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The Marital Earnings Premium: An IV Approach

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Abstract

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. As such, I exploit exogenous variation in marriage induced by the introduction of no-fault divorce laws in the US. I find a 38% causal increase of marriage on earnings of husbands. 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.

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 new credible estimates of the causal effect of marriage on earnings and providing evidence on the mechanism that produces that effect.

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 shifts with unobservable changes during the lifetime), the fixed effects strategy no longer uncovers a causal effect. The lack of research design (with a few exceptions) could be a major driver of why the literature has not settled on the magnitude of the effect or whether there is a causal effect in the first place.

This paper establishes causality using an instrumental variable approach. 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 have shifted into marriage those men who would not have married under the pre-existing laws. Couples with low match quality may have considered entering marriage as the union could be more easily dissolved than before, thus reducing the costs of marriage *a priori*. At the same time, the passage of no-fault divorce could also have prevented couples considering marriage from forming the union. If marriage is seen as a commitment device, the no-fault legislations weakened its credibility.[1]

To identify the effect of marriage on earnings while considering the opposing effects of the legislations on marriage decisions, I employ a strategy proposed by de Chaisemartin (2017). That strategy obtains a Local Average Treatment Effect (LATE) of marriage on earnings that is robust to the presence of defiers in the treatment group.[2] The price to pay is that the estimated LATE applies only to a subpopulation of compliers.[3] The key identification assumptions are that (1) there exists another subpopulation of compliers equal the the subpopulation of defiers, and that (2) they both have the same average treatment effect. For the first subpopulation of compliers I estimate an increase of 38% in weekly labor earnings after marriage.

There are several possible explanations for the observed gap in earnings. First, it is possible 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 as opposed to household work, allowing men to work harder and longer in their jobs, receiving higher wages later. 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

[1] Chiappori et al. (2015) provide a theoretical argument under which couples may divorce or not depending on the prevailing divorce legislation. What is interesting is that counterintuitive results obtain depending on the realization of individual match qualities and consumption after divorce. For example, a couple may choose to divorce under mutual consent legislation but would choose to remain married under unilateral divorce. Since couples would then internalize the possibility of divorce at the time of marriage, shifting divorce legislation can affect the decision to marry.

[2] 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.

[3] 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 (2017) notes, this subpopulation of compliers is the same size as the population of compliers under the standard LATE.

in a way that rewards married men.

The literature has focused on examining selection. 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. Chun and Lee (2001) find that the marriage wage premium is not explained by selection, but rather is due to specialization within the household. However, Ginther and Zavodny (2001) analyze shotgun weddings (which are weddings arranged following an unintended pregnancy). Under the assumption that premarital conception followed by marriage is random, a comparison between men who were shotgun-married with single men should provide a causal effect of marriage on earnings. They find that men with shotgun marriages earn 15% more than never-married men. Howerver, they conclude that less than 10% of the marriage premium remains after controlling for selection. Similarly, 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.

Other papers find little to no effect of marriage. Loughran and Zissimopoulos (2009) find that marriage lowers the wage growth of men by between 2 and 4 percentage points. Similarly, Killewald and Lundberg (2017) argue that changes in wages predate changes in marital status (both entry into marriage and divorce), and therefore there is no causal effect of marital status on wages.

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 in hours worked. Ahituv and Lerman (2007) is an exception. 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. This paper contributes to that literature by decomposing 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. I find that after marriage, hours of work increase 25%, and that also there is an increase of 13% in weekly wages. Furthermore, I find suggestive evidence that after marriage, men spend less time in housework. My findings are compatible with a story emphasizing the specialization of men, and therefore with a causal interpretation of the marital earnings premium.

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), since men perceive an increase in earnings following marriage, in contrast to women who do not. It helps in understanding the determinants of individual wages (Loh, 1996). It has also been considered as a mechanism to address child poverty (Lerman, 1996), since it is hypothesized that married men have a stronger commitment to find a good job and work, which translates into improvements in child poverty. Finally, to the extent that the 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).

The rest of the paper is organized as follows: Section 2 documents the divorce reforms, Section 3 discusses how the divorce reforms affected marriage decisions, Section 4 describes the data and sample used in the estimation, Section 5 explains the estimation strategy and the results are presented in Section 6, and Section 7 concludes.

2 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.

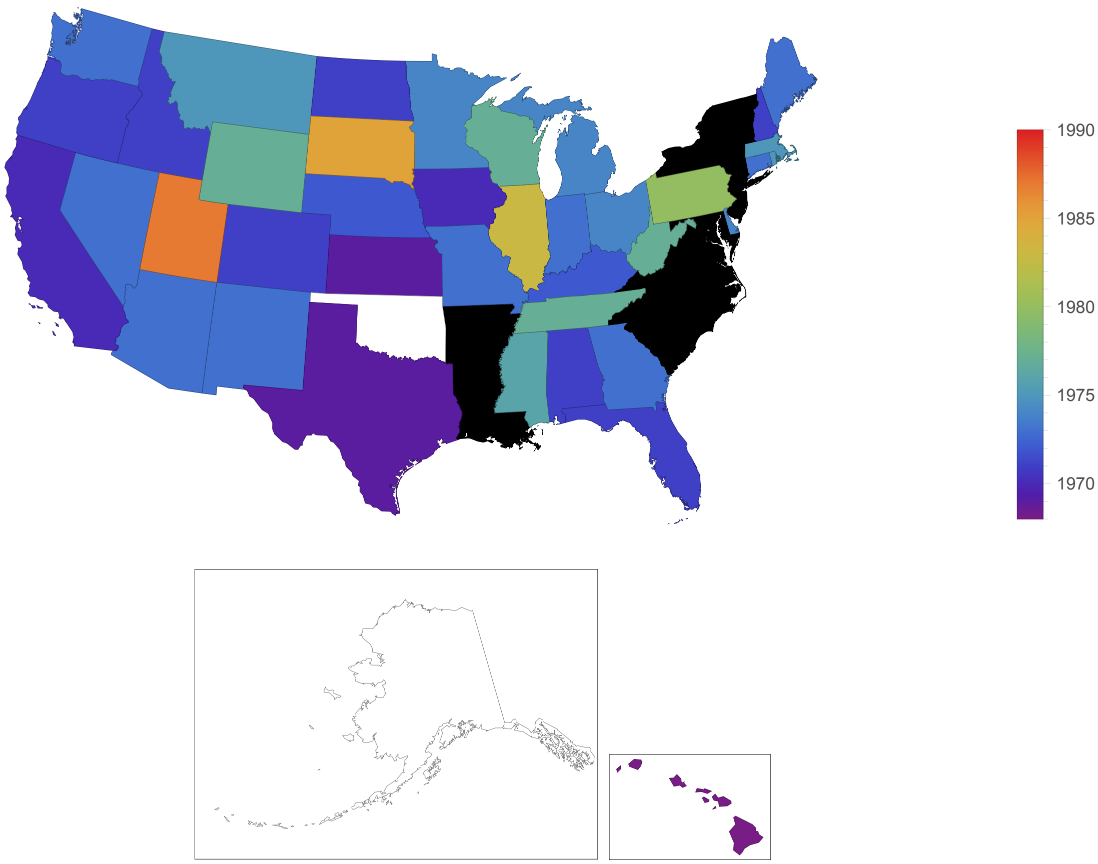
In the 1970s and early 1980s, several states introduced no-fault divorce clauses to their existing divorce regimes. Before these laws were passed, typically a divorce was granted on the grounds of wrongdoing by one of the spouses. Such grounds included adultery, cruelty, abandonment, mental illness, 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 by state, 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). All of that has led to a numbers of researchers to argue that the changes in divorce legislation were exogenous with respect to the behavior of people married or in the marriage market (Friedberg, 1998; Gruber, 2004; Wolfers, 2006).

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

In particular, Friedberg (1998) finds that "state characteristics did not influence the timing of the legal change." In the case of this article, a potential concern is the correlation of earnings or other policies that could affect earnings with the enactment of no-fault divorce legislation. I address such threats to identification in section 5.2.1.

3 The Role of the Divorce Reforms

Here I discuss how the divorce reforms affect the formation of marriages. I rely on the variation in marriage induced by the change in divorce legislation as a source of identification for the effect of marriage on earnings. The ambiguities on the effect of divorce legislation on marriage decisions, discussed below, motivate the empirical strategy discussed in Section 5.

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).

Another branch of the literature emphasizes the contractual nature of marriages. That literature identifies two effects of lowering divorce costs. First, there is an "incentive effect" of lower divorce costs that induces *already married* couples to divorce. Second, there is a "selection effect" that affects the *composition* of couples who decide to marry in the first place. The selection effect has ambiguous consequences for couples who end up in a marriage. First, individuals know that they can dissolve a marriage if it is beneficial to them. But also individuals may find themselves in a marriage in which their spouse would prefer to leave. Rasul (2006) employs a model of search and learning in marriage markets without transferable utility to find that moving from mutual consent to unilateral divorce has those opposing effects on the incentives to marry.[7]

Similarly, Matouschek and Rasul (2008) examine how different reasons for marriage are affected by a reduction in the costs of divorce. In particular they examine three hypotheses: marriage as a source of utility, marriage as commitment device (which fosters cooperation between spouses or induces relationship-specific investments), and marriage as signaling device (in which partners signal their true love). They find that regardless of which hypothesis prevails, lower divorce costs increase divorces for couples already married. However, the results of the changing composition of marriages that form under lower divorce costs is heterogeneous across hypotheses. If marriage serves as a commitment device, lower divorce costs can prevent couples of low quality from marrying in the first place, reducing marriage overall.[8] For the other two hypotheses, the effect is the opposite, lower quality couples would marry more often under lower costs of divorce, increasing marriage overall.[9]

Regarding divorce decisions, a prima facie, it may look like the no-fault divorce legislation can induce couples to divorce. In this 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 (at least in the long run), but will affect the intra-household distribution of power and utility in subsequent marriages.[10] 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

[7]He also finds that unilateral divorce increased selection into marriage, and potentially decreased divorce rates in the long run.

[8]Under the hypothesis of marriage as commitment, lower divorce costs would also lead to lower propensity to divorce because only high match quality couples would marry in the first place.

[9]The effect on divorce propensity is also the opposite, as the average match quality of couples would be lower, therefore increasing the propensity to divorce.

[10]See Wolfers (2006) for review and discussion of the empirical literature, and see Chiappori and Mazzocco (2017) for a review of the collective theory.

efficient decisions, the new allocation will convince partners to remain married.[11] 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.

What about the effect of the divorce legislation on the marital premium itself? It is hard to think that divorce legislation affects earnings of men, except through their effect on marriage. However, it is plausible that the size of the effect (the average marital premium) changed with the legislation. That is, it is possible that the marital earnings premium was made larger or smaller (if it indeed existed) with the introduction of no-fault divorce. However, the empirical strategy used in this paper, described in Section 5, can only identify the average treatment effect of marriage only for a subpopulation of those who married because of the new divorce legislation.[12] That represents an important limitation of this paper.

4 Data

This paper uses data from the Panel Study of Income Dynamics (PSID) from 1968 to 1993. I consider working males between ages 16 to 60 who have completed their education and who are single the year before the enactment of no-fault divorce legislation in their state of residence.[13],[14] The data also include information on age, education, state of residence, marital status, total labor earnings and hours of work. I also include data on Gross State Product.

[11]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.

[12]The details of that subpopulation are contained in Section 5. A characterization of that subpopulation is contained in Section 6.1.

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

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

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, which means that men could have been married before the reforms but divorced by the time the reforms were passed, hence 25% of the sample were married at some point before the reforms. The variable *exp* represents cumulative hours of experience, therefore its mean 18223 is equivalent to roughly 9 years of experience, and 25358 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.

Table 1: Descriptive statistics.

Variable	Mean	Std. Dev.	Min	Max
Before reforms				
age	33.40	9.64	20	60
married	0.24	0.43	0	1
labor earn	685.23	490.51	106.21	3224.48
exp	18223.36	13281.22	0	56789
work hrs	39.59	13.91	6.73	90
After reforms				
age	36.18	8.07	19	60
married	0.76	0.42	0	1
labor earn	749.15	482.48	100.02	4215.63
exp	25358.06	17065.92	232	92827
work hrs	41.56	12.17	4.46	112.31
Overall				
age	35.93	8.26	19	60
married	0.72	0.45	0	1
labor earn	743.55	483.47	100.02	4215.63
exp	24732.57	16888.08	0	92827
work hrs	41.38	12.35	4.46	112.31
individuals	405			
total observations	4517			

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: Distribution of total labor earnings per week by marital status.

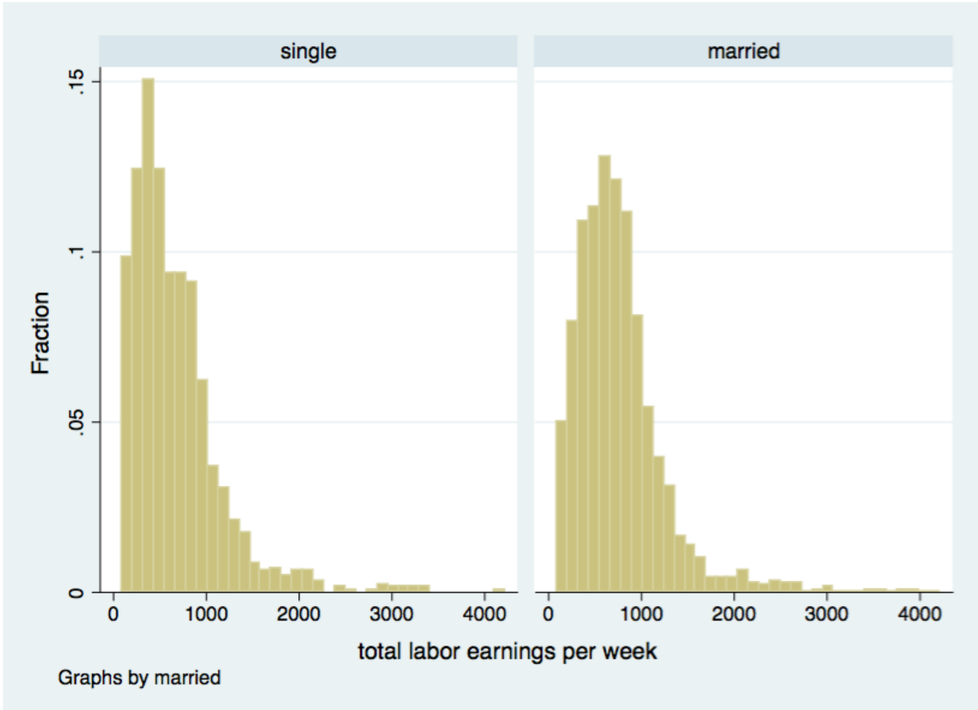


Figure 3: Distribution of total hours of work per week by marital status.

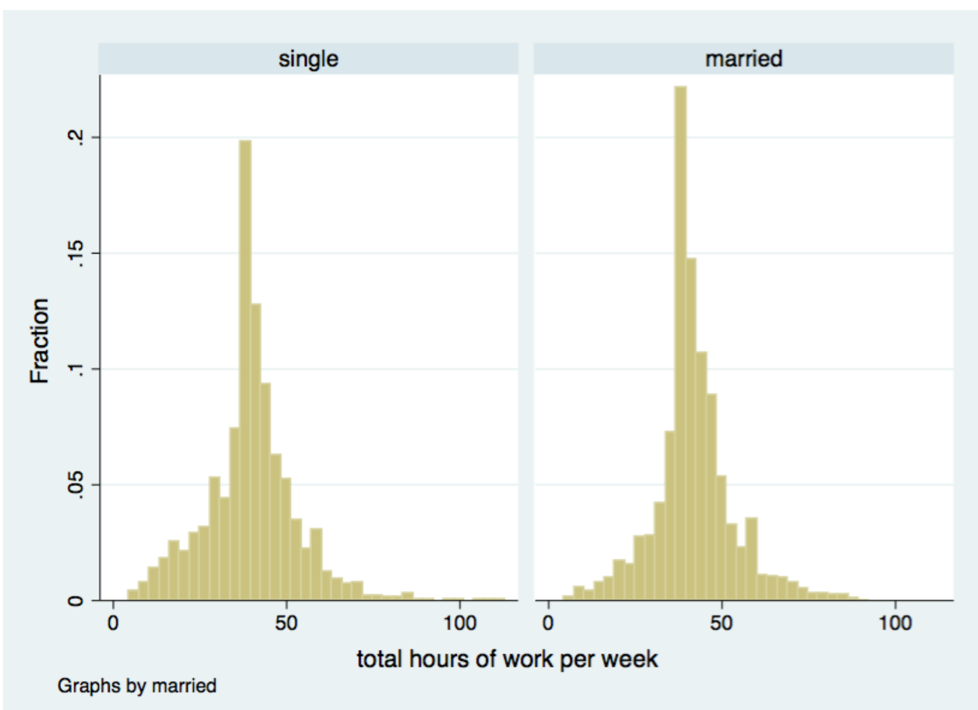
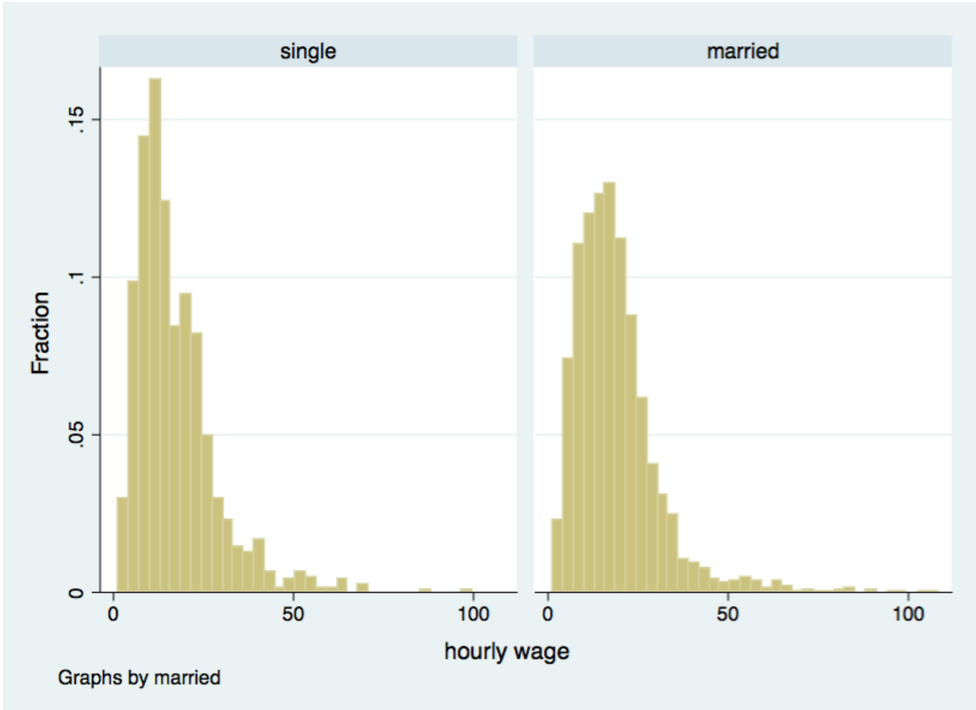


Figure 4: Distribution of hourly wages by marital status.



5 Empirical Strategy

This section discusses the estimation strategy. The results are presented in the next section.

5.1 The Standard Approach

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, 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 unlikely to be 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).

5.2 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

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

$$marr_{ist} = a \, z_{st} + b \cdot x_{ist} + d_i + \ell_t + g_s + e_{ist}, \tag{3}$$

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 previous sections, 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 (2017). He relaxes the monotonicity assumption of the standard LATE. Under the assumptions that (1) there is a subpopulation of compliers that have the same treatment effect of defiers, and that (2) the group of defiers and that subpopulation of compliers have the same size, he is able to identify the average treatment effect for the rest of the population of compliers (this subpopulation of compliers is called the compliers-survivors or "comvivors").[15] In the Appendix, I go into more detail and sketch the main result in de Chaisemartin (2017). For now, it is important to note that this subpopulation of "comvivors" under the assumptions of de Chaisemartin (2017) 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 for a subpopulation of compliers (de Chaisemartin, 2017).[16]

5.2.1 Potential Threats to Identification

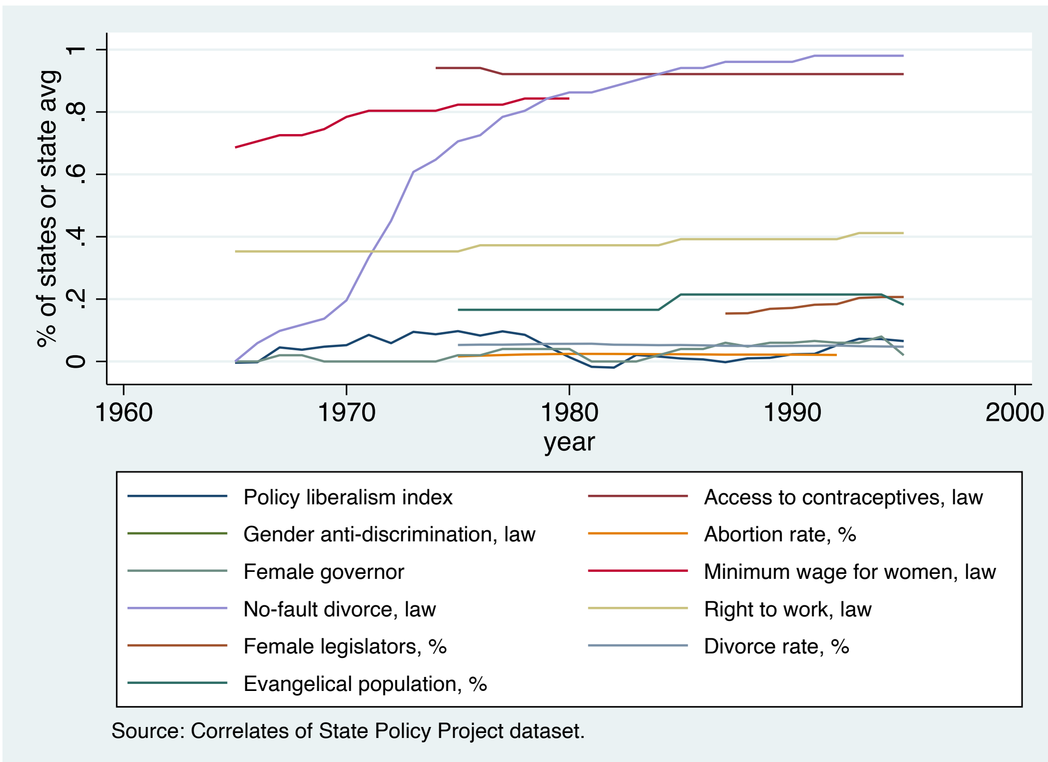
The validity of changes in no-fault divorce laws as an instrument relies on the exclusion restriction, that is, that those changes are not correlated with the error term in the structural equation (4). The exclusion restriction would be violated if both the instrument and the marriage decision were influenced 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

[15] The Appendix contains an empirical check of the testable implications arising from the assumptions required for the identification of the LATE with defiers.

[16] If there are no defiers in the population, then the estimated effects in this paper would have the interpretation of a standard LATE as in Imbens and Angrist (1994).

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.[17] The graph shows the share of states implementing the policies enumerated, or the average level of state characteristics.[18] It is clear from the graph that no-fault legislations seem to be independent of the other potential factors that could confound the results, as most lines are rather flat.

Figure 5: Distribution of potential confounding factors 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, wages, and weekly hours of work than late adopters of the reforms. Figures 6, 7, and 8 plot the mean log of labor income, wages, and weekly hours of work, purged of time and state effects, for single and married men as a function of years relative to the introduction of the divorce reforms,

[17] Miller (2008) uses the same approach in his study of suffrage rights for women and their effect on child survival.

[18] These are "progressive" policies that could confound the effect of no-fault divorce legislation.

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

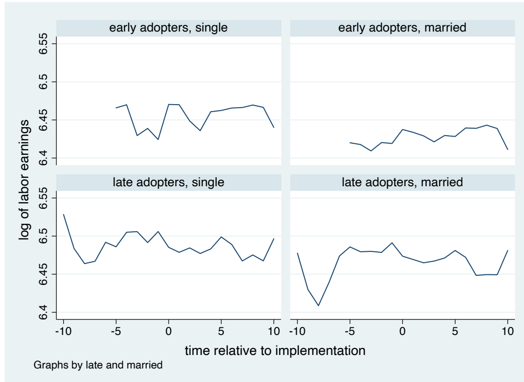
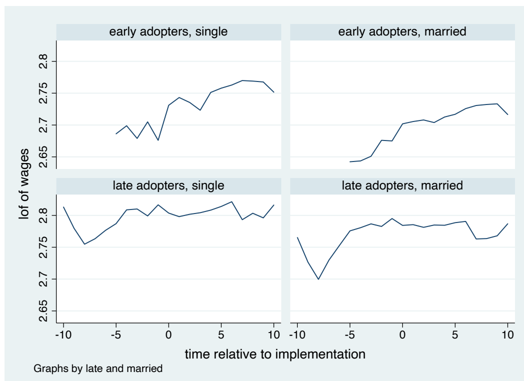


Figure 7: Log of wages by early/late adoption.

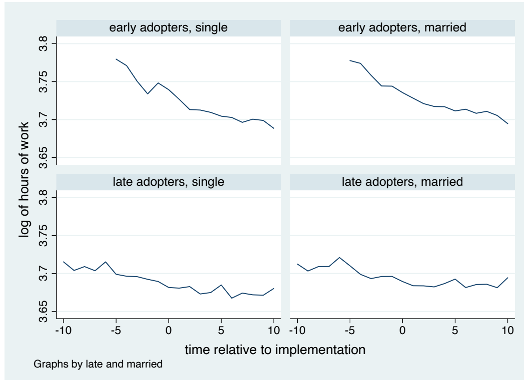


for states that adopted no-fault divorce laws before 1973 (early adopters), the median year of adoption, and those who adopted them from 1974 onwards (late adopters). The idea is to assess whether there were changes in those variables at the time of the reforms and whether those trends were different in states that were early adopters compared to states that were late adopters. Although the graphs are noisy, there are no striking differences between the groups, especially between singles.

6 Results

Table 2 presents the results of different specifications for the first stage regression. The instrument induces marriage between 23% and 51% of the cases and those estimates are highly significant. From a technical point of view, it means that the instrument is relevant

Figure 8: Log of weekly hours of work by early/late adoption.



for the subpopulation under examination.[19] 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 some 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 with respect to age.

[19] The table also includes the Sanderson-Windmeijer χ^2-test of under identification and F-test of weak identification (Sanderson and Windmeijer, 2016). The χ^2-statistic is distributed $\chi^2(1)$ under the null hypothesis of underidentification of the endogenous regressor. The F-statistic is distributed $F(1, n-k)$ under the null of weak identification of the endogenous regressor, where k is the number of exogenous regressors.

Table 2: First stage, predicting marriage.

VARIABLES	(1) OLS	(2) FE1	(3) FE2
no-fault div	0.507***	0.243***	0.231***
	(0.0222)	(0.0323)	(0.0237)
age	0.0702***	0.0254***	0.0240***
	(0.00567)	(0.00612)	(0.00679)
age sq	-0.000865***	-0.000363***	-0.000348***
	(7.25e-05)	(7.64e-05)	(6.27e-05)
std(exp)	-0.0791***	-0.0919***	-0.0194
	(0.00701)	(0.00712)	(0.0253)
real GSP	-0.0213***	-0.0509	0.201***
	(0.00704)	(0.0767)	(0.0579)
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
Sanderson-Windmeijer under id		57.75***	105.62***
Sanderson-Windmeijer weak id		56.89***	94.65***

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)	0.00220	0.0209**	0.226***	0.0298	0.234***
	(0.0102)	(0.0106)	(0.0393)	(0.0212)	(0.0403)
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 lifetime labor earnings by 38% on average for the subpopulation of comvivors.[20]

One interesting aspect in Table 3 is that the OLS estimates of the effects of marriage in columns 1, 2 and 3 are smaller than both IV estimates in columns 4 and 5. One possibility is that the OLS estimates reflect attenuation bias due to measurement error.[21] That attenuation bias is overcome by using the IV strategy. Another more fundamental explanation is

[20]With Assumptions 1, 2 and 3 in Section A.2, one can also derive nonparametrically partially identified worst case bounds for the LATE. Table A.4 in the Appendix shows that using that approach the treatment effect for comvivors is between -1.3 and 1.6.

[21]The data is captured through a survey but it is unlikely that subjects misreport whether they are married, nor their age, so the only plausible sources for measurement error are experience and the outcomes (earnings, hours of work and wages).

that the OLS estimates reflect omitted variable bias. If we assume that there is one single omitted variable in the OLS regressions, then it must necessarily be such that the correlation between the omitted variable and marriage, and the omitted variable and earnings are of opposite signs (given other covariates). For example, high tastes for work would satisfy that condition as presumably it is negatively correlated with marriage (because the husband would spend more time at work and not at home or searching for a potential mate) but positively correlated with earnings (through higher hours of work).

The analysis of the bias becomes more complex if one admits the existence of more than one omitted variable in the OLS regressions.[22] In addition to tastes for work, consider also productivity as an omitted variable in the OLS regressions. One must then make statements about the relative correlations between earnings and productivity, marriage and productivity, and the relative strength between the interactions of those correlations vis-à-vis the interaction between the correlations mentioned above. Presumably, earnings and productivity are positively correlated, however it is hard to think about the correlation between productivity and marriage, except that, either positive or negative, it is likely smaller in absolute value than the correlation between marriage and tastes for work. However, without knowing the relative effect of productivity and tastes for work on earnings, it is hard to determine with precision what drives the sign of the bias to be negative.[23]

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

[22] Basu (2019) shows that the asymptotic bias ($\boldsymbol{\delta}^j \boldsymbol{\gamma}$) is equivalent to $\|\boldsymbol{\delta}^j\| \|\boldsymbol{\gamma}\| \cos(\theta)$, where $\boldsymbol{\gamma}$ is a vector of the coefficients of omitted variables in the causal equation, $\boldsymbol{\delta}^j$ is a vector of the coefficients of projecting the omitted variable indexed by j onto the included regressors in the causal equation, and θ is the angle between those two vectors in the Euclidean space. He notes that if $\boldsymbol{\delta}^j$ and $\boldsymbol{\gamma}$ are both nonzero but are neither orthogonal, nor lie in the same or in opposite orthants, then the direction of bias cannot be determined based on the signs of partial effects alone.

[23] Similar analyses apply for comparing the OLS and IV estimates in Table 4 and Table 5 below.

by 25% and it is statistically significant. A 25% increase in weekly hours of work is equivalent to an increase of 10 hours per week (2 hours per day) for a work week of 40 hours.[24]

Table 4: Hourly earnings.

VARIABLES	(1) OLS	(2) OLS FE1	(3) OLS FE2	(4) IV FE1	(5) IV FE2
married	0.0660***	0.130***	0.0423*	-0.0428	0.126
	(0.0197)	(0.0212)	(0.0231)	(0.189)	(0.153)
age	0.0823***	0.0901***	0.0751***	0.0937***	0.0723***
	(0.00805)	(0.00867)	(0.0100)	(0.00951)	(0.0112)
age sq	-0.000864***	-0.000932***	-0.000850***	-0.000987***	-0.000825***
	(0.000103)	(0.000108)	(9.33e-05)	(0.000123)	(0.000104)
std(exp)	-0.0516***	-0.0362***	0.0610	-0.0527**	0.0633*
	(0.00989)	(0.0103)	(0.0376)	(0.0207)	(0.0379)
real GSP	0.0817***	0.120	0.135	0.0916	0.124
	(0.00983)	(0.107)	(0.0854)	(0.111)	(0.0877)
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	0.0670***	0.0763***	0.0332**	0.341***	0.253**
	(0.0113)	(0.0124)	(0.0169)	(0.115)	(0.114)
age	0.0139***	0.0173***	-0.000397	0.0118**	-0.00764
	(0.00461)	(0.00509)	(0.00735)	(0.00582)	(0.00838)
age sq	-0.000192***	-0.000234***	-0.000375***	-0.000151**	-0.000308***
	(5.89e-05)	(6.37e-05)	(6.83e-05)	(7.55e-05)	(7.79e-05)
std(exp)	0.0538***	0.0570***	0.165***	0.0825***	0.171***
	(0.00566)	(0.00604)	(0.0275)	(0.0127)	(0.0283)
real GSP	-0.0151***	0.0323	0.138**	0.0756	0.110*
	(0.00563)	(0.0628)	(0.0625)	(0.0681)	(0.0654)
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

[24]The results are robust to regressions with state trends. See Table A.5 in the Appendix.

6.1 Characterization of Comvivors

One can recover the mean of any covariate X for the subpopulation of comvivors (de Chaisemartin, 2017). If apart from assumptions (1), (2) and (3) in Section A.2, one is ready to assume that

$$\mathbb{E}[X \mid C_F] = \mathbb{E}[X \mid F],$$

we have

$$\mathbb{E}[X \mid C_V] = W_{XD},$$

where

$$W_{XD} = \frac{\mathbb{E}[XD \mid Z=1] - \mathbb{E}[XD \mid Z=0]}{\Pr(D=1 \mid Z=1) - \Pr(D=1 \mid Z=0)}.$$

With these results one can characterize the covariates for the subpopulation of comvivors. I perform that analysis for the covariates age, experience, and individual fixed effects. The results are displayed in Table 6. Comvivors are older and have more experience than the average individual. Their fixed effects, however, are smaller relative to the full population.

We can go a bit further. Make use of the sample mean, the result above and the fact that the first stage identifies the proportion of comvivors C_V to arrive at the joint average X for defiers F, always takers A and never takers N. The expected value of variable X can be decomposed as

$$\begin{aligned}
\mathbb{E}[X] &= \Pr(C_V)\,\mathbb{E}[X \mid C_V] + (1-\Pr(C_V))\,\mathbb{E}[X \mid \overline{C_V}] \\
&\Rightarrow \frac{\mathbb{E}[X] - \Pr(C_V)\,\mathbb{E}[X \mid C_V]}{1-\Pr(C_V)} = \mathbb{E}[X \mid \overline{C_V}] = \mathbb{E}[X \mid C_F \cup F \cup A \cup N].
\end{aligned}$$

The left hand side of the last equation has sample counterparts, which implies that the right hand side is identified. Table 6 also shows the results of the analysis to the rest of the groups in terms of age, experience and individual fixed-effects. We can see that comvivors are 2.87 years older than the average individual who is not a comvivor.

Table 6: Analysis of covariates.

Covariate	Mean for comvivors C_V	Mean for full sample	Implied mean for C_F, F, A, and N
age	38.14	35.93	35.27
exp(std)	0.12	0.00	-0.04
individual FE	-0.05	0.00	0.02

The implied average value of experience excluding comvivors is -0.04. That means that, excluding comvivors, all other individuals have experience 0.04 standard deviations below the mean and that comvivors are 0.16 standard deviations above non-comvivors. The implied average for the individual fixed effects excluding comvivors is 0.02.

The most striking result of this analysis is that there is a clear delineation between comvivors, for whom I have estimated a (local) average treatment effect, and the rest of the individuals. It is reassuring that comvivors also have accumulated more experience, since that is the expectation for older individuals. Note, however, that these results follow with the rather restrictive assumption that a portion of the compliers have the same average value of the covariates as the defiers, therefore the interpretation of this result is not without difficulties.

6.2 Discussion and Mechanisms

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 of the divorce reforms and had completed their education, this group is the "initial" population of single men. There could be concerns about the effects of the new divorce regime in the marriage market on the prospects of marriage for those initial single men. In general, compositional effects in the pool of men in the marriage market, due to the change in divorce legislation, 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.[25] 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 younger entrants into the marriage market coming from having completed their education. These recent graduates are 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[26] somewhat diminishes those effects, as the initial population of single men will be relatively older. Disentangling both of the concerns described above would require a dynamic equilibrium framework of the marriage market and is left for future research.

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 in the labor market and hours of housework. This provides a mechanism through which the earnings premium may operate and is consistent with a story in which men specialize in labor market work after marriage, at least for the

[25] See Chiappori and Mazzocco (2017) for a full discussion.
[26] See for example Choo and Siow (2006) and Choo (2015) for some results.

portion of compliers for whom I estimate the treatment effect.[27] To examine the possible specialization of men within marriage I look at hours of housework after marriage. Table 7 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,[28] they are compatible with a story emphasizing the specialization of men after marriage.

Table 7: Weekly hours of housework.

VARIABLES	(1) OLS	(2) OLS FE1	(3) OLS FE2	(4) IV FE1	(5) IV FE2
married	-0.103*	-0.267***	-0.116	-0.488	-0.240
	(0.0526)	(0.0580)	(0.0794)	(0.490)	(0.501)
age	0.102***	0.0345	-0.00667	0.0393	0.00139
	(0.0218)	(0.0230)	(0.0437)	(0.0251)	(0.0543)
age sq	-0.00147***	-0.000641**	0.000260	-0.000710**	0.000215
	(0.000282)	(0.000294)	(0.000362)	(0.000327)	(0.000405)
std(exp)	-0.00222	-0.0256	0.138	-0.0570	0.116
	(0.0323)	(0.0341)	(0.177)	(0.0771)	(0.197)
real GSP	0.0264	-0.0788	-0.260	-0.125	-0.234
	(0.0273)	(0.339)	(0.326)	(0.351)	(0.342)
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

7 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.

[27]This does not imply, however, that women specialize in house work. Specialization of one spouse does not necessarily imply a specialization of the other spouse.

[28]Partly due to a smaller sample size (see Section A.6 in the Appendix for evidence that low statistical power can explain the low precision of the estimates).

Although several mechanisms have been proposed to explain the gap in earnings, none has withstood 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, for a fraction of compliers, marriage increases lifetime earnings by 38% over single men over their lifetimes. I further decompose that number into an increase in hourly wages of 13% (not statistically significant), and an increase in time spent working in the labor market of 25%. My results are compatible with a specialization story in which men who marry spend more time working in the labor market, 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.

In general, my work lends further credence to a causal interpretation of the marital earnings premium in line with previous research. For example, Cornwell and Rupert (1997) find a 5% to 7% effect on wages, and Korenman and Neumark (1991) report a 6% increase. Those effects on wage rates are not far from mine. The effect on hours of work and total earnings represent a point of departure of my work relative to previous work. Ahituv and Lerman (2007) find that marriage increases hours of work by 160 per year, resulting in an increase of 15.9% in annual earnings. That represents less than half of what I find. While, a portion of that difference is certainly attributable to different methodologies and datasets, further research is required to explain the divergence. A promising, but more structural, approach is offered by integrating the advances in the solution and estimation of dynamic models of marriage with those of dynamic models of within-household behavior.

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A Appendix

A.1 A Simple Framework of Marriage and Divorce

This section provides a framework of marriage and divorce for analyzing the effect of divorce legislation on marriage decisions.[29] Consider individual utilities for partners 1 and 2:

$$U_i = u_i(q_i, Q) + \theta_i, \quad \text{for } i = 1, 2, \tag{A.1}$$

where q_i is individual consumption, Q is public good consumption, and θ_i is the quality of the match which is assumed to be independent between partners and independent of their incomes. Agents live for two periods. In the first period, agents marry and consume. The quality of the match is revealed at the end of the first period and agents decide whether to remain married or divorce. In the second period, couples who remained married consume as before, while members of couples who divorced consume as newly divorced singles. Under Pareto efficiency (in a collective model), the economic gain from marriage is

$$G(t) = \max \left\{ \sum_i u_i(q_i, Q) \text{ s.t. } p \cdot \sum_i q_i + P \cdot Q = t \right\}, \tag{A.2}$$

and the total gain from marriage is $G(t) + \sum_i \theta_i$, where t is total income. In addition, let $u_i^D(q_i, Q)$ denote utility when $i = 1, 2$ is divorced. If and when agents divorce, they each maximize their respective divorce utilities subject to their respective budget constraints. To that end, let y_1 and y_2 denote their respective individual incomes. In the case of divorce, the woman gets $d_1(y_1, y_2) = d(y_1, y_2)$, and the man gets $d_2(y_1, y_2) = y_1 + y_2 - d(y_1, y_2)$. The indirect utility function is

$$v_i^D(d_i) = \max \left\{ u_i^D(q_i, Q) \text{ s.t. } p \cdot q_i + P \cdot Q = d_i \right\}. \tag{A.3}$$

[29] This section uses the basic setup and notation from Chiappori (2017, Section 5.5).

In the second period, partners divorce if the total surplus is larger when divorced than when married, that is

$$\sum_i v_i^D(d_i(y_1, y_2)) > G(y_1 + y_2) + \sum_i \theta_i, \tag{A.4}$$

and remain married otherwise.

From the inequality above, we can easily compute the probability of divorce as follows. Let F be the CDF of $\theta_1 + \theta_2$, then

$$\Pr(\text{divorce}) = F\left(\sum_i v_i^D(d_i(y_1, y_2)) - G(y_1 + y_2)\right). \tag{A.5}$$

Notice that the probability of divorce depends on y_1, y_2, and d, so we can write $\Pr(\text{divorce}) = \Pi(y_1, y_2, d, v^D)$, where $v^D = (v_1^D, v_2^D)$.

In the first period agents marry based on the expected surplus generated by marriage in both periods, but taking into consideration the probability of divorce. Let $\theta = \theta_1 + \theta_2$, the total gains realized from marriage in both periods is

$$\begin{aligned}\bar{G}(y_1 + y_2) =& G(y_1 + y_2) + \theta \\ &+ \beta(1 - \Pi(y_1, y_2, d, v^D))(G(y_1 + y_2) + \theta) \\ &+ \beta\Pi(y_1, y_2, d, v^D) \sum_i v_i^D(d_i(y_1, y_2)).\end{aligned} \tag{A.6}$$

Since θ is not observed prior to marriage, we can take the expectation of $\bar{G}$ to obtain the expected gains from marriage (but prior to marriage):

$$\begin{aligned}
&\mathbb{E}[\bar{G}(y_1+y_2)] = \\
&\quad G(y_1+y_2) + \mathbb{E}[\theta] \\
&\quad + \beta(1-\Pi(y_1,y_2,d,v^D))\left(G(y_1+y_2) + \mathbb{E}\left[\theta \mid \theta \geq \sum_i v_i^D(d_i(y_1,y_2)) - G(y_1+y_2)\right]\right) \\
&\quad + \beta\Pi(y_1,y_2,d,v^D)\sum_i v_i^D(d_i(y_1,y_2)).
\end{aligned} \tag{A.7}$$

We are interested in analyzing the effect of divorce legislation on marriage decisions. Marriage decisions are embodied in $\mathbb{E}[\bar{G}]$, the expected gains from marriage. Larger gains induce higher propensity for marriage. Divorce legislation is harder to pin in this general framework, however we can think about divorce legislation affecting the utility of parters after divorce, that is v_i^D. In that regard, we can take the derivative of the expected marriage gains, which determine marriage decisions, with respect to utilities after divorce, which act as the channel through which divorce legislation affects marriage decisions and represents welfare after divorce. The total effect of v_i^D on $\mathbb{E}[\bar{G}(\cdot)]$ can be decomposed as

$$\frac{\partial\,\mathbb{E}[\bar{G}(\cdot)]}{\partial v_i^D(\cdot)} = \frac{\partial\,\mathbb{E}[\bar{G}(\cdot)]}{\partial \sum_i v_i^D(\cdot)}\frac{\partial \sum_i v_i^D(\cdot)}{\partial v_i^D(\cdot)}. \tag{A.8}$$

The derivative in the first term has a simple expression: $\frac{\partial\,\mathbb{E}[\bar{G}(\cdot)]}{\partial \sum_i v_i^D(\cdot)} = \beta\Pi(\cdot)$.[30] Then we have:

$$\frac{\partial\,\mathbb{E}[\bar{G}(\cdot)]}{\partial v_i^D(\cdot)} = \beta\Pi(\cdot)\frac{\partial \sum_i v_i^D(\cdot)}{\partial v_i^D(\cdot)}. \tag{A.9}$$

Equation (A.9) says that the effect of divorce legislation on (the gains of) marriage depends on how the legislation changes total welfare after divorce, and that change should be weighted

[30] See A.1.1 for details.

by the probability of divorce and discounted over time. The second term of equation (A.9) will depend on the particular divorce legislation under analysis. The change from mutual consent to no-fault divorce clearly increases the post-divorce utility for the spouse that initially has less bargaining power, but the other spouse can experience a decrease in their post-divorce utility. In general, divorce legislation will make $v_1^D(\cdot)$ and $v_2^D(\cdot)$ interdependent, for example increasing one spouse's utility at the expense of the other, which results in a non-trivial calculation of the derivative that could result in a positive or negative overall effect on the expected gains from marriage and therefore on the marriage decision.[31]

In particular, Chiappori et al. (2015) show that there are cases where mutual consent induces couples to divorce when no-fault would sustain the marriage, and vice versa. We can then conclude that the effect of divorce on marriage depends on the utilities at divorce of both partners, and that effect can go in one direction or another. Moreover, since the only source of heterogeneity in this simple model is individual incomes, a richer model would induce different types of ambiguity in the effects of divorce on marriage.

[31] v^D will also depend on remarriage probabilities in the marriage market, but I am abstracting from that in this simple model.

A.1.1 Proof of equation (A.9)

We take the derivative of $\mathbb{E}[\bar{G}(\cdot)]$ with respect to $\sum_i v_i^D(\cdot)$:

$$\begin{aligned}
\frac{\partial\, \mathbb{E}[\bar{G}(\cdot)]}{\partial \sum_i v_i^D(\cdot)} =& \beta(1-\Pi(\cdot))\frac{\partial}{\partial \sum_i v_i^D(\cdot)}\,\mathbb{E}\left[\theta \mid \theta \geq \sum_i v_i^D(\cdot) - G(\cdot)\right] \\
& - \beta\Pi'(\cdot)\left(G(\cdot) + \mathbb{E}\left[\theta \mid \theta \geq \sum_i v_i^D(\cdot) - G(\cdot)\right]\right) \\
& + \beta\Pi(\cdot) + \beta\Pi'(\cdot)\sum_i v_i^D(\cdot) \\
=& \beta(1-\Pi(\cdot))\frac{\partial}{\partial \sum_i v_i^D(\cdot)}\,\mathbb{E}\left[\theta \mid \theta \geq \sum_i v_i^D(\cdot) - G(\cdot)\right] \\
& - \beta\Pi'(\cdot)\left(G(\cdot) - \sum_i v_i^D(\cdot) + \mathbb{E}\left[\theta \mid \theta \geq \sum_i v_i^D(\cdot) - G(\cdot)\right]\right) \\
& + \beta\Pi(\cdot).
\end{aligned} \tag{A.10}$$

Let $A(v^D) = \{\theta : \theta \geq \sum_i v_i^D(\cdot) - G(\cdot)\}$. Then the conditional expectation in equation (A.7) can be simplified.

$$\begin{aligned}
&\mathbb{E}\left[\theta \mid \theta \geq \sum_i v_i^D(d_i(y_1, y_2)) - G(y_1 + y_2)\right] \\
&= \frac{\mathbb{E}[\theta \mathbb{1}_{A(v^D)}]}{\Pr(A(v^D))} \\
&= \frac{\mathbb{E}[\theta \mathbb{1}_{A(v^D)}]}{1 - \Pi(y_1, y_2, d, v^D)} \\
&= \frac{\int_{\sum_i v_i^D(\cdot) - G(\cdot)}^{\infty} \theta \,\mathrm{d}F(\theta)}{1 - \Pi(y_1, y_2, d, v^D)}.
\end{aligned} \tag{A.11}$$

Taking the derivative of the last expression we obtain

$$\begin{aligned}
\frac{\partial}{\partial \sum_i v_i^D(\cdot)} \mathbb{E}\left[\theta \mid \theta \geq \sum_i v_i^D(\cdot) - G(\cdot)\right] \\
&= \frac{-(1-\Pi(\cdot))(\sum_i v_i^D(\cdot) - G(\cdot))F'(\sum_i v_i^D(\cdot) - G(\cdot)) + \int_{\sum_i v_i^D(\cdot)-G(\cdot)}^{\infty} \theta \,\mathrm{d}F(\theta)\,\Pi'(\cdot)}{(1-\Pi(\cdot))^2} \\
&= \frac{-(1-\Pi(\cdot))(\sum_i v_i^D(\cdot) - G(\cdot))\Pi'(\cdot) + \mathbb{E}[\theta \mathbb{1}_{A(v^D)}]\Pi'(\cdot)}{(1-\Pi(\cdot))^2} \\
&= \frac{\Pi'(\cdot)}{1-\Pi(\cdot)} \left(\mathbb{E}[\theta \mid A(v^D)] - \sum_i v_i^D(\cdot) + G(\cdot)\right).
\end{aligned} \tag{A.12}$$

Plugging that last expression into equation (A.10) we obtain

$$\frac{\partial\,\mathbb{E}[\bar{G}(\cdot)]}{\partial \sum_i v_i^D(\cdot)} = \beta \Pi(\cdot). \tag{A.13}$$

A.2 A Local Average Treatment Effect with Defiers

This section sketches the main result in de Chaisemartin (2017), 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

(2017) relaxes these conditions to:

$$\begin{aligned}&\text{(3) There exists a subpopulation of compliers } C_F \text{ such that}\\&\Pr(C_F) = \Pr(F),\\&\mathbb{E}[Y_1 - Y_0 \mid C_F] = \mathbb{E}[Y_1 - Y_0 \mid F].\end{aligned}$$

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 (2017) 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.3 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.

VARIABLES	(1) Total earnings	(2) Hours worked	(3) Hourly wage	(4) 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 individuals	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

Another potential threat to identification arises from different compositions in the population of men across different states. Specifically, I assess whether there are differences in the ages of single men in states that enacted no-fault divorce laws early and states that enacted no-fault divorce laws later. I regress age on a dummy that indicates early enactment (before 1973), while controlling for time fixed effects. The results are shown in Table A.3 below. The coefficient on *early* is not statistically different from zero.

Table A.3: Comparison between early and later adopters.

	(1)
VARIABLES	age
early	0.699
	(0.521)
Observations	1,270
Time FE	Yes

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

A.4 Testability of the Identification Assumptions

Here I check the validity of the implications of the assumptions necessary for the LATE with defiers. This represents a mathematically necessary condition for identification, as if the check is not satisfied then the assumptions in Section A.2 cannot hold, which renders invalid the identification of the treatment effect. Specifically, as de Chaisemartin (2017) points out, if Assumptions 1, 2, and 3 in Section A.2 are satisfied, then

$$\underline{L} \leq W \leq \overline{L}, \tag{A.14}$$

where

$$\underline{L} = \mathbb{E}[Y \mid D = 1, Z = 1, U_{11} \leq p_1] - \mathbb{E}[Y \mid D = 0, Z = 0, U_{00} \geq 1 - p_0],$$
$$\overline{L} = \mathbb{E}[Y \mid D = 1, Z = 1, U_{11} \geq 1 - p_1] - \mathbb{E}[Y \mid D = 0, Z = 0, U_{00} \leq p_0],$$

$p_d = \frac{FS}{\Pr(D=d|Z=d)}$, and U_{dz} is the rank of an observation in the distribution of $Y|(D = d, Z = z)$. The intuition is that the LATE W is also partially identified since C_V is also included in the population for whom $\{D = 0, Z = 0\}$, and it accounts for p_0% of that population. Therefore $\mathbb{E}[Y_0 \mid C_V]$ cannot be larger than the mean of Y_0 for the p_0% with highest Y_0. Also, it cannot be smaller than the mean of Y_0 for the p_0% with lowest Y_0. One can establish analogous bounds for $\mathbb{E}[Y_1 \mid C_V]$. When one combines those two results, one can arrive at worst-case bounds for the treatment effect of comvivors $\mathbb{E}[Y_1 - Y_0 \mid C_V]$, which are $\underline{L}$ and $\overline{L}$. Then the point-identified treatment effect W must lie within those bounds.

Using the data in this application, I compute all the components of condition (A.14) above. They are displayed in the table below. Condition (A.14) is satisfied.

Table A.4: Components of condition (A.14).

p_1	.288
p_0	.341
$\mathbb{E}[Y \mid D=1, Z=1, U_{11} \leq p_1]$	5.734
$\mathbb{E}[Y \mid D=0, Z=0, U_{00} \geq 1-p_0]$	7.041
$\mathbb{E}[Y \mid D=1, Z=1, U_{11} \geq 1-p_1]$	7.140
$\mathbb{E}[Y \mid D=0, Z=0, U_{00} \leq p_0]$	5.503
$\underline{L}$	-1.307
$\overline{L}$	1.637

A.5 Other Regressions

A.5.1 Specifications with state trends

Table A.5: Specifications with state trends

	(1)	(2)	(3)
VARIABLES	Total earn	Hrly wage	Work hrs
married	0.375**	0.126	0.250**
	(0.163)	(0.153)	(0.114)
age	0.0656***	0.0725***	-0.00691
	(0.0120)	(0.0112)	(0.00838)
age sq	-0.00114***	-0.000818***	-0.000317***
	(0.000111)	(0.000105)	(7.81e-05)
std(exp)	0.234***	0.0622	0.171***
	(0.0403)	(0.0379)	(0.0282)
real GSP	0.216**	0.106	0.110*
	(0.0935)	(0.0879)	(0.0656)
Observations	4,517	4,517	4,517
Number of individuals	405	405	405
Individual FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
State trends	Yes	Yes	Yes

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

A.5.2 Regressions for Women

Table A.6: Weekly labor earnings, women

VARIABLES	(1) IV	(2) FS	(3) OLS	(4) RF
married	0.285		-0.105***	
	(0.593)		(0.0207)	
age	-0.0300	0.0454***	-0.0117*	-0.0171**
	(0.0287)	(0.00536)	(0.00670)	(0.00671)
age sq	-0.000392	-0.000398***	-0.000546***	-0.000505***
	(0.000247)	(5.70e-05)	(7.15e-05)	(7.13e-05)
experience	0.555***	-0.0763***	0.527***	0.533***
	(0.0554)	(0.0276)	(0.0343)	(0.0345)
real GSP	0.298**	-0.160**	0.226***	0.253***
	(0.143)	(0.0708)	(0.0873)	(0.0886)
no-fault div		0.0433**		0.0123
		(0.0196)		(0.0246)
Observations	4,058	4,058	4,058	4,058
Number of individuals	387	387	387	387
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

Table A.7: Hourly wages, women

VARIABLES	(1) IV	(2) FS	(3) OLS	(4) RF
married	0.800		0.0242	
	(0.608)		(0.0182)	
age	-0.0241	0.0454***	0.0122**	0.0122**
	(0.0294)	(0.00536)	(0.00587)	(0.00586)
age sq	-5.64e-05	-0.000398***	-0.000364***	-0.000374***
	(0.000253)	(5.70e-05)	(6.27e-05)	(6.23e-05)
experience	0.293***	-0.0763***	0.238***	0.232***
	(0.0568)	(0.0276)	(0.0300)	(0.0301)
real GSP	0.353**	-0.160**	0.209***	0.225***
	(0.146)	(0.0708)	(0.0765)	(0.0774)
no-fault div		0.0433**		0.0346
		(0.0196)		(0.0215)
Observations	4,058	4,058	4,058	4,058
Number of individuals	387	387	387	387
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

Table A.8: Weekly hours of work, women

VARIABLES	(1) IV	(2) FS	(3) OLS	(4) RF
married	-0.515		-0.130***	
	(0.510)		(0.0176)	
age	-0.00588	0.0454***	-0.0239***	-0.0293***
	(0.0246)	(0.00536)	(0.00567)	(0.00570)
age sq	-0.000335	-0.000398***	-0.000183***	-0.000131**
	(0.000212)	(5.70e-05)	(6.05e-05)	(6.06e-05)
experience	0.262***	-0.0763***	0.289***	0.301***
	(0.0476)	(0.0276)	(0.0290)	(0.0293)
real GSP	-0.0541	-0.160**	0.0172	0.0283
	(0.123)	(0.0708)	(0.0739)	(0.0753)
no-fault div		0.0433**		-0.0223
		(0.0196)		(0.0209)
Observations	4,058	4,058	4,058	4,058
Number of individuals	387	387	387	387
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

Table A.9: Weekly hours of housework, women

VARIABLES	(1) IV	(2) FS	(3) OLS	(4) RF
married	-0.584		0.288***	
	(1.124)		(0.0460)	
age	0.131*	0.0454***	0.0774***	0.0969***
	(0.0719)	(0.00536)	(0.0194)	(0.0197)
age sq	-0.00155***	-0.000398***	-0.00119***	-0.00131***
	(0.000494)	(5.70e-05)	(0.000165)	(0.000166)
experience	-0.0834	-0.0763***	0.0490	0.00680
	(0.203)	(0.0276)	(0.102)	(0.103)
real GSP	-0.0873	-0.160**	-0.104	-0.124
	(0.218)	(0.0708)	(0.200)	(0.207)
no-fault div		0.0433**		-0.0273
		(0.0196)		(0.0489)
Observations	2,413	4,058	2,413	2,413
Number of individuals	342	387	342	342
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

Table A.10: Labor force participation, women

VARIABLES	(1) IV	(2) FS	(3) OLS	(4) RF
married	-14.62		-0.128***	
	(71.86)		(0.00890)	
age	0.270	0.0185***	0.00426*	7.38e-06
	(1.320)	(0.00270)	(0.00238)	(0.00247)
age sq	-0.00319	-0.000213***	-0.000103***	-7.48e-05***
	(0.0153)	(2.77e-05)	(2.52e-05)	(2.54e-05)
experience	-0.123	-0.0132	0.0696***	0.0703***
	(0.967)	(0.0104)	(0.00939)	(0.00948)
real GSP	1.207	0.0841**	-0.0400	-0.0229
	(6.204)	(0.0372)	(0.0327)	(0.0341)
no-fault div		-0.00230		0.0337***
		(0.0114)		(0.0104)
Observations	11,244	11,244	11,244	11,244
Number of individuals	793	793	793	793
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

A.6 Power of Estimates for Hours of Housework

Table 7 shows that after marriage, comvivors reduce their hours of housework by 24%. However, I fail to reject the null hypothesis that the effect of marriage is equal to zero. Note that the sample size in that table is smaller than the sample size in Tables 3, 4, and 5. The lack of statistical significance in Table 7 can be due to low power. To explore that possibility, I reestimate the regressions in column IV FE2 of Tables 3, 4, and 5 but keeping the same sample as in Table 7. Table A.11 shows the results of such exercise, it illustrates the loss of power when using a smaller sample size.

Table A.11: Significance of marriage on outcome variables, restricted sample.

VARIABLES	(1) tot earn (Table 3)	(2) hrly earn (Table 4)	(3) hrs work (Table 5)
effect of marriage full sample	0.379	0.126	0.253
effect of marriage restricted sample	0.244	0.112	0.132
s.e. effect restricted sample	0.204	0.195	0.153
p-value	0.232	0.568	0.387
Observations	2,387	2,387	2,387
Individual FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes