

School of Economics and Finance

Do Marriage Markets Influence the Divorce Hazard?

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Working Paper No. 685

December 2011

ISSN 1473-0278



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Abstract

This paper demonstrates that a woman's propensity to separate from her husband or live-in partner depends positively on male wage inequality on her local marriage market - the more heterogeneous potential future mates are in terms of earnings power, the more likely a woman is to end her relationship. This effect is strongest for couples, where one has a college education but the other one does not. Because of the high degree of assortative matching according to education on the marriage market, college educated individuals are those most likely to marry a college graduate - if they are not currently married to one, they have the most to gain from divorcing and going back to the marriage market. This incentive becomes stronger if the college premium (the wage advantage college graduates enjoy over non-graduates) rises. The effect is robust to the inclusion of a variety of controls on the individual level, as well as state and time fixed effects and state specific time trends.

Keywords: Education, Inequality, Divorce *JEL Classification:* J12, J31, D31, I24

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1 Introduction

1.1 Motivation

Most of the economics literature on divorce concentrates on match quality - couples divorce because they either, with time, learn that the quality of their match is lower than they anticipated (e.g. Weiss and Willis, 1997), or because the match quality changes with time (e.g. Marinescu, 2011). But are couples at all influenced by their prospects for remarriage? That is, can it be shown empirically that women are more likely to divorce their husbands if their marriage market prospects change for the better? If we assume that women prefer men with a high earnings capacity (as shown by Hitsch et al. (2010) using a revealed preference framework) would they be more inclined to leave their husbands if more high wage men became available on the (re-)marriage market?

It has been shown before that male wage inequality influences the time young women spend "searching" for a husband (see Gould and Paserman, 2003; Loughran, 2002; Coughlin and Drewianka, 2011). This literature applies an idea coming from search theory to the marriage market: a woman's value of continuing her search for a husband increases as the heterogeneity of potential mates (as indicated by the larger dispersion in male wage rates) increases. These papers show that an increase in wage inequality in a locally defined marriage market is associated with a higher age at first marriage for women who live in this area. I apply this idea to divorce, emphasising the outside option, as captured by a an individual's remarriage prospect as a deciding factor in the decision to divorce (as opposed to the inherent quality of the the persons' current match).

Assume that individuals meet randomly on the marriage market, or are, at least at the time of the wedding, sufficiently influenced by factors other than their partners' education or earnings power - e.g. Chiappori et al. (2010) show that matching on the marriage market is influenced by physical attractiveness (as indicated by a low Body Mass Index), while Hitsch et al. (2010) show that looks matter to both genders when choosing a mate (these results pertain to dating however, and not marriage). Once the honeymoon period is over, an individual might assess the quality of the match, and his or her outside opportunities, more soberly. If the wage difference between men who are college graduates and men who are not is relatively small, a woman currently married to a man whose highest qualification is a high school degree is likely to find the prospect of divorcing her husband and spending some time searching for another partner not very attractive. But, if the wage premium associated with a college degree is sizeable, a woman who finds herself married to a man without a college degree might find it worthwhile to divorce and try her luck again.

I make this point more formally in Hye (2011). In this paper, I develop a model of the marriage market, were couples randomly meet and bargain over their individual consumption level once married. A person with a better education contributes more income to the

union than a person who is not well educated.¹ Bargaining takes the form of a Rubinstein - type alternating offers game with outside options - individuals can divorce and go back to the marriage market if they are unsatisfied with the offer their current partner makes. The value of this outside option - the expected utility of divorce and remarriage - is determined completely endogenously within the model, and depends on the quality of prospective spouses on the marriage market and the distribution of resources within other couples. If the education premium - the wage premium college graduates enjoy over non-graduates - is small, prospective spouses on the marriage market are relatively homogeneous. Even if one finds him- or herself attached to a spouse with little formal education, it is not worthwhile to divorce and search for another mate. If there is considerable wage inequality between college graduates and non-graduates, however, divorcing a less educated mate in the hopes of making a better match on the marriage market becomes a more interesting option. Although I do not directly test the model I propose in Hye (2011), it provides crucial theoretical insight into the relationship between (income-) heterogeneity on the marriage market and the divorce hazard that is the basis of this empirical work: it has the empirical implication, that, other things equal, the overall divorce rate could increase with the education premium (especially if the education premium is already relatively high).² This effect should, however, not be present for couples where both spouses have a college degree. The reason is that these couples, neither spouse can improve their situation by going back to the marriage market; they are already married to a person with the highest qualification, and therefore, nothing better awaits them on the marriage market. Individuals who are married to partners who do not have a college degree, however, have a higher incentive to dissolve their marriage and try to marry someone with more education, when the education premium increases.³

Since most of this theoretical argument is just about *earnings* inequality, one could argue that I should in fact look at the effect of overall wage inequality, rather than the return to education. Although I do repeat the analysis with a measure for overall wage inequality as a robustness check, I concentrate on the return to education for two reasons. First, it connects better to the model I develop in Hye (2011). Secondly, I want to investigate the differential effect of wage inequality on couples of different earnings power; when looking at couples of different educational attainment, using a between-group measure of inequality

¹In this model world, there are only two types of individuals - "educated" - which I understand to mean that they have a college degree - and "uneducated".

²In Hye (2011), the divorce rate depends not only on wage inequality but also on the proportion of men and women with a college degree; whether a change in the education premium influences the divorce rate or not is therefore ambiguous.

³In fact, in the model I develop in Hye (2011), only couples formed of two uneducated partners divorce in equilibrium. "Mixed" couples, where one partner has a college degree while the other does not, never divorce, but the educated partner can increase their share of household resources when the education premium rises.

is more consistent. Of course I could define cut-off points for low and medium incomes etc., but I think that education has a special appeal in the marriage market context: it is strongly correlated with income, easily observable, and most people marry when they have completed most of their formal education, while the full extent of a person's earnings power is typically only revealed later in life.

1.2 Aggregate Trends in Divorce and Wage inequality

It is a seldom noticed fact that divorce rates in the United States have been falling in recent decades. While marital dissolution was on the rise for much of the twentieth century, reaching a plateau in the late 1970s, the divorce rate in 2005 was at the level of the mid-1960s (Stevenson and Wolfers, 2007).⁴ The time period covered by my dataset (1990-2007) is therefore a period of overall declining divorce rates.⁵ This might be attributable to the decline in marriage rates that took place in the second half of the twentieth century - entry into marriage in the US is now at an all time low (Stevenson and Wolfers, 2007) - that increased the average match quality in marriages that did form.

During the same time period overall wage inequality has only risen moderately (Coughlin and Drewianka, 2011). My main indicator of wage inequality is the ratio of mean log weakly wages of full time working men with and without a college degree. Nationally, this education premium only rose from roughly 1.16 to 1.22 in the period 1990-2007 (although the development of the education premium differed significantly across states, even falling in some like New Mexico). That is, on average, a college educated man earned about 16% more than a man without a college degree in 1990; in 2007 the average difference was 22%, see figure 1.2.

My theoretical argument therefore postulates a positive relationship between two variables that, on the national aggregate, changed only mildly, and in opposite directions, during the period covered by my study - should we expect to see the same, negative relationship between the education premium and the divorce risk in the micro data? That is, should we expect women who are faced with a higher male education premium in their local marriage market to have a lower divorce risk, as is indicated by the macro trend?

I investigate this question using a dataset of married women and women with live-in partners from the Survey of Income and Program Participation (SIPP), 1990-2007.⁶ Exploiting disparities in the returns to education across US states and across time, I show that increases in the returns to college education for men are indeed associated with an

⁴The shift from a consent to a unilateral divorce law regime in many states, that has been associated with a rise in divorce rates (see, e.g. Wolfers, 2006, for a recent discussion) had already been completed at the beginning of my panel (1990).

⁵I do not produce a graph on divorce-rates because the SIPP is not nationally representative for individuals who divorce or separate (Westat and Mathematica Policy Research, 2001).

⁶I partly use the excellent clean CEPR extracts, (Center for Economic and Policy Research, 2011a).

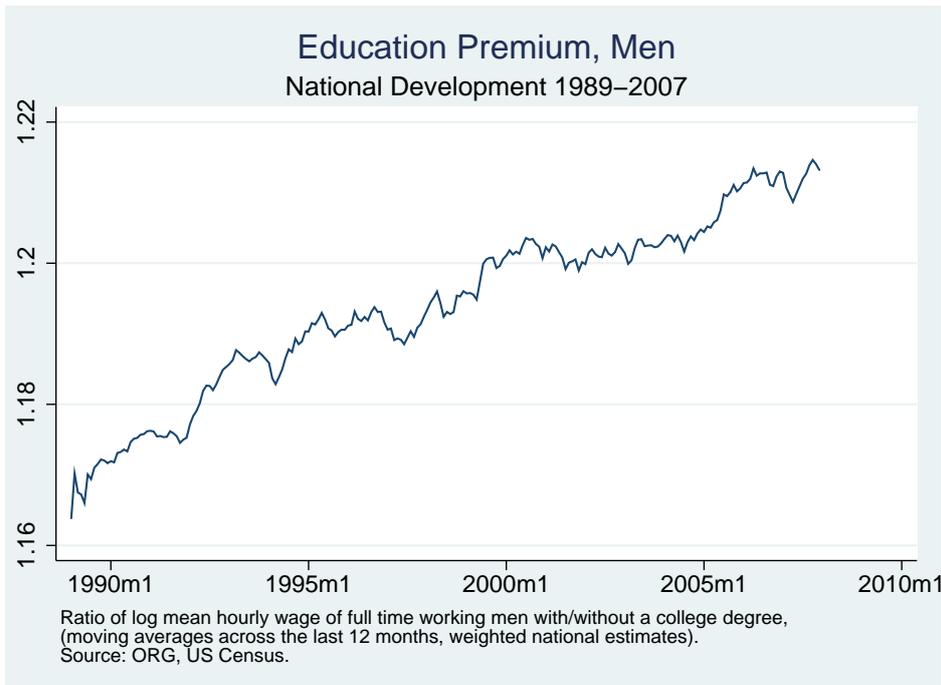


Figure 1: Male education premium

increased divorce hazard for young women. The effect is robust to the inclusion of state and year fixed effects, and even to the inclusion of state specific linear time trends, while controlling for a number of socio-economic characteristics that have been shown to influence match quality. Moreover, they are statistically more significant for couples in which at least one partner has no college education than for couples in which both partners are college graduates. In fact, the effect is most significant for couples in which one partner has at least some college education while the other one does not. Because of assortative mating on the marriage market, individuals with some college education or a college degree are more likely to marry a college graduate than those with less education. Therefore, women with some college education or a college degree who are married to a man whose highest educational attainment is a high school degree have the highest expected gain from going back to the marriage market in response to an increase in the male education premium.

The paper is structured as follows: in the next section, I briefly review the relevant literature on divorce, and the small literature on wage inequality and marriage rates. Section 3 describes the data sources I use, while section 4 discusses my estimation strategy. Section 5 presents descriptive statistics on the socio-economic characteristics of young married and cohabiting women in the United States from 1990-2007, as well as patterns of assortative mating according to education. I present the main results of my paper in section 6, while section 7 concludes. Section 8 provides the reader with additional detailed information on the complex SIPP survey design and how I construct many of the variables I use.

2 Related Literature

The seminal work on divorce is Becker et al. (1977), who also discuss the role of the remarriage market in the decision to divorce. Not surprisingly, they predict that the possibility to remarry after a divorce tends to increase marital instability. Unexpected developments in the traits and qualities of either spouse in the course of a marriage - "surprises", that were not anticipated at the beginning of the marriage - can have a stabilizing or damaging effect on a relationship. The direction of the effect depends on whether they increase or decrease the gains from the relationship, and whether and to which extent couples are able to reallocate these gains freely between them.⁷

Becker's theory of marriage emphasises the role of the division of labour in marriage (e.g. Becker, 1981). Consequently he argues that the effect of education on the probability of the dissolution of a marriage is ambiguous: on the one hand, a couple's joint education should increase the gains from marriage, because income tends to increase with education, on the other hand, gains from specialization between spouses decrease because highly educated women are more attached to the labour market.⁸ In the modern literature on the topic, the general finding is that education is a very important factor contributing to the stability of marriages (Weiss and Willis, 1997). This could be due to the changing nature of the gains from marriage over the past decades, away from household production to joint consumption.⁹ Using a novel approach, Lundberg (2010) provides evidence that the personality traits that are predictive of individuals sorting into marriage changed from cohorts born in post war years in Germany to younger cohorts born in the 1960s and 1970s: while for older birth cohorts, men and women were selected into marriage by differential traits indicating a division of labour within the couple, there is no gender difference in the traits that influence a person's likelihood to be married for the later cohorts, indicating that specialization within married couples lost significance.

As mentioned above, most of the empirical literature on divorce concentrates on learn-

⁷Say, a husband's earnings capacity is revealed to be higher than expected at the beginning of a marriage. In the absence of a remarriage market, this should strengthen the relationship, because it increases the gains to marriage (especially if the spouses are specialized in household and market tasks, it increases the gains to specialization). If however there is a remarriage market, such a development might raise the probability of marital dissolution - the husband might find that he could attract a more attractive spouse, and might find it worthwhile to try if his current wife can not shift more of the increased surplus to him.

⁸Becker et al. (1977) corroborate this conjecture in their empirical analysis.

⁹Isen and Stevenson (2010) argue that improvements in household production technology, that made time dedicated to household tasks less important in a couple's overall time budget, decreased the importance of specialization in household and market tasks. Families enjoy more leisure time and higher levels of consumption than in the 1950s and 1960s, which makes consumption complementarities between spouses and the joint consumption of public goods more valuable. They cite the increasing fraction of college educated women who marry, and increasing levels of assortative mating according to education in the marriage market, as indications of this shift in the source of the gains to marriage.

ing about, or shocks to, match quality. Weiss and Willis (1997) use data from the high school class of 1972 (that is, the birth cohort of 1954) to show that "earnings-surprises" (revealed earnings that differ from what was a reasonable expectation at the time of marriage) influence the divorce hazard. If the husband's earnings exceed the expected value at the time of marriage, it stabilizes the union; while higher than expected earnings of the wife tend to increase the divorce hazard. This corroborates the theory that specialization within marriage played a role in divorce for this generation. Also, Charles and Stephens (2004) test the "surprises" theory of divorce by examining the effect of negative earnings shocks on a couple's likelihood to separate. They look at negative earnings shocks following three types of job losses: lay-offs, redundancies and negative health shocks (disabilities). They show that lay-offs are the only type of earnings shocks that increase the divorce hazard - lay-offs seem to convey more information about the future earnings capacity to a spouse.¹⁰

Using the same dataset as I do in this paper, the 1990-2004 panels of the SIPP, Marinescu (2011) investigates the relative importance of learning about match quality versus real changes in match quality on the probability of divorce. She finds that learning seems to be relatively unimportant as a determinant for the divorce hazard.¹¹ She does however find support for a model of changes in match quality, again using job loss as an example of a wage shock.¹²

The literature most closely related to this paper is the literature on wage inequality and age at marriage, because it is, to my knowledge, the only literature that explicitly addresses the "marriage market side" of marital behaviour. Gould and Paserman (2003) use male wage inequality to explain changes in the proportion of single females ages 21-30 over time in different metropolitan areas, using data from the 1970, 1980 and 1990 1% Public use micro data sample (PUMS) of the US census. They show that women do delay marriage

¹⁰Also, divorcing a spouse who lost their job because their factory closed down, or because they became disabled, is likely to be met by judgement from family and friends, while leaving someone who got fired is more likely to be accepted by society.

¹¹Specifically, she finds that the divorce hazard does not approach zero even after twenty years of marriage (at which point she argues that one should know everything that is important about the quality of a match).

¹²She finds that job loss is associated with an increase in the divorce hazard, and that this effect becomes stronger with relationship duration - the effect of a husband being laid off increases the divorce hazard more for couples who have been married for a while than for new couples. She conjectures that this is because the quality of a relationship tends to decrease over time, leading to a higher proportion of "fairly mediocre" relationships at higher marriage durations. Being at risk of dissolution anyway, these relationships are more likely to be pushed over the brink by an adverse earnings shock. She confirms this hypothesis by using marital happiness data from the National Survey of Families and Households. She finds that wives do become less likely to report the highest level of happiness with their marriage the longer they are married, which, in itself, is an argument against a learning-about-match-quality model of divorce.

in response to increasing male wage inequality - that is, increasing heterogeneity on the marriage market. They estimate that rising male wage inequality can explain between 18% and 28% of the decline of the marriage rate of young women between the ages of 21 and 30. Using the same dataset, Loughran (2002) looks at the influence of male wage inequality on female marriage rates within educationally, racially and geographically segregated marriage markets, thus taking into account assortative matching on the marriage market. He also finds that increased male wage inequality is associated with women remaining single for longer. His results attribute between 7% and 18% of the decline of the marriage rate of young white women (ages 22-30) between 1970 and 1990 to increased male wage inequality.

In a recent paper, Coughlin and Drewianka (2011) investigate the effect of wage inequality on aggregate marriage rates. In contrast to Gould and Paserman (2003) and Loughran (2002), and relevant to my study, they also consider somewhat older women.¹³ While they are able to replicate the results of Gould and Paserman (2003) for young women (younger than 30) for the time period 1970-1990, they find that the effect diminished significantly in the time period 1990-2005. They conclude that slower growing wage inequality during this time, coupled with a relatively old "single pool" (a consequence of low marriage rates in the preceding decades) led to a weakening of the link between marriage rates and male inequality.¹⁴

My contribution to the literature on wage inequality and marriage consists in showing that heterogeneity of potential partners on the marriage market not only influences young singles, but also married women - my results indicate that women in existing relationships do keep an eye on the marriage market. I add to the literature on divorce by emphasising effect of the *aggregate marriage market situation* a woman faces in her geographical environment at a given point in time. Although I do control for factors commonly associated with the quality of the match in the empirical literature, my focus is not on changes of, or shocks to, match quality on the couple level, but changes in the quality of potential candidates on the relevant marriage market.

¹³One of their dependent variables is the aggregate marriage rate by state, defined as the number of marriages in a given state and year, gathered from administrative sources, and divided by the single population younger than 55.

¹⁴They argue that, although it has been shown that (young) women delay marriage in response to a more heterogeneous supply of young men on the marriage market, most women get married eventually. Consequently, male wage inequality should diminish in significance for a woman's decision to marry as she grows older. Since wage inequality has been increasing from 1970-1990, the time period covered by Coughlin and Drewianka (2011) and Loughran (2002), women delaying marriage would lead to a fall in the aggregate marriage rate in the short run, but it would be expected to pick up again once the pool of singles becomes "old enough".

3 Data

3.1 Marital History and Socio-economic Background

I use data from the 1990 - 2004 Panels of the Survey of Income and Program Participation (SIPP).¹⁵ The SIPP is a panel study that follows a nationally representative sample of the US population for three to four years. At the end of each panel the sample is dropped and a new one is drawn. The periodicity of the SIPP is monthly; interviews are conducted four times a year, covering the previous four months. The main reason why I chose this survey is its size - contrary to public perception, divorce is actually a quite rare phenomenon. Since I want to exploit variations between states and over time, I need a large sample. Furthermore, I need detailed and reliable information on the exact timing and locality of a divorce or separation. Other popular micro datasets (like the PUMS and the CPS) do not provide information on the timing of divorce, only of current marital status;¹⁶ while other panel studies, like the National Longitudinal Study of Youth, do not provide geographical information, or are too small to analyse divorce (like the British Household Panel Survey).

The SIPP contains rich information on personal characteristics (race, marital history, fertility, family composition etc.), and, because it is a household panel survey, it contains matched data on married couples, and information on unmarried couples living together (from 1996 onwards). The SIPP also provides detailed information on labour force participation, wage rates and income, both on the person as well as on the household level.

I only keep observations of women who are married or cohabiting with a partner during the reference period. A woman who was single at the beginning of the reference period enters my sample upon her marriage or when she begins living with a partner; conversely, a woman who was married or had a live-in partner at the beginning of the reference period shows up as divorced or separated in the first month she reports her marital status as divorced, or is no longer living with her partner, and then exits the panel. If a divorced woman remarries or is reported to have a live-in partner at a later point during the panel, she re-enters my sample. Of course, a cohabiting relationship can end in marriage; these women remain in the panel.

My sample contains data on 125,074 women who were married or cohabiting during the panel - the large majority of these women, 116,723, are married.¹⁷ The maximum period

¹⁵In part from Center for Economic and Policy Research (2011a).

¹⁶It is not a problem for Gould and Paserman (2003) that their dependent variable, the proportion of single women in a given metropolitan area, is a stock, because married women of in this age group must have taken their marriage decision quite recently, in an economic climate very similar to the one in the census year. Knowing when exactly they got married (information that is not asked in the US census) is therefore not vital.

¹⁷Marinescu (2011) also uses the SIPP for all to date available panel years, and reports only having data on 93,505 marriages. This is very likely due to the fact that, using a Cox proportional hazard model, she needs data for both spouses at each point in time, and therefore cannot use the observations on women

of time I observe any woman is 47 months or a little under four years; on average, I observe a woman for 33 months. Of these women, 6.927 or 5.5% dissolve their relationship during the reference period, 4.927 divorce and 2000 terminate their cohabiting relationship.

3.2 Wage inequality

The SIPP is not designed to be representative at the state level (Westat and Mathematica Policy Research, 2001). Because I am interested in the effects of marriage market characteristics, I use monthly earnings data from the Outgoing Rotation Group (ORG) of the Current Population Survey (CPS) to calculate wage inequality indicators at the state level.¹⁸ Because I am especially interested in the role of education, I use the ratio of the means of the log weekly wages of educated and uneducated men in each state as my basic measure of wage inequality.¹⁹ As "educated" I count men who hold a college degree or higher qualification, as "uneducated" men who do not (this includes men who report their highest educational attainment to be "some college"). In order to limit these measures more closely to presumable "marriage material" for women ages 18-45, I only consider the mean wages of men ages 18-50 (see section 8 for details).

I use the weights of the CPSORG to calculate the sex-ratio of a cohort of "marriageable age" at the state level - the ratio of the number of males between the ages of 18 and 45 to the total population of that age group - as well as the ratio of that age group who hold a college degree, by sex.

There is seasonality in the wage data. I deal with this by regressing my measure of the education premium on a full set of month/ state interactions (thus allowing the seasonality to vary by state), and using the residual wage inequality that can not be accounted for by pure monthly variations; the R-squared of this regression is 0.23. Another way to deal with the seasonality would be to use the raw data, and directly control for the month of the observation in the regression - this does not allow the seasonality to vary at the state level. I ran the base regressions using the raw education premium and a full set of month dummies, these results are available upon request. The coefficient estimates of the regressions do not differ from those I present here, but the standard errors become smaller - the approach I use is therefore more conservative.²⁰ For the sake of consistency, I also

who report their spouse to be absent; she also makes more use of the SIPP's labour market data that has more missing values than the socio-economic data I use.

¹⁸The ORG is designed to be representative at the state level (Center for Economic and Policy Research, 2011b).

¹⁹Gould and Paserman (2003) use the standard deviation of the log weekly earnings of full time male employees, but their study does not focus on education like mine.

²⁰ An obvious alternative would be to use yearly averages, as do Gould and Paserman (2003); Coughlin and Drewianka (2011) and Loughran (2002). This poses the conceptual problem that, since my data is monthly, it would incorporate anticipated shocks by calendar year. Also it saturates the regression when I control for state specific linear time trends in addition to state and time fixed effects.

deseasonalise the other marriage market indicators in the same fashion.

3.3 Geography

Unfortunately, the smallest geographical entity the SIPP identifies is the state, so I have to consider the state as the relevant local marriage market.²¹ The 2004 panel identifies fifty states and the District of Columbia, the 1996 and 2001 panel identify forty-five states and the District of Columbia, while the pre- 1996 panels only identify 41 states and the district of Columbia. The states that are not identified are small states that are combined into groups to safeguard respondents' privacy. I therefore have to exclude observations from Maine, Vermont, Iowa, North Dakota, South Dakota, Alaska, Idaho, Montana and Wyoming.²² I do not lose too many observations this way however: in my dataset, only 2,274 married women were from these states.

4 Empirical Methodology

I am interested in the effect of male wage inequality - more precisely, the wage advantage college educated individuals enjoy over those without a college degree - on the probability that a woman divorces in a given month. The basic empirical specification is:

$$P(d_{ijt} = 1) = \Phi(\alpha \cdot Ineq_{jt} + \beta \cdot Ineq_{jt} \cdot Couple_educ_{ijt} + \gamma \cdot Z_{ijt} + \delta \cdot Y_{ijt} + \eta_{jt}), \quad (1)$$

where $d_{ijt} = 1$ if married woman i , residing in state j got divorced in month t and 0 otherwise, Φ is the functional form that determines the relationship between the dependent and independent variables (in the case of the probit model, the standard normal distribution), $Ineq_{jt}$ is a measure of wage inequality in state j at time t , $Couple_educ_{ijt}$ is a dummy variable indicating the joint educational level of husband and wife, Z_{ijt} is a vector of controls for the local marriage market j in which woman i lives at time t (see below), X_{ijt} is a vector of a couple's individual characteristics and of characteristics that indicate the quality of the match (this includes a full set of joint education dummies that I allow into the regressions *without* interaction with the inequality measure), and η_{jt} is a state-time effect,

²¹Coughlin and Drewianka (2011) also use the state as the relevant local marriage market. They are able to replicate previous estimates of the link between age at marriage and male wage inequality closely, which indicates that treating the state as the local marriage market does not introduce a worrying degree of measurement error.

²²It is tempting to treat the state groups of the SIPP as local marriage markets. The problem is that the groups are not the same across panels: only the Main/ Vermont group stays the same, and it is difficult to justify that these two states should form one marriage market when there is New Hampshire wedged between them that is treated as one marriage market. In the 1996 panel, the two Dakotas and Wyoming could pass as a marriage market, but in the pre- 1996 panels Idaho and Alaska are added to this group, so this again seems arbitrary from a marriage market perspective.

that influences individual divorce probabilities independently of individual characteristics and wage inequality at the state level.

My main measure of wage inequality is the ratio of the mean log hourly wage rates of full time working men who are university graduates to the mean of those who are not, at each time t , and state j . This indicator measures the mean wage premium university graduates enjoy as compared to those who do not hold a university degree.²³

In addition to including an *overall* effect of the male education premium in the model, I allow the effect of the male education premium to vary with the joint educational attainment of the couple. The first term of equation (1) measures the direct effect of the level of wage inequality on the divorce hazard, while the second term measures whether this effect varies across educational groups. The theoretical prediction of the model I develop in Hye (2011) is that the effect of changes in the male wage premium on the divorce hazard should be stronger for couples in which at least one partner is not a college graduate (because women married to college graduates cannot improve on the education of their spouse on the marriage market). It would therefore support my hypothesis if there was little or no *overall* effect of the male education premium, but if the effect only worked through the interactions with the education dummies. I verify this by also running a base regression excluding the interactions of the education premium with the the education dummies. As a measure of a couple's education, I use the same three dummies Marinescu (2011) uses in her study with the same dataset: a dummy equalling one if either both spouses' highest degree is high school, or if one of the spouses is a high school graduate while the other one is not; a dummy indicating if one of the spouses' highest qualification is high-school, while the other spouse has some college education or more (a college degree or a post-graduate degree); and a dummy equal to one if both spouses have some college education or more. The base is a couple were both partners did not successfully graduate from high school. Also couples for which the educational attainment of husband and wife differ widely are not captured by either of these dummies - these are couples were one partner is a high school drop-out while the other partner has some college education or a higher qualification. Due to assortative mating on the marriage market, these couples are very rare (see table 1), only 3.5% of my sample belong to this category.

I proxy the local marriage market conditions by the sex ratio of the relevant age group in a given state, by the proportions of men and women of that age group who hold a college degree, and the mean hourly wage rate of full time working men.

Equation (1) does not include a term controlling for the latent quality of the match. Although my dataset is a panel study, I do not actually take advantage of the panel design in my analysis. Using panel data techniques (like individual fixed effects) would restrict my sample to women who separate multiple times during the panel reference period, which

²³I use hourly wages to abstract from labour supply decisions, see section 3.2 for details on the calculation of this measure.

would be a very special sub-population of women, especially given that the maximum duration of each panel is four years.²⁴ Instead, I control for marriage specific match quality with control variables that are likely to influence the quality of the match, an approach also used by Weiss and Willis (1997) and Charles and Stephens (2004). I use the same controls as Marinescu (2011), with some slight modifications. I include cohabiting couples in my main sample (from 1996 onwards) and include a dummy variable for cohabiting relationships. My results are very similar if I exclude cohabiting relationships, and all of my main variables of interest retain significance without them, but they do provide me with extra variation because of their higher propensity to dissolve.

Marinescu (2011) controls for the number of a couple's own children interacted with the duration of their relationship - this specification limits cohabiting partnerships to those that started during the panel reference period, because the start date of a relationship is only asked for marriages; which excludes most cohabiting relationships from the analysis. This is sensible in the context of her modelling approach, because she uses a Cox proportional hazard model where the divorce hazard depends on the relationship duration. Because this is not true in my set-up, I just control for the number of own children in the household.²⁵ Furthermore, I follow her lead in including a dummy indicating if there are other children under the age of eighteen living in the household, a dummy indicating if there is an age-difference larger than five years between the spouses, a dummy indicating if the man is white, and a dummy indicating if the partners share the same racial background. I also add a dummy indicating if the couple are home-owners, and the number of times previously married for both partners. I also control for the age of both partners - Marinescu (2011) does not, but she controls for relationship duration, which is strongly correlated with the partner's ages.

With regards to the exact specification of the state-time fixed effect η_{jt} , I follow Gould and Paserman (2003) in running several regressions of varying degrees of conservativeness.

I want to make a short comment on the estimation of standard errors. Since my main variables of interest, the marriage market indicators - especially the male education premium, the sex-ratio, and the proportion of university educated men and women - vary at the state- and not the individual level, current practice in the applied micro-econometric literature would indicate that I cluster the standard errors at the state level. Ignoring the group structure of the data can lead to serious underestimation of the sample variances (e.g. Angrist and Pischke, 2008). The dataset I use (the Survey of Income and Program Participation) is based on a complex survey design I describe in some detail in section 8.1. In a nutshell, the SIPP is not a random sample of the US population, but has a

²⁴The panel structure of the dataset is important for me since it enables me to observe the exact time when women transition in and out of relationships.

²⁵I have however estimated the same model with the interaction of relationship duration - hence with only a very small number of cohabiting couples in the sample - and arrive at very similar results.

multi-stage stratified sampling design - the US territory is divided into *strata*, and groups of counties or independent cities are sampled within those strata (these are the primary sampling units, or PSU, of the SIPP). The US Census Bureau provides recommendations for the estimation of robust standard errors under this complex survey design. It is not possible to cluster the standard errors at the state level *and* follow the Census Bureau guidelines, since those cluster the standard errors at the smaller primary sampling units, which can cross state lines (Siegel and Mack, 1998). Because clustering the standard errors at the state level disregards between-PSU-variation, the estimated standard errors are actually *smaller* when clustered at the state level, as opposed to according to U.S. Census Bureau recommendations. This only influences the standard errors, and not the point estimates of the coefficients in the probit regressions. The differences between the two specifications are small (at most one percentage point difference in the p-value of the t-statistic of the probit regression), and do not cause any of my main variables of interest to become insignificant. Also, on an individual level analysis, the effects of the survey design on the variances should not be too important. Working with the same dataset, Marinescu (2011) completely disregards the SIPP survey design and treats it as a simple random sample (since hazard models are difficult to estimate using weights). I ran all regressions I discuss here both clustering the standard errors at the state level, and following US Census Bureau guidelines. I present the regressions with the standard errors clustered according to US Census Bureau recommendations because they are more conservative (that is, the standard errors are larger).

5 Descriptive Statistics

5.1 Marriage and Divorce in the SIPP 1990-2004

I do not include same sex relationships in my sample because the marriage market implications of such relationships are unclear: in the case of women, it is difficult to construct a good measure of female wage inequality because of non-participation in the labour force and part time work. For men, reverse causation would be an issue, because men in same-sex relationships "fish in the same pond" for potential partners.

The SIPP has several categories of marital status: it distinguishes between individuals who are married with a present or absent spouse (absent spouses are either institutionalized, mostly in the correctional system or the armed forces, or permanently away from home for reasons *not* connected to marital problems). I subsume these categories under "married". However, absent spouses do not have any data on personal characteristics. Whenever I observe them living at home at some point in the panel, I extend their relatively permanent characteristics (education, race etc.) over the period in which they are reported as being absent; if I never observe them living at home, their data is missing.

The SIPP has a marital status category "separated", meaning an individual lives permanently away from his or her spouse due to marital problems. I count these individuals as "divorced".²⁶

Note that, for the sample statistics in this section, I use the SIPP weights, to account for the survey design - these statistics are therefore representative for the sub-population of married women in the US. Of the 125,074 women in my sample, I observe 9,149 transition into marriage (either from being single, widowed or divorced). The mean age at marriage is 32.2 years, 26.3 years for first marriages. Figure 2 depicts the age distribution of women in the month they transition into marriage (note that is for first and higher order marriages). More than eighty percent of all women are below the age of 45 when they get married.

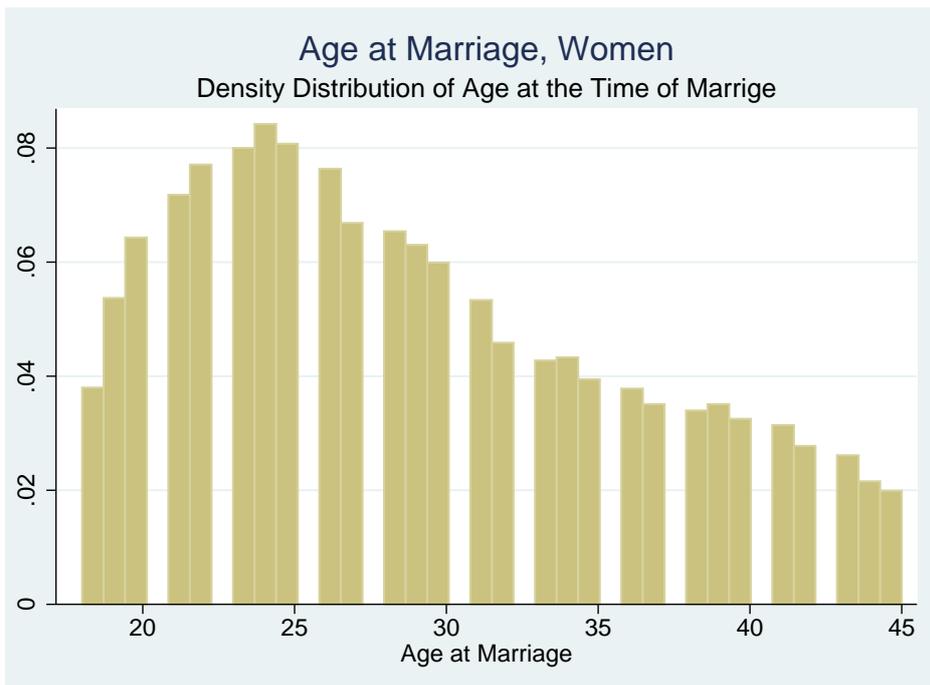


Figure 2: Age at Marriage

The overall divorce hazard across the observation period for the women in my sample is 7.5%.²⁷ By education, the divorce hazard is highest for couples where one spouse has a high school degree while the other spouse does not (9.6%), even higher than for couples where both spouses are high school drop-outs (7.7%). Couples where one partner has a high school degree and the other a higher qualification have a probability of 8.1% to split up during the observation period, and couples where both have at least some college have the lowest divorce hazard at 5.1%. The divorce hazard according to education is therefore not linear,

²⁶Also Charles and Stephens (2004) treat legally married couples who live in different households as divorced couples.

²⁷This is higher than the sample statistic I reported earlier because it incorporates the sample weights. That is, this figure is more indicative of the incidence of divorce in the US population, while the percentage in section 3 was calculated directly from the sample.

with college education clearly protecting against divorce.²⁸ The age distribution at the time of divorce is similar: women’s mean age in the month of divorce is 36.4 years; around 82% of all women are below the age of 46 when they decide to end their marriage. If we also consider women who separate from their live-in partner, the mean age at the time of separation slightly decreases to 35.1 years (see figure 3). Note that 44% of all women in my sample are older than 45. Because of the concentration of these marital events at quite young ages, I conclude that for women who get divorced later in life, remarriage prospects are not as important. I therefore limit my analysis to women aged 46 and younger.²⁹ The rest of the summary statistics in this section pertains to this young sub-sample of married and cohabiting women.

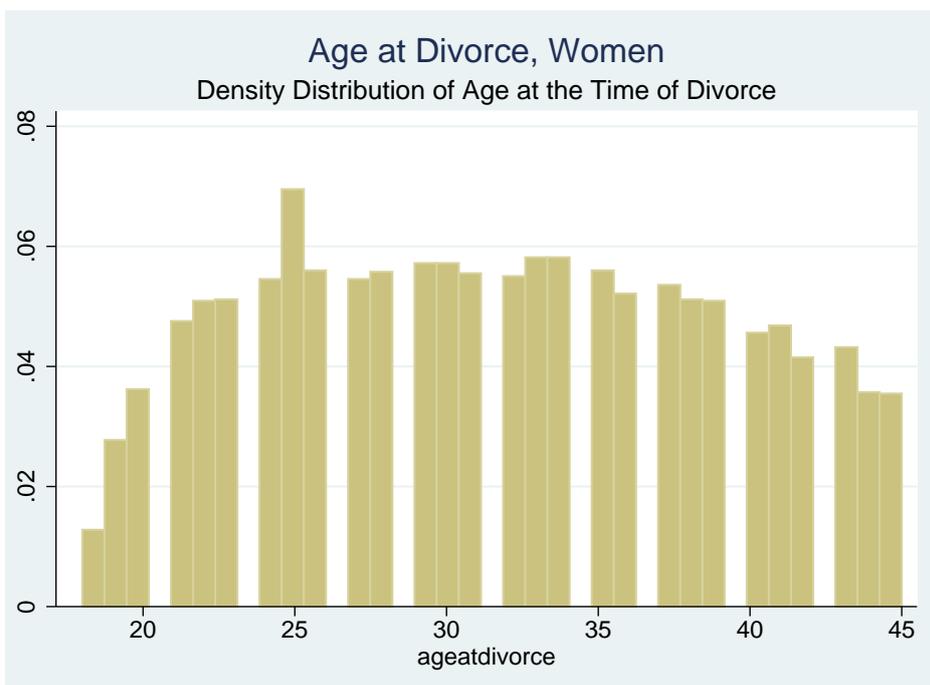


Figure 3: Age at Divorce or Separation from Live-In Partner

5.2 Assortative Mating

Table 1 depicts the educational composition of married and cohabiting couples in the dataset. The restriction to young women (below 47 years of age) pushes the distribution of the highest educational degree to the favour of women: fewer women than men do not

²⁸Using the marital history files of the 2004 SIPP, Isen and Stevenson (2010) find that individuals with some college education, but without a degree, are the most likely to divorce. Note that my summary statistics pertain to the period 1990-2004, and I only look at women 46 and younger who divorce during the sample period, while Isen and Stevenson (2010) look at the marital history files, and therefore at all divorces sample members went through at any point in the past.

²⁹Coughlin and Drewianka (2011) limit their analysis to women 55 and younger.

have a high school degree, more women do have a high school degree or some college. It is a phenomenon of the last two decades that women tend to be better educated than men, (see Goldin et al., 2006). This is however for the whole sample period, thus disregarding trends over time. The table shows clearly the tendency to assortative matching of couples according to education, with the highest proportion of couples located along the diagonal.

Table 1: Educational Attainment of Married Couples

Wife's Education	Husband's Education				Total
	less than high school	high school	some college	college graduate	
less than high school	0.059	0.032	0.012	0.002	0.105
high school	0.039	0.159	0.077	0.029	0.305
some college	0.017	0.087	0.141	0.072	0.317
college	0.003	0.027	0.061	0.182	0.273
Total	0.119	0.305	0.291	0.286	1

Weighted proportion estimates, calculated from women/ month observations (takes into account qualifications acquired during the sample period), women aged 46 and younger, 1990-2007

Figure 4 depicts the evolution of assortative mating patterns over time. It indicates two main developments over the last seventeen years: first, later cohorts are better educated than earlier ones; the fraction of couples in which both husband and wife are college graduates has increased, while the fraction of couples formed of two individuals who do not have a high school degree decreased. Second, the fraction of couples where she is more educated than he is increased. With time, there are more couples where she has some college education while he has a high school degree, or where she is a college graduate while he has only some college, at the expense of couples where the reverse holds. This is a consequence of women overtaking men in terms of educational attainment (Goldin et al., 2006). This distributional chart is interesting because it gives an idea about the relative frequency of educational pairings in the below probit regressions.

5.3 Married vs. Cohabiting Couples

Cohabiting relationships are very different from marriages. They are shorter lived - out of the 7,421 cohabitations I observe, 3,622 (48%) end during the reference period; 1,846 (24%) end in marriage and 1,776 (23%) in separation. In contrast, only 4,170 (5.6 %) out of 74,944 marriages I observe end in divorce. Married couples are more likely to have

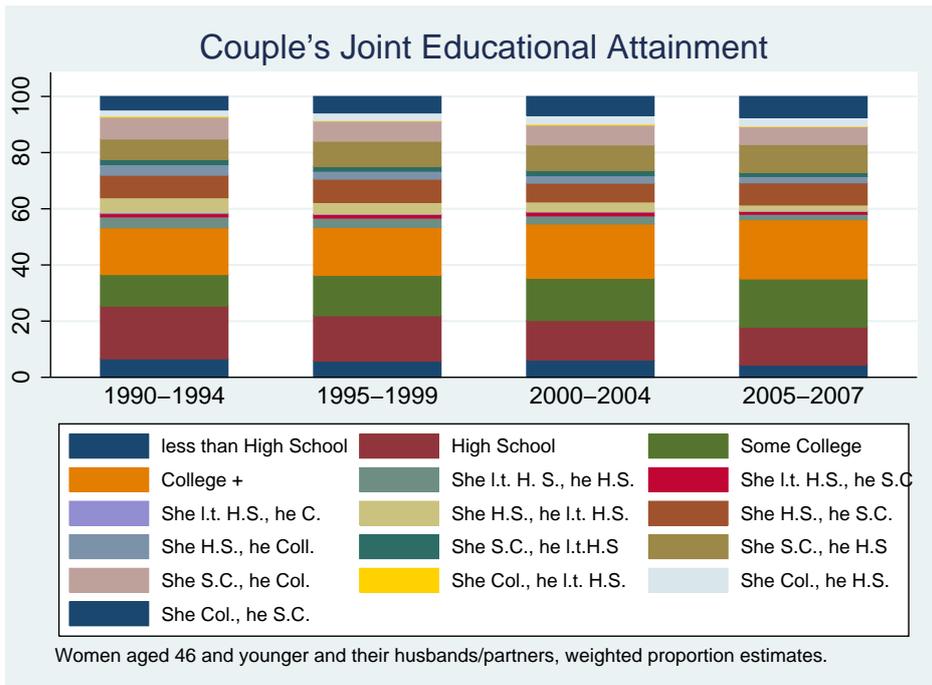


Figure 4: Joint educational attainment as a percentage of all couples, 1990-2007.

children, 73% of married women in my sample have at least one child of their own living in their household, while only 30% of cohabiting women do.

There is selection into cohabitation: cohabiting couples are younger than married ones, with a mean age of 31.1 as compared to 35.5 (recall that these are statistics for women 46 and younger only). African Americans are more likely to cohabit than whites, see table 2.

Table 2: Racial Background of Married and Cohabiting Women

	Married Women	Cohabiting Women
White	0.74	0.72
African American	0.07	0.09
Hispanic	0.13	0.14
Other	0.06	0.05

Weighted proportion estimates, women 46 years and younger, 1990-2007.

The educational attainment of those living in unmarried partnerships is markedly lower than that of married couples, see figures 5 and 6. Most strikingly, only 16% of cohabiting men in my sample have a college degree, while 29% of married men do. This is consistent with other studies for the United States - e.g. Brien et al. (2006) use the National Longitudinal Study of the High School Class of 1972, who were followed until 1986. They also

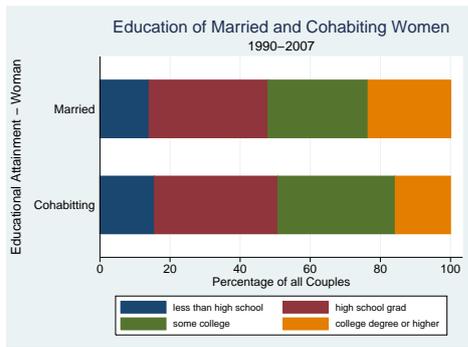


Figure 5: Weighted proportion estimates, women aged 46 and younger.

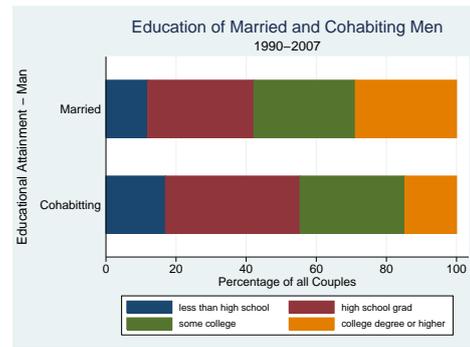


Figure 6: Weighted proportion estimates, partners of women aged 46 and younger.

find that only about 20% of cohabitations are still intact after three years, with an end defined as either formal marriage or separation. Similarly, Stevenson and Wolfers (2007), using data from the National Survey of Family Growth, report that more than half of all couples who were cohabiting in 1997 had split up five years later, while only a quarter had married by this time.

Given this obvious differences between married and cohabiting relationships, it could be argued that I should exclude cohabitations. Because their separation risk is so much higher than for marriages, they do however provide a lot of additional variation. In the probit regressions, I control for cohabitations with a dummy variable. I also ran all probit regressions separately for a sub-sample including only married couples. My main results are robust to the exclusion of cohabitations (except for the interaction of the inequality measure with the highest educational group, see below).

6 The Effect of the Marriage Market on the Divorce Hazard

I run probit regressions on the probability that a married woman divorces or a woman with a live-in partner dissolves her relationship in any given month, according to equation (1). As mentioned above, I run all regressions twice, once allowing for clustering of the standard errors according to recommendations issued by the US Census Bureau, and once clustering the standard errors at the state level.³⁰ The clustering of the standard errors does not influence the coefficients, and the difference in standard errors between the two specifications is is very small. I report the results from the regressions using clustering according to US Census Bureau recommendations because they are more conservative (i.e. the variables of interest are significant at higher p-values). I report the results of these regressions in table 3.

³⁰See also section 8.1 for a detailed discussion of this point.

When modeling the state-time effect η_{jt} , I follow Gould and Paserman (2003) in their stepwise approach. My first specification only includes year fixed effects. The pure time fixed effects specification takes advantage of all the regional and time variation in the male education premium, only controlling for US-wide trends in attitudes towards marriage and relationships, changes in household production technology, housing prices, or other developments that might be correlated with the education premium and aggregate divorce rates at the same time. The results of this first regression are reported in the second column of table 3.³¹

Here, the male education premium has an overall negative effect on the divorce hazard, that is significant at the 5% level. This is at odds with the implications of the model I develop in Hye (2011) - I would expect the difference between the wages of university graduates and non-graduates to work through the interactions with the dummies indicating the joint educational attainment of the couple, and I would want this effect to be positive. This result is however due to the education premium picking up some unobserved heterogeneity at the state level, since the effect is not significant anymore once state fixed effects are introduced (columns three and four of table 3). Also the fraction of men of a "marriageable age" who are college graduates is significant in this specification, but this also vanishes once state fixed effects are introduced. The same is true for the mean wage of full time working men.

My main variables of interest, the interactions of the male education premium with the dummies for the couple's joint educational attainment, are significant at the 5% level for the lower two education groups, while the interaction for couples where both have at least some college education is not significant at the 10% level. This is in line with my story: because individuals in these couples are already married to someone with a college education, they cannot expect to improve their situation by going back on the marriage market and marrying someone more educated. Therefore, changes in the returns to college education should not influence their propensity to divorce. For the other two groups, this effect is positive and significant - at the 5% level for those with at most a high school degree, and at the 2% level for the middle group of couples (one partner is high-school graduate, and the other has at least some college education). Thus, quantitatively, the interaction of the education premium has the biggest influence on couples who are in the centre of the education distribution. Because there is assortative mating in the marriage market, those with a college education are the most likely to marry a college educated spouse. Hence, the expected value of divorcing and going back to the marriage market should be the greatest for those who have a college degree, but are married to a partner with only high school education.

One might worry that the education premium, varying at the state level, picks up

³¹I report the coefficients associated with the covariates in the probit regressions. Note that although these are not marginal effects, their signs indicate the direction the covariates influence the divorce hazard.

unobserved state characteristics that influence divorce rates. I therefore estimate another regression including state and year fixed effects. State fixed effects control for omitted variables that influence divorce rates, vary between states and are constant over time. Note that this means disregarding all cross-sectional variation in divorce rates and returns to education across states. I report the results of this specification in the third column of table 3. The overall effect of the education premium is not significant at the 10% level any more, indicating that it indeed picked up some state-inherent characteristics that influence both the returns to education and the divorce rate, corroborating my hypothesis. In this specification the proportion of women with a high school degree becomes significant at the 6% level; it has a positive effect on the divorce hazard. I am careful to interpret this result as a marriage market effect, since it could simply indicate that more educated women are more likely to leave an unsatisfying marriage because of their better economic standing. The coefficients and significance levels of the interaction of the education dummies with the education premium only change very slightly for the first two groups. For the highest educational group, the coefficient turns significant at the 10% level when controlling for state fixed effects, though.

Finally, it could be the case that changes in the male education premium are correlated with *changes* in omitted variables at the state level, that also influence divorce rates. Again, following Gould and Paserman (2003), I deal with this by introducing state specific linear time trends (in addition to state and year fixed effects). This random growth specification is conservative, since it not only throws away all between states cross sectional variation, but also all within state trends in the returns to education and divorce rates. All remaining variation comes from *deviations* in the education premium and the divorce rate from a state specific linear time trend. Results for this specification are listed in the fourth column of table 3. The proportion of educated women remains significant at the 8% level. The coefficients associated with the interaction of the returns to education with the education dummies only decrease very slightly in comparison to the regression without random growth specification for the first two educational groups, and the probability scores associated with them increase only very slightly (the interaction term is significant at the 9% level for the "worst" educational group, and at the 3% level for the middle group). It is good news for my story that the p-value associated with the interaction with the highest educational group is just about 10% when state specific time trends are included.

Table 3: Probit for divorcing this month

	Pooled	State fixed effects	State fixed effects + state linear trend
Marriage Market Controls			
Male Education Premium	-0.878	-0.632	-0.574

Continued from the previous page

	Pooled	State fixed effects	State fixed effects + state linear trend
	(0.405)	(0.405)	(0.406)
Sex-ratio	0.311	0.279	0.246
	(0.375)	(0.358)	(0.367)
Proportion of women 18-45 who hold a college degree	0.208	0.433	0.414
	(0.186)	(0.225)	(0.230)
Proportion of men 18-45 who hold a college degree	-0.383	-0.270	-0.243
	(0.218)	(0.225)	(0.229)
Mean wage (full time working men)	-0.300	0.028	-0.018
	(0.097)	(0.136)	(0.148)
Interaction of the Couple's Education with the Inequality Measure			
At most one of the spouses is a HS dropout	0.830	0.771	0.724
	(0.419)	(0.408)	(0.413)
One is a HS graduate, the other has at least SC	1.217	1.178	1.151
	(0.505)	(0.492)	(0.494)
Both have at least some College	0.754	0.764	0.727
	(0.457)	(0.447)	(0.446)
Socio-Economic Controls			
At most one of the spouses is a HS dropout	-0.979	-0.912	-0.853
	(0.499)	(0.486)	(0.492)
One is a HS graduate, the other has at least SC	-1.433	-1.387	-1.353
	(0.605)	(0.589)	(0.591)
Both have at least some College	-0.962	-0.975	-0.931
	(0.549)	(0.579)	(0.536)
Cohabiting Couple	0.582	0.578	0.583
	(0.037)	(0.037)	(0.037)
Number of own kids under 18 in the household	-0.043	-0.043	-0.044
	(0.008)	(0.008)	(0.008)
Number of other kids under 18 in the household	0.209	0.219	0.220
	(0.029)	(0.029)	(0.029)
Wife more than 5 years older than husband	0.056	0.057	0.056
	(0.042)	(0.041)	(0.042)
Husband more than 5 years older than wife	0.029	0.033	0.033
	(0.028)	(0.028)	(0.028)
Number of times previously married, wife	0.149	0.146	0.145
	(0.015)	(0.015)	(0.015)
Number of times previously married, husband	-0.010	-0.015	-0.016
	(0.019)	(0.019)	(0.019)

Continued from the previous page

	Pooled	State fixed effects	State fixed effects + state linear trend
Husband white	0.090 (0.019)	0.072 (0.020)	0.071 (0.020)
Couple is the same race	-0.108 (0.031)	-0.105 (0.031)	-0.107 (0.031)
House owned by occupants	-0.387 (0.018)	-0.397 (0.018)	-0.399 (0.018)
Wife's Age	-0.006 (0.003)	-0.006 (0.003)	-0.006 (0.003)
Husband's Age	-0.004 (0.003)	-0.004 (0.003)	-0.004 (0.003)
Year fixed effects	Yes	Yes	Yes
State fixed effects	No	Yes	Yes
State specific linear time trends	No	No	Yes
Constant included	Yes	Yes	Yes
Number of Observations	1,367,623	1,367,070	1,367,070

Weighted probit regressions, dependent variable "got divorced this month", women aged 46 and younger, 1990-2007. Variables significant at the 10% level printed in bold, standard errors in parenthesis. Standard errors clustered according to US Census Bureau recommendations, (U.S. Census Bureau, 2009).

The coefficients associated with the socio-economic characteristics I control for all have the expected sign and do not change dramatically in magnitude or significance levels across the three specifications I estimate. With regards to the pure effect of the couple's educational attainment, all three educational dummies are negative. This is to be expected, since the base group for these dummies are a couple where both spouses are high school drop-outs. It is well established in the literature that better educated individuals are less likely to divorce (e.g Weiss and Willis, 1997).

As discussed above, cohabiting couples are more likely to separate than legally married couples, so we would expect the coefficient associated with the cohabitation dummy to be negative and significant. The presence of own children in the household and jointly owned property (here, home ownership) are both investments in the relationship that increase the gains to marriage as well as the cost of divorce, and have been shown to decrease the divorce hazard in previous studies (e.g. Weiss and Willis, 1997; Charles and Stephens, 2004). The presence of other kids (foster children or children who are otherwise related to the couple) increases the divorce hazard. I include two dummies for the age difference between the partners to allow for the possibility that a large age difference decreases

the gains to marriage. Although the coefficients are both positive, the influence is not significant conditional on the other controls I include.

The number of times previously married has a positive impact on the divorce hazard. Most of the around 18% of the women in my sample who are in their second or higher order marriage are divorced (97%), which is not surprising given that the sample is restricted to quite young women. Thus, having experienced a divorce increases the chances to see subsequent marriages fail as well (Charles and Stephens, 2004, arrive at the same result). Becker et al. (1977) reason that individuals who were divorced in the past have either a higher variance in their traits (they experienced earnings- or other shocks in the past, that indicate that they are more likely to experience shocks in the future) or lower expected gains from marriage (because they invest less in marriage specific capital or have higher search costs) that caused their first marriage to break down. These same qualities also increase the likelihood that their second marriage, too, will be dissolved. Lundberg (2010) shows that personality traits are important in determining the risk of divorce. Most notably, individuals with a high degree of *openness to new experience* are more prone to divorce - which should not be surprising, since this personality trait is associated with a taste for variety and change. For men, extroversion is associated with a higher divorce risk, which Lundberg (2010) interprets as a lower cost to meeting new potential partners. Since personality traits are very stable in adulthood, individuals who score high in these traits are likely to divorce in higher order marriages as well.

As a robustness check, I repeated the regressions reported in table 3 without the term interacting the couple's joint educational attainment with the male education premium, and for a common measure of overall wage inequality; I report these results in table 4. For ease of comparison I also include my base estimates, omitting the socio-economic controls that are the same as above. Consistent with the theoretical model I developed in Hye (2011), the coefficient associated with the male education premium is not significant when the interactions are dropped - the effect indeed only affects women who have a chance to marry a college educated man in a second marriage. As a measure of overall wage inequality, I use the standard deviation of hourly wages for the marriage market relevant subgroup of men aged 18-50. The effect of overall wage inequality is somewhat stronger than the effect of the education premium, which is not surprising because there is more variation (the education premium is only a measure of between group variation). The pure effect of the standard deviation of log male earnings only disappears when state specific linear time trends are included in the specification, and the effect on the least educated couple is not significant in any specification, but the coefficients are similar in size across the two specifications.

Table 4: Probit for divorcing this month

	Pooled	State fixed effects	State fixed effects + state linear trend
Male education premium			
Pure effect	-0.878 (0.405)	-0.632 (0.405)	-0.574 (0.406)
Interaction of the Couple's Education with the Inequality Measure			
At most one of the spouses is a HS dropout	0.830 (0.419)	0.771 (0.408)	0.724 (0.413)
One is a HS graduate, the other has at least SC	1.217 (0.505)	1.178 (0.492)	1.151 (0.494)
Both have at least some College	0.754 (0.457)	0.764 (0.447)	0.727 (0.446)
Male education premium - pure effect only			
Pure effect	-0.072 0.130	0.153 0.148	0.178
Standard deviation of log male wages			
Pure Effect	-1.326 (0.442)	-0.799 (0.469)	-0.780 (0.479)
Interaction of the Couple's Education with the Inequality Measure			
At most one of the spouses is a HS dropout	0.490 (0.450)	0.402 (0.454)	0.393 (0.463)
One is a HS graduate, the other has at least SC	1.359 (0.487)	1.308 (0.489)	1.319 (0.494)
Both have at least some College	1.293 (0.466)	1.282 (0.471)	1.306 (0.479)

Socio-Economic controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
State fixed effects	No	Yes	Yes
State specific linear time trends	No	No	Yes
Constant included	Yes	Yes	Yes

Weighted probit regressions, dependent variable "got divorced this month", women aged 46 and younger, 1990-2007. Variables significant at the 10% level printed in bold, standard errors in parenthesis. Standard errors clustered according to US Census Bureau recommendations. Standard deviation of log hourly and ratio of mean log wages of college graduates/ non-graduates calculated for full time working men, ages 18-50,

It is interesting to see the quantitative influence of male wage inequality on the divorce

risk. Table 6 reports the marginal effect of an increase in the male education premium on the divorce hazard - I evaluate the marginal effect at every observation (with all covariates held constant at the value for this observation), and then calculate the average over all women in the sample, as recommended by Greene (2000). That is, I calculate the *mean effect* and not the *effect at the mean*.³² Table 6 reports the *difference* in the marginal effects of an increase in the male education premium for individuals who belong to the respective educational group as compared to all other individuals. These slopes should be taken with a pinch of salt because they are *changes in monthly divorce risk* in response to a change in the marriage market conditions. Since the monthly divorce risk is already very low, these effects are quantitatively small. I report them for the model specification with time and state fixed effects (column number three in table 3). Note that it is not possible to evaluate the marginal effect of a change in the male education premium only through the *interaction term* with the educational class a couple belongs to - changes in the education premium influence the divorce hazard in this model both through the pure effect and through the interaction with the educational dummies.

Table 5: Marginal effect of a change in the education premium on the divorce hazard

	Slope
At most one spouse is a HS dropout	0.000
One is a HS graduate, the other has at least SC	0.003
Both have at least some College	-0.004

Slopes from probit estimation including time and state fixed effects, standard errors adjusted according to US Census Bureau recommendations, women aged 46 and younger.

Table 6 tells us that there is practically no effect on couples in which at most one partner is a high school drop out, or both have a high school degree. For the middle couple, in which one spouse is a high school graduate and the other has at least some college education, a marginal increase in the return to a college degree increases the *monthly* divorce hazard by a third of a percent. The negative influence associated with the highest educational group is due to the pure negative influence of the divorce hazard that outweighs the positive interaction with the educational dummy for this group, because this effect is very small. I also checked if the marginal effect of the education premium is constant across the range on which the education premium varies, which seems to be the case.

³²I compute these marginal effects using stata's margins command, applying derivative (dydx) option over the three educational dummies.

Coughlin and Drewianka (2011) argue that, since most women do marry at some point, we should expect the effect of the male education premium on a woman's marriage hazard to weaken with the woman's age - you cannot wait forever. Young women have more to gain by behaving strategically on the marriage market. To see if this is the case, I calculated the differential effect of the education premium on women of different ages, and indeed find that the effect of the education premium declines with age, although the differences between the age groups are very small.³³

As a robustness check, I checked for seasonality in the divorce hazard by including month dummies (recall that the macro-economic variables are already deseasonalised using state-month interaction dummies, and the socio-economic variables are unlikely to vary monthly) in my main regressions. It turns out that there is some mild seasonality in the propensity to divorce; couples are more likely to split up in the summer and autumn than in the winter and spring. This however only very marginally influences the significance levels and size of the coefficients.

7 Conclusions

It has been shown before that young women do consider the quality of available young men in their decision to marry - if there is a lot of variation in the wages of young men, they are prepared to stay single for longer to increase their chances of finding a husband with a high earnings power. In this paper, I show that also women who are already in a relationship are influenced by the marriage market, and are more likely to separate if male wage inequality increases.

My conjecture is that the value of being married to a man without a college degree *decreases*, other things equal, if the wage returns to such a degree increase - college educated men become more attractive in comparison. Therefore, the expected value of divorcing and going back to the marriage market increases for a woman who is married to a man who does not have a college degree. Using data from the Survey of Income and Program Participation for the years 1990-2007, I show that the ratio of wages for men with and without a college degree indeed does influence a woman's propensity to divorce. Moreover, this effect is strongest for women who are most likely to marry a man with a college degree (due to assortative mating) - women who have a high school degree or some college education. The effect, although present, is statistically weaker for women with a college education or higher, who are married to man with a similar education. These women do not have as much to gain on the remarriage market, as they are already married to a highly educated spouse. This effect is robust to accounting for time trends and regional fixed effects.

³³I did this using the `at` option of the `margins` command.

The period I analyse - the 1990-2007 - was characterized by slow growth in earnings inequality as compared to the 1970s and 1980s. This weakened the positive relationship between wage inequality and age at marriage (Coughlin and Drewianka, 2011), that should be expected to be stronger than the relationship between divorce and wage inequality, due to the transaction costs of resolving a marriage. The fact that I still find a robust positive association between wage inequality and divorce is therefore encouraging.

The big caveat of this study is data availability: to my knowledge, the SIPP dataset is the only dataset available at the moment that is big enough to allow meaningful analysis of marital dissolution. While Gould and Paserman (2003) and Coughlin and Drewianka (2011) can use the much larger US census, my sample size and the shorter time period covered restrict the possibilities of this study. Gould and Paserman (2003) and Coughlin and Drewianka (2011) show that the marriage rates of very young women (below the age of thirty) are much more responsive to changes in wage inequality than the marriage rates of more mature women, which is to be expected, because younger women have a higher expected payoff from strategic behaviour on the marriage market. If there were a bigger dataset to analyse divorce, the same analysis I presented here could be meaningfully conducted for a younger sample, say, women below the age of thirty five. Further more, a larger sample size would facilitate the definition of geographically smaller marriage markets - for example at the metropolitan, instead of the state level - thus allowing for more cross-sectional variation. Because of data constraints, this is not possible at this time.

8 Appendix: Data

8.1 Survey Design

8.1.1 Sample Selection

The SIPP has a very complex survey design that aims to make the SIPP representative of the entire non institutionalized US population (i.e. of all individuals residing in the United States who are not permanently in prison, the armed forces or mental health facilities) while also delivering reliable estimates for special subgroups (low and high income households, ethnic minorities etc). It is not a random sample of the US population, but has a multi-stage survey design. The SIPP's primary sampling units (PSU) are either counties or independent cities, that are either grouped together with neighbouring counties (these are called non-self-representing counties) or constitute PSUs by themselves (are self-representing). The bigger, self-representing PSUs are in fact not PSUs but strata, because they are selected into the sample with probability one (U.S. Census Bureau, 2009). The smaller, non-self representing PSU's are grouped together with other PSUs in the same region - the Census Bureau distinguishes four regions, South, North-east, Midwest and West - to form *strata*. PSU's that are similar according to socio-economic information

from the decennial Census are grouped in the same strata (U.S Census Bureau, 1998). Within most strata, two PSU's are sampled (U.S. Census Bureau, 2009), with a probability proportional to the number of housing units within the PSU (U.S Census Bureau, 1998). Within those PSU's, clusters are sampled, and finally addresses are randomly sampled within clusters, and the households residing in these addresses are interviewed.

This discussion has important implications for the computation of variances based on SIPP data. If the data is treated as a random sample of the US population, variance estimates will generally underestimate the true variances (U.S. Census Bureau, 2009). The Census Bureau provides a variance stratum code to identify the stratum each observation was selected from, and a half-sample code to identify the PSU within the stratum, for variance estimation purposes. Not only do strata cross state lines (they never cross region lines however), but also some PSUs span across state boundaries (Siegel and Mack, 1998). Therefore, clustering the standard errors at the state level (because the main variables of interest in my analysis are measured at the state level) as recommended in the modern micro-econometrics literature (see, e.g. Angrist, 2002) necessarily comes at the cost of ignoring the the SIPP sample design. Ignoring the SIPP sample design means ignoring the between PSU variance, which is why the standard errors are lower in this specification than they are if I disregard US census recommendations and treat the state as the PSU.³⁴ I cannot cluster the standard errors at the state level *and* take the SIPP survey design into account at the same time, specifically because the SIPP strata and some PSU's cross state boundaries. I cannot have my cake and eat it too. There are two solutions to this problem: first, I could simply ignore the sampling procedure, treat the sample as a random sample, and cluster the standard errors at the state level. Working with the same dataset on a similar research question, Marinescu (2011) completely disregards the survey structure - variance variables and survey weights (see below) and treats the sample as a random sample. With this strategy, I could still use the weights to account for survey attrition, I would only ignore between PSU variance, and underestimate my variances. The second possibility is to follow US Census Bureau recommendations, and forgo the clustering of the standard errors at the state level. This should not be a problem for the majority of the PSU's that do not cross state boundaries, but comes at the risk of disregarding a correlation in the variances within states for PSU's that do cross state lines. Intuitively, I tend towards the second option because it seems to be the more conservative one (the standard errors are larger).

8.1.2 Data Collection and Weighting

SIPP interviews take place at a quarterly basis, and all individuals currently residing in sampled households are interviewed. If *original sample members* move out of a sample, the

³⁴I was made aware of this by US Census Bureau Staff.

SIPP aims to follow them (unless they become institutionalized or move abroad). However, as the survey progresses, *survey attrition* (the loss of sample members because they move or refuse interviews) becomes a problem, especially since survey attrition varies among socio-economic groups.

The SIPP aims to correct for this using sampling weights. The sampling weight associated to a person or household is an estimate of the number of individuals in the US population that the person or household represents. Ignoring the weights can lead to biases: for example, the 2004 SIPP included an oversampling of low income households; as a consequence, the fraction of female headed households with no spouse present in the sample is higher than in the US population. At the person level, the magnitude of the differences between population and sample is lower, but still appreciable, e.g. over representation of non-whites (Westat and Mathematica Policy Research, 2001). As the sample progresses, adjustments are made to the weights to correct for survey attrition, since different subgroups might differ in their propensity to move out of the sample. In particular, individuals who separate, divorce or become widowed are subject to higher survey attrition than individuals in steady relationships; although the SIPP sample weights try to correct for this, the incidence of family breakdown is subject to a downward bias in the SIPP.

The sample weight for a person in any reference- month is a product of four components: the inverse of the probability of the person's address being selected into the sample, a "duplication control factor" (if the dwelling turns out to be larger than expected, and only a part of the dwelling is selected into the sample), a household non-interview adjustment factor (controls for different rates of non-response in more than 500 non-response adjustment classes defined on characteristics such as social strata, census region, race, property ownership, metropolitan status, household size) and Wave 1 second stage calibration adjustment that adjusts the sample with independent estimates of population totals. The characteristics used for this include age, race, sex, Hispanic origin, family relationship, household type and state (Westat and Mathematica Policy Research, 2001, p 8-8 of the revised edition). As the survey progresses, the weight from the previous month is always carried over, and adjusted to compensate for changes in the sample resulting from movers and non-response. The non-interview adjustment classes are defined on the basis of household characteristics, mostly demographic characteristics, house ownership, household type, and other characteristics such as poverty status, type of income, financial assets, census division etc. The Census Bureau provides a whole battery of weights. Since my analysis is on a person/ month level I use the person weight.

8.2 Identifying Married Couples

For each married individual who is currently living in the same household with his or her spouse (i.e. whose marital status is reported as "married, spouse present") the SIPP

provides an ID number of the spouse. Thus, I can associate couples by generating a joint couple id (by concatenating the wife's and husband's id numbers). However, there are mistakes in the spouse ID numbers: from the 3,159,906 observations of women who report to be married and living with their spouse in a given month and who provide a spouse ID, I cannot match a husband to 411,884 or about 13% of all cases. That these non-matches are not equally distributed across panels, with 27% of all non-matches coming from the 1996 panel, corroborates the suspicion that these are mistakes in the recording of the spouse IDs. Another possibility to match couples is to match married individuals of opposite sexes who are counted as belonging to the same subfamily.³⁵ The problem arising from this approach is that it is not always clear who constitutes a couple: if for example a married couple resides with their daughter in law while their son is serving in the armed forces, it would be difficult to correctly match the wife, and not the daughter in law, to the husband. I have, however, information on the year of the last marriage for most married individuals.³⁶ So I match individuals who I could not match using the more direct approach of the spouse id numbers using the unique subfamily id and year of last marriage. I think it is very unlikely that two individuals who report to be married with their spouse present in the same subfamily would report the same year of marriage.³⁷ This could only be the case if a married couple was living with their married child, and both generations would have wedded in the same year. Using this approach, I can match another 328,421 women/month observations, and bring the women/ month observations in my sample who are not matched to husbands down to 83,463.

8.3 Identifying Cohabiting Couples

Up to the 1996 panel, the SIPP did not record unmarried partners separately, but subsumed them under the category "house-mate/ room-mate". Therefore I only include cohabiting couples from the 1996 panel onwards. The variable ERRP (household relationship) defines the relationship of each person living in the household to the household reference person (who would typically be the one interviewed, or the "householder" in the more conservative sense). Therefore I cannot include unmarried couples, when neither of the partners is ever the household reference person - this would be couples who durably live with their parents

³⁵In the SIPP, a family, as opposed to a household, is a group of people residing together who are related to each other by blood, marriage or adoption. A subfamily is a nuclear family residing with other related individuals in one household. For example, a primary family would be a married couple living with their son and daughter in law, and the son and daughter in law would be a subfamily. So a subfamily can never contain more than one married couple.

³⁶This question is asked in the marital history topical module file in the second wave of the panel, so it is primarily missing from individuals who get married during the panel, but after the second wave.

³⁷I drop any observations where there are two individuals of the same sex in the same subfamily reporting the same year of marriage. These are only a couple of dozen observations that are most likely due to measurement error.

or are roomers/ boarders of the reference person. There is no question in the SIPP that directly addresses the resolution of unmarried relationships;³⁸ I therefore assume that an unmarried couple is separated if they no longer live in the same household, as advised by US Census Bureau staff. The only possible source of noise with this approach is if one of the partners joins the armed forces, and therefore leaves the SIPP sample universe.

8.4 Wages and Earnings

When comparing wage data over time, some comparability issues have to be considered, most importantly the Census Bureau's practice of *topcoding* weekly earnings, their treatment of tips, commissions and overtime for hourly paid workers, their treatment of outliers and changes of the recording of hours worked over time (see Schmitt, 2003, for a thorough discussion of these issues).³⁹ I follow the CEPR's recommendations for dealing with these issues, specifically, I use the CEPR wage series that uses a log normal imputation to adjust for topcoding, excludes extreme values for hourly wages, disregards tips, commissions and overtime for hourly paid workers and uses imputed data for workers who report their hours "vary".⁴⁰ The wage rates I use are in constant 2010 US Dollars.

8.5 Combining Datasets from different Panels

The 1990 - 1993 panels were overlapping, that is multiple panels ran at the same time.⁴¹ To deal with this overlap, the US Census Bureau (see U.S. Census Bureau, 1993, p.6) recommends to drop the first wave of every panel because the questionnaires of the first wave and subsequent waves differ somewhat, and therefore the first wave data are not comparable to data of simultaneously running later waves. Furthermore, to ensure that the weights sum to the total US population in each month, the Census Bureau recommends to apply a weighting factor that is calculated using the number of interviews conducted in the respective panels. I follow these recommendations, and use the adjustment factors provided by the Census Bureau (see Westat and Mathematica Policy Research, 2001, p. 8-22).

³⁸I was cautioned by US Census Bureau staff against the use of ULFTMAIN because it is an unedited variable.

³⁹topcoding is the practice of truncating weekly earnings at a maximum value to protect the privacy of high earners. Because the Census Bureau only sporadically adjusts the topcoding threshold, the earnings distribution becomes skewed over time. Also, the census allows individuals to state that their weekly hours of work "vary", which can lead to systematic biases.

⁴⁰The CEPR deals with topcoding by assuming a lognormal distribution of wages above the topcode, that is allowed to differ by gender. Hours worked that "vary" are imputed using regression analysis, see again Schmitt (2003)

⁴¹The 1990 panel covers the period October 1989-August 1993, the 1991 panel October 1990-August 1993, the 1992 panel October 1991-December 1994 and the 1993 panel goes from October 1992-December 1995

The Census Bureau does not provide adjustment factors for post- 1996 panels, because they only overlap at the fringes.⁴² For the sake of consistency, I drop first month from the 1996 and the first three months from the 2004 panel.

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⁴²The 1996 panel covers December 1995 to February 2000, the 2001 panel October 2000- December 2003, and the 2004 panel October 2003 - December 2007.

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