Working From Home and Sorting of Female and Male Workers

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Abstract

Remote work has dominated labour market debates in recent years: some high-profile employers have recalled staff to the office, citing productivity concerns and provoking backlash from employees, who see working from home (WFH) as a prime non-wage benefit. Although the pandemic accelerated a trend already under way, we still lack a complete picture of why firms adopt or reject remote work and of how worker demand, job feasibility, and managerial discretion translate into actual take-up. Even less is known about whether remote work, via recruitment and retention, alters how firms and workers match in the labour market. In this paper I shed some light on these questions by combining German matched employer-employee administrative records with unique survey data on remote work prevalence and stated motives from both workers and firms (2012-2020). Early adopting firms are larger, more productive, and employ more women, but this selection declines as remote work spreads. Within the same firm and job, high-productivity workers, especially high-productivity women, are more likely to work remotely. Cross-sectional evidence suggests that remote work weakens productivity-based assortative matching and even reverses it for women. This pattern is confirmed by my event study: results show that, after WFH adoption, firms improve hiring and retention of very productive female job-to-job movers, while the average quality of other inflows and outflows stays unchanged. These women trade off firm quality to get the amenity offered by treated firms. Finally, their productivity exceeds that of the workers the treated firms would get under perfect positive assortative matching. This further widens the firm's distance from that benchmark.

1 Introduction

Working-from-home (WFH) has been reshaping when we do and do not work (Pabilonia and Vernon, 2022; Bloom, Han, and Liang, 2022), how we get compensated (De Fraja et al., 2022), and where we choose to live (Ramani, Alcedo, and Bloom, 2024). Covid-19 pandemic marked an inflection point, boosting both the share of firms offering WFH and the share of workers performing their jobs away from the employer's premises, though the trend had been rising well before 2020 (Figure 1). [1]

Recently, some high-profile employers made headlines by abruptly mandating a full return to the office, citing productivity, collaboration, and monitoring concerns, but generating a backlash from employees, who now see WFH as a prime work perk. Despite media attention, we still know little about what drives firms to adopt or reject remote work beyond pandemic-related necessities. We know that WFH is more common among larger firms, those in urban areas, and in certain industries, but a lot of variation remains unexplained (Alipour, Falck, and Schüller, 2023, Barrero, Bloom, and Davis, 2023, Hansen et al., 2023). Similarly, we lack the full picture of what determines individual level take-up. We see that workers value WFH, especially women, and often want more of it than employers are willing to offer, yet job-level technical feasibility explains only a small part of this shortfall, leaving most of the variation in actual uptake still unexplained (Mas and Pallais, 2017, Breda, Dutronc-Postel, and Pecheu, 2024, Barrero, Bloom, and Davis, 2021). In the first part of this paper, I address the questions: Which firms adopt WFH, and what factors drive that choice? Which employees work from home once the policy is in place and the job is feasible?

Understanding these adoption and take-up patterns leads to the question examined in the second part of this paper: does WFH change how workers and firms match? If the firms that adopt WFH and the workers who take it up are each selected from their respective productivity distributions, can we identify whether and how this policy alters productivity-based assortative matching between them? While some studies analyse how WFH directly affects incubent

[1] See Figure 12 for a local illustration of the everyday tension between supporters of in person versus remote work.

workers' productivity, retention, or human capital accumulation, much less is known about whether it operates as a recruitment tool for the firm, the types of workers it attracts, and how this shapes productivity-based matching (Aksoy et al., 2025, Emanuel, Harrington, and Pallais, 2023, Bloom et al., 2015). Are WFH-firms highly productive employers offering high wages and valued amenities, or lower-productivity firms using WFH to compensate for lower wage prospects? Are the workers entering WFH firms highly productive individuals attracted thanks to flexibility, or lower-productivity workers drawn by the opportunity to "shirk-from-home"?

In this paper, I study these questions by linking German matched employer–employee administrative records with a novel survey that reports remote-work incidence and motives for both firms and workers. The resulting panel, covering 2012–2021 and representative of the German economy, provides what is, to the best of my knowledge, the first complete descriptive picture of WFH prevalence across firms and workers. Because the data track both sides, I can separate worker selection into WFH-friendly firms from within-firm individual uptake and, thanks to the panel structure, analyse how adoption evolves over time and how it relates to patterns of firm–worker assortative matching. Finally, I complement the quantitative analysis with unique qualitative evidence: the survey records why firms adopt or reject WFH, which employees prefer it and for what reasons, and why some workers in feasible jobs remain on-site even in establishments with an active policy.

To answer the question of which firms adopt and why, I first document that WFH firms are positively selected on size, number of applications per skilled vacancy, workforce education, female share, and productivity, proxied through average wages and AKM effects (Abowd, Kramarz, and Margolis, 1999). They also employ workers who live further from the workplace, so average commuting distances are longer. As WFH diffuses, many but not all of these selection patterns fade in magnitude and significance, suggesting that later adopters are still better-than-average firms but not as much as early adopters. Finally, future adoption of WFH is predicted only by two of these firm characteristics: the AKM-proxied level of productivity of full time employees and the average commuting distance of the entire workforce. This suggests two “push factors” as particularly relevant for future WFH introduction: lowering commuting costs for current and future employees and helping firms attract and retain highly productive talent.

Turning to stated motivations, firms differ in their reasons for implementing WFH. Many managers see it as an amenity attracting employees by improving their work–life balance, while a similarly large share stresses the possibility of reaching remote workers outside the office. Firms that do not adopt WFH often attribute the choice to technical constraints. However, these barriers seem to be frequently removed within a short time (e.g., two years), suggesting they may not reflect binding technological limitations requiring long investments. A possible interpretation is that some constraints are organisational and can be addressed more quickly through changes in firm practices, in line with existing interpretations of the determinants of WFH adoption and lack of thereof (Barrero, Bloom, and Davis, 2023).

To address the question of who takes up WFH, I analyse the employee-side of my data. Comparing remote workers with their counterparts in fully in-person establishments, I find that selection into WFH-friendly firms explains much of the variation in who works remotely. Yet sizeable heterogeneity remains within firms and occupations. Within remote-friendly firms, employees in the same job are more likely to work remotely if they hold a university degree and exhibit greater productivity, measured by wages or AKM worker effects. Women are also more likely to work remotely. When using wages as a measure of productivity, the gender gap in take-up results entirely driven by high-productivity women, even within narrowly defined occupations (3-digit codes).

Qualitative evidence suggests that both worker preferences and internal firm discretion shape within-firm uptake. Among fully in-person male workers, one in ten reports both a willingness and a job compatible with WFH, yet is not allowed to do so despite an establishment-level policy being in place. Female workers are almost twice as likely to report the same situation, suggesting a gendered discretionary component in the allocation of WFH within firms.

In the second part of the paper, I investigate the role of WFH in productivity-based assortative matching of workers and firms. Using productivity proxies estimated before

WFH implementation for both firms and workers, I find suggestive cross-sectional evidence that WFH weakens positive assortative matching, and even reverses it for women. This pattern could reflect high-productivity WFH firms attracting low-productivity (female) workers, or highly productive (female) workers accepting jobs at lower-productivity WFH firms to access the amenity. To explore this further, I exploit the panel structure of the data and run an event study on the short-run firm-level effects of WFH adoption on workforce composition. I find that adoption increases firms' ability to attract and retain productive female workers, but only among those transitioning directly between jobs, a group of workers that is more educated and more experienced than the average ("pick-of-the-crop" effect). Although the identifying variation is limited and I cannot directly claim causality, the evidence suggests that these workers are drawn to the firm by the implemented WFH policy: they come from a broader geographic area and exhibit high WFH propensity based on occupation and education. These women do not appear to undergo an occupational downgrade to join WFH firms, but they may be trading off firm quality and wages for access to the amenity. Finally, I construct a continuous measure of deviation from perfectly positive sorting and find that this distance increases in adopting firms for female job-to-job switchers. No such pattern is observed for men or other worker categories.

These results suggest that WFH may widen gender differences in sorting across firms, while compressing differences in productivity-based sorting among women, narrowing the average firm productivity gap between high-productivity remote female workers and lower-productivity in-person ones.

My paper contributes to the literature on working from home on two levels.

First, it delivers an economy-wide view of WFH incidence. I merge firm-reported adoption with worker-reported take-up in a panel that follows the same firms and employees over time. These data also record, for each side, the stated reasons for offering or using WFH, which helps opening the black boxes of organisational incentives and individual preferences.
Prior studies connect WFH adoption to workplace amenities, industry, region, and management

style, yet they rely on imputed measures for WFH policy or on cross-sectional evidence (Alipour, Falck, and Schüller, 2023; Hansen et al., 2023; Bergeaud, Cette, and Drapala, 2022; Sockin, 2022; Barrero, Bloom, and Davis, 2023). I use a manager-reported indicator of an active WFH policy and, thanks to the panel dimension of my data, I trace how adoption patterns evolve over time. I pair these quantitative patterns with unique information on firms' own stated reasons for embracing or rejecting WFH.

At the worker level, a growing literature uses willingness-to-pay experiments (Maestas et al., 2023) or survey data (Breda, Dutronc-Postel, and Pecheu, 2024; Barrero, Bloom, and Davis, 2021; Dingel and Neiman, 2020) to study workers' preferences for WFH, the technical feasibility of their jobs, and the actual equilibrium take-up. Taken together, results from these papers point at to two distinct gaps: one between preferences and outcomes and another between outcomes and occupational feasibility. For example, women consistently report higher willingness to pay for WFH than men (Nagler, Rincke, and Winkler, 2024; Mas and Pallais, 2017) yet gender gaps in realised take-up are modest or absent (Barrero, Bloom, and Davis, 2023, Kley and Reimer, 2023). Some papers based on firm-level experiments allow linking preferences to realised outcomes by documenting self-selection into remote work once the policy is available, but they cover only one employer and occupation (Emanuel and Harrington, 2024 Atkin, Schoar, and Shinde, 2023). Leveraging contemporaneous employer–employee data, I separate worker sorting into WFH-friendly firms and job-level feasibility from within-firm take-up, giving an economy-wide view of remote work take-up once the policy is available. I also draw on novel information on workers self-reported desires and underlying motivations while observing their actual WFH status, shedding further light on the roles of worker preferences and managerial discretion in realised equilibrium allocation.

The second contribution of this paper is to provide new evidence on the implications of firm-level WFH adoption. Existing work has examined its effects on the retention and productivity of incumbent workers, the expansion of firms' local labour markets, and changes in workers total compensation (Akan et al., 2025; Coskun et al., 2024; Emanuel, Harrington, and Pallais, 2023; De Fraja et al., 2022; Vos, Ham, and Meijers, 2019; Bloom et al., 2015). Most studies rely either on short (one- to two-year) single-firm experiments with occupationally uniform workforces or on cross-sectional correlations. Some of this literature also examines gender heterogeneity in WFH effects on wages and labour supply (Nagler, Rincke, and Winkler, 2024; Pabilonia and Vernon, 2022; Harrington and Kahn, 2023).

I extend this literature by analysing, at the economy level and over a four-year period, a new potential implication of WFH adoption: changes in productivity-based assortative matching of firms and workers, and the gender heterogeneity of this effect. Coskun et al. (2024) show that, using occupational feasibility as a proxy for WFH, post-pandemic German workers, especially new hires, accept longer home-to-work distances, likely because of the lower commuting costs allowed by WFH. Interpreting the authors findings through the firm lens, longer acceptable commute distances should translate into a larger applicant pool for each vacancy, potentially increasing the average quality of new hires. This effect, if there, should be stronger for women because the same commute imposes greater disutility on them (Nagler, Rincke, and Winkler, 2024). I test the validity of this framework by comparing changes in new-hire quality, separation quality, and commuting distance at adopting and non-adopting firms before and after WFH introduction. Worker and firm quality are measured using pre-adoption estimates, so they are not contaminated by compensating differentials or by the direct productivity effects of remote work.

The rest of the paper is structured as follows. Section 2 describes the matched employer–employee dataset and defines firm- and worker-level WFH measures. Section 3 documents descriptive evidence on firm adoption patterns and worker take-up determinants, both across and within firms. Section 4 presents suggestive evidence on WFH role in productivity-based assortative matching between firms and employees. Section 5 outlines the event-study design and reports the short-run effects of firm-level WFH adoption. Section 6 concludes.

2 Data

2.1 Data products

2.1.1 The LPP Survey

I use confidential administrative data collected by the German Federal Employment Agency and managed by the Institute of Employment Research (IAB).[2] My main dataset is the Linked Personnel Panel (LPP), an employer-employee survey with information on WFH policy adoption at the firm level and take-up at the worker level. The employer side consists of interviews with establishment managers and contains information on HR practices while the employee side surveys a subsample of each firm's workforce among the workers covered by social security, and contains information on job characteristics and personal attitudes. The LPP begins in 2012 and is conducted every two years until 2020, resulting in five waves. Each wave includes on average 850 establishments and 6,000 workers. Table 1 reports the distribution of establishments and workers across waves. Further details on the Panel structure are reported in Tables 15 and 16 in the Appendix.

The LPP is representative of all private-sector establishments in Germany with at least 50 employees covered by social security.[3] At the worker level, it represents the workforce of these establishments. All industries are included except agriculture, forestry and fishery, the civil service, and charity organizations.

I link the LPP to three additional data sources.

2.1.2 The ADIAB

First, I link it to administrative records on firms and workers (ADIAB). The ADIAB covers all firms in the LPP and provides annual firm-level data on size, workforce composition, location, sales, labour costs, and capital expenditure. At the worker level, it includes detailed

[2]The data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access. See Mackeben et al., 2023 for more detailed information.

[3]According to the German Federal Statistical Office, about 60% of German private sector employment is located into enterprises with 50 or more workers (figure refers to firms rather than establishments and includes self-employment).

demographics (age, gender, nationality) and job characteristics (occupation, wage, managerial status) for all employees covered by social security in establishments surveyed by the LPP.[4] For both firms and workers, ADIAB variables are available not only in LPP survey years (biennially from 2012 to 2020), but also in the years before and after each wave, up to 2021. On the worker side, the data also include employment spells before and after the job in an LPP firm.

2.1.3 The AKM wage effects

Second, I merge in firm and worker wage effects estimated using the AKM methodology (Abowd, Kramarz, and Margolis, 1999). These estimates are produced by the Institute providing my data (the IAB), which has access to administrative wage records covering the universe of German workers and firms since 1990. Estimation follows the approach introduced by Abowd, Kramarz, and Margolis (1999) and refined by Card, Heining, and Kline (2013), with minor adjustments as described in Lochner, Wolter, and Seth (2024). Wage effects for workers and firms are computed within each non-overlapping interval of 5 to 8 years between 1985 and 2021, resulting in five distinct potential estimates for each firm and each worker in my sample. The AKM estimations cover all firms and all full-time workers.

In this paper, I use the worker and firm wage effects as a complement measure to log wages.[5] These effects provide a proxy for worker productivity and allow me to decompose wages into worker and firm components. I mainly rely on the effects from 2007–2013, which are estimated immediately before my analysis period and are available for all workers employed full-time at least one year in that period (around 89% of all full-time workers in my sample). This maximises coverage while avoiding using effects estimated contemporaneously to my analysis, thus partially addressing the concern of such estimates being endogenously affected by working from home, e.g., through changes in productivity due to working from home or

[4] In the original dataset, wages are right-censored. To address this, I impute values at the top threshold using a standard two-step procedure similar to Card, Heining, and Kline (2013) and Dustmann, Ludsteck, and Schönberg (2009). I also deflate them to 2015 values using the CPI.

[5] Gross wages are right censored, so I impute censored values with a standard two-step procedure (Card, Heining, and Kline, 2013; Dustmann, Ludsteck, and Schönberg, 2009); I deflate them to 2015 equivalent and log-approximate their yearly value with the inverse hyperbolic sine.

compensating differentials. This becomes particularly relevant in my event-study analysis (see 5.3). Finally, I do not use the raw AKM values and instead I rank workers and firms separately by their respective wage effects and rescale these ranks into percentiles (1 to 100). This facilitates comparability between worker and firm AKM effects, which would be otherwise on different scales, and improves interpretability, as the raw AKM effects have a meaning only in relative terms.

2.1.4 The local labour markets

I follow Kosfeld and Werner (2012) to aggregate firm location data from the 401 administrative districts (*Kreise*) into local labour markets (commuting zones), resulting in 141 distinct areas. To measure worker commuting behaviour, I define two variables. The first is commuting distance, defined as the distance between the centroids of the *Kreise* of residence and of employment, log-approximated using the inverse hyperbolic sine. The second is an indicator for cross-zone commuting, equal to one if a worker resides and works in different commuting zones.

2.2 Definition of WFH for firms and workers

I classify a firm as WFH-allowing if it answers "Yes" to the question: *Does your establishment/office allow employees to work from home?* and as non-allowing if it answers "No". This question is included in all waves of the survey except the first one (2012). At the worker level, I define a worker as working from home if four conditions are met: (1) the worker is employed at a WFH-allowing firm; (2) they answer "Yes" to the question *Do you work from home for your employer – even if occasionally?*; (3) they report working from home for full days, not just a few hours during the workday; and (4) they indicate doing so during working hours, not only outside them. Workers who respond to the WFH question but do not meet all four criteria are classified as not working from home.

While workers are asked about their teleworking habits in all LPP waves, the construction of my WFH measure requires firm-level information (requirement (1)). As a result, it is available only from 2014 onward.

Buckman et al. (2025) shows that the prevalence of WFH is sensitive to how it is defined. My definition captures what I aim to measure: a systematic arrangement stemming from a firm-level policy, reflecting a regular work practice rather than a one-off or informal situation. It also ensures to capture the WFH-related benefits of reduced commuting costs.

Finally, note that empirically my measure mostly registers hybrid WFH. The share of fully remote workers within my definition is just 20% [6]. Such a low incidence of fully remote workers likely reflects the nature of my data, which covers only workers under standard employment contracts subject to social security contributions. Fully remote WFH is more common among workers in non-standard arrangements, such as external contractors (Barrero, Bloom, and Davis, 2023).

Figure 2 reports the annual share of workers doing WFH according to my definition.

3 Descriptive analysis

In this section I examine the relationship between remote work and firm and worker characteristics. I first compare firms with an active WFH policy with those without, and assess whether differences between the two groups vary over time. I then check if any characteristic helps predict future adoption and complement that with qualitative evidence. I then compare the characteristics of remote and non-remote workers. The structure of the data allows me to distinguish selection into WFH-friendly firms from selection into remote work itself.

3.1 Firms

Column 1 of Table 2 reports the coefficients from a linear probability model regressesing an indicator for having a WFH policy on firm characteristics, while controlling for industry -by-year and local-labour-market -by-year fixed effects. These estimates summarise the cross-sectional correlation between WFH adoption and firm attributes. The estimates point to positive selection into WFH: adopting firms are larger, employ a better-educated workforce,

[6] I define workers as fully remote when they report to work 90% or more of their hours remotely

attract more applicants per skilled vacancy, and employ workers who live farther from the workplace and therefore, on average, face longer commuting distances.[7] These firms also have a higher share of female and part-time workers and pay higher wages.

Column 2 asks whether the large wage premium observed in Column 1 reflects firm or worker wage effects. Using AKM effects estimated in the period 2007–2013 and ranked 1–100, both firm and worker fixed effects correlate positively with WFH presence. The worker effect, however, is larger and more precisely estimated than the firm effect. When using contemporaneous AKM estimates (2014–2021), only the worker component remains significant (Appendix Table 18).

Column 3 adds to the previous set of regressors the average wage of female workers, to test whether the correlation between WFH and firm average wage is affected differently by female and male wage levels. Column 4 repeats the exercise using pre-estimated AKM effects (2007-2013) rather than wages. While the coefficient for the average wage of female workers is positive but not significant, the one for the average female AKM effect is significant, and appears to fully account for the positive association between highly productive workforce and WFH adoption, as the average worker AKM effect becomes insignificant in this specification. Using contemporaneously estimated AKM wage effects (2014-2021) confirms this latter pattern (Column 3 of Table 18). The difference between wage-based and AKM-based estimates may reflect the lack of AKM effects for part-time workers, who make up nearly 40% of the female workforce in my sample, compared to under 4% of males. This would suggest that WFH firms are more likely to employ highly productive female workers and not necessarily highly productive male workers, but this pattern is only evident when restricting to full-time workers.

The last two Columns of Table 2 repeat the specifications of Columns 3 and 4 but exclude firms that always report allowing WFH all along the observational period. The dependent variable now is still 0 for non-adopters but it is equal to 1 only for “later adopters” , i.e. firms that I observe switching from not allowing to allowing WFH during the surveyed period. Most of the previously significant coefficients loose significance, while shrinking in size and even

[7]Results unchanged if controlling flexibly for multiple size dummies to account for non-linearities in size. Only firms with at least 50 employees are included in the LPP.

changing sign in some cases (e.g., workforce education and average wage for male workers). In this group of firms, selection remains positive in terms of the number of skilled applications received per vacancy and the average commuting distance of workers. This may suggest that earlier adopters of WFH might be more positively selected, and that over time the policy diffuses to less selected firms.

In Appendix Section D.1 I explore associations between the presence of WFH policies and firm-level financial indicators (profit, labour share, etc.), in addition to the main covariates seen so far. Because financial indicators are available only for a subsample of firms, I include all my firms in these regressions, without the split of later from earlier adopters. Results, detailed in Table 17, are consistent with those reported in Table 2.

Table 3 exploits the panel structure of the data to test whether firm attributes predict later WFH adoption. The sample now contains only later adopters (dependent variable = 1) and never adopters (dependent variable = 0). For each firm, covariates are lagged to their first-occurrence values, so for adopters they refer to the pre-adoption values. Under this restriction, only two predictors appear relevant: workforce productivity and commuting distance. Workers productivity is positively associated with future adoption when measured through wages, but positive and significant only when measured with AKM workers effects, possibly suggesting that the effect is driven by full-time workers. The coefficient for the workforce average commuting distance is of similar size across specifications in Column 1 and 2, but only marginally significant in the second one. Re-estimating the same specification with contemporaneous AKM effects for 2014–2021 leaves the productivity coefficient positive and of same order of magnitude, though no longer significant, while commuting distance remains positive and significant (Column 4 of Table 18). The sub sample used for this analysis includes fewer than 1,000 firms, which limits statistical power and may explain the lack of additional associations and partial inconsistency in the significance levels across specifications. Nevertheless, the results open to interpreting commuting costs and talent considerations as potential push factors behind future WFH adoption. For example, firms might adopt WFH viewing it as an amenity to retain the current highly productive workers or reduce the commuting costs of highly geographically dispersed ones. Another possibility is that the firm sees WFH as a tool to keep attracting workers similar to the current ones in these respects.

Overall, this regression analysis shows that WFH is associated with positive firm-level characteristics, such as worker average wages and AKM-measured productivity, with possibly stronger associations for the average productivity of full-time female workers. This positive selection appears to weaken among later adopters. The panel evidence further suggests that longer average commuting distance and higher productivity of full-time employees are not only correlated with presence of WFH, but might also predict firm adoption decisions, providing insight into potential motivations for adoption of WFH policies.

To complement these quantitative findings, I draw on qualitative survey evidence that directly explores firm motivations for adopting or not adopting WFH. In the fourth wave of the LPP survey (2018), establishment managers were asked to select all relevant reasons for allowing remote work and to specify the main one. Table 4 summarises their responses. Consistent with the higher shares of female and part-time workers, the two most frequently cited reasons for adoption are related to increasing employees' flexibility and improving their work-life balance. The third most cited reason, *To extend employee reachability*, may also reflect efforts to relax standard office-hour constraints, though likely aimed more at increasing workers availability to the firms' advantage rather than at improving their work-life balance. Saving commuting time is frequently mentioned as well, consistent with regression evidence of longer commuting distances among employees, though it is rarely indicated as the main reason. Notably, 40% of the firms cite increased productivity as one motivation for adopting WFH, with roughly 14% identifying it as the primary reason. Given that this survey predates the COVID-19 pandemic, these responses suggest that some firms already viewed remote work not only as an employee perk but also as a productivity-enhancing arrangement (Barrero, Bloom, and Davis, 2021). Consistent with earlier findings on hybrid WFH (Barrero, Bloom, and Davis, 2023), few firms cite optimising office space as a motivation.

Table 5 examines justifications reported by firms for not adopting WFH policies. Both before and after the pandemic, the most common reason is the perceived unsuitability of tasks for remote work. However, more than a third of the firms citing task unsuitability in 2016 subsequently adopted WFH within two years, and slightly less than half adopted it within four years. This pattern suggests that references to task unsuitability do not always signal a hard technological constraint that cannot be relaxed or that can be only be eased through long-term investment. A possible interpretation is that reporting this reason often reflects organisational preferences, as hypothesised by Barrero, Bloom, and Davis (2023) and empirically documented for the Japanese economy by Kambayashi and Ohyama (2025). Other frequently reported concerns include collaboration difficulties and lack of technical infrastructure. While worries about collaboration grow more common after the pandemic, the opposite is true for technical constraints, likely reflecting improved technologies and increased familiarity with WFH tools. Finally, the survey does not provide separate clear options for worries about monitoring and about workers productivity. The closest option (*Complicates management*) is rarely chosen, and even less so after Covid-19, in sharp contrast with some employers claims, widely covered by the media, that unmonitored shirking employees are a main reason to ban remote work (Figure 12).

Taken together, Tables 2, 4, and 5 reveal a mix of reasons behind WFH adoption or its absence. These sometimes point in different directions across firms, reinforcing the idea that, rather than being driven by a single factor such as technological feasibility, WFH adoption is due to a mix of factors, including firm culture and organisational practices.

3.2 Workers: between firms comparison

This section compares teleworkers in WFH-adopting firms to all workers in non-WFH firms, who by definition cannot work remotely. Non-teleworkers in WFH firms are excluded from the comparison (see Panel A, Table 6). The goal is to isolate the part of the observed differences between teleworkers and non-teleworkers that is driven by selection across firms, rather than by within-firm selection into WFH once the policy becomes available (analysed in Section 3.3). To this aim, I first estimate a baseline model including the full set of characteristics and controls. I then re-estimate the same specifications including firm fixed effects. This second approach absorbs all cross-firm variation and identifies only within-firm differences among workers in establishments that switch to WFH during the observation period. Comparing the two sets of estimates provides an approximation of how much of the observed gaps in the first set are due to sorting across firms, as opposed to within-firm selection into remote work. Moreover, the specifications without firm fixed effect show whether the remote share of the firm workforce has a role in explaining the firm-level differences observed in the firm section, or if those reflect broader firm-level characteristics.

Table 7 presents the results of this exercise, performed through a linear probability model where I control for industry -by-year, local labour markets -by-year, time trends, and 2-digit occupational classifications (robust to 3 digits). Column 1 shows that teleworkers are more likely to be female, highly educated, better paid, and to face longer commutes to the office. They are also more likely to be in part-time employment.[8]

Column 2 replaces wages with AKM-based wage effects ranks. Results remain similar to those in Column 1, the main difference being the age: this specification suggests that teleworkers are on average younger than non-WFH workers, though not the youngest, consistent with Barrero, Bloom, and Davis (2023). Including parental status does not affect the results (Appendix Table 20).

Columns 3 and 4 replicate the specifications in 1 and 2, adding firm fixed effects. Most coefficients shrink substantially, and some lose statistical significance. In particular, both measures of worker productivity (log wages and AKM ranks) are reduced to about a tenth of their original magnitude. This suggests that the majority of the baseline differences observed in Columns 1 and 2 are driven by workers sorting across firms. Notice that, while column 3 and 4 are useful to understand the role of workers selection across firms when compared to 1 and 2, they do not represent a full within firm take-up comparison, since they do not compare contemporaneous in-person and remote workers within the firm.

[8]Commuting distance is measured as the inverse hyperbolic sine of the distance between the centroids of the worker's and the firm's *Kreise*. Roughly 20 percent of teleworkers are fully remote and may not commute at all.

Appendix Table 19 extends the analysis by interacting gender with each of the two productivity measures. The interaction terms are small and not statistically significant both with and without firm fixed effects, indicating that high-productivity men and women are equally likely to sort into WFH firms.

3.3 Workers: within firm comparison

This section compares teleworkers and non-teleworkers within firms that have adopted a WFH policy, excluding all workers in firms that do not offer this amenity (see Panel B, Table 6). The aim is to isolate the part of the observed worker-level differences that reflects within-firm selection into remote work, once the policy becomes available. The analysis controls for industry, local labour market, relative time trends, and 2-digit occupation (3-digit occupation codes are used as a robustness check) aiming to capture the characteristics influencing take-up beyond job-level feasibility.

Table 8 presents the results from a linear probability model performing this comparison. Column 1 shows that, conditional on industry, occupation, and commuting zone, workers who take up WFH are more likely to be female, part-time, highly educated, and better paid. They also have shorter tenure and longer commuting distances. Column 2 replaces log wages with pre-estimated AKM-based productivity ranks and confirms that WFH workers are more productive on average. Columns 3 and 4 add interactions between gender and the two productivity measures. The wage-based specification in Column 3 suggests that, even if all high-wage workers are more likely to take up WFH, high-wage women are more likely than high-wage men, indicating that the gender gap in WFH participation observed in Column 1 is driven by highly productive female workers. However, this pattern is not confirmed using AKM effects (Column 4), where no significant gender differences appear either on average or among high-productivity workers. The discrepancy between the two productivity measures may reflect the reduction in the female subsample when using AKM-based estimates, only available for full-time employees, or just uniform take-up between full-time male and female, with more productive workers from both genders being more likely to take-up.

Appendix Table 22 explores gender differences in WFH take-up linked to education and parental status. The interaction between gender and education is positive and significant, replicating the pattern of the interaction of gender and wage. Being a mother is associated to a slightly higher uptake in WFH (Column 2), not entirely explaining the gender based results.

By definition, the observed patterns in the equilibrium level of within-firm WFH take-up are the results of supply and demand factors for this amenity, and might be affected by worker preferences, within-occupation tasks feasibility, and potentially firm-side discretion in the allocation of this amenity. I turn to the qualitative evidence from my data to help shed some light on the role of these factors.

In 2014, firms' managers interviewed for the LPP report the share of workers entitled to WFH and the share actually using it. On average, in firm with active WFH policies, in that year users represent about half of those entitled, with a slightly narrower gap among managerial staff (Appendix Table 21), suggesting non-uniform demand for this amenity, with some workers preferring to work fully in person. In waves 2 to 5, workers who do not telework are asked whether they would like to. Looking exclusively within firms with an active WFH policy, I see that between 13% and 15% of men and women answer yes to this question (Panel A, Table 9). In waves 2 (2014), 3 (2016) and 5 (2020), fully in-person workers are also asked why they do not WFH (Panel B, Table 9). The most cited reason for both men and women is job infeasibility, consistent with occupational constraints (these figures are unconditional averages). Other reasons reflect both demand- and supply-side factors. Almost half of the workers cite a desire to separate work and private life or concerns about collaboration. At the same time, more than half report a lack of informal approval from their direct supervisor, and around 40% of eomen and 50% of men report inadequate technical equipment. Notably, less than 11% of men but more than 18% of women report that they are not allowed to work remotely even though it would be technically feasible, with the 7.1 percentage points gap highly significant in the t-tests reported in the last column of the Table. These shares point to a potential role for firm discretion in WFH allocation, even when jobs are considered feasible by workers themselves, with women disproportionately affected.[9]

Overall, the within-firm comparison reveals that remote workers are, on average, the most educated and productive employees in the firm, with lower tenure and coming from a larger area within the local labour market. Evidence also suggests that women, and particularly highly paid women, are more likely to take up WFH than their male counterparts. However, these results alone do not disentangle the roles of worker preferences and employer discretion in shaping this equilibrium. While I do not quantify their relative importance, the qualitative data suggest that both matter. In particular, survey responses indicate that some workers choose not to take up WFH even when entitled, while others would like to but cannot, even if they report their job to be feasible and the policy is in theory in place at the establishment level. This last piece of evidence highlights the role of firm discretion in access to WFH, which might be operating differently for male and female workers.

4 Suggestive evidence on sorting

The previous sections document cross-sectional selection into WFH for more productive firms and workers. This pattern persists when measuring productivity through wages, pre estimated AKM wage effects (2007-2013) or contemporaneously estimated effects (2014-2021), and holds within industries, local labour markets, and occupations (up to the 3-digit level).

These results motivate an examination of whether WFH influences the assortative matching of firms and workers by productivity level. I adopt a between-firm cross-sectional perspective and I compare workers in WFH firms who work from home to workers in non-WFH firms who do not. I exclude non-WFH workers in WFH firms, as their matching is unlikely to have been affected by telework availability (see Table 6).

To assess sorting patterns, I estimate the following worker-level regression model:

[9] Due to limited sample size (two waves), I cannot check for the significance of the gender gaps in each reason using the LPM specification from Table 8.

$$Y_j = \beta_0 x_{i,j} + \beta_1 WFH_{j,t} + \beta_2(x_{i,j} \cdot WFH_{j,t}) + \beta_3 Z_{i,j,t} + \epsilon_{i,j,t}$$

Here, Y_j denotes the productivity of firm j, and $x_{i,j}$ is the productivity of worker i employed in firm j. $WFH_{j,t}$ is a dummy equal to 1 if worker i is working from home at time t in firm j, and by definition also identifies whether firm j has an active WFH policy. The coefficient β_0 captures the average association between worker and firm productivity in firms without WFH. The coefficient β_2 is the main parameter of interest, identifying whether WFH mediates the strength and direction of this sorting. The term $Z_{i,j,t}$ includes the same set of controls used in the individual-level analysis, except for wages. These are worker demographics, 2-digit occupation, firm industry, local labour market fixed effects, and relative time trends.

Table 10 reports results using productivity ranks based on AKM effects from 2007–2013. First column includes all workers, while columns 2 and 3 split the group by gender. Consistently with Section 3.1, being employed in a firm with an active WFH policy strongly correlates with being in a more productive firm, for both male and female workers (β_1). The coefficient β_0 is positive and significant, capturing the average strength of positive assortative matching in firms without WFH. The interaction term β_2 is negative, significant, and large relative to β_0, indicating that WFH reduces the extent of positive sorting by approximately 80% in the full sample.

This negative effect of WFH on firm–worker sorting is driven by women: in the female subsample the interaction term ($\beta_2 = -0.13$) exceeds the baseline sorting coefficient (β_0= 0.12) in absolute value, implying that although higher-productivity women normally match with higher-productivity firms, this pattern vanishes in the remote labour market. For men, the interaction coefficient is also negative and sizeable, but not statistically significant.[10]

Results are robust to controlling for occupation at the 3-digit level instead of two. Exploring the role of parenting status is not conclusive. The relevant sample becomes small when focusing on parents, due to two constraints: parenting is only observed for children under 14, and AKM effects are only available for full-time workers, which disproportionately reduces the

[10] The *Men + Women* results do not exactly add up to the *All* sample because of some singleton fixed effect cells which are dropped when splitting the sample by gender.

observations of mothers of young children. In this restricted sample, all the main coefficients including the interaction coefficient maintain their sign and a similar or even larger magnitude, but lose significance.[11]

Due to the potential endogeneity between WFH and contemporaneous wages or firm and worker AKM effects, Table 10 remains my preferred specification. However, for comparison, I repeat the analysis using contemporaneous productivity measures. Appendix Table 23 uses contemporaneously estimated AKM effects (2014–2021), while Table 24 uses log wages. To increase comparability, I restrict these samples to workers and firms with valid AKM measures for 2007–2013. This excludes all part-time workers and approximately 10% of the overall full-time sample, reflecting cases where the firm or worker was not employed in any of the 2007-2013 years and therefore has no productivity record for that period.
The main patterns remain unchanged. The interaction term consistently reduces the positive sorting coefficient by 60% to 80%, depending on the specification, remaining statistically significant for both men and women, rather than for women only. I also repeat the regressions using all workers and firms with a contemporaneous productivity estimate (wages or 2014-2021 AKM), without requiring valid 2007–2013 AKM effects. Results remain unchanged except for when using wages as a productivity measure. In that case, the sorting interaction term for women becomes insignificant. This is likely due to the inclusion of part-time female workers, who were excluded from the AKM-based estimates. The negative sorting patterns for remote female workers seem to mainly concern the full-time ones.[12]

5 Event Study

Motivated by the cross-sectional evidence on the role of WFH in weakening positive assortative matching, I exploit the panel structure of my data to assess how the introduction of a WFH policy at the firm level affects its productivity based match with workers. Specifically, I

[11] Results available upon request

[12] Results available upon request

analyze changes in employee inflows and outflows, investigate how these dynamics vary by gender, and explore the resulting implications for both workers and firms.[13]

5.1 Treatment and control structure

In the data, I observe the relevant firm-level variables annually (from the ADIAB), while WFH status is reported biennially from 2014 onwards (LPP source). To construct a complete treatment Panel, I impute WFH status for the intervening and prior years using a conservative rule. Firms that never report their WFH status, as well as those always reporting an active policy ("always treated") are excluded. If a firm has no prior WFH declaration and is later observed with a negative record of the policy, I assume it is untreated in all earlier and later years until a positive report is observed. After the first declaration of WFH adoption, I consider the firm treated in all subsequent years, regardless of later declarations. Fewer than 10% of ever adopters report revoking the policy during the observation period. This structure allows me to interpret the estimates as a lower bound of the short-term effects of WFH implementation.

Table 11 reports the number of treated and control firms over time, while Table 12 shows the number of treated firms observed at each event time. Since I impose at least one year of pre-trends for my firms to be included in the study, the treatment can happen in 2016, 2018 and 2020. I retain 225 unique treated firms with at least one year of pre and post-treatment observation. Due to staggered treatment timing and survey attrition, only 26 treated firms are observed four and five years after adoption. I therefore restrict the analysis to a three-years event window before and after adoption.[14]

[13] An individual-level event study could reinforce the firm-level results, but data limitations on the employee side prevent this.

[14] Assuming random attrition from the LPP survey, firm selection is unlikely to bias results. Over 90% of treated firms continue operating after exiting the LPP; the remaining 10% may also remain active, although I cannot observe them.

5.2 Regression equation

I estimate an event study model with staggered treatment timing, comparing firm-level outcomes before and after adoption between firms who adopt WFH (treated firms) and non adopters and not-yet-adopters (control firms). The regression equation is:

$$Y_{jt} = \alpha_j + \sum_{k=T_0}^{T_N} \beta_k \cdot Treat_{j,t+k} + \tau_t + \gamma_g \tau_t + \rho_r \tau_t + \epsilon_{jt} \tag{1}$$

Subscript jt refers to firm j at time t. Y_{jt} is my outcome of interest (e.g., average female workers productivity), α_j, τ_t, γ_g and ρ_r are the firm, year, industry and local labour market fixed effects. I control for trends in industry ($\gamma_g \tau_t$) and in local labout market ($\rho_r \tau_t$).

I estimate my event study using the local projection difference-in-differences (LP-DID) method of Dube et al. (2023). This estimator addresses the problems of negative weighting and treatment effect heterogeneity that affect standard two-way fixed-effects approaches (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). LP-DID performs comparably to alternative approaches such as Sun and Abraham (2021) and Callaway and Sant'Anna (2021), but is computationally more efficient, an important consideration given the remote access constraints of the IAB data environment.[15]

Before estimating the event study, I test whether firms that will adopt working from home differ systematically from those that never do. I focus on the year 2015, the last pretreatment year for the first group of adopters in the sample. The comparison is between firms that adopt WFH at any point later and firms that never adopt it.

Table 13 reports the results from a series of OLS regressions, where each firm-level characteristic is regressed on an indicator for future adoption. All regressions include controls for firm size, industry, and local labour market (Model 2). Column (1) shows the average value of each variable among never-adopting firms. Column (2) reports the difference for future adopters.

[15]Due to remote access, data work must be scheduled at the IAB data centre in London, which has limited availability. Faster computation is therefore an important practical advantage.

In most cases, differences are small and marginally significant or insignificant. The only exceptions are the two productivity proxies, average wage and average worker fixed effects (AKM based), which are both higher and statistically significant in firms that will adopt WFH.

Table 14 reports a complementary test. Here, the dependent variable is the future adoption dummy, and the regressors are the firm-level characteristics considered before (only those available for the full sample). This joint test confirms the earlier results. In both specifications, productivity proxies remain the only significant predictors of future adoption, while the other covariates are virtually identical between future adopters and controls.

Overall, future treated and untreated firms appear broadly similar. This is consistent with the evidence in Section 3.1, where differences across firms decline as WFH becomes more widespread (my treated firms are all adopting after 2015).

5.3 Changes in workforce productivity

I start by examining how the firm workforce productivity evolves following the introduction of WFH. My primary outcomes are the average productivity changes of incoming and outgoing employees. I proxy productivity with the rankings of the AKM-based effects estimated over 2007–2013. This measure is time-invariant and cannot be influenced by any wage or productivity shifts resulting from WFH adoption. The analysis is confined to workers with valid AKM estimates for that period, i.e. full-time workers with at least one year of employment at any firm between 2007 and 2013. To maintain consistency, I apply this same sample restriction to all event-study outcomes in Sections 5.3 to 5.6.[16]

Figure 3 shows the changes in the average productivity of all new hires joining the firms, split by gender (Panels a and b). New hires include all workers entering the firm from unemployment, inactivity, self-employment, or other employment, in either the public or private sector. On average, I find no significant change in the productivity rank of this group

[16] Analogous results with full sample are similar to those presented and available upon request

following WFH adoption.

I then focus on the subset of new hires who transition between private-sector jobs (employer -to-employer, or EE movers). This subgroup is positively selected relative to the full set of new hires, both for women and men, based on observed labour market attachment, wage and education (see Table 25 in the Appendix), which suggests that firms may be more likely to seek high-productivity recruits from this group.

For these workers, patterns differ sharply by gender. The average productivity rank of male EE movers does not change significantly after WFH adoption. In contrast, female EE movers show a substantial increase in average productivity, reaching a point estimate of around 40 points on the 0–100 scale, which is roughly ten times the initial gap between treated and control firms for this group. When I rank workers AKM effect within their own firm, rather than against the national workforce, the pattern is largely similar (Appendix Figure 13). This suggests that after WFH adoption the average productivity of female EE workers increases also with respect to the average productivity of incumbent workers.

In the Appendix, I present estimates for the productivity changes of all new-hire subgroups, split by gender and entry origin (e.g., unemployment), and show that the productivity of other subgroups remains stable (Appendix Figure 14). As a result, the relative quality gap between female EE movers and the rest of the new hires increases up to threefold, depending on the normalisation used (Appendix Figure 15).

Next, I examine the composition of worker outflows, by gender. Panels a and b of Figure 4 report the average AKM-based rank of workers leaving the firm for another job. By focusing on job-to-job movers, I exclude anyone who simply exits the labour force. After WFH adoption, the average quality of leavers does not rise. If anything, female leavers see a small drop in productivity of leavers, although not statistically significant. Taken together, these results indicate a net positive shift in workforce quality in treated firms, driven by improved composition among female entrants and stable or declining quality among female leavers. I then calculate the average productivity of all leavers, whether they find a new job or not, before and after the policy (Appendix Figure 16). The flat pre-trends for both entrants and leavers support parallel trends and rule out anticipatory shifts in firm composition (for example i.e. introducing WFH to buffer loss of talented workers).

Finally, I test whether the observed changes in the average productivity of female EE workers are accompanied by changes in the size of this group. Panels a to d of Figure 5 show no significant change in the share of female (or male) EE hires or in the share of female (or male) workers leaving treated firms for another job, before or after WFH adoption. The post-treatment increase in the average productivity of female EE workers is therefore entirely compositional. This finding on the unchanged size of the EE subgroup is robust to alternative normalizations, such as dividing the total number of EE workers by the total number of new hires (rather than total employees in the firm) or by the firm total female headcount. [17]

A natural question is whether the observed shift in composition of EE female workers is directly related to remote work take-up. That is, are the more productive workers joining treated firms also the ones who engage in WFH ? While I cannot measure individual take-up in the event study sample, I provide suggestive evidence on this point in the next section.[18]

5.4 Are the new, highly productive workers doing WFH?

To investigate whether the highly productive female EE workers joining treated firms are also those taking up WFH, I construct a proxy for individual WFH propensity. I estimate this measure using the subsample of "always treated" firms, that is, those reporting an active WFH policy in every LPP wave in which they appear. These firms are excluded from the event study by construction, so the propensity score is estimated on a completely separate set of workers from those it is applied to. I estimate a logit model separately for each year, using the 35 two-digit occupational categories and their interaction with a university degree indicator as predictors of individual WFH take-up.

[17] Results available upon request.

[18] The employee-side of the LPP is representative of the overall workforce in the surveyed firms but not of individual firms, as it includes only a small subsample of each firm's employees. I therefore cannot determine who or which share of new or incumbent workers in a given treated firm is working remotely using the LPP employees survey.

I then test the predictive performance of this measure on workers in firms that adopt WFH during the sample period (the treatment group in the event study). The propensity score performs well in identifying actual remote workers in this sample.[19] I also experiment with alternative specifications of the logit, including 3-digit occupations, pooled-year estimation, and additional interactions with industry and local labour market. None of these alternatives outperform the occupation-by-education model in out-of-sample prediction.

Next, I use this measure to track whether the observed increase in the average quality of female EE workers is accompanied by an increase in their WFH propensity. While a stable level of propensity would not rule out take-up (e.g., if high to begin with), an increase offers stronger indirect evidence. Panel d of Figure 6 shows a sharp rise in the average WFH propensity of female EE workers after policy adoption. This shift is specific to this group: I find no comparable changes in the propensity of male EE movers or of new hires overall, including when split by gender (Panels a to c of Figure 6).

The increase in WFH propensity among female EE workers does not necessarily imply a shift in their occupational or educational composition, despite these being the variables used to construct the score. Appendix Figure 17 confirms that the education profile of this group remains quite stable, while Appendix Table 26 and 27 shows that the prevalence of WFH across occupations varies substantially over time. Since my propensity score is re-estimated each year, the observed increase is consistent with stable education and occupation characteristics in this group.[20]

To complement this evidence, I also examine changes in commuting behaviour for this group. This serves both as an indirect indication for WFH take-up and as a way to assess whether WFH adoption expands the effective labour market reach of the adopting firm. Panel a of Figure 7 shows a significant increase in the average commuting distance of female EE workers, conditional on living and working in different districts (*Kreise*). Commuting distance

[19] Prediction results available upon request.

[20] When I estimate the propensity model using only earlier years, holding constant the occupation–WFH relationship, I do not replicate the post-treatment pattern. This suggests that the increase in female EE workers' propensity is not driven by occupational shifts.

is measured as the IHS-transformed distance between the centroids of the worker's district of residence and the district where the plant is located. These estimates should be interpreted qualitatively, as they reflect centroid-to-centroid distances and do not map directly into kilometres. In fact, a back-of-the-envelope calculation suggests that the point estimate at year 3 is consistent with an actual increase in commuting distance ranging anywhere from approximately 5 to 60 kilometres. Panel b shows a marginally significant increase in the probability that female EE workers are cross-zone commuters, meaning they are more likely to reside in a district located in a different local labour market than the firm. This suggests not only longer commuting distances within commuting zones, but also a higher likelihood of commuting across zones.

This change in commuting behaviour supports the interpretation that these new entrants are indeed taking up remote positions after WFH adoption. At the same time, it points to an expansion of the firm's effective local labour market, at least for this subgroup of workers. This is consistent with the observed increase in their average quality being compositional: a broader geographical hiring pool allows the firm to choose more workers at the top of the productivity distribution.

Appendix results show no comparable changes in commuting patterns for other groups of new hires in treated firms (Figure 18).

5.5 Career implications

Figure 8 shows changes in the average characteristics of female EE workers joining treated firms. Panels a and b report their wage levels and wage growth at the time of the switch, while Panels c and d display their wage growth one year after joining and the probability of remaining with the same firm one year later. Panels e and f present, respectively, the probability of keeping the same 3-digit level occupation when switching firms and the average change in firm quality, proxied by the rank of firm wage effects in the AKM wage decomposition (2007-2013).

Female EE workers joining treated firms appear to earn higher (log) wages than female EE movers in control firms, in line with their higher productivity. Their wage growth at the time of the switch and after one year does not differ substantially from the wage growth of their counterparts in control firms (same for wage growth after 2 years, Appendix Figure 19). While not conclusive, these results suggest that highly productive female EE workers attracted by WFH are not trading off wages for flexibility, and may be maintaining their expected wage trajectories. The absence of compensating differentials may reflect the fact that firms are not (yet) fully using the amenity value of WFH to adjust wages, as suggested by Cullen, Pakzad-Hurson, and Perez-Truglia (2025), or that such wage effects appear only later in a worker's career.

These workers are also more likely to remain with the firm one year after entry, reinforcing their positive contribution to the firm's average workforce productivity over time. There is no evidence of systematic occupational change when entering the firm, at either the 2-digit or 3-digit level, and, if anything, they seem less likely to change their occupational trajectory at the transition (not significant).

Finally, Panel f shows that these workers overall seem to experience a downgrade in firm quality, as measured by the AKM firm wage rank based on 2007–2013 data.

5.6 Sorting Implications

Using AKM wage effects as a proxy for worker and firm productivity, previous results show a substantial increase in the average quality of female EE workers after WFH adoption (Figure 3, Panel d). At the same time, the average productivity of the firms from which these workers come appears higher than that of the treated firms they join (Figure 8, Panel f). Taken together, these findings suggest that WFH may play a negative mediating role in assortative matching between firms and high-productivity female workers.

To provide more direct evidence of this mechanism and quantify its magnitude, I construct a measure of distance between the observed and the perfect positive assortative matching, focusing on new hires as that is my adjustment margin. For each year, I rank both firms and all new hires based on their AKM effects (estimated in 2007-2013), obtaining two distributions. I then simulate a perfectly sorted scenario by assigning a set of workers from the worker distribution to each firm, proportional to the observed number of new hires in the firm that year. Specifically, the first-ranked firm receives the first n_1 workers in the worker ranking distribution, the second receives the next n_2, and so on, where n_k is the number of observed employees joining firm k in that year.

For each firm, I compute the average rank of its assigned workforce in the perfectly positive sorted scenario and compare it to the actual average rank of the workers it hires. The absolute value of this difference gives a firm-level measure of distance from the perfect positive assortative match. Figure 9 shows that, for female EE workers, this distance increases substantially after WFH adoption, by roughly two standard deviations. Taken together with the observed increase in the average rank of female EE workers relative to incumbent workers (Figure 13), this result confirms that the rise in the quality of newly recruited female EE workers exceeds what would be expected under perfect assortative matching.[21]

This pattern does not appear for other groups of new hires, for whom the sorting distance remains stable. The result is robust to redefining the perfectly assorted scenario using only new female hires or only EE movers.

5.7 Employment growth

As shown in Figure 5, the size of the inflow of EE workers does not change in treated firms after WFH adoption, for either male or female workers. Figure 10 provides further detail on changes in total worker flows. Panel a shows that employment grows by about 10 percent more in treated firms than in control firms. Panels b–d reveal that this growth comes entirely from higher inflows, with outflows unchanged. Female hires account for only a small share of the extra inflow; most of the increase stems from male hires. As previously shown (Panel a of Figure 3), male hires in treated firms do not appear more or less productive than the male hires in control firms, nor they differ in their average WFH propensity (Panel a of Figure 6). In Figure 11 I focus on male workers in the top and bottom of the WFH propensity distribution. The surge in male employment is replicated by male hires at the bottom of the propensity distribution (Panel a and b), while male hires at the top of the distribution remain flat (Panel

[21]If the distance is computed as $perfect - actual$ (without the absolute value), the measure decreases for the treatment group rather than increasing. This confirms that the increase in mismatch is driven by female EE workers who are "too" productive for the firms they join.

c and d). This pattern implies that the post-WFH employment boost arises from workers unlikely to use remote work.[22]

One potential explanation for these findings is task-based complementarity: increased productivity of (female) workers in WFH-compatible occupations may raise the marginal value of hiring additional (male) workers in WFH-incompatible roles. An alternative explanation relates to possible endogeneity in WFH adoption. Firms introducing WFH may simultaneously pursue broader workforce expansion strategies, aimed at improving both the quantity and quality of hires. While the rise in high-productivity female EE hires is plausibly linked to WFH availability, the increase in male hiring may reflect other firm-level adjustments. I do not observe major shifts in vacancy posting or other observable firm characteristics around the time of WFH implementation (Figure 20), but the available firm-level information on the changes implemented by the firm remains limited.

6 Conclusion

This paper investigates which firms offer remote work, which workers take it up, and how it shapes productivity sorting between firms and workers.

Using rich matched employer–employee data from Germany, I show that early adopters of WFH tend to be larger, more productive, and more female-intensive firms, but this selection weakens over time as the policy spreads across firms. At the worker level, both between and within firms, remote work is more common among higher-wage, more educated, and longer-commuting individuals, with female and part-time workers also more likely to take it up. Qualitative evidence shows that not all workers in feasible jobs are allowed to WFH, even when the policy is in place at the firm level, and that women are more likely to report being in this situation.

Motivated by cross-sectional evidence suggesting a negative role of WFH in the productivity-based

[22] I also find no change in the commuting behaviour of newly hired male workers in treated firms after WFH adoption.

sorting of firms and workers, I exploit the dynamic dimension of my data to study whether firm-level WFH adoption shifts workforce average productivity via changes in the productivity of inflows and outflows.

From the firm perspective, WFH acts as an effective recruitment tool. While it does not change the average productivity of workers hired from unemployment, inactivity, or self-employment, it does raise the quality of female job-to-job movers, a subset already more selected than other hires, resulting in a "pick-of-the-crop" effect. These workers are more productive than earlier movers, more productive than the firm current staff, and more productive than the workers the firm would get under perfect positive assortative matching. A continuous measure of sorting distance confirms a rise in mismatch between worker and firm productivity after WFH adoption for this subgroup of new hires.

Because WFH adoption is a firm-level decision that may correlate with other unobserved changes, I cannot fully claim causal identification for my findings. However, the evidence points to WFH as the likely driver of the observed changes in workforce composition. The female job-switchers entering the adopting firms have a high predicted propensity to work remotely, longer work commutes and are more likely to cross local labour markets to go to work, consistent with remote work enabling broader geographical recruitment. No similar shifts are observed for other groups, reinforcing the interpretation that the observed changes in the composition of female job-to-job transitioners are due to WFH.

From the worker perspective, the implications of WFH adoption are more nuanced. The highly productive female movers hired by adopting firms do not experience significant changes in wage trajectories in the first years, though longer-term effects cannot be ruled out. They also gain access to the amenity value of WFH, which I cannot measure directly in my data. Importantly, the available productivity measures do not allow for a clean test of whether remote work affects individual productivity. Pre-adoption AKM estimates are fixed by construction. The alternative, based on contemporaneous AKM effects (2014–2021), is wage-based and cannot separate true productivity changes from potential compensating differentials.

From a gender perspective, the evidence suggests that WFH may widen differences in how men and women sort across firms by productivity. High-productivity men keep matching with high-productivity firms, while high-productivity women seem more likely to join lower-productivity firms when they offer remote work. At the same time, the gender gap in total compensation may remain stable or even shrink, as women enjoy the amenity value of WFH and do not seem to take-up wage cuts. Finally, within firms, WFH might lead to greater dispersion in the productivity levels of female workers.

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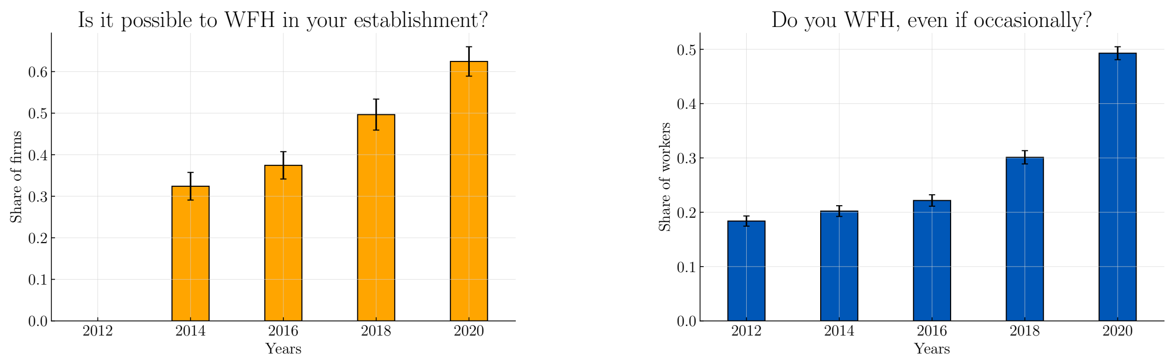
A Figures

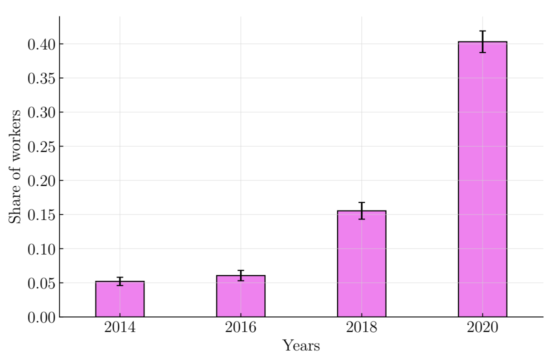
Figure 1: Trends in Employer WFH Policies and Employee Telework, Germany (2012–2020)



(a) Firms **(b) Workers**

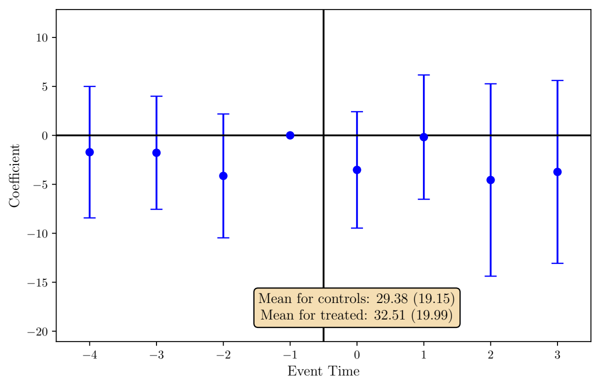
Notes: Panel a reports the percentage of firms that, in each survey wave from 2014 through 2020, indicate they have an active work-from-home policy (*Does your establishment/office allow employees to work from home?*). The sample is representative of German private-sector establishments with at least 50 employees registered for social security. Weighted tabulation. Panel b reports the percentage of employees who, in each wave from 2012 through 2020, state that they work from home for their employer (*Do you work from home for your employer – even if only occasionally?*). Sample representative of the workforce at the LPP establishments. Weighted tabulation.

Figure 2: Trends in Employee Telework, Germany (2014-2020)- Stricter teleworking definition

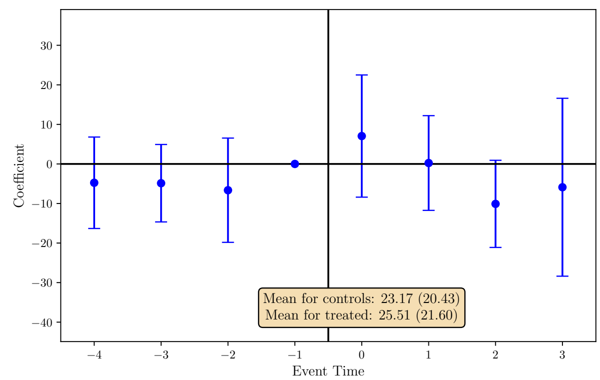


Notes: This figure shows the percentage of employees classified as teleworkers under the definition used in my empirical analysis. To qualify, employees must (i) work for a firm that permits WFH, (ii) answer "Yes" to "Do you work from home for your employer—even occasionally?", (iii) report full-day home working rather than just a few hours, and (iv) indicate that this takes place during official working hours. Respondents who answer the WFH question but fail any one of these four criteria are classified as non-teleworkers. The sample is representative of the workforce at LPP establishments. Weighted tabulation.

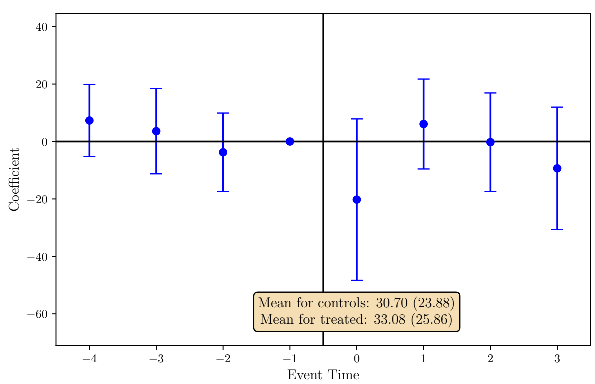
Figure 3: Event study estimates: Productivity of new workers



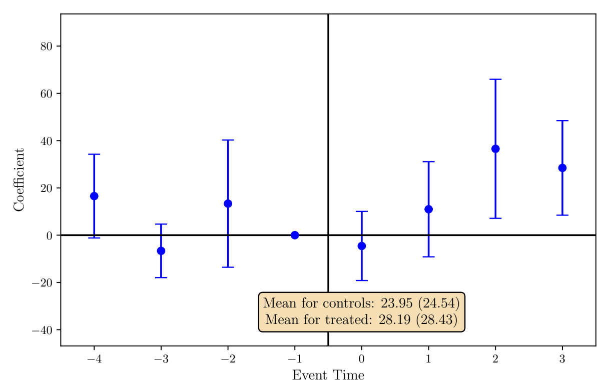
(a) Male hires



(b) Female hires



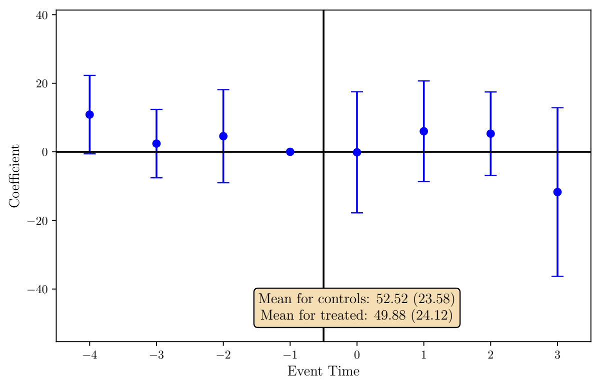
(c) EE male hires



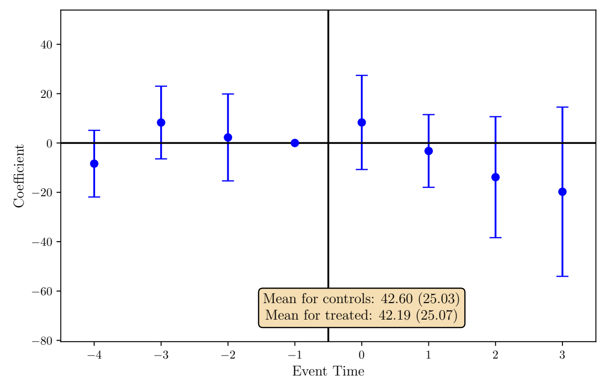
(d) EE female hires

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) average productivity of male hires; (b) average productivity of female hires; (c) average productivity of male hires moving employer-to-employer (EE); (d) average productivity of female hires moving employer-to-employer (EE). Productivity is proxied by the percentile rank (1–100) of worker wage effects, estimated with the AKM method over the period 2007–2013.

Figure 4: Event study estimates:
Productivity of workers leaving for another firm



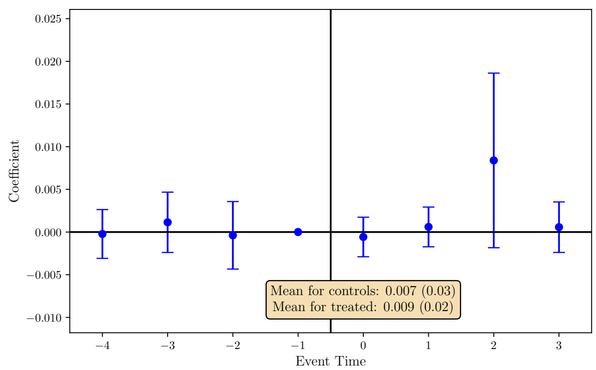
(a) Male EE leavers



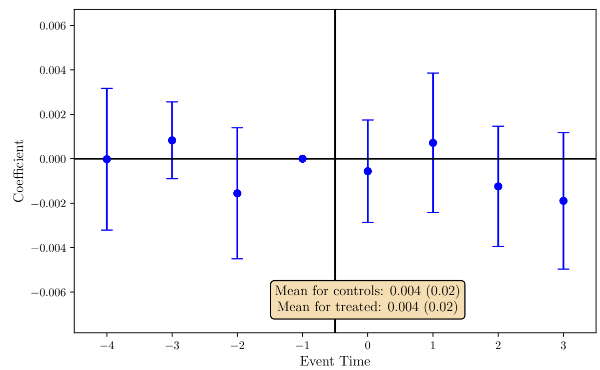
(b) Female EE leavers

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) average productivity of male workers transitioning to another firm (EE); (b) average productivity of female workers transitioning to another firm (EE). Productivity is proxied by the percentile rank (1–100) of worker wage effects, estimated with the AKM method over the period 2007–2013.

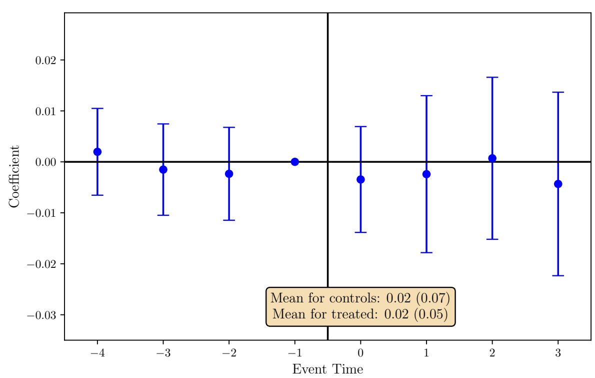
Figure 5: Event study estimates:
Size of between firms inflows and outflows



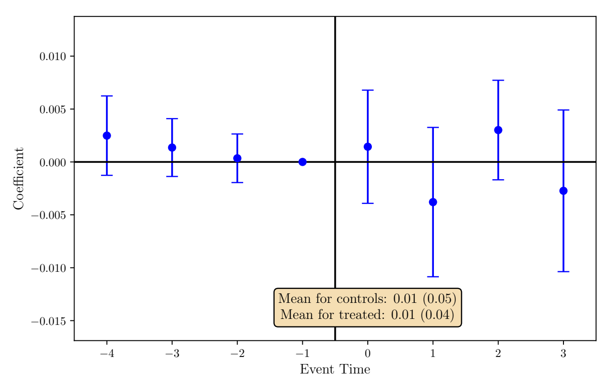
(a) EE male hires



(b) EE female hires



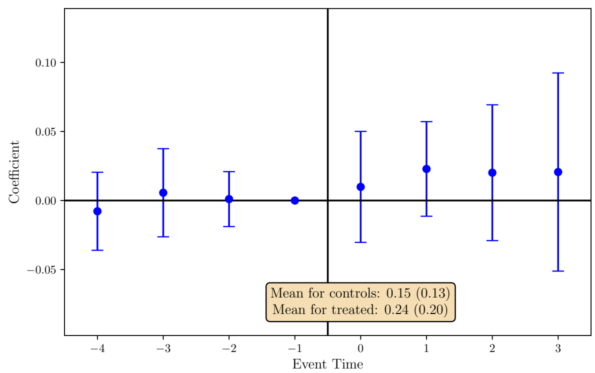
(c) EE male leavers



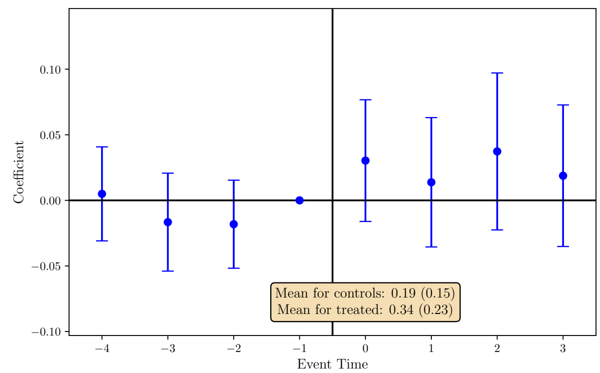
(d) EE female leavers

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) male employer-to-employer hires as a share of total firm employment; (b) female employer-to-employer hires as a share of total firm employment; (c) male employer-to-employer leaving workers as a share of total firm employment; (d) female employer-to-employer leaving workers as a share of total firm employment.

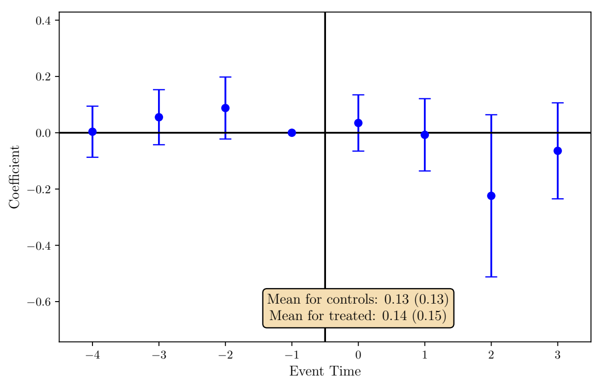
Figure 6: Event study estimates:
Propensity to WFH based on occupation, education and year of observation



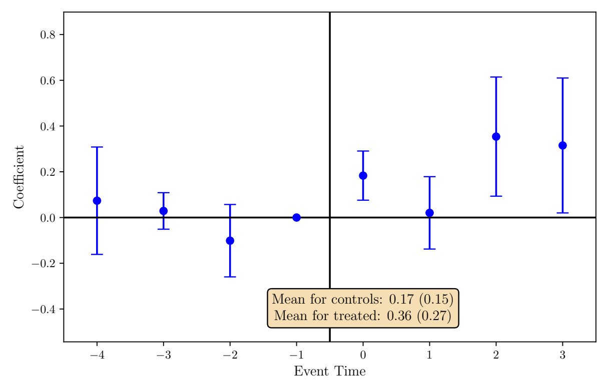
(a) Male hires



(b) Female hires



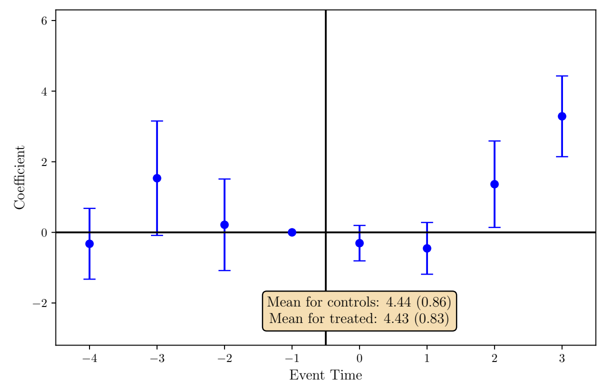
(c) EE male hires



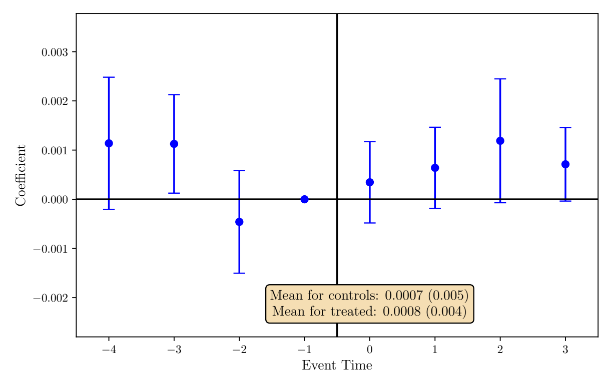
(d) EE female hires

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) WFH propensity for male hires; (b) WFH propensity for female hires; (c) WFH propensity for male hires moving employer-to-employer; (d) WFH propensity for female hires moving employer-to-employer. WFH propensity is estimated based on occupation (2 digits), occupation-by-university and year.

Figure 7: Event study estimates: Commuting distance and cross-zone status



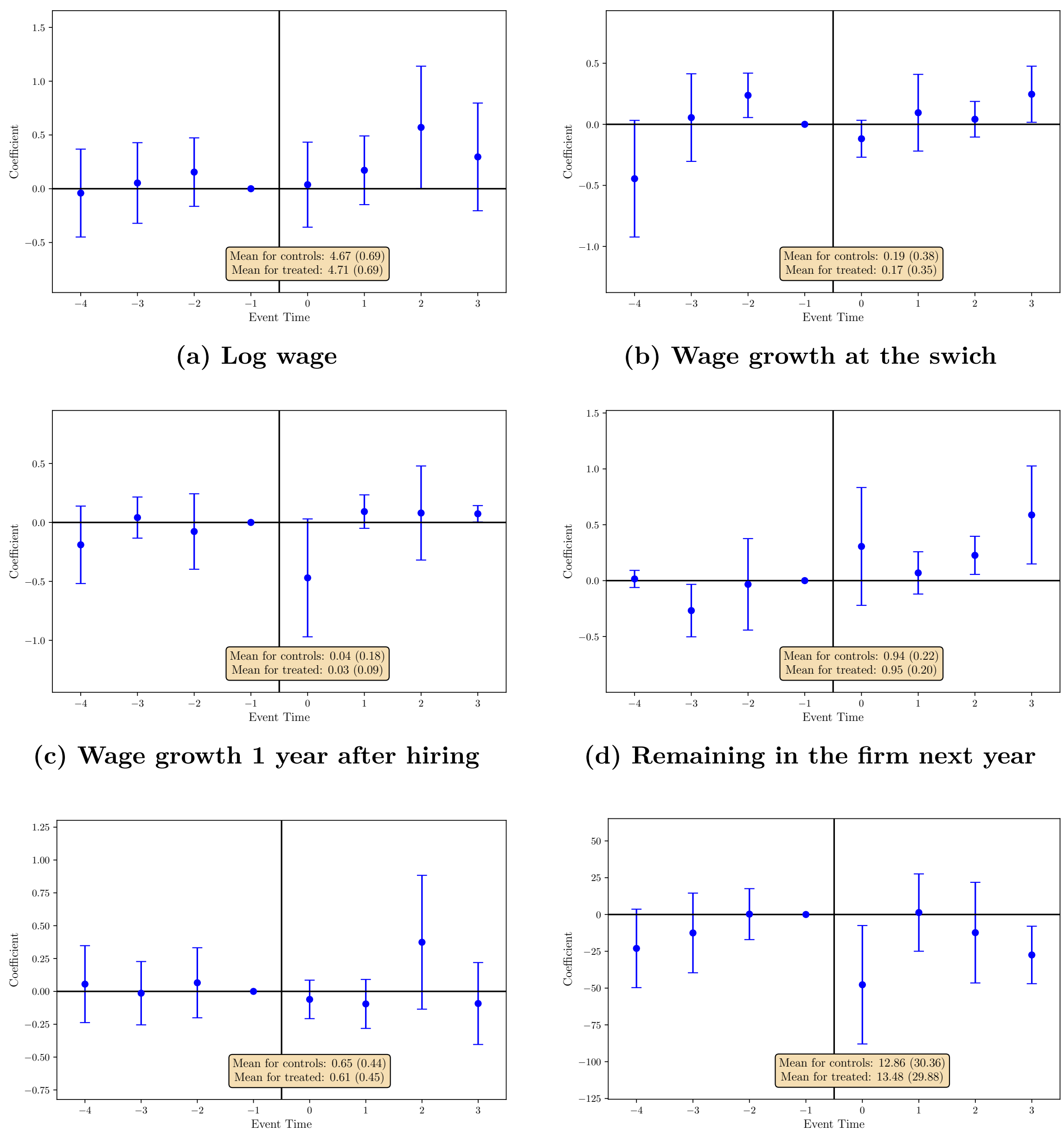
(a) Commuting distance for EE female hires



(b) Share of EE female hires commuting across llm

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the two panels refer to female hires moving employer-to-employer and are: (a) average commuting distance of female hires moving employer-to-employer, transformed with the inverse hyperbolic sine; (b) average probability to live and work in different commuting zones for female hires moving employer-to-employer. Commuting distance is computed as the distance between the centroids of the district (*Kreise*) of residence and the district where the centroid is located. There are 401 districts in Germany, divided across 141 commuting zones (local labour markets).

Figure 8: Event study estimates:
Career implications for EE female hires



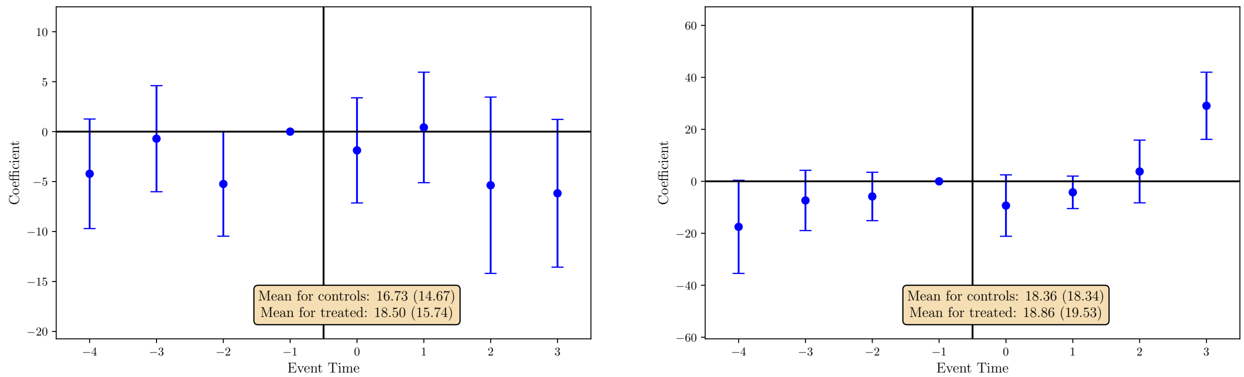
(a) Log wage **(b) Wage growth at the swich**

(c) Wage growth 1 year after hiring **(d) Remaining in the firm next year**

(e) Occupational persistence (3-digit) **(f) Firm quality: current-past**

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the six panels refer to female hires moving employer-to-employer and are: (a) average wage (log approximated using the inverse hyperbolic sine); (b) average wage growth when entering the treated firm; (c) average wage growth after 1 year in the treated firm; (d) average probability of remaining in the treated firm for one year after hiring; (e) average probability of remaining in the same (3-digits) occupation when entering the treated firm; (f) Difference between the treated firm productivity and the mean productivity of the movers' origin firms. Productivity is proxied by the percentile rank (1–100) of firms wage effects, estimated with the AKM method over the period 2007–2013.

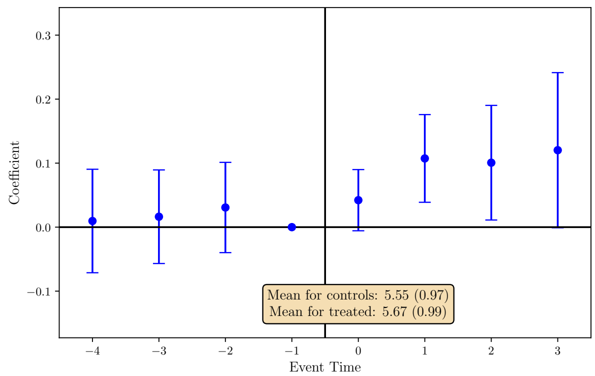
**Figure 9: Event study estimates:
Distance between actual and perfectly sorted firm-workers quality**



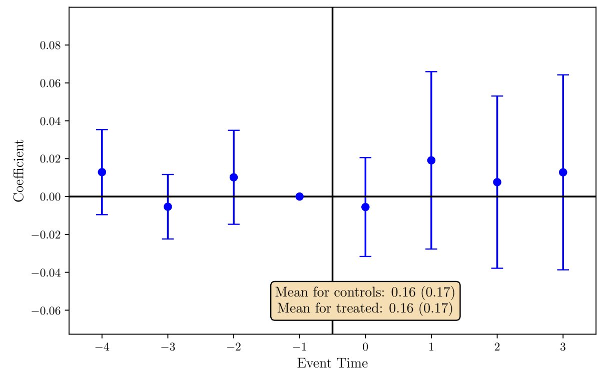
(a) All hires **(b) EE female hires**

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) average absolute distance from the perfectly assortative benchmark for all new hires; (b) average absolute distance from the perfectly assortative benchmark for female hires moving employer-to-employer. "Distance from perfect assortative matching" is defined, for each firm-year, as the absolute gap between the firm's actual average AKM rank of newly hired workers and the average rank those hires would possess under a perfectly positively sorted allocation (i.e., workers and firms matched in descending order of their AKM effects, preserving each firm's observed hiring volume).

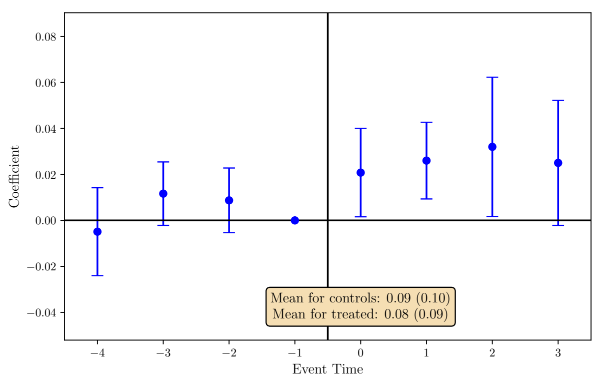
Figure 10: Event study estimates: Employment growth



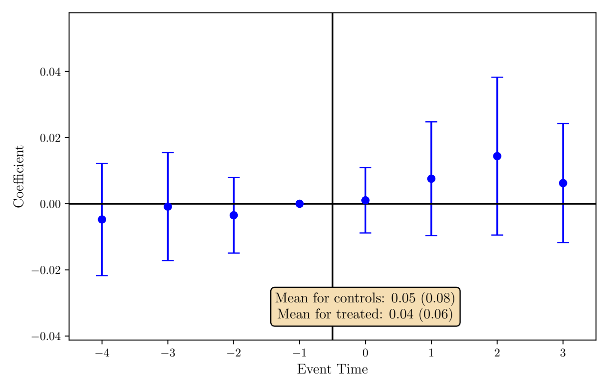
(a) Log of total employment



(b) Share of leaving workers



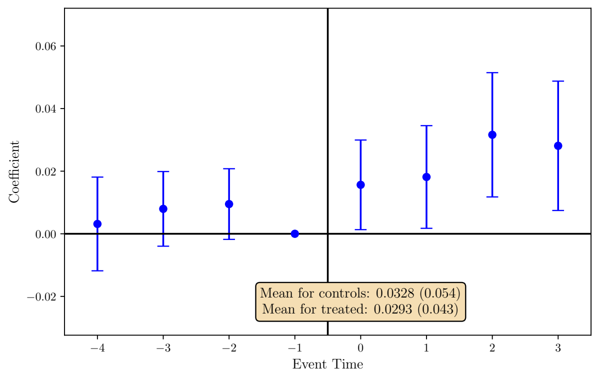
(c) Share of male hires



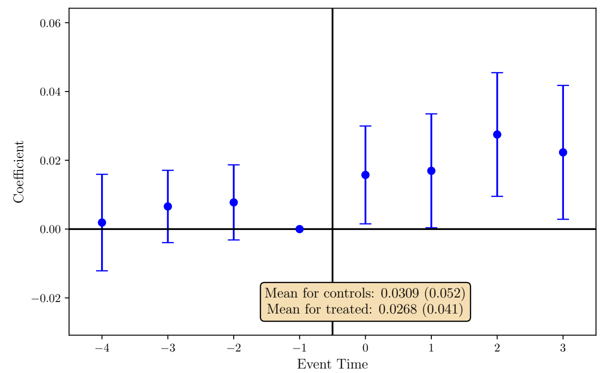
(d) Share of female hires

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) Total firm employment (log approximated with the inverse hyperbolic sine); (b) leaving workers as a share of total firm employment; (c) male hires as a share of total firm employment; (d) female hires as a share of total firm employment.

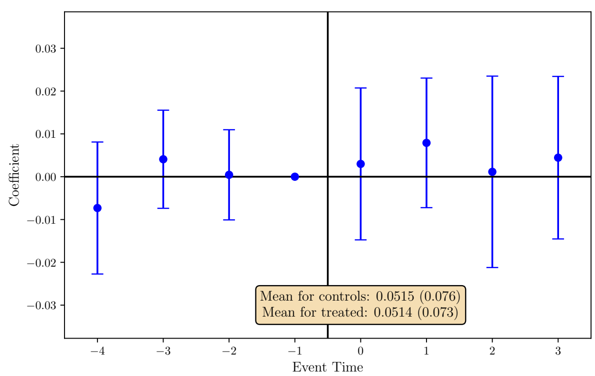
Figure 11: Event study estimates:
Male hires as a share of total firm employment, by position in the WFH-propensity distribution



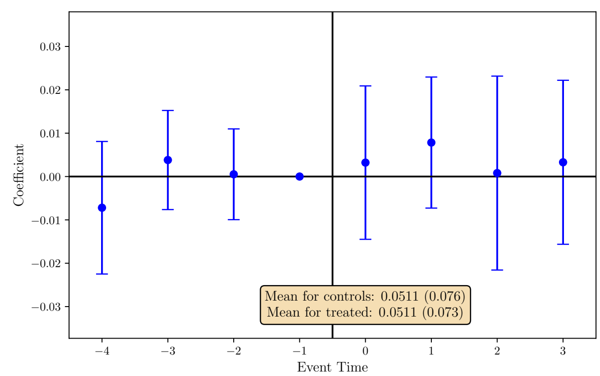
(a) Bottom tercile of WFH distribution



(b) Bottom quartile of WFH distribution



(c) Top tercile of WFH distribution



(d) Top quartile of WFH distribution

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) Male hires in bottom tercile of WFH propensity distribution as a share of total firm employment; (b) Male hires in bottom quartile of WFH propensity distribution as a share of total firm employment; (c) Male hires in top tercile of WFH propensity distribution as a share of total firm employment; (d) Male hires in top quartile of WFH propensity distribution as a share of total firm employment. WFH propensity distribution is estimated based on occupation (2 digits), occupation-by-university and year.

B Tables

Table 1: LPP Survey structure

Wave year	Employers	Employees
2012	1,208	6,592
2014	763	6,370
2016	832	6,032
2018	751	5,427
2020	723	6,791
Total	4,277	31,212

Notes: This table summarizes sample sizes for each wave of the Linked Personnel Panel (LPP) survey. The Employers column shows the number of establishments participating in the survey, which is representative of the population of German private sector establishments with at least 50 employees registered to social security. The Employees column lists the workers surveyed within those same establishments. While the number of establishments declines over time, the number of workers remains roughly constant. This reflects a survey design feature: since workers are a subsample of each establishment's workforce, attrition on the firm side is offset by increasing the number of sampled workers in the remaining establishments.

Table 2: Selection into WFH: firm-level correlations

Dependent variable: Presence of WFH policy (Mean: 0.46; SD: 0.49)

	All firms				Later adopters	
	(1)	(2)	(3)	(4)	(5)	(6)
Total workers (log)	0.032**	0.035**	0.029*	0.033*	-0.003	0.002
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
University share	0.591***	0.587***	0.714***	0.743***	-0.055	0.073
	(0.18)	(0.20)	(0.21)	(0.24)	(0.21)	(0.26)
Applications per skilled vacancy	0.031**	0.030**	0.031**	0.027*	0.034**	0.034**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Target:increase female managers	0.034	0.038	0.030	0.035	0.038	0.059*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Commuting distance	0.098***	0.103***	0.103***	0.104***	0.046***	0.051***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Part-time share	0.027	-0.206**	0.058	-0.207**	0.097	-0.053
	(0.10)	(0.10)	(0.11)	(0.10)	(0.10)	(0.09)
Female share	0.252***	0.280**	0.201***	0.264***	-0.061	-0.030
	(0.09)	(0.09)	(0.10)	(0.10)	(0.09)	(0.09)
Mean log wage	1.147***		0.807**		-0.114	
	(0.30)		(0.47)		(0.47)	
Female mean log wage			0.374		0.624*	
			(0.37)		(0.36)	
Worker rank (AKM 2007–2013)		0.003**		0.002		0.002
		(0.001)		(0.001)		(0.002)
Female worker rank				0.002*		0.002
				(0.001)		(0.001)
Firm rank (AKM 2007–2013)		0.001*		0.001*		0.001
		(0.001)		(0.001)		(0.001)
Observations	2405	2342	2382	2292	1288	1248
R-squared	0.428	0.424	0.427	0.426	0.484	0.498
Other controls	Y	Y	Y	Y	Y	Y

Notes: Linear probability model of work-from-home policy on firm characteristics. All specifications include controls for firm age, year, industry (by year) llm (by year). Firm and worker ranks are derived from the distributions of pre-estimated AKM wage-effects (2007–2013). Columns (1)–(4) include all firms in the sample, while Columns (5) and (6) are restricted to firms that adopted WFH during the observation period. Weighted regression. Standard errors clustered at the firm level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3: Selection into WFH: firm-level correlations using baseline values

Dependent variable: Presence of WFH policy (Mean: 0.46; SD: 0.49)

	Later adopters, baseline values	
	(1)	(2)
Total workers (log)	0.005	0.008
	(0.02)	(0.03)
University share	0.419	0.083
	(0.35)	(0.35)
Applications per skilled vacancy	-0.001	0.001
	(0.02)	(0.02)
Target:increase female managers	0.044	0.046
	(0.04)	(0.04)
Distance (log)	0.035	0.040*
	(0.02)	(0.02)
Part-time share	0.011	-0.119
	(0.13)	(0.11)
Female share	-0.017	0.055
	(0.11)	(0.11)
Mean log wage	0.283	
	(0.35)	
Female mean log wage	0.168	
	(0.74)	
Worker rank (AKM 2007–2013)		0.005**
		(0.001)
Firm rank (AKM 2007–2013)		-0.001
		(0.001)
Female worker rank		-0.001
		(0.001)
Observations	925	915
R-squared	0.594	0.598
Other controls	Y	Y

Notes: Linear probability model of work-from-home policy on firm characteristics. The sample includes only later adopters (firms that introduced a WFH policy during the observation period). All time-varying regressors are lagged to their first appearance in the data, and by at least two years. All specifications control for firm age, year, industry-by-year, and LLM-by-year fixed effects. Firm and worker ranks are based on the distributions of pre-estimated AKM wage-effects (2007–2013).
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 4: Firm-reported reasons to adopt working-from-home policy

Reasons	Reason (%)	Main Reason (%)
To increase flexibility for employees	67.7	23.13
To increase reconcilability of family and working life	63.7	19.94
To extend employees reachability	59.8	22.35
To increase employer attractiveness	44.1	10.80
To save commuting time	42.2	<5
To increase productivity	39.8	13.98
To provide employees a quieter working place	22.7	<5
To optimize office space usage	6.4	<5
Other	8.8	9.80
Observations: 365		

Notes: Frequency tabulation of the establishment manager's answer to the question *For what reasons do you offer the possibility of mobile working? If more than one, what is the main reason?*. Column *Reason* reports the share of firm managers selecting that option as one of the reasons to adopt. Column *Main Reason* reports the share of firm managers selecting that option as the main reason to adopt. Results based on wave 4 of LPP Employer Survey (2018). Weighted tabulation.

Table 5: Firm-reported reasons for not adopting working-from-home policy

	Respondents		*No* in 2016 but adopts in:	
	All (%)	in 2016 (%)	2018(%)	2020 (%)
Reason for No WFH adoption	**(1)**	**(2)**	**(3)**	**(4)**
Type of work not suitable	92.35	89.78	33.01	44.37
Complicates collaboration	21.02	9.34	46.18	n/a
Data protection and security	13.61	15.02	n/a	n/a
Lack of technical equipment	12.05	22.42	n/a	n/a
Complicates management	8.53	10.03	n/a	n/a
Employees not interested	< 5	< 5	n/a	n/a
Never thought about it	< 5	< 5	n/a	n/a
Observations	732	505		

Notes: Frequency tabulation of the establishment manager's answer to the question *Why is work from home not possible in your company?* asked in waves 3 (2016) and wave 5 (2020). Managers could select multiple options. Column (1) reports the share of firms selecting each option (respondents from 2016 and 2020). Column (2) restricts the sample to 2016 respondents only Respondents from 2020 cannot be shown alone for data protection. Columns (3) and (4) show the share of firms that selected the reason of the current row in 2016 and subsequently adopted WFH in 2018 and/or in 2020. Options are ordered by the values in column (1). n/a indicates results not reportable due to data protection. First row of column (3) is based on 253 observation, second row on 53. First row of column (4) is based on 139 observations. Weighted tabulation. Table results based on waves 3, 4 and 5 of LPP Employer Survey (2016, 2018, 2020).

Table 6: Worker comparisons: between and within firms

Panel A: Between firms comparison

		Firm allows to WFH	
		Yes	No
Worker in WFH	Yes	**1**	n/a
	No	excluded	**0**

Panel B: Within firms comparison

		Firm allows to WFH	
		Yes	No
Worker in WFH	Yes	**1**	n/a
	No	**0**	excluded

Notes: This table displays the worker-level comparisons used in the cross-sectional regressions reported in Table 7, 8, and 10. Panel A illustrates the between-firm comparison: workers who do WFH in firms with an active WFH policy (**1**) are compared to workers who do not do WFH in firms without such a policy (**0**). Workers who do not do WFH but are employed in firms that allow it are excluded. Panel B illustrates the within-firm comparison: workers who do WFH in firms with an active WFH policy (**1**) are compared to workers who do not do WFH within the same group of firms (**0**). Workers in firms without WFH policies are excluded.

Workers in firm without WFH policy cannot be working-form-home by my worker-level definition of working-from-home (n/a).

Table 7: WFH workers versus non-WFH workers. Between firms comparison.

Dependent variable: Dummy for worker WFH status (Mean: 0.32; SD: 0.47)

	No firm FE		Firm FE	
	(1)	(2)	(3)	(4)
Female	0.046***	0.022	0.006**	0.007*
	(0.01)	(0.01)	(0.002)	(0.003)
University degree	0.076***	0.126***	0.022****	0.039***
	(0.02)	(0.03)	(0.01)	(0.01)
Vocational training	0.028	0.061**	0.012**	0.025**
	(0.02)	(0.02)	(0.01)	(0.01)
Age	-0.001	0.008***	-0.001	0.001
	(0.00)	(0.00)	(0.00)	(0.00)
Age squared	-0.0002	-0.0002***	0.0001	-0.0001
	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	-0.001	0.001	0.0001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Commuting distance (log)	0.015***	0.017***	0.001**	0.001**
	(0.000)	(0.000)	(0.00)	(0.00)
Part-time	0.037**		0.0002	
	(0.02)		(0.001)	
Log wage	0.142***		0.012***	
	(0.02)		(0.00)	
Worker rank (AKM 2007–2013)		0.001***		0.0001**
		(0.000)		(0.000)
Adjusted R^2	0.77	0.77	0.97	0.97
Observations	8,338	6,708	8,094	6,455
Fixed effects	Y	Y	Y	Y

Notes: Linear probability models of work-from-home status on individual characteristics. All regressions include 2-digit-occupation, year, industry-by-year, and local-labour-market-by-year fixed effects. Between-firm comparison: WFH workers in WFH firms versus non-WFH workers in non-WFH firms (see Panel A in Table 6). Columns (3) and (4) add firm fixed effects. Columns (2) and (4) replace log wages with workers' wage rank, defined within the distribution of AKM wage-effects estimated for full-time workers (2007–2013). Weighted regression. Standard errors clustered at the firm level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 8: WFH workers versus non-WFH workers. Within firms comparison.

Dependent variable: Dummy for worker WFH status (Mean: 0.25; SD: 0.43)

	Baseline		Female interaction	
	(1)	(2)	(3)	(4)
Female	0.050**	0.023	-0.472*	0.103
	(0.02)	(0.03)	(0.24)	(0.07)
University degree	0.167***	0.213***	0.173***	0.211***
	(0.04)	(0.045)	(0.04)	(0.04)
Vocational training	0.0001	0.034	-0.002	0.035
	(0.03)	(0.024)	(0.03)	(0.02)
Age	-0.006	0.006	-0.005	0.007
	(0.01)	(0.006)	(0.00)	(0.01)
Age squared	0.0001	0.0001	0.0001	0.0001
	(0.00)	(0.00)	(0.00)	(0.00)
Tenure	-0.002**	-0.002**	-0.002**	-0.002**
	(0.001)	(0.001)	(0.001)	(0.00)
Commuting distance (log)	0.009*	0.008	0.010**	0.006
	(0.00)	(0.007)	(0.005)	(0.01)
Part-time	0.061*		0.070**	
	(0.03)		(0.03)	
Log wage	0.131***		0.104***	
	(0.02)		(0.03)	
Female × Log wage			0.092**	
			(0.04)	
Worker rank (AKM 2007–2013)		0.001**		0.002***
		(0.001)		(0.001)
Female × Worker rank				-0.001
				(0.00)
Adjusted R-squared	0.42	0.41	0.42	0.41
Observations	9537	7950	9537	7950
Fixed effects	Y	Y	Y	Y
Firm fixed effect	Y	Y	Y	Y

Notes: Linear probability models of work-from-home status on individual characteristics. All regressions include firm fixed effect, 2-digit-occupation, year, industry-by-year, and local-labour-market-by-year fixed effects. Within-firm comparison: WFH workers and non-WFH workers within firms with active WFH policy (see Panel b in Table 6). Columns (1) and (2) use the log wages as regressor, while Columns (3) and (4) replace them with workers' wage rank, defined within the distribution of AKM wage-effects estimated for full-time workers (2007–2013). Weighted regression. Standard clustered at the firm-worker level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 9: preferences and barriers self-reported by non-WFH workers in WFH firms

(a) Preferences for WFH (%)
Years 2014, 2016, 2018 and 2020

	Men	Women
Would like to WFH	13.70	14.69
Observations	5,760	1,957

(b) Reasons for not WFH (%)
Years 2014, 2016 and 2020

	Men	Women	Difference
Job unfeasible	79.38	66.18	12.85***
Obs	3,434	1,085	4,519
Demand related			
Wants to separate private/professional life	47.93	42.43	5.70
Obs	3,426	1,081	4,507
Collaboration with colleagues difficult	46.21	45.83	-0.38
Obs	3,412	1,081	4,493
Concerned about promotion opportunities	5.29	4.16	1.13
Obs	3,411	1,082	4,493
Supply related			
Supervisor does not like	52.60	50.33	2.26
Obs	3,398	1,071	4,469
No technical requirements	49.88	38.88	11.01**
Obs	3,431	1,083	4,514
Not allowed but feasible	10.90	18.03	-7.13***
Obs	3,434	1,078	4,512

Notes: (a) Share of non-WFH workers in WFH firms report to desire WFH on a regular basis (*Would you like to work from home? [...] On a regular or an occasional basis?* Coded as 0 if *No* or *Yes, occasionally*, coded as 1 if *Yes, regularly*). Information based on wave 2 to 5 (2014, 2016, 2018, 2020) of the LPP Employee Survey. (b) Self-reported reasons for not working from home among workers in WFH firms (*What are the reasons why you are not working from home?*); respondents could select multiple items. Percentages refer to the share of each gender not currently WFH who cites that reason. Observations shown below each percentage. T-test for the difference in mean between men and women reported in the last column. Categorization in *supply* and *demand* related reasons done by author. Information based on wave 2 (2014) and wave 3 (2016) of the LPP Employee Survey. Weighted tabulation.

Table 10: Mediating role of WFH in firm-workers assortative matching
Firm and worker AKM wage-effect ranks from 2007–2013

	Dependent Variable: Firm rank		
	All	Men	Women
WFH	12.456***	11.809***	14.521***
	(2.74)	(3.00)	(4.47)
Worker rank	0.080***	0.055***	0.121***
	(0.02)	(0.02)	(0.04)
Worker rank × WFH	-0.064**	-0.053	-0.132***
	(0.03)	(0.03)	(0.05)
R-squared	0.70	0.70	0.72
Observations	6684	5352	1253
Mean of dependent variable	72.18	74.36	63.88
SD	(25.71)	(24.65)	(27.84)
Controls and FE	Y	Y	Y

Notes: Linear probability model regressing firm wage-effect rank on worker wage-effect rank, a work-from-home (WFH) dummy, and their interaction. Wage-effects are pre-estimated over years 2007–2013 using the AKM methodology. Worker level regression. The interaction term should capture the role of WFH in firm worker assortative matching based on their productivies. Between firm specification: comparison of WFH workers in WFH firms with non-WFH workers in non-WFH firms (see Panel A in Table 6). Controls include sex, education (university and vocational training dummies), age, age squared, tenure, log distance between residence and firm, and fixed effects for year, industry, industry-by-year, local labour market, local labour market-by-year, and 2-digit-occupation. Column 1 reports estimates for the full sample; Columns 2 and 3 report results separately for men and women. Weighted regression. Standard errors clustered at the firm-worker level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.
Sum of female plus male observations does not adds up to total because of a few singleton observations in the specified FE cells

Table 11: Treatment and Control Group Size By Year

	2014	2015	2016	2017	2018	2019	2020	2021	**Total**
Control	500	497	473	464	313	301	196	187	2,931
Treatment	0	0	74	74	136	133	143	140	700

Notes: Sample size of the control and treatment groups over time. The number of observations varies by year because of attrition from the LPP survey, not firm selection. “Total” column reports the cumulative number of observations over the entire period. Firm level tabulation.

Table 12: Treated Firms by Event Time

	-5	-4	-3	-2	-1	0	1	2	3	4	5
Treated firms	162	176	175	226	225	225	225	91	89	26	26

Notes: Number of treated firms by event time. The sample size varies across event years due to staggered implementation and for attrition of firms in the LPP (not firm selection). Event time is measured relative to the treatment year, with 0 indicating the year of treatment. Only firms with at least 1 year of pre-trends are included.

Table 13: Balancing test 1: difference in characteristics of future treated and control firms

$$Y_j \ = \ \beta_0 \ + \ \beta_1 \ Treat_j \ + \ \text{FEs} \ + \ \varepsilon_j \tag{2}$$

Dependent variable (Y_j)	(1) Constant (β_0)	(2) *Treat* (β_1)	N
Women share	0.444***	-0.034*	855
	(0.06)	(0.02)	
Uni degree sh.	0.096**	0.014	855
	(0.04)	(0.01)	
Voc. training sh.	0.836***	-0.014	855
	(0.05)	(0.01)	
Part-time share	0.195***	-0.035	855
	(0.07)	(0.02)	
Average age	45.393***	0.447	855
	(1.46)	(0.35)	
Tenure (years)	5.873***	0.861**	855
	(1.12)	(0.34)	
Av. dist. work-home	2.034***	0.117	855
	(0.25)	(0.08)	
Average wage (log)	2.263***	0.021***	855
	(0.03)	(0.01)	
Workers rank (AKM)	31.553***	3.193***	849
	(4.92)	(1.19)	
Firm rank (AKM)	20.037***	3.257*	843
	(5.10)	(1.70)	
New hires share	0.216***	-0.020**	855
	(0.04)	(0.01)	
Leaving workers sh.	0.395***	-0.027	855
	(0.06)	(0.01)	
Firm age (years)	12.813***	1.502*	855
	(2.23)	(0.86)	
Profit change (%)	2.537	0.777	635
	(2.89)	(0.97)	
Total employment (log)	5.483***	0.141*	876
	(0.04)	(0.08)	

Notes: The table reports the results from a series OLS regressions comparing future treated firms and control firms in 2015, last year before treatment starts being adopted (Model 2). Each row corresponds to a different regression where the dependent variable is a firm characteristic, and the main regressor is a dummy variable equal to one if the firm adopts working from home (WFH) at any point after 2015. All regressions include controls for industry, local labour market fixed effects and size in employment terms (except last row where size is the dependent variable). Column (1) reports the estimated constant in each regression, capturing the average value of the dependent variable for controls. Column (2) reports the coefficient on the future treatment dummy, capturing the average difference in the firm characteristic between future adopters and never adopters, conditional on the included controls. Standard errors are clustered at the firm level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 14: Balancing test 2: Joint significance of the differences in characteristics of future treated and control firms

	Dependent variable	
	Future treatment dummy	
	(1)	(2)
Women share	-0.054	-0.055
	(0.09)	(0.10)
Uni degree sh.	-0.337	-0.226
	(0.24)	(0.28)
Voc. training sh.	-0.307	-0.252
	(0.20)	(0.21)
Part-time share	0.030	-0.173*
	(0.10)	(0.10)
Average age	-0.045	-0.004
	(0.04)	(0.05)
Age squared	0.001	0.0002
	(0.00)	(0.00)
Av. dist. work–home	0.028	0.023
	(0.02)	(0.02)
Average wage (log)	0.812**	
	(0.34)	
Total employment (log)	0.010	0.029
	(0.02)	(0.02)
New hires share	-0.063	0.072
	(0.14)	(0.19)
Leaving workers sh.	-0.076	-0.207
	(0.12)	(0.13)
Firm age (years)	0.002	0.001
	(0.00)	(0.00)
Workers rank (AKM)		0.003*
		(0.00)
Firm rank (AKM)		-0.001
		(0.00)
Observations	855	839
R^2	0.194	0.196
Mean dep. var.	0.26	0.26
Fixed effects	Yes	Yes

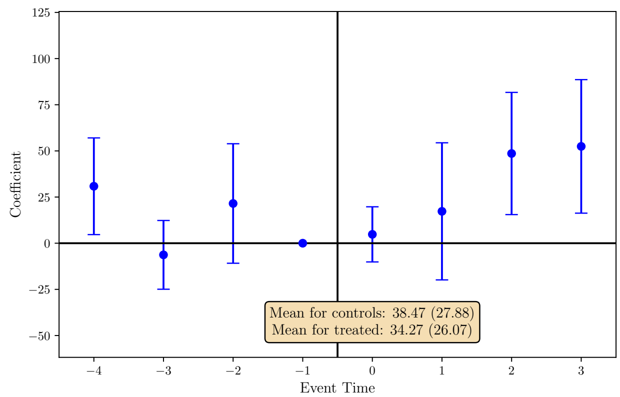
Notes: The table reports the results from two OLS regressions where the dependent variable is a dummy equal to one if the firm adopts working from home WFH at any point after 2015 (future treated firms). Only observations from 2015 are included. The regressors include all firm-level characteristics used in Table 13 (when available for the full sample). Column (1) uses average firm wage as the proxy for productivity; column (2) replaces it with the average worker and firm ranks of the fixed effects from an AKM wage decomposition. Both specifications control for industry and local labour market fixed effects. Standard errors are clustered at firm level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

C Appendix Figures

Figure 12: WFH–RTO Conflict: Field Evidence

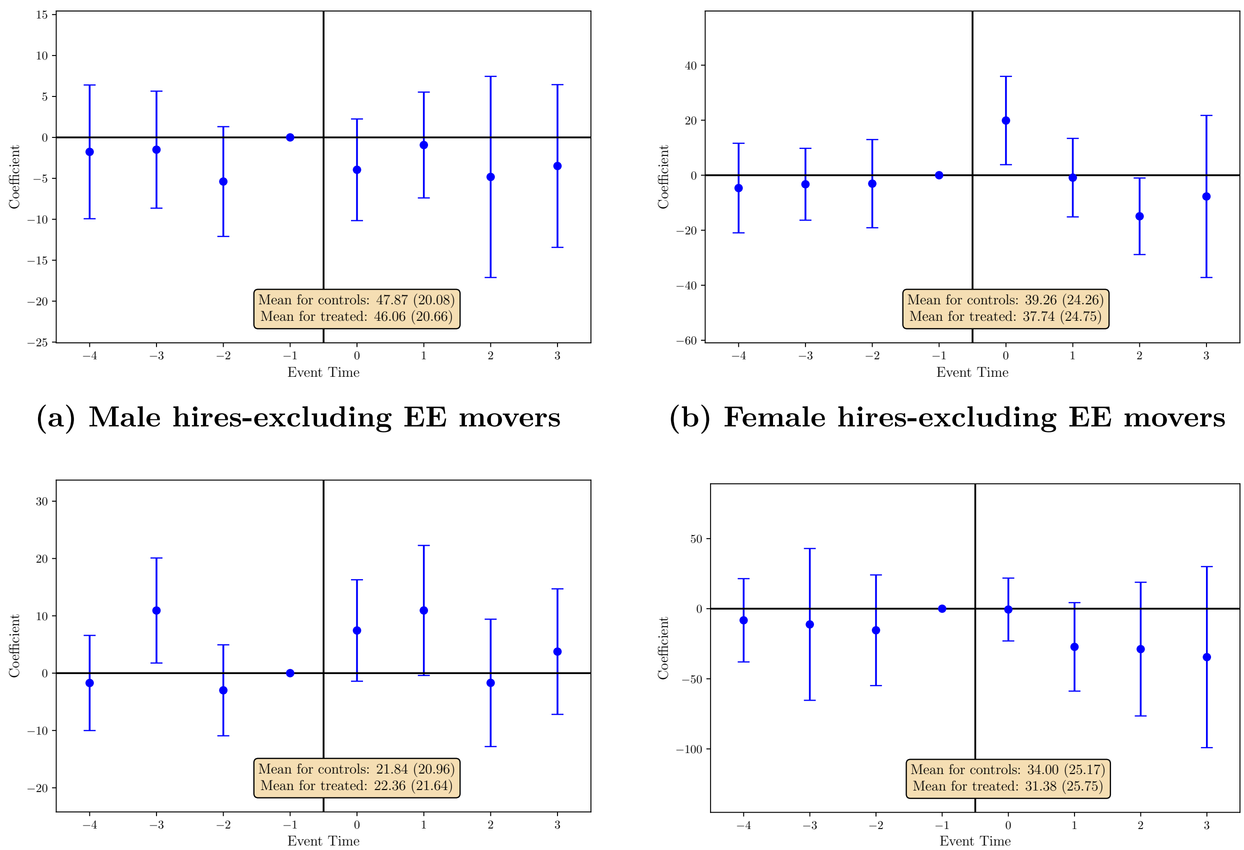
Notes: Street advertisement spotted by the author in Shoreditch, London (2023). Despite appearances, *Belcor* is neither a return-to-office consultancy nor a corporate task force commissioned by Elon Musk. It is, in fact, a local office-space letting agency

Figure 13: Event study estimates:
Average productivity of female EE workers scaled on incumbent workforce productivity



Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variable is the average firm-relative productivity of female hires transitioning job-to-job. Productivity is proxied by the percentile rank (1–100) of worker wage effects, estimated with the AKM method over the period 2007–2013. Firm-relative means that the percentile ranks are re-scaled within the destination firm's workforce (i.e. relative to incumbent coworkers rather than the national workforce).

Figure 14: Event study estimates:
Changes in average productivity new workers, by gender

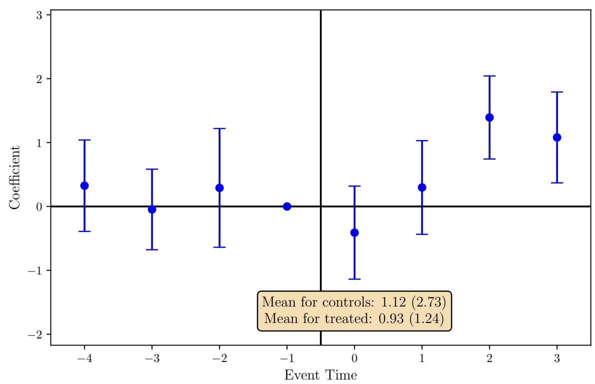


(a) Male hires-excluding EE movers **(b) Female hires-excluding EE movers**

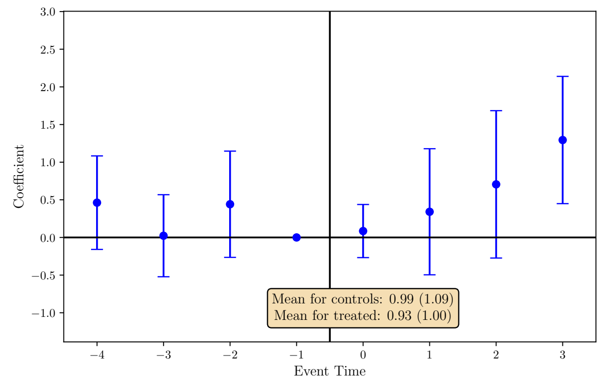
(c) Male hires from unemployment **(d) Female hires from unemployment**

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) average productivity of male hires excluding employer-to-employer movers; (b) average productivity of female hires excluding employer-to-employer movers; (c) average productivity of male hires coming from unemployment; (d) average productivity of female hires coming from unemployment. Productivity is proxied by the percentile rank (1–100) of worker wage effects, estimated with the AKM method over the period 2007–2013.

Figure 15: Event study estimates:
Productivity ratio: female EE over all hires



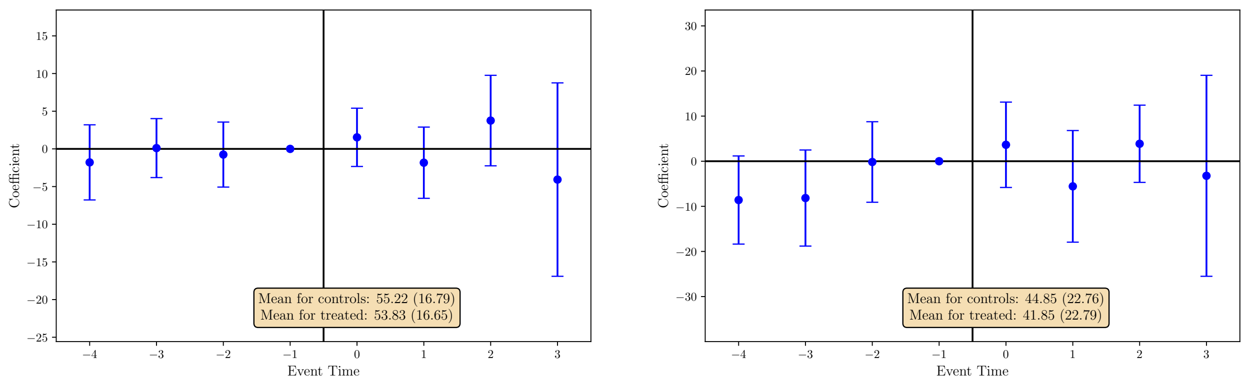
(a) Absolute ratio



(b) Firm-relative ratio

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) average productivity of female employer-to-employer movers over average productivity of all new hires; (b) average firm-relative productivity of female employer-to-employer movers over average firm-relative productivity of all new hires. Productivity is proxied by the percentile rank (1–100) of worker wage effects, estimated with the AKM method over the period 2007–2013. Firm-relative means that the percentile ranks are re-scaled within the destination firm's workforce (i.e. relative to incumbent coworkers rather than the national workforce).

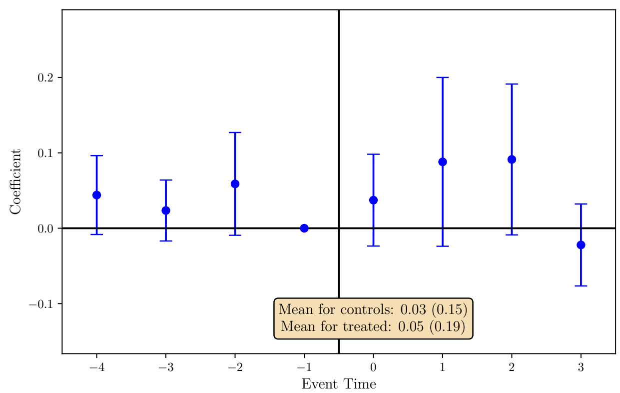
Figure 16: Event study estimates:
Productivity ranking of workers leaving the firm



(a) Male workers **(b) Female workers**

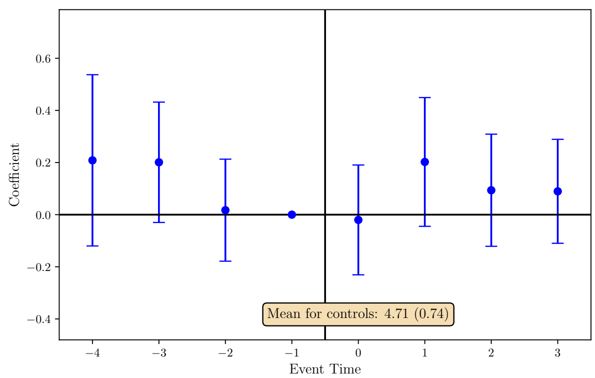
Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) average productivity of all male leavers; (b) average productivity of all female leavers. Productivity is proxied by the percentile rank (1–100) of worker wage effects, estimated with the AKM method over the period 2007–2013.

Figure 17: Event study estimates:
Share of workers with University education among female EE workers

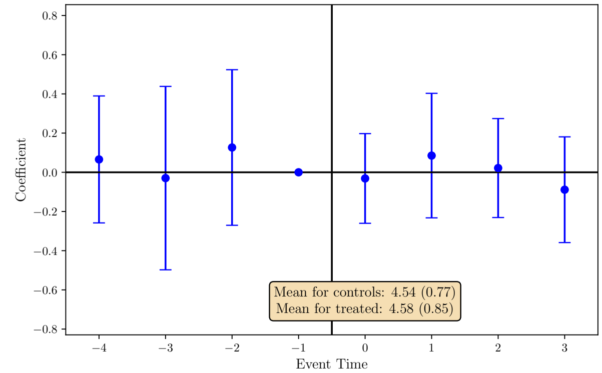


Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variable is the share of female hires transitioning employer-to-employer who hold a university degree.

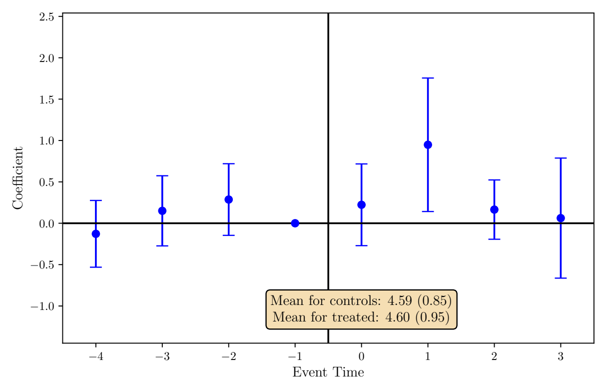
Figure 18: Event study estimates:
Commuting distance and cross-zone status for new hires and male EE



(a) Commuting distance for male hires



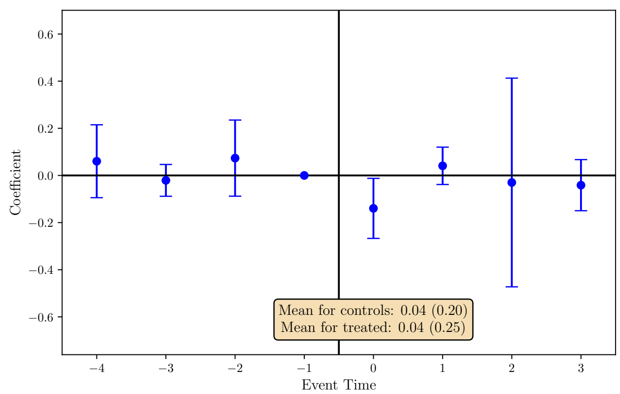
(b) Commuting distance for female hires



(c) Commuting distance for male EE

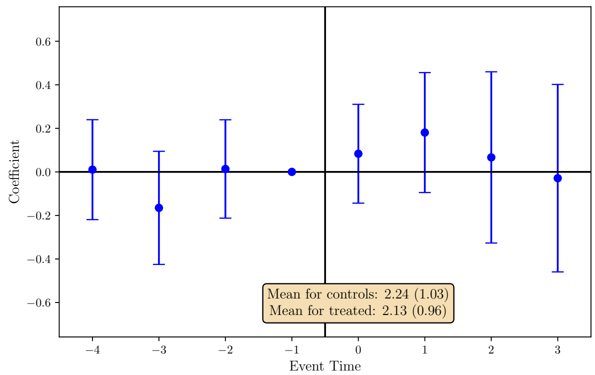
Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) average commuting distance for male hires; (b) average commuting distance for female hires; (c) average commuting distance for male hires moving employer-to-employer. Productivity is proxied by the percentile rank (1–100) of worker wage effects, estimated with the AKM method over the period 2007–2013.

**Figure 19: Event study estimates:
Wage growth 2 years after hiring for female EE**

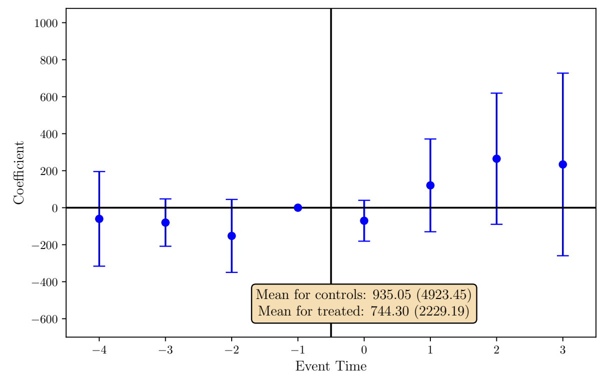


Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variable is wage growth after 2 years in the firm for female hires moving employer-to-employer. Productivity is proxied by the percentile rank (1–100) of worker wage effects, estimated with the AKM method over the period 2007–2013.

**Figure 20: Event study estimates:
Vacancies and profits**



(a) Log of vacancies



(b) Profit (euros, tens of thousands)

Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating equation 1 using the sample of firms who switch from not allowing to allowing employees to work remotely (treated group) and the sample of firms who keep declaring not to allow their employees to do so (control group). Estimates are performed with the local-projection difference-in-difference estimator (Dube et al., 2023), which accounts for the pitfalls of the two-way fixed-effects estimator (De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021). The coefficients are estimated on a balanced sample between -1 and +1. The coefficients are plotted relative to the difference between the treated and control group the year before the switch from not allowing to allowing, which is normalized to zero. Standard errors are clustered at the establishment level. Dependent variables reported in the four panels are: (a) log of vacancies normalized on firm employment; (b) firms profits (total revenues minus intermediates, labour costs, and capital expenses).

D Appendix Tables

Table 15: Panel structure of the LPP survey

	Firms		Workers	
Survey Participation	Count	Share (%)	Count	Share (%)
First time in survey	2,129	49.8	17,750	56.9
Second time in survey	1,058	24.7	7,740	24.8
Third time in survey	597	14.0	3,463	11.1
Fourth time in survey	340	8.0	1,619	5.2
Fifth time in survey	153	3.6	640	2.1
Total	4,277	100.0	31,212	100.0

Notes: The table reports the number and share of firms and workers by how many times they participated in the Linked Personnel Panel (LPP) survey. Repeated participation reflects the panel structure of the survey, with up to five appearances possible across (five) waves. Shares are expressed as a percentage of the total number of participating firms or workers.

Table 16: Number of survey participations by workers and wave year in the LPP

	Wave year					
Survey Participation	2012	2014	2016	2018	2020	**Total**
First time in survey	6,592	3,433	2,172	2,504	3,049	17,750
Second time in survey	/	2,937	2,121	1,035	1,647	7,740
Third time in survey	/	/	1,739	983	741	3,463
Fourth time in survey	/	/	/	905	714	1,619
Fifth time in survey	/	/	/	/	640	640
Total	6,592	6,370	6,032	5,427	6,791	31,212

Notes: The table reports the number of employees in the LPP survey by wave year and number of participations. Each row corresponds to workers grouped by how many times they took part in the survey, and columns indicate when these participations occurred. The table reflects the rotating panel structure of the LPP, with a mix of new entrants and repeat respondents in each wave (the equivalent tabulation for the firm side cannot be reproduced due to data protection restrictions).

D.1 Financial indicators and WFH policy: firm level correlations

Table 17 explores contemporaneous correlations between firm-level WFH presence and establishment financial indicators. The two columns replicate the specifications from Columns 1 and 2 of Table 2, using the same set of regressors while additionally including firm-level financial indicators data. The results confirm a positive association between WFH adoption and average wages, female workforce share, and average commuting distance. The female-specific average wage also becomes significant in this specification. Firm size is no longer significant when measured by employment but it is when measured by revenues. Furthermore, WFH presence is positively associated with capital share, which I define as the ratio of total revenues minus intermediate materials to total cost of capital. Surprisingly, presence of the policy is negatively correlated with the presence of work and staff councils at the firm level.

Table 17: Financial indicators

Dependent variable: Presence of WFH policy (Mean: 0.46; SD: 0.49)

	All firms	
	(1)	(2)
Total Revenues (log)	0.068**	0.068**
	(0.03)	(0.03)
Capital share	0.0001***	0.0001***
	(0.00)	(0.00)
Labour share	0.002	0.001
	(0.00)	(0.00)
Profit performance: decreased	0.014	0.015
	(0.04)	(0.04)
Profit performance: increased	-0.034	-0.037
	(0.04)	(0.04)
Work/Staff councils	-0.122***	-0.129***
	(0.04)	(0.05)
Female share	0.419***	0.276*
	(0.12)	(0.15)
Mean log wage	1.600***	0.848
	(0.49)	(0.66)
Female mean log wage		0.912*
		(0.47)
Target: increase female managers	0.031	0.033
	(0.04)	(0.04)
Applications per skilled vacancy	0.008	0.008
	(0.02)	(0.02)
Part-time share	0.005	0.133
	(0.16)	(0.18)
University share	0.397	0.467
	(0.26)	(0.32)
Total workers (log)	-0.005	-0.021
	(0.03)	(0.03)
Distance (log)	0.088***	0.084***
	(0.02)	(0.02)
Observations	1220	1208
R-squared	0.554	0.557
Other controls	Y	Y

Notes: *Notes*: Linear probability model of work-from-home policy on firm characteristics, including financial indicators. Both specifications include controls for firm age, year, industry (by year) llm (by year). Column (2) insert also the covariate for the average wage of female workers.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 18: Selection into WFH: firm-level correlations
AKM wage effects from 2014-2021

Dependent variable: Presence of WFH policy (Mean: 0.46; SD: 0.49)

	All firms		Later adopters *contemp.*	*baseline*
	(1)	(2)	(3)	(4)
Total workers (log)	0.028*	0.031*	-0.012	0.001
	(0.02)	(0.02)	(0.02)	(0.03)
University share	0.501**	0.667**	-0.009	0.078
	(0.23)	(0.26)	(0.26)	(0.37)
Applications per skilled vacancy	0.028**	0.027**	0.030**	-0.001
	(0.01)	(0.01)	(0.01)	(0.02)
Target:increase female managers	0.027	0.010	0.046	0.047
	(0.03)	(0.03)	(0.03)	(0.03)
Distance (log)	0.086***	0.094***	0.054***	0.040*
	(0.02)	(0.02)	(0.02)	(0.02)
Part-time share	-0.259**	-0.278***	-0.045	-0.101
	(0.10)	(0.11)	(0.09)	(0.11)
Female share	0.318***	0.326***	0.001	0.046
	(0.11)	(0.11)	(0.09)	(0.11)
Worker rank (AKM 2014–2021)	0.005***	0.002	0.001	0.002
	(0.00)	(0.00)	(0.00)	(0.001)
Firm rank (AKM 2014–2021)	0.001	0.001	0.001	-0.0001
	(0.00)	(0.00)	(0.00)	(0.001)
Female worker rank		0.004***	0.004**	0.001
		(0.00)	(0.00)	(0.001)
Observations	2487	2440	1262	917
R-squared	0.302	0.309	0.497	0.596
Other controls	Y	Y	Y	Y

Notes: Linear probability model of work-from-home policy on firm characteristics. All specifications include controls for firm age, year, industry (by year) llm (by year). Firm and worker ranks are derived from the distributions of contemporaneously estimated AKM wage-effects (2014–2021). Columns (1)–(2) include all firms in the sample, while Columns (3) and (4) are restricted to firms that adopted WFH during the observation period, with Column (4) using the lagged values for all the covariates rather. Weighted regression. Standard errors clustered at the firm level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 19: WFH workers versus non-WFH workers. Between firms comparison.
Specifications with interactions

Dependent variable: Dummy for worker WFH status (Mean: 0.32; SD: 0.47)

	No firm FE		Firm FE	
	(1)	(2)	(3)	(4)
Female	0.033	0.016	0.018	0.009*
	(0.08)	(0.02)	(0.02)	(0.001)
University degree	0.066***	0.126***	0.022****	0.038***
	(0.02)	(0.03)	(0.01)	(0.01)
Vocational training	0.026	0.061**	0.012**	0.025**
	(0.02)	(0.02)	(0.01)	(0.01)
Age	-0.000	0.008***	-0.001	0.000
	(0.00)	(0.00)	(0.00)	(0.00)
Age squared	-0.0001	-0.0001****	0.0001	-0.0001
	(0.00)	(0.00)	(0.00)	(0.00)
Tenure	-0.0002	0.0002	0.0001	0.0001
	(0.00)	(0.00)	(0.00)	(0.00)
Commuting distance (log)	0.014****	0.016***	0.001	0.001*
	(0.00)	(0.00)	(0.00)	(0.00)
Part-time	0.039**		0.001	
	(0.02)		(0.00)	
Log wage	0.138****		0.012***	
	(0.02)		(0.00)	
Female × Log wage	0.002		-0.001	
	(0.02)		(0.00)	
Worker rank (AKM 2007–2013)		0.001***		0.0001**
		(0.00)		(0.00)
Female × Worker rank		0.0001		-0.0001
		(0.00)		(0.00)
Adjusted R^2	0.77	0.77	0.97	0.97
Observations	8,094	6,455	8,094	6,455
Fixed effects	Y	Y	Y	Y

Notes: Linear probability models of work-from-home status on individual characteristics. All regressions include 2-digit-occupation, year, industry-by-year, and local-labour-market-by-year fixed effects. Between-firm comparison: WFH workers in WFH firms versus non-WFH workers in non-WFH firms (see Panel A in Table 6). Columns (3) and (4) add firm fixed effects. Columns (2) and (4) replace log wages with workers' wage rank, defined within the distribution of AKM wage-effects estimated for full-time workers (2007–2013). All columns feature the interaction of the *Female* dummy with wages or wage ranks. Weighted regression. Standard errors clustered at the firm level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 20: WFH workers versus non-WFH workers. Between firms comparison. Parental status specification.

Dependent variable: Dummy for worker WFH status (Mean: 0.32; SD: 0.47)

	(1)	(2)	(3)
Female	0.043***	0.042***	0.028
	(0.01)	(0.01)	(0.02)
Parent	0.010	0.010	0.003
	(0.01)	(0.01)	(0.00)
Mother	0.024	0.122	-0.029
	(0.02)	(0.15)	(0.04)
University degree	0.075***	0.062***	0.023***
	(0.02)	(0.02)	(0.01)
Vocational training	0.028	0.026	0.012**
	(0.02)	(0.02)	(0.01)
Age	-0.002	-0.002	-0.001
	(0.00)	(0.00)	(0.00)
Age squared	0.0002	0.0002	0.0001
	(0.00)	(0.00)	(0.00)
Tenure	-0.001	-0.001	0.0001
	(0.00)	(0.00)	(0.00)
Log wage	0.144***	0.145***	0.011**
	(0.02)	(0.02)	(0.00)
Part-time	0.033**	0.031**	0.001
	(0.01)	(0.01)	(0.00)
Commuting distance (log)	0.015***	0.014***	0.001**
	(0.00)	(0.00)	(0.00)
Female × Log wage		-0.002	-0.004
		(0.00)	(0.00)
Mother × Log wage		-0.029	0.009
		(0.03)	(0.01)
Adjusted R-squared	0.77	0.77	0.97
Observations	8338	8338	8094
Fixed effects	Y	Y	Y
Firm fixed effect	N	N	Y

Notes: Linear probability model regressing dummy for work from home status on individual characteristics. All specifications include controls for 2-digit occupation, year, industry (by year), llm (by year), a dummy for parental status and a dummy for mothers. *Parent* equals one if the worker lives with at least one child younger than 14. *Mother* is the interaction of *Female* and *Parent*. Between firms comparison: WFH workers in WFH firms versus non-WFH workers in non-WFH firms (see Panel a in Table 6). Columns (2) introduced the interaction of *Female* and *Mother* dummies with the (log) wages. Columns (3) repeats Column (2) specification inserting firm fixed effect. . Weighted regression. Standard errors clustered at the firm-worker level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 21: WFH entitlement and Usage by Management Status

	Management	**Non-management**
Entitled	52%	22%
Users	31%	10%
Observations	191	

Notes: Share of employees entitled to WFH and share actually using WFH, separately for management and non-management employees. Questions: *Referring to the employees with and without managerial responsibility, what is the percentage of employees in each group that could make use of this opportunity? (entitled employees)* and *Referring to all entitled employees with and without managerial responsibility, what is the percentage of employees in each group that actually use this opportunity? (actual users)* Observations refer to establishments with valid responses. Wave 2 of the LPP Employer Survey (2014). Weighted tabulation.

Table 22: WFH workers versus non-WFH workers. Within firms comparison. Education and parenatl status specifications.

Dependent variable: Dummy for worker WFH status status (Mean: 0.25; SD: 0.43)

	Educ.	Parent specifications		
	(1)	(2)	(3)	(4)
Female	0.024	0.050**	-0.481**	0.155
	(0.02)	(0.02)	(0.21)	(0.10)
Parent		0.046	0.052*	0.054*
		(0.03)	(0.03)	(0.03)
Mother		0.066*	-0.051	-0.260**
		(0.04)	(0.42)	(0.11)
University degree	0.142***	0.157***	0.156***	0.202***
	(0.04)	(0.04)	(0.04)	(0.04)
Vocational training	-0.005	-0.004	-0.007	0.029
	(0.04)	(0.03)	(0.03)	(0.03)
Age	-0.006	-0.013	-0.011	0.001
	(0.01)	(0.01)	(0.01)	(0.01)
Age squared	0.0001	0.0002	0.0001	-0.0001
	(0.00)	(0.00)	(0.00)	(0.00)
Tenure	-0.002**	-0.002**	-0.001	-0.002
	(0.00)	(0.00)	(0.00)	(0.00)
Log wage	0.133***	0.137***	0.097***	
	(0.02)	(0.02)	(0.03)	
Part-time	0.062*	0.040	0.061**	
	(0.03)	(0.02)	(0.02)	
Commuting distance (log)	0.009*	0.009*	0.008	0.007
	(0.01)	(0.01)	(0.01)	(0.01)
Female × University	0.083*			
	(0.05)			
Female × Log wage			0.107**	
			(0.05)	
Mother × Log wage			-0.008	
			(0.08)	
Worker rank (AKM 2007–2013)				0.002***
				(0.00)
Female × Worker rank				-0.001
				(0.00)
Mother × Worker rank				0.003**
				(0.00)
Adjusted R-squared	0.42	0.43	0.43	0.41
Observations	9,537	9,537	9,537	7,950
Fixed effects	Y	Y	Y	Y
Firm fixed effect	Y	Y	Y	Y

Notes: Linear probability models of work-from-home status on individual characteristics. All regressions include firm fixed effect, 2-digit-occupation, year, industry-by-year, and local-labour-market-by-year fixed effects, a dummy for parental status and a dummy for mothers. *Parent* equals one if the worker lives with at least one child younger than 14. *Mother* is the interaction of *Female* and *Parent*. Within-firm comparison: WFH workers and non-WFH workers within firms with active WFH policy (see Panel B in Table 6). Column (1) interacts gender and education (dummy for holding a university degree), while column (2) introduces the *Parent* and a *Mother* dummies. Column (3) introduces the interaction of female and mother dummies with the (log) wages. Column (4) repeats Column (3) specification replacing (log) wages with workers' wage rank, defined within the distribution of AKM wage-effects estimated for full-time workers (2007–2013). Weighted regression. Standard clustered at the firm-worker level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 23: Mediating role of WFH in firm-workers assortative matching
Firm and worker AKM wage-effect ranks from 2014–2021

Wage effects estimated over 2014-2021

	Dependent Variable: Firm rank		
	All	Men	Women
WFH	15.769***	17.797***	8.789
	(3.18)	(3.57)	(6.11)
Worker rank	0.193***	0.171***	0.236***
	(0.03)	(0.03)	(0.05)
Worker rank × WFH	-0.128***	-0.141***	-0.123*
	(0.04)	(0.05)	(0.07)
R-squared	0.66	0.65	0.70
Observations	6414	5129	1208
Mean of dependent variable	69.61	71.91	60.91
SD	(25.64)	(24.60)	(27.60)
Controls and FE	Y	Y	Y

Notes: Linear probability model regressing firm wage-effect rank on worker wage-effect rank, a work-from-home (WFH) dummy, and their interaction. Wage-effects are pre-estimated over years 2014-2021 using the AKM methodology. Worker level regression. The interaction term should capture the role of WFH in firm worker assortative matching based on their productivies. Between firm specification: comparison of WFH workers in WFH firms with non-WFH workers in non-WFH firms (see Panel A in Table 6). Controls include sex, education (university and vocational training dummies), age, age squared, tenure, log distance between residence and firm, and fixed effects for year, industry, industry-by-year, local labour market, local labour market-by-year, and 2-digit-occupation. Column 1 reports estimates for the full sample; Columns 2 and 3 report results separately for men and women. Weighted regression. Standard errors clustered at the firm-worker level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.
Sum of female plus male observations does not adds up to total because of a few singleton observations in the specified FE cells

Table 24: Mediating role of WFH in firm-workers assortative matching
Firm and worker contemporaneous (log) wages

	Dependent Variable: (Log) firm wage		
	All	Men	Women
WFH	1.000***	1.093***	0.922**
	(0.35)	(0.39)	(0.43)
Worker (log) wage	0.366***	0.350***	0.392***
	(0.03)	(0.04)	(0.06)
Worker (log) wage × WFH	-0.146**	-0.155**	-0.146*
	(0.06)	(0.07)	(0.08)
R-squared	0.83	0.84	0.82
Observations	6684	5352	1253
Mean of dependent variable	5.48	5.63	5.15
SD	(0.59)	(0.53)	(0.58)
Controls and FE	Y	Y	Y

Notes: Linear probability model regressing firm average (log) wage on worker (log) wage, a work-from-home (WFH) dummy, and their interaction. (log) wages are approximated with the inverse hyperbolic sine. Worker level regression. The interaction term should capture the role of WFH in firm worker assortative matching based on their productivies. Between firm specification: comparison of WFH workers in WFH firms with non-WFH workers in non-WFH firms (see Panel A in Table 6). Controls include sex, education (university and vocational training dummies), age, age squared, tenure, log distance between residence and firm, and fixed effects for year, industry, industry-by-year, local labour market, local labour market-by-year, and 2-digit-occupation. Column 1 reports estimates for the full sample; Columns 2 and 3 report results separately for men and women. Weighted regression. Standard errors clustered at the firm-worker level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.
Sum of female plus male observations does not adds up to total because of a few singleton observations in the specified FE cells

Table 25: Comparing employer-to-employer transitioners (EE) with all new hirees (non-EE). By gender

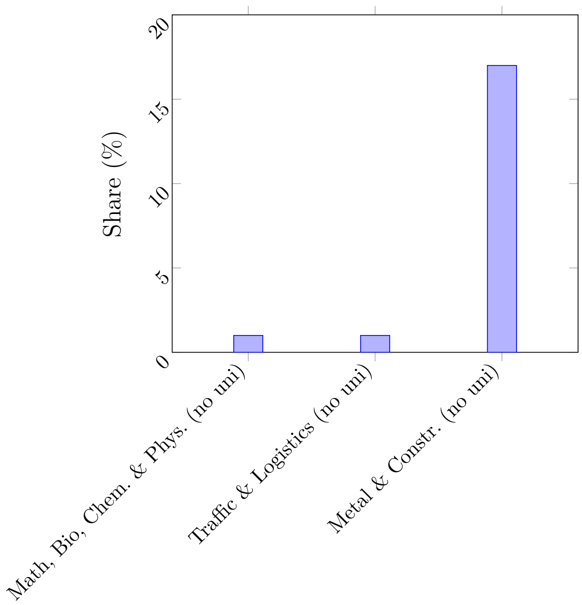
	Female		Male	
A.	Non-EE	EE	Non-EE	EE
Variable				
University degree	0.23	0.24	0.22	0.24
	(0.42)	(0.43)	(0.41)	(0.43)
Vocational training	0.50	0.65	0.53	0.68
	(0.50)	(0.48)	(0.50)	(0.47)
Log wage	4.49	5.01	4.87	5.46
	(0.94)	(0.87)	(0.87)	(0.70)
Worker rank	30.21	36.40	34.73	47.23
	(29.54)	(30.77)	(29.25)	(29.73)
Firm rank	56.85	66.94	65.56	76.14
	(30.62)	(29.39)	(30.11)	(26.55)
Labour mkt exper. (yrs)	7.14	14.21	7.77	16.78
	(8.01)	(10.16)	(8.58)	(11.79)
Manager	0.02	0.04	0.03	0.06
	(0.14)	(0.19)	(0.17)	(0.23)
B.				
Job characterization (%)				
Unskilled/semiskilled task	32.80	25.35	25.52	18.20
Skilled task	47.14	50.03	50.44	50.66
Complex task	9.99	12.82	11.58	13.81
Highly complex task	10.07	11.81	12.45	17.33

Notes: The Table reports means and standard deviations of individual characteristics for female and male workers in their first year at a firm, comparing employer-to-employer transitioners (EE) with all other new hires (Non-EE). Panel A presents (in order of appearance): the share with a university degree; the share with vocational training; the natural logarithm of gross wage; the percentile rank of workers in the distribution of individual AKM wage effects ("Worker rank"); the percentile rank of firms in the distribution of firm AKM wage effects ("Firm rank"); years of labor-market experience; and the share in managerial positions. Panel B reports the percentage of workers in each KldB 2010 skill category (unskilled/semiskilled, skilled, complex, and highly complex tasks). Statistics cover all new hires observed in the ADIAB from 2014 to 2020. Sample size varies by variable; the smallest sample is for Worker rank (74,000 females; 301,000 males).

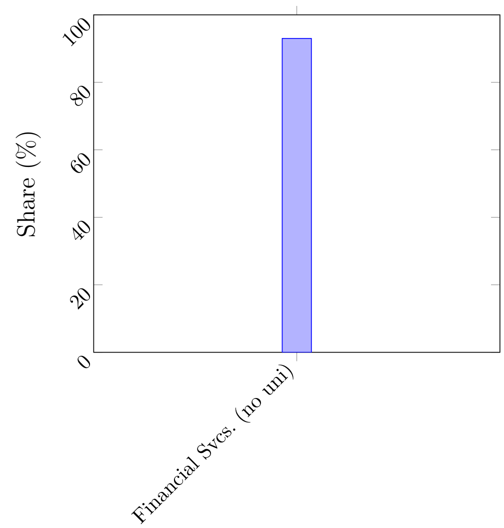
Table 26: Occupation-education cells at the top and bottom of the WFH share distribution.

Panel A: Period 2014–2016

(a) Occupation-education cells with less than 20% WFH

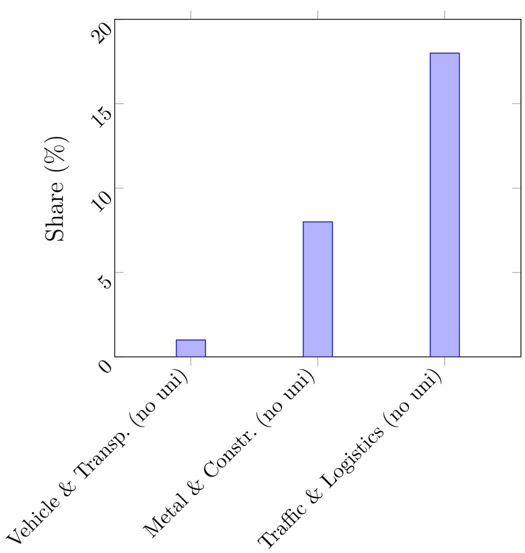


(b) Occupation-education cells with more than 80% WFH

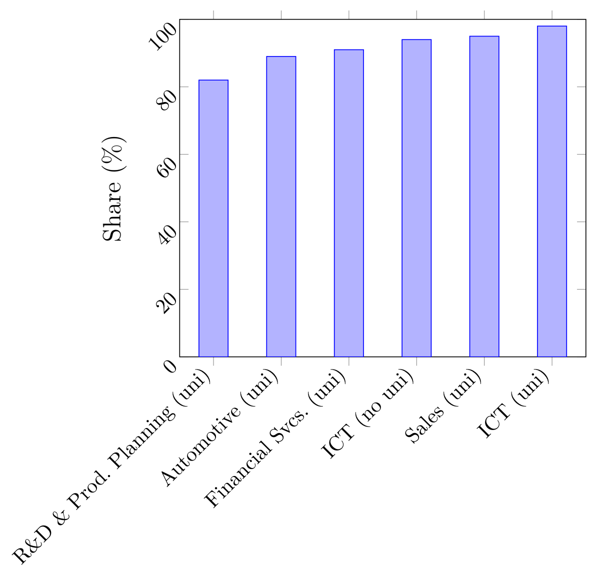


Panel B: Period 2018–2020

(c) Occupation-education cells with less than 20% WFH



(d) Occupation-education cells with more than 80% WFH



Notes: The table shows the distribution of occupation-education cells by the share of employees working from home, split into two periods: Panel A (2014–2016) and Panel B (2018–2020). In each panel, occupation-education cells with less than 20% of WFH workers appear on the left and those with more than 80% WFH workers on the right. Occupation-education cells are defined as the interaction of the 2-digit level occupational classification (KldB) with the University dummy, to replicate the logit model predicting WFH propensity used in Section. Only occupation-education cells with at least 100 observations are included. Only observations within firms with an active WFH policy are used to produce the statistics, to remove the role of occupational selection across firms. 5.4.

Table 27: Female share by WFH intensity

	Work-from-home intensity		
	WFH $\leq$ 20%	20% < WFH < 80%	WFH $\geq$ 80%
		Panel A: Period 2014–2016	
Share of female workers	11%	23%	39%
Observations		4,192	
		Panel B: Period 2018–2020	
Share of female workers	18%	27%	22%
Observations		5,511	

Notes: The table shows the share of female workers within occupation-education cells with a given share of WFH workers (irrespectevely of their gender). Panel A refers to period 2014–2016 and Panel B to 2018–2020. Occupation-education cells are defined as the interaction of the 2-digit level occupational classification (KldB) with the University dummy, to replicate the logit model predicting WFH propensity used in Section. Only occupation-education cells with at least 100 observations are included. Only observations within firms with an active WFH policy are used to produce the statistics, to remove the role of occupational selection across firms. 5.4.