The Marital Earnings Premium: An IV Approach

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Abstract

Conventional wisdom suggests and numerous studies find that married men earn more than single men. However, identifying whether and why marriage affects earnings is complicated by the fact that marriage market outcomes are jointly determined with potential earnings. To address this issue, I exploit exogenous variation in marriage induced by the geographically-staggered introduction of no-fault divorce laws in the United States over two decades. I find an average causal effect of marriage on earnings of husbands of a 38% increase. This increase in earnings is explained by a large increase in labor market work after marriage. My findings are robust to the possibility of unobserved heterogeneity in the effect of marriage on earnings across individuals, and support the idea that husbands specialize in labor market work after marriage.

Keywords: marital earnings premium, marriage, divorce laws, local average treatment effects.

JEL Classification: J12, J22, J31, K36.

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1 Introduction

Married men have higher labor earnings than single men. This phenomenon was first documented by Hill (1979) and initiated a large body of work. Researchers have documented the phenomenon in Australia, Canada, Europe, Israel, the United Kingdom and the United States (Schoeni, 1995). It has also been found to hold since at least the 19th century (Goldin, 1990). However, there is no consensus on the size of the effect or the channel through which it operates (Jakobsson and Kotsadam, 2016). This paper contributes to the literature by providing a new credible identification strategy to estimate the effect of marriage on the earnings of men – the so-called "marital premium" (Korenman and Neumark, 1991).

Men who are married earn 10% to 50% more than single men (Antonovics and Town, 2004). There are several possible explanations for this gap in earnings. First, it is likely that selection is at play, men that are productive for some idiosyncratic reason are also attractive partners in the marriage market. It is therefore expected that men who marry have higher wages than men who do not marry. Second, marriage allows within-marriage specialization. Traditionally, husbands would specialize in labor market work and wives would specialize in household work, allowing men to work harder and longer in their jobs, receiving higher wages later on. Third, employers may perceive marriage as a signal of characteristics hard to observe but prized in work such as honesty, loyalty, responsibility, etc., and statistically discriminate in a way that rewards married men.

A significant challenge to the identification of the effect of marriage on earnings, and the channel through which it operates, is the lack of exogenous variation in marriage. Most previous studies either ignore this problem or assume individual-specific time-invariant heterogeneity as the only source of endogeneity, which can be accounted for with individual fixed-effects. However, if men who get married change their behavior after marriage (work more hours or more intensely than before marriage, or their propensity to marry changes

with unobservable changes during the lifetime), the fixed effects strategy no longer uncovers a causal effect.

This paper takes a different approach to establish causality. I rely on exogenous variation in marriage brought about by the introduction of no-fault divorce regimes across states in the US in the 1970s and 1980s. The introduction of these laws may shift into marriage those men who would not have married under the pre-existing laws. Couples with low match quality may consider entering marriage as the union could be more easily dissolved than before therefore, reducing the costs of marriage a priori. At the same time, the passage of no-fault divorce could also prevent couples considering marriage from forming the union. If marriage is seen as a commitment device, the no-fault legislations weakened its credibility. To identify the effect of marriage on earnings while considering the opposing effects of the legislations on marriage decisions, I employ a new strategy proposed by de Chaisemartin (2016). This new strategy obtains a Local Average Treatment Effect (LATE) of marriage on earnings that is robust to the presence of defiers in the treatment group.[1] The price to pay is that the estimated LATE applies only to a subpopulation of compliers.[2]

Even though the body of work on the effect of marriage on earnings is extensive, there is little research examining the mechanisms through which the marriage premium operates. In particular, there is very little work on the effect of marriage on earnings through the intensive margin, that is, hours worked. Another contribution of this paper is to decompose the effect of marriage on total labor earnings into the effect of marriage on hours of work and the effect of marriage on hourly wages. Specifically, I look at the effect of marriage on men's weekly hours of work and hours of housework to shed light on the mechanism which produces the marital premium.

[1] Using the terminology common in the LATE literature, a defier is a man who would have married under the previous divorce regime but who decides not to marry after the passage of the no-fault divorce legislation.

[2] A complier is a man who married only because of the introduction of no-fault divorce laws and would not have married otherwise. As de Chaisemartin (2016) notes, this subpopulation of compliers is the same size as the population of compliers under the standard LATE.

Identification of the marital wage premium is important as it helps in elucidating gender-based discrimination in labor markets. The male marital premium has been recognized as a possible cause for the gender gap in earnings (Neumark, 1988; Waldfogel, 1997; Waldfogel and Mayer, 2000). It also helps in understanding the determinants of individual wages (Loh, 1996). It has also been addressed as a mechanism to address child poverty (Thomas and Sawhill, 2002). Finally, to the extent that marital premium reflects productivity differences, changes in the marital composition of the labor force translate into productivity differences of the labor force (Korenman and Neumark, 1991).

Studying the marital wage premium, apart from intrinsic academic interest, can be of use to policy-makers. Several countries have considered income splitting of couples for tax purposes.[3] The United States and Germany currently allow for income splitting, while Canada considered implementing some form of income splitting as recent as in its 2015 budget.

The rest of the paper is organized as follows: Section 2 provides a brief review of recent work, Section 3 documents the divorce reforms, Section 4 describes the data and sample used in the estimation, Section 5 explains the estimation strategy and discussion of the results, and Section 6 concludes.

2 Recent Work

Selection bias is a major concern for assessing the effect of marriage on earnings. Ginther and Zavodny (2001) aim to overcome selection bias by analyzing shotgun weddings to evaluate the marital wage premium. Under the assumption that premarital conception followed by marriage is random, a comparison between men who were shotgun-married with single

[3]Income splitting is the practice of attributing income from one spouse to the other spouse. This often has redistributive effects as it reduces the total tax bill paid by rich households in which both spouses are present.

men should provide a causal effect of marriage on earnings. Using data on they find that men with shotgun marriages earn 15% more than never-married men. They conclude that less than 10% of the marriage premium remains after controlling for selection. With the same concern over selection bias in mind, Antonovics and Town (2004) use data on monozygotic twins and find that the estimated premium increases from 19% to 26% when controlling for genetic endowment within twins. They conclude that the marital wage premium cannot be attributed to selection. Similarly, Chun and Lee (2001), using a switching regression framework on CPS data for 1999, find that the marriage wage premium is not explained by selection, but rather is due to specialization within the household.

Most papers find a positive effects of marriage on wages. However, Loughran and Zissimopoulos (2009) find that marriage lowers the wage growth of men by between 2 and 4 percentage points using data from the NLSY. Similarly, Killewald and Lundberg (2017) argue that changes in wages predate changes in marital status (both entry into marriage and divorce), using data from the NLSY79, and therefore there is no causal effect of marital status on wages.

In contrast to this paper, much of the literature was dedicated to estimate the marital *wage premium*, ignoring the effect of marriage on hours of work. An exception is Ahituv and Lerman (2007), who estimate a series of equations relating marriage to hours of work and wages for 23 waves of the NLSY. They consider entry into first marriage, divorce and remarriage as functions of hours of work and wages. They find that entry into marriage increases hours of work by 160 per year, and increases wage rates by 12% relative to never-married. They translate those effects to an increase of 15.9% in annual earnings. However, Jakobsson and Kotsadam (2016) find that selection accounts for most of the differences in hours worked between married and non-married men in Europe, and that the effect on wages dissipated after 1990s.

3 The Divorce Reforms

This section discusses the institutional background of changes in the divorce regime, its causes and the timing of its adoption across the United States.

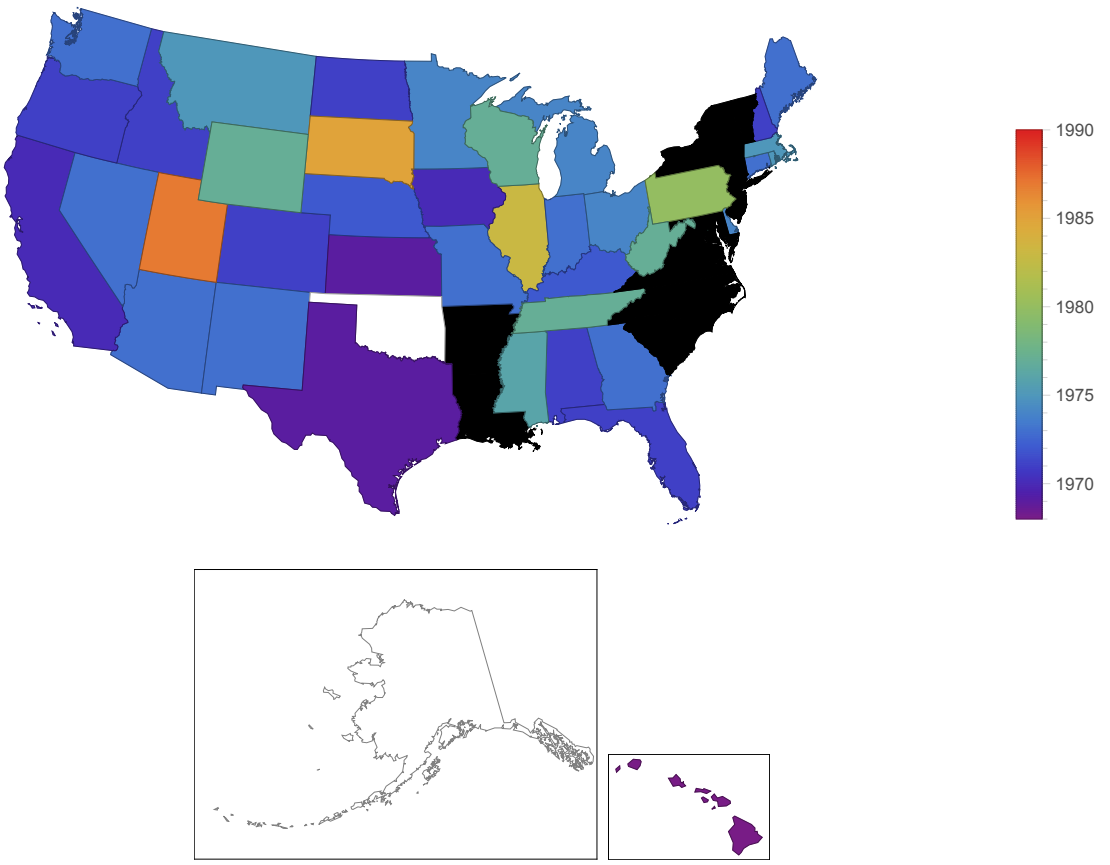
In the 1970s and early 1980s, several states introduced no-fault divorce clauses to their existing divorce regimes. Before these laws were passed, typically a divorce was granted on the grounds of wrongdoing by one of the spouses. Such grounds included adultery, cruelty, abandonment, mental illness, criminal conviction, and substance abuse, among others. The reforms allowed spouses to divorce under no-fault clauses such as separation, irreconcilable differences or irretrievable breakdown of the marriage.

Figure 1 presents a map of the timing of the adoption of no-fault clauses in the divorce legislation for each state. States in white[4] had reforms pre-dating 1968, while states in black[5] had not passed any no-fault legislation by 1990. Most of the states adopted no-fault legislation between 1970 and 1975, with the median year being 1973.

[4]The states in white in Figure 1 are Alaska and Oklahoma.

[5]The states in black in Figure 1 are Arkansas, Louisiana, Maryland, New Jersey, New York, North Carolina, South Carolina, Vermont, Virginia.

Figure 1: Timeline of adoption of no-fault divorce, 1968-1990.



Source: Mechoulan (2005).

Several reasons have been put forward to explain the introduction of no-fault divorce across states. The main legislative reason is that the reforms attempted to save the judicial system from hypocrisy and perjury (Mechoulan, 2005), as many couples engaged in collusion to be granted a divorce by the court bypassing the requirement of determining fault in a marriage.[6] These changes were largely unanticipated as they were considered "routine policy refinement" that passed "with little notice or dissent" and without the participation of the public or any interest groups (Jacob, 1988). This suggests that the timing of the regime changes in divorce legislation are plausibly exogenous (Wolfers, 2006).

[6]These collusive behaviors included alleging cruelty by the husband, as most cruelty cases went uncontested, or "collusive adultery." in which the couple presented to court staged evidence of adultery.

The changes in divorce laws affect marriage through sorting and self-selection of couples into marriage. Bargaining models point out that the allocation of utility within the household depends on the outside options of the spouses, since divorce laws partly determine the value of the outside option, the introduction of the laws is important for the distribution of intra-household bargaining power and therefore the characteristics of couples who decide to marry (Stevenson, 2007). This mechanism is theoretically ambiguous because, on the one hand, lower divorce costs can induce lower-quality couples to marry, since the partners know that they can exit the marriage easier after the reforms.[7] However, on the other hand, the credibility marriage as a commitment device is weakened; therefore only high-quality couples will marry for this purpose.[8]

A prima facie, it may look like the no-fault divorce legislation can induce couples to divorce. In which case, the divorce revolution would introduce exogenous variation on the decision to divorce which could be used to estimate whichever effect divorce has on earnings – including a possible reverse marital premium. However, a more careful analysis indicates that easier divorces do not affect the probability or propensity to divorce, but will affect the intra-household distribution of power and utility. After the introduction of no-fault divorce, married couples on the brink of divorce will renegotiate the distribution of utility within a marriage, and assuming efficient decisions, the new allocation will convince partners to remain married.[9] In any case, it turns out that the instrument is not very useful in inducing divorce for the subsample of married men. I will therefore consider only men who are single at the time no-fault divorce legislation is passed in their state of residence.

[7] That is, couples may be more willing to marry because they can divorce at lower cost later on.

[8] Alternatively, couples may become more selective after the reforms, which leads to a rise in match quality.

[9] However, theoretically, this version of the Becker-Coase theorem holds only under strong conditions on the utility function of the partners, see Chiappori et al. (2015). Only couples that cannot reach a mutually profitable agreement under the new legislative regime would eventually divorce.

4 Data

This paper uses data from the Panel Study of Income Dynamics (PSID) from 1968 to 1993. The sample contains individuals who are Head of household in the year of the interview. I consider working males between ages 16 to 60 who have completed their education and who are single one year before the passing no-fault divorce legislation in their state of residence.[10][11] The data also include information on age, education, state of residence, marital status, total labor earnings and hours of work.

For the divorce reforms, I use the classification by Mechoulan (2005). The author identifies the year each state enacted specific no-fault provisions for divorce, based on legal research. I use that data to construct a dummy variable that varies over time for each state, corresponding to one if the state passed no-fault divorce legislation at a given year and zero otherwise.

Table 1 presents descriptive statistics for the sample. The table reflects a few features of the sample that are important mentioning. As noted above, I consider men who are single at the time of the divorce reforms in their respective states, that means that men could have been married before the reforms but divorced by the time the reforms were passed, hence the 25% of the sample were married at some point before the reforms. The variable *exp* represents cumulative hours of experience, therefore its mean 18190 is equivalent to roughly 9 years of experience, and 25353 translates roughly to 12 years of experience. Labor earnings and work hours increase after the reforms, these could be due to secular increases as men age or accumulate more labor experience. Therefore it is crucial to control for those variables. Also, including individual fixed-effects will control for the effect of education and innate ability on earnings.

[10]Note, however, that individuals can remain single for many periods after the passing of the no-fault divorce legislation.

[11]These individuals are the ones "at risk of marriage," and therefore the ones to be potentially induced into marriage by the divorce reforms.

Table 1: Descriptive statistics.

Variable	Mean	Std. Dev.	Min	Max
Before reforms				
age	33.38	9.63	20	60
married	0.25	0.43	0	1
labor earn	684.26	490.27	106.21	3224.48
exp	18190.61	13280.49	0	56789
work hrs	39.55	13.91	6.73	90
After reforms				
age	36.17	8.07	19	60
married	0.76	0.42	0	1
labor earn	749.02	482.48	100.02	4215.63
exp	25353.35	17066.53	232	92827
work hrs	41.55	12.18	4.46	112.31
Overall				
age	35.92	8.26	19	60
married	0.72	0.45	0	1
labor earn	743.34	483.47	100.02	4215.63
exp	24724.09	16889.15	0	92827
work hrs	41.37	12.35	4.46	112.31
individuals	405			
total observations	4519			

Figures 2 and 3 show histograms of total labor earnings per week and weekly hours of work for married and single men. Several things are apparent: single men are more likely to have lower earnings than married men; the distribution of earnings and hours for married men has a larger mean and higher variance than the distribution for single men; however, married men seem to have lower variance in hours of work than single men. In Figure 4, the distribution of wages for married men seems to be slightly to the right of that of singles, implying that wages for married men are just higher than for single men.

Figure 2: Total labor earnings per week by marital status.

Figure 3: Total hours of work per week by marital status.

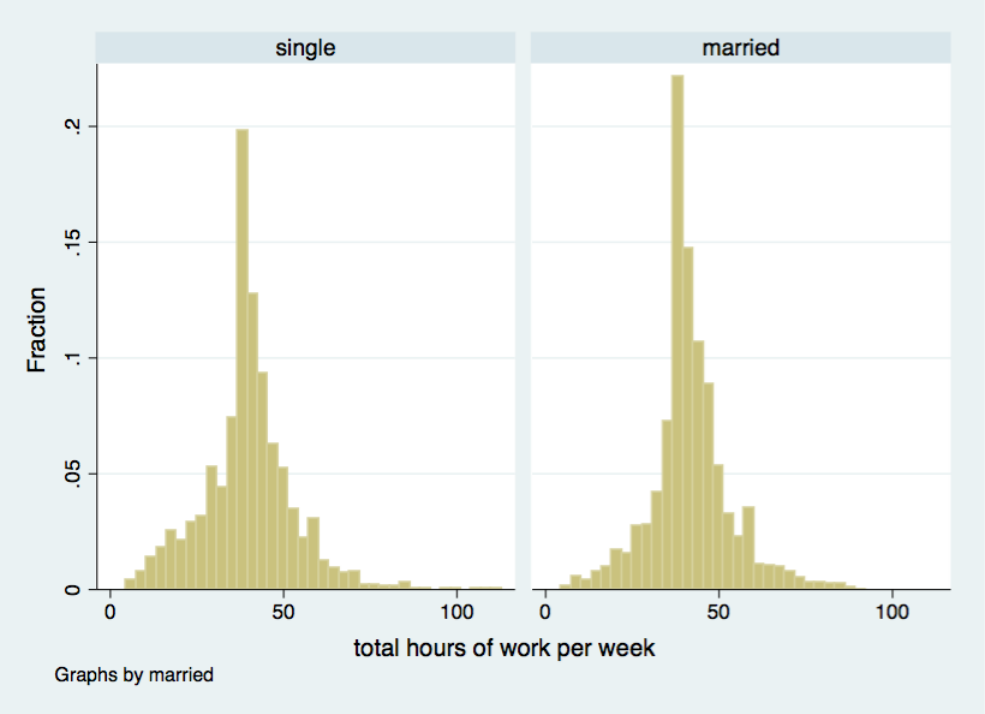


Figure 4: Hourly wages by marital status.

5 Empirical Strategy

This section discusses the estimation strategy and the results. The causal regression of interest is:

$$y_{ist} = \alpha marr_{ist} + \beta \cdot x_{ist} + \delta_i + \lambda_t + \gamma_s + \varepsilon_{ist}. \tag{1}$$

Where y_{ist} is the outcome variable of interest (each of log of total weekly labor earnings, log of hourly wages or log of weekly hours of work), $marr_{ist}$ is a dummy variable that indicates whether individual i in state s is married in year t, x_{ist} is a vector of covariates, δ_i captures time-invariant individual characteristics, and λ_t controls for time-specific factors that affect all individuals, γ_s controls for state-specific factors, and ε_{ist} represents unobserved factors that explain y_{ist}. The parameter α represents the effect of marriage on the outcome of interest.

The parameter α can be consistently estimated as long as $\text{cov}(marr_{ist}, \varepsilon_{ist}) = 0$. However, marriage is not as good as randomly assigned even when controlling for covariates and fixed effects. In that case, marriage is correlated with unobservable changes in behavior across the lifetime. If men are more likely to get married due to unobservable factors, then fixed-effect estimates of the marital premium will be biased upwards. However, if marriage is negatively correlated with unobservable factors, then the fixed-effect estimates will be biased downwards. Finally, if men are more likely to decide to get married after receiving a positive wage shock, then marriage will be correlated with a lower income due to regression to the mean (Antonovics and Town, 2004). This is a particular case of reverse causality, in which income determines marriage. In all the previous cases α would not be identified by a fixed-effects regression like equation 1.

5.1 The IV Estimator and LATE

In empirical work, a standard solution to the problems presented above is to use an instrumental variable (IV) to induce exogenous variation in the variable of interest. In the marital premium literature, the IV strategy is not often used. This can be due to the prevalence of cross-sectional analyses with limited data, which restricts the range of potential instrumental variables, or to the intrinsic difficulty in finding good instruments for marriage.

As explained before, the IV strategy becomes crucial when there are unobservable changes in the lifetime of an individual that are correlated with marriage. In this setting, the causal model is given by the equations

$$y_{ist} = \alpha marr_{ist} + \beta \cdot x_{ist} + \delta_i + \lambda_t + \gamma_s + \varepsilon_{ist}, \tag{2}$$

$$marr_{ist} = az_{st} + b \cdot x_{ist} + d_i + \ell_t + g_s + e_{ist}. \tag{3}$$

Where z_{st} is an indicator variable equal to 1 if divorce reforms have been implemented in state s in year t, and equal to 0 otherwise.

However, it is not hard to imagine that marriage has a different effect on different individuals. For example, some individuals may increase their hours of work more than others, which in turn increases their total earnings more than for others. Imbens and Angrist (1994) developed a framework under which it is possible to estimate the effect of interest under heterogeneity in the responses to both the instrument and the treatment. This Local Average Treatment Effect (LATE) is a characterization of the Two-Stage Least Squares (2SLS) estimator in the presence of heterogeneous treatment effects. It brings the 2SLS estimator to the potential outcomes framework and gives it a causal interpretation as an average treatment effect of marriage on wages for those individuals induced into treatment by the instrument when the treatment effect can vary among individuals. Allowing for heterogeneous treatment effects, the causal equation then becomes:

$$y_{ist} = \alpha_{ist} marr_{ist} + \beta \cdot x_{ist} + \delta_i + \lambda_t + \gamma_s + \varepsilon_{ist}. \tag{4}$$

The key assumption for the standard LATE is that the instrument (weakly) induces all individuals into marriage or all individuals out of marriage. That is, all individuals respond to the instrument in the same direction, albeit their reaction is potentially different in magnitude. However, not all individuals respond to the divorce reforms in the same direction. The subpopulation for which the reforms caused individuals to select out of marriage is called the defier group and its presence represents an important threat to the identification of causal effects as it can bias the magnitude and sign of the estimated parameter.

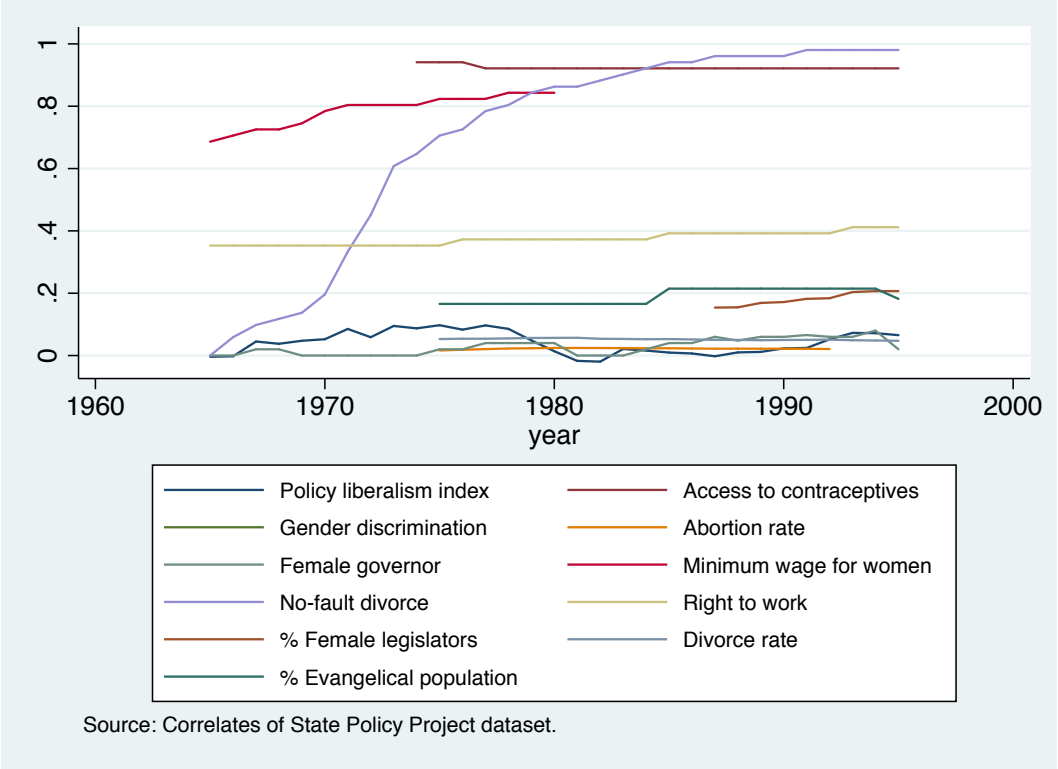
As explained in Section 3, easier divorce may induce some people into marriage, but induce some people not to marry in the first place. To allow for the presence of those defiers, I employ a novel LATE by de Chaisemartin (2016). He relaxes the monotonicity

assumption of the standard LATE. Under the assumption that there is a subpopulation of compliers that have the same treatment effect of defiers, and that the group of defiers and that subpopulation of compliers have the same size, he estimates the average treatment effect for the rest of the population of compliers (this subpopulation of compliers is called the compliers-survivors or "comvivors"). In the Appendix, I go into more detail and sketch the main result de Chaisemartin (2016). For now, it is important to note that this subpopulation of "comvivors" under the assumptions of de Chaisemartin (2016) is the same size as the subpopulation of compliers under the assumptions of Imbens and Angrist (1994). Therefore, by allowing for defiers, I am not restricting the size of the subpopulation for which I am identifying and estimating the average treatment effect. In summary, I estimate equation (4) instrumenting marriage with the passing of no-fault divorce laws (Mechoulan, 2005), and I can give the estimated parameter the interpretation of a LATE (de Chaisemartin, 2016).

5.1.1 Potential Threats to Identification

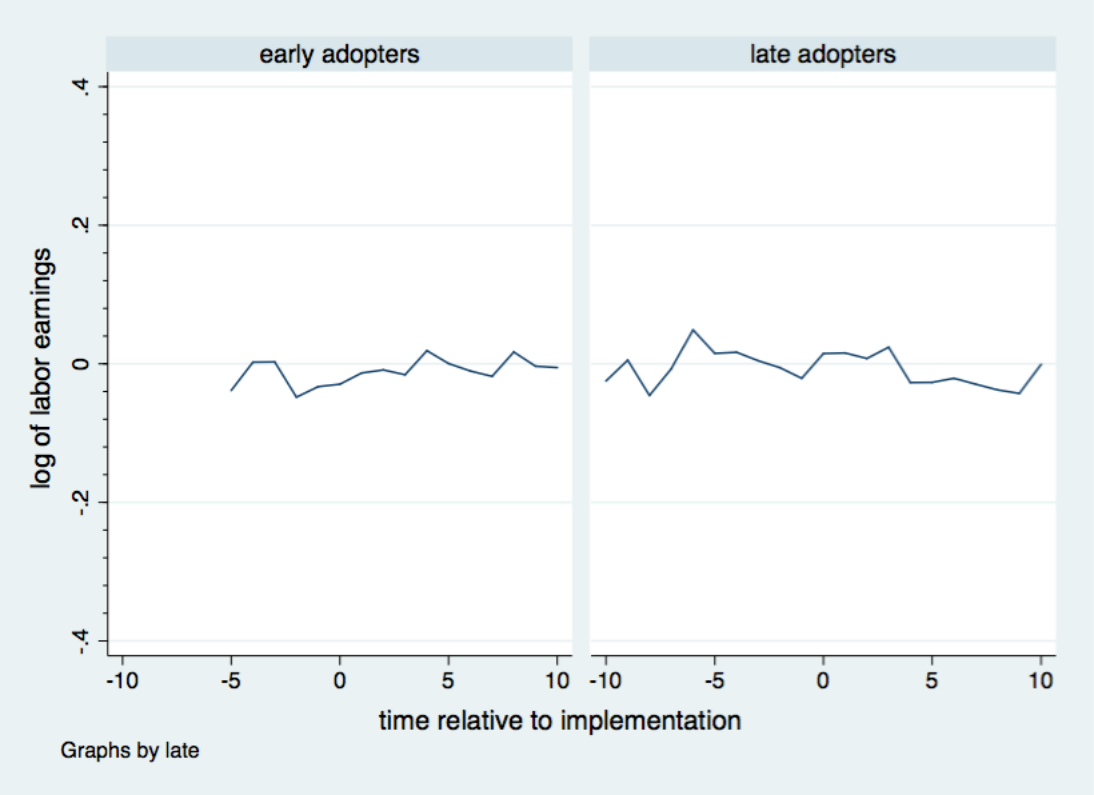
The validity of changes in no-fault divorce laws as an instrument relies on the exclusion restriction, that is, that those changes are not correlated with the error term in the structural equation (4). The exclusion restriction would be violated if both the instrument and the marriage decision were influenced by a third factor which was also correlated with the structural error term. One way to examine this correlation is to look at the timing of the reforms and the trends of potentially confounding variables. In Figure 5 below I present a graph of several of potential third factors that could affect the decisions to marry and could be potentially correlated with the structural error term. It is clear from the graph that no-fault legislations seem to be be independent of the other potential factors that could confound the results, as most lines are rather flat.

Figure 5: Distribution of events over time



Another way to look at the timing of the reforms is to examine whether states that were early adopters of the no-fault legislation had different trends in labor income than late adopters of the reforms. Figure 6 plots the mean log of labor income, purged of time and state effects, as a function of years relative to the introduction of the divorce reforms, for states that adopted no-fault divorce laws before 1973 (early adopters), the median year of adoption, and those who adopted them from 1974 onwards (late adopters). The idea is to assess whether there were changes in log wages at the time if the reforms and whether those trends were different in states that were early adopters compared to states that were late adopters. The differences in mean log wages between early and late adopters amount to less than 1.5%.

Figure 6: Log of total earnings by early/late adoption.



5.2 Results

Table 2 presents the results of different specifications for the first stage regression.[12] The instrument induces marriage between 22% and 50% of the cases and those estimates are highly significant. From a technical point of view, it means that the instrument is relevant for the subpopulation under examination. Moreover, notice that as I include fixed effects to the regression (going from column 1 to 2, and 3) the effect of the instrument in inducing marriage decreases. This decrease is due to the fact that people tend to marry sooner or later, and the inclusion of time fixed effects will capture some of that tendency. In addition, individual fixed-effects capture individual specific heterogeneity in the propensity to marry. All in all, the covariates and fixed-effects pick up any individual, state, and time effects that might influence the decision to marry as well as for the gradient of the propensity to marry

[12]In the appendix, I show the result of the first stage regressions for the full sample (including men who are married at the time of the reforms). As an instance of the Becker-Coase theorem, the instrument is irrelevant for that full sample.

with respect to age.

Table 2: First stage.

VARIABLES	(1) OLS	(2) FE1	(3) FE2
no-fault div	0.502***	0.248***	0.220***
	(0.0222)	(0.0317)	(0.0235)
age	0.0686***	0.0252***	0.0302***
	(0.00565)	(0.00611)	(0.00657)
age sq	-0.000848***	-0.000362***	-0.000356***
	(7.24e-05)	(7.64e-05)	(6.27e-05)
std(exp)	-0.0894***	-0.105***	-0.0314
	(0.00800)	(0.00811)	(0.0288)
Observations	4,517	4,517	4,517
Individual FE	No	No	Yes
Time FE	No	Yes	Yes
State FE	No	Yes	Yes
Number of individuals			405

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3 shows the results of using the IV strategy described above for total labor earnings. In that table, column 1 shows a simple OLS estimation, column 2 is OLS with state and time fixed-effects, column 3 is the OLS estimation with a full set of fixed-effects (equation 1), column 4 is the LATE estimation with state and time fixed-effects, and column 5 shows the LATE estimation with a full set of fixed-effects (equation 4). For interpretation purposes, I standardize cumulative hourly experience to have mean 0 and variance 1.

Table 3: Total weekly labor earnings.

VARIABLES	(1) OLS	(2) OLS FE1	(3) OLS FE2	(4) IV FE1	(5) IV FE2
married	0.133***	0.206***	0.0756***	0.299	0.379**
	(0.0203)	(0.0217)	(0.0242)	(0.192)	(0.163)
age	0.0961***	0.107***	0.0747***	0.106***	0.0647***
	(0.00830)	(0.00889)	(0.0105)	(0.00970)	(0.0119)
age sq	-0.00106***	-0.00117***	-0.00123***	-0.00114***	-0.00113***
	(0.000106)	(0.000111)	(9.75e-05)	(0.000126)	(0.000111)
std(exp)	0.00251	0.0238**	0.258***	0.0340	0.267***
	(0.0116)	(0.0120)	(0.0448)	(0.0241)	(0.0460)
real GSP	0.0665***	0.152	0.273***	0.167	0.234**
	(0.0101)	(0.110)	(0.0892)	(0.113)	(0.0932)
Observations	4,517	4,517	4,517	4,517	4,517
Individual FE	No	No	Yes	No	Yes
Time FE	No	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Number of individuals			405		405

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Recall that the coefficient of *married* is the average effect of marriage on total earnings for the subpopulation of individuals who are induced to marry by the divorce reforms and who survive elimination with the defiers (the "comvivors"). Moreover, notice that this LATE is averaged over the lifetime of individuals (as we are estimating $\mathbb{E}[\alpha_{ist} \mid i \text{ is comvivor}]$). This estimate implies that marriage increases total labor earnings by 38% on average for the subpopulation of comvivors.

Table 4 presents the results for hourly wages. There is an increase of hourly wages of almost 9.6% for individuals who get married. The effect, however it is not significant. Table 5 shows the results for hours of work, it provides a channel through which total labor earnings increase after marriage. It shows that men who marry increase their weekly working hours by 23% and it is statistically significant. A 23% increase in weekly hours of work is equivalent to an increase of 9 hours per week (1.8 hours per day) for a work week of 40 hours.

Table 4: Hourly earnings.

VARIABLES	(1) OLS	(2) OLS FE1	(3) OLS FE2	(4) IV FE1	(5) IV FE2
married	0.0625***	0.129***	0.0436*	-0.0731	0.0958
	(0.0199)	(0.0212)	(0.0231)	(0.182)	(0.159)
age	0.0892***	0.0907***	0.0790***	0.0948***	0.0771***
	(0.00807)	(0.00865)	(0.00974)	(0.00941)	(0.0113)
age sq	-0.000941***	-0.000939***	-0.000857***	-0.00100***	-0.000841***
	(0.000103)	(0.000108)	(9.32e-05)	(0.000122)	(0.000106)
std(exp)	-0.0619***	-0.0415***	0.0636	-0.0636***	0.0655
	(0.0114)	(0.0117)	(0.0427)	(0.0230)	(0.0431)
Observations	4,517	4,517	4,517	4,517	4,517
Individual FE	No	No	Yes	No	Yes
Time FE	No	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Number of individuals			405		405

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Weekly hours of work.

VARIABLES	(1) OLS	(2) OLS FE1	(3) OLS FE2	(4) IV FE1	(5) IV FE2
married	0.0676***	0.0761***	0.0345**	0.316***	0.226*
	(0.0113)	(0.0124)	(0.0169)	(0.110)	(0.118)
age	0.0126***	0.0175***	0.00355	0.0127**	-0.00347
	(0.00459)	(0.00508)	(0.00714)	(0.00569)	(0.00842)
age sq	-0.000178***	-0.000236***	-0.000382***	-0.000163**	-0.000322***
	(5.87e-05)	(6.36e-05)	(6.83e-05)	(7.38e-05)	(7.85e-05)
std(exp)	0.0619***	0.0650***	0.182***	0.0912***	0.189***
	(0.00645)	(0.00689)	(0.0313)	(0.0139)	(0.0321)
Observations	4,517	4,517	4,517	4,517	4,517
Individual FE	No	No	Yes	No	Yes
Time FE	No	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Number of individuals			405		405

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

5.3 Discussion

Despite their significance, there are some caveats to the results presented in the previous section. The introduction of no-fault divorce laws changed bargaining power within the

marriage, altered the composition of singles available for marriage and even likely changed the educational attainment of individuals in the long run. However, the focus of this paper is to determine how those changes in the divorce regime induced single men into marriage and how those men's earnings increased as a result. The subpopulation of analysis is men who were single at the time and had completed their education, this group is the "initial" population of single men. In general, compositional effects in the pool of men in the marriage market affect more the initial population of single men the longer those men stay single after the reforms. This is because they would compete more and more with recently divorced men entering the marriage market over time. However, the divorce reforms do not directly induce couples to divorce. Before divorce happens, couples first try to renegotiate the distribution of the marital surplus and only divorce when there is no possible redistribution of the surplus that makes both partners better off staying married than divorcing.[13] Therefore, the potential impact that the no-fault divorce laws have on the supply of newly divorced men to the marriage market gets dampened by the pre-divorce renegotiation of the surplus.

In addition to competing with recently divorced men, the initial population of single men has to compete more and more with new entrants into the marriage market coming from having completed their education. These recent graduates are arguably better equipped to compete in the marriage market because they would have incorporated the new divorce regime when choosing the level of education they wanted to attain. In that sense, their level of education is an optimal response to the new situation in the marriage market. However, the fact that people marry partners of similar age[14] somewhat diminishes those effects, as the initial population of single men will be relatively older. Disentangling both of the concerns described above requires an dynamic equilibrium framework of the marriage market and is left for future research.

[13]See Chiappori and Mazzocco (2017) for a full discussion.

[14]See for example Choo and Siow (2006) and Choo (2015) for some results.

The fact that the initial single men cannot adjust their education in response to the new divorce regime means that the only other margin of adjustment they have in the face of new divorce legislation is hours of work. This provides a mechanism through which the earnings premium may operate. This is consistent with a story in which men specialize in labor market work after marriage.[15] To examine the specialization of men within marriage I look at hours of housework after marriage. Table 6 presents the results of examining weekly hours of housework. It shows, in all specifications (both OLS and IV), that men reduce the amount of time spent in housework after marriage. Although the estimates are not precise,[16] they provide suggestive evidence of a specialization story of men after marriage.

Table 6: Weekly hours of housework.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	OLS	OLS FE1	OLS FE2	IV FE1	IV FE2
married	-0.105**	-0.267***	-0.119	-0.444	-0.178
	(0.0526)	(0.0579)	(0.0793)	(0.469)	(0.535)
age	0.104***	0.0340	-0.0161	0.0376	-0.0119
	(0.0216)	(0.0229)	(0.0421)	(0.0246)	(0.0571)
age sq	-0.00149***	-0.000636**	0.000288	-0.000688**	0.000266
	(0.000280)	(0.000293)	(0.000360)	(0.000321)	(0.000415)
std(exp)	-0.00256	-0.0290	0.172	-0.0577	0.160
	(0.0368)	(0.0388)	(0.201)	(0.0847)	(0.230)
Observations	2,387	2,387	2,387	2,387	2,387
Individual FE	No	No	Yes	No	Yes
Time FE	No	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Number of individuals			310		310

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

6 Conclusion

Numerous studies have identified a gap between the earnings of married and single men. However, finding causal estimates of marriage on earnings has proven a difficult task. The difficulty lies on the simultaneity and possible reverse causality of marriage and earnings.

[15] This does not imply, however, that women specialize in house work.
[16] Partly due to a smaller sample size.

Although several mechanisms have been proposed to explain the gap in earnings, none has stood serious scrutiny.

In this paper I use exogenous variation on marriage decisions brought about by the staggered passing of no-fault divorce laws across US states and over time. Even though the no-fault divorce laws induce variation on the decision to marry, the direction of the effect is theoretically ambiguous. In addition, the effect of marriage on earnings is likely heterogeneous. Thus I employ a novel methodology that allows for hetergeneous treatment effects while considering the presence of defiers to estimate a Local Average Treatment Effect on a subpopulation of compliers.

The results indicate that married men earn 32% more than single men over their lifetimes. I further decompose that number into an increase in hourly wages of 11% (not statistically significant), and an increase in time spent working in the labor market of 21%. My results suggest a specialization story in which men who marry spend more time working, which in turn can lead to promotions and pay raises. I validate this story by providing evidence that after marriage men reduce their time spent in housework.

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A Appendix

A.1 A Local Average Treatment Effect with Defiers

This section sketches the main result in de Chaisemartin (2016), the estimation of a Local Average Treatment Effect under the presence of defiers. Consider a binary instrument Z, let $D_z \in \{0,1\}$ be the treatment when the instrument takes a value $Z = z$ and Y_{dz} denote the outcome when the instrument takes value z and treatment takes value $d \in \{0,1\}$. Only Z, $D \equiv D_Z$ and $Y \equiv Y_{DZ}$ are observed. Four subpopulations are defined:

1. Never takers (NT): individuals for whom $D_0 = 0$ and $D_1 = 0$.

2. Always takers (AT): individuals for whom $D_0 = 1$ and $D_1 = 1$.

3. Compliers (C): individuals for whom $D_0 = 0$ and $D_1 = 1$.

4. Defiers (F): individuals for whom $D_0 = 1$ and $D_1 = 0$.

Now, under the assumptions that (1) the instrument is independent of the potential values of D and Y

$$(Y_{00}, Y_{01}, Y_{10}, Y_{11}, D_0, D_1) \perp\!\!\!\perp Z,$$

and (2) Z does not enter the structural equation,

$$Y_{d0} = Y_{d1} = Y_d \qquad \forall d \in \{0,1\},$$

the Wald estimator W can be written as:

$$W = \frac{\Pr(C)\,\mathbb{E}[Y_1 - Y_0 \mid C] - \Pr(F)\,\mathbb{E}[Y_1 - Y_0 \mid F]}{\Pr(C) - \Pr(F)}.$$

In addition, if either $\Pr(F) = 0$ or $\mathbb{E}[Y_1 - Y_0 \mid C] = \mathbb{E}[Y_1 - Y_0 \mid F]$, W is the average causal effect of treatment on the compliers $\mathbb{E}[Y_1 - Y_0 \mid C]$ and the coefficient of the first stage in a

2SLS framework is equal to the subpopulation of compliers $FS = \Pr(C)$. de Chaisemartin (2016) relaxes these conditions to:

$$\begin{aligned}
&\text{There exists a subpopulation of compliers } C_F \text{ such that} \\
&\Pr(C_F) = \Pr(F), \\
&\mathbb{E}[Y_1 - Y_0 \mid C_F] = \mathbb{E}[Y_1 - Y_0 \mid F].
\end{aligned}$$

In words, it says that there exists a subpopulation of compliers (the compliers-defiers or "comfiers") that has the same size as the subpopulation of defiers and that the average treatment effect for these two subpopulations is the same.

Theorem 2.1 in de Chaisemartin (2016) then applies:

$$\begin{aligned}
&C_V = C \setminus C_F \text{ satisfies} \\
&FS = \Pr(C_V), \\
&W = \mathbb{E}[Y_1 - Y_0 \mid C_V].
\end{aligned}$$

That is, the Wald estimator identifies the treatment effect on the subpopulation of compliers that "survive" elimination with the defiers (these compliers-survivors are the "comfiers"). A sufficient condition for the last theorem to hold is

$$\Pr(F \mid Y_1 - Y_0) \leq \Pr(C \mid Y_1 - Y_0).$$

This last expression says that at any point of the distribution of treatment effects $(Y_1 - Y_0)$, there are more compliers than defiers. This condition is significantly stronger than the necessary conditions since it requires that the distribution of treatment effects for compliers and defiers to fully overlap.

A.2 Other Potential Threats to Identification

To assess whether the relationship between the instrument and the marriage decisions is spurious, I regress the marriage decision on the instrument and dummies for up to 3 years before the introduction of the no-fault divorce laws. The results are presented in table A.1 below. Globally, the dummy variables are indistinguishable from zero.

Table A.1: Robustness of the instrument

	(1)	(2)	(3)	(4)
VARIABLES	Total earnings	Hours worked	Hourly wage	FS
no-fault div				0.231***
				(0.0258)
d1				-0.0463*
				(0.0266)
d2				-0.0200
				(0.0237)
d3				0.0172
				(0.0218)
age	0.0738***	-0.00363	0.0774***	0.0289***
	(0.0119)	(0.00838)	(0.0113)	(0.00664)
age sq	-0.00117***	-0.000322***	-0.000843***	-0.000350***
	(0.000111)	(7.81e-05)	(0.000105)	(6.31e-05)
experience	0.220***	0.164***	0.0558	-0.0252
	(0.0394)	(0.0277)	(0.0372)	(0.0248)
married	0.315*	0.226*	0.0891	
	(0.165)	(0.116)	(0.156)	
Observations	4,519	4,519	4,519	4,519
Number of x11101ll	405	405	405	405
Individual FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
F-statistic for joint null of no effect of d1, d2, d3 is 1.562 (p-value .196).

In addition, I explore whether other potential instruments induce the decision to marry. I restrict my search to variables that a priori may be correlated or be confounded with the introduction of no-fault divorce or that can be thought of as instruments themselves. Table A.2 shows the results of using different variables as instruments for marriage in a regression of total earnings in the lhs. None of the reported coefficients is significant.

Table A.2: Other potential instruments

Variable	FS	IV
contraceptive access	-.002	-32.841
% evangelical pop	.002	-1.040
divorce rate	.005	-6.412
social capital	-.008	-.258
female governor	-.017	4.347
State House ideology	.013	.202