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Family and employment: The impact of marriage and children on labour market outcomes

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FACULTÉ DES SCIENCES SOCIALES ET POLITIQUES

INSTITUT DES SCIENCES SOCIALES

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THÈSE DE DOCTORAT

présentée à la

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pour l'obtention du grade de

Docteur ès Sciences Sociales

par

Patrick McDonald

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Jury de thèse
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IMPRIMATUR

Le Décanat de la Faculté des sciences sociales et politiques de l'Université de Lausanne, au nom du Conseil et sur proposition d'un jury formé des professeurs

- Daniel OESCH, directeur de thèse, Professeur à l'Université de Lausanne
- Stephanie STEINMETZ, Professeure à l'Université de Lausanne
- Katrin AUSPURG, Professeure à l'Université de Munich
- Marlis BUCHMANN, Professeure à l'Université de Zurich
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autorise, sans se prononcer sur les opinions du candidat, l'impression de la thèse de Monsieur Patrick McDonald, intitulée :

« Family and employment: The impact of marriage and children on labour market outcomes »

Marie SANTIAGO DELEFOSSE
Doyenne

Lausanne, le 21 avril 2020

Summary

This thesis studies the impact of marriage and children on wages, as well wage expectations by gender and family situation. As its principal source of empirical analysis, it uses data from a factorial survey (LIVES-JOBVUL) amongst Swiss employers and recruiters. The experimental design allows for the analysis of the impact of discrimination of employers on wages, an often theorised but empirically under-studied mechanism behind wage differences between groups. To complement this analysis it uses data from nationally-representative panel surveys (Swiss Household Panel, Swiss Labour Force Survey), both to contextualise the findings of the factorial survey results and to provide results from the supply (employee) side of the labour market. Results show that for men, marriage is associated with a wage premium that is largely explained by selection of more productive men into marriage, but with some effect of productivity improvements and employer preferences. For women, motherhood is associated with a wage penalty that is impacted by employer discrimination, for young mothers especially. This discrimination is shown to stem from all types of recruiters irrespective of their own gender and family situation, though women without children hand out slightly higher penalties than others. Finally, using data from the German Socio-Economic Panel Innovation Sample, expected wages and their link to actual wages is investigated, with results indicating that gender gaps in expected wages are partly linked to different types of work and family situations. In sum, the thesis concludes that gender gaps persist on the Swiss labour market, though not uniformly across occupations, and are the result of multidimensional mechanisms linked to both labour supply and demand.

Résumé

Cette thèse étudie l'impact du mariage et des enfants sur les salaires, ainsi que les attentes salariales selon le sexe et la situation familiale. Comme principale source d'analyse empirique, elle utilise les données d'une enquête factorielle (LIVES-JOBVUL) auprès des employeurs et des recruteurs suisses. La conception expérimentale permet d'analyser l'impact de la discrimination des employeurs sur les salaires, un mécanisme souvent théorisé mais empiriquement sous-étudié derrière les différences de salaires entre les groupes. Pour compléter cette analyse, elle utilise des données provenant d'enquêtes par panel représentatives au niveau national (Panel suisse des ménages, Enquête suisse sur la population active), afin de contextualiser les résultats de l'enquête factorielle et de fournir des résultats du côté de l'offre (employés) du marché du travail. Les résultats montrent que pour les hommes, le mariage est associé à une prime salariale qui s'explique en grande partie par la sélection d'hommes plus productifs dans le mariage, mais avec un certain effet d'amélioration de la productivité et des préférences des employeurs. Pour les femmes, la maternité est associée à une pénalité salariale qui est influencée par la discrimination de l'employeur, en particulier pour les jeunes mères. Il est démontré que cette discrimination provient de tous les types de recruteurs, indépendamment de leur propre sexe et de leur situation familiale, bien que les femmes sans enfants infligent des pénalités légèrement plus élevées que les autres. Enfin, à l'aide des données de l'échantillon d'innovation du panel socio-économique allemand, les salaires attendus et leur lien avec les salaires réels sont étudiés, les résultats indiquant que les écarts entre les sexes en matière de salaires attendus sont en partie liés à différents types de situations professionnelles et familiales. En résumé, la thèse conclut que les écarts entre les sexes persistent sur le marché du travail suisse, même s'ils ne sont pas uniformes d'une profession à l'autre, et qu'ils sont le résultat de mécanismes multidimensionnels liés à la fois à l'offre et à la demande de travail.

Acknowledgements

I initially began my thesis with the phrase “nothing happens in isolation”, a statement that was both intended to convey my feelings about the PhD experience and one that has taken on an entirely new definition in the past few months. Still, I stand by the original sentiments and with that in mind would like to pay tribute to a few people without whom this dissertation would not have been possible.

If all PhD students had Daniel Oesch as their supervisor, academia would undoubtedly be a better place. Your combination of encouragement, patience, intellect and good humour have made me a far better researcher than I thought I could be, and I have nothing but appreciation for the time and energy you have devoted to my academic development.

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I was lucky enough to benefit from a semester as a guest researcher at the University of Munich during my time at the University of Lausanne. For that, I am grateful both to the Institute of Social Sciences in Lausanne for their generosity in allowing and funding such an opportunity, as well as to Katrin Auspurg’s Chair of Quantitative Social Research for welcoming me. Not only was it a highly valuable research experience, it was also an excellent opportunity to meet new people and strengthen ties with old colleagues. In particular, my time in Munich was

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1. Introduction

This thesis delves into two work-family constellations: the labour market outcomes of married men and women with children. Both cases have been extensively analysed in the sociological, economic and social psychology literature, usually with the use of large-scale workforce and/or panel surveys. These observational studies provide a solid basis for our understanding of the two situations – namely, that marriage coincides with higher wages for men, while the arrival of children is linked to a weakening of women's attachment to the labour market and a decrease in wages relative to women who remain childless. The innovation of this thesis is to build on observational data from the labour supply-side of workers and to combine it with new survey experiment data on the labour demand-side of employers: the impact of recruiters on wage penalties and premiums for mothers and married men respectively. The experimental nature of these data provides strong internal validity and prises open the black box of unexplained wage differences: the role of employer discrimination. The use of national panel data provides external validity to findings and contextualises the extent of employer discrimination in the overall wage gap. Finally, a study of the preferences of workers themselves, going beyond an analysis of observed wages alone, completes a multidimensional picture of the intersection of family dynamics and labour market outcomes.

The research presented in this thesis is motivated by both methodological and theoretical contributions to the scholarly discussion around wage differentials based on personal and family characteristics. While a number of well-established theories are put forward to explain why it is that such dynamics exist, they have in some respects not kept up with sweeping societal change and often reflect outdated notions of family roles and labour market participation. Moreover, institutional factors are not always taken into account, and supply-

and demand-side factors, while theorised, have proven difficult to disentangle. In this thesis, I will provide new theoretical avenues that better reflect current circumstances, account for the impact of factors at the policy, occupation and employer level, and can assess the importance of mechanisms on the employee and employer side of the labour market. The use of several data sources and analytical strategies allow for a complete empirical exploration of these theories and underscore the importance of appropriate, sound methodology for approaching multifaceted social phenomena.

In this introductory chapter I will proceed by first giving a general overview of the literature relating to male marriage premiums and motherhood wage penalties in order to position this thesis in the academic discussion surrounding these two questions. This is followed by a presentation of the labour market context in Switzerland. Switzerland is the subject of most of the empirical work of this thesis and a further important contribution it makes is providing evidence on the link between family roles and wages in the country – a continuing and animated debate in Swiss society. I then move to a discussion of the data and analytical strategy that links the four empirical chapters, with a particular focus on an outline of factorial survey analysis, the principal tool used in this thesis to analyse the questions of both employer discrimination and preferences of workers. I will further explore how it is well-suited to the analysis of labour market questions, the recruitment process in particular. Finally, I will conclude with a summary of the four empirical chapters of the thesis and their principal results.

1.1 Theoretical perspectives on employment, marriage and children

Until the end of the Second World War, family units and their interactions with the labour market took on a multitude of differing forms within and between Western societies. It was only in the post-war years that relative peace and strong economic growth gave rise to a

hegemonic model of full, stable employment for men, with women primarily perceived as housewives and child-rearers – the "male breadwinner" model (Esping-Andersen 1999). In two influential papers, Gary Becker (1973, 1974) sketches the economic consequences of this paradigm, both within and outside of the family sphere. Individuals in a couple, in Becker's reckoning, specialise in either paid or domestic work. Concretely, upon marriage, men can focus their productivity on earning a wage, while women put their productivity into keeping house and raising children – maximising the productivity and returns of each in the two individual domains, rather than both spreading their efforts across two different tasks that have little in common with one another. It is logical, then, that a married man will make more money than a single one who still has to tend to domestic tasks after work. Conversely, a woman with children will have less productivity leftover to give to a paid job, and therefore will earn less than her single, childless counterpart. Overall, this situation results long-term in entrenched wage gaps both between men and women as well as between childless women and mothers.

Becker's theory may seem antiquated from a 21st-century viewpoint, but the productivity hypothesis forms one of the key explanations of wage gaps that have persisted until today despite continually rising labour market participation of women, mothers in particular. The other key explanations are, in the case of married men, selection, in the case of mothers, compensating differentials, and, in both cases, employer discrimination, be it positive or negative.

1.1.1 Productivity, selection and the male marriage premium

While marriage may result in men being able to do less housework and therefore devote more energy to their jobs, further effects of marriage on men's productivity are theorised in the literature. A key thesis here is that of marital responsibility: once married, a man will see it as

his duty to provide for his family and thus change his behaviour as to be more productive at work (Thompson and Walker 1989). This may manifest itself in a willingness to work longer hours, an acceptance of less-than-favourable conditions, or in increased dedication to work tasks in order to gain promotions and/or wage rises. Outside the office, behaviour may change in a way that improves productivity at work: fewer late nights spent at bars or indulging in risky behaviour, for instance (Schwartz 1990). Productivity may also improve thanks to the fact that married men are, on average, happier and healthier than single (Kiecolt-Glaser and Newton 2001) and may therefore take fewer sick days and be more reliably productive than unmarried. These factors should be expected to, all else being equal, result in higher wages and better chances on the labour market for married men than single.

A second theoretical proposition for why married men may be better off than single men on the labour market argues that rather than – or in addition to – marriage making men more productive, it is in fact more productive men who select into marriage (Killewald and Lundberg 2017, Ludwig and Brüderl 2018). This argument assumes that the characteristics that make good workers also make good husbands: better educational credentials, safe employment and good prospects for career progression, developed social networks, and high levels of health and wellbeing are important characteristics on the marriage market (Hakim 2000, Blossfeld and Timm 2003, Schwartz and Mare 2005) as on the labour market. Under this theory, it is men who are already on a steeper career trajectory or those in higher paying, more prestigious jobs who will ultimately find themselves in marriages. However, while the mechanisms may differ, the outcome is the same: married men will earn more and have better career prospects than single. Moreover, the two are not mutually exclusive: marriage may result in an already steep wage trajectory becoming steeper, or a jump into a higher category, augmenting an already existing gap.

The two mechanisms detailed above have been the subject of substantial empirical analysis. In earlier times results of such studies primarily tended towards a confirmation of the productivity hypothesis. Korenman and Neumark (1991) analyse wage and performance review data from a single firm and find that after marriage men's wages grow faster, mainly because they are promoted faster and are therefore more concentrated in managerial positions. Moreover, their performance reviews are systematically better than single men's, suggesting they are indeed more productive at work. Gorman (1999) uses the National Longitudinal Survey of Youth to investigate the premium, hypothesising that marriage changes men's behaviour to make them more sensitive to promotion opportunities and therefore pushes them to be more productive – a hypothesis that is confirmed by the empirical analysis, which shows that married men are more attuned to opportunities to increase their wages both with a current employer and by seeking new job opportunities, while becoming more risk-averse and less likely to quit a current job without a guarantee of a new one. Some recent evidence, such as the analysis of Killewald and Gough (2013), confirms this change in behaviour improves the wages of married men.

More recent evidence, however, has generally shown the selection effect accounting for up to and beyond half of the wage difference between single and married men. In a study of Norway, Petersen et al. (2011) find that about 50% of the male marriage premium is present before marriage, mostly because men who will eventually marry sort into high-earning occupations well before marriage. Killewald and Lundberg (2017), using the 1979 National Survey of Youth, more forcefully argue that any wage premium is due to men marrying while they are already on an increasing wage trajectory. Finally, Ludwig and Brüderl (2018) also study the National Longitudinal Survey of Youth, adding the innovation of accounting for selection into differing wage trajectories as well as differing wage levels. They find that this addition

accounts for almost the full premium, arguing therefore that the male marriage premium can almost entirely be explained by selection of men with better earnings prospects into marriage, with little to no effect of improved productivity upon marriage.

While this thesis will test these two theories in the context of the Swiss labour market, its unique contribution to the academic discussion concerning this topic concerns a third theory: that of employer preferences. This theory suggests that part of the difference between married and unmarried men may stem from a preference among employers for the former, expressed primarily by higher wages. There are several reasons to imagine that employer preferences are important for male marriage premiums. Employers may trust that married male workers are more productive and reliable than their single counterparts for the aforementioned reasons (Schwartz 1990) – often referred to as *statistical discrimination* (see Bielby and Baron 1986 for an example). Another possibility may be that if recruiters and employers are mostly married men themselves, they may prefer to hire people more similar to themselves, since they may not have concrete, reliable information on the productivity of the candidates they are presented – a process influenced by *homogamy* (McPherson et al. 2001). Finally, social mores and expectations in a given context, such as continuing conservative norms that men remain the primary earners – if not the sole breadwinners – for the family (Levy 2013, Valarino and Gauthier 2016) may push employers to pay a married man, expected to be supporting at least a wife if not children, more than a single man who must only support himself.

These mechanisms are difficult to measure in a standard observational setting. Indeed, while the male marriage premium has been well-documented in the literature, there remains a gap in the research concerning employers' impacts on the premium. This thesis will address this research gap by measuring the male marriage premium in Switzerland using nationally

representative panel data, followed by investigating the impact of employer preferences through the use of a survey experiment amongst recruiters and employers themselves.

1.1.2 Motherhood wage penalties

Important family events may have a positive effect on men's labour market situation, wages in particular, but for women the story is more mitigated. Historically subject to wage gaps based on gender (Blau and Kahn 1996, 2000, Weichselbaumer and Winter-Ebmer 2005, Ridgeway 2011), motherhood is associated with a within-gender wage gap in most countries (Waldfogel 1997, 1998, Budig and England 2001, Gangl and Ziefle 2009, Harkness 2016), with the exception of Scandinavian countries (Gash 2009, Petersen et al. 2014).

Unlike for married men, who the literature posits select into marriage, the principal economic explanation for this motherhood wage penalty has always been solely related to productivity and human capital. On the one hand, having children has historically meant a career break for women, often long-term (Gangl and Ziefle 2009, arresting the accumulation of human capital through continuous work experience. This in turn hinders the possibility of promotion to higher-paying roles, which often require a record of uninterrupted work and a willingness to work long hours at times not conducive to work and family harmonisation (Weeden et al. 2016). Mothers who return to work may also, following Becker's theory of family economics, have less productivity to dedicate to their jobs than those without children, further compounding the productivity effect.

On the other hand, mothers often also cluster in roles that start reasonably well-paid, do not induce large penalties for time out of the market and can easily be done on reduced and/or flexible hours (Polachek 1981, 1984). Occupations such as teaching, nursing and clerical work

– all of which feature an overrepresentation of women in most labour markets (Charles and Grusky 2004) – fit these criteria in many cases, though the evidence that mothers choose these jobs more than women in general is contested (England 2005). Such jobs, on the other hand, often have flatter earnings profiles than other jobs that require more flexibility and more commitment in terms of time, meaning that the wages of workers in such occupations are likely to be more stagnant, and that mothers' wages, across the career, are more likely to be lower, though possibly more predictable, on average than those of women without children.

As with the marriage premium for men, empirical evidence has demonstrated that a motherhood penalty of up to 10 per cent per child, controlling for productivity, human capital and occupational decisions. Depending on the country, the penalty remains either stubbornly high or disappears altogether when such controls are introduced, with an average of approximately half explained. For the US, Waldfogel (1997) uses the National Longitudinal Survey of Young Women for years 1968-1988, in one of the early examples of using fixed-effects models to avoid spurious correlations between motherhood and wages stemming from unobserved heterogeneity. After controlling for work experience, marital status and ethnicity she finds penalties of 4-6% for mothers of one child, increasing to over 10% for mothers with two or more children. Budig and England (2001) build on this study using the National Longitudinal Survey of Youth from 1983-1992 and find a premium of 5-7% per child, reduced to 3% per child when taking into account productivity-related variables. Meanwhile, Gangl and Ziefle (2009) undertake a cross-national analysis of the penalty and find that while productivity and human capital factors can account for the full penalty in the US and UK, in Germany a 10% penalty remains even after controlling for these factors.

Country-level differences are clearly important in the penalty. In addition to the stark differences found between countries by Gangl and Ziefle, studies have shown that in Northern European countries especially the motherhood wage penalty is now almost non-existent. Gash (2009) also undertakes a cross-country analysis with the intention of investigating the link between family policy and institutions on the motherhood wage penalty. While she finds a gross penalty of 2.8% and 3.8% respectively for the UK and West Germany, countries with less well-developed family policies, she finds no gross penalty for Denmark, France and Finland – countries where family policies are more supportive of mothers working. While controlling for standard socio-economic variables results in a penalty in all the countries, there are still stark differences in the penalty between regimes supportive of working mothers and those that are less so. These findings are largely corroborated by Petersen and colleagues (2014), who, in a longitudinal analysis of Norwegian register data from 1979-1996 find that during that time the motherhood wage penalty all but disappears.

Notwithstanding some notable country differences, the literature on motherhood wage penalties generally confirm that productivity and human capital explanations do indeed have an important impact. There remains, however, in many countries a significant unexplained portion. Two possible explanations, employer discrimination and mothers' preferences, will be addressed in this thesis.

Employer discrimination against mothers rests on two possible mechanisms, similarly to the male marriage premium: statistical discrimination and the influence of social norms. In the former, employers assume mothers as a group will be less productive or committed to work and therefore offer them lower wages (Correll et al. 2007). In the latter, when social norms dictate that mothers take charge of child-rearing, it may be that employers react by offering

mothers lower wages than women without children irrespective of rational considerations of productivity but simply because they believe that their focus should be on family matters rather than paid work, and that focusing on both and producing satisfactory results is not possible (Krüger and Levy 2001, Auspurg et al. 2017). Studying both cases requires experimental data or correspondences studies, which have been used in the US to show the existence of discrimination against mothers, though with a sample of students for the experiment, and with only information on whether or not a candidate would be invited to a job interview from the correspondence studies (Correll et al. 2007). This thesis furthers this line of inquiry by analysing the decisions employers themselves make when presented with hypothetical job applicants, some of whom are mothers. By contextualising these results within observed wage penalties on the labour market, I offer a multidimensional picture of the motherhood wage penalty unique in the literature to date.

Having access to data deriving from recruiters also allows for a more fine-grained analysis of the dynamics behind any employer discrimination against mothers. Employers' penalties may be based on their own personal or professional situations, for instance. While recruiters who are mothers may be more generous to applicants who are mothers, in line with expectations of the homogamy theory (McPherson et al. 2001, Gorman 2001), female recruiters without children may be harsher on mothers than the overall average, either because they expect that women with families cannot possibly be as productive as themselves (Staines et al. 1974, Ellemers et al. 2004) or because they have, in order to succeed professionally, had to conform to the prevailing norms of their organisation which may penalise working mothers (Cohen and Huffman 2007, van Hek and van der Lippe 2019). Childless men, who are more likely to be younger than the average, may tend to have more liberal views about combining parenthood and work than other profiles. Married men, who are likely to be most representative of the

dominant organisational norms, may therefore hand out the penalties that best reflect the overall average. Using the individual-level data collected in the survey experiment, we are able to ascertain whether or not such characteristics do indeed play a role in wage penalties, or whether motherhood is an equally salient signal for all employers, and motherhood penalties therefore vary little across different groups. This is a depth of information that thus far has not been exploited when studying the role of discrimination on the motherhood wage penalty.

A final aspect of the motherhood penalty – indeed, of the gender wage gap in general – is the role of wage expectations of women themselves. Women may expect, and therefore accept, lower wages than men. This may be because their wage negotiation strategy differs from men's and they do not see the need to set high expected wages in order to be "met in the middle" by a prospective employer (Pfeifer and Stephan 2018), or it could be because they know that they are in a lower-paid occupation and set their wage expectations accordingly (Fransson and Biel 2000). On the other hand, it could be due to expectations of job characteristics and career trajectories that are shaped by raising a family. Women may hold to a more multidimensional definition of what is a good job, while men still embrace classical definitions of professional success – high (and increasing) wages and job prestige. If women are willing to trade wages for other conditions, such as flexibility, part-time work, childcare and supportive co-workers and managers, their expectations may be commensurately lower than men's (Becker 1985, Blau and Ferber 1991). Finally, women may anticipate stretches out of the labour market, or acknowledge the effects of accumulating human capital more slowly and set their wage expectations in function. Using a unique dataset with information about expected and average wages of a representative cross-section of society, the final empirical chapter of this thesis seeks to document this relationship between expected and realised wages, and how they are affected by individual attributes.

1.2 Context: Family and work in Switzerland

Three of four empirical chapters in this thesis use data from the Swiss labour market. Switzerland's liberal labour market is characterised by high wages, high rates of workforce participation, and a well-educated workforce that is largely trained for specific jobs through a highly developed vocational education system – a trait shared with neighbouring Germany and Austria in particular. Contractual work hours, at an average of 41.7 per week for a full-time job (OFS 2016a), are long compared to many other European countries, and employee protections are low: there is no statutory minimum wage and employment contracts may be terminated with minimal justification (Oesch 2011). Chronic labour shortages are met in part by high levels of immigration.

Switzerland also has a number of specificities pertaining to women's labour market participation and the conciliation of work and family life. Although women's employment rates are high in Switzerland – among the highest in the OECD – most Swiss women are employed part-time (Stadelmann-Steffen 2007). For mothers this situation is even more extreme, with 83% of working mothers employed part-time (OFS 2016a) This is clearly linked to the prevalence of traditional cultural norms that place strong importance on gender roles of men (fathers) as breadwinners and women (mothers) as homemakers and child-rearers (Hong Li et al. 1998, Bernardi et al. 2013, Valarino and Gauthier 2016). The male breadwinner plus female part-time model (Lewis 1992), where the main family income from a full-time working man is augmented by the part-time salary of a woman, is the prevailing form of dual-income household (Buchmann et al. 2010, Stadelmann-Steffen 2011, Clerc and Kels 2013).

This socio-cultural paradigm is linked to policies and institutional settings that serve to continue the status quo of conservative family life. Some of these may seem trivial at first

glance but may have significant impact on mothers trying to conciliate family expectations and work responsibilities. As an illustration, at primary school especially, contact hours are short and often irregular – in many cantons, it is the norm for schoolchildren to return home for lunch and have Wednesday afternoons off (Buchmann and Charles 1995). Such hurdles are exacerbated by the fact that childcare coverage is patchy and, where available, often expensive, (Ernst Stähli et al. 2009), though in urban areas access and payment support is generally better than in the regions (Abrassart and Bonoli 2015). Earlier into parenthood, statutory paid maternity leave is short – 14 weeks – and was only introduced federally in 2004. While mothers are protected from unfair dismissal during the leave period itself, these protections expire the day they return to work. Paid paternity leave, at 1 day, is functionally non-existent. Overall, these policy settings can be expected to hold down mothers' employment, notably in terms of hours worked for those who choose to work. Indeed, Stadelmann-Steffen (2011) exploits the fact that most family polices in Switzerland are implemented at the cantonal or even municipal level to test the influence of such policies on mothers' workforce participation. She finds that greater access to childcare is strongly linked to greater workforce participation, while on the other hand, more generous cash transfers for families with children decreases the likelihood that mothers will participate and their level of participation. There is some variance in the effects based on the individual characteristics of mothers – those with medium levels of education, for whom work may be seen as an optional extra, are more likely to be influenced than those with low education, for whom work is a necessity, and those with high education, who are likely to place more importance on career progression and development.

In addition to the family policy environment having an impact on workforce participation, the structure of the labour market itself, both independently and considering social and policy factors, may influence employment patterns. Buchmann and colleagues (2010) note that a

number of features of the Swiss labour market restrict the career choices of mothers especially and push them into “marginal” part-time jobs that are low prestige and have little room for career development. According to the authors, given the caregiving expectations on mothers it is difficult for many with young children to work more than a 50% of full-time job, especially in areas of low childcare coverage. Most of these jobs are in marginal service occupations, with high risk of redundancy in case of economic downturns, often concentrated in small enterprises which react more to prevailing market conditions than larger firms, and offer little room for promotion or job changes, particularly given access to many occupations in Switzerland requires very specific educational certification. Positions of authority and prestige, on the other hand, generally require a period of uninterrupted service and long hours, in many cases incompatible with family duties for women. The result of this paradigm for wage gaps is likely to be significant: men and childless women may see benefits while mothers lose out.

In sum, the work and family frameworks in Switzerland suggest two competing possibilities for the influence of the Swiss context on motherhood wage penalties and male marriage premiums. On the one hand, expectations towards gender roles, generally low policy and institutional support for working mothers, and a highly segmented labour market structure that favours long hours and uninterrupted service for career development suggest that the motherhood wage penalties and male marriage premiums that are the focus of this thesis are likely to be on the higher end of those reported in the literature. On the other hand, low unemployment and chronic labour shortages imply an environment unfavourable to employer discrimination.

1.3 Data I: Factorial Survey Analysis[1]

All four empirical chapters of this thesis rely on factorial survey analysis (FSA) for some or all of their analytical design. Given FSA's importance to the findings presented throughout these four chapters, and the novelty of using such surveys for labour market analyses, I will outline in some detail the history and use of FSA, its advantages and disadvantages, and how it can be used in labour market sociology to provide information about labour demand (employer)-side processes. I will then introduce the two factorial surveys used in this thesis: the LIVES-JOBVUL study and the SOEP-IS factorial study.

1.3.1 FSA: Background and advantages

FSA examines the basis and influences of social judgements by posing a series of fictional *vignettes* to respondents, and asking them to make a judgement based on the information provided. Vignettes are made up of *dimensions* (variables), the characteristics of the vignette that vary in their *levels* (values) (Wallander 2009). Examples include vignettes based on the housing market, where the neighbourhood, price, and age of the house are the varying dimensions, or on punishment for criminals, where the crime, circumstances, and mental state of the criminal may be the varying dimensions (Auspurg and Hinz 2015). Such surveys are used either to elicit a *normative judgement* – where a respondent is asked to judge how they think something *should be* (choosing a punishment, rating the fairness of a situation) – or a *positive belief* – how they think something *is* (reflect on how they would behave in a given situation, estimate an outcome) (Jasso 2006: 335-36; Wallander 2009: 509).

[1] This section is partly adapted from McDonald, P. (2019) "How factorial survey analysis improves our understanding of employer preferences", *Swiss Journal of Sociology,* 45:2, 237-260.

Similar techniques are popular across a broad range of social, economic, and commercial research. FSA is closely related to conjoint analysis, or discrete choice experiments, a technique frequently used in marketing and recently picked up by political scientists, where consumers (or voters) are shown different products with similar features and asked to choose between them (Green et al. 2001; Ryan et al. 2001). Despite their different names and theoretical backgrounds, these techniques are practically the same (Hainmueller et al. 2014). Together with other lesser-used methods such as Paper People Studies and Policy Capturing Studies, they form what is termed "Experimental Vignette Methodology" (EVM) (Aguinis and Bradley 2014).

One pitfall of statistical analysis of survey data is the difficulty of disentangling causal relationships. While observational data can detect correlation, it is difficult to pinpoint causality without using sophisticated statistical designs (Winship and Morgan 1999). As an alternative, Winship and Morgan suggest a return to "experimental thinking". FSA fits this call by embedding an experiment within a survey. By having complete control over, and fully randomising all the inputs (in this case, the vignette dimensions), causal relationships can be identified. The survey designer is aware of all the information contained in the survey, all of which is uncorrelated, meaning that the influence of unobserved heterogeneity and collinearity can be discounted. This experimental design is key to FSA's attractiveness as a tool for studying labour market issues.

We are interested specifically in the benefits of using FSA in labour market sociology, and especially as a way of measuring employer decision-making in recruitment processes. Here, it has a number of advantages: It provides a good simulation of the hiring process and reflects the multidimensional nature of recruitment decisions. Furthermore, it has advantages in

studying employer's decision-making processes, albeit hypothetically, over other analytical strategies. For example, audit studies[2] have the advantage of analysing real hiring processes, but they either give up detail to gain this realism – the most that can be hoped from a correspondence audit study is to know if certain profiles would be invited to interview – or, in the case of in-person audits, require large investments and the resolution of ethics questions before being implemented. FSA, on the other hand, can ask employers questions about salary, company fit, ease to train, amongst others. Such questions add necessary depth to our knowledge of employers' practices and reinforce the utility of FSA as an analytical tool for decision-making processes, though it is not without its drawbacks.

1.3.2 Criticism and drawbacks

FSA can be criticised in a number of ways. Commonly, vignette studies are dismissed as not reflecting real life, in terms of the urgency of decision-making, the make-up of the sample, or the design of the vignettes. Responding to a series of vignettes may also lead to fatigue and thus respondent-induced error. A number of significant concerns regarding vignettes' real-life relevance come out of the work of Pager and Quillian (2005), who use a follow-up telephone interview to a correspondence audit study to show that employers' responses in surveys do not always reflect their real-life behaviour[3]. They pinpoint three key weak points of survey-based research, in which FSA can be included and which form a useful basis for the discussion of its

[2] Audit studies are a subset of field experiments carried out by sending fictional applications for real jobs – either "in-person audits" where a written application is followed up with a person sent for an interview, or "correspondence audits", which rely only on a written application (Gaddis 2018). They vary 1-2 dimensions, typically race or gender, and analyse whether this artificial variation leads to lower or higher overall recall to interviews or offers of employment. This is often supplanted with qualitative data on a selection of the firms and organisations targeted (see Pager et al. 2009, Protsch and Solga 2015).

[3] Hainmueller et al. (2015), in contrast, find that behaviour in experimental settings closely mirrors that displayed in real life.

weaknesses: (i.) social desirability bias, (ii.) abstract situations and external validity, and (iii.) priming intensity.

First, social desirability bias can be a crippling challenge for any survey-based research project where subjects are at least partially aware of their participation. Respondents are likely to downplay any bias against women, minorities, and/or people with a criminal record, when answering a vignette survey as opposed to an actual recruitment process (Pager and Quillian 2005, 364-65). Vignettes could thus underestimate the actual effects of discrimination in reality, especially if respondents are able to guess at the purpose of the survey. There are methods, however, that can be used to mitigate social desirability bias in FSA. Auspurg and Hinz (2015) outline two key ways that this can be achieved. First, the subtlety of questioning that FSA can facilitate: rather than asking questions on contentious issues outright, a well-designed factorial survey will embed them in a set of constantly-changing dimensions, making the dimension in question difficult to discern and therefore minimising the risk of a respondent giving a response motivated by social desirability. This is linked to the second advantage of FSA in minimising social desirability bias: the need to use trade-offs when judging vignettes. With several dimensions changing in each vignette, a respondent often must make a decision about which perceived negative characteristic is more or less important, making it difficult to give socially desirable answers across the board. It is clearly not possible to avoid socially desirable answers altogether. A well-designed factorial survey, however, should be able to mitigate social desirability to some extent.

Second, a critical issue that arises with data from any simulated rather than real situation is the external validity of the results[4]. FSA is an exercise in ascertaining behavioural intentions, and does not measure actual behaviour or decisions. Though vignettes are designed to closely resemble realistic scenarios, they cannot do so perfectly and risk being useless without thoughtful interpretation. A profile accepted in a vignette survey may be rejected in real life. This may be partly because of differing circumstances in a specific hiring situation and the availability of more detailed information in reality, but it may also be due to the fact that in an FSA, there is no pressure or urgency to hire, and therefore respondents can accept profiles more easily knowing there will be no consequences. FSA in the employer survey context should therefore be considered hypothetical baseline intentions which may vary from actual behaviour depending on actual circumstances. One way of addressing this weakness is to combine a factorial analysis with other data sources – labour force surveys or audit studies, for example – and compare the results.

Third, priming intensity refers to the detail of description of the subject or situation in question. Just as meeting someone in real life and reading their description on paper are two different things, so can the type of description vary significantly. A vague CV leaving a respondent to "fill in the gaps" may lead to an entirely different result than a long and detailed résumé complete with photo. Priming may also refer to the realism of the stimuli presented. This is a challenge for FSA in recruitment studies in particular, when the vignettes are often designed to simulate actual CVs. Researchers must balance the informational needs of the survey with the necessity of presenting realistic CVs, which (depending on the labour market context) are

[4] McDermott (2011) defines internal validity as "the extent to which an experimenter can be confident that his or her findings result from experimental manipulation" (p. 28), and external validity as "the generalizability of findings from a study, or the extent to which conclusions can be applied across different populations or situations" (p. 34).

unlikely to contain some of the information of interest to researchers, particularly if this includes personal and family-related information. A series of CVs that do not conform to accepted norms may prime respondents less well than a series resembling actual CVs, which may in turn contain less information and therefore be less useful in analytical terms.

Similarly, respondent fatigue due to survey length and complexity of the survey may cause problems if not properly addressed. Having too many vignettes to analyse and/or too many dimensions per vignette can result in cognitive overload, and therefore inconsistent evaluations. Sauer et al. (2011: 98) estimate the limit for general population surveys to be at a maximum of around 20 vignettes per respondent, each with at most 12 dimensions, though this may be increased for studies of professionals.

The weaknesses of FSA can be summarised, then, as lacking external validity, its hypothetical nature – which may be exacerbated by poor design or non-realistic stimuli – and lingering concerns that it does not fully address social desirability problems. While these concerns do not discredit its use altogether (Pager & Quillian 2005: 372) they plead for prudent and conservative interpretation of results obtained from FSA.

1.3.3 Use in labour market sociology

As FSA has gained a foothold in labour market sociology, its use in analysing different topics has expanded significantly. FSA is particularly useful in analysing the effect of signals (Spence 1973), or observed characteristics from which unobserved characteristics may be inferred, on employers. Education, in particular, is widely accepted to signal quality in a candidate (Connelly et al. 2011), but empirical evidence has suggested that other characteristics are often used as signals by employers – names can signal ethnicity, participating in sporting activities

can signal fitness and be positive for jobs requiring manual labour, and participation in active labour market programmes can either signal diligence and willingness to learn, or discrepancies in necessary knowledge (Liechti et al. 2017). How employers react to these signals and indices in either a CV or a job interview is an essential knowledge in the recruitment process.

Beyond signalling, FSA has also shown its use in queuing theory (Thurow 1975, Di Stasio 2014) – whereby employers will place applicants in a virtual queue based on their qualities, attributes, and indeed the signals present in their CVs or interviews. Including questions on likelihood to hire, or invite to an interview, in FSA can be used to provide information on what characteristics affect the place of an applicant in the labour queue, and how this may vary for different jobs.

FSA also provides a useful way of measuring discrimination in hiring and wages. Using traditional survey data, discrimination is difficult to measure as it is likely to be confounded or correlated with other variables, and untangling the different effects is far from easy. FSA proposes a solution by providing data where variables are uncorrelated, meaning that discrimination, or lack thereof, can be identified. Moreover, rather than relying on reported wages and employment statistics to infer possibilities of discrimination, FSA allows direct measurement of stated intentions for various profiles containing different characteristics that may be discriminated against, such as age, sex and ethnicity, as well as the interaction between such variables. Finally, FSA may be able to reduce social desirability problems faced by measuring discrimination through direct questioning, meaning its responses can get closer to reflecting true discrimination. While the mechanics of the hiring process are almost exclusively the domain of employer-based studies, FSA has been used in general population samples to study examples of discrimination, particularly concerning gender, with Jasso and Webster

(1997, 1999) finding that respondents both in the general population and among university students assign higher "just wages" to men than women, though the difference amongst male respondents is greater. A paper by Auspurg et al. (2017) based in Germany finds similar results.

FSA has been used to survey employers directly across a number of issues relating to the labour market, several key areas stand out: older workers (see for example Karpinska et al. 2013, 2015, Mulders et al. 2014), discrimination based on nationality (van Beek et al. 1997, Damelang and Abraham 2016), and education (Di Stasio 2014, Di Stasio and van de Werfhorst 2016), with a move into further subjects such as unemployment scarring and the influence of ALMP on hiring decisions in recent times (Liechti et al. 2017, Shi et al. 2018). These subjects show clearly the advantage of FSA's use for the analysis of two "black box" issues that are hard-to-reach in other analytical formats: How employers react to factors that may signal productivity when little other information is known, and to what extent certain characteristics may lead to discrimination either in terms of wages or likelihood to be employed. In both cases, these effects may be at the heart of puzzling outcome gaps that have not yet been explained by observational data, and are thus useful in completing the picture of the mechanisms and outcomes at play on the labour market.

1.3.4 Discussion

The analytical options available to the quantitative sociological researcher have expanded in recent years and, in the case of labour market sociology, mechanisms on both the supply and demand side can be more readily analysed than ever. Factorial survey analysis is one example of these new methods. It is particularly well-suited to the study of the demand (employer) side of the labour market equation, as it can target recruiters directly and present both complex and varying worker profiles for judgement. These judgements can then in turn be analysed for

evidence of employer preferences based on certain characteristics, such as gender, age, nationality or educational attainment. By ensuring that all the dimensions are uncorrelated and eliminating unobserved heterogeneity, FSA satisfies the criteria for causal design.

FSA should not, however, be blindly accepted as the panacea to a lack of reliable demand-side data. Respondents are aware that they are participating in a survey and may therefore be prone to deliver socially desirable responses. Moreover, the fact that FSA is not carried out in a real-world setting means that all the judgements made are based on hypothetical situations and can therefore be considered behavioural intentions that may not be replicated in real recruitment scenarios. Finally, FSA is by design an exercise in simplification, where only a few examples (of occupations, for instance) can be judged. Its results may thus not be readily generalisable and therefore FSA, while being internally valid, is not externally valid in all circumstances.

These criticisms are not enough to discredit the use of FSA in labour market sociology. Rather, they plead for a holistic approach to its use. Comparisons with labour force and employer surveys, audit studies and field experiments may all provide opportunities to increase external validity and compare preferences expressed within the vignette survey setting with real-world data. Vignette surveys may be embedded within larger surveys to increase sample size (Karpinska et al. 2013; Auspurg et al. 2017). To better reflect actual issues and trends on the labour market, recruiters currently hiring could be invited to complete a survey based on their job advertisements, an approach currently being pioneered by the NEGOTIATE project (see Shi et al. 2018). With thoughtful implementation and interpretation, FSA can be very useful in pinpointing the factors and influences that play a part in labour market outcomes.

1.3.5 The LIVES-JOBVUL project

Three of the following empirical chapters use data from the LIVES-JOBVUL project (Liechti et al. 2019). This project, supported by the NCCR LIVES and On the Move research centres, sought to better understand employers' decision-making processes when confronted with job application profiles that varied on marital status, number of children, country of origin, and participation in Active Labour Market Programmes (ALMP).[5]

The survey presented vignettes in the form of stylised CVs for three different occupations: an accountant, a human resources assistant and a building caretaker. The three positions were chosen because they represent a range of skills, are present in many organisations irrespective of economic sector, and, while not completely gender-neutral, are neither male nor female-dominated. So that profiles would not be judged on their motivation for applying for a new job or deficiencies in their human capital and productivity, the introduction to the survey specified that all profiles had been unemployed for a period of six months due to the closure of their previous firm. While introducing unemployment carries its own issues, the fact that the unemployment is due to an exogenous shock minimises these to an extent. Respondents were asked to rate the vignettes on two different questions: the likelihood of being invited to a job interview (0-10 Likert scale), and the monthly wage that would seem adequate for a given profile, in CHF (Swiss francs). The different dimensions and their levels meant that each of the three occupations contained a vignette universe that was too large to simply use random assignment of variables. Therefore, an algorithm was used to draw a sample of the vignette universe that is *D*-efficient (Auspurg and Hinz 2015): all dimensions and levels are orthogonal

[5] This survey was carried out in 2016 by Fabienne Liechti, Flavia Fossati, Patrick McDonald, Daniel Oesch, Giuliano Bonoli and Daniel Auer at the University of Lausanne. It is available at https://forsbase.unil.ch/project/study-public-overview/16840/0/ for download and secondary analysis.

and there are enough instances of each to allow for robust analysis of individual and two-way interaction effects. The order in which the occupations and the vignettes within them appear was randomised. Information on respondents' professional situation and experience, and some biographical data, was also collected.

The data were collected between June and November 2016 across Switzerland, after having gone through several rounds of testing in German and French. An online questionnaire was disseminated through a large human resources association in Switzerland with members in all regions of Switzerland. The human resources association which distributed the survey counts some 4500 members across Switzerland. In total, 714 respondents began the survey (16% response rate). Some respondents who began the survey provided no ratings for any vignettes, while others did not rate all the vignettes presented to them. Taking out these missing ratings leaves a sample of 537 respondents (12% response rate) rating 5764 vignettes, an average of 11 vignettes per respondent. The response rate is in line with other large-scale vignette surveys using professional associations as respondent samples (see Liechti et al. 2017 for an example). However, response rates varied greatly from region to region and the sample is not strictly representative of human resources professionals in Switzerland. Most of the respondents were active human resources professionals, with 91% stating they were actively involved in recruitment in the previous year. 4 respondents in 5 indicated that they had decisive influence in hiring in their organisation. Urban and suburban areas were highly represented, with fewer responses from rural regions, Central Switzerland in particular, and the Italian-speaking Ticino region. Large firms were overrepresented, with 50% of respondents working in organisations counting 250 or more employees.

Chapters 2, 3, and 4 use the LIVES-JOBVUL study to explore the labour demand-side mechanisms behind the male marriage premium and the motherhood wage penalty, both in conjunction with panel data analysis (chapters 2 and 3), and alone (chapter 4). More specific details on the vignettes are described in depth in these chapters.

1.4 Data II: Panel Surveys

The use of FSA in this thesis is instructive in aiming to understand the behaviour of employers when confronted with different potential worker profiles, and of job seekers when shown potential job offers. However, as discussed above, they are based in hypothetical situations and while their internal validity is high, their external validity is not. To address this issue, the empirical analysis of the vignette data in this thesis is combined with analysis of panel data, notably in chapters 2 and 3. Panel data analysis contains a number of complementary advantages to FSA. First, in the cases of this thesis I choose two large-scale representative samples of Switzerland – one of Swiss society in general and one of the labour market specifically. These datasets therefore provide statistically robust, representative analyses that have high external validity and lend credibility to the results of the vignette analyses, provided these show similar trends. Second, panel data allows for a longitudinal analysis of the mechanisms in question. This is important as it can to some extent account for unobserved individual characteristics that may be correlated to the effects studied by deploying the right analytical strategies. Third, as the mechanisms and outcomes in question in this thesis should not be assumed to be constant over time, the ability to account for temporality is important. Finally, and particularly for the subject of this thesis, the use of panel data adds the labour supply-side dimension to the analysis.

Two panel studies are used in this thesis, as well as data from a single wave of the German Socio-Economic Panel's Innovation survey. The first, the Swiss Household Panel (SHP), has been collected since 1999 and currently consists of 19 waves. Its remit is to provide a broad snapshot of life in Switzerland and it therefore contains a broad range of social, cultural, political, economic, family and health-related variables (Voorpostel et al. 2018). The SHP interviews approximately 10,000 individuals in 5000 households per year, chosen to be representative of the Swiss population. The retention rate year to year is approximately 50%. Aside from its initial sample, the survey has been refreshed twice, in 2004, and 2013, so that as of 2017, the dataset contains information for over 35,000 individuals. The SHP is used for empirical analyses in both chapters 2 and 3.

Chapter 3 uses a second panel study in addition to the SHP: the Swiss Labour Force Survey (SLFS). Unlike the SHP, the SLFS is designed with the specific purpose of observing and analysing the Swiss labour force. The principal advantage of this is that it captures detailed work and employment-related data for a large sample: since 2010, 126,000 people are interviewed on a quarterly basis, an increase from 35,000 a year between 2002-2009 and 16,000 a year between its inception in 1991 and 2008 (OFS 2018). However, unlike the SHP, the SLFS is not a continuous panel – between 1991 and 2009, a rotating panel was followed for 5 years, but since 2010 the timeframe has changed to 5 quarters. As a result, longitudinal analysis of the data post-2009 is less instructive. Moreover, the SLFS, with its narrower focus on labour market issues, does not contain the same array of variables as the SHP. Nevertheless, its size and scope make it a valuable resource for analysis of labour market questions in Switzerland.

The fourth empirical chapter of this thesis draws on data from the German Socio-Economic Panel's Innovation Sample (SOEP-IS), specifically the "Job Preferences and Willingness to Accept Job Offers" module, collected as a part of the 2013 wave of the survey. The module includes variables such as information on expected wages, reservation wages, and, where applicable, actual wages of each of the respondents. Extensive socio-cultural and economic variables are also available for each of the respondents. 1712 individuals representing a random sample of the German population responded to the survey. 1217 answered one of the two partner questionnaires, while 495 reported no partner. Chapter 5 uses the SOEP IS data to study the impact of wage expectations on actual wages, and their differences by gender. Motherhood, as a significant factor in many women's lives, is an equally important consideration in the analysis.

1.5 Analytical Design

The empirical chapters of this thesis use an array of analyses that recognise the importance of accounting for the nested nature of the datasets used and a desire to minimise the impact of unobserved heterogeneity. Analyses of the panel data are generally based on respondent-level fixed effects regression. Fixed-effects only takes into account the changes within individuals across time, unobserved time-constant characteristics are eliminated from the analysis, meaning that correlations between time-constant unobserved heterogeneity and the dependent variables can be accounted for. However, fixed-effects cannot be considered a fully causal design, as time-varying unobserved characteristics will not be accounted for (Mu and Xie 2016).

Chapter 2 includes an expansion of fixed-effects by introducing fixed-effects individual slopes (FEIS) models (Brüderl and Ludwig 2015) to the panel data analysis. As their name suggests,

FEIS models fit fixed-effects models with distinct slopes for each observation, based on a measured time-varying characteristic, such as age or years of experience. The benefit of such models is that they are able to account for the complexity of differing growth curves. While fixed-effects regressions assume that a dependent variable will change at the same rate over time across all observations, FEIS allows the rates of change to differ. This is an especially important consideration for analysis where wages are the dependent variable, as wages growth can be expected to differ considerably across individuals. Chapter 2 also uses entropy balancing (Hainmueller 2012) to match observations on pre-labour market variables that would influence in-work productivity. By making individuals in the control and treatment groups as similar as possible on pre-labour market characteristics, we can be more confident that effects are due to the variables under analysis. Entropy balancing differs from standard matching procedures by calculating scalar weights so that the treatment and control groups are matched on a group level, rather than matching individual observations. In this way all observations are maintained for the analysis, though the weighting results in some having very little impact. As a robustness tests, analyses using entropy balancing are replicated using nearest-neighbour matching with replacement of observations.

The basis of the vignette analysis in chapters 2 and 3 is also fixed-effects models. In this case, fixed-effects models are applied at the respondent level, acknowledging that the rating procedure in the vignette surveys is likely to be a cumulative process – previous ratings will influence future ratings – and that respondents are likely to have differing rating baselines based on their own individual characteristics. These individual characteristics are of direct interest in chapter 4 and as such in this chapter I estimate random-intercept multilevel models, with vignette variables on level 1 and respondent variables on level 2. This analytical method

allows us to ascertain to what degree the characteristics of respondents to the survey are linked to their rating of the vignettes.

Chapter 5 introduces a new angle of analysis. We analyse data from the SOEP IS module, using OLS linear regressions to assess the impact of gender, personal and job characteristics on expected and actual wages. To test the link between expected wages and actual wages, we replicate the analysis, but with the difference between expected wages and actual wages as the dependent variable.

1.6 Structure of the thesis and summary of results

At its core, this thesis is interested in improving our knowledge of the mechanisms and dynamics behind wage inequalities based on parenthood and marital status. Specifically, it uses a wide range of analyses and several datasets to study the male marriage premium in Switzerland, finding a small premium that is largely explained by selection into marriage; and the motherhood wage penalty, where it finds a more substantial penalty affected both by productivity differences and recruiter discrimination. It further delves into the recruiter impact on the motherhood wage penalty, showing that all recruiters, irrespective of their own gender and parenthood status, penalise mothers as compared to childless women. Finally, it takes a step back and enquires about the relationship between the wage expectations of individuals and the actual wages of workers, and demonstrates that wage expectations are also affected by differing family and work statuses, and that there may be a close link between these expectations and the actual reported wages of those in work. I briefly present the structure of the remainder of the thesis and how each chapter contributes to these results.

Chapter 2 of this thesis focuses on the male marriage premium in Switzerland and the three mechanisms that are theorised to be behind it: selection, improved productivity, and the preference of employers for married over single men. The first two theories are analysed empirically using the Swiss Household Panel, applying fixed-effects, FEIS models and entropy balancing to account for selection effects and isolate the portion of the premium that can be linked to productivity or, possibly, employer favouritism. This analysis finds that of a 9% premium to married men shown in pooled OLS models, the varying strategies used to account for selection reduce the premium to about 3%. The inclusion of a set of variables designed to capture in-work productivity do not greatly reduce the penalty, suggesting that productivity differences before marriage are more important than changes following marriage. To investigate the link between employer preferences and the premium, I proceed to an analysis of the JOBVUL factorial survey. Here, for the reference category of accountants, I find a premium to married men of 2%, but no clear evidence of a premium for HR assistant and concierge profiles. Marriage premiums based on employer preferences, then, cannot be said to be uniform across the labour market and are likely to vary across differing job characteristics.

Chapter 3 is the first of three empirical chapters concerning motherhood and gender. In this chapter we explore the motherhood wage penalty in a similar way to the male marriage premium in chapter 2. First, the residual motherhood wage penalty is established through analyses of the Swiss Household Panel and Swiss Labour Force Survey. These analyses show an unexplained penalty of 4-8% per child. How much of this penalty may be explained by employer discrimination is the subject of the second part of the analysis, again using the JOBVUL factorial survey. Here we find that wages assigned to mothers are 2-3% lower than those of non-mothers, for the reference category of HR assistants, further evidence that the effects of personal and family characteristics are not constant across occupations. Further

analysis shows that this penalty is mostly driven by younger profiles – the penalty for mothers 40 and under is 6%, while it is negligible for mothers over 40. Moreover, there is no penalty for mothers applying for the concierge position – evidence that motherhood is considered differently for different groups of women.

In chapter 4, I extend the analysis of chapter 3 by investigating to what extent the penalties handed out by recruiters to mothers vary by respondent characteristics. Specifically, I interrogate the differences in penalties, if any, between four groups: mothers, women without children, fathers and men without children. After an illustrative analysis using fixed-effects models for each of these four groups, I turn to random-intercept multilevel models to uncover the size and significance, if any, of these respondent-level characteristics. While the analysis shows that women without children hand out the largest penalties and men without children the smallest, the differences are small and not statistically significant. Motherhood, then, seems to be a salient signal of productivity by employers of all groups.

While the focus of chapters 2-4 is largely on employers, in chapter 5 we turn to the other side of the equation and ask how expected and reservation wages influence the gender wage gap. Although the focus here is on gender more generally, motherhood is nevertheless an important factor in the analysis. If women anticipate a slower accumulation of human capital or reduced productivity due to the possibility of becoming mothers, this will be reflected in an expected wage gap. If women with children who have husbands with decent salaries require strong incentives to re-enter the labour market, this may be reflected by a smaller reservation wage gap, amongst women and men who are not active in the labour market in particular. We test these theories with the SOEP IS vignette module, which gives us information on expected wages and reported wages for all respondents, and actual wages for those in employment. The

analysis indeed finds that the reservation wage gap is smaller amongst those not in employment than those in employment. Moreover, the results show that expected wages closely mirror actual wages, suggesting that wage gaps are deeply ingrained and anticipated irrespective of labour market outcomes.

The final chapter of the thesis synthesises the key findings and considers their implications. I also reflect on the limitations of the analysis and offer some thoughts on future directions for the questions raised.

2. The male marriage premium: Selection, Productivity, or Employer Preferences?[1]

2.1 Introduction

In most western countries, married men earn more than unmarried. This premium is substantial – empirical studies show a bonus to married men of up to 20% in the US (Korenman and Neumark 1991, Chun and Lee 2001, Cohen 2002), 10% in Australia (Breusch and Gray 2004), and 10-15% in European countries (Gupta et al. 2007, Barg and Belbo 2007).

Three main mechanisms may explain the marriage premium. First, productivity: married men may work harder and longer because they have wives at home to take care of the domestic work, or because marriage makes them more responsible than bachelors. Second, selection: good workers with strong labour market chances make better husbands. Finally, the premium may come from employer preferences: employers prefer to hire married men because they believe the aforementioned characteristics will result in them being more reliable workers, whether this is the case or not, or because social norms suggest it is better to hire married men. These effects can be difficult to measure through surveys, and residual premiums often remain after taking productivity and selection into account.

The aim of this paper is to elaborate on the marriage premium for men through the combination of a panel data analysis and a factorial survey. We use data from a nationally representative longitudinal survey to conduct fixed effects regressions to identify the existence of a male marriage premium in Switzerland, using matching and fixed-effects regressions with individual

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slopes to differentiate selection effects from productivity and, possibly, employer preferences. To further analyse this latter effect we use the results of a vignette study, where a sample of 512 recruiters were shown fictional job candidate profiles and asked to indicate the likelihood they would invite a candidate to a job interview, and their wage if they were to be hired. The vignettes contain a set of randomly varying dimensions, including gender, marital status, and age, that enable us to identify the effect of marriage on men's wages. This dataset has already been used to analyse the effect of employer discrimination on the motherhood wage penalty (Oesch et al. 2017).

The contribution of this paper is to combine the analysis of longitudinal population surveys with a factorial experiment among recruiters – this is the first study to our knowledge that uses a factorial survey experiment to analyse the male marriage wage premium. In this way we can provide insight from both the employer and employee perspective. Our factorial survey is also unique in that it focuses on the members of a cross-industry human resources professional association, where we question recruiters across the Swiss labour market, rather than focusing on one sector, or using students as the target of the survey. The experimental design, where we control all the inputs and completely randomise the dimensions presented to the respondents, goes some way towards meeting the requirements of a causal research design. Finally, by presenting fictive CVs to active recruiters, we are able to analyse more detailed information about the hiring process, albeit information gleaned from a hypothetical setting. We can therefore also observe an important but little-analysed part of the job search and recruitment process – the initial sorting and judging of CVs, and how recruiters react to information on family status during this process. In short, we provide a fuller picture of the mechanisms operating behind the male marriage premium by using multiple strategies to disentangle selection, productivity and employer bias effects, as well as providing unique information on

the “black box” that is the impact of employer decisions on wage outcomes for married and unmarried men.

In the following section, we will present the competing theoretical explanations for the male marriage wage premium. We then present our data and analytical strategies, before moving to the results and analysis of the panel studies and factorial survey respectively. We conclude with a discussion of our findings and their implications.

2.2 Theoretical and empirical background

The sociological and economic literature posits three main theoretical explanations for the existence of a marriage premium for men: the specialization or productivity theory, the selection theory, and the employer preferences theory. We discuss these theories in turn in order to form our hypotheses.

2.2.1 Productivity

The productivity theory argues that marriage makes men more productive and therefore grants them higher wages. Historically, the key pillar of the productivity theory was household specialization, which for many years was the most widely accepted explanation of the marriage premium for men. It argues that married men are more productive than unmarried men and are therefore better remunerated. This stems from the idea that married men “specialise” in paid work, because they have wives at home who specialise in unpaid domestic work (Becker 1973). Conversely, unmarried men are expected to do both and therefore will have less time and energy to devote to their jobs.

While household specialization is no longer considered the key reason for the male marriage premium, the productivity theory offers further explanations. Productivity may also be improved by behavioural changes in married men stemming from the belief that husbands have a responsibility to provide for their wives and families. A qualitative analysis of the marriage premium in Russia finds that married men assume a persona of masculine responsibility, meaning they are more likely to take employment more seriously than non-married men (Ashwin and Isupova 2014: 52). Thompson and Walker (1989: 852) suggest that married men, particularly from the working class, see it as their duty to provide for the family and therefore will work harder to preserve a job, or toil longer in unfavourable working conditions. Additionally, married men may be more productive simply because they are happier and healthier than single men. Moreover, there is ample evidence to show married men are, overall, happier and healthier than unmarried, and that marriage has a positive effect on men's health. (Kiecolt-Glaser & Newton 2001). Married men, on average, are more satisfied with their lives, less prone to substance abuse and other high-risk behaviours (Umberson et al. 2010), and can make use of "communal coping" mechanisms to better address ill health (Lewis et al. 2006). These improved health outcomes combined with greater senses of responsibility amongst married men may lead to employees who are more productive at work than before they were married.

Notwithstanding that the traditional male-breadwinner model is on the wane in much of the western world, there is some evidence of a productivity effect in empirical studies in the US (Korenman and Neumark 1991; Gorman 1999; Chun and Lee 2001; Killewald and Gough 2013), Australia (Breusch and Gray 2004), China (Hughes and Maurer-Fazio 2002), and Germany (Barg and Belbo 2007), though the strength of this evidence varies from study to study. The strongest results come from large-sample panel studies. At the same time, some

research suggests the productivity theory is losing some of its validity, or that indeed it was much less important than imagined. Budig and Lim (2016: 1037) argue that marriage premiums are higher for millennials, but that they are gender-neutral (with the premium going to the breadwinner irrespective of gender), and that single-earner households are generally economically disadvantaged in comparison to those where both partners work. Hersch and Stratton (2000: 90-91) analyse the time men spend doing housework, and find that it does not explain the wage differential between married and unmarried men. However, this explanation does not discount the improved health and responsibility thesis.

The productivity theory brings us to our first hypothesis:

H1 (productivity): Marriage results in higher productivity for men, which is rewarded by higher wages.

2.2.2 Selection

The selection theory suggests that rather than marriage resulting in increased productivity, it is more productive men who get married in the first place. Characteristics that make men good workers, such as higher education, motivation, strong social networks and better physical health, make them better marriage prospects.

Empirical evidence on selection is mixed. In Germany, Barg and Beblo (2007: 70) use a matching approach to study the selection theory of the marriage premium and find that about half of the 9% wage premium for married men is due to a positive mix of characteristics in married men. Petersen et al. (2011: 300), in a study of Norway, find that most of the premium for married men is present before marriage – suggesting that more productive men are indeed more likely to get married, rather than marriage being the cause of productivity improvements.

Economic research in the US has pointed to selection being the key driver of the male marriage premium (Nakosteen and Zimmer 2001, Chiodo and Owyang 2002). A study of the US National Longitudinal Survey of Youth from Ludwig and Brüderl (2018: 747, 757) suggests that most of the male marriage premium comes from selection – making a distinction between men on higher wage "tracks" and men with higher earnings potential, both of which favour married men, while Killewald and Lundberg (2017: 1025), analysing marriages and divorces using the same data, find no evidence of a causal effect of marriage on wages, suggesting that the premium is rather due to men marrying when they are already on an increasing wage trajectory. On the other hand, Chun and Lee (2001: 318) analyse data from the Current Population Survey in the US and find that selection does not explain the penalty in any way. Ginther and Zavondy (2001: 326-327) test the selection hypothesis using the novel approach of analysing "shotgun" (unplanned) weddings brought on by unexpected pregnancy. By assuming that shotgun weddings are a random event the authors also assume they are not subject to selection effects. By comparing the marriage premiums of men in shotgun marriages with those married more "conventionally", they find a difference in the marriage premium of only 10% in favour of the traditionally married, suggesting that selection plays only a small part in the premium.

In sum, it seems that selection could offer an explanation for the male marriage premium but depending on the type of data analysed and the geographical context, estimations of its size (or its existence at all) vary widely. Part of this variation comes from measurement difficulties – estimating a selection effect requires either the use of counterfactual analysis or ample data in order to estimate the change in wages before and after marriage. There nevertheless remains enough evidence to argue that selection does account for at least some of the premium, but that

neither it nor productivity may be enough to account for it all. We must turn then to the question of employer preferences.

The empirical and theoretical literature on selection brings us to a second hypothesis:
H2 (selection): Men are positively selected into marriage, resulting in higher wages for married men irrespective of productivity changes.

2.2.3 Employer preferences

A third possibility for a marriage premium is the preference of employers for married men over other candidates. Sociology has long been sceptical of the notion that wages are decided purely on questions of productivity. Besides power resources, other factors such as employers' tastes and employees' personalities may come into play.

In terms of the marriage premium, employer favouritism could take on two forms in particular. First, it may be that employers simply prefer married men – perhaps they believe that married men are more reliable or feel the need to offer them a "family wage". Employers may also have greater affinity with married men: with married men over-represented in positions with hiring and firing power, they may simply hire the profiles closest to their own. Employers could prefer married workers because they are less likely to cause unrest – a married man is perceived to have more to lose from unemployment and therefore will be less likely to join a union, participate in a strike, or otherwise put his job in danger (Schwartz 1990: 69). The second possibility is that employers and managers believe in the specialization theory and expect married men to be the most productive workers but overshoot the actual productivity difference (if there is one at all).

It is important to emphasise here that employers may not necessarily express their preferences in terms of wages and may instead do so by hiring married men more often than single men. England and Farkas (1986: 125-126) point out that employers have "imperfect information" about prospective job candidates before they hire them and must therefore use what information they do have to screen applicants, often straying into statistical discrimination when their decision-making encompasses considering such characteristics as age, sex, and marital status. If it is true that employers do prefer married men over unmarried, they should be expected to express this preference in terms of their hiring behaviour as well as (or in place of) their wage-setting patterns.

Measuring employer preferences is difficult, especially in traditional employment surveys. Doing so usually involves the analysis of firm-level hiring data, experiments, and correspondence studies, where fictive job applications are sent for real openings – the drawback being that this method can only ascertain if a given profile would be invited to a job interview. Perhaps as a result of this difficulty in obtaining authoritative data, there are relatively few empirical studies of the male marriage premium with a focus on employer preferences. One paper that comes close is that of Bygren et al. (2017), which uses an audit study in Sweden to uncover evidence of employer preferences for hiring fathers. They find no evidence of systematic preferences for fathers, though it is important to note that the mechanisms behind the fatherhood and marriage premiums may differ, and that while the analysis can draw conclusions in terms of hiring, it cannot for wages.

Other studies of employer preferences focus on the gender gap more widely (see, for example, Bielby and Baron 1986, Carlsson 2011). The empirical evidence is therefore inconclusive but suggests that there may be a premium for married men linked to employers' preferences,

especially if it follows the trends of other gender-based employer preferences demonstrated in the literature. However, to confirm the existence of a premium caused by employer preferences a more complete analytical strategy is required. It is here where this paper makes one of its key contributions, which will be elaborated upon further in the following section.

The discussion of the employer preferences theory brings us to a third and final hypothesis:
H3 (employer preferences): Employers prefer married men to unmarried, and will therefore offer married men higher wages.

2.3 Institutional context, data and method

2.3.1 Country

Our paper uses data from Switzerland. Switzerland combines the dual education/apprenticeship system, strong links between education and employment, and industry-level wage bargaining common in central European countries such as Germany and Austria. However, lower levels of worker protection and less generous family allocations move it closer to more liberal economies such as Britain and the United States. While the women's employment rate is amongst the highest in OECD nations, the country also has one of the highest rates of female part-time employment, partly due to strong normative expectations for women to act primarily as child-rearers and housekeepers, with men expected to be the primary breadwinner for their family (Levy 2013, Valarino and Gauthier 2016). All this leads us to the expectation that employer preferences should weigh quite strongly on providing generous premiums for married men.

2.3.2 Data and methods

Longitudinal survey: overview.

We examine the existence of a male marriage premium by using a panel study based in Switzerland. The Swiss Household Panel (SHP) consists of 19 annual waves between 1999 and 2017 (Voorpostel et al. 2018). It contains a large number of socio-demographic variables as well as information on employment and wages. The SHP interviews a sample of approximately 10,000 individuals in 5000 households per year. It has a response rate of approximately 50% of a previous year's sample in the following year and has been "refreshed" twice, in 2004 and 2013. Information on children under 15 in a household is collected using a proxy questionnaire, and, as of 2007, new additions to the household who are not children of the respondents are also added to the sample, while respondents leaving the household are followed as a new household unit. Therefore the 2017 dataset contains a total of 35,060 individuals, from birth to old age, who have provided information for at least one wave.

We restrict our dataset to men aged 25 to 50, who were unmarried at their first observation: the dataset contains 6426 men in this age range, 1692 of who had never been married on the date of their first interview. This age restriction means we generally cover early careers without too much interference from those still in education (Swiss apprentices or interns may still report wages) and cover the first marriage of the vast majority of individuals who will eventually marry.

We further restrict our analyses to men employed as wage-earners who have reported wages across at least two waves, in order that we can run individual fixed-effects regressions. To carry out the entropy balancing matching procedure outlined in the method section, we must also disregard all person-years for which we do not have observations (actual or imputed) for the

variables used in the analysis. In order to maximise the number of observations available, where a respondent does not indicate marital status, we assume this status has not changed since their last response and do likewise with the matching and control variables. We use information on questions from annual wages and contractual working hours to construct the wage variable – we impute working hours from reported hours where contractual hours are missing but we do not impute income from any other sources than work, in case these values are confounded with other income sources and may therefore bias the analysis. Ultimately, these restrictions leave us with 847 persons and 6871 person years (8.1 observations per respondent), reduced to 707 persons and 4450 person-years, an average of 6.3 observations per respondent, to meet the entropy balancing requirement. This equates to slightly less than half of the total number of observations available for unmarried men aged between 25-50. 31% of the sample eventually marries.

Our focus is on the marriage premium and thus implies a comparison between married and single men. We do not consider divorced cases in the analysis, as we would expect different mechanisms to be at play. In any case, very few of the marriages observed in the sample end in separation or divorce in the timeframe of the survey – only 26 of the 264 marriages (10%). This number may seem low. However, given that we mostly observe only the *beginning* of marriages in the sample and divorce procedures in Switzerland can be lengthy, most divorces occur after 10 years of marriage. We also choose to focus specifically on civil marriage status: Switzerland has a lower instance of cohabitation compared to many countries (OFS 2016b) due to a legal regime that offers very little in the way of rights or recognition for non-married couples (Perelli-Harris & Gassen 2012). Cohabiting persons account for less than 10% of the cohorts born before the late-1980s – and these cohorts represent the vast majority of our

observations. Cohabitation is seen more as a transitional than permanent arrangement for the great majority of couples.

Longitudinal study: Analytical method

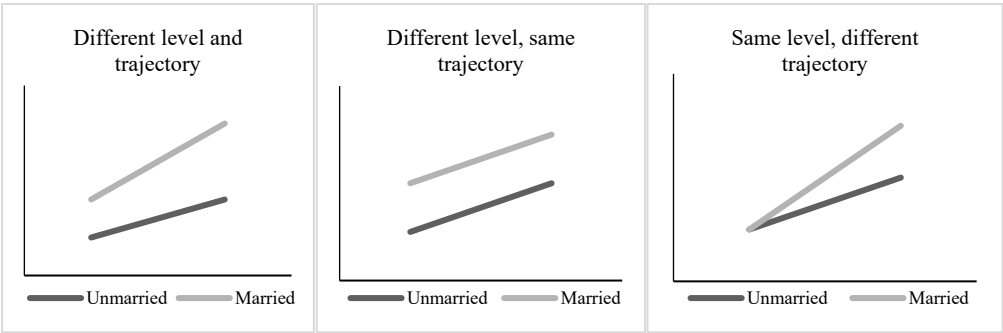
We first report the overall wage premium for married men using a pooled OLS regression, to define a baseline premium without any productivity and selection effects. To test the productivity hypothesis, we use a respondent-level fixed-effects model with civil status (married or unmarried) as the independent variable and a set of controls to capture productivity at work: management status, hours of work, job prestige, and participation in professional training. While the results will not be free of unobserved productivity differences, and it is not possible to completely disentangle productivity from possible effects of employer preferences, these variables should give some indication of the productivity effect in relation to the overall premium. We add a further step to the fixed-effects analysis by matching the never-married observations with those who marry at some point during the observation period, using pre-labour market variables for the matching: years of education; the prestige of the father's job based on the Treiman scale, which we recode into 5 levels following Hoffmeyer-Zlotnik and Warner's (2011: 47) classification based on autonomy, leadership, and task complexity; health satisfaction; and nationality (Swiss or non-Swiss). We also include the decade of birth to account for potential cohort effects (with 1959, the earliest year of birth in the sample, being included in the 1960-69 decade). We opt for entropy balancing to carry out the matching. Entropy balancing is a matching technique that calculates scalar weights for the treatment group which can then be used in a regression analysis (Hainmueller 2012). This strategy nullifies the problem of orphan observations while simultaneously acknowledging that outlier observations may nevertheless be of little relevance to the analysis by assigning them lower weights. As a robustness check, all the matched analyses are replicated using nearest-neighbour

matching with replacement, the results of which can be found in the appendix (Table A2.5), and which broadly conform with the results obtained by the entropy balancing analysis. The matching process has the benefit of creating a quasi-experimental design: by making sure that all our observations are the same on relevant variables before they marry, any remaining effect is likely caused by marriage either through productivity changes or a preference of employers for married men, though the issue of unobserved productivity differences persists.

Standard fixed-effects, even with matching introduced, will take into account selection into wage *levels*, but not wage *trajectories* – whether or not someone's wage grows faster irrespective of its starting point. Previous studies (see, for example, Killewald and Lundberg 2017; Cheng 2016) have indeed argued that selection may occur on both, and there is an obvious logic to this: a more productive man is also more likely to accumulate human capital more quickly and therefore enjoy faster-increasing returns. If such men select into marriage, then this ought to be taken into account. Figure 2.1 illustrates some possible scenarios of differing wage growth and levels to demonstrate the necessity of considering both of these dimensions in analyses of the male marriage premium. While the second case (different level, same trajectory) can be identified by standard fixed-effects, the first and third violate the parallel trends assumption and therefore will not be identified by such an analytical strategy, which assumes that the coefficient for each individual is the same, though the intercept may differ. Our solution to this quandary is to add a third analytical approach, following Ludwig and Brüderl (2018) by using a fixed-effects, individual slopes (FEIS) model. FEIS models apply the standard principles of FE models but add a step by estimating a separate slope for each individual, based on a relevant time-varying characteristic (Brüderl and Ludwig 2015). In this case, we take age as a proxy for work experience. This allows us to observe whether unmarried men's wages grow more slowly than the wages of the married, in addition to whether

or not they have different wage levels to begin with. Given that men on a steeper wage trajectory as well as on a higher wage level may select into marriage, the addition of the FEIS model allows for a stronger test of the selection hypothesis than using standard FE models alone.

Figure 2.1 Example wage growth scenarios for married/unmarried men.



Our dependent variable is the natural logarithm of hourly wages. We cap working hours at 60 per week to account for potential over-reporting of hours and address potential outliers by excluding wages above CHF 136 per hour, as well as any wages below CHF 20 per hour, corresponding to approximately the top and bottom 1% of the distribution. Inflation is controlled for by using 2015 constant Swiss Francs.

We estimate the following model for both the matched and unmatched samples:

$$Y_{it} = \beta_0 + \beta_1 CIVIL_{it} + \beta_2 AGE_{it} + \beta_3 AGE^2_{it} + \beta_4 CONTROLS_{it} + \alpha_t + \varepsilon_{it} \qquad (t=1, 2, \ldots T)$$

where Y is the natural logarithm of hourly wages of an individual *i* at time *t*. Our independent variable, $CIVIL_{it}$, is the time-varying dummy variable of the marital status (either never married or married) of an individual *i* at time *t*. AGE_{it} and AGE^2_{it} are covariates specifying the age of the individual. In our regression models, we include age in years as a covariate to account

for increasing job experience and the fact that our observations occur across a period in the life course when human capital and salaries increase very quickly. $CONTROLS_{it}$ is a vector of control variables designed to capture in-work productivity. α_t represents a set of time-variant unobserved characteristics that is perfectly correlated with all individual predictors, while ε_{it} represents the idiosyncratic standard error. We correct for the clustering of observations across waves by using clustered standard errors. For the FEIS model, age drops out of the equation and becomes instead the variable defining the individual slope for each person in the sample. The FEIS analysis is "data-hungry", requiring at least four observations per respondent to set the parameters of the individual slope, resulting in a smaller sample size (431 individuals, 3853 observations). To ensure any observed differences between the FE and FEIS models are indeed due to the change of model and not due to the change in number of observations, we reestimate the FE models on the smaller sample and present the results in the appendix (Table A2.7). The results of the analysis on this smaller sample are almost identical to those of the larger sample.

Survey experiment: overview.

Factorial survey experiments, or vignette studies, are a common analytical tool in many branches of social science (Rossi and Nock 1982, Jasso 2006) that have until recently been little used to study employer preferences. Since the early 2000s researchers have begun to utilise this method more frequently for these kinds of questions (e.g. de Wolf and van der Velden 2001, Di Stasio 2014, Liechti et al. 2017).

Factorial surveys can be used to effectively simulate the hiring process, by presenting employers and recruiters with vignettes that resemble the CVs of applicants, but which vary key dimensions of the profiles. Vignette studies have two key advantages over traditional surveys when analysing employer preferences. First, by randomly varying several attributes,

respondents are less likely to identify the changes and should therefore provide responses that are less prone to social desirability bias (Auspurg and Hinz 2015). Second, because factorial surveys use an experimental design, the researcher fully controls what is presented to respondents. This removes the possibility of unobservable characteristics being correlated to marriage such as personality traits and work attitudes, meaning that unlike traditional surveys, factorial surveys satisfy the requirements of experimental design.

Vignette studies are not without drawbacks: they present hypothetical information rather than real events and can at best be considered stated intentions. This means that while internal validity is strong, their external validity is weaker. These problems are amplified when the target population is a randomly drawn sample of adults or students, rather than professionals in the field.

Our factorial survey experiment nullifies the last of these issues by surveying human resources managers, recruiters and employers. We targeted a large association of human resources professionals with over 4000 members in Switzerland, who received a link to a web survey in 2016. 512 individuals provided at least one rating for a vignette, for a response rate of about 13%, which is at a similar level to other large-scale experiments of a similar nature (e.g. Liechti et al. 2017). Respondents were concentrated in urban areas, with the major population centres of Zurich and Bern especially overrepresented, and managers and recruiters working in large organizations making up over half of the sample. This does deviate somewhat from the Swiss labour market in general, which has a reasonably high percentage of the workforce employed in small and medium-sized firms and companies based outside the main population centres (OFS 2019a, b). With this in mind it is possible that our sample underestimates the effects of employer bias in the Swiss labour market overall. Larger organizations with professionalised

human resources services have been shown to discriminate less on personal characteristics of applicants (Fuller 2018), probably because their recruiters receive more training and instruction on avoiding judgements based on group stereotypes.

As well as the vignettes themselves, we collected information about the respondents. 63% were female and 37% male, close to the 60%-40% split in the HR profession in Switzerland overall, with an average age of 46. 70% responded to the survey in German and 30% in French, approximately mirroring the linguistic split in Switzerland. 60% were HR managers or directors, while slightly under 10% were owners of general managers of their firm. 93% had been actively involved in recruiting in the last 12 months, 82% with decisive influence, meaning our survey did indeed target recruiters.

The recruiters were told that our study sought to understand different hiring practices across Swiss regions. Three different job vacancies were described, with each vacancy followed by 4 vignettes – 12 in total per respondent. We selected three occupations – an accountant, a HR assistant, and a building caretaker ("concierge") – that in Switzerland would not be seen as male or female-dominated jobs – accountants are close to 50-50 male-female, while HR employees and building caretakers and cleaning/maintenance staff are both slightly over 60% female – and that cover a spectrum of skills required to carry out the tasks associated with the occupation. The vignette order was randomised. After each vignette, recruiters were asked how likely they were to invite someone for a job interview (on a scale from 0 to 10), and, regardless of the probability to invite him or her, to designate a monthly salary in Swiss francs, assuming the job was fulltime.

Our vignettes were made up of 11 dimensions including age, gender, nationality, civil status, children, type of education (vocational or general, reflecting Switzerland's dual-education system) and work experience. These dimensions represent standard information found in a CV: it is customary, though not obligatory, to report marital status and number of children on a CV in Switzerland, as in many other European countries. While this practice has seen some decline, it remains common. This resulted in a vignette universe of 5,529,600 vignettes per occupation, from which we drew an orthogonal (d-efficient) design of 1080 per occupation. All vignette dimensions and levels, an example vignette and the vignette correlation table can be found in the appendix – figure A1.1 and tables A1.1 and A.1.2.

Not all respondents provided answers to the questions on all vignettes. Taking out non-responses and restricting our sample to active recruiters, we are left with an analytical sample of 491 recruiters and 2834 vignettes – approximately 7.4 vignettes per respondent.

Survey experiment: analytical method.

We again use a fixed-effects regression as our primary analytical strategy, with civil status as the key independent variable. Here, the fixed-effects refer to the within-effect for survey respondents: by measuring only the differences within respondents, we take into account varying rating thresholds and the fact that it is likely that respondents will compare the vignettes they rate to those they have previously rated. 3 civil status levels were possible – single, married and divorced. Our analysis treats only the difference between single and married. As the candidates apply for different jobs, we use occupation and the interaction between occupation and civil status as controls. Although all dimensions are uncorrelated and should therefore have no impact on the results, we nevertheless also include education, nationality, and experience as controls to convince sceptical readers of our results.

We take the natural log of monthly wages, to determine whether the marriage premium is expressed in terms of higher wages for married men than unmarried. As a check for our results, we take the likelihood, between 0 and 10, of being invited to a job interview, to see whether the premium expressed in terms of recruiters' preferences for hiring married men over unmarried is similar to that of the wage premium. The general equation is expressed as:

$$Y_{ir} = \beta_0 + \beta_1 OCC_{ir} + \beta_2 CIVIL_{ir} + \beta_2 OCC_{ir} * CIVIL_{ir} + \beta_3 CONTROLS_{ir} + \alpha_r + \varepsilon_{ir} \quad (r=1, 2, \ldots R)$$

where Y_{ir} is either the logarithm of monthly wages for a vignette *i* evaluated by respondent *r*, or the likelihood of interview rating respondent *r* assigns vignette *i*. OCC_{ir} is a control for occupation, $CIVIL_{ir}$ is a categorical variable of civil status (either single, married, or divorced), $OCC_{ir} * CIVIL_{ir}$ is an interaction term and $CONTROLS_{ir}$ is a vector of control variables. α_r is a set of unobserved variables that correlate with all individual predictors. ε_{ir} is the idiosyncratic random error, and we correct for clustering of observations within respondents by using robust standard errors. Although it would also be possible to estimate ordered logit models for the analysis of the likelihood to invite to a job interview, the literature suggests that for data with a nested structure, fixed-effects linear models remain the best choice where individual heterogeneity needs to be taken into account, as well as for simplicity and intuitiveness of interpretation (Riedl and Gieshecker 2014). We nevertheless provide the results of an ordered logit analysis in the appendix (Table A2.6), which generally confirm the trends shown in the FE models.

2.4 Panel data evidence for the male marriage premium

2.4.1 Descriptive statistics, SHP

Table 2.1 sets out the mean values for our data sample for the independent, dependent and control variables for the full SHP sample, as well as for the group of married observations, and the group of never-married observations, both before and after the entropy balancing procedure, to show the effectiveness of the balancing algorithm.

Table 2.1 Descriptive statistics for SHP analysis

	Variable	All observations	Married	Never married, unweighted	Never married, weighted
	Married (in %)	46%	-	-	-
Independent variable	Average wage (CHF/hour)	46.84	49.2	44.84	45.59
	SD	16.87	17.19	16.34	16.75
Control	Age (years)	38.4	37.9	38.7	38.1
	SD	6.42	6.13	6.63	6.59
	Age cohort (1 = 1959-1969; 3 = 1980-1988)	2.53 (circa 1974)	2.59 (1975)	2.47 (1974)	2.59 (1975)
	SD	0.70	0.72	0.72	0.75
	Years of education	14.9	15.2	14.7	15.2
	SD	2.87	2.81	2.90	2.93
Entropy balancing variables	Health (satisfaction on 0-10 scale)	8.1	8.2	8	8.2
	SD	1.44	1.31	1.54	1.43
	Nationality (% Swiss)	89%	91%	88%	91%
	Social origin (1-5 ranking based on Treiman scale)	2.6	2.7	2.6	2.7
	SD	1.16	1.16	1.16	1.19
	N obs	4450	2043	2407	2407

The first thing to note is that the entropy balancing is indeed effective: All the balancing variables (age cohort, years of education, health satisfaction, nationality and social origin) are perfectly matched when weights are applied to the unmarried group. There is a 9% wage gap between the married and unmarried group that reduces to 7% following the balancing. For a fulltime job of 42 hours per week, this represents an annual premium of a month and a half's median wage in favour of married men over unmarried. The two groups are otherwise remarkably similar, with generally only slight differences: the single men are slightly older and marginally less educated but the difference in social origin is minute and there is almost no

difference at all in terms of health satisfaction. The married men group is slightly more Swiss than the unmarried but both groups are significantly more Swiss than the resident population in Switzerland as a whole, suggesting our dataset does not do a very good job of reaching foreign residents in Switzerland.

With so much similarity between the two groups it is likely that much of the gap in wages seen here is linked to the marital status. To confirm this, we turn to our fixed-effects analysis of the data.

2.4.2 Multivariate analysis

Tables 2.2-2.4 show the male marriage premium for men aged 25 to 50 in Switzerland, as obtained using pooled OLS, fixed-effects and FEIS models, respectively. Concerning the pooled OLS models in table 2.2, we see a premium slightly under 11% when controlling for age and education. Introducing covariates associated with in-work productivity, as well as parenthood status, the penalty is reduced by 1.4 percentage points, suggesting that productivity does make a small contribution to the penalty when looked at cross-sectionally – with the caveat that effects of employer preferences may also be at play in many of these variables.

We introduce selection into wage level and account for unobserved heterogeneity on the individual level by estimating fixed-effects models, the results of which are presented in table 2.3. The first model shows the results for the unmatched observations, where we see a premium to married men of 3.5%. This is a within-effect, meaning it measures the change over time in wages of each individual, and could therefore broadly be interpreted as a productivity effect, if no employer preferences towards married men are present and men on steeper wage trajectories do not select into marriage. This result is considerably lower than other estimates found using similar panel data analyses: Budig and Lim (2016), Killewald and Gough (2013) and Hersch

and Stratton (2000) find marriage premiums in the order of 7-9% in the US, while in Germany, Barg and Beblo (2007) estimate a wage premium for married men of 9.5%.

Table 2.2 Wage premium for married men, SHP, pooled OLS analysis

Dimension	*Level*	POLS (1)	POLS (2)
Marital status	Married	0.108***	0.094***
(ref. Single)		(0.001)	(0.009)
Age		0.063***	0.058***
		(0.008)	(0.007)
Age^2		-0.001***	-0.001***
		(0.000)	(0.000)
Years ed.		0.049***	0.035***
		(0.002)	(0.002)
Controls		No	Yes
Intercept		1.588***	2.09***
		(0.153)	(0.139)
	Observations	4450	
	R^2	0.305	0.430

Note: Pooled OLS regressions on (log) wages for men aged 25-50. Standard errors in parentheses. Control variables include parenthood status, job prestige (Treiman scale), hours of work and in-work training. Full M2 model is shown in the Appendix Table A2.1.
***p<0.01

Model 2 in table 2.3 adds a further dimension to the analysis of the selection effect by reweighting the analytical sample with entropy balancing, effectively making the group of never-married men and those who do marry the same on a set of pre-labour market socio-economic characteristics. Any remaining premium should therefore be the direct result of the fact of being married, net of any pre-labour market selection effects. This model finds that the premium has been reduced to 3.4%. Adding entropy balancing has only as small effect, because the fixed effects have already accounted for much of the selection effect. The entropy balancing models are nevertheless a valuable addition: the small change reflects the fact that entropy balancing estimates weights for all observations to make the married and never-married groups identical on a set of pre-labour market predictors of probability, while avoiding orphan observations and reducing the influence of outliers. In models 3 and 4 we repeat the analyses, adding the vector of controls. These covariates reduce the premium in both the matched and unmatched cases by 0.4 and 0.2 percentage points, respectively, a smaller effect than in the

POLS models. This suggests that while productivity may have some effect on the premium, selection is by far the stronger mechanism.

Table 2.3 Wage premium for married men, SHP, fixed-effects regression models

Dimension	*Level*	FE: Unmatched (1)	FE: Matched (2)	FE: Unmatched (3)	FE: Matched (4)
Marital status	Married	0.035***	0.034***	0.031**	0.029**
(ref. single)		(0.019)	(0.020)	(0.017)	(0.018)
Age		0.076***	0.076***	0.081***	0.079***
		(0.013)	(0.013)	(0.011)	(0.012)
Age^2		-0.001***	-0.001***	-0.001***	-0.001***
		(0.001)	(0.001)	(0.000)	(0.000)
Controls		No	No	Yes	Yes
	Individuals		707		
	Observations		4450		
	Within R^2	0.219	0.212	0.360	‘.360

Note: Individual fixed-effects regressions on (log) wages for men aged 25-50. Clustered standard errors in parentheses.
***p<0.001, **p<0.01
Observations in the matched sample are matched on social origin (Treiman scale), nationality, decade of birth, years of education, health satisfaction. Control variables include parenthood status, managerial status, job prestige (Treiman scale), hours of work and in-work training. Full M3 and M4 models shown in the Appendix Table A2.1.

In the next step, shown in table 2.4, we remove the matching weights and consider the different wage trajectories of married men compared to single through the FEIS model. Here, we find the premium is similar to the matched FE model without additional controls, at 3.6% (table 2.4, model 1). These two findings are in line with more recent empirical research showing that selection accounts for between 50% and 80% of the premium (see Petersen et al. 2011, Nakosteen and Zimmer 2001). Our results are in line with those of Ludwig and Brüderl’s (2018: 762-763) longitudinal analysis of the premium in the US, which suggests that almost all of the premium can be explained by either married men being in a higher wage track or on a faster growing wage trajectory, particularly when we add the vector of controls for productivity to model 2. Here we find that the premium reduces to 2.6% and is no longer statistically significant. The difference between this model and the matched FE model, could be taken as confirmation that the pre-labour market productivity characteristics used in the matching procedure are indeed linked to later productivity and a faster-growing wage.

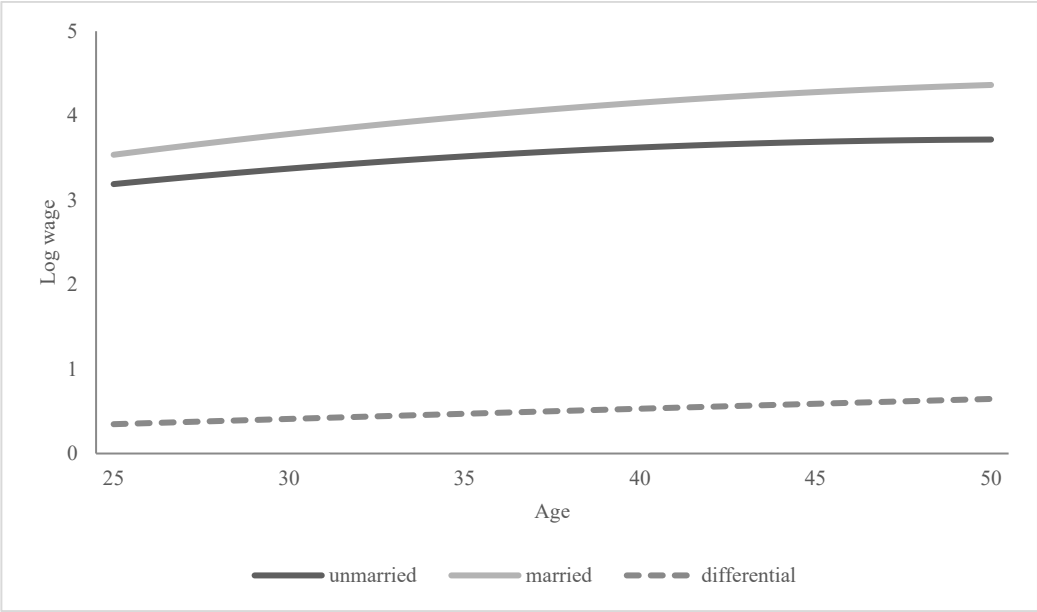
Table 2.4 Wage premium for married men, SHP, fixed-effects individual slope models

Dimension	*Level*	FEIS (1)	FEIS (2)
Marital status	Married	0.036*	0.026
(ref. single)		(0.018)	(0.018)
Controls		No	Yes
	Individuals	431	
	Observations	3853	
	Within R^2	0.001	0.239

Note: Individual fixed-effects regressions with individual slopes on (log) wages for men 25-50. Clustered standard errors in parentheses.
Age and age-squared used to calculate individual slopes. Control variables include parenthood status, managerial status, job prestige (Treiman scale), hours of work and in-work training. Full M2 model shown in the Appendix Table A2.1.
*p<0.05

To visualise the differing wage curves of married and unmarried men, in figure 2.2 we plot the predicted values of table 2.4, Model 1 for the median wages of the married and never-married man. This graph shows the importance of taking into account both the wage level and trajectory: the married man both begins on a slightly higher level than the single and has a somewhat steeper wage trajectory. Combined with the 3.5% marital premium, the model predicts a steadily increasing wage differential across the career that is mostly driven by the difference in the wage curve: from a 20% wage difference at the beginning of the career, the married man can expect average wage increases of over 3% per year, while the unmarried man's increases are in the order of slightly over 2%. This represents a mild version of the "different level and trajectory" plot shown in figure 2.1, and justifies the inclusion of models that can account for both.

Figure 2.2 Wage curves for married and unmarried men by age (log wages)



Predicted values derived from FEIS model reported in Table 2.4, Model 1.

These results suggest that in Switzerland at least, the selection of more productive men into marriage has a considerably larger impact on the male marriage premium than productivity changes stemming from marriage, or, possibly, employer preferences. We explore this last element of the premium – employer favouritism – in further detail the next section.

2.5 Experimental evidence for the male marriage premium

2.5.1 Fixed-effects analysis

Our analysis of Swiss Household Panel data points to a baseline, cross-sectional male marriage premium of approximately 11%, reduced to 3-3.5% when taking into account selection into wage levels and trajectories. If any part of this unexplained component is due to the preferences of employers, we should expect to see evidence of it in our factorial survey, which explores the hiring preferences of firms and organizations by surveying HR professionals, company managers and business owners. Table 2.5 shows the effect of marriage on the wages our sample of employers and recruiters consider adequate for male job candidates. The first model is a

simple regression of civil status on wages, where we see a small premium of 1.3% for married men compared to single. This result would seem to be in line with our panel data analysis.

Table 2.5 Wage recommendations for men depending on their civil status

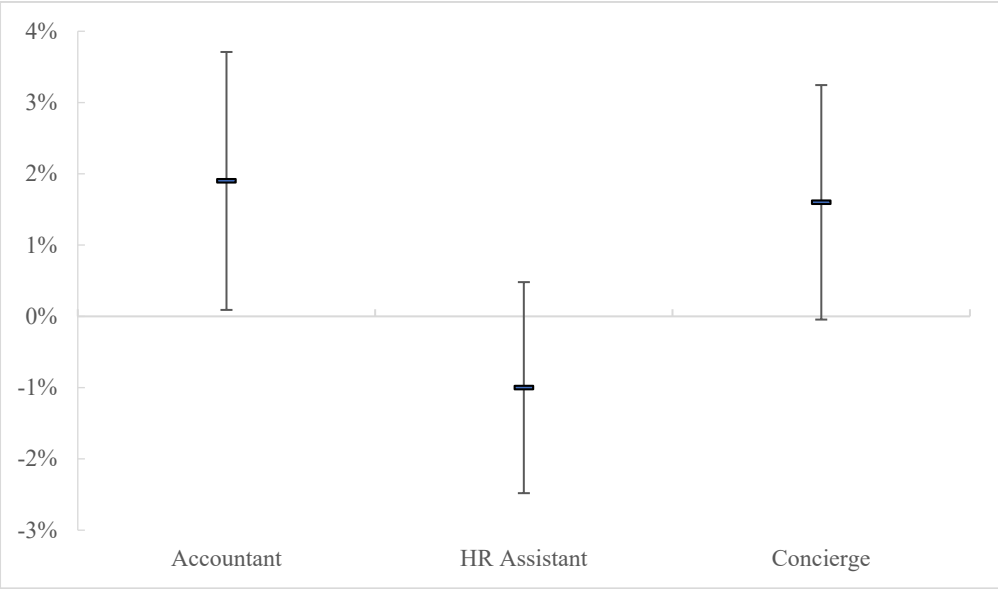
Dimension	*Level*	Model 1	Model 2	Model 3	Model 4
Marital status (ref: unmarried)	Married	0.013 (0.011)	0.009* (0.006)	0.021** (0.011)	0.019* (0.012)
Occupation (ref: Accountant)	HR assistant		-0.18*** (0.01)	-0.167*** (0.013)	-0.167*** (0.013)
	Concierge		-0.348*** (0.008)	-0.352*** (0.014)	-0.352*** (0.014)
Interaction terms (ref: Acct, unmarried)	HR assistant*Married			-0.031** (0.014)	-0.030** (0.014)
	Concierge*Married			-0.004 (0.015)	-0.003 (0.015)
Controls		No	No	No	Yes
	Within R^2	.001	.6968	.6983	.7031
	Respondents		395		
	Observations		2083		

Note: Respondent fixed-effects regressions on (log) wages for men. Clustered standard errors in parentheses. Additional controls in M4 include: children, nationality, education, experience; full M4 model is shown in the Appendix Table A2.2. Wald test for joint significance of interactions (M3): Chi-squared(4, 1680) = 8.667, p<0.1. *p<0.1; **p<0.05; ***p<0.01

Our profiles, however, refer to three different occupations with differing job functions and cognitive and physical requirements, and therefore in model 2 we control for occupation. Despite a small reduction in the size, we find that the premium for married men of approximately 1% becomes statistically significant at p<0.1. In a third model, we follow the method of Oesch et al. (2017) and account for the fact that the effect of marriage on recruiters' wage indications are likely to vary between the three occupations by introducing an interaction term between occupation and civil status. Here we see an increase in the premium from the model without interaction terms, to a premium of 2% for the reference occupation of accountants. Introducing further controls in model 4 results in no significant changes, as should be expected from a survey experiment. Full regression tables including all controls can be found in the appendix (Table A2.2).

The coefficients reported in models 2-4 refer to the reference category of accountants. The interactions show us that there is indeed a difference between occupations in how civil status affects wages. Figure 2.3 illustrates these differences: a married HR assistant in fact receives a 1% wage penalty, while a married concierge receives a premium of 1.5%, though in these cases the 90% confidence interval crosses 0.

Figure 2.3 Wage recommendations for married men, by occupation (ref single)



Note: Results from respondent fixed-effects regressions on (log) hourly wages for men, model with control and interaction terms. See the appendix (Table A2.2) for regression tables including the full models. 90% confidence intervals shown.

2.5.2 Further analyses and robustness checks

Our factorial survey, then, shows small premiums for married men in two of the three occupations, in a context where marriage is uncorrelated from productivity and selection thanks to the experimental design of the survey. We are therefore able to more clearly isolate the effect of marriage itself on wages, and can surmise that employer preferences do in some cases affect the marriage premium, though certainly to a lesser degree than selection and on a similar scale to productivity, as best as we can measure it in the panel data.

Our respondents were also asked to rate the likelihood of inviting profiles to a job interview, a process which also requires expressing a preference for a certain type of candidate. Given that our wage regressions report large standard errors, we can use these likelihoods to interview values as a robustness check – if they also report preferences for married men, this is a strong indication of the presence of employer preferences in favour of married men. Table 2.6 reports the regression results for the likelihood to get a job interview dependent the on civil status. Here we see a preference of 0.22 points for married men in the empty model, remaining similar when controls for occupation are added.

Table 2.6 Probability to invite to a Job interview (on a scale from 0 to 10) for men depending on their marital status

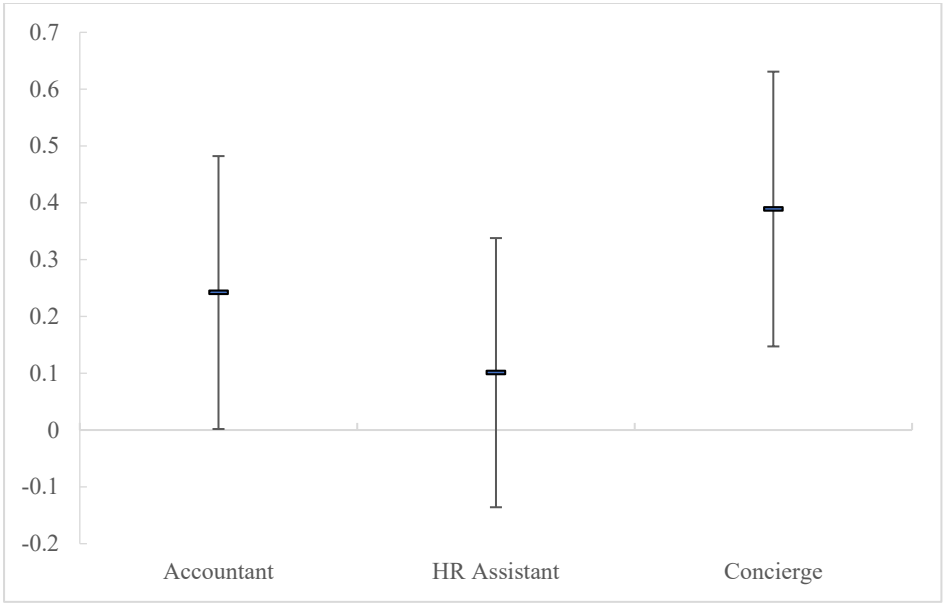
Dimension	*Level*	Model 1	Model 2	Model 3	Model 4
Marital status	Married	0.217*	0.237**	0.250*	0.242*
(ref: unmarried)		(0.091)	(0.083)	(0.131)	(0.132)
Occupation	HR assistant		-1.088***	-1.137***	-1.144***
(ref: Accountant)			(0.11)	(0.15)	(0.15)
	Concierge		0.411***	0.265*	0.267
			(0.103)	(0.164)	(0.163)
Interaction terms	HR assistant*Married			-0.172	-0.141
(ref: Acct, unmarried)				(0.196)	(0.197)
	Concierge*Married			0.136	0.147
				(0.206)	(0.207)
Controls		No	No	No	Yes
	Within R^2	0.003	0.145	0.148	0.1541
	Respondents		491		
	Observations		2665		

Note: Respondent fixed-effects regressions on likelihood to invite to a job interview for men. Clustered standard errors in parentheses. Additional controls in M4 include: children, nationality, education, experience; full M4 model is shown in the Appendix Table A2.2. Wald test for joint significance of interactions (M3): Chi-squared(4, 2166) = 7.875, p<0.1. *p<0.1; **p<0.05; ***p<0.01

Figure 2.4 shows that although married men are generally preferred over single, this preference varies in size from 0.1 point for HR assistants (where the 90% confidence interval crosses 0), through to 0.24 points for accountants and 0.39 points for building concierges – for comparison, this is a greater preference shown than the preference for Swiss men over candidates with a Turkish background (0.26 points). There may be several explanations for the

fact that the values for hiring preferences are greater than those for wages – expressing a preference for inviting to a job interview for fictive profiles may be a simpler process for recruiters than allocating wages, or, recruiters prefer married men over single but are reticent to express this in offering higher wages. Either way, the results point to a preference for married men, reinforcing the notion that employers are prone to favouring married men, at least at the higher and lower ends of the job prestige scale.

Figure 2.4 Probability of inviting for a job interview (on scale from 0 to 10) for married men, by occupation (ref unmarried)



Note: Results from respondent fixed-effects regressions on probability of inviting candidate for a job interview, model with control and interaction terms. See the appendix Table A2.2 for regression tables including the full models.
90% confidence intervals shown.

2.6 Discussion and conclusion

This paper has made a twofold contribution to the discussion of the male marriage premium. First, we confirm the existence of the male marriage premium in Switzerland using national panel data, finding a baseline premium of 11% with a pooled OLS model. This decreases to approximately 3-3.5% using fixed-effects models with matching of individuals who remain unmarried across the period of observation with individuals who start single but marry during the period of observation, and when we account for different wage trajectories between married and unmarried men, arguing for a strong selection effect. Controlling for productivity differences further reduces the penalty, though only by small amounts. In terms of our initial hypotheses, depending on which model we use, we find some evidence that marriage makes men productive, in line with H1. H2 – that more productive men select into marriage – is clearly confirmed by the data.

Following our panel data analysis, we use a survey experiment amongst Swiss recruiters to ascertain how much of the unexplained component is due to employer preferences. We find that recruiters give a 1-2% premium to married men, but that this varies based on occupation. These results conform with the lower-end coefficients from our panel data analysis, and suggest that in some cases there is a small effect of recruiters' preferences for married men on wages. Our third hypothesis (H3) – that employers prefer married men and therefore offer them a higher wage – is partially confirmed, with this being the case for occupations at the top (accountant) and bottom (concierge) of the workforce, but not in the middle (HR assistant).

We also uncover that wages are only part of the employer preferences story. We find somewhat stronger preferences expressed for married men over single in terms of the probability of inviting for a job interview – in the case of concierges, up to one-third of a point on an 11-point

scale, comparable to, and in fact sometimes larger than the premium for Swiss applicants as opposed to foreigners. This suggests that more than rewarding married men in terms of the salaries offered to them, employers also express their preferences earlier, at the point of interview. This is a unique contribution made by this paper, as modelling outcomes for yet-to-be hired profiles is very difficult using survey data and while possible with audit studies, in this context only an indication of whether or not a profile would be invited to a job interview is possible.

We see differences between occupations, with the higher (accountant) and lower (concierge) prestige jobs more affected than the middle occupation (HR assistant). There is evidence that social norms of the good working husband and stay-at-home-wife are stronger in higher-prestige jobs (Carlsson 2011, Berghammer 2014), while in lower-prestige jobs, reliability is valued, and marriage sends a strong reliability signal (Schwartz 1990). These observations seem to be reinforced by the results of this analysis.

Our analysis is not without its drawbacks. Concerning the survey experiment, it is clear that while such a setup has the benefit of estimating effects without confounders, given its hypothetical nature, the results can at best be considered stated intentions and not necessarily predictions of actual behaviour – though research by Hainmueller et al. (2015) suggests that responses given in an experimental context are in fact quite close to real-world behaviour. Our estimates are also likely to be lower-bound – research by Fuller (2018) into the presence of a motherhood wage penalty in Canada suggests that a professional HR department decreases the penalty. We would expect the same to hold true for marriage premiums, given the employer preferences mechanisms are similar. Given that most Swiss work in SMEs, which often do not have a dedicated HR department, it is likely that the actual effects of employer preferences are

higher than those we find in our survey experiment, which is heavily biased towards large enterprises in urban areas which are more progressive and have vastly superior employment and family services than rural areas.

Our analysis also raises both policy and methodological issues that require consideration and discussion. Employers and recruiters attribute slightly higher wages to married men than single for certain occupations, suggesting that employer preferences, based either on statistical discrimination or social norms in Switzerland do still have some (small) impact on wage decisions when it comes to civil status in some cases. Moreover, we find a preference for married men at the point of interview, and that this "interview premium" is stronger than the wage one in our survey experiment analysis. In other words, it is harder for unmarried men to get a "foot in the door" than their married counterparts. Given this is the case even where civil status has no relation to productivity at work, it is high time that the practice of reporting personal information such as civil status on CVs, still common in many European countries, be left in the past. Moreover, more effort should be put into ensuring that recruiters and HR staff are not acting on such information should it still be present in the CV or become apparent during the interview process.

Finally, using analytical strategies that can identify causal relationships and analyse employers and HR staff directly is important when wishing to understand questions of discrimination, bias, or favouritism on the part of employers. This paper has shown that their effect is non-negligible – they should not be brushed aside as irrelevant or too difficult to analyse.

3. The wage penalty for motherhood: evidence on discrimination from panel data and a survey experiment for Switzerland[1]

3.1 Introduction

Mothers tend to earn lower wages than non-mothers across the Western World. While differences in work experience and job characteristics go some way in accounting for this gap, most studies find a non-trivial unexplained wage residual. In Britain, Germany and the United States, this residual varies between a low of 3-5% and a high of 8-10% per child and suggests that mothers incur an earnings penalty (e. g. Budig and England 2001, Gangl and Ziefle 2009, Glauber 2007, Killewald and Gough 2013, Harkness 2016).

The unexplained wage residual for motherhood may arise from two sources: from the unobserved effects of motherhood on work productivity, or from employer discrimination against mothers. The challenge faced by the literature is to empirically disentangle the influence of these two sources (Mu and Xie 2013). Although surveys ask detailed questions about training, tenure and occupation, it is impossible to measure all dimensions of work productivity. Therefore, if the goal is to open the black box of the motherhood wage residual, one needs to resort to experimental methods that tap directly into discrimination (Correll et al. 2007: 1332).

However, while experiments provide high internal validity and allow researchers to draw causal conclusions, their external validity and hence the degree to which the results can be generalised to other contexts is often limited. In the context of the motherhood wage penalty,

[1]This chapter is co-authored with Daniel Oesch and Oliver Lipps and has been published in *Demographic Research*, *37* (2017), 1793–1824.

it is doubtful whether the wage recommendations of undergraduate students in an experimental study accurately reflect the actual wage-setting behaviour of employers. Consequently, the two analytical methods – population surveys and experiments – should be combined within the same study to increase the robustness of findings.

This paper tries to do so and examines the motherhood wage gap by drawing on evidence from both panel surveys and a survey experiment. We first use two nationally representative longitudinal surveys and fixed-effects regressions to examine whether having children is associated with lower wages for working mothers. This allows us to establish the size of the unexplained wage residual for motherhood. We then test wage discrimination directly by carrying out a factorial survey experiment, a method also known as vignette study. We show the résumés (vignettes) of fictional job candidates to 714 HR managers and ask them to indicate the wage that seems adequate for these candidates. By randomly varying a set of dimensions for each vignette (such as education, age, nationality, children), we are able to identify if – and how – the presence of children affects the wages that recruiters assign to candidates.

Our study uses data from Switzerland, a country that shares many labour market features with Germany, notably a large vocational education system, a tight link between education and employment, and wage bargaining at the industry level. However, Switzerland offers much less public support for maternal employment and, in terms of family policy, is more closely aligned to Britain or the United States. To the extent that comparative research has found systematic differences in the motherhood wage gap between these countries (Gangl and Ziefle 2009, Gash 2009), adding evidence on Switzerland helps us to better understand how this gap varies across institutional settings.

Our study is the first to combine the analysis of longitudinal surveys – the Swiss Household Panel and the Swiss Labour Force Survey – with a factorial survey experiment. The use of national population surveys provides us with insight into the supply-side of the labour market – workers and their wages – and gives us strong external validity. The factorial survey experiment on recruiters, in turn, informs us of the demand-side of the labour market – employers and their ratings – and helps us to unravel causal effects. Unlike earlier experimental studies on the topic, our vignette study does not rely on undergraduate students (Correll et al. 2007) or the general employed population (Denny 2016), but on active recruiters and thus on individuals who, unlike students or workers, actually wield influence over wage setting.

We develop our theoretical argument in section 2 by discussing the two mechanisms behind the motherhood wage gap: productivity and discrimination. Section 3 presents the institutional context of Switzerland and explains the design of our panel studies and factorial survey experiment. Section 4 provides empirical evidence for the population surveys and section 5 for the factorial survey experiment. Section 6 summarises our key findings.

3.2 The theoretical links between motherhood and wages

3.2.1 A motherhood wage gap driven by productivity

Motherhood may hamper the evolution of wages because having children often interferes with mothers' labour market attachment and thus reduces their productivity at work. On the one hand, children may lead to career interruptions and thus slow down mothers' accumulation of human capital. On the other hand, raising children takes time and effort and may leave mothers with less energy for paid work.

According to the first explanation, the birth of children increases the likelihood that women interrupt their careers and thus accumulate less tenure and work experience than they would have without children. Survey research has identified mothers' reduced work experience as the key determinant of the motherhood wage gap (Gangl and Ziefle 2009). In the United States, the sharp increase in wages for extended work hours – and mothers' lower propensity to take on such jobs – has further contributed to the wage penalty associated with children (Weeden et al. 2016). More generally, when returning to employment, mothers often take on part-time jobs. This more tenuous attachment to the labour market decreases the incentive for mothers – and their employers – to invest in training and further education (Polavieja 2012). As a result, motherhood may slow down the growth in job-specific skills and lead to flatter career trajectories.

An alternative explanation also expects motherhood to decrease productivity at work. However, the central mechanism is not children's effect on human capital, but on time and energy. The idea is that raising children takes effort and may thus interfere with the demands of paid work. To the extent that mothers carry the brunt of childcare – such as picking children up from school, caring for them when sick, attending their activities – they may have less energy to bring to the labour market and be less productive workers (Budig and England 2001: 206, Gangl and Ziefle 2009: 342).

This argument is taken a step further in the theory of compensating differentials. In the traditional household division of labour, mothers are expected to trade wages for family-friendly employment that is compatible with childcare duties (Becker 1973). Mothers may thus avoid jobs that pay well, but make great demands in terms of constant availability, non-standard hours, overtime work, or long travels. Once they have children, mothers may therefore switch

to jobs with predictable work schedules – and hence forgo well-paying jobs for part-time jobs in the public sector that allow them to take time off for family duties.

What does the empirical evidence tell us? The best studies on the motherhood wage gap use longitudinal surveys that make it possible to include person-specific fixed-effects that account for women's time-constant predispositions and ability. If we focus on studies with this design, there seems to be a gross wage penalty for the United States of 5 to 10 percent per child (Budig and England 2001: 219, Killewald and Gough 2013: 488). When controlling for work experience, seniority and shifts to family-friendly jobs, the motherhood wage gap in the United States decreases to about 3 percent per child (Budig and England 2001: 219, Glauber 2007: 955, Kahn et al. 2014: 66). The jury is still out as to whether the motherhood penalty is larger among low-earning (Budig and Hodges 2010, Cooke 2014) or high-earning women (England et al. 2016).

In a panel study of American, British and German mothers, once work experience is accounted for, the motherhood wage gap disappears in the United States and Britain, but remains at over 10 percent per child in Germany (Gangl and Ziefle 2009: 363). A study using the European Community Household Panel also finds a particularly large gap for Germany and, to a lesser extent, for Britain (Gash 2009). In contrast, there does not seem to be much of a wage penalty for mothers in Denmark, Finland (Gash 2009) or Norway (Petersen et al. 2014).

3.2.2 A motherhood wage gap driven by discrimination

The finding of a sizeable wage gap associated with motherhood leaves us wondering whether this unexplained residual is due to unobserved differences in productivity – or to employer discrimination.

Traditionally, sociologists have been critical of the idea that wages are solely determined by workers' marginal productivity (e. g. Jacobs and Steinberg 1990: 460). The importance of the bargaining process for wage setting suggests that non-economic factors such as power resources, cultural beliefs and social norms also affect workers' earnings, leaving room for discrimination.

The notion of statistical discrimination assumes that one group of workers (e.g. women with small children) are less productive than another group (e.g. women without small children). Since measuring the productivity of each individual is costly and time-intensive, employers pay higher wages to workers from the more productive group (Correll et al. 2007: 1302). Some employers possibly use the presence of children to infer women's unobserved productivity and create a wage penalty for mothers relative to non-mothers.

Rather than statistical discrimination, the wage gap may be driven by social norms. This is the case if employers – consciously or unconsciously – favour one social group over another based not on the group's expected productivity, but on their own cultural beliefs. One such cultural belief expects a conflict between the normative demands on the ideal worker – to be constantly available for his or her employer – and the normative expectations on the good mother – to be constantly available for her children (Correll et al. 2007: 1306). The dominant social norm across much of the Western World, notably in the German-speaking countries, considers mothers' primary role to be at home with their children, whereas her paid job appears of secondary importance. The opposite norm applies to fathers whose primary role is to play the breadwinner, not to be the caretaker at home (Krüger and Levy 2001). Fathers are therefore seen as needing a family wage to support wife and children, whereas mothers appear as secondary earners whose wages merely supplement total household income. Motherhood may

thus be a status characteristic that yields lower expectations about the adequate wage (Auspurg et al. 2017: 182).

An American study combining an experiment among students with an audit study among employers shows that childless women are rated significantly higher than mothers in terms of competence, work commitment, promotion prospects, and recommendations for hire – with otherwise identical résumés (Correll et al. 2007). The participants in the experiment recommended a starting wage for mothers that was 7 percent lower than the wage offered to women without children. Likewise, the audit study showed that prospective employers called mothers back only half as often as childless women. In comparison, fathers were not disadvantaged at any stage of the hiring process (Correll et al. 2007: 1333).

While research shows discrimination against white mothers in the United States, the evidence of discrimination against African-American or Latino mothers is weaker. A vignette study among employed adults (Denny 2016) and a longitudinal survey study (Glauber 2007) find a wage penalty in the U.S. only for white mothers, but not for mothers from minority groups. One explanation is that the dominant social norm of the "good mother" who should be constantly available for her children only applies to white women. In contrast, as women of colour "were incorporated into the United States largely to [serve] as labour", their family life and status as mothers may seem secondary to their status as workers (Denny 2016: 30).

3.3 Country selection, data and methods

3.3.1 Country differences in the wage penalty

Our study analyses the extent of the motherhood wage gap for Switzerland. The Swiss labour market shares many commonalities with Austria and Germany, notably a strong reliance on vocational education, an occupational labour market with a close link between education and employment, collective bargaining at the industry level, and low unemployment. However, in terms of family policy and public support for maternal employment, Switzerland comes closer to the U.S. and Britain than Germany. Swiss legislation only provides for 14 weeks of paid maternity leave, and there is no statutory right to either parental leave for fathers or subsidised childcare. Institutional childcare is expensive and covers a minority of children below 4 years. Moreover, since a normal full-time job implies long weekly hours (an average of 41.7 hours) and short holidays (20 days per year), the great majority of mothers in Switzerland work part-time. Among mothers with at least one child below 6 years, 83 percent work part-time (OFS 2016a).

The weakness of public support for reconciling family and work implies that mothers in Switzerland are, much like in the United States, "exposed to the unfettered operation of … labour markets" (Gangl and Ziefle 2009: 347). Interestingly, comparative studies indicate a larger raw wage gap between mothers and non-mothers in Britain and the United States than in Germany. However, once differences in work experience and human capital are accounted for, the net wage gap is larger in Germany than the two English-speaking countries (Gangl and Ziefle 2009, Gash 2009). Depending on whether the motherhood wage gap is driven by the institutions governing the labour market or by family policy, one would expect results for Switzerland to be more closely aligned to those of Germany or the UK and US.

3.3.2 Longitudinal surveys

Data

Survey data on wages and working hours are fraught with missing observations and measurement error. We therefore try to increase the robustness of our results by following the practice of "identical analysis of parallel data" and use two different data sets (Firebaugh 2008). The replication across two panel studies permits us to gauge the uncertainty in the results due to common errors in surveys linked to coverage, sampling, non-response and measurement (Groves 2004).

We use the Swiss Household Panel 1999-2015 (SHP) which provides us with 17 annual waves between 1999 and 2015 (Voorpostel et al. 2016) and the Swiss Labour Force Survey (SLFS) which included, between 1991 and 2009, an annual rotating panel that followed the same respondents for five years. While the SHP is a longer running panel that covers larger stretches of mothers' careers and includes a richer choice of variables, the SLFS has been specifically designed for the analysis of the workforce and provides us with a much larger sample.

For both datasets, we restrict our analytical sample to women aged 20 to 50 and person-years when respondents were employed as wage-earners. We only include respondents with wage observations in at least two waves. For the SHP 1999-2015, this leaves us with 3,115 persons and 12,769 person-years, each respondent contributing, on average, 4.1 years of observation. For the SLFS 1991-2009, this leaves us with a sample of 26,409 persons and 71,531 person-years, each respondent contributing 2.7 years of observation.

Measures

Our dependent variable is hourly wage, which we obtain by dividing monthly wages by working hours. In the SHP, we use contractual working hours and supplement them, where missing, with usual working hours. In the SLFS, we use reported working hours and, where missing, translate the percentage of employment into weekly hours (employment of 100% corresponding to 42 hours). In order to account for "overwork" (Weeden et al. 2016), we cap weekly working hours only at 60 hours in both datasets. We control for inflation and use the natural logarithm of constant Swiss francs (for 2010 in the SHP, 2011 in the SLFS), excluding potential outliers by dropping the top and bottom 1 percent of the hourly wage distribution.

Our key independent variable is the number of children in the household, measured as a categorical variable: no child, one, two, three or more children. In the SHP, we only include biological children. In the SLFS, the child variable captures all children aged 15 or under who live in the household (before 1997, only biological children were recorded)

We use a set of controls for differences in *work productivity*: years of education (SHP) or detailed levels of education (SLFS), participation in job training, supervisory status and, in the SHP, whether the respondent reports to be over-, under- or inadequately qualified for her job. Since there are no direct measures of work experience, we construct our own measure in the SHP by calculating, for each person-year, the observed years in employment. In the SLFS, we are forced to use job tenure as proxy for experience.

We account for *compensating differentials* with control variables for the public sector, part-time employment, fixed-term contracts, firm size and, for the SHP, job change and employer

change in the last year. We further control for the share of women in a given occupation[2] (Murphy and Oesch 2016) and, in the SHP, for the prestige of an occupation (Treiman scale). In the SLFS, we further control for occupation (ISCO 1-digit) and sector (NOGA). Finally, we enter variables for marriage and, in the SHP, the availability of external childcare and the weekly number of hours spent on housework (capped at 42 hours). All our models include dummies for the calendar year and age in years which allows us to account for the age-earnings curve between 20 and 50.

Model specification

The effect of children on wages may be plagued by unobserved heterogeneity. If less productive women are more likely to have children, this leads to a spurious correlation between the number of children and wages. It has thus become standard to estimate the motherhood wage gap with fixed-effects panel models (e.g. Budig and England 2001, Gangl and Ziefle 2009, Kahn et al. 2014). The fixed-effects estimator only takes account of the within-variance stemming from changes in women's lives over time. This eliminates all observed and unobserved characteristics of the individual that do not vary over time such as time-constant preferences and abilities which may affect both the decision to have children and the evolution of wages. In contrast, fixed-effects do not eliminate unobserved *time-varying* characteristics that affect both a woman's fertility decisions and her wage (Mu and Xie 2013).

The general equation for our person fixed-effects linear regressions is given as:

[2] We calculate the proportion of women in a job at the level of ISCO-1988 2-digit occupations on the basis of the Swiss Labor Force Survey for each year – and impute these proportions to the SHP.

$$Y_{it} = \beta_0 + \beta_1 CHILDREN_{it} + \beta_2 CONTROLS_{it} + \alpha_i + \varepsilon_{it} \qquad (t=1, 2, \ldots, T)$$

where Y is the logarithm of hourly wages for an individual *i* at time *t*. Our main predictor $CHILDREN_{it}$ is a time-varying categorical measure of the number of children of a woman *i* at time *t*. $CONTROLS_{it}$ is a vector of control variables. We remove all time-invariant characteristics that differ between individuals by allowing α_i, a set of unobserved time-constant variables, to correlate with all of our individual predictors. $\boldsymbol{\varepsilon_{it}}$ represents idiosyncratic error. Finally, we correct for the clustering of observations across waves by using panel-corrected standard errors.

3.3.3 Factorial survey experiment

The logic behind factorial survey experiments

The second part of our empirical analysis is based on a factorial survey experiment. Factorial surveys, also known as vignette studies, have been used in social sciences for decades (Rossi and Nock 1982), but were taken up only recently in economic sociology to study the preferences of employers (e.g. Di Stasio 2014, Liechti et al. 2017).

Factorial surveys simulate the hiring process by presenting the résumés of fictitious job candidates to respondents and asking them for ratings. They have two advantages over conventional surveys. First, they are less subject to social desirability bias. Sensitive attributes (dimensions) such as age, gender, nationality or motherhood status are randomly combined in the résumés (vignettes). These ever changing combinations make it difficult for respondents to pick out and provide a politically correct answer to the one dimension which researchers are interested in. Auspurg et al. (2017) illustrate this point in a study of the gender wage gap.

Second, factorial surveys use an experimental set-up where the researcher fully controls the information shown to respondents. This eliminates a source of bias that is omnipresent in conventional surveys: unobserved characteristics (such as the abilities of a job candidate) that are collinear with the variable of interest (such as wages).

Note, however, that factorial surveys are experiments and thus present hypothetical scenarios and only reflect stated intentions, not observable actions. Accordingly the external validity of findings is open to discussion. This is particularly the case when survey experiments on recruitment are done with students (Correll et al. 2007) or random adults (Denny 2016), whose judgements may well differ from the specific group of recruiters.

The construction of our experiment

Our experiment addresses this last issue by surveying the members of a large association of human resources management in Switzerland. In 2016, we sent a web-based questionnaire to 4687 HR managers. We obtained responses from 714 individuals and thus a response rate of about 15 percent.[3] 91 percent of all respondents had been actively involved in at least one recruitment over the last 12 months, the median number of recruitments being ten. Our analytical sample only includes these active recruiters – among whom 82 percent claimed to have decisive influence over who gets hired for a job. Overall, our recruiters disproportionately came from large firms, the canton of Zurich and the public sector.

[3] 63 percent of the respondents were female and 37 percent male, with a mean age of 46 years. 70 percent of the respondents took the survey in German and 30 percent in French.

We framed our experiment as a study into the hiring practices in different regions and sectors. It started with the description of a job vacancy in three occupations, followed by the presentation of fictitious résumés (see the vignette shown in Figure A-1 of the appendix). Each respondent was asked to evaluate a random set of 12 résumés, four for each occupation. We selected three occupations – accountant, human resources assistant and building maintenance worker (caretaker) – that cover a broad skills spectrum, are not clearly male- or female-dominated, and can be found in many firms across sectors. The order in which vignettes were presented was randomised.

For the résumé of each job candidate, we asked recruiters to answer two questions: the likelihood that they would invite him or her to an interview on a scale from 0 to 10, and the monthly wage that seemed adequate for a given candidate, regardless of the likelihood of a job interview.

The profiles of our job candidates – the vignettes – are made up of 11 dimensions, including age, gender, nationality, civil status, the type of education and work experience as well as the number of school-age children (see table A2.1 in the appendix). Note that it is common in Switzerland to indicate the number of children in a CV.

From the combination of all possible vignettes (5,529,600 unique vignettes per occupation), we drew an orthogonal (d-efficient) sample of 720 vignettes per occupation (Auspurg and Hinz 2015).[4] Thanks to randomization, the vignette dimensions such as nationality, civil status or

[4] We implemented a D-efficient design that uses an algorithm to minimize inter-correlation among vignette dimensions and interaction terms, while maximizing the variance and balance of the frequency of levels.

motherhood are uncorrelated to each other (see table A1.2 in the appendix for the correlation matrix).

Taking out non-responses, using only active recruiters and focusing on female job candidates aged 50 or younger, we are left with 385 recruiter responses that provide us with wage recommendations for 1,644 candidates. These 1,644 wage observations constitute our analytical sample.

Estimation method

Our dependent variable is the natural logarithm of the monthly wage. Figure A1.2 in the appendix shows the distribution of monthly wages and suggests that the recruiters attributed plausible values to the different job candidates.

Our key independent variable is the number of school-age children, ranging from 0 to 3. A quarter each of our fictitious job candidates has values of 0, 1, 2 and 3 children. As job candidates applied for different occupations, occupation and the interaction between occupation and children are the two central control variables. In addition, we also control for civil status, nationality, the type of education and work experience. Note, however, that due to the experimental set-up, these control variables do not alter the results.

Our factorial survey produces data with a nested structure as the same respondent rates up to 12 vignettes. Depending on a respondent's own labour market experience, he or she is prone to set a lower or higher baseline in terms of wages. Moreover, respondents are likely to attribute wages to successive vignettes by comparing them with the wage they attributed to the first vignette. These arguments plead for a respondent fixed-effects regression model that eliminates

differences in respondents' individual baselines. We thus only use the within-respondent variance, that is the differences stemming from a respondent's ratings of different vignettes. Our model's equation is given as:

$$Y_{ir} = \beta_0 + \beta_1 OCC_{ir} + \beta_2 CHILD_{ir} + \beta_2 OCC_{ir}*CHILD_{ir} + \beta_3 CONTROLS_{ir} + \alpha_r + \varepsilon_{ir}$$

$$(r=1, 2, \ldots R)$$

where Y_{ir} is the logarithm of monthly wages for a vignette *i* evaluated by a respondent *r*. OCC_{ir} is a control for occupation, $CHILD_{ir}$ is a categorical measure of the number of children, $OCC_{ir}*CHILD_{ir}$ is an interaction term and $CONTROLS_{ir}$ a vector of socio-demographic control variables of the fictitious job candidates. We remove all characteristics that differ between respondents by allowing α_r, a set of unobserved variables, to correlate with all of our individual predictors. $\boldsymbol{\varepsilon_{ir}}$ represents the idiosyncratic random error. We correct for the clustering of observations within respondents by using clustered standard errors.

3.4 Panel data evidence for the motherhood wage gap

Figure 3.1 reports the wage penalty associated with having children for women aged 20 to 50 for the two longitudinal population surveys, showing the percentage change in earnings for each child. Table A3.1 in the appendix shows the precise coefficients and standard errors. We first discuss the gross motherhood wage penalty – the raw gap in hourly wages without any other controls than age and calendar year – and then move on to the net wage penalty by controlling for differences in human capital and the work setting.

As expected, having children is associated with a relative decrease in women's hourly wages. However, we find a much lower gross wage penalty for motherhood than studies on the United

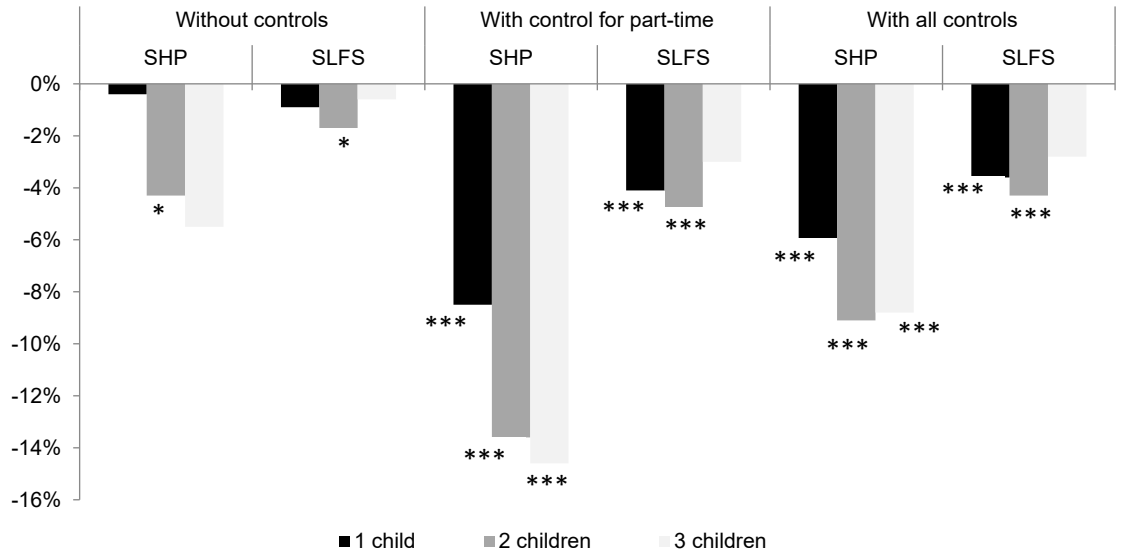
States. The first child has basically no influence in either dataset. The second child goes along with a decrease in wages of 2 (SLFS) to 4 percent (SHP). Estimations for the third child are less precise and vary between basically no penalty (SLFS) and 5 percent (SHP).

Of course, once women have children, their absolute *monthly* wages fall massively – by almost 20 per cent per child according to the SLFS. Yet this decrease is largely due to a reduction in weekly working time by 6 (first child), 9 (second child) and 10 hours (third child) among mothers who remain in the labour force. As a result, the decrease in *hourly* wages is much smaller. However, the wage returns to hours diminish above 40 hours per week (Morgan and Arthur 2005). Therefore, dividing the wages of full-time employees (who are paid for 40 to 42 contractual hours) by reported working hours (e.g. 45 to 55 hours) may lead to an underestimation of their hourly earnings relative to part-time employees. This division bias problem understates the initial wages of those in full-time employment and overstates those of workers in part-time employment (Manning and Swaffield 2008: 993). This is crucial because motherhood in Switzerland often goes along with a transition from full-time to part-time employment – with reported working hours (notably those above 40 hours per week) falling more than earnings.

Therefore, we re-estimate the gross wage penalty by introducing an additional dummy variable for part-time employment which may capture over-reporting of hours among full-time employees (and diminishing wage returns to hours beyond the full-time schedule of 40 hours). When controlling for the transition to part-time employment, the wage penalty for children becomes much larger. It amounts to 4 percent (first child), 5 percent (second child) and 3 percent (third child) in the SLFS and to 9 (first), 14 (second) and 15 percent (third child) in the SHP.

In a last model, we account for differences in human capital and job characteristics and are thus able to decrease the wage penalty of motherhood by a third in the SHP. It falls to 6 percent for the first child and to 9 percent for the second and third child. In the SLFS, where we have no measures for job and employer change or the match of qualifications and housework, the full model only leads to a small decrease of the motherhood wage penalty which remains at 3 to 4 percent per child.

Figure 3.1 The reduction in hourly wages for motherhood in Switzerland



Notes: SHP stands for the Swiss Household Panel, SLFS for Swiss Labour Force Survey. Results from fixed-effects linear regressions on (log) hourly wages for women aged 20 to 50 who are in the labour force. Strictly speaking, results are not percentages, but log points.

The model without controls includes age in years and calendar years, the model with controls includes variables for part-time, human capital, the type of job, and the household arrangement. See Tables A3.1 and A3.2 for the complete models.

*** p<0.01 ** p<0.05, * p<0.1

These net wage penalties are upper-bound estimates. Our measure of work experience is far from perfect and may well overestimate mothers' effective labour force experience – thereby leading to an overly large wage residual for motherhood. While we would have expected a larger decrease in the *net* wage penalty once we control for job characteristics, our result echoes the findings of Gash (2009: 580) who, based on the European Community Household Panel,

reports an increase in the motherhood wage penalty for Denmark, France, Germany and the United Kingdom once differences in human capital and the work setting are controlled for.

Overall, our two longitudinal surveys provide negative estimates for children's effect on mothers' hourly wages that range between a lower bound estimate of 2 to 4 percent and a higher bound of 6 to 9 percent per child – with possibly a smaller penalty for the first than the second and third child. Since these coefficients may hide the unobserved effects that children have on women's work productivity (and thus cannot be interpreted as solely reflecting wage discrimination against mothers), we turn to the factorial survey experiment which tries to directly tap into discrimination.

3.5 Experimental evidence for the motherhood wage gap

Table 3.1 shows how the presence of children affects the wages that recruiters deem adequate for female job candidates. In model 1, we simply regress the number of children on wages. While one child has no influence, recruiters assign wages to women with two or three children that are 3 percent lower than those deemed adequate for women without children, everything else being equal. The unexplained wage residual thus also pops up in our factorial survey experiment.

However, since the résumés of job candidates refer to three different occupations, we need to control for occupation. When doing so in model 2, the wage penalty for the second and third child is halved to 1.5 percent and there is still no effect for the first child. The wage effect of motherhood will differ across occupations if recruiters consider the presence of children more or less problematic depending on the occupation. We account for this non-linear effect by introducing an interaction term between children and occupation into model 3. This is our

preferred specification and produces a wage penalty of 2.5 percent for the first child and 3 percent for both the second and third child.

Table 3.1 Wage recommendations for women depending on the number of children they have (factorial survey experiment)

Dimension	*Level*	Model 1	Model 2	Model 3	Model 4
Children	1 child	-0.004	-0.005	-0.025**	-0.026**
(ref: no child)		(0.015)	(0.008)	(0.012)	(0.012)
	2 children	-0.026*	-0.015**	-0.031***	-0.030***
		(0.014)	(0.008)	(0.011)	(0.011)
	3 children	-0.032**	-0.016*	-0.033**	-0.033**
		(0.015)	(0.009)	(0.014)	(0.014)
Occupation	Accountant		0.172***	0.159***	0.159***
(ref: HR assistant)			(0.010)	(0.014)	(0.014)
	Caretaker		-0.177***	-0.203***	-0.204***
			(0.009)	(0.015)	(0.015)
Children*Occupation	Accountant*1child			0.018	0.018
(ref: no child,				(0.017)	(0.017)
HR assistant)	Accountant*2child			0.010	0.010
				(0.017)	(0.017)
	Accountant*3child			0.027	0.027
				(0.020)	(0.020)
	Caretaker*1child			0.040**	0.041**
				(0.018)	(0.018)
	Caretaker*2child			0.037**	0.038**
				(0.017)	(0.017)
	Caretaker*3child			0.026	0.026
				(0.019)	(0.019)
Additional controls		no	no	no	yes
	Constant	8.709***	8.704***	8.717***	8.727***
		(0.009)	(0.008)	(0.010)	(0.014)
	N vignettes	1,644	1,644	1,644	1,644
	N respondents	385	385	385	385
	R2	0.006	0.708	0.710	0.712

Note: respondent fixed-effects regressions on (log) wages for women aged 35 to 50. Clustered standard errors in parentheses. Additional controls in M4 include: civil status, nationality, type of education, type of work experience; the full model M4 is shown in Table A3.3 in the appendix.

*** p<0.01, ** p<0.05, * p<0.1

This effect refers to the reference category of HR assistants. The positive interaction terms for building caretakers with children indicate that motherhood is deemed much less consequential for building caretakers than HR assistants. If we add up the main effects for children with the interaction effects between children and occupation, we find that motherhood has no effect on

the wage recommendations for caretakers. In the third occupation of accountants, there is a wage penalty only for mothers with two children (2 percent).

As expected given our experimental design, all the coefficients remain unchanged if we further control, in model 4, for civil status, nationality, the type of education and work experience (see Table A3.3 in the appendix for the full model).

Our finding of wage discrimination against HR assistants with children holds true regardless whether we select the answers of female or male recruiters. Female respondents are no less likely to discriminate against mothers than male respondents – on the contrary: female recruiters hand out even somewhat larger wage penalties to mothers than do male recruiters (see Table A3.4 in the appendix).

Our results raise the question why recruiters would assign a wage penalty for children to HR assistants, but not to building caretakers and only partly so to accountants? Our sample consists of HR managers who were involved in hiring new staff over the last year. It is likely that HR managers have clearer expectations towards job candidates destined to support them in their daily work – HR assistants – than towards building caretakers or accountants, with whom they interact very loosely at best. Consequently, among the three occupations, they should have the strongest and most clearly formulated preferences towards HR assistants.

If the recruiters in our sample consider female HR assistants with children to be less productive as co-workers than female HR assistants without children, we should also observe a negative effect of motherhood on the propensity to be hired. We test this assumption in Table 3.2 by regressing the number of children on the likelihood to be invited to a job interview (measured on an 11-point scale), controlling for occupations and the interaction between occupation and

children (as well as for the other socio-demographic characteristics). We find that the probability to be invited for a job interview decreases linearly with the number of children – and the effect is again much stronger for HR assistants than accountants and, above all, building caretakers for whom there is no negative motherhood effect. While the standard errors are too wide for one or two children to be statistically significant, the effect size is large. Having two children decreases the likelihood for a HR assistant to be invited to a job interview by 0.24 units – and thus corresponds, in terms of disadvantage, to the ethnic penalty that Polish job candidates face relative to native Swiss candidates. Having three children reduces the probability of being invited to a job interview by 0.40 units – and hence exceeds the ethnic penalty faced by Turkish job candidates relative to native Swiss candidates (0.29 units) (see Table 2). Note that female respondents are again more reluctant to invite mothers (as compared to non-mothers) to a job interview than are male respondents.

In a final analysis, we dissect the motherhood wage penalty by looking at different age groups. On average, younger women have younger children – and it is likely that mothers primarily incur a wage penalty while they have small children (Kahn et al. 2014). It is small children who reduce mothers' productivity at work (and lead to statistical discrimination) or who activate the social norm expecting mothers to be at home child-rearing. The vignettes in our factorial survey simply indicated "school-age children", leaving open an age range from 4 to 18. 20 per cent of our fictitious female job candidates were attributed an age of 35, 40, 45, 50 or 55 respectively. We thus divide our pool of female job candidates into two age bands: 35-40 and 45-55.

Figure 3.2 shows how the effect of children on wages varies for these two age groups for the reference category of HR assistants. As expected, the wage loss incurred among mothers aged

35 to 40 is twice as large as that recorded for the extended sample of mothers shown above (aged 35 to 50). While it amounts to 2-3 percent in the extended sample, the wage penalty for a child doubles in the younger age group and reaches 6 percent for the first, second and third child. Although these effects are estimated with large standard errors, they are all statistically significant at the 5 percent level and suggest that recruiters principally discriminate against younger mothers – possibly because they expect them to still be in the midst of their child-rearing years. In contrast, there is no wage penalty for older mothers aged 45 to 55.

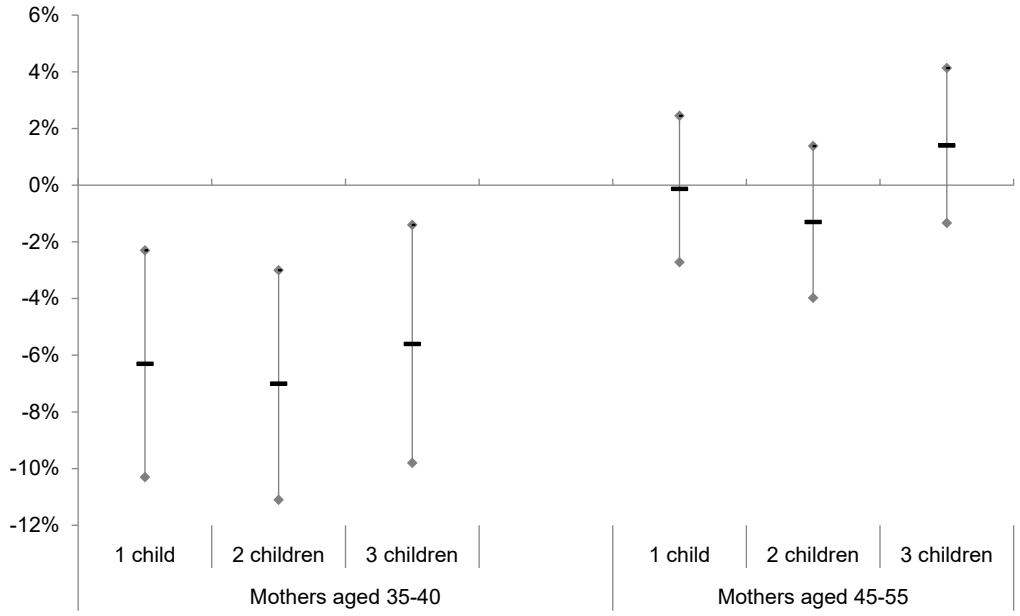
Table 3.2 The likelihood to get invited to a job interview for women depending on the number of children they have (factorial survey experiment)

Dimension	*Level*	Full model
Children	1 child	-0.137
(ref: no child)		(0.186)
	2 children	-0.242
		(0.167)
	3 children	-0.400**
		(0.177)
Occupation	Accountant	0.527***
(ref: HR assistant)		(0.190)
	Caretaker	0.408**
		(0.192)
Children*Occupation	Accountant*1child	0.235
(ref: no child, HR assistant)		(0.270)
	Accountant*2child	0.096
		(0.240)
	Accountant*3child	0.055
		(0.243)
	Caretaker*1child	0.225
		(0.255)
	Caretaker*2child	0.412*
		(0.248)
	Caretaker*3child	0.385
		(0.234)
Nationality	Spanish	-0.012
(ref: Swiss)		(0.107)
	Polish	-0.249**
		(0.099)
	Turkish	-0.285**
		(0.099)
	Constant	6.782***
		(0.166)
	N vignettes	2,625
	N respondents	486
	R2	0.052

Note: respondent fixed-effects regressions on the likelihood to get invited to a job interview (on a scale 0-10) for women ages 35 to 50. Clustered standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Figure 3.2 The motherhood wage penalty for two different age groups of female job candidates (factorial survey experiment)



Note: the graph shows the point estimates and 95% confidence intervals of the (log) wage differences between women with children relative to women without children. These log points can be interpreted as percentage point differences.

3.6. Conclusion

Survey-based research finds a sizeable wage gap between mothers and non-mothers in most affluent countries. However, we do not know whether this gap is due to (unobserved) differences in productivity or stems from discrimination against working mothers.

We have tried to open this black box and improve our understanding of the motherhood wage gap by combining two analytical methods. We first used two national panel studies to examine how women's wages evolve before and after the birth of children. After controlling for human capital and job characteristics, we found a net wage penalty of between 4 and 8 percent per child. This leaves us with wage residuals that are similar to those found for Germany (Gangl and Ziefle 2009, Gash 2009), but larger than those reported for the United States (Budig and England 2001, Glauber 2007).

We further probed into this unexplained wage residual by doing a factorial survey experiment among HR managers. We tried to improve on earlier experiments by surveying those individuals whose daily job it is to hire workers. Our results show that HR managers assign wages that are 2 to 3 percent lower to female HR assistants with children than to HR assistants without children – although the two groups are otherwise strictly identical. Consistent with statistical discrimination, the wage penalty is larger for younger mothers, increasing to 6 percent for ages 40 and less. Overall, our experiment thus confirms the findings made by Correll et al. (2007) for the United States that employers offer lower wages to mothers than non-mothers.

An open question is why recruiters offer lower wages to mothers. It is possible that our recruiters do not make their wage recommendations on the basis of their social norms, but mainly take into account their earlier experiences at work that mothers are, on average, less productive than non-mothers. Their lower productivity would stem from the fact that mothers – and not fathers – still are the primary caregivers in Switzerland. Our experimental analysis cannot rule out that there are (unobserved) productivity differences between women with children and women without children. However, even if the lower wage recommendations for mothers were based on rational expectations, it would still qualify as statistical discrimination to treat mothers differently from non-mothers on the sole basis of a characteristic – having children – that *per se* has no bearing on an individual's skills and performance.

Interestingly, our experiment shows no wage penalty for mothers working in the least skilled of our three occupations, as building caretakers. Having children reduces the likelihood of getting invited to a job interview and the recommended starting wage for HR assistants and, to a lesser extent, for accountants, but not for caretakers. Of course, HR managers have a clearer

idea of whom they want as their assistants than they have of building caretakers. At the same time, it is possible that children are considered to interfere less with work productivity in manual activities than in an office job. The social norm of the good mother who is constantly available for her children may apply less to women employed in subordinate working-class positions than in a white-collar job, echoing the finding for the United States that white mothers are discriminated against, but not Latino or African-American mothers (Glauber 2007, Denny 2016).

Our analysis is not without its shortfalls. The varying coefficients of our population surveys suggest that there is considerable uncertainty as to the precise extent of the motherhood wage penalty. By using two different datasets and showing both results with and without controls, we have tried to make this uncertainty transparent. Still, our panel-data results of an unexplained residual wage gap of 4 to 8 percentage points are upper-bound estimates that probably suffer from unobserved determinants of work productivity. This should not distract from the main finding that motherhood in Switzerland is associated with a decrease in relative wages.

A caveat also applies to our factorial survey where respondents skew towards large organizations. This means that our findings, while remaining internally valid, are not easily generalised to the Swiss labour market as a whole. In many small- and medium-sized enterprises, the recruitment process is overseen directly by the owner-proprietor-employer – and not in the hands of well-trained HR managers who likely are less prone to discrimination. Evidence from a Canadian firm-level survey suggests that the motherhood wage gap is much smaller in establishments with a human resources department than in establishment without

formalised personnel service (Fuller 2018). Our results from the survey experiment of a wage penalty of 2 to 3 percent are thus probably lower-bound estimates.

These questions aside, our experiment throws doubt on the assumption that the wage penalty found in the panel surveys is solely driven by work productivity. The lower wages attributed to mothers in general – and young mothers in particular – strongly suggest that elements such as statistical discrimination, social norms and cultural beliefs also play a role. Child-rearing is an activity that, if done well and with a bit of luck, provides ample benefits to the society as a whole. Therefore, it seems problematic that women with children have to face an uphill struggle to get paid the same wage as childless women.

4. Who hands out the motherhood wage penalty? A survey experiment on wage recommendations by recruiters

4.1 Introduction

Wage differences for workers with similar jobs and experience but differing family situations persist across countries and sectors. Specifically, mothers can expect to be paid less than non-mothers, for the same job, throughout most the western world (Budig and England 2001; Gangl and Ziefle 2009). Moreover, despite continuing development of family-friendly policies in some countries and an increasing percentage of working mothers in just about all national workforces, this penalty does not seem to be decreasing, in the US at least (Jee et al. 2018). Recent experimental research has shown that a sizable part of is penalty is due to employer discrimination (Correll et al. 2007, Oesch et al. 2017). This may be statistical discrimination: employers expect lower productivity from mothers and set wages accordingly, or discrimination based on social norms (taste discrimination), whereby the expectation of mothers is primarily as a homemaker and therefore her labour-market work will be less valued and less well-remunerated (Budig and England 2001).

The aim of this paper is to further investigate the relationship between employer discrimination and the motherhood wage penalty by asking whether or not an employer's gender and parenthood status impacts on the wage penalty they give to mothers. Employers on aggregate give penalties to mothers (Oesch et al. 2017), but it is less clear whether these penalties vary based on the characteristics of recruiters – their own gender and parenthood status, for instance. Exploring these differences may be key to improving our understanding of, and therefore our responses to, motherhood wage penalties caused by employer discrimination.

We explore the differences in motherhood wage penalties given by recruiters depending on their gender and parenthood status using a factorial survey experiment among human resources professionals in Switzerland. In this survey, respondents were asked to assign wages and a likelihood of being invited to a job interview to fictional CVs that varied on several characteristics including gender and parenthood status. The use of the factorial survey experiment setting has a number of benefits for such a study. First, all the input variables were uncorrelated, meaning that differences in wages may be directly attributed to recruiters' reaction to variation in parenthood status. The survey allows us to go beyond the information on recruitment discrimination that can be obtained from correspondence studies by asking more detailed questions of recruiters while minimizing social desirability through the use of ever-changing multidimensional vignettes, albeit in a hypothetical setting. Importantly for this paper, the survey also collected detailed respondent-level information, allowing us to study in greater detail whether or not the motherhood penalty assigned is the same amongst all recruiters, or if recruiters who are mothers themselves give different penalties to mothers than other recruiters. While factorial surveys have been used to study differences in opinion about just earnings for men and women in general (see Jasso and Webster 1997, Auspurg et al. 2017 for examples), they have not studied the case of working mothers and not targeted employers specifically.

In the following section we review the literature to develop four theoretical approaches to the question of differences in the motherhood wage penalty by employers' gender and parenthood status: homophily, the "Queen Bee" syndrome, status characteristics theory and relatedly the "cogs in the machine" theory. While homophily-based theory would suggest that employers who are mothers themselves offer higher wages to mothers than other recruiters, the Queen Bee theory argues that the reverse will be the case: women who accede to the upper echelons

of the occupational hierarchy are likely to conform to or actively support the prevailing organisational culture, with the additional possibility of women who feel they had to sacrifice motherhood to succeed professionally being especially critical towards mothers. Status characteristics theory and cog in the machine theory, in contrast to the other two possibilities, expect that motherhood will be a salient signal to all recruiters of expected lower productivity and commitment to work, and that consequently motherhood penalties will not vary greatly across differing recruiter groups. The similar expected outcomes of the status characteristics and cogs in the machine theory lead us to form three hypothesis about differences in judgements towards mothers in the recruiter groups.

Following the literature review and the development of our hypotheses, in section 3 we further explain our data, the Swiss context and our analytical strategy. Section 4 presents our results and we conclude, in section 5, with a summary and discussion of our key findings.

4.2 Theoretical expectations about recruiters' wage-setting for mothers

A wage penalty for motherhood is a long-documented phenomenon within economics and sociology. The traditional theoretical explanation expects that mothers have lower productivity as they specialise in child-rearing at the expense of paid work (Becker 1973), and that they choose flexible or less demanding jobs to better combine parenting duties and employment (Weeden et al. 2016). Empirical evidence shows that productivity accounts for some proportion of the motherhood wage penalty (Budig and England 2001; Gangl and Ziefle 2009), though in most cases a residual penalty remains after controlling for productivity factors. Unobserved productivity-related characteristics may explain part of this residual, but there is mounting empirical evidence that employer discrimination alone can result in motherhood wage penalties (Correll et al. 2007; Oesch et al. 2017). This might be statistical discrimination: when mothers

as a group are more likely to work part time, take career breaks to raise children and be called away from work for childcare or emergencies, productivity is lower at the group level. Employers may then treat all mothers the same and discriminate against them, whether or not the individual case in question is affected by these factors. On the other hand, it may be discrimination based on social norms ("taste discrimination"), whereby the discrimination is not based on any expectations of productivity but rather a distaste for hiring a certain profile (Budig and England 2001: 208). For mothers, this may be informed by an expectation to act primarily as a homemaker, not active participants in paid labour.

As recruiters and employers do not form a homogeneous group, their behaviour concerning wages for mothers may differ. Specifically, recruiters who are also mothers may behave differently when assigning wages to female job candidates with children than recruiters who are childless women or men. We discuss four possible mechanisms that may be present in the decision-making process, leading to three hypotheses about the motherhood wage penalty: homophily, where mothers will pay mothers more, the so-called "Queen Bee" phenomenon, where women, especially non-mothers, will be harsher towards mothers, and status characteristics and cogs in the machine theory, differing mechanisms that produce a similar outcome: expectations concerning mothers will be similar between groups of recruiters and there should therefore not be large differences in wage penalties.

4.2.1 Homophily

The homophily principle rests on the idea that people are most comfortable associating with the people most similar to themselves (McPherson et al. 2001). Applied to recruitment of mothers, this suggests that female recruiters who have children themselves will be the most generous to mothers applying for jobs. There are a number of possible explanations behind

this. First, mothers may feel a closer camaraderie or empathy towards other mothers and therefore be less likely to discriminate on the basis of the applicant being a mother. Linked to this explanation is the more rational idea that a female recruiter with children may more accurately estimate the productivity of mothers than other recruiters, being mothers themselves, and therefore come closer to attributing the true "just wage" to mothers applying for jobs. In the case that mothers are indeed unduly discriminated against, this would mean that the wages given by female recruiters with children would be higher than those given by others.

Though there is ample evidence on the impact of women in decision-making positions on outcomes for female employees, little of it is related to mothers specifically. However, we believe it is a reasonable assumption that the effects and mechanisms shown to apply to women overall will apply to mothers particularly. Though there is little empirical study on the "positive" side of this theory, such as homogamy, on the negative side – wage inequality, for instance – empirical evidence suggests that motherhood is a strong driver of outcomes that are observed for women overall (Goldin et al. 2017). Concerning gender and homophily, several studies show that the presence of more women in managerial positions leads to better outcomes for women in terms of wages and promotion opportunities, suggesting that homophilious relationships have a positive impact for women. Gorman (2005) focuses on hiring practices of US law firms in the 1990s, and finds that female recruiters are more likely to hire female applicants – with the caveat that job descriptions with stereotypically male characteristics (such as ambition, assertiveness, knowledge, leadership) lead to a higher proportion of men being hired than women. Cohen and Huffman (2007) use census data to analyse the effect of a higher concentration of female managers on the gender pay gap, finding that while a higher proportion of female managers is linked to the reduction of a gender wage gap within a firm, the strongest effect comes when these women are in the highest management positions. A longitudinal study

of a large company in the US by Castilla (2011) finds that both horizontal and vertical homogamy are important in the development of more equal workplaces. Specifically, homogamy within the managerial group leads to similar behaviours amongst managers, which may in turn create an environment where vertical homogamy (manager-employee) is more pertinent.

In Europe, Hultin and Szulkin (1999) analyse data from the 1990 Living in Sweden survey, and find that a relative increase of the proportion of female managers in an organisation leads to a relative decrease of the gender wage gap. A longitudinal study in Portugal by Cardoso and Winter-Ebmer (2010) shows that female managers reduce the gender wage gap by 1.5 percentage points, but notes that wages and promotion possibilities are reduced for both men and women when more women are in positions of power. Hirsch (2013: 350) finds a significant decrease in the gender wage gap as the percentage of women in management positions increases – a 0.5 log point decrease in the gap for a 10 percent increase of women in management – with a stronger effect at the middle-management position, rather than the executive level. Finally, Hedija (2015) analyses wage data from two Czech hospitals in 2010. She finds that a female head of department decreases the gender wage gap by up to 6 percentage points (Hedija 2015: 56), which is quite significant given that the average pay gap across the sample amounted to 12 percent.

In sum, the homophily theory leads to the expectation that women will be more generous to other women, and we may expect that this general effect should hold for mothers specifically. We therefore form a first hypothesis:

H1: Female recruiters with children will give higher wages to mothers than other recruiters.

4.2.2 The "Queen Bee" syndrome

The so-called Queen Bee syndrome refers to a concept in social psychology that suggests women in positions of power may be more critical towards female subordinates, rather than more accommodating (Staines et al. 1974; Ellemers et al. 2004; Parks-Stamm et al. 2008). This may be due to women rising to positions of power, which are generally male dominated, not wishing to jeopardise the culture or environment in which they have been so successful by seeing it more feminised: an act of self-preservation on the part of women high up in the organisational hierarchy. Ellemers (2001) argues that women in management positions may feel they have been required to suppress their gender identity in order to succeed or feel that their gender identity is unimportant to their work. Allowing more women into the organisation may result in them being associated with a group that they do not wish to be associated with, either because they do not feel any great attachment to it or because they feel that group membership may be harmful to their prospects. They may therefore be more reluctant to allow more women into their organisations, and this may affect mothers specifically if they are perceived to be particularly harmful to prospects of success, due to their perceived lower productivity or lower commitment to work.

Alternatively, it could be that the Queen Bee phenomenon stems from the fact that women who feel they have been required to sacrifice a great deal for their career – be it family or other pursuits – are less likely to look kindly upon women whom they feel have not been required to do so, whether this is the case or not. These mechanisms may be affected by an overall sexist workplace culture (Derks et al. 2011), which promotes a sense of exceptionalism amongst female managers, as well as more "masculine"-influenced beliefs on the workplace behaviour of women in general. Moreover, we can reasonably expect mothers applying for jobs to be strongly affected by the Queen Bee syndrome as their productivity and commitment to work is

already perceived to be less than that of other women. We should expect this to be especially the case for childless female recruiters judging applicants who are mothers.

There is a large body of empirical evidence that examines the Queen Bee syndrome in workplaces and finds ambiguous results. Two studies from the 1990s, Eisenman (1991) and Cooper (1997) find evidence of both homophily and the Queen Bee syndrome, with the Queen Bee syndrome being prevalent amongst women identifying as having conservative values, while liberal women tend towards homophily. A study of the perception of commitment of PhD students in the Netherlands and Italy by Ellemers et al. (2004) is less equivocal. They find that female faculty members consider female PhD students to be less committed to their work than male students, and this more so than male faculty members, despite no evidence of this being the case (Ellemers et al. 2004: 333). There is further evidence in a paper by Correll et al. (2007) of the Queen Bee phenomenon, though this was not the primary aim of the study. In an experimental study of the motherhood penalty, they find that female participants were harsher towards mothers than male (Correll et al. 2007: 1323), though the difference in evaluations between mothers and women without children was smaller amongst female respondents than male participants.

This discussion of the Queen Bee syndrome theory leads us to a second hypothesis:
H2: Female recruiters will give lower wages to mothers than male recruiters, especially if they are childless.

4.2.3 Status characteristics theory and "cogs in the machine"

A final possibility concerning wage assignment to mothers is to expect no difference between female or male recruiters, with or without children. There is one key theory that may explain

this: status characteristics theory (Berger et al. 1972; Ridgeway & Correll 2004; Penner et al. 2012). This theory argues that beliefs held based on certain characteristics (for example, motherhood), are both widely shared and affect participation in and attachment to societal groups and structures. In the case of mothers and work, the generally-held belief that women are of lower status and competence than men and mothers hold lower levels of commitment to work than non-mothers (Penner et al. 2012), will affect judgements amongst all recruiters. Status characteristics theory will therefore expect that all recruiters believe mothers to be less productive than other workers, and that therefore all recruiters will offer mothers lower wages than non-mothers.

Status characteristics theory has been confirmed in the case of female workers in many different empirical settings, and notably so by three factorial experiments in differing contexts. Jasso and Webster (1997: 76), using a general population sample from Baltimore in 1974, find that both male and female respondents consider lower wages for female profiles than male to be just. However, a follow-up study (Jasso and Webster 1999: 377-78) using a sample of undergraduate students from the south-eastern US surveyed in 1995 finds that both men and women assign higher "just base wages" to women than men, though men's experience is rewarded at a higher rate than women's. This suggests that while status characteristics theory may still hold, its results may have flipped in favour of women over men – with the caveat that the sample of undergraduates may not act the same way as employers and managers. Finally, a more recent factorial survey analysis by Auspurg et al. (2017: 199, 202), using a sample drawn from a 2009 general population survey in Germany, finds that both men and women consider lower wages for female profiles, and for profiles with children, to be "fair" – gender or parenthood status being the only productive attribute that differed between the profiles.

Beyond the factorial setting, firm-based evidence by Penner and colleagues show in several studies that gender of firm owners and managers is unrelated to employee gender wage gaps. In a first paper, Penner and Toro-Tulla (2010) find no wage differences by gender in small businesses in the US based on the gender of the business owner. In a second paper, Penner et al. (2012), through an analysis of employment records of a US grocery chain, find that female and male managers penalise women to similar levels in terms of wages. These results suggest that in both small and large businesses, with and without dedicated HR departments, women are treated similarly by recruiters regardless of the recruiter's gender – though Fuller (2018) finds that discrimination against mothers is stronger in firms without a dedicated HR department than those with, where HR managers are likely to have been trained in non-discriminatory hiring behaviour.

Another reason we may not expect to see significant differences between employer groups pertains to the "cogs in the machine" phenomenon: the idea here being that women in managerial positions will act within the prevailing organisational culture to keep the machine working, rather than acting as agents for change within their organisations (Cohen and Huffman 2007: 684). Maume (2011: 296) uses the National Study of the Changing Workforce survey in the US to explore whether employees perceive their managers to be supportive of their career progression. He finds that female respondents are more likely to consider their female managers as working as cogs in the machine rather than change agents and therefore conform to organisational cultures that favour men over women. Using data that combines information about employees, employers and their organisations through the European Sustainable Workforce Survey, van Hek and van der Lippe (2019) find that neither concentration of women in management nor having a female manager directly, reduce gender wage gaps.

If we expect that social beliefs about groups are held widely across society and thus become stereotypes, and/or that women in managerial positions conform to the prevailing organisational culture, this leads us to form a third and final hypothesis:

H3: All recruiter groups will penalise mothers compared to non-mothers.

4.3 Context, data, and analytical strategy

4.3.1 Country context

We test our three hypotheses on the basis of a factorial survey experiment undertaken amongst the members of a human resources professional association in Switzerland, which we describe in detail below. Although a comparatively small labour market, Switzerland contains some properties that means lessons learned from analysing Swiss data can be extrapolated to other contexts.

First, Switzerland shares several characteristics of its German-speaking neighbours, Germany and Austria, including a highly developed dual system of education marked by apprenticeships, an occupational labour market with close ties between education and employment, and a large proportion of export-oriented small-to-mid-sized firms. Industry-level wage bargaining is not unusual but not as widespread as in Germany. The countries share similar conservative family values, with the expectation that mothers take care of the household and therefore focus less on paid work, while fathers remain the main breadwinners (Charles et al. 2001; Valarino & Gauthier 2016).

However, unlike in Germany, policies and institutions supporting working motherhood are not well developed in much of Switzerland (Buchmann et al. 2010, Stadelmann-Steffen 2011). The right to paid maternity leave was only recently enshrined in law, there is no legal right to paid

paternity leave (though it does exist in some firm-level collective agreements), and access to institutional childcare is patchy, especially outside of the cities. Switzerland, moreover, has relatively weak laws concerning employee protections, including no-fault termination and no statutory minimum wage (Oesch 2011), that bring it closer in line with the liberal economies of the UK and USA. Finally, despite the hurdles to employment that the lack of childcare institutions presents to mothers, the country nevertheless has a high workforce participation rate of women (over 80 percent), including those with children. Mothers, however, rarely work full-time: 83 percent of working mothers with children under 6 have a part-time job (OFS 2016a).

The aforementioned characteristics of the Swiss labour market lead us to expect that wage penalties for mothers will be substantial. Indeed, recent evidence from Oesch et al. (2017) using analysis of the Swiss Household Panel and Swiss Labour Force Survey shows a wage penalty of up to 8% for mothers after controlling for productivity and human capital measures. Using the same vignette data as this paper will employ, they also find a wage penalty of 2-3% from recruiters, though significantly higher amongst the vignettes aged 35-40 (Oesch et al. 2017: 1810).

4.3.2 Data

We analyse the question of how recruiter characteristics affect motherhood wage penalties by using a factorial survey experiment. Factorial surveys, or vignette studies, have a long history in many of the social sciences (Wallander 2009) but have only recently been taken up in earnest by labour market sociologists (see Di Stasio 2014, Auspurg et al. 2017, Liechti et al. 2017, Oesch et al. 2017, for examples).

In fact, many characteristics of factorial surveys make them an ideal method for studying labour market questions, particularly related to recruitment (McDonald 2019). Factorial surveys take the form of vignettes, or short stimuli that vary the levels of several different dimensions, that respondents to the survey are required to judge or rate. Such vignettes can, for example, take the form of CVs, with respondents asked to rate the likelihood that an applicant with such a CV may be invited to a job interview, or to indicate a wage they consider adequate for such a profile. Factorial surveys are generally designed such that all the dimensions are uncorrelated, meaning that effects can be attributed to specific variables and not confounders, thus facilitating the possibility of causal analysis. Moreover, with a series of dimensions changing at random, social desirability bias should be minimised (Auspurg and Hinz 2015). While this means that factorial surveys have high internal validity, the fact that they are hypothetical and necessarily restricted to a set of examples means that their external validity may be called into question. On this point, Hainmueller et al. (2015) demonstrate that results obtained with the various forms of factorial surveys closely mirror those from real-life situations as measured with popular referenda, coming within 5 percent of actual effects.

The survey we analyse in this study adds to the benefits of factorial surveys outlined above by targeting recruiters directly. In 2016 we sent a web-based factorial survey to 4000 Swiss employers and human resources professionals through a national HR association (Liechti et al. 2020). 514 respondents provided evaluations for 5764 vignettes. The response rate of 13 percent is in line with the response rates of similar large-scale factorial survey experiments (e.g. Liechti et al. 2017). These responses were concentrated in large companies (250 employees or more) based in urban areas, principally the Zurich, Bern and Lausanne regions (the first, fifth, and fourth-most populous metropolitan areas of Switzerland respectively). This means that while the results retain a high level of internal validity, their representativeness of

the Swiss labour market and therefore external validity, are weaker than the ideal. This is especially the case for the lack of responses from smaller firms and firms based in rural areas. Evidence from Fuller (2018) suggests that in these smaller organisations, without a dedicated HR department, discrimination against mothers may be higher. We would expect this to be all the more so in Swiss regions with limited access to childcare, and therefore expect that our results may be conservative estimates of the actual effect.

Respondents to the survey were asked to rate 12 vignettes of three different occupations (4 vignettes per occupation): an accountant, human resources assistant, and building caretaker (concierge). The three occupations were chosen because they are present in many organisations, they represent the higher, mid and lower range of the job skills spectrum, and because none of them are highly dominated by one gender, all being at least one-third male or female (OFS 2016c). The vignettes varied on 11 personal and work-related dimensions, including, importantly for this study, parenthood status, with vignettes having either no children or between 1 and 3 school-age children. A full list of vignette dimensions and levels are shown in Table A2.1 in the appendix. 5,529,200 unique vignettes were possible for each occupation, from which we drew an orthogonal (*D*-efficient) sample of 720 per occupation. *D*-efficient designs use an algorithm to minimise correlations between variables and interaction terms, while maximizing dimension level variance and frequency of appearance, in cases where the vignette universe is too large for all possible vignettes to be rated (Auspurg and Hinz 2015). The design of our survey allows for estimation of all individual effects and two-way interactions between vignette variables. Table A1.2 shows the vignette dimension correlations for the overall dataset. The dimensions are also all uncorrelated in the subsets used for this analysis.

After reading each vignette, respondents were asked to indicate a likelihood, on a scale from 0 to 10, that they would invite such a candidate to a job interview, as well as a monthly wage should such a profile be employed full-time. After rating all the vignettes, respondents were asked a series of questions about themselves, including their age, gender, education, their hiring experience and decision-making influence within the firm, and whether or not they themselves are parents. 93% of the sample (501 individuals) indicated they were actively involved in recruitment, and we restrict our analysis to these individuals, 385 of whom recorded valid responses to the wage question in the vignettes. 328 of these individuals provided information on both their gender and parenthood status. The sample is approximately 63 percent female, with an average age of 46. While overall 41 percent indicated they had children, this was largely skewed by gender, with 37 percent of female respondents being mothers but 74 percent of male respondents being fathers. This results in a large difference in group size: While women without children, women with children and men with children represent 121, 80 and 91 respondents respectively, men without children account for only 36 respondents. The overall average number of children amongst respondents who were parents was 2 – 1.78 for women and 2.22 for men.

We introduce two further restrictions to the dataset at the vignette level. First, we select female vignettes only. Second, the ages of the vignette profiles varied in 5-year increments between 35 and 55. Given the impact of motherhood in terms of reduced productivity due to child-minding activities is likely to no longer be of great importance by the mid-50s (when children are likely to be in their late teens), we drop the profiles aged 55 from the sample. Further analyses of the full sample (see table A4.4 in the appendix for a replication of the multilevel models with the full sample) indeed shows that when all ages are included, coefficients are either unchanged or slightly smaller and measures of statistical significance weaker, giving

justification to a focus on younger profiles. The two vignette-level restrictions leave us with a final number of 2229 vignettes which constitute our analytical sample.

4.3.3 Method

We propose a two-step analytical strategy to answer the question of whether or not there are differences in the motherhood wage penalty by gender and parenthood status of respondents. In a first step, we run respondent-level fixed-effects regressions, splitting respondents into four groups: women with children, women without children, men with children and men without children, to see if these groups of recruiters behave differently in giving motherhood wage penalties. Fixed-effects regressions take into account the possibility that the baseline wages may vary across individuals, and therefore only consider the within-respondent differences based on number of children. We control for occupation, as CVs are tailored for each occupation and ratings and wages are likely to vary non-randomly across occupations, and include an interaction term between number of children and occupation, given the three occupations have vastly different profiles. There is also strong evidence both from the US (England et al. 2016) and Europe (Berghammer 2014) that motherhood may affect women's wages and work attachment differently depending on their occupational prestige, with high-skilled, high-earning women experiencing higher penalties than those with lower wages and skills, further underlining the importance of accounting for these occupational-level differences. Since the vignette dimensions are uncorrelated, no further controls are required (the correlation matrix can be found in Table A1.2 in the appendix). We can therefore summarise our first analytical model as follows:

$$Y_{ir} = \beta_0 + \beta_1 CHILD_{ir} + \beta_2 OCC_{ir} + \beta_3 OCC_{ir} * CHILD_{ir} + \alpha_r + \varepsilon_{ir} \quad (r=1, 2, \ldots R) \qquad (1)$$

where Y_{ir} is the logarithm of monthly wages for a vignette *i* evaluated by respondent *r*. $CHILD_{ir}$ is a categorical variable of the number of children (from none to 3), OCC_{ir} is a control for occupation, with OCC_{ir}*$CHILD_{ir}$ an interaction term between occupation and the number of children. α_r is a set of unobserved variables that correlate with all individual predictors, and ε_{ir} is the idiosyncratic random error. We correct for within-respondent clustering of observations by using robust standard errors.

The second step of the analysis is to determine whether the difference between the four groups – if any – is statistically significant. To do so, we employ multilevel models (Hox et al. 2017) with random slopes at the respondent level. The vignette-level (level 1) variables in this model are the same as the fixed-effects model. At the respondent level (level 2), we create a categorical variable of gender and parenthood status to match the four groups we use in the split-sample fixed effects regressions. We include a cross-level interaction of respondent gender and parenthood status with children in the vignette. Our second analytical model can therefore be summarised as follows:

$$Y_{ir} = \beta_{00} + \beta_{10}CHILD_{ir} + \beta_{20}OCC_{ir} + \beta_{30}OCC_{ir}*CHILD_{ir} + \beta_{01}RESPONDENT_r + \beta_{11}CHILD_{ir}*RESPONDENT_r + \alpha_{0r} + \varepsilon_{ir} \quad (r=1, 2, \ldots R) \quad (2)$$

which contains the same terms as our fixed-effects model 1, with the addition of a level 2 variable $RESPONDENT_r$, a categorical variable corresponding to the four respondent-level groups we create for the fixed-effects regressions: women without children, women with children, men without children and men with children, as well as the cross-level interaction terms $CHILD_{ir}$*$RESPONDENT_r$. α_{0r} represents the random error associated with the deviation

of the intercept of respondent *r* from the overall intercept, while ε_{ir} represents the residual error at the vignette level.

4.4 Results

4.4.1 Fixed-effects regressions on wages

Table 4.1 presents the results of the fixed effects regressions, first for the full sample, then for the sample divided into groups based on the gender and parenthood status of respondents. Here we see that for the overall sample, motherhood penalties are in the order of 2.5 to 3.5 percent, and are statistically significant for women with 1, 2 and 3 children for the reference occupation of HR assistant.

Running the same models on the split sample, we find that the penalties given by women without children and by men with children are larger the average of all respondents – women without children give penalties of between 4.6 percent and 6 percent, while men with children give penalties of between 2.9 percent and 5.6 percent. For women with children and men without children (by far the smallest category with only 36 respondents), the penalties are smaller and in no cases statistically significant. In short, mothers offer low penalties that are not statistically significant, in line with the homogamy hypothesis. Women without children give the largest penalties, providing some support for the Queen Bee hypothesis, at least in the sense that women without children are less supportive of working mothers in terms of wages than other groups of recruiters. At the same time, all recruiters give penalties, albeit of varying degrees, which may be interpreted as evidence of the status characteristics theory or cogs in the machine – motherhood is seen by all respondents as a drawback in a job applicant, though more so for some groups of recruiters than others.

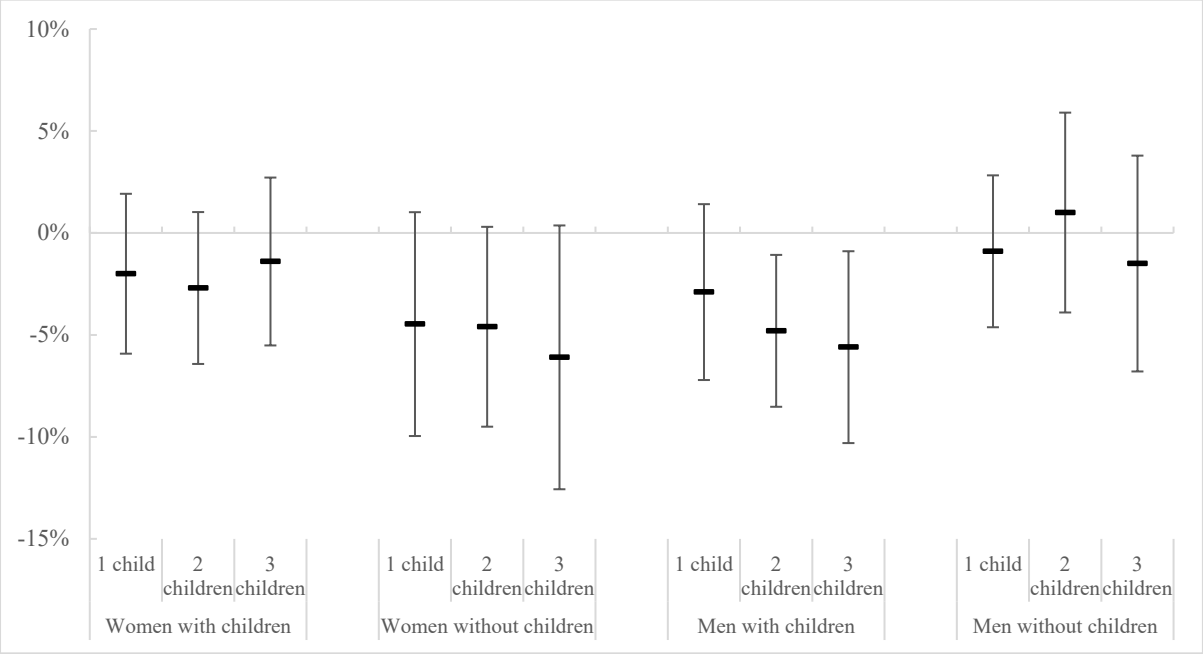
Table 4.1 Fixed-effects regressions of log monthly wages on number of children, full sample and split samples by respondent gender and parenthood status

Variables	Levels	full sample log wage	fem with child log wage	fem w/o child log wage	male with child log wage	male w/o child log wage
Children	1 child	-0.025^{+}	-0.02	-0.047^{+}	-0.029	-0.009
(ref. no children)		(0.012)	(0.02)	(0.028)	(0.022)	(0.019)
	2 children	-0.031*	-0.027	-0.046^{+}	-0.048*	0.01
		(0.011)	(0.019)	(0.025)	(0.019)	(0.026)
	3 children	-0.033**	-0.014	-0.06*	-0.056^{+}	-0.015
		(0.014)	(0.021)	(0.033)	(0.024)	(0.027)
Occupation	Accountant	0.159***	0.199***	0.135***	0.156***	0.013***
(ref. HR assistant)		(0.014)	(0.029)	(0.03)	(0.023)	(0.029)
	Concierge	-0.203***	-0.215***	-0.211***	-0.222***	-0.016***
		(0.015)	(0.03)	(0.31)	(0.027)	(0.037)
Interactions		Yes	Yes	Yes	Yes	Yes
R^2		0.71	0.73	0.68	0.75	0.81
No. vignettes		1644	361	551	428	172
No. respondents		385	80	121	91	36

Robust standard errors in parentheses. Full regression tables, including the interaction terms, in the appendix (Table A4.1). Full sample includes respondents who did not indicate gender and/or parenthood status.
*** p<0.001, ** p<0.01, * p<0.05, $^{+}$ p<0.1

Figure 4.1 plots the above results graphically with 95 percent confidence intervals. What immediately stands out is that while the penalty patterns do differ between the four groups, the confidence intervals overlap substantially and cross zero in many cases, though this may be a result of the small sub-group sizes. The central prediction values are nevertheless lower for women without children than mothers, while men with children also assign lower wages to mothers than profiles without children (with the exception of mothers with one child where the effect is less clear), whereas practically no difference can be seen in the wage assignments for mothers and non-mothers by men without children.

Figure 4.1 Motherhood wage penalties by respondent gender and parenthood status for HR assistants (reference: no children).



Source: Table 1 regressions (fixed-effects). 95% confidence intervals shown.

In sum, the fixed effects results show weak, mixed evidence on differences in the motherhood penalties between groups of respondents based on their gender and parenthood status. Women without children and fathers give the largest penalties, while mothers and childless men give penalties that are smaller. However, the differences are not large, and the 95 percent confidence intervals of the coefficients overlap in most cases. Moreover, the results of the fixed-effects analysis do not indicate whether the differences between the groups are statistically significant. To investigate the relationship between respondent gender and parenthood status and motherhood penalties in greater detail, we turn to the multilevel analysis.

4.4.2 Multilevel analysis of wages

We present the results of the multilevel random-intercept models in Table 4.2. M3 in this table is the model that corresponds to the full sample fixed-effects model in Table 4.1, and we see that the results are very similar. In the following model, M4, we add the level-2 variable of

respondent gender and parenthood status, and find that it does not have a large effect: while all groups give higher wages than the reference category of female respondents without children, only in the case of men without children is the effect significant, and in the case of women with children the difference is vanishingly small. On the whole, therefore, it can be concluded that gender and parenthood status do not impact on the wages respondents indicate for vignettes.

This result, however, does not give us information on whether or not respondents' gender and parenthood status affect motherhood wage penalties, only if it affects respondents' ratings of female vignettes overall. To answer this question in M5 we include a cross-level interaction term of respondent gender and parenthood status with the number of children in the vignettes. We see the main effects coefficients decrease, meaning the motherhood wage penalties increase, for the respondent reference category of women without children. This group of respondents gives penalties between 2 percent and 5 percent to vignettes where the job candidates are mothers. These results are in line with the split-sample fixed-effects results that show that women without children give the largest wage penalties to mothers. Yet while the respondent-level and cross-level interaction coefficients confirm the narrative of some differences between groups based on their gender and parenthood status, differences are not statistically significant: the penalties are reduced by 0-1.5 percentage points in the case of mothers, 0.5-5 percentage points in the case of fathers, and 1.5-5 percentage points in the case of men without children with respect to female respondents without children. As with the fixed-effects analysis, we are left with the impression that status characteristics or cogs in the machine remain the most plausible paradigms of motherhood wage penalties: while the magnitude may differ, in almost all cases our recruiters offer lower wages to female candidates with children than to identical candidates without.

Table 4.2 Multilevel random-intercept analysis of motherhood wage penalties, respondents grouped by gender and parenthood status combined

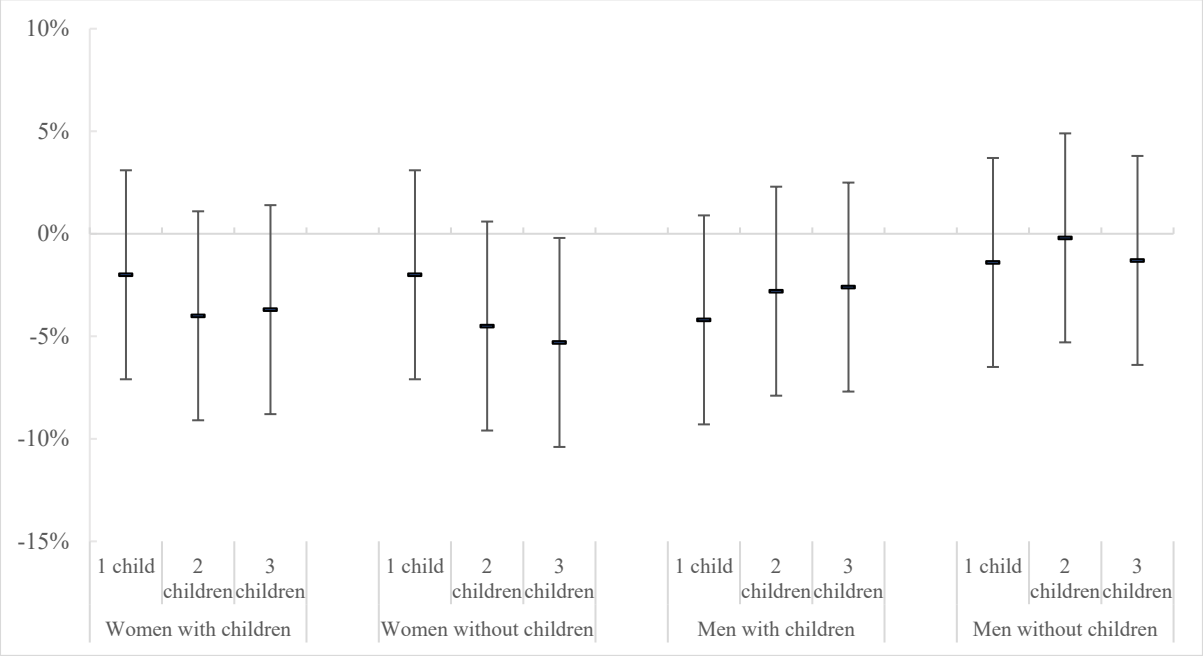
Variables	Levels	M1 log wage	M2 log wage	M3 log wage	M4 log wage	M5 log wage
Vignette-level fixed effects:						
Children	1 child	-0.0003	-0.004	-0.021	-0.024^{+}	-0.02
(ref. no child)		(0.013)	(0.008)	(0.013)	(0.014)	(0.017)
	2 children	-0.03^{+}	-0.016*	-0.03*	-0.034*	-0.045**
		(0.013)	(0.007)	(0.013)	(0.013)	(0.017)
	3 children	-0.026^{+}	-0.013^{+}	-0.03*	-0.037*	-0.053**
		(0.014)	(0.008)	(0.014)	(0.015)	(0.018)
Occupation	Accountant		0.174***	0.158***	0.159***	0.158***
(ref. HR Assistant)			(0.006)	(0.013)	(0.014)	(0.014)
	Concierge		-0.178***	-0.197***	-0.2***	-0.2***
			(0.006)	(0.013)	(0.014)	(0.014)
Vignette-level interactions		No	No	Yes	Yes	Yes
Respondent-level fixed effects						
Gender/children	Female, with children				0.004	-0.001
(ref. female, no children)					(0.018)	(0.022)
	Male, no children				0.049*	0.026
					(0.018)	(0.03)
	Male, with children				0.017	0.011
					(0.018)	(0.021)
Cross-level interactions						
Children*respondent gender/children	1 child, female with children					-0.0003
(ref. No child*female, no children)						(0.021)
	2 children, female with children					0.004
						(0.02)
	3 children, female with children					0.016
						(0.021)
	1 child, male no children					0.006
						(0.026)
	2 children, male no children					0.043
						(0.026)
	3 children, male no children					0.039
						(0.027)
	1 child, male with children					-0.22
						(0.02)
	2 children, male with children					0.018
						(0.019)
	3 children, male with children					0.026
						(0.02)
Intercept		8.71***	8.71***	8.72***	8.71***	8.71***
		(0.012)	(0.009)	(0.011)	(0.015)	(0.016)
Marginal R-squared		0.003	0.469	0.469	0.482	0.483
Pseudo R-squared		0.299	0.788	0.789	0.791	0.791
No. vignettes		1512	1512	1512	1512	1512
No. respondents		328	328	328	328	328

Standard errors in parentheses. Full regression tables, including the interaction terms, in the appendix (Table A4.2).

*** p<0.001, ** p<0.01, * p<0.05, $^{+}$ p<0.1

We again summarise these results graphically, in Figure 4.2. As in Figure 4.1, we see differences in the magnitude of the penalty between groups as well as in the pattern of penalties as the number of children increases. However, we once again see significant overlap between the 95 percent confidence intervals – all the confidence intervals overlap to some degree at all points – suggesting no real difference between the four groups – as well as confidence intervals crossing zero in all bar one of the cases – for 3 children, the penalties of female respondents without children fall fully within the negative range. Again, we cannot be certain whether the uncertainty in these estimates is due to the number of observations available, or if it is indeed showing that overall differences in salaries offered to mothers versus non-mothers is not different from zero. Given we know that overall the recruiters hand down non-negligible wage penalties to mothers and that at the group level the difference also tends towards the negative, we can conclude that status characteristics/cogs in the machine is the most likely effect, while there is some, albeit weak, evidence of a Queen Bee effect.

Figure 4.2 Motherhood wage penalties by respondent gender and parenthood status for HR assistants (reference: no children).



Source: Table 2 regressions (random intercept multilevel models). 95% confidence intervals shown.

4.4.3 Further analyses, robustness checks

Our main results show minimal differences between respondent groups divided by both their gender and parenthood status. It is logical, however, to ask whether there is any effect of gender or parenthood status specifically. We re-run the multilevel models with age and gender separately in Table A4.3 in the appendix, to check whether differences are more prevalent when looking at gender and parenthood status only. Models M1 and M2 use respondent gender as the level-2 variable while models M3 and M4 use respondent parenthood status. The only significant result is that men give higher wages than women overall – in all others, we find no statistically significant difference in respondents by gender or parenthood status on the wages they deem adequate for mothers. This result provides still further evidence for the status characteristics/cogs in the machine hypothesis.

4.5 Discussion and conclusion

This paper has sought to deepen our understanding of the mechanisms behind the motherhood wage penalty. We analysed the variations in wage penalties given by recruiters by their gender and parenthood status on the basis of a factorial survey experiment amongst Swiss human resources professionals who were asked to assign wages to fictive CVs which varied, amongst other things, on their parenthood status.

The respondents as a group gave penalties of between 3-5 percent to mothers. When we divided the respondents into groups based on their gender and parenthood status, we find that all groups, on average, give penalties to mothers: women without children give penalties ranging from 4.5 to 6 percent, mothers penalties from 1.4 to 2.5 percent, fathers penalties from 2.5 to 5.6 percent, and, in the case of the small group of men without children, penalties are close to zero and negligible. Using multilevel models with random intercepts for respondents, we find the differences between groups not to be statistically significant. The confidence intervals of

all the groups cross, reinforcing the finding of little, if any, differences in motherhood wage penalties between groups. Moreover, in most cases, the 95% confidence intervals of the estimates cross zero, making it difficult to form firm conclusions about the group-level effects. We nevertheless propose some possible interpretations of the results, based on the theoretical framework and hypotheses proposed at the beginning of the paper, in order to also suggest future research directions on this topic.

Our literature review and theoretical framework proposed three hypotheses concerning differences in wage penalties between the four respondent groups. The first hypothesis, suggesting that homogamic feelings from female recruiters, mothers especially, towards job candidates who are mothers would lead to lower penalties from these groups. This hypothesis is clearly rejected. To the extent that there are any differences between groups of recruiters, it is women who give the largest penalties, with women without children giving, on average, the largest penalties of all four groups. These results are contrary to some of the established literature that suggest that the presence of women in management reduces the wage gap, though on this point, two caveats should be noted: first, Hultin and Szulkin (1999) and Cohen and Huffman (2007) point out the importance of the relative concentration of women in management roles. Our survey contains no information on this matter, and it could therefore still be possible that female recruiters in offices with a higher concentration of women are more generous towards mothers than women in male-dominated contexts. Second, Cohen and Huffman (2007) in particular argue that female managers at higher levels are more important to the reduction of the wage penalty than those in middle management. The respondents in our dataset, while all involved in recruitment and most (4 in 5) with “decisive influence” over hiring decisions, represent different levels of organisational hierarchy. An analysis of hiring decisions based on hierarchical position in the management may clarify this point, but the size

of our sample limits further division into levels of management, which are moreover not always clear in the names of the positions the respondents assigned themselves.

The results seem to lend some credence to our second hypothesis, that of the so-called Queen Bee syndrome – the idea being that women in positions of power will be harsher than men towards working mothers. This may either be because they have become comfortable in their masculine environments or are concerned about the impact of further feminization of the organisation on their own position, a feeling which may be held by all female recruiters. Alternatively, they are not sympathetic towards women with children because they see them as lacking the necessary sacrifice of family life for career success – a position most likely to be held by women without children. Our results show that female recruiters both with and without children give penalties to mothers, usually larger than those of male recruiters – indeed, in the regression models where the women without children group is the reference category, the effect of children is negative and significant. However, when comparing to other groups, we note that the differences are small and often with large, overlapping confidence intervals, and we therefore reject the hypothesis for lack of concrete information.

We can more readily confirm our third hypothesis. This hypothesis posited that recruiters would treat mothers similarly regardless of their own gender and parenthood status, either because motherhood is not a salient attribute for them, or because stereotypes are widely shared and thus recruiters as a group expect mothers to be less productive (or are all similarly influenced by norms that suggest that mothers should be more focused on home duties and less on work) – status characteristics theory. It is for this reason that we confirm this hypothesis: motherhood indeed seems to be interpreted as a negative signal by all groups of recruiters, as evidenced by the motherhood penalty found in the combined sample and the lack of statistical

difference between the groups in the multilevel analysis. The results may also suggest some evidence for the "cog in the machine" theory – having adapted to the prevailing organisational culture that assumes mothers to be less productive or expects them to be mothers ahead of workers, women penalise mothers in a similar way to men.

We acknowledge the drawbacks of this analysis. First, our recruiter sample deals with hypothetical situations in the factorial survey and only covers three large occupations. While we expect the results here to reflect the reality of the labour market to some degree – and indeed the previous comparative studies of the vignette survey we use and panel data suggest that the results are compatible (Oesch et al. 2017, McDonald 2020) – the external validity of the factorial survey is less strong than for observational data. Second, our sample consists almost entirely of recruiters who are human resources professionals and who have therefore received training in recruitment best practice and how to avoid undue discrimination during the hiring process (a small number of respondents reported being small business owners). In an instructive paper on motherhood penalties in Canada, Fuller (2018) shows that this training and experience is important: firms with dedicated HR departments show lower levels of discrimination than those without, everything else being equal. This, combined with the fact that the respondents are concentrated in Switzerland's larger urban areas, where childcare and other work-life support schemes are more readily accessible than in other, more rural parts of the country, leads us to the conclusion that our results should be considered conservative, lower-bound estimates. Third, much of the literature discussed in this paper refers to the importance of the gender composition of management in organisations. As this information is not available in the survey, we are unable to test whether this affects the judgements of our recruiters. Finally, the small size of some of the employer groups, notably that of men without children, means that large confidence intervals may be masking some real differences between

groups. It would therefore be useful to pursue this research with group sizes that allowed for more confidence in the estimation of these effects – this paper may be considered a starting point for research on a question that has received little attention to date despite its importance. Understanding motherhood wage penalties requires more information on employer-side information. This is important, because ultimately the decisions of employers and recruiters have an enormous impact – indeed, usually the final say – on what workers are paid.

Politicians and policymakers across the western world have to some extent pinned their hopes on the notion that as more women enter the workforce and accede to positions of power, the organisations they lead will become more equal over time, and without the need for any heavy-handed interference. The results of this paper suggest this remains a work in progress. Recruiters of both genders both with and without children all give mothers wage penalties – despite, in our study, their profiles showing no discernible differences in productivity, qualification or experience. In the short term, addressing this issue is more likely to involve sensitizing recruiters and employers to this behaviour, which may well be subconscious but nevertheless runs contrary to norms of social equity. It will also involve addressing the perception of mothers having lower productivity by providing better resources for working parents. These investments may not come cheap but have the long-term payoff of a more engaged workforce, and more equal wages for all workers.

5. Wage expectations by gender: the link between expected and actual wages for men and women in Germany[1]

5.1 Introduction

The gender wage gap is a well-established phenomenon across economies of various types and sizes. And while much of the gap has been empirically shown to be due to productivity effects (Weichselbaumer and Winter-Ebmer 2005) and occupational segregation by gender (Polachek 1987), a significant unexplained portion of the wage gap has led to speculation about what unobserved mechanisms may be behind the remaining penalty. Much of this attention has been devoted to labour demand-side analysis and actual wages – namely, that employers discriminate against women. Many studies have indeed shown that employers pay women less than men and that female profiles are seen as deserving lower wages than male profiles (see for example Neumark 1988, Auspurg et al. 2017, Blau & Kahn 2017).

Lesser-studied but nevertheless interesting mechanisms relate to the labour supply-side – that women may expect, and therefore accept, lower wages than men. This could be because of differing negotiating behaviours and perceptions of their value on the labour market: men may consider themselves to be more valuable while women have lower (or more realistic) expectations, which may lead to more assertive wage bargaining and therefore higher wages for men. In addition, in a multidimensional consideration of compensation for labour and working conditions, women may consider other factors to be more salient than men, who may focus more strongly on wages. If women consider more flexible working conditions – the ability to work part-time, and the availability of family-friendly structures such as childcare

[1] This chapter is co-authored with Laila Schmitt, PhD student at LMU Munich.

and the possibility to take time out of the workforce without a large penalty, for instance – to be reasonable trade-offs for wages, they may lower their wage expectations accordingly. This mechanism of compensating differentials (Polacheck 1981) is likely to be most strongly influenced by mothers or women planning to have children. Finally, women's wage expectations may be lower than men's due to their anticipation of a lower accumulation of human capital (again most likely driven by motherhood) or awareness of the differences in the wage profiles of men and women in general, partly due to job segregation, with women still concentrated in lower-wage professions than men.

It is clear then that lower expected wages could well lead to lower wages for women on the labour market. Empirically testing this hypothesis, however, is not so straightforward. While data from unemployment services, which often collect information about expected wages, can provide insight into reservation and accepted wages, it is likely that these estimates will be downwardly biased as people who have endured a spell of unemployment will tend to have lower wage expectations than those in work. Likewise, surveys that enquire about the expected wages of working adults – rare in any case – can be expected to be positively biased as those in work will expect higher pay. Both types of survey will miss observations of individuals voluntarily inactive on the labour market for reasons such as motherhood as well as individuals involuntarily long-term unemployed. Taking such profiles into account is, however, important, as not accounting for non-random selection into work may bias estimates of wage penalties. Surveys of students may collect useful information about expected wages at the beginning of the career, but cannot account for the effects of labour market experience and probably not of differing family situations.

There is, therefore, a gap in our understanding of the gender wage gap and how it is related to gendered differences in wage expectations. This paper will address the gap by analysing data from the 2013 German Socio-Economic Panel Innovation Sample (SOEP-IS). The 2013 data release contains a module exploring respondents' preferences for jobs and willingness to accept jobs that includes information about expected wages for all respondents (wage levels the respondents see as acceptable for their profile) and actual wages for those in employment. We are therefore able to use these data to investigate gaps in expected and actual wages by gender, ascertain whether they vary between those in- and out-of-work, and empirically test the strength of the link between expected and realised wages. If gender gaps are consistent, this may be a sign that the persistence of the gender wage gap lowers women's expectations of their wages. A higher gender gap in realised than expected wages may on the contrary reinforce the notion that discrimination or other demand-side explanations are a strong driver of gender wage differences. Finally, if expected wages exceed the wages individuals can realise, this might be a strong reason why some individuals withdraw from the labour market. Using information available on labour market experience and family situations we are also able to measure the effect of these variables on the differences observed.

In the next section we will discuss the existing literature concerning the gender wage gap and expected wages. This will be followed by an outline of our data and analytical strategy, and presentation of our results. We conclude with a discussion of the implications of the study and suggestions for future directions.

5.2 Theoretical approaches and empirical evidence

5.2.1 Supply-side explanations and mechanisms behind expected wage gaps by gender

In order to understand the relationship between expected and actual wages, we must first understand why wage gaps exist in expected wages between men and women. The literature identifies four mechanisms that may explain the difference: (1) differences in negotiation strategies (Kiessling et al. 2019, Pfeifer and Stephan 2018), (2) anticipation of productivity differences, perhaps leading to (3) different priorities for job characteristics, and (4) anticipation of discrimination. These latter three mechanisms, which are the focus of this paper, may also be affected by expectations based on occupational segregation and are likely to be strongly linked to one another. Expectations of forming a family, or having a partner and/or children already, will also colour expectations related to the final three mechanisms.

If women anticipate a slower accumulation of human capital than men and therefore lower productivity at work, they may also have lower expected wages than men. The human capital theory is a common explanation of gender wage gaps, notably applied to women who take time out of the workforce in order to raise children and take care of a family. This time out of the workforce, or a reduced attachment to work, results in a slower accumulation of human capital and therefore lower wages. It may also indirectly affect career choice, with women more likely to choose careers that are less risky and more amenable to time out of the workforce, but with less opportunity for promotion and lower returns to experience resulting in a flatter earnings profile (Polachek 1981).

Additionally, women might have lower expected wages than men because they have different work priorities. Due to the division of labour in connection with child-raising, women might expect to exert less effort on paid work (Becker 1985). This may also result in flatter wage

profiles for women than for men and can also mean that women choose different jobs requiring less work effort.

A preference for less risky careers with flatter earnings profiles may also be considered a manifestation of the compensating differentials theory (Filer 1985). This theory argues that women will choose careers or jobs that can easily be done on a part-time basis and allow work time flexibility and a supportive environment for work-family harmonisation which may include official structures such as access to childcare but also more informal aspects such as supportive co-workers and supervisors. If women, especially those who are mothers or planning to become mothers, already hold differing job preferences to men based on these factors, and are willing to trade off wages for more acceptable working conditions, it is reasonable to expect that this will be borne out in expected wages.

On the other hand, gender-specific perceptions of what fair wages are may result in anticipation of lower wages for women on the labour market (Auspurg et al. 2017). Same-gender referents refer to the idea that men and women see other men and women as having similar careers to themselves, and therefore benchmark their own career success and expectations mostly against others of their own gender, especially in fields with a high concentration of a single gender (Gibson and Lawrence 2010). The evidence that same-gender referents influence perceptions of fairness of labour market outcomes is strong – given that women compare themselves to other women, who generally have lower wages than men, this equally means accepting lower wages than men to be fair (Major 1987; Bylsma and Major 1992, 1994), particularly given the clustering of women in lower-wage and lower-status jobs (Charles and Grusky 2004). If similar mechanisms are at play in the setting of wage expectations it can safely be assumed that these will be lower for women than men.

Moreover, women may also anticipate discrimination on the labour market from employers leading to lower wages. Gender-based discrimination is generally agreed to fall into one of two categories, though how these two interact and ultimately affect wage outcomes remains a topic of some discussion amongst economists and sociologists (Guryan and Charles 2013): statistical discrimination, and discrimination based on taste or prevailing norms (Bielby and Baron 1986, Blau and Kahn 2007a). The former argues that commonly held stereotypes, such as women (mothers especially) being less productive than men, will be applied to an entire group. If these stereotypes are indeed commonly held, it should be expected that women will also take them into account when formulating expectations about their wages. If motherhood is the primary driver of productivity differences between men and women, this stereotype will be particularly consequential for women with children.

Discrimination based on taste or social norms argues that prevailing social norms will lead to favouritism of one group over another on the labour market, irrespective of productivity considerations but rather based on the prejudices and preconceptions of employers (Becker 1957). In this framework, it may then be the case that women, mothers especially are expected to primarily take care of domestic duties and child-rearing, with work a secondary consideration – a social norm that remains prevalent in German-speaking countries (Krüger and Levy 2001). In this case, an employer may respond to the tension arising from expectations of a good worker and expectations surrounding a good wife and mother by offering lower wages. Anticipation of this type of discrimination, then, is likely to lead to depressed wage expectations, amongst women in general but mothers in particular.

Based on this discussion of career preferences and anticipation of discrimination, we form a first pair of hypotheses:

H1: Women's expected wages are lower than men's, and this effect will be moderated by

H1.1: women's choice of family-friendly work arrangements (part-time work)

H1.2: the presence of children in the household

5.2.2 Selection into employment

Wage expectations, and their differences by gender, do not only matter because of their influence on wages people may accept on the labour market. They may also be linked to whether or not a person chooses to work in the first place. Mechanisms behind the decision to work, unemployment or labour market inactivity, and what influence expected wages may have, are likely to differ based on gender, as well as other socio-economic variables (Olivetti and Petrongolo 2005, Herrmann and Machado 2012; Elwert and Winship 2014). For instance, women with higher earnings potential may select into employment at a higher rate than that of women with lower earnings potential, and this differential may result in smaller actual gender wage gaps than if this selection is to be accounted for. On the other hand, if women, married women and mothers especially, place higher value on working less (or not at all) or being flexible with their time, they may require higher wages to become or remain active on the labour market, especially if they have high-earning partners and family wellbeing does not rest on their employment. In this case, the gender wage gap in expected wages may be smaller amongst inactive people than active, as women may be voluntarily inactive and therefore require stronger incentives to re-enter the workforce while men are more likely to be involuntarily inactive and therefore more willing to accept lower wages.

On balance, we expect the combined impact of high-earning women choosing to continue to work, women (mothers especially) with high-earning husbands choosing to reduce or stop work and men who are unemployed being more likely to come from lower-pay, lower-prestige

jobs to be that expected wage gaps will be smaller amongst those who are not working. We therefore propose a second hypothesis:

H2: The gender gap in expected wages amongst non-working individuals is smaller than the gender gap in expected wages overall, due to a higher expected wage amongst women who choose not to work.

The discussion of selection into employment based on wage expectations has wider implications and has been the subject of debate in the sociological and economic literature for some time, given, as outlined above, it is likely that not accounting for mechanisms that drive the decision to work or not may bias estimates of wage gaps observed on the labour market, particularly concerning gender and motherhood. As Elwert and Winship (2014: 41-42) demonstrate in their overview of various selection biases, motherhood influences the employment levels of women through both the wages they expect to be paid in order to work (which, in line with the discussion above, are likely to increase under many circumstances), and the wages offered to them by employers, which in many cases will decrease compared to non-mothers. Those returning to the labour market after having children may not then share the same characteristics as those who do not.

Addressing this selection issue (commonly referred to as *Heckman selection bias*) is therefore important. Several methods of doing so are proposed by the literature. The first, the Heckman correction, can be used where information on the potential wages of those out of the labour market is unavailable (see Neuman and Oaxaca 2003 for an example). Briefly, researchers estimate a probability model of an individual being employed (whether they are or not), based on other observable characteristics, and integrate this into wage prediction models (Heckman 1979). However, a paper by Ferber and Green (1985) suggests it may also misrepresent the

wage expectations of non-working women. They use a small dataset collected specifically to test the differences between three methodological options for estimating potential earnings for women who do not work: first, regressing wage coefficients obtained from working women on the non-working, second, including the probability term of being employed in a regression of all respondents (Heckman correction procedure), or third, analysing the expected wages of non-working women. They find that the Heckman correction produces lower wage estimates for non-working women than using the expected wages the respondents report themselves and that consequently, the information directly from those out of the labour force is the ideal. We use such data for our analyses here and can therefore provide more accurate information on non-working women and whether their expectations may be linked to labour market inactivity.

5.2.3 The importance of expected wages

Beyond being a reason for selection into employment, why do wage expectations matter? Gender gaps in expected wages may be of little consequence for the employed if they do not reflect or influence observed wages on the labour market. Existing theory and empirical studies suggest, however, that expectations strongly affect wages. The simplest theoretical explanation is that if women expect lower wages and/or perceive lower wages to be fair, they will accept and be satisfied with lower wages than men (Keaveny and Inderrieden 2000). If this is linked to a normative belief of women as being less productive than men, or of an overall underestimate of the value of women's work, then observed wages on the labour market are in turn likely to reflect these lower expectations (Jackson et al. 1992, Major et al. 1984). These expectations may then become self-fulfilling and fuel a cycle of lower expectations linked to lower outcomes: women expect and therefore accept lower wages, feeding a continuing expectation of lower wages than men (Ridgeway 1997).

Our final hypothesis, then, concerns the link between expected and actual wages:

H3: Gender wage gaps in expected wages are consistent with gender wage gaps in actual wages.

5.2.4 Empirical evidence for expected gender wage gaps

To what extent anticipating the aforementioned mechanisms affects expected wages, the effect of expected wages on actual wages, and wage-based selection into employment has been the subject of some empirical study. An early example comes from Blau and Ferber (1991), who analyse a questionnaire of business graduates in Illinois. Their results show that while both men and women expect similar wages upon graduation, these expectations diverge significantly when asked about later in the career: an expected gap of $25,000 in annual income 20 years into the career is established (Blau and Ferber 1991: 588). While women expect to work fewer years than men, this is not found to have an effect on expected wages. The authors suggest therefore that while women may expect little discrimination at the beginning of the career, they are sensitive to its possibility later on, as well as anticipating a flatter career than men (Blau and Ferber 1991: 598-99). These findings are consistent with the expectations of both the theories of slower accumulation of capital and anticipation of discrimination. In terms of compensating differentials, differing job characteristics are not a principal focus of the study. It can nevertheless be seen that while there are slight variations in preferences for job characteristics – women place slightly more importance on a pleasant work environment and flexible working hours than men – such preferences have no bearing on expected wages. These findings are confirmed by Jackson et al. (1992: 658-9), who equally find that preferences for different job characteristics are not linked with differences in expected wages, at least within occupations, though it should be noted that compensating differential mechanisms may equally be operating in terms of occupational choice. This study, however, finds that women systematically perceive lower pay than men to be fair for themselves, and that this perception

of lower pay as fair accounts for much of the expected pay gap at the beginning of the career, though less when respondents are asked about peak expected pay. These results provide strong evidence that same-gender referents are an important factor in setting of expected wages as well as in accepting lower wages for women.

Orazem et al. (2003: 317-18) survey a group of graduates in Iowa, first six weeks prior to graduation and then again six months later, with questions about expectations regarding wages, career outlook and gender roles, among others. Female graduates in this survey expected slightly lower wages than males upon graduation – a $2000 gap that was found to be strongly correlated with expectations of discrimination on the labour market. The study also offers some limited evidence on the relationship between expected and realised wages. When respondents were surveyed again after entering the workforce, it was found that men overestimated their starting pay by 2 per cent while women underestimated theirs by 5 per cent, an indication perhaps that while women correctly identify the likelihood of lower pay, they overestimate the magnitude of the gap (Orazem et al. 2003: 310).

Similar results are found in European countries. Brunello et al. (2004) use a sample of 3000 students from 10 countries and find a gender gap in expected wages amongst business graduates that is closely linked to women's expectations of worse job prospects than their male counterparts, suggesting anticipation of discrimination does indeed have an effect on lower expected wages from women. No follow-up on actual wages once entering the labour market is included. However, in the same year, Webbink and Hartog (2004) conduct a study to find out whether or not graduates can accurately predict their salary, by asking them about their expected wages at graduation, and then enquiring about their actual wages 4 years after their leaving education. Though not specifically a study of the gender wage gap, they nonetheless

find that students' predictions of their starting salaries are both reasonably accurate and reflect the earnings structure of the actual labour market (Webbink and Hartog 2004: 106), including predictions of lower starting salaries from women.

More recent, large-scale evidence from Germany is provided by Kiessling et al. (2019), who analyse information from 15,000 university students. They find that women do indeed expect flatter earnings profiles and lower wages overall than men, with a lifetime cumulative difference of over €500 000. While a decomposition analysis of the gap shows that the largest part can be explained by sorting into different areas of study and occupational groups, career-planning effects nonetheless explain 14% of the gap, mostly due to women expecting to work fewer hours, rather than due to time off work for family reasons, which the authors suggest women in the sample underestimate (Kiessling et al. 2019: 26-27). When they ask the students about the actual wages of graduates, they find that the predictions slightly underestimate the actual gap reported by the graduates in the sample – a 19% actual gap compared to a 15% predicted gap (Kiessling et al. 2019: 16). Contrary to the findings in the US of Orazem et al., here the students slightly underestimate starting wages of both male and female graduates, though in this instance, the two are distinct samples and not the same respondents reporting their wages at a given time after graduation.

The studies mentioned so far revolve around students, but similar effects are seen in samples of the unemployed, concerning reservation wages, as well as a handful of studies of people active on the labour market. Fransson and Biel (2000) seek to understand differences in expected wage gaps in male- and female-dominated sectors. They survey equal numbers of men and women in a male-dominated (metalwork) and two female-dominated sectors (nursing and retail). In their findings, pay expectations are strongly affected by years of work experience

and current salary, exposing a pitfall in relying on student-only surveys to infer conclusions for the labour market in general. Furthermore, they find that in most cases gender does not influence expected pay, suggesting that in highly gender-segregated jobs at least, pay expectations are similar amongst men and women. This could be interpreted as evidence that occupational segregation is one of the key drivers of expected wage gaps as well as observed wage gaps. However, in this study respondents were asked to give a "general" acceptable wage (paid to male *and* female workers), not one for themselves, and it is possible that this result could differ from those garnered from questions pertaining to the individuals themselves.

It can be seen, then, that while the theoretical expectations of the mechanisms behind expected wages and why they matter may be clear, the empirical analysis thus far focuses mostly on expected wages alone and only hints at whether or not these expectations affect realised wages. Given most analyses thus far revolve around students, do not interrogate the same samples or ask for general rather than individual expectations of wages, a link between expected and realised wages cannot be confidently established. This is especially important as Fransson and Biel's study (2000: 11) argues that experience is an important factor in differences in expected wages, yet most studies to date ask students what they expect their wages will be at certain points later in their careers rather than questioning those on the labour market directly.

5.3 Data and methods

5.3.1 Data

We use a module from the 2013 German Socio-Economic Panel Innovation Sample (SOEP-IS), "Job Preferences and Willingness to Accept Job Offers" to carry out our analyses. The SOEP-IS is a representative panel run separately to the general questionnaire designed to facilitate innovative forms of analysis. In addition to the innovation modules itself, the SOEP-

IS contains a condensed version of the SOEP household and personal questionnaires, providing annual information on employment status and wages if working (the actual wage variable), as well as a suite of variables on education, family situation, and social background. The "Job Preferences and Willingness to Accept Job Offers" module of the SOEP-IS is constructed around a factorial survey experiment designed to capture variations in willingness to accept work based on different characteristics of a job. The factorial survey itself is not ideally designed for the analysis of our hypotheses, but in this module, respondents were asked to assign themselves a gross full-time monthly wage they felt was appropriate for their qualifications and experience. We use the responses to this question as the expected wage variable.

The dependent variables are the gross hourly expected wage and the gross hourly actual wage including overtime pay. The hourly wages were calculated by dividing the gross monthly earnings by the actual working hours per week including overtime, multiplied by 4.33. The main independent variables are female, work status and children. Work status was measured by full-time work, part-time work including marginal work and non-working including maternity leave and unemployed. A dummy indicating a child under 14 in the household measures children. As we are interested in gender gaps in wages, we include interaction terms of female with work status and children in the main analysis for expected wages. Log gross hourly wages are used in the multivariate analyses. In descriptive analyses, however, de-logged values are presented for easier interpretation. Additionally, we include several variables controlling for confounders. We include education (in years), age (in 4 categories 17-30, 31-40, 41-50, 51-60 years), partnership status (partner, not living together; partner, living together; married), and migration background. We further include ISCO 1-digit occupation categories

(last-held occupation for the out-of-work) in some of the regression models to account for further occupational effects.

A total of 1,678 observations (889 women) are available for the analysis, representing a cross-section of the German population. We exclude respondents from Eastern Germany, a comparatively small group (*N*=306) where we would expect differing wage structures and socio-economic factors to be at play (Matysiak and Steinmetz 2008). All analyses are based on the same selection criteria (e.g. observations in the sample need to have non-missing values on all variables). In total, then, we are left with 1,210 observations (567 women) for the expected wage analysis, and a subsample of 1,028 observations (508 women) in employment, self-employment or civil service for the actual wages. Finally, for a comparison of the expected and actual wage we use a larger subsample of 1,078 observations (543 women) including the last actual wage in the years before 2013 (in 2010 prices) for some non-working persons (50 observations, 35 women).

Table 5.1 Variable Means for Samples

	Expected wage sample		Actual wage sample		Last actual wage sample	
	Men	Women	Men	Women	Men	Women
	Mean/N (sd/%)	Mean/N (sd/%)	Mean/N (sd/%)	Mean/N (sd/%)	Mean/N (sd/%)	Mean/N (sd/%)
N	567 (46.9%)	643 (53.1%)	520 (50.6%)	508 (49.4%)	535 (49.6%)	543 (50.4%)
Expected wage in €/h	21.8 (11.1)	15.4 (7)	22.4 (11.2)	16.2 (7.2)	22.3 (11.2)	16 (7.1)
Actual wage in €/h	-	-	19.04 (10.6)	13.7 (6.7)	18.8 (10.6)	13.6 (7)
Education in years	12.6 (2.7)	12.4 (2.7)	12.7 (2.7)	12.5 (2.6)	12.6 (2.7)	12.5 (2.6)
Age in years	42.9 (10.6)	42.7 (10.7)	42.9 (10.5)	43.6 (10.4)	42.9 (10.5)	43.3 (10.5)
Full-time	488 (86.1%)	200 (31.1%)	488 (93.9%)	200 (39.4%)	488 (92.2%)	200 (36.8%)
Part-time	32 (5.6%)	308 (47.9%)	32 (6.2%)	308 (60.6%)	32 (6%)	308 (56.7%)
Non-working	47 (8.3%)	135 (21%)	-	-	15 (2.8%)	35 (6.5%)
Child under 14 in HH	191 (33.7%)	237 (36.9%)	182 (35%)	157 (30.9%)	186 (34.8%)	180 (33.2%)

Notes: For all samples, persons need to have a valid expected wage. The subsample last actual wage includes non-working persons whose wage is the last actual wage in the years before 2013.

5.3.2 Analytical strategy

In order to test H1, we estimate OLS regressions of expected wages, with gender as the main independent variable. We first include the interaction of female and work status: whether a respondent is working fulltime or part-time, or is non-working, to test if the effects of female varies by these explanatory variables. In a next step we remove the work status measures and then include the interaction of female and child, before adding the work measures back to the analysis in the last model. All models include the basic controls that are partnership status (partner, not living together; partner, living together; married), education (in years), age (in 4 categories) and migration background.

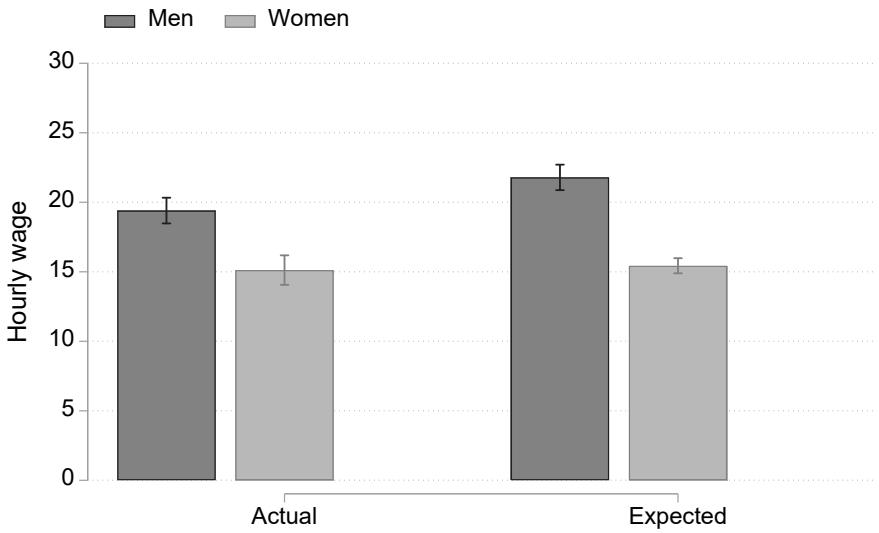
For H2, concerning the different sizes of the expected wage gaps amongst different groups of respondents, we use the results of the initial OLS models to test H1, to estimate the change in log-points of the expected wages from being female over the three different work statuses: Full-time employed, part-time employed, and non-working. Results presented in the appendix further divide the non-working into voluntarily out of the workforce (stay-at-home mothers, for instance), and involuntarily unemployed, however, group sizes here are small and not compatible with the last actual wage analyses.

To test H3 we also turn to OLS regressions in a first step. Here, we estimate regressions for the logarithm of expected and the logarithm of actual wages, as well as a third regression with the difference of these two as the dependent variable. The control variables remain the same as in the regressions for H1.

5.4 Analyses and results

Figure 5.1 shows mean actual and expected wages for Western Germany, divided by gender. We see in both cases a gap of 22-29% between men and women. The gap in the expected wages is larger than in actual wages, but this is mostly due to men's expected wages being higher than the observed. Women, then, seem to more accurately estimate their labour market value, suggesting they may indeed take into account the mechanisms that lead to the wage gap when setting their wage expectations. For both men and women, expected wages are higher than the observed actual wages, though only very slightly so for women – respondents may be expressing confidence that they can obtain higher wages through a job change, or that they would only change jobs for a better wage.

Figure 5.1 Actual wage and expected wage by work status in Euros per hour

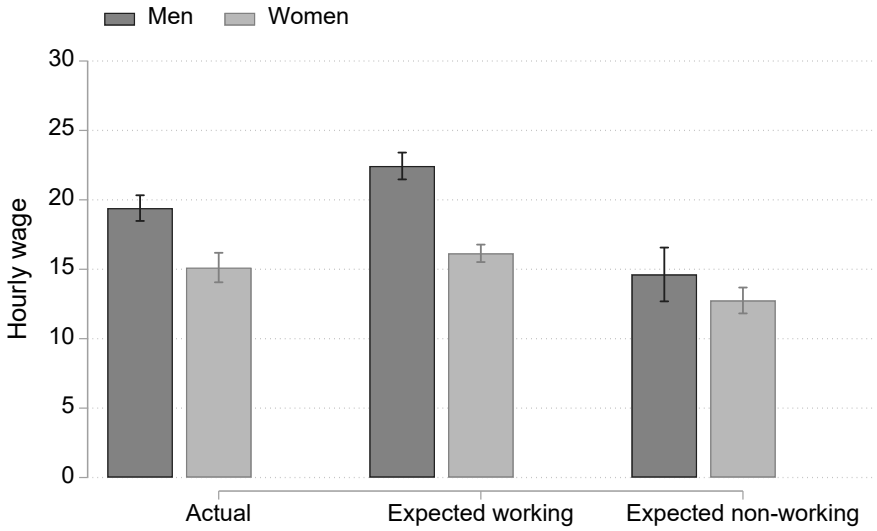


Notes: Gap actual wages, 22.1%; expected wages, 29.1%. Mean wages. For actual only full-time, since expected was asked for fulll-time job. N actual: men: 488, women 200, expected: men: 567, women 643

We divide respondents into work and non-working groups in Figure 5.2. Here, we find a larger wage gap in expected wages than actual wages among the working, which, as in Figure 1, are higher than the actual wages reported. For the non-working, the expected and reservation wages of men are significantly lower than for the employed, but for women this difference is only small – the resulting expected gender wage gaps is 12.8%, as opposed to 28.0% among

the working. This may reflect the compositions of the inactive groups (men involuntarily, women voluntarily unemployed) or selection into the labour market based on wage expectations. Descriptively, therefore, we see evidence in favour of all three of our hypotheses. We turn to the multivariate analysis to confirm these findings and delve deeper into the mechanisms that may be behind these differences.

Figure 5.2 Actual wage and expected wage by work status in Euros per hour



*Notes:*Gap actual wages, 22.1%; expected (working individuals), 28%; expeected (non working individuals), 12.8%. Mean wages. For actual only full-time, since expected was asked for fulll-time job. N actual: men: 488, women 200, expected working: men: 520, women 508, expected non-working: men: 47, women: 135

5.4.1 Expected wage

To test the hypotheses that women's lower expected earnings hold after adjusting for controls, especially when considering different work status groups and family situations (H1/H2), Table 5.2 presents the results of ordinary least squares (OLS) regressions of log expected hourly wages. The models are estimated for the total expected wage sample, including working and non-working persons. We begin with the discussion of gender gaps in expected wages in differing work status groups, and then turn to the effect of children on expected wages.

Model OLS 1 includes gender, work status and the interaction between these two as the main independent variables. Additional controls are partnership status (partner, not living together; partner, living together; married), education (in years), age (in 4 categories) and migration background – the full regression tables are available in the Appendix (Table A5.1). The key variable for testing the effect of women's lower wages is the interaction of female and work status, which shows the different gender wage gaps for full-time and part-time working persons and non-working persons. In line with our H1, there is a significantly lower expected wage of females for full-time work of 20.4%. Part-time work decreases the expected wage relative to full-time work by 0.361 log points, but only for men. The effect for female is less negative for part-time than for full-time, resulting in a gender gap in expected wages in part-time of 0.055 log points. Non-working incurs a negative effect compared to full-time work of 0.324 log points for men. The gender gap in expected wages for non-working persons of 0.145 log points is somewhat smaller in magnitude than the gender gap in expected wages for full-time persons – though this difference is not statistically significant.

Table 5.2 OLS regressions of log expected hourly wage

	OLS 1 Female x Work Interaction	OLS 2 Female x Child Interaction	OLS 3 Female x Work + Family Interaction
Female	-0.229***	-0.286***	-0.224***
	(0.029)	(0.026)	(0.031)
Part-time	-0.361***		-0.361***
	(0.063)		(0.063)
Non-working	-0.324***		-0.322***
	(0.052)		(0.052)
Child under 14 in HH		0.053	0.037
		(0.035)	(0.034)
Female x Part-time	0.174*		0.166*
	(0.071)		(0.071)
Female x Non-working	0.084		0.068
	(0.065)		(0.066)
Female x Child under 14 in HH		-0.098*	0.002
		(0.043)	(0.043)
Constant	1.767	1.663	1.764
Adjusted R-square	0.486	0.440	0.486
N person-years	1210	1210	1210

Notes: Additional controls: Partnership status (partner, not living together; partner, living together; married), education (in years), age (in 3 categories) and migration background.
*p < .05, **p < .01, ***p < .001

Figure 5.3 Effects of female on expected hourly wage within work statuses.

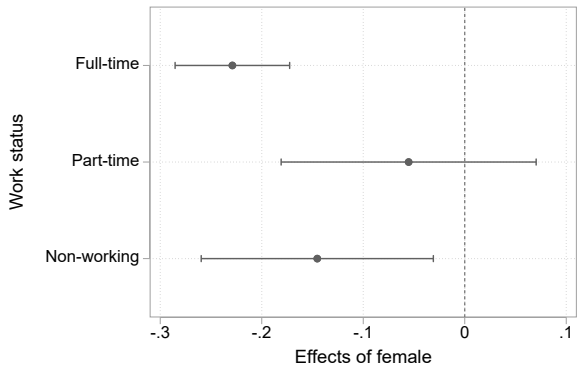


Figure 5.4 Effect of work status on expected hourly wage for male and female respondents.

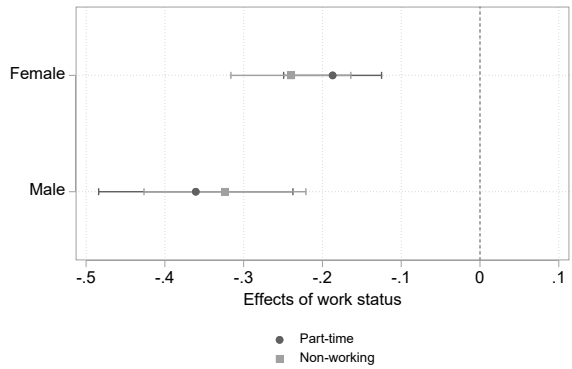
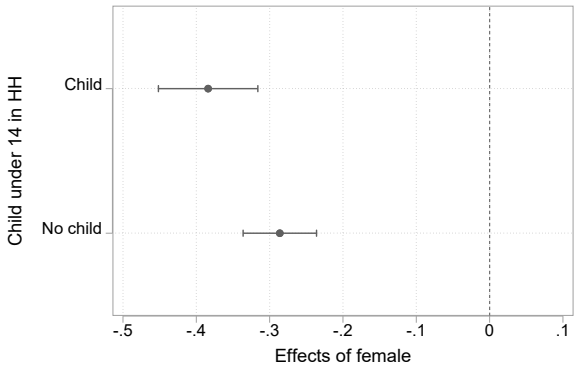


Figure 5.5 Effects of female on expected hourly wage within parental status.



*Notes :*Values drawn from Table 5.2, OLS 1 (Figures 5.3 and 5.4); OLS 2 (Figure 5.5). Effects shown in log points with 95% confidence intervals.

Figure 5.3 illustrates graphically how this effect of gender differs with work status. The gender gaps in expected wages are estimated with the nested interactions of gender and work status, which is equivalent to estimating separate regressions for work status subgroups. This figure demonstrates clearly the expected gender wage gaps by levels of employment: largest for full-time workers, smaller for the out of work and again smaller, indeed, no different from zero at the 95% confidence level, for part-time workers. The implications for our hypotheses are mixed: while the existence of an expected gender wage gap is overall confirmed, we expected this to be driven by the choice of women to move into work circumstances that were better suited to work-family balance. Part-time work, however, is shown to reduce the expected wage

gap, suggesting men in this group revise their expectations down even more than women do. Indeed, this is confirmed by results presented in Figure 5.4, where we show the impact of work status by gender. This reinforces the notion that while part-time work may be an acceptable way for women to manage family and work expectations, it remains a fringe work type for men that dampens wage expectations somewhat more than for women. Moreover, it may point towards the effect of part-time work on career development and human capital accumulation: those in part-time work are likely to be stuck at lower levels of the hierarchy, in occupations with little room for career and/or salary progression (Russo and Hassink 2008). And while this may affect men and women in part-time work equally – these results are in line with statistics showing that in Germany there is almost no wage gap for part-time workers (Eurostat 2020) – it is clear that given that part time work is disproportionately female, it remains an important issue to solve for the gender wage gap, for working mothers in particular, as they are far more likely to reduce working time than other women.

H2 expects wage gaps amongst the non-working to be smaller than those in work. The results go in the direction of smaller gender gaps in expected wages for the non-working than for the full-time working, but the difference is not statistically significant. We also expected that the selection of high-earning women into full-time work especially will result in smaller wage gaps in the group of non-working as well. This is clearly rejected. When we plot the differences in wage expectations by type of work separately for men and women (Figure 5.4), we see that for both men and women, part-time and non-working groups have lower expected wages than full-time workers. However, the difference for men is larger than women, over 0.361 and 0.324 log points as opposed to 0.187 and 0.240 log points for women, for part-time and non-work respectively. While this is a rejection of the expectation of H2 in the sense that the difference between expected wage gaps amongst the non-working and amongst the full-time working is

not significant, it is clear that this is because non-working women do not raise the wages they expect in order to return to the labour market. It may rather be the effect of out-of-work people generally revising their wages downwards.

Further analyses do indeed seem to confirm this line of argument: in Table A5.3 in the appendix we show gender gaps for involuntarily unemployed and those out-of-work for other reasons. While there remains a gender gap in expected wages for the "voluntarily" out of work (those raising children or otherwise inactive but not searching for work) that is similar in magnitude to that of those in work, amongst the unemployed, as with part-time workers, there is practically no gap, suggesting both that unemployment may be a strong driver of wage expectations towards a fixed point, such as the minimum wage, and that the lower wage expectations of the non-working overall may be a result of this downward wage revision. However, given that both the group sizes of the out-of-work when divided in this manner are small, and the results of this analysis are not compatible with the last-reported wage analysis for lack of data, they should be treated with caution and would warrant further investigation with a larger dataset.

We continue to investigate the effect of children on the gender gap in expected wages, by including in model OLS 2 the effect of gender and its interaction with children under 14 in the household. While there is no effect of a child on the expected wages for men, there is a significant interaction effect of female and children, representing a motherhood wage penalty of 0.098 log points. This penalty results in a larger gender gap in expected wages for parents of 0.384 log points than for persons without a child of 0.286 log points. Figure 5.5 shows the results graphically with the effect of female over having a child. While the expected gender wage gap is larger amongst those with children than those with children, the size of the gap is modest.

Model OLS 3 includes the family and work measures, and shows a much smaller gender gap in expected wages, similar to model OLS 1, underlining that the work status explains much of the gender gap in expected wages. Notably, in this model the interaction of female and child that tests for a motherhood penalty in expected wage is non-significant after controlling for part-time work and non-work status. This is strong evidence of a link between motherhood and part-time work – that is, that mothers are the most likely to reduce their working time in order to raise children, and temper their wage expectations as a result. Mothers who remain in full-time work are therefore perhaps more career-oriented and less representative of working mothers as a whole, which may in turn explain the smaller than anticipated difference in expected wage gaps between parents and those without children shown in figure 5.5.

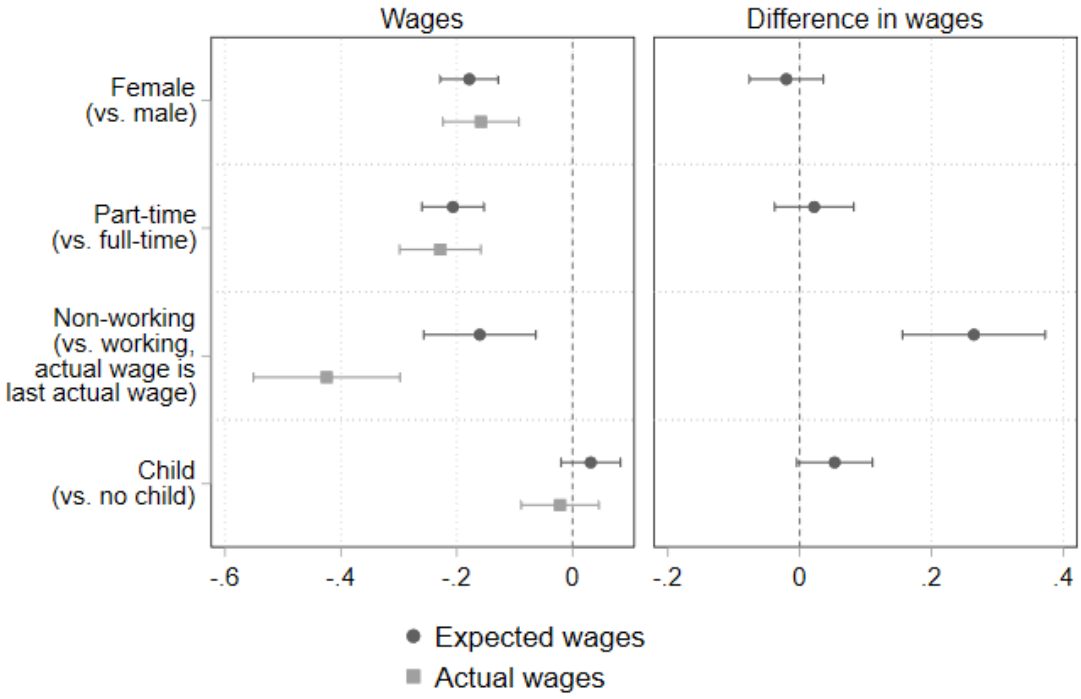
5.4.2 Expected and actual wages

To test hypotheses 3, that gender wage gaps in expected wages are consistent with gender wage gaps in actual wages, we estimate models for expected wages, actual wages, and difference of expected and actual wages. Figure 5.6 presents the results of ordinary least squares (OLS) models including work status and children as the main dependent variables but excluding interaction effects due to the small sample of non-working persons who have a last actual wage (N=50, 35 women). The models also include controls for occupational groups (ISCO 1-digit). The results table in the appendix shows that accounting for occupations does reduce the overall wage gaps, both expected and actual. However, the effect is small and the overall results do not change significantly. Further analysis, including interaction effects, is restricted by small group sizes, both due to the small sample of non-working with last available wage and some small occupational categories.

The left panel of figure 5.6 shows separate regressions of the expected hourly wages and the actual hourly wages, where the dependent variables are the logarithm of expected and

logarithm of actual wages. The right panel shows the differences between expected and actual wages by analysing their difference at the individual level. The dependent variable is the difference between the logarithm of expected earnings and the logarithm of actual earnings, (ln(exp)-ln(act)). Testing the coefficients in this regression against zero is equal to testing the difference of the coefficient for expected and for actual wages.

Figure 5.6 OLS regressions of expected wages, actual wages, and difference of expected and actual wages



Note: Three separate regressions of expected wages, actual wages, and difference of expected and actual wages at the individual level (ln(exp)-ln(act)). Results are estimated from ordinary least squares (OLS) models shown in table A5.2 (models OLS 4-6), reported in log points with 95% confidence intervals. The sample is the last actual wage sample that is a subsample of the expected wage sample. Actual wage for non-working/unemployed persons is the last actual wage in the years before 2013. Dependent variable is the hourly wage in 2010 prices. N=1,073.

There does not seem to be a difference between the gender gap in expected and in actual wages on the group level in the left panel amongst those in work. This is confirmed at the individual level in the right panel. There is also no significant effect of the difference in wages for part-time work–working part-time seems to push down both expected and actual wages to a similar

degree. For in-work individuals at least, these results confirm H3, that the expected and actual wage values are strongly linked, and that the gender wage gap is consistent between the two, but on a lower level (constant= -0.051, not plotted), in line with the trends shown in the descriptives. This may be a sign that women's expectations do indeed influence the actual wage gap (e.g. due to more reserved wage negotiations), and that the persistence of the wage gap lowers women's expectations of their wages, in line with the expectation of a feedback loop situation concerning expected and actual wages (Ridgeway 1997). These results provide no evidence that either women are too pessimistic in their wage expectations or that men are overly confident – actual labour market experience has a strong enough effect to account for these trends seen amongst student populations in some studies (Orazem et al. 2003, for instance), but, on the other hand, do not show the underestimation of the gap seen in Kiessling et al. (2019) either.

That our question of expected earnings is incorporated in an ongoing panel also allows us to examine the difference of expected and actual wages for non-working persons – allowing for a further analysis of the selection hypothesis. Whether the gender gap in expected wages is also consistent with the gender in actual wages for non-working persons can be answered at the individual level by estimating a model for the expected earnings in 2013 and for the last actual earnings in the years before 2013. Results for this analysis show that non-working persons' expected wages are higher than their last actual wage. The significant difference between the intercepts of 0.264 log points shows that the last actual wage disadvantage for part-time is substantially higher than the expected wage disadvantage. Although the magnitude of the non-working effect differs for expected and actual wages, they are both negative compared to full-time work. It seems likely based on these results that the out-of-work are a lower-earning group than their employed counterparts, and that their expected wages do indeed represent a

reluctance to re-enter the labour market unless they find better paying work. We note however that we have only a very small sample with which to estimate the previous wage of the out-of-work, and that these interpretations should be treated with caution.

Overall, then, our results point to expected wage effects that are similar to actual wage effects for those in work. That previous wages amongst the non-employed on the contrary are lower than the expected wages for the same group suggests that there is some impact of selection into the labour market. Expected wages are lower amongst the non-working than those working full-time and similar for those working part-time but only for men. Gender gaps in expected wages seem to be impacted by choice of work and family situation, with no expected wage gap found for part-time work due in large part to significantly lower wage expectations of men in these situations, which may represent more "normal" choices for women.

5.5 Discussion

5.5.1 Discussion of results

This paper has analysed expected and actual wages through the use of survey data from Germany. It has specifically been interested in the existence of an expected gender wage gap and the mechanisms behind it (H1), whether there is any evidence of selection into employment based on expected wages (H2), and link between expected and actual wages for men and women (H3).

Concerning H1, we find that there is indeed a gender expected wage gap, and that work characteristics and family situations explain a large portion of it. There is a gender gap in expected wages for full-time working and non-working persons, but not for those working part-time, because part-time work lowers men's wages even more than women's wages. This may

be a result of part-time work being concentrated in low-level jobs and occupations with flatter organisational hierarchies. Given these jobs are often female dominated, the small group of men in this situation are likely unrepresentative of the labour market experience of men as a group. The negative effect for mothers on expected wages is an effect of maternal part-time work. The residual wage gap may be related to anticipation/expectation of discrimination, but we do not have the data available to test this effect. We therefore confirm the hypothesis that women's expected wages are lower than men's but are more guarded about the mechanisms: it seems that a choice of family-friendly career may be important, but further investigation of the other effects would be useful to form a clearer judgement.

H2 addresses the difference between expected wage gaps for the working and non-working individuals. We find that there is no difference in the two: while the gender wage gap amongst the non-working is somewhat smaller than the gender gap for those working full-time, this difference is not statistically significant. We hypothesised that a smaller gender wage gap for non-working persons may be due to the fact that while women with children who elect to be stay-at-home mothers may expect a higher wage to re-enter the workforce, therefore increasing their expected wages, men are more likely to be involuntarily unemployed and therefore lower their expected wages in order to find work. A further analysis of the non-working separated into involuntarily unemployed and those out of work for other reasons seems to reject this line of reasoning: gender gaps amongst the unemployed are non-existent, while for those out-of-work for other reasons they are of similar magnitude to those in full-time work. We therefore reject H2, that expected gender wage gaps are smaller amongst the non-working than the working, but caution against over-interpretation of this result due to small sample sizes of the non-working. Finally, we see differing results in expected wages for the different employment groups. This finding sends a signal that matters of selection, both into employment, and into

differing forms of employment (full- or part-time) should be taken seriously by research into labour market gender gaps.

Finally, H3 anticipated that the magnitude of the gender gap in expected wages would be similar to that of actual wages. This is confirmed by the analysis for the gender wage gap in expected and actual wages for work statuses overall. We cannot estimate the effect of female within work status here because of too few observations of non-working persons for actual wages. That the overall gender gap in expected and actual wages is consistent suggests that the relationship between the wages is important – either because expected wages influence realised wages, realised wages influence expected wages, or because of the "self-fulfilling cycle" (Ridgeway 1997) of lower expectations and lower outcomes for women. However, for those out-of-work for whom we have actual wages from the previous year, we see that expected wages are considerably higher than the previous actual wage. This may be interpreted as a selection effect that may simply be taking place amongst a generally lower-earning population. The non-working seem to be generally lower-earning, but within this group it is possible that some stay out of the labour force because their expected wages are higher than the last actual wage they could realize. However, we are not able to pinpoint the exact mechanisms behind this (possible) selection effect.

5.5.2 Limitations

While our analysis has found empirical evidence of some important mechanisms and effects concerning expected wages and the gender wage gap, we see some limits that could be addressed in future research on the subject. First, while it is an advantage of our dataset to be able to access information on actual and expected wages for the same individuals at the same time point, without a longitudinal design we are not able to measure changes over time

– important, notably, to be able to measure wage growth and whether or not women do indeed settle for less risky careers with lower wage growth but more stability and/or more possibilities to harmonise work and family duties. While this has been studied in the context of actual wages, quality longitudinal data on expected and actual wages remains a missing element in this research field.

Second, we are able to include some job-related variables in the analysis of expected wages – mostly hours of work, and only for those in employment. We also include occupation in some of the expected-versus-actual wage analyses, finding only a small effect that does not explain observed gender gaps. To further this line of inquiry, it would be useful to include information on how all respondents, not just those in work, would respond to jobs with varying characteristics in terms of possibilities to better combine home and work life. Such an analysis would provide a more complete test of how expected wages, actual wages and compensating differentials interact, as well as complement recent empirical research that suggests that such factors have an influence on participation rates (Barbieri et al. 2019, Stadelmann-Steffen 2020) by providing the wages angle. Finally, having such information available for analysis for all respondents would allow for a stronger test of the selection hypothesis, as well as allow for a methodological test of the efficiency of strategies for accounting for Heckman selection bias.

5.6 Conclusion

This paper has furthered the discussion on the expected wage gap by providing an analysis of expected and actual wages in tandem for a cross-section of the German population. We have shown that women assign themselves lower wages than men overall, and that these expected wages are more accurate estimates of their labour market wages than men's, suggesting women account for discrimination, slower accumulation of human capital and possible differing

priorities when setting wage expectations. These dampened wage expectations may in turn lead women to accept lower wages than men on the labour market.

Furthermore, we show expected wages are linked to family and work circumstances, with mothers and part-time workers expecting lower wages than non-parents and full-time workers. Motherhood and part-time work are closely linked, leading to a concentration in lower wage expectations amongst this group. Since we also show that the link between expected and actual wages is quite strong, this finding is further evidence that an important avenue for mitigating wage penalties would be either to normalise part-time work, or have better structures and policies in place for full-time work for parents. Finally, we have established differences in expected wages between working and non-working groups, due to the pessimistic wage expectations of the involuntarily unemployed, particularly men.

While this research has not been able to establish causal links, we have nevertheless uncovered important differences in expected wages by gender, work and family status, and strong enough links between expected and actual wages that should spur further research on this underdeveloped subject. Further investigation should focus on probing and causally analysing the expected-actual wage relationship, and better understand the effect of selection into the labour market, which continues to be an issue for studies of gender wage gaps.

6. Conclusion

The goal of this thesis has been to further our understanding of the impact of family decisions and situations – marriage for men, parenthood for women – on labour market outcomes. It has used a variety of analytical techniques to interrogate both the labour supply (worker) and demand (employer) side of the market, for a more complete picture of the mechanisms at work behind the well-established phenomena of marital wage premiums for men and motherhood wage penalties. In this concluding chapter, I provide a synthesis of the results of the empirical chapters of the thesis, the main contributions and limitations of the research, and reflect on implications and future directions for the field.

6.1 Results of the thesis

The results of the four empirical chapters of the thesis point to a labour market situation in Switzerland whereby married men earn more than their single counterparts and mothers earn less than women without children. It has also found that while work and productivity-related characteristics are an important contributor to these circumstances, the impact of employer discrimination is not to be discounted, and, on a broader level, the expectations of individuals concerning their own wages may also have an impact.

6.1.1 Results of the empirical chapters

In Chapter 2, I studied the impact of marriage on wages for men, with the goal of bringing together the three main strands of argument that explains the marriage premium: that more productive men select into marriage, that marriage makes men more productive and/or that employers express a preference for married men. I find evidence of all three mechanisms at play. While most of the premium is explained by selection, in line with the expectations of

recent empirical literature (Killewald and Lundberg 2017, Ludwig and Brüderl 2018), I nevertheless find some evidence of higher productivity amongst married men impacting the premium, albeit in a small way. Moreover, I further find a wage premium of up to 2% given to married men as compared to single by recruiters and HR specialists, for job application profiles that are no different in terms of productivity level or human capital, suggesting a preference for married men over single.

In Chapter 3 we move to the analysis of the motherhood wage penalty. Again we bring together several strands of previous research to analyse concurrently, aiming to understand whether the motherhood wage penalty in Switzerland can be explained by reduced productivity, decisions by mothers to trade wages for more family-friendly work conditions (compensating differentials) or discrimination against mothers on the part of employers. Net of productivity and work-related factors, we find wage penalties of 4-8% for mothers, in line with recent research for Germany (Gash 2009, Gangl and Ziefle 2009). The analysis of the HR professionals sample finds 3% penalties per child, with higher penalties for younger mothers than older – for profiles aged 50 and 55, there are no differences in wages for mothers and nonmothers, while for those aged 35 and 40, the penalty is 6% per child. These results are in line with those of Correll et al. (2007) showing wage penalties from mothers.

I delve further into the wage penalties given by recruiters in Chapter 4, which seeks to uncover differences, if any, in the motherhood penalty given by recruiters depending on their own gender and parenthood status. Theoretical approaches to organisational behaviour suggest that recruiters who are mothers may be more generous to mothers than other recruiters (homophily) due to shared experience or a better estimation of a mother's in-work productivity. In contrast, women without children, may perceive other women to be a threat or to have lower

expectations of the productivity of mothers and therefore give larger penalties ("Queen Bee" phenomenon). Finally, motherhood may be interpreted as a salient signal of productivity by all recruiters, and all recruiters will give similar penalties (status characteristics/ "cogs in the machine"). While we find some evidence of women with children assigning the highest penalties, the strongest evidence is in favour of status characteristics or cogs in the machine – little difference between differing groups is recorded and the differences that are present may be spurious.

Chapter 5 presents a new angle on the question of differing labour market outcomes. We study the expected and actual wages of men and women on the German labour market, to see whether anticipation or acknowledgement of differing life trajectories (including motherhood) or productivity influences the expected gender wage gap, whether there is a link between expected and actual wages for those in work, and whether the expected gender wage gap varies between the employed and the economically inactive, suggesting selection into the labour market based on wage expectations. We find that family and work situations explain some of the expected gender wage gap, and there is indeed a strong link between the gap in expected and actual wages. We also find that the expected gender wage gap is smaller amongst the economically inactive than workers. This may be due to effects of selection into the workforce, notably mothers raising the wages they expect in order to return to work, in line with the theoretical expectation of Elwert and Winship (2014). However, the results on this point are inconclusive.

Chapter 5 is the only empirical study that does not relate to Switzerland, instead relying on German data. We would, however, expect results to be similar (though not necessarily identical) between the two countries, given their similar labour market structures and family norms. Indeed, the motherhood penalties found in chapter 3 are very similar to recent findings

for Germany (Gangl and Ziefle 2009), supporting this expectation of generally mutually applicable findings. Therefore, we can reasonably safely assume the SOEP analysis may also provide useful information for the Swiss labour market. Importantly, this also suggests that the findings based on Swiss data may also be more widely applicable to the larger German labour market, as well as similar labour markets throughout Europe. The results of this thesis are therefore of interest beyond Swiss borders.

6.1.2 Results synthesis

Though the results of each of the four empirical chapters can stand alone and make their own individual contribution to the sociological debate surrounding inequality on the labour market, they also hold together to provide an integrated theoretical contribution. Here, I outline the four key points that are common to the separate empirical studies in order to synthesise the overarching message of the thesis.

Gender gaps exist and persist on the labour market and are not as simple as men earning more than women. While many of the findings of this thesis are on the lower end of the existing empirical research, particularly older studies, they nevertheless show clear differences in wages for mothers as opposed to non-mothers, and married men as opposed to single, both of which should be expected to contribute to ingrained differences on the labour market between men and women. This thesis therefore positions itself within the sociological and economic discussion that generally agrees that while gender and family-based wage discrimination is on the decline, it is proving stubbornly difficult to eradicate (Cohen 2002, Blau and Kahn 2007a, b).

Inequality on the labour market is affected by multiple supply-and demand-side mechanisms. Sociological and economic analysis of wage dynamics and labour force participation have come a long way since Becker's (1973, 1974) theories of marriage and household division of labour. Yet, it remains rare for empirical studies of wage inequality to simultaneously consider multiple mechanisms that may help to explain and understand such inequality, particularly concerning employer and recruiter-based discrimination. This thesis has shown that this is an important blind spot of wage inequality analysis. Both male marriage premiums and motherhood wage penalties are seen to be affected by productivity, selection and employer-preference factors, and personal characteristics are shown to have similar impact across recruiter groups. Moreover, the analysis of wage expectations shows that individuals themselves expect to be paid differently based on their economic and family situations. If these expectations carry over to the wages realised on the labour market, this is further evidence of the multidimensional nature of wage inequalities that researchers should be taking into account.

Occupations matter when considering wage inequality and discrimination. The penalties and premiums found in the factorial survey analysis do not apply equally for the three occupations under study. While the contribution of occupational segregation to gender wage inequalities is well-documented (Polachek 1987), here we see occupations not heavily dominated by one gender and note that motherhood wage penalties are largest for HR assistants, lowest (in fact, non-existent) for building caretakers, and somewhere in between for accountants. Concerning marriage premiums, we find the largest effects for accountants, followed by building caretakers, with no real premium for HR assistants. These findings are broadly in line with several related theoretical strands of research: first, that conservative family norms of good mothers and economically responsible husbands may be more prevalent in higher economic and social strati than lower (Berghammer 2014), while motherhood as an identity is less

important in working class contexts (Denny 2016, Glauber 2007). At the same time, marriage may act as a signal for responsibility and reliability amongst working-class men (Schwartz 1990), underlining diverging mechanisms of inequality for men and women in lower-status jobs.

Finally, *the labour market in Switzerland is evolving but wage inequality remains prevalent.* The theoretical framework for this thesis references Swiss case studies dating back as far as 1995, and while this thesis may provide a more optimistic view of the labour market situation in Switzerland for mothers in particular than the empirical studies that have preceded it, it shows that wage parity remains very much a work in progress. In a society where the breadwinner-and-a-half model remains the most common family setup, driven both by family norms and weak support for working mothers, wage inequality remains a reality, affected both by differing work behaviours and discrimination of recruiters – even in large organisations with HR departments nominally committed to non-discriminatory hiring and wage-setting practices.

6.2 Contributions

This thesis makes a number of useful contributions to the existing labour market literature on marriage, motherhood, gender and wages. First, it simultaneously considers the different mechanisms that theory and previous research argue are behind differing wage outcomes dependent on personal situations, rather than treating them separately. It does so by using several datasets providing information on both the supply and demand side of the labour market – the Swiss Household Panel (chapters 2 and 3), the Swiss Labour Force Survey (chapter 3), factorial survey experiment (LIVES-JOBVUL) (chapters 2-4) and the German Socio-Economic Panel Innovation Sample (chapter 5). Simultaneously analysing supply and demand-side data also allows for the smaller-scale employer study to be contextualised in the results of

the larger, representative panel studies, lending credibility and external validity to these results – in all cases, the results found in the analysis of the vignette study are plausible in the context of what is found in the panel data analyses.

The empirical chapters of this thesis apply several analytical techniques to ascertain statistically robust results. Concerning the panel data, in addition to the longitudinal regression techniques of fixed-effects models (chapters 2-4), FE models with individual slopes (Brüderl and Ludwig 2015) (chapter 2) and random-intercept multilevel models (Hox et al. 2017) (chapter 4), we also address the necessity of aiming for analytical designs that better facilitate causal analysis in observational studies (Winship and Morgan 1999, 2007) by introducing matching – entropy balancing (Hainmueller 2012) and nearest neighbour matching – in chapter 2. Causal analysis of the vignette dataset is facilitated by the data's design, with *D*-efficient samples (Auspurg and Hinz 2015) for each of the three occupations presented ensuring that the vignette dimensions – CV characteristics – were uncorrelated and effects found could therefore be ascribed to the variable of interest. By controlling all the inputs, we further minimise the risk of unobserved heterogeneity impacting on the results.

The use of the LIVES-JOBVUL vignette study itself is a unique contribution. While experimental studies have previously been used to study gender wage gaps more broadly, (Jasso and Webster 1997, 1999, Auspurg et al. 2017), there is only one recent study in the sociological literature of the motherhood penalty specifically with an experimental setup (Correll et al. 2007), which uses a combination of students and employers as its analytical sample. Moreover, while vignette studies have recently begun to see more use with employers and recruiters as the target sample (Di Stasio 2014, Liechti et al. 2017), the LIVES-JOBUVL study is the first to our knowledge that uses a large-scale, cross-sectional sample targeting

recruiters across an economy. The use of this dataset therefore allows the further contributions of studying how recruitment behaviours may lead to discrimination based on marital or parenthood status, through an analysis of the recruiters themselves.

Finally, concerning expected wages, we are able to provide an analysis of concurrent expected and actual wages for those who are active on the labour market – previous studies (Blau and Ferber 1991, Kiessling et al. 2019) are generally restricted to student samples, where labour market experience will not yet intervene in wage expectations. Moreover, while such studies can compare gender gaps in expected wages with wage gaps on the labour market, it cannot do so for the same individuals. That we are able to do so allows us not only to make a more meaningful comparison of the two, it also permits a test of the relationship between them.

6.3 Limitations

While the research compiled in this thesis has several strengths, there are limitations to be acknowledged. While as much as possible is done to account for unobserved heterogeneity in the panel data samples in chapters 2 and 3, including the use of tools to facilitate causal inference in chapter 2 especially, we cannot be fully certain that we account for all unobserved characteristics. While this is no different to any other study using observational data, it is nevertheless important to note the drawbacks of such analyses.

The vignette survey presents its own drawbacks in terms of its design limitations and its sample of respondents. While the results of the analysis of the vignette study fit in with the broader context provided by the panel data analysis, they are nevertheless for three specific job profiles and it cannot be guaranteed that the effects found for these three jobs would be replicated across the labour market. Moreover, in order to answer research questions related to ALMP (active

labour market programmes), vignettes were devised such that all applicants had endured a spell of unemployment, although this was due to an exogenous shock (the closure of their previous firm). It may be that respondents would react differently to a currently employed profile. The vignettes also miss information on the age of children beyond the fact they are at school – an important consideration for the motherhood wage penalty. We take account of this by using the mother's age as a proxy – the idea being that a mother in her mid-30s to mid-40s is more likely to have young school age children requiring more care and supervision than a mother in her 50s – but acknowledge that this may not be a perfect approximation.

On the respondent level, the representativeness of the sample is not ideal – respondents are concentrated in urban areas and large organisations. As well as these HR professionals being more likely to have undertaken anti-discrimination training (Fuller 2018), these areas also have better institutional support for working mothers than rural areas (Stadelmann-Steffen 2011). The result is that estimates of discrimination are likely to be conservative. There is a further lack of some useful information on the recruiters themselves, who give information on the number of children they have (if any) but not on their own marital status. In order to ensure a maximum number of useful responses, personal information collected from the recruiters was kept to a minimum, but it is clear that more would be useful for better analysis of respondent-level effects. Finally, as with the panel data analysis, the information collected on the respondents will not be free of problems linked to unobserved heterogeneity.

The analysis of the SOEP IS data also contains some limitations. With only one datapoint for expected wages, only cross-sectional analysis is possible, limiting the possibility for causal analysis. The vignettes themselves are also problematic for the analysis of wages: first, the wages themselves are embedded in the vignettes as a variation of the expected wages given,

rather than an outcome variable. Second, the vignettes to an extent represent "bad jobs" – all require a commute of at least one hour, if not a move to a new city. We therefore choose not to analyse them, which limits the scope of our study: while we still have valid information on expected and actual wages and can successfully carry out analysis of these, excluding the vignettes mean we miss information on whether or not men's and women's reservation wages vary differently based on job characteristics – notably on the flexibility to choose one's own hours and the availability of childcare, which recent research suggests affects women's likelihood to participate in the labour market (Barbieri et al. 2019, Stadelmann-Steffen 2020). It can reasonably be assumed, then, that these factors would also have an impact on the wages at which women are willing to accept a job.

6.4 Implications, future directions

This thesis has shown that motherhood wage penalties and male marriage premiums remain a feature of the labour market in Switzerland, and that their explanations are complex. How they are perceived and combatted in general society, if at all, is therefore also a multifaceted question. For researchers, the ever-present "where to go from here?" question for the analysis of parenthood, civil status and adherence to the labour market is also one with several answers and priorities.

Reducing productivity differences that stem from differing family roles would go some way to reducing penalties or premiums based on parenthood or marital status. There is evidence of this occurring naturally: male marriage premiums are reducing as cohabitation becomes more common, and women's educational achievements matching and surpassing those of men mean that gendered division of labour in the household is no longer as stark as it once was (Cohen 2002). Nevertheless, Switzerland's conservative family norms may be holding back further

progress on this front. The expectation that women's primary duty in the family is taking care of household matters likely plays a role in the high percentage of women working part-time and results in a slower accumulation of human capital. The same is true for motherhood penalties based on productivity and compensating differentials. Norms change slowly, and history suggests not without struggle, but if wage equality is seen as admirable goal – and it should indeed be seen as worth striving for both in terms of gender equality and economic benefits – then the fight is one that should be had. Sensitising society to dual-breadwinner households, working mothers, and, in Switzerland, the benefits of spending time in structured childcare and playing at school at lunchtime, would go some way to improving the lot of mothers on the labour market.

In the meantime, policy prescriptions to support working parenthood may have more immediate impacts, provided they are the right ones. Stadelmann-Steffen (2011) points out that expanding access to childcare and in/after school care increases women's employment participation, while tax breaks and cash transfers reduce it. If female participation rates in terms of work percentages were to increase, and time off to take care of children were reduced (or the burden during early childhood were more evenly spread) it stands to reason that the human capital gap would diminish. The heavy lifting, however, should not be left to mothers – encouraging fathers, perhaps through increased family allocations, to reduce their work hours may have the effect of mothers increasing theirs, resulting in more balanced household income, perhaps from two parents working four days a week. At the firm level, job sharing could be encouraged to incentivise reducing work hours without affecting firm-level productivity.

Discrimination is more difficult, though not impossible, to address. Already, a policy environment that supports women's employment, especially of mothers, should reduce

statistical discrimination by reducing aggregate productivity differences. Taste discrimination, or discrimination based on social norms, may be difficult to address directly but indirect measures may be effective: time should be clearly called on the still fairly-standard practice in Switzerland of including personal information such as marital and parental status on CVs. Legal obligations of recruiters to not request information on such matters, or attempt to find it out from other sources, should be more carefully monitored and adhered to. As with societal norms in general, though, passive, administrative-level change will not be enough. The behaviour of recruiters, as evidenced by the factorial survey analysis, remains discriminatory based on family situations. Not only is this illegal, it is probably undesirable for aggregate output. Whether this situation could be improved by better monitoring of recruitment processes either at the government or organisational level, better education of HR professionals to raise awareness of their possibly discriminatory practices, or encouraging job applicants to be firm about not answering inappropriate questions, remains an open question, but all these avenues should be more actively explored.

For labour market research, there are a number of future directions of research to be pursued and lessons to be learned. First, while this thesis has focused on wage penalties for mothers and wage premiums for married men, the inverse cases also warrant analysis: the data from the LIVES-JOBVUL survey provides the means to do so. The question of selection into work raised by the analysis of the SOEP IS dataset could be further studied, by using data that provides information on reservation wages in function of hypothetical job characteristics. This would allow a extension of the recent empirical analysis of the impact of compensating differentials – availability of part-time work or working from home in particular – on labour force participation, both in survey (Barbieri et al. 2019) and experimental (Stadelmann-Steffen 2020) analysis. Finally, the results of this thesis should encourage labour market researchers to

always consider the multidimensional nature of inequality. This does not necessarily mean that all empirical studies of the motherhood wage penalty need to include an analysis of demand-side factors, or that all analyses of the gender wage gap in general need to make use of expected wage differences. However, awareness of analytical blind spots and limitations should be more systematically considered, and narrow-focus analyses purporting to have uncovered the source of wage inequality met with a heavy dose of scepticism.

Above all, while the analysis of the LIVES-JOBVUL data contributes to a body of literature that points towards non-negligible effects of discrimination on wage differentials, it should be taken as a contribution that builds on previous findings to create a more solid picture of the impact of employers on wage outcomes. Sniderman (2018: 260) speaks of the "modesty" inherent in survey experiments – they are, by nature, only able to provide information on a particular set of circumstances at a given point in time. He therefore pleads for such analytical designs to be considered "not as one-off omnibus surveys but as a progression of experimental trials, simultaneously extending and cross-validating lines of research" (Sniderman 2018: 260). This thesis, through its analysis of one such survey experiment alongside multiple other data sources, has found in a given context clear evidence of employer-based wage discrimination. To extend and confirm these findings in a wider context, it is vital that it be followed by new analyses based on different datasets that combine experimental thinking with the scope of survey analysis.

In the context of parenthood, marriage and work, there are many factors to be considered when seeking to understand and explain why different family roles result in different labour market outcomes. In particular, employer-based discrimination is often theorised without being empirically tested – either because its effects are considered too small or too difficult to get at.

This thesis has shown both that its effects persist and that they are measurable, given the right choice of analytical design. Family life is complicated, and so is the labour market. When the two collide, simple explanations for the differing outcomes they produce for different people may be seductive but are unlikely to be accurate. As such, the final message of this thesis is to consider the complexities. By doing so, family and labour market research will be able to provide better information, propose more effective solutions, and better assist in improving labour market outcomes for as many people as possible.

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8. Appendix

8.1 Appendix for all chapters

Figure A1.1 Example vignette (Translated from French/German).

Building caretaker

Application:	The Regional Unemployment Office has sent you the application of Mr. Pedro Martinez.
Personal details:	He is 45 years old, has no children and is married.
Education:	He completed an apprenticeship as a commercial building maintenance specialist.
Professional experience:	He has, amongst other work experience, 8 years of building maintenance experience in the private sector.
Language skills:	Mr. Martinez speaks German and Spanish.
Hobby:	In his free time, he volunteers as a driver for the Red Cross.
Further information:	Alongside his job search, he has a part-time job as a sales assistant in a retail business.

Invite for an interview:	**Monthly gross salary (100%):**
☐0 ☐1 ☐2 ☐3 ☐4 ☐5 ☐6 ☐7 ☐8 ☐9 ☐10	_____ CHF

Accountant

Application:	A few days ago, you received a spontaneous application from Ms Nathalie Rochat.
Personal details:	She is 40 years old, has one school-aged child, and is divorced.
Education:	She completed professional business studies and has a diploma in accounting.
Professional experience:	She has, amongst other work experience, 8 years of accounting experience in the public sector.
Language skills:	Ms Rochat speaks French

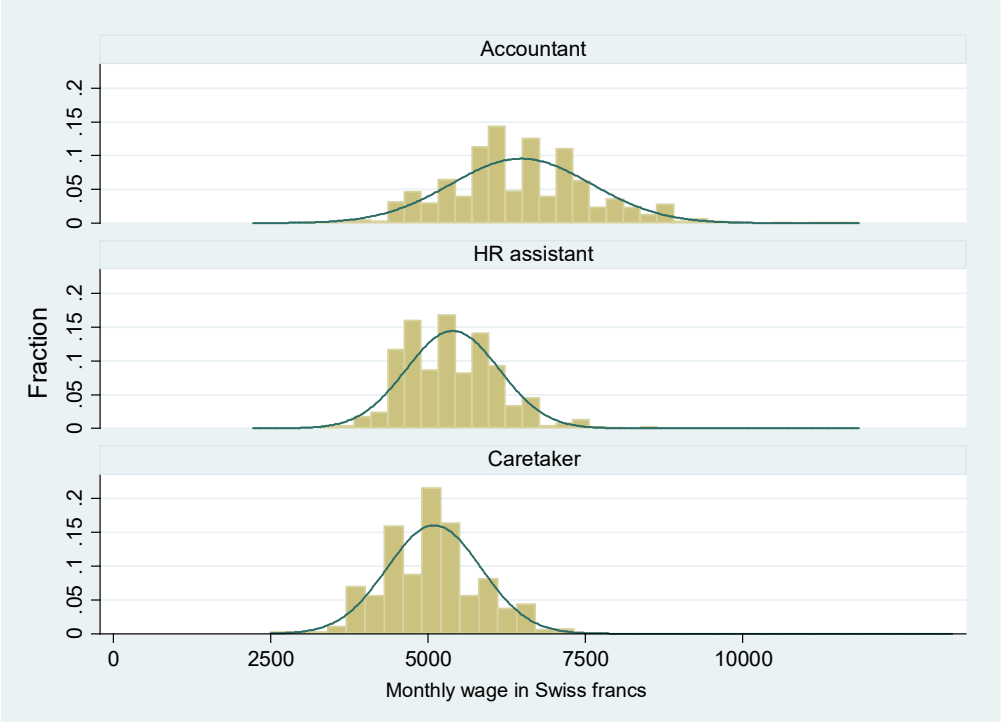
Invite for an interview:	**Monthly gross salary (100%):**
☐0 ☐1 ☐2 ☐3 ☐4 ☐5 ☐6 ☐7 ☐8 ☐9 ☐10	_____ CHF

Table A1.1 Variables included in the vignettes of the factorial survey experiment

Dimension (variable)	*Levels (values)*
Gender*	Male, female
Age	35, 40, 45, 50, 55
Children	0, 1, 2, 3
Civil Status	Unmarried, married, divorced
Type of education	Vocational, general
Type of work Experience	Private sector, public sector
Nationality*	Swiss, Spanish, Turkish, Polish
Mother tongue	German/French, German/French plus an additional language
Participation in active labour market program (ALMP)**	None, training program, occupational program (matched and unmatched), subsidy, temporary employment
Channel of Application	Advertisement, referral from current employee, unsolicited application, regional employment service
Hobby**	None, swim coaching, board member of a Swiss/foreign cultural association, volunteer for Red Cross driving service

* *These dimensions were denoted by the names of applicants*
** *"None" implies that this dimension did not appear in the vignette.*

Figure A1.2 Distribution of wages for job candidates for three different occupations



N respondents (active recruiters only): 418
N vignettes: 4137

Table A1.2 Correlations between vignette dimensions (Cramer's V)

	1	2	3	4	5	6	7	8	9	10	11
1 Gender	1										
2 Age	.018	1									
3 Children	.034	.024	1								
4 Civil status	.015	.036	.011	1							
5 Education	.003	.017	.009	.017	1						
6 Experience	.003	.014	.019	.014	0	1					
7 Nationality	.013	.021	.022	.019	.015	.013	1				
8 Language	.001	.009	.018	.003	.018	.014	.014	1			
9 ALMP	.015	.023	.017	.028	.013	.02	.02	.022	1		
10 Channel of applic.	.02	.017	.017	.01	.016	.016	.016	.016	.015	1	
11 Hobby	.015	.024	.021	.011	.015	.015	.027	.009	.029	.027	1

No correlation is statistically significant at $p<0.05$

8.2 Appendix for Chapter 2

Table A2.1 Wage premium for married men, SHP, POLS, FE and FEIS models

Dimension	*Level*	POLS Table 2 M2	FE: Unmatched Table 3 M3	FE: Matched Table 3 M4	FEIS Table 4 M2
Marital status	Married	0.094***	0.031**	0.029**	0.026
(ref. single)		(0.009)	(0.017)	(0.018)	(0.018)
Age		0.058***	0.081***	0.079***	*Ind. Slope*
		(0.007)	(0.011)	(0.012)	
Age^2		-0.001***	-0.001***	-0.001***	*Ind. Slope*
		(0.000)	(0.000)	(0.000)	
Years ed.		0.035***	*Matching*	*Matching*	*Not included*
		(0.002)			
Manager status	Supervisory	-0.041**	-0.017	0.019	-0.037*
(ref. manager)		(0.014)	(0.023)	(0.024)	(0.018)
	Production	-0.157***	-0.061***	-0.066***	-0.044*
		(0.014)	(0.023)	(0.024)	(0.019)
	Other	-0.144***	-0.046+	-0.043+	-0.049
		(0.032)	(0.036)	(0.037)	(0.035)
Work hours	20 or less	0.189***	0.400***	0.394***	0.537***
(ref. 40-43)		(0.024)	(0.059)	(0.062)	(0.084)
	21-31	0.078***	0.209***	0.208***	0.291***
		(0.017)	(0.034)	(0.036)	(0.039)
	32-39	0.016	0.088***	0.087***	0.107***
		(0.013)	(0.019)	(0.019)	(0.021)
	44-50	-0.157***	-0.118***	-0.119***	-0.141***
		(0.013)	(0.015)	(0.016)	(0.016)
	50+	-0.272***	-0.317***	-0.323***	-0.295***
		(0.030)	(0.032)	(0.034)	(0.024)
Prof. Training	Yes	0.019*	-0.004	-0.004	-0.008
(ref. no)		(0.008)	(0.008)	(0.008)	(0.007)
Job Prestige	2	0.057***	-0.048*	-0.047*	-0.029
(ref. cat. 1)		(0.015)	(0.044)	(0.045)	(0.044)
	3	0.153***	-0.012	-0.013	0.001
		(0.014)	(0.034)	(0.036)	(0.039)
	4	0.186***	0.004	0.001	-0.005
		(0.014)	(0.035)	(0.037)	(0.044)
	5	0.142***	0.028	0.022	0.103
		(0.018)	(0.060)	(0.062)	(0.075)
Intercept		2.09***			
		(0.139)			
	Individuals		707	707	431
	Observations	4450	4450	4450	3853
	R^2	.430	.36	.36	.238

Note: Regressions on (log) wages for men aged 25-50. Standard errors in parentheses, clustered standard errors reported for FE/FEIS models. ***p<0.001, **p<0.01, *p<0.05, +p<0.1. Within-R^2 values reported for FE models. Observations in the matched sample are matched on social origin (Treiman scale), nationality, decade of birth, years of education, health satisfaction.

Table A2.2 Wage recommendations and probability to invite to a job interview (on a scale from 0 to 10) for men, depending on their marital status

Dimension	*Level*	Wages	Interview
Marital status	Married	0.019*	0.242*
(ref: unmarried)		(0.011)	(0.132)
Occupation	HR assistant	-0.167***	-1.144***
(ref: Accountant)		(0.013)	(0.15)
	Concierge	-0.352***	0.267
		(0.014)	(0.163)
Education	Higher	0.019***	0.129*
(ref: lower education)		(0.005)	(0.07)
Nationality	Spanish	-0.009	-0.046
(ref: Swiss)		(0.007)	(0.092)
	Polish	-0.01	-0.159*
		(0.007)	(0.088)
	Turkish	-0.010*	-0.26***
		(0.006)	(0.09)
Experience	Private	0.007	0.046
(ref: Public sector)		(0.005)	(0.075)
Interaction terms	HR assistant*Married	-0.03**	-0.141
(ref: Acct, unmarried)		(0.014)	(0.197)
	Concierge*Married	0.003	0.147
		(0.015)	(0.207)
Children	1 child	0.005	-0.112
(ref: no child)		(0.007)	(0.108)
	2 children	-0.01	-0.121
		(0.007)	(0.109)
	3 children	0.003	-0.079
		(0.007)	(0.102)
	R^2	.7031	.1541
	Respondents	395	491
	Observations	2083	2665

Note: Respondent fixed-effects regressions on (log) wages and likelihood to invite to a job interview for men. Clustered standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table A2.3 Wage premium for married men, SHP (nearest-neighbor matching with replacement replication)

Dimension	*Level*	POLS (1)	POLS (2)	FE: Unmatched (3)	FE: Matched (4)	FE: Unmatched (5)	FE: Matched (6)
Marital status	Married	0.094***	0.081***	0.024+	0.023+	0.019+	0.024*
(ref. single)		(0.011)	(0.010)	(0.017)	(0.017)	(0.015)	(0.016)
Age		0.070***	0.065***	0.084***	0.067***	0.093***	0.087***
		(0.010)	(0.009)	(0.014)	(0.014)	(0.013)	(0.013)
Age^2		-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years ed.		0.051***	0.040***	*Matching*	*Matching*	*Matching*	*Matching*
		(0.002)	(0.002)				
Controls		No	Yes	No	No	Yes	Yes
Intercept		1.422***	1.684***				
		(0.178)	(0.166)				
	Individuals	573	573	573	573	573	573
	Observations	3030	3030	3030	3030	3030	3030
	R^2	.333	.441	.27	.27	.407	.403

Note: Regressions on (log) wages for men aged 25-50. Standard errors in parentheses, clustered standard errors reported for FE/FEIS models. ***p<0.001, **p<0.01, *p<0.05, +p<0.1. Within-R^2 reported for FE models. Observations in the matched sample are matched on social origin (Treiman scale), nationality, decade of birth, years of education, health satisfaction. Control variables include managerial status, job prestige (Treiman scale, collapsed to 5 categories with 1= least and 5= most prestigious), hours of work and in-work training in the last year.

Table A2.4 Probability to invite to a job interview (on a scale from 0 to 10) for men depending on their marital status, ordered logit model

Dimension	*Level*	Model 1	Model 2	Model 3	Model 4
Marital status	Married	0.217*	0.237**	0.250*	0.242*
(ref: unmarried)		(0.091)	(0.083)	(0.131)	(0.132)
Occupation	HR assistant		-1.088***	-1.137***	-1.144***
(ref: Accountant)			(0.11)	(0.15)	(0.15)
	Concierge		0.411***	0.265*	0.267
			(0.103)	(0.164)	(0.163)
Interaction terms	HR assistant*Married			-0.172	-0.141
(ref: Acct, unmarried)				(0.196)	(0.197)
	Concierge*Married			0.136	0.147
				(0.206)	(0.207)
Controls		No	No	No	Yes
	R^2	.003	.145	.148	.1541
	Respondents		491		
	Observations		2665		

Note: Ordered logit regressions on likelihood to invite to a job interview for men. Standard errors in parentheses. Additional controls in M4 include: children, nationality, education, experience.
*p<0.1; **p<0.05; ***p<0.01

Table A2.5 Wage premium for married men, SHP (restricted to FEIS sample)

Dimension	*Level*	FE: Unmatched (1)	FE: Matched (2)	FE: Unmatched (3)	FE: Matched (4)
Marital status	Married	0.034**	0.035**	0.029*	0.029*
(ref. single)		(0.019)	(0.02)	(0.017)	(0.018)
Age		0.074***	0.072***	0.078***	0.077***
		(0.013)	(0.013)	(0.012)	(0.012)
Age^2		-0.001***	-0.001***	-0.001***	-0.001***
		(0.001)	(0.000)	(0.000)	(0.000)
Controls		No	No	Yes	Yes
	Individuals		431		
	Observations		3853		
	Within R^2	.224	.224	.371	.37

Note: Individual fixed-effects regressions on (log) wages for men aged 25-50. Clustered standard errors in parentheses.

***p<0.001, **p<0.01

Observations in the matched sample are matched on social origin (Treiman scale), nationality, decade of birth, years of education, health satisfaction. Control variables include parenthood status, managerial status, job prestige (Treiman scale), hours of work and in-work training.

Table A2.6 Cell sizes for SHP sample

Dimension	*Level*	
Marital status	Married	1266
	Unmarried	3184
Manager status	Manager	500
	Supervisory	1537
	Production	2190
	Other	78
Work hours	20 or less	126
	21-31	245
	32-39	449
	40-43	2903
	44-50	493
	50+	234
Prof. Training	Yes	1543
	No	2907
Job Prestige	Cat 1	475
	Cat 2	747
	Cat 3	1199
	Cat 4	1536
	Cat 5	482
N. Observations		4450
N. Respondents		707

Table A2.7 Cell sizes for factorial survey

Dimension	*Level*	Wage evals	Ratings
Marital status	Married	700	886
	Single	705	905
Occupation	Accountant	666	863
	HR assistant	725	922
	Concierge	692	880
Education	Lower	1048	1322
	Higher	1035	1343
Nationality	Swiss	545	674
	Spanish	502	667
	Polish	516	654
	Turkish	520	670
Experience	Public sector	1044	1341
	Private	1039	1324
Interactions	Acct, married	225	293
	Acct, single	231	298
	HR assistant*Married	244	299
	HR Assistant, single	235	306
	Concierge*Married	231	294
	Concierge, single	239	301
Children	No children	490	633
	1 child	533	691
	2 children	528	661
	3 children	532	680
N obs		2083	2665
No. Respondents		395	491

8.3 Appendix for Chapter 3

Table A3.1 Fixed-effects linear regression on (log) hourly wages of women

	Swiss Household Panel 1991-2015			Swiss Labour Force Survey 1991-2009		
	Empty model	With part-time dummy	Full model	Empty model	With part-time dummy	Full model
1 child	-0.004	-0.085***	-0.060***	-0.009	-0.041***	-0.036***
	(0.018)	(0.020)	(0.019)	(0.007)	(0.007)	(0.007)
2 children	-0.043*	-0.136***	-0.091***	-0.017*	-0.048***	-0.043***
	(0.025)	(0.026)	(0.026)	(0.010)	(0.010)	(0.009)
3 children	-0.055	-0.146***	-0.088**	-0.006	-0.030	-0.028
	(0.042)	(0.043)	(0.043)	(0.020)	(0.020)	(0.019)
Controls	no	no	yes	no	no	yes
N observations	12,769	12,769	12,769	71,531	71,531	71,531
N persons	3,115	3,115	3,115	26,409	26,409	26,409
R2	0.101	0.128	0.166	0.024	0.053	0.065

Notes: coefficients are for the female labour force. All models include age (in years) and calendar years. Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A3.2 Fixed-effect linear regression on (log) hourly wages of women

	SHP 1999-2015	*SLFS 1991-2009*
1 child	-0.060***	-0.036***
	(0.019)	(0.007)
2 children	-0.091***	-0.043***
	(0.026)	(0.009)
3 children	-0.088**	-0.028
	(0.043)	(0.019)
Part-time	0.180***	0.136***
	(0.013)	(0.005)
Years (SHP) / level (SLFS) of Education	0.022***	0.011***
	(0.005)	(0.003)
% female in occupation	0.001	-0.012
	(0.038)	(0.019)
Public sector	0.040***	0.008
	(0.011)	(0.032)
Firm size	0.010***	0.003***
	(0.002)	(0.001)
Supervision	0.024***	0.017***
	(0.007)	(0.003)
Occupational prestige	0.004***	
	(0.001)	
Work experience (SHP) / Tenure (SLFS)	0.005	0.006***
	(0.005)	(0.002)
Job change last year	-0.038	
	(0.029)	
Employer change last year	0.015	
	(0.010)	
Fixed-term job	-0.121***	-0.047***
	(0.022)	(0.008)
Inadequate qualification	-0.061***	
	(0.018)	
Overqualified	-0.041***	
	(0.009)	
Underqualified	-0.023	
	(0.023)	
Training1	0.016**	0.000
	(0.007)	(0.002)
Training2	0.021***	
	(0.008)	
Married	-0.046***	-0.013**
	(0.014)	(0.005)
Hours of housework	-0.002***	
	(0.001)	
External childcare	0.010	
	(0.010)	
Constant	2.348***	3.041***
	(0.116)	(0.150)
N observations	12,769	71,531
N persons	3,115	26,409
R2	0.166	0.065

Clustered standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: coefficients are for the female labour force, aged 20-50. All models include age in years and calendar years and, for the SLFS, a dummy variable for 13 different economic sectors and 9 ISCO 1-digit groups (not shown).

Table A3.3 Wage recommendations for women depending on the number of children they have (factorial survey experiment)

Dimension	*Level*	
Children	1 child	-0.026**
(ref: no child)		(0.012)
	2 children	-0.030***
		(0.011)
	3 children	-0.033**
		(0.014)
Occcupation	Accountant	0.159***
(ref: HR assistant)		(0.014)
	Caretaker	-0.204***
		(0.015)
Children*Occupation	Accountant*1child	0.018
(ref: no child, HR assistant)		(0.017)
	Accountant*2child	0.010
		(0.017)
	Accountant*3child	0.027
		(0.020)
	Caretaker*1child	0.041**
		(0.018)
	Caretaker*2child	0.038**
		(0.017)
	Caretaker*3child	0.026
		(0.019)
Nationality	Spanish	-0.019
(ref: Swiss)		(0.014)
	Polish	-0.034***
		(0.011)
	Turkish	-0.014
		(0.012)
Type of education	General	0.006
(ref : vocational)		(0.009)
Civil status	Divorced	0.012
(ref : married)		(0.010)
	Single	0.007
		(0.010)
Type of work experience	Private sector	-0.006
(ref : public sector)		(0.008)
	Constant	8.727***
		(0.014)
	N vignettes	1,644
	N respondents	385
	R2	0.712

Respondent fixed-effects regressions on (log) wages for women aged 35 to 50. Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A3.4 Wage recommendations for women by respondent gender (factorial survey experiment)

		Female respondents	Male respondents
Children (ref: no child)	1 child	-0.038**	-0.023
		(0.018)	(0.016)
	2 children	-0.038**	-0.027*
		(0.016)	(0.015)
	3 children	-0.044**	-0.031*
		(0.021)	(0.023)
Occcupation (ref: HR assistant)	Accountant	0.155***	0.157***
		(0.021)	(0.019)
	Caretaker	-0.214***	-0.196***
		(0.021)	(0.023)
Children*Occupation (ref: no child, HR assistant)	Accountant*1child	0.047*	0.001
		(0.026)	(0.022)
	Accountant*2child	0.008	0.025
		(0.024)	(0.026)
	Accountant*3child	0.023	0.044
		(0.029)	(0.027)
	Caretaker*1child	0.065**	0.021
		(0.026)	(0.025)
	Caretaker*2child	0.043**	0.036
		(0.022)	(0.027)
	Caretaker*3child	0.020	0.041
		(0.026)	(0.030)
Additional controls		yes	Yes
	Constant	8.723***	8.742***
		(0.020)	(0.016)
	N vignettes	918	609
	N respondents	203	129
	R2	0.702	0.750

Respondent fixed-effects regressions on (log) wages for women aged 35 to 50. Clustered standard errors in parentheses. Additional controls include: civil status, nationality, type of education, type of work experience.
*** p<0.01, ** p<0.05, * p<0.1

8.4 Appendix for Chapter 4

Table A4.1 Fixed-effects regressions of log monthly wages on number of children, full sample and split samples by respondent gender and parenthood status, including interaction terms

Variables	Levels	full sample log wage	fem with child log wage	fem w/o child log wage	male with child log wage	male w/o child log wage
Children	1 child	-0.025^{+}	-0.02	-0.047^{+}	-0.029	-0.009
(ref. no children)		(0.012)	(0.02)	(0.028)	(0.022)	(0.019)
	2 child	-.031*	-0.027	-0.046^{+}	-0.048*	0.01
		(0.011)	(0.019)	(0.025)	(0.019)	(0.026)
	3 children	-0.033**	-0.014	-0.06*	-0.056^{+}	-0.015
		(0.014)	(0.021)	(0.033)	(0.024)	(0.027)
Occupation	Accountant	0.159***	0.199***	0.135***	0.156***	0.013***
(ref. HR assistant)		(0.014)	(0.029)	(0.03)	(0.023)	(0.029)
	Concierge	-0.203***	-0.215***	-0.211***	-0.222***	-0.155***
		(0.015)	(0.03)	(0.31)	(0.027)	(0.037)
Children*occupation	1 child*accountant	0.018	0.002	0.067^{+}	-0.014	0.056
(ref. HR assistant,		(0.017)	(0.037)	(0.036)	(0.031)	(0.03)
no children)	2 children*accountant	0.01	-0.044	0.033	-0.056	0.013
		(0.017)	(0.03)	(0.035)	(0.033)	(0.033)
	3 children*accountant	0.027	-0.049	0.063^{+}	0.061	0.068
		(0.02)	(0.038)	(0.042)	(0.033)	(0.043)
	1 child*concierge	0.04*	0.045	0.082*	0.039	-0.009
		(0.018)	(0.039)	(0.036)	(0.03)	(0.42)
	2 children*concierge	0.037*	0.066	0.027	0.056	0.006
		(0.017)	(0.029)	(0.033)	(0.031)	(0.046)
	3 children*concierge	0.026	0.021	0.015	0.084*	-0.011
		(0.019)	(0.036)	(0.038)	(0.034)	(0.051)
R^2		0.71	0.73	0.68	0.75	0.81
No. vignettes		1644	361	551	428	172
No. respondents		385	80	121	91	36

Robust standard errors in parentheses.
*** p<0.001, ** p<0.01, * p<0.05, $^{+}$ p<0.1

Table A4.2 Multilevel analysis of motherhood wage penalties, respondents grouped by gender and parenthood status combined, including vignette-level interaction terms

Variables	Levels	M1	M2	M3	M4	M5
Vignette-level fixed effects:						
Children	1 child	-0.0003	-0.004	-0.021	-0.024^{+}	-0.02
(ref. no child)		(0.013)	(0.008)	(0.013)	(0.014)	(0.017)
	2 children	-0.03^{+}	-0.016*	-0.03*	-0.034*	-0.045**
		(0.013)	(0.007)	(0.013)	(0.013)	(0.017)
	3 children	-0.026^{+}	-0.013^{+}	-0.03*	-0.037*	-0.053**
		(0.014)	(0.008)	(0.014)	(0.015)	(0.018)
Occupation	Accountant		0.174***	0.158***	0.159***	0.158***
(ref. HR Assistant)			(0.006)	(0.013)	(0.014)	(0.014)
	Concierge		-0.178***	-0.197***	-0.2***	-0.2***
			(0.006)	(0.013)	(0.014)	(0.014)
Vignette-level interactions	Accountant, 1 child			0.019	0.023	0.025
Children*Occupation				(0.018)	(0.019)	(0.019)
(ref. HR assistant, no children)	Accountant, 2 children			0.014	0.02	0.021
				(0.018)	(0.019)	(0.019)
	Accountant, 3 children			0.033^{+}	0.039^{+}	0.038^{+}
				(0.019)	(0.02)	(0.02)
	Concierge, 1 child			0.034^{+}	0.037^{+}	0.039*
				(0.019)	(0.02)	(0.02)
	Concierge, 2 children			0.03	0.032^{+}	0.033^{+}
				(0.018)	(0.019)	(0.019)
	Concierge, 3 children			0.017	0.023	0.023
				(0.019)	(0.02)	(0.021)
Respondent-level fixed effects						
Gender/children	Female, with children				0.004	-0.001
(ref. female, no children)					(0.018)	(0.022)
	Male, no children				0.049*	0.026
					(0.018)	(0.03)
	Male, with children				0.017	0.011
					(0.018)	(0.021)
Cross-level 2-way interactions						
Children*respondent gender/children	1 child, female with children					-0.0003
(ref. No child*female, no children)						(0.021)
	2 children, female with children					0.004
						(0.02)
	3 children, female with children					0.016
						(0.021)
	1 child, male no children					0.006
						(0.026)
	2 children, male no children					0.043
						(0.026)
	3 children, male no children					0.039
						(0.027)
	1 child, male with children					-0.22
						(0.02)
	2 children, male with children					0.018
						(0.019)
	3 children, male with children					0.026
						(0.02)
Intercept		8.71***	8.71***	8.72***	8.71***	8.71***
		(0.012)	(0.009)	(0.011)	(0.015)	(0.016)
Marginal R-squared		0.003	0.469	0.469	0.482	0.483
Pseudo R-squared		0.299	0.788	0.789	0.791	0.791
No. vignettes		1512	1512	1512	1512	1512
No. respondents		328	328	328	328	328

Standard errors in parentheses.
*** p<0.001, ** p<0.01, * p<0.05, $^{+}$ p<0.1

Table A4.3 Multilevel analysis of motherhood wage penalties, respondents grouped by gender and parenthood status separately, including vignette-level interaction terms

Variables	Levels	M1	M2	M3	M4
Vignette-level fixed effects:					
Children	1 child	-0.025^{+}	-0.02	-0.024^{+}	-0.018
(ref. no child)		(0.014)	(0.015)	(0.014)	(0.016)
	2 children	-0.033*	-0.041**	-0.034*	-0.034*
		(0.013)	(0.015)	(0.013)	(0.016)
	3 children	-0.035*	-0.044**	-0.037*	-0.043**
		(0.014)	(0.016)	(0.014)	(0.017)
Occupation	Accountant	0.157***	0.157***	0.159***	0.159***
(ref. HR Assistant)		(0.014)	(0.013)	(0.014)	(0.014)
	Concierge	-0.199***	-0.019***	-0.2***	-0.2***
		(0.014)	(0.014)	(0.014)	(0.014)
Vignette-level interactions:					
Children*Occupation	Accountant, 1 child	0.024	0.026	0.023	0.023
(ref. HR assistant, no children)		(0.019)	(0.019)	(0.019)	(0.019)
	Accountant, 2 children	0.018	0.019	0.02	0.02
		(0.019)	(0.019)	(0.019)	(0.019)
	Accountant, 3 children	0.035^{+}	0.034^{+}	0.039^{+}	0.038^{+}
		(0.02)	(0.02)	(0.02)	(0.02)
	Concierge, 1 child	0.035^{+}	0.037^{+}	0.037^{+}	0.038^{+}
		(0.02)	(0.02)	(0.02)	(0.02)
	Concierge, 2 children	0.03	0.031	0.032^{+}	0.032^{+}
		(0.019)	(0.019)	(0.018)	(0.019)
	Concierge, 3 children	0.02	0.019	0.024	0.023
		(0.02)	(0.02)	(0.02)	(0.02)
Respondent-level fixed effects:					
Respondent gender	Male	0.027^{+}	0.02		
(ref. female)		(0.015)	(0.018)		
Respondent children	Parent			-0.0004	-0.001
(ref. no respondent children)				(0.014)	(0.017)
Cross-level 2-way interactions:					
Children*respondent gender	1 child, male		-0.015		
(ref. No child*female)			(0.016)		
	2 children, male		0.019		
			(0.016)		
	3 children, male		0.022		
			(0.016)		
Children*respondent children	1 child, parent				-0.012
(ref. No child*no respondent children)					(0.016)
	2 children, parent				0.002
					(0.015)
	3 children, parent				0.013
					(0.016)
Intercept		8.71***	8.71***	8.72***	8.72***
		(0.013)	(0.014)	(0.014)	(0.015)
Marginal R^2		0.475	0.476	0.478	0.477
Pseudo R^2		0.788	0.789	0.79	0.79
No. vignettes		1512	1512	1512	1512
No. respondents		328	328	328	328

Standard errors in parentheses.
*** p<0.001, ** p<0.01, * p<0.05, $^{+}$ p<0.1

Table A4.4 Multilevel analysis of motherhood wage penalties, with vignettes aged 55.

Variables	Levels	M1	M2	M3	M4	M5
Vignette-level fixed effects:						
Children	1 child	-0.005	-0.002	-0.019^{+}	-0.023^{+}	-0.018
(ref. no child)		(0.010)	(0.008)	(0.012)	(0.012)	(0.015)
	2 children	-0.012	-0.013*	-0.03**	-0.036**	-0.042**
		(0.012)	(0.007)	(0.011)	(0.012)	(0.015)
	3 children	-0.018	-0.011	-0.02^{+}	-0.027*	-0.040*
		(0.012)	(0.007)	(0.012)	(0.013)	(0.016)
Occupation	Accountant		0.170***	0.154***	0.154***	0.154***
(ref. HR Assistant)			(0.006)	(0.011)	(0.012)	(0.012)
	Concierge		-0.182***	-0.201***	-0.203***	-0.203***
			(0.006)	(0.011)	(0.012)	(0.012)
Vignette-level interactions	Accountant, 1 child			0.016	0.022	0.023
Children*Occupation				(0.016)	(0.017)	(0.017)
(ref. HR assistant, no children)	Accountant, 2 children			0.026	0.033*	0.034*
				(0.016)	(0.017)	(0.017)
	Accountant, 3 children			0.020	0.025	0.023
				(0.017)	(0.018)	(0.018)
	Concierge, 1 child			0.034*	0.037*	0.037*
				(0.016)	(0.017)	(0.017)
	Concierge, 2 children			0.032*	0.034*	0.033*
				(0.016)	(0.017)	(0.017)
	Concierge, 3 children			0.013	0.015	0.015
				(0.017)	(0.017)	(0.017)
Respondent-level fixed effects						
Gender/children	Female, with children				0.004	-0.007
(ref. female, no children)					(0.018)	(0.021)
	Male, no children				0.048*	0.036
					(0.024)	(0.028)
	Male, with children				0.012	0.008
					(0.018)	(0.021)
Cross-level 2-way interactions						
Children*respondent gender/children	1 child, female with children					0.003
(ref. No child*female, no children)						(0.018)
	2 children, female with children					-0.0003
						(0.018)
	3 children, female with children					0.009
						(0.019)
	1 child, male no children					-0.004
						(0.023)
	2 children, male no children					0.028
						(0.024)
	3 children, male no children					0.028
						(0.023)
	1 child, male with children					-0.18
						(0.018)
	2 children, male with children					0.012
						(0.017)
	3 children, male with children					0.027
						(0.018)
Intercept		8.71***	8.71***	8.72***	8.71***	8.72***
		(0.010)	(0.008)	(0.010)	(0.014)	(0.015)
Observations		2054	2054	2054	2054	2054
Marginal R-squared		0.002	0.466	0.468	0.480	0.480
Pseudo R-squared		0.298	0.788	0.788	0.790	0.791
No. Respondents		389	389	389	389	389

Standard errors in parentheses
*** p<0.001, ** p<0.01, * p<0.05, $^{+}$ p<0.1

8.5 Appendix for Chapter 5

Table A5.1 OLS regressions of log expected hourly wage, full table

	OLS 1 Female x Work Interaction	OLS 2 Female x Child Interaction	OLS 3 Female x Work + Family Interaction
Female	-0.229***	-0.286***	-0.224***
	(0.029)	(0.026)	(0.031)
Part-time (ref.: full-time)	-0.361***		-0.361***
	(0.063)		(0.063)
Non-working	-0.324***		-0.322***
	(0.052)		(0.052)
Child under 14 in HH		0.053	0.037
		(0.035)	(0.034)
Female x Part-time	0.174*		0.166*
	(0.071)		(0.071)
Female x Non-working	0.084		0.068
	(0.065)		(0.066)
Female x Child under 14 in HH		-0.098*	0.002
		(0.043)	(0.043)
Partner, not living together (ref.: no partner)	0.042	0.063	0.043
	(0.039)	(0.040)	(0.039)
Partner, living together	0.004	0.014	0.001
	(0.037)	(0.038)	(0.037)
Married	0.096***	0.092**	0.085**
	(0.028)	(0.030)	(0.029)
Education (years)	0.082***	0.087***	0.082***
	(0.004)	(0.004)	(0.004)
Age 31-40 (years) (ref.: 17-30 years)	0.120***	0.102**	0.109*
	(0.034)	(0.036)	(0.035)
Age 41-50 (years)	0.242***	0.221***	0.238***
	(0.032)	(0.036)	(0.032)
Age 51-60 (years)	0.236***	0.211***	0.248***
	(0.033)	(0.033)	(0.034)
Migration background	-0.054*	-0.052*	-0.057*
	(0.023)	(0.024)	(0.0233)
Constant	1.767	1.663	1.764
Adjusted R-square	0.486	0.440	0.486
N person-years	1210	1210	1210

*$p < .05$, **$p < .01$, ***$p < .001$

Table A5.2 OLS regressions of expected wages, actual wages, and difference of expected and actual wages

	OLS 1 Expected wage	OLS 2 Actual wage	OLS 3 Diff. expected and actual wages (ln(exp)-ln(act))	OLS 4 OLS 1 + occupation controls	OLS 5 OLS 2 + occupation controls	OLS 6 OLS 3 + occupation controls
Female	-0.189***	-0.183***	-0.007	-0.178***	-0.158***	-0.021
	(0.026)	(0.033)	(0.027)	(0.026)	(0.033)	(0.029)
Part-time (ref.: full-time)	-0.235***	-0.272***	-0.037	-0.206***	-0.228***	0.022
	(0.028)	(0.037)	(0.030)	(0.027)	(0.036)	(0.031)
Non-working	-0.214***	-0.494***	-0.280***	-0.160***	-0.425***	0.264***
	(0.052)	(0.067)	(0.055)	(0.049)	(0.065)	(0.055)
Child under 14 in HH	0.033	-0.020	0.054	0.031	-0.021	0.053
	(0.027)	(0.036)	(0.029)	(0.026)	(0.034)	(0.029)
Partner, not living together	0.041	0.005	0.057	0.058	0.002	0.056
(ref.: no partner)	(0.039)	(0.054)	(0.044)	(0.039)	(0.052)	(0.044)
Partner, living together	-0.005	-0.028	-0.023	0.004	-0.016	-0.020
	(0.040)	(0.051)	(0.042)	(0.0438)	(0.049)	(0.042)
Married	0.102**	0.084*	0.018	0.118**	0.101*	0.018
	(0.032)	(0.041)	(0.034)	(0.030)	(0.040)	(0.034)
Education (years)	0.081***	0.071***	0.010	0.051***	0.034***	0.017**
	(0.004)	(0.005)	(0.004)	(0.005)	(0.007)	(0.006)
Age 31-40 (years) (ref.: 17-30	0.128**	0.201***	-0.073	0.127**	0.200***	-0.072
years)	(0.038)	(0.049)	(0.040)	(0.036)	(0.047)	(0.040)
Age 41-50 (years)	0.263***	0.346***	-0.083*	0.237***	0.316***	-0.079*
	(0.035)	(0.046)	(0.037)	(0.033)	(0.044)	(0.037)
Age 51-60 (years)	0.267***	0.297***	-0.031	0.248***	0.270***	-0.022
	(0.036)	(0.047)	(0.039)	(0.035)	(0.046)	(0.039)
Migration background	-0.054*	-0.051	-0.003	-0.048*	-0.0548	-0.000
	(0.025)	(0.033)	(0.027)	(0.024)	(0.032)	(0.027)
Managers (ISCO group 1)				0.041	-0.014	0.055
(ref.: Professionals (ISCO group 2))				(0.047)	(0.061)	(0.053)
Technicians and associate				-0.153***	-0.179***	0.026
professionals (ISCO group 3)				(0.034)	(0.045)	(.039)
Clerks (ISCO group 4)				-0.243***	-0.332***	0.089
				(0.043)	(0.057)	(0.048)
Service and sales workers				-0.332***	-0.473***	0.141**
(ISCO group 5)				(0.043)	(0.056)	(0.048)
Skilled agricultural, forestry				-0.443***	-0.486***	0.042
and fishery workers (ISCO group 6)				(0.087)	(0.113)	(0.097)
Craft and related trades				-0.231***	-0.265***	0.034
workers (ISCO group 7)				(0.043)	(0.056)	(0.048)
Plant and machine operators				-0.308***	-0.334***	0.026
(ISCO group 8)				(0.050)	(0.064)	(0.055)
Elementary occupations				-0.426***	-0.505***	0.079
(ISCO group 9)				(0.051)	(0.067)	(0.057)
Constant	1.703	1.629	0.075	2.256	2.307	-0.051
Adjusted R-square	0.456	0.343	0.030	0.512	0.401	0.034
N person-years	1,073	1,073	1,073	1,073	1,073	1,073

Note. Difference of expected and actual wages at the individual level (ln(exp)-ln(act)). The sample is the last actual wage sample that is a subsample of the expected wage sample. Actual wage for non-working/unemployed persons is the last actual wage in the years before 2013. Dependent variable is the hourly wage in 2010 prices. N=1,073.

*p < .05, **p < .01, ***p < .001

Table A5.3 Gender gaps in expected wages within work statuses

Work status	Effect of female in log points within work status	Effect of male in log points compared to men's FT	Effect of female in log points compared to female's FT
Fulltime (FT)	-0.220***	-	-
	(.029)		
Part-time/irregular (PT)	-0.080	-0.335***	-0.194***
	(0.063)	(0.062)	(0.032)
Non-working/maternal	-0.263**	-0.214*	-0.257***
	(0.090)	(0.084)	(0.044)
Unemployed	-0.080	-0.378***	-0.238***
	(0.082)	(0.065)	(0.060)

Note. Data are estimated from model 3, Table 5.2, but without the interactions female x age and female x family. Within work status estimated with the interaction of gender and work status. N=1,210.
*$p < .05$, **$p < .01$, ***$p < .001$