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Spatial Wage Disparities and Household Location Choices

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[1] See Figure E1 at the very end of this document.

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[2] Je tiens cependant à préciser ici que la recherche était *déjà* un métier.

Note to the Reader

The three chapters of this dissertation are self-contained research articles and can be read separately. They are preceded by an introduction which summarizes the research presented in this dissertation. The terms "paper" or "article" are used to refer to chapters. Chapter 3 is co-authored, which explains the use of the "we" pronoun.

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Introduction

"We are never going to have gender equality until we also have couple equity."

– Claudia Goldin, *The New York Times*[1] (2023)

"The folly of building-centric urban renewal reminds us that cities aren't structures; cities are people."

– Edward Glaeser, *Triumph of the City* (2011)

Over the past several decades, women have achieved remarkable progress in closing gender gaps across multiple dimensions of human capital and labor market achievements. Participation in paid work represents perhaps the most striking transformation: while the participation rate of women in France was only 55% in 1975, it increased to 70% in 2021 (Raynaud, 2023). This transformation has, in turn, increased the prevalence of dual-earner couples and the importance of within-household trade-offs in explaining the remaining gender earnings gap.

In most of the world's economies, there remains a significant and persistent difference between the earnings of men and women, which cannot be fully explained by differences in human capital (Bertrand, 2020). In France for instance, women have doubled their representation in white-collar occupations, going from 21% to 42% between 1982 and 2019 (Forment et al., 2020), and now outperform men in educational attainment, with higher college graduation rates (Roussel, 2022). Yet, despite this convergence, women working in the French private sector in 2021 earned 24.4% less than men in annual wages (Godet, 2023). When comparing workers in comparable positions however, this gap reduces to just 4%, pointing to the importance of understanding why men and women hold different types of jobs.

[1] Smialek, 2023

Researchers have identified multiple factors behind these divergent career paths, such as women's lower enrollment in STEM fields (science, technology, engineering and mathematics) (Ceci et al., 2014; Bertrand, 2018); norms and stereotypes guiding, for instance, the choice of educational track (Breda et al., 2023); and behavioral differences in confidence or taste for competition (Gneezy et al., 2003; Niederle et al., 2007) translating into different professional and educational decisions (Buser et al., 2014; Flory et al., 2015). However, the recent literature increasingly emphasizes household-level factors as key drivers of the gender gap.

Goldin (2024) highlights, for instance, the importance of a job's flexibility to accomodate for women's care responsibilities, and the associated cost of this flexibility. In this vein, the large literature on the child penalty (Angelov et al., 2016; Kleven, Landais, Posch, et al., 2019; Meurs et al., 2019) demonstrates that parenthood has a persistent negative impact on women's income. This occurs through several channels: higher rate of exit from the labor market, reduction in working time and wages, and sorting into different occupations, sectors, and firms. Relatedly, Cortés et al. (2019) underline the role of within-couple specialization in paid versus domestic work, while Azmat et al. (2022) analyze the cost of flexibility through the lense of presenteeism and show that parenthood pushes women away from more unique, better-paid jobs. Bertrand, Kamenica, et al. (2015) reveal how gender norms around relative income lead women who could out-earn their husbands to reduce their labor market participation.

When household choices and norms around the distribution of care work and prioritization of paid work systematically favor one gender in career-critical decisions, they can substantially impact the aggregate gender gap. This thesis explores such household-level mechanisms with a focus (particularly in the first two chapters) on the specific angle of household location and geography.

Spatial income disparities in France are large: while the monthly net salary of a private sector worker in Paris is €3,222 on average, it is €2,282 in the rest of France (Le Fillâtre et al., 2024). These large differences have already been widely studied by an extensive literature in urban economics on the costs and benefits of agglomeration to people.

The main advantage of cities arises from the way they bring together people and businesses in geographically concentrated spaces that foster agglomeration economies. As workers and firms congregate, their productivity increases through mechanisms which are often grouped into three categories, since Duranton et al. (2004). Cities first improve both the quality and quantity of matches between workers and firms. They also make it possible to share infrastructure, inputs, but also gains from variety and specialization. Finally, they generate more knowledge and help diffuse it across workers, firms and generations. While these ideas have been discussed since Marshall (1890), empirical estimation of the urban wage premium (the effect of a city's density on wages) has long been limited by methodological complications.

One of the main threats to the identification of the effect of density on productivity and wages is the sorting of individuals into cities: high-skilled workers tend to live in denser cities which, it not taken into account, creates an upward bias in the estimation of the urban wage premium. To solve this issue, Combes et al. (2008) proposed a (now standard) two-step approach, which consists in first estimating a worker-level wage regression including city fixed-effects but also individual fixed-effects to control for unobserved worker heterogeneity. The city effects obtained in the first stage can then be used as a measure of a city's wages, net of its workforce composition. Using this method, various papers have shown that workers earn higher wages and are more productive in large cities (D'Costa and Overman, 2014; Carlsen et al., 2016; De la Roca et al., 2017; Hirsch, Jahn, et al., 2022; Berson et al., 2024).

Yet, despite the fact that the location choices at the heart of the sorting mechanisms are made by households and not individuals, very little attention has been given to the potential heterogeneity of the urban wage premium by gender or household structure. As households are the ones making the decision on where to live and work, this opens the door for within-couple bargaining and norms to create a discrepancy in the economic returns of location choices for men and women. Furthermore, the constraints on women's career induced by their care responsibilities could affect their ability to fully benefit from the matching or learning benefits of living in large cities. Such potential gender differences in urban returns would have important implications for understanding the role of cities in closing the gender gap in earnings.

This dissertation examines the interactions between households, geography and labor markets under three different facets. Each chapter focuses on a different dimension of how joint household decisions affect labor market outcomes of men and women.

The first chapter explores the gendered consequences of household location decisions, examining how geographic moves affect labor market outcomes differently for men and women. Using French administrative data and a stacked difference-in-difference design, I show that household relocation decisions appear to systematically prioritize men's career opportunities over women's. Even when women are the primary earners in their households, moves tend to benefit men's careers while imposing temporary but significant earnings penalties on women. These findings challenge income-maximizing models of household behavior and instead point to the persistent influence of gender norms in household decision-making processes.

The second chapter investigates a central feature of urban economics, the urban wage premium, exploring how the benefits of working in dense urban areas vary by gender and family structure. I find that the returns to density are significantly larger for women than men regardless of marital status. I also show that this advantage is almost entirely eliminated for mothers of young children. These patterns suggest that urban environments, while generally beneficial for women's careers, interact with childcare constraints and household responsibilities in ways that limit mothers' ability to benefit from cities. Furthermore, I document the existence of a participation premium of density which is significantly larger for women than men, indicating that the benefits of large cities occur not only at the intensive margin, but also at the extensive margin.

The third chapter, co-authored with Eric Maurin and Dominique Goux, studies within-household spillovers of work-from-home arrangements. We exploit a change in the French institutional setting around work-from-home, which made the implementation of remote work after the 2020 pandemic shock easier in establishments that had previously signed a collective agreement on remote work. We find strong complementarities in the decision to work from home between spouses: individuals whose partner works in a firm with a work-from-home collective agreement were more likely to switch to remote work themselves following the pandemic. Furthermore, we show that these spillover effects extend to working hours: partners of treated individuals increase their hours, and this increase is even larger in magnitude than the direct effect on treated individuals. These findings show that the overall effects of the rise in work from home are greatly underestimated when we don't pay attention to household spillovers.

Together, these three chapters illuminate the importance of considering the household dimension when studying the labor market decisions and outcomes of men and women. Below, I describe each chapter in more detail.

Chapter 1: Following Along: The Gender Gap in Returns to Geographic Mobility

The stark increase in female labor force participation over recent decades has transformed household decision-making, with dual-career couples becoming increasingly prevalent. As these households must lead two parallel careers, this may generate within-couple trade-offs in terms of career opportunities. Location decisions are one obvious avenue for such trade-offs: when faced with different geographical opportunities, couple members may need to make career-compromising choices to maintain their relationships. This may lead to two type of phenomena, both introduced in Mincer (1978)'s seminar work: *tied-movers* and *tied-stayers* –individuals whose location decisions are driven by household-level considerations rather than individual gains.

In turn, the household location trade-off may contribute to the earnings gender gap. If households systematically prioritize men's careers, whether due to their status as primary earners or due to gender norms in household responsibility allocation, the phenomenon of tied moves could exacerbate the existing gender income gap. Yet there is surprisingly little causal evidence on the effects of geographic relocation on the income of the respective partners.

In this paper I measure how cross-city moves affects labor market outcomes differently for men and women both coupled and single, and study the role of gender norms in household location choices.

Using a novel French administrative dataset which provides information on income, location and household composition for the universe of French residents from 2014 to 2019, I examine the effects of moves on labor income and unemployment by gender and couple status. In order to control for the endogeneity of mobility, I rely on a staggered difference-in-differences design with *not-yet-treated* individuals as the control group. I find that partnered women –either married or cohabiting– are uniquely disadvantaged by moves, experiencing substantial temporary income losses of approximately 7% during the first two years following relocation. Moreover, this group experiences a disproportionate increase in their likelihood of receiving unemployment benefits compared to other demographic groups. These effects are however not persistent: three years after moving, coupled women appear to have recovered from their relocation. These findings suggest that women in relationships are more likely to be tied-movers, with household location decisions significantly compromising their labor market trajectories. Comparison between the outcomes of single and partnered men reveals that men in relationships also experience lower returns to mobility, suggesting that while men generally benefit more from moves, couple-based constraints affect both partners' labor market outcomes.

I then investigate whether this gender gap in the effect of mobility is related to fertility dynamics. A large literature has documented the large impact of children on the gender gap in earnings, including in France (Kleven, Landais, Posch, et al., 2019; Meurs et al., 2019). The potential mechanisms for an interaction of this effect with mobility are twofold. If couples relocate post-childbirth, households may prioritize men's careers due to women's already diminished earnings and reduced work hours. Alternatively, when planning for children, households might preemptively favor men's professional trajectories in anticipation of future "child penalities".

To test empirically this potential channel, I reproduce my analysis by age groups, revealing a consistent gender gap in mobility returns across different life stages, even post childbearing years. I obtain further insight by stratifying the sample according to fertility events surrounding geographic moves. Even among couples not experiencing childbirth around their moves, a gender-differentiated mobility premium persists, though it is less pronounced. Notably, households experiencing childbirth both preceding and following relocation exhibit the most substantial gender divergence in mobility returns. These findings suggest that while the intersection of child penalties and relocation amplifies mobility-related earnings disparities, it is not their only determinant.

I then explore two potential mechanisms underlying the observed gender gap in mobility effects. One hypothesis is that households aim to maximize total income when making location decisions, favoring men's careers due to their higher pre-move earnings and potential returns. Alternatively, this pattern could reflect persistent traditional gender norms, where men are viewed as primary family providers and women's professional contributions are secondary.

To distinguish between these explanations, I analyze households stratified by the gender of the pre-move primary earner. I find that even in households where women were the higher earners prior to relocation, men still benefit more from the move. Furthermore, across most household categories, total labor income declines post-relocation. These findings suggest that the gender gap in mobility returns is not solely explained by men's being primary earners, nor by household-level income maximization.[2]. Instead, the results

[2]Households are likely maximizing utility rather than income, and would hence choose their location based on income but also on prices and amenities (Roback, 1982; Rosen, 1979). Yet it is not obvious how this would generate a gender difference in returns to relocation, as prices and amenities in a given location

point to the likelihood that gender norms influence household location decisions and subsequent earnings disparities, which in turn reduces the households' total income

My paper is related to a strand of literature on joint location decisions by households. Early papers such as Mincer (1978) introduced the idea that family ties are an additional constraints in location decisions, and can result in tied-moves. Since then, work by Nivalainen (2004) in Finland has shown that migration is more likely to be determined by the husbands' career, while Jürges (2006) documents that this is only the case in Germany in couples who exhibit "traditional" values, measured through their division of housework. Tenn (2010) study the evolution of wives' contribution to inter-state migration decisions in the US, and shows that despite the rise in female attachment to the labor market, men have remained the primary determinant of migration between 1960 and 2000. Relatedly, Costa et al. (2000) argues that colocation choices of "power couples" are at the heart of the increased concentration of college-educated couples in large cities in the US. Compton et al. (2007) test this hypothesis and find that migration behavior is only predicted by the husband's education, and not by couples' joint education profile.

While I do not directly observe the household decision-making process, this literature provides a plausible interpretative framework for my findings. The fact that women who move while in a relationship experience the most significant income losses is consistent with the concept of tied migration. In contrast, the large returns to relocation and the small gender gap for single individuals highlights the critical role of joint household decision-making in explaining the different returns to mobility within couples.

I also contribute to an empirical literature which has long tried to estimate the differential effect of moves on the income of husbands and wives. While a large part of it was not concerned with identifying causal effects (Sandell, 1977, Spitze, 1984, Bielby et al., 1992, Boyle et al., 2001, Cooke, 2003...), some more recent papers made use of a fixed effects model (Cooke et al., 2009) or a difference-in-differences in which non-movers are used as control (Blackburn, 2010). All of those articles, which focus on the US and UK context, find that moves had a temporary negative effects on women's career (measured through wages, earnings, or probability of being employed), while they have either a positive or no effect on men's career. The closest paper to my work belongs to this empirical strand of literature. In a working paper, Jayachandran et al. (2024) use an event-study approach on German and Swedish data and find a persistent gap in the returns to moving, with

should be on average the same for men and women. It could be however that couples are choosing a new location which favours the man's career and the woman's preferences in local amenities, compensating the utility loss men gaining significantly more than women from relocation. Finally, Gemici (2007) and Buchinsky et al. (2023) both estimate models of household decision making, in the US and Israel respectively, to quantify the negative impact of joint-decisions on women's labor market outcomes.

I complement this literature by using a different identification strategy, which uses not-yet-movers as control group, rather than relying simply on within-individual variation. Using this control group allows me to better estimate the causal effect of moving for the full sample of French movers. While moving is of course not an exogenous event, and households choose to relocate based on their expected returns from migration, my difference-in-differences approach allows me to examine the differential effect of this decision on men and women. This is made possible by exploiting the differential timing of migration for households who all move over a limited period of time and are very similar to each other.

Finally, I contribute to a large literature on the source of gender gaps in labor market outcomes, and more precisely on the role of household-level mechanisms. As underlined previously, many papers have shown that the "child penalty" is an important contributor to observed gaps on the labor market (recently, Kleven, Landais, and Søgaard, 2019, Kleven, Landais, Posch, et al., 2019, Meurs et al., 2019, Cortés et al., 2023). Research has also shown the negative impact on women's career of family constraints which limits their flexibility on the labor market. Goldin (2014) and Cortés et al. (2019) show for instance that constraints which prevent women from working long hours affect negatively their career progression. Le Barbanchon et al. (2021) find that female job-seekers in France have a lower willingness to commute than men, which translates into a lower wage upon reemployment. Bertrand, Kamenica, et al. (2015) study the prevalence of gender norms in within-household specialization. They find that wives who earn more than their husbands are not protected from gender roles, and tend to spend even more time on household chores. In this paper, I document another dimension through which family constraints and prioritization of men's career may affect the gender pay gap : women's tied relocations, a channel which itself reinforces the child penalty.

Chapter 2: Gender, Households, Children, and the City

The urban economics literature has largely established the existence of an urban wage premium: workers are more productive and earn higher wages in large cities, even after accounting for the sorting of high-skilled individuals in large urban areas. In France, Combes et al. (2008) quantify this relationship, estimating the elasticity of wages with respect to density at approximately 0.03. At the same time, despite significant progress in recent decades, the gender gap in earnings remains a persistent economic reality (Goldin, 2014). Yet the intersection of these two fundamental economic phenomena –agglomeration effects and gender disparities– remains largely unexplored. Do women and men benefit equally from the productivity advantages of dense urban environments?

In this project, I provide novel evidence on the way agglomeration economies differ by gender and household type. A key difficulty in understanding the mechanisms of differential agglomeration gains by gender is the lack of large panel datasets containing information on both labor market outcomes and household composition. I overcome this challenge by accessing a unique administrative dataset that covers the universe of French residents. By chaining it across years, I am able to track individuals and households over time, and to observe income, location, gender and household structure between 2014 and 2019. Applying a standard two-step approach to this new data, I estimate the urban wage premium by category and find that, once accounting for spatial sorting, the urban wage and participation premiums are significantly larger for women. I find that while men have an elasticity of earnings to density of approximately 0.05, this rises significantly to 0.068 for women.

Several theoretical channels could explain differential returns to density by gender. As women tend to have interrupted employment trajectories, and a smaller job search radius (Le Barbanchon et al., 2021), the better job matching in large cities could be more beneficial for them.Furthermore, if women are more likely to be tied movers (Gemici, 2007, Venator, 2023), meaning they move due to the gains of their spouses even if it is not in their direct interest, the improved job matching in large cities could help them adapt to their new residence. On the other hand, career interruptions could attenuate the effects of learning spillovers on wage growth. High congestion cost (in particular childcare and commuting) could also have a stronger netgative impact of women than on men. Finally, large cities could differ from smaller ones in their gender norms and discriminatory behavior.

Many of these mechanisms are related to household composition, either through child-related consideration or intra-household decision. In order to assess their relevance to the gender gap in urban wage premium I explore a further heterogeneity dimension and estimate the urban wage premium by gender and couple status, as well as by gender and parental status. If the household related mechanisms played an important role, we would find that the urban wage premium is larger for mothers or coupled women than for single or childless ones. I show instead that women who are in a partnership or have children do not benefit more from density. The significant gender gap in the urban wage premium is driven neither by married women, nor by mothers. Instead, I find that mothers of young children experience a density penalty which almost fully wipes off the female extra agglomeration gain. This suggests that congestion costs linked to child-related constraints wipe off potential benefits from improved matching.

This paper contributes to a large body of work on individual agglomeration gains. Starting with Glaeser and Mare (2001), many papers have documented the existence of an urban wage premium, and studied its mechanisms : Combes et al. (2008) in France; D'Costa and Overman (2014) in the UK; De la Roca et al. (2017) in Spain. The empirical part of this literature highlights the existence of large skills sorting, explaining part of the observed wage premium: individual skills are positively correlated to city-size. However, sorting does not explain all of the city size premium, and the literature has shown that there are still gains from density even when sorting is accounted for. Few articles however have explored the heterogeneity of agglomeration gains for different types of worker. Carlsen et al. (2016) estimate the urban wage premium by education level in Norway, and find that college-educated workers have a higher return to working in cities. Another example is Ananat et al. (2018) who show that black workers in the US receive a significantly lower employment density premia than white workers. More recently, Le Roux (2025) shows that returns to density in South Africa are significantly higher for high-income than low-income individuals.

A small set of articles have studied how this wage premium varies by gender. Both Phimister (2005) and Hirsch, König, et al. (2013) find evidence of a small urban participation premium for women but are only able to compare urban and rural women. More recently D'Costa (2024) also estimates the impact of working in cities –defined by opposition to rural areas– on men and women's wages, and finds that before 2008 the urban wage premium for women was twice that of men, but that this difference disappeared after the 2008 crisis. In a different context, Le Roux (2025) finds no difference in the gains from density by gender in South Africa. The closest article to my work is and Elass et al. (2024), who use a similar two-step approach on another French dataset to estimate the impact of urban density on the gender wage gap. They also find that that women benefit more than men from urban density, and after decomposing this gap, they conclude that occupational segregation plays a large part in explaining the difference, followed by childcare and commute constraints. I complement their research by exploring another dimension of heterogeneity: the role of household structure, in particular couple status and presence of children.

While various studies document heterogeneity in agglomeration gains by gender, there is little literature on the mechanisms of these differential gains. In this paper, I explore the particular channel of household structure: as men and women have become more and more similar in their productive characteristics, household-level factors become more important to explain the remaining gender gap. The recent literature on the gender gap in earnings and its sources underlines the importance of children and intra-household specialization in explaining the persistence of this gap (Goldin, 2014, Angelov et al., 2016, Kleven, Landais, and Søgaard, 2019, Cortés et al., 2023, Goldin, 2024). Within-couple specialization, with one member (often the man) focusing on paid work and the other taking on more of the childrearing and domestic work, may lead women to choose jobs which offer more flexibility but lower wages: part-time (Manning et al., 2008), requiring less presenteeism (Azmat et al., 2022), with less commute (Le Barbanchon et al., 2021)... The role of household factors in agglomeration economies has been put forward by the "power couples" literature dating back to Costa et al. (2000), which argues that returns to larger cities may be larger for (highly-educated) couples than other households. This paper is the first to systematically study the role of intra-household factors in differential agglomeration gains by gender.

In a last section I contribute to a small strand of literature which studies geographical disparities in labor market participation by women, dating back to Odland et al. (1998). Black et al. (2014) document large and persistent variation in female participation across US cities, with women in large cities being less likely to work. Similarly, Moreno-Maldonado (2022) finds that women with children are less likely to work when located in big cities. Both papers argue that this is driven by differences in commuting and childcare cost, which create an incentive for intra-household specialization: one spouse working long hours while the other stays out of the labor market. I contribute to this literature by showing that accounting for individual fixed effects in estimation of the participation gap reverses the findings: once sorting is accounted for, I find a positive effect of density on participation which is even larger for women than men.

Chapter 3: Rise in Home Working and Spousal Labor Supply

The pandemic shock of the early 2020s catalyzed an unprecedented expansion of work from home worldwide. A growing number of experimental and quasi-experimental studies are examining whether and how this development might impact the productivity or well-being of affected employees, with mixed and still-debated results (e.g., Atkin et al., 2023, Emanuel, Harrington, and Pallais, 2023, Angelici et al., 2024, Bloom, Han, et al., 2024, Emanuel and Harrington, 2024). In contrast, the implications of this development for those living with teleworkers have received far less attention and remain largely unexplored. Exploring these implications is all the more important as the interdependence of individual decisions within families has long been identified as a key parameter for understanding the full range of consequences that can result from developments or from reforms that directly affect only part of the population (e.g., Ashenfelter et al., 1974, Gelber, 2014, Goux et al., 2014, Lalive et al., 2017, Johnsen et al., 2022).

As working from home drastically reduces commute times, it has the potential to considerably change how men and women spend their days and interact with each other within families. Several scenarios are possible. For example, the transition to working from home for some employees may lead them to take on a greater share of domestic work and childcare, freeing up time for their partners, enabling the latter to invest more in their work and increase the number of hours they work. Given the importance that the ability to work long hours can have in many occupations, the consequences can be considerable for both spouses and their relative occupational status (e.g., Bertrand, Goldin, et al., 2010, Goldin, 2014, Cortés et al., 2019). Conversely, having one partner work from home might encourage the other to also work remotely, not necessarily to work more hours, but simply to spend more time with the family, without any major impact on the number of hours worked by either spouse. Depending on which of these scenarios dominates the other, the consequences of the rise in home working on the number of hours worked in the economy or on inequalities between men and women within couples are potentially very different.

The aim of this article is to shed light on these issues and to estimate the causal effect of an employee's choice to work from home on his or her spouse's labor outcomes. To the best of our knowledge, there are still no studies that have addressed these issues, one of the difficulties being to find independent variations in spouses' exposure to WFH. Our research strategy draws on the particular institutional context in which the 2020 epidemic shock hit French firms. In late 2017, France passed legislation that facilitated the adoption of work-from-home (WFH) through collective bargaining agreements. This reform created two groups of establishments: those that had signed WFH agreements in 2018 or 2019 (our treatment group) and those that had only signed agreements on other topics during the same period (our control group). While both groups showed similar remote work patterns in the years before the pandemic, the 2020 shock led to significantly larger increases in WFH in treatment group firms. We leverage this variation by comparing labor market outcomes of employees whose spouses work at treatment versus control group firm, essentially comparing workers whose partners faced high versus low exposure to pandemic-induced WFH adoption.

This approach first suggests the existence of very significant cross effects on WFH: employees were significantly more likely to work from home in the years following the 2020 pandemic if their spouse worked at a treatment group establishment, regardless of their own establishment's treatment status. Importantly, no such differences existed in the pre-pandemic period, supporting our identification strategy. Employees whose spouses are in the treatment increased their WFH by an amount equal to roughly 80% of their spouses' increase. This suggests that when one spouse adopts WFH, it raises the probability of the other spouse also working from home by approximately 0.8. Unsurprisingly, these cross-effects on WFH are only noticeable in couples whose members have remotable occupations and not in couples whose occupations are difficult to perform remotely.

Employees with spouses in the treatment group were not only more likely to work from home after the epidemic shock, but they also significantly increased their usual number of hours worked per week compared to employees with spouses in the control group. According to our estimates, an employee's switch to home working is followed on average by a 20% increase in the spouse's usual weekly working time. A closer look at this increase shows that it essentially corresponds to the substitution of long workweeks (40 hours or more) for weeks of 35 hours or less (35 hours being the legal length of the workweek). Consistent with the idea that these cross-effects on hours worked are linked to the effects on WFH, they are also only noticeable in couples whose members have remotable occupations.

Additional analyses reveal that cross-effects on WFH and hours worked primarily affect men whose spouse is in the treatment group, while almost no cross-effects are detected for women whose spouse is in the treatment group. Conversely, direct effects on WFH and hours worked affect women in the treatment group much more than men in the treatment group. We show that these profound asymmetries between men and women are consistent with a simple model where less-paid spouses (in the vast majority of cases, women) work from home as much as legally possible in their companies, independently of the choices of their partners, while better-paid spouses (in the vast majority of cases, men) only increase their rate of work at home to the extent that their partners also increase it, so as to be able to benefit from the time saved on the home-work commute without having to worry about having to increase their contribution to domestic tasks or childcare.

As an employee's move to WFH greatly increases his or her partner's propensity to work from home, we may ask whether this is not also accompanied by a change in their place of residence. We find no evidence to support this hypothesis, as the switch to home working for an employee is not accompanied by any significant change in his or her partner's home-work distance, or in the likelihood of the couple deciding to live away from urban centers. The rise of remote working has reduced the commuting costs associated with moving away from urban centers for the many white-collar workers working and living in those centers, but not enough to offset the other costs of such distance, notably in terms of reduced access to better educational, medical, or cultural infrastructure. Ultimately, the reduction in the frequency of home-to-work journeys induced by the switch to WFH does not seem to be offset by an increase in home-to-work distances, which helps to explain the gains in available time (particularly for work) achieved by couples with remotable occupations.

Our article contributes to the long-standing literature exploring the influence that workers have on each other within couples. An important strand of this literature has shown how workers respond to changes in their spouses' earnings or work hours, whether at the time of their spouses' retirement, during unemployment spells, or after a tax reform (e.g., Lundberg, 1988; Bingley et al., 2007; Gelber, 2014; Lalive et al., 2017; Johnsen et al., 2022). Using changes in the regulation of public holidays or the legal workweek, another stream of literature has further highlighted the importance that workers place on the possibility of adjusting and synchronizing their working hours with those of their spouse (e.g., Hunt et al., 1998, Goux et al., 2014; Hamermesh et al., 2017; Georges-Kot et al., 2024). In this article, we highlight the value employees place on being able to coordinate their presence at home with that of their spouse, and the far-reaching consequences this coordination can have on their working hours.

We also contribute to the burgeoning literature exploring the causes and consequences of the rise in WFH that has followed the pandemic shock. Several articles have shown that workers, and especially women, have a distaste for commuting and a strong willingness to pay for remote work (e.g. Mas et al., 2017, He et al., 2021, Chen et al., 2023, Cullen et al., 2025, Le Barbanchon et al., 2021, Bütikofer et al., 2024). Our article suggests that the value employees place on working from home reflects at least in part the particular value they place on interactions within the couple, with employees' demand for working from home appearing all the stronger the more their partner works from home themselves.

Another important strand of the literature focuses on the effects of WFH on the productivity and labor outcomes of the employees involved (e.g., Bloom, Liang, et al., 2015, Choudhury et al., 2021, Emanuel, Harrington, and Pallais, 2023, Gibbs et al., 2023, Barrero et al., 2023, Atkin et al., 2023, Angelici et al., 2024, Emanuel and Harrington, 2024, Bloom, Han, et al., 2024). This literature is based on local experiments and quasi-experiments conducted in specific companies and focuses on the effects of working from home on the employees concerned. Using a large-scale natural experiment, we focus on a different question: the induced effects on the spouses of the employees concerned. We find that when an employee switches to WFH, this greatly increases the likelihood of his or her spouse switching to WFH, but it also increases the number of hours the spouse devotes to work, particularly the better-paid spouse. These results suggest that we have a very incomplete view of the effects of WFH if we do not take into account the strong interdependencies existing within couples.

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1

Following Along: The Gender Gap in Returns to Geographic Mobility

Abstract

This paper examines how household location decisions differentially affect labor market outcomes across gender and partnership status. Implementing a staggered difference-in-difference design with French administrative data, I find that only coupled women experience income losses following moves: their earnings decline by approximately 7% in the two years post-relocation before they recover. Moves typically reduce total household income and disproportionately benefit men's careers –even in couples where women were the primary earners pre-move. These findings are not consistent with income-maximizing models of household decision-making that predict prioritization of the higher earner's career. Instead, the results suggest that gender norms play a crucial role in household location choices, with men's career opportunities being systematically prioritized over women's, regardless of their relative economic contributions.

1. Introduction

The stark increase in female labor force participation over recent decades has transformed household decision-making, with dual-career couples becoming increasingly prevalent. As these households must lead two parallel careers, this may generate within-couple trade-offs in terms of career opportunities. Location decisions are one obvious avenue for such trade-offs: when faced with different geographical opportunities, couple members may need to make career-compromising choices to maintain their relationships. This may lead to two type of phenomena, both introduced in Mincer (1978)'s seminar work: *tied-movers* and *tied-stayers* –individuals whose location decisions are driven by household-level considerations rather than individual gains.

In turn, the household location trade-off may contribute to the earnings gender gap. If households systematically prioritize men's careers, whether due to their status as primary earners or due to gender norms in household responsibility allocation, the phenomenon of tied moves could exacerbate the existing gender income gap. Yet there is surprisingly little causal evidence on the effects of geographic relocation on the income of the respective partners.

In this paper I measure how cross-city moves affect labor market outcomes differently for men and women both coupled and single, and study the role of gender norms in household location choices.

Using a novel French administrative dataset, which provides information on income, location and household composition for the universe of French residents from 2014 to 2019, I examine the effects of moves on labor income and unemployment by gender and couple status. In order to control for the endogeneity of mobility, I rely on a staggered difference-in-difference design with *not-yet-treated* individuals as the control group. I find that partnered women –either married or cohabiting– are uniquely disadvantaged by moves, experiencing substantial temporary income losses of approximately 7% during the first two years following relocation. Moreover, this group experiences a disproportionate increase in their likelihood of receiving unemployment benefits compared to other demographic groups. These effects are however not persistent: three years after moving, coupled women appear to have recovered from their relocation. These findings suggest that women in relationships are more likely to be tied-movers, with household location decisions significantly compromising their labor market trajectories. Comparison between the outcomes of single and partnered men reveals that men in relationships also experience lower returns to mobility, suggesting that while men generally benefit more from moves, couple-based constraints affect both partners' labor market outcomes.

I then investigate whether this gender gap in the effect of mobility is related to fertility dynamics. A large literature has documented the large impact of children on the gender gap in earnings, including in France (Kleven, Landais, Posch, et al., 2019; Meurs et al., 2019). The potential mechanisms for an interaction of this effect with mobility are twofold. If couples relocate post-childbirth, households may prioritize men's careers due to women's already diminished earnings and reduced work hours. Alternatively, when planning for children, households might preemptively favor men's professional trajectories in anticipation of future "child penalties".

To test empirically this potential channel, I reproduce my analysis by age groups, revealing a consistent gender gap in mobility returns across different life stages, even post childbearing years. I obtain further insight by stratifying the sample according to fertility events surrounding geographic moves. Even among couples not experiencing childbirth around their moves, a gender-differentiated mobility premium persists, though it is less pronounced. Notably, households experiencing childbirth both preceding and following relocation exhibit the most substantial gender divergence in mobility returns. These findings suggest that while the intersection of child penalties and relocation amplifies mobility-related earnings disparities, it is not their only determinant.

I then explore two potential mechanisms underlying the observed gender gap in mobility effects. One hypothesis is that households aim to maximize total income when making location decisions, favoring men's careers due to their higher pre-move earnings and potential returns. Alternatively, this pattern could reflect persistent traditional gender norms, where men are viewed as primary family providers and women's professional contributions are secondary.

To distinguish between these explanations, I analyze households stratified by the gender of the pre-move primary earner. I find that even in households where women were the higher earners prior to relocation, men still benefit more from the move. Furthermore, across most household categories, total labor income declines post-relocation. These findings suggest that the gender gap in mobility returns is not solely explained by men being primary earners, nor by household-level income maximization.[1]. Instead, the results point to the likelihood that gender norms influence household location decisions and subsequent earnings disparities, which in turn reduces the households' total income

My paper is related to a strand of literature on joint location decisions by households. Early papers such as Mincer (1978) introduced the idea that family ties are an additional constraint in location decisions, and can result in tied-moves. Since then, work by Nivalainen (2004) in Finland has shown that migration is more likely to be determined by the husband's career, while Jürges (2006) documents that this is only the case in Germany in couples who exhibit "traditional" values, measured through their division of housework. Tenn (2010) study the evolution of wives' contribution to inter-state migration decisions in the US, and shows that despite the rise in female attachment to the labor market, men have remained the primary determinant of migration between 1960 and 2000. Relatedly, Costa et al. (2000) argues that colocation choices of "power couples" are at the heart of

[1]Households are likely maximizing utility rather than income, and would hence choose their location based on income but also on prices and amenities (Roback, 1982; Rosen, 1979). Yet it is not obvious how this would generate a gender difference in returns to relocation, as prices and amenities in a given location should be on average the same for men and women. It could be however that couples are choosing a new location which favours the man's career and the woman's preferences in local amenities, compensating the utility loss

the increased concentration of college-educated couples in large cities in the US. Compton et al. (2007) test this hypothesis and find that migration behavior is only predicted by the husband's education, and not by couples' joint education profile.

While I do not directly observe the household decision-making process, this literature provides a plausible interpretative framework for my findings. The fact that women who move while in a relationship experience the most significant income losses is consistent with the concept of tied migration. In contrast, the large returns to relocation and the small gender gap for single individuals highlights the critical role of joint household decision-making in explaining the different returns to mobility within couples.

I also contribute to an empirical literature which has long tried to estimate the differential effect of moves on the income of husbands and wives. While a large part of it was not concerned with identifying causal effects (Sandell, 1977, Spitze, 1984, Bielby et al., 1992, Boyle et al., 2001, Cooke, 2003...), some more recent papers made use of a fixed effects model (Cooke et al., 2009) or a difference-in-differences in which non-movers are used as control (Blackburn, 2010). All of those articles, which focus on the US and UK context, find that moves had temporary negative effects on women's career (measured through wages, earnings, or probability of being employed), while they have either a positive or no effect on men's career. The closest paper to my work belongs to this empirical strand of literature. In a working paper, Jayachandran et al. (2024) use an event-study approach on German and Swedish data and find a persistent gap in the returns to moving, with men gaining significantly more than women from relocation. Finally, Gemici (2007) and Buchinsky et al. (2023) both estimate models of household decision making, in the US and Israel respectively, to quantify the negative impact of joint-decisions on women's labor market outcomes.

I complement this literature by using a different identification strategy, which uses not-yet-movers as control group, rather than relying simply on within-individual variation. Using this control group allows me to better estimate the causal effect of moving for the full sample of French movers. While moving is of course not an exogenous event, and households choose to relocate based on their expected returns from migration, my difference-in-differences approach allows me to examine the differential effect of this decision on men and women. This is made possible by exploiting the differential timing of migration for households who all move over a limited period of time and are very similar to each other.

Finally, I contribute to a large literature on the source of gender gaps in labor market outcomes, and more precisely on the role of household-level mechanisms. As underlined previously, many papers have shown that the "child penalty" is an important contributor to observed gaps on the labor market (recently, Kleven, Landais, and Søgaard, 2019, Kleven, Landais, Posch, et al., 2019, Meurs et al., 2019, Cortés et al., 2023). Research has also shown the negative impact on women's career of family constraints which limit their flexibility on the labor market. Goldin (2014) and Cortés et al. (2019) show for instance that constraints which prevent women from working long hours affect negatively their career progression. Le Barbanchon et al. (2021) find that female job-seekers in France have a lower willingness to commute than men, which translates into a lower wage upon reemployment. Bertrand et al. (2015) study the prevalence of gender norms in within-household specialization. They find that wives who earn more than their husbands are not protected from gender roles, and tend to spend even more time on household chores. In this paper, I document another dimension through which family constraints and prioritization of men's career may affect the gender pay gap : women's tied relocations, a channel which itself reinforces the child penalty.

The rest of the article is as follows: Section 2 presents the data I am using as well as the sample selection, and defines the main variables used in my analysis. Section 3 details the empirical approach I use, while section 4 presents my main results on the effect of moves by gender. Section 5 details the effect of moving on within household outcomes, and discusses the role of gender norms. Finally, section 6 concludes the paper.

2. Data and definitions

My main source of data is an administrative dataset, the Housing and Individual Demographic Files (FIDELI), which originates from the combination of several tax sources and covers all residents of France from 2014 to 2019.

Panel creation

The dataset's original structure consists of consecutive two-year cross-sections, where individual identifiers enable tracking across adjacent years only. I circumvent this limitation by taking advantage of the structure of the data to chain it together and create a panel spanning from 2014 to 2019. This approach leverages the overlapping structure of consecutive files. For example, both the 2016 and 2017 files contain information for 2016 on the same population. By exploiting the granularity of the provided variables, I am able to uniquely match most individuals and link individual records across consecutive files. Figure A1 in the Appendix illustrates the matching rates across consecutive years, disaggregated by age and gender. The procedure achieves unique matches for over 95% of adults aged 25-60, with matching precision increasing with age.

Demographics and income

For each person living in France in that time period, I observe demographic information such as age, gender, city of birth, and marital status. The data also details pre-tax annual income of different types, including unemployment insurance, wages, and income from independent professions (agricultural, industrial, commercial and non-commercial profit). The sum of wage and independent income is what I will define as ”labor income” in the rest of the paper.

Location and moves

The FIDELI dataset provides precise residential addresses for all households as of January 1st of each year, enabling the identification of residential mobility through changes in city of residence. A limitation of the data is that it only captures the fact that a move occurred between two consecutive January 1st dates, without specifying the exact timing or potential multiple moves within the same year. However this limitation is unlikely to significantly affect the analysis, as repeated movers seem to be relatively rare: only 8% of mover individuals relocate in two consecutive years, and the share is likely to be lower within a year.

Given my focus on labor market outcomes, I concentrate specifically on inter-city moves that entail a change in local labor markets. For this purpose, I define movers as households who relocate across Urban Areas, defined by the National Institute of Statistics (INSEE) for the year 2010. Figure A2 provides a map of all French Urban Areas, with the Paris Urban Area highlighted in blue. These administrative units are designed around a central urban core containing at least 1,500 jobs. The Urban Area boundaries are then extended to include surrounding municipalities where at least 40% of the working population is employed within the urban area.

Households

A last dimension of the FIDELI data, particularly important to this research, is its detailed household composition information. The data links married couples and those in civil unions through a shared household identifier, as they are required to file joint tax returns. This identifier also connects dependents, including children, to their reference household, allowing me to observe both the presence and age of children. The precise housing data, which specifies the exact flat or house of residence, enables me to further identify cohabiting individuals. Throughout this paper, I define couples as individuals who are either married, in a civil union, or cohabiting in a two-adult household at the beginning of the year.

To validate the accuracy of my couple identification methodology, particularly for cohabiting couples, I compare the distribution of couple types in my panel with French Census data. This comparison is made possible by the 2015 revision of the French Census, which introduced explicit questions about both cohabitation ("live in a couple") and marital status. Figure 1.1 demonstrates that the proportion of individuals who are married, in civil unions, or cohabiting (corresponding to my definition of couples) shows remarkable consistency between these two data sources. Even though married or civil union couples are slightly more numerous in the Fideli data, the share of individuals who are cohabiting, married or in a civil union (corresponding to my definition of couples) is the same in both data sources for all years considered.

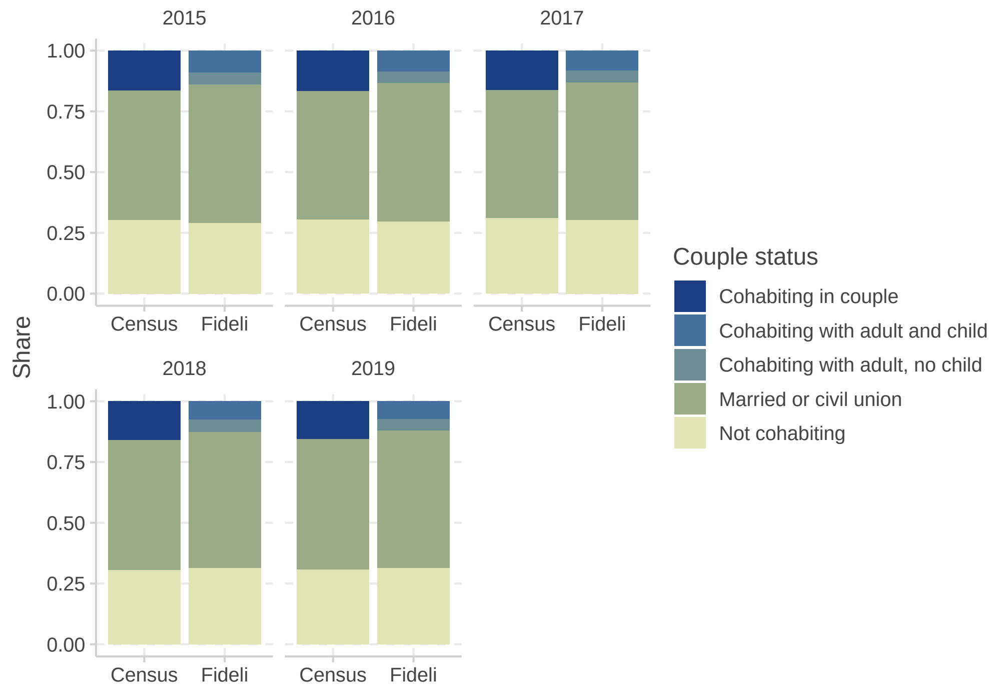
To address potential concerns that my baseline definition of couples may be overly inclusive and capture other living arrangements, I conduct robustness checks using two more restrictive definitions. The first considers only individuals who are married or in civil unions, while the second extends this to include cohabiting adults in two-adult households with at least one minor child present. The results of these alternative specifications, presented in Online Appendices B.1 and B.2, confirm my main findings.

Sample selection

My sample consists of individuals aged 25-60 who live in mainland France's urban areas (excluding Corsica and overseas territories) and relocated between urban areas between 2015 and 2019. I focus on individuals who moved exactly once during this period, representing 84% of all movers, as households which move repeatedly over so short a period may differ from others.

Several data limitations necessitate additional sample restrictions. My dataset only records household addresses and composition on January first of each year, preventing me from observing the within-year timing of events. This creates challenges for analyzing joint moves: when household composition changes in the same year as a move, I cannot determine whether the move preceded or followed the household change. Therefore, I exclude all households that experience both a move and a composition change in the same year.

Figure 1.1. Distribution of couple status, in Fideli and Census data



Note: This figure presents the distribution of individuals' marital and partnership status by year in two different data sources. It is computed from two data sources: Fideli, and 2017 Census. In both data sources the sample used is made of individuals aged 25 to 60 years old, and living in an urban area in mainland France. Shares from the census are computed using individual weights. Not cohabiting category for census data includes individuals who report being single, widowed or divorced. Marriage, civil union, and cohabitation are directly reported by individuals in the census.

Using my main definition of couples (married and cohabiting), this restriction removes 15.3% of moves, including 1.3% coinciding with divorces or breakups and 14% with marriages or new cohabitation.

I also exclude same-sex couples (approximately 1% of moving couples), as these households likely face different gender norms than those I aim to identify. Furthermore, I cannot calculate gender-specific income share for those households, a crucial variable for my analysis of gender norms.

To maintain sample balance, I also exclude couples where one member falls outside the sample restrictions (e.g., one partner is over 60 or moved multiple times). The final sample comprises approximately 1 million individuals, observed on average over 5 years, with 64% of individuals across all years.

Table 1.1 presents descriptive statistics across my three alternative couple definitions. Single movers differ notably from coupled movers: they are younger, less likely to have children, and earn lower annual labor income on average. The definition of couples affects sample characteristics: restricting couples to married individuals only (versus including cohabiting couples) yields a sample with fewer children and lower pre-move income. The third definition—including both married couples and cohabiting couples with children—produces characteristics between these two extremes.

Table 1.1: Descriptive statistics by couple status

Variable	Married or cohabiting (main definition) Yes	No	Married Yes	No	Married or cohab. w/ child Yes	No
Age	39.05	38.34	39.25	37.64	39.49	37.70
	(8.60)	(10.18)	(8.43)	(9.54)	(8.47)	(10.08)
Female	0.50	0.51	0.50	0.52	0.50	0.51
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Number minor children	1.21	0.33	1.37	0.41	1.32	0.26
	(1.12)	(0.75)	(1.13)	(0.84)	(1.11)	(0.68)
Has minor child	0.69	0.21	0.73	0.25	0.75	0.17
	(0.46)	(0.41)	(0.45)	(0.44)	(0.44)	(0.37)
Has child under 10	0.57	0.13	0.59	0.17	0.62	0.11
	(0.49)	(0.34)	(0.49)	(0.38)	(0.49)	(0.31)
Has child under 6	0.45	0.09	0.46	0.12	0.49	0.07
	(0.50)	(0.28)	(0.50)	(0.32)	(0.50)	(0.25)
Has child under 3	0.28	0.05	0.30	0.06	0.30	0.04
	(0.45)	(0.21)	(0.46)	(0.24)	(0.46)	(0.19)
Has newborn	0.09	0.02	0.10	0.02	0.10	0.01
	(0.29)	(0.13)	(0.30)	(0.14)	(0.30)	(0.12)
Oldest child age (if any)	8.71	11.20	9.06	10.67	8.71	11.40
	(6.14)	(6.44)	(6.41)	(6.47)	(6.14)	(6.50)
Youngest child age (if any)	5.70	8.90	5.84	8.41	5.68	9.16
	(5.49)	(6.34)	(5.70)	(6.24)	(5.48)	(6.48)
Annual labor income	27,125	20,590	28,331	20,751	27,637	20,805
	(32,375)	(21,211)	(33,831)	(19,331)	(33,556)	(20,260)
Annual unemployment income	1,737	1,720	1,114	1,451	1,731	1,739
	(4,652)	(4,100)	(4,152)	(3,881)	(4,701)	(4,116)
Received some unemployment	0.26	0.29	0.15	0.24	0.26	0.29
	(0.44)	(0.45)	(0.36)	(0.43)	(0.44)	(0.45)
Urban area population - Origin	3,615,967	2,404,701	3,692,419	2,630,196	3,609,156	2,478,014
	(5,194,276)	(4,393,591)	(5,233,260)	(4,579,706)	(5,193,786)	(4,445,277)
Urban area population - Destination	1,239,380	1,711,544	1,197,212	1,439,880	1,238,303	1,739,745
	(2,891,209)	(3,645,677)	(2,821,426)	(3,291,056)	(2,895,170)	(3,657,715)
Number of individuals	492884	378638	379774	498918	448260	499688

Note: Standard deviation in parenthesis. This table is computed from Fideli data. All variables are measured in the year preceding the move. The sample is made of individuals living in an urban area in mainland France, aged 25-60, who moved once between 2014-2019 without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped.

3. Empirical approach

I estimate the impact of moving on income using the staggered difference-in-differences estimator developed by Callaway et al., 2021. Because moving is not an exogenous event, I cannot use a traditional difference-in-differences approach using non-movers as a control group. Households may choose to move for many reasons, but many will be related to either labor market events (new job opportunity, unemployment spell...), or change in household composition (birth of a child, marriage...) which can themselves affect labor market outcomes. The decision to move is made in accordance to both realized shocks, anticipated ones, and other life choices, making households who change city very different to those who don't. Furthermore, households may anticipate the fact that they want to move in the close future, and adjust their labor supply accordingly. As a result, movers and non-movers are likely to differ both in characteristics and behaviors, and it is unlikely that their income would follow parallel trends absent treatment.

Instead, I use "not-yet-treated" individuals as my control group: people who move during 2014-2019 but have not moved by a given year. In its simplest version, the Callaway et al. (2021) estimator first groups treated individuals into cohorts based on their moving year. For each cohort and time period, it estimates treatment effects by comparing outcome changes between the treated cohort (e.g., wage evolution between 2014 and 2016 for 2015 movers) and not-yet-treated cohorts (e.g., wage changes between 2014 and 2016 for 2017-2019 movers). To analyze moving's dynamic effects on income, I employ their time-to-event aggregation approach, which computes weighted averages of group-time treatment effects by time from treatment.

I implement this estimator with covariates including age and indicators for children in different age groups (under 1, 3, 6, 10, and 18 years). Following Sant'Anna et al. (2020), I use their "double-robust" approach, which combines two covariate adjustments. First, it uses a regression method to model the conditional expectation of the evolution of the outcome variable for both groups. Second, it uses Abadie (2005)'s inverse probability weighting to model individuals' conditional probability of belonging to a certain group.

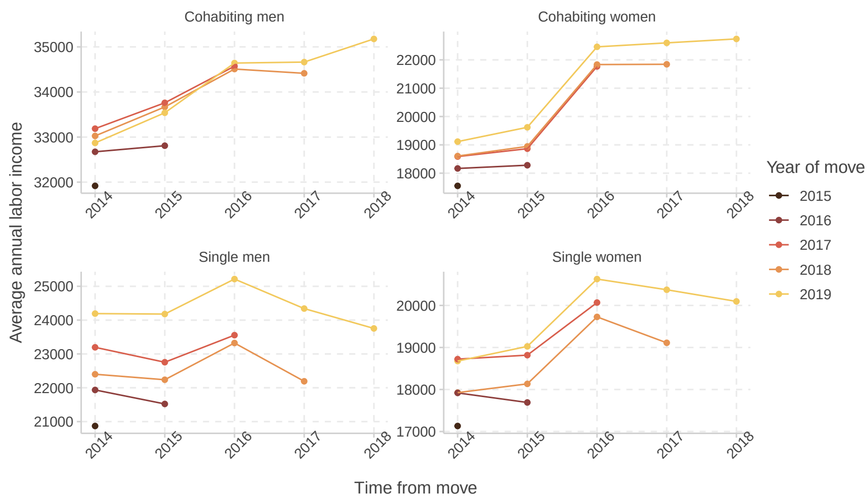
The key identifying assumption when using not-yet-treated cohorts as controls is conditional parallel trends: absent treatment and conditional on covariates, movers' income would have followed the same trend as those who haven't yet moved.

In order to assert the plausibility of this assumption, I plot unconditional pre-move trends by mover cohort in Figure 1.2 and 1.3. Figure 1.2 shows average annual labor income before moving, separated by gender and couple status at move time. The trends are remarkably similar across cohorts.[2] While this similarity in pre-move trends doesn't prove

[2]The sharp income increase in 2016 reflects a rise in individuals reporting salaried income. This discontinuity disappears when examining only individuals with non-zero labor income. Whether this represents a real change or data inconsistency is unclear, but since it affects all mover cohorts identically, it does not

the validity of the parallel trends assumption, it suggests that not-yet-movers serve as a plausible control group for analyzing the impact of moves on income.

Figure 1.2. Pre-move average income by gender and couple status



Note: This figure presents the pre-move average annual labor income by gender, cohabiting status, and year of move. It is computed from Fideli data. The sample is made of individuals living in an urban area in mainland France, aged 25-60, who moved once between 2014-2019 without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. Annual labor income is defined as the sum of wage and independent income (agricultural, industrial, commercial and non-commercial profit). Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing.

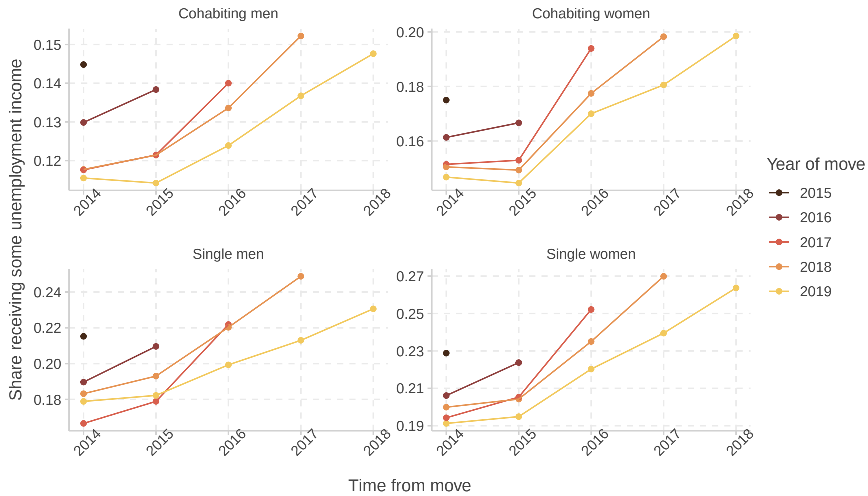
In Figure 1.3, I examine my second outcome variable: the share of individuals receiving unemployment income during the year. Here, the parallel trends assumption appears less tenable, as trajectories begin diverging one year before the move. This likely reflects the endogenous nature of moving decisions: households may be more likely to move following an unemployment shock, creating a correlation between unemployment status and treatment assignment. This endogeneity threatens a causal interpretation of the difference-in-differences estimates for this outcome.

However, my primary interest lies in analyzing gender *gaps* in moving effects, making my approach more analogous to a triple-difference. Since men and women exhibit similar pre-move anticipation patterns, any gender differences that emerge post-move likely stem from the move itself. Therefore, I will still apply the Callaway et al. (2021) estimator to unemployment outcomes, but shift my focus from the absolute treatment effects (which may be biased by endogeneity) to the gender gap in these effects.

Figures A3, A4, A5 and A6 in Appendix present more detailed descriptive graphs. For each mover cohort, I plot annual mean labor income and share receiving unemployment

threaten identification.

Figure 1.3. Pre-move share receiving some unemployment by gender and couple status



Note: This figure presents the pre-move share of people receiving non-zero unemployment income during the year by gender, cohabiting status, and year of move. It is computed from Fideli data. The sample is made of individuals living in an urban area in mainland France, aged 25-60, who moved once between 2014-2019 without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing.

insurance against their corresponding *not-yet-treated* control group, extending through post-move years. These graphs illustrate the variation I exploit to identify moving effects in my empirical strategy.

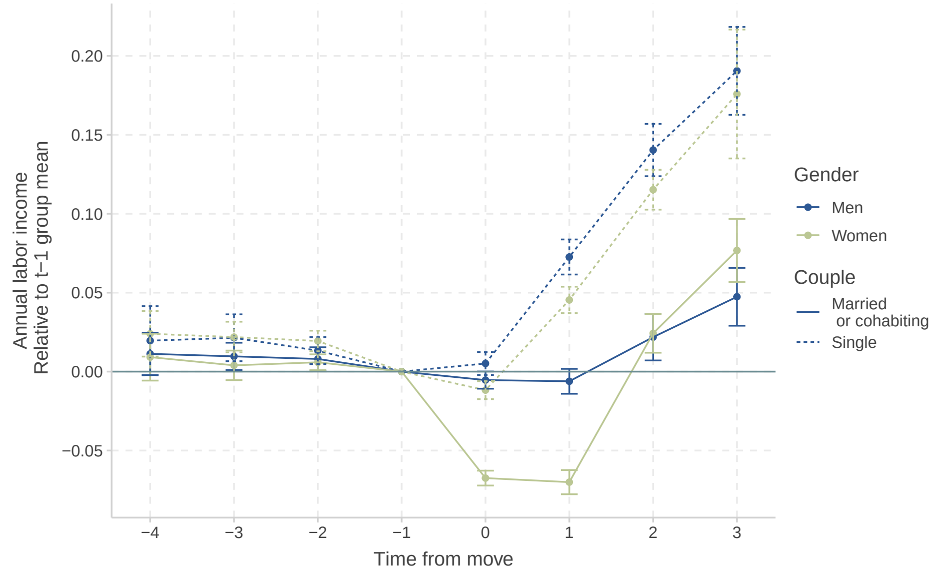
4. Effect of a move by gender and marital status

4.1. *Main results: The gendered effect of moving*

Figure 1.4 shows the dynamic effects of moving on labor income by gender and partnership status, relative to each group's average income in the year preceding the move. The results reveal a striking pattern: women in relationships are the only group who experience immediate income losses from moving, suffering an annual 7% decline in the first two years post-move. While these women eventually recover and see gains from mobility, their male partners benefit from the move after two years without experiencing initial losses.

The comparison with single movers provides additional insight. While there is a small gender gap in the effect of moves for single individuals, it is much smaller in magnitude, and the dynamic effect of relocation follows very similar trajectories for both genders. Both groups begin experiencing positive returns one year after moving, suggesting that gender

Figure 1.4. Effect of a move on annual labor income, by gender and couple status



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. Regressions are run separately by gender and couple status. Regressions use annual labor income in euros as outcome variable and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2. Annual labor income is defined as the sum of wage and independent income (agricultural, industrial, commercial and non-commercial profit). The number of observations for each regression is: single men: 644,409 (178,307 individuals); single women: 714,127 (185,332 individuals); cohabiting men: 1,062,644 (243,927 individuals); cohabiting women: 996,229 (239,690 individuals)

disparities in mobility returns primarily emerge within couples.

This contrast between single and coupled movers suggests that household location decisions differ from individual ones, though this comparison needs to made with caution given potential selection into marriage and systematic differences between single and partnered individuals. In particular, it is interesting to note that while coupled men gain more from moves than their female partners, they benefit less than single men do. This pattern could reflect the fact that, although household moves tend to prioritize male careers, men may not have complete control over location choices, leading to suboptimal outcomes for their own careers relative to what they could achieve without household constraints.

Another notable feature in Figure 1.4 is that, after two years, the dynamic effect of move becomes more positive for partnered women than for men. This could be partly explained by the fact that in France individuals who quit their job follow their partner qualify for up to two years of unemployment insurance. Consequently, while women may not initially benefit from the move, they have time to job search post-relocation, which could lead to better long-term outcomes than for men. It is important to note, however, that since Figure

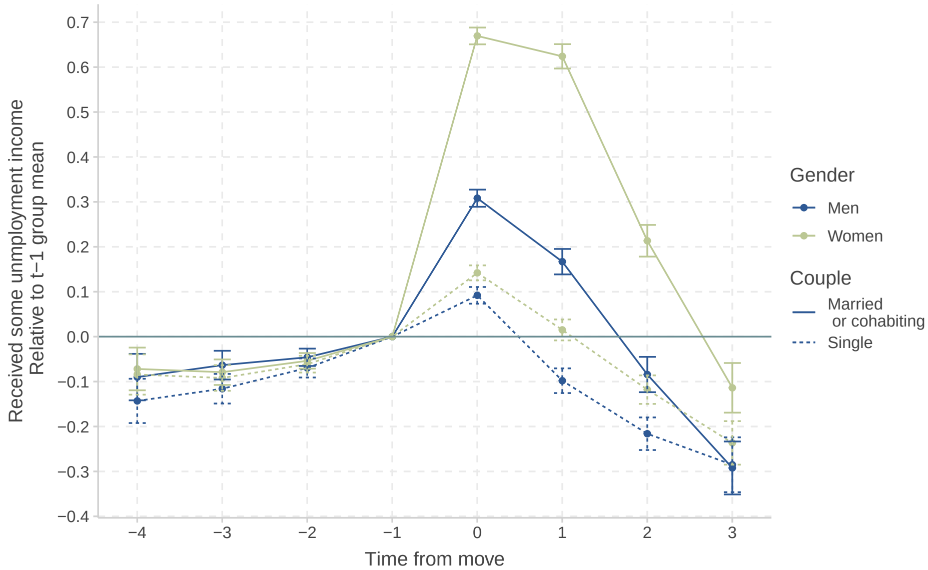
Figure 1.4 is based on income relative to pre-move levels, this relative improvement partly reflects the lower pre-move income of married women.

Because the outcome here is *annual* labor income, this pattern could stem from either intensive margin effects, where women find jobs with lower hourly wages, or extensive margin effects, where they work fewer hours annually post-relocation. Unfortunately, the FIDELI data lacks information on hours worked, preventing separate testing of these mechanisms. To approximate this distinction, I replicate the analysis using an indicator for receiving any unemployment benefits during the year. Although the variable is coarse and covers unemployment spells of various lengths, it helps study the likelihood to fall into unemployment around a move, which is an extreme case of reduction in hours worked. It is worth noting however that this indicator does not perfectly capture unemployment status, as some unemployed individuals do not receive benefits due to reasons like exhausting entitlements. Notably, individuals who quit their job to follow a partner are eligible for unemployment benefits even if they are cohabiting without being married.

Figure 1.5 presents results using this new measure. As anticipated from the descriptive statistics in Figure 1.3, significant pre-trends complicate the direct interpretation of treatment effect magnitude. However, in line with a triple-difference approach, it's notable that while pre-move estimates show no gender difference, a gap emerges immediately post-relocation, particularly for couples. Cohabiting women experience the largest and most persistent increase in unemployment probability after the move. Again, comparisons with single individuals (while limited by selection concerns) further suggest that household joint decisions yield different results than those of singles, with both coupled men and women experiencing worse post-move outcomes than their single counterparts.

Given the nature of the outcome variable, this result combines individuals with long periods out of the labor market and those experiencing only brief unemployment spells. To check if the previous finding is driven by married women having more frequent but shorter unemployment periods, I reproduce the estimation using the annual amount of unemployment income received as the outcome. This measure, which correlates with unemployment duration, helps differentiate the effect: if married women mainly experience brief, repeated unemployment spells, then the increase in receiving *some* unemployment income would be correspond to a much smaller effect on the total amount received. Figure B5 in Appendix shows that the gap between coupled men and women remains, and is even more pronounced and persistent when considering the amount of unemployment income.

Figure 1.5. Effect of a move on the probability of receiving some unemployment, by gender and marital status



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. Regressions are run separately by gender and couple status. Regressions use as an outcome an indicator for receiving UI, and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2. The number of observations for each regression is: single men: 644,409 (178,307 individuals); single women: 714,127 (185,332 individuals); cohabiting men: 1,062,644 (243,927 individuals); cohabiting women: 996,229 (239,690 individuals)

To further explore both the intensive and extensive margins of annual labor income, I examine in Figure B6 in Appendix whether married women are more likely to exit the labor force entirely. I replicate the analysis using an indicator for any labor income during the year. Interestingly, moving appears to negatively impact this probability for both partnered men and partnered women. Yet, no clear gender gap emerges, suggesting that the increase in unemployment probability shown in Figure 1.4 does not primarily result from married women leaving the labor force. In the medium term, about three years post-move, moving seems to raise the probability of receiving labor income by 4%, aligning with earlier results indicating that married women recover from relocation effects after two to three years.

Overall, these results indicate that joint location decisions within households generally favor men's career outcomes relatively to women. They suggest that partnered women are more likely to be "tied-movers" and experience longer adjustment periods after relocating, marked by income losses and an increased likelihood of receiving unemployment benefits. At the end of the third year post-move, the average annual effect on these women's careers remains negative, as seen in Figure A7 and Tables A1 and A2, which summarize the average treatment effects (ATT) by category. Each ATT coefficient corresponds to the average annual income change due to relocation after three years. Due to the time limitations in the data, I cannot confirm whether coupled women eventually see positive effects from relocation after additional years, though this seems plausible given the upward trend in the dynamic effect.

4.2. *A child penalty effect?*

In the previous analysis, I controlled for the presence of children in the household. However, gender differences in relocation outcomes may still be shaped by specific groups, such as those having children near the time of the move.

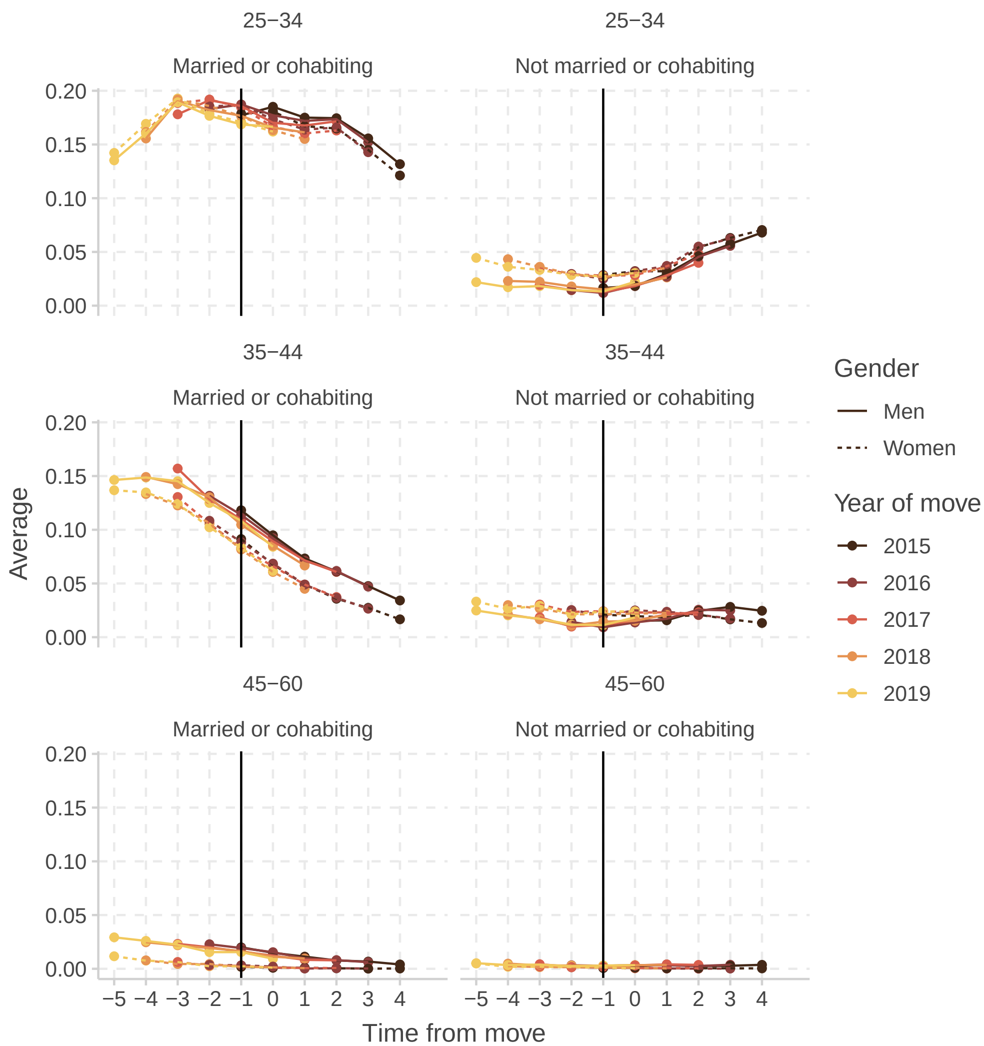
A large literature on the "child penalty" shows that women's careers are negatively affected by motherhood (Kleven, Landais, and Søgaard, 2019). Couples who recently had children and are making a location decision may hence choose to keep favoring the man's career, while couples who are considering having a child after moving could also anticipate the child penalty in their decision making. While this does not threaten my identification strategy, as long as moves and childbirth events are not perfectly correlated, it is interesting to understand how much of my main result can be interpreted as a facet of the child penalty.

Effects of move by age group

To assess the relevance of this channel in explaining gender differences in mobility outcomes, I first examine the relationship between relocation and childbirth. Figure 1.6 shows the share of individuals having a new child each year, by age at the time of relocation. Specifically, I divide movers into three age groups: 25 to 34, 35 to 44, and 45 to 60 years old. One initial observation from this figure is that while many movers, especially younger ones, have children around the time of relocation, there is no clear spike in childbirth in any year around the move. The figure also highlights that movers aged 25 to 44 years old are generally within their prime childbearing years, suggesting that relocation decisions for these groups could be more intertwined with family planning choices. The oldest group (45 to 60), however, is mostly beyond their childbearing years, so fertility considerations are less likely to influence their relocation decisions. Additional descriptive statistics for these age groups are available in Table A3.

Building on these observations, I next repeat the difference-in-differences analysis for each

Figure 1.6. Fertility around a move by age group

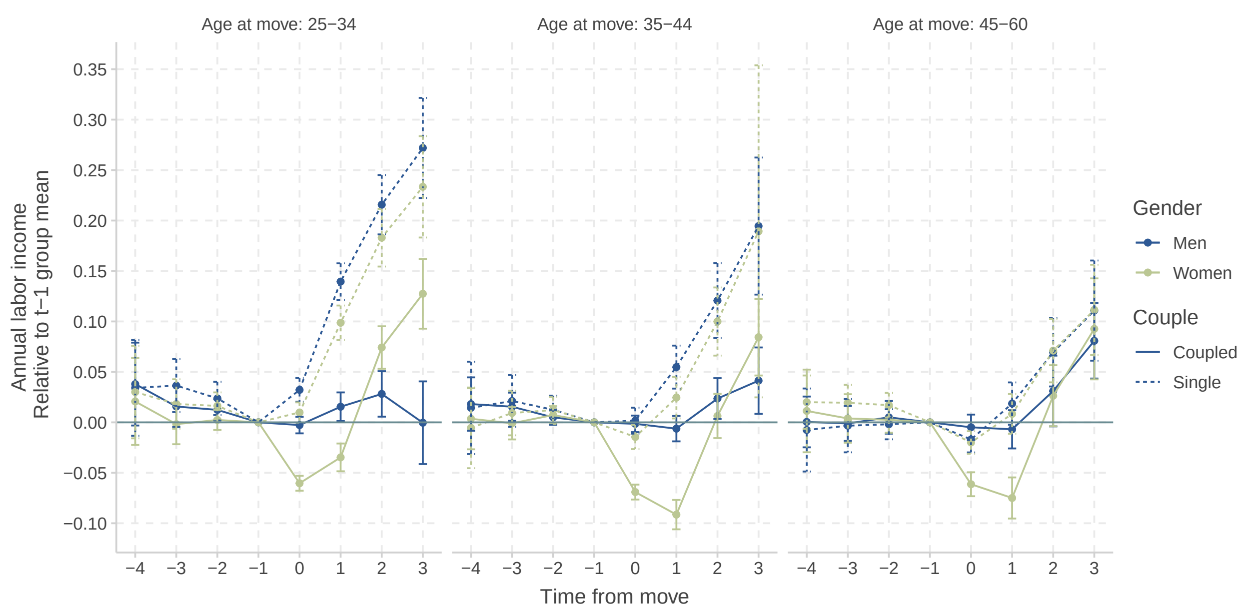


Note: This figure presents the share of individuals having a new child in a given year around a move, by gender, cohabiting status, year of move, and age at move. Age at move is grouped in three categories (25-34, 35-44, and 45-60). It is computed from Fideli data, on the sample described in section 2. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing.

age group separately. Figures 1.7 and 1.8 reveal that the main patterns emerge across age groups. For all groups, relocation is associated with a notable income loss for married women –around 5 to 10% annually inthe first two years post-move– while married men show no similar shock. Additionally, relocation more negatively affects women's employment status than men's. In the oldest group of movers (ages 45+), who are less likely to be making relocation decisions based on fertility, the observed gender gap in the economic impact of moving is less likely to be driven by the timing of childbirth and location decisions.

As before, the effect of moving on labor income turns positive for women after two years, particularly for younger movers. For younger movers, the combination of this catch up effect for cohabiting women and an almost null effect of moving for cohabiting men lead the total ATT on labor income, presented in Figure A9 to be approximately the same. The difference is also not significant for older movers, contrary to the 35-44 age groups for whom, three years after the move occurred, the average annual effect of relocation is still negative for women, and positive for men.

Figure 1.7. Effect of a move on annual labor income by age groups



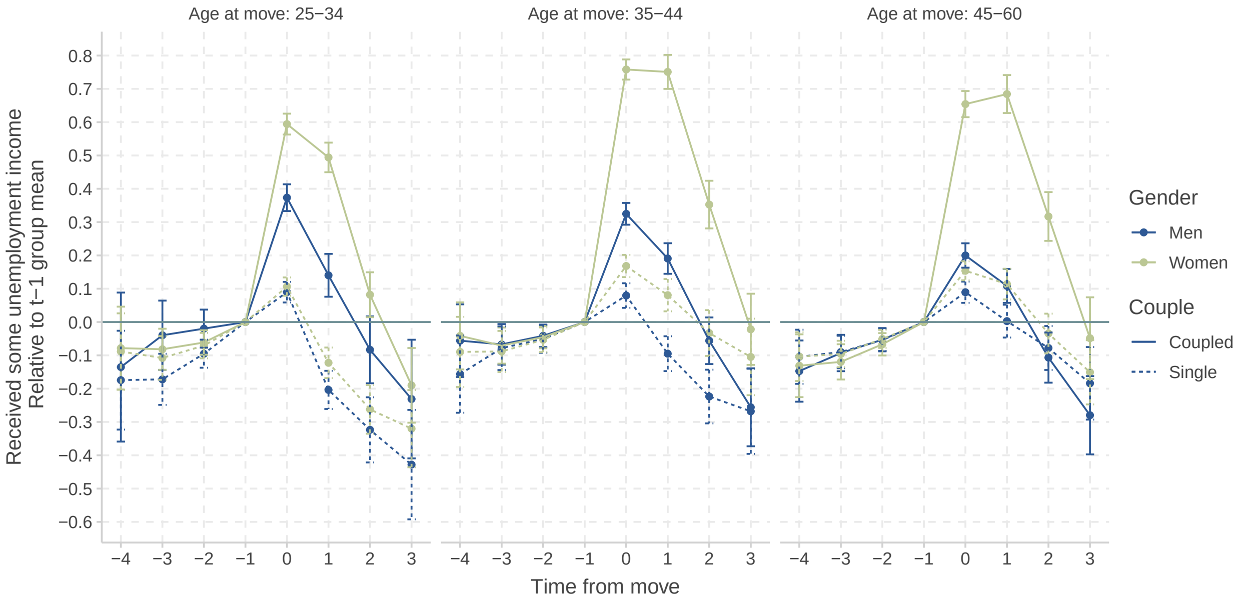
Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and age at move. Regressions use annual labor income in euros as outcome variable and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. Couple status is defined at the year of the move, as well as the age at move. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. The number of observations for each regression is: single men aged 25-34: 270,454; single women aged 25-34: 253,165; cohabiting men aged 25-34: 300,219; cohabiting women aged 25-34: 372,867; single men aged 35-44: 168,435; single women aged 35-44: 192,455; cohabiting men aged 35-44: 426,340; cohabiting women aged 35-44: 381,366; single men aged 45-60: 205,520; single women aged 45-60: 268,507; cohabiting men aged 45-60: 336,085; cohabiting women aged 45-60: 241,996

Effects of move by fertility profile

To further examine the role of children in the impact of relocation, I leverage the fertility information in my data to categorize the sample into four distinct groups:

(a) Movers without a child aged ten or younger at the time of the move, who do not have a new child afterward;

Figure 1.8. Effect of a move on the probability of receiving some unemployment by age groups



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and age at move. Regressions use as an outcome an indicator for receiving UI, and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The regressions are run separately by gender, couple status, and age at move. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2. The number of observations for each regression is: single men aged 25-34: 270,454; single women aged 25-34: 253,165; cohabiting men aged 25-34: 300,219; cohabiting women aged 25-34: 372,867; single men aged 35-44: 168,435; single women aged 35-44: 192,455; cohabiting men aged 35-44: 426,340; cohabiting women aged 35-44: 381,366; single men aged 45-60: 205,520; single women aged 45-60: 268,507; cohabiting men aged 45-60: 336,085; cohabiting women aged 45-60: 241,996

(b) Movers with a child aged ten or younger at the time of the move, who do not have another child afterward;
(c) Movers without a child aged ten or younger at the time of the move, who have one new child afterward;
(d) Movers with a child aged ten or younger at the time of the move, who have another child afterward.

I then apply the difference-in-differences estimation for each group separately, allowing a closer look at how the gender gap in relocation effects varies across these "fertility profiles."

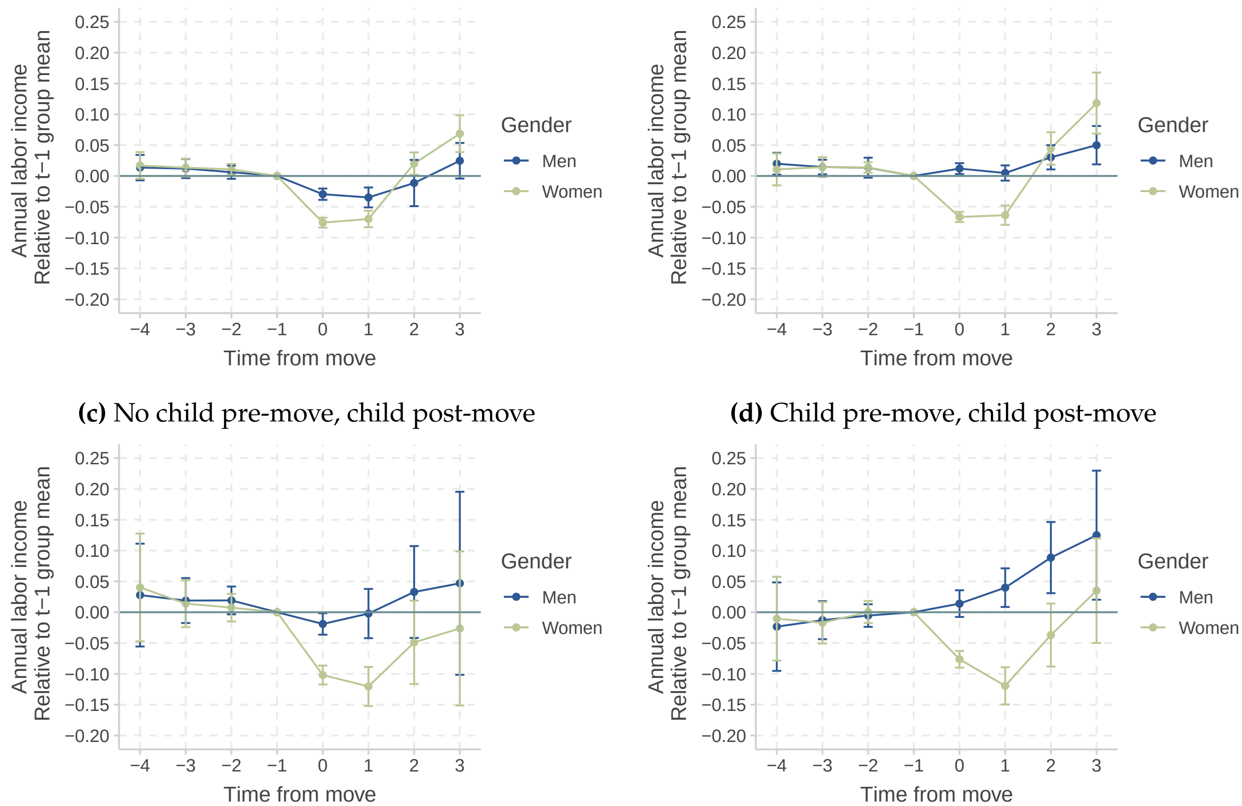
Yet it is worth noting that this approach also comes with two caveats. The first one is a measurement issue. While I can use the age of the youngest child in a household to identify all people who had a child in the ten years before the move (except for rare child mortality events), I only know whether someone has a child *after* moving for as long as they remain in the data. This means that, while I know the fertility of 2015 movers for four years post-move, I only know that of 2018 movers for one year post-move. As a result, my restrictions do not affect all cohort of movers in the same way, meaning that the control and treatment groups might be slightly different.

The second important limit of this approach is that I cannot use covariates for the presence of children in the household when splitting my sample based on fertility. This results from the way the Callaway et al. (2021) estimator uses variation in covariates among the control units to then partial out the variation in outcome that is due to the covariates. When I restrict households to those not having a child before moving, it means that there is no variation in fertility among control units (not-yet-movers) and I cannot apply this approach. For this reason, the results presented below only include age as a covariate. This is an important aspect to keep in mind: it means for instance that, by construction, in the third category, I am comparing a control group made of people who do not have a child (not-yet-treated) to a treatment group who are having children (as they are treated *and* have a child post-move). The effect I estimate is hence a combination of the effect of the move, and the child penalty effect. Still, since there is no clear spike in the probability of having a child exactly in the year of the move (as can be seen in Figure 1.6), the timing of two treatments is not be perfectly correlated. Furthermore, this limitation does not affect the estimation for the group of individuals who do not have children before or after the move, and who should be exempt from child penalty considerations at the time of the move.

Figures 1.9 and 1.10 presents the result of the estimation conducted on coupled movers only. Because there are not many single people who have children in the few years around they relocate, the very few observations in some categories mean that the estimation is very noisy and becomes hard to read. Figures A11 and A12 in Appendix present the results including single movers. While the ten years-old threshold is arbitrarily chosen, I show in Online Appendix B.4 that using different age thresholds (6 or 3 years-old respectively) yields very similar results.

If household relocation decisions are indeed influenced by either existing income losses from children or the anticipation of a future child penalty, we would expect different effects across these household types. Specifically, the gender gap should be widest for households that relocate between two childbirth events. In these cases, the birth of a first child may have already shifted the household's focus toward the man's career, and the prospect of another child could reinforce this priority. This is precisely what we observe in panel (d) of Figure 1.9, where the gender gap in the impact of relocation is largest and most

Figure 1.9. Effect of a move on annual labor income, by fertility profile



(a) No child pre-move, no child post-move

(b) Child pre-move, no child post-move

(c) No child pre-move, child post-move

(d) Child pre-move, child post-move

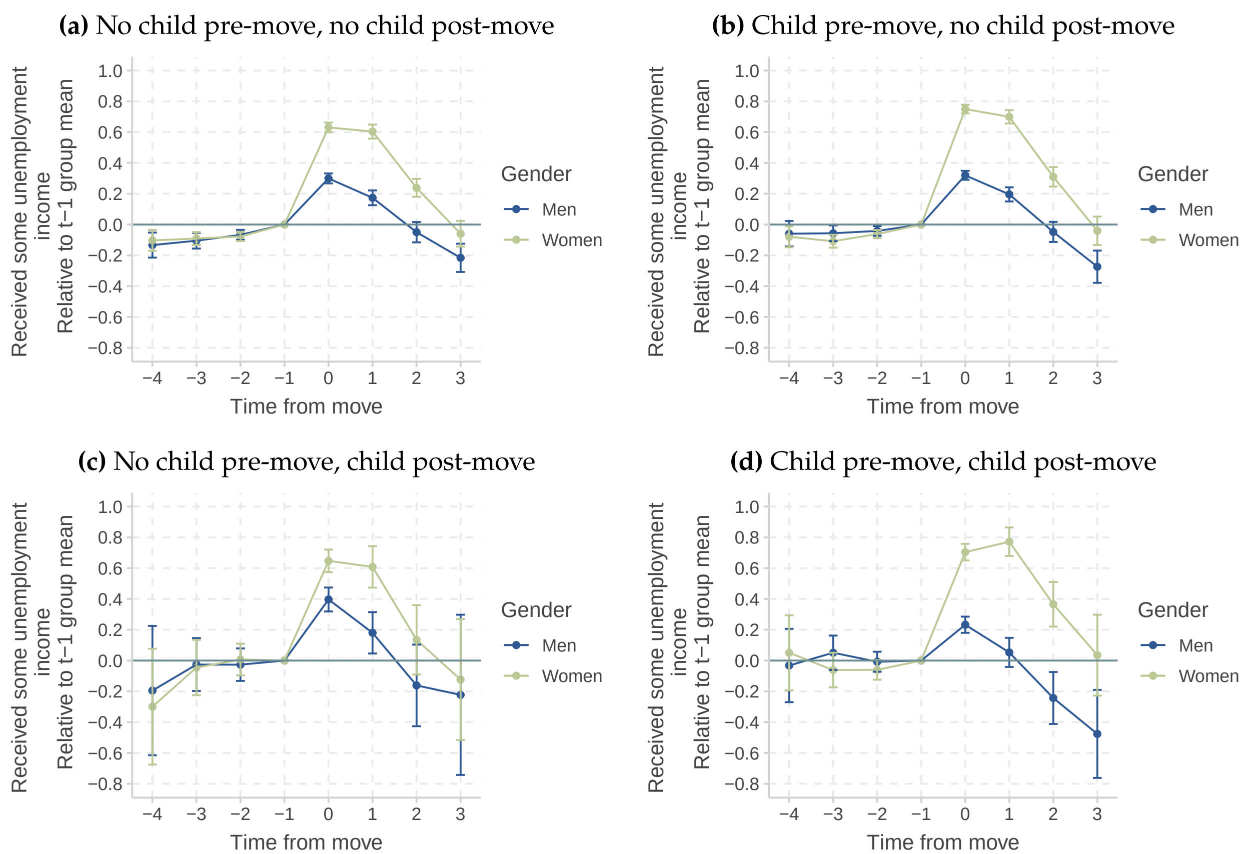
Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use annual labor income in euros as outcome variable and include age as a covariate. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged ten or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged ten or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged ten or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged ten or younger at the time of the move, who have another child afterward The number of observations for each regression is: cohabiting men of panel (a): 373,284; cohabiting women of Panel (a): 350,874; cohabiting men of panel (b): 458,226; cohabiting women of panel (b): 427,676; cohabiting men of panel (c): 93,690; cohabiting women of panel (c): 91,347; cohabiting men of panel (d): 137,444; cohabiting women of panel (d): 126,332

persistent out of all groups.

Conversely, we would expect the smallest gap among households without a young child at the time of the move and who do not have one afterward. Although some of these couples may have had a child more than ten years ago, any career impact on the woman should have largely stabilized by the time of relocation. As shown in panel (a) of Figure 1.9, a small gap appears at the time of the move even for this group. Some women in this group may have had a child over ten years before, and the associated income loss might have reduced these women's bargaining power, indirectly contributing to the observed gap. However, there is no reason to attribute this difference to a recent or anticipated child penalty for this group.

Panels (b) and (c) reveal that the gender gap is larger for couples who have a child post-move (panel (c)) than for those who had a child before moving (panel (b)). This is again consistent with the fact that, as discussed previously, the estimate in panel (c) will capture by construction some of the impact of having a child on female income.

Figure 1.10. Effect of a move on unemployment status, by fertility profile



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use as an outcome an indicator for receiving UI and include age as a covariate. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) correspond to coupled movers without a child aged ten or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged ten or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged ten or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged ten or younger at the time of the move, who have another child afterward The number of observations for each regression is: cohabiting men of panel (a): 373,284; cohabiting women of Panel (a): 350,874; cohabiting men of panel (b): 458,226; cohabiting women of panel (b): 427,676; cohabiting men of panel (c): 93,690; cohabiting women of panel (c): 91,347; cohabiting men of panel (d): 137,444; cohabiting women of panel (d): 126,332

The same patterns emerge when examining the effect of relocation on the likelihood of receiving some unemployment benefit. As shown in Figure 1.10, across all fertility profiles, women are consistently more likely than men to experience unemployment as a result of moving. Again, the effect is largest in households relocating between two childbirth events, and in households who have a child post-move but no child pre-move. This time however, the gap is also large and significant for households who do not have children at all around a move. The corresponding annual ATT can be found in Figure A14 in Appendix.

Overall, results in this section support the idea that the effect of joint location decisions and childbirth can exacerbate each other. Once households have chosen to prioritize men's labor market outcomes, and women have started to sacrifice their career, it is more likely they will make a similar choice again. Yet it is not the case that the entirety of the gender gap in returns from relocation can be explained by simultaneity of relocation and fertility events. In Appendix A.5, I also show that excluding households which undergo a matrimonial event around a move does not significantly affect my results.

5. The role of gender norms

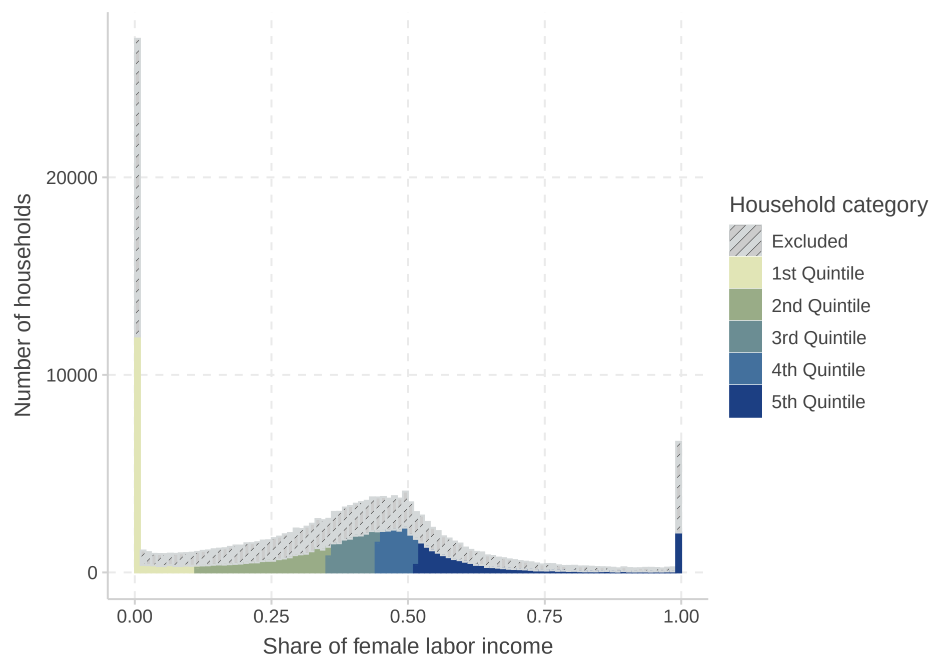
Two different non-exclusive decision making process could explain the occurrence of a gender gap in the returns to moving. A first explanation could be that, when households make their location choices, their objective is to maximize total household income. Prioritizing the career of the primary earner would then be a way to ensure that the costs incurred by the tied-mover are compensated by the benefits earned by the primary earner, whose returns from moving should be higher on average. If men are more likely to be the main earner of their household, this would translate in a general pattern where they benefit more from moves than women. An alternative explanation could be that the persistence of gender norms in which men should be the ones in charge of providing for their family could lead households to put more weight on men's career in their decision making, even in cases where women are not out-earned by their partner.

In order to disentangle these two mechanisms, I distinguish different types of households, based on the gender of the primary earner. If households made location decisions based entirely on maximizing total income, and were not influenced by gender norms, we should see that on average moving has a positive impact on the total labor income in the household. We would also expect to find that in general the primary earner of the household is the one that benefits the most from relocating. In this section, I show that this is not the case. For most couples, moving leads to a loss of total labor income, and men benefit more from moves than their partners, even in couples in which they are the lesser earner.

5.1. *Defining the primary earner*

Before testing these predictions, I first need to classify households according to which member is the primary earner before they move. An important challenge to this classification is that, because I only observe realized income I may mis-classify households in which one member experienced a bad shock in a given year but still has the higher earning *potential*. A high-skilled individual who falls into unemployment or becomes sick and earns little labor income for a year could still have a higher potential return from moving than their partner. It could even be that households in this situation choose to move specifically to allow a faster recovery of the individual who experienced such a negative shock. In their case, prioritizing the career of the lowest earning member could be consistent with maximizing the total household income.

Figure 1.11. Distribution of share of income earned by woman in year preceding the move



Note: This figure presents the distribution of the share of female labor income (female annual labor income divided by total household annual labor income). Excluded households in grey are those for which at least one member experienced before they moved a yearly change in labor income below the tenth or above the ninetieth percentile of the distribution for their gender. The figure is computed from Fideli data. The sample is made of households who moved while in a couple between 2015-2019. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped.

To avoid this pitfall, I focus on households in which both members have a relatively stable income before they move, in order to exclude individuals whose status as a low earner is a result of a temporary shock. More precisely, I calculate the absolute change in labor income for each individual by year and gender. I then exclude households where one member experienced an income change, in any pre-relocation year, that falls below the 10th or above the 90th percentile of the distribution for their gender and year. Table A4 in Appendix provides the corresponding bounds, as well as the median change in yearly income for each category. This restriction also means that I exclude households who moved in 2015, as I only observe income one year prior to their move, and cannot measure how their income changes for any year before the move. This leaves me with over seventy five thousand households.

For the remaining households, I compute the share of the household's labor income earned by the female member.

Finally, I group households by quintiles of this share in the year immediately preceding the move. Figure 1.11 shows the distribution of the share in the year immediately preceding a move, together with household category. Table 1.2 shows some descriptive statistics at the household level for each of the household categories, in the year preceding the move.

With a share of labor income earned by the woman of 1% on average, the First Quintile category is one in which men are very clearly the primary earner. As could be expected, this category is the most likely to have children. They also have the lowest average total income. It is worth noting that the average female income is so low in this group of households (at €594 annually), that it is almost impossible for it to decrease, and that the effect of moves on female income is almost bounded to be above zero. In the second and third quintiles, the average share of female labor income remains below half, at 26 and 40% respectively. Households in those two categories are slightly younger and less likely to have children than in the first one, and earn over €20,000 more in total annual income. The fourth quintile corresponds to a category of almost egalitarian couples: women in these households earn on average 48% of the total household income. Finally, in the fifth quintile, women are the primary earner of their household, with a share of female labor income of 66% on average. Most men in these households seem to participate in the labor market, and their average income of €18,000 is above the annual salary of a full-time minimum wage worker – and significantly higher than the €594 earned by women of the first quintile. While households in the first category, and to a lesser extent those in the last category, seem to differ on important characteristics (in particular total household income) the three middle groups appear to be very similar except for the within household distribution of income.

Table 1.2: Descriptive statistics by household category

Variable	First quintile	Second quintile	Third quintile	Fourth quintile	Fifth quintile	Excluded households
Female labor income	594	14,903	23,041	27,076	30,146	19,123
	(1,523)	(8,263)	(8,123)	(9,757)	(14,599)	(21,244)
Male labor income	34,406	40,848	34,348	29,410	18,243	34,979
	(24,569)	(19,496)	(12,126)	(10,565)	(12,113)	(52,252)
Share female labor income	0.01	0.26	0.40	0.48	0.66	0.37
	(0.03)	(0.07)	(0.03)	(0.02)	(0.16)	(0.27)
Total household labor income	35,000	55,751	57,389	56,486	48,389	54,102
	(25,091)	(26,385)	(20,005)	(20,164)	(24,223)	(58,398)
Woman age	40.23	39.55	37.59	36.90	38.31	37.55
	(8.71)	(8.42)	(7.88)	(7.78)	(8.17)	(8.44)
Man age	42.58	41.36	39.30	38.36	40.00	39.50
	(8.49)	(8.41)	(8.04)	(8.02)	(8.53)	(8.69)
Have minor child	0.75	0.72	0.72	0.68	0.67	0.69
	(0.43)	(0.45)	(0.45)	(0.47)	(0.47)	(0.46)
Have child under 10	0.59	0.53	0.58	0.56	0.53	0.57
	(0.49)	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)
Have child under 6	0.46	0.39	0.45	0.45	0.41	0.46
	(0.50)	(0.49)	(0.50)	(0.50)	(0.49)	(0.50)
Have child under 3	0.28	0.23	0.29	0.29	0.25	0.31
	(0.45)	(0.42)	(0.45)	(0.45)	(0.43)	(0.46)
Have newborn	0.07	0.09	0.10	0.10	0.09	0.10
	(0.26)	(0.28)	(0.31)	(0.30)	(0.29)	(0.31)
Ever changed household 2014-2019	0.03	0.06	0.08	0.09	0.09	0.08
	(0.18)	(0.23)	(0.27)	(0.29)	(0.28)	(0.27)
Urban Area Population - Origin	2,458,095	3,148,324	4,146,253	4,413,465	4,009,783	3,936,034
	(4,459,540)	(4,924,845)	(5,436,536)	(5,539,139)	(5,383,556)	(5,331,375)
Urban Area Population - Destination	1,330,078	1,090,379	948,368	988,904	991,192	1,231,624
	(3,155,094)	(2,643,613)	(2,301,447)	(2,383,177)	(2,426,161)	(2,861,047)
Number of households	15488	15488	15488	15488	15489	104653

Note: Standard deviation in parenthesis. This table is computed from Fideli data. All variables are measured in the year preceding the move. The sample is made of households who moved while in a couple between 2015-2019. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped.

5.2. Results by primary earner

Total income

If couples abstracted from gender consideration in their decision making, and only tried to maximize total household income, we should find that on average couples move when the gain to one partner compensates for the cost incurred by the other, and that moving has a positive impact on the total labor income of the household. I test this prediction in Figure 1.12, by applying my difference-in-differences set-up to the total labor income of the household, for each of the five categories, and show that this is not the case. For households in which the man was almost the only earner before the move, in panel (a), it does appear that moving leads to an immediate income gain for the household. In households where women are the primary earner before the move, the picture is less clear-cut, with moves leading to a small loss of income on average in the first two years of the move, before it becomes positive. For all other types of households however, moving has a large and significant negative effect, which peaks in the year following the move, when households experience a 10% income loss. Table A5 summarizes this by showing that over the first three years of the move, households in the three middle quintiles experience an average income loss of 2500 to 3700 annual euros (4.5 to 6.6% of pre-move income). Household in the first quintile however gain an average €1900 annual income (5.4 % of pre-move income), and moving has a non-significant effect for households in the last quintile. This supports the idea that households facing strong dual-constraints in their decision making, with relatively similar income, find it more difficult to optimize their decisions.[3]

In order to further explore the role of gender norms, I now study how this total income loss is distributed among household members,

Income by gender

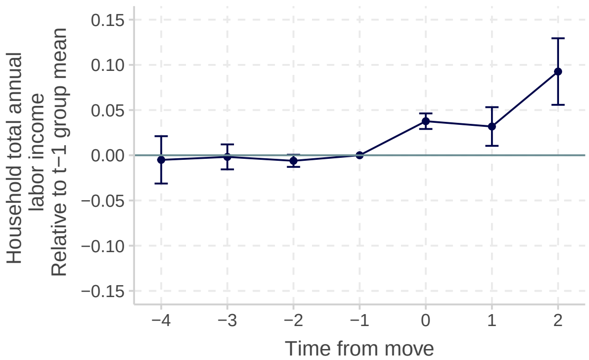
Under the reasonable assumption that individuals with high-earnings have higher expected returns from moving, we should expect primary-earners to benefit more from moves on average. If households make their location decision based only on the total household income, they should be more likely to move for the career of the primary earner, who could compensate their partner for the loss they incur. In the egalitarian households of the fourth quintile, we should see a more balanced effect: men should be as likely as women to be the tied-mover in their relationship. In the fifth quintile, we should even see the reverse pattern, with women benefiting more from the move on average as they were previously the highest earner. In Figure 1.13, I estimate the effect of moving on male and female labor income for each category of households.

The broad pattern that emerges from this Figure is that in all categories of couples where both members are working, women benefit less from moves than men. Earning approximately the same as their partner (in panel (d)), or being the primary earner of the household before the move (in panel (e)) does not lead to significantly different outcomes for women: moving is associated to to a 10 to 20% annual income loss, while their partners

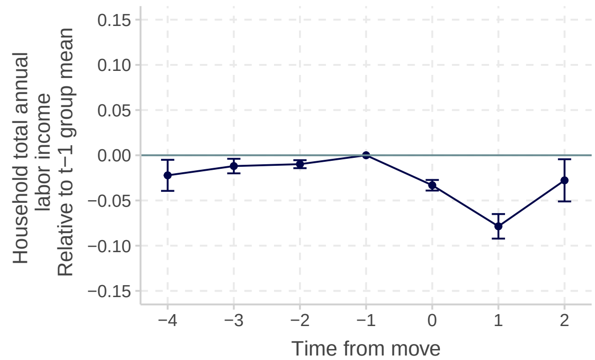
[3] It is worth noting however that this analysis relies on annual nominal income, and is not corrected for differences in local cost of living. Given that households move on average towards smaller cities, as can be seen in Table 1.2, it is possible that they experience an income loss which is compensated by a lower cost of living. Yet, the cost of living being the same by definition for men and women who live together means that the gender gap in nominal or real income is identical.

Figure 1.12. Effect of a move on total household income, by share of female labor income pre-move

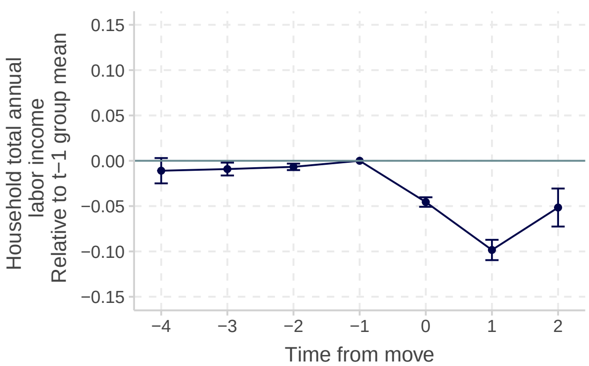
(a) First Quintile
(average share of female income 1%)



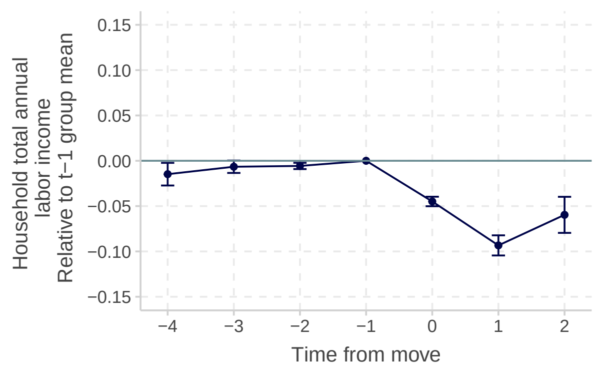
(b) Second Quintile (average share of female income 26%)



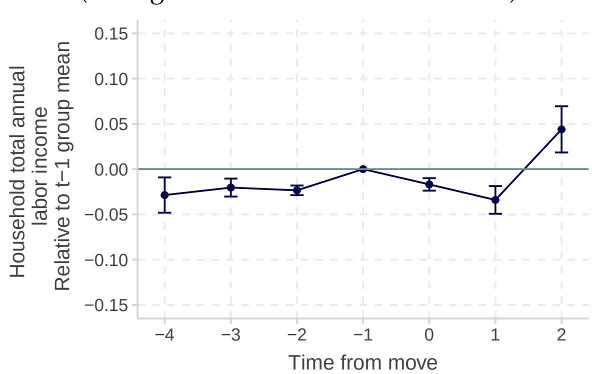
(c) Third Quintile
(average share of female income 40%)



(d) Fourth Quintile
(average share of female income 48%)



(e) Fifth Quintile
(average share of female income 66%)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender and share of female income category (quintiles). Regressions use household total annual labor income as an outcome, and include covariates for average age of couple members, and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. The sample is made of households who moved while in a couple between 2015-2019. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. The number of observations for each regression is: Panel (a): 73,028 (15,488 households); Panel (b) 73,836 (15,488 households); Panel (c): 73,700 (15,488 households); Panel (d): 73,271 (15,488 households); Panel (e) 73,006 (15,489 households) either benefit from the move (in panel (e)) or experience much smaller losses.

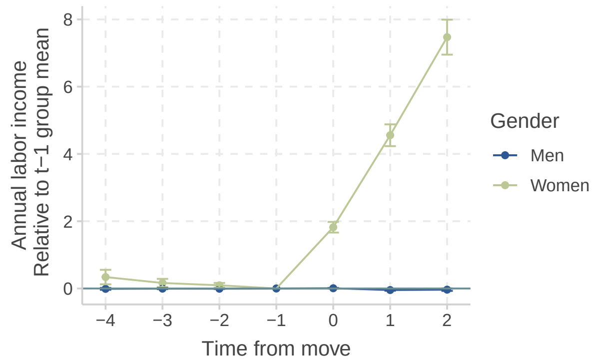
The only category in which women appear to gain more from moving than their partner is the first group, in which they are by far the lowest earner. The result suggest that for women of this group, moving leads to a striking 800% increase in income. While this result seems counter-intuitive at first, two important notes must be made before interpreting it. First, as discussed above, the pre-move income for women in the first category is so low that the scaled estimate, while significantly positive, becomes mechanically much higher than for any other categories. Results expressed in absolute value, which are easier to read for this category in particular, can be found in Figure A17. As can be seen, the 800% higher income in the second year post-move corresponds to a reasonable €5,000 annual income gain. Furthermore, 67% of women in these households earn no income at all before moving, which means that, for them, the estimate is bounded at zero and cannot be negative.

To better visualize how the gender gap in the effect of moves changes with quintiles, I plot in Figure 1.14 the average annual effect of moves at the end of the second-year post move, by category. The background color shows the bounds of each quintile. Each point represents the average annual treatment effect over the first three years of the move, for the corresponding quintile and gender, and is positioned horizontally according to the average share of female income pre-move in the quintile. Because the percent effect is large for women of the first group, for reasons previously explained, the y-axis is split in two, for readability. Again, a version of this graph showing results in annual euros rather than percent can be found in Appendix in Figure A18.

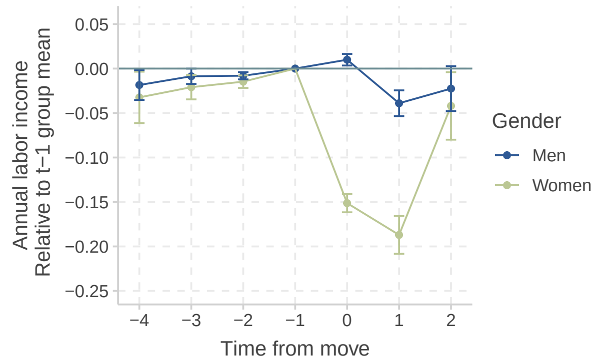
It is striking that, for men of the first four quintiles, moves are associated with an annual income loss which remains very stable, around 1 to 2% of pre-move income. It is only in the last category, where they are the lowest earner of their relationship before the move, that the effect of the move becomes positive, with an annual 10% income gain. For women however, there is more variation. When looking at quintiles above the first, there seems to be a positive correlation between the share of female labor income pre-move and the effect of move, which is consistent with households putting more weight on the career of women in their decision making, when they contribute significantly to the household income. This however is counterbalanced by the fact that, despite this positive correlation, women never experience an income *gain* due to the move, and the gap remains in all categories in favor of men. Interpretation of the results of the fifth quintile could be complicated by the fact that, despite my attempt to restrict to household who have stable income, part of the effect I am capturing may still arise from men who have lower income, but a high

Figure 1.13. Effect of a move on labor income by gender, by primary earner category

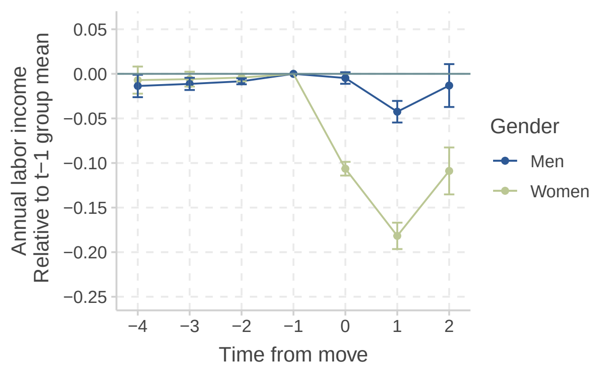
(a) First Quintile
(average share of female income: 1%)



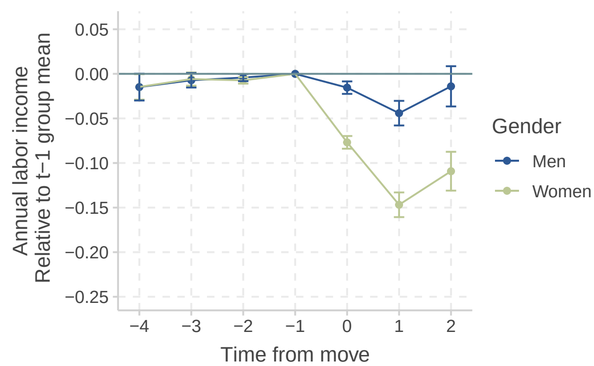
(b) Second Quintile
(average share of female income: 26%)



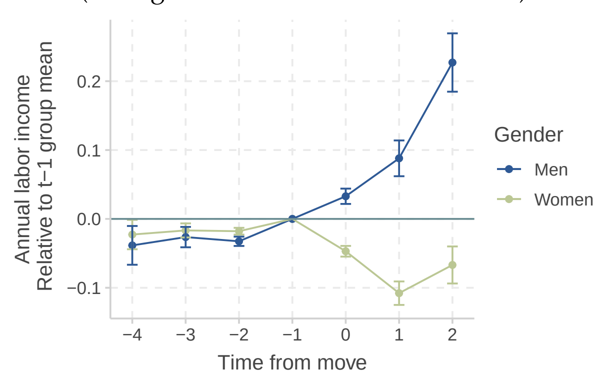
(c) Third Quintile
(average share of female income: 40%)



(d) Fourth Quintile
(average share of female income: 48%)

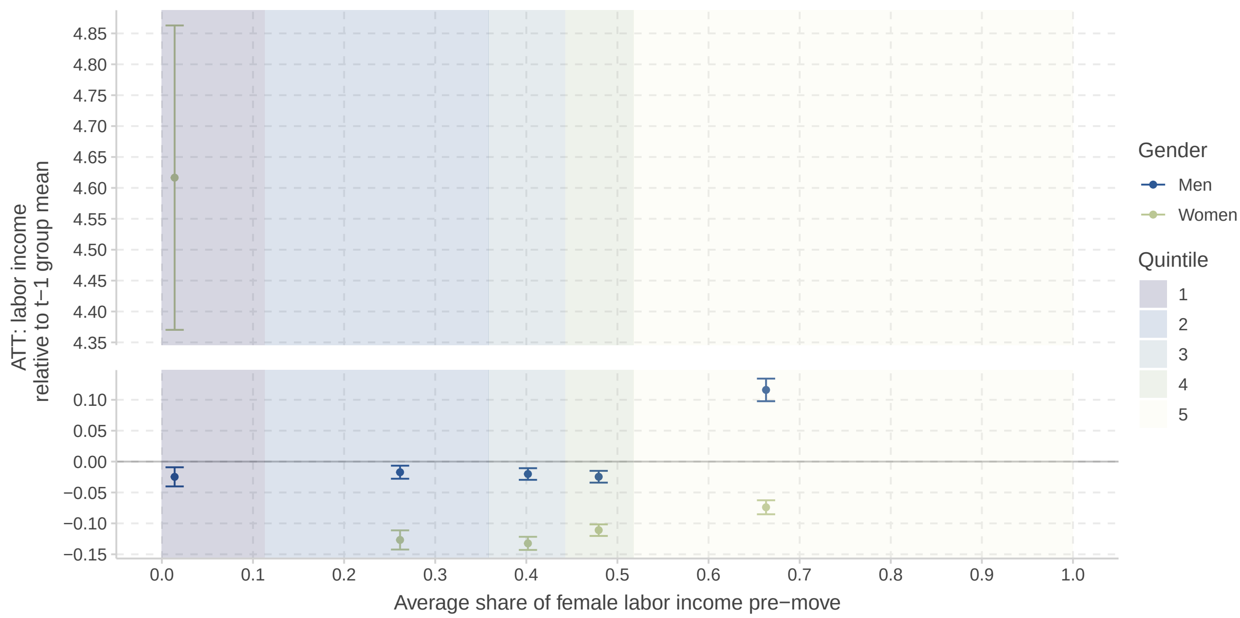


(e) Fifth Quintile
(average share of female income: 66%)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender and share of female income category (quintiles). Regressions use annual labor income as an outcome, and include covariates for average age of couple members, and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The regressions are run separately by gender. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. The sample is made of households who moved while in a couple between 2015-2019. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. The number of observations for each regression is: Panel (a): 73,028 (15,488 households); Panel (b) 73,836 (15,488 households); Panel (c): 73,700 (15,488 households); Panel (d): 73,271 (15,488 households); Panel (e) 73,006 (15,489 households) potential. This could explain both the striking positive returns to moves for men, and the gender gap in those returns. In the fourth quintile where couples are almost equalitarian however, this should not be as much as a concern. The persistence of a gender gap for both these category points to a role of gender norms in the decision-making of households.

Figure 1.14. Annual labor income ATT, by gender and primary earner quintiles



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. Regressions use annual labor income as an outcome, and include covariates for average age of couple members, and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The regressions are run separately by gender and share of female income category (quintiles). Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The plotted coefficient corresponds to the average annual treatment effect in the first three years of the move, relative to the group average one year before moving. The sample is made of households who moved while in a couple between 2015-2019. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. The number of observations for each regression can be found in the notes of Figure 1.13

In Online Appendix B.5, I show that the same patterns can be observed when splitting the sample into deciles rather than quintiles of share of female labor income.

6. Conclusion

Despite significant progress toward gender equality in recent decades, a persistent income gap between men and women remains. This gap largely reflects gendered patterns in intra-household division of labor, with women often reducing work hours after childbirth or selecting less demanding occupations. In this paper, I document another crucial mechanism through which household decision-making perpetuates income disparities: asymmetric returns to geographic mobility. Using comprehensive administrative data from France and a difference-in-differences framework, I show that residential moves generate substantial –though temporary– income losses and increased unemployment risk for partnered women, while benefiting their male partners. This gap is exacerbated by the interaction of location decisions with fertility choices. However, I show that the "child penalty" is not its primary driver.

While this result is in line with some of the previous literature, I am able to further explore the role of gender norms in this gap, by examining households based on the primary earner's gender. I show that even in households which were egalitarian before the move, or in which women were earning a larger share of the total household income, men benefit more from moves than their partner. This systematic prioritization of men's careers, even when they are not the primary earner, points to the persistent influence of gender norms in household decision-making rather than pure income maximization.

One puzzling result found in this analysis is the average negative effect of moves for households in the short run. Notably, this income loss is concentrated in households with relatively equal earnings and absent in households where women have almost no labor income. This pattern could reflect an efficiency-equity trade-off in household decision-making: while men may drive mobility decisions, they might still incorporate their partner's potential outcomes in their calculations. As a result, they may accept lower personal gains than what they could achieve if optimizing solely for their own career trajectory, effectively trading off some income gains for reduced within-household inequality.

A second potential explanation is that households are not solely maximizing their nominal income, but their overall utility, and hence take into account both amenities and cost-of-living in their decision making. This hypothesis is difficult to test due to the absence of local price index data in France, though one could be constructed from housing transaction data. However, accounting for cost-of-living would not affect the core finding of this paper regarding the role of norms in explaining the gender gap in returns from moving: men and women within the same couple move to the same places. Future research could explore this dimension by studying how differences in destination choices contribute to the gap in returns from geographic mobility between single men and single women, who may optimize for different local characteristics.

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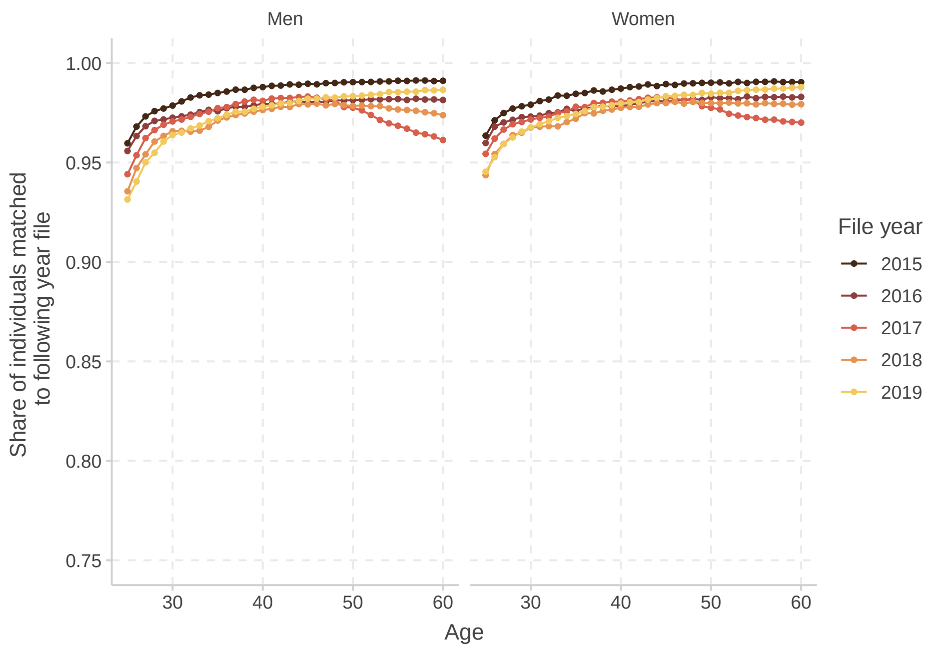
A. Main appendix

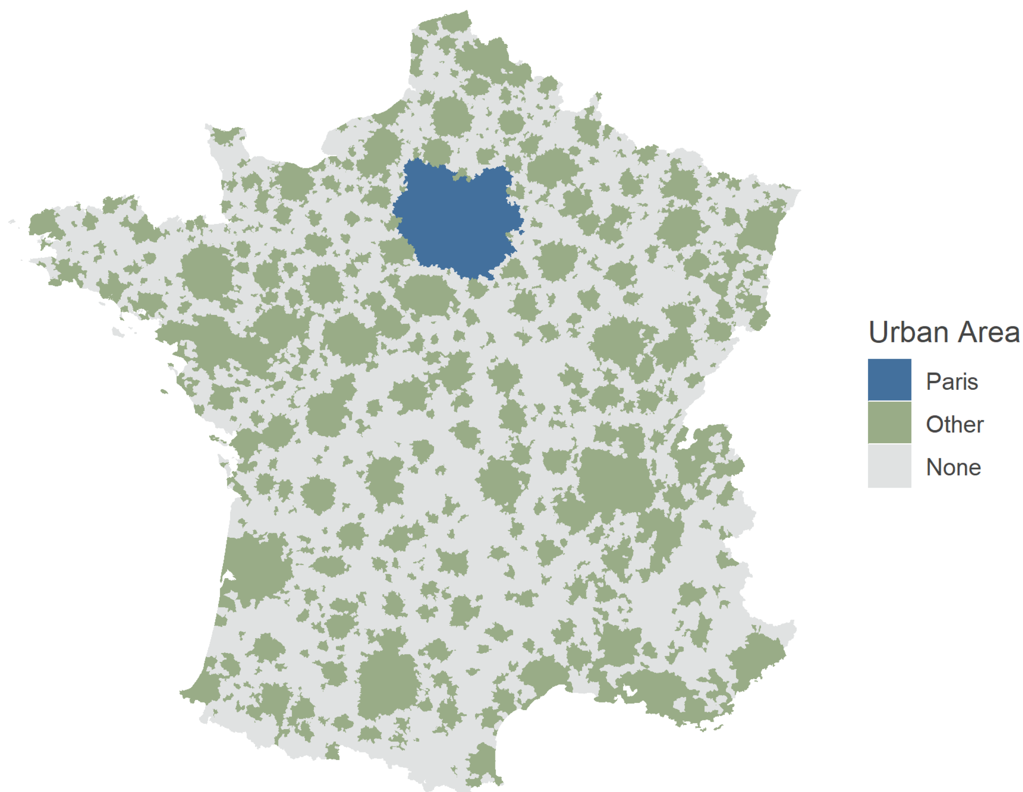
A.1. Descriptive statistics main sample

Figure A1. Share of individuals matched between consecutive files



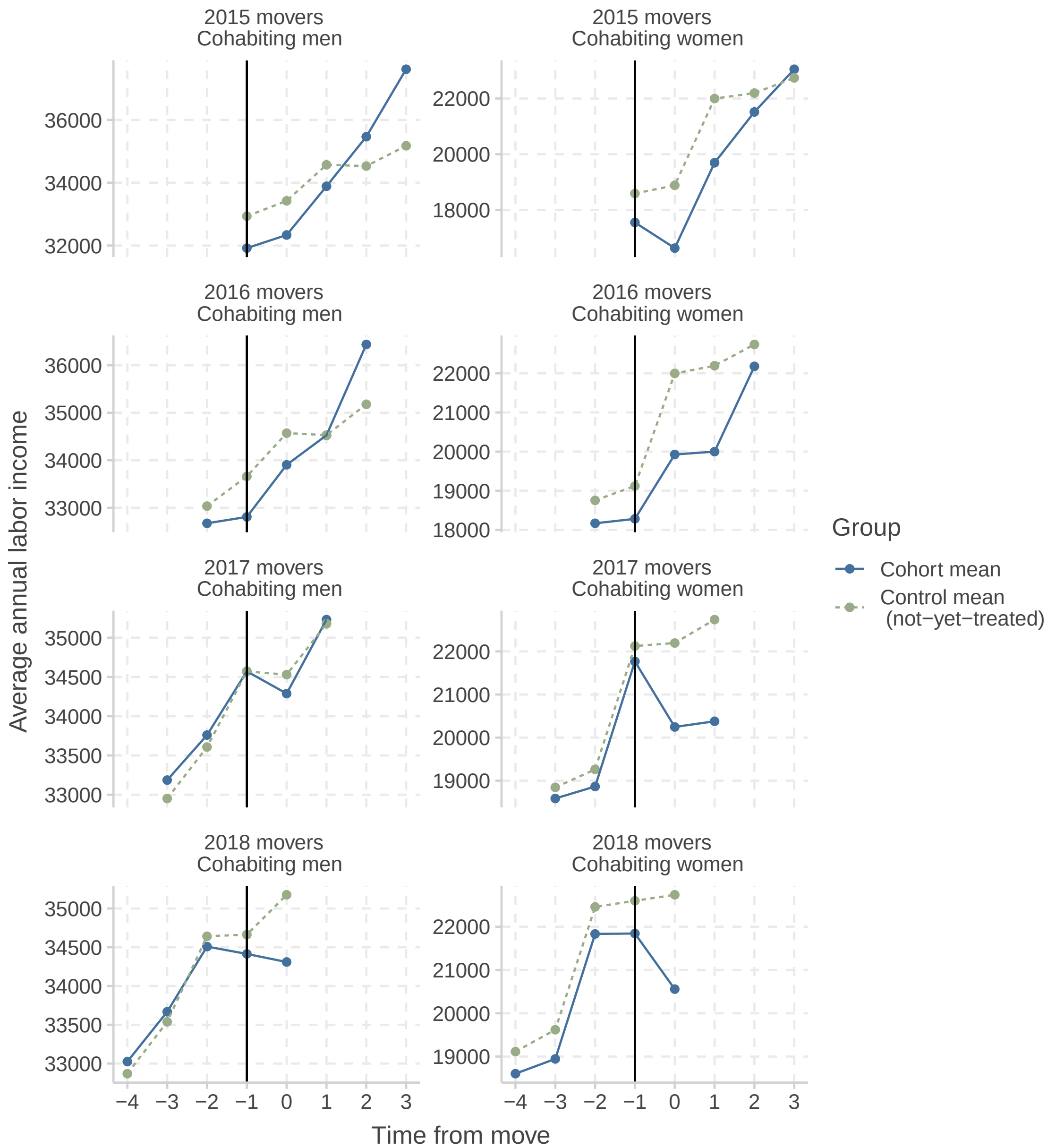
Note: This figure presents the results of the panelisation process described in section 2. It is computed from Fideli data. For Fideli files 2015 to 2019, it presents the share of individuals who were matched to a single individual in the following year file, by gender and age. The sample includes all individuals aged 25 to 60 living in a house or an apartment in France.

Figure A2. Map of Urban Areas in France



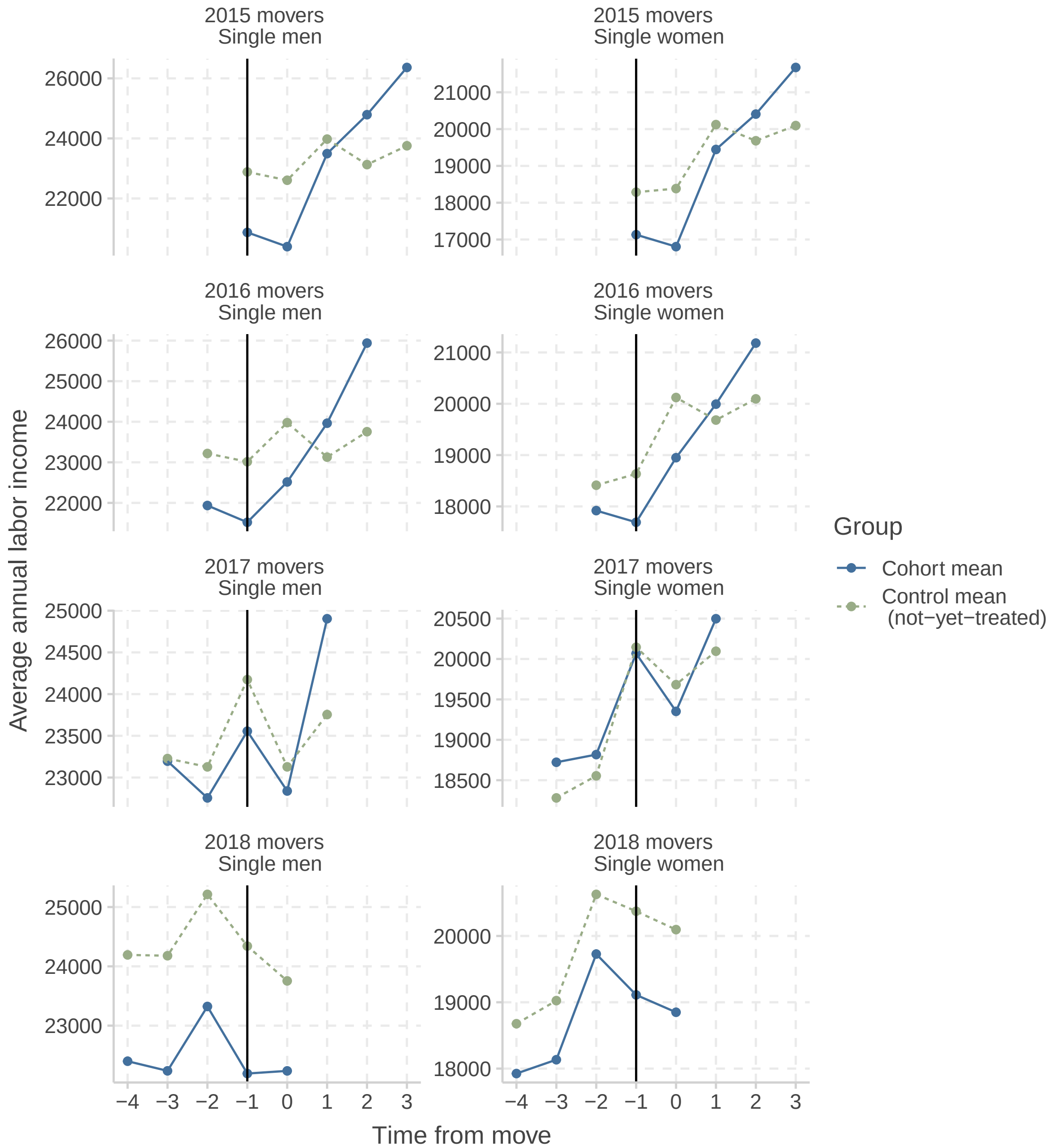
Note: This map presents the geography of French Urban Areas, with the Paris Urban Area highlighted in blue to distinguish it from neighbouring urban areas.

Figure A3. Labor income around a move, by cohort and treatment status, couples



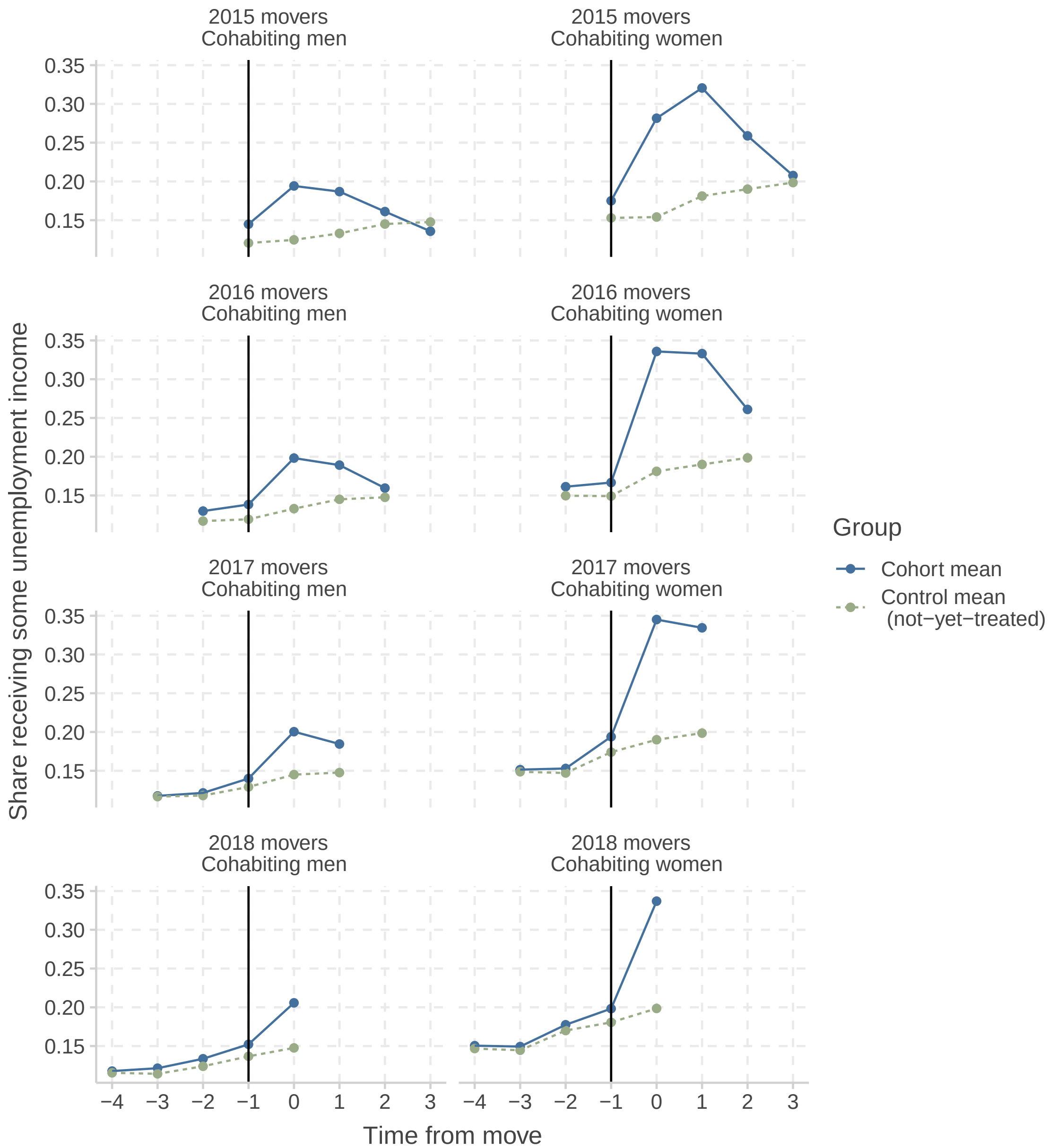
This figure presents the average annual labor income around a move by gender, year of move, and treatment group, for individuals who move while in a relationship. For each panel, the "Cohort mean" line corresponds to the average annual labor income of people who moved in that year. The "Control mean" line corresponds to the average annual labor income of individuals who have not yet moved at that point, defined in the same way as in Callaway et al. (2021). For pre-move years, not-yet movers are individuals who move after the considered cohort. For instance, in the 2017 panel, the not-yet movers at time 0 (year 2017) are defined as individuals who move in 2018 or 2019. The not-yet movers at time 1 (year 2018) are individuals who move in 2019. The not-yet movers at time -3 to -1 are individuals who move in 2018 or 2019. The figure is computed from Fideli data. The sample is made of individuals living in an urban area in mainland France, aged 25-60, who moved while in a relationship between 2014-2019 without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. Annual labor income is defined as the sum of wage and independent income (agricultural, industrial, commercial and non-commercial profit). Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing.

Figure A4. Labor income around a move, by cohort and treatment status, singles



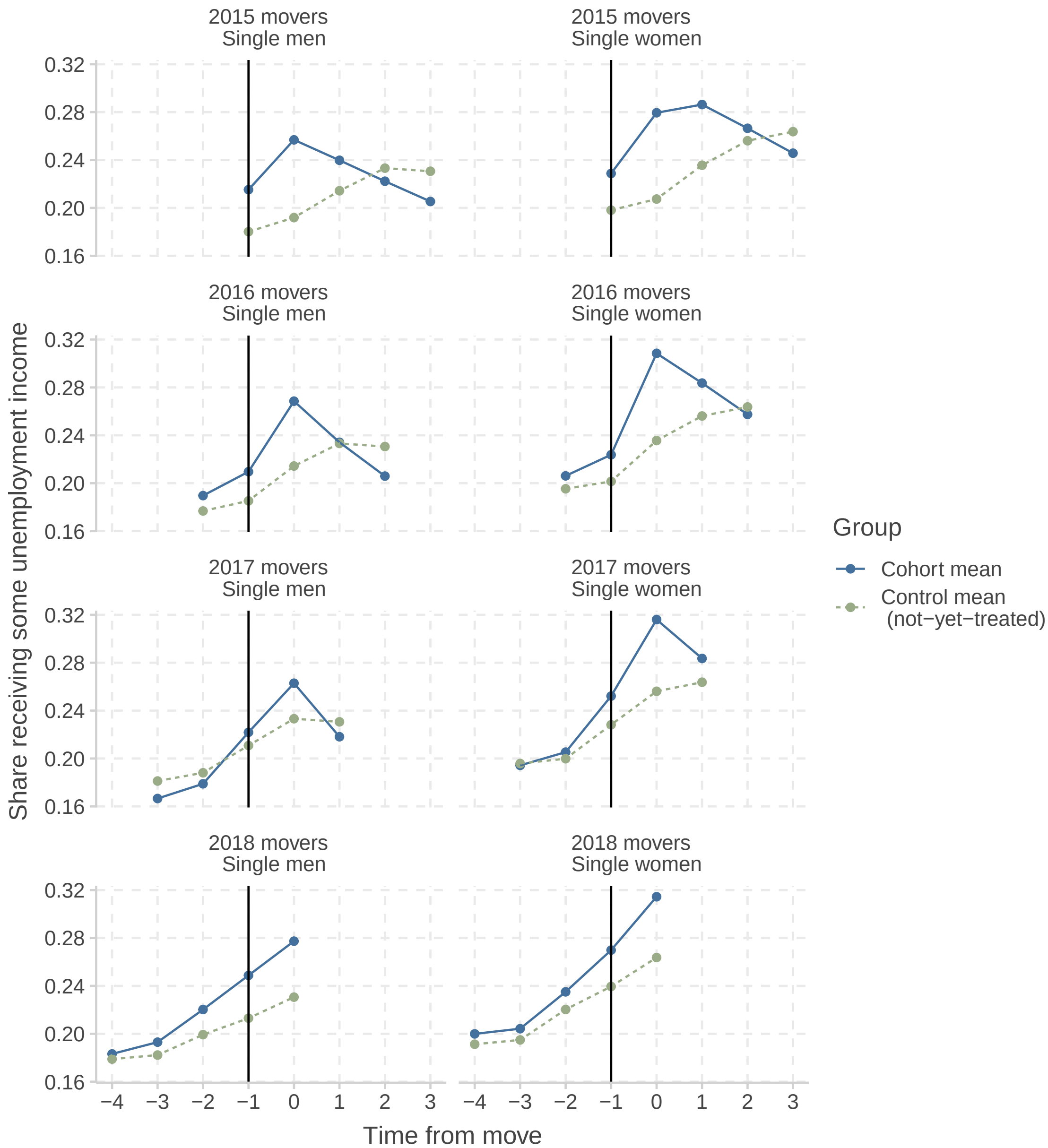
This figure presents the average annual labor income around a move by gender, year of move, and treatment group, for individuals who move while single. For each panel, the "Cohort mean" line corresponds to the average annual labor income of people who moved in that year. The "Control mean" line corresponds to the average annual labor income of individuals who have not yet moved at that point, defined in the same way as in Callaway et al. (2021). For pre-move years, not-yet movers are individuals who move after the considered cohort. For instance, in the 2017 panel, the not-yet movers at time 0 (year 2017) are defined as individuals who move in 2018 or 2019. The not-yet movers at time 1 (year 2018) are individuals who move in 2019. The not-yet movers at time -3 to -1 are individuals who move in 2018 or 2019. The figure is computed from Fideli data. The sample is made of individuals living in an urban area in mainland France, aged 25-60, who moved while single between 2014-2019 without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. Annual labor income is defined as the sum of wage and independent income (agricultural, industrial, commercial and non-commercial profit). Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing.

Figure A5. Share receiving some unemployment around a move, by cohort and treatment status, couples



This figure presents the share of people earning some unemployment income around a move by gender, year of move, and treatment group, for individuals who move while in a relationship. For each panel, the "Cohort mean" line corresponds to the share of individuals earning unemployment income for people who moved in that year. The "Control mean" line corresponds to the same share for individuals who have not yet moved at that point, defined in the same way as in Callaway et al. (2021). For pre-move years, not-yet movers are individuals who move after the considered cohort. For instance, in the 2017 panel, the not-yet movers at time 0 (year 2017) are defined as individuals who move in 2018 or 2019. The not-yet movers at time 1 (year 2018) are individuals who move in 2019. The not-yet movers at time -3 to -1 are individuals who move in 2018 or 2019. The figure is computed from Fideli data. The sample is made of individuals living in an urban area in mainland France, aged 25-60, who moved while in a relationship between 2014-2019 without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing.

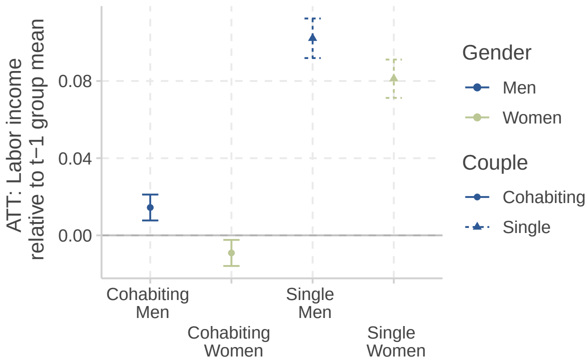
Figure A6. Share receiving some unemployment around a move, by cohort and treatment status, singles



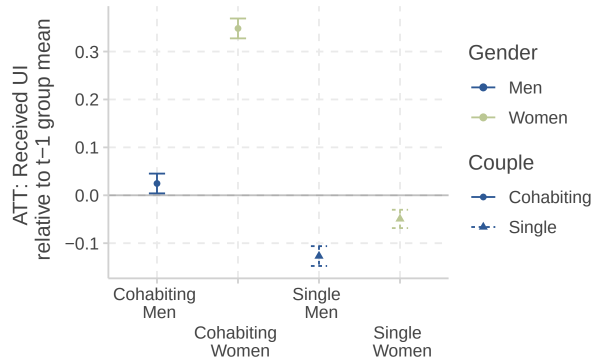
This figure presents the share of people earning some unemployment income around a move by gender, year of move, and treatment group, for individuals who move while single. For each panel, the "Cohort mean" line corresponds to the share of individuals earning unemployment income for people who moved in that year. The "Control mean" line corresponds to the same share for individuals who have not yet moved at that point, defined in the same way as in Callaway et al. (2021). For pre-move years, not-yet movers are individuals who move after the considered cohort. For instance, in the 2017 panel, the not-yet movers at time 0 (year 2017) are defined as individuals who move in 2018 or 2019. The not-yet movers at time 1 (year 2018) are individuals who move in 2019. The not-yet movers at time -3 to -1 are individuals who move in 2018 or 2019. The figure is computed from Fideli data. The sample is made of individuals living in an urban area in mainland France, aged 25-60, who moved while single between 2014-2019 without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing.

A.2. Main results: ATT tables

Figure A7. ATT: annual labor income and receiving UI, by gender and couple status



(a) Outcome: Annual labor income



(b) Outcome: Receiving UI

Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender and couple status. Panel (a) uses annual labor income in euros as outcome. Panel (b) use as an outcome an indicator for receiving UI. Regressions in both panels include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average annual treatment effect in the first four years of the move, relative to the group average one year before moving. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2. The number of observations for each regression is: single men: 644,409 (178,307 individuals); single women: 714,127 (185,332 individuals); cohabiting men: 1,062,644 (243,927 individuals); cohabiting women: 996,229 (239,690 individuals)

Table A1: ATT: Labor income

	Married or cohabiting		Not married or cohabiting	
	Men	Women	Men	Women
ATT	487.8416	-185.6927	2295.3488	1533.7830
	(115.7955)	(70.2281)	(117.5906)	(95.8374)
Pre-move average	33797.57	20372.84	22471.22	18892.88
Number of individuals	243,927	239,690	178,307	185,332
Number of observations	1,062,644	996,229	644,409	714,127

Note: Standard errors in parenthesis. Regressions use annual labor income in euros as outcome variable and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average annual treatment effect in the first four years of the move. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2.

Table A2: ATT: Received UI

	Married or cohabiting		Not married or cohabiting	
	Men	Women	Men	Women
ATT	0.0036	0.0648	-0.0289	-0.0123
	(0.0015)	(0.0020)	(0.0024)	(0.0024)
Pre-move average	0.145	0.186	0.228	0.250
Number of individuals	243,927	239,690	178,307	185,332
Number of observations	1,062,644	996,229	644,409	714,127

Note: Standard errors in parenthesis. Regressions use as outcome variable an indicator for receiving some unemployment income during the year and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average annual treatment effect in the first four years of the move. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2.

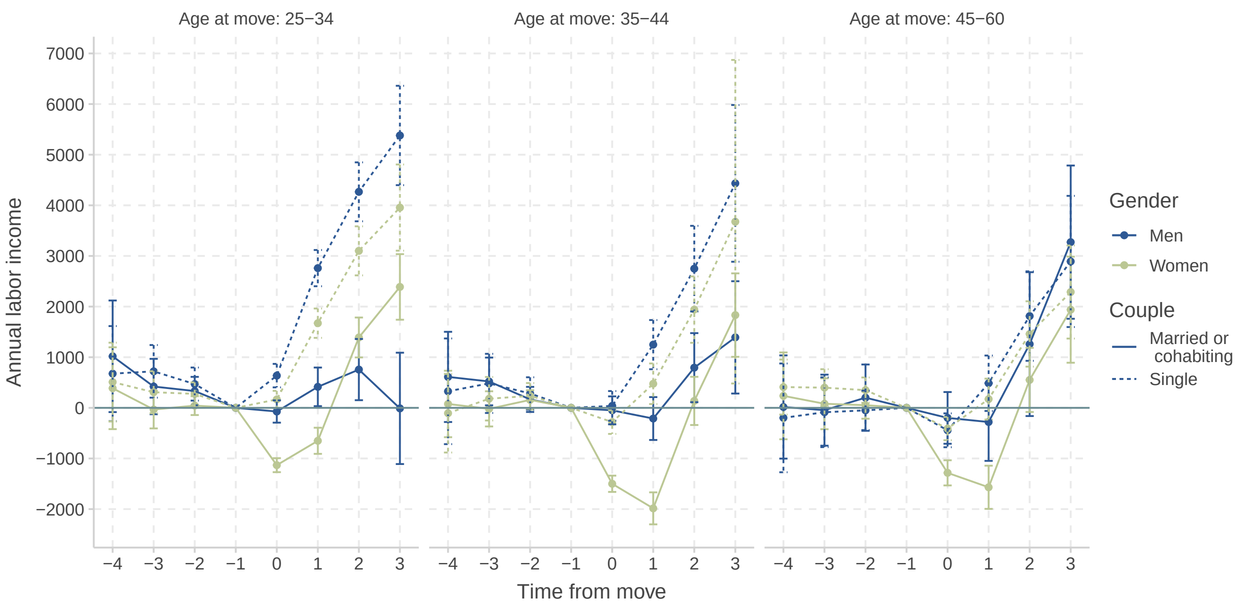
A.3. Results by age at move

Table A3: Descriptive statistics by couple status and age at move

Variable	Married or cohabiting 25-34	Married or cohabiting 35-44	Married or cohabiting 45-60	Not married or cohabiting 25-34	Not married or cohabiting 35-44	Not married or cohabiting 45-60
Age	29.81	38.05	49.85	28.76	38.28	50.78
	(2.24)	(2.85)	(4.30)	(2.32)	(2.92)	(4.41)
Female	0.57	0.49	0.44	0.48	0.53	0.56
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Number minor children	0.92	1.74	0.91	0.25	0.70	0.33
	(0.98)	(1.10)	(1.09)	(0.68)	(1.11)	(0.72)
Has minor child	0.62	0.89	0.54	0.15	0.38	0.22
	(0.49)	(0.32)	(0.50)	(0.36)	(0.49)	(0.42)
Has child under 10	0.61	0.78	0.22	0.15	0.25	0.05
	(0.49)	(0.41)	(0.41)	(0.35)	(0.44)	(0.23)
Has child under 6	0.59	0.59	0.10	0.12	0.14	0.02
	(0.49)	(0.49)	(0.30)	(0.32)	(0.35)	(0.14)
Has child under 3	0.47	0.33	0.04	0.07	0.06	0.01
	(0.50)	(0.47)	(0.20)	(0.25)	(0.24)	(0.08)
Has newborn	0.18	0.10	0.01	0.02	0.02	0.00
	(0.38)	(0.30)	(0.10)	(0.14)	(0.13)	(0.04)
Oldest child age (if any)	3.53	8.34	15.55	5.55	11.09	16.51
	(3.29)	(4.99)	(5.12)	(3.89)	(5.29)	(4.41)
Youngest child age (if any)	1.76	4.64	12.12	3.68	7.81	14.46
	(2.00)	(3.90)	(5.71)	(3.08)	(4.81)	(5.02)
Annual labor income	22,344	28,043	32,338	18,419	21,004	22,957
	(17,363)	(25,780)	(47,983)	(14,886)	(18,919)	(27,072)
Annual unemployment income	995	1,134	1,352	1,284	1,581	1,600
	(2,970)	(3,979)	(5,227)	(3,117)	(3,987)	(4,673)
Received some unemployment	0.18	0.16	0.16	0.25	0.25	0.22
	(0.38)	(0.36)	(0.36)	(0.43)	(0.43)	(0.42)
Urban area population - Origin	3,830,600	4,024,305	3,083,558	2,417,480	2,693,443	2,496,981
	(5,270,185)	(5,386,338)	(4,915,280)	(4,359,634)	(4,652,936)	(4,520,932)
Urban area population - Destination	1,228,911	1,147,156	1,097,129	2,050,656	1,351,404	990,312
	(2,838,823)	(2,695,619)	(2,750,288)	(3,963,952)	(3,167,230)	(2,670,362)
Number of individuals	151729	173639	125986	125756	80462	105884

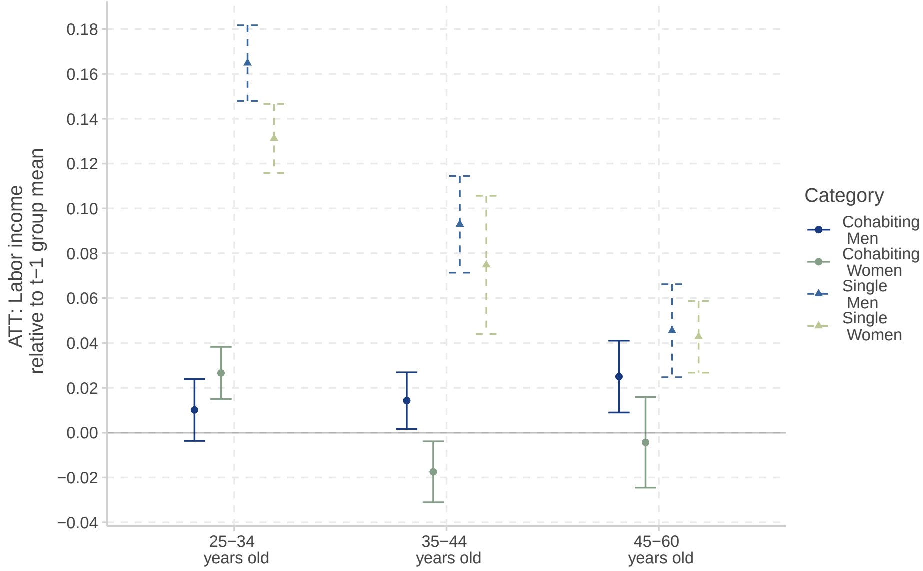
Note: Standard deviation in parenthesis. This table is computed from Fideli data. All variables are measured in the year preceding the move, by couple status and age at the time of the move. The sample is made of individuals living in an urban area in mainland France, aged 25-60, who moved once between 2014-2019 without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped.

Figure A8. Effect of a move on annual labor income by age groups in absolute terms (euros)



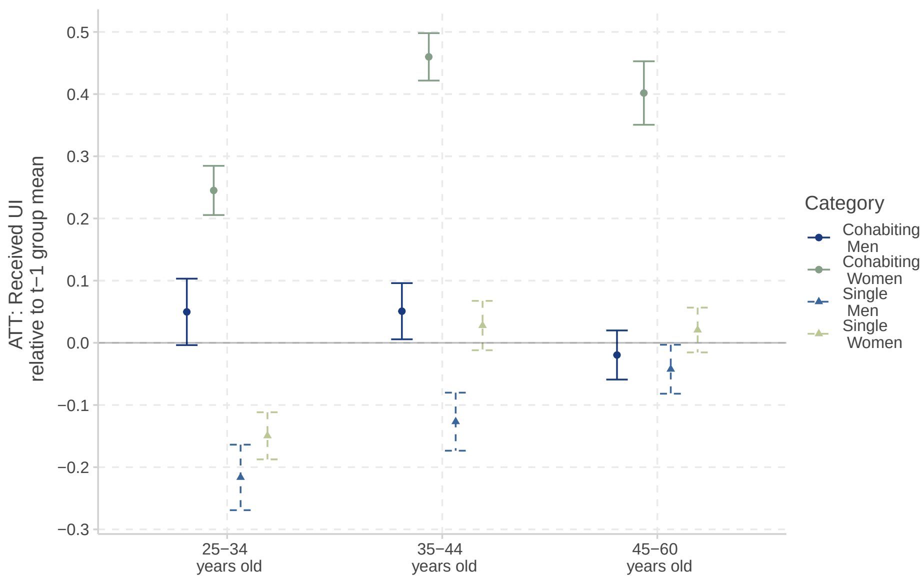
Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and age at move. Regressions use annual labor income in euros as outcome variable and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. Couple status is defined at the year of the move, as well as the age at move. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. The number of observations for each regression is: single men aged 25-34: 270,454; single women aged 25-34: 253,165; cohabiting men aged 25-34: 300,219; cohabiting women aged 25-34: 372,867; single men aged 35-44: 168,435; single women aged 35-44: 192,455; cohabiting men aged 35-44: 426,340; cohabiting women aged 35-44: 381,366; single men aged 45-60: 205,520; single women aged 45-60: 268,507; cohabiting men aged 45-60: 336,085; cohabiting women aged 45-60: 241,996

Figure A9. ATT: Annual labor income, by gender, couple status and age at move



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and age at move. Regressions use annual labor income in euros as outcome variable and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average annual treatment effect in the first four years of the move. Couple status is defined at the year of the move, as well as the age at move. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2.

Figure A10. ATT: Receiving unemployment income, by gender, couple status and age at move

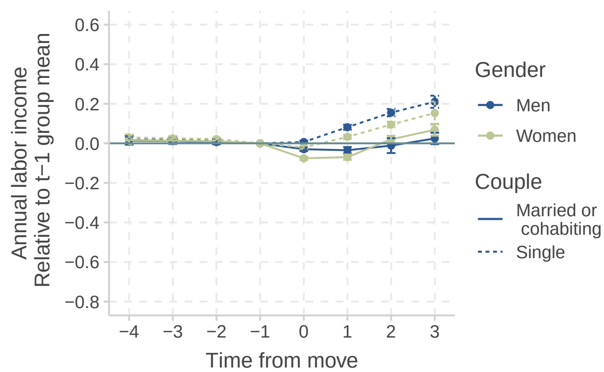


Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and age at move. Regressions use as outcome an indicator for receiving some unemployment income during the year and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average annual treatment effect in the first four years of the move. Couple status is defined at the year of the move, as well as the age at move. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2.

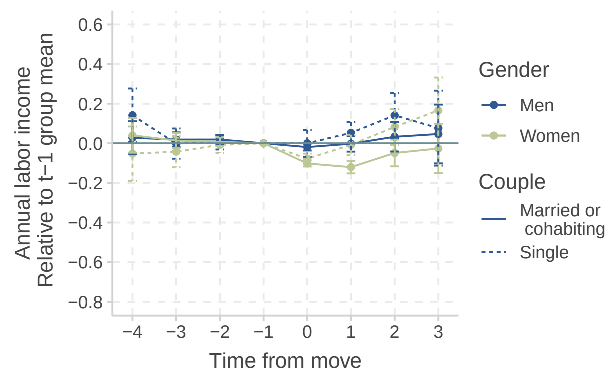
A.4. Results by fertility profile

Figure A11. Effect of a move on annual labor income, by fertility profile (child ten or younger), all categories

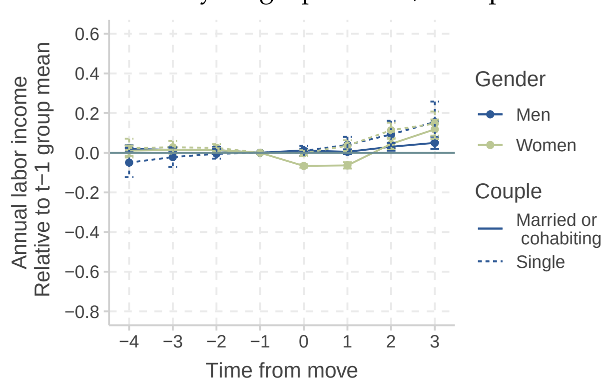
(a) No child 10 or younger pre-move, no child post-move



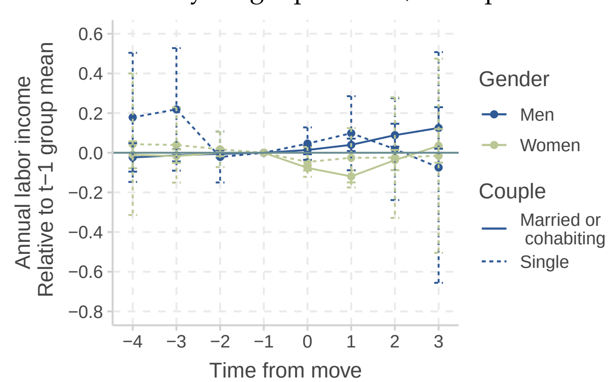
(b) Child 10 or younger pre-move, no child post-move



(c) No child 10 or younger pre-move, child post-move



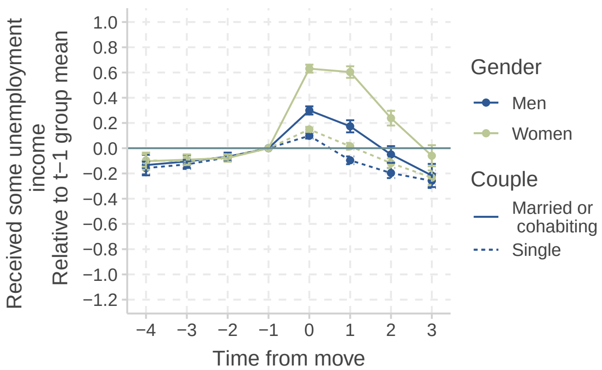
(d) Child 10 or younger pre-move, child post-move



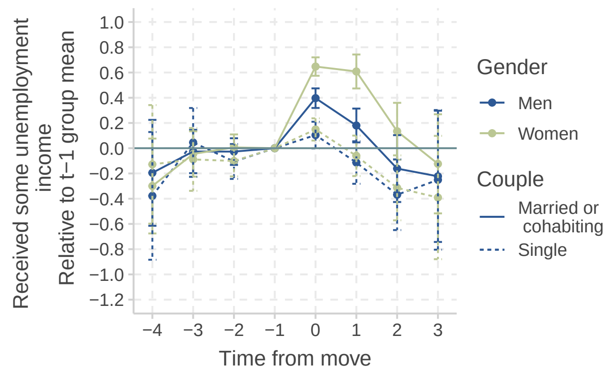
Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use annual labor income in euros as outcome variable and include age as a covariate. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged ten or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged ten or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged ten or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged ten or younger at the time of the move, who have another child afterward. The number of observations for each regression is: cohabiting men of panel (a): 373,284; cohabiting women of Panel (a): 350,874; single men of Panel (a) 561147; single women of Panel (a): 556432; cohabiting men of panel (b): 458,226; cohabiting women of panel (b): 427,676; single men of panel (b): 46483; single women of Panel (b): 116998; cohabiting men of panel (c): 93,690; cohabiting women of panel (c): 91,347; single men of panel (c) 31,005; single women of panel (c): 28,673; cohabiting men of panel (d): 137,444; cohabiting women of panel (d): 126,332; single men of panel (d): 5774; single women of panel (d): 12024

Figure A12. Effect of a move on unemployment status, by fertility profile (child ten or younger)

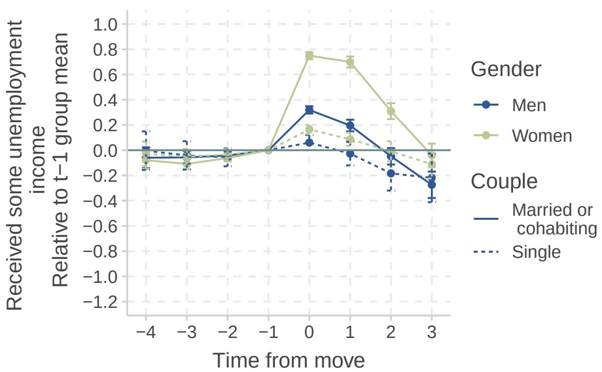
(a) No child 10 or younger pre-move, no child post-move



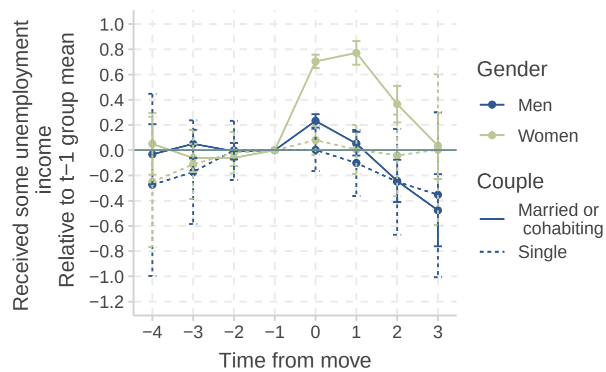
(b) Child 10 or younger pre-move, no child post-move



(c) No child 10 or younger pre-move, child post-move

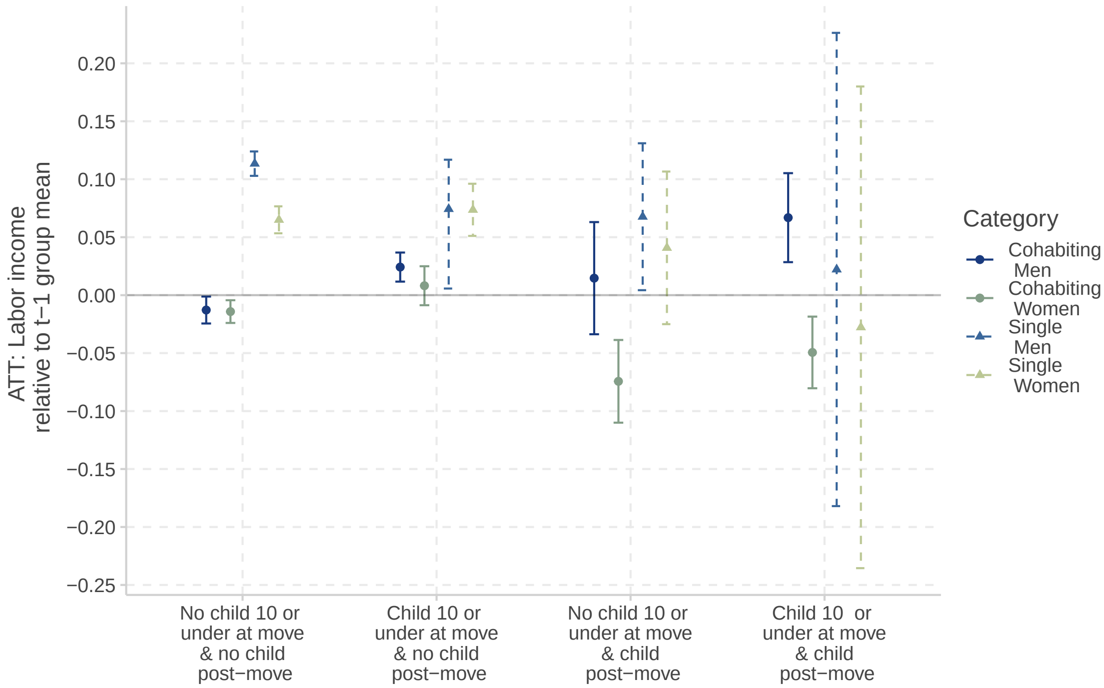


(d) Child 10 or younger pre-move, child post-move



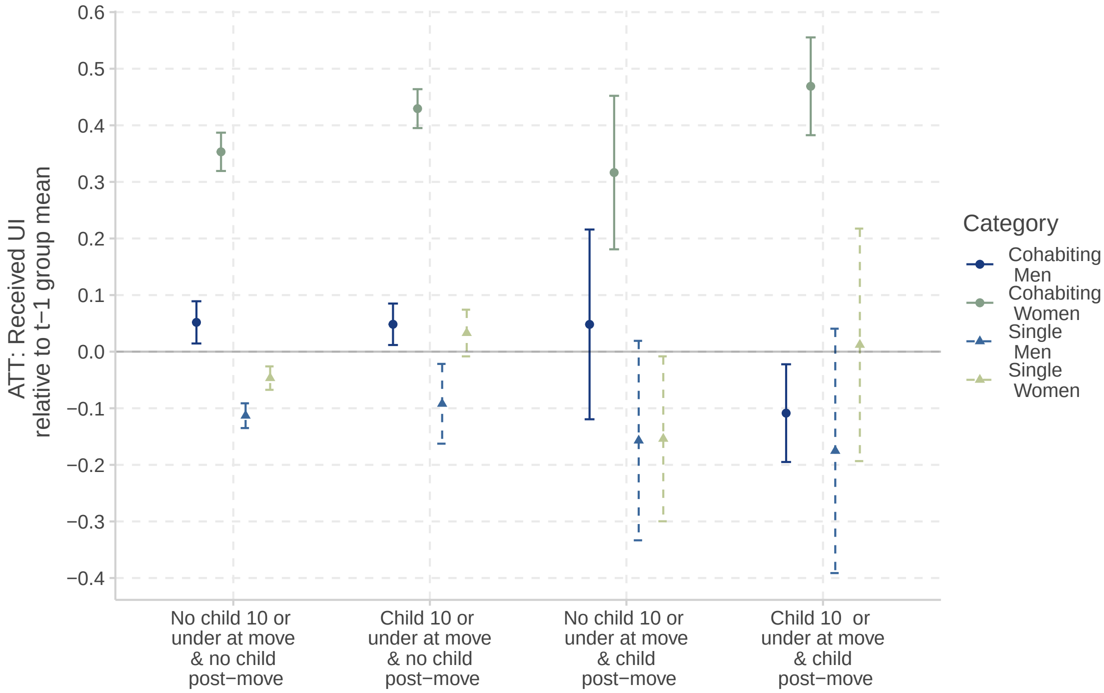
Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use as an outcome an indicator for receiving UI and include age as a covariate. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged ten or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged ten or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged ten or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged ten or younger at the time of the move, who have another child afterward. The number of observations for each regression is: cohabiting men of panel (a): 373,284; cohabiting women of Panel (a): 350,874; single men of Panel (a) 561147; single women of Panel (a): 556432; cohabiting men of panel (b): 458,226; cohabiting women of panel (b): 427,676; single men of panel (b): 46483; single women of Panel (b): 116998; cohabiting men of panel (c): 93,690; cohabiting women of panel (c): 91,347; single men of panel (c) 31,005; single women of panel (c): 28,673; cohabiting men of panel (d): 137,444; cohabiting women of panel (d): 126,332; single men of panel (d): 5774; single women of panel (d): 12024

Figure A13. ATT: Annual labor income, by gender, couple status and fertility profile (child ten or younger)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use annual labor income in euros as outcome variable and include age as a covariate. The plotted coefficient corresponds to the average annual treatment effect in the first four years of the move. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged ten or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged ten or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged ten or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged ten or younger at the time of the move, who have another child afterward.

Figure A14. ATT: Receiving unemployment income, by gender, couple status and fertility profile (child ten or younger)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use as an outcome an indicator for receiving UI and include age as a covariate. The plotted coefficient corresponds to the average annual treatment effect in the first four years of the move. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged ten or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged ten or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged ten or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged ten or younger at the time of the move, who have another child afterward.

A.5. Household changes

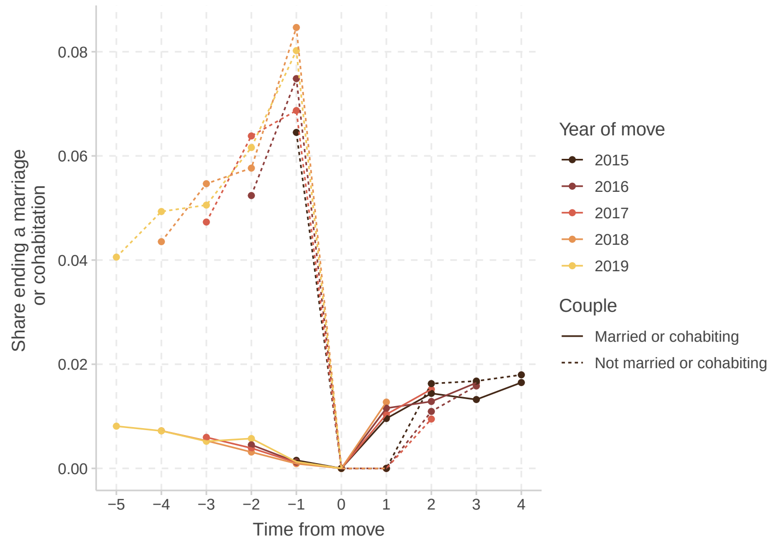
Another potential channel for the gap in the effect of a move relates to changes in household composition, meaning couple formation or break-up. In all the regressions above, household type is defined in the year of the move: individuals who move while in a relationship are defined as "partnered" in all years, regardless of their current status. Yet, partnership status can evolve over time, and as a result some of the individuals belonging to the 'partnered at move' category may have ended their relationship after they move. A recent literature underlines the existence of a gender gap in the effect of divorce (Leopold, 2018). If moves lead to an increased probability of a change in household composition, this could explain a part of the observed gap. In order to evaluate the importance of this channel, I plot in Figure A15 the share of individuals who change household around a move. Note that since I exclude the people who change relationship in the same year they move, the share in some years are set to zero by construction.

- For all groups, the share is always zero in the year of the move
- For single people: the share quitting their partnership one year after they move is zero. Since they were single at the beginning of year 0 and of year 1, they have no partnership to quit in year 1.
- For single people: the share entering a partnership one year before they move: if they entered a partnership in year -1 it would mean they were in a relationship at the beginning of year 0, which is contradictory with them being single at move

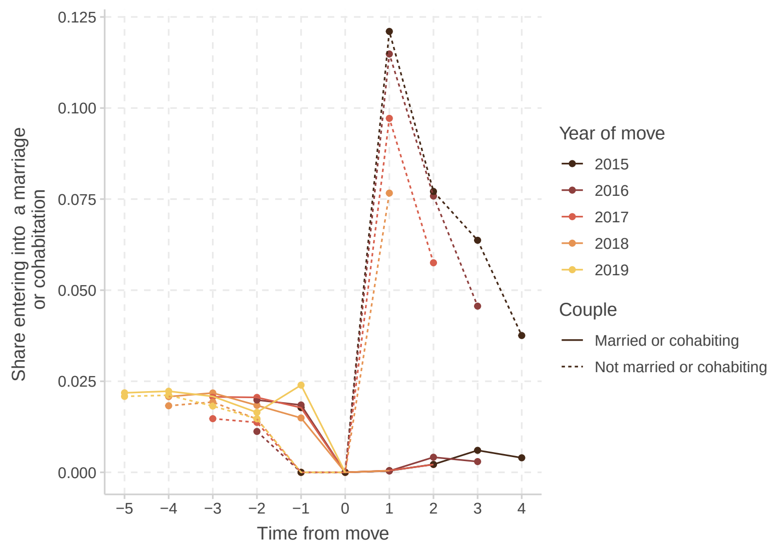
A large share of single movers undergo matrimonial events around moves: around 8% of them exited a relationship in the year preceding the move, while 12% enter one immediately after moving. Among partnered movers, the shares entering or exiting relationships around moves are much lower, with around 1.5% of couples breaking up in the year after the move.

To explore how much household formation and dissolution events contribute to my results, I reproduce my estimation for the subsample of households who stay identical over the 2014-2019 period, meaning that the adult members remain either single or in a relationship with the same individual. Excluding individuals who enter or exit relationships does not affect my results at all, as can be seen in Figure A16. This indicates that, the gender gap in the effect of moves is not driven by changes in household composition around a move which affect men and women differently.

Figure A15. Yearly share of individuals switching household around a move, by couple category



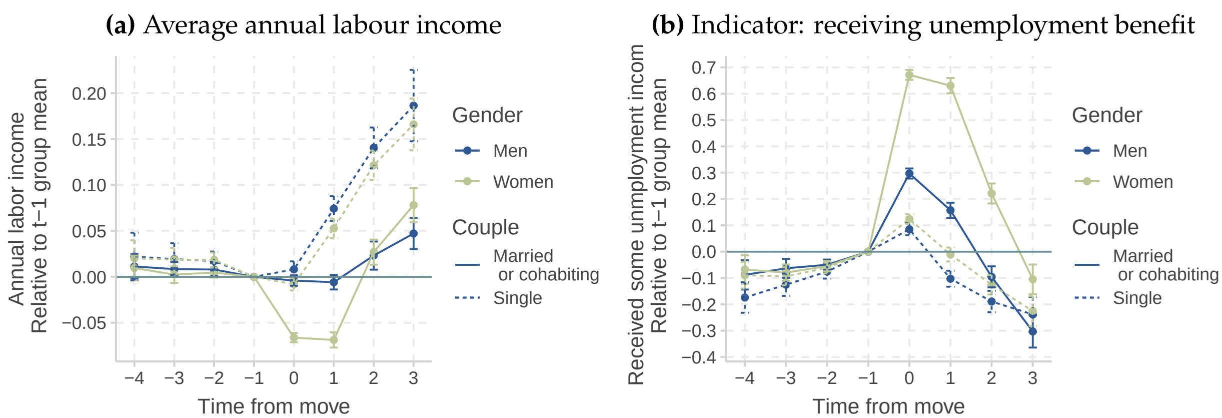
(a) Ending a marriage or cohabitation



(b) Entering into a marriage or cohabitation

Note: This figure presents the share of individuals going through a matrimonial event around a move, by year of move, gender, and couple status in the year of the move. Entering into a marriage or cohabitation in year t, means being part of a different household in Jan 1st of year t and Jan 1st of year t+1, with the year t+1 household being a couple. Ending a marriage or cohabitation in year t means being part of a different household in Jan 1st of year t and Jan 1st of year t+1, with the year t household being a couple. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. It is computed from Fideli data, on the sample described in section 2.

Figure A16. Effect of a move on labor outcomes for stable households



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. Regressions are run separately by gender and couple status, for individuals who remain in the same households over the 2014-2019 years. Panel (a) uses annual labor income in euros as outcome. Panel (b) use as an outcome an indicator for receiving UI. Regressions in both panels include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2. Annual labor income is defined as the sum of wage and independent income (agricultural, industrial, commercial and non-commercial profit).

A.6. Results by primary earner category

Table A4: Percentiles yearly income change

	Deciles bounds - Yearly income change for not-yet-movers			
Gender	Year	10%	50%	90%
Women	2015	-5,042	172	6,312
Men	2015	-4,797	551	7,196
Women	2016	-4,699	187	6,462
Men	2016	-4,838	592	7,685
Women	2017	-5,115	302	6,393
Men	2017	-5,433	710	8,050
Women	2018	- 5,454	296	6,765
Men	2018	-5,473	891	9,374

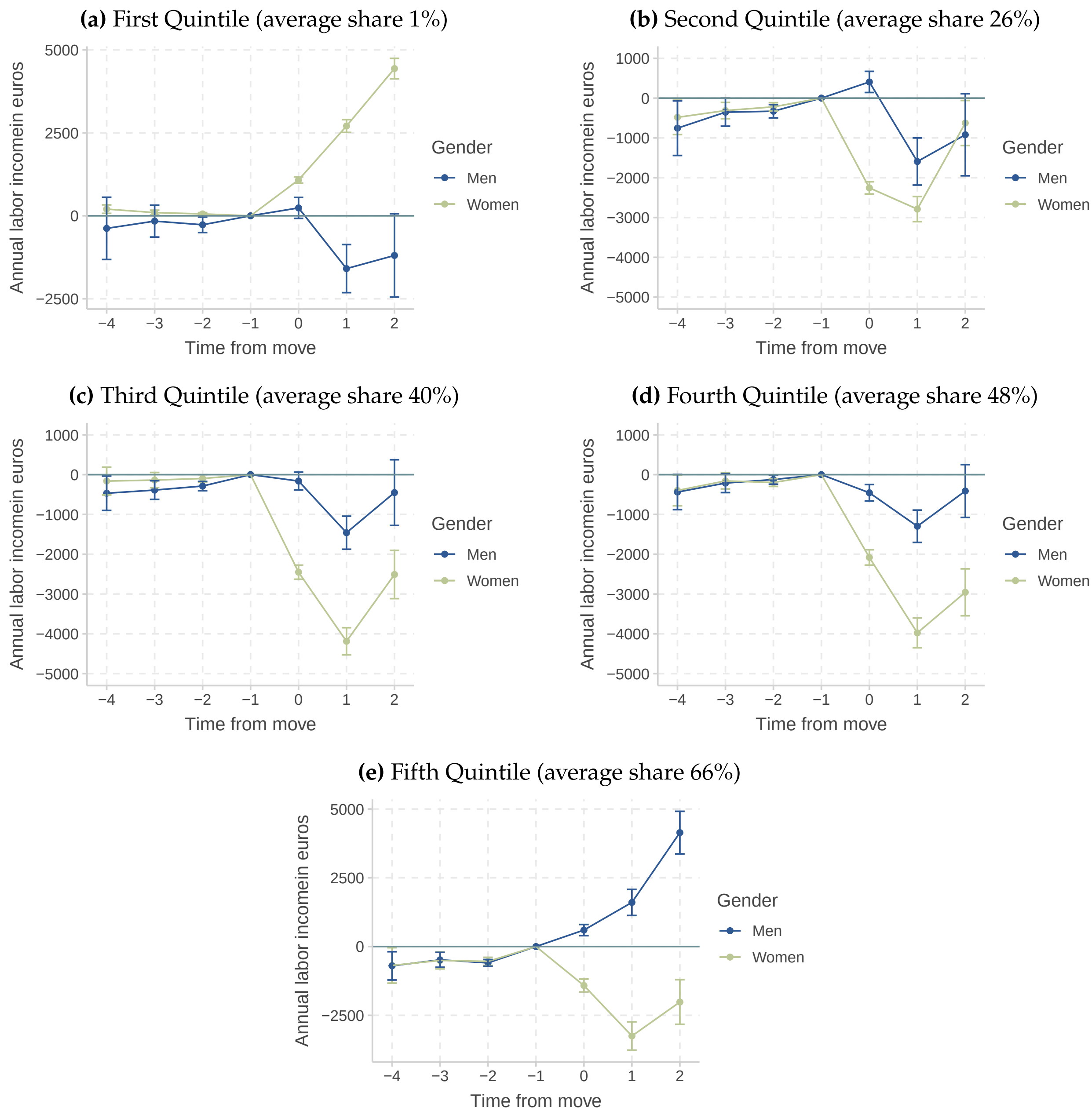
This table provides the 10^{th}, 50^{th} and 90^{th} percentile of absolute change in annual labor income relative to the previous year, for individuals who have not yet moved, by gender and year. This table is computed from Fideli data. The sample is made of individuals who moved while in a couple between 2014-2019, defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped.

Table A5: ATT: Total household income by quintiles of pre-move share of female income

	Quintiles - share of female income				
	1	2	3	4	5
ATT	1891.3931	-2591.9568	-3739.5528	-3725.6304	-112.6316
	(275.0246)	(282.7955)	(230.3772)	(238.7352)	(261.8020)
Pre-move average	34999.80	55751.01	57389.24	56486.49	48388.78
Pre-move share female income	0.014	0.261	0.402	0.479	0.663
Number of households	15,488	15,488	15,488	15,488	15,489
Number of observations	73,028	73,836	73,700	73,271	73,006

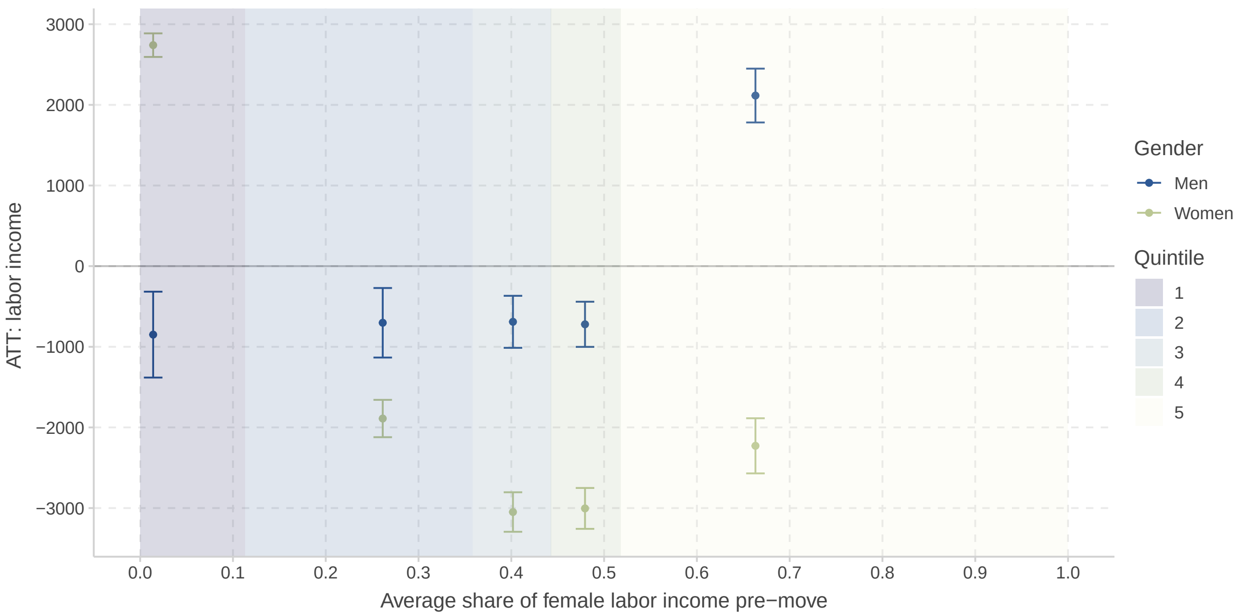
Note: Standard errors in parenthesis. The regressions are run separately by gender and share of female income category (quintiles). Regressions use annual labor income as an outcome, and include covariates for average age of couple members, and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The regressions are run separately by gender. The ATT coefficient corresponds to the average treatment effect by duration of exposure to the treatment. The sample is made of households who moved while in a couple between 2015-2019. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped.

Figure A17. Effect of a move on labor income by gender, by share of female labor income pre-move, in absolute terms (euros)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender and share of female income category (quintiles). Regressions use annual labor income as an outcome, and include covariates for average age of couple members, and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The regressions are run separately by gender. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment. The sample is made of households who moved while in a couple between 2015-2019. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. The number of observations for each regression is: Panel (a): 73,028 (15,488 households); Panel (b) 73,836 (15,488 households); Panel (c): 73,700 (15,488 households); Panel (d): 73,271 (15,488 households); Panel (e) 73,006 (15,489 households)

Figure A18. ATT: Annual labor income, by gender and primary earner quintiles, in absolute terms (euros)

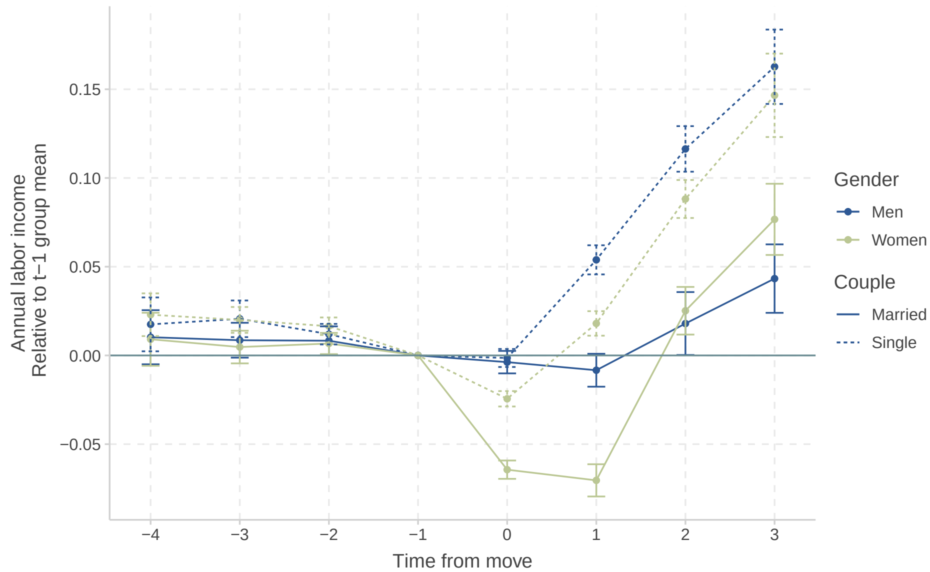


Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. Regressions use annual labor income as an outcome, and include covariates for average age of couple members, and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The regressions are run separately by gender and share of female income category (quintiles). Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The plotted coefficient corresponds to the average annual treatment effect in the first three years of the move. The sample is made of households who moved while in a couple between 2015-2019. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. The number of observations for each regression can be found in the notes of Figure 1.13.

B. Supplementary appendix

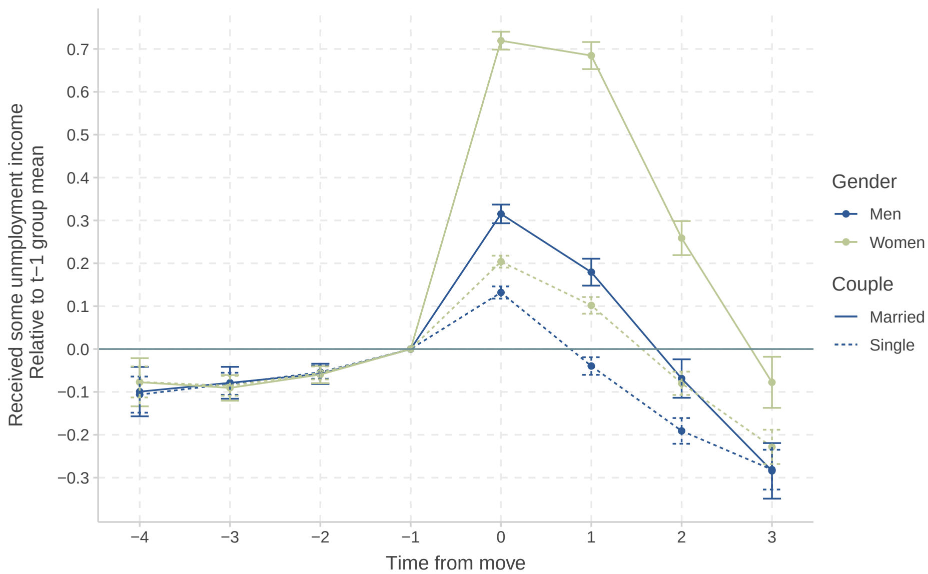
B.1. Alternative couple definitions: marriage or civil union

Figure B1. Effect of a move on annual labor income, by gender and marital status



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. Regressions are run separately by gender and couple status. Regressions use annual labor income in euros as outcome variable and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18.The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couples defined as pairs of adults linked by a marriage or civil union (PACS). Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2. Annual labor income is defined as the sum of wage and independent income (agricultural, industrial, commercial and non-commercial profit). The number of observations for each regression is: single men: 1,070,419 (283,858 individuals); single women: 1,147,221 (294,512 individuals); married men: 896,564 (201,933 individuals); married women: 834,025 (198,006 individuals)

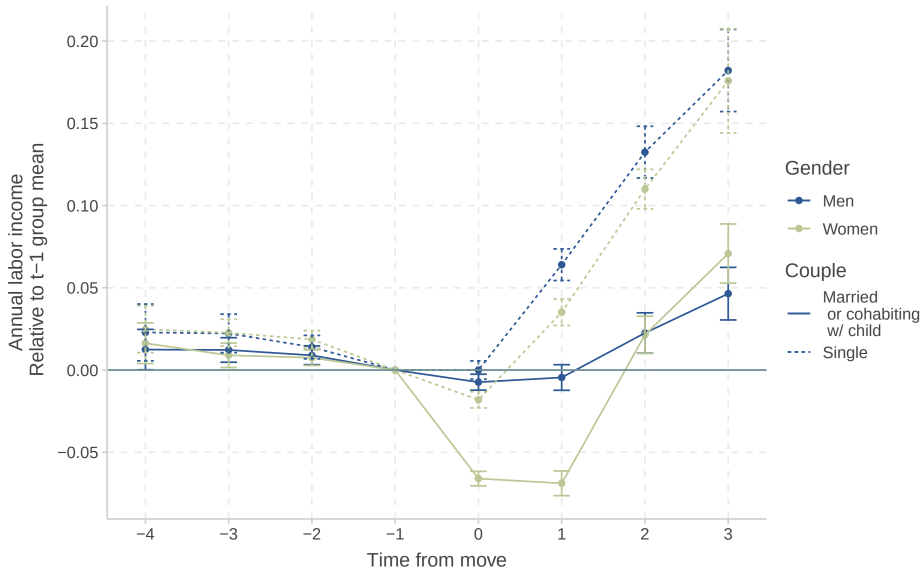
Figure B2. Effect of a move on the probability of receiving some unemployment, by gender and marital status



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. Regressions are run separately by gender and couple status. Regressions use as an outcome an indicator for receiving UI, and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing in which a minor child is also living. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2. The number of observations for each regression is: single men: 1,070,419 (283,858 individuals); single women: 1,147,221 (294,512 individuals); married men: 896,564 (201,933 individuals); married women: 834,025 (198,006 individuals)

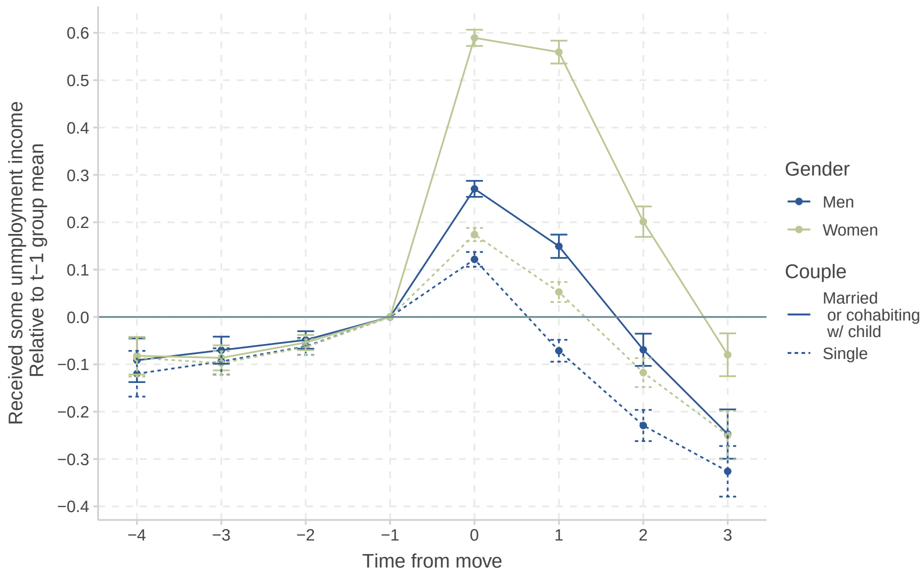
B.2. Alternative couple definitions: marriage, civil union, or cohabitation in presence of a child

Figure B3. Effect of a move on annual labor income, by gender and couple status (marriage or cohabitation with child)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. Regressions are run separately by gender and couple status. Regressions use annual labor income in euros as outcome variable and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18.The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing in which a minor child is also living. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2. Annual labor income is defined as the sum of wage and independent income (agricultural, industrial, commercial and non-commercial profit). The number of observations for each regression is: single men: 868,966 (236,550 individuals); single women: 941,264 (245,507 individuals); cohabiting men: 1,284,855 (288,513 individuals); cohabiting women: 1,239,617 (294,302 individuals)

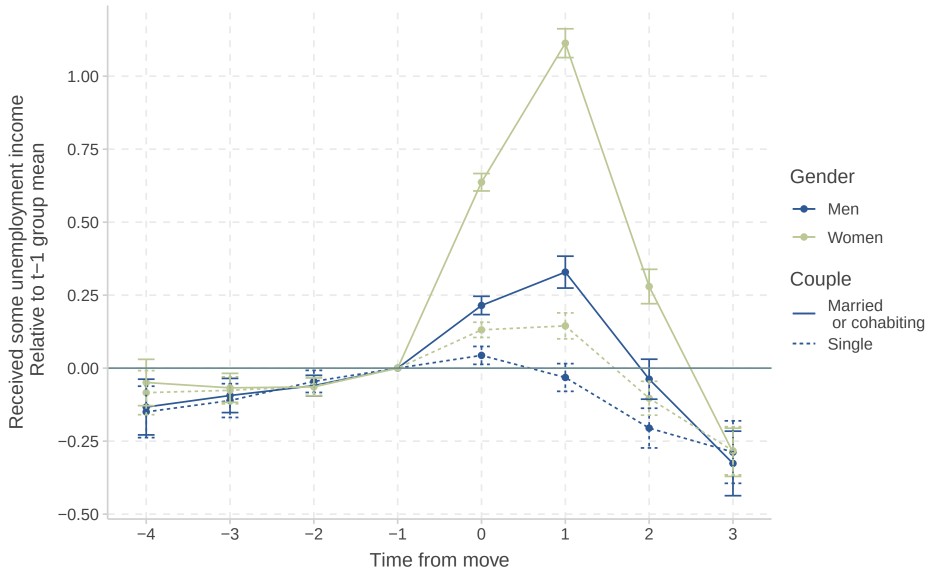
Figure B4. Effect of a move on the probability of receiving some unemployment, by gender and couple status (marriage or cohabitation with child)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. Regressions are run separately by gender and couple status. Regressions use as an outcome an indicator for receiving UI, and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing in which a minor child is also living. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2. The number of observations for each regression is: single men: 868,966 (236,550 individuals); single women: 941,264 (245,507 individuals); cohabiting men: 1,284,855 (288,513 individuals); cohabiting women: 1,239,617 (294,302 individuals)

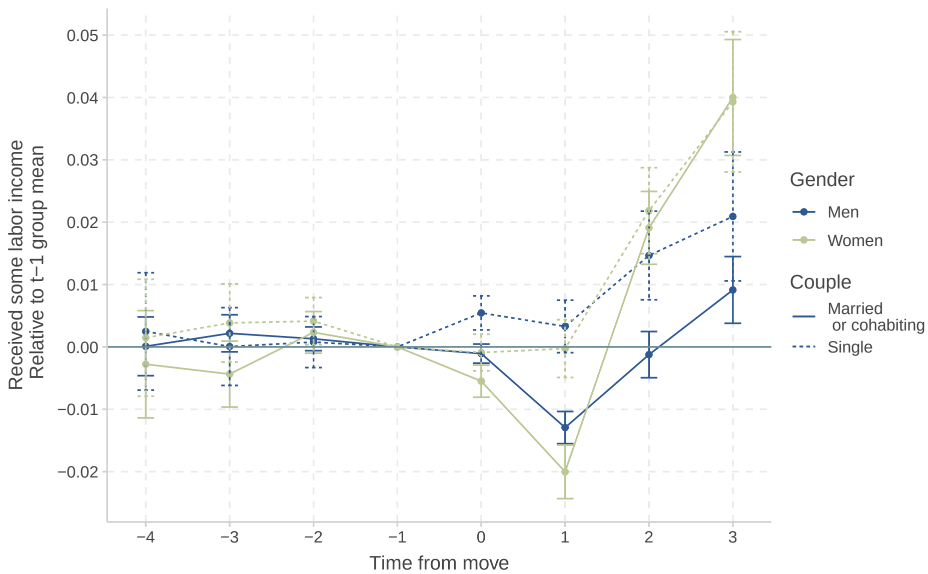
B.3. Results on unemployment income and participation

Figure B5. Effect of a move on unemployment income, by gender and marital status



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender and couple status. Regressions use annual unemployment income in euros as outcome variable and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before the move. Couples defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2. The number of observations for each regression is: single men: 644,409 (178,307 individuals); single women: 714,127 (185,332 individuals); cohabiting men: 1,062,644 (243,927 individuals); cohabiting women: 996,229 (239,690 individuals)

Figure B6. Effect of a move on probability of receiving some labor income, by gender and marital status



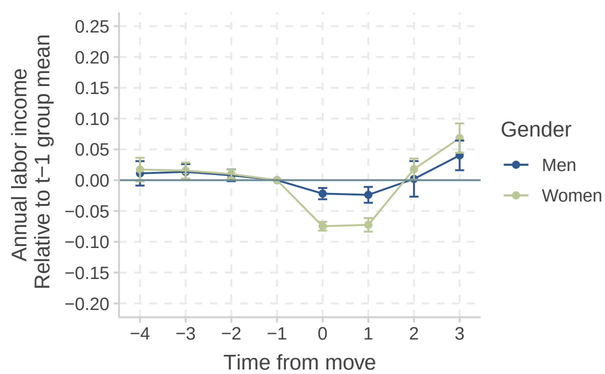
Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender and couple status. Regressions use as an outcome an indicator for some labor income, and include covariates for age and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The regressions are computed from Fideli data, and the sample described in section 2. The number of observations for each regression is: single men: 644,409 (178,307 individuals); single women: 714,127 (185,332 individuals); cohabiting men: 1,062,644 (243,927 individuals); cohabiting women: 996,229 (239,690 individuals)

B.4. Results by fertility profile

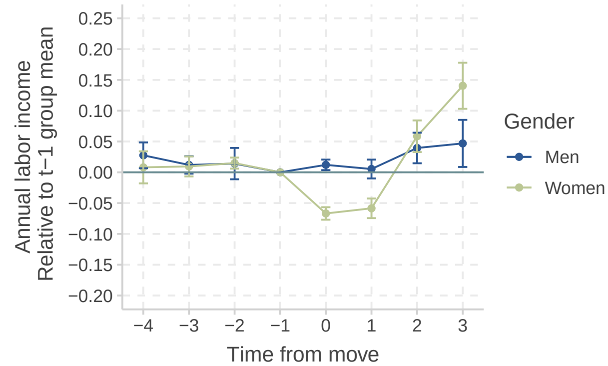
Child six or younger

Figure B7. Effect of a move on annual labor income, by fertility profile (child six or younger)

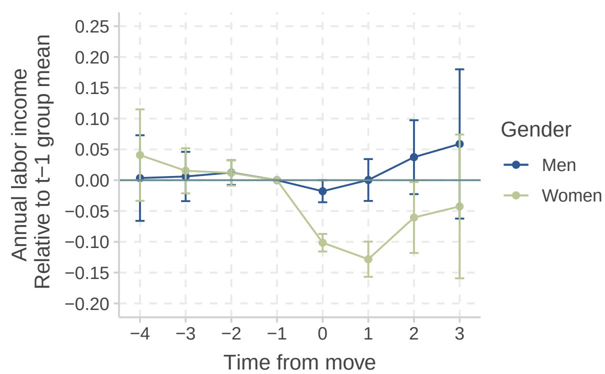
(a) No child 6 or younger pre-move, no child post-move



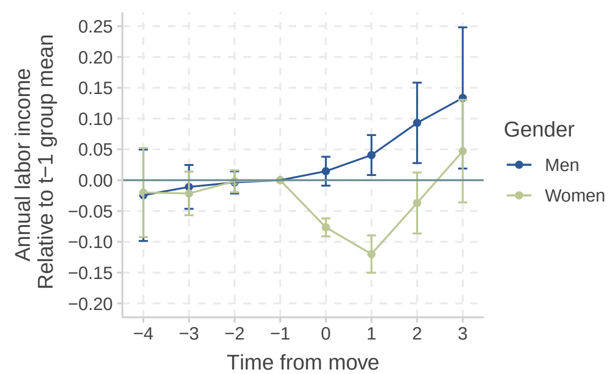
(b) Child 6 or younger pre-move, no child post-move



(c) No child 6 or younger pre-move, child post-move



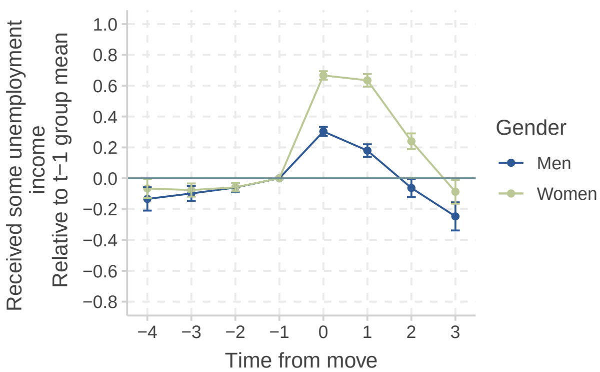
(d) Child 6 or younger pre-move, child post-move



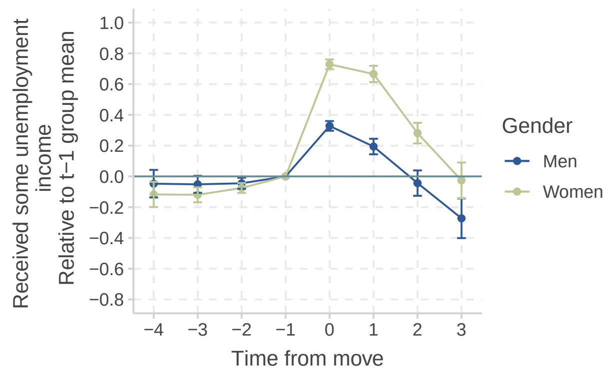
Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use annual labor income in euros as outcome variable and include age as a covariate. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged six or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged six or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged six or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged six or younger at the time of the move, who have another child afterward. The number of observations for each regression is: cohabiting men of panel (a): 489,079; cohabiting women of Panel (a): 460,107; single men of Panel (a) 577,944; single women of Panel (a): 606,581; cohabiting men of panel (b): 342,431; cohabiting women of panel (b): 318,443; single men of panel (b): 29,686; single women of Panel (b): 66,849; cohabiting men of panel (c): 101,078; cohabiting women of panel (c): 98,240; single men of panel (c) 31,863; single women of panel (c): 32,023; cohabiting men of panel (d): 130,056; cohabiting women of panel (d): 119,439; single men of panel (d):4,916; single women of panel (d): 8,674

Figure B8. Effect of a move on unemployment status, by fertility profile (child six or younger)

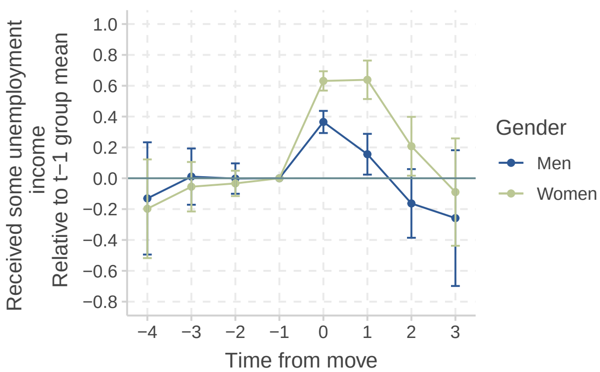
(a) No child 6 or younger pre-move, no child post-move



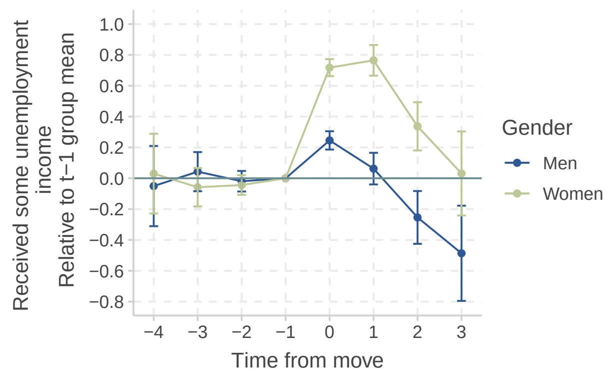
(b) Child 6 or younger pre-move, no child post-move



(c) No child 6 or younger pre-move, child post-move

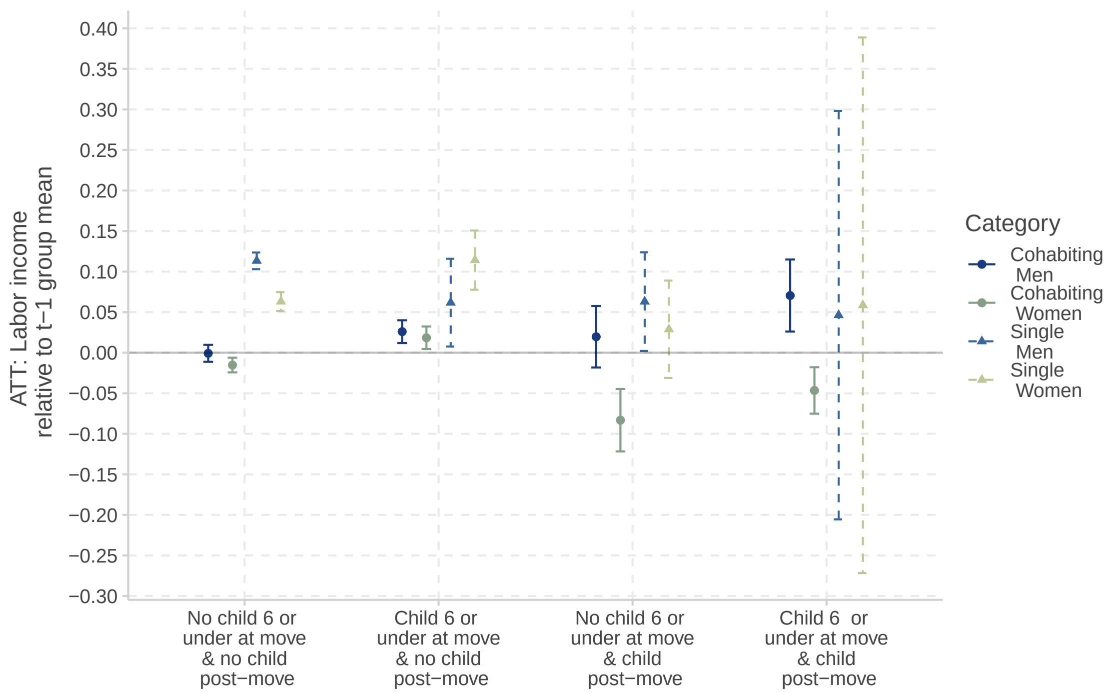


(d) Child 6 or younger pre-move, child post-move



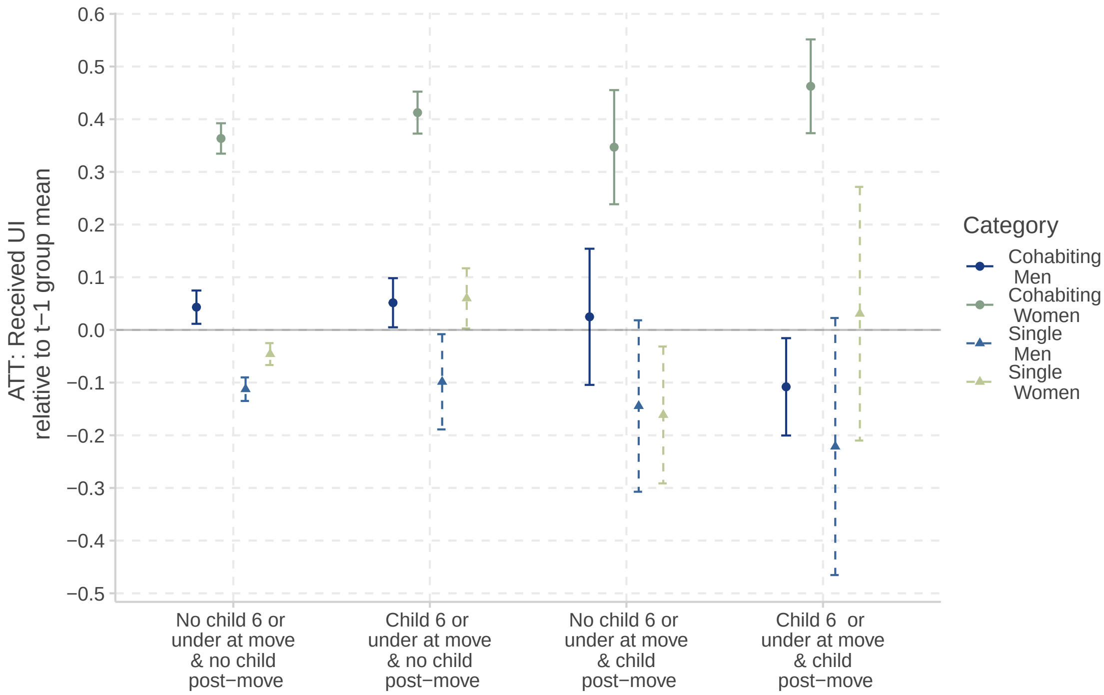
Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use as an outcome an indicator for receiving UI and include age as a covariate. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged six or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged six or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged six or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged six or younger at the time of the move, who have another child afterward. The number of observations for each regression is: cohabiting men of panel (a): 373,284; cohabiting women of Panel (a): 350,874; single men of Panel (a) 561147; single women of Panel (a): 556432; cohabiting men of panel (b): 458,226; cohabiting women of panel (b): 427,676; single men of panel (b): 46483; single women of Panel (b): 116998; cohabiting men of panel (c): 93,690; cohabiting women of panel (c): 91,347; single men of panel (c) 31,005; single women of panel (c): 28,673; cohabiting men of panel (d): 137,444; cohabiting women of panel (d): 126,332; single men of panel (d): 5774; single women of panel (d): 12024

Figure B9. ATT: Annual labor income, by gender, couple status and fertility profile (child six or younger)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use annual labor income in euros as outcome variable and include age as a covariate. The plotted coefficient corresponds to the average annual treatment effect in the first four years of the move. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged six or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged six or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged six or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged six or younger at the time of the move, who have another child afterward.

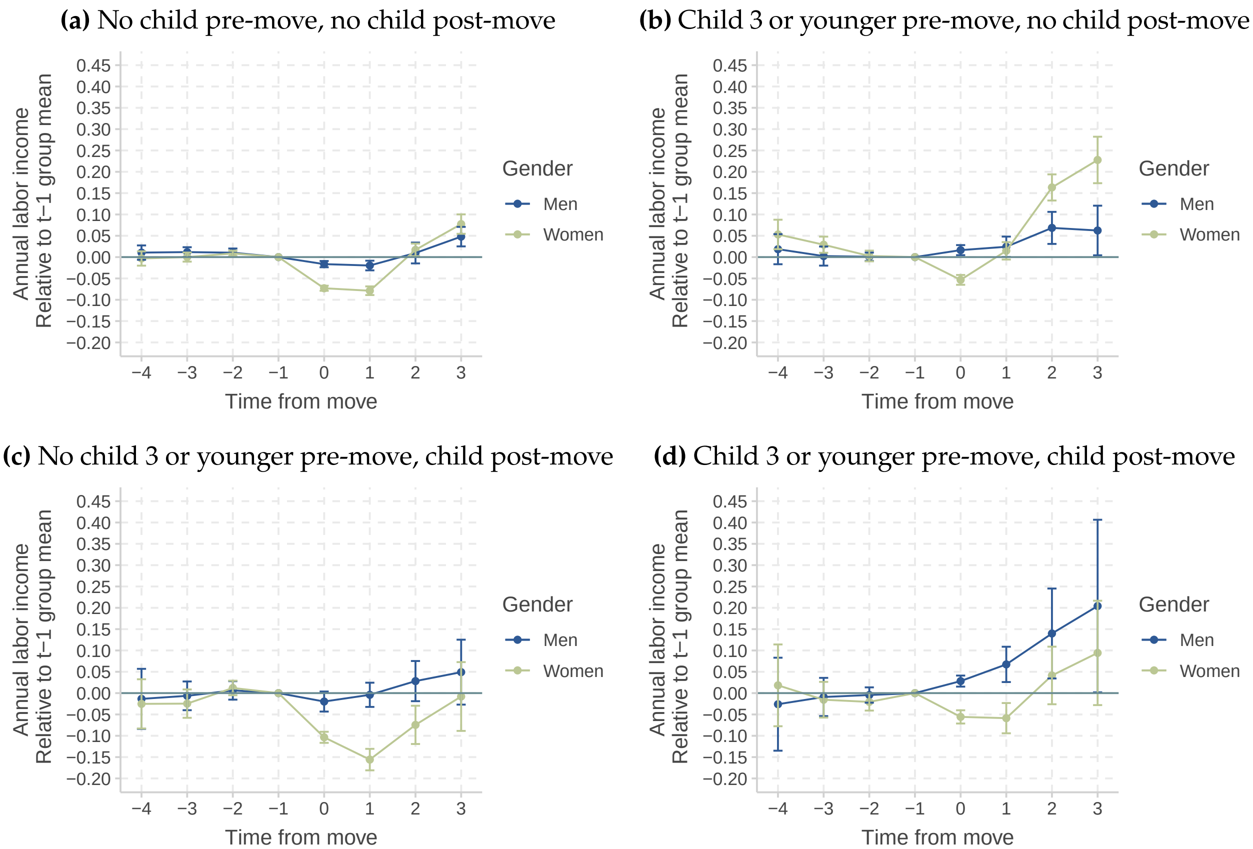
Figure B10. ATT: Receiving unemployment income, by gender, couple status and fertility profile (child six or younger)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use as an outcome an indicator for receiving UI and include age as a covariate. The plotted coefficient corresponds to the average annual treatment effect in the first four years of the move. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged six or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged six or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged six or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged six or younger at the time of the move, who have another child afterward.

Child three or younger

Figure B11. Effect of a move on annual labor income, by fertility profile (child three or younger)

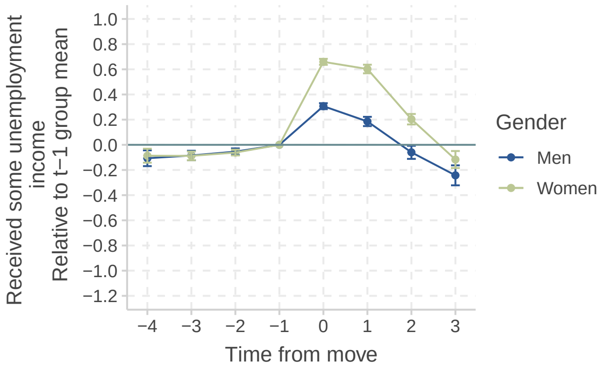


(a) No child pre-move, no child post-move
(b) Child 3 or younger pre-move, no child post-move
(c) No child 3 or younger pre-move, child post-move
(d) Child 3 or younger pre-move, child post-move

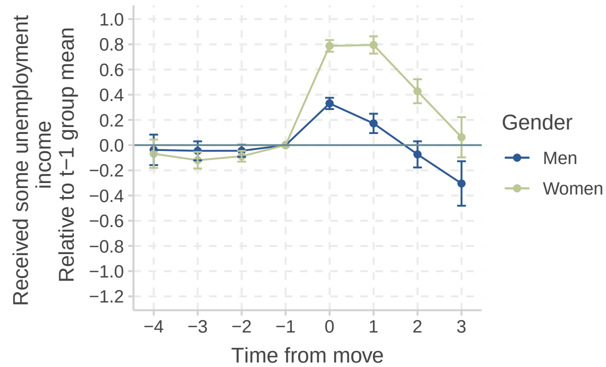
Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use annual labor income in euros as outcome variable and include age as a covariate. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged three or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged three or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged three or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged three or younger at the time of the move, who have another child afterward. The number of observations for each regression is: cohabiting men of panel (a): 623,432; cohabiting women of Panel (a): 585,841; single men of Panel (a) 592,780; single women of Panel (a): 644,143; cohabiting men of panel (b): 208,078; cohabiting women of panel (b): 192,709; single men of panel (b): 14,850; single women of Panel (b): 29,287; cohabiting men of panel (c): 133,392; cohabiting women of panel (c): 128,031; single men of panel (c) 33,457; single women of panel (c): 36,182; cohabiting men of panel (d): 97,742; cohabiting women of panel (d): 89,648; single men of panel (d): 3,322; single women of panel (d): 4,515

Figure B12. Effect of a move on unemployment status, by fertility profile (child three or younger)

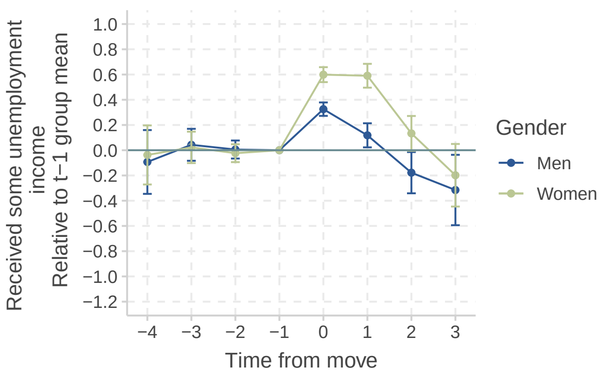
(a) No child 3 or younger pre-move, no child post-move



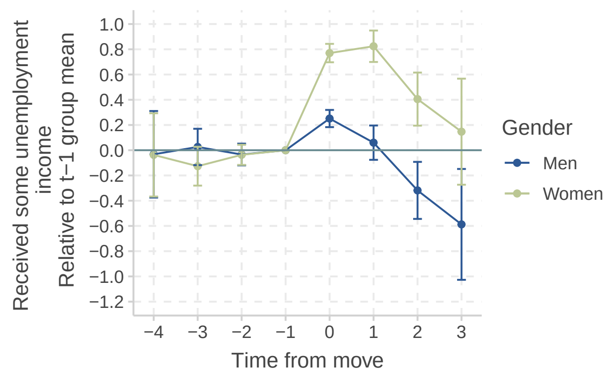
(b) Child 3 or younger pre-move, no child post-move



(c) No child 3 or younger pre-move, child post-move

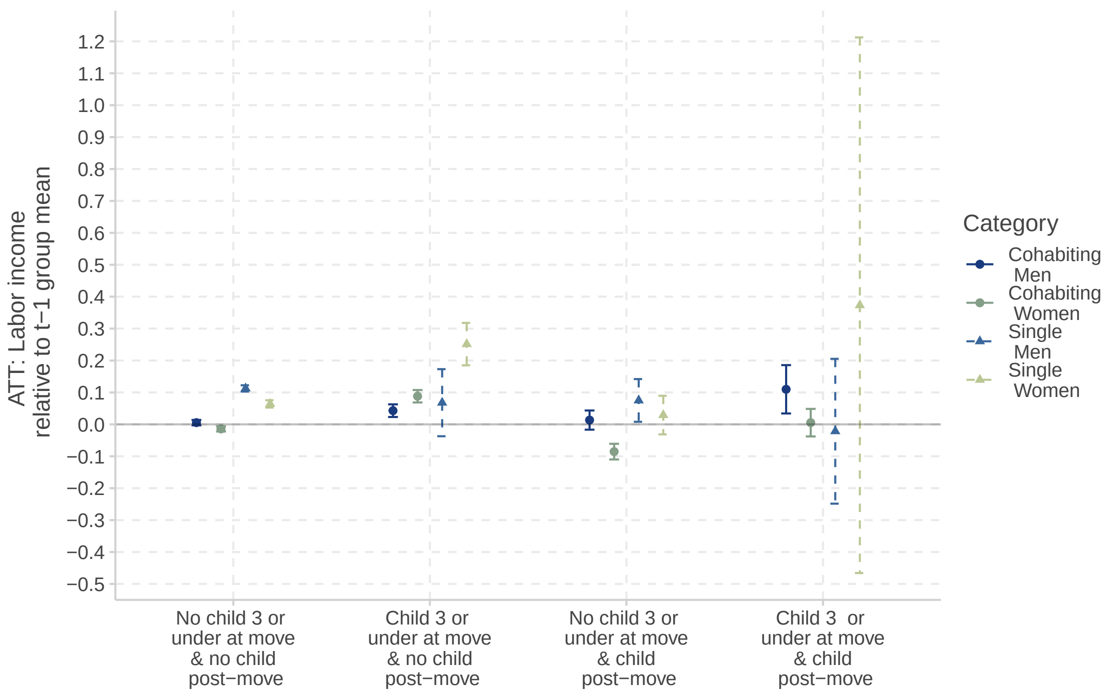


(d) Child 3 or younger pre-move, child post-move



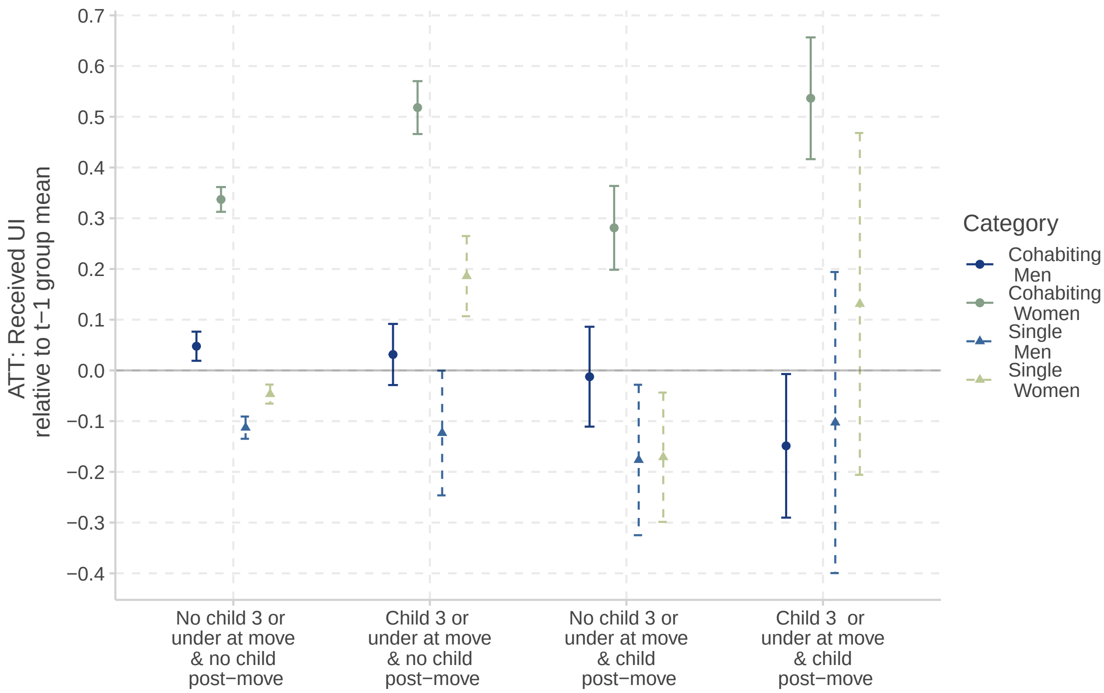
Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use as an outcome an indicator for receiving UI and include age as a covariate. The plotted coefficient corresponds to the average treatment effect by duration of exposure to the treatment, relative to the group average one year before moving. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged three or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged three or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged three or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged three or younger at the time of the move, who have another child afterward. The number of observations for each regression is: cohabiting men of panel (a): 623,432; cohabiting women of Panel (a): 585,841; single men of Panel (a) 592,780; single women of Panel (a): 644,143; cohabiting men of panel (b): 208,078; cohabiting women of panel (b): 192,709; single men of panel (b): 14,850; single women of Panel (b): 29,287; cohabiting men of panel (c): 133,392; cohabiting women of panel (c): 128,031; single men of panel (c) 33,457; single women of panel (c): 36,182; cohabiting men of panel (d): 97,742; cohabiting women of panel (d): 89,648; single men of panel (d): 3,322; single women of panel (d): 4,515

Figure B13. ATT: Annual labor income, by gender, couple status and fertility profile (child three or younger)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use annual labor income in euros as outcome variable and include age as a covariate. The plotted coefficient corresponds to the average annual treatment effect in the first four years of the move. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged three or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged three or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged three or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged three or younger at the time of the move, who have another child afterward.

Figure B14. ATT: Receiving unemployment income, by gender, couple status and fertility profile (child three or younger)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. The regressions are run separately by gender, couple status, and fertility profile. Regressions use as an outcome an indicator for receiving UI and include age as a covariate. The plotted coefficient corresponds to the average annual treatment effect in the first four years of the move. Couple status is defined at the year of the move. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. The regressions are computed from Fideli data, and the sample described in section 2. Panel (a) corresponds to coupled movers without a child aged three or younger at the time of the move, who do not have a new child afterward. Panel (b) corresponds to coupled movers with a child aged three or younger at the time of the move, who do not have another child afterward. Panel (c) corresponds to coupled movers without a child aged three or younger at the time of the move, who have one new child afterward. Panel (d) corresponds to coupled movers with a child aged three or younger at the time of the move, who have another child afterward.

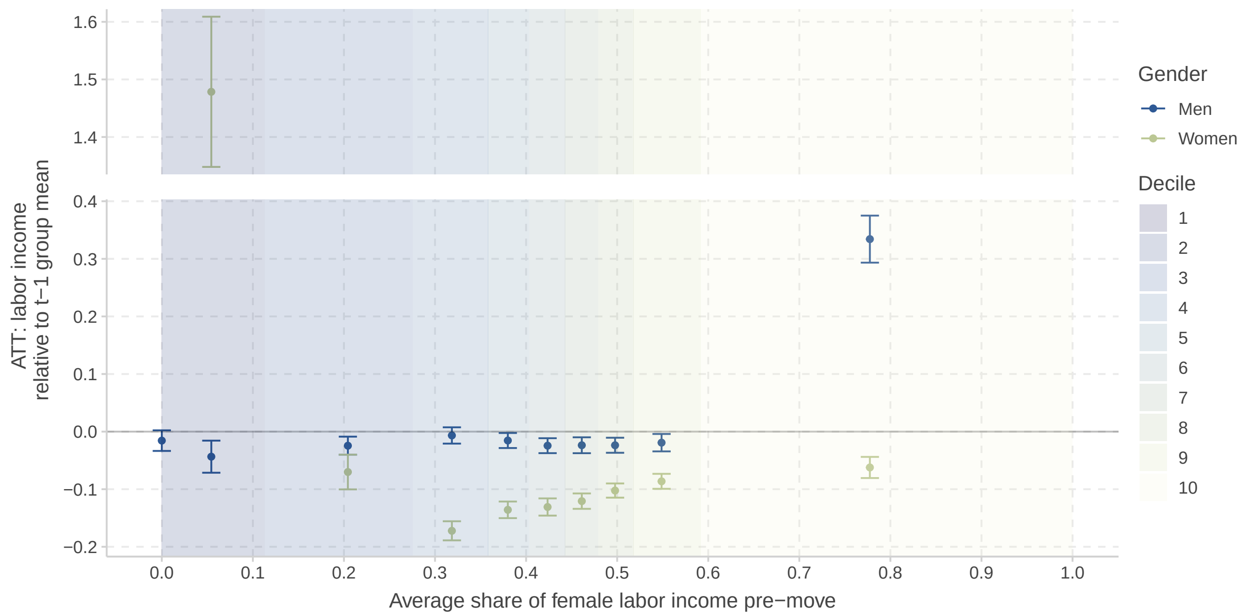
B.5. *Results by primary earner category (deciles)*

Table B1: ATT: Total household income by deciles of pre-move share of female income

	Deciles - share of female income									
	1	2	3	4	5	6	7	8	9	10
ATT	1984.549	1682.651	-1775.920	-3524.028	-3535.640	-3961.191	-3881.279	-3535.205	-3104.275	1972.586
	(320.0677)	(580.7692)	(422.2530)	(358.0256)	(328.2386)	(343.0139)	(334.0753)	(322.0715)	(333.5645)	(441.2901)
Pre-move average	32606.23	41895.03	52202.30	59299.71	57797.55	56980.92	56715.81	56257.11	55416.32	41362.16
Pre-move share female income	0.000	0.054	0.204	0.319	0.380	0.424	0.461	0.498	0.549	0.777
Number of households	11,497	3,991	7,744	7,744	7,744	7,744	7,745	7,743	7,744	7,745
Number of observations	54,111	18,917	36,833	37,003	36,946	36,754	36,555	36,716	36,665	36,341

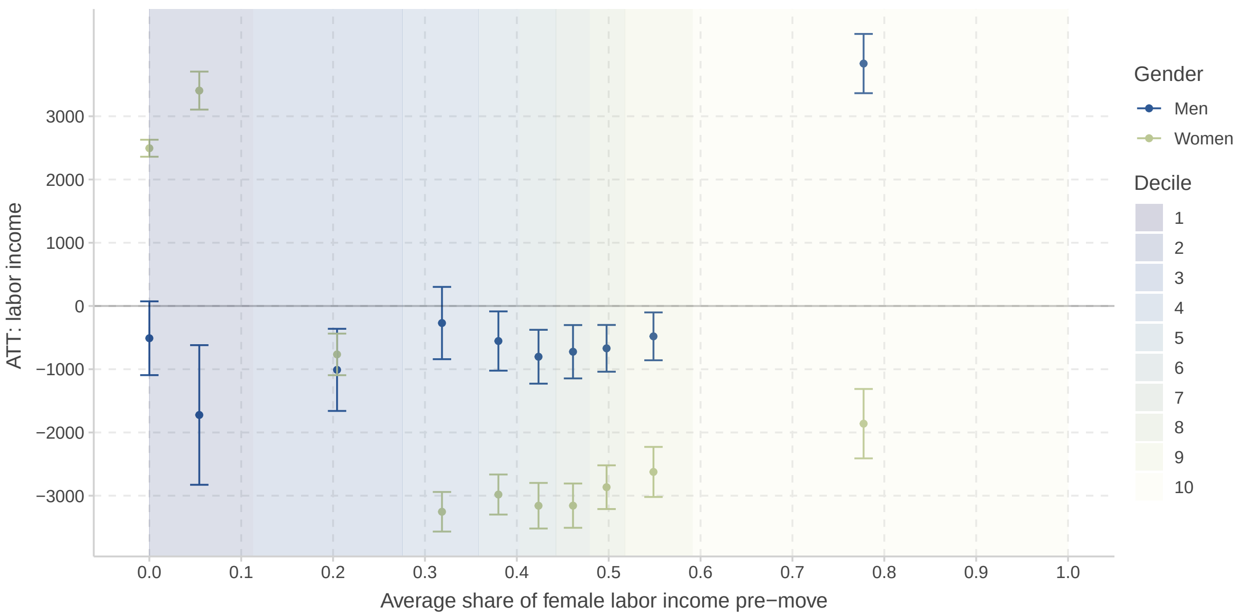
Note: Standard errors in parenthesis. The regressions are run separately by gender and share of female income category (deciles). Regressions use annual labor income as an outcome, and include covariates for average age of couple members, and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The regressions are run separately by gender. The ATT coefficient corresponds to the average treatment effect by duration of exposure to the treatment. The sample is made of households who moved while in a couple between 2015-2019. Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped.

Figure B15. ATT: Annual labor income, by gender and primary earner deciles, in percent



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. Regressions use annual labor income as an outcome, and include covariates for average age of couple members, and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The regressions are run separately by gender and share of female income category (deciles). Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The plotted coefficient corresponds to the average annual treatment effect in the first three years of the move, relative to the group average one year before moving. The sample is made of households who moved while in a couple between 2015-2019. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. The number of observations for each regression is: First decile: 54,111 (11,497 households); Second decile: 18917 (3,991 households); Third decile: 36,833 (7,744 households); Fourth decile: 37,003 (7,744 households); Fifth decile: 36,946 (7,744 households); Sixth decile: 36,754 (7,744 households); Seventh decile: 36,555 (7,745 households); Eighth decile: 36,716 (7,743 households); Ninth decile: 36,665 (7,744 households); Tenth decile: 36,341 (7,745 households).

Figure B16. ATT: Annual labor income, by gender and primary earner deciles, absolute terms (euros)



Note: Bars represent 95% simultaneous confidence band, computed from bootstrapped standard errors, clustered at the individual level. Regressions use annual labor income as an outcome, and include covariates for average age of couple members, and dummies for the presence of children aged under 1, 3, 6, 10 and 18. The regressions are run separately by gender and share of female income category (deciles). Couples are defined as pairs of adults linked by a marriage or civil union (PACS), or cohabiting in a two-adult housing. Couple status is defined at the year of the move. The plotted coefficient corresponds to the average annual treatment effect in the first three years of the move. The sample is made of households who moved while in a couple between 2015-2019. The sample is further restricted to people living in an urban area in mainland France, aged 25-60, who moved without changing household composition during their move year. Same-sex couples are excluded, and balanced sampling is ensured by removing entire households if one member was dropped. The number of observations for each regression is: First decile: 54,111 (11,497 households); Second decile: 18917 (3,991 households); Third decile: 36,833 (7,744 households); Fourth decile: 37,003 (7,744 households); Fifth decile: 36,946 (7,744 households); Sixth decile: 36,754 (7,744 households); Seventh decile: 36,555 (7,745 households); Eighth decile: 36,716 (7,743 households); Ninth decile: 36,665 (7,744 households); Tenth decile: 36,341 (7,745 households).

2

Gender, Households, Children, and the City

Abstract

This paper studies the gender gap in the urban wage premium and its heterogeneity by couple and parental status. Using administrative data on the universe of French residents, I find that the urban wage premium is 30 to 50% larger for women than men once spatial sorting is accounted for. Contrary to expectations, this gender gap is not driven by partnered women or mothers benefiting more from density. Instead, mothers of young children experience a density penalty that nearly eliminates their additional agglomeration gains, suggesting a large role of congestion costs interacting with childcare constraints. I also find a positive effect of density on labor market participation that is stronger for women than men after controlling for sorting, and is not related to household structure.

1. Introduction

The urban economics literature has largely established the existence of an urban wage premium: workers are more productive and earn higher wages in large cities, even after accounting for the sorting of high-skilled individuals in large urban areas. In France, Combes, Duranton, and Gobillon (2008) quantify this relationship, estimating the elasticity of wages with respect to density at approximately 0.03. At the same time, despite significant progress in recent decades, the gender gap in earnings remains a persistent economic reality (Goldin, 2014). Yet the intersection of these two fundamental economic phenomena –agglomeration effects and gender disparities– remains largely unexplored. Do women and men benefit equally from the productivity advantages of dense urban environments?

In this project, I provide novel evidence on the way agglomeration economies differ by gender and household type. A key difficulty in understanding the mechanisms of differential agglomeration gains by gender is the lack of large panel datasets containing information on both labor market outcomes and household composition. I overcome this challenge by accessing a unique administrative dataset that covers the universe of French residents. By chaining it across years, I am able to track individuals and households over time, and to observe income, location, gender and household structure between 2014 and 2019. Applying a standard two-step approach to this new data, I estimate the urban wage premium by category and find that, once accounting for spatial sorting, the urban wage and participation premiums are significantly larger for women. I find that while men have an elasticity of earnings to density of approximately 0.05, this rises significantly to 0.068 for women.

Several theoretical channels could explain differential returns to density by gender. As women tend to have interrupted employment trajectories, and a smaller job search radius (Le Barbanchon et al., 2021), the better job matching in large cities could be more beneficial for them.Furthermore, if women are more likely to be tied movers (Gemici, 2007, Venator, 2023), meaning they move due to the gains of their spouses even if it is not in their direct interest, the improved job matching in large cities could help them adapt to their new residence. On the other hand, career interruptions could attenuate the effects of learning spillovers on wage growth. High congestion cost (in particular childcare and commuting) could also have a stronger negative impact on women than on men. Finally, large cities could differ from smaller ones in their gender norms and discriminatory behavior.

Many of these mechanisms are related to household composition, either through child-related consideration or intra-household decision. In order to assess their relevance to the gender gap in urban wage premium I explore a further heterogeneity dimension and estimate the urban wage premium by gender and couple status, as well as by gender and parental status. If the household related mechanisms played an important role, we would find that the urban wage premium is larger for mothers or coupled women than for single or childless ones. I show instead that women who are in a partnership or have children do not benefit more from density. The significant gender gap in the urban wage premium is driven neither by married women, nor by mothers. Instead, I find that mothers of young children experience a density penalty which almost fully wipes off the female extra agglomeration gain. This suggests that congestion costs linked to child-related constraints wipe out potential benefits from improved matching.

This paper contributes to a large body of work on individual agglomeration gains. Starting with Glaeser et al. (2001), many papers have documented the existence of an urban wage premium, and studied its mechanisms : Combes, Duranton, and Gobillon (2008) in France; D'Costa and Overman (2014) in the UK; De la Roca et al. (2017) in Spain. The empirical part of this literature highlights the existence of large skills sorting, explaining part of the observed wage premium: individual skills are positively correlated to city-size. However, sorting does not explain all of the city size premium, and the literature has shown that there are still gains from density even when sorting is accounted for. Few articles however have explored the heterogeneity of agglomeration gains for different types of worker. Carlsen et al. (2016) estimate the urban wage premium by education level in Norway, and find that college-educated workers have a higher return to working in cities. Another example is Ananat et al. (2018) who show that black workers in the US receive a significantly lower employment density premia than white workers. More recently, Le Roux (2025) shows that returns to density in South Africa are significantly higher for high-income than low-income individuals.

A small set of articles have studied how this wage premium varies by gender. Both Phimister (2005) and Hirsch et al. (2013) find evidence of a small urban participation premium for women but are only able to compare urban and rural women. More recently D'Costa (2024) also estimates the impact of working in cities –defined by opposition to rural areas– on men and women's wages, and finds that before 2008 the urban wage premium for women was twice that of men, but that this difference disappeared after the 2008 crisis. In a different context, Le Roux (2025) finds no difference in the gains from density by gender in South Africa. The closest article to my work is and Elass et al. (2024), who use a similar two-step approach on another French dataset to estimate the impact of urban density on the gender wage gap. They also find that that women benefit more than men from urban density, and after decomposing this gap, they conclude that occupational segregation plays a large part in explaining the difference, followed by childcare and commute constraints. I complement their research by exploring another dimension of heterogeneity: the role of household structure, in particular couple status and presence of children.

While various studies document heterogeneity in agglomeration gains by gender, there is little literature on the mechanisms of these differential gains. In this paper, I explore the particular channel of household structure: as men and women have become more and more similar in their productive characteristics, household-level factors become more important to explain the remaining gender gap. The recent literature on the gender gap in earnings and its sources underlines the importance of children and intra-household specialization in explaining the persistence of this gap (Goldin, 2014, Angelov et al., 2016, Kleven et al., 2019, Cortés et al., 2023, Goldin, 2024). Within-couple specialization, with one member (often the man) focusing on paid work and the other taking on more of the childrearing and domestic work, may lead women to choose jobs which offer more flexibility but lower wages: part-time (Manning et al., 2008), requiring less presenteeism (Azmat et al., 2022), with less commute (Le Barbanchon et al., 2021)... The role of household factors in agglomeration economies has been put forward by the "power couples" literature dating back to Costa et al. (2000), which argues that returns to larger cities may be larger for (highly-educated) couples than other households. This paper is the first to systematically study the role of intra-household factors in differential agglomeration gains by gender.

In a last section I contribute to a small strand of literature which studies geographical disparities in labor market participation by women, dating back to Odland et al. (1998). Black et al. (2014) document large and persistent variation in female participation across US cities, with women in large cities being less likely to work. Similarly, Moreno-Maldonado (2022) finds that women with children are less likely to work when located in big cities. Both papers argue that this is driven by differences in commuting and childcare cost, which create an incentive for intra-household specialization: one spouse working long hours while the other stays out of the labor market. I contribute to this literature by showing that accounting for individual fixed effects in estimation of the participation gap reverses the findings: once sorting is accounted for, I find a positive effect of density on participation which is even larger for women than men.

The rest of this article is organised as follows. In section 2 I expand on the theoretical mechanisms which could explain a gender gap in agglomeration economies. Section 3 details the data used in the analysis, the sample, and the definition of main variables. Section 4 explains the empirical approach to estimate the urban wage premium by group, and section 5 discusses the results, while section 6 focuses on the urban participation premium. Finally, section 7 concludes.

2. Theoretical mechanisms

Agglomeration economies encompass various mechanisms through which larger cities positively affect individual and firm income (Combes and Gobillon, 2015). Since Duranton et al. (2004), those theoretical channels are often classified into three categories: matching, learning and sharing. *Matching* describes how large cities boost productivity and wages by improving both match quality between workers and firms and the probability of finding a match. *Learning* (defined broadly to encompass knowledge acquisition, diffusion and creation) occurs more easily in large cities through firm-to-firm and worker-to-worker interactions. Finally, *sharing* includes multiple dimensions: access to more inputs ("gains from variety"), risk sharing, industry specialization, and access to local indivisible goods and facilities.

These mechanisms suggest several pathways through which the urban wage premium might differ by gender.

The matching channel is particularly relevant for women, in particular mothers, who experience more frequent career interruptions. Denser labor markets could provide them with improved job-matching opportunities that mitigate these disruptions. Furthermore, large cities may offset women's typically smaller job search radius (Le Barbanchon et al., 2021) by offering more employment options within accessible distances. Furthermore, since women are more likely to be "tied-movers" –meaning that households location decisions often prioritize male partners' careers (Mincer, 1978; Tenn, 2010; Feuillade, 2025)–large cities could significantly reduce the career penalty women face by providing more diverse employment opportunities. Following a spouse to a large city rather than a small one would improve expected job quality and reduce search time. Overall the larger array of positions and better matching offered in large cities could help offset the constraints faced by women in their job search behavior.

The sharing mechanism may similarly benefit women differently than men. Access to public goods in dense areas, such as childcare facilities and public transportation, could especially advantage women with children or those in dual-earner households. Better transportation infrastructure could alleviate the within-city dual-location problem faced by couples and expand women's job search radius.

On the other hand, the congestion costs of large cities, which may induce longer commuting times and higher housing and childcare costs could impact women's (and in particular mother's) incentive to work (Moreno-Maldonado, 2022).

These mechanisms all relate to intra-household decisions and constraints. If these mechanisms predominantly operate through household constraints and childcare responsibilities, we would expect gender differences in agglomeration benefits to be concentrated among partnered women and mothers, while single, childless women –who face neither child-related interruptions nor dual-career constraints– should exhibit patterns more similar to men.

Instead, I find a gender-specific gain from agglomeration that benefits all women, including those who are single and childless, suggesting mechanisms beyond household composition are at play. I find no significant heterogeneity by couple status, which could either mean that the household-related channel are not at play, or that the positive effects of matching and sharing and the negative congestion effects approximately offset each other for coupled women. However, I show that mothers of young children experience a substantial negative density effect that eliminates their gender-specific agglomeration benefit, indicating the dominant role of congestion costs for this particular group.

A remaining question is what might explain the presence of a gender gap in agglomeration benefits for single and childless women. Occupational segregation could offer one potential explanation. Papageorgiou (2022) argue that the higher number of available occupations in large cities is a key explanation for the existence of an urban wage premium. Since smaller cities have fewer occupational options, gender-based occupational segregation may be more pronounced in these areas. Given that male-dominated occupations generally exhibit higher wages, this mechanism could generate gendered returns to density. Elass et al. (2024) confirm the importance of this channel, though they find that occupational differences alone cannot fully explain the gender gap in urban wage premiums.

Finally, gender heterogeneity in the urban wage premium could also arise from lower gender-based discrimination. While this is not something I can measure, it is plausible that large cities exhibit more egalitarian gender views and less discriminatory behavior due to local cultural norms or to the sorting of individuals with less discriminatory attitudes. This could create an additional benefit for women in dense labor markets that operates independently of household composition or occupational structure.

3. Data and summary statistics

In this paper, I leverage the Housing and Individual Demographic Files (FIDELI), an administrative dataset covering the universe of French residents between 2014 and 2019. This dataset integrates information across three levels –individuals, households, and housing units– all linkable through unique identifiers.

At the individual level, FIDELI provides basic demographic characteristics (age, gender, birthplace) and detailed income information. While it lacks specific job variables (occupation, hours, industry), it breaks down pre-tax annual income by source (wages, pensions, benefits...). This allows me to construct a comprehensive measure of annual labor income that includes both wages and earnings from independent professions (agricultural, industrial, commercial, or non-commercial). I also use unemployment income data to identify individuals experiencing unemployment within a given year.

A key advantage of FIDELI is the precise geocoding of each individual's address on January 1st of each year. Additionally, the dataset's tax-source origins enable direct identification of fiscal households, allowing me to determine which individuals are married or in civil unions. This measure of couples however misses individuals who are simply living together without any legal status, and cannot file taxes together. To capture those unmarried couples, I expand my definition to include all pairs of adults who live in the same house or flat, with no other adult present. To address potential misclassification concerns, I replicate all results using two alternative, more restrictive definitions: tax-filing couples only, and cohabiting adult pairs with at least one minor child in the household. Results in Appendix C.2 confirm that my findings remain robust across these alternative specifications.

The spatial unit of interest in this analysis are Urban Areas, as defined in 2010 by the National Institute of Statistics (INSEE). Urban Areas are large units defined to encompass an urban core containing at least 10,000 jobs [1] and surrounding municipalities in which at least 40% of the working population is employed within the urban area, in a recursive approach. My estimation of the urban wage premium will rely on within-individual and across-urban areas variation in annual income. However, since I only observe individuals' location at the beginning of each calendar year, I cannot attribute their income to the city it is earned in when people move during the year. I therefore exclude these transition years from my analysis.

My final sample consists of individuals aged 25-60 living in urban areas within mainland France, excluding Corsica and overseas territories. This leaves me with a panel of 19,825,498 people, and over 100 million observations.

Table 2.1 provides some descriptive statistics of the main variables for men and women separately. Because of the very high number of observations, all the difference are statistically significant. Yet the male and female sample are very similar in terms of demographic characteristics: age, couple status, presence of children, number of children (conditional on having at least one). The largest differences are observed for labor market outcomes: women in my sample earn 30% lower annual labor income than men. They are also more likely to be unemployed or to not be receiving any labor income.

Importantly, men and women appear to be quite similar in their mobility. Women are 7% less likely than men to move over the period. Conditional on moving, they appear to be

[1] In their 2010 definitions, INSEE provides three categories of urban areas, with the smallest ones built around a core of 1,500 or 5,000 jobs. As those are small cities, with little variation in density, I exclude them from the analysis and focus instead on relatively large urban areas. Those correspond to 93% of urban area residents

about as likely as men to move towards a smaller (resp. larger) city. They live in cities that are approximately the same size and are as likely to be living in Paris.

Table 2.1: Descriptive statistics by gender

	Male	Female	Difference	T-Statistic
Age	43.157	43.189	0.328^{***}	179.010
Has child	0.494	0.516	-0.022^{***}	-219.510
Number children (conditional)	1.833	1.789	0.043^{***}	176.480
Married	0.576	0.565	0.011^{***}	114.110
Married or cohabiting	0.715	0.688	0.026^{***}	287.580
Married or cohab w/ child	0.662	0.643	0.019^{***}	200.700
Labor income	30,855	21,289	$9{,}566^{***}$	$1,418.290$
Receives UI	0.136	0.151	-0.015^{***}	-204.010
Receives labor income	0.949	0.917	0.033^{***}	642.510
Mobility	0.07	0.065	0.005^{***}	90.410
Number of moves	0.074	0.069	0.005^{***}	82.640
Number of moves (conditional)	1.151	1.139	0.012^{***}	39.130
Moves to smaller city (conditional)	0.621	0.628	-0.007^{***}	-16.140
Moves to larger city (conditional)	0.484	0.468	0.017^{***}	38.710
City population	3,677,053	3,772,937	$-45,884^{***}$	-44.890
Lives in Paris	0.264	0.268	-0.004^{***}	-44.780
Number of years in panel	5.591	5.641	-0.050^{***}	-263.820
Number of observations	48,484,931	53,602,445		

Note: This table is computed from Fideli data. The sample is made of individuals living in an urban area in mainland France, aged 25-60. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}p < 0.01$

4. Estimating the urban wage premium: the two-step approach

The standard method to estimate the density wage premium in the urban economics literature is a two-step approach developed by Combes, Duranton, and Gobillon (2008). This method first estimates individual wages using a model with individual and city fixed effects, then regresses the estimated city effects on city size to determine the elasticity of wages to city size. I adapt this method to estimate gender-specific elasticities as follows.

First I run the regression described in equation (2.1):

$$y_{i,t} = \theta_i + \mathbf{X_{i,t}}\gamma_{G(i,t)} + \beta_{c(i,t),G(i,t),t} + \varepsilon_{it} \tag{2.1}$$

In this equation the outcome variable $y_{i,t}$ represents to annual labor income. θ_i is an individual fixed effect, included to control for the sorting of individuals across cities along unobserved characteristics. The vector X_{it} contains individual characteristics, which in practice consist of age squared interacted with gender. My coefficient of interest, $\beta_{c(i,t),G(i,t),t}$ is a fixed effect for each urban area - group - year combination. Depending on the specification, the group $G(i,t)$ corresponds to a combination of gender $g(i)$, marital or cohabiting status $m(i,t)$, and parental status $p(i,t)$.

In a second step, I regress the estimated $\widehat{\beta}_{c,G,t}$ on group indicators interacted with city size variables, as well as year fixed effects. Equation 2.2 formalizes the second step regression for the simple case when groups are defined based on gender only, using population density ($d_{c,t}$) as a single measure of city size.

$$\widehat{\beta}_{c,g,t} = \alpha + \eta.\mathbb{1}_g + \phi.\log d_{c,t} + \delta.\mathbb{1}_g.\log d_{c,t} + \lambda_t + \nu_{c,g,t} \tag{2.2}$$

Population density is a typical measure of city size since the seminal paper by Ciccone et al. (1996). However, while it provides insight into a city's "compactness", it represents only one dimension of city size –a highly populated city could have low density but large surface area. To account for this, I also introduce land area (interacted with group indicators) in the second stage regression. When both variables are included, density represents the intensive margin of agglomeration: an increase in population for a given city surface. Land area on the other hand captures the extensive margin of agglomeration: an increase in the city footprint holding constant the overall density.

In equation 2.2 density and area are centered around the average for men. This allows η to be interpreted as the difference in city fixed effects between men and women living in the average city *of men*. The coefficient ϕ captures the effect of a change in density relative to men's average city, while δ, my main coefficient of interest, represents the gender gap in the urban wage premium –specifically how women's wages respond differently to city density compared to men's. In cases where groups are defined as the interaction of gender with couple and/or parenthood, the city size variables are centered around single and/or childless men.

When estimating equation 2.2, I weight the regression by the number of observations in each group from the first stage. This allows to interpret these results from the perspective of individuals, as this equation becomes consistent with a model in which the city-group-year effects $\beta_{c(i,t),G(i,t),t}$ in equation 2.1 would be replaced by their expression in equation 2.2. It also has the advantage of putting more weight on the most precisely estimated city-group-year effects. The standard errors in this second step are clustered at the city-year level, as there is no variation in density and area within each city-year group.

A common concern when estimating equation 2.2 is that these regressions may suffer from a series of endogeneity bias, as wages and city size are jointly determined when households and firms choose where to locate. For instance, high productivity and wages could encourage migration to a city, leading to an issue of reverse causality. Omitted local variables could also be affecting both wages and population of a city. To address those concernss, I use a standard strategy and instrument cities' density and area with a set of historical city characteristics and geological features, following Ciccone et al. (1996) and Combes, Duranton, Gobillon, and Roux (2010). More precisely, I use historical values of population density in 1793, 1800, 1836, and 1856, obtained from historical censuses' municipality-level population counts. These instruments should be highly correlated to current density, thanks to the strong persistence of urban infrastructure. Yet, given the large changes in production function in the last 150 years, the unobserved local determinants of city size should no longer be the same than in the past, meaning that these instruments should satisfy the exclusion restriction. Geological characteristics[2] should also have a good predictive power as soil fertility determined the way that population was distributed in the past, when agriculture was more important, but we do not expect it to be correlated to productivity and wages in modern cities.

[2] I use as geological characteristics the proportions of the city area by levels of depth to rock, soil erodability, hydrogeological class, subsoil mineralogy and topsoil organic carbon content

5. Gaps in the urban wage premium by gender and marital status

5.1. Results by gender

Table 2.2 presents the results of the two-step approach described in the previous section, in which fixed effects are computed by gender city and year combination. The corresponding first stage results can be found in Table C1 in Appendix. Columns (1) and (2) correspond to the estimation of equation 2.2, while columns (3) and (4) instrument density and land area with historical and geological variables.

Table 2.2: The density earnings premium by gender

	OLS		IV	
	(1)	(2)	(3)	(4)
Density centered	0.098***	0.050***	0.091***	0.016***
	(0.014)	(0.005)	(0.016)	(0.005)
Female × Density centered	0.049***	0.018***	0.059***	0.025***
	(0.010)	(0.005)	(0.011)	(0.006)
Area centered		0.040***		0.069***
		(0.004)		(0.005)
Female × Area centered		0.025***		0.029***
		(0.005)		(0.007)
Year FE	Yes	Yes	Yes	Yes
Historical IV	No	No	Yes	Yes
Soil IV	No	No	Yes	Yes
R^2	0.965	0.977	0.958	0.968
KPW F-Stat			206.316	7.418
N	2736	2736	2712	2712

Note: This table is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. Robust clustered (urban area × year) standard errors in parentheses. Regressions weighted by the size of each city x gender x year group in 1st stage. The dependent variable is the urban area x gender x year fixed effect obtained from equation 2.1, in which the outcome variable was the log of work income. Density and area are in logarithm, centered around the mean of men. Historical instruments are historical population density in 1861, 1931 and 1954 . Geological instruments are proportions of the city area by levels of depth to rock, soil erodability, hydrogeological class, subsoil mineralogy and topsoil organic carbon content.

Results in column (1) indicate that women who work benefit significantly more from city density than men: the elasticity of their wages with respect to density is around 50% higher than men's. Living in a city with double the density leads to an increase in average wage of 6.95% for men, and 10.4% for women[3]. By introducing land area in columns (2), the elasticity of wages to density reduces by half for both men and women, indicating that part of the density premium in column (1) was capturing the fact that city with higher density also have higher population on average. By controlling for land area, we are now able to disentangle those two dimensions. Interestingly, this adjustment affect men and women in the same way, approximately halving the elasticity of wages to density for both. This suggests that the intensive and extensive margin of agglomeration have around the same relative importance for men and women.

In columns (3) and (4) I report the results of the regression when instrumenting both density and area with historical and geological variables. When introducing only density in the second stage equation in columns (3) I obtain estimates which are very similar to column (1), confirming the common finding of the literature that the potential endogeneity biases are not large. In column (4) however the estimated coefficients on density and land area are no longer so stable. This is likely caused by lack of power in my instruments, due to the strong correlation between a city's density and its land area.

It is worth noting in interpreting the results of Table 2.2 that the outcome variable is *annual* labor income. This means that I am capturing many potential mechanisms, which I cannot always observe directly in my data. In particular I am not able to measure hourly or daily wage, which would be a better measure of productivity. An increase in annual income in my data could be caused either by an increased wage rate or by an increase in the number of hours worked. Given the higher prevalence of part-time work among women, this may be a highly relevant channel. It is plausible that part of the agglomeration premium may occur through an increase in the probability of working full time, conditional on being employed.

5.2. *The role of household structure*

As discussed in section 2, many of the potential mechanisms for this gender gap in the urban wage premium stem from joint household considerations, and/or from childcare considerations. If these mechanisms are indeed important ones, we should find significant heterogeneity in the urban wage for married women or mothers.

[3] If two cities are such that $dens_c = 2 \times dens_{c'}$, it means that $\log(w_c/w_{c'}) = \phi \log(dens_c/dens_{c'}) = \phi \log 2$, with ϕ the coefficient on $\log dens$. So $w_c/w_{c'} = 2^{\phi}$. This means that a doubling of density is associated with a $(2^{\delta} - 1) \times 100\%$ increase in wage.

Couples

In order to test whether coupled women benefit more than single women from agglomeration, I reproduce the two-step approach described in section 4, with the a city-group-year effect $\beta_{c,G,t}$ defined by gender and couple status.

Table 2.3 shows the result of the corresponding second stage estimation. As before, the estimated urban wage premium in columns (1) and (2) is around 50% larger for women than for men. When controlling for land area in column (2), I find a 0.049 elasticity of annual income to density for single men, which rises to 0.069 for single women. This is almost identical to the results of Table 2.2, where I found an elasticity of 5.0% for men and 6.8% for women. This is a first indication that couple status may not matter much in explaining the gender gap in the urban wage premium. If the gendered gains from agglomeration happened exclusively through household-related channels, we should not find any difference between single men and single women, but rather a positive effect of the interaction of gender, couple and city size.

Instead, this triple interaction is negative for both density and land area implying that if anything cohabiting women gain less than single ones from cities' density. However it is worth noting that while this triple interaction is statistically significant in column (2), it is of much smaller magnitude than the interaction of gender with city size. I also find no differential effect of city size on earnings for cohabiting men. Using the instrumental variable approach in columns (3) and (4) does not affect these results.

I reproduce this estimation using alternative definitions of couples in Table C2 (married or civil union) and C3 (married, civil union, or cohabiting *in the presence of a minor child*). In both cases I find very similar results: a strong and significant gender gap in the urban wage premium, and a small and negative coefficient on the intersection of gender, couple status and city size although it is more strongly statistically significant when using those two definitions.

Children

Could the real driver of the observed gender heterogeneity in the density earnings premium be motherhood? As discussed previously, the disruption that children create in women's career (Cortés et al., 2023) may affect the way they can take advantage of agglomeration economies. The direction of this effect is however unclear: while matching and sharing mechanisms would suggest a positive impact, this could be counteracted by congestion costs. Furthermore such a differential effect of city size on mothers should

Table 2.3: The density earnings premium, by gender and cohabiting status

	OLS		IV	
	(1)	(2)	(3)	(4)
Density centered	0.100***	0.049***	0.094***	0.014**
	(0.014)	(0.005)	(0.016)	(0.005)
Female × Density centered	0.051***	0.020***	0.060***	0.026***
	(0.010)	(0.005)	(0.011)	(0.006)
Cohabiting × Density centered	-0.003***	0.001	-0.004***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Cohabiting × Female	-0.002	-0.002*	-0.003	-0.001
× Density centered	(0.002)	(0.001)	(0.003)	(0.001)
Area centered		0.041***		0.071***
		(0.004)		(0.004)
Female × Area centered		0.024***		0.028***
		(0.005)		(0.007)
Cohabiting × Area centered		-0.001***		-0.003***
		(0.000)		(0.001)
Cohabiting × Female		0.001		0.001
× Area centered		(0.001)		(0.002)
Year FE	Yes	Yes	Yes	Yes
Historical IV	No	No	Yes	Yes
Soil IV	No	No	Yes	Yes
R^2	0.963	0.976	0.955	0.966
KPW F-Stat			164.7	9.193
N	5472	5472	5424	5424

Note: This table is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. Robust clustered (urban area × year) standard errors in parentheses. Regressions weighted by the size of each city x gender x couple x year group in 1st stage. The dependent variable is the urban area x gender x couple x year fixed effect obtained from equation 2.1, in which the outcome variable was the log of work income. Density and area are in logarithm, centered around the mean of single men. Historical instruments are historical population density in 1861, 1931 and 1954 . Geological instruments are proportions of the city area by levels of depth to rock, soil erodability, hydrogeological class, subsoil mineralogy and topsoil organic carbon content.

vary depending on the age of the youngest child. Mothers of younger children, who face stronger childcare constraints and may have had more recent career interruptions, might be more likely to have different returns to city size.

In order to test these hypothesis I reproduce the analysis including in the first stage regression city year effects $\beta_{c,G,t}$ which are defined by gender and parental status. I repeat the estimation with three different age thresholds, related to average age of entry in the different school levels in France. I define parents alternatively as those who have a minor child (under 18), a child under 10 years old (corresponding to children pre-middle school entry); under 6 years old (pre-primary school); and under 3 years old (pre-kindergarten). The detailed results can be found in Appendix, in Tables C4, C5, C6, and C7. For comparability purposes, Figure 2.1 displays graphically the coefficients from the the specification in which the $\widehat{\beta}_{c,G,t}$ are regressed on both density and land area, with no instrumental variables.

As before, I find a large gender gap in the wage premium, with women's gains from density and area being around 50% larger than for men. Depending on the age threshold considered, the overall elasticity of earnings to density (when controlling for land area) is estimated to be between 6.7% and 7.4%, which remains similar to the 6.8% for women overall found in Table 2.2. This indicates that the overall gender gap in agglomeration gains is not driven specifically by children-related mechanisms.

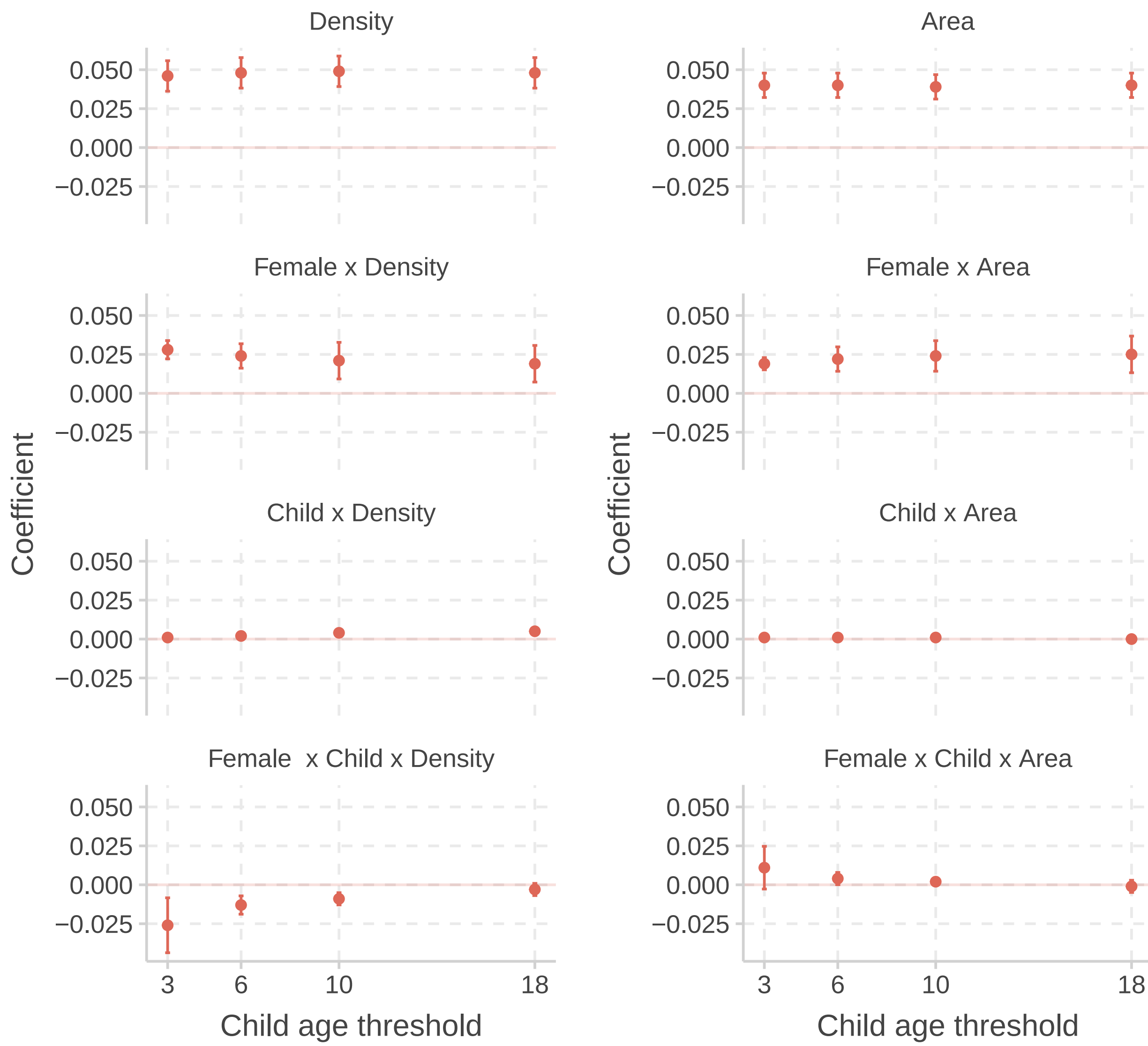
Instead I find significant negative coefficients on the interaction of gender, parenthood and density, which suggests that agglomeration may be harmful to mothers' labor income. When focusing on mothers of children under three (Table C7), the coefficient of -0.026 on the triple interaction almost fully cancels out the extra gain from density accrued by women. For mothers of children under six (Table C6) and ten (Table C5), the triple interaction only reduces by half the gender gap in the density premium, while for mothers of minor children it is no longer statistically significant.

This pattern is consistent with congestion in large cities having a large negative impact on mothers income: high commuting and childcare costs would be particularly harmful to mothers of young children who face more child-related constraints than those of older children.

If these congestion costs are indeed an important factor, removing land area from the regression should decrease the magnitude of the triple interaction coefficient on density. When both population density and land area are included in the regression (Column 2), we capture a scenario where cities cannot expand geographically as population increases. In this case, greater density leads to more severe congestion effects –housing prices rise and commuting becomes more difficult as people compete for limited space.

In contrast, when only density is controlled for (Column 1), the model implicitly allows for city expansion, which would attenuate congestion effects. The empirical results support this interpretation: the triple interaction coefficient (density × female × young child) is consistently more negative when land area is controlled for than when only density is

Figure 2.1. Coefficients on group characteristics interacted with city size, by child age threshold - density earnings premium



Note: This figure is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. Bars represent 95% confidence interval computed from robust standard errors, clustered at the urban area × year level. The regressions are run separately for each age threshold. Regressions are weighted by the size of each city x gender x parenthood x year group in 1st stage. The dependent variable is the urban area x gender x parenthood x year fixed effect obtained from equation 2.1, in which the outcome variable was the log of work income. Density and area are in logarithm, centered around the mean of childless men.

included. This larger negative effect when land area is fixed provides additional evidence that congestion costs are a key mechanism through which density reduces the earnings of mothers with young children.

Overall, these results imply that the motherhood-related matching mechanisms described in section 2 are not at the heart of the gender gap in the urban wage premium. Instead, the negative effect of congestion seems to be harming mothers of young children, who benefit less than other women from agglomeration gains. Interestingly, men with young children do not face the same density penalty: the interaction of density and parenthood is consistently positive, although very small. This could be consistent with intra-household specialization: while the combination of congestion costs and childcare constraints hinder women's income in large cities, their partners might compensate for this relative loss.

6. A participation premium?

So far, in section 5, I used log annual labor income as my main outcome variable, which automatically excluded non-working individuals from my estimations. This approach ignored the extensive margin of labor supply: participation in paid employment. However, the agglomeration mechanisms described in section 2 may differentially impact men's and women's participation rates.

The matching channels could directly affect women's employment probability: finding a job after career interruptions or when relocating with a partner may be more difficult in smaller cities. High living costs in large cities might also drive women to work to supplement household income. Conversely, the substantial congestion costs in large cities could act as a fixed cost of working, potentially discouraging some women from labor market participation altogether (Black et al., 2014).

To investigate this question, I replicate my two-step empirical approach with a new outcome variable: an indicator for earning positive labor income during the year. Table 2.4, columns (1) and (2), present results where first-stage city-year effects are defined by gender only.

I find a substantial positive effect of agglomeration on employment probability. Doubling city density (allowing for land area to adjust) is associated with a 0.009 percentage point increase in the probability of earning labor income for men and a 0.018 percentage point increase for women.[4] Including land area in column (2) reduces these magnitudes, consistent with congestion costs counterbalancing agglomeration's positive effects: when the city fringe cannot adjust, increasing population density becomes less beneficial.

Columns (3) and (4) present results where first-stage city-year effects are defined by both gender and couple (cohabiting) status. Introducing this additional heterogeneity dimen-

[4]Denote p the probability of an individual earning some labor income. If two cities are such that $dens_c = 2 \times dens_{c'}$, and $p_c = \phi dens_c$, it means that $p_c - p_{c'} = \phi \log(dens_c/dens_{c'}) = \phi \log 2$.

Table 2.4: The employment density premium, by gender and couple status

	By gender		By gender & couple	
	$\widehat{\beta}_{c,G,t}$ from Work income > 0			
	(1)	(2)	(3)	(4)
Density centered	0.014***	0.003**	0.015***	0.003**
	(0.003)	(0.001)	(0.003)	(0.001)
Female × Density centered	0.012***	0.007***	0.012***	0.007***
	(0.004)	(0.003)	(0.004)	(0.002)
Cohabiting × Density centered			-0.001***	-0.000
			(0.000)	(0.000)
Cohabiting × Female × Density centered			0.000	0.000
			(0.002)	(0.001)
Area centered		0.009***		0.009***
		(0.001)		(0.001)
Female × Area centered		0.004*		0.004**
		(0.005)		(0.002)
Cohabiting × Area centered				-0.001***
				(0.000)
Cohabiting × Female × Area centered				0.000
				(0.001)
Year FE	Yes	Yes	Yes	Yes
R^2	0.948	0.963	0.944	0.960
N	2736	2736	5472	5472

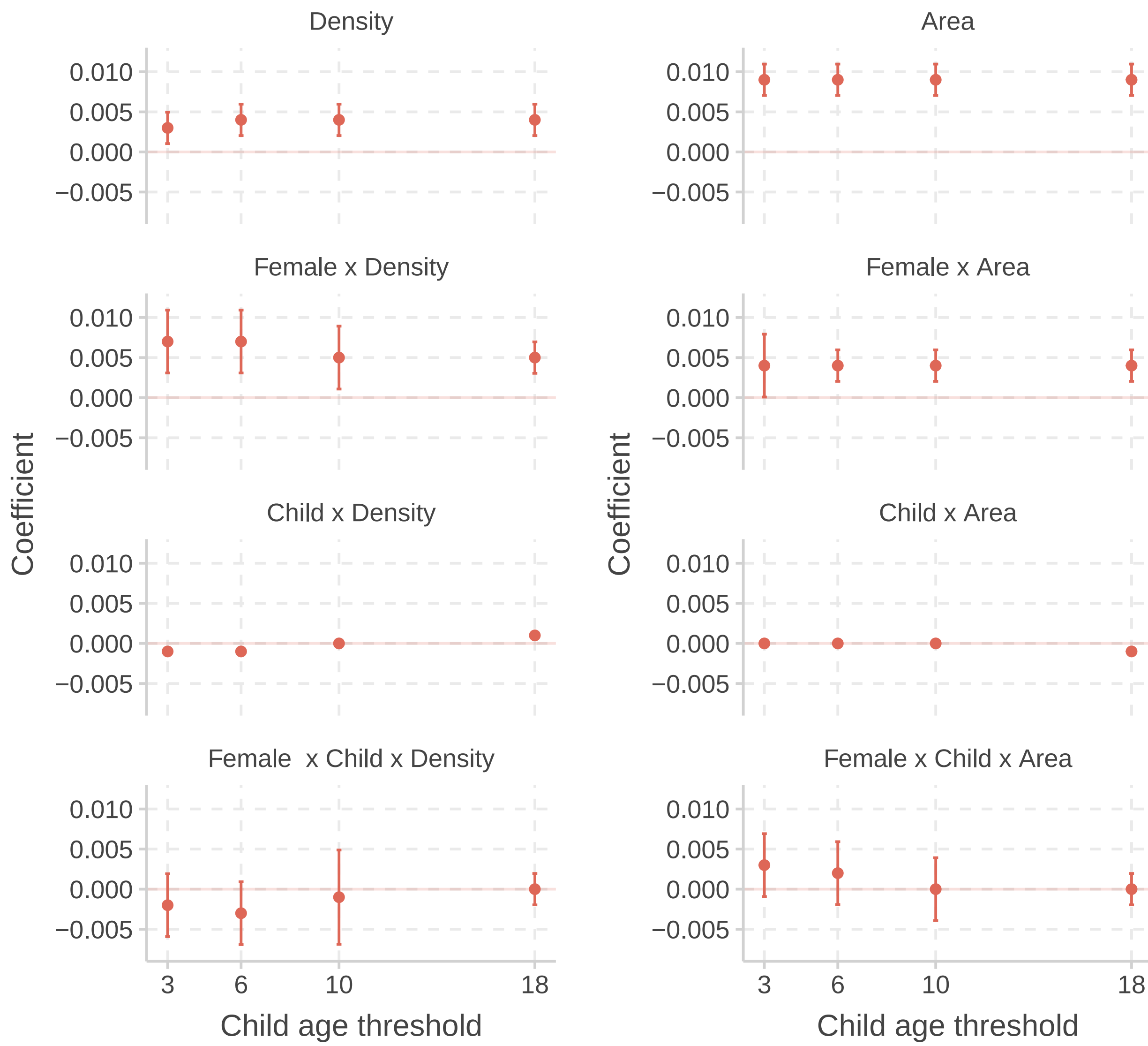
Note: This table is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. Robust clustered (urban area × year) standard errors in parentheses. Regressions weighted by the size of each group x year combination in the 1st stage. The dependent variable in columns (1) and (2) is the group x year fixed effect obtained from equation 2.1, in which the outcome variable was a dummy variable for having earned some labor income during the year. In columns (1) and (2) groups refer to city x gender; in columns (3) and (4) to city x gender x couple. Density and area are in logarithm, centered around the mean of men in columns (1) and (2), single men in columns (3) and (4).

sion barely affects the previously estimated coefficients on the city size and gender interaction. The triple interaction estimates are precisely estimated zeros, indicating this employment density premium is not driven by partnered women.

However, parenthood likely represents a more relevant heterogeneity dimension than cohabitation status, as career interruptions typically relate more to having children than living with a partner.

To allow for this possibility, I reproduce the estimation with city-year-parenthood fixed effects, defined using the same threshold as previously: 18, 10, 6 and 3. In Figure 2.2 I plot the coefficients obtained from a regression of these effects on cities' density and area. I find again a large effect of agglomeration on employment, with a significant gender gap: women benefit at least twice as much as men from density in terms of employment. The coefficient on the gender-parenthood-agglomeration variables is not statistically significant. This indicates that the female employment density premium is not primarily driven by mothers, contrary to what theory might suggest about career interruptions. Instead, the employment benefits of density appear to apply broadly across women's demographic groups.

Figure 2.2. Coefficients on group characteristics interacted with city size, by child age threshold - density employment premium



Note: This figure is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. Bars represent 95% confidence interval computed from robust standard errors, clustered at the urban area × year level. The regressions are run separately for each age threshold. Regressions are weighted by the size of each city x gender x parenthood x year group in 1st stage. The dependent variable is the urban area x gender x parenthood x year fixed effect obtained from equation 2.1, in which the outcome variable was the log of work income. Density and area are in logarithm, centered around the mean of childless men.

7. Conclusion

This paper documents significant gender differences in urban agglomeration economies. Using French administrative data and controlling for spatial sorting through a standard two-steps approach, I find that women experience a substantially larger urban wage premium than men, with elasticities of earnings to density of 0.068 and 0.05 respectively. Contrary to what several theoretical mechanisms might predict, neither partnership status nor motherhood in general drives these differential returns to density. On the contrary, mothers of young children appear to experience negative density effects that counteract the additional benefits women typically receive from urban agglomeration. This suggests that specific constraints related to childcare and urban congestion disproportionately affect mothers in high-density areas, effectively neutralizing the improved job matching opportunities that cities might otherwise offer them.

These findings suggest that urban density potentially offers a mechanism to reduce the gender wage gap, yet this potential is constrained by the disproportionate effect of children on women's labor market outcomes.

Additionally, I find a positive relationship between urban density and labor market participation after controlling for individual fixed effects, countering previous findings in the literature that did not account for sorting. This urban participation premium is more pronounced for women than men, but is again not driven specifically by partnered women nor mothers.

Overall, the fact that the extra gains to density for women are not explained by intra-household mechanisms or the presence of children suggests that other factors –possibly including occupation, hours worked, public sector employment, or discrimination– may play more significant roles in explaining why women benefit more from urban agglomeration. In the future it would be interesting to decompose this gender gap into its different margins, using alternative data. By estimating the density premium separately for daily income and hours worked, rather than annual income alone, we would gain a better understanding of the mechanisms driving gender differences in agglomeration gains. Another dimension that would be worth exploring is the role of occupations: is it the case that women work in occupations for which the gains of density are higher? This should all be possible using the French matched-employer employee data.

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C. Additional tables and figures

C.1. First stage results

Table C1: First stage regressions, annual labor income

	Fixed-effects defined by:					
	Gender	Gender × Couple	Gender × Child <3	Gender × Child <6	Gender × Child <10	Gender × Child <18
	(1)	(2)	(3)	(4)	(5)	(6)
Age squared	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age squared × Female	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.773	0.773	0.774	0.774	0.773	0.773
Adj. R^2	0.718	0.718	0.719	0.718	0.718	0.718
N	88,343,901	88,343,901	88,343,901	88,343,901	88,343,901	88,343,901

Note: This table is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. Couples are restricted to people who are married or in a civil union. The dependent variable is the log of annual labor income.

C.2. Alternative couple definitions

Table C2: The density earnings premium, by gender and marital status

	OLS		IV	
	(1)	(2)	(3)	(4)
Density centered	0.097***	0.047***	0.091***	0.013***
	(0.014)	(0.005)	(0.016)	(0.005)
Female × Density centered	0.052***	0.021***	0.061***	0.026***
	(0.010)	(0.005)	(0.011)	(0.006)
Married × Density centered	0.001	0.004***	-0.000	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Married × Female	-0.007***	-0.004**	-0.006**	-0.002
× Density centered	(0.002)	(0.002)	(0.003)	(0.001)
Area centered		0.041***		0.070***
		(0.004)		(0.004)
Female × Area centered		0.025***		0.029***
		(0.005)		(0.006)
Married × Area centered		-0.001***		-0.003***
		(0.000)		(0.001)
Married × Female		-0.001		-0.001
× Area centered		(0.001)		(0.002)
Female; Married; Married × Female	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Historical IV	No	No	Yes	Yes
Soil IV	No	No	Yes	Yes
R^2	0.960	0.974	0.951	0.962
KPW F-Stat			174.8	9.355
N	5472	5472	5424	5424

Note: This table is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. Couples are restricted to people who are married or in a civil union. Robust clustered (urban area × year) standard errors in parentheses. Regressions weighted by the size of each city x gender x couple x year group in 1st stage. The dependent variable is the urban area x gender x couple x year fixed effect obtained from equation 2.1, in which the outcome variable was the log of work income. Density and area are in logarithm, centered around the mean of single men. Historical instruments are historical population density in 1861, 1931 and 1954 . Geological instruments are proportions of the city area by levels of depth to rock, soil erodability, hydrogeological class, subsoil mineralogy and topsoil organic carbon content.

Table C3: The density earnings premium, by gender and cohabiting status (with children)

	OLS		IV	
	(1)	(2)	(3)	(4)
Density centered	0.099***	0.048***	0.094***	0.013**
	(0.010)	(0.005)	(0.016)	(0.006)
Female × Density centered	0.052***	0.021***	0.060***	0.027***
	(0.010)	(0.005)	(0.011)	(0.006)
Cohabiting w/ child × Density centered	-0.002**	0.002**	-0.003***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
Cohabiting w/child × Female × Density centered	-0.005*	-0.004**	-0.005*	-0.004***
	(0.003)	(0.002)	(0.003)	(0.001)
Area centered		0.041***		0.071***
		(0.004)		(0.004)
Female × Area centered		0.024***		0.028***
		(0.005)		(0.006)
Cohabiting w/child × Area centered		-0.001**		-0.003***
		(0.000)		(0.001)
Cohabiting w/child × Female × Area centered		0.001		0.001
		(0.001)		(0.002)
Female; Cohabiting w/child; Interactions	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Historical IV	No	No	Yes	Yes
Soil IV	No	No	Yes	Yes
R^2	0.960	0.974	0.950	0.962
KPW F-Stat			163.9	8.656
N	5472	5472	5424	5424

Note: This table is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. Couples are restricted to people who are married, in a civil union, or living in a shared housing with one other adult and at least one minor child. Robust clustered (urban area × year) standard errors in parentheses. Regressions weighted by the size of each city x gender x couple x year group in 1st stage. The dependent variable is the urban area x gender x couple x year fixed effect obtained from equation 2.1, in which the outcome variable was the log of work income. Density and area are in logarithm, centered around the mean of single men. Historical instruments are historical population density in 1861, 1931 and 1954 . Geological instruments are proportions of the city area by levels of depth to rock, soil erodability, hydrogeological class, subsoil mineralogy and topsoil organic carbon content.

C.3. Tables by gender and parenthood status

Table C4: The density earnings premium, by gender and parental status (minor child)

	OLS		IV	
	(1)	(2)	(3)	(4)
Density centered	0.096***	0.048***	0.090***	0.014**
	(0.015)	(0.005)	(0.016)	(0.005)
Female $\times$ Density centered	0.049***	0.019***	0.060***	0.028***
	(0.011)	(0.006)	(0.013)	(0.007)
Child < 18 $\times$ Density centered	0.005***	0.005***	0.004***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Child < 18 $\times$ Female	-0.005*	-0.003	-0.006*	-0.007***
$\times$ Density centered	(0.003)	(0.002)	(0.003)	(0.002)
Area centered		0.040***		0.069***
		(0.004)		(0.005)
Female $\times$ Area centered		0.025***		0.027***
		(0.006)		(0.008)
Child < 18 $\times$ Area centered		0.000		-0.001
		(0.000)		(0.001)
Child < 18 $\times$ Female		-0.001		0.000
$\times$ Area centered		(0.002)		(0.003)
Female; Child < 18; Interactions	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Historical IV	No	No	Yes	Yes
Soil IV	No	No	Yes	Yes
R^2	0.966	0.977	0.960	0.969
KPW F-Stat			100.390	17.111
N	5472	5472	5424	5424

Note: This table is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. The parent group is restricted to those with a child below 18 years old. Robust clustered (urban area $\times$ year) standard errors in parentheses. Regressions weighted by the size of each city x gender x parent x year group in 1st stage. The dependent variable is the urban area x gender x parent x year fixed effect obtained from equation 2.1, in which the outcome variable was the log of work income. Density and area are in logarithm, centered around the mean of childless men. Historical instruments are historical population density in 1861, 1931 and 1954 . Geological instruments are proportions of the city area by levels of depth to rock, soil erodability, hydrogeological class, subsoil mineralogy and topsoil organic carbon content.

Table C5: The density earnings premium, by gender and parental status (child under 10 years old)

	OLS		IV	
	(1)	(2)	(3)	(4)
Density centered	0.096***	0.049***	0.089***	0.014**
	(0.015)	(0.005)	(0.016)	(0.006)
Female × Density centered	0.050***	0.021***	0.060***	0.029***
	(0.011)	(0.006)	(0.012)	(0.007)
Child < 10 × Density centered	0.006***	0.004***	0.006***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Child < 10 × Female × Density centered	-0.006***	-0.009***	-0.006***	-0.010***
	(0.002)	(0.002)	(0.002)	(0.002)
Area centered		0.039***		0.069***
		(0.004)		(0.005)
Female × Area centered		0.024***		0.027***
		(0.005)		(0.008)
Child < 10 × Area centered		0.001*		-0.002**
		(0.000)		(0.001)
Child < 10 × Female × Area centered		0.002*		0.003
		(0.001)		(0.002)
Female; Child < 10; Interactions	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Historical IV	No	No	Yes	Yes
Soil IV	No	No	Yes	Yes
R^2	0.967	0.978	0.962	0.970
KPW F-Stat			145.118	14.087
N	5472	5472	5424	5424

Note: This table is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. The parent group is restricted to those with a child below 10 years old. Robust clustered (urban area × year) standard errors in parentheses. Regressions weighted by the size of each city x gender x parent x year group in 1st stage. The dependent variable is the urban area x gender x parent x year fixed effect obtained from equation 2.1, in which the outcome variable was the log of work income. Density and area are in logarithm, centered around the mean of childless men. Historical instruments are historical population density in 1861, 1931 and 1954 . Geological instruments are proportions of the city area by levels of depth to rock, soil erodability, hydrogeological class, subsoil mineralogy and topsoil organic carbon content.

Table C6: The density earnings premium, by gender and parental status (child under 6 years old)

	OLS		IV	
	(1)	(2)	(3)	(4)
Density centered	0.096***	0.048***	0.090***	0.015***
	(0.014)	(0.005)	(0.016)	(0.005)
Female × Density centered	0.051***	0.024***	0.058***	0.028***
	(0.008)	(0.004)	(0.009)	(0.005)
Child < 6 × Density centered	0.004***	0.002**	0.004***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Child < 6 × Female × Density centered	-0.008***	-0.013***	-0.005*	-0.009***
	(0.003)	(0.003)	(0.003)	(0.003)
Area centered		0.040***		0.069***
		(0.004)		(0.004)
Female × Area centered		0.022***		0.026***
		(0.004)		(0.005)
Child < 6 × Area centered		0.001**		-0.002***
		(0.000)		(0.000)
Child < 6 × Female × Area centered		0.004***		0.003
		(0.002)		(0.002)
Female; Child < 6; Interactions	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Historical IV	No	No	Yes	Yes
Soil IV	No	No	Yes	Yes
R^2	0.953	0.969	0.939	0.953
KPW F-Stat			144.984	11.627
N	5472	5472	5424	5424

Note: This table is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. The parent group is restricted to those with a child below 6 years old. Robust clustered (urban area × year) standard errors in parentheses. Regressions weighted by the size of each city x gender x parent x year group in 1st stage. The dependent variable is the urban area x gender x parent x year fixed effect obtained from equation 2.1, in which the outcome variable was the log of work income. Density and area are in logarithm, centered around the mean of childless men. Historical instruments are historical population density in 1861, 1931 and 1954 . Geological instruments are proportions of the city area by levels of depth to rock, soil erodability, hydrogeological class, subsoil mineralogy and topsoil organic carbon content.

Table C7: The density earnings premium, by gender and parental status (child under 3 years old)

	OLS		IV	
	(1)	(2)	(3)	(4)
Density centered	0.095***	0.046***	0.090***	0.015***
	(0.014)	(0.005)	(0.015)	(0.005)
Female × Density centered	0.052***	0.028***	0.056***	0.027***
	(0.007)	(0.003)	(0.007)	(0.004)
Child < 3 × Density centered	0.004***	0.001	0.004***	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Child < 3 × Female × Density centered	-0.012	-0.026***	-0.003	-0.011
	(0.012)	(0.009)	(0.013)	(0.008)
Area centered		0.040***		0.069***
		(0.004)		(0.003)
Female × Area centered		0.019***		0.025***
		(0.002)		(0.003)
Child < 3 × Area centered		0.001***		-0.001*
		(0.001)		(0.001)
Child < 3 × Female × Area centered		0.011*		0.006
		(0.007)		(0.010)
Female; Child < 3; Interactions	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Historical IV	No	No	Yes	Yes
Soil IV	No	No	Yes	Yes
R^2	0.925	0.950	0.873	0.899
KPW F-Stat			145.167	10.608
N	5472	5472	5424	5424

Note: This table is computed from Fideli data. The sample is made of individuals aged 25-60 living in urban areas in mainland France. The parent group is restricted to those with a child below 3 years old. Robust clustered (urban area × year) standard errors in parentheses. Regressions weighted by the size of each city x gender x parent x year group in 1st stage. The dependent variable is the urban area x gender x parent x year fixed effect obtained from equation 2.1, in which the outcome variable was the log of work income. Density and area are in logarithm, centered around the mean of childless men. Historical instruments are historical population density in 1861, 1931 and 1954 . Geological instruments are proportions of the city area by levels of depth to rock, soil erodability, hydrogeological class, subsoil mineralogy and topsoil organic carbon content.

3

Rise in home working and spousal labor supply

This chapter is based on an article co-authored with Dominique Goux (INSEE, CREST) and Eric Maurin (PSE, EHESS)

Abstract

This article explores how an employee's choice to work from home (WFH) influences his or her spouse's outcomes. Drawing on the specific features of the French experience, we show that a spouse's switch to WFH leads to a sharp increase in the probability that his or her partner will also switch to WFH, as well as in the number of hours worked by the partner. These cross-effects are particularly strong for men, who seem to primarily condition their decision to work from home on that of their partner. The effects of WFH on the volume of hours worked are greatly underestimated when spillovers within couples are neglected.

1. Introduction

The pandemic shock of the early 2020s catalyzed an unprecedented expansion of work from home worldwide. A growing number of experimental and quasi-experimental studies are examining whether and how this development might impact the productivity or well-being of affected employees, with mixed and still-debated results (e.g., Atkin et al., 2023, Emanuel, Harrington, and Pallais, 2023, Angelici et al., 2024, Bloom, Han, et al., 2024, Emanuel and Harrington, 2024). In contrast, the implications of this development for those living with teleworkers have received far less attention and remain largely unexplored. Exploring these implications is all the more important as the interdependence of individual decisions within families has long been identified as a key parameter for understanding the full range of consequences that can result from developments or from reforms that directly affect only part of the population (e.g., Ashenfelter et al., 1974, Gelber, 2014, Goux, Maurin, and Petrongolo, 2014, Lalive et al., 2017, Johnsen et al., 2022).

As working from home drastically reduces commute times, it has the potential to considerably change how men and women spend their days and interact with each other within families. Several scenarios are possible. For example, the transition to working from home for some employees may lead them to take on a greater share of domestic work and childcare, freeing up time for their partners, enabling the latter to invest more in their work and increase the number of hours they work. Given the importance that the ability to work long hours can have in many occupations, the consequences can be considerable for both spouses and their relative occupational status (e.g., M. Bertrand et al., 2010, Goldin, 2014, Cortés et al., 2019). Conversely, having one partner work from home might encourage the other to also work remotely, not necessarily to work more hours, but simply to spend more time with the family, without any major impact on the number of hours worked by either spouse. Depending on which of these scenarios dominates the other, the consequences of the rise in home working on the number of hours worked in the economy or on inequalities between men and women within couples are potentially very different.

The aim of this article is to shed light on these issues and to estimate the causal effect of an employee's choice to work from home on his or her spouse's labor outcomes. To the best of our knowledge, there are still no studies that have addressed these issues, one of the difficulties being to find independent variations in spouses' exposure to WFH. Our research strategy draws on the particular institutional context in which the 2020 epidemic shock hit French firms. In late 2017, France passed legislation that facilitated the adoption of work-from-home (WFH) through collective bargaining agreements. This reform created two groups of establishments: those that had signed WFH agreements in 2018 or 2019 (our treatment group) and those that had only signed agreements on other topics during the same period (our control group). While both groups showed similar remote work patterns in the years before the pandemic, the 2020 shock led to significantly larger increases in WFH in treatment group firms. We leverage this variation by comparing labor market outcomes of employees whose spouses work at treatment versus control group firm, essentially comparing workers whose partners faced high versus low exposure to pandemic-induced WFH adoption.

This approach first suggests the existence of very significant cross effects on WFH: employees were significantly more likely to work from home in the years following the 2020 pandemic if their spouse worked at a treatment group establishment, regardless of their own establishment's treatment status. Importantly, no such differences existed in the pre-pandemic period, supporting our identification strategy. Employees whose spouses are in the treatment increased their WFH by an amount equal to roughly 80% of their spouses' increase. This suggests that when one spouse adopts WFH, it raises the probability of the other spouse also working from home by approximately 0.8. Unsurprisingly, these cross-effects on WFH are only noticeable in couples whose members have remotable occupations and not in couples whose occupations are difficult to perform remotely.

Employees with spouses in the treatment group were not only more likely to work from home after the epidemic shock, but they also significantly increased their usual number of hours worked per week compared to employees with spouses in the control group. According to our estimates, an employee's switch to home working is followed on average by a 20% increase in the spouse's usual weekly working time. A closer look at this increase shows that it essentially corresponds to the substitution of long workweeks (40 hours or more) for weeks of 35 hours or less (35 hours being the legal length of the workweek). Consistent with the idea that these cross-effects on hours worked are linked to the effects on WFH, they are also only noticeable in couples whose members have remotable occupations.

Additional analyses reveal that cross-effects on WFH and hours worked primarily affect men whose spouse is in the treatment group, while almost no cross-effects are detected for women whose spouse is in the treatment group. Conversely, direct effects on WFH and hours worked affect women in the treatment group much more than men in the treatment group. We show that these profound asymmetries between men and women are consistent with a simple model where less-paid spouses (in the vast majority of cases, women) work from home as much as legally possible in their companies, independently of the choices of their partners, while better-paid spouses (in the vast majority of cases, men) only increase their rate of work at home to the extent that their partners also increase it, so as to be able to benefit from the time saved on the home-work commute without having to worry about having to increase their contribution to domestic tasks or childcare.

As an employee's move to WFH greatly increases his or her partner's propensity to work from home, we may ask whether this is not also accompanied by a change in their place of residence. We find no evidence to support this hypothesis, as the switch to home working for an employee is not accompanied by any significant change in his or her partner's home-work distance, or in the likelihood of the couple deciding to live away from urban centers. The rise of remote working has reduced the commuting costs associated with moving away from urban centers for the many white-collar workers working and living in those centers, but not enough to offset the other costs of such distance, notably in terms of reduced access to better educational, medical, or cultural infrastructure. Ultimately, the reduction in the frequency of home-to-work journeys induced by the switch to WFH does not seem to be offset by an increase in home-to-work distances, which helps to explain the gains in available time (particularly for work) achieved by couples with remotable occupations.

Our article contributes to the long-standing literature exploring the influence that workers have on each other within couples. An important strand of this literature has shown how workers respond to changes in their spouses' earnings or work hours, whether at the time of their spouses' retirement, during unemployment spells, or after a tax reform (e.g., Lundberg, 1988; Bingley et al., 2007; Gelber, 2014; Lalive et al., 2017; Johnsen et al., 2022). Using changes in the regulation of public holidays or the legal workweek, another stream of literature has further highlighted the importance that workers place on the possibility of adjusting and synchronizing their working hours with those of their spouse (e.g., Hunt et al., 1998, Goux, Maurin, and Petrongolo, 2014; Hamermesh et al., 2017; Georges-Kot et al., 2024). In this article, we highlight the value employees place on being able to coordinate their presence at home with that of their spouse, and the far-reaching consequences this coordination can have on their working hours.

We also contribute to the burgeoning literature exploring the causes and consequences of the rise in WFH that has followed the pandemic shock. Several articles have shown that workers, and especially women, have a distaste for commuting and a strong willingness to pay for remote work (e.g. Mas et al., 2017, He et al., 2021, Chen et al., 2023, Cullen et al., 2025, Le Barbanchon et al., 2021, Bütikofer et al., 2024). Our article suggests that the value employees place on working from home reflects at least in part the particular value they place on interactions within the couple, with employees' demand for working from home appearing all the stronger the more their partner works from home themselves.

Another important strand of the literature focuses on the effects of WFH on the productivity and labor outcomes of the employees involved (e.g., Bloom, Liang, et al., 2015, Choudhury et al., 2021, Emanuel, Harrington, and Pallais, 2023, Gibbs et al., 2023, Barrero et al., 2023, Atkin et al., 2023, Angelici et al., 2024, Emanuel and Harrington, 2024, Bloom, Han, et al., 2024). This literature is based on local experiments and quasi-experiments conducted in specific companies and focuses on the effects of working from home on the employees concerned. Using a large-scale natural experiment, we focus on a different question: the induced effects on the spouses of the employees concerned. We find that when an employee switches to WFH, this greatly increases the likelihood of his or her spouse switching to WFH, but it also increases the number of hours the spouse devotes to work, particularly the better-paid spouse. These results suggest that we have a very incomplete view of the effects of WFH if we do not take into account the strong interdependencies existing within couples.

The paper proceeds as follows. Section 2 provides an overview of the French institutional context. Section 3 develops a simple model for understanding the effects on an employee's working time (and on the proportion of that time spent at home) of a shock that specifically increases his or her spouse's opportunities to work from home. Section 4 describes the data used. Sections 5 and 6 present our main graphical and regression results. Section 7 further discusses the effects of the rise of WFH on residential choices. Section 8 concludes.

2. Institutional Context

In September 2017, France changed the legal framework for teleworking, with the aim of reducing administrative barriers to the use of teleworking for employers and employees. This reform marked a profound change from the previous framework, which required complete formal revisions of employment contracts for any new working from home (WFH) arrangement, even temporary.

The new legal provisions eliminate the need to modify employment contracts on a case-by-case basis. Instead, employers can sign collective agreements outlining both the eligibility criteria and implementation procedures for telework. Once such an agreement is in place, employees can initiate or modify WFH arrangements through a simple email exchanges with their employer, streamlining what was previously a more formal negotiation process.

While collective agreements facilitated the adoption of telework, they did not guarantee its implementation. Even when a collective agreement is in place, the law maintains a voluntary principle: teleworking requires mutual consent from both parties. Employers cannot mandate telework (with exceptions during extraordinary circumstances such as lockdown periods, which we exclude from our analysis), and refusal by employees does not constitute grounds for dismissal. Conversely, employers retain the right to decline telework requests, although they need to provide justification. Either party can terminate the telework arrangement upon request, reverting to on-site work. The legislation ensures teleworkers maintain equal rights and benefits compared to their on-site colleagues. The legislation further specifies that switch to telework cannot affect other employment terms (such as remuneration, working hours, leave entitlements...).

The law outlines several necessary components for telework agreements to address. All agreements should first define activities and occupations eligible for telework, as well as criteria for employees' eligibility (if any). They should also include permissible telework locations, which most often corresponds to employee's primary or secondary residences but can also include designated shared spaces. Finally, the agreement should detail employer provisions for technology-related expenses.

Following this legal change, approximately 2,600 telework agreements were established in 2018 or 2019, before the COVID-19 pandemic. As we will come back to later, our research strategy will be based on comparing employees in establishments that signed these telework agreements (treatment group) with employees in establishments that signed agreements on other themes during the same 2018-2019 period (control group), before and after the 2020 pandemic shock. It is likely that many of the establishments in the control group ended up signing a telework agreement in the years following the pandemic shock, so our strategy amounts at least in part to comparing employees in early and late signatory establishments.

3. Conceptual framework

In this section, before moving on to the empirical analysis, we develop a labor supply model to understand how and why a shock affecting the WFH opportunities of a group of employees can influence their spouses' choice to work from home as well as the number of hours worked by their spouses. This model helps identify some of the fundamental reasons why cross-effects can be very different from one spouse to another, depending in particular on the commuting time of each spouse, but also on their respective pay levels. The model also allows us to understand the importance that certain domestic tasks can play, namely those whose sharing between spouses potentially varies greatly depending on who remains working at home.

3.1. The Model

We consider a sample of individuals married or cohabiting, and denote c their consumption level, ℓ their leisure time, h their paid work time, d the time they allocate to domestic tasks (and childcare), and m their home-to-work commuting time. We further denote π the fraction of their working days they spend at home. If m_0 represents commuting time in the absence of work-from-home, the effective commuting time can be written as $m = (1-\pi)m_0$. With these notations, the time constraints faced by individuals can be written as:

$$T_0 = \ell + h + (1 - \pi)m_0 + d \tag{3.1}$$

where T_0 represents the total number of available hours. Similarly, if ℓ_s, h_s, d_s, and m_s represent the leisure time, work time, domestic task contribution, and commuting time of spouse s, we have:

$$T_0 = \ell_s + h_s + (1 - \pi_s)m_{0s} + d_s \tag{3.2}$$

where π_s is the fraction of working days spent at home by s. In this context, increasing the share of work time spent at home has the obvious advantage of reducing commuting time and increasing time available for other activities.[1] We will assume this comes at the cost of increasing the share of domestic work and childcare time for the individuals concerned. More precisely, denoting d_0 as the total volume of domestic work that the spouses must do (volume assumed to be constant), we will write,

$$d = d_0 f(\pi, \pi_s) \text{ and } d_s = d_0 - d, \tag{3.3}$$

where $f(\pi, \pi_s)$ represents the share of domestic work performed by individuals when they spend a fraction π of their working days at home and their spouse spends a fraction π_s. The function $f(\pi, \pi_s)$ will be assumed to be increasing with π and decreasing with π_s.[2]

3.2. *Preferences and choices*

Regarding preferences, we will represent the utility that individuals derive from leisure and consumption by a function $U(\ell, c)$ increasing in each of its arguments and quasi-concave. Similarly, we will represent the utility that spouses derive from leisure and consumption by a function $U_s(\ell_s, c_s)$, which is also well-behaved. In the spirit of (Chiappori, 1992), both spouses are assumed to make their choices cooperatively to maximize a linear combination of individual utilities:

[1] Commuting time is on average around 50 minutes per person per day in France (Zilloniz, 2015). Aksoy et al. (2023) estimates that the average daily commute time savings when working from home are 72 minutes in a sample of 27 countries.

[2] To our knowledge, there is still little evidence on the causal effect of WFH on the sharing of housework and childcare in the post-pandemic period. See, however, Gaudecker et al. (2024) or Schüller (2025) who provide evidence that employees in remotable occupations have increased their childcare contribution in the post-pandemic period. Also, in the experiment conducted in an Italian bank by Angelici et al. (2024), employees randomly selected to work from home significantly increased the time spent on domestic tasks and childcare.

$$
\begin{aligned}
& \max \mu U(\ell, c) + (1-\mu) U_s(\ell_s, c_s) && (3.4)\\
\text{subject to}\quad & T_0 = \ell + h + (1-\pi)m_0 + d_0 f(\pi, \pi_s) \\
& T_0 = \ell_s + h_s + (1-\pi)m_{0s} + d_0(1 - f(\pi, \pi_s)) \\
& c + c_s = w_0 h + w_{0s} h_s \;\; ; \;\; \pi \leq D \;\; ; \;\; \pi_s \leq D_s
\end{aligned}
$$

where μ is a measure of the individual's bargaining power, while w_0 and w_{0s} represent the hourly wages of the individual and their spouse.[3] By convention, the subscript s will be reserved for the less-paid spouse and we will therefore assume $w_0 \geq w_{0s}$.[4] The parameters D and D_s represent the constraints on the fraction of working days that can be spent at home. They typically capture the limits that employers place on their employees' teleworking possibilities.

Finally, in the remainder of this section, we will denote π^*, ℓ^*, d^*, h^* (resp., π_s^*, ℓ_s^*, d_s^*, h_s^*) the optimal choices for π, ℓ, d, and h (resp., π_s, ℓ_s, d_s, and h_s) and our objective will be to identify the effects of exogenous increases in D_s (resp. D) on these quantities.

3.3. *Work from Home Decisions*

The resolution of the program of the couple is detailed in the appendix. We can first show that it is always optimal for spouse s (the less paid one) to set π_s at its maximum level, namely,

$$\pi_s^* = D_s. \tag{3.5}$$

Working more from home not only saves commuting time for less-paid spouses but also leads them take on a larger share of domestic work and childcare, which is always optimal for the household because it opens up the possibility of substituting better-paid working time for less-paid working time.

With respect to the better-paid partners, their optimal π^* depends on several parameters, including the sensitivity of the share that they take in domestic work to their rate of work at home (as captured by the first derivative of function f). If the share that they take in domestic work varies little according to their rate of work at home, then then there is no

[3]Note that the parameter μ potentially depends on w_0 and w_{0s}, the latter being assumed to be constant in our discussion.

[4]As discussed below, the less-paid partner in the couple happens to be the woman in more than two-thirds of the couples in our work sample.

real cost to them of working more at home and they too will have an interest in setting π^* at its maximum value, namely $\pi^* = D$.

If, on the other hand, the share they take in domestic work varies strongly depending on their rate of WFH, then they will not necessarily have an interest in setting their rate of WFH at its maximum value. They will arbitrate between saving commuting time and losing time on domestic work and childcare, which will lead them to choose π^* satisfying the first-order condition,

$$(w_0 - w_{0s})d_0 f_1'(\pi^*, D_s) = m_0 w_0. \tag{3.6}$$

The left-hand side of the equation represents what the couple loses (due to changes in the sharing of domestic tasks) from an elementary increase in the frequency of WFH by the partner with the highest hourly wage, while the right-hand side represents what the couple gains (due to gains in commuting time).

3.4. Cross Effects on Work from Home

From the above discussion, it emerges that an increase ΔD_s in WFH possibilities for less-paid spouses has a very direct impact on their actual remote work time, namely $\Delta\pi_s = \Delta D_s$. The next question is whether such a shock might not also have a significant cross-effect on the remote work time of their partners.

If we first focus on partners whose optimal remote work time is constrained (i.e., π^*=D), the answer is, by construction, negative: an increase in D_s has no effect on their optimal π^*, which remains D. In this first case, there are no cross-effects on the better-paid partners.

On the other hand, if we focus on partners whose optimal remote work time is unconstrained (i.e., $\pi^* < D$), an increase in D_s leads to a modification of their optimal level of WFH π^*. The sign and magnitude of the shift depend on the shape of $f(\pi, \pi_s)$. In the case where this function can be written simply in the form $g(\pi - \pi_s)$ with g increasing and convex function, it is not difficult to show that $\frac{\partial \pi^*}{\partial D_s} = 1$.[5] In this case, the cross effect on the remote work time of the better-paid partners is of the same order of magnitude as the direct effect on their spouses.

So far we have focused on the cross effects on WFH likely to be observed following a re-

[5] If, for example, higher-paid partners (typically male partners) only start increasing their contribution to housework and childcare when they are alone at home when working remotely (which necessarily happens when $(\pi > \pi_s)$) then there are positive parameters α and β such that the function f can be approximated by $\alpha + \beta(\pi - \pi_s)1(\pi - \pi_s > 0)$, that is, by a convex increasing function of $(\pi > \pi_s)$.

laxation of constraints limiting the WFH possibilities of the less-paid spouses. The cross effects likely to be observed following a relaxation of constraints limiting the WFH possibilities of the better-paid spouses are a priori much more limited, since their partners work from home as much as possible anyway.

To sum up, we expect that an increase in WFH opportunities in some companies will primarily have an effect on the WFH choices of the less-paid spouses directly affected, but also a significant cross-effect on the choices of some of their better-paid spouses. As we shall see later, the available data are consistent with these predictions.

3.5. *Cross Effects on Hours Worked*

As we have just shown, an increase ΔD_s in WFH possibilities for less-paid spouses can have significant cross-effects on their partners' WFH decisions. We will now explore whether such a shock might not also have significant cross-effects on the number of hours worked by their partners.

If we first focus on those of these partners whose optimal WFH is constrained (i.e., $\pi^*=D$), an increase in D_s does not change their optimal choice of WFH, but potentially reduces their contribution to housework and childcare, with the consequence of increasing the time available for leisure and paid work. To the extent that consumption and leisure are normal goods, we can therefore detect a significant cross effect on their number of paid hours h.

If we now focus on partners whose optimal WFH is unconstrained (i.e., $\pi^* < D$), an exogenous increase in D_s can lead to a significant increase in their optimal choice of WFH π^*, without necessarily being accompanied by an increase in participation in domestic work or childcare. Here again, we can finally detect a significant cross effect on the number of paid hours h, even if, this time, it is the consequence of the cross effect on WFH (and the associated decrease in commuting time) rather than the consequence of a possible decrease in participation in domestic tasks.

Just as an exogenous increase in D_s can have a positive cross-effect on the number of paid hours h, an exogenous increase in D for higher-paid spouses can also have a positive cross-effect on h_s, the number of hours worked of the lower-paid partner, by inducing a decrease in housework for this partner. It should be noted, however, that this type of cross-effects can only be observed in the case where the better-paid spouse works at home to the maximum of his or her possibilities ($\pi^* = D$), that is, in the case where his or her contribution to domestic tasks is not very sensitive to his or her work at home. The cross-effects on the less-paid partner are therefore by construction likely to be of small magnitude.

Ultimately, even though it is highly stylized and only takes one modeling path among many others, our conceptual framework allows us to understand some of the reasons why an exogenous shock on the possibilities of working remotely can have effects on the spouses of the workers concerned, even if these spouses are not themselves directly affected. Beyond that, this conceptual framework also allows us to understand why the direct effects of such a shock are likely to be greater on the spouses with the lowest salaries in the couple (typically women) and, conversely, the indirect effects are greater on the spouses with the highest salaries (typically men), particularly if a significant portion of the home tasks are likely to fall to them when they are alone working from home.

4. Data and variables

We use the French Labor Force Survey (LFS) conducted each year by the French statistical office between 2013 and 2023. For each household member aged 15 or above, the LFS provides information on gender, marital status, employment status, detailed occupation, firm size, seniority, education, industry, employer's identification number, monthly earnings, and usual number of hours worked per week.[6] The survey also provides information on the proportion of their working time that respondents spent at home during the 4 weeks preceding the interview (0%, more than 0% but less than 50%, between 50% (included) and 100% (excluded), 100%). Between 2013 and 2020, this information (as well as the information on monthly wage) is collected for one third of the sample. From 2021, this information is collected for one sixth of the sample.

In addition to the LFS data, we also used the administrative database on collective agreements (so called D@ccord database). This database is operated by the Ministry of Labor and lists all agreements between employers and employee representatives. The database covers the period between 2013 and 2019. For each agreement, the register provides the date of the agreement, the identifiers of the employers concerned by the agreement as well as the topics covered by the agreement (and in particular if it relates to teleworking). Using establishment identifiers, we were able to match the LFS with this administrative database and to supplement the LFS with information on whether and when respondents' establishments had signed an agreement with workers' representative (and on whether this agreement covered teleworking). Prior to 2018, agreements on teleworking were very rare

[6]The legal length of the working week is 35 hours in France, but a majority of white-collar workers have a contract (called *forfait jour*) which stipulates only the number of days they must work per year (which must be below 218 days), and there is no limitation to their working hours.

and not listed as such in the database. They only began to be listed as such (rather than placed in the "other" category) from 2018 onwards.

Treatment and control groups

To identify the cross-effects of WFH, we consider establishments that signed a collective agreement with workers' representatives during the period 2018-2019 following the 2017 law and preceding the 2020 epidemic shock (whether or not this agreement covered WFH). We focus on individuals who work in these establishments and who are married (or cohabit) with individuals who also work in these establishments. We also focus on opposite-sex couples and exclude observations collected during the lock-down periods decided when the first waves of the Covid-19 epidemic hit the country between March 2020 and May 2021.[7] All in all, our main working sample comprises about 36,000 observations. Individuals working for an establishment who signed an agreement on WFH will be considered part of the treatment group while those working for an establishment who signed agreements on other subjects only will form our control group. In 13% of cases, the individual and their spouse are both in the treatment group, while in 55% of cases the individual and their spouse are both in the control group, and in 32% of cases, one of the two is in the treatment group and the other in the control group. Figure D1 in the Online Appendix shows that these proportions remained very stable throughout the period studied, with no perceptible change at the time of the pandemic shock.

Using this sample and treatment definition, our first main objective will be to verify that the epidemic shock induced a larger increase in the probability of the spouse working from home for individuals whose spouse is in the treatment group, whereas no significant difference existed in the years preceding the shock. Once this fact is established, our central research question will then be to identify the consequences that this may have had for the individuals themselves (independently of whether they belong to the treatment group or the control group).

Table D1 in the online appendix provides a set of statistics comparing the characteristics of employees in our work sample with the average characteristics of married (or cohabiting) employees in the private sector. The table shows that the employees in our sample are close to the average in terms of age and gender. However, they appear to be better edu-

[7] There were three periods of national lockdown in France, the first between March 7 and May 11, 2020, the second between October 30 and December 5, 2020, and the last between April 3 and May 3, 2021, or about 4 months in total. Working from home was only mandatory (for those who could) during these specific periods.

cated and more frequently employed in large firms, which is in line with the fact that we focus on firms where agreements are concluded with employee representatives. The table also shows that there are no major differences in age, gender, education, or employer size between employees whose spouse is in the treatment group and those whose spouse is in the control group. As in any difference-in-differences design, the key point will however be to show (as we will do in the following sections) that the differences between these two groups of employees have not changed at the time of the epidemic shock.

By construction, individuals in our work sample belong to establishments that signed collective agreements in 2018-2019. Table D2 in the online appendix presents the themes of these collective agreements. It also shows that these themes are similar for individuals whose spouse is in the treatment group and individuals whose spouse is in the control group, regardless of individuals own treatment status.

Remotable vs non-remotable occupations

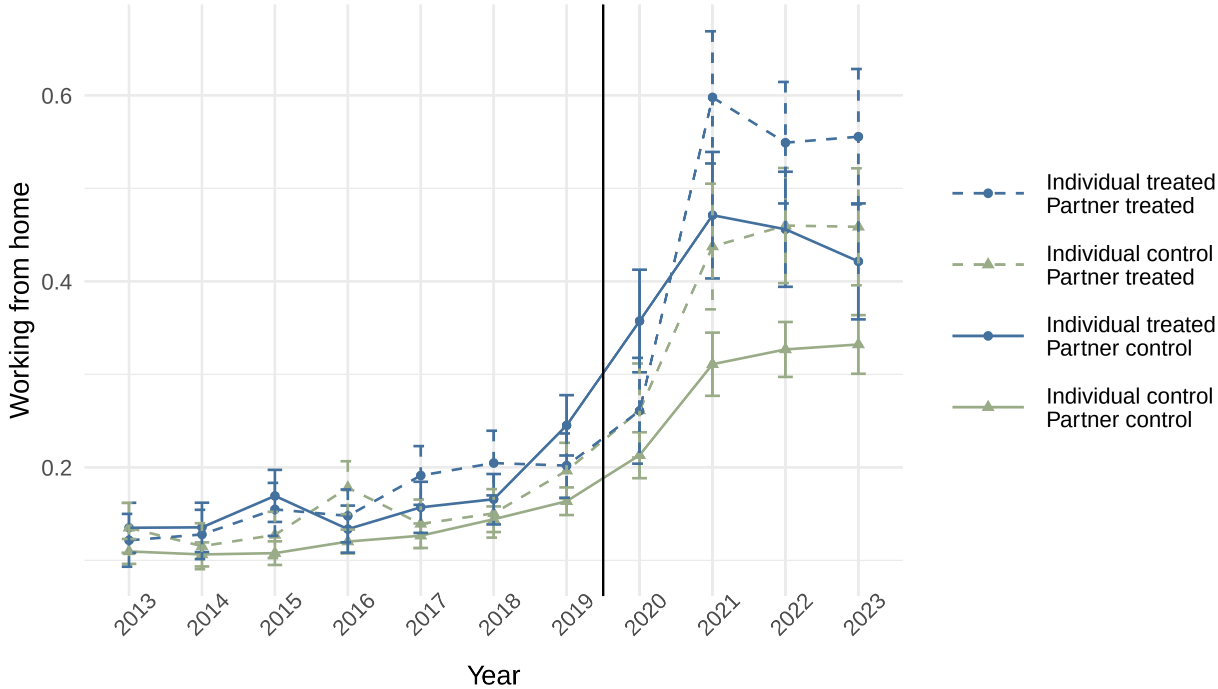
Before moving on to the empirical analysis, it should be emphasized that available information on the occupation of respondents makes it possible to identify those who have an occupation that is very difficult to carry out remotely (i.e., manual workers, transport workers, sale assistants, nursery or care assistants, etc.). Specifically, following Goux and Maurin (2025), we used the French (2-digit) occupational classification to define non-remotable occupations as those for which the WFH rate remained below 10% during the three periods of confinement, when the official health protocol required all those who could to work at home. Other occupations will be considered remotable.[8] Using this definition, part of our analyses will be conducted by distinguishing between couples whose spouses have remotable occupations and couples where at least one member has a non-remotable occupation. The first group represents about 51% of respondents in our sample and the second group about 49%. In the following, the effects of exposure to WFH on employees and their spouses will be mainly detected on the sample of couples with remotable occupations.

5. Cross effects on WFH: graphical analysis

Figure 3.1 shows the evolution of the probability of WFH in our sample, depending on whether or not they or their spouse are in the treatment group. More precisely, the figure shows the evolution of the proportion of employees working from home separately for

[8] The detailed list of non-remotable and remotable occupations is provided in Table D3 in Appendix.

Figure 3.1. Evolution of WFH by own and partner treatment status



Note: This figure refers to our working sample of employees working in a private sector establishment that signed at least one collective agreement in 2018 or 2019 and who are married (or cohabiting) with an employee working in the same type of establishment. For each combination of own and spousal treatment status, the Figure displays the evolution of the share of employees who worked from home over the last four weeks. The green (light) lines correspond to individuals who belong to the control group, and the blue (dark) lines to those who belong to the treatment group. The full lines refer to individuals whose spouse is in the control group, and the dotted line to individuals whose spouse is in the treatment group. The bars represent 95% confidence intervals. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor.

the four groups defined by the treatment status of the employees themselves and the treatment status of their spouse. The proportion of WFH remains similar (and relatively low) for all 4 groups throughout the years preceding the epidemic shock, with a slight overall upward trend. The shock then induced a rapid increase, followed by stabilization at levels significantly higher than those preceding the shock. Above all, this increase is even more marked for employees in the treatment group, and for those whose spouse is in the treatment group. To be more specific, whether we consider employees whose spouse is in the control group or employees whose spouse is in the treatment group, a gap of about 10 percentage points widens after the 2020 epidemic shock between those in the treatment group and those in the control group. Similarly, whether we consider employees in the control group or employees in the treatment group, a gap again close to about 10 percentage points widens after the shock between those whose spouse is in the treatment group and those whose spouse is in the control group.

To better visualize these developments, Figure 3.2a shows the evolution of the differences in WFH between employees in the treatment group and those in the control group. The figure confirms that the difference remains stable and small in the period preceding the epidemic shock, before rising to over 12 percentage points in the years 2021-2023 following the shock, with 2020 (the year of the shock itself) at an intermediate level. Figure 3.2b further shows the evolution of the differences in WFH between employees whose spouse is in the treatment group and those whose spouse is in the control group. Again, the figure shows that the difference remains stable and small throughout the years preceding the shock, and even in the year of the shock itself, fluctuating around 3 percentage points, before growing to about 15 percentage points in the years 2021-2023 following the shock.

Figure D2 in the online appendix further confirms that a similar diagnosis is obtained whether we restrict our analysis to employees in the control group or those in the treatment group. Whatever the employee's own status, the status of his or her spouse appears to make a significant difference after the shock.

All in all, our different graphical results confirm that the epidemic shock catalyzed a particularly strong increase in WFH in the treatment group and suggest that this increase in turn induced a strong increase in WFH among the spouses of the individuals in the treatment group, even when these spouses were not themselves in a particularly pro WFH environment.

6. Cross Effects on Labor Outcomes: Regression Results

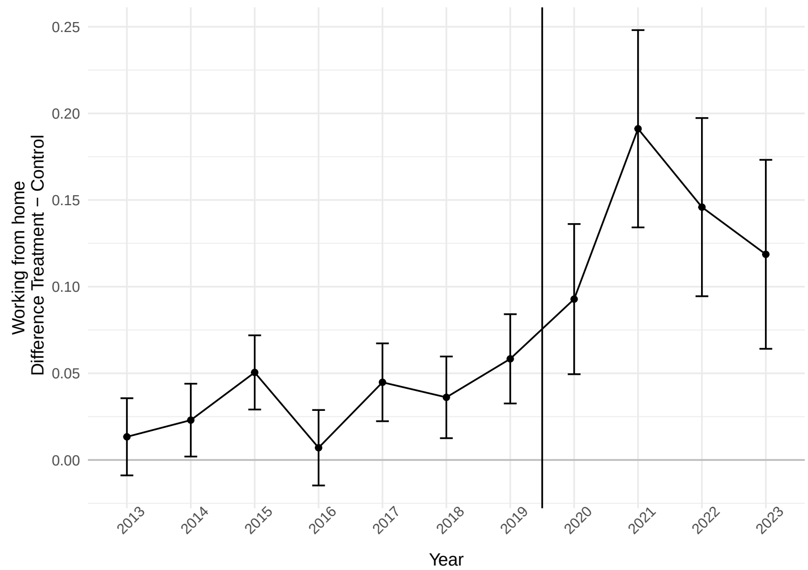
The graphical analysis in the previous section suggests that employees are more likely to work from home when their spouse also works from home, consistent with the idea that people value WFH more highly when their partner does too. In the following section, we test the robustness of this finding and we also ask whether spouses' WFH has an effect on the number of hours worked by their partners, or on their hourly wages. To be more specific, we consider the same LFS sample as that used for the graphical analysis and we estimate the following model,

$$Y_{i,t} = \underbrace{\alpha T_{i,t} + \beta T_{i,t} * Post_t}_{\text{Own treatment}} + \underbrace{\gamma T_{s(i),t} + \delta T_{s(i),t} * Post_t}_{\text{Partner's treatment}} + \underbrace{X_{i,t}\theta + X_{s(i),t}\psi}_{\text{Own \& Partner's char.}} + \underbrace{\mu_t}_{\text{Year FE}} + u_{i,t} \quad (3.7)$$

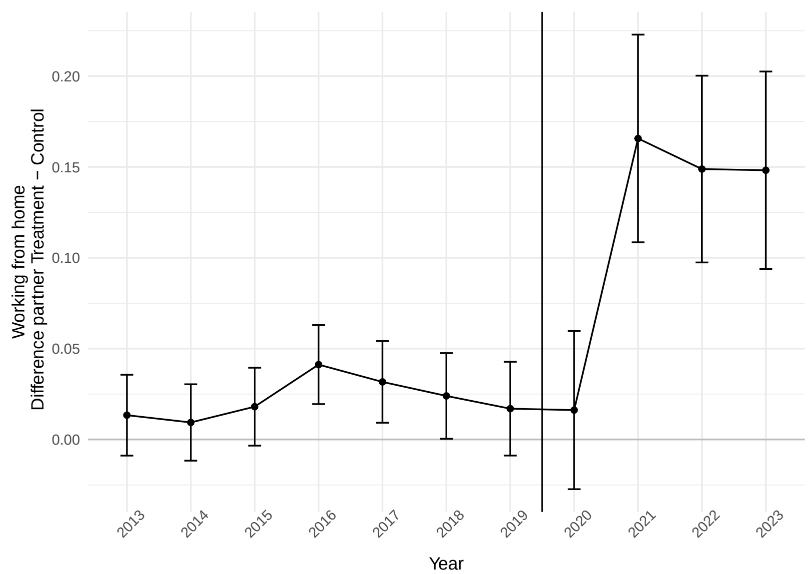
where $Y_{i,t}$ represents the outcome of individual i on year t while $T_{i,t}$ (resp. $T_{s(i),t}$) repre-

Figure 3.2. Evolution of Differences in WFH between Groups Defined by Own or Spouse's Treatment Status

(a) Differences in WFH between groups defined by own treatment status



(b) Differences in WFH between groups defined by spouse's treatment status



Note: This figure refers to the same sample as Figure 3.1. Panel (a) displays the difference in the evolution of the share of employees working from home in the last four weeks between those who belong to the treatment and control groups. Panel (b) displays the evolution of the same difference between employees whose spouse belongs to the treatment and control groups. The bars represent 95% confidence intervals. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor.

sents a dummy variable indicating that individual i (resp. the spouse of i) works in year t in an establishment that has signed a telework agreement in the two years following the 2017 law. The $Post_t$ variable is a dummy variable indicating that the observation year is 2020 or later while μ_t represents year fixed effects. Finally, $X_{i,t}$ (resp. $X_{s(i),t}$) represents a set of control variables including the gender, education and age of i (resp. the spouse of i) as well as the interactions of these control variables with $Post_t$. Standard errors are clustered at the household level.

The two main parameters of interest are β and δ. The β parameter captures the degree to which the epidemic shock induced a different evolution of labor outcomes between employees in the treatment group and those in the control group. For its part, the δ parameter captures cross-effects, i.e. the degree to which the epidemic shock induced a different evolution of labor outcomes between employees whose spouses were in the treatment group and those whose spouses were in the control group.

6.1. Workforce Composition

Before moving on to the analysis of hours worked and wages, we will use model (3.7) to compare the evolution before and after the pandemic shock of the characteristics of employees in the treatment group and the control group as well as the evolution of the characteristics of employees depending on whether their spouses are in the treatment group or in the control group. The aim is to assess the extent to which the pandemic shock induced differential changes in the composition of the groups defined either by the treatment status of employees or by the treatment status of their spouses. Such changes could for example be detected if the shock had led some employees in the treatment group to stay in their firm rather than leaving it, or had led some unemployed people to apply to firms in the treatment group rather than those in the control group.

To test for the existence of such post-pandemic shifts, Table 3.1 shows the main regression results when the dependent variable is, in turn, (a) an age variable, (b) a gender dummy, (c) a high-school graduation dummy, (d) a dummy indicating whether the employee has less than 4 years of seniority (i.e., was hired after the 2020 shock), (e) a dummy indicating that the employee holds a "remotable" occupation,[9] (e) a firm size dummy, (f) a set of industry dummies. For each of these dependent variables, the first column shows the estimated δ

[9]As mentioned above, the list of occupations considered as remotable is provided in Table D3 in Appendix. It includes all upper-level and mid-level occupations (one-digit items 4 and 3 of the French classification of occupations) to which we add lower-lever administrative occupations (two-digit item 54 of the French classification).

Table 3.1: Direct and Cross-Effects on Employee Characteristics

	Own Treatment × Post (1)	Partner Treatment × Post (2)	Mean
Age	-0.1816	-0.0639	42.99
	(0.2664)	(0.2665)	
Woman	0.0151	-0.0151	0.5
	(0.0180)	(0.0180)	
High school grad	0.0137	0.0055	0.652
	(0.0134)	(0.0137)	
Seniority under 4 years	-0.0042	-0.0159	0.230
	(0.0130)	(0.0134)	
Remotable occupation	0.0026	-0.0005	0.646
	(0.0140)	(0.0142)	
Firm size ≥ 50	0.0221	-0.0022	0.723
	(0.0140)	(0.0144)	
Agriculture	-0.0017	-0.0023	0.003
	(0.0018)	(0.0018)	
Manufacturing	-0.0179	0.0228*	0.250
	(0.0128)	(0.0136)	
Energy	-0.0088	0.0018	0.033
	(0.0076)	(0.0068)	
Construction	0.0009	-0.0054	0.040
	(0.0069)	(0.0064)	
Commerce	0.0086	-0.0040	0.150
	(0.0106)	(0.0114)	
Transport	-0.0183	-0.0182*	0.104
	(0.0124)	(0.0109)	
Hospitality	0.0071*	0.0002	0.017
	(0.0040)	(0.0040)	
Information, communication	-0.0072	-0.0051	0.045
	(0.0081)	(0.0077)	
Finance, Insurance	0.0199*	0.0157	0.081
	(0.0111)	(0.0101)	
Real Estate	0.0022	0.0031	0.012
	(0.0033)	(0.0034)	
Science and Tech	0.0172	-0.0104	0.129
	(0.0110)	(0.0114)	
Administration	-0.0078	-0.0014	0.121
	(0.00297)	(0.0108)	
Arts	0.0059	0.0033	0.015
	(0.0044)	(0.0045)	
Observations	36,192		

Note: The table refers to our working sample of employees working in a private sector establishment that signed at least one collective agreement in 2018 or 2019 and who are married (or cohabiting) with an employee working in the same type of establishment. Each row corresponds to a specific dependent variable. Column (1) reports the regression coefficient corresponding to the direct treatment variable in model (3.7), meaning the interaction of own treatment status and post. Column (2) reports the coefficient corresponding to the cross treatment variable: the interaction of one's partner's treatment status and post. Column (3) reports the mean of the dependent variable. The dependent variable is, in turn, a continuous variable for age (row 1), a gender dummy (row 2), a dummy indicating high-school graduation (row 3), a dummy indicating a seniority within the firm below four years (row 4), a dummy indicating that the respondent works in a remotable occupation (row 5), a series of dummies for the size of the firm the respondent works in (rows 6, 7, 8), and a series of dummies for the industry the respondent works in (rows 9 and below). Standard errors (in parentheses) are clustered at the household level. Regressions include control for year dummies. Due to missing values, there are only 35,178 observations in the firm size regressions. ***: p<0.01, **: p<0.05, *: p<0.1. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor.

parameter (i.e., the direct effect) while the second column shows the estimated β parameter (i.e., the cross effect).

Whatever the dependent variable considered, the table shows that the two estimated parameters are small and almost never statistically significant at standard levels, in line with the idea that the pandemic shock induced only little differential changes in the composition of the treatment group or in the composition of the group of employees whose spouse is in the treatment group.[10] In particular, no differential variations in age, gender, level of education, type of occupation or firm size were detected between groups defined by the treatment status of individuals or defined by the treatment status of their spouses. The absence of differential change in the share of employees with 4 or more years of seniority suggests that the rise of WFH in the treatment group did not particularly encourage existing employees (or their spouses) to leave (or stay with) their employer. Also, the absence of differential change in the proportion of remotable jobs confirms that the pandemic shock did not coincide with a change in the structure of occupations that would have been more particularly favorable to the development of WFH in the treatment group or among the partners of individuals in the treatement group.

All in all, we have a set of results in line with idea that the characteristics of employees in the treatment group (and the characteristics of their spouses) did not evolve differently than those of employees in the control group (and those of their spouses) after the pandemic shock. If in the next section we detect a differential evolution in the number of hours worked or in wages, these evolutions can be interpreted without too much ambiguity as a consequence of the rise in WFH among employees in the treatment group and/or among their spouses.

6.2. *Cross Effects on Hours Worked and Earnings*

Table 3.2 shows the main regression results when the dependent variable is in turn (a) a variable indicating that the employee has spent at least part of his/her working time at home during the last 4 weeks, (b) a variable indicating that the employee has spent 50% or more of his/her working time at home in the last 4 weeks, (c) the number of hours usually worked per week, (d) a dummy variable indicating that the employee usually works 40 hours or more per week (long work week), (e) the (log of) hourly wage. Panel A shows the results obtained on the full sample while panel B shows the results obtained on the sample where both spouses have a remotable occupation and panel C on the sample where one

[10]Specifically, among the 2x20=40 estimated parameters, none is significant at the 5% level.

Table 3.2: Direct and Cross-Effects on Labor Outcomes

	WFH (1)	WFH $\geq 50\%$ (2)	Usual weekly hours (3)	Hours ≥ 40 (4)	Log hourly wage (5)
Panel A: Full sample					
Own WFH agreement × Post	0.0596***	0.0514***	0.1280	0.0286*	-0.0128
	(0.0137)	(0.0102)	(0.2210)	(0.0149)	(0.0109)
Partner WFH agreement × Post	0.0498***	0.0175*	0.4575**	0.0247*	0.0058
	(0.0134)	(0.0099)	(0.2225)	(0.0147)	(0.0108)
Dependent variable mean	0.178	0.037	37.8	0.322	2.53
Observations	36,192	36,192	36,192	36,192	36,192
Panel B: Remotable sample					
Own WFH agreement × Post	0.0756***	0.0727***	0.4166	0.0401**	-0.0135
	(0.0191)	(0.0152)	(0.2992)	(0.0204)	(0.0140)
Partner WFH agreement × Post	0.0525***	0.0200	0.7844***	0.0406**	0.0100
	(0.0190)	(0.0149)	(0.2977)	(0.0202)	(0.0140)
Dependent variable mean	0.296	0.061	39.8	0.462	2.71
Observations	18,376	18,376	18,376	18,376	18,376
Panel C: Non-remotable sample					
Own WFH agreement × Post	0.0091	0.0048	-0.3480	-0.0006	-0.0073
	(0.0163)	(0.0096)	(0.3067)	(0.0194)	(0.0163)
Partner WFH agreement × Post	0.0186	0.0018	-0.0693	-0.0101	-0.0048
	(0.0156)	(0.0091)	(0.3208)	(0.0192)	(0.0154)
Dependent variable mean	0.056	0.013	35.8	0.179	2.35
Observations	17,816	17,816	17,816	17,816	17,816

Note: Panel A refers to the same sample as Table 3.1. Panel B focuses on the subsample where both spouses have a remotable occupation. Panel C focuses on the subsample where at least one spouse has a non-remotable occupation. Each column corresponds to a specific dependent variable. In each panel, the first row refers to the regression coefficient for the direct treatment variable in model (3.7) (i.e., the interaction between dummies indicating own treatment status and the post-pandemic period) while the second row refers to the coefficient corresponding to the cross-treatment variable (i.e., the interaction between dummies indicating spouse's treatment status and the post-pandemic period). The dependent variable is in turn (1) a dummy variable indicating that the respondent worked at home in the previous 4 weeks, (2) a dummy variable indicating that s/he worked at home at least 50% of the time in the previous 4 weeks, (3) a variable indicating the number of hours usually worked per week, (4) a dummy variable indicating that the respondent usually works 40 hours or more per week, (5) the log of hourly wage. Standard errors (in parentheses) are clustered at the household level. Regressions include controls for year (dummies) as well as age, education (high school or more) and gender of both members of the couple (and the interaction of these variables with a $Post$ dummy indicating the post-pandemic period). ***: p<0.01, **: p<0.05, *: p<0.1. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor of the two spouses has a non-remotable occupation.

If the post-pandemic increase in WFH among employees whose spouses are in the treatment group truly reflects spillover effects from their partners' increased remote work, we would expect this pattern to vary by occupation type. Specifically, the cross-effects on WFH should be strongest when both spouses can work remotely (Panel B), since both partners have the flexibility to make WFH decisions. Conversely, when one spouse has a non-remotable occupation (Panel C), we expect much weaker cross-effects on WFH, as at least one partner lacks the ability to increase remote work regardless of the pandemic shock.

To begin with, the results given in the first two columns of panel A confirm that the epidemic shock is followed by a significantly stronger rise in WFH for employees in the treatment group, but also (holding own treatment status constant) for employees whose spouses belong to the treatment group. The direct effect is estimated at around 6.0 percentage point, while the cross-effect is estimated at around 5.0 percentage point (a 28% increase). Almost all of the direct effect and a third of the cross effect correspond to an increase in arrangements where employees spend 50% or more of their working time at home. Assuming that the cross effect of 5.0 percentage points on employees whose spouses are in the treatment group can be interpreted as the consequence of the direct effect of 6.0 percentage points on their spouses, these results suggest that the transition to WFH of an employee's spouse leads on average to an increase of around 0.8 in the probability that the employee himself/herself will move to WFH (with $0.8 \approx 5.0/6.0$). These results are consistent with our previous graphical analysis and in line with the assumption that employees tend to be all the more inclined to work from home the more their spouses work from home.

The last columns of Panel A further show that the epidemic shock is not followed by any differential change in hourly wages. However, the shock appears to be followed by a differential increase in the number of hours worked by employees whose spouse belongs to the treatment group. We detect an increase of about 0.46 in the number of hours usually worked per week and an increase of about 2.5 percentage points in the probability of working 40 hours or more per week. Assuming again that these cross-effects can be interpreted as a consequence of the direct effect on spouses' WFH, these results suggest that the transition to WFH of an employee's spouse leads on average to an increase of about 20% in the number of hours of the employee himself/herself (with $0.2 \approx 0.46/(0.06 \times 37.8)$). Remarkably, these cross-effects on hours worked by individuals whose spouse is in the treatment group tend to be much larger than the direct effects on hours worked by individuals in the treatment group, although the difference between the two estimated effects is not statistically significant. This is suggestive that the time freed up for work by an increase in remote work is larger when this increase in remote work responds to an increase in the spouse's remote work, in line with the idea that an increase in the spouse's remote work protects the individual from an excessive increase in participation in domestic tasks if they also choose to work from home.

Panel B shows the results obtained by replicating the analysis of Panel A on the subsample where both spouses are in remotable occupations. The first two columns confirm that both direct and cross effects on WFH are stronger with this subsample than with the full sample. Specifically, the direct effect on WFH is estimated at about 7.6 percentage point, while the cross-effect is estimated at about 5.2 percentage point (a 18% increase).

The last columns of Panel B further show that the cross-effect on the number of hours worked is also stronger in this sub-sample. In particular, when we focus on these predominantly mid-level and upper-level employees, we detect a 0.78 hours increase in the number of hours usually worked per week (and a 4.1 percentage point increase in the proportion of long work week) for those whose spouse is in the treatment group compared with those whose spouse is in the control group. Again, these cross effects on the number of hours worked by those whose spouse is in the treatment group tend to be even more significant than the direct effects on the number of hours worked by those in the treatment group.[11]

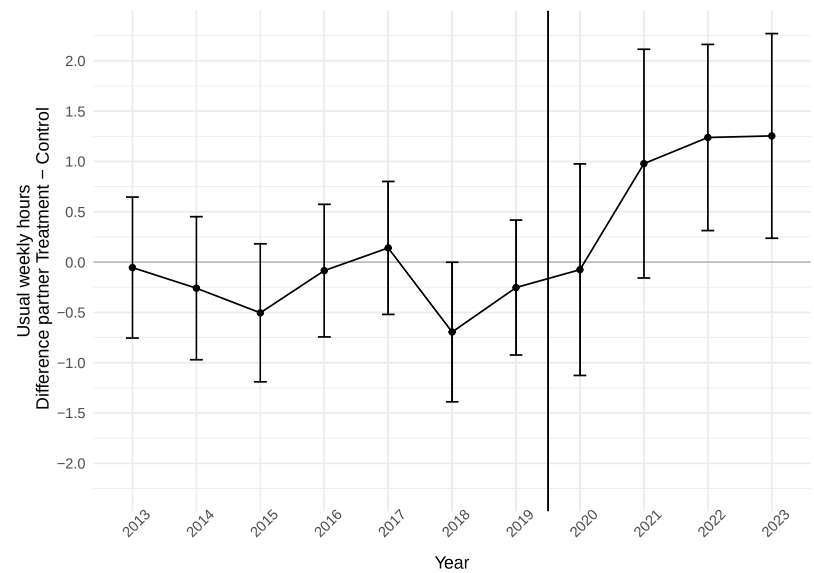
Finally, Panel C of Table 3.2 confirms that there is no cross effect on WFH when one of the spouses has a non-remotable occupation. Reassuringly, it is only when both spouses have a job in which WFH is possible that we observe a significant post-epidemic increase in WFH for those whose spouse is in the treatment group. It should also be noted that we do not observe any cross-effect on hours worked in this sample either, which suggests that individuals whose spouse is in the treatment group only increase their hours worked to the extent that they are able to first increase their rate of working from home and reduce their commuting time.[12]

[11] To go further, Figure D3 in the online appendix plots the estimated cross effects on the probability of usually working h hours or more in the week, for all possible values of h between 25 and 65 hours. Significant cross-effects are detected for h between 36 and 45 hours. They fluctuate between a little less than 4 percentage points (for h=36 hours or h=45 hours) and 5 percentage points (for h=39 hours). Simplifying a bit, these results suggest that the cross effects essentially consisted of a drop of about 4 percentage points in the probability of working between 35 and 40 hours a week, combined with a symmetrical increase in the probability of working 45 hours or more.

[12] The sample used in panel C can be broken down into three sub-samples, namely the one where individuals have a remotable occupation but not their spouses, the one where individuals have a non-remotable

To better visualize the cross-effect on hours worked by spouses with remotable occupations, Figure 3.3 focuses on the same sample as Panel B and plot the yearly evolution of the difference in hours worked between the group of employees whose spouse is in the treatment group and the group whose spouse is in the control group. The figure shows that the difference in hours worked between the two groups is statistically non-significant (fluctuating around -0.25 hours) throughout the pre-shock period, with no clear trend, in line with the familiar assumption of parallel trends. The difference then increases to about one hour, significantly above its pre-pandemic average level. As with the cross-effect on WFH, the cross-effect on hours worked does not materialize immediately at the time of the pandemic shock, but in the years 2021-2023 following the shock, again in line with the idea that the cross-effect on hours worked is conditional on the cross-effect on WFH.

Figure 3.3. Evolution of Differences in Hours Worked between Groups Defined by Spouse's Treatment Status



Note: This figure refers to the remotable sample (i.e., same sample as Panel B of Table 3.2). It displays the difference in the evolution of number of hours usually worked per week between employees whose spouse belongs to the treatment and control groups. The bars represent 95% confidence intervals. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor.

To further test the robustness of the main results of Table 3.2 , Table D4 in the online appendix shows the results of replicating our regression analysis using the remotable sample and focusing in turn (a) on the subsample of employees with 4 years of seniority or more (i.e., who were already in their company at the time of the pandemic shock), (b) on

occupation and their spouses a remotable occupation and the one where neither spouse has a remotable occupation. We verified that no cross effects are observed on either WFH or paid hours for any of these three sub-samples.

the subsample obtained by removing the two epidemic years, i.e., the two years 2020-2021 which precede the generalization of vaccination in France, and (b) the subsample obtained by removing the two years 2020-2021 and focusing on employees with 4 years or more of seniority.

When we focus on employees with 4 years of seniority in the company or more, we obtain results very similar to those in Table 3.2. Our main results reflect changes in the choices and outcomes of employees who were already present in their company before the pandemic shock. When we remove the years 2020-2021 and focus on the years following the return to post-epidemic normality, estimated cross effects on the probability of WFH become slightly stronger (+7.2 percentage point), but now only concern the probability of working from home less than half the time. Cross effects on the number of hours worked also become more significant than with the full sample (+1.2 hours), in line with Figure 3.3.The results remain similar when we remove the years 2020-2021 and focus on employees with 4 or more years' seniority.

6.3. *Heterogeneous Effects*

The regression results obtained so far suggest that by working more from home, many employees not only induce their spouses to work more from home but also enable them to work longer hours. To better interpret these results we will now explore whether they are equally valid for the better and the less-paid spouses within couples, one of our main working hypothesis being that responses to increased remote working opportunities are likely to be very different depending on the relative pay level of the spouse concerned, as discussed above. This subgroup analysis is reported in Panel A of Table 3.3, focusing on couples in which both partners work in a remotable occupation.

Consistent with our conceptual framework, the table first confirms a major difference between the effects observed for the better-paid spouses and those observed for the less-paid spouses. For the latter, we observe very significant direct effects on those in the treatment group, but almost no cross-effects on those whose (better-paid) spouse is in the treatment group. This result is consistent with the idea that less-paid spouses often tend to work from home to the maximum extent possible in their companies, which makes them particularly responsive to shocks affecting WFH opportunities in their companies, but much less so to shocks affecting WFH opportunities in their spouses' companies.

Table 3.3: Direct and Cross Effects on Labor Outcomes by Gender and Relative Pay

	WFH	WFH $\geq$50%	Usual weekly hours	WFH	WFH $\geq$50%	Usual weekly hours
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Spouse's Relative Wage	**Better-paid spouse**			**Less-paid spouse**		
Own WFH agreement × Post	0.0597**	0.0449**	-0.0480	0.0992***	0.1036***	0.8326**
	(0.0273)	(0.0213)	(0.4364)	(0.0274)	(0.0224)	(0.4162)
Partner WFH agreement × Post	0.0833***	0.0379*	1.417***	0.0173	0.0035	0.0745
	(0.0277)	(0.0215)	(0.4466)	(0.0268)	(0.0213)	(0.4027)
Dependent variable mean	0.335	0.063	40.6	0.258	0.058	38.96
Observations	9,062	9,062	9,062	9,062	9,062	9,062
Panel B: By Gender	**Men**			**Women**		
Own WFH agreement × Post	0.0517*	0.0538***	0.1516	0.0975***	0.0898***	0.7026*
	(0.0275)	(0.0207)	(0.4313)	(0.0270)	(0.0227)	(0.4212)
Partner WFH agreement × Post	0.0719***	0.0221	0.9787**	0.0347	0.0195	0.6057
	(0.0278)	(0.0205)	(0.4363)	(0.0265)	(0.0220)	(0.4141)
Dependent variable mean	0.327	0.057	41.7	0.265	0.065	37.8
Observations	9,188	9,188	9,188	9,188	9,188	9,188

Note: This table refers to the remotable sample (same as Panel B of Table 3.2). Columns 1 to 3 of Panel A correspond to the better-paid spouse (with the highest hourly wage in the couple), and columns 4 to 6 to the less-paid spouse. Couples in which both partners earn the same hourly wage are excluded. Columns 1 to 3 of Panel B correspond to men, and columns 4 to 6 to women. In each panel, the first row refers to the regression coefficient for the the interaction of own treatment status and $Post$ in model (3.7) while the second row refers to the coefficient corresponding to the interaction of spouse's treatment and $Post$. The dependent variable is in turn (1) a dummy variable indicating that the respondent worked at home in the previous 4 weeks, (2) a dummy variable indicating that the respondent worked at home at least 50% of their time in the previous 4 weeks, (3) a variable indicating the number of hours usually worked per week Standard errors (in parentheses) are clustered at the household level. Regressions include controls for year (dummies) as well as gender (in panel A only), age, and education (high school or more) of both members of the couple (and their interaction with $Post$). ***: p<0.01, **: p<0.05, *: p<0.1. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor

For better-paid spouses, the situation is the opposite. We observe, in particular, cross-effects on the number of hours worked by those whose spouse is in the treatment group that are much more significant than the direct effects on the number of hours worked by those in the treatment group. These results are consistent with the idea that better-paid spouses do not necessarily work from home to the maximum extent possible within their companies, preferring instead to adjust their working hours at home to those of their spouse. When their (less-paid) spouse benefits from new WFH opportunities, they respond by increasing their WFH time in parallel, reducing their commuting time without increasing their contribution to domestic tasks, which ultimately allows them to free up time to significantly increase their working hours.[13]

When we repeat the analysis by gender in Panel B, we find results consistent with men being on average the better-paid individual of their couple.[14] For men, the cross-effects on the number of hours worked for those whose partners are in the treatment group are, for example, much greater than the direct effects on the number of hours worked of those who are themselves in the treatment group. For women, the opposite is true; only the direct effects on those in the treatment group are statistically significant. The cross-effects for those whose partners are in the treatment group are weaker and statistically nonsignificant.

To go a little further, Table D5 in the appendix shows the regression results separately for the four subgroups defined by gender and relative salary within the couple. This analysis confirms that the cross-effects primarily concern better-paid spouses, even when these better-paid spouses are women, and even if these cross-effects are less well estimated for the sample of better-paid women due to its small sample size. In contrast, there are virtually no cross-effects on less-paid spouses, even when the less-paid spouse is a man. This analysis suggests that it is not gender per se that determines the asymmetry of responses to a shock on homeworking opportunities, but rather the relative position within the couple, with the direct effects of the shock primarily affecting less-paid spouses and its cross-effects primarily affecting better-paid spouses.

Finally, Table D7 in the appendix explores whether results differ in families with children and families without children. This analysis does not reveal any major differences between the two types of families, although the small size of the subsamples makes it difficult to

[13]As shown in Appendix Table D6, we find similar results when excluding from our sample the two pandemic years 2020-2021 as well as when focusing on employees with at least four years seniority in their firm. In both subsamples, the direct effects are significant only for the less-paid partners and the cross-effects only for the better-paid partners.

[14]Men have the higher hourly wage in 67% of couples in our full sample. In 1.6% of couples both partners have exactly the same hourly wage.

draw very precise conclusions. Whether or not there are children in the household, the cross-effects appear again primarily noticeable for better-paid spouses, while the direct effects are more significant for less-paid spouses.

6.4. Triple differences

The difference-in-differences model used so far assumes that the labor outcomes of employees whose spouse is in the treatment group would have evolved in the same way as that of employees whose spouse is in the control group, had there been no pandemic shock in 2020. In this section, we develop a triple-difference (DDD) approach, based on the assumption that the differences in labor outcomes between the group of individuals living in couples with remotable occupations and the group living in other couples would have evolved in the same way in the treated group and the control group, had there been no pandemic shock in 2020. To be more specific, Table 3.4 focuses on the same subsamples of better-paid and less-paid spouses as Table 3.3 and shows the results of regressing the main outcomes of interest on the three-way interactions between a post-pandemic dummy variable, an occupational group dummy (i.e., a dummy variable indicating whether both spouses have a remotable occupation) and dummies indicating either the treatment status of the respondent or the treatment status of his/her spouse, controlling for the same variables as in model (1) and for their interactions with the occupational group dummy. In this set-up, for each of the outcomes studied, the three-way interaction coefficients capture how the gap between the two occupational groups has evolved after the epidemic shock in the treatment group compared with the evolution in the control group, whether the treatment group is defined by the individual's treatment status or that of his/her spouse.

The table first confirms that the epidemic shock coincided with an increase in the WFH gap between occupational groups which is significantly stronger for respondents in the treatment group than for those in the control group. Consistent with the previous DD analysis, this specific change in the WFH gap is particularly marked for less-paid spouses.

The table further confirms that the shock also coincided with an increase in the gap in the number of hours worked between occupational groups which is significantly stronger for respondents whose spouses are in the treatment group than for those whose spouses are in the control group. Also, consistent with the DD analysis, this shift in the gap in the number of hours worked is most noticeable for the better-paid spouses.

Table 3.4: Direct and Cross Effects on Labour Outcomes: A Triple Difference Approach by Relative Pay

	WFH	WFH $\geq$ 50%	Usual weekly hours	WFH	WFH $\geq$ 50%	Usual weekly hours
	(1)	(2)	(3)	(4)	(5)	(6)
	Better-paid spouse			**Less-paid spouse**		
Own WFH agreement	0.0247	0.0013	-0.5424	-0.0053	0.0055	-0.1617
× Post	(0.0241)	(0.0137)	(0.4578)	(0.0206)	(0.0133)	(0.4426)
Partner WFH agreement	0.0290	-0.0060	0.0846	0.0122	0.0085	-0.1421
× Post	(0.0246)	(0.0138)	(0.4623)	(0.0186)	(0.0121)	(0.4779)
Own WFH agreement ×	0.0341	0.0446*	0.5103	0.1081***	0.0981***	0.9847
Both remotable × Post	(0.0364)	(0.0254)	(0.6330)	(0.0343)	(0.0261)	(0.6065)
Partner WFH agreement ×	0.0571	0.0439*	1.316**	0.0034	-0.0042	0.2292
Both remotable × Post	(0.0371)	(0.0257)	(0.6428)	(0.0326)	(0.0246)	(0.6253)
Dependent variable mean	0.206	0.039	38.57	0.151	0.035	37.0
Observations	17,806	17,806	17,806	17,806	17,806	17,806

Note: This table refers to our main working sample (same as Table 3.1). Columns 1 to 3 correspond to the better-paid spouse (with the highest hourly wage in the couple), and columns 4 to 6 to the less-paid spouse (couples earning equal hourly wage are excluded). These regressions correspond to the triple-difference model described in section 6.4. The first and second row report respectively the coefficients corresponding to own and partner's treatment variable interacted with $Post$. The third and fourth rows report the coefficient corresponding respectively to own and partner treatment status interacted with $Post$ and with a dummy indicating that both spouses have remotable occupations. The dependent variable is in turn (1) a dummy variable indicating that the respondent worked at home in the previous 4 weeks, (2) a dummy variable indicating that they worked at home at least 50% of their time, (3) a variable indicating the number of hours usually worked per week. Standard errors (in parentheses) are clustered at the household level. Regressions include controls for year (dummies) as well age, education (high school or more) and gender of both members of the couple (and their interactions with $Post$ and the occupational group dummy). ***: p<0.01, **: p<0.05, *: p<0.1. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor

Ultimately, the results of this triple-difference approach are entirely consistent with those of the double-difference approach, confirming that the direct effect of the pandemic shock was primarily on the probability of less-paid spouses working remotely, and resulted in a simultaneous rise in the WFH and the number of hours worked by their (better-paid) spouses.

7. Commuting Distance and Residential Choices

Working from home reduces the frequency of commuting, and the freed-up time can be used, as we have seen, to increase the number of hours worked. In theory, the reduced frequency of commuting also makes it possible to change residence, move further away from one's workplace, choose a place to live far from city centers, where housing prices are much lower and where it is possible to have much more spacious accommodation at a lower cost.[15] Moving away from expensive urban centers, however, comes at the cost of moving away from the best educational infrastructure as well as the best medical (and personal service) infrastructure, so it is not clear that the greater possibilities for working from home are enough to change the residential equilibrium and the distribution of households across the territory.[16]

To explore these issues, we replicated our main regression analysis using the distance to work as the dependent variable. We measure this as the euclidean distance between the exact establishment location and the centroid of the municipality of residence. This analysis is reported in Table 3.5. It reveals no differential effect of the pandemic shock on the distance to work of individuals in the treatment group or on the distance to work of individuals whose spouse is in the treatment group. This finding holds for both men and women, in families with children as well as in families without children. The data suggest that the specific increase in working from home did not coincide with any distance from the workplace, either for the treatment group or for the group whose spouses are in the treatment group.

To go further, we also regressed a set of three dependent variables characterizing households' residential choices on a set of dummy variables indicating (a) whether only the man is in the treatment group, (b) whether only the woman is in the treatment group,

[15] According to the French Statistical Office, in urban units with more than 700,000 inhabitants, house prices vary, for example, by a factor of two between the most central and the most peripheral areas (P. Bertrand, 2025). In urban units with more than 200,000 inhabitants, prices are 80% higher in the center than in the most peripheral areas.

[16] Regarding territorial inequalities in access to cultural or medical facilities in France, see for example Couleaud et al. (2021) or Legendre (2021).

Table 3.5: Direct and Cross Effects on Commuting Distance, by Gender and Family Type

	Distance between firm and residence municipality (km)		
	(1)	(2)	(3)
Panel A: Full Sample	**All**	**Men**	**Women**
Own WFH agreement × Post	-0.9250	0.0569	-1.825
	(2.418)	(3.810)	(2.845)
Partner WFH agreement × Post	0.0317	2.852	-2.971
	2.522	(3.982)	(3.024)
Dependent variable mean	26.0	30.3	21.7
Observations	35,648	17,795	17,853
Panel B: Remotable sample	**All**	**Men**	**Women**
Own WFH agreement × Post	-3.283	-1.676	-4.129
	(3.522)	(5.354)	(4.527)
Partner WFH agreement × Post	-0.4338	3.713	-5.401
	(3.730)	(5.677)	(4.725)
Dependent variable mean	32.2	37.4	26.9
Observations	18,119	9,057	9,062

Note: Panel A refers to the same sample as Table 3.1. Panel B focuses on the subsample where both spouses have a remotable occupation. Columns 1 to 3 refer to men and columns 4 to 6 to women. Columns 2 and 5 focus on the subsample without children, and columns 3 and 6 to the sample with children. In each panel, the first row refers to the regression coefficient for interaction of own treatment status and $Post$ in model (3.7) while the second row refers to the coefficient corresponding to the interaction between spouse's treatment status and $Post$. The dependent variable in all columns is the euclidean distance between individuals' exact firm location, and the centroid of their municipality of residence. Standard errors (in parentheses) are clustered at the household level. Regressions include controls for year (dummies) as well as age, education (high school or more) and gender of both members of the couple (and their interaction with $Post$). ***: p<0.01, **: p<0.05, *: p<0.1. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor

(c) whether both are in the treatment group, (d) whether neither is in the treatment group (taken as reference), as well as the interaction of these dummy variables with a variable indicating the post-pandemic shock period. The three dependent variables are dummy variables indicating whether the household resides (a) in the central city of an urban unit[17], (b) a suburban city of an urban unit, (c) a small isolated municipality (i.e., urban unit without suburbs) or in a rural area (i.e., outside urban units). Panel A of Table D8 in the appendix shows the regression results obtained with the full sample while Panel B refers to the sample of couples holding remotable occupations. These analyses reveal no clear differential effect of the pandemic shock on the residential choices of families of individuals in the

[17] A urban unit is defined as a city or group of cities with a continuous built-up area (no gap of more than 200 meters between two buildings) with at least 2,000 inhabitants.

treatment group. Whether one or both spouses were particularly affected by the increase in WFH, no change in the likelihood of residing in urban centers, suburbs, or rural areas was detected.

8. Conclusion

The rise of working from home reduces commuting time and considerably changes the way men and women spend their days and interact within couples. To shed light on these transformations, this article draws on the specificities of the French experience, a country where the 2020 epidemic shock was followed by a much larger increase in WFH in establishments where a collective agreement on remote work had been signed in the years preceding the shock. We show that the spouses of employees working in these establishments also started working more from home after the shock, regardless of whether there was a WFH agreement in their own establishment, suggesting very strong complementarity in the choice of WFH between spouses. When an employee switches to remote work, we find that the probability of his or her partner also working from home increases by about 0.8. Furthermore, by switching to WFH, partners save time on commuting, allowing them to considerably increase their number of hours worked.

Further investigations reveal that these cross-effects on WFH and number of hours worked are much more significant for men than for women while the opposite is true for the direct effects of the pandemic shock: they are much stronger for women than for men in the treatment group. Men are, in the vast majority of cases, those who have the best occupational positions in the couple, and the importance of cross-effects for men suggests that better-paid spouses are even more responsive to an increase in their spouse's remote work than to an increase in remote work opportunities granted by their companies. These findings are consistent with a model where couples coordinate so that the better-paid spouse only increases the number of days he or she works from home to the extent that his or her partner works more from home and can take on more domestic tasks and childcare.

Aside from time use, another potentially very important consequence of the rise in WFH could have been a change in where people live, away from urban centres. However, we find no evidence of any such change,i.e. no evidence that the adoption of work-from-home arrangements leads to residential relocation or changes in commuting distances. The rise of remote work opportunities has reduced the commuting costs associated with moving away from urban centers, but not enough to actually incentivize affected workers to move away from the educational, medical, and cultural amenities of urban centers. In the end, the time gains from remote work does not seem to be offset by longer commutes, which makes it possible to understand the very significant cross-effects on working time that we have been able to identify.

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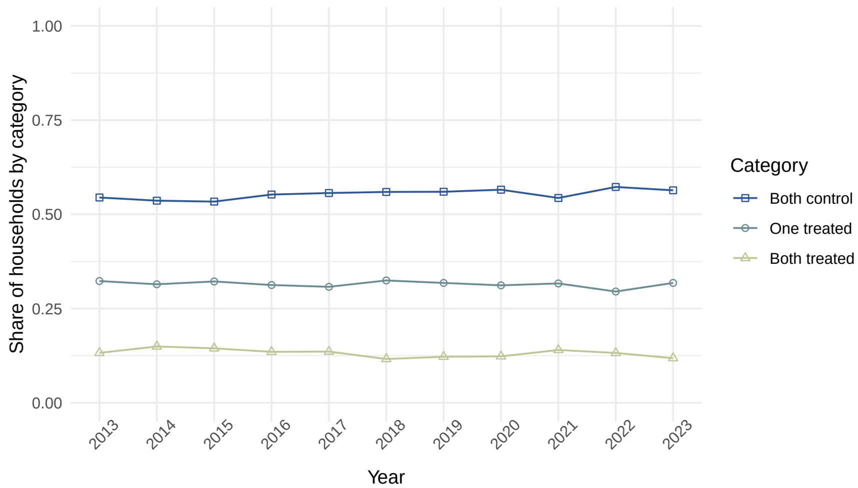
D. Appendix

D.1. Additional Figures and Tables

Figure D1. Share of Households by Treatment Category



Note: This figure refers to our main working sample (same as Figure 3.1). It displays the share of houseolds in each treatment category : both members working in treated firms; only one member working in a treated firm; no member working in a treated firm. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor.

Table D1: Sample Composition by Own and Partner Treatment Status

			Own: Control		Own: Treatment	
		Full	Partner		Partner	
	All	Sample	Control	Treatment	Control	Treatment
	(1)	(2)	(3)	(4)	(5)	(6)
Age	44.088	42.985	42.714	42.803	43.008	44.305
	(10.568)	(9.528)	(9.582)	(9.433)	(9.484)	(9.357)
Woman	0.462	0.5	0.5	0.531	0.469	0.5
	(0.499)	(0.5)	(0.5)	(0.499)	(0.499)	(0.5)
High school grad	0.553	0.651	0.61	0.676	0.707	0.73
	(0.497)	(0.477)	(0.488)	(0.468)	(0.455)	(0.444)
Seniority <4 yrs	0.31	0.23	0.246	0.273	0.203	0.145
	(0.462)	(0.421)	(0.43)	(0.445)	(0.403)	(0.352)
Remotable occupation	0.534	0.646	0.591	0.685	0.707	0.754
	(0.499)	(0.478)	(0.492)	(0.465)	(0.455)	(0.43)
Firm size above 50	0.492	0.723	0.712	0.692	0.742	0.785
	(0.5)	(0.447)	(0.453)	(0.462)	(0.438)	(0.411)
Agriculture	0.005	0.003	0.003	0.002	0.002	0.001
	(0.072)	(0.051)	(0.057)	(0.048)	(0.042)	(0.038)
Manufacturing	0.18	0.25	0.299	0.238	0.164	0.161
	(0.384)	(0.433)	(0.458)	(0.426)	(0.37)	(0.367)
Energy	0.022	0.033	0.02	0.019	0.058	0.075
	(0.146)	(0.179)	(0.139)	(0.136)	(0.234)	(0.263)
Construction	0.075	0.04	0.037	0.039	0.057	0.037
	(0.264)	(0.197)	(0.188)	(0.194)	(0.233)	(0.189)
Commerce	0.158	0.15	0.173	0.179	0.104	0.077
	(0.365)	(0.357)	(0.378)	(0.383)	(0.305)	(0.266)
Transport	0.074	0.104	0.06	0.055	0.193	0.243
	(0.262)	(0.306)	(0.237)	(0.228)	(0.395)	(0.429)
Hospitality	0.033	0.017	0.02	0.017	0.013	0.01
	(0.18)	(0.129)	(0.138)	(0.129)	(0.115)	(0.099)
Information Communication	0.036	0.045	0.035	0.042	0.065	0.07
	(0.186)	(0.208)	(0.184)	(0.2)	(0.246)	(0.255)
Finance, Insurance	0.047	0.081	0.048	0.073	0.148	0.15
	(0.212)	(0.273)	(0.213)	(0.26)	(0.355)	(0.357)
Real Estate	0.015	0.012	0.014	0.011	0.009	0.013
	(0.122)	(0.11)	(0.116)	(0.104)	(0.093)	(0.114)
Science and Technology	0.136	0.129	0.135	0.158	0.108	0.099
	(0.343)	(0.336)	(0.341)	(0.364)	(0.31)	(0.299)
Administration	0.153	0.121	0.143	0.152	0.068	0.052
	(0.36)	(0.326)	(0.35)	(0.359)	(0.251)	(0.222)
Arts	0.058	0.015	0.015	0.015	0.012	0.014
	(0.235)	(0.12)	(0.123)	(0.122)	(0.109)	(0.117)
Number of observations	233212	36192	19962	5718	5718	4794

Note: The table gives the mean value of variables indicating the gender, age, education, seniority, age, occupation, firm size or industry of individuals (one variable per row). Each column corresponds to a different sample: (1) full sample of married (or cohabiting) employees who are working in a private sector establishment, (2) main working sample (same as Table 3.1), (3) subsample where the individual and his/her spouse are both in the control group, (4) subsample where the individual is in the control group and his/her spouse is in the treatment group, (5) subsample where the individual is in the treatment group and his/her spouse is in the control group, (6) subsample where the individual and his/her spouse are both in the treatment group. Standard deviation in parenthesis. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor.

Table D2: Type of Agreement by Own and Partner Treatment Status

		Own: Control		Own: Treatment	
	Full	Partner		Partner	
	Sample	Control	Treatment	Control	Treatment
	(1)	(2)	(3)	(4)	(5)
Wage	0.177	0.236	0.229	0.044	0.033
	(0.382)	(0.424)	(0.421)	(0.204)	(0.177)
Working time	0.201	0.178	0.19	0.238	0.27
	(0.401)	(0.382)	(0.393)	(0.426)	(0.444)
Job Preservation	0.092	0.076	0.072	0.11	0.158
	(0.289)	(0.265)	(0.259)	(0.313)	(0.365)
Digital disconnection	0.03	0.017	0.016	0.063	0.064
	(0.172)	(0.131)	(0.127)	(0.242)	(0.244)
Working conditions	0.198	0.031	0.024	0.637	0.573
	(0.398)	(0.174)	(0.152)	(0.481)	(0.495)
Training	0.013	0.014	0.011	0.012	0.009
	(0.111)	(0.118)	(0.103)	(0.107)	(0.095)
Pension, Insurance	0.036	0.046	0.04	0.016	0.015
	(0.187)	(0.209)	(0.196)	(0.124)	(0.123)
Profit sharing	0.222	0.306	0.321	0.012	0.007
	(0.416)	(0.461)	(0.467)	(0.11)	(0.083)
Diversity	0.16	0.152	0.141	0.168	0.203
	(0.366)	(0.359)	(0.348)	(0.374)	(0.402)
Other	0.332	0.337	0.34	0.298	0.345
	(0.471)	(0.473)	(0.474)	(0.457)	(0.475)
Number of observations	36192	19962	5718	5718	4794

Note: This table refers to our main working sample (same as Table 3.1). Each column corresponds to a different subsample: (1) full sample, (2) the individual and his/her spouse are both in the control group, (3) the individual is in the control group and his/her spouse in the treatment group, (4) the individual is in the treatment group and his/her spouse in the control group, (5) the individual and his/her spouse are both in the treatment group. There are ten possible topics for collective agreements and each row corresponds to a specific topic. It provides the share of individual working in a firm which signed a collective agreement on the corresponding topic in 2018-2019, for each subsample. Standard deviation in parenthesis. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor.

Table D3: Remotable and Non-Remotable Occupations

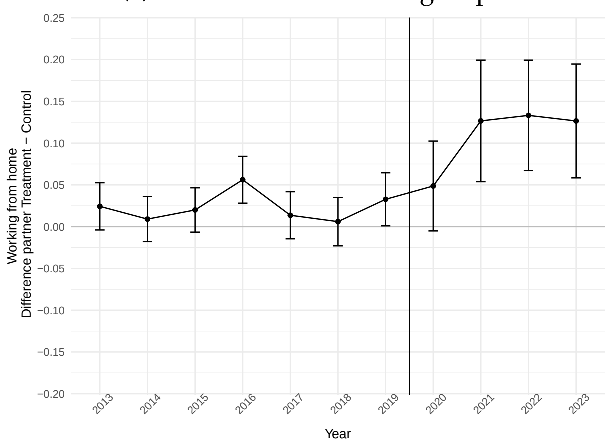
Panel A - Remotable occupations	Sample Share (1)	Lockdown WFH Share (2)
33 - Public administrative and technical executives	0.008	0.774
34 - Teachers and advanced scientific occupations	0.005	0.531
35 - Media and arts professionals	0.007	0.720
37 - Coporate managers and executives	0.117	0.776
38 - Technical managers and engineers	0.125	0.742
42 - Elementary, vocational, adult education and sports professionals	0.004	0.351
43 - Healthcare and social work practitioners	0.041	0.250
45 - Mid-level public administration	0.004	0.526
46 - Mid-level business administrators and sales professionals	0.125	0.513
47 - Technicians	0.076	0.297
48 - Foremen, intermediate level supervisors	0.042	0.184
54 - Corporate administrative staff	0.088	0.397

Panel B - Non-remotable occupations	Sample Share (1)	Lockdown WFH Share (2)
52 - Public administrative employees, health auxiliaries, care assistants	0.047	0.044
53 - Law enforcement, military, fire service, and security personnel	0.006	0.000
55 - Commercial employees, retail workers	0.058	0.100
56 - Personal service workers	0.019	0.061
62 - Skilled industrial workers	0.074	0.020
63 - Skilled craft/artisanal workers	0.026	0.019
64 - Transport vehicle drivers, delivery drivers, and couriers	0.029	0.030
65 - Heavy equipment operators, warehouse staff, and non-road transport workers	0.031	0.015
67 - Low-skilled industrial workers	0.048	0.010
68 - Low-skilled craft/artisanal workers	0.019	0.041

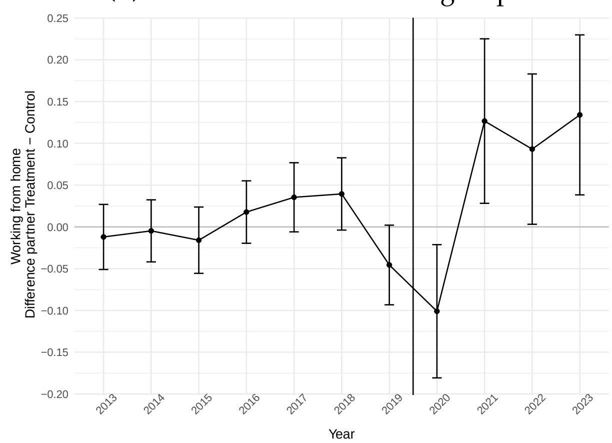
Note: This table shows how occupations (as defined by the 2-digit French classification) are distributed across remotable and non-remotable occupations. Panel A refers to occupations that we define as remotable, while Panel B refers to those that we define as non-remotable. The sample used for the first column corresponds to our main work sample. It shows the distribution of workers across occupations. The sample used for the second column 2 corresponds to the observations made during the lockdown periods. It shows for each occupation the share of employees in our work sample who worked from home during these particular periods. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor.

Figure D2. Difference in WFH Between Groups Defined by Spouse's Treatment Status

(a) Individuals in control group



(b) Individuals in treatment group



Note: This figure refers to our main working sample (same as Figure 3.1). It displays the difference in the share of employees who worked from home in the previous four weeks, between employees whose spouse belongs to the treatment and control groups. Panel (a) corresponds to individuals who are themselves part of the control group, while panel (b) corresponds to individuals part of the treatment group. The bars represent 95% confidence intervals. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor.

Table D4: Direct and Cross Effects on Labor Outcomes: a Subsample Analysis

	WFH	WFH $\geq$ 50%	Usual weekly hours	Hours $\geq$ 40	Log hourly wage
	(1)	(2)	(3)	(4)	(5)
Panel A: subsample with at least 4 years of seniority					
Own WFH agreement × Post	0.0616***	0.0585***	0.3968	0.0422*	-0.0097
	(0.0217)	(0.0173)	(0.3450)	(0.0228)	(0.0158)
Partner WFH agreement × Post	0.574***	0.0199	0.7392**	0.0229	0.0155
	(0.0217)	(0.0172)	(0.3420)	(0.0228)	(0.0159)
Dependent variable mean	0.291	0.060	39.6	0.454	2.743
Observations	14,458	14,458	14,458	14,458	14,458
Panel B: subsample excluding years 2020-2021					
Own WFH agreement × Post	0.0525**	0.0723***	0.2137	0.0429	-0.0271
	(0.0255)	(0.0209)	(0.3892)	(0.0271)	(0.0190)
Partner WFH agreement × Post	0.0724***	0.0003	1.213***	0.0665**	0.0116
	(0.0255)	(0.0205)	(0.3844)	(0.0269)	(0.0188)
Dependent variable mean	0.276	0.0433	39.7	0.459	2.71
Observations	16,658	16,658	16,658	16,658	16,658
Panel C: subsample with at least 4 years of seniority, excluding years 2020-2021					
Own WFH agreement × Post	0.0511*	0.0778***	0.2270	0.0474	-0.0181
	(0.0291)	(0.0237)	(0.4345)	(0.0296)	(0.0217)
Partner WFH agreement × Post	0.0914**	-0.0113	1.179***	0.0426	0.0138
	(0.0294)	(0.0235)	(0.4347)	(0.0297)	(0.0216)
Dependent variable mean	0.270	0.043	39.6	0.450	2.74
Observations	13,112	13,112	13,112	13,112	13,112

Note: This table refers to three sub-samples from the remotable sample. Panel A focuses on individuals with at least four years of seniority in their firm. Panel B excludes observations recorded in 2020 or 2021. Panel C combines both restrictions. Each column corresponds to a specific dependent variable. In each panel, the first row refers to the regression coefficient for the direct treatment variable in model 3.7 (the interaction of own treatment status and $Post$) while the second row refers to the coefficient corresponding to the cross-treatment variable (the interaction of spouse's treatment status and $Post$). The dependent variable is in turn (1) a dummy variable indicating that the respondent worked at home in the previous 4 weeks, (2) a dummy variable indicating that they worked at home at least 50% of their time, (3) a variable indicating the number of hours usually worked per week, (4) a dummy variable indicating that the respondent usually works 40 hours or more per week, (5) the log of hourly wage. Standard errors (in parentheses) are clustered at the household level. Regressions include controls for year (dummies) as well as age, education (high school or more) and gender of both members of the couple (and their interaction with $Post$). ***: p<0.01, **: p<0.05, *: p<0.1. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor

Table D5: Direct and Cross Effects on Labor Outcomes, by Gender of Better-Paid Spouse

	WFH	WFH $\geq$50%	Usual weekly hours	WFH	WFH $\geq$50%	Usual weekly hours
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: better-paid male spouse	**Men** (better paid)			**Women** (less paid)		
Own WFH agreement × Post	0.0475	0.03	-0.1456	0.1118***	0.1027***	1.021**
	(0.0336)	(0.0249)	(0.5222)	(0.0333)	(0.0276)	(0.4896)
Partner WFH agreement × Post	0.1007***	0.0251	1.452***	0.0239	-0.0037	0.1871
	(0.0342)	(0.0250)	(0.5301)	(0.0322)	(0.0263)	(0.4669)
Dependent variable mean	0.348	0.059	41.717	0.244	0.062	37.548
Observations	6,092	6,092	6,092	6,092	6,092	6,092
Panel B: better-paid female spouse	**Men** (less-paid)			**Women** (better-paid)		
Own WFH agreement × Post	0.0737	0.01120***	0.4753	0.0763*	0.0637	0.1275
	(0,0491)	(0,0386)	(0,7799)	(0,0464)	(0,0397)	(0,7990)
Partner WFH agreement × Post	0,0061	0,0165	-0,0299	0,0469	0,0676*	1,292
	(0,0486)	(0,0365)	(0,7774)	(0,0473)	(0,0408)	(0,8277)
Dependent variable mean	0,286	0,051	41,869	0,30875	0,07003	38,168
Observations	2,970	2,970	2,970	2,970	2,970	2,970

Note: This table refers to the remotable sample (same as Panel B of Table 3.2). Panel A refers to the subsample of couples where the man earns the highest hourly wage. Panel B refers to the subsample where the woman earns the highest hourly wage. Columns 1 to 3 correspond to men, and columns 4 to 6 to women. In each panel, the first row refers to the regression coefficient for the the interaction of own treatment status and $Post$ in model (3.7) while the second row refers to the coefficient corresponding to the interaction of spouse's treatment status and $Post$. The dependent variable is in turn (1) a dummy variable indicating that the respondent worked at home in the previous 4 weeks, (2) a dummy variable indicating that they worked at home at least 50% of their time, (3) a variable indicating the number of hours usually worked per week Standard errors (in parentheses) are clustered at the household level. Regressions include controls for year (dummies) as well as age, and education (high school or more) of both members of the couple (and their interaction with $Post$). ***: p<0.01, **: p<0.05, *: p<0.1. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor

Table D6: Direct and Cross Effects on Labor Outcomes: a Subsample Analysis, by Relative Pay

	WFH	WFH ≥50%	Usual weekly hours	WFH	WFH ≥50%	Usual weekly hours
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Subsample with at least 4 years of seniority						
	Better-paid spouse			**Less-paid spouse**		
Own WFH agreement × Post	0.0296	0.0243	-0.4837	0.0965***	0.0967***	1.253**
	(0.0304)	(0.0231)	(0.4897)	(0.0315)	(0.0265)	(0.4946)
Partner WFH agreement × Post	0.0902***	0.0355	1.799***	0.0181	0.0050	-0.4291
	(0.0309)	(0.0236)	(0.5008)	(0.0309)	(0.0257)	(0.4747)
Dependent variable mean	0.328	0.061	40.4	0.252	0.059	38.8
Observations	7,382	7,382	7,382	7,382	7,382	7,382
Panel B: Subsample excluding 2020-2021						
	Better-paid spouse			**Less-paid spouse**		
Own WFH agreement × Post	0.0312	0.0348	-0.3126	0.0936***	0.1102***	0.7485
	(0.0367)	(0.0284)	(0.5883)	(0.0363)	(0.0295)	(0.5287)
Partner WFH agreement × Post	0.1303***	0.0204	1.741***	0.0089	-0.0281	0.5744
	(0.0364)	(0.0280)	(0.5805)	(0.0368)	(0.0290)	(0.5205)
Dependent variable mean	0.31572	0.04542	40.515	0.23755	0.04042	38.972
Observations	8,213	8,213	8,213	8,213	8,213	8,213
Panel C: Subsample with at least 4 years of seniority, excluding years 2020-2021						
	Better-paid spouse			**Less-paid spouse**		
Own WFH agreement × Post	-0.0048	0.0088	-0.7963	0.1152***	0.1491***	1.204*
	(0.0406)	(0.0305)	(0.6467)	(0.0417)	(0.0351)	(0.6187)
Partner WFH agreement × Post	0.1594***	0.0278	2.336***	0.0124	-0.0591*	-0.0636
	(0.0404)	(0.0312)	(0.6480)	(0.0423)	(0.0345)	(0.6144)
Dependent variable mean	0.310	0.045	40.4	0.230	0.040	38.8
Observations	6,689	6,689	6,689	6,689	6,689	6,689

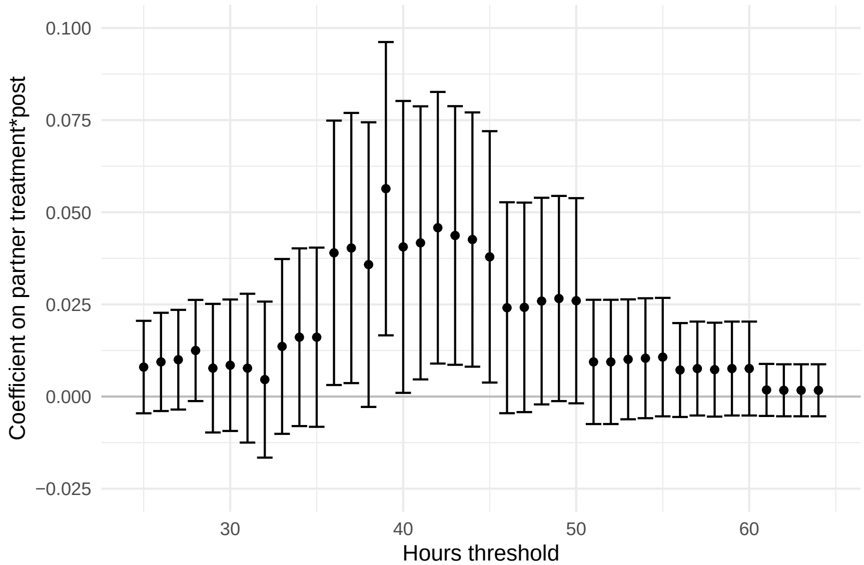
Note: This table refers to the remotable sample (same as Panel B of Table 3.2). Panel A focuses on individuals with at least four years of seniority in their firm. Panel B excludes observations recorded in 2020 or 2021. Panel C combines both restrictions. Columns 1 to 3 correspond to the better-paid spouse (with the highest hourly wage in the couple), and columns 4 to 6 to the lower paid spouse. Couples earning equal hourly wage are excluded. In each panel, the first row refers to the regression coefficient for the the interaction of own treatment status and $Post$ in model 3.7 while the second row refers to the coefficient corresponding to the interaction of spouse's treatment status and $Post$. The dependent variable is in turn (1) a dummy variable indicating that the respondent worked at home in the previous 4 weeks, (2) a dummy variable indicating that they worked at home at least 50% of their time, (3) a variable indicating the number of hours usually worked per week Standard errors (in parentheses) are clustered at the household level. Regressions include controls for year (dummies) as well as age and education (high school or more) of both members of the couple (and their interaction with $Post$). ***: p<0.01, **: p<0.05, *: p<0.1. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor

Table D7: Direct and Cross Effects on Labor Outcomes, by Relative Pay and Family Type

	WFH (1)	WFH ≥50% (2)	Usual weekly hours (3)	WFH (4)	WFH ≥50% (5)	Usual weekly hours (6)
Panel A: Without children	**Better-paid spouse**			**Less-paid spouse**		
Own WFH agreement × Post	0.1327***	0.0337	-1.127	0.1354***	0.0931**	0.5722
	(0.0453)	(0.0353)	(0.7817)	(0.0451)	(0.0368)	(0.7229)
Partner WFH agreement × Post	0.1153**	0.0136	1.198	0.0505	-0.0012	-0.5054
	(0.0460)	(0.0363)	(0.7867)	(0.0440)	(0.0353)	(0.6816)
Dependent variable mean	0.302	0.062	40.3	0.219	0.057	39.3
Observations	3,080	3,080	3,080	3,080	3,080	3,080
Panel B: With children	**Better-paid spouse**			**Less-paid spouse**		
Own WFH agreement × Post	0.0197	0.0516*	0.5016	0.0807**	0.1118***	0.9348*
	(0.0340)	(0.0267)	(0.5185)	(0.0345)	(0.0283)	(0.5061)
Partner WFH agreement × Post	0.0678**	0.0499*	1.557***	-0.0011	0.0063	0.4615
	(0.0344)	(0.0267)	(0.5387)	(0.0337)	(0.0267)	(0.4948)
Dependent variable mean	0.352	0.063	40.7	0.278	0.059	38.8
Observations	5,982	5,982	5,982	5,982	5,982	5,982

Note: This table refers to the remotable sample (same as Panel B of Table 3.2). Panel A focuses on individuals living in a household without children, and panel B on individuals living in a household with children. In each panel, the first row refers to the regression coefficient for the interaction of own treatment status and $Post$ in model (3.7) while the second row refers to the coefficient corresponding to the interaction of spouse's treatment status and $Post$. The dependent variable is in turn (1) a dummy variable indicating that the respondent worked at home in the previous 4 weeks, (2) a dummy variable indicating that they worked at home at least 50% of their time, (3) a variable indicating the number of hours usually worked per week. Standard errors (in parentheses) are clustered at the household level. Regressions include controls for year (dummies) as well as gender age and education (high school or more) of both members of the couple (and their interaction with $Post$). ***: p<0.01, **: p<0.05, *: p<0.1. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor

Figure D3. Cross-Effect on the Distribution of Hours Worked



Note: This figure refers to the sample where both spouses have a remotable occupation (same as Panel B of Table 3.2). The x-axis corresponds to a series of hours thresholds h. The dots display the estimated cross-effects for the regression in which the dependent variable is a dummy variable indicating that the number of usual hours worked per week is larger or equal to the threshold h. The bars represent 95% confidence intervals, computed from standard errors clustered a the household level. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor.

Table D8: Treatment Effects on Residential Choices

	Household lives in:		
	Suburbs	Urban center	Rural area or Small town
	(1)	(2)	(3)
Panel A: All			
Only the man is in group T × Post	0.0060	-0.0200	0.0140
	(0.0248)	(0.0210)	(0.0238)
Only the woman is in group T × Post	-0.0184	0.0192	-0.0008
	(0.0253)	(0.0216)	(0.0249)
Both spouses are in group T × Post	-0.0234	0.0213	0.0022
	(0.0280)	(0.0235)	(0.0265)
Dependent variable mean	0.401	0.225	0.375
Observations	61,183	61,183	61,183
Panel B: Remotable sample			
Only the man is in group T × Post	0.0473	-0.0487*	0.0014
	(0.0334)	(0.0274)	(0.0301)
Only the woman is in group T × Post	-0.0018	0.0176	-0.0158
	(0.0345)	(0.0294)	(0.0314)
Both spouses are in group T × Post	-0.0016	0.0077	-0.0061
	(0.0354)	(0.0297)	(0.0324)
Dependent variable mean	0.462	0.235	0.303
Observations	31,575	31,575	31,575

Note: Panel A of this table refers to our main working sample (same as Table 3.1). Panel B focuses on the subsample where both spouses have a remotable occupation. In each panel, the rows correspond to regression coefficients for the interaction of the $Post$ dummy variable with three dummy variables indicating that (1) only the male partner is in the treatment group, (2) only the female partner is in the treatment group, (3) both are in the treatment group. The dependent variable is in turn a dummy variable indicating that the household lives (1) in the suburbs, (2) in a city center, (3) in a rural area or a small isolated town. Standard errors (in parentheses) are clustered at the household level. The control variables include the four variables indicating whether both spouses belong to the control group, whether they belong to the treatment group, whether only the man belongs to the treatment group, or whether only the woman belongs to the treatment group. The controls also include year fixed effects as well as variables describing the gender, age, and education of both spouses (and the interaction of these demographic variables with $Post$). ***: p<0.01, **: p<0.05, *: p<0.1. Source: LFS, 2013-2023, INSEE, and D@ccord database, Ministry of Labor

D.2. Model details

With the notations of the main text, solving the couple's program reduces to maximizing over the six variables π, h, c, π_s, h_s and c_s the following objective function V:

$$\begin{aligned} V =& \mu U(T_0 - h - (1-\pi)m_0 - d_0 f(\pi,\pi_s), c) + \\ & (1-\mu)U_s(T_0 - h_s - (1-\pi_s)m_{0s} - d_0(1-f(\pi,\pi_s)), c_s) \\ \text{subject to} \quad & c + c_s = w_0 h + w_{0s} h_s \\ & \pi \leq D \\ & \pi_s \leq D_s. \end{aligned} \tag{3.8}$$

From the first order conditions for the variables c, c_s, h and h_s we get, for each of the two spouses, the usual equality between the real wage and the marginal rate of substitution of consumption for leisure, that is:

$$\frac{U_1}{U_2} = w_0 \quad \text{and} \quad \frac{U_{s1}}{U_{s2}} = w_{0s}. \tag{3.9}$$

These same first-order conditions also lead to similar equalities between the marginal rate of substitution of the individual's leisure to that of their spouse and their relative wage; as well as between the marginal rate of substitution of the individual's consumption to that of their spouse and the relative price of these consumptions (here assumed to be unity), that is,

$$\frac{\mu U_1}{(1-\mu)U_{s1}} = \frac{w_0}{w_{0s}} \quad \text{and} \quad \frac{\mu U_2'}{(1-\mu)U_{s2}} = 1. \tag{3.10}$$

Regarding now the decisions relative to π_s, we verify:

$$\frac{\partial V}{\partial \pi_s} = ((1 - \frac{w_0}{w_{0s}})d_0 f_2' + m_{0s})(1-\mu)U_{1s}'. \tag{3.11}$$

Since U_{1s}' is positive, f_2' negative and $w_0/w_0 s \geq 1$, this derivative is always positive. The lower-paid spouse of the two (with our conventions, assumed to be spouse s) always has an interest in setting π_s to its maximum value, that is $\pi_s^* = D_s$

Regarding finally the decisions relative to π, we find:

$$\frac{\partial V}{\partial \pi} = ((1 - \frac{w_0}{w_{0s}})f_1'(\pi, D_s)d_0 + m_{0s}\frac{w_0}{w_{0s}})(1-\mu)U_{s1}'. \tag{3.12}$$

In our set-up, the individual's choices regarding remote work will therefore depend crucially on the domain of variation of the function $f_1'(\pi, D_s)$. In what follows, we will organize the discussion according to whether this domain of variation is or is not sufficiently large for the optimal choice of π to be interior to the segment $(0, D)$ rather than constrained to be on the boundary of the segment.

Constrained case:

We begin with the case where $f_1'(\pi, D_s)$ remains small for all values of $\pi \in (0, D)$ (i.e., never exceeds the threshold $\frac{\frac{w_0}{w_{0s}}m_0}{(\frac{w_0}{w_{0s}} - 1)d_0)}$. In this case the partial derivative of V with respect to π remains positive and the better-paid spouse of the two always has an interest in working from home to the maximum of their possibilities, that is

$$\pi^* = D.$$

This situation corresponds to the case where the sharing of tasks between the two spouses varies little depending on which of the two works (or not) at home. In this case, the couple's program can be rewritten,

$$V = \mu U(\ell, c) + (1-\mu)U_s(\ell_s, c_s) \tag{3.13}$$

$$\text{subject to} \quad c + c_s + w_0\ell + w_{0s}\ell_s + w_0T_1 + w_{0s}T_{1s} = (w_0 + w_{0s})T_0, \tag{3.14}$$

where T_1 and T_{1s} represent the cumulative time spent on domestic tasks and commuting by each of the spouses. These quantities are fixed and depend only on D and D_s. More precisely, they are written,

$$T_1 = d + m = (1-D)m_0 + f(D, D_s)d_0 \tag{3.15}$$

$$\text{and} \quad T_{1s} = d_s + m_s = (1-D_s)m_{0s} + (1 - f(D, D_s))d_0. \tag{3.16}$$

With these notations, an exogenous increase in D_s affects the couple's decisions only insofar as it affects the quantity $(w_0T_1 + w_{0s}T_{1s})$, which can be interpreted as the income lost by the couple in commuting time or unpaid domestic activities.

Since $w_0T_1 + w_{0s}T_{1s} = w_{0s}(T_1 + T_{1s}) + (w_0 - w_{0s})T_1$, and both T_1 and $(T_1 + T_{1s})$ are decreasing with D_s, we deduce that $(w_0T_1 + w_{0s}T_{1s})$ is also decreasing with D_s. An exogenous increase in D_s thus resembles an exogenous increase in income. Insofar as leisure and consumption are normal goods, we can therefore expect this increase to result in an increase in leisure, consumption and paid work income. The increase in D_s has no cross-effect on the WFH of the better-paid spouse, but (by freeing up time for them otherwise devoted to domestic work) it allows them to work more.

Unconstraind case:

If $f_1'(\pi, D_s)$ is increasing and can exceed the threshold $\dfrac{\frac{w_0}{w_{0s}}m_0}{(\frac{w_0}{w_{0s}} - 1)d_0}$ then the better-paid spouse of the two no longer necessarily has an interest in working from home to the maximum of their possibilities. Their optimal behavior π^* is implicitly defined by the following equation,

$$f_1'(\pi^*, D_s) = \frac{\frac{w_0}{w_{0s}}m_0}{(\frac{w_0}{w_{0s}} - 1)d_0} \tag{3.17}$$

In this case, an exogenous increase in D_s has a cross-effect on the remote work decisions of the better-paid spouse. The sign and amplitude of this cross-effect depend on the form of the function f. By deriving the previous equation with respect to D_s we show,

$$\frac{\partial \pi*}{\partial D_s} = -\frac{f_{12}''}{f_{11}''} \tag{3.18}$$

Assuming that an increase in remote work is accompanied by an increase in domestic work that is all the weaker as the spouse already works at home, we can speculate that $f_{12}'' < 0$ and $f_{11}'' > 0$, with the consequence of a positive cross effect of an increase in D_s on the remote work rate of the better paid partner. This situation corresponds for example to the case where $f(x, y) = g(x - y)$ with g increasing and convex. In this latter case, the cross-effect is even very important, since we find that $\dfrac{\partial \pi*}{\partial D_s} = 1$.

Regarding now the cross effect of an increase in D_s on work time, it depends (as in the constrained case) on the effect that this increase has on the income lost by the couple in commuting time or unpaid domestic activities, that is (with the previous notations) on the quantity $(w_0T_1 + w_{0s}T_{1s})$. Now this effect is unambiguously negative. We verify in fact on one hand that

$$\frac{\partial T_1}{D_s} = f_2'd_0 + \frac{\partial \pi}{\partial D_s}\frac{m_0}{\frac{w_0}{w_{0s}} - 1} \tag{3.19}$$

and on the other hand that

$$\frac{\partial(T_1 + T_{1s})}{\partial D_s} = -m_{0s} - m_0\frac{\partial \pi}{\partial D_s} \tag{3.20}$$

which allows us to conclude that

$$\frac{\partial(w_0T_1 + w_{0s}T_{1s)}}{\partial D_s} = -w_{0s}m_{0s} + (w_0 - w_{0s})d_0f_2' < 0 \tag{3.21}$$

Again, an exogenous increase in D_s resembles a positive income shock. Under the maintained hypothesis that consumption and leisure are normal goods, we can expect a joint increase in leisure time, consumption and income from work, with therefore a priori once again a positive cross-effect on paid work time.

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Disparités Spatiales de Revenus et Choix de Localisation des Ménages

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Résumé en Français

"Nous n'atteindrons jamais l'égalité entre les femmes et les hommes tant que nous n'aurons pas aussi l'équité au sein des couples."

– Claudia Goldin, *The New York Times*[1] (2023)

"La folie du renouvellement urbain centré sur les bâtiments nous rappelle que les villes ne sont pas des structures ; les villes, ce sont les gens."

– Edward Glaeser, *Triumph of the City* (2011)

AU Au cours des dernières décennies, les femmes ont accompli des progrès remarquables dans la réduction des écarts de genre dans de multiples dimensions tant sur le plan de leur niveau d'éducation que de leurs résultats sur le marché du travail. L'évolution de leur participation à l'emploi illustre particulièrement bien cette dynamique : alors que le taux de participation des femmes en France n'était que de 55% en 1975, il a augmenté à 70% en 2021 (Raynaud, 2023). Cette transformation a, à son tour, augmenté la prévalence des couples à double revenus et l'importance des arbitrages intra-ménages dans l'explication de l'écart salarial persistant entre hommes et femmes.

Dans la plupart des économies, il demeure une différence significative entre les revenus des hommes et des femmes, qui ne peut plus être entièrement expliquée par les différences de capital humain (Bertrand, 2020). En France par exemple, les femmes ont doublé leur représentation dans les professions de cadres, passant de 21% à 42% entre 1982 et 2019 (Forment et al., 2020), et surpassent désormais les hommes en matière de niveau d'éducation, avec des taux d'obtention de diplômes universitaires plus élevés (Roussel, 2022). Pourtant, malgré cette convergence, les femmes travaillant dans le secteur privé français en 2021 gagnaient 24,4% de moins que les hommes en salaires annuels (Godet, 2023). Lorsque

[1]Smialek, 2023

l'on compare des travailleurs occupant des postes comparables, cet écart se réduit à seulement 4%. Ce contraste révèle que la majeure partie de l'écart salarial s'explique désormais par une forme de ségrégation professionnelle : hommes et femmes, bien qu'ayant des caractéristiques similaires, n'occupent pas les mêmes types d'emplois. Comprendre les mécanismes qui conduisent à cette différenciation professionnelle devient donc essentiel pour expliquer la persistance des inégalités salariales.

Une large litérature académique a identifié de multiples facteurs derrière ces parcours professionnels divergents : la plus faible inscription des femmes dans les domaines STEM (sciences, technologie, ingénierie et mathématiques) (Ceci et al., 2014) ; les normes et stéréotypes guidant le choix du domaine d'études (Breda et al., 2023) ; et les différences comportementales en termes de confiance ou de goût pour la compétition (Gneezy et al., 2003; Niederle et al., 2007) qui se traduisent dans les choix éducationnels et professionnels (Buser et al., 2014; Flory et al., 2015). Cependant, la littérature récente met de plus en plus l'accent sur le rôle central des facteurs intra-ménages dans l'écart salarial entre hommes et femmes.

Goldin (2024) souligne, par exemple, le coût de la flexibilité particulièrement recherchée par les femmes afin de concilier carrière et responsabilités domestiques. Cette perspective s'inscrit dans la vaste littérature sur la pénalité salariale liée aux enfants (Angelov et al., 2016; Kleven, Landais, Posch, et al., 2019; Meurs et al., 2019) qui démontre l'impact négatif persistant de la parentalité sur les revenus des femmes. Cet effet s'explique par plusieurs mécanismes : suite à la naissance d'un enfant, on observe chez les femmes une réduction de la participation au marché du travail, mais aussi des heures travaillées et des salaires, et une orientation vers des métiers, secteurs et entreprises différents de ceux des hommes. Par ailleurs, Cortés et al. (2019) soulignent le rôle de la spécialisation intra-couple entre travail rémunéré et travail domestique, tandis que Azmat et al. (2022) analysent le coût de la flexibilité à travers le prisme du présentéisme et montrent que la parentalité éloigne les femmes des emplois les plus uniques et les mieux rémunérés. Enfin, Bertrand, Kamenica, et al. (2015) révèlent comment les normes de genre autour du revenu relatif amènent les femmes qui pourraient gagner plus que leurs maris à réduire leur participation au marché du travail.

Lorsque les choix et normes des ménages autour de la répartition du travail domestique et de la priorisation du travail rémunéré favorisent systématiquement un genre (souvent les hommes) lors de décisions déterminantes pour pour leur carrière, cela peut avoir un effet important sur l'écart salarial agrégé entre hommes et femmes. Cette thèse explore de tels mécanismes au niveau des ménages en se concentrant (particulièrement dans les deux premiers chapitres) sur l'angle spécifique de la localisation des ménages et de la géographie.

Les disparités spatiales de revenus en France sont importantes : alors que le salaire net mensuel d'un travailleur du secteur privé à Paris est de €3,222 en moyenne, il est de €2,282 dans le reste de la France (Le Fillâtre et al., 2024). Ces différences géographiques ont déjà été largement étudiées par une littérature extensive en économie urbaine sur les coûts et bénéfices de l'agglomération pour les individus.

L'avantage principal des villes provient de leur capacité à concentrer dans un espace géographique restreint un grand nombre de personnes et d'entreprises, favorisant ainsi l'émergence d'économies d'agglomérations. Celles-ci désignent les différents mécanismes par lesquels, lorsque les travailleurs et les entreprises sont rassemblés dans un espace restreint, leur productivité augmente. Depuis Duranton et al. (2004) elles sont généralement regroupées en trois catégories. Tout d'abord, les villes améliorent à la fois la qualité et la quantité des appariements entre travailleurs et entreprises. Elles permettent aussi de partager les infrastructures, les facteurs de production, mais aussi les gains de la variété et de la spécialisation. Enfin, elles génèrent plus de connaissances et aident à les diffuser à entre travailleurs, entreprises et générations. Bien que ces idées aient été soulevées depuis Marshall (1890), l'estimation empirique de la prime salariale urbaine (l'effet de la densité d'une ville sur les salaires) a longtemps été limitée par des complications méthodologiques.

L'une des principales menaces à l'identification de cette prime provient du fait que les travailleurs et travailleuses hautement qualifié.e.s tendent à vivre dans des villes plus denses ce qui, si cela n'est pas pris en compte, crée un biais à la hausse dans l'estimation de l'effet de la densité sur la productivité. Pour résoudre ce problème, Combes et al. (2008) proposent une approche en deux étapes (désormais standard), qui consiste à d'abord estimer une régression salariale individuelle incluant des effets fixes de ville mais aussi des effets fixes individuels pour contrôler de l'hétérogénéité non observée des travailleurs. Les effets de ville obtenus dans la première étape peuvent alors être utilisés comme mesure des salaires d'une ville, nets de la composition de sa force de travail. En utilisant cette méthode, diverses études ont montré que les salarié.e.s sont plus productif.ves et mieux rémunéré.e.s dans les grandes villes (D'Costa and Overman, 2014; Carlsen et al., 2016; De la Roca et al., 2017; Hirsch, Jahn, et al., 2022; Berson et al., 2024).

Pourtant, malgré le fait que les choix de localisation au cœur de la surreprésentation des travailleurs qualifiés dans les grandes villes soient faits par les ménages et non les individus, très peu d'attention a été accordée à l'hétérogénéité potentielle de la prime salariale urbaine selon le genre ou la structures des ménages. Les ménages étant l'entité décidant où vivre et travailler, cela ouvre la possibilité que les négociations et normes intra-couple créent une divergence dans l'effet de ces choix sur la carrière des hommes et des femmes. De plus, les contraintes sur les carrières des femmes induites par leurs responsabilités au sein du foyer pourraient affecter leur capacité à pleinement bénéficier des avantages d'appariement ou d'apprentissage associés aux grandes villes. De telles différences de genre dans les rendements urbains auraient des implications importantes pour comprendre le rôle des villes dans la réduction de l'écart salarial entre hommes et femmes.

Cette dissertation examine les interactions entre ménages, géographie et marchés du travail sous trois facettes différentes. Chaque chapitre étudie sous un angle propre la façon dont les décisions conjointes des ménages affectent les résultats des hommes et des femmes sur le marché du travail.

Le premier chapitre explore les conséquences genrées des décisions de localisation des ménages, examinant comment les déménagements affectent différemment les trajectoires professionnelles des hommes et des femmes. En utilisant des données administratives françaises et un design de différence-en-différences empilées, je montre que les décisions de relocalisation des ménages semblent systématiquement prioriser les carrières des hommes par rapport à celles des femmes. Même lorsque les femmes sont les principales pourvoyeuses de revenus dans leurs ménages, les déménagements tendent à bénéficier aux hommes tout en imposant des pénalités salariales temporaires mais significatives aux femmes. Ces résultats sont en contradiction avec les modèles selon lesquels les ménages maximisent leurs revenus sans se préoccuper du genre,et pointent plutôt vers l'influence persistante des normes de genre dans les processus de prise de décision des ménages.

Le deuxième chapitre étudie une caractéristique centrale de l'économie urbaine, la prime salariale urbaine, explorant comment le bénéfices de travailler dans des zones urbaines denses varie selon le genre et la structure familiale. Je trouve que les rendements de la densité sont significativement plus importants pour les femmes que pour les hommes, indépendamment du statut marital. Je montre aussi que cet avantage est presque entièrement éliminé pour les mères de jeunes enfants. Ces résultats suggèrent que les environnements urbains, bien que généralement bénéfiques pour les carrières des femmes, interagissent avec les contraintes spécifiques aux jeunes mères (garde d'enfant, absences temporaires...) et limitent la capacité de celles-ci à bénéficier des villes. De plus, je documente l'existence d'une prime de participation au marché du travail dans les villes plus denses qui est significativement plus importante pour les femmes que pour les hommes, indiquant que les bénéfices des grandes villes se produisent non seulement à la marge intensive, mais aussi à la marge extensive.

Le troisième chapitre, co-écrit avec Eric Maurin et Dominique Goux, étudie les effets indirects intra-ménage du recours au télétravail. Nous exploitons un changement dans le cadre institutionnel français autour du télétravail, qui a facilité la mise en œuvre du travail à distance après le choc pandémique de 2020 dans les établissements qui avaient préalablement signé un accord collectif sur le sujet. Nous trouvons de fortes complémentarités entre conjoints dans la décision de télétravailler : les individus dont le partenaire travaille dans une entreprise dotée d'un accord collectif sur le télétravail sont plus susceptibles de basculer vers le travail à distance après la pandémie. De plus, nous montrons que ces effets de contagion s'étendent aux heures travaillées : les partenaires des individus traités augmentent leurs heures, et cette augmentation est même plus importante en magnitude que l'effet direct sur les individus traités. Ces résultats montrent que les effets globaux de l'essor du télétravail sont largement sous-estimés lorsque l'on ne prête pas attention aux effets indirects intra-ménage.

Ensemble, ces trois chapitres éclairent l'importance de considérer le rôle des ménages pour étudier les décisions et résultats sur le marché du travail des hommes et des femmes. Ci-dessous, je décris chaque chapitre plus en détail.

Chapitre 1 : Suivre le mouvement : l'écart entre hommes et femmes dans les retours à la mobilité géographique

L'augmentation marquée de la participation des femmes au marché du travail au cours des dernières décennies a transformé la prise de décision au sein des ménages, les couples à double carrière devenant de plus en plus fréquents. Ces ménages doivent désormais concilier deux trajectoires professionnelles parallèles, ce qui peut engendrer des arbitrages intra-ménage en matière d'opportunités professionnelles. Les décisions de localisation constituent une voie évidente pour de tels arbitrages : face des opportunités professionnelles dans des villes différentes, les membres du couple peuvent être amenés à faire des compromis professionnels pour préserver leur relation. Cela peut conduire à deux phénomènes introduits par l'article séminal de Mincer (1978) : les *suiveurs contraints* (tied-movers) et les *restants contraints* (tied-stayers), des individus dont les décisions de localisation sont motivées par des considérations familiales plutôt que par des gains personnels.

Ces arbitrages géographiques peuvent à leur tour contribuer à l'écart de revenus entre hommes et femmes. Si les ménages privilégient systématiquement la carrière des hommes –en raison de leur statut de principal apporteur de revenu ou des normes de genre dans la répartition des responsabilités–, les déménagements induits peuvent accentuer les inégalités de revenus. Pourtant, il existe étonnamment peu de preuves causales sur les effets des relocalisations géographiques sur le revenu des membres du couple.

Dans cet article, je mesure comment les déménagements entre villes affectent différemment les trajectoires professionnelles des hommes et des femmes, qu'ils soient en couple ou célibataires, et j'étudie le rôle des normes de genre dans les choix de localisation des ménages.

En mobilisant des données administratives françaises fournissant des informations sur les revenus, la localisation et la composition des ménages pour l'ensemble des résidents français entre 2014 et 2019, j'examine les effets des déménagements sur les revenus du travail et le chômage selon le genre et le statut conjugal. Pour contrôler l'endogénéité de la mobilité, je m'appuie sur une méthode de différences-de-différences échelonnées, utilisant les individus *pas encore traités* comme groupe de contrôle. Je trouve que les femmes en couple (qu'elles soient mariées ou en concubinage) sont particulièrement désavantagées par les déménagements, avec des pertes de revenus substantielles, de l'ordre de 7% au cours des deux années suivant le déménagement. En outre, cette population connaît une hausse disproportionnée de la probabilité de percevoir des allocations chômage par rapport aux autres groupes démographiques. Ces effets ne sont cependant pas persistants : trois ans après le déménagement, les femmes en couple semblent avoir récupéré de leur perte de revenu. Ces résultats suggèrent que les femmes en couple sont plus souvent des suiveuses contraintes, et que les décisions de localisation compromettent significativement leur trajectoire professionnelle. En comparant les résultats des hommes seuls et des hommes en couple, j'observe que ces derniers obtiennent également des retours plus faibles à la mobilité, suggérant que si les hommes bénéficient généralement davantage des déménagements, les contraintes liées au couple affectent les deux partenaires.

J'explore ensuite le rôle de la parentalité dans cet écart de genre dans les retours à la mobilité . Une littérature abondante a documenté l'impact des enfants sur l'écart de revenus entre hommes et femmes, y compris en France (Kleven, Landais, Posch, et al., 2019; Meurs et al., 2019). Deux mécanismes peuvent expliquer une interaction entre cet effet et la mobilité. D'une part, si les couples déménagent après une naissance, ils peuvent privilégier la carrière de l'homme en raison de la baisse déjà observée des revenus et des heures travaillées des femmes. D'autre part, si un couple prévoit d'avoir un enfant, il peut anticiper les pénalités futures liées aux enfants et, par précaution, favoriser la trajectoire professionnelle masculine.

Pour tester empiriquement cette hypothèse, je reproduis mon analyse par groupes d'âge, ce qui révèle un écart de genre persistant dans les retours à la mobilité, quel que soit le stade de vie, y compris pour les individus de plus de 45 ans chez qui il est peu probable que ces préoccupations liées aux naissances jouent un rôle important. J'approfondis l'analyse en scindant l'échantillon selon la survenue d'une naissance autour du déménagement. Même parmi les couples n'ayant pas eu d'enfant au moment du déménagement, un écart de genre persiste dans les retours à la mobilité, bien qu'il soit moins prononcé. Les écarts les plus importants sont observés dans les couples ayant eu un enfant avant et après le déménagement. Ces résultats suggèrent que l'interaction entre naissances et mobilité amplifie les inégalités de revenus, mais n'en est pas l'unique origine.

J'explore ensuite deux mécanismes potentiels sous-jacents à l'écart observé. Une première hypothèse est que les ménages cherchent à maximiser leur revenu total lors des décisions de localisation, et privilégient la carrière du membre ayant les revenus initiaux les lus élevés, ou les meilleures perspectives, soit, dans la majorité des cas, l'homme. Une autre hypothèse est que ces choix reflètent des normes traditionnelles persistantes, selon lesquelles les hommes sont considérés comme les pourvoyeurs principaux du foyers, et les contributions professionnelles des femmes comme secondaires.

Pour départager ces deux explications, j'analyse les ménages en fonction du genre de la personne ayant les plus forts revenus avant le déménagement. Je constate que même dans les couples où les femmes gagnaient davantage avant la relocalisation, ce sont les hommes qui bénéficient le plus du déménagement. Par ailleurs, dans la majorité des cas, le revenu du travail total du ménage diminue après le déménagement. Ces résultats suggèrent que l'écart de genre dans l'effet des déménagements ne s'explique pas uniquement par les revenus préalablement plus élevés des hommes, ni par la maximisation du revenu total du ménage.[2] Les résultats mettent plutôt en évidence l'influence des normes de genre sur les décisions de localisation et les écarts de revenus qui en résultent, réduisant in fine le revenu total du ménage.

Mon article s'inscrit dans une littérature sur les décisions de localisation conjointes au

[2]Les ménages cherchent probablement à maximiser leur utilité plutôt que leur revenu, choisissant leur localisation en fonction du revenu, mais aussi des prix et des aménités locales (Roback, 1982; Rosen, 1979). Il n'est cependant pas évident d'expliquer comment cela générerait un écart de genre dans les retours à la mobilité, puisque les prix et aménités sont a priori les mêmes pour les hommes et les femmes. Il est possible en revanche que les couples choisissent une localisation favorisant la carrière de l'homme et les préférences de la femme en matière d'aménités, compensant ainsi une perte d'utilité.

sein des ménages. Les travaux pionniers tels que Mincer (1978) ont introduit l'idée que les liens familiaux constituent une contrainte dans les décisions géographiques, pouvant donner lieu à des migrations contraintes. Depuis, Nivalainen (2004) montre que, en Finlande, les migrations sont plus souvent déterminées par la carrière de l'homme, tandis que Jürges (2006) observe que cela n'est vrai en Allemagne que pour les couples ayant des valeurs « traditionnelles », mesurées à travers la répartition des tâches ménagères. Tenn (2010) étudie l'évolution du rôle des femmes dans les décisions de migration inter-états aux États-Unis et montre que, malgré la progression de la participation féminine sur le marché du travail, les hommes restent le principal moteur des décisions entre 1960 et 2000. Par ailleurs, Costa et al. (2000) avance que les décisions de colocalisation des « couples puissants » expliquent la concentration des couples diplômés dans les grandes villes américaines. Compton et al. (2007) testent cette hypothèse et trouvent que la migration est prédite par le niveau d'éducation du mari, et non par le niveau moyen du couple.

Bien que je n'observe pas directement le processus de décision intra-ménage, cette littérature offre un cadre interprétatif pertinent pour mes résultats. Le fait que les femmes en couple connaissent les pertes de revenu les plus importantes est cohérent avec le concept de migration contrainte. À l'inverse, les forts retours à la mobilité pour les célibataires, et l'écart de genre quasi nul dans ce groupe, soulignent le rôle central de la prise de décision conjointe pour expliquer les écarts de trajectoire après une mobilité.

Je contribue également à une littérature empirique cherchant à estimer les effets différenciés des déménagements sur les revenus des époux. Une large part de cette littérature n'a pas d'objectif d'identification causale (Sandell, 1977, Spitze, 1984, Bielby et al., 1992, Boyle et al., 2001, Cooke, 2003...), mais des travaux plus récents utilisent des modèles à effets fixes (Cooke et al., 2009) ou une stratégie de différences-en-différences avec les non-migrants comme groupe de contrôle (Blackburn, 2010). Ces articles, centrés sur les États-Unis et le Royaume-Uni, trouvent que les déménagements ont un effet négatif temporaire sur la carrière des femmes (mesurée par le salaire, les revenus ou la probabilité d'emploi), mais un effet neutre ou positif sur celle des hommes. L'article le plus proche du mien dans cette littérature est Jayachandran et al. (2024), qui utilise une approche en étude d'événement sur des données allemandes et suédoises, et trouve un écart persistant dans les retours à la mobilité, les hommes en bénéficiant bien davantage. Gemici (2007) et Buchinsky et al. (2023) estiment quant à eux des modèles de prise de décision conjointe aux États-Unis et en Israël, et quantifient l'impact négatif des décisions collectives sur les trajectoires féminines.

Je complète cette littérature en mobilisant une stratégie d'identification différente, reposant sur l'utilisation des futurs migrants comme groupe de contrôle plutôt que sur les seules variations individuelles. Cette approche permet une meilleure estimation de l'effet causal du déménagement sur l'ensemble de l'échantillon de migrants français. Déménager n'est évidemment pas un événement exogène : les ménages font ce choix en prenant en compte leur situation actuelle et les retours attendus. Cependant, ma stratégie en différences-de-différences permet d'identifier les effets différentiels selon le genre, en exploitant les différences de timing précis du déménagement entre des ménages qui déménagent tous sur une période relativement courte et se ressemblent fortement.

Enfin, je contribue à une vaste littérature sur les sources des écarts de genre dans les trajectoires professionnelles, et plus précisément sur le rôle des mécanismes familiaux. Comme souligné précédemment, de nombreux travaux ont montré que la « pénalité enfant » est un facteur central des écarts observés (Kleven, Landais, and Søgaard, 2019, Kleven, Landais, Posch, et al., 2019, Meurs et al., 2019, Cortés et al., 2023). D'autres travaux ont montré l'impact négatif des contraintes familiales limitant la flexibilité des femmes sur le marché du travail. Goldin (2014) et Cortés et al. (2019) montrent par exemple que l'impossibilité de travailler de longues heures freine la progression de carrière des femmes. Le Barbanchon et al. (2021) montrent que les femmes en recherche d'emploi en France sont moins disposées à faire de longs trajets domicile-travail, ce qui se traduit par un salaire plus faible à la reprise d'emploi. Bertrand, Kamenica, et al. (2015) analysent la prévalence des normes de genre dans la spécialisation au sein des couples : elles montrent que les femmes qui gagnent plus que leur conjoint ne sont pas protégées des rôles traditionnels, et ont même tendance à assumer davantage de tâches ménagères. Dans ce papier, je documente une nouvelle dimension de ces contraintes familiales : les relocalisations contraintes des femmes, qui constituent un mécanisme distinct mais complémentaire à la pénalité liée à la maternité.

Chapitre 2 : Genre, ménages, enfants et ville

La littérature en économie urbaine a largement établi l'existence d'une prime salariale urbaine : les travailleurs sont plus productifs et mieux rémunérés dans les grandes villes, même après avoir pris en compte la surreprésentation des individus les plus qualifiés des les grandes agglomérations. En France, Combes et al. (2008) quantifient cette relation et estiment l'élasticité des salaires à la densité à environ 0,03. Parallèlement, malgré des progrès notables au cours des dernières décennies, les inégalités de genre en matière de revenus restent une réalité économique persistante (Goldin, 2014). Pourtant, l'intersection entre ces deux phénomènes économiques fondamentaux – économies d'agglomération et inégalités de genre – demeure largement inexplorée. Les femmes et les hommes bénéficient-ils de manière égale des gains de productivité associés aux environnements urbains denses ?

Dans ce projet, je fournis de nouvelles preuves sur la manière dont les économies d'agglomération diffèrent selon le genre et la composition du ménage. Une difficulté centrale pour comprendre les mécanismes de gains différentiels d'agglomération selon le genre est le manque de données longitudinales de grande taille incluant à la fois les résultats sur le marché du travail et la composition des ménages. Je surmonte cet obstacle en accédant à une base de données administrative unique couvrant l'ensemble des résidents français. En la chaînant dans le temps, je suis en mesure de suivre les individus et les ménages au fil des années, et d'observer leur revenu, localisation, genre et structure familiale entre 2014 et 2019. En appliquant sur ces nouvelles données une méthode standard en deux étapes, j'estime la prime salariale urbaine par catégorie et montre qu'une fois le tri spatial pris en compte, les primes de salaire et de participation sont significativement plus élevées pour les femmes. J'estime que l'élasticité des revenus à la densité est d'environ 0,05 pour les hommes, mais qu'elle s'élève à 0,068 pour les femmes.

Plusieurs mécanismes théoriques pourraient expliquer des une différence de genre dans les économies d'agglomération. Les femmes ayant souvent des trajectoires d'emploi plus discontinues et un rayon de recherche d'emploi plus réduit (Le Barbanchon et al., 2021), le meilleur appariement observé dans les grandes villes pourrait leur être particulièrement bénéfique. Par ailleurs, si les femmes sont plus souvent contraintes dans leur déménagement (Gemici, 2007, Venator, 2023) –c'est-à-dire qu'elles déménagent en raison des gains professionnels de leur conjoint, même si cela ne correspond pas à leur intérêt personnel–, alors le meilleur appariement urbain entre entreprises et force de travail pourrait leur permettre de mieux s'adapter à leur nouveau lieu de résidence. À l'inverse, les interruptions de carrière peuvent atténuer les effets d'apprentissage induits par les grandes villes. Par ailleurs, les coûts de congestion (notamment en matière de garde d'enfants et de transport) pourraient avoir un effet négatif plus important sur les femmes. Enfin, les normes de genre et les comportements discriminatoires peuvent différer entre grandes et petites villes.

Bon nombre de ces mécanismes sont liés à la composition familiale, soit via les contraintes liées aux enfants, soit par les décisions intraménages. Pour évaluer leur pertinence dans l'explication de l'écart de genre dans la prime salariale urbaine, j'explore une dimension supplémentaire d'hétérogénéité et estime la prime urbaine selon le genre et la situation conjugale, ainsi que selon le genre et le statut parental. Si les mécanismes liés au ménage étaient déterminants, on s'attendrait à ce que la prime urbaine soit plus forte pour les mères ou les femmes en couple que pour les femmes seules ou sans enfant. Je montre au contraire que les femmes en couple ou avec enfants ne bénéficient pas davantage de la densité. L'écart significatif de genre dans la prime urbaine n'est pas spécifique aux femmes mariées, ni aux mères. A l'inverse, les mères de jeunes enfants subissent même une pénalité de densité qui efface presque totalement le gain d'agglomération supplémentaire des femmes. Cela suggère que les coûts de congestion liés aux contraintes parentales annulent les bénéfices potentiels de l'agglomération.

Cet article contribue à une littérature importante sur les gains individuels d'agglomération. Depuis Glaeser and Mare (2001), de nombreux travaux ont documenté l'existence d'une prime salariale urbaine et en ont étudié les mécanismes : Combes et al. (2008) pour la France ; D'Costa and Overman (2014) au Royaume-Uni ; De la Roca et al. (2017) en Espagne. Cette littérature empirique met en évidence une inégale répartition spatiale des travailleurs selon leurs compétances, expliquant une partie de la prime : les compétences individuelles sont positivement corrélées à la taille des villes. Toutefois, cette différence de composition entre villes ne permet pas d'expliquer entièrement la prime, et des gains de densité subsistent même lorsqu'elle est prise en compte. Peu d'articles ont cependant étudié l'hétérogénéité de ces gains selon les types de travailleurs. Carlsen et al. (2016) estiment la prime urbaine selon le niveau d'éducation en Norvège, et montrent que les travailleurs diplômés du supérieur tirent un meilleur bénéfice des grandes villes. Un autre exemple est Ananat et al. (2018), qui montrent que les travailleurs noirs aux États-Unis bénéficient de primes d'emploi à la densité bien inférieures à celles des travailleurs blancs. Plus récemment, Le Roux (2025) montrent que les gains de densité en Afrique du Sud sont nettement plus élevés pour les personnes à revenu élevé que pour celles à revenu faible.

Un nombre limité d'articles ont étudié comment cette prime varie selon le genre. Phimister (2005) et Hirsch, König, et al. (2013) trouvent une petite prime de participation urbaine pour les femmes, mais ne comparent que les zones urbaines et rurales. Plus récemment, D'Costa (2024) estime également l'effet du travail en ville – défini par opposition aux zones rurales – sur les salaires des femmes et des hommes, et conclue qu'avant 2008 la prime urbaine pour les femmes était deux fois plus élevée que celle des hommes, mais que cet écart disparaît après la crise de 2008. Dans un autre contexte, Le Roux (2025) ne trouvent aucune différence selon le genre en Afrique du Sud. L'article le plus proche du mien est Elass et al. (2024), qui utilisent une méthode similaire en deux étapes sur une autre base de données française pour estimer l'effet de la densité urbaine sur l'écart salarial de genre. Ils montrent également que les femmes bénéficient davantage de la densité urbaine, et concluent, après décomposition, que la ségrégation professionnelle explique en grande partie cet écart, suivie des contraintes liées aux enfants et aux trajets domicile-travail. Je complète leur recherche en explorant une autre dimension d'hétérogénéité : le rôle de la structure familiale, notamment le statut de couple et la présence d'enfants.

Si plusieurs travaux documentent une hétérogénéité des gains d'agglomération selon le genre, la littérature est quasi inexistante sur les mécanismes de ces différences. Dans cet article, j'explore la dimension spécifique de la structure familiale : alors que les hommes et les femmes deviennent de plus en plus similaires dans leurs caractéristiques productives, les facteurs liés aux ménages deviennent centraux pour expliquer la persistance des inégalités. La littérature récente sur l'écart de genre en matière de revenus souligne l'importance des enfants et de la spécialisation intraménage pour expliquer cet écart (Goldin, 2014, Angelov et al., 2016, Kleven, Landais, and Søgaard, 2019, Cortés et al., 2023, Goldin, 2024). La spécialisation dans le couple, avec un membre (souvent l'homme) concentré sur l'emploi rémunéré et l'autre sur les tâches domestiques et parentales, peut conduire les femmes à choisir des emplois plus flexibles mais moins rémunérateurs : à temps partiel (Manning et al., 2008), avec moins d'exigence de présence (Azmat et al., 2022), ou plus proches du domicile (Le Barbanchon et al., 2021)… Le rôle des facteurs familiaux dans les économies d'agglomération a été mis en avant par la littérature sur les *power couples*, dès Costa et al. (2000), qui soutient que les retours à la taille urbaine peuvent être plus élevés pour les couples diplômés que pour les autres ménages. Cependant cet article est le premier à étudier systématiquement le rôle des facteurs intraménages dans les différences de gains d'agglomération selon le genre, en analysant la prime salariale urbaine selon le statut conjugal et parental.

Dans une dernière section, je contribue à une petite littérature étudiant les disparités géographiques de participation au marché du travail des femmes, initiée par Odland et al. (1998). Black et al. (2014) documentent des écarts persistants et importants entre villes américaines, avec une moindre participation des femmes dans les grandes villes. De même, Moreno-Maldonado (2022) montre que les femmes avec enfants travaillent moins lorsqu'elles vivent en grande ville. Ces deux articles avancent que des coûts de transport et de garde d'enfants plus élevés incitent à une spécialisation intraménage : un conjoint travaillant davantage pendant que l'autre reste hors du marché du travail. Je contribue à cette littérature en montrant qu'une estimation de l'écart de participation prenant en compte les effets fixes individuels renverse ce constat : une fois les caractéristiques individuelles prises en compte, la densité a un effet positif sur la participation, et cet effet est même plus fort pour les femmes que pour les hommes.

Chapitre 3: Essor du travail à domicile et offre de travail des conjoints

Le choc pandémique du début des années 2020 a catalysé une expansion sans précédent du télétravail dans le monde entier. Un nombre croissant d'études expérimentales et quasi-expérimentales examine si et comment cette évolution pourrait impacter la productivité ou le bien-être des employés concernés, avec des résultats mitigés et encore débattus (Atkin et al., 2023; Emanuel, Harrington, and Pallais, 2023; Angelici et al., 2024; Bloom, Han, et al., 2024; Emanuel and Harrington, 2024). Cependant, les implications de cette évolution pour ceux qui vivent avec des télétravailleurs ont reçu beaucoup moins d'attention et demeurent largement inexplorées. Explorer ces implications est d'autant plus important que l'interdépendance des décisions individuelles au sein des familles a longtemps été identifiée comme un paramètre clé pour comprendre l'ensemble des conséquences qui peuvent résulter d'évolutions ou de réformes qui n'affectent directement qu'une partie de la population (par exemple, Ashenfelter et al., 1974, Gelber, 2014, Goux et al., 2014, Lalive et al., 2017, Johnsen et al., 2022).

Le télétravail réduisant drastiquement les temps de trajet, il a le potentiel de considérablement changer la façon dont les hommes et les femmes passent leurs journées et interagissent les uns avec les autres au sein des familles. Plusieurs scénarios sont possibles. Par exemple, la transition vers le télétravail pour certains employés peut les amener à prendre en charge une plus grande part du travail domestique et de la garde d'enfants, libérant du temps pour leurs partenaires, et permettant à ces derniers d'investir davantage dans leur travail et d'augmenter le nombre d'heures qu'ils travaillent. Étant donné l'importance que peut avoir la capacité à travailler de longues heures dans de nombreuses professions, les conséquences peuvent être considérables pour les deux conjoints et leur statut professionnel relatif (Bertrand, Goldin, et al., 2010; Goldin, 2014; Cortés et al., 2019). À l'inverse, avoir un partenaire qui travaille à domicile pourrait encourager l'autre à également travailler à distance, pas nécessairement pour travailler plus d'heures, mais simplement pour passer plus de temps en famille, sans impact majeur sur le nombre d'heures travaillées par l'un ou l'autre des conjoints. Selon lequel de ces scénarios domine l'autre, les conséquences de l'essor du travail à domicile sur le nombre d'heures travaillées dans l'économie ou sur les inégalités entre hommes et femmes au sein des couples sont potentiellement très différentes.

L'objectif de cet article est d'éclairer ces questions et d'estimer l'effet causal du choix d'un employé de travailler à domicile sur les résultats professionnels de son conjoint. À notre connaissance, il n'existe encore aucune étude qui ait abordé ces questions, l'une des difficultés étant de trouver des variations indépendantes dans l'exposition des conjoints au télétravail. Notre stratégie de recherche s'appuie sur le contexte institutionnel particulier dans lequel le choc épidémique de 2020 a frappé les entreprises françaises. Fin 2017, la France a adopté une législation qui facilitait l'adoption du télétravail par le biais d'accords de négociation collective. Cette réforme a créé deux groupes d'établissements : ceux qui avaient signé des accords sur le télétravail en 2018 ou 2019 (notre groupe de traitement) et ceux qui n'avaient signé que des accords sur d'autres sujets pendant la même période (notre groupe de contrôle). Bien que les deux groupes aient des tendances similaires sur le recours au travail à distance dans les années précédant la pandémie, le choc de 2020 a conduit à des augmentations significativement plus importantes du télétravail dans les entreprises du groupe de traitement. Nous exploitons cette variation en comparant les résultats sur le marché du travail des employés dont les conjoints travaillent dans des entreprises du groupe de traitement versus du groupe de contrôle, comparant essentiellement les travailleurs dont les partenaires ont fait face à une exposition élevée versus faible à l'adoption du télétravail induite par la pandémie.

Cette approche suggère d'abord l'existence d'effets croisés très significatifs sur le télétravail : les employés étaient significativement plus susceptibles de travailler à domicile dans les années suivant la pandémie de 2020 si leur conjoint travaillait dans un établissement du groupe de traitement, indépendamment du statut de traitement de leur propre établissement. Il est important de noter qu'aucune différence de ce type n'existait dans la période pré-pandémique, confortant notre stratégie d'identification. Les employés dont les conjoints sont dans le groupe de traitement ont augmenté leur télétravail dans une proportion équivalente à environ 80% de l'augmentation de leurs conjoints. Cela suggère que lorsqu'un conjoint adopte le télétravail, cela augmente la probabilité que l'autre conjoint travaille également à domicile d'environ 0,8. Sans surprise, ces effets croisés sur le télétravail ne sont perceptibles que dans les couples dont les membres ont des professions télétravaillables et non dans les couples dont les professions sont difficiles à exercer à distance.

Les employés avec des conjoints dans le groupe de traitement n'étaient pas seulement plus susceptibles de travailler à domicile après le choc épidémique, mais ils ont également significativement augmenté leur nombre habituel d'heures travaillées par semaine comparé aux employés avec des conjoints dans le groupe de contrôle. Selon nos estimations, le passage d'un employé au travail à domicile est suivi en moyenne par une augmentation de 20% du temps de travail hebdomadaire habituel du conjoint. Un examen plus attentif de cette augmentation montre qu'elle correspond essentiellement à la substitution de semaines de travail longues (40 heures ou plus) aux semaines de 35 heures ou moins (35 heures étant la durée légale de la semaine de travail). En cohérence avec l'idée que ces effets croisés sur les heures travaillées sont liés aux effets sur le télétravail, ils ne sont également perceptibles que dans les couples dont les membres ont des professions télétravaillables.

Des analyses supplémentaires révèlent que les effets croisés sur le télétravail et les heures travaillées affectent principalement les hommes dont la conjointe est dans le groupe de traitement, tandis que presque aucun effet croisé n'est détecté pour les femmes dont le conjoint est dans le groupe de traitement. À l'inverse, les effets directs sur le télétravail et les heures travaillées affectent les femmes du groupe de traitement beaucoup plus que les hommes du groupe de traitement. Nous montrons que ces asymétries profondes entre hommes et femmes sont cohérentes avec un modèle simple où les conjoints les moins bien payés (dans la grande majorité des cas, les femmes) travaillent à domicile autant que légalement possible dans leurs entreprises, indépendamment des choix de leurs partenaires, tandis que les conjoints les mieux payés (dans la grande majorité des cas, les hommes) n'augmentent leur taux de travail à domicile que dans la mesure où leurs partenaires l'augmentent aussi, afin de pouvoir bénéficier du temps économisé sur le trajet domicile-travail sans avoir à s'inquiéter d'augmenter leur contribution aux tâches domestiques ou à la garde d'enfants.

Le passage d'un employé au télétravail augmentant grandement la propension de son partenaire à travailler à domicile, nous pouvons nous demander si cela ne s'accompagne pas également d'un changement de lieu de résidence. Nous ne trouvons aucune preuve soutenant cette hypothèse : le passage au travail à domicile pour un employé ne s'accompagne d'aucun changement significatif dans la distance domicile-travail de son partenaire, ou dans la probabilité que le couple décide de vivre loin des centres urbains. L'essor du travail à distance a réduit les coûts de transport associés à l'éloignement des centres urbains pour les nombreux cadres travaillant et vivant dans ces centres, mais pas suffisamment pour compenser les autres coûts d'une telle distance, notamment en termes d'accès réduit aux meilleures infrastructures éducatives, médicales ou culturelles. En fin de compte, la réduction de la fréquence des trajets domicile-travail induite par le passage au télétravail ne semble pas être compensée par une augmentation des distances domicile-travail, ce qui aide à expliquer les gains en temps disponible (particulièrement pour le travail) réalisés par les couples ayant des professions télétravaillables.

Notre article contribue à la littérature explorant de longue date l'influence que les travailleurs ont les uns sur les autres au sein des couples. Un pan important de cette littérature a montré comment les travailleurs répondent aux changements dans les revenus ou les heures de travail de leurs conjoints, que ce soit au moment de la retraite de leurs conjoints, pendant les périodes de chômage, ou après une réforme fiscale (Lundberg, 1988; Bingley et al., 2007; Gelber, 2014; Lalive et al., 2017; Johnsen et al., 2022). En utilisant les changements dans la réglementation des jours fériés ou de la semaine de travail légale, un autre courant de littérature a également souligné l'importance que les travailleurs accordent à la possibilité d'ajuster et de synchroniser leurs heures de travail avec celles de leur conjoint (Hunt et al., 1998; Goux et al., 2014; Hamermesh et al., 2017; Georges-Kot et al., 2024). Dans cet article, nous soulignons la valeur que les employés accordent à pouvoir coordonner leur présence à domicile avec celle de leur conjoint, et les conséquences profondes que cette coordination peut avoir sur leurs heures de travail.

Nous contribuons également à la littérature naissante explorant les causes et conséquences de l'essor du télétravail qui a suivi le choc pandémique. Plusieurs articles ont montré que les travailleurs, et en particulier les femmes, ont une aversion pour les trajets et une forte disposition à payer pour le travail à distance (Mas et al., 2017; He et al., 2021; Chen et al., 2023; Cullen et al., 2025; Le Barbanchon et al., 2021; Bütikofer et al., 2024). Notre article suggère que la valeur que les employés accordent au télétravail reflète au moins en partie la valeur particulière qu'ils accordent aux interactions au sein du couple, la demande des employés pour le télétravail apparaissant d'autant plus forte que leur partenaire travaille lui-même davantage à domicile.

Un autre pan important de la littérature se concentre sur les effets du télétravail sur la productivité et les résultats professionnels des employés impliqués (Bloom, Liang, et al., 2015; Choudhury et al., 2021; Emanuel, Harrington, and Pallais, 2023; Gibbs et al., 2023; Barrero et al., 2023; Atkin et al., 2023; Angelici et al., 2024; Emanuel and Harrington, 2024; Bloom, Han, et al., 2024). Cette littérature est basée sur des expériences locales et des quasi-expériences menées dans des entreprises spécifiques et se concentre sur les effets du télétravail sur les employés concernés. En utilisant une expérience naturelle à grande échelle, nous nous concentrons sur une question différente : les effets induits sur les conjoints des employés concernés. Nous trouvons que lorsqu'un employé passe au télétravail, cela augmente grandement la probabilité que son conjoint passe également au télétravail, mais cela augmente aussi le nombre d'heures que le conjoint consacre au travail, particulièrement le conjoint le mieux payé. Ces résultats suggèrent que nous avons une vision très incomplète des effets du télétravail si nous ne prenons pas en compte les fortes interdépendances existant au sein des couples.

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Figure E1. Illustration of Chapters 1 and 2 by Agathe Denis

Note: This illustration was made by Agathe Denis on March 29th 2024 while I was presentating a former version of Chapters 1 and 2 at the *Friday internal Sciences Po seminar*. While some of the results have changed since then, it is still the best illustration I have ever seen of a tied-move. Thanks Agathe.