The Male Marriage Premium: Selection, Productivity, or Employer Preferences?

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ABSTRACT

Objective: This paper empirically tests the three theories put forward to explain the male marriage premium.

Background: Married men continue to earn more than single across the Western world, despite significant changes to family life. Three theories are put forward to explain this phenomenon: 1) Marriage makes men more productive; 2) more productive men select into marriage; 3) employers prefer married men and therefore offer them higher wages.

Method: We use a multi-pronged strategy to test all three theories. First, we analyze a Swiss national panel survey using fixed-effects regressions with observations matched using entropy balancing, as well as fixed effects with individual slopes (FEIS) models to isolate the selection effect. Second, we use a factorial survey experiment of over 500 recruiters in Switzerland to study the preferences of employers.

Results: Pooled OLS regressions show a marriage premium of 11%, which is reduced to approximately 3.5% when accounting for selection and 2.5-3% when introducing controls related to in-work productivity. The results of the survey experiment show that employers assign wages 2% higher to married men, with large differences between occupations. They are also more likely to invite married men to a job interview.

Conclusion: While selection is found to be the largest contributor to the male marriage premium, it does not explain it all – both employer preferences and productivity changes also play a role.

Keywords: Male marriage premium, labor market, productivity, selection, employer preferences, survey experiment data.

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 labor 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 slopes to differentiate selection effects from productivity and, possibly, employer preferences. To further analyze 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 analyze 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 analyze 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 labor 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 randomize 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 analyze more detailed information about the hiring process, albeit information gleaned from a hypothetical setting. We can therefore also observe an important but little-analyzed 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.

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.

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 "specialize" in paid work, because they have wives at home who specialize 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 behavioral 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 unfavorable 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 behaviors (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) analyze 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.

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 favor married men, while Killewald and Lundberg (2017: 1025), analyzing 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) analyze 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 analyzing "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 favor 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 analyzed 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.*

Employer Preferences

A third possibility for a marriage premium is the preference of employers for married men over other candidates. Sociology has long been skeptical 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 favoritism 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 emphasize 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 behavior 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, Carlsson 2011, Bielby and Baron 1986). 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.

INSTITUTIONAL CONTEXT, DATA AND METHOD

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.

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 maximize 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 2016) 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

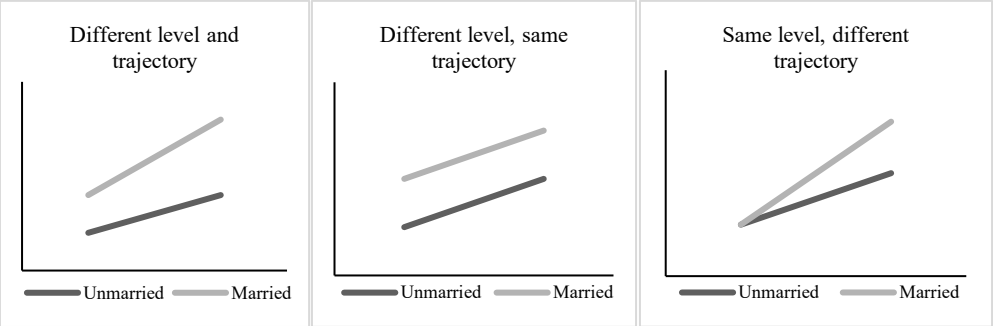
We first report the overall wage premium for married men using a pooled OLS regression, to define a baseline premium without any productivity and selection effects. To test the productivity

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-labor 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-neighbor matching with replacement, the results of which can be found in the online supplement (Table S.1), 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 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 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 supplementary material (Table S.3). 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 utilize 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 analyzing 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 2014). 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 labor 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 centers (OFS 2019i, ii). With this in mind it is possible that our sample underestimates the effects of employer bias in the Swiss labor market overall. Larger organizations with professionalized 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 randomized. 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 as well as an example vignette can be found in the appendix – figure A.1 and table A.1

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 skeptical 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:

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

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 supplementary material (Table S.2), which generally confirm the trends shown in the FE models.

Descriptive Statistics, SHP

Table 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.

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 favor 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.

Table 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

Multivariate Analysis

Tables 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, 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.

Table 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	.305	.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 A.3.
***p<0.01

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 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%.

Model 2 in table 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-labor market socio-economic characteristics. Any remaining premium should therefore be the direct result of the fact of being married, net of any pre-labor 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-labor 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 3. *Wage Premium for Married Men, SHP, Fixed-Effects Regression Models*

		FE: Unmatched	FE: Matched	FE: Unmatched	FE: Matched
Dimension	*Level*	(1)	(2)	(3)	(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	.219	.212	.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 A.3.

In the next step, shown in table 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 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-labor market productivity characteristics used in the matching procedure are indeed linked to later productivity and a faster-growing wage.

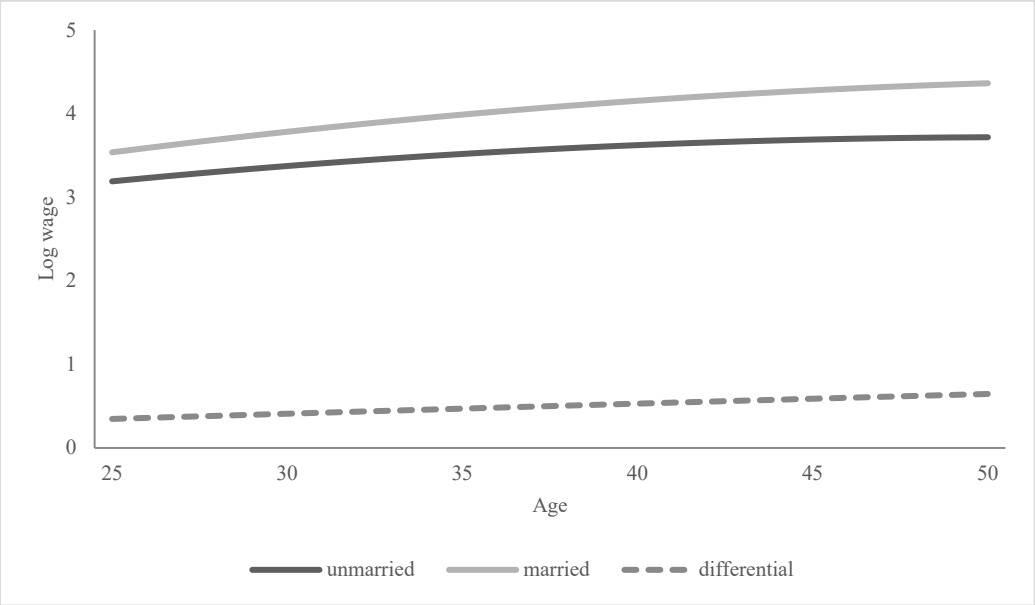
Table 4. *Wage Premium for Married Men, SHP, Fixed-Effects Individual Slope Models*

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 A.3.
*p<0.05

To visualize the differing wage curves of married and unmarried men, in Figure 2 we plot the predicted values of Table 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 1, and justifies the inclusion of models that can account for both.

FIGURE 2. WAGE CURVES FOR MARRIED AND UNMARRIED MEN BY AGE (LOG WAGES)



Predicted values derived from FEIS model reported in Table 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 favoritism – in further detail the next section.

EXPERIMENTAL EVIDENCE FOR THE MALE MARRIAGE PREMIUM

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 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.

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 A.4).

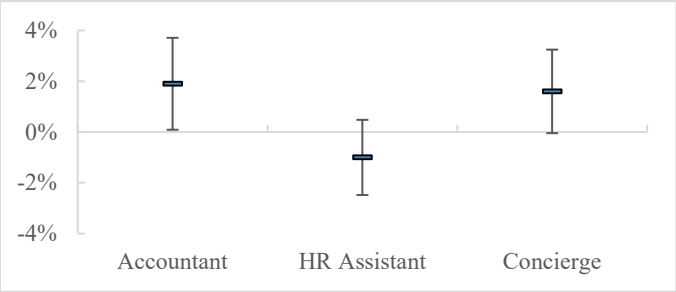
The coefficients reported in Models 2-4 refer to the reference category of accountants. The interactions show us that there is indeed a difference between occupations in how civil status affects wages. Figure 3 illustrates these differences: a married HR assistant in fact receives a 1% wage penalty, while a married concierge receives a premium of 1.5%, though in these cases the 90% confidence interval crosses 0.

Table 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	0.009*	0.021**	0.019*
		(0.011)	(0.006)	(0.011)	(0.012)
Occupation (ref: Accountant)	HR assistant		-0.18***	-0.167***	-0.167***
			(0.01)	(0.013)	(0.013)
	Concierge		-0.348***	-0.352***	-0.352***
			(0.008)	(0.014)	(0.014)
Interaction terms (ref: Acct, unmarried)	HR assistant*Married			-0.031**	-0.030**
				(0.014)	(0.014)
	Concierge*Married			-0.004	-0.003
				(0.015)	(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 A.4. Wald test for joint significance of interactions (M3): Chi-squared(4, 1680) = 8.667, $p<0.1$.
*$p<0.1$; **$p<0.05$; ***$p<0.01$

FIGURE 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 A.4) for regression tables including the full models. 90% confidence intervals shown.

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 favor of married men. Table 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 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	.003	.145	.148	.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 A.4. 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 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 favoring married men, at least at the higher and lower ends of the job prestige scale.

FIGURE 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 A.4 for regression tables including the full models. 90% confidence intervals shown.

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. The data also confirms that the marriage premium for men exists whether or not children are controlled for in the analysis.

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. This suggests that employer preferences play a small role in the male marriage premium – although it does conform with the lower-end coefficients from our panel data analysis. 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 half 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 behavior – though research by Hainmueller et al. (2015) suggests that responses given in an experimental context are in fact quite close to real-world behavior. 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, 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. 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 analyze employers and HR staff directly is important when wishing to understand questions of discrimination, bias, or favoritism 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 analyze.

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APPENDIX

FIGURE A.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

Table A.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 labor 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.*

Table A.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

Table A.3. *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. 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 A.4. *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

The male marriage premium: Selection, Productivity, or Employer Preferences?

SUPPLEMENTARY MATERIAL

Table S.1 *Wage Premium for Married Men, SHP (Nearest-Neighbor Matching with Replacement Replication)*

		POLS	POLS	FE: Unmatched	FE: Matched	FE: Unmatched	FE: Matched
Dimension	*Level*	(1)	(2)	(3)	(4)	(5)	(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 S.2. *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 S.3. *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 S.4 *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 S.5 *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