Expression of Interest

The Review of Economics and Statistics Fellowship

NBER Summer Institute 2025

Name: Dr. Filippos Maraziotis

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Current Position and Affiliation: Career Development Fellow (Postdoctoral Fellow), School for Business & Society, University of York, UK

PhD Completion: PhD in Economics, Department of Economics, University of York, 2023

Personal Website: Link

Summary of Research

My research interests lie in gender, family, and labor economics. I primarily investigate how gender norms, family dynamics, and family policies shape economic outcomes, applying microeconometric techniques for causal inference. In my recently published single-authored article in the *British Journal of Industrial Relations* (2024), I use the German Socio-Economic Panel and worker-firm spell fixed effects to demonstrate that flexible working time arrangements significantly narrow the gap between women's and men's working hours. Another single-authored study, titled *When She Works More Than Her Partner: Gender Norms and Women's Life Satisfaction in Australia* (R&R at *Work, Employment & Society*), analyzes multiple waves of the HILDA Survey to show that women's life satisfaction declines when their working hours exceed those of their male partners—especially when domestic labor remains unevenly distributed. Extending the seminal insights of Bertrand, Kamenica, and Pan (2015), by decompozing the well-being costs of non-conformity to traditional gender roles and imbalances in household chores, this study underscores the deep-rooted influence of gender norms on individual welfare.

In another project, co-authored with F. Bilenkisi and M.A. Yardimci, we adopt a novel dynamic Event Study design with continuous dose-treatment at the OECD level to evaluate post-pandemic minimum wage hikes. Our findings suggest that a 10% increase in the minimum wage boosts inflation by 0.87% at the second month, dissipates by month eight, and shows minimal spillover to wage growth or profit margins. This research has been under review at the *Journal of Applied Econometrics* for 2 months.

Finally, a single-authored work, currently under review at *Labour Economics*, explores how

intra-couple career alignment and parental leave shape economic outcomes, drawing on HILDA. The identification of the treatment effect relies on a couple-level fixed-effects design and a unique definition of treatment, as a scenario where one spouse transitions into the other's occupation while that other spouse remains in the same job. The findings suggest that women earn 5-6% more when their husbands enter the same occupation, whereas men's wages remain unchanged. This highlights a notable gender asymmetry in spousal career spillovers, and the findings hold under a variety of robustness checks, including an Event Study and an Instrumental Variables approach. In ongoing research, *Paternity Leave and Child's Long-Run Cognitive and Non-Cognitive Skills* (with M.A. Yardimci), we exploit the 2007 German paternity leave reform using a Regression Discontinuity design to investigate whether greater paternal involvement can affect children's cognitive and non-cognitive outcomes into adolescence and early adulthood. We further employ a Marginal Treatment Effects approach to account for heterogeneity in treatment exposure, and we document variations by family background and gender.

Distinctive Perspective and Contribution

In the Summer Institute, I seek to broaden the conversation about gender and family dynamics in labor economics by showing how intra-household behavior can not only amplify existing gender inequalities but also, in some cases, foster more equitable labor market outcomes. While research has rigorously documented how household dynamics and family responsibilities often reinforce gender gaps in employment and wages, the potential for progressive, gender-equalizing effects of household dynamics remains under-explored. My work aims to identify and measure these positive effects—such as mutual career support and shared parenting—thereby informing policy interventions that not only mitigate inequalities but also harness the benefits of more egalitarian family arrangements.

From a methodological perspective, I employ recent advancements in causal inference. Specifically, I engage with the theoretical and empirical progressions in Difference-in-Differences designs—pioneered by scholars such as Goodman-Bacon, Sant'Anna, Callaway, Borusyak, and de Chaisemartin—and the use of Marginal Treatment Effects, which move beyond conventional Instrumental Variables. These methodological innovations facilitate more robust causal inference by accommodating complex interventions, such as those with variable treatment timing or pronounced heterogeneity in treatment effects. I am not only proficient in the theoretical

foundations of these techniques but have also cultivated substantial practical expertise in applying them to empirical research—expertise I look forward to sharing and refining through active participation at the Summer Institute.

What I Hope to Gain

Attending the Summer Institute will offer a unique opportunity for academic enrichment, especially for an early-career researcher like myself. Interacting with a global cohort of leading scholars, particularly those whose work has profoundly shaped and still influences my academic identity such as Claudia Goldin, Claudia Olivetti, and David Autor, will expose me to the most cutting-edge perspectives on labor and gender economics. Engaging directly with their critical thinking and synthetic mindset will be transformative for my own work.

Moreover, by joining the presentations and discussions in each session, I expect to gain insights into emerging data sources, research questions, analytical frameworks, and research communication skills that can steer my current and future projects. Moreover, rigorous work on labor and gender economics increasingly demands an interdisciplinary lens—drawing on sociology and psychology for social-norm mechanisms, and demography for household structure and evolving marriage patterns. Seeing first-hand how established scholars weave these perspectives into high-quality empirical economic research will sharpen my ability to integrate cross-disciplinary knowledge and to collaborate beyond traditional economics boundaries. This exchange of ideas is therefore crucial to my development: observing how leading academics tackle new questions and methods will inform my thinking for years to come. *Ultimately, the Summer Institute is the ideal forum to deepen my commitment and passion to—as Gary Becker underlined—use Economics as a methodology to analyze social issues that range beyond those usually considered by economists.*

Sessions of Interest

— **Labor Studies** (July 21–23, 2025)

— **Gender in the Economy** (July 22–23, 2025)

— **Children and Families** (July 24–25, 2025)

Thank you for considering my application for the Review of Economics and Statistics Fellowship. Please find my CV and a recent research paper in the following pages.

Filippos Maraziotis

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CURRENT POSITION

Career Development Fellow July 2023 - Present
School for Business and Society, University of York
Research Clusters:
Human Resources Management and Workplace Inclusion
Applied Microeconometrics

EDUCATION

PhD in Economics October 2019 - June 2023
Thesis: Topics in Applied Labour Economics
University of York

MSc in Economics and Public Policy September 2018 - September 2019
Distinction, 75%
University of York

BSc in International and European Economics September 2014 - July 2018
Valedictorian/1st in cohort, 8.64/10
Athens University of Economics and Business

Other Educational Accomplishments

— Postgraduate Certificate in Academic Practice (PGCAP) September 2023 - Present
— Summer School on Socioeconomic Inequality July 2021
HCEO Global Working Group, University of Chicago
— Summer School in Microeconometrics and Policy Evaluation: June 2021
Modern Estimation Methods and Machine Learning, Paris School of Economics

RESEARCH

Research Fields
Labour Economics, Gender & Family Economics, Applied Microeconometrics

Research Interests
Gender Norms and Economic Outcomes; Family, Marriage and Employment Decisions; Minimum Wages Policies; Parental Leave Reforms; Organisational Practices and Workplace Inclusion

Publications

— Maraziotis, F. (2024). Flexibility for Equality: Examining the Impact of Flexible Working Time Arrangements on Women's Convergence in Working Hours. **British Journal of Industrial Relations**, 62(2), 410-445.

Working Papers

— When She Works More Than Her Partner: Gender Norms and Women's Life Satisfaction in Australia (R&R, **Work, Employment & Society**)

— Partnering for Career Success? Investigating the Wage Effects of Couples' Occupational Linkage (under Review)

— To be (worried) or not to be? An empirical analysis of minimum wage and inflation during the post-pandemic era, with F. Bileksisi and M. A. Yardimci (under Review)

Research In Progress

— Paternity Leave and Child's Long-run Cognitive and Non-cognitive Skills, with M.A. Yardimci

TEACHING EXPERIENCE

Module Leader
Dissertation Module, Undergraduate level — September 2024 - Present
Dissertation Supervisor, Undergraduate level
Quantitative Methods, Undergraduate level — December 2021 - August 2022

Module Instructor — September 2023 - June 2024
Quantitative Methods, Lecturer, Undergraduate level
Business Economics, Seminar Leader, Undergraduate level
Strategy and Strategic Management, Workshop Leader, Postgraduate level

Teaching Assistant — October 2019 - September 2022
Applied Economics, Econometrics II, Microeconomics II

SELECTED TALKS AND CONFERENCE PRESENTATIONS

Department for Work and Pensions (UK Government), Royal Economic Society (Online), Society of the Economics of the Household Annual Meeting (London), 24th Applied Economics Meeting (Mallorca), Annual Meeting of the Society of Economics of the Household (Boston), Workshop on Labour and Family Economics (York), Workshop on Gender and the Labour Market (Trier), Western Economic Association International Conference (Online), Australian Gender Economics Workshop (Online), Young Economists' Meeting (Brno), Asian and Australasian Labour Economics Conference (Beijing)

REFEREE SERVICE

Empirical Economics (2024), Economic Modelling (2024), Scottish Journal of Political Economy (2023)

NON ACADEMIC EXPERIENCE

Chief Research Economist at KMOP - Centre for Social Action and Innovation — October 2022 - Present
EU Horizon Projects: BENEFITS, SCIREARLY
Economic Advisor at the Hellenic Telecommunications & Post Commission — January 2024 - March 2025

AWARDS AND SCHOLARSHIPS

Nominated for the Teacher of the Year Excellence Award (2022), Best Practice Teaching Award (2021, 2020), Teaching Scholarship (2019-2022), Fotia Foundation Award for the Best Undergraduate Performance (2018)

SKILLS AND LANGUAGES

Stata, SPSS, R Programming, Microsoft Office, LaTeX, Python (beginner); Greek (native), English (fluent), French (advanced), Spanish (beginner)

REFERENCES

Dr Jeremy Aroles
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Partnering for Career Success?
Investigating the Wage Effects of Couples' Occupational Linkage

Filippos Maraziotis

University of York

March 2025

Abstract

This paper investigates whether one spouse's entry into the other's occupation affects the incumbent partner's wages. Using longitudinal data from the Household, Income and Labour Dynamics in Australia (HILDA) survey (2001–2018), a couple-level fixed-effects design identifies the causal impact of spousal co-specialisation while mitigating time-invariant unobserved heterogeneity. Across multiple robustness checks—including different specifications, placebo regressions, a non-absorbing event-study framework, and an instrumental variable approach leveraging past peer transitions—evidence shows that women gain five to six percent in hourly wages when their husbands move into their occupation. By contrast, men do not experience a comparable wage boost when wives make the same transition. The well-being boost and the husband's bargaining power explain a sizable portion of the female wage premium, whereas skill spillovers and schedule alignment appear to be less influential mechanisms. These results highlight how spousal alignment can foster women's earnings via enhanced motivation, transparency, and negotiation leverage. They also underscore a noteworthy gender asymmetry, indicating that men's wages remain relatively insensitive to occupational linkage.

Keywords: Work-linked Couples, Wage Effects, Assortative Matching, Dual Earner

JEL Classification: J12, J24, J3, M51

1. Introduction

Household structures in high–income countries have evolved over recent decades, with dual–earning spouses now the norm. This shift has prompted extensive research on positive assortative matching by education or income—the tendency of individuals with similar educational or income backgrounds to form partnerships (Chiappori et al., 2025; Chiappori et al., 2018; Blau and Kahn, 2017). However, one comparatively niche phenomenon—where spouses work in the same or closely related occupations—remains less understood. Although these couples, henceforth named work-linked, may appear uncommon at first glance, there is growing recognition that occupational co–specialisation can alter both household dynamics and individual labour–market outcomes (Hennecke and Hetschko, 2025; Calvo, Lindenlaub, and Reynoso, 2024). Indeed, an increasing share of couples are formed in the workplace or professionally adjacent settings (Han and Qian, 2021; Mansour and McKinnish, 2018), suggesting that the potential wage implications of spousal alignment require closer investigation. This paper examines whether and how a spouse's entry into the other's occupation affects the "incumbent" partner's wages.

Recent work shows that spouses in the same or closely related occupations often enjoy significant non-pecuniary benefits. From an organisational psychology perspective, Ferguson et al. (2016) find that work linkage fosters better work–life balance, higher job and family satisfaction, and lower work-related stress. Halbesleben et al. (2010) similarly document increased spousal support and reduced emotional exhaustion among work-linked couples, while Halbesleben, Wheeler, and Rossi (2012) add that co–specialisation decreases time and behaviour–based work–family conflicts but may heighten strain–based conflict. In the economic literature, Hennecke and Hetschko (2025) show that German work-linked couples experience significant gains in income satisfaction—especially in highly educated households—though they observe only modest gains in overall life satisfaction and no significant improvements in leisure or work–family balance. They also find no evidence of gender heterogeneity and suggest that income progression might be the principal channel underpinning these positive well-being outcomes.

Few studies relate occupational proximity to spousal employment outcomes but leave open questions about precise wage effects. Mansour and McKinnish (2018) show that the workplace is a critical marriage market, leading many couples to form through coworking relationships. In particular, they highlight that individuals often match within occupation because of the lower searching costs. However, their analysis says less about how such relationships alter long-term earnings trajectories and leaves an open question regarding the employment outcomes of work-linked partners. Zinovyeva and Tverdostup (2021), using Finnish data, find that couples who begin coworking in the same firm experience a compression of earnings shares toward 0.5 alongside a marked increase in women's earnings relative to observationally similar women in non-coworking marriages. This result, which partially reflects firm–level pay compression, also seems driven by potential joint negotiations or

favouritism, challenging traditional gender identity norm arguments about female earnings parity. However, it should be noted that coworking spouses are plausibly selected on shared preferences, making them less prone to conflicting gender norms. Beyond firm–level overlap, Zinovyeva and Tverdostup (2021) additionally observe that in broader occupation–related marriages, the proportion of couples where wives slightly outearn husbands is roughly twice as high as in the general population, suggesting that co-specialisation can foster more equitable household income dynamics.

Several mechanisms may explain why spousal occupational linkage boosts labour-market monetary outcomes. A key mechanism arises from shared preferences. Kalmijn (1994) first argued that spouses who share similar cultural or lifestyle values enjoy greater marital surplus by aligning their professional spheres, while Mansour and McKinnish (2018) contend that individuals in the same occupation often have more homogenous tastes regarding leisure, childrearing, and other family–oriented decisions. By matching on these preferences, couples can coordinate economic and household choices more seamlessly, potentially unlocking efficiency gains that translate into higher earnings.

Another channel is skills and knowledge transfer, or cross-spousal human capital spillovers (Janning, 2006). When partners specialise in similar tasks, they exchange professional insights and solve job–related problems together, effectively becoming colleagues at home. Mas and Moretti (2009) and Herbst and Mas (2015) show that peer effects in productivity and wage outcomes can be amplified in daily interactions. Similarly, Cornelissen, Dustmann, and Schönberg (2017) find strong evidence that co–workers' productivity affects individual wages, while Papay et al. (2020) document performance gains from high– and low–skilled co–workers collaborating on shared tasks.

A further mechanism concerns work and family coordination. Spouses sharing an occupation may more easily align schedules, reduce mutually conflicting commitments, and facilitate the positive spillovers often termed enrichment (Kelliher et al., 2019; Greenhaus & Powell, 2006). Ferguson et al. (2016), for instance, show that work linkage promotes better work–life balance, while Bryan and Sevilla (2017) argue that coordinating work hours allows spouses to enjoy more shared leisure time, thereby improving overall life quality.

A final mechanism involves negotiation gaps and wage transparency, particularly as they relate to gender. Kiessling et al. (2024) show that women's lower initial wage claims and reservation wages contribute substantially to the persistent male–female gap in expected earnings, complementing earlier findings on gender differences in negotiation behaviour (Säve–Söderbergh, 2019). In particular, Säve–Söderbergh (2019) observes that even top–earning women request lower salaries than men, while Rousille (2024) confirms a sizable ask gap that directly feeds into pay disparities. In the context of occupational co–specialisation, one sub-mechanism might be that women learn about the prevailing wage structure—including any gender pay gap—by observing their spouse's salary negotiations and subsequently adjusting their own bargaining stance. Additionally, the incumbent spouse can benefit if the newly moving partner has strong bargaining leverage or higher status, thereby improving the household's collective ability to negotiate better terms.

This paper examines whether one spouse's entry into the other's occupation affects the wages of the partner who remains in place, and whether men and women benefit equally, drawing on data from the Household, Income and Labour Dynamics in Australia (HILDA) survey (2001–2018), and exploiting within–couple variation to measure how one spouse's occupational switch influences the other spouse's wages. The empirical strategy uses couple–level fixed effects, thus ruling out time–invariant unobserved factors shared by both partners, such as preferences for sharing common experiences. The treatment is defined as a scenario in which one spouse transitions into the other's occupation while that other spouse remains in the same job. This setup ensures that any observed wage changes reflect the newcomer's entry rather than the underlying partner's occupational change. Under this design, the main finding is that women experience a significant wage gain of five to six per cent, whereas men do not benefit similarly. A series of robustness checks—including alternative specifications, placebo tests, a dynamic event study, and an instrumental variable approach leveraging past peer-driven shocks—indicate that these results are unlikely to arise from endogenous mobility or reverse causality, while the positive wage gains for women exist for at least three years after linkage. Further analysis suggests that preferences for work linkage and the husband's bargaining power are the most likely channels driving women's wage gains.

Overall, these findings underscore a pronounced gender asymmetry in the earnings impact of spousal co–specialisation: while women gain substantially in wage terms, men show little corresponding benefit. This outcome dovetails with persistent evidence on gender wage gaps (Blau and Kahn, 2017), indicating that work linkage may serve as a valuable channel for reducing such disparities by affording women extra bargaining leverage and knowledge–sharing opportunities. Ultimately, the results complement existing research on non-pecuniary benefits of co-working couples, reinforcing the idea that occupational alignment can disproportionately strengthen women's labour–market outcomes.

This paper makes three main contributions. To the best of my knowledge, it provides the first causal estimate of how one spouse's entry into the other's occupation influences the incumbent partner's wages—thus extending a small but growing strand of research on coworking couples beyond non–monetary effects or firm–level overlap. In particular, it moves beyond Hennecke and Hetschko's (2025) causal evidence on well-being effects of work linkage and Zinovyeva and Tverdostup's (2021) finding that female spouses in same-firm couples achieve higher earnings, by capturing how an occupation-specific transition shapes wage outcomes for the partner who remains in place.

Second, in contrast to studies of productivity spillovers among colleagues in the workplace (e.g., Cornelissen, Dustmann, and Schönberg, 2017; Mas and Moretti, 2009), this paper demonstrates that spouses function as powerful colleagues at home. Sharing both a household and an occupation intensifies the exchange of wage benchmarks, negotiation strategies, and professional contacts, creating stronger peer–like effects than those typically observed among co–workers. By revealing how household interactions amplify labour–market gains, this study bridges the peer–effects theory and the economics of household decision–making.

Third, while much scholarship on the gender wage gap focuses on education, labour supply, or family formation (Bertrand et al., 2021; Olivetti and Petrongolo, 2016; Goldin, 2014), the findings highlight spousal occupational linkage as an additional channel that can elevate women's earnings. Co–specialisation disproportionately benefits women by fostering wage transparency and negotiation support, thereby strengthening their labour–market prospects and offering a fresh perspective on how within–household dynamics can help narrow persistent pay gaps. Moreover, this channel has broader implications beyond immediate labour outcomes: because partners' relative wage rates often serve as a proxy for household bargaining power (Shenhav, 2021; Aizer, 2010), an increase in female wages can shift the balance of household production and decision–making.

The article proceeds as follows. Section 2 details the empirical methodology, and Section 3 describes the HILDA data and outlines sample construction. Section 4 presents the main results, along with robustness checks and an exploration of mechanisms. Section 5 concludes, discussing the broader implications for labour market policy and research on household decision-making.

2. Empirical Strategy

The empirical analysis proceeds by estimating a wage equation in which the individual *i*'s log real hourly wage is regressed on their partner's occupational transition and a rich set of controls, including couple-specific fixed effects. Denoting individuals by i, time by t, and couples by c, the main specification can be written as

$$\ln w_{ict} = \alpha Link_{ict} + X'_{ict}\beta + \gamma_c + \lambda_t + \delta_{r\times t} + \mu_o + \rho_s + \varepsilon_{ict} \qquad (1)$$

Here, $\ln w_{ict}$ is the individual *i*'s log real hourly wage. The logarithm of wage is preferred rather than levels or semi-log forms, in line with standard practice when studying wage determinants. This approach allows to interpret the coefficient of interest, α, as the percentage change in real hourly wages associated with the partner's occupational transition. Moreover, wage distributions often exhibit right-skewness; logging them mitigates the impact of outliers and typically improves model fit. Consequently, the log-wage specification offers both interpretive convenience and empirical stability, motivating its use throughout the analysis.

$Link_{ict}$ indicates whether in period t the partner has moved into *i*'s occupation while individual i has remained unchanged in the previous occupation. To control for determinants of human capital accumulation and the allocation of labour supply within the household, a vector of time–varying characteristics $\mathbf{X}_{ict}$ is included. This vector initially comprises age, age squared, years of work experience, and the number of children under four years of age. Further specifications additionally include the partner's human capital–related characteristics as well as *i*'s lagged wage rate. The

specification also includes region–by–wave interactions $\delta_{r\times t}$, wave fixed effects λ_t, and fixed effects for each occupation μ_o and industry ρ_s. The region-by-wave interactions help isolate fluctuations in wages driven by local economic shocks, while occupation and industry fixed effects net out average wage trends specific to particular sectors or professions.

Most critically, the specification incorporates a couple–level fixed effect, γ_c. By focusing on changes within each couple over time, this approach eliminates bias arising from time-invariant couple–specific unobserved traits such as shared preferences, match quality, or latent family characteristics. Identification of α thus hinges on within–couple variation in the partner's occupational transition. For instance, one observes the same couple before and after the husband shifts to the wife's occupation and industry, while the wife remains in the same occupation and industry as in the previous wave. Moreover, since the estimation is confined to couples in stable partnerships (with no relationship dissolution throughout the observation window), it removes confounding pathways that might arise if separation or remarriage coincided with occupational mobility.

Under this framework, any systematic difference in partner *i*'s wage trajectory in the treated couples, compared with the trajectory in periods when no such occupational switch has occurred, is attributed to the partner's transition. The couple fixed effect γ_c further mitigates selection on unobservables that do not vary over time, which is particularly relevant in cases where unobserved complementarities within the household could lead couples with stronger career synergies to coordinate more frequently. Once these fixed effects are accounted for, the only remaining threats to identification are time–varying confounders correlated with both the husband's (wife's) decision to move and the wife's (husband's) earnings potential. These might include evolving household financial constraints, short–run shocks in local labour markets that affect each spouse differently, or significant health events. The region–by–wave interactions and the inclusion of children variables aim to address part of this concern, while additional robustness analyses—including specifications with additional controls, a dynamic event-study design, placebo tests, Oster selection–on–unobservables checks, and an Instrumental Variables approach—provide further support for the validity of the identification assumption.

Lastly, since wages may shift in response to broader occupational pay trends, occupation and industry fixed effects enter the model together with the region–by–wave indicators. This is intended to net out occupation–level shocks and local market fluctuations, ensuring that the estimated treatment effect is not merely capturing a fortuitous boom in individual *i*'s line of work. With these controls, the identification of α rests on the premise that any remaining variation in the partner's occupational switch is essentially idiosyncratic to the couple and uncorrelated with latent influences on *i*'s wage. Although such an assumption can never be tested definitively, the combination of high-dimensional fixed effects and a strategically defined treatment variable—centred on the partner's move rather than a joint spousal transition—provides a credible basis for interpreting α as the causal effect of occupational linkage on *i*'s wage.

3. Data

This study uses data from the Household, Income, and Labour Dynamics in Australia (HILDA) Survey, a nationally representative longitudinal dataset covering the period 2001–2018.[1] HILDA includes rich information on household composition, income, education, and employment, making it particularly suited to examining occupational linkages between spouses. The survey tracks respondents over time, enabling the identification of stable partnerships—crucial for studying within-couple wage dynamics.

To focus on sustained, dual–earner partnerships, the sample is restricted to heterosexual couples, either married or de facto spouses, that remain intact (no separation or divorce) over the observation window. Individuals aged below 24 or above 60 are excluded, as the former group may still be completing education, and the latter often transitions out of the labour force. Furthermore, observations above the 99th or below the 1st percentile of the real wage distribution are removed to prevent outliers from skewing the analysis. Some extreme values are also removed. In particular, the maximum hours worked in the main job for each person are capped at 55 hours per week, eliminating the top 5% of the hours' distribution. Individuals working more than 55 hours per week are excluded to avoid potential biases arising from extreme work–life imbalances, ensuring that the focus remains on typical work-life dynamics. These individuals are likely to have less time for interaction and communication with family members, which could distort the analysis of work–linked couples. Lastly, self-employed individuals and those working in family businesses are omitted to ensure a consistent comparison within standard employment relationships and wage settings.

After these sample restrictions, 11,446 observations of work-unrelated couples (i.e. couples who do not share the same occupation), while 251 observations relate to work-linked couples[2] (i.e. at a given point in time, spouses share the same occupation), over 18 years from the 13 regional clusters in HILDA[3]. The work linkage is constructed using the Australian and New Zealand Standard

[1] The HILDA Project was initiated and is funded by the Australian Government Department of Social Services (DSS) and is managed by the Melbourne Institute of Applied Economic and Social Research (Melbourne Institute). The findings and views reported in this paper, however, are those of the author and should not be attributed to either the DSS or the Melbourne Institute.

[2] Note that the 251 figure refers specifically to couple-years in which one spouse transitions into the other's occupation while the incumbent remains in place. By tracking each couple across multiple waves, the panel design generates far more total observations than a simple cross-section. Moreover, our identification strategy employs couple-level fixed effects, leveraging within-couple changes in occupational status and wages before and after that targeted transition. Thus, even with a relatively small number of "treated" couple-years, what truly matters is having (i) observations from pre-treatment periods, (ii) observations from post-treatment periods, and (iii) an unlinked comparison group. This setup isolates time-varying, couple-specific shocks without requiring a large cross-sectional sample. Importantly, the estimated standard errors across specifications in the following sections remain narrow and stable, reflecting the repeated-observation structure and rich within-couple variation, despite the limited treatment-group size. While 251 is modest for the tightly defined treatment, it is worth noting that if we count all couple-years in which spouses share the same occupation—regardless of who moved—this accounts for roughly 7 per cent of the data (about 800 observations), underscoring that spousal work linkage is not trivial. Prior research in applied labour economics often estimates causal effects of rare transitions (e.g. job displacement, relocation, fertility shocks), where treatment groups are small, but variation is rich (e.g. Oreopoulos, von Wachter, and Heisz, 2012; Black, Devereux, and Salvanes, 2008; Jacobson, LaLonde, and Sullivan, 1993). Our approach follows a similar logic, focusing on well-identified within-unit changes rather than cross-sectional power.

[3] Sydney, Rest of New South Wales, Melbourne, Rest of Victoria, Brisbane, Rest of Queensland, Adelaide, Rest of South Australia, Perth, Rest of Western Australia, Tasmania, Northern Territory, Australian Capital Territory.

Classification of Occupations 2006 Edition (ANZSCO 2006), a two–digit classification which provides an accurate and specific definition of occupation, generating 50 different occupational categories.[4] The occupations where partners appear to link more frequently are: Office Managers and Program Admins; Specialist Managers; Business, HR, and Marketing Professionals; Health Professionals; Carers; Education Professionals; Engineering, ICT, and Science Technicians; Clerical and Administrative Workers; Sales Representatives and Agents.

Table 1 compares the main employment characteristics of the work-linked and work-unrelated couples, disaggregated by gender. Women in work–linked couples are notably less likely to hold part-time positions (35%) compared to those in work–unrelated couples (51%). For men, the gap is smaller—3% versus 7%—but still indicates that those in work–linked couples are more likely to hold full–time roles. These differences in part-time status translate into average weekly hours: 33.9 for work-linked women versus 30.1 for work–unrelated women, and 42.3 for work–linked men versus 41.4 for men in work–unrelated couples.

Table 1. Summary Statistics, by Work Linkage

	Women				**Men**			
	Work-linked		Work-unrelated		Work-linked		Work-unrelated	
Variable	**Mean**	**Std. Dev. (within)**	**Mean**	**Std. Dev. (within)**	**Mean**	**Std. Dev. (within)**	**Mean**	**Std. Dev. (within)**
Part–time	0.35	0.48 (0.16)	0.51	0.50 (0.32)	0.03	0.18 (0.00)	0.07	0.26 (0.17)
Weekly Working Hours	33.90	10.74 (3.14)	30.13	11.84 (7.12)	42.29	5.63 (1.58)	41.35	7.87 (4.90)
Real Hourly Wage	37.97	16.55 (4.27)	27.92	12.89 (8.07)	37.21	17.26 (4.15)	33.28	15.96 (9.15)
Experience	18.86	8.55 (1.60)	19.98	9.12 (3.60)	23.45	10.08 (1.21)	25.20	10.44 (3.74)
Age	40.40	8.65 (1.63)	42.07	9.33 (3.81)	41.95	8.59 (1.21)	42.15	9.31 (3.76)

Notes: Data from HILDA Release 18.

The real hourly wage, computed by dividing weekly earnings from the main job by weekly hours worked and then deflating using the annual Australian Consumer Price Index, also differs substantially. Among women, those in work–linked couples earn on average 37.97 AUD per hour, while their work-unrelated counterparts earn 27.92 AUD. For men, the difference is smaller—37.21 versus 33.28 AUD—but still suggests that work-linked men occupy somewhat higher–paying roles. By contrast, experience (years in the labour market) is slightly lower for the work-linked group in both

[4] Supplementary analysis defines occupational linkage using 1-digit codes, as well as a combination of 2-digit occupational codes with 1-digit industry codes.

genders (18.86 versus 19.98 for women; 23.45 versus 25.20 for men), as is age (40.4 versus 42.1 for women; 42.0 versus 42.2 for men). This pattern may indicate that work–linked couples, despite having somewhat lower average experience, secure better–paid or more specialised roles, particularly for women.

In addition to the overall variability, the table also reports a within–couple standard deviation for each variable, which measures how much that characteristic changes over time within the same couple. The presence of a non–trivial within–couple standard deviation suggests that couples do not remain locked into a single work arrangement but instead exhibit meaningful time variation in these dimensions. This within–couple variation is essential for the subsequent fixed–effects analysis, which exploits changes over time in one spouse's occupation to identify wage effects for the partner who remains in place. If variables remained largely static within couples, it would be difficult to disentangle the causal impact of occupational linkage from stable couple–level characteristics. By contrast, the meaningful within–couple changes observed here enhance confidence that the fixed-effects strategy can isolate the effect of spousal occupational transitions rather than merely capturing unchanging preferences or persistent differences across couples.

Table 2. Level of Education, by Work Linkage

	% of Women		% of Men	
Level of Education	Work-linked	Work-unrelated	Work-linked	Work-unrelated
Up to 11 years	10.08	19.54	8.20	14.16
12 years	14.73	14.61	13.11	10.42
Certificate III or IV	4.65	15.69	9.83	31.27
Diploma	5.43	10.76	9.85	11.87
College degree	30.23	21.56	31.97	17.66
Graduate Diploma	17.83	11.24	12.30	7.48
Master's or Doctorate Degree	17.05	6.96	14.75	7.14

Notes: Data from HILDA Release 18.

Table 2 shifts the focus to educational attainment. Consistent with the broader assortative mating literature, individuals who share an occupation also appear, on average, to share higher levels of schooling. For instance, 30.23% of work–linked women hold a college degree compared to 21.56% of work–unrelated women, while 17.05% have attained a master's or doctoral qualification (versus 6.96% among the work–unrelated). A similar pattern emerges for men: over 30% of men in work–linked couples hold a college degree, yet fewer than 18% of their work–unrelated counterparts do. At the same time, men in work–linked couples are less likely to have only completed a Certificate III or IV. These patterns support the notion that work–linked couples are more prevalent at higher skill and educational levels, possibly reflecting the value of advanced qualifications in shaping career trajectories that converge.

The figures that follow illustrate differences in the wage distributions for work–linked and work–unrelated couples. Figure 1 plots the cumulative distribution functions (CDFs) of real hourly wages for women, comparing those who share an occupation with their partner (dotted line) and those who do not (solid line). The CDF for work–linked women consistently lies to the right, signifying that at any given wage threshold, a smaller fraction of work-linked women earn less than that threshold. This difference is especially pronounced around the median wage, suggesting that the central portion of the wage distribution is shifted upward for work–linked women. By contrast, at the extreme high and low ends of the distribution, the two lines converge somewhat, implying that wage outliers might be less affected by occupational linkage. A formal Kolmogorov–Smirnov test (Kaplan, 2019) rejects the equality of the two distributions at the 1 per cent level, reinforcing the visual impression that wage outcomes differ systematically between work–linked and work–unrelated women. Although these results do not establish causality, they underscore the descriptive finding that women who share an occupation with their partner tend to occupy higher–wage segments of the distribution.

Figure 1. CDF of women's real wage rate

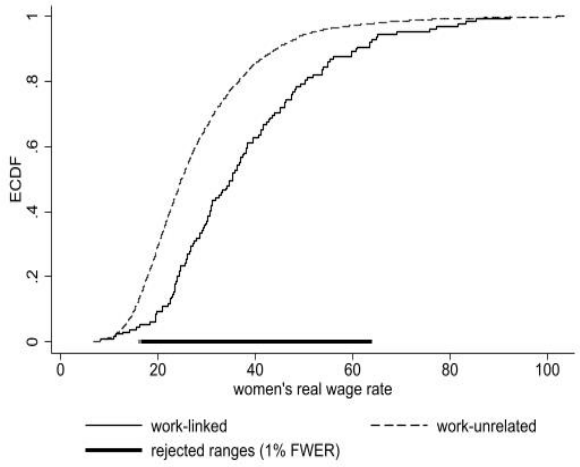
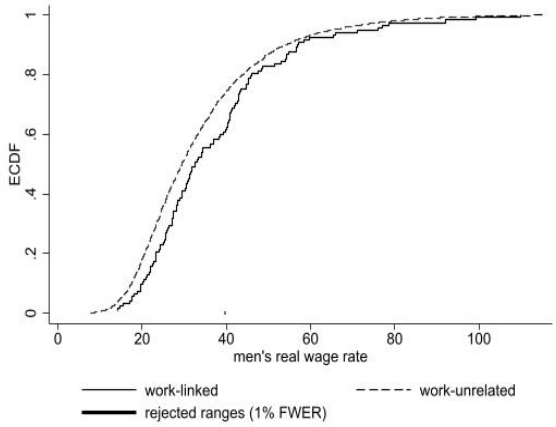


Figure 2. CDF of men's real wage rate



Notes: These figures plot the cumulative distribution functions (CDFs) of real hourly wages for women (Figure 1) and men (Figure 2), comparing individuals in work-linked couples (dotted line) to those in work-unrelated couples (solid line). Real hourly wages are calculated by dividing the weekly wage from the main job by weekly hours in that job and then deflating using the annual Australian Consumer Price Index from the Australian Bureau of Statistics. A Kolmogorov–Smirnov test rejects equality of the distributions at the 1 per cent level for women, indicating that work-linked women systematically earn higher wages. By contrast, for men, the test does not reject equality, suggesting that the wage distribution for work-linked and work-unrelated men is more similar. Data from HILDA Release 18.

A comparable plot for men's real hourly wages is shown in Figure 2. Here, the differences are more modest. While the work–linked CDF again lies slightly to the right for certain wage ranges, there are intervals—particularly at mid-range wages—where the two lines overlap or cross. The Kolmogorov–Smirnov test fails to reject the equality of the distributions at conventional significance levels, suggesting that the wage distribution for men in work-linked couples does not diverge as strongly from that of men in work–unrelated couples. In other words, although there is some indication that work–

linked men may earn marginally more on average, the overall distributional contrast is less pronounced than it is for women. This pattern suggests that for men, being in a similar occupation to their partners provides a marginal wage advantage, but perhaps less consistently across the wage distribution than it does for women.

Taken together, these descriptive statistics and figures point to important differences along the dimensions of occupation, education, and pay. Work–linked couples cluster among individuals who possess higher qualifications and, for women, substantially higher wages. This pattern offers an initial hint that occupational linkage may be associated with measurable wage advantages for women. The subsequent sections determine whether these differences reflect genuine causal effects of work linkage rather than pre–existing attributes that sort some couples into similar occupations.

4. Results

4.1 Main Specification

Table 3 presents the couple–level fixed–effects regression results for women's and men's real hourly wages, each estimated under three specifications. Columns (1) and (4) include the baseline controls (individual age, age squared, experience, and children under four, along with region–by–year, occupation, and industry fixed effects). Columns (2) and (5) augment the baseline by adding the partner's age and experience variables, while Columns (3) and (6) further include the individual's own lagged wage.

Focusing on women (Columns 1–3), the coefficient on Occupational Linkage consistently points to a wage gain of around 5.5 per cent, significant at the 5 per cent level. The stability of this treatment effect across specifications underscores that the result is not driven by spousal life–cycle effects or unobserved persistence in wages. Indeed, incorporating the partner's age and experience (Column 2) ensures that any concurrent human capital changes on the husband's side—such as his evolving life-cycle position or continuing education—do not spuriously inflate the wife's wage gain from occupational linkage. The estimate remains positive (0.056) and significant, indicating that the woman's wage premium persists even after netting out partner–specific factors.

Similarly, adding the individual's own lagged wage (Column 3) accounts for potential state dependence on wages. If unobserved wage shocks persist over time and also shape subsequent occupational transitions, omitting this factor could bias the coefficient on occupational linkage. Although including a lagged dependent variable in a fixed–effects framework may introduce some dynamic panel bias, the relatively long panel—up to 18 years—mitigates this concern. Consistent with that expectation, the estimate on Occupational Linkage remains around 0.05 and statistically significant—albeit at the 10 per cent level—suggesting that the positive effect is robust to controlling for any lingering wage shocks from the previous year.

Table 3. Main Results: The Effect of Occupational Linkage on Real Hourly Wages

	Women			Men		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Occupational Linkage	0.056** (0.027)	0.056** (0.027)	0.051* (0.027)	0.005 (0.022)	0.005 (0.022)	–0.018 (0.023)
Lagged Wage	–	–	0.105*** (0.019)	–	–	0.195*** (0.024)
Age	0.049*** (0.009)	0.031 (0.026)	0.050*** (0.012)	0.039*** (0.006)	0.053** (0.024)	–0.027 (0.027)
Age²	0.00023*** (0.00006)	–0.00011 (0.00023)	0.00021*** (0.00006)	0.00038*** (0.00006)	–0.00054* (0.00029)	0.00024*** (0.00006)
Experience	0.029*** (0.008)	0.029*** (0.008)	0.021** (0.009)	0.088*** (0.022)	0.088*** (0.022)	0.085*** (0.026)
# Children < 4	0.027*** (0.008)	0.027*** (0.008)	0.014 (0.009)	0.007 (0.007)	0.007 (0.007)	0.003 (0.007)
Partner's Age	–	0.016 (0.019)	–	–	–0.059* (0.030)	–
Partner's Age²	–	–0.00013 (0.00023)	–	–	0.00018 (0.00028)	–
Partner's Experience	–	0.003 (0.016)	–	–	–0.005 (0.005)	–
Region×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Occupation & Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.382	0.382	0.390	0.448	0.449	0.476
Observations	11,575	11,575	8,219	11,568	11,568	8,212
Couples	1,886	1,886	1,539	1,889	1,889	1,541

Notes: The dependent variable for Columns (1)–(3) is the log of women's real hourly wage; for Columns (4)–(6) it is the log of men's real hourly wage. Columns (2) and (5) add partner's controls (e.g. the spouse's age, age², experience). Columns (3) and (6) add the individual's own lagged wage. Standard errors in parentheses are clustered at the couple level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. All specifications include region×year, occupation, and industry fixed effects. Columns (3) and (6) have fewer observations because individuals without t−1t-1 wage data are dropped.

In contrast, men's wage outcomes (Columns 4–6) display no comparable linkage benefit. The estimated effects are very close to zero in the first two specifications and slightly negative (though insignificant) in the lagged–wage model. This pattern indicates that husbands do not appear to gain (or lose) in terms of hourly wages when wives move into their occupation, pointing to a pronounced gender asymmetry in how occupational alignment affects pay. These results echo previous findings (e.g. Zinovyeva and Tverdostup, 2021) that highlight women's distinct advantage in gleaning informational or bargaining benefits from shared career spaces.

Beyond the treatment indicator, the control variables generally conform to the standard human capital theory. Age and experience exert positive effects on wages for both women and men, although

the magnitude declines with age squared, indicating diminishing returns to labour market tenure. The presence of a child under age four is associated with a noticeable wage penalty for women, reflecting well–established patterns of childcare responsibilities reducing female labour supply or bargaining power; for men, the coefficient on young children is smaller and often not significant. Including partner-level characteristics (such as the spouse's age and experience) has minimal impact on the main results, suggesting that changes in one spouse's human capital are not driving the wage effects attributed to occupational linkage. These control patterns reinforce the view that gendered household responsibilities and life–cycle factors remain key drivers of individual wages, while underscoring that the estimated wage premium for work–linked women is not merely a product of standard human capital variables.

Overall, these results[5] suggest that women experience a short–run wage increase of around 5.5 per cent following occupational linkage, and this gain does not seem to be driven by confounding spousal human capital changes or persistent shocks to the woman's prior earnings. Men, by contrast, exhibit no significant wage response to occupational alignment. The analysis thus underscores a noteworthy gender asymmetry. It is also consistent with the notion that women may benefit disproportionately from shared occupational expertise, possibly because they can leverage spousal information for wage bargaining or job–search strategies in ways that men, on average, do not. Overall, the main specification findings point to a meaningful wage premium for women associated with spousal occupational linkage, underscoring the importance of investigating how household structures and job choices intersect to shape gendered pay dynamics.

4.2 Robustness Tests

4.2.1 Oster's test for unobservables

Oster's (2019) test is a framework designed to assess how robust a regression estimate is to potential omitted–variable bias. The intuition behind Oster's approach is that if adding observable controls reduces the coefficient or increases the R–squared in a predictable way, then one can infer how large (and how correlated with the treatment) any unobserved variables would need to be to drive the treatment effect to zero.

Below is a concise table capturing the Oster (2019) robustness check ($\boldsymbol{\delta}$) for women's Occupational Linkage coefficient under different assumptions about the maximum possible R–squared (R–max). In each case, $\boldsymbol{\delta}$ represents how much stronger the unobserved selection would have to be—relative to the

[5] Further robustness checks regarding alternative occupational classifications, using the two-digit International Standard Classification of Occupations (ISCO) and an Occupation *x* Industry spell, combining of the two-digit Australian and New Zealand Standard Classification of Occupations 2006 Edition (ANZSCO 2006) with the one-digit Australian and New Zealand Standard Classification of Industries 2006 Edition (ANZSCI 2006), have been employed. The results are similar to the estimates presented in Table 3. Moreover, another specification defining occupational linkage using the one-digit Australian and New Zealand Standard Classification of Occupations 2006 Edition (ANZSCO 2006) is employed, yielding neither sizeable nor significant estimates. This finding suggests that women's wage gains are specific to a precise occupational match. Additionally, the main specification is also estimated adding as controls the household disposable income, the household size and individual self-assessed health condition, with the estimates of Occupational Linkage to range between 0.55 and 0.57.

observed selection—to drive the estimated effect to zero. As shown in Table 5, **δ** exceeds unity under a wide range of assumptions for R-max. The key intuition is that Oster's (2019) framework compares how much the coefficient on the treatment changes—and how much the R–squared increases—once observable controls are added, and then asks how large the unobserved factors would have to be (relative to the observed controls) to reduce the coefficient to zero. For instance, with R-max =1.0, δ is 1.43, meaning that unobserved determinants of wages would need to be about 1.4 times as influential as the observed covariates—in addition to what is already captured—to fully explain away the treatment effect. At more conservative values of R–max (e.g. 0.6 or 0.5), δ rises to 4.0 or 7.3, indicating even greater robustness. At 0.4, δ exceeds 40—an extremely high threshold for omitted–variable strength. Such consistently high δ estimates suggest that omitted–variable bias would have to be substantially stronger than the bias from the included controls to nullify the positive linkage effect for women. Consequently, these Oster results reinforce the conclusion that women's approximate 5.5 per cent wage gain is unlikely to be an artefact of unobserved confounders.

Table 5. Oster's δ for Women's Main Specification

R-max	δ
1.0	1.43115
0.8	2.11033
0.6	4.01640
0.5	7.32390
0.4	41.49499

Notes: This table reports Oster's (2019) robustness test results (δ) for the main specification estimated on women's wages. The Uncontrolled regression includes no covariates, while the Controlled corresponds to the fully specified model with region-by-wave, occupation, and industry fixed effects, as well as individual characteristics (e.g. age, experience). Each row in the table shows how large unobserved selection would need to be—relative to the observed selection—for the estimated effect to be driven to zero, under different assumptions about the maximum attainable *R*-squared. Larger values of δ indicate greater robustness: for instance, $\delta>1$ suggests that unobserved factors would have to be more influential than the observed covariates combined to eliminate the treatment effect. In practice, the high δ values observed here indicate that omitted-variable bias would need to be substantially stronger than the included controls to explain away the positive wage effect for women. Uncontrolled regression: Coefficient = 0.07300, R-squared = 0.001; Controlled regression: Coefficient = 0.05298, R-squared = 0.379

Table 6 reports Oster's δ estimates for men's main specification. In the uncontrolled regression (no covariates), the coefficient on Occupational Linkage is 0.0310 with an R–squared near 0.000; once controls are added (region–by–year, occupation, industry, and individual characteristics), the coefficient falls to 0.00524 with an R-squared of 0.448. Oster's δ then measures how strong unobserved selection would need to be—relative to observed selection—to reduce the (already small) controlled estimate to zero under different assumptions about the maximum attainable R-squared.

Table 6. Oster's δ for Men's Main Specification

R-max	δ
1.0	0.15837
0.8	0.24834
0.6	0.57500
0.5	1.67963
0.45	42.57779

Notes: This table reports Oster's (2019) robustness test results (δ) for the main specification estimated on men's wages. The Uncontrolled regression includes no covariates, while the Controlled corresponds to the fully specified model with region-by-wave, occupation, and industry fixed effects, as well as individual characteristics (e.g. age, experience). Each row in the table shows how large unobserved selection would need to be—relative to the observed selection—for the estimated effect to be driven to zero, under different assumptions about the maximum attainable R-squared. Larger values of δ indicate greater robustness: for instance, δ>1 suggests that unobserved factors would have to be more influential than the observed covariates combined to eliminate the treatment effect. In practice, the high δ values observed here indicate that omitted-variable bias would need to be substantially stronger than the included controls to explain away the positive wage effect for men. Uncontrolled regression: Coefficient = 0.0310, R-squared = 0.000; Controlled regression: Coefficient = 0.00524, R-squared = 0.448

As shown in Table 6, δ remains below 1 when R–max is set to 1.0, 0.8, or 0.6, indicating that only modest unobserved confounding (less than the strength of the observed controls) could nullify the near–zero coefficient. When R–max is lowered to 0.5, δ exceeds 1, but this is largely driven by the fact that the men's coefficient is already extremely small: even minor omitted factors could push it fully to zero or negative. At R–max = 0.45, δ jumps to 42.58, reflecting how sensitive Oster's metric can become when the controlled R-max is very close to the assumed maximum—here 0.448 vs. 0.45.

Overall, these Oster results for men underscore that the linkage coefficient is negligible in the controlled specification and can be driven to zero by relatively weak omitted variables—consistent with the regression findings that men do not experience a meaningful wage response to occupational linkage. Because the point estimate is already close to zero, it does not require powerful unobserved confounders to explain it away. Thus, for men, Oster's test reinforces that the estimated effect is not robustly positive once key controls are included. In other words, the result is already near zero, so it is trivially susceptible to even modest unobserved selection. This finding confirms that men do not experience a robust wage gain from occupational linkage.

4.2.2. Placebo Test

In this subsection, each individual's wage is replaced by a leave–one–out average wage based on other workers in the same occupation, region, and wave. This construction is intended to capture common shocks—for instance, an across–the–board wage spike affecting all employees in a given occupation or region. If the positive effect for women (or the near–zero result for men) in the main specification were merely reflecting such area–wide or occupation–wide booms that happened to align with spousal transitions, one would expect a significant and positive coefficient on Occupational Linkage in these placebo regressions.

However, as shown in Table 7, both coefficients remain small and statistically insignificant. For women (Column 1), the estimated effect is 0.009 ($p = 0.882$), and for men (Column 2), 0.038 ($p = 0.539$). These near–zero estimates strongly suggest that the main results are not driven by any localised or occupation–level wage surge coinciding with occupational linkage events. Instead, the absence of a significant placebo effect indicates that the observed wage changes in the primary analysis reflect genuine individual–level responses when one spouse moves into the partner's occupation, rather than collective shifts that affect all workers in that occupation at the regional level.

Table 7. Placebo Test Using Leave-One-Out Average Wage

	Women's Placebo	**Men's Placebo**
Occupational Linkage	0.009 (0.066)	0.038 (0.062)
Age	0.014 (0.060)	–0.048** (0.018)
Age²	0.0007*** (0.0001)	0.0004*** (0.0001)
Experience	–0.032** (0.013)	–0.040 (0.036)
Region×Year FEs	Yes	Yes
Occupation & Industry FEs	Yes	Yes
Within R^2	0.432	0.410
Observations	11,304	11,328
Couples	1,882	1,886

Notes: The dependent variable in Column (1) is the leave-one-out average wage for *women* in the same occupation, region, and wave, excluding the individual's own wage. Column (2) applies the same approach to *men*. Standard errors (in parentheses) are clustered by couple. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively. Both models include the same covariates used in the respective main specifications (e.g. age, age², experience), as well as region×year, occupation, and industry fixed effects.

By confirming that spousal transitions do not align with a concurrent wage boom for the broader peer group, the placebo test fosters the credibility of the main results. It rules out the possibility that the linkage effect is simply an artefact of being in a high–growth occupation or region at the time of transition, thereby strengthening the causal interpretation of the wage gains observed among women.

4.2.3 Event-Study Approach

A final robustness exercise employs an event–study framework grounded in the non–absorbing, heterogeneity–robust Difference–in–Differences estimator recently proposed by de Chaisemartin and D'Haultfœuille (2024). This method generalises the conventional event–study design by allowing treatment to turn on and off multiple times (non–absorbing) and by accommodating heterogeneous treatment effects across different groups and time periods. In practice, it interacts a series of leads and lags of the treatment indicator with the log wage outcome, thereby mapping how earnings evolve

relative to the spouse's occupational switch. Unlike simpler designs that assume a uniform treatment start date or a constant effect once treated, the de Chaisemartin and D'Haultfœuille's (2024) approach flexibly captures the exact timing of each switch and permits lagged and potentially decaying or cumulative treatment effects. Moreover, it overcomes certain biases that arise in standard two–way fixed–effects event–study models when treatment timing varies across units. By testing whether wages begin trending upward before the spouse's move, it also provides a check against reverse causality: if earnings were already rising in anticipation of occupational linkage, that would undermine a causal interpretation. Conversely, observing no such pre-trend and a clear post–switch jump supports the view that the wage gains are indeed triggered by the spouse's occupational entry. More specifically, a series of leads and lags of the Occupational Linkage indicator interacts with the log wage outcome, mapping how each spouse's wage evolves before and after one partner's transition. Figures 3 and 4 depict these estimated coefficients for women and men, respectively, while Table 7 reports the numerical values.

Figure 3. Event Study Estimates for Women

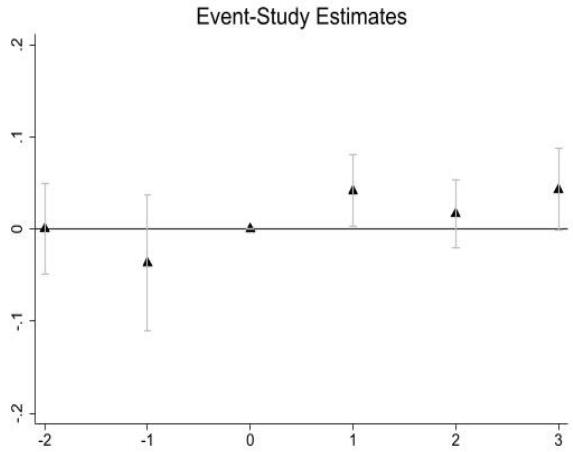
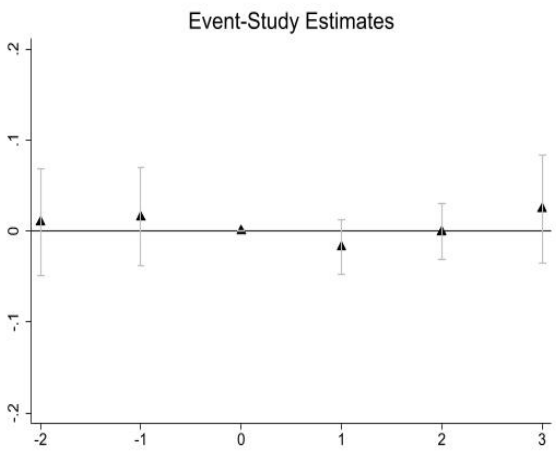


Figure 4. Event Study Estimates for Men



Notes: These figures display the event-study estimates from the de Chaisemartin and D'Haultfœuille (2024) approach, plotting the dynamic effects of a spouse's occupational switch on log real hourly wages. The horizontal axis measures time relative to the switch date, with negative leads indicating pre-treatment periods and positive lags capturing post-treatment years. The points represent the estimated wage effects in each lead or lag, and the vertical bars depict 95% confidence intervals. In Figure 3 (women), the absence of significant pre-treatment trends (t–2, t–1) and a clear jump after t+1 corroborate a causal wage gain of about five percent. In Figure 4 (men), none of the post-switch estimates differ significantly from zero, confirming the null effect.

For women, Figures 3 and Table 7 reveal that wage levels remain almost flat in the two pre-treatment years (t–2, t–1), indicating no significant upward trend before the spouse's occupational switch. At t+1, the wife's wage rises by roughly four percentage points, with similar positive estimates persisting at t+2 and t+3. The joint significance test for the post–switch coefficients yields a p–value of around 0.08, suggesting a moderate yet discernible cumulative effect. In fact, the average total effect across the post-switch years is around five percent, closely aligning with the main specification's estimate. The absence of a pre–treatment rise, coupled with an immediate and sustained post–switch jump, strongly supports a causal interpretation: wages do not begin climbing until after the spouse enters her occupation, thus reducing concerns about reverse causality. Moreover, the event study estimates for women indicate that

occupational linkage does not generate short–lived, one–shot wage effects but provides strong evidence that these positive effects last at least for up to three years.

By contrast, the men's event–study estimates in Figure 4 and Table 7 show no statistically significant changes before or after the wife's transition. The small negative coefficient at t+1 reverts slightly by t+3, but none of these effects is significantly different from zero. Hence, men exhibit no notable wage trajectory shifts linked to spousal occupational entry, mirroring the null findings in Column (6) of the main results. However, it remains questionable if the upward trend which appears after linkage is translated into sizable positive wage gains for men in the longer–run.

Table 7. Event-Study Estimates Using De Chaisemartin and D'Haultfœuille (2024) Estimator

	Women	**Men**
t+1 Effect	0.042** (0.0225)	–0.017 (0.0154)
t+2 Effect	0.017 (0.0189)	–0.001 (0.0154)
t+3 Effect	0.043** (0.0225)	0.024 (0.0302)
Joint Significance of Effects	p = 0.084	p = 0.418
Average Total Effect	0.050** (0.0227)	–0.002 (0.0223)
Placebo t-1 (pre-trend)	–0.037 (0.0376)	0.015 (0.0275)
Placebo t-2 (pre-trend)	0.00045 (0.0250)	0.010 (0.0301)
Joint Significance of placebos	p = 0.612	p = 0.810

Notes: Coefficients represent the De Chaisemartin and D'Haultfœuille (2024) estimates of wage changes ℓ years after the spouse's occupational switch (t+ℓ), relative to the pre-switch wage. Standard errors appear in parentheses. The p-values for the Joint Significance of effects refers to testing whether all post-switch effects are simultaneously zero. Similarly, "Joint Significance of placebos" tests whether the lead (pre-treatment) coefficients differ jointly from zero. "Average Total Effect" is the cumulative wage effect over all post-treatment periods. Placebo t-1, Placebo t-2 capture the coefficients on leads (pre-switch periods), helping to identify any anticipatory or reverse-causality effects.

These dynamic patterns confirm the main conclusion that women experience a short–to–medium run wage boost of around five percent, whereas men do not. The event–study design further demonstrates that wages remain flat in the lead periods, ruling out the possibility that any observed wage changes are driven by an already–underway earnings trend or by selection into the spouse's occupation based on rising wages. Moreover, the estimated average treatment effects for women and men match those from the lagged–wage specification, bolstering the notion that the wage gain for women is both robust and tied directly to the spousal occupational switch. In contrast, men exhibit no discernible wage trend before the spouse's entry into their occupation, and the estimated effects for the

first three post–transition years are statistically indistinguishable from zero. A small negative coefficient in the first year partially reverts by t+3, but never achieves significance, leaving the average total effect effectively zero. This pattern aligns with the null result in Column (6), further confirming that husbands do not systematically profit from wives joining their occupation.

In additional sensitivity analyses (see Appendix A), an instrumental variables approach leverages exogenous variation in one spouse's occupational switch driven by past peer transitions in spousal occupation. The first–stage results (Table A) confirm that the past peer transition rate strongly predicts whether a spouse moves into their partner's occupation. Despite the natural increase in standard errors, the 2SLS estimates in Table B align closely with the main findings: for women, the estimated effect of Occupational Linkage remains near 5 per cent, though lacking statistical significance, while for men it remains near zero. This corroboration further reduces the likelihood that unobserved endogeneity is driving the results since even with an exogenous instrument, the magnitude of the occupational linkage effect remains consistent with the baseline evidence.

By confirming the consistency of the estimated wage effect in a series of robustness tests, it is demonstrated that the analysis is robust to a wide range of modelling assumptions, data perturbations, and potential confounders. These checks collectively support a causal interpretation of spousal occupational linkage with a measured positive impact on the affected partner's hourly wage.

4.3 Heterogeneity Analysis

Table 8 explores how the wage premium for women under occupational linkage may vary by personal and contextual factors. Similar analyses have been conducted for male partners, and results remain neither sizeable nor statistically significant for any source of heterogeneity, in line with the main findings, thus not presented in the paper.

An initial source of heterogeneity arises from whether the woman in the couple works part–time or full–time. The estimates in column (1) indicate that part-timers experience a notably larger wage gain from occupational linkage—around 22 percentage points—compared to their full-time counterparts, whose estimated effect is at 0.043. This sizable differential is broadly consistent with evidence that part–time workers, especially women, face diminished career progression opportunities (Van Osch and Schaveling, 2017). By entering into the same occupation as the part–time spouse, the husband may facilitate more efficient knowledge transfer, skill sharing, and professional networking, thereby compensating for the structural disadvantages commonly associated with part–time work. This mechanism is particularly plausible in occupations where information can open access to higher–paying niches, better scheduling, or targeted upskilling.

For full–time employees, the benefit of spousal co–specialisation appears less pronounced. Women already working full–time may have more direct connections and resources within their field, thus

reducing the potential gains from an incoming spouse's job–specific insights and bargaining support. In other words, part–timers stand to benefit more from the spouse's occupational entry because it can bridge informational or negotiating gaps that are less critical to those already well–established full–time in the occupation. Taken together, these results indicate that how the woman participates in the labour market—in terms of hours, flexibility, or level of commitment—meaningfully shapes the wage premium derivable from the spousal occupational linkage.

A second source of heterogeneity emerges from whether the spouses possess college degrees. In Column (2), the estimated coefficients on interactions between Occupational Linkage and either spouse's degree, and especially the triple interaction term (Occupational Linkage × Man with degree × Woman with degree), indicate that women's wage gains are highest when both partners hold a college degree. This outcome aligns with the concept of power couples (Costa and Kahn, 2000), where similarly high-skilled partners share knowledge, negotiation strategies, and professional networks (Becker, 1973). Such couples may capitalise on occupational co–specialisation more effectively, pooling their human capital to navigate wage structures, uncover promotional opportunities, and leverage comparative advantages within their shared field. The high-skill synergy thereby amplifies the wage premium for women.

Women in male–dominated fields often face greater wage opacity and higher risks of underpayment (Rousille, 2024), so one might expect that spousal alignment would yield more pronounced gains there. Interestingly, the estimates in Column (3) show the opposite pattern: the wage benefit from occupational linkage grows as the female–to–male ratio increases, indicating a stronger effect in more female–balanced or female–dominated occupations. One possible explanation is that in less male–dominated environments, women may be better positioned to leverage their spouse's occupational entry—sharing negotiation tactics, comparing wage benchmarks, or accessing broader professional networks—without having to overcome deeply entrenched gender biases. In such contexts, even a marginal improvement in information or bargaining support can translate into a more tangible wage gain.

By contrast, when women are heavily outnumbered, structural barriers or cultural norms might limit the efficacy of spousal linkage. Regardless of the exact mechanism, this result underscores that how occupational alignment translates into wage benefits depends not just on individual–level traits, but also on the overall gender composition of the field in question.

Column (4) shows that women with a preschool–aged child experience a notably larger wage gain from occupational linkage than those without young children. By sharing an occupation, spouses can more easily coordinate schedules, reduce disruptions related to childcare, and engage in joint job–search or negotiation strategies. These benefits help offset the typical child penalty, indicating that co-specialisation may be especially valuable during periods of intense family obligations. In essence, aligning occupations with a spouse may alleviate some of the work–family conflicts that would otherwise curtail women's wage progression.

Table 8. Heterogeneity in Women's Wage Response to Occupational Linkage

	(1) Part-Time vs. Full-Time	(2) Woman's & Man's College Degree	(3) Occup. Gender Ratio	(4) Child <4	(5) Job Satisfaction & Partner Satisfaction
Occupational linkage	0.0430* (0.0220)	0.0127 (0.0415)	–	0.0487* (0.0261)	0.1632** (0.0657)
Part-time	0.0961*** (0.0087)	–	–	–	–
Part-time x Occupational linkage	0.1802*** (0.0571)	–	–	–	–
Man with degree	–	–0.0300 (0.0374)	–	–	–
Woman with degree	–	0.1267** (0.0498)	–	–	–
Man with degree x Woman with degree	–	0.1182** (0.0578)	–	–	–
Occupational linkage x Man with degree	–	0.1451* (0.0807)	–	–	–
Occupational linkage x Woman with degree	–	0.1729 (0.1306)	–	–	–
Occupational linkage x Man with degree x Woman with degree	–	0.1867*** (0.0661)	–	–	–
Female-to-male ratio	–	–	0.0020 (0.0047)	–	–
Female-to-male ratio x Occupational linkage	–	–	0.0230** (0.0134)	–	–
Child's age below 4	–	–	–	0.0470*** (0.0102)	–
Child's age below 4 x Occupational linkage	–	–	–	0.1198** (0.0596)	–
Satisfaction with partner	–	–	–	–	0.0156 (0.0119)
Job satisfaction	–	–	–	–	0.0425*** (0.0123)
Satisfaction with partner x Job satisfaction	–	–	–	–	0.0467*** (0.0124)
Satisfaction with partner x Occupational linkage	–	–	–	–	0.1265** (0.0502)
Job satisfaction x Occupational linkage	–	–	–	–	–0.0531 (0.0682)
Satisfaction with partner x Job satisfaction x Occupational linkage	–	–	–	–	0.0821** (0.0383)
Age	0.0420*** (0.0096)	0.0496*** (0.0096)	0.0511*** (0.0095)	0.0486*** (0.0095)	0.0514*** (0.0096)
Age²	–0.00015*** (0.00005)	–0.000246*** (0.0000578)	–0.000243*** (0.0000581)	–0.000225*** (0.0000584)	–0.000240*** (0.0000597)
Experience	0.0322*** (0.0082)	0.0300*** (0.0080)	+0.0286*** (0.0080)	+0.0293*** (0.0079)	0.0275*** (0.0079)
Region×Wave, Occupation, Industry FEs	YES	YES	YES	YES	YES
Within R²	0.393	0.382	0.381	0.383	0.383
Observations	11,575	11,575	11,575	11,575	11,091
Couples (id)	1,886	1,886	1,886	1,886	1,858

Notes: Each column corresponds to a separate specification that includes both the listed interaction terms and the baseline set of controls (age, age², experience) plus region×wave, occupation, and industry fixed effects. The dependent variable is the woman's log real hourly. Standard errors, in parentheses, are clustered by couple ID. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. (1) Compares full-time (baseline) to part-time. (2) Explores triple interactions of whether the wife and/or husband hold a college degree. (3) Interacts Occupational Linkage with the female-to-male ratio. (4) Looks at the presence of a child under 4. (5) Involves job satisfaction > 8 and partner satisfaction > 8. Data from HILDA Release 18.

Finally, the analysis reveals that women's wage gains also vary with their job satisfaction and satisfaction with their partner. Even women reporting high baseline satisfaction see a modest boost from occupational linkage, suggesting that alignment with a spouse can reinforce existing synergies at home and work. By contrast, those who begin with lower satisfaction levels display an even larger improvement, implying that there is greater scope for positive change when spouses co–specialise—perhaps because occupational alignment tackles unmet needs such as insufficient career support or wage transparency. These findings resonate with broader sociological and psychological research emphasising how spousal support and emotional well–being intersect with labour–market outcomes (Hennecke and Hetschko, 2025; Halbesleben et al., 2010). In short, closer occupational and relational ties appear to foster a supportive environment that can elevate women's earnings potential, particularly when initial satisfaction is low and the margin for gains is highest.

5. Mechanisms

The previous sections have established a robust wage effect for women under occupational linkage, but these estimates do not clarify why such a gain arises. To investigate possible channels, a two–stage fixed–effects partial mediation analysis has been implemented. In the first stage, each hypothesised mediator is regressed on Occupational Linkage and the same controls used in the main specification. The predicted portion of that mediator, reflecting only the component explained by the treatment, is then included in the second–stage wage equation. This design is conceptually similar to formal causal mediation approaches (e.g., Imai et al., 2010), although it is applied here in a more tractable, stepwise manner. A key reason for incorporating predicted mediators rather than the raw variables is to address potential endogeneity: if the mediator itself (for instance, life satisfaction) also responds to wage changes or unobserved factors, the partial mediation estimates would be confounded. Using only the treatment–induced component of each mediator in the wage equation helps isolate whether that mechanism genuinely explains part of the linkage effect.

Table 9 summarises the second–stage results for each mechanism, listing the estimated coefficient on Occupational Linkage under seven specifications: the "Baseline" with no mediator and five variants in which each mediator is introduced one at a time. In each variant, the mediator's predicted values are included in the wage equation, while the same couple–level fixed effects, regional/time controls, occupation dummies, and industry dummies remain in place. Standard errors are clustered by couple, and the within R-squared (about 0.38) is consistent across specifications.

Consistent with theories that underscore the non-pecuniary dimensions of work linkage (Hennecke and Hetschko, 2025; Halbesleben et al., 2010), life satisfaction emerges as a salient factor. When the portion of a woman's life satisfaction explained by co–specialisation is accounted for, the Occupational Linkage coefficient drops notably (from 0.056 to 0.025). This shift suggests that emotional or

motivational benefits—such as day–to–day support, shared professional values, or heightened engagement—may underlie a substantial part of the wage premium.

Table 9. Partial Mediation Estimates for Occupational Linkage

Mechanism	Estimate
Baseline (Main Estimate)	0.056** (0.027)
Wellbeing (Life Satisfaction)	0.025 (0.031)
Skills/Knowledge (Experience Difference)	0.050* (0.027)
Hours Coordination (Working Hours Difference)	0.057** (0.027)
Bargaining (Partner's Bargaining Power)	0.032 (0.030)
Bargaining (Observed Gender Pay Gap)	0.046* (0.027)
Control Variables	Age, AgeÂ², Experience, Number of children <4, Region—Wave FEs, Occupation FEs, Industry FEs
Within R^2	0.38
Observations	11,575
Couples	1,886

Notes: Each column reports the estimated coefficient on Occupational Linkage (with clustered standard errors in parentheses) from a partial-mediation, two-stage fixed-effects approach. In the first stage, the relevant mechanism (e.g., life satisfaction) is regressed on the treatment indicator and control variables; the predicted values are then included in the second-stage wage equation. "Baseline" denotes the main specification without any mechanism. Rows labelled "Wellbeing," "Skills/Knowledge," "Hours Coordination," and "Bargaining" show how the effect of occupational linkage changes once the respective mediator is accounted for. All regressions include age, age², experience, number of children under four, region×wave fixed effects, occupation fixed effects, and industry fixed effects. Life Satisfaction measures the overall subjective well-being. Experience Difference captures the difference in years of experience between spouses, used as a proxy for potential skills/knowledge transfer. Hours Coordination is the absolute difference in working hours between spouses, intended to proxy for work–family coordination. The partner's Bargaining Power is the husband's prior wage advantage relative to men in the wife's occupation (constructed as the log difference). Observed Gender Pay Gap is defined as the log difference between average male and female wages in the wife's occupation. The within R-squared is approximately 0.38 in each specification, and the sample comprises 11,575 observations from 1,886 couples. Statistical significance at the 1%, 5%, or 10% level is indicated by ***, **, *, respectively. Both the first–stage (mechanism) regressions and the second–stage wage regressions are reported in the Appendix.

Another channel relates to bargaining power. Drawing on wage bargaining frameworks (Brenzel, Gartner, and Schnabel, 2014; Hall and Krueger, 2012) and the role of wage transparency (Rousille, 2024; Säve-Söderbergh, 2019), the husband's wage advantage is measured as the last wage rate in the previous occupation relative to men's average wage in his wife's occupation. Including this proxy attenuates the linkage coefficient (to 0.032), consistent with arguments that female partners benefit from a spouse's negotiation strategies or salary benchmarks. However, controlling for an observed occupation–level gender pay gap has a more modest impact, suggesting that while broad awareness of

male–female wage disparities matter (Blau and Kahn, 2017), it may not be the central driver in our setting.

Skills and knowledge transfer—a major focus of co–working literature (e.g., Mansour and McKinnish, 2018; Janning, 2006)—shows only a marginal mediating effect when captured by simple experience differences between spouses. This limited shift does not necessarily dismiss the broader notion of human capital spillovers (Becker, 1973); rather, it may reflect those intangible insights, networks, and on–the-job referrals cannot be fully proxied by raw experience measures. Similarly, hours coordination (Bryan and Sevilla, 2017) brings no change in the linkage coefficient, at least when operationalised by the absolute difference in spouses' working hours. Data constraints—such as incomplete information on childcare time or home production—further complicate the measurement of scheduling synergies.

The interpretation of these results should be approached with caution. Partial mediation cannot rule out reciprocal causation if wage changes alter the mediators. Some hypothesised mechanisms—such as skill spillovers—are inherently difficult to observe directly in survey data. The proxies (e.g., experience differentials, job satisfaction) are coarse and may only partially capture the underlying constructs. These limitations are not unique to this setting. As noted in Imai et al. (2010) and Heckman and Pinto (2015), mediation analysis is sensitive to proxy quality and measurement error. The goal of this section is not to offer a full causal decomposition but rather to identify suggestive correlates of the observed wage effects. Nonetheless, two plausible mechanisms emerge clearly. First, improvements in the woman's subjective well–being, reflecting psychological and motivational benefits from sharing an occupation, appear to be the most influential mediator. Second, bargaining power, particularly the husband's prior wage advantage, also accounts for a nontrivial portion of the effect. Together, these findings highlight how the interplay of emotional support, informational gains, and negotiation strategies can help explain why occupational linkage yields wage benefits for women.

6. Conclusion

This paper examines whether one spouse's entry into the other's occupation meaningfully affects the wages of the partner who remains in place. Drawing on data from the Household, Income and Labour Dynamics in Australia (HILDA) survey (2001–2018) and exploiting within–couple variation, the analysis shows that women gain an average wage increase of five to six per cent when their husbands move into their occupation, whereas men do not experience a similar boost when their wives switch. A series of robustness checks—including placebo regressions, an event–study framework, and an instrumental variable design leveraging peer transitions—confirms that this result is unlikely to be driven by endogeneity or common shocks, while the positive gains for women last, at least, for up to three years.

These findings fill an important gap in the coworking–couples literature, complementing earlier evidence on non–pecuniary benefits (Hennecke and Hetschko, 2025; Ferguson et al., 2016; Halbesleben, Wheeler, and Rossi, 2012; Halbesleben et al., 2010) with a clear focus on wage outcomes. They also confirm Zinovyeva and Tverdostup's (2021) evidence that women working in the same firm with their husbands earn more than observationally similar women. Yet, in this setting, men do not appear to benefit monetarily when their wives enter their occupations. While the underlying mechanisms behind this gender asymmetry likely include differences in negotiation styles, wage benchmarking, and work–family dynamics, our analysis suggests that men's wages remain relatively insensitive to co–specialisation in this context—implying that women stand to gain disproportionately from shared occupational paths.

Turning to why these wage gains materialise for women, the partial mediation analysis points to a combination of psychological and bargaining factors. First, life satisfaction emerges as a pivotal channel: sharing an occupation can increase motivation, engagement, and day–to–day support, all of which contribute to higher pay. Second, women may capitalise on the husband's wage bargaining position or salary benchmarks, revealing a bargaining–power transmission channel. Although skill spillovers and hours coordination prove less influential under our specific measures, they cannot be ruled out entirely—particularly given data limitations that may mask subtler dimensions of knowledge sharing and scheduling synergies.

These results carry several implications for labour–market policies and organisational practices. Encouraging flexible structures that allow partners to coordinate job tasks, schedules, or career development could be especially advantageous for women seeking to address persistent pay gaps. For instance, firms might design mentorship or joint–hiring programs that leverage spousal knowledge transfers without inadvertently discriminating against dual–career couples. Additionally, policies promoting wage transparency—be it through legislative mandates or organisational norms—might amplify the bargaining benefits women already appear to extract under co–specialisation. Although occupational alignment is not a one–size–fits–all solution, recognising its potential to enhance female earning power provides a new angle for discussions around gender equality and workplace innovation.

Three considerations warrant caution in interpreting these findings. First, the analysis relies on Australian data, and while the within–couple fixed–effects design fosters internal validity, it remains uncertain how the results generalise to other institutional settings with different labour–market and family norms and structures. Second, although the study leverages a long panel, the fraction of couples meeting the strict definition of treatment (one spouse moving into the other's occupation) is relatively small. The checks undertaken mitigate many small–sample concerns, but more extensive treatment events could enable deeper investigation into subgroup variation and longer–term outcomes. Third, the mechanisms explored—bargaining power, well–being, skill spillovers, and hours coordination—cannot fully capture intangible forms of knowledge exchange or subtle household dynamics that might shape

wages. While the partial–mediation approach offers insights, some elements of intra–household synergy remain unmeasured.

Future research might clarify how firm–level coworking differs from broad occupational overlap, given that couples employed by the same organisation could be subject to distinct managerial practices, pay structures, or direct peer evaluations. Distinguishing these same–firm interactions from the simpler alignment of occupations would help isolate firm–specific mechanisms—such as employer favouritism or uniform wage tiers—that might accelerate wage convergence. Beyond this, it would be valuable to track long–term trajectories, examining whether early wage gains expand through promotions or job mobility or whether co-specialisation simply generates a short-term boost. Mapping the career evolution of work–linked couples would reveal if women's initial advantages persist, grow, or even erode over time, or whether men are affected in the longer-term by this occupational alignment. Lastly, household dynamics deserve closer scrutiny: examining the ongoing allocation of resources, bargaining outcomes, and division of responsibilities within the household would shed light on how co-specialisation reshapes broader family economics.

Taken together, these results highlight an underexplored channel for improving women's wages, offering new insight into how spousal job choices can influence gender pay dynamics. By revealing that women can boost their wage rates through co–specialisation, this study underscores the importance of household considerations in wage determination and contributes to a broader understanding of how workplace peer effects operate even within a marriage. In the broader trajectory of household and labour economics research, these insights reinforce the need to integrate family decision–making and gender–specific negotiation strategies into analyses of wage outcomes—thereby advancing our grasp of why gender wage gaps persist and what novel policies or organisational practices might help narrow them.

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APPENDIX

A.1 Instrumental Variables

An instrumental variables strategy is adopted that exploits peer transition rates among individuals who share similar age, region, and broad occupational background with the spouse. A fraction of these peers who switched occupations in the previous wave is calculated, thereby indicating how many comparable workers have recently changed their roles. Observing that more peers have transitioned is assumed to reduce uncertainty and signal better job prospects, which raises the probability that the spouse will also move. Meanwhile, the partner's wage is not expected to be influenced by the peer transition rate unless the spouse's move occurs, thus satisfying the exclusion restriction. Region-by-wave fixed effects and occupation fixed effects are included to capture local economic conditions and broader wage trends, ensuring that variation in the spouse's occupational move arises primarily from these peer dynamics. In the first stage, higher past peer transition rates are shown to strongly predict the spouse's upcoming entry into the other partner's occupation; in the second stage, the predicted probability of spousal movement is employed to explain wage variation that is exogenous to unobserved household-level confounders. This approach thereby further addresses any residual endogeneity concerns.

Table A displays strong and highly significant coefficients on the past Peer Transition Rate for both women and men, far exceeding 1.0, indicating that the instrument is strongly related to the probability of spousal movement. Other controls—such as age, age², and experience—play a lesser role in determining the spouse's occupational transition, reinforcing that peer dynamics drive much of the treatment variation.

Once Occupational Linkage is instrumented, the 2SLS point estimates (Table B) remain similar in magnitude to the main fixed-effects results but, as expected, show larger standard errors. For women, the coefficient on Occupational Linkage is approximately 0.05, highly consistent with the OLS estimate of around 0.05–0.06. Although the estimate is not statistically significant, its alignment with the baseline results indicates that this is a genuine causal effect that is simply harder to detect with 2SLS. For men, the IV coefficient remains small (–0.03) and statistically indistinguishable from zero, mirroring the baseline null result. While IV designs can never fully guarantee exogeneity, the strong first-stage and plausible exclusion restriction argue that peer transitions represent a valid source of variation in spousal occupational changes. Coupled with the other robustness checks—Oster's delta analysis, placebo tests, and event studies—this IV evidence supports the conclusion that women's wage gains under occupational linkage are unlikely to reflect omitted variables or reverse causality. Instead, they appear to capture a genuine causal relationship, albeit one that is less precisely estimated when using an instrument instead of a direct within-couple variation.

Table A. First-Stage Regressions

	Women	**Men**
Age	–0.00281	0.00030
	(0.00213)	(0.00183)
Age²	–0.00001	0.00001
	(0.00002)	(0.00002)
Experience	0.00102	0.00585
	(0.00139)	(0.00452)
# Children < 4	–0.00393	0.00308
	(0.00287)	(0.00294)
Peer Transition Rate	**1.32048*** **	**1.46064*** **
	(0.23182)	**(0.24268)**
Region×Wave FEs	Yes	Yes
Occupation & Industry FEs	Yes	Yes
Within R^2	0.063	0.073
Observations	11,329	11,325
Clusters (id)	1,872	1,875

Notes: The dependent variable in Column (1) is the indicator that the husband moves into the wife's occupation (while the wife stays put). In Column (2), the dependent variable indicates that the wife moves into the husband's occupation. Peer Transition Rate is constructed by tracking similar-age, same-region, broad-occupation peers and measuring their propensity to switch occupations in the previous wave. Standard errors (in parentheses) are clustered by couple. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively. Additional fixed effects (region×wave, occupation, industry) are included but omitted here for brevity.

Table B. 2SLS Regressions

	Women	**Men**
Occupational Linkage	0.051	–0.031
	(0.138)	(0.123)
Age	0.050***	0.041***
	(0.010)	(0.007)
Age²	–0.00024***	–0.00040***
	(0.00006)	(0.00006)
Experience	0.033***	0.094***
	(0.008)	(0.023)
# Children < 4	0.027***	0.007
	(0.008)	(0.007)
Region×Wave FEs	Yes	Yes
Occupation & Industry FEs	Yes	Yes
Within R^2	0.381	0.447
Observations	11,329	11,325
Clusters (id)	1,872	1,875

Notes: The endogenous regressor, Occupational Linkage, is instrumented using the spouse's lagged peer transition rate, as described in the first stage (Table A). Standard errors (in parentheses) are clustered by couple. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. All specifications include region×wave, occupation, and industry fixed effects, as well as individual controls (age, age², experience, and children under 4).

A.2 Two-Stage Fixed Effects for Partial Mediation

Table C. First-Stage Fixed Effect Regressions on Mediators

	Life Satisfaction	Experience Difference	Hours Coordination	Partner's Bargaining Power	Gender Pay Gap
Occupational Linkage	0.069 (0.078)	0.007 (0.019)	0.048 (0.822)	0.023 (0.026)	–0.010 (0.016)
Age	–0.072* (0.042)	0.957*** (0.012)	2.166*** (0.455)	0.049*** (0.011)	0.022*** (0.007)
Age²	0.0006*** (0.0001)	–0.00003 (0.00008)	–0.016*** (0.002)	–0.0003*** (0.00007)	–0.00001 (0.00003)
Experience	0.014 (0.019)	–0.983*** (0.006)	–0.935*** (0.232)	–0.009 (0.007)	–0.005* (0.026)
# Children < 4	–0.051** (0.024)	–0.003 (0.008)	4.659*** (0.279)	0.016* (0.009)	0.001 (0.003)
Region×Year FEs	Yes	Yes	Yes	Yes	Yes
Occupation & Industry FEs	Yes	Yes	Yes	Yes	Yes
Within R^2	0.048	0.920	0.154	0.142	0.287
Observations	11,573	11,575	11,575	8,219	11,575
Couples	1,886	1,886	1,886	1,539	1,886

Notes: This table reports the first-stage fixed-effects regressions for the proposed mediators in the female sample. In each specification, the dependent variable is one of the mediators through which the wage effect of occupational linkage is hypothesized to operate. The first column presents the regression where the dependent variable is the woman's own life satisfaction. The second column examines the difference in years of experience between spouses, serving as a proxy for skills or knowledge transfer. The third column uses the absolute difference in working hours between spouses as a measure of work–family scheduling alignment. The fourth column estimates the partner's bargaining power using a proxy constructed from the husband's prior wage advantage; note that this regression is estimated on a subsample (8,219 observations from 1,539 couples) due to data limitations arising from the time-lag nature of this variable. The fifth column reports the regression for the observed gender pay gap, defined as the log difference between average male and female wages in the wife's occupation. Standard errors in parentheses are clustered at the couple level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. All specifications include region×year, occupation, and industry fixed effects.

Table D. Women's Wage Rates Controlling for Predicted Mediators

	(1)	(2)	(3)	(4)	(5)
Occupational Linkage	0.0249 (0.032)	0.050* (0.027)	0.057** (0.027)	0.032 (0.030)	0.046* (0.027)
Age	0.081*** (0.01872)	–0.641* (0.365)	0.104*** (0.029)	0.0002 (0.028)	0.069*** (0.013)
Age²	–.00053*** (0.00017)	–0.0002*** (0.00006)	–0.0006*** (0.0002)	0.0001 (0.0002)	–0.0002*** (0.00006)
Experience	0.02247*** (0.00861)	0.738** (0.374)	0.005 (0.014)	0.038*** (0.009)	0.0240*** (0.008)
# Children < 4	0.04990*** (0.01446)	0.029*** (0.007)	0.146** (0.063)	0.011 (0.011)	0.027*** (0.007)
Life Satisfaction	0.45125* (0.23815)	–	–	–	–
Experience Difference	–	0.721* (0.380)	–	–	–
Working Hours Difference	–	–	–0.025* (0.013)	–	–
Partner's Bargaining Power	–	–	–	0.996* (0.526)	–
Gender Gap	–	–	–	–	–0.896* (0.473)
Region×Year FEs	Yes	Yes	Yes	Yes	Yes
Occupation & Industry FEs	Yes	Yes	Yes	Yes	Yes
Within R²	0.38	0.38	0.38	0.38	0.38
Observations	11,575	11,575	11,575	11,575	11,575
Couples	1,886	1,886	1,886	1,886	1,886

Notes: Each column reports the estimated coefficients from a second-stage fixed-effects regression of the log of women's real hourly wages after including the predicted value of a mediator. The mediators are obtained from the first-stage regressions presented in Table C. Standard errors in parentheses are clustered at the couple level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. All specifications include region×year, occupation, and industry fixed effects.