Three worlds of marriage effects? Gendered marriage earning differences in the United States, Germany, and Sweden

Geist, Claudia

Claudia Geist

Three worlds of marriage effects? Gendered marriage earning differences in the United States, Germany, and Sweden

Gibt es drei verschiedene Welten der Ehe-Effekte? Geschlechterspezifische Einkommensunterschiede von Verheirateten in den Vereinigten Staaten, Deutschland und Schweden

Abstract:
Being married is associated with many advantages. However, we do not know enough about the actual impact of *entering* marriage on individuals' earnings, especially for women. In this paper, I examine the immediate and the short-term impact of marriage on men's and women's earnings in the United States, Germany, and Sweden. Studying the impact of marriage on earnings in three distinct socio-political settings provides insights into the context dependency of the link between marriage and earnings. Fixed effects models show that marriage transitions are not associated with women's earnings in the United States and Sweden. For German women, I find an earnings penalty for marriage. Once I adjust for selection into employment, I find that employed German women with low employment propensities may experience instantaneous earnings boosts when they enter marriage, but that among women who are more firmly attached to the labor market, there is a short-term marriage penalty. For men in all three countries, I find no effect of marital transitions once employment likelihood is taken into account.

Key words: marriage, earnings, Germany, Sweden, United States of America, selection, women, men

Zusammenfassung:
Verheiratet zu sein wird mit vielen Vorteilen in Verbindung gebracht, aber wir wissen nicht genug über den tatsächlichen Effekt des Eheeintritts auf individuelles Einkommen, insbesondere für Frauen. In diesem Beitrag werden die unmittelbaren und kurzfristigen Effekte untersucht, die eine Verehelichung auf die Einkommen von Männern und Frauen in den USA, in Deutschland und in Schweden hat. Die Untersuchung des Einkommenseffekts der Ehe in drei unterschiedlichen sozial-politischen Settings ermöglicht Einsichten in die Kontextabhängigkeit des Zusammenhangs zwischen Heirat und Einkommen. Mit Fixed-Effects-Modellen wird aufgezeigt, dass der Übergang zur Ehe weder in den Vereinigten Staaten noch in Schweden mit dem Einkommen von Frauen zusammenhängt, während deutsche Frauen dadurch Einkommenseinbußen hinnehmen müssen. Kontrolliert man jedoch die Selektionseffekte bei der Aufnahme einer Beschäftigung, so kommt man zu dem Ergebnis, dass erwerbstätige Frauen in Deutschland mit niedriger Beschäftigungsneigung bei Ehe-Eintritt umgehend Einkommenszuwächse erfahren, dass aber Frauen, die stärker in den Arbeitsmarkt eingebunden sind, dann kurzfristig negativ sanktioniert werden. Für die Männer in den drei Ländern kommt es jedoch beim Übergang in die Ehe zu keinen Einkommenseffekten, wenn deren Beschäftigungswahrscheinlichkeit berücksichtigt wird.

Schlagwörter: Ehe, Einkommen, Deutschland, Schweden, Vereinigte Staaten von Amerika, Selektion, Frauen, Männer

https://doi.org/10.3224/zff.v29i3.01

Marriage is associated with many economic advantages as well as better physical and mental health and improved social networks for both men and women (Ross/Mirowsky/Goldsteen 1990; Sigle-Rushton/McLanahan 2002; Simon 2002; L. J. Waite 1995; L. J. Waite/Gallagher 2000). For men, being married is associated with higher wages in a broad range of countries, but the evidence for women is less clear (Geist, unpublished document).

These results provide an important snapshot of existing marriage earnings differences, yet they do not tell us about the *impact* of entering marriage on individuals' earnings. In this paper, I examine the immediate and short-term impact of marriage on men's and women's earnings in the United States, Germany, and Sweden. A longitudinal and comparative approach has two advantages. First, the longitudinal nature of the data allows me to account for unmeasured individual characteristics that may be associated with both the chances of entering marriage and earnings. Second, examining the impact of marriage on earnings for men and women in comparative perspective provides me with three distinct institutional settings in which marriage earnings differences can manifest themselves, and as a result, this study provides important insights into the possible context variability of the short-term impact of marriage on individuals' earnings.

In this paper, I focus on the immediate and short-term impact of marriage entry on men's and women's individual earnings in the United States, Germany, and Sweden. My study addresses both selection into employment and, to a lesser extent, into marriage, and is therefore uniquely suited to illustrate the net effect of marriage for women and men in the three different contexts. Although even longitudinal data cannot fully assess the causal nature of the relationship of the association between marriage and earnings I use the phrase "marriage effect" to refer to the association between a change in marital status and a change in earnings.

Specifically, this study addresses three broad research questions:

1. What are the short-term effects of marriage on earnings for men and women's earnings?
2. To what extent is the impact of marriage on earnings caused by underlying differences in individual and labor market characteristics between those who enter and do not enter marriage?
3. Is there evidence that the impact of marriage on earnings varies across contexts for men and women?

In the next sections, I provide a brief description of the literature on marriage earnings differences, with a specific emphasis on issues of selectivity in employment and into marriage. I also discuss the importance of using a comparative perspective for situating the association between marriage and earnings in a broader context. I then describe the data and analytic strategy, before turning to results and conclusions.

Marriage entry and earnings

There are numerous economic and social advantages associated with marriage that have been discussed elsewhere (Ribar 2004; Linda J. Waite/Gallagher 2000), but not enough is known about the short-term economic impact of entering marriage for men and women in comparative perspective. Marriage, for heterosexual couples, is a highly gendered institution that comes with different sets of expectation for husbands and wives. The role of husband is closely associated with that of a breadwinner, whereas being a wife, despite relatively high levels of labor force participation among married women cross-nationally, is not conditioned to labor market success in the same sense.

Research on men has identified a marriage earnings advantage and a specific earnings benefit of marriage entry (Bellas 1992; Cohen/Haberfeld 1991; Kaufman/Uhlenberg 2000; Nakosteen/Zimmer 1997), but recent work by Killewald and Lundberg (2017) has cast doubt on the causality of this association. For women, Light (2004) finds that while household income increases when US women start to cohabit or enter marriage, women's earnings suffer. Korenman and Neumark (1992), however, find no direct effect of entering marriage on women's wages. Some studies examine marriage earnings premiums in longitudinal samples outside the United States (e.g, Coppin 2000), but to my knowledge, no studies include an examination of marriage earnings gaps for both men and women in multiple countries.

Economic Selection

Some studies show that men's employment and favorable economic circumstances increase the chance of marriage for men (D. T. Lichter/Kephart/McLaughlin/Landry, 1992; Daniel T. Lichter/Landry 1991; Oppenheimer 1994; Smock/Manning 1997; Speare/Goldscheider 1987). The observation that men with higher earnings potential are also more likely to enter marriage than those with lower earnings potential has been offered as a central explanation of the marriage bonus for men (Blackburm/Korenman 1994). This implies that observed wage differences between married and unmarried men are explained by the fact that highly productive men with increased earnings potential have better chances of both entering marriage and remaining married, thus raising the average earnings level among those who are married (Nakosteen/Zimmer 1997). Killewald and Lundberg (2017) argue that marriage and rising earnings are merely co-occurring during the transition to the life course for men in the United States.

Recent empirical evidence implies that increased earnings power and economic independence may also increase women's chances of entering marriage under certain circumstances (Ono 2003; Sassler/Schoen 1999). However, the increased chance of entering marriage may be partially offset by the fact that women's financial autonomy reduces the pressure to get married.

Studies have addressed the issue that the marriage earnings gap may be due to selection by using longitudinal data. This allows the researcher to examine the effect of marriage entry within an individual's earnings trajectory, rather than simply comparing earnings across two possibly very different groups of individuals. However, examining the ef-

fect of marriage on earnings is subject to yet another selection mechanism that has largely been ignored. Earnings and marriage earnings effects can only be observed for men and women who actually are employed and have earnings. As such, we know little about how marriage may affect those who are not employed, in the event they "beat the odds" and enter marriage. While I am unable to solve the issue in the present study, I take into account employment propensity in addition to the standard human capital controls for modeling earnings. This, combined with my comparative approach allows me to examine the extent to which the impact of marriage on earnings is context specific with respect to national policy differences, but I can also differentiate between individuals who can be expected to have a stable employment over the life course and those who may be more at risk for unemployment. This is particularly important for men, for whom stable employment and high levels of human capital have been described as a precondition for marriage formation.

Attitudes as source of differences

Of course, economic circumstances are not the only way in which those who enter marriage may be different from those who remain single. Favorable attitudes towards marriage are associated with a greater likelihood of marriage net of other factors (Sassler/Schoen 1999). The results reported by Sassler and Goldscheider (2004) suggest a decreased role of economic considerations, like men's employment, in the marriage decision, and an increase in the importance of values. Non-economic characteristics that make men more attractive spouses may also make them more attractive employees who are rewarded with higher earnings by employers. Those who are married have stronger preferences for stability and a reduced tendency to engage in high-risk behavior compared to singles, especially among men.

Women's positions in the labor market have undergone more rapid changes than the gendered relationships between potential spouses. Nevertheless, we must account for the possibility that non-economic characteristics that make individuals more attractive spouses may also shape their earnings potential in complex ways. In my study, I exclude the effect of differences in (stable) underlying characteristics between married and unmarried individuals by examining marriage effects *within*, rather across individuals.

Productivity differences

For men, the marriage wage gap has also been attributed to the productivity-enhancing characteristics of marriage. Marriage is thought to increase productivity through improved physical and mental health. Along with the emotional benefits associated with marriage researchers cite household specialization as a crucial mechanism through which marriage enhances men's productivity (Chun/Lee 2001). The (male) main breadwinner spends more time and effort on employment, resulting in higher earnings, and the female partner focuses on domestic production, even if she continues to be employed (Becker 1981). Korenman and Neumark (1991) find that men's wages increase faster after marriage, they receive more frequent promotions and better performance evaluations (see also Gray

1997). However, based on their findings, Cornwell and Rupert (1997) and Hersch and Stratton (2000) doubt that marriage enhances productivity through specialization. For women, research suggests that marriage is associated with changes consistent with the specialization approach: marriage has been shown to increase domestic labor and may lead to both lower levels of participation and productivity in paid work (Becker 1981; Kalleberg/Rosenfeld 1990), with the possibility of subsequent wage loss. However, evidence has also cast doubt on both selection and specialization arguments, and further suggest that other mechanisms are at play (de Linde Leonard/Stanley 2015).

The policy context of marriage

Public policy often supports marriage, for example through explicit financial or institutional support, but potentially even more through framing of marriage as a highly valued family form. Policy constellations shape the meaning of marriage and the very circumstances in which marriage occurs – most notably, the centrality of the male breadwinner role, the views towards women's labor force participation, and the role and importance of marriage vary cross-nationally (Alwin/Braun/Scott 1992; Crompton 1999; Esping-Anderson 1999). Countries have different sets of social institutions, such as labor market policies, gender relations, and existing or lack of state support for certain family forms.[1] State policy shapes the relationship between states, markets, families, and individuals. In the presence of a strong government safety net, unemployment may not have a dramatic impact on people's lives, including their marriage behavior. The same may not be the case, however, in a policy context where unemployment that is more than short-term is often associated with poverty (Gangl 2004). The meaning of economic prosperity, poverty, or employment is affected by the institutional setting.

Framing family and gender ideologically

One of the most popular categorical frameworks for comparative research on advanced western economies relies on Esping-Anderson's (1990) classification of welfare regimes into social-democratic, liberal, and conservative policy clusters. Esping-Anderson focuses on the relationship between states, markets, and families (Esping-Andersen 1990) as well as the dimension of gender relations more explicitly (Esping-Anderson 1999).

Social democratic regimes actively advocate gender equity. The Swedish state, for example, understands itself as instrumental towards that goal (Sainsbury 1996). The state promotes female labor force participation and equal pay. Liberal regimes' focus on individual and gender equality is not as actively pursued through government activity as it is in the social democratic regimes. The liberal country cluster has been characterized as taking a "laissez-faire" approach with only limited state interventions in both the economic and private spheres. This results in a generally neutral stance towards issues such as family and gender, and an emphasis on the regulatory power of markets. In liberal re-

1 Of course, there is a reciprocal relationship between public attitudes and state policies.

gimes, individualism supports the idea of equality of men and women through the universal breadwinner model which promotes both men's and women's full-time employment.
In contrast to the active support for egalitarian gender relations in countries that are part of the social democratic policy cluster, and the more passive support for egalitarianism of the liberal tradition, conservative regimes actively support traditional gender roles. Women are discouraged from participating (full-time) in the labor market, as they are seen as "naturally" responsible for caring for the home and the family (Mósesdóttir 2000). Another difference between the conservative and the liberal policy groups is that this familistic approach does not target the individual (like in liberal regimes), but the family as a "unit." Closely tied to this concept, however, are strong ideas about the nature of families. The male breadwinner model, which is prevalent in many conservative countries, assumes or encourages the notion that the husband supports the family and the wife focuses on domestic responsibilities. Conservative policy seeks to maintain existing structures by supporting a traditional division of labor with an expansive set of social and economic policies, particularly seeking to strengthen the traditional family.
State policies target both employment patterns and family formation, but I argue that they also affect the link between family and employment. States have several policy tools at their disposal. When it comes to the areas of family and labor market policy, taxation is often considered as one of the most powerful tools to set incentives. Dingeldey (2001) suggests, however, that taxation systems alone do not clearly shape employment patterns in Europe. Her results indicate that coordinated policy packages can affect individuals' labor force participation if a certain family participation model is favored. There is evidence that in policy contexts where women's employment is not supported, women's overall participation rates and women's full-time employment rates are lower.
In the "three worlds" scheme, labor market institutions in Germany, the ideal type of the conservative regime, may incorporate mechanisms that reflect the strong male breadwinner model by favoring married men. For example, tax-splitting makes income inequality between spouses economically advantageous. Typically, the higher earning partner is taxed at a lower rate compared to the lower earner. This has the potential to further depress the labor supply especially of wives. This also suggests that men's economic circumstances are crucial in predicting transitions into marriage. The United States represents the prototypical liberal regime, where full-time employment is promoted for men and women, regardless of marital or parenthood status. Consequently, family status should play less of a role in shaping employers' responses. In Sweden, the exemplar of a social democratic regime, principles of universalism and egalitarianism might be expected to inhibit or limit gender differences in the labor market effects of marriage formation, especially given the great social acceptability of cohabitation.

More recently, scholars have suggested that three policy typologies may not be enough, even to capture the policy circumstances (i.e. Ferrera 1996). However, even with new taxonomies, Germany, the United States, and Sweden remain understood as three differing contexts that are meaningful to compare (Aisenbrey/Evertsson/Grunow 2009).

The current study: Cross-national differences in the effect of marriage

There is cross-national variation with respect to the centrality and nature of marriage as indicated by cross-national variation in marriage rates, average age at first marriage, and marriage dissolution rates (2017). Given cross-national differences in support for (and women's rates of) labor force participation, the political support for traditionally gendered marriage, and differences in gender equality and women's power, we can expect that there is also variation in the extent to which marriage entry affects women's earnings. In this study, I focus on three countries that are ideal types of Esping-Anderson's (1990) three welfare state categorizations. There are many different ways to assess gender equality within and across society. A cross-national comparison of the association between marriage and earnings for both men and women allows us insights into the relevance of marriage as an institution that may or may not further perpetuate inequality across policy contexts.

The United States is the prime example of the liberal policy regime, and has a very high marriage rate (crude marriage rate in 2000 is 8.4[2]); at the end of the 1990s U.S. Census cohabitation estimates in the United States reached just under 10% for of all never married individuals (Casper/Cohen, 2000). As of 2011, about 12% of 20-34 year olds were cohabiting with a partner.

Germany is an example of a conservative corporate country, with below European average crude marriage rates of 5.1[2], and Sweden represents a social democratic country with a low marriage rate of 4.5.[2] Official tracking of cohabitation in Europe remains lacking, but recent sources suggest that cohabitation is common in both Germany and Sweden, with 17% and 29% (respectively) of 20-34 year olds in 2011 living with a partner (but not married) (OECD Family Database, table SF3.3 A). State policy that encourages a traditional division of labor between husband and wife may favor men's incomes and discount women's earnings in the allocation of housework (Geist 2005). I thus expect that in conservative policy contexts, the male bonus and the earnings penalty for women that are associated with marriage are particularly large. I expect this not only as a direct result of government policies, but also as a consequence of employers' unconscious (or conscious) discrimination whose action follow commonly shared policy goals (of favoring married men and discouraging women's employment) at the company level.

DiPrete (2002) examines life course risks and mobility consequences in the United States, Germany, and Sweden and points out that state policies may buffer the impact of negative life events. Gangl (2004) shows that this is partially due to unemployment insurance. Germans experience more stable income and career trajectories, compared to individuals in the United States. Finally, McManus and DiPrete (2000) find that variety of life events are associated with more instability in household income associated in the United States compared to Germany, partly because of greater turbulence in earnings in the United States.

Based on these findings and the variation regarding the centrality of marriage in state policy in the three countries, one could expect the marriage advantage for men and the marriage penalty for women to be largest in the conservative policy context of Germany.

2 Marriage rates were obtained from data from Geist (2017).

This type of policy context is the most supportive of marriage and traditional division of labor within marriage. Since there is comparatively less pressure to enter marriage in social democratic countries, and there are few policies directed towards influencing the link between family and economy in liberal countries[3], smaller marriage earnings differences may be expected here.

DiPrete and McManus' (2000) examination of the long term household income and individual earnings consequences of life changes in the United States and Germany, however, fails to find that union formation is associated with greater earnings benefits for German men or greater disadvantages for German women. Instead, they find that adding a partner reduces women's labor earnings in the United States over a 5- and 7-year span, and increases men's labor earnings over the 5-year period. In Germany, neither women's nor men's labor earnings are affected over the 5- and 7-year time span.

Data, methods, and measures

In this paper, I use panel data from the United States, Germany, and Sweden. For the United States, I use data from 1986 through 1996 from the Panel Study of Income Dynamics (PSID).[4] For Germany, I use data from the German Socioeconomic Panel (GSOEP) for the years 1984 to 2001[5]. I use measures from the Cross-National Equivalency File (CNEF) to augment the US and German data sets, which has been established to make measures from the PSID and the GSOEP comparable across countries. For Sweden, I use the 1996 and 1998 waves of the Panel Study of Market and Nonmarket Activities (HUS).[6] Although the time frames do not perfectly overlap, they are close enough to allow comparability. To maximize comparability with the Swedish data I did not choose more recent waves of the German and US data. I restrict my analyses to those who are either single or married, excluding those who are separated, divorced, or widowed. For the US and German data, I can exclude those who report that their current marriage is not their first. I restrict my analyses to adults between the ages of 22 and 45, the age range when most marriages occur.[7] For analyses of the marriage earnings advantage, I restrict my sample to those who are employed and report positive earnings.[8]

3 There has been a discussion about providing marriage incentives in the United States. However, this discussion and the limited number of policies that exist have mostly targeted poor single women, rather than the entire population.

4 I model marriage earnings effects for years following 1988, since experience and tenure measures are only available starting those years. I use data for 1986 and 1987 to create short-term marriage history.

5 I model marriage earnings effects for the years starting with 1986, but I use the marriage information for the year 1984 and 1985 to create the short-term marriage history.

6 Other waves exist, but data and codebooks are only available in Swedish for earlier years.

7 Because of the low average age at marriage in the United States, I chose 22 years as lower bound for the age range, to maximize the transitions into first marriage.

8 Due to the challenge of assessing earnings for self-employed individuals they are excluded.

Analytic strategy

In this paper, I use a combination of two methods to identify marital status earnings disparities, fixed effects models and propensity score matching. I first estimate the effect of marriage on earnings separately for men and women in each of the three countries. I do so by using a fixed effects model. In the analyses of individual level, data analysts face the challenge to discern the effect of a measure of interest from unmeasured heterogeneity. When panel data are available, the fixed effects model is an elegant and conservative way to address the problem of unmeasured time-invariant heterogeneity (Baum 2006; Greene 2000). In the fixed-effects estimation, every observation is observed from the unit average, in my case the individual cross-year average. As such, the fixed-effects model removes all between-unit variation from the data and it cannot estimate coefficients for time-invariant characteristics.

By estimating a fixed effects regression with marital status as the sole dependent variable, I can ascertain the effect of marital transitions on earnings net of unmeasured, stable characteristics. I use two different measures of marital status. One reflects marital status in the current year. Thus any earnings effect associated with this measure points to an instantaneous earnings impact of marriage. I also estimate models for both the German and US data that examine the impact of being married in the current or previous two years on earnings, reflecting a more general short-term effect of marriage on earnings. For the Swedish data, I have less information about marital history, and, therefore, I estimate a fixed effects model that only includes the effect of marital status on earnings. I distinguish between married and unmarried individuals, which includes singles and cohabiters.

In a second step, I also account for changes in the individual and labor market characteristics, to obtain the net effect of entering marriage on earnings, including the measures for the German and US data described above. This allows me to isolate the impact of marital status change net of other changes that may have occurred in individuals' lives.

Although other recent studies have used sophisticated fixed effects models to stringently test causal association between married and earnings for men in the United States (Killewald/Lundberg 2017), they have not accounted for the fact that we only observe earnings for those who are currently employed. In a third step, I further adjust for individuals' employment propensity for the US and German respondents, to examine whether selection into employment is at the heart of any of the observed marriage earnings differences (ibd.). To obtain individual earnings propensities, I use propensity score matching techniques, implemented in the "pscore" command in Stata. In general, propensity matching is used to examine whether treatment effects are a result of by differential selection into treatment and control groups.

The propensity score matching procedure groups observations in clusters with similar propensity scores. Those who are and are no employed are then compared with respect to their similarity in the control variables (age, education, etc.). If the group means are not statistically different, the balancing properties are satisfied. In the absence of balanced groups, it may be difficult to make claims that a selection correction has fully taken place, since there are significant group differences between the group who is and is not employed. While balancing properties are not satisfied for all propensity clusters in all of the subgroups for which I estimate the propensity scores, I argue that these differences are not

as substantively problematic because of the sample restriction. For example, if there are age differences between the two groups within a propensity score cluster, this is not as problematic since the age range is already restricted to about five years.

Therefore, a probit model estimates selection into these groups, which makes it possible to identify observations that have received treatment but are otherwise comparable to those who did not. My case is slightly more complex. My interest lies in examining marriage effects on earnings, but earnings can only be observed for those who are in employment. Propensity score matching allows me to identify whether an individual is very likely to be employed, or, even more importantly, is not very likely to be employed, but still is in employment during the observation time. I obtain earnings propensity scores by estimating probit models with employment as the dependent variable. The parsimonious model for employment status includes age (linear and squared) and education (in years),[9] and an indicator for disability status, which distinguishes between those who are and are not disabled. For the German models, I also include an indicator for residence in East Germany. These measures reflect the employment changes or also the difficulty of finding employment. Additionally, I include the natural log of household post-government income in the previous year. This income measure includes household income, public transfers, and benefits, minus paid taxes. I include this measure to account for the financial pressure of a specific household member to seek employment. These models are estimated for specific subsets of the population. Grouping variables are gender, whether or not there are children in the household (ranged from 0 to 4), age, and education. For the United States, I also estimate models separately for Whites and Nonwhites.[10] For Nonwhites, I distinguish between Blacks and "Other" racial groups. The "other" racial group includes Native American, Asian, Latino,[11] and other. Table 1 provides an overview over the different groups for which the employment models are estimated.

Table 1: Subgroups for propensity matching analyses

Gender	Parenthood	Age Group	Education group		Race
			United States	Germany	United States
Women	with children	Age 22-27	0-11 years of schooling	10 years of schooling and/or training or less	White
Men	without children	Age 28-35	12 years of schooling	10.5 years of schooling and/or training	Nonwhite
		Age 36-45	13-15 years of schooling	11 or 11.5 years of schooling and/or training	
			16 years of schooling or more	more than 11.5 years of schooling and/or training	

Of course, my sample of employed individuals with positive earnings will have a large proportion of individuals with high employment propensity scores. I use the "common support" option, where employed individuals, who have employment propensity scores

9 The German education measure is a combination of years of schooling and vocational education.

10 Individuals are assigned the race of the household head. Moreover, this measure does not allow me to identify multi-racial individuals.

11 A separate measure allows all respondents to identify Hispanic origin, regardless of racial category, but it is not included in this analysis.

that are higher or lower than the maximum or minimum propensity scores for those who are not employed. The propensity matching procedure ensures that the sample of those who are employed is also representative of those who are not employed. In my analyses, I want to be able to account for the selection into employment, so I estimate separate models for whose employment propensity is at or above the median level, and for those whose scores are below the median.

Each individual was assigned a specific employment propensity score in addition to their reported employment status. Consequently, the propensity matching procedure ensures that the sample of those who are employed is also representative of those who are not employed (see Table 2 for sample sizes). To obtain fixed effects models adjusted for employment propensity, I estimate separate fixed effects models for those with low and high employment propensity separately to examine whether selection into employment is at the heart of any of the observed marriage earnings differences. Those who are at or below the 25th percentile of employment propensity scores are considered to have a low score, those with an employment propensity at or above the 75th percentile are considered to have a high employment propensity. These cutoff points are chosen to capture "extremes," while also ensuring a sufficient group size.

Table 2: Sample sizes by country and gender (in person-years)

	All		Low employment propensity		High employment propensity	
	Women	**Men**	**Women**	**Men**	**Women**	**Men**
United States	16304	17059	2710	2786	2788	2794
	(3390)	(3339)	(1170)	(1232)	(1145)	(1166)
Germany	12137	17140	1545	2069	1545	2069
	(2603)	(3336)	(665)	(859)	(545)	(734)
Sweden	375	339				
	(264)	(242)				

Note: Number of respondents in parentheses.

Key outcome: Earnings

The key outcome of interest in this study is individual labor earnings. The data from Germany and the United States are harmonized and includes wages and salary from all employment before taxes, self-employment (although in my analyses, self-employed respondents are excluded), as well as income from bonuses, and overtime. These earnings do not include special bonuses, for example for children, and are thus a conservative measure of actual income. Earnings data from Sweden represent the "regular earnings, before taxes and other deductions." This measure is annualized as respondents have the option to report hourly, weekly, monthly, and annual earnings. All analyses used the natural log of the earnings measure.

Marriage

Analyses focus on the association between marriage and earnings. I include an indicator of whether a respondent is married or not in any given year and a second measure that indicates whether an individual is married/has been married in the current year ("married"),

the previous year, or the year before ("married recently"). Since those classified as "single" include individuals who are cohabiting or are divorced, the findings are only a conservative estimate of the effect of heterosexual relationships on men's and women's earnings. In supplemental analyses, I examine the impact of marriage compared to singles, and simultaneously compared cohabiters to singles. The observed marriage effect was not substantially affected, so the results are not shown. The institution of marriage varies across the three countries, with marriage rates highest in the United States, and age at first marriage highest in Sweden (Geist 2017). In models presented below, unmarried individuals are the reference group.

Key independent variable and controls

In the earnings models estimated for the US and German data, I include three measures of human capital: age, experience in the labor market, and tenure at current job (all are included in a linear and squared term). In the German data, tenure is measured in years, in the US data, it is measured in months. I include annual hours (in 100s) as a measure of labor supply, and I further include the number of children in the model. In the Swedish earnings model, I include age, education, and tenure (in linear and squared terms), as well as the weekly hours worked. Education is included since labor market experience is not available. I also include an indicator for employment in the private sector (vs. the public sector) and the number of children.

Results

In a first step, I compare earnings of married and unmarried individuals of the three countries (see Table 3). I find that in Germany and Sweden, unmarried women have higher average earnings than their married counterparts, but these differences are not significant for women in the United States. Among men, married men have higher earnings than unmarried men in all three countries.

Table 3: Average labor earnings for married and unmarried men and women

	Women		Men	
	Unmarried	**Married**	**Unmarried**	**Married**
United States	9.48 (3538)	9.50 (12766)	9.72 (3026) †	10.25 (14033)
Germany	9.67 (2387) †	9.37 (9750)	9.82 (4087) †	10.02 (13053)
Sweden	12.01 (165) †	11.94 (210)	12.21 (171) †	12.32 (168)

Note: † denotes that the test for group differences is significant at the .05 level. The number of observations is in parentheses. The outcome is log annual earnings, so the underlying unit is not substantively meaningful (log US Dollars, Euros, and Kronor).

In a next step, I examine whether these differences are a result of marriage lowering women's earnings and enhancing men's, or whether these differences are in fact a result of selection of individuals with different underlying characteristics into marriage. I esti-

mate fixed effects models that illustrate the effect of a change in marital changes on earnings net of other unobserved characteristics. Table 4 illustrates these effects of marriage within individuals. Columns 1 and 3 show the immediate impact of marriage (in the current year), and the columns 2 and 4 show short term effect of marriage, by indicating whether somebody entered marriage in the current or the previous two years. For Sweden, I only have two years available, so I can only assess the impact of entering marriage between those two years.

Table 4: The effect of marriage on earnings, net of stable respondent characteristics (fixed effects models, no other controls)

	Women		Men	
	Married this year	**Recently married**	**Married this year**	**Recently married**
United States	0.023	0.018	0.127**	0.179**
	(0.70)	(0.54)	(5.11)	(6.95)
Germany	0.199**	0.203**	0.505**	0.513**
	(4.29)	(4.29)	(22.39)	(22.51)
Sweden	0.101	0.026	0.028	0.055
	(1.11)	(0.22)	(0.39)	(0.66)

Note: Statistics in parentheses are t values, * p<.05, ** p<.01. See Table 1 for sample sizes and Appendix A for complete results.

Among women, I only find marriage earnings effects in Germany. For German women, marriage is associated with a significant earnings boost. Marriage is not associated with individual earnings for women in the United States or Sweden. These results suggest that in Sweden, the earnings disadvantage of married women presented in Table 3 may have been due to the fact that singles as a group have higher earnings potential. In Germany, on the other hand, it seems that the earnings disadvantage observed for married women in the cross-sectional comparison is to be due to selection, and entering marriage actually is beneficial for women's earnings.

For men, the marriage earnings advantage shown in Table 3 persist, suggesting that the observed marriage earnings advantages could be a result of the actual benefits of marriage for men's earnings. In Sweden, I do not find that men's earnings change once they enter marriage.

The impact of marriage I find within individuals, however, could also be due to other changes in individuals' lives. It could be that the earnings changes are a result of changes in labor supply, tenure, or experience. Table 5 shows marriage earnings differences adjusted for changes in these individual-level characteristics. As in previous models, there are no indications that marriage is associated with women's earnings in the United States and Sweden. When taking into account changes in human capital characteristics, labor supply, and number of children, however, German women experience an earnings penalty, net of other changes that occur in their lives. One interpretation is that the earnings bonus of marriage that was documented in Table 4 is due to the fact that German women who marry and remain employed (rather than drop out of the labor force) are those with better education and better-paying jobs. Once trends in labor supply and, tenure and other

human capital characteristics are taken into account, the results show that everything else equal, marriage actually has a negative immediate impact on their earnings.

Table 5: The effect of marriage on earnings, net of stable respondent characteristics and changes in human capital[a] (fixed effects models)

	Women		Men	
	Married this year	**Recently married**	**Married this year**	**Recently married**
United States	0.019	0.004	-0.004	0.013
	(0.71)	(0.16)	(0.18)	(0.57)
Germany	-0.110**	-0.111**	0.042*	0.041*
	(3.08)	(3.05)	(2.49)	(2.41)
Sweden	0.040	-0.045	0.005	-0.043
	(0.49)	(0.44)	(0.10)	(0.71)

Note: Statistics in parentheses are t values, * p<.05, ** p<.01. See Table 1 for sample sizes.

[a]Models include controls for human capital, labor supply, and number of children. See Appendix B for full models.

The earnings bonus of marriage for men in the United States disappears once changes in human capital and labor supply are taken into account, but for German men, a significant, albeit smaller, marriage bonus remains significant. This suggests that in Germany, marriage affects men's earnings, whereas in the United States, marriage does not have a direct impact.

Table 6: The effect of marriage on earnings, net of stable respondent characteristics and changes in human capital[a] by level of employment propensity (fixed effects models)

Women	**Married this year** **Low[b]**	**Recently married**	**Married this year** **High[b]**	**Recently married**
United States	0.159	0.063	-0.060	-0.028
	(1.37)	(0.57)	(1.39)	(0.66)
Germany	0.515*	0.032	-0.126	-0.092*
	(2.40)	(0.45)	(1.66)	(2.07)
Men	**Low**		**High**	
United States	-0.072	0.044	0.015	-0.017
	(0.93)	(0.58)	(0.33)	(0.35)
Germany	0.109	-0.034	0.012	0.035
	(1.41)	(0.60)	(0.18)	(1.41)

Note: Statistics in parentheses are t values, * p<.05, ** p<.01. See Table 1 for sample sizes.

[a]Models include controls for human capital, labor supply, and number of children. See Appendix C for full models.

[b]Employment propensity scores are derived from a series of probit models that estimated the probability of employment. Low propensity scores are scores that are less or up to the 25th percentile of employment propensities with in each gender. High propensity scores are at or above the 75th percentile of scores for men and women respectively.

To assess whether these findings apply to both those who are firmly embedded in the labor market and those who are less likely to be employed, I estimate the marriage effect in the United States and Germany separately for those with low and high employment propensities. Table 6 shows the immediate and the short-term marriage gaps after I account for employment propensity. The results for German women are contradictory. While employed women with a low employment propensity seem to experience a substantial immediate marriage earnings bonus, there is no impact of a short-term marriage effect on earnings among this group. For women with high employment propensity (e.g., those with a high level of education and no young children), marriage is associated with an earnings penalty when entry into marriage in the current and previous two years is considered. For men, there seem to be no significant earnings effects at the extremes at the high and low end of employment likelihood.

Summary and conclusions

This study provides a thorough analysis of marriage earnings differences for men and women in the United States, Germany, and Sweden. I use fixed effects models to estimate marriage earnings effects net of unmeasured differences across those who are married and those who remain unmarried. I find that overall, married women have lower average earnings in Germany and Sweden, but no differences can be found for US women. Married men have higher average earnings than single men in all three countries. This marriage earnings advantage persists when I examine how marriage shapes earnings within individuals, net of unmeasured stable characteristics. The fixed effects models show that marriage transitions are not associated with changes in women's earnings in the United States and Sweden. In Germany, however, marriage entry seems to be associated with an earnings boost, if all other individual characteristics are assumed stable over time.

However, some characteristics that are crucial in shaping individual earnings are likely to change over time. In the models that adjust for possible changes in human capital and labor supply, I find an earnings penalty for marriage among German women, and a marriage earnings bonus for married men in Germany. For men in the United States, however, this bonus disappears when change in human capital and labor supply is taken into account. In the United States, married men may experience changes in human capital and labor supply that result in higher earnings, but earnings benefits are not a result of changes in marital status itself.

In a final step, I assess marriage earnings difference for those who have very high employment propensities, and those who, even though they are employed, have a low employment propensity. The results suggest that there may be an instantaneous earnings boost for women with low employment propensity, but that among women who are more firmly attached to the labor market, there is a short-term marriage penalty. For men, I find no effect of marital transitions on earnings at either the high or the low end of employment likelihood.

Several conclusions can be drawn from the results of this study. One way to interpret the results is that for women, marriage has no clear impact on earnings, since the cross-

sectional earnings disadvantages of married women compared to singles are a result of the selection of women with lower earnings capacity and lower employment propensity into marriage. The men's marriage earnings advantages are largely explained by different characteristics of those who marry and those who remain single, as well as different trajectories in human capital and labor supply.

In sum, one possible conclusion is to focus on the fact that there are few effects of earnings net of unmeasured heterogeneity, human capital, labor supply, and employment propensity. However, there is another interpretation of the present results.

With respect to cross-national differences, I find that in the United States and Sweden, marriage has no effect on women's earnings, but in Germany, I find evidence that entering marriage reduces earnings. In an addition to the literature on marriage and earnings, I further adjust for employment propensity. Women who enter marriage in Germany have lower employment propensity, which is associated with lower earnings, and the marriage earnings penalty for women was a reflection of this difference. For German men, the immediate earnings benefit associated with marriage is more robust even when the fact that men who marry also have a higher employment propensity (and consequently a higher earnings propensity). The results do suggest that the conservative policy context that has supported traditional marriage centered on a male breadwinner family may be responsible for these earnings benefits for German men. In contrast, in Sweden, where pressures to enter marriage are rather small and there is no emphasis on the male breadwinner, men experience no marriage earnings advantage, not even without a selection correction.

This study is not without limitations. While my analyses contain some selection corrections, including the selection into employment, the use of fixed effects measures only provides an imperfect correction to the possible selection into marriage, and the issue of causality is not resolved. Data limitations do not allow for a more fine-grained assessment of the causality of the relationship akin to Killewald and Lundberg (2017). Additionally, the comparisons presented here are based on legal marriage, rather than cohabitation between partners. Cohabitation is more common in Sweden compared to the United States and Germany, as well as the extent to which being in a relationship impacts the division of labor in the household, work effort, or the level of discrimination experienced in the work place. The earnings effects are not fully work-time standardized, in part due to the country differences in the measurement of work time. Moreover, the goal for this study is not to identify the effect of marriage net of housework time, which is closely linked to time spent in in employment. Sobel (2012) goes as far as shedding doubt on the use of traditional fixed effects methods for estimating marriage earnings effects altogether, since important time varying confounders such as substance abuse, depression, etc. are not commonly available alongside high quality earnings measures, let alone over time. This study also cannot capture the considerable variability in the way men and women perceive and express their masculinity, both among men and women and across countries. The study's limit to those who are cisgender and heterosexual enables us to rely on traditional notion of the meaning of marriage and behavioral expectations towards husbands and wives.

These limitations notwithstanding, one key finding of this study are the initial results of how earnings differ between those who are married and those who are not. Even if the causal relationship cannot be established beyond a reasonable doubt and the mechanisms

remain unclear, these differences matter in the people's live experiences. Earnings disadvantages of married women compared to single women may largely be due to selection, but for men, there is stronger evidence that, at least in Germany, entering marriage increases men's earnings. The results suggest that marriage is a family institution yet also a stratifying factor. While marriage itself may not have a clear causal impact on earnings, earnings differences between those who are married and unmarried highlight that changes in economic circumstances and marriage at least co-occur. Existing research has shown that married women have higher household income yet, and in times of marriage instability, reduced individual earnings may put women at risk in the long term. Additional research needs to distinguish between immediate and the short-term effect of marriage entry. Examining those nuances more closely will help us further understand the gendered institution of marriage across contexts. Future research needs to explore how gendered patterns of labor force participation in conjunction with marriage transitions reflect gender equality.

References

Aisenbrey, S., Evertsson, M. & Grunow, D. (2009). Is there a career penalty for mothers' time out? A comparison of Germany, Sweden and the United States. *Social Forces, 88, 2*, pp. 573-605. doi:10.1353/sof.0.0252.

Alwin, D. F., Braun, M. & Scott, J. (1992). The separation of work and the family: Attitudes towards women's labour-force participation in Germany, Great Britain, and the United States. *European Sociological Review, 8, 1*, pp. 13-37. doi:10.1093/ oxfordjournals.esr.a036620.

Baum, C. F. (2006). *An introduction to modern econometics using Stata*. College Station: Stata Press.

Becker, G. (1981). *A treatise on the family*. Cambridge: Harvard University Press.

Bellas, M. L. (1992). The effects of marital-status and wives' employment on the salaries of faculty men – The (house) wife bonus. *Gender & Society, 6, 4*, pp. 609-622. doi:10.1177/089124392006004005.

Blackburn, M. & Korenman, S. (1994). The declining marital-status earnings differential. *Journal of Population Economics, 7, 3*, pp. 249-270. doi:10.1007/bf00517299.

Casper, L. M. & Cohen, P. N. (2000). How does POSSLQ measure up? Historical estimates of cohabitation. *Demography, 37, 2*, pp. 237-245. doi:10.2307/2648125.

Chun, H. & Lee, I. (2001). Why do married men earn more: Productivity or marriage selection? *Economic Inquiry, 39, 2*, pp. 307-319. doi:10.1111/j.1465-7295.2001.tb00068.x.

Cohen, Y. & Haberfeld, Y. (1991). Why do married men earn more than unmarried men? *Social Science Research, 20, 1*, pp. 29-44. doi:10.1016/0049-089x(91)90002-k.

Coppin, A. (2000). Does type of conjugal union matter in the labor market? Evidence from a Caribbean economy. *Review of Black Political Economy, 28, 1*, pp. 7-27. doi:10.1007/s12114-000-1007-8.

Cornwell, C. & Rupert, P. (1997). Unobservable individual effects, marriage and the earnings of young men. *Economic Inquiry, 35, 2*, pp. 285-294. doi:10.1111/j.1465-7295.1997.tb01910.x.

Crompton, R. (1999). *Restructuring gender relations and employment: The decline of the male breadwinner.* Oxford University Press.

de Linde Leonard, M. & Stanley, T. (2015). Married with children: What remains when observable biases are removed from the reported male marriage wage premium. *Labour Economics, 33,* pp. 72-80. doi:10.1016/j.labeco.2015.02.010.

Dingeldey, I. (2001). European tax systems and their impact on family employment patterns. *Journal of Social Policy, 30,* pp. 653-672.

DiPrete, T. A. (2002). Life course risks, mobility regimes, and mobility consequences: A comparison of Sweden, Germany, and the United States. *American Journal of Sociology, 108, 2*, pp. 267-309. doi:10.1086/344811.

DiPrete, T. A. & McManus, P. A. (2000). Family change, employment transitions, and the welfare state: Household income dynamics in the United States and Germany. *American Sociological Review, 65, 3*, 343-370. doi:10.2307/2657461.

Esping-Andersen, G. (1990). *Three worlds of welfare capitalism*. London: Polity Press.

Esping-Anderson, G. (1999). *Social foundation of post-industrial society*. Oxford: University Press.

Ferrera, M. (1996). The 'southern model' of welfare in social Europe. *Journal of European Social Policy, 6, 1*, pp. 17-37. doi:10.1177/095892879600600102.

Gangl, M. (2004). Welfare states and the scar effects of unemployment: A comparative analysis of the United States and West Germany. *American Journal of Sociology, 109, 6*, pp. 1319-1364. doi:10.1086/381902.

Geist, C. (2005). The welfare state and the home: Regime differences in the domestic division of labour. *European Sociological Review, 21, 1*, pp. 23-41. doi:10.1093/esr/jci002.

Geist, C. (2017). Marriage formation in context: Four decades in comparative perspective. *Social Sciences, 6, 1*, pp. 9. doi.org/10.3390/socsci6010009.

Gray, J. S. (1997). The fall in men's return to marriage: Declining productivity effects or changing selection? *Journal of Human Resources, 32, 3*, pp. 481-504. doi:10.2307/146180.

Greene, W. H. (2000). *Econometric analysis*. Upper Saddle River: Prentice Hall.

Hersch, J. & Stratton, L. S. (2000). Household specialization and the male marriage wage premium. *Industrial and Labor Relations Review, 54, 1*, pp. 78-94. doi:10.2307/2696033.

Kalleberg, A. L. & Rosenfeld, R. A. (1990). Work in the family and in the labor market: A cross-national reciprocal analysis. *Journal of Marriage and the Family, 52, May*, pp. 331-346. Doi:10.2307/353030.

Kaufman, G. & Uhlenberg, P. (2000). The influence of parenthood on the work effort of married men and women. *Social Forces, 78, 3*, pp. 931-947. Doi:10.2307/3005936.

Killewald, A., & Lundberg, I. (2017). New evidence against a causal marriage wage premium. *Demography, 54, 3*, pp. 1007-1028. doi:10.1007/s13524-017-0566-2.

Korenman, S. & Neumark, D. (1991). Does marriage really make men more productive? *Journal of Human Resources, 26, 2*, pp. 282-307. Doi:10.2307/145924.

Korenman, S. & Neumark, D. (1992). Marriage, motherhood, and wages. *Journal of Human Resources, 27, 2*, pp. 233-255. doi:10.2307/145734.

Lichter, D. T., Kephart, G., McLaughlin, D. K. & Landry, D. J. (1992). Race and the retreat from marriage – A shortage of marriageable men. *American Sociological Review, 57, 6*, pp. 781-799. doi:10.2307/2096123.

Lichter, D. T. & Landry, D. J. (1991). Labor force transitions and underemployment: The stratification of male and female workers. *Research in Social Stratification and Mobility, 10,* pp. 63-87.

Light, A. (2004). Gender differences in the marriage and cohabitation income premium. *Demography, 41, 2*, pp. 263-284. Doi:10.1353/dem.2004.0016.

McManus, P. A. & DiPrete, T. A. (2000). Market, family, and state sources of income instability in Germany and the United States. *Social Science Research, 29, 3,* pp. 405-440. doi:10.1006/ssre.2000.0675.

Nakosteen, R. A. & Zimmer, M. A. (1997). Men, money, and marriage: Are high earners more prone than low earners to marry? *Social Science Quarterly, 78, 1*, pp. 66-82.

Ono, H. (2003). Women's economic standing, marriage timing, and cross-national contexts of gender. *Journal of Marriage and the Family, 65, 2*, pp. 275-286. doi:10.1111/j.1741-3737.2003.00275.x.

Oppenheimer, V. K. (1994). Women's rising employment and the future of the family in industrial societies. *Population and Development Review, 20, 2*, pp. 293-342. doi:10.2307/2137521.

Ribar, D. (2004). *What do social scientists know about the benefits of marriage?: A review of quantitative methodologies*. Bonn: Forschungsinstitut zur Zukunft der Arbeit.

Ross, C. E., Mirowsky, J. & Goldsteen, K. (1990). The impact of the family on health – The decade in review. *Journal of Marriage and the Family, 52, 4*, pp. 1059-1078. doi:10.2307/353319.

Sassler, S. & Goldscheider, F. (2004). Revisiting Jane Austen's theory of marriage timing: Changes in union formation among American men in the late 20th century. *Journal of Family Issues, 25, 2*, pp. 139. doi:10.1177/0192513x03257708.

Sassler, S. & Schoen, R. (1999). The effect of attitudes and economic activity on marriage. *Journal of Marriage and the Family, 61, 1*, pp. 147-159. doi:10.2307/353890.

Sigle-Rushton, W. & McLanahan, S. (2002). Richer or poorer? Marriage as an anti-poverty strategy in the United States. *Population, 57, 3*, pp. 519-538. doi:10.2307/3246637.

Simon, R. W. (2002). Revisiting the relationships among gender, marital status, and mental health. *American Journal of Sociology, 107, 4*, pp. 1065-1096. doi:10.1086/339225.

Smock, P. J. & Manning, W. D. (1997). Cohabiting partners' economic circumstances and marriage. *Demography, 34, 3*, pp. 331-341. doi:10.2307/3038287.

Sobel, M. E. (2012). Does marriage boost men's wages? Identification of treatment effects in fixed effects regression models for panel data. *Journal of the American Statistical Association, 107, 498*, pp. 521-529. doi:10.1080/01621459.2011.646917.

Speare, A., Jr. & Goldscheider, F. K. (1987). Effects of marital status change on residential mobility. *Journal of Marriage and the Family, 49, 2*, pp. 455-464. doi:10.2307/352314.

Waite, L. J. (1995). Does marriage matter? *Demography, 32, 4*, pp. 483-507. doi:10.2307/2061670.

Waite, L. J. & Gallagher, M. (2000). *The case for marriage*. New York: Doubleday.

Submitted on/Eingereicht am: 01.06.2016
Accepted on/Angenommen am: 11.10.2017

Address of the author/Anschrift der Autorin:

Prof. Dr. Claudia Geist
Sociology Department
University of Utah
380 South 1530 East
Room 301
Salt Lake City, UT 84112
United States of America/Vereinigte Staaten von Amerika

E-Mail: claudia.geist@soc.utah.edu

Appendix

Appendix A: Baseline fixed effects models for the United States, Germany, and Sweden

US	Women		Men	
	Model 1	Model 2	Model 1	Model 2
Married	0.023		0.127**	
	(0.70)		(5.11)	
Recently Married		0.018		0.179**
		(0.54)		(6.95)
Constant	9.481**	9.485**	10.051**	10.006**
	(360.00)	(356.77)	(483.80)	(458.56)
Observations	16304	16304	17059	17059
Respondents	3390	3390	3339	3339
R-squared	<0.001	<0.001	<0.001	<0.001

Germany	Women		Men	
	Model 1	Model 2	Model 1	Model 2
Married	0.199**		0.505**	
	(4.29)		(22.39)	
Recently Married		0.203**		0.513**
		(4.29)		(22.51)
Constant	9.275**	9.272**	9.590**	9.583**
	(247.19)	(242.82)	(552.48)	(545.59)
Observations	12137	12137	17140	17140
Respondents	2603	2603	3336	3336
R-squared	<0.001	<0.001	0.04	0.04

Sweden	Women		Men	
	Model 1	Model 2	Model 1	Model 2
Married	0.101	0.026	0.028	0.055
	(1.11)	(0.22)	(0.39)	(0.66)
Cohabiting		-0.096		0.039
		(1.03)		(0.63)
Constant	11.914**	11.978**	12.252**	12.230**
	(227.45)	(147.24)	(333.96)	(239.04)
Observations	375	375	339	339
Respondents	264	264	242	242
R-squared	0.01	0.02	<0.001	0.01

Appendix B: Fixed effects models with individual level controls for the United States, Germany, and Sweden

	United States				Germany			
	Women		Men		Women		Men	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Married	0.019		-0.004		-0.110**		0.042*	
	(0.71)		(0.18)		(3.08)		(2.49)	
Recently married		0.004		0.013		-0.111**		0.041*
		(0.16)		(0.57)		(3.05)		(2.41)
Age	-0.029	-0.028	0.046	0.045	0.008	0.008	0.049**	0.049**
	(1.56)	(1.49)	(1.31)	(1.29)	(0.57)	(0.58)	(3.38)	(3.39)
Age squared	0.001**	0.001**	-0.001	-0.001	0.001**	0.001**	0.000	0.000
	(4.98)	(4.94)	(1.95)	(1.93)	(5.48)	(5.47)	(0.03)	(0.03)
Experience	0.055**	0.055**	0.061**	0.061**	0.028**	0.028**	0.047**	0.047**
	(5.02)	(4.95)	(2.96)	(2.94)	(2.71)	(2.71)	(3.71)	(3.71)
Exp. squared	-0.002**	-0.002**	0.000	0.000	-0.001**	-0.001**	-0.001**	-0.001**
	(7.56)	(7.55)	(0.39)	(0.41)	(5.76)	(5.75)	(6.64)	(6.64)
Tenure	0.005**	0.005**	0.003**	0.003**	0.009**	0.009**	0.002	0.002
	(17.39)	(17.38)	(11.28)	(11.28)	(3.91)	(3.91)	(1.16)	(1.16)
Ten. Squared	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	-0.000*	-0.000*
	(12.65)	(12.64)	(7.56)	(7.55)	(4.12)	(4.12)	(2.48)	(2.47)
Ann. work hrs.	0.059**	0.059**	0.031**	0.031**	0.038**	0.038**	0.021**	0.021**
	(72.01)	(71.98)	(43.78)	(43.77)	(45.00)	(44.98)	(38.19)	(38.19)
Number of Children	-0.062**	-0.062**	0.013	0.013	-0.030**	-0.030**	0.014**	0.014**
	(6.65)	(6.63)	(1.74)	(1.69)	(3.04)	(3.03)	(3.14)	(3.16)
Constant	7.678**	7.665**	7.791**	7.792**	7.156**	7.155**	7.178**	7.178**
	(24.50)	(24.45)	(14.95)	(14.95)	(28.35)	(28.35)	(26.74)	(26.73)
Observations	16304	16304	17059	17059	12137	12137	17140	17140
# of resp.	3390	3390	3339	3339	2603	2603	3336	3336
R-squared	0.39	0.39	0.21	0.21	0.45	0.45	0.52	0.52

Appendix B: Fixed effects models with individual level controls for the United States, Germany, and Sweden

Sweden	**Women**		**Men**	
	Model 1	**Model 2**	**Model 1**	**Model 2**
Married	0.040	-0.045	0.005	-0.043
	(0.49)	(0.44)	(0.10)	(0.71)
Private sector employment	0.323*	0.326*	0.205*	0.207*
	(2.47)	(2.50)	(2.30)	(2.33)
Cohabiting		-0.111		-0.070
		(1.38)		(1.46)
Age	0.108	0.092	0.114	0.130
	(1.08)	(0.92)	(1.70)	(1.92)
Tenure	-0.009	-0.012	0.025	0.023
	(0.54)	(0.71)	(1.84)	(1.66)
Age squared	-0.000	-0.000	-0.001	-0.001
	(0.33)	(0.17)	(1.01)	(1.19)
Tenure squared	0.000	0.000	-0.001	-0.001
	(0.19)	(0.34)	(1.09)	(1.01)
Education (in years)	-0.019	-0.023	-0.006	-0.003
	(0.78)	(0.95)	(0.37)	(0.20)
Weekly hours worked	0.015**	0.015**	0.019**	0.020**
	(4.04)	(4.13)	(6.23)	(6.40)
Number of children	0.010	0.011	-0.012	-0.006
	(0.23)	(0.25)	(0.49)	(0.27)
Constant	8.318**	8.750**	8.494**	8.167**
	(4.56)	(4.75)	(7.38)	(7.01)
Observations	375	375	339	339
Number of respondents	264	264	242	242
R-squared	0.35	0.36	0.53	0.54

Appendix C: Separate fixed effects models for men and women with low and high employment propensity scores (United States and Germany)

United States	**Women Low**		**Women High**		**Men Low**		**Men High**	
	Model 1	**Model 2**	**Model 1**	**Model 2**	**Model 1**	**Model 2**	**Model 1**	**Model 2**
Married	0.159		-0.060		-0.072		0.015	
	(1.37)		(1.39)		(0.93)		(0.33)	
Recent marriage		0.063		-0.028		0.044		-0.017
		(0.57)		(0.66)		(0.58)		(0.35)
Age	-0.020	-0.018	0.125	0.117	0.418**	0.410**	-0.071	-0.065
	(0.22)	(0.20)	(1.96)	(1.82)	(3.71)	(3.63)	(0.70)	(0.65)
Age squared	0.002	0.002	-0.001	-0.001	-0.006**	-0.005**	-0.001	-0.001
	(1.37)	(1.34)	(1.48)	(1.42)	(3.55)	(3.46)	(0.42)	(0.46)
Experience	-0.020	-0.018	0.022	0.027	-0.128*	-0.129*	0.175*	0.174*
	(0.42)	(0.38)	(0.58)	(0.71)	(2.28)	(2.29)	(2.34)	(2.32)
Exp. squared	-0.001	-0.001	0.000	0.000	0.004**	0.004**	-0.001	-0.001
	(0.54)	(0.55)	(0.00)	(0.01)	(3.24)	(3.23)	(0.69)	(0.69)
Tenure	0.007**	0.007**	0.003**	0.003**	0.003**	0.003**	0.001*	0.001*
	(6.11)	(6.08)	(4.33)	(4.33)	(2.90)	(2.88)	(2.34)	(2.34)
Tenure squared	-0.000**	-0.000**	-0.000**	-0.000**	-0.000	-0.000	-0.000	-0.000
	(4.61)	(4.59)	(2.98)	(2.98)	(1.08)	(1.06)	(1.05)	(1.07)
Ann. work hrs.	0.065**	0.065**	0.046**	0.046**	0.051**	0.051**	0.013**	0.013**
	(24.85)	(24.82)	(24.04)	(24.08)	(22.19)	(22.15)	(7.09)	(7.08)
Number of children	-0.087**	-0.085**	-0.022	-0.021	0.037	0.023	0.045*	0.046*
	(2.62)	(2.59)	(0.70)	(0.69)	(1.25)	(0.84)	(2.02)	(2.05)
Constant	7.054**	7.095**	6.077**	6.210**	1.694	1.764	10.537**	10.463**
	(4.99)	(5.02)	(6.09)	(6.18)	(1.01)	(1.05)	(6.71)	(6.67)
Observations	2710	2710	2788	2788	2786	2786	2794	2794
Number of respondents	1170	1170	1145	1145	1232	1232	1166	1166
R-squared	0.37	0.37	0.35	0.35	0.29	0.29	0.15	0.15

Appendix C: Separate fixed effects models for men and women with low and high employment propensity scores (United States and Germany)

Germany	**Women Low**		**Women High**		**Men Low**		**Men High**	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Married	0.515*		0.109		-0.126		0.012	
	(2.40)		(1.41)		(1.66)		(0.18)	
Recent marriage	-0.130	-0.102	-0.180	-0.170	-0.211**	-0.212**	0.096	0.100
	(1.73)	(1.38)	(1.85)	(1.76)	(3.27)	(3.31)	(0.83)	(0.86)
Age	0.003**	0.002*	0.001	0.001	0.001	0.001	-0.002**	-0.002**
	(2.69)	(2.40)	(1.35)	(1.28)	(1.57)	(1.66)	(3.25)	(3.40)
Age squared	0.039	0.030	0.230**	0.226**	0.262**	0.257**	0.143	0.147
	(0.89)	(0.68)	(3.13)	(3.07)	(6.05)	(5.95)	(1.29)	(1.32)
Experience	-0.001	-0.000	-0.003**	-0.003**	-0.002**	-0.002**	-0.001**	-0.001**
	(0.77)	(0.62)	(2.86)	(2.87)	(3.47)	(3.35)	(2.76)	(2.75)
Exp. squared	0.020*	0.021*	0.007	0.007	-0.001	-0.002	-0.011**	-0.011**
	(2.11)	(2.19)	(1.04)	(1.03)	(0.27)	(0.30)	(2.77)	(2.77)
Tenure	-0.001	-0.001	-0.001*	-0.001*	0.000	0.000	0.000	0.000
	(1.84)	(1.94)	(2.32)	(2.35)	(0.08)	(0.09)	(1.44)	(1.45)
Tenure squared	0.047**	0.047**	0.030**	0.030**	0.021**	0.021**	0.008**	0.008**
	(15.58)	(15.51)	(15.09)	(15.08)	(10.85)	(11.03)	(5.21)	(5.24)
Ann. work hrs.	0.037	0.046	0.052	0.054	0.008	-0.000	0.017	0.021
	(1.04)	(1.32)	(1.78)	(1.85)	(0.22)	(0.01)	(1.20)	(1.45)
Number of children		0.032		-0.034		-0.092*		0.035
		(0.45)		(0.60)		(2.07)		(1.41)
Constant	8.694**	8.660**	10.998**	10.872**	12.332**	12.300**	6.884**	6.832**
	(6.78)	(6.72)	(6.62)	(6.55)	(10.83)	(10.81)	(2.96)	(2.94)
Observations	1545	1545	2069	2069	1545	1545	2069	2069
Number of respondents	665	665	859	859	545	545	734	734
R-squared	0.39	0.38	0.36	0.36	0.38	0.38	0.51	0.51