmonised definition of occupational shortages could help strengthen this research. A second limitation is that the relationships identified between skills possession and labour transitions do not necessarily reflect causal relationships.

Data enabling the identification of the reasons why workers do or do not transition into green shortage occupations are still lacking and the use of experimental approaches or ad-hoc surveys capable of uncovering these determinants could be an avenue for future research. □

Link to the Online Appendix:

www.insee.fr/en/statistiques/fichier/9066706/ES550_Barslund-et-al_Online-Appendix.pdf

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APPENDIX 1

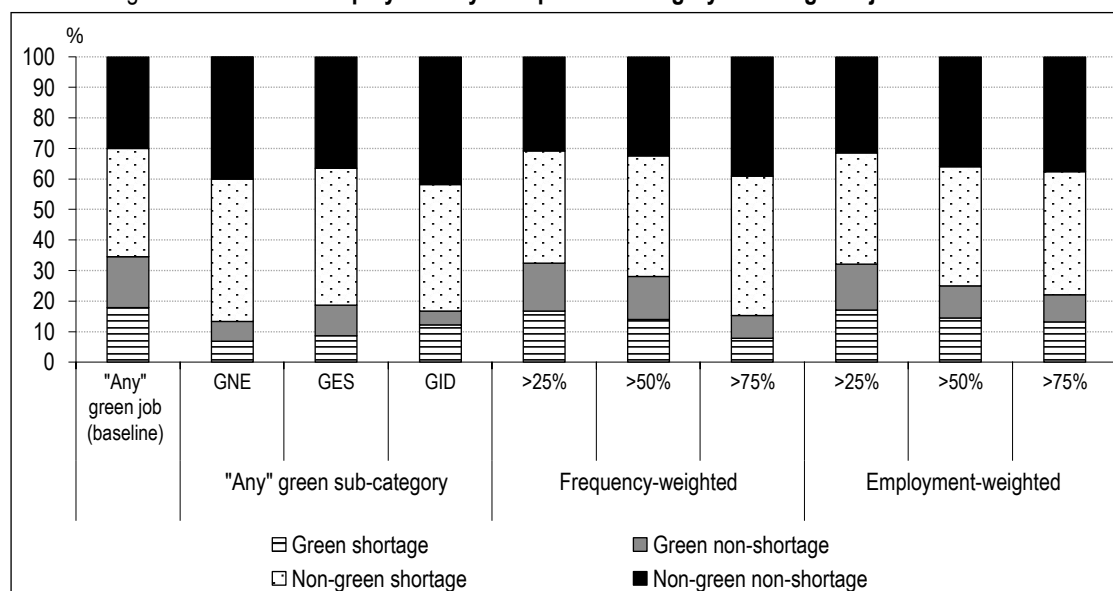
GREEN JOB CLASSIFICATION

Crosswalking measures of green jobs from the SOC classification to the ISCO 4-digit level requires several choices regarding weighting and aggregation. We start from the binary O*NET classification, which labels occupations as green or non-green at the 8-digit SOC level and use the same classification over the entire period of our analysis (2017–2024). Thereafter, our two-step procedure first aggregates the 8-digit SOC classification into the 6-digit SOC and then maps it to ISCO 4-digit codes.

For the first step, as Vona *et al.* (2019) note, “most of the green skill variation is at the 6-digit level,” while employment data at the 8-digit level are unavailable. We therefore assume a uniform distribution of occupations across the 8-digit SOC codes belonging to the same 6-digit SOC code (as in Bowen *et al.*, 2018). In our baseline specification, a 6-digit SOC code is classified as green whenever at least one of its associated 8-digit SOC subcategories is identified as green. In the second step, we use the BLS crosswalk matrix to map occupations from the SOC classification into the ISCO classification. At this stage, we consider three approaches, inspired in part by Scholl *et al.* (2023): (i) a binary indicator, whereby an ISCO occupation is classified as green if at least one of its associated SOC subcomponent is green; (ii) a continuous indicator measuring the share of green components, assuming a uniform distribution across SOC subcomponents (frequency-weighted); and (iii) a continuous indicator measuring the share of green components based on an employment-weighted distribution across SOC subcomponents. The latter combines BLS data for the US with LFS data for Belgium (following Dingel & Neiman, 2020) to weight SOC-ISCO correspondences by their relative employment importance and reduce biases stemming from multiple mappings between the two classifications. Our baseline measure is based on the first approach. To test its robustness, we replicate the exercise by consecutively considering the two other approaches and also defining green jobs by the subcategories of green jobs—new and emerging, enhanced skills, and increasing demand. As our analysis requires a categorical variable, different thresholds are applied to the continuous indicators to classify occupations as green or non-green.

As for the classification of occupations into green and non-green jobs, using a 25% threshold yields results similar to those obtained with the baseline definition, indicating that cases where a 6-digit SOC occupation contains only a small proportion of green components are relatively rare (Figure A1). The risk of classifying an ISCO code mostly composed of non-green occupations as a green occupation is thus small. The share of shortage jobs among green jobs remains comparable to that among non-green jobs, confirming robustness of this result. That being said notable differences appear across green subcategories, with the increasing demand group exhibiting a high incidence of shortage jobs. Our baseline definition is relatively broad, classifying 34% of employment as green. Nevertheless, even the most restrictive definition (frequency-weighted index greater than 75%, resulting in 15% of employment being classified as green) shows a similar pattern, with the share of shortage jobs among green jobs slightly above 50%.

Figure A1 – Share of employment by occupational category – other green jobs definitions



Note: GNE = green new and emerging, GES = green enhanced skills, GID = green increasing demand.

Source: Authors' own calculations based on shortage occupation lists, O*NET classifications and Belgian LFS (2017–2024 excluding 2020–2021).

Table A1-1 replicates column 1 of Table 5 with different definitions of green occupations. Regressions using a stricter definition of green jobs produce highly consistent results in both magnitude and significance, particularly for skills. As expected, the probability of transitioning to green jobs decreases as the definition becomes more restrictive.

Finally, we replicate the analysis in Table 5 for transitions to the different sub-categories of green jobs (Table A1-2). It shows that transitioning to “new and emerging” green jobs shows no significant association with technical skills, and the variable for recent training loses significance, unlike in the other categories. Transitions to “Increasing demand” green jobs are strongly associated with technical skills but not with content skills and are more common among low- or medium-educated workers.

Table A1-1 – Probability of transitioning to a green shortage job – restrictive definition of green job

	Any green job	Frequency-weighted			Employment-weighted		
		>25%	>50%	>75%	>25%	>50%	>75%
Level of education (ref.: high):							
Low	0.002 (0.002)	0.003 (0.002)	0.001 (0.002)	-0.002 (0.002)	0.001 (0.002)	0.003 (0.002)	0.003 (0.002)
Medium	0.002 (0.001)	0.002 (0.001)	0.001 (0.001)	0.000 (0.001)	0.002 (0.001)	0.003** (0.001)	0.003** (0.001)
Content skills	0.011** (0.004)	0.010** (0.004)	0.009** (0.004)	0.010*** (0.003)	0.012*** (0.004)	0.009** (0.004)	0.008** (0.004)
Process skills	-0.003 (0.005)	-0.001 (0.006)	0.001 (0.005)	0.003 (0.004)	-0.003 (0.005)	0.001 (0.005)	0.000 (0.005)
Social skills	-0.002 (0.003)	-0.002 (0.003)	-0.002 (0.003)	-0.002 (0.003)	-0.002 (0.003)	-0.002 (0.003)	-0.003 (0.003)
Complex problem-solving skills	-0.002 (0.004)	-0.001 (0.004)	0.001 (0.004)	-0.004 (0.003)	-0.003 (0.004)	-0.003 (0.004)	-0.002 (0.004)
Technical skills	0.006*** (0.002)	0.006*** (0.002)	0.003* (0.002)	0.004*** (0.001)	0.005*** (0.002)	0.005*** (0.002)	0.004** (0.002)
Systems skills	-0.010** (0.004)	-0.011*** (0.004)	-0.010*** (0.004)	-0.005 (0.003)	-0.009** (0.004)	-0.007* (0.004)	-0.006* (0.004)
Resource management skills	0.010*** (0.002)	0.010*** (0.002)	0.008*** (0.002)	0.006*** (0.002)	0.010*** (0.002)	0.007*** (0.002)	0.007*** (0.002)
Age category (ref.: 18–29):							
30–49	-0.004** (0.002)	-0.004** (0.002)	-0.003** (0.002)	-0.001 (0.001)	-0.004** (0.002)	-0.004*** (0.002)	-0.004** (0.002)
50–64	-0.009*** (0.002)	-0.008*** (0.002)	-0.008*** (0.002)	-0.004*** (0.001)	-0.009*** (0.002)	-0.008*** (0.002)	-0.008*** (0.002)
Male	0.012*** (0.001)	0.011*** (0.001)	0.009*** (0.001)	0.005*** (0.001)	0.011*** (0.001)	0.009*** (0.001)	0.009*** (0.001)
Household size (ref.: 1)							
2	0.000 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.001)	0.001 (0.002)	-0.001 (0.002)	0.000 (0.002)
3	0.000 (0.002)	0.001 (0.002)	0.000 (0.002)	0.002 (0.001)	0.000 (0.002)	0.000 (0.002)	0.000 (0.002)
4 or more	0.000 (0.002)	0.001 (0.002)	0.000 (0.002)	0.001 (0.001)	0.000 (0.002)	0.000 (0.002)	-0.001 (0.002)
Nationality (ref.: Belgian)							
EU non-Belgian	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	-0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)
Non-EU	0.003 (0.004)	0.002 (0.004)	0.001 (0.004)	-0.002 (0.002)	0.002 (0.004)	0.002 (0.004)	0.002 (0.004)
Region (ref.: Wallonia)							
Brussels	-0.006*** (0.001)	-0.005*** (0.001)	-0.003** (0.001)	-0.001 (0.001)	-0.006*** (0.001)	-0.003*** (0.001)	-0.003** (0.001)
Flanders	0.003** (0.001)	0.003** (0.001)	0.004*** (0.001)	0.003*** (0.001)	0.003** (0.001)	0.005*** (0.001)	0.005*** (0.001)
Permanent contract	-0.006*** (0.002)	-0.006*** (0.002)	-0.006*** (0.002)	-0.003* (0.002)	-0.006*** (0.002)	-0.006*** (0.002)	-0.006*** (0.002)
Working full-time	-0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)	0.000 (0.002)	-0.001 (0.002)	0.000 (0.002)	-0.001 (0.002)

→

Table A1-1 – (contd.)

	Any green job	Frequency-weighted			Employment-weighted		
		>25%	>50%	>75%	>25%	>50%	>75%
Professional status (ref.: employee):							
Civil servant	-0.004** (0.002)	-0.005*** (0.002)	-0.005*** (0.002)	-0.001 (0.002)	-0.005** (0.002)	-0.004** (0.002)	-0.004** (0.002)
Self-employed	-0.009*** (0.001)	-0.010*** (0.001)	-0.009*** (0.001)	-0.005*** (0.001)	-0.009*** (0.001)	-0.008*** (0.001)	-0.008*** (0.001)
Other	-0.002 (0.006)	-0.002 (0.006)	0.000 (0.006)	0.005 (0.006)	-0.001 (0.006)	0.001 (0.006)	0.001 (0.006)
Training in the past four weeks	0.005*** (0.002)	0.005*** (0.002)	0.003* (0.002)	0.003** (0.001)	0.005*** (0.002)	0.005** (0.002)	0.004** (0.002)
Initial sector of activity (ref.: non-market services)							
Primary & utilities	0.007* (0.004)	0.006 (0.004)	0.008* (0.004)	0.008** (0.004)	0.007* (0.004)	0.006 (0.004)	0.006 (0.004)
Manufacturing	0.017*** (0.003)	0.017*** (0.003)	0.016*** (0.002)	0.012*** (0.002)	0.017*** (0.003)	0.015*** (0.003)	0.014*** (0.002)
Construction	0.009*** (0.003)	0.008*** (0.003)	0.008*** (0.002)	0.009*** (0.002)	0.008*** (0.003)	0.007*** (0.002)	0.006*** (0.002)
Business services	0.009*** (0.001)	0.009*** (0.001)	0.008*** (0.001)	0.007*** (0.001)	0.008*** (0.001)	0.006*** (0.001)	0.006*** (0.001)
Probability of transition to this category (%)	1.3	1.2	1.1	0.7	1.3	1.1	1.0
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	96,003	97,440	100,544	107,486	97,063	100,003	101,580

Note: Probability of transitioning to a given job type one year later for the population of all people aged 18–64 in employment, excluding workers already employed in that job type. Job-related characteristics (sector, type of contract, skills, training, professional status) and individual characteristics (household size, region of residence, age) are based on the job and the situation held before the transition. Average marginal effects based on a complementary log-log regression, with standard errors (clustered at the individual level) in bracket. Sample weights are used. *** p<.01, ** p<.05, * p<.1.

Source: Authors' own calculations based on the list of shortage occupations, O*NET and Belgian LFS (2017–2024 excluding 2020–2021).

Table A1-2 – Probability of transitioning to a green shortage job – by subcategory of green job

	Any green job		GNE		GES		GID	
Level of education (ref.: high):								
Low	0.002	(0.002)	0.000	(0.002)	−0.002	(0.002)	0.003*	(0.002)
Medium	0.002	(0.001)	−0.001	(0.001)	−0.002	(0.001)	0.003**	(0.001)
Content skills	0.011**	(0.004)	0.006**	(0.003)	0.008***	(0.003)	0.005	(0.003)
Process skills	−0.003	(0.005)	−0.005	(0.004)	−0.005	(0.004)	0.000	(0.005)
Social skills	−0.002	(0.003)	0.000	(0.002)	0.001	(0.002)	−0.005**	(0.003)
Complex problem-solving skills	−0.002	(0.004)	−0.001	(0.003)	0.000	(0.003)	0.000	(0.003)
Technical skills	0.006***	(0.002)	0.002	(0.001)	0.005**	(0.002)	0.004***	(0.002)
Systems skills	−0.010**	(0.004)	−0.003	(0.003)	−0.006**	(0.003)	−0.005	(0.003)
Resource management skills	0.010***	(0.002)	0.005***	(0.002)	0.008***	(0.002)	0.004**	(0.002)
Age category (ref.: 18–29):								
30–49	−0.004**	(0.002)	−0.002*	(0.001)	−0.002	(0.001)	−0.004***	(0.001)
50–64	−0.009***	(0.002)	−0.003**	(0.001)	−0.004***	(0.001)	−0.007***	(0.001)
Male	0.012***	(0.001)	0.005***	(0.001)	0.005***	(0.001)	0.007***	(0.001)
Household size (ref.: 1)								
2	0.000	(0.002)	0.001	(0.001)	0.002	(0.002)	−0.001	(0.002)
3	0.000	(0.002)	0.001	(0.001)	0.000	(0.002)	−0.001	(0.002)
4 or more	0.000	(0.002)	0.002	(0.001)	0.001	(0.002)	−0.002	(0.002)
Nationality (ref.: Belgian)								
EU non-Belgian	0.001	(0.002)	0.000	(0.002)	0.000	(0.002)	0.002	(0.002)
Non-EU	0.003	(0.004)	−0.002	(0.002)	0.000	(0.002)	0.004	(0.004)
Region (ref.: Wallonia)								
Brussels	−0.006***	(0.001)	−0.001	(0.001)	−0.003**	(0.001)	−0.004***	(0.001)
Flanders	0.003**	(0.001)	0.004***	(0.001)	−0.001	(0.001)	0.004***	(0.001)

→

Table A1-2 – (contd.)

	Any green job	GNE	GES	GID
Permanent contract	-0.006*** (0.002)	-0.002 (0.002)	-0.001 (0.001)	-0.005*** (0.002)
Working full-time	-0.001 (0.002)	0.002 (0.002)	-0.001 (0.002)	0.003 (0.002)
Professional status (ref.: employee):				
Civil servant	-0.004** (0.002)	-0.002 (0.002)	-0.003** (0.001)	-0.003* (0.002)
Self-employed	-0.009*** (0.001)	-0.005*** (0.001)	-0.004*** (0.001)	-0.006*** (0.001)
Other	-0.002 (0.006)	-0.001 (0.005)	0.000 (0.004)	-0.001 (0.005)
Training in the past four weeks	0.005*** (0.002)	0.001 (0.001)	0.003** (0.001)	0.003** (0.002)
Initial sector of activity (ref.: non-market services)				
Primary & utilities	0.007* (0.004)	0.006* (0.003)	0.007** (0.003)	0.003 (0.003)
Manufacturing	0.017*** (0.003)	0.009*** (0.002)	0.006*** (0.001)	0.012*** (0.002)
Construction	0.009*** (0.003)	0.005*** (0.002)	0.003*** (0.001)	0.003** (0.002)
Business services	0.009*** (0.001)	0.004*** (0.001)	0.007*** (0.001)	0.004*** (0.001)
Probability of transition to this category (%)	1.3	0.7	0.7	0.9
Year dummies	Yes	Yes	Yes	Yes
Observations	96,003	108,904	109,839	106,351

Note: Probability of transitioning to a given job type one year later for the population of all people aged 18–64 in employment, excluding workers already employed in that job type. Job-related characteristics (sector, type of contract, skills, training, professional status) and individual characteristics (household size, region of residence, age) are based on the job and the situation held before the transition. Average marginal effects based on a complementary log-log regression, with standard errors (clustered at the individual level) in bracket. Sample weights are used. *** p<.01, ** p<.05, * p<.1.

Source: Authors' own calculations based on the list of shortage occupations, O*NET and Belgian LFS (2017–2024 excluding 2020–2021).

APPENDIX 2

MOST REPRESENTED GREEN OCCUPATIONS

Table A2-1 – Most represented green occupations by employment share

Occupations (ISCO level)	Related green SOC sub-categories (O*NET)	Green category	Share of total employment (%)
Stock clerks	Shipping, Receiving, and Traffic Clerks	GES	1.3
Heavy truck and lorry drivers	Vehicle, Equipment and Systems Inspectors, (excl. Aviation) Industrial Truck and Tractor Operators	GES GID	1.3
Sales and marketing managers	Green Marketers	GNE	1.1
Managing directors and chief executives	Chief Sustainability Officers General and Operations Managers	GNE GES	1.1
Freight handlers	Recycling Coordinators Laborers and Freight, Stock, and Material Movers, Hand	GNE GID	1.1
House builders	Construction Managers	GES	0.9
Carpenters and joiners	Construction Carpenters Rough Carpenters	GID GID	0.8
Software developers	Software Developers, Systems Software	GID	0.8
Building and related electricians	Electricians Solar Photovoltaic Installers	GID GNE	0.8
Commercial sales representatives	Sales Representatives of Technical and Scientific Products Solar Sales Representatives and Assessors	GES GNE	0.7
Shop keepers	General and Operations Managers	GES	0.7
Construction supervisors	Solar Energy Installation Managers	GNE	0.7
Transport clerks	Dispatchers, Except Police, Fire, and Ambulance Freight Forwarders	GID GNE	0.7
Plumbers and pipe fitters	Pipe Fitters and Steamfitters Plumbers	GES GES	0.6
Motor vehicle mechanics and repairers	Supervisors of Mechanics, Installers, and Repairers Automotive Specialty Technicians Bus and Truck Mechanics	GID GES GES	0.6
Management and organization analysts	Logistics Engineers Logistics Analysts	GNE GNE	0.6
Agricultural and industrial machinery mechanics and repairers	Millwrights Wind Turbine Service Technicians Industrial Machinery Mechanics Supervisors of Mechanics, Installers, and Repairers	GID GNE GID GID	0.6
Policy administration professionals	Energy Auditors Sustainability Specialists	GNE GNE	0.6
Legal professionals not elsewhere classified	Arbitrators, Mediators, and Conciliators	GES	0.5
Retail and wholesale trade managers	General and Operations Managers	GES	0.5
Bus and tram drivers	Bus Drivers, Transit and Intercity	GID	0.5
Manufacturing supervisors	Supervisors of Production and Operating Workers	GID	0.5
Odd job persons	Helpers - Installation, Maintenance, and Repair Workers	GID	0.5
Building architects	Architects (excl. Landscape)	GES	0.4

Note: GID = Green Increasing Demand, GES = Green Enhanced Skills and GNE = Green New and Emerging.
Source: Belgian LFS, O*NET, 2017–2024 excluding 2020–2021.

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