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The Work-from-home Wage Premium*

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Abstract

Using administrative data from France, we document that within the same detailed occupation, industry, and commuting zone, workers who work from home earn on average 12% higher hourly wages than fully on-site workers. Approximately half of this wage premium is accounted for by observable worker characteristics (such as education, gender, and age) and firm characteristics (such as size and productivity). The remaining 6% wage premium largely reflects selection: workers who work from home after the COVID-19 pandemic already earned higher wages before the pandemic.

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1 Introduction

Since the COVID-19 pandemic, work-from-home (WFH) has become a significant feature of economic life around the world. For example, a survey by Aksoy *et al.* (2023), conducted in 34 countries, reported that one in three full-time workers engaged in hybrid or fully remote work arrangements in 2023. The widespread adoption of WFH and the recent debates over return-to-office mandates have generated substantial interest in how WFH affects productivity, inequality, and the functioning of the labor market (see e.g. the Economic Report of the President, Council of Economic Advisers, 2025).

In particular, the impact of WFH on wages is unclear. On the one hand, a large body of empirical research finds that WFH is an important amenity, and that workers are willing to accept economically significant wage discounts to be allowed to do more of it.[1] On the other hand, if WFH were to increase worker productivity, one would expect it to lead to higher wages.[2] Prior research has found that in the raw data, workers who work from home earn on average higher wages or income than workers who do not. An important part of this premium reflects occupational and educational differences, since higher-paying occupations and better-educated workers do more WFH.[3] However, some recent work suggests that a premium remains even after controlling for occupation, education, and other observable worker characteristics (Rossi-Hansberg *et al.*, 2023; Pabilonia and Vernon, 2025; Sano, 2025).

In this paper, we re-examine the link between WFH and hourly wages, using administrative data from France. We match the French Labor Force Survey to exhaustive social security records and firm registry data covering the universe of French firms. The resulting linked employer–employee dataset allows us to document several new empirical facts on WFH and wage outcomes. Our main finding is that the WFH wage premium is not accounted for by employer heterogeneity (either observed firm characteristics or firm fixed effects) but could be explained by selection on unobservable worker characteristics (such as ability, negotiation skills or bargaining power).

Our study combines three datasets. First, the French Labor Force survey asks par-

[1]See Mas and Pallais (2017); Barrero *et al.* (2021); Lewandowski *et al.* (2022); Aksoy *et al.* (2022); Maestas *et al.* (2023); Buckman *et al.* (2025); Cullen *et al.* (2025) and Harrington and Kahn (2025).

[2]The evidence on the productivity effects of WFH is mixed: while some studies report positive effects, others find negative effects in certain settings. Bloom *et al.* (2015); Choudhury *et al.* (2021); Angelici and Profeta (2024) and Aksoy *et al.* (2025) provide evidence for positive productivity effects of WFH. On the other hand, Monteiro *et al.* (2019); Battiston *et al.* (2021); Atkin *et al.* (2023); Gibbs *et al.* (2023); Emanuel and Harrington (2024) and Liu and Su (2024) report negative effects.

[3]This has been documented in Dingel and Neiman (2020); Mongey *et al.* (2021); Althoff *et al.* (2022), and Chetty *et al.* (2024).

ticipants whether and how much they work from home, and contains a large amount of additional information about individual worker characteristics (including occupation, gender, place of residence, etc.). We match this to a second dataset on the universe of firms in France, to obtain firm-level data on the worker's employer (including sales, employment and age). Finally, we also match the worker to their social security records, providing us with their employment and wage history.

For our baseline analysis, we focus on a post-pandemic sample, between the second quarter of 2022 (when all pandemic-related restrictions were lifted) and the forth quarter of 2024. Using this data, we first confirm that there is a WFH wage premium for hourly wages: in a regression that only controls for year fixed effects, the hourly wage of workers who work from home is 35.2% higher than the one of workers who do not.[4] However, as in the United States, WFH in France is more prevalent among higher-paying occupations and better-educated workers. Once we control for occupation, education and location, the WFH premium falls to 12.0%. When we add other observable worker characteristics (including gender, age, or tenure), the remaining WFH premium is 6.6%, a number that is comparable to results found in the United States in similar setups.

We then take two new steps with respect to the literature. First, as we can match workers to their employer, we are able to control for employer characteristics in our regressions. To the best of our knowledge, this has not been done before, despite evidence that firm characteristics matter for wages (Card *et al.*, 2013; Song *et al.*, 2018), and that firm policies on WFH are widely different, depending on attributes such as office location, management practices, and CEO demographics (Hansen *et al.*, 2023, Flynn *et al.*, 2024, Lamorgese *et al.*, 2023, Duchin and Sosyura, 2025). Thus, the WFH wage premium could in principle be driven by selection across firms: for example, larger and more productive firms may be more likely to offer WFH and also pay higher wages. However, when we control for a number of observable firm characteristics (including employment, age, productivity, CEO age and gender), the WFH premium falls only very slightly, to 6.4%. The premium is also roughly unchanged when we introduce firm-year fixed effects, effectively comparing two workers within the same firm. Thus, differences in firm characteristics cannot explain the WFH wage premium.

Second, we use the fact that our data allows us to retrieve workers' employment and wage history. Using this feature, we find that even when including all worker and firm-level controls, pre-pandemic hourly wages (an average of hourly wages in 2018 and 2019) are positively associated with post-pandemic WFH. That is, workers who work from home

[4] Most existing studies on the WFH wage premium only observe total wages, and are therefore unable to distinguish the contributions of hourly wages and working hours.

post-pandemic already made higher hourly wages before the pandemic. This fact explains the entire WFH wage premium: when introducing pre-pandemic hourly wages as a control variable, the premium drops to just 1.1% and becomes statistically indistinguishable from zero. Similarly, we show that workers who work from home after the pandemic also did not experience higher wage growth. Finally, when we control for another proxy of worker quality, the estimated worker fixed effect from an Abowd *et al.* (1999) regression, the same results are confirmed, and the WFH wage premium disappears.

Overall, our findings suggest that the empirically observed WFH wage premium may be due to higher-paid workers selecting into WFH, rather than a causal effect of WFH on wages. Moreover, since WFH is an amenity, our results imply that differences in hourly wages understate welfare inequality between workers.

Related literature. Our findings relate to several strands of the literature. Our study of the WFH wage premium as an economy-wide phenomenon complements existing studies that examine selection in more specific contexts. Emanuel and Harrington (2024) argue that remote workers may be negatively selected in a call center of a Fortune 500 firm, while Aksoy *et al.* (2025) find evidence of positive selection in a call center in Turkey. Similarly, Atkin *et al.* (2023) document positive selection on worker productivity in a randomized assignment to a data entry task in India. For pre-pandemic Germany, Arntz *et al.* (2022) find that workers who transition into WFH experience increases in hourly wages.[5] For the post-pandemic US, Rossi-Hansberg *et al.* (2023) also provides evidence consistent with selection. They document a wage premium associated with remote work in the American Community Survey, after controlling for individual characteristics and broad occupation and industry fixed effects. They find no premium in the National Longitudinal Survey of Youth, where they additionally control for individual fixed effects.

Beyond their direct insights on labor market outcomes, our findings also have implications for the literature on the productivity gains from WFH. If these gains arise at the worker level and are shared between the worker and the firm, one would expect them to raise wages. Thus, our finding of a zero WFH wage premium after controlling for selection leaves three possibilities: (i) WFH does not raise productivity in most settings, (ii) WFH raises productivity, but these gains are not passed on to workers, or (iii) the positive wage effect of higher productivity is canceled out by a wage penalty arising from the amenity value of WFH. Further research is needed to investigate these alternatives. However, the wage premium documented in our paper is an important reduced-form fact that can help

[5] Using the same German data, Bernardi (2025) also finds evidence for positive selection into WFH, but argues that this has weakened after the pandemic.

discipline structural models on the impact of WFH or return-to-office (RTO) mandates on productivity and inequality, such as those proposed by Davis *et al.* (2024); Richard (2024); Delventhal and Parkhomenko (2024); Flynn *et al.* (2024), or Sedlácek and Shi (2025).

Beyond the WFH literature, our work relates to the literature documenting wage premia, including Krueger and Summers (1988), Abowd *et al.* (1999), Card *et al.* (2013), Song *et al.* (2019), and Setzler and Tintelnot (2021). We contribute to this literature by showing that WFH has become an important dimension for understanding wage differentials. WFH is now adopted by a large share of workers, and the WFH wage premium we estimate is comparable in magnitude to widely cited gaps such as the gender wage gap.[6] The paper is organized as follows. In Section 2, we document the WFH wage premium, and in Section 3, we present evidence for it being driven by selection. Section 4 concludes.

2 The WFH Wage Premium in France

2.1 Data Sources

We use three data sources. First, we obtain worker-level measures of work-from-home (WFH) from the Labor Force Survey (Enquête Emploi en Continu), which surveys a representative sample of households in France. The survey includes approximately 90,000 individuals aged 15 and above, of whom about 40,000 are actively employed. The survey is conducted quarterly, with each respondent being tracked for six consecutive quarters.

The Labor Force Survey provides consistent data on WFH for the period 2014-2024. Prior to 2021, respondents were asked about their WFH status in both the first and last quarters of their participation. Since 2021, respondents are only asked in their first quarter. The survey's WFH question asks whether an employee has worked from home during the past four weeks, providing four answer options:[7]

1. Yes, this [my home] is my workplace.

2. Yes, for half of my working hours or more.

3. Yes, for less than half of my working hours.

[6] For context, Eurostat reports that in 2023 women's average gross hourly wages were 12.0% below men's in the European Union. See Blau and Kahn (2017) for a comprehensive review of the gender wage gap for the United States.

[7] Answers are translated from the French original. The Online Appendix contains the original wording, as well as further details on the survey and its questions on WFH.

4. No.

Our baseline measure of WFH is a dummy variable that equals 1 if a worker chooses one of the first three options, and 0 otherwise. However, in robustness checks, we also consider several more stringent definitions of WFH.

Throughout, we limit the sample to employees aged between 18 and 70 years who worked at least one hour during the reference week of the Labor Force Survey (i.e., we exclude self-employed and unemployed people).[8] In addition to their WFH status, the survey collects information on worker characteristics including gender, age, education, occupation, and place of residence. Moreover, it also provides self-reported hours worked and earnings, which we use to construct hourly wages, our main outcome variable. More precisely, we measure an employee's earnings as net monthly wages (variable SALRED) and her hours as the effectively worked number of hours during the survey's reference week (variable EMPNBH before 2021, and HEFFEMP after 2021). We winsorize hourly wages at the 1st and the 99th percentiles. In robustness checks, we also use administrative measures of hourly wages.

Our second dataset, measuring firm-level characteristics, is FARE (Fichier Approché des Résultats d'ESANE), which is based on tax filings and covers the universe of active firms in France. We link FARE to the Labor Force Survey using a common firm identifier (SIREN) present in both datasets.

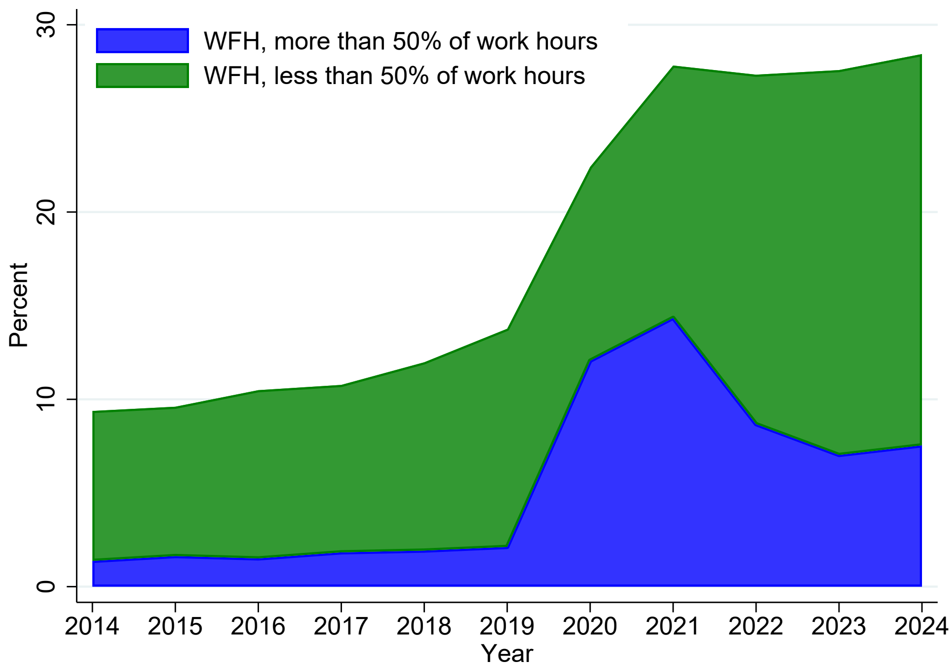
Finally, the third dataset is the DADS (Déclarations Annuelles des Données Sociales), an employer-employee administrative dataset comprising mandatory social security filings by all firms with paid employees. The DADS provide detailed information on wages, hours worked, and other worker characteristics such as occupation and demographics. We use the data to extract a worker's pre-pandemic employment and wage history. This requires substantial work, as the DADS does not contain a personal identifier that could be used for matching individuals across years or to the Labor Force Survey (for privacy reasons, the DADS person identifier changes every year). To transform the DADS into a panel, we follow Babet *et al.* (2025), who exploit the fact that the data gives outcomes for the current and for the previous year to string observations together. Similarly, we use observable characteristics to match the DADS to the Labor Force Survey: if there is a unique worker in the DADS that shares the same birth year, gender, municipality of residence, département of birth and employer establishment number than a worker in the Labor Force Survey, we assume that these records belong to the same person.

[8] During the COVID-19 pandemic, this criterion excludes workers on "temporary layoffs" from our sample. Moreover, we omit workers in agriculture, fishing, and public administration (APE codes 1, 2, 3 and 84).

2.2 WFH and the WFH Wage Premium

Figure 1 depicts the evolution of WFH in France between 2014 and 2024. The blue area represents the share of workers who work from home for more than 50% of their hours, while the green area corresponds to those who work from home occasionally (less than half of their hours).[9] The combined area thus reflects the share of workers engaging in any degree of WFH.

Figure 1: The prevalence of WFH in France



Note: Labor Force Survey (Enquête Emploi en Continu). The figure shows the percentage of workers in our sample that report WFH, distinguishing between workers who do more or less than half of their work from home. To compute aggregate shares, we weigh individual responses with the survey weights provided by the Labor Force Survey.

As in most other developed countries, the prevalence of WFH was gradually rising in France prior to the COVID-19 pandemic, with about 15% of employees reporting some WFH in early 2020. This share rose to nearly 30% in 2021, driven by an increase in the share of workers working mostly from home (likely in response to public health mandates). Since then, the share of workers working mostly from home has declined to about 7%, but remains well above pre-pandemic levels. Occasional WFH, however, has remained

[9] As mentioned above, the Labor Force Survey data is collected quarterly, but each worker is surveyed on WFH only once a year. The figure therefore simply pools answers over the four quarters in each year.

substantially more common than before the pandemic, so that the overall share of workers engaging in any WFH has been stable at around 30% in 2023 and 2024. This level is close to the estimate reported by Buckman *et al.* (2025) for the United States.

For our baseline analysis, we focus on the post-pandemic years 2022 to 2024, when WFH was an established feature of the labor market and public health mandates were no longer binding.[10] However, we also consider wages before and during the pandemic in robustness checks.

Our main focus is on wage differences between workers who work from home, and workers who do not. To document these, we start by running the following regression:

$$\ln w_{it} = \alpha_t + \beta \text{WFH}_{it} + \gamma \mathbf{X}_{it} + \varepsilon_{it}, \tag{1}$$

where w_{it} is the hourly wage of employee i in year t, α_t is a year fixed effect, WFH_{it} is our baseline measure of WFH which takes value 1 if a worker reports any amount of WFH in the reference week of year t and 0 otherwise, and $\mathbf{X}_{it}$ is a vector of controls. Standard errors are clustered at the firm level. Table B.1 in the Online Appendix presents summary statistics for all variables used in the regression.

Column (1) of Table 1 reports the estimated WFH wage premium, β, from equation (1) without any controls besides year fixed effects. Over our sample period, workers who report WFH earn on average 35.2% higher hourly wages than those who do not. However, as emphasized in a large literature, WFH status is strongly correlated with occupation, industry, and location. Therefore, in column (2), we add detailed controls for occupation (fixed effects for 437 ISCO four-digit codes), industry (fixed effects for 74 two-digit APE codes), and commuting zone (fixed effects for 305 area codes), each interacted with year dummies. With these controls, the estimated WFH wage premium falls to 12.0%.

WFH may also be correlated with wages through other worker characteristics, such as education or gender. We therefore add in column (3) controls for gender, 5 age and tenure categories, 16 education levels, marital status, a dummy for permanent employment contracts, a dummy for presence of young children, and a dummy for whether the worker lives in the commuting zone of their workplace, all interacted with year dummies. These additional controls reduce the WFH wage premium from 12% to 6.6%.

While our baseline analysis focuses on hourly wages to control for labor input, we also consider total pay. To do so, column (4) runs the same regression as column (3), but uses the natural logarithm of the total monthly income as the dependent variable. We find a

[10]Precisely, we only use data starting from the second quarter of 2022, after the end of all pandemic-related restrictions.

larger coefficient than in the baseline, implying that workers who work from home have on average 3.6% longer working hours (corresponding roughly to one additional hour of work in a 35-hour workweek). Thus, their monthly income is 10.2% higher than that of similar workers who do not work from home.

Table 1: The WFH wage premium and observable characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln Hourly Wage			Ln Wage	Ln Hourly Wage	
WFH	0.352^{***}	0.120^{***}	0.066^{***}	0.102^{***}	0.064^{***}	0.073^{***}
	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)	(0.016)
Ln Firm Employment					0.005^{***}	
					(0.001)	
Ln Firm Age					0.005^{*}	
					(0.003)	
Ln TFP					0.011^{***}	
					(0.002)	
Year FE	Y					
Occupation × Year FE		Y	Y	Y	Y	Y
Industry × Year FE		Y	Y	Y	Y	Y
Commuting Zone × Year FE		Y	Y	Y	Y	Y
Worker Char. × Year FE			Y	Y	Y	Y
Other Firm Controls					Y	
Firm × Year FE						Y
R^2	0.171	0.446	0.544	0.642	0.548	0.753
N	24311	24118	23880	23880	23444	6587

Note: This table presents estimates for the regression specified in equation (1). Column (1) includes no control variables except for year fixed effects. Column (2) adds interactions between year and occupation (4-digit ISCO codes), industry (2-digit APE codes), and commuting zone fixed effects. Columns (3)–(6) add the following worker characteristics, each interacted with year dummies: gender, education (16 categories for highest qualification obtained), employee age and tenure with the current employer (each split into five bins), marital status, a dummy for a permanent contract (contrat à durée indeterminée, CDI), a dummy for having a child below age 3, and a dummy for residing in a different commuting zone than the establishment's location. Column (5) adds the following firm characteristics: the logarithm of firm employment, the logarithm of revenue TFP, the logarithm of firm age, leverage defined as long-term debt over total assets, the share of buildings in total assets, the share of intangibles in total assets, CEO age, and a dummy for female CEOs; and column (6) adds firm-year fixed effects. The sample period is 2022Q2–2024Q4. All specifications are estimated by WLS using survey weights from the Labor Force Survey. Standard errors clustered at the firm level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Our findings so far suggest that there is a sizable WFH wage premium, which cannot be explained by observable worker characteristics. However, this premium might be explained by employer characteristics. Indeed, it is well known that larger firms pay systematically higher wages to workers with similar skills (Card *et al.*, 2013; Song *et al.*, 2019). These firms may also be more likely to offer WFH arrangements. Moreover, other firm characteristics, such as real estate ownership, and the age and gender of the CEO, also matter for WFH policy (see e.g. Flynn *et al.*, 2024), and may affect wages as well. Given these considerations, we add a large number of firm characteristics to our set of control variables in column (5) of Table 1. Precisely, we include firm size (number of employees), firm age, revenue total factor productivity (TFP), leverage, the share of buildings in total assets, the share of intangibles in total assets, CEO age, and a dummy for female CEOs.[11] As shown in column (5), we find that larger, older, and more productive firms do tend to pay higher wages. However, accounting for these and other firm characteristics reduces the WFH wage premium only marginally—from 6.6% to 6.4%.

By construction, the previous specification only controls for observable firm characteristics. Due to the size of our sample, we can further improve on this by including firm-year fixed effects. That is, we can compare the hourly wages of two workers who have identical observable characteristics, and are also working for the same firm. The results for this specification are shown in column (6). They indicate that even within the same firm and year, workers who WFH have on average 7.3% higher wages than workers who do not. Thus, we can conclude that even though firm characteristics (both observable and unobservable) matter for wages, they can not explain the WFH wage premium.

2.3 Robustness Checks and WFH Intensity

Our results so far suggest that there is an economically significant WFH wage premium, which is not explained by observable worker characteristics or observable and unobservable firm characteristics. In Table 2, we show that this result is robust to a number of different specifications, sample periods and WFH measures.

Alternative measures of WFH. After the COVID-19 pandemic, the French Labor Force Survey was extended to capture more aspects of WFH. Besides our baseline measure of WFH, workers are now also asked about “remote work” (télétravail), defined as WFH that is explicitly specified in the worker’s contract, and carried out by remotely connecting to the firm’s IT system. While we mainly rely on our baseline WFH measure (which is available for a longer time period and captures a less restrictive definition of WFH), we conduct robustness checks with this alternative remote work variable. Precisely, in column (1) of Table 2, we reproduce our baseline regression with all worker and firm controls, replacing

[11] To determine the age and gender of the CEO, we rely on the DADS dataset, and identify the CEO by the CEO occupation code when available, and otherwise as the firm’s highest-paid employee. Revenue TFP is computed as a firm-level Solow residual. That is, it is the difference between the natural logarithm of value added and a weighted average of the natural logarithms of employment and capital. Weights are given by industry-level sales shares.

the WFH dummy variable with a dummy variable for remote work. This is the equivalent to column (5) of Table 1, and results are similar: there is an 8.0% remote work wage premium even after controlling for observable worker and firm characteristics.

Table 2: Robustness of the WFH wage premium

	(1)	(2)	(3)	(4)	(5)
	Ln Hourly Wage				
		$>$ 1 Day Only			
	Alt. WFH Measure		WFH Intensity	Admin Wage	2014-2019
Remote Work	0.080***	0.075***			
	(0.008)	(0.009)			
Some WFH			0.057***		
			(0.007)		
Mostly WFH			0.091***		
			(0.013)		
Fully WFH			0.104***		
			(0.019)		
WFH				0.084***	0.062***
				(0.010)	(0.004)
Occupation × Year FE	Y	Y	Y	Y	Y
Industry × Year FE	Y	Y	Y	Y	Y
Commuting Zone × Year FE	Y	Y	Y	Y	Y
Worker Char. × Year FE	Y	Y	Y	Y	Y
Firm Controls	Y	Y	Y	Y	Y
R^2	0.549	0.539	0.548	0.716	0.540
N	23444	21215	23444	8521	113120

Note: This table presents several variations on the specification presented in column (5) of Table 1. All columns include occupation (4-digit ISCO codes) interacted with year dummies, industry (2-digit APE codes) interacted with year dummies, commuting-zone fixed effects interacted with year dummies; worker characteristics interacted with year dummies; and firm characteristics. In columns (1) and (2), we replace the WFH variable with a variable capturing remote work. In column (2), we additionally drop workers who report remote work for one day per week or less. In column (3), we replace the WFH variable with three dummy variables capturing some WFH (between 0 and 50% of working hours), mostly WFH (between 50% and 100% of working hours) and fully WFH (100% of working hours). In column (4), we replace the dependent variable by the natural logarithm of the hourly wage coming from administrative data (DADS). In column (5), we consider a pre-COVID sample, using data between 2014Q1 and 2019Q4. All specifications are estimated by WLS using survey weights from the Labor Force Survey. Standard errors clustered at the firm level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

WFH intensity. In our baseline analysis, we pooled all workers that work from home, irrespective of the number of hours or days per week for which they do so. A potential concern is that this could include workers with negligible levels of WFH (e.g., one or two hours per week). We address this concern in two ways. First, in column (2) of Table 2, we use the same remote work variable as column (1) but drop workers who work remotely for one day per week or less from the sample. This yields a similar premium to that in column (1). Second, in column (3), we return to our baseline WFH measure, but now introduce separate dummies for workers who work from home for less than half of their hours ("some WFH"), workers who work from home for more than half of their hours, but not always ("mostly WFH"), and workers who only work from home ("fully WFH"). We find that the WFH wage premium is higher for workers who work from home more often, but economically significant at all levels of WFH intensity.

Alternative measures of the hourly wage. Our baseline analysis constructs hourly wages from net monthly wages and working hours that are self-reported by workers in the Labor Force Survey. This could be of concern if workers that work from home systemically over-state their income or under-state their hours relative to workers who do not work from home. To address this concern, column (4) of Table 2 replaces the hourly wage from self-reported data with the hourly wage constructed from DADS social security records.[12] This shrinks the sample considerably, as the DADS dataset for the year 2024 was not available at the time of writing, and because some workers from the Labor Force Survey cannot be matched to their social security records. However, results are again unchanged: we find an 8.4% WFH premium for hourly wages.

Pre-pandemic data. Our main analysis uses post-pandemic data. However, when we estimate our baseline specification (with all worker and firm controls) on pre-pandemic data between 2014 and 2019, we find a 6.2% hourly wage premium, very close to the post-pandemic estimate. This is shown in column (5) of Table 2.[13]

Heterogeneity. In Online Appendix Table B.2, we examine whether the WFH wage premium differs by gender, education, and age. We find substantial WFH wage premia within all groups. The largest difference is observed by education, where the WFH wage premium is 3 percentage points higher in a sample of workers with a university education with respect to a sample of workers without a university education.

3 Why is there a WFH Wage Premium?

The WFH wage premium estimated in Tables 1 and 2 might seem puzzling at first, as the literature typically finds that workers are willing to take substantial wage cuts to work more from home (see the references cited in footnote 1). In a competitive labor market,

[12]Because DADS reports wages and hours separately, we compute this hourly wage using only DADS data.

[13]In Online Appendix figure B.1, we show the evolution of the WFH wage premium over time, by running the regression in equation (1), with all worker and firm controls, separately for each year. We find that the premium has been roughly stable over time.

this should lead to lower wages for workers who work from home, as those workers are compensated with a non-monetary amenity. Given these considerations, our findings could be due to two factors (which are not mutually exclusive):

1. **Productivity**. WFH could increase worker productivity. If these productivity gains are shared with the worker, they would show up in higher wages.[14]

2. **Selection**. Workers who work more from home might be positively selected on some unobservable characteristic, such as ability, negotiation skills or bargaining power.

In this section, we gauge the contribution of selection to the wage premium by leveraging information on worker's wage before the pandemic. That is, we use a worker's past wage as a proxy for relevant unobservable characteristics such as ability or bargaining power within the firm.

As a preliminary step, in column (1) of Table 3, we reproduce our baseline regression (shown in column (5) of Table 1), restricting the sample to workers for whom we observe WFH status and hourly wages in the post-pandemic period, but for whom we can also recover hourly wages in 2018 and 2019 through the DADS. For this sample, we still find a positive and significant WFH wage premium (which, at 4.5%, is slightly smaller than in the baseline). In column (2), for the same sample, we run a regression of our baseline WFH dummy on all worker and firm controls, and on the natural logarithm of the worker's average hourly wage in 2018 and 2019. This shows a strong positive correlation between (post-pandemic) WFH and pre-pandemic hourly wages. This finding suggests that selection could explain the WFH wage premium. Workers who worked from home post-pandemic already had higher wages before the pandemic. Thus, the post-pandemic higher wages of these workers are more likely due to their individual characteristics rather than due to a direct effect of WFH on wages.

To test this intuition, in column (3) of Table 3, we introduce the average hourly wage in 2018-2019 as an additional control variable in our baseline specification. With this control variable, the WFH wage premium drops by a factor of four to just 1.1%, and becomes statistically indistinguishable from zero. Thus, it indeed seems that the WFH wage premium reflects selection on some individual characteristics that are difficult to measure by researchers, but are reflected in wages. Column (4) of Table 3 conveys the same message in a slightly different specification. Here, the dependent variable is the growth rate of hourly wages with respect to the previous year. The control variables are the same as in our baseline specification, and include on top of them a measure of past

[14]The productivity hypothesis is the tentative conclusion of Pabilonia and Vernon (2025), who conjecture that workers who work from home enjoy longer sleep hours, and can therefore work more efficiently.

wage growth (the growth rate of hourly wages between 2018 and 2019). As shown in the table, there is no significant difference in the rate of wage growth between workers who work from home and workers who do not.

Table 3: The WFH wage premium and pre-pandemic wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
					Only Occupations with Large WFH Increases		
	Ln Hourly Wage	WFH	Ln Hourly Wage	Wage Growth	Ln Hourly Wage	Ln Hourly Wage	Wage Growth
WFH	0.045***		0.011	-0.007	0.042**	0.005	-0.010
	(0.011)		(0.010)	(0.006)	(0.020)	(0.018)	(0.010)
Ln Hourly Wage$_{2018,19}$		0.157***	0.443***			0.464***	
		(0.020)	(0.015)			(0.030)	
Wage Growth$_{2018,19}$				-0.019			-0.044
				(0.016)			(0.031)
Occupation × Year FE	Y	Y	Y	Y	Y	Y	Y
Industry × Year FE	Y	Y	Y	Y	Y	Y	Y
Commuting Zone × Year FE	Y	Y	Y	Y	Y	Y	Y
Worker Char. × Year FE	Y	Y	Y	Y	Y	Y	Y
Firm Controls	Y	Y	Y	Y	Y	Y	Y
R^2	0.569	0.569	0.637	0.239	0.619	0.693	0.405
N	8784	8784	8784	5743	2161	2161	1417

Note: This table presents variations of the specification presented in column (5) of Table 1. In column (1), we restrict the sample to workers for whom we observe hourly wages in both 2018 and 2019 (through the DADS). In column (2), our WFH dummy is the dependent variable, and we add the natural logarithm of the average of 2018 and 2019 hourly wages as a control variable. In column (3), we include this latter control variable in our baseline specification. In column (4), the dependent variable is the growth rate of hourly wages (measured in log changes) between year t and year $t-1$, computed with DADS data. Columns (5)–(7) re-estimate the specifications in columns (1), (3), and (4), for the restricted sample of occupations with the largest increase in the share of workers working from home between the pre-pandemic period 2014–2019 and our post-pandemic sample period 2022Q2–2024Q4. Specifically, we compute for each occupation the difference in the fraction of workers who report WFH in 2022Q2–2024Q4 relative to 2014–2019, and restrict the sample to those occupations in which this increase exceeds 25 percentage points. The sample period is 2022Q2-2024Q4. All equations are estimated with WLS, using survey weights provided by the Labor Force Survey. Standard errors clustered at the firm level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

One caveat in interpreting these results is the fact that while we observe pre-pandemic wages, we cannot observe the pre-pandemic WFH status of workers. Thus, it could in principle be possible that for some workers, pre-pandemic wages already reflect a causal effect of WFH (if the worker already worked from home in 2018 and 2019).

Given the large increase in the prevalence of WFH shown in Figure 1, this concern can only be relevant for a minority of workers. Nevertheless, to address it, columns (5) to (7) of Table 3 re-estimate the specifications in columns (1), (3), and (4) of the same table for occupations that saw a very large increase in WFH between the pre- and post-pandemic periods, by above 25 percentage points. In these occupations, the concern that WFH already affects pre-pandemic wages potentially applies to an even smaller minority of workers.[15] In this restricted sample, we find very similar results as in our baseline.

Finally, to complement the previous evidence that uses pre-pandemic wages as a proxy

[15]For context, the occupations with the greatest increases in WFH include "website technicians", "systems analysts", "chemical engineers" and "security finance dealers and brokers".

for unobservable worker characteristics, we also use the method of Abowd *et al.* (1999) (hereafter AKM), in its implementation by Babet *et al.* (2025), to decompose the pre-pandemic hourly wage into worker and firm fixed effects. While a growing literature shows that these fixed effects do not perfectly capture invariant worker and firm characteristics (Andrews *et al.*, 2008; Bonhomme *et al.*, 2020; Kline *et al.*, 2020; Borovičková and Shimer, 2024), our aim is simpler: we show that the post-pandemic WFH wage premium disappears when controlling for these pre-pandemic fixed effects, pointing to an important role for selection.

To implement this method, we estimate the following regression on DADS data, for the pre-pandemic period 2010-2019:

$$\ln w_{it} = \theta_i + \psi_{J(i,t)} + \beta X_{it} + u_{it}, \tag{2}$$

where w_{it} is the hourly wage earned by worker i in year t and $J(i,t)$ is the employer of worker i in year t. θ_i and $\psi_{J(i,t)}$ are worker and firm fixed effects, while X_{it} controls for a cubic polynomial in age and year dummies. We restrict the sample to the largest connected set of workers and firms. In this setup, the worker fixed effect captures all characteristics that make a worker earn high wages (except for her age, employer, and time). Thus, it captures the part of an employee's pre-pandemic hourly wage that can be attributed to the worker rather than the firm.

Using the estimated worker (and firm) fixed effects, Table 4 repeats a similar analysis to the one previously conducted with pre-pandemic wages. First, column (1) reproduces our baseline regression (column (5) of Table 1) for the sample of workers for which we are able to estimate a worker fixed effect for the pre-pandemic period.[16] In this smaller sample, the WFH wage premium is smaller than in the baseline, but still strongly significant. Column (2) then considers a regression in which our WFH dummy is the outcome variable, and the AKM worker and firm fixed effects are added as controls. We find a strong positive correlation between these fixed effects and WFH: even after controlling for a large number of observables, workers who work from home had higher pre-pandemic wages and were employed by higher-paying firms. This is in line with the evidence in Table 3, using only pre-pandemic wages, and suggests that selection on unobservables could play an important role in explaining the WFH wage premium.

[16]By construction, this is only possible for workers who were employed and changed jobs at least once during this period. Additionally, we need to be able to match these workers to their Labor Force Survey response in 2022-2024.

Table 4: Further evidence from AKM fixed effects

	(1)	(2)	(3)	(4)	(5)
	Ln Hourly Wage	WFH	Ln Hourly Wage		
WFH	0.037***		0.009	0.029**	-0.000
	(0.012)		(0.011)	(0.012)	(0.011)
Worker FE (AKM)		0.052***	0.144***		0.147***
		(0.008)	(0.006)		(0.006)
Firm FE (AKM)		0.029***		0.052***	0.057***
		(0.007)		(0.005)	(0.004)
Occupation × Year FE	Y	Y	Y	Y	Y
Industry × Year FE	Y	Y	Y	Y	Y
Commuting Zone × Year FE	Y	Y	Y	Y	Y
Worker Char. × Year FE	Y	Y	Y	Y	Y
Firm Controls	Y	Y	Y	Y	Y
R^2	0.570	0.589	0.625	0.581	0.638
N	7596	7596	7596	7596	7596

Note: This table presents variations of the specification presented in column (5) of Table 1. We consider worker and firm fixed effects estimated from an AKM model over the period 2010-2019, using the same data and specification as in Babet *et al.* (2025). In column (1), we restrict the baseline sample to workers for whom we can compute both a worker fixed effect and a firm fixed effect. In column (2), WFH is the dependent variable, and we add worker and firm fixed effects as control variables. In column (3), we add worker fixed effects as a control variable to the baseline specification. In column (4), we add firm fixed effects to the baseline specification. In column (5), we include both worker and firm fixed effects. The sample period is 2022Q2-2024Q4. All equations are estimated with WLS, using survey weights provided by the Labor Force Survey. Standard errors clustered at the firm level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In column (3), we test this hypothesis by introducing the AKM worker fixed effect as a control variable in our baseline regression. Doing so reduces the WFH wage premium to just 0.9%, and makes it statistically indistinguishable from zero. Instead, when we only control for firm fixed effects for the current employer, as shown in column (4), the WFH premium is reduced, but remains positive and significant. Finally, column (5) jointly introduces worker and firm fixed effects, which makes the WFH wage premium drop to zero. Overall, our results for the AKM fixed effects therefore confirm the ones obtained by using pre-pandemic wages as a control variable, and suggest that the WFH wage premium is driven by selection on unobservables, predominately across workers.

4 Conclusion

Our paper documents a sizable and statistically significant WFH wage premium: using French administrative data and controlling for a rich set of worker and firm characteristics, we find that workers who work from home earn higher hourly wages than those who do not. Our evidence suggests that this premium is driven by selection on unobservable worker characteristics (which could include ability, negotiation skills or bargaining power). Indeed, WFH was more prevalent for workers who already had high hourly wages before the pandemic, and was not associated with higher post-pandemic wage growth.

These findings have implications for several areas of research. First, they indicate that in a world with more widespread WFH, differences in hourly wages may significantly understate inequality, as the best-paid workers are also more likely to receive the WFH amenity. Second, by emphasizing the role of selection, they suggest that changes in WFH policies (e.g., through widely debated RTO mandates) could have important implications for the allocation of talent and for aggregate productivity. Our findings are consistent with case-study evidence that firms offering WFH disproportionately attract more educated and experienced workers (Bloom *et al.*, 2024; Aksoy *et al.*, 2025; Hsu and Tambe, 2025), and speak to recent concerns that stringent RTO mandates may induce the most productive employees to leave firms that do not offer WFH.

Finally, our findings have implications for the literature on the direct productivity effects of WFH. Even though we show that the WFH wage premium could be explained by selection, this does not imply that there are no positive (or negative) effects of WFH on productivity: these gains (or losses) might not be passed on to workers, or, in the case of gains, they could be canceled out by an amenity wage discount.[17] While more research is needed to investigate these alternative explanations, the WFH premium documented in our paper is an important reduced-form fact that can help discipline structural models of WFH and its impact on aggregate outcomes.

[17] In the latter case, one can interpret our estimates as suggesting an upper bound for the productivity effect of WFH, which could be at most as large as the amenity value of WFH (often estimated at around 6-8% of current wages, as in Mas and Pallais, 2017).

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The Work-from-home Wage Premium

by Huiyu Li, Julien Sauvagnat and Tom Schmitz

Online Appendix

A Additional details on the data

A.1 Survey Structure

The French Labor Force Survey is built on a representative sample of housing units. When a housing unit is selected for the survey, all members of the household respond to the survey question at a quarterly frequency. Each household is kept in the survey for six consecutive quarters. The response rate is high, with 75% of selected households answering the survey questions.

A.2 WFH in the French Labor Force Survey

In 2013, the Labor Force Survey introduced a new question about WFH.[18] This question, labeled MAISOC, asked respondents whether they had worked from home in the four weeks before the reference week, and provided respondents with the four answer options listed in the main text. Below, for completeness, we reproduce the French original of the question and the answer options.

Nous allons maintenant nous intéresser aux quatre semaines du lundi ... au dimanche ... (incluant la semaine de référence). Durant ces quatre semaines, (dans le cadre de votre emploi principal), vous est-il arrivé de travailler à votre domicile?

1. *Oui, c'est mon lieu de travail.*
2. *Oui, la moitié des heures de travail ou plus.*
3. *Oui, moins de la moitié des heures de travail.*
4. *Non.*

[18] Before 2013, the survey already asked about work from home, but the reference period and the answer options given to respondents were not the same. Therefore, WFH data before and after 2013 is not comparable.

Importantly, this question was not asked every quarter, but for the household's first and last quarter in the survey.

In 2021, the Labor Force Survey experienced major changes.[19] First of all, questions about WFH were now only asked during the first quarter in which an individual was part of the survey. Second, the number of questions relating to WFH greatly increased. The survey now first asks a screening question on "remote work" (télétravail) in the four weeks before the reference week. The question, labeled TELETRAV, defines remote work as working outside of the employer's worksite, but with access to the employer's IT system and with the employer's written agreement. Respondents have to answer with yes or no. Again, for completeness, the French original of the question and the answer options is shown below.

Pendant ces quatre semaines-là, (dans votre emploi principal), vous est-il arrivé de télétravailler ? Le télétravail consiste à travailler hors des locaux de son employeur, pendant ses horaires habituels de travail. Il suppose de pouvoir se connecter au système informatique de son établissement. Le télétravail est formalisé par écrit avec l'employeur. Rapporter du travail à la maison, travailler lors de déplacements professionnels, chez un client ou de façon mobile (pendant les trajets, entre les réunions) ou encore travailler sur site distant n'est pas du télétravail.

1. *Oui.*
2. *Non.*

Respondents who answer "No" to the remote work question are then asked the exact same WFH (MAISOC) question from the earlier surveys. Respondents who answer yes, on the other hand, are asked a question with very slightly different wording, but with the same answer options. Precisely, they are asked how much they have worked at home in the last 4 weeks, remotely or otherwise. Again, the French original is shown below.

Toujours pendant ces quatre semaines-là, (dans le cadre de votre emploi principal,) combien représente le temps où vous avez travaillé à la maison, en télétravail ou non ? Par exemple: répondre à des mails, lire un dossier, corriger des copies, faire sa comptabilité...

1. *100 % : vous avez travaillé exclusivement à la maison sur la période*
2. *La moitié de vos heures de travail ou plus*
3. *Moins de la moitié de vos heures de travail*
4. *0% : vous n'avez pas travaillé à la maison sur la période*

[19] For a general overview of these changes, see Guillaumat-Tailliet and Tavan (2021).

In our baseline analysis, we use the MAISOC variable to measure WFH, as it has a broader scope and is available for longer. However, as discussed in the main text, our results are robust to defining WFH as remote work, as measured by TELETRAV.

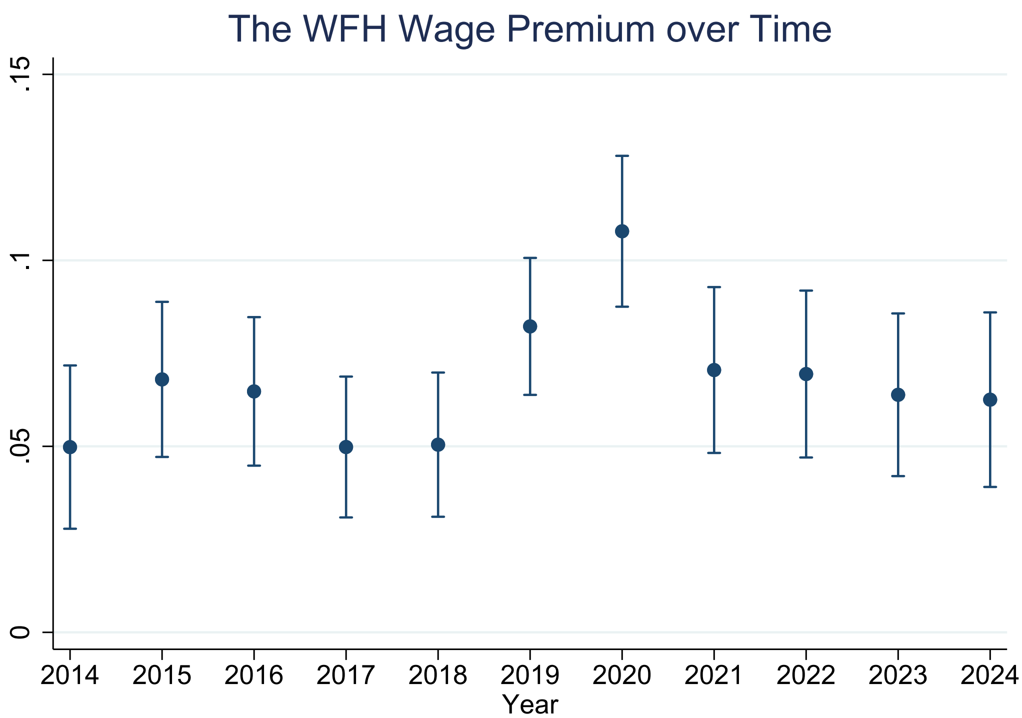
B Additional Figures and Tables

Table B.1: Summary statistics

	Obs.	Mean	SD	P1	P50	P99
Wages						
Ln Hourly Wage (Survey)	24321	2.531	0.391	1.577	2.485	3.650
Ln Wage (Survey)	24321	7.583	0.436	6.397	7.550	8.769
Ln Hourly Wage (Admin Data)	8869	2.736	0.385	2.159	2.659	3.841
Wage Growth	7963	0.058	0.124	-0.357	0.052	0.512
Work-from-Home						
WFH	24321	0.267	0.442	0.000	0.000	1.000
Some WFH	24321	0.197	0.398	0.000	0.000	1.000
Mostly WFH	24321	0.050	0.217	0.000	0.000	1.000
Fully Remote	24321	0.020	0.140	0.000	0.000	1.000
Telework	24321	0.221	0.415	0.000	0.000	1.000
Worker Characteristics						
Female	24321	0.398	0.489	0.000	0.000	1.000
Young Children Dummy	24321	0.087	0.281	0.000	0.000	1.000
Tenure (years)	24123	9.822	10.167	0.000	6.000	39.000
Employee Age	24321	41.271	12.112	19.000	42.000	63.000
University Education	24321	0.247	0.431	0.000	0.000	1.000
Married	24321	0.367	0.482	0.000	0.000	1.000
Permanent Contract	24321	0.881	0.324	0.000	1.000	1.000
Resides in Other CZ	24321	0.280	0.449	0.000	0.000	1.000
Firm Characteristics						
Firm Employment	24321	4471	19164	1	89	157672
Ln TFP	23880	1.998	1.606	-0.239	1.555	9.891
Firm Age	24321	30.026	23.213	1.000	26.000	122.000
Leverage	24160	0.202	0.229	0.000	0.134	1.027
Share Buildings	24160	0.103	0.197	0.000	0.009	1.025
Share Intangibles	24160	0.121	0.187	0.000	0.039	0.898
CEO Age	24321	49.289	9.382	25.000	51.000	66.000
Female CEO	24321	0.231	0.399	0.000	0.000	1.000
AKM						
Worker FE	8644	-0.014	0.996	-1.516	-0.245	3.371
Firm FE	21415	0.001	1.048	-2.750	-0.002	2.663

Note: This table presents summary statistics for our baseline post-COVID sample period, ranging from 2022Q2 to 2024Q4.

Figure B.1: The WFH wage premium – Yearly estimates



Note: The figure displays yearly estimates and 95% confidence intervals from the same specification as column (5) of Table 1 over the period 2014–2024. The specification includes, as control variables, interactions between year and occupation (4-digit ISCO codes), industry (2-digit APE codes), and commuting-zone fixed effects; worker characteristics interacted with year dummies; and firm characteristics. All specifications are estimated by WLS using survey weights from the Labor Force Survey. Standard errors are clustered at the firm level.

Table B.2: The WFH wage premium and observable characteristics - Subsamples

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln Hourly Wage					
	Gender		Education		Age	
	Female	Male	Univ	No Univ	<40	≥40
WFH	0.0594***	0.0710***	0.0811***	0.0509***	0.0525***	0.0605***
	(0.0102)	(0.0099)	(0.0133)	(0.0087)	(0.0108)	(0.0099)
Occupation × Year FE	Y	Y	Y	Y	Y	Y
Industry × Year FE	Y	Y	Y	Y	Y	Y
Commuting Zone × Year FE	Y	Y	Y	Y	Y	Y
Worker Char. × Year FE	Y	Y	Y	Y	Y	Y
Firm Controls	Y	Y	Y	Y	Y	Y
R^2	0.596	0.565	0.606	0.475	0.584	0.580
N	9053	14043	5393	17608	10219	12850

Note: This table presents the same specification presented in column (5) of Table 1, estimated on subsamples defined by worker characteristics. Columns (1) and (2) split the sample by gender. Columns (3) and (4) split the sample by education (no university degree versus university degree). Columns (5) and (6) split the sample by age, separately for workers younger than 40 and workers aged 40 and above. The sample period is 2022Q2-2024Q4. All equations are estimated with WLS, using survey weights provided by the Labor Force Survey. Standard errors clustered at the firm level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.