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Marriage as insurance: job protection and job insecurity in France

Andrew E. Clark
Conchita D'Ambrosio
Anthony Lepinteur

Abstract
Job insecurity is one of the risks that workers face on the labour market. As with any risk, individuals can choose to insure against it. We here consider marriage as a way of insuring against labour-market risk. The 1999 rise in the French Delalande tax, paid by large private firms when they laid off workers aged 50 or over, led to an exogenous rise in job insecurity for the uncovered (younger workers) in the affected firms. A difference-in-differences analysis using French panel data reveals that this greater job insecurity for the under-50s led to a significant rise in their probability of marriage, and especially when the partner had greater job security, consistent with marriage providing insurance against labour-market risk.

Key words: marriage, insurance, employment protection, perceived job security, difference-in-differences
JEL codes: I38; J13; J18

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Andrew E. Clark, Paris School of Economics and Centre for Economic Performance, London School of Economics. Conchita D'Ambrosio, University of Luxembourg. Anthony Lepinteur, University of Luxembourg

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1. Introduction

Why do people get married? Weiss (1997) suggests that marriage comes with a number of economic advantages. The first reflects the benefits of specialisation between spouses (Becker, 1973 and 1981). Second, in a context of imperfect credit markets, marriage may also relax credit constraints via implicit credit arrangements within households (Borenstein and Courant, 1989) and enhance investment (for example, one partner may work while the other is in education investing in their human capital). Collective and non-rival goods are also jointly produced and consumed within partnerships, with common examples being children or housework (Chiappori, 1992, and Van Klaveren *et al*., 2008).

Stevenson and Wolfers (2007), in their empirical review of changing trends in marriage and divorce in the US, highlight the roles of pre-marital cohabitation (which has risen), specialisation in marriage (now argued to be less important), the tax implications of partnership, birth control, changes in relative wages, Divorce Laws, and the marriage "matching function" (via education, the workplace and the internet).

The benefit from marriage that we will address here, which appears in both Weiss (1997) and Stevenson and Wolfers (2007), is that of risk-sharing. As noted by Hess (2004) and Shore (2010), partnerships provide insurance by allowing couples to diversify risk, if the exogenous income shocks that the two partners receive are not perfectly (positively) correlated. Couples can in addition adjust their relative labour supply to reduce the impact of shocks.

A number of contributions have provided indirect evidence that is consistent with the insurance role of marriage. In Rosenzweig and Stark (1989), arranged marriages between families from different villages in South India significantly reduced the variability in food consumption. Farm households with greater income risks were more likely to engage in arranged marriages at longer distances. In US data, Halla and Scharler (2011) find that the influence of idiosyncratic output-growth shocks on consumption is smaller in States where the percentage of married is higher. Bertocchi *et al*. (2011) consider household investment decisions, and conclude from their analysis of 14 years of Italian SHIW data that the married invest more in risky assets (as marriage is considered to be a safe asset). Anderson and Ray (2019) show that marriage protects against the risk of death, especially for women. In a similar vein, Van den Berg and Gupta (2015) use individual data from Dutch registers (from 1815 to 2000) and find a protective effect of marriage against mortality for men.

With respect to the labour market, there is a considerable literature on marriage and the business cycle. Schaller (2013) analyses 32 years of US State-level panel data: marriage is shown to be pro-cyclical, with unemployment being associated with less marriage. Lichter *et al*. (2006) appeal to individual-level NLSY-79 data, and conclude that the probability of transition from cohabitation to marriage rises with partner's education and the partner working; it is lower for the unemployed. Education and employment are found to have a similar influence on marriage probability in Chinese data (Yu and Xie, 2015). Early labour-market experiences also seem to matter. Ekert-Jaffé and Solaz (2001 and 2002) and Landaud (2019) analyse different French datasets to show that early-career unemployment and temporary jobs reduce the probability of forming a couple; De La Rica and Iza (2005) come to similar conclusions using Spanish data. One interpretation is that unemployment provides a negative signal about the potential partner's unobserved characteristics. Consistent with this interpretation, in Charles and Stephens (2004), job loss increased the risk of divorce when resulting from layoff but not as a result of plant closings: they note that the former may convey information about the partner's noneconomic suitability as a mate.

Some work has explicitly looked not at events that have already occurred, but rather future risk on the labour market. Schneider *et al*. (2018) discuss the role of economic resources in marriage, which they extend to include wealth and expected future earnings. Schneider and Reich (2018) continue in the same line, and find that union membership (as an indicator of economic security) is a predictor of marriage in NLSY-79 data. Xie *et al*. (2003) are also individual-level and forward-looking, and calculate five different measures of current and future "earnings potential": they show that all five are positively correlated with the transition from cohabitation to marriage using US Census and cohort data.

We also consider labour-market risk, but here with respect to job loss. The role of forward-looking job insecurity in partnership has already been mentioned: Schneider *et al*. (2018) explicitly cite job security as a component of future earnings, but note that it is difficult to pin down exogenous changes in resources, and in particular that job security is not observed in their PSID data. However, we are not aware of any work that has been able to appeal to exogenous variation in job insecurity at the individual level in this context. We here propose a difference-in-differences analysis from a quasi-natural experiment to establish a causal effect of individual job insecurity on the probability of marriage. As opposed to past individual unemployment or current macroeconomic shocks, we are able to identify a plausible exogenous change in job insecurity: our analysis sample consists of workers, some of whom became more at risk than others of future job loss following a French labour-market reform.

As in Clark and Lepinteur (2021), we use 1994-2001 French data from the European Household Community Panel (ECHP) to show that the 1999 rise in the French Delalande tax on the layoffs of those aged 50+ in large firms increased job insecurity for the under-50s in these firms (as compared to the under-50s in small firms, where the Delalande tax did not change in 1999). We are interested in the way in which this exogenous greater risk affects behaviour. Clark and Lepinteur (2021) suggest that one reaction is to reduce risk exposure, via lower fertility; we instead here ask whether individuals will take out more insurance against risk, via their marital status.[1]

[1] The analysis in Clark and Lepinteur (2020) does not use the same sample as we do here. Their fertility analysis is carried out on the sample of workers who were already married before the reform and continued to be married after it. On the contrary, we explicitly model moves between marital statuses.

We conclude that greater job insecurity amongst French workers increased women's probability of marriage by four percentage points; there is no effect for men. This is consistent with evidence on gender differences in preferences (Croson and Gneezy, 2009): women are generally less willing to take risks in the context of lotteries (Hartog *et al*., 2002; Holt and Laury, 2002; Fehr-Duda *et al*., 2006) and portfolio selection (Sunden and Surette, 1998; Finucane *et al*., 2000; Charness and Gneezy, 2012). Dohmen *et al*. (2018) measure risk aversion by both a self-assessed measure and revealed preference from a series of binary choices between a lottery and a sure return. Women were found to be more risk averse than men in almost all of the 76 countries for which they collected data (see their Figure III). As such women may react more to the threat of future job loss than do men.

We check that our estimates are robust to a number of potential confounding factors, such as European macroeconomic trends and the French 35-hour workweek that was announced in 1998 and introduced in 2000; they are also qualitatively similar using a number of different estimation methods. While we do identify an insecurity effect on marriage, there is no change in the probability of entering a partnership in general, or indeed of leaving one: the greater probability of marriage for women then mostly reflects a shift into marriage from pre-reform cohabitation. The effect of job insecurity on women's marriage probability is the same by age, education and wage, but larger for women who were already mothers before the reform. Last, as predicted by risk-sharing, the probability of marriage only rises when the partner is employed and did not experience greater job insecurity due to the layoff-tax rise.

The remainder of the paper is organised as follows. Section 2 describes the institutional background and the identification strategy for our analysis of individual-level insecurity, and Section 3 the ECHP data and the estimation sample. The main results then appear in Section 4, and the robustness checks and heterogeneity analyses in Section 5. Section 6 discusses the importance of partner characteristics in the light of risk-sharing theory. Last, Section 7 concludes.

2. Institutional Background and Identification

a. The Advantages of Marriage in France

Marriage in France brings a number of financial advantages, some of which are particularly attractive for workers facing economic insecurity. First, French legislation considers cohabiting couples as two separate tax units but married couples as a single tax unit. As shown in Leturcq (2012), the formula used by the French government to calculate annual income tax in the early 2000s yields a considerably lower income tax rate for the married as compared to the cohabiting. These gains from marriage are particularly high when the income gap between the spouses is large. According to Echevin (2003), around 50% of married couples in 1999 benefited from a lower yearly income tax figure of at least 1000 Euros. Buffeteau and Echevin (2004) provide additional simulations of the financial gains from marriage in France in the early 2000s.

There are a number of marital-property regimes in France. Up until the end of the 1990s, the most common form of property regime was that of the "*regime de communauté de biens réduite aux acquêts*" (Fremeaux and Leturcq, 2013), in which any property owned by one of the spouses before marriage continues to be treated as belonging only to that spouse in the case of divorce. However, all property, assets and income acquired during the marriage are treated as common property and are subject to division in the case of divorce.

Article 212 of the French Civil Code states that "*les époux se doivent mutuellement […] secours*". The "*devoir de secours*" (obligation of assistance) rarely takes the form of a legal and formal transfer of resources during marriage; it is supposed to come about naturally via resource pooling and solidarity between the two spouses. However, in the case of divorce or even during the divorce proceedings, the spouse with the most-favourable financial situation can be asked to transfer resources to the other spouse.

Last, unemployed married individuals can more easily benefit from various social benefits (such as social security and health insurance) when their partner is employed. In the case of the death of a partner, marriage also simplifies the inheritance procedures for the surviving spouse and gives the survivor the right to receive a part of the pension of the dead spouse ("*pension de reversion*").

b. The Delalande Tax

The Delalande tax was introduced into French Law in 1987 to tackle the rise in layoffs among older workers. Despite a number of changes over time, the experience-rating principle of the tax remained unchanged: firms laying off workers of over a certain age have to pay the Delalande tax to the government. This tax, which was also meant to help balance the unemployment-insurance system, is proportional to the laid-off worker's gross wage and is applied to private-sector workers with permanent contracts. From its introduction in 1987 up to 1992, this tax was three months of gross wages for all workers aged 55 and over who were laid off. Major changes to this tax were then introduced in July 1992, January 1993 and January 1999; in particular, starting in 1992 the layoff tax applied to all workers aged 50 or above. The Delalande tax was finally abolished in 2008.

Table 1 shows how the Delalande tax changed over time as a function of the age of the laid-off worker and firm size. From 1993 to December 1998, the tax was a function of worker age only, and independent of firm size. The tax then rose in January 1999, but only for firms with 50 or more employees.

As predicted by the theoretical model in Behaghel (2007), a higher Delalande tax reduces the separation rate of those workers covered by the reform (those aged 50 or more) but increases the separation rate of the younger workers (aged under 50) in the same firms. Using the same

ECHP data as we do here, Georgieff and Lepinteur (2018) empirically confirm this prediction: the higher Delalande tax had perverse effects on younger workers in larger French firms by increasing both their perceived job insecurity and their actual risk of layoff.

We here wish to assess the causal effect of this exogenous change in job security from the tax reform on the marriage probability of younger workers. We do so by exploiting the firm-size discontinuity and the resulting rise in job insecurity for younger workers in larger firms. The reform then provides a natural quasi-experimental design for a difference-in-differences (D-i-D) estimation, in which younger workers in large private firms are the treatment group and younger workers in smaller private firms the control group.

The 1999 reform to the Delalande tax was announced by the French government one year beforehand (in early 1998), and the reintroduction of the firm-size discontinuity was also public knowledge by the end of 1998 (the ECHP interviews took place in November and December of each year). As such, employers may have been able to strategically adjust their labour demand before the change in the Law. Georgieff and Lepinteur (2018) find evidence of such anticipation effects, as employers in (to-be) treated firms laid off relatively more workers just after the announcement of the reform but prior to its implementation. Higher layoffs are found for both those aged over 50 and younger workers, so that the tax rise brought about some restructuring in larger firms. Given this anticipation, job insecurity for the under-50s in large firms may well have started to rise before the reform's official implementation. We take this possibility into account by estimating the following D-i-D equation, including both a post-1999 treatment effect and a 1998 announcement effect of the reform:

$$Y_{it} = \alpha_1 Treat_{it} + \alpha_2 Treat_{it} * \lambda_{1998} + \alpha_3 Treat_{it} * Post1998_t + \alpha_4 X_{it} + \mu_i + \lambda_t + \varepsilon_{it}. \quad (1)$$

This equation is estimated only for workers under the age of 50. Here Y_{it} first represents the perceived job security of worker *i* in year *t* and then a dummy for worker *i* being married in year t.[2] The variable $Treat_{it}$ is the treatment dummy, and is one for younger private-sector workers in large firms (50+ employees) and zero for younger private-sector workers in smaller firms. The variable $Post1998_t$ is a dummy for observations after January 1999 (the implementation date of the higher Delalande tax), λ_t are year fixed effects (so that λ_{1998} refers to the policy-announcement year of 1998) and X_{it} is a vector of standard individual socio-demographic controls. This includes age dummies (in five-year bands), the lagged number of children in the household,[3] health status, weekly working hours, the log of the monthly wage, and occupation and region dummies. This equation also controls for individual fixed effects μ_i. The year dummies in equation (1) entirely subsume the main effects of λ_{1998} and $Post1998_t$. The coefficients of interest in the above equation are α_2 and α_3, which respectively capture the effects of the reform's announcement in 1998 and its implementation starting in 1999 on first job security and then marital status.

The risk-sharing hypothesis predicts positive and significant α_2 and α_3 coefficients for workers in the marriage-probability regression, reflecting their rise in risk from job insecurity and the subsequent greater demand for insurance. However, it is also possible that the treatment reduce the probability of marriage, as workers who have become more at risk of layoff are now seen to be "riskier" partners.

We expect the effect of the reform's announcement and implementation to be of the same sign, so that we can also estimate the following D-i-D regression:

$$Y_{it} = \beta_1 Treat_{it} + \beta_2 Treat_{it} * Post1997_t + \beta_3 X_{it} + \mu_i + \lambda_t + \varepsilon_{it}, \qquad (2)$$

[2] We also consider in Online Appendix Table A2 the following types of marital outcomes: partnered, partnered but not married, divorced or separated, and never in a relationship.

[3] It is not clear whether children should be included in the list of control variables. Marriage may encourage parenthood, or alternatively those who already have children may be more likely to marry. Including the lagged number of children in the household as a control somewhat attenuates concerns about reverse causality. Dropping the lagged children variable changes our key estimated coefficient of the effect of job insecurity on marriage only very little.

where $Post1997_t$ is a dummy for observations after 1998 (the announcement year of the higher Delalande tax). The coefficient β_2 in equation (2) now captures what we will call the total effect of the reform, starting from its announcement (β_2 thus corresponds to equation (1) when $\alpha_2 = \alpha_3$). Both Equations (1) and (2) will be estimated using linear techniques (although we will also estimate non-linear regressions in the robustness checks).

3. Data and Estimation Sample

The data we use here come from the European Community Household Panel (ECHP), a nationally-representative longitudinal survey of households that covered 15 European countries (including France) between 1994 and 2001. The sample size in France was on average 12,000 adults per wave. The interviews mainly took place between November and December of each year. The ECHP contains detailed information on respondents' socio-economic characteristics, income, employment conditions, social relations and so on.[4] Respondents report their marital status in each ECHP wave.

The 1999 rise in the Delalande tax only applied to firms with 50 or more employees. The ECHP records the number of employees in the firm in which the respondent works using the following categories: "None", "1 to 4", "5 to 19", "20 to 49", "50 to 99", "100 to 499" and "500 or more". This is the variable that we will use to assign younger workers to the treatment or control groups.

In the first part of our analysis, we show that the higher layoff tax for those aged 50 or over in larger firms increased job insecurity for younger (aged under 50) workers in these same firms; the main analysis will then address the question of whether this same tax rise affected marriage.

Regarding job insecurity, we consider the following ECHP question on perceived job security:

[4] More details are available at http://ec.europa.eu/eurostat/web/microdata/european-community-household-panel.

"How satisfied are you with your present job in terms of job security?"

The answers to this satisfaction question were given on a 6-point scale, with 1 referring to *"Not Satisfied"* and 6 "*Fully Satisfied*".[5] This measure of perceived job security is a strong predictor of individual choices such as future job quits (Clark, 2001), and reflects objective changes in layoff and hiring rates (Böckerman *et al*., 2011).[6] It has also been shown to be related to permanent contracts, unemployment-insurance benefits and employment protection legislation (Clark and Postel-Vinay, 2009).

We consider respondents between the ages of 20 and 49 working in the private sector with permanent contracts, and with valid information on the sociodemographic variables, job characteristics and perceived job security. As the reform was announced many months before its implementation, we last may suspect that firms strategically adjusted their workforce. To rule out concerns regarding self-selection into the treatment, we will analyse only workers who reported no change in their firm's size from 1997 onwards. This ends up dropping only around six per cent of the baseline sample (the resulting estimated coefficients for the effect of job insecurity on the probability of being married are actually not materially affected by this restriction). Note that we do not consider here the workers who were protected by the reform, i.e. workers aged 50+. Almost 80% of respondents in this group were already married, and there was little subsequent movement out of marriage (97.5% of those aged 50+ reported the same marital status during the years before and after the reform's announcement). This limited within-variation considerably attenuates the statistical power for this older group.

Our analysis sample consists of 10,371 observations on 2,797 different individuals from 1995 to 2001 (we cannot use the first 1994 wave of ECHP here, as the information on whether the firm is public or private is missing) between the ages of 20 and 49. Figure A1 in the Online

[5] Figure A2 in the Online Appendix shows the distribution of self-reported job security. It can be seen that 70% of responses on the 1-6 scale were 4 or 5. This negative skewness is commonly found for subjective measures.
[6] We can replicate this kind of analysis in the ECHP data: the probability of both the end of the job match, and of layoffs in particular, at time t is negatively correlated with self-reported job security at time t-*1*.

Appendix shows the number of observations at each stage of our sample selection.[7] The descriptive statistics for the analysis sample appear in Table 2. Just under half (47%) of the sample are in the treated group. With respect to our two dependent variables, 57% of observations come from individuals who report being married, and average job security on the one-to-six scale is a little over four.

Although we focus on a specific part of the French population, i.e. young private-sector workers with a permanent contract, the share of married workers in our estimation sample is similar to the national value (see Figure 1). The share of unmarried partnered workers in our sample is slightly higher than the national figure, but this is very likely because we exclude individuals above age 49. The marriage share in the French adult population has steadily decreased from 1990 to 2009 in Figure 1, falling from 56% to 52%. Over the same period, the share of unmarried partnered individuals rose markedly by 9.5 percentage points (from 6.7% to 16.2%).

4. Main Results

Table 3 first shows how reported job security for younger workers changes with the employment protection of older workers in the French sample of ECHP panel data. We here use the analysis sample described in Table 2. Columns (1), (3) and (5) of Table 3 refer to the D-i-D estimates of Equation (1), where we allow for a post-1999 treatment effect as well as an announcement effect of the reform in 1998; the results in columns (2), (4) and (6) come from the estimation of Equation (2), where the announcement and implementation dummies are

[7] The fall in the number of observations due to missing values (from 13,966 to 10,936 observations) is mostly due to missing firm size. As firm size is essential in defining treatment status, we do not impute missing values here. Balance tests confirm that the observable characteristics of the observations dropped due to missing firm size are not significantly different from those with valid information. Around 1,000 observations are also lost because of missing values for weekly working hours. Again, these dropped observations have characteristics similar to those in the estimation sample.

combined into one "total" effect. Columns (1) and (2) of Table 3 show the results for the whole sample, while columns (3) - (4) and (5) - (6) respectively refer to those for women and men.

As can be seen in column (1), the 1999 rise in the layoff tax for older workers in larger firms significantly reduced the perceived job security of younger workers in these larger firms (as compared to younger workers in smaller firms, where there was no change in the layoff tax). Job security fell significantly after both the announcement and the implementation of the rise in the Delalande tax. The magnitude of the estimates is in line with the findings in Georgieff and Lepinteur (2018) and Clark and Lepinteur (2021).[8] Pairwise Wald-tests confirm that the estimated announcement and implementation effects of the reform in rows 1 and 2 are not statistically different from each other. Column (2) then combines these into the total effect of the reform, which is naturally negative and significant and of a similar size to the figures in column (1). We estimate that the higher Delalande tax reduced the perceived job security of younger workers in treated firms by 0.2 points, which from Table 2 is around one-sixth of a standard deviation. We find similar estimates for women and men in the remaining columns of Panel A. Although the reform effects look larger for men, given the associated standard errors these gender differences are not significant.[9] We can replicate these results using a simple binary variable for job security (comparing values 1-4 against 5-6): all of the treatment estimates continue to be significant, with an effect that is again larger, but not significantly so, for men.

[8] The figures are not exactly the same as the analysis samples differ across the papers. In Georgieff and Lepinteur (2018), the ECHP analysis sample pools together workers above and below age 50, and the authors estimate a triple difference-in-differences to separately identify the effect of the reform on workers below and above age 50. Clark and Lepinteur (2021) analyse the same age group as we do here, namely the under-50's, but focus on the fertility behaviour of those who were already married before the reform and continued to be so afterwards.

[9] Using an estimation sample with the same characteristics as in this paper and the French Labour Force Survey, Georgieff and Lepinteur (2018) show that the actual layoff probability of the treated workers below age 50 significantly increased after the announcement of the reform. We have replicated their analysis separately for men and women and find no significant gender differences. These results are available upon request.

Panel B of Table 3 then estimates the relationship between job insecurity and the probability of being married in the same analysis sample.[10] The estimated coefficients for the whole sample in columns (1) and (2) are positive but not significant at conventional levels. As it has been suggested that women are on average more risk averse than men (Sunden and Surette, 1998; Finucane *et al*., 2000; Hartog *et al*., 2002; Holt and Laury, 2002; Fehr-Duda *et al*., 2006; Croson and Gneezy, 2009; Charness and Gneezy, 2012), we might expect these estimates to differ by gender. The results in columns (3) and (4) show that women are significantly more likely to be married following the implementation of the reform. The pairwise Wald-tests again reveal that the anticipation and implementation effects of the reform are statistically identical. The results combining these two effects in column (4) reveal that the higher Delalande tax increased the probability of being married for women by just over four percentage points.[11] This corresponds to 7.5% of the pre-reform share of married women in the treated group (57%). It is half of the observed difference in female marriage rates between the early and late 20's (see Table A1).

There is no significant marriage effect for the corresponding sample of men in columns (5) and (6). A first possibility is that prime-age women who worked in permanent private-sector jobs in the 1990s in France were differentially selected than their male counterparts. The comparison of the pre-reform characteristics of men and women who do and do not appear in our estimation sample actually reveals only small differences: there is more positive selection of women into the estimation sample by education than there is for men, and less selection by hours of work. However, we control for both of these variables in our estimations (and will, in

[10] The full set of marriage results, including the estimated coefficients on all of the control variables, appears in Online Appendix Table A1.

[11] These regressions include a number of sociodemographic controls, as noted at the foot of the table. The inclusion of these controls has no effect on our treatment effects. This is to be expected: if the assignment to the treatment is random, the inclusion of additional controls will not affect the point estimates but should increase their precision. The results without these control variables are available upon request.

addition, show below that there is no differential effect of the reform treatment on marriage by either education or hours of work).

Second, this may reflect that men are on average less risk-averse: the rise in job insecurity from the 1999 rise in the Delalande tax may not have had much effect on their demand for insurance. Last, men who looked for insurance through marriage may not have been able to find it: their greater job insecurity may make them less attractive. This is consistent with Ekert-Jaffé and Solaz (2001 and 2002) and Landaud (2019), where men without a job or in temporary employment were less likely to be partnered.

One of the requirements for D-i-D estimation to produce causal effects is that there would have been a common trend in the dependent variable (here perceived job security and the probability of being married) in the control and treatment groups in the absence of the policy reform. Figure 2 thus plots the estimated yearly effects of being in the treatment (as opposed to the control) group on perceived job security (in Panel A) and the probability of being married (Panel B). The left-hand side shows the results for the whole sample and the right-hand figure those for men and women separately.[12] None of the pre-reform announcement estimates (that can be considered as placebos) in Figure 2 are significantly different from zero, providing evidence in favour of the common-trend assumption.[13]

We also need to make sure our reform effect is not influenced by changes in other individual characteristics. For most of the observable characteristics, we find no significant differences in the gaps between the treatment and control groups before and after the reform's announcement, as shown in Tables A2 and A3. However, we do see that the weekly working

[12] In these latter figures we have slightly horizontally-shifted the curves for men and women so that the confidence intervals around the estimates can be seen clearly.

[13] Figure A3 in the Online Appendix plots the time profile of average perceived job security and the probability of being married in the treatment and control groups, providing additional supporting evidence for the parallel-trend assumption. In the bottom-right panel of Figure A3, the marriage rate of women in the treatment group is lower than that of women in the control group before the reform. Table A3 reveals that women in the treatment group are more educated and work more hours, both of which are associated with less marriage in this age group.

hours in the treatment groups dropped significantly more (in the whole and female sample only) than in the control groups. This is not surprising: in 2000, standard weekly hours in France dropped from 39 to 35 for workers in firms with 20 employees and more. While we do not worry about the whole sample (because we do not find a significant effect of the 1999 rise of the Delalande tax on the probability to be married for the whole sample), our treatment estimates for the sample of women might capture the influence of the mandatory 2000 working-time reduction. We will rule out this possibility in our robustness section by excluding workers from companies with fewer than 20 employees from our estimation sample.

Last, we ask in Table A4 whether the total effect of the rise in the Delalande Tax led to other changes in terms of marital status and couple formation. The first column of this table reproduces the estimated marriage coefficients from columns (2), (4) and (6) of Table 3 for comparison purposes. We then look in column (2) at the probability of being in a partnership, irrespective of legal marital status: the estimates show that the greater job insecurity from the reform had no effect on being in a couple in general. Combined with the results for marriage, we thus expect (for women) a negative reform effect on cohabitation. This is indeed what we find in column (3) of Panel B: women whose job insecurity rose following the change in the Delalande tax are less likely to be in a non-married relationship. As the ECHP is a household panel, where all adults are interviewed, and as we here focus on the transition from cohabitation to marriage, we might expect the estimation results here for men and women to be identical (as we are looking at either side of a couple). This turns out not to be the case in Table A4. The explanation is that the men and women in this table do not come from the same sample of cohabitees: fully one half of treated women who we observe switching from cohabitation to marriage marry men who are not in the sample in Panel C (as the jobs of the latter are temporary or in the public sector, they are not active in the labour force, or they are aged 50 or over).

Last, Columns (4) and (5) of Table A4 look in turn at being divorced/separated or never having been in a relationship, finding no significant effect for men or women, or for the whole sample.[14]

5. Robustness Checks and Heterogeneity

The results in Table 3 refer to the effect of the reform on first job security and then marital status for the whole analysis sample. We now turn to a number of robustness tests, and then ask whether the effects of job insecurity on the probability of being married are larger for certain types of workers. The analyses that we will carry out here refer to the total effect of the reform, as in Equation (2).[15] We have also estimated the analogous effect of all of our robustness and heterogeneity tests for the estimated effect of the reform on perceived job security; these results appear in Online Appendix Table A5.

a. Robustness Checks

i. Ruling out confounding shocks

As noted above, the 2000 working-time reduction was another notable labour-market reform around the time of the change in the Delalande tax. In 1998, the French Ministry of Labour announced a reduction in the standard workweek from 39 to 35 hours in companies with more than 20 employees. This could potentially have affected the perceived job security of workers in those firms, and hence our estimated coefficients. We check that our main estimates do not pick up the effect of the 35-hour week by re-running our baseline regression excluding workers in firms with under 20 employees (which ensures that all of the individuals in our estimation sample were equally-affected by the 35-hour week): this drops around 15%

[14] The marital-status categories in Table A4 are not mutually-exclusive: an individual can be both divorced and in a new relationship. We find similar results to those in Table A4 when we estimate a multinomial-logit model on the following mutually-exclusive categories: "Married in a partnership", "Non-married in a partnership", "Divorced or separated with no partner" and "Never in a relationship" (we exclude widows here, as there are too few observations). These results are available upon request.

[15] The results of the robustness and heterogeneity analyses when we split the reform up into announcement and implementation effects are very similar.

of the initial estimation sample. The estimated reform coefficients in column (1) of Table 4 are similar to those in the baseline (in columns (2), (4) and (6) of Panel B in Table 3). Note that when we exclude workers in firms with under 20 employees from our estimation sample, the differences in weekly working hours observed in Tables A2 and A3 are no longer statistically different from zero.

As part of our identification relies on changes over time, we would like to be sure that these changes reflect the evolution in the French layoff tax, rather than some broader macro-economic developments. One way of testing for the latter is to re-estimate our baseline regressions on similar samples of workers in neighbouring and arguably similar countries (where of course the French layoff tax did not apply), as the ECHP is harmonised across European countries. Data limitations restrict this comparison to Spain, Italy and Denmark.[16] We present the resulting D-i-D estimates for these three countries in columns (2), (3) and (4) of Table 4, none of which is significantly different from zero. Macroeconomic trends do not then seem to lie behind our main result of greater job insecurity increasing the share of married women.

ii. The estimation method

Our main results above come from fixed-effect analyses, comparing the same individual before and after the labour-market reform. We expect fixed-effects and OLS analyses to produce different estimates, as the former introduce attenuation bias when there is measurement error (with the resulting OLS estimates being larger than their FE counterparts in absolute terms) and for omitted-variable reasons, where the OLS estimates will be biased if the treatment is correlated with unobserved individual time-invariant characteristics. The baseline

[16] The information in the final waves of the ECHP in Belgium and Germany does not allow us to accurately distinguish the public from the private sector, or to measure perceived job security.

results without fixed effects in column (5) of Table 4 are slightly larger than the baseline fixed-effect estimates, but not significantly so.

We also ask whether these results are affected by the way in which we define the dependent variable. Our baseline regressions treat the probability of being married as a cardinal variable (and are thus fixed-effect linear-probability models). However, it can be argued that non-linear estimation is more suitable for this dummy dependent variable. Column (6) of Table 4 thus re-estimates our main regression using a conditional fixed-effect logit model; the results here continue to produce a positive significant estimated coefficient for women, with job insecurity increasing the probability of being married.

iii. Sample composition

Firm size is reported by the respondent and may not be accurate. In this case, individuals can be mis-allocated to firm-size groups, and hence to the control or treatment groups. This mis-reporting may be random, in which case we estimate a lower bound of the treatment (as some of the control group are treated, and some of the treated group are not). A potentially more serious problem arises if the mis-allocation is not random, conditional on the control variables in our regressions. We address mis-reporting using a test inspired by the donut regression-discontinuity design, and drop workers who are close to the firm-size treatment threshold of 50 employees. From the firm-size categories listed in Section 3, this implies re-estimating the treatment without respondents who say they work in firms with “20 to 49 employees” or “50 to 99 employees”. Intuitively, mis-judgement may cause workers to erroneously report a firm-size category above or below the correct value, but not to jump three categories (so that they report a firm size of under 20 employees when the real value is over 100 employees, for example).

The estimated treatment coefficients from the baseline analysis (in panel B of Table 3) and these “donut” regressions (in the last column of Table 4) are of the same size, although the estimate of the latter for women is no longer significant (due to the smaller sample size when we drop two firm-size categories, producing larger standard errors). That the estimated coefficient does not change between the two specifications helps to dispel any worries about the systematic mis-reporting of firm size.[17]

We last consider attrition. From the announcement of the reform onwards, 16% of our treatment group left the estimation sample before the last wave (this figure is the same in the control group, with no noticeable differences between men and women). We may worry that attrition is not independent of the reform's implementation so that our estimates are biased, especially if the marriage probability of leavers decreases after they leave the sample. A first point is that the ECHP does supply two weights, both of which are called attrition weights (our main regression results do not use weights). Applying these attrition weights does not change the nature of our results.

We can also calibrate the "unobserved" difference-in-difference in the attrition group that would be required produce a main estimate of zero: this is 0.21 (= 0.04*84%/16%). Our main result is that the difference in the probability of being married between women in large and small firms rose by 4 percentage points post-reform. To cancel this figure out, we would require the analogous figure for the 16% of women in the attrition group to be a fall of 21 percentage points after the reform. This figure is more than double the largest age effect on marriage in column (4) of Table A1, and does not seem plausible.

b. Heterogeneity

The D-i-D estimates in Panel B of Table 3 show the average treatment effect for workers in large firms. In Table 5 we consider whether these effects might differ across groups. We

[17] As in Clark and Lepinteur (2021), we can consider public-sector workers as an alternative control group but none of our treatment estimates was significantly different from zero at conventional levels. Note however that the marriage probability pre-trends differ significantly between the treatment group and public-sector workers, which cast doubts about the validity of the estimation strategy in this particular sample. All of these results are available upon request.

first consider age, and interact the total effect of the reform with a dummy for being born in 1963 or before. The resulting estimates appear in columns (1) and (6) of Table 5 respectively for women and men: neither interaction term here is significant.[18]

We second know that the relationship between fertility decisions and job insecurity likely depends on education and earnings (Chevalier and Marie, 2017; Clark and Lepinteur, 2021). We here ask whether an analogous relationship is found for marriage. We thus interact the reform with "High-education" and "High-wage" dummies, corresponding respectively to workers with post-secondary education and those with above-median wage: both of these interaction variables are measured in the pre-reform years only.[19] The resulting estimates on these interactions in columns (2), (3), (7) and (8) of Table 5 indicate no significant difference in the effect of job insecurity on marriage by education or wages.[20]

Our last interactions concern pre-reform family characteristics. Columns (4) and (9) show the estimated coefficient on the interaction with a dummy for the treated workers having children before the higher Delalande tax: this is positive and significant only for women. One natural interpretation is that women with children are exposed to greater risk than men with children, as in France the former ended up with sole custody of the children in 80% of separations in the early 2000s.[21] The figures in columns (5) and (10) refer to the interactions with the number of children the respondent had at the time of the reform. The resulting estimated coefficients are positive and significant (and of the same size) only for women with one or two children (although that for women with three or more children is not statistically different from that for mothers with one or two children).

[18] 1963 is the median birth year in our estimation sample. The interaction terms from different birth-year thresholds continue to be insignificant.
[19] We also considered an interaction between the treatment and a continuous measure of monthly wages (in logs), which produces similar results. Using household income rather than the monthly wage also makes no difference.
[20] With respect to hours of work, interactions with pre-reform hours either as a continuous variable or as a dummy for part-time work (between 1 and 29 hours per week) yielded no significant estimates.
[21] See Table 2 in http://www.justice.gouv.fr/art_pix/stat_Infostat%20132%20def.pdf.

Why does the effect of job insecurity differ across some groups of workers? One possibility is that the relationship between the layoff tax and job insecurity is more pronounced for some workers, and especially those who already had children. We look for evidence of this in Online Appendix Table A6, where the dependent variable is now perceived job security. None of the interaction terms in this table is significantly different from zero: as such, all of the different groups of employees in this table react similarly to the reform in terms of their perceived job security.

A second possibility is that the link between perceived job security and marital status may differ across groups, for reasons of risk-aversion. In Görlitz and Tamm (2015), the higher risk-aversion due to parenthood is larger for women. As such, mothers arguably constitute the most risk-averse group of workers in our estimation sample, which might explain why they are the most likely to get married after a rise in their job insecurity.

Risk-Sharing Theory

Our results above are in line with risk sharing theory, as the rise in marriage after the 1999 rise in the Delalande tax appears only for the arguably most risk-averse workers (i.e. women with children), although we have not yet provided any explicit test of this theory. In Hess (2004) and Shore (2010), couples manage income risk by trying to ensure that the two partners' exogenous income shocks are not positively perfectly correlated. In the context of the reform we analyse here, we expect a stronger treatment effect for women whose partner has a stable job.

This is what we test in Table 6, where we interact the reform effect with dummy variables for different types of partners. Columns (1) and (4) show the average effect of the reform for women and men, as in Panel B of Table 3, while columns (2) and (5) show the estimated coefficients of the interaction of the reform with a dummy for "Employed Partner". As risk-sharing theory would predict, the shift from cohabitation to marriage (following the results in

Table A4) is only higher for women whose partners are currently working.[22] We find no significant results for any of the groups of men.

We then take the treatment status of the partner into account. Under risk-sharing, household members try to avoid correlated risks. The marriage incentives from job insecurity should then be weaker for women whose partner is also affected by the reform. The estimates in column (3) are in line with this prediction: marriage only rises significantly for women whose partner does not currently suffer from exogenously-higher job insecurity. We found no significant differences in this respect for the partner working in the public *vs.* private sector or having a permanent *vs.* temporary contract (although this latter might reflect a lack of statistical power, as only around 3% of partners in our estimation sample had a temporary contract). We continue to find no significant marriage results for any of the groups of men (in the last column of Table 6).

6. Conclusion

Job insecurity increases the probability of marriage for women. The 1999 layoff-tax reform in the French labour market protected older workers, but to the detriment of younger workers. As this reform was applied only in larger firms, we can carry out a difference-in-differences analysis. The exogenous change in the future probability of job loss affected perceived job security; it also produced a robust significant rise in the probability of being married for women. Our identification strategy here evaluates the effect of job insecurity on the probability of being married for workers who were employed both before and after the reform (and not those who changed their labour-force status): this is probably what most people will understand by job insecurity, in the sense that insecurity is forward-looking.

[22] The interactions in Table 6 refer to the partner's current labour-market position. As such, the employed in row 2 may have started work after their partner was treated. Equally, in rows 3 and 4, partner treatment may have caused individuals to switch into more secure jobs (i.e. those that are not affected by the higher Delalande tax). If we only consider the partner's pre-reform employment status, we do not allow for these behavioural reactions (if we do so we actually find similar estimates, although the coefficients are less precisely-estimated).

Our results are novel as they are, to the best of our knowledge, the first that appeal to a natural labour-market experiment to show that risk-sharing is one of the causes of marriage. Job insecurity increased the probability of marriage for women, and more so for those who are probably more risk-averse (mothers). In line with risk-sharing theory, we show that this marriage effect was not found for couples in which the layoff risk for both partners rose after the 1999 tax rise (i.e. couples where both members worked in treated firms). The lack of any effect for men may reveal their lower risk-aversion with respect to job insecurity, or the greater difficulty that insecure men face on the marriage market.

Part of the attraction of marriage then seems to be the risk-sharing it provides. Why then don't all couples get married? There are a wide variety of factors at play here, including cultural norms. Some of these can be argued to be economic. In the same way that employment protection legislation might discourage hiring due to the costs of firing, more rigid or expensive divorce procedures may discourage some couples from marrying. In this case, more flexible divorce procedures may lead to more people getting married, as would a more advantageous tax treatment of the married relative to the cohabiting. The flexibility of marriage and the labour market can thus be considered as intertwined.

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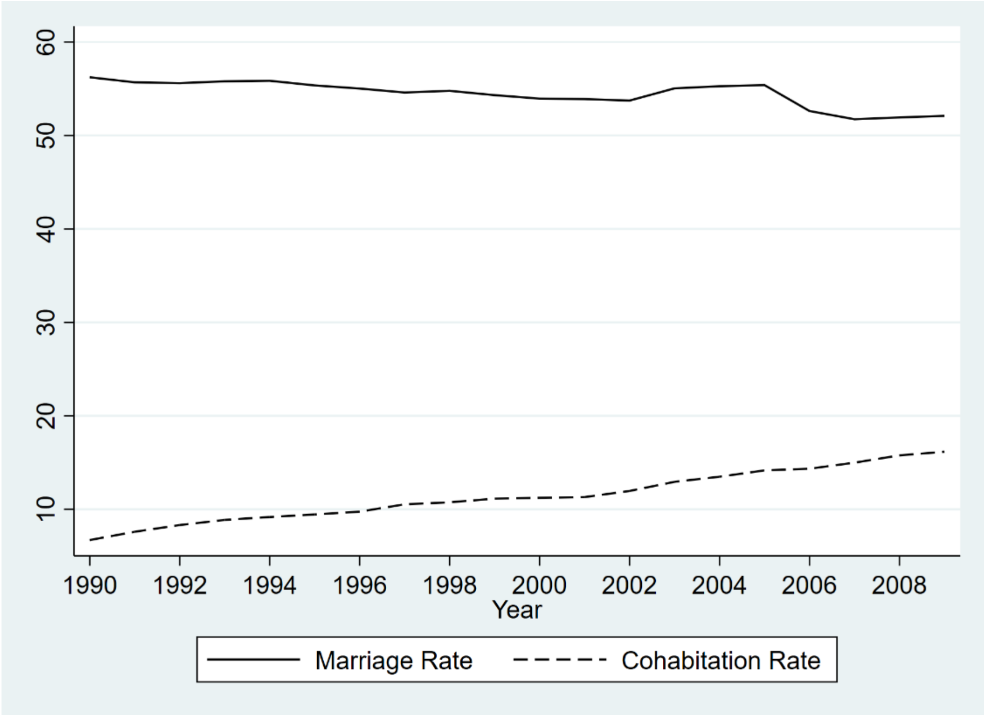
Figures and Tables

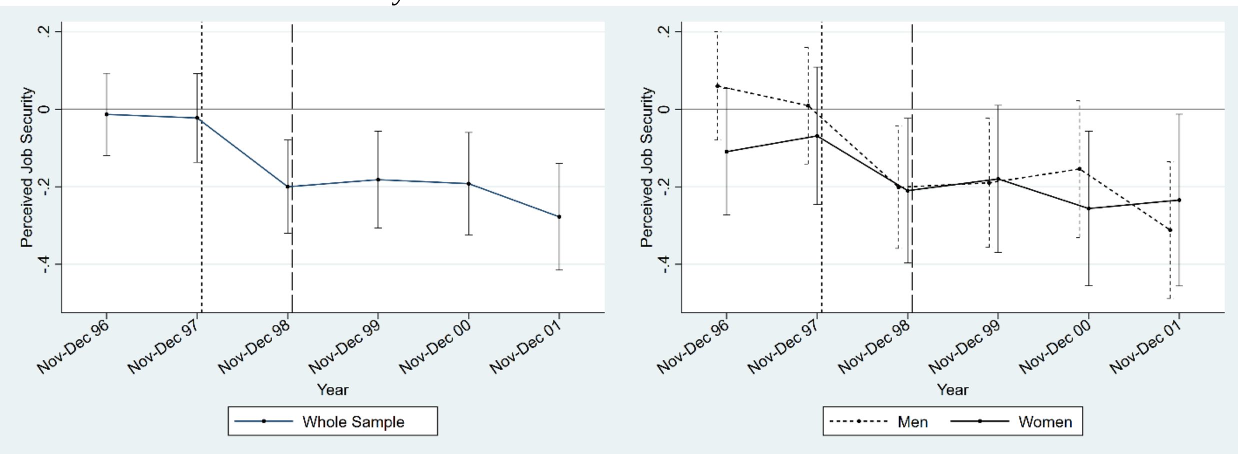
Figure 1: Marriage and Cohabitation Rates in France from 1990 to 2009



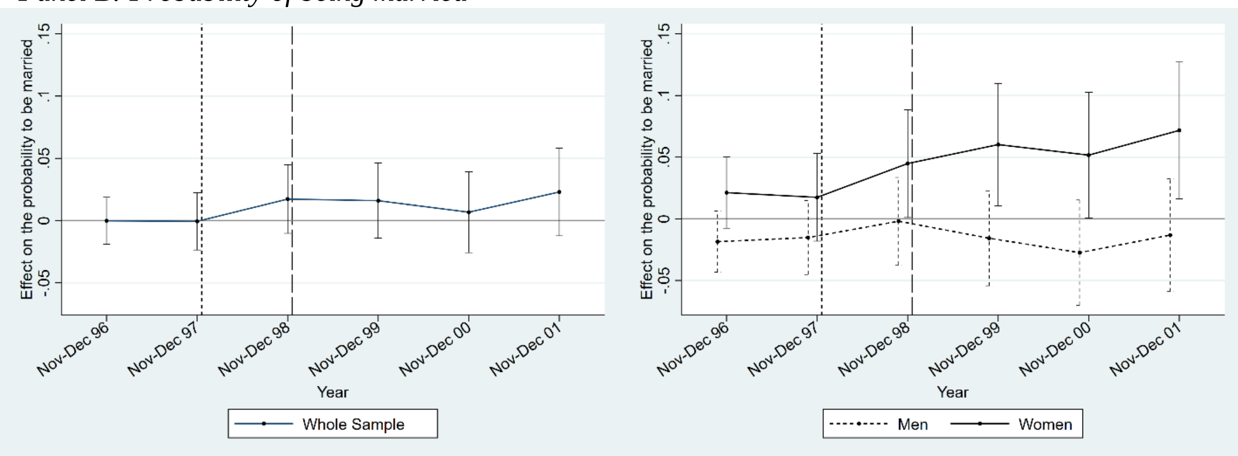
Notes: The marriage and cohabitation rates correspond respectively to the number of married and cohabiting individuals divided by the size of the adult population. These rates were calculated using data from the INED time series on the number of married and cohabiting couples (https://www.ined.fr/fr/tout-savoir-population/chiffres/france/couples-menages-familles/couples_menages_familles/) and from the UN website (http://data.un.org/Data.aspx?d=POP&f=tableCode%3A22) for the adult population.

Figure 2: Parallel-Trend Assumption – Panel Results

Panel A: Perceived Job Security



Panel B: Probability of being married



Notes: The sample here is permanent-contract private-sector workers in the French ECHP between the ages of 20 and 49. There are 10371 observations in the whole sample (2797 individuals), 4248 observations in the sample of women (1175 individuals) and 6123 observations in the sample of men (1622 individuals). Each point shows the gap in the outcome between the treatment group (i.e. being a younger worker in a firm with 50 or more employees) and the control group (being a younger worker in a firm with fewer than 50 employees) in that year, as compared to the same gap in the omitted year (1995). These numbers come from regression analyses that include individual and year fixed effects, as well as age dummies (in five-year bands), the number of children in the household (lagged), health status, weekly working hours, the log of the monthly wage, and occupation and region dummies. The error bars represent the 90% confidence intervals. The dotted vertical line indicates the date at which the rise in the Delalande tax was announced, while the dashed vertical line shows the date of its implementation. Men and women were interviewed at the same date; we have slightly left-shifted the points for men so that the confidence intervals can be seen.

Table 1: The Evolution of the Delalande Layoff Tax over Time

		Worker's age								
		50	51	52	53	54	55	56-57	58	59
July 1987-June 1992	All firm sizes						3	3	3	3
July 1992 - Dec. 1992	20 or more employees	1	1	2	2	4	5	6	6	6
	Under 20 employees	0.5	0.5	1	1	2	2.5	3	3	3
Jan 1993-Dec 1998	All firm sizes	1	1	2	2	4	5	6	6	6
January 1999-2008	50 or more employees	2	3	5	6	8	10	12	10	8
	Under 50 employees	1	1	2	2	4	5	6	6	6

Source: Legislative texts.
Notes: For each age group, the table shows the tax to be paid by the firm to the unemployment-insurance system if it lays off a worker of that age. The tax is a function of the worker's wages, and is stated in months of gross wage. This tax applies to private-sector workers with permanent contracts only.

Table 2: Descriptive Statistics of the ECHP Analysis Sample

	Mean	SD	Min	Max
Dependent Variable:				
Perceived Job Security [1-6]	4.13	1.17	1	6
Married	0.57		0	1
Partnered	0.77		0	1
Partnered but not Married	0.19		0	1
Divorced or Separated	0.04		0	1
Never in a Relationship	0.20		0	1
Difference-in-differences Variables:				
Treatment Group	0.47		0	1
Post Period	0.37		0	1
Individual Characteristics:				
Age	34.60	6.85	20	49
Female	0.41		0	1
High Education (Post-Secondary)	0.24		0	1
Number of Children in Household (lagged)	0.95	0.97	0	8
Health Status [1-5]	3.88	0.72	1	5
Job Characteristics:				
Weekly Working Hours	39.39	7.86	2	96
Monthly Wage (log)	8.98	0.48	5.08	11.60
Observations	10371			
Individuals	2797			

Note: These numbers refer to permanent-contract private-sector workers in the French ECHP between the ages of 20 and 49.

Table 3: The Delalande Tax, Job Security and the Probability of Being Married – Panel Results

Panel A: Job Security (1-6)	Whole Sample		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform Announcement	-0.188***		-0.149*		-0.225***	
	(0.057)		(0.090)		(0.075)	
Reform Implementation	-0.202***		-0.159*		-0.240***	
	(0.053)		(0.086)		(0.067)	
Total Effect of the Reform		-0.198***		-0.156**		-0.236***
		(0.048)		(0.077)		(0.062)
Panel B: Married (Dummy)	Whole Sample		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform Announcement	0.018		0.031		0.010	
	(0.012)		(0.020)		(0.016)	
Reform Implementation	0.015		0.047*		-0.007	
	(0.015)		(0.025)		(0.019)	
Total Effect of the Reform		0.016		0.042**		-0.002
		(0.013)		(0.021)		(0.017)

Notes: These are linear regressions. The sample here is permanent-contract private-sector workers in the French ECHP between the ages of 20 and 49. There are 10371 observations in the whole sample (2797 individuals), 4248 observations in the sample of women (1175 individuals) and 6123 observations in the sample of men (1622 individuals). The announcement effect of the reform refers to the treatment from the beginning of 1998, when the reform to the Delalande tax was announced, up to its implementation on January 1st 1999; the reform-implementation effect considers the implementation treatment starting on January 1st 1999. These two effects correspond to α_2 and α_3 in Equation (1). The total effect of the reform corresponds to β_2 in Equation (2). Standard errors in parentheses are clustered at the individual level. All of the regressions include individual and year fixed effects, as well as age dummies (in five-year bands), the number of children in the household (lagged), health status, weekly working hours, the log of the monthly wage, and occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table 4: The Rise in the Delalande Tax and the Probability of being Married – Robustness Checks

	20+ employees (1)	Spanish sample (2)	Italian sample (3)	Danish sample (4)	OLS (5)	Conditional FE Logit (6)	Donut DiD (7)
Panel A: Whole Sample							
Total Effect of the reform	0.009	-0.003	0.011	0.025	0.033	0.193	0.014
	(0.018)	(0.013)	(0.013)	(0.026)	(0.020)	(0.370)	(0.015)
Individual Time-Invariant Controls	.	.	.	.	Yes	.	.
Individual Fixed Effects	Yes	Yes	Yes	Yes	.	Yes	Yes
Observations	8897	8938	11373	4682	10371	10371	7523
Individuals	2360	3127	3411	1432	2797	2797	2067
Panel B: Women							
Total Effect of the reform	0.050^{**}	-0.040	0.022	-0.014	0.058^{*}	1.530^{**}	0.043
	(0.024)	(0.025)	(0.023)	(0.041)	(0.033)	(0.772)	(0.028)
Individual Time-Invariant Controls	.	.	.	.	Yes	.	.
Individual Fixed Effects	Yes	Yes	Yes	Yes	.	Yes	Yes
Observations	3494	3084	4265	1739	4248	4248	3018
Individuals	944	1134	1340	572	1175	1175	860
Panel C: Men							
Total Effect of the reform	-0.019	0.010	0.006	0.055	0.022	-0.176	-0.005
	(0.025)	(0.016)	(0.015)	(0.033)	(0.025)	(0.498)	(0.019)
Individual Time-Invariant Controls	.	.	.	.	Yes	.	.
Individual Fixed Effects	Yes	Yes	Yes	Yes	.	Yes	Yes
Observations	5403	5584	7108	2943	6123	6123	4505
Individuals	1416	1993	2071	860	1622	1622	1207

Notes: These are linear regressions, except in column (6). The sample here is permanent-contract private-sector workers in the French ECHP between the ages of 20 and 49 (except in columns (2), (3) and (4), where it is their Spanish, Italian and Danish counterparts). The total effect of the reform corresponds to β_2 in Equation (2). Standard errors in parentheses are clustered at the individual level, except in columns (6). The conditional FE logit coefficients in column (6) refer to the log of the odds ratio. All of the regressions include year fixed effects, age dummies (in five-year bands), the number of children in the household (lagged), health status, weekly working hours, the log of the monthly wage, and occupation and region dummies. The individual time-invariant controls are gender and education dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table 5: The Rise in the Delalande Tax and the Probability of being Married – Heterogeneity Analysis

	Women					Men				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Total Effect of the Reform	0.062	0.014	0.019	-0.040	-0.040	0.018	-0.002	-0.012	0.014	0.014
	(0.039)	(0.022)	(0.023)	(0.033)	(0.033)	(0.031)	(0.018)	(0.029)	(0.024)	(0.024)
Interacted with:										
Born before 1963	-0.031					-0.033				
	(0.045)					(0.036)				
High Education		0.080					-0.018			
		(0.055)					(0.049)			
High Monthly Wage			0.039					0.014		
			(0.039)					(0.036)		
Parent beforehand				0.114***					-0.023	
				(0.042)					(0.032)	
1 child beforehand					0.113**					-0.015
					(0.053)					(0.050)
2 children beforehand					0.124**					-0.047
					(0.052)					(0.037)
3+ children beforehand					0.065					0.022
					(0.046)					(0.041)

Notes: These are linear regressions. The sample here is permanent-contract private-sector workers in the French ECHP between the ages of 20 and 49. There are 4248 observations in the sample of women (1175 individuals) and 6123 observations in the sample of men (1622 individuals). The total effect of the reform corresponds to β_2 in Equation (2). Standard errors in parentheses are clustered at the individual level. All of the regressions include individual and year fixed effects, as well as age dummies (in five-year bands), the number of children in the household (lagged), health status, weekly working hours, the log of the monthly wage, and occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

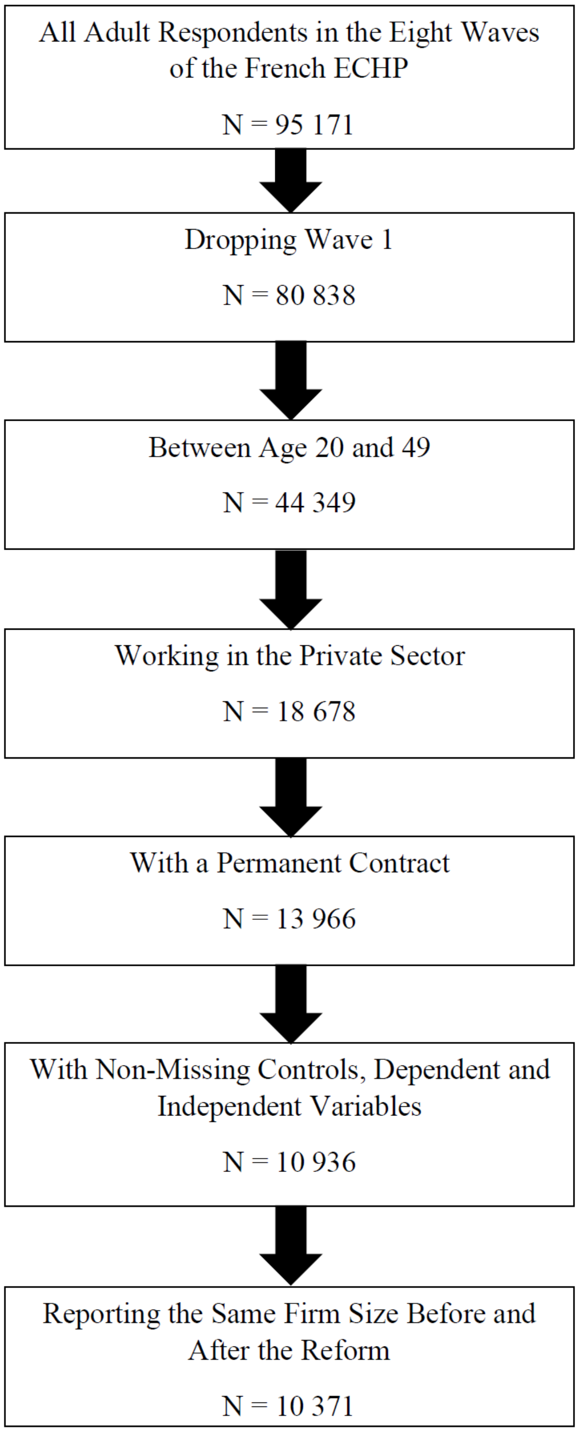
Table 6: The Rise in the Delalande Tax and the Probability of being Married by Partner's Employment Status – Panel Results

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Total Effect of the Reform	0.042**	-0.016	-0.015	-0.002	-0.018	-0.020
	(0.021)	(0.031)	(0.031)	(0.017)	(0.021)	(0.021)
Interacted with:						
Employed Partner		0.093**			0.032	
		(0.036)			(0.027)	
Employed Partner Not Currently Affected by the Higher Delalande Tax			0.101**			0.029
			(0.039)			(0.028)
Employed Partner Currently Affected by the Higher Delalande Tax			-0.017			-0.008
			(0.042)			(0.055)

Notes: These are linear regressions. The sample here is permanent-contract private-sector workers in the French ECHP between the ages of 20 and 49. There are 4248 observations in the sample of women (1175 individuals) and 6123 observations in the sample of men (1622 individuals). The total effect of the reform corresponds to β_2 in Equation (2). The standard errors in parentheses are clustered at the individual level. All of the regressions include individual and year fixed effects, as well as age dummies (in five-year bands), the number of children in the household (lagged), health status, weekly working hours, the log of the monthly wage, and occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

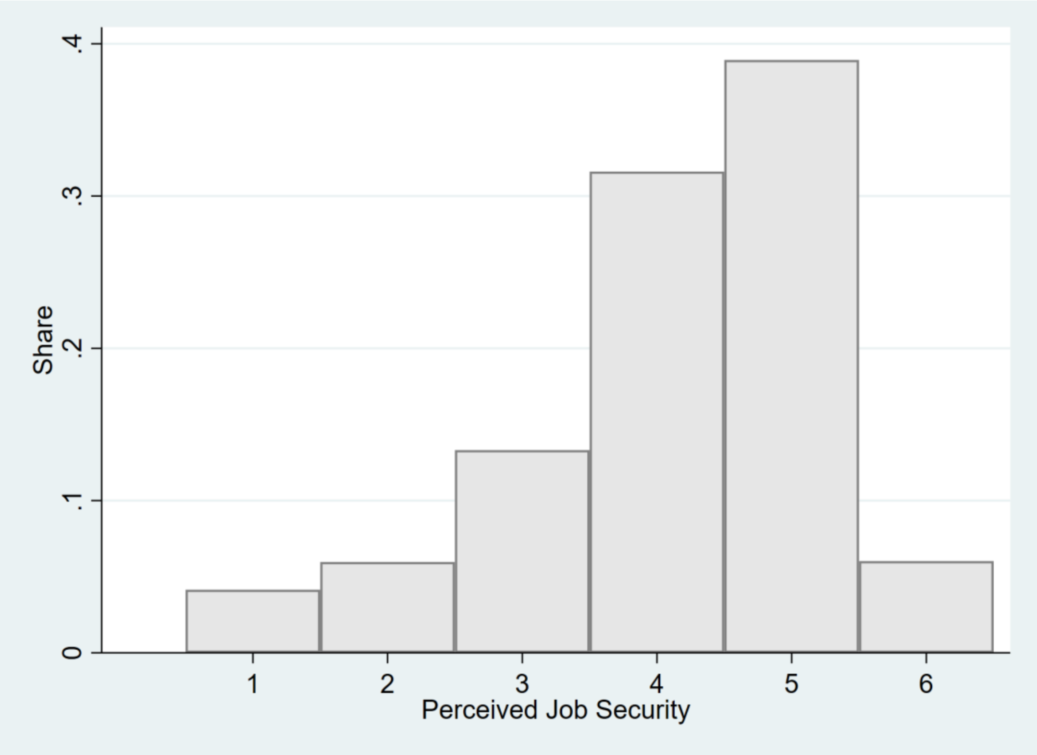
Online Appendix

Figure A1: Sample Selection



Note: Each box shows the number of observations in the French ECHP sample, from the raw data (95 171 observations) to the sample that is used in our empirical analyses (10 371 observations).

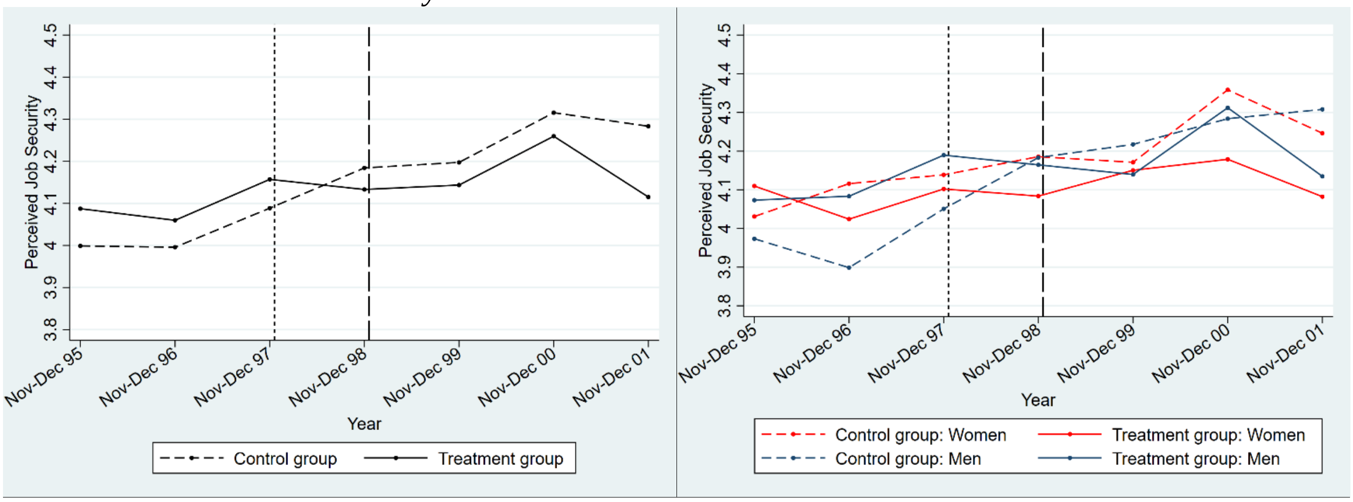
Figure A2: The Distribution of Perceived Job Security in the ECHP



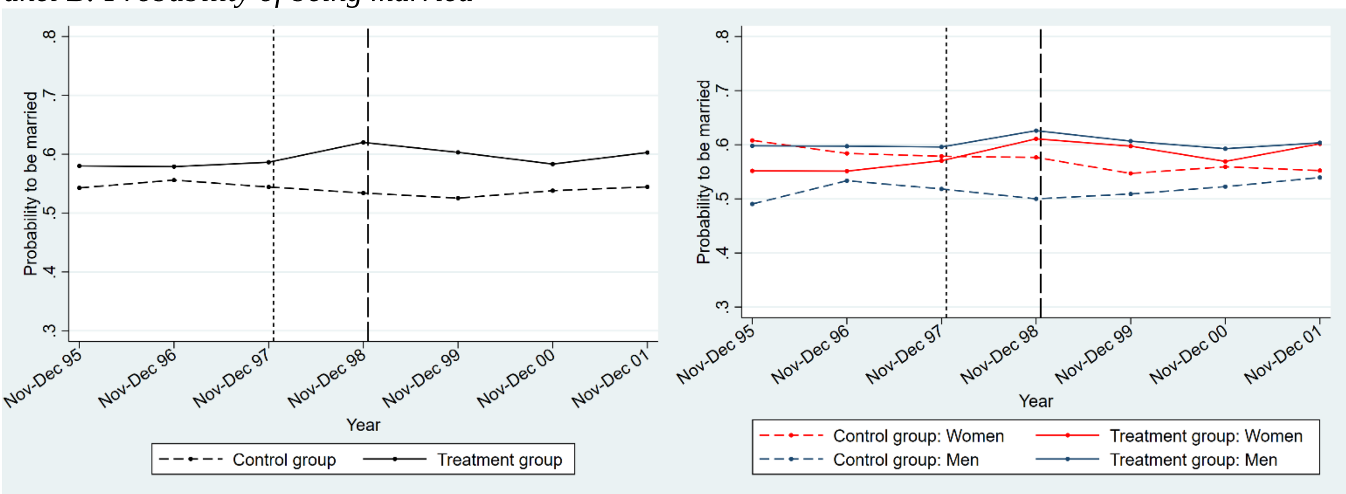
Note: This figure refers to permanent-contract private-sector workers in the French ECHP between the ages of 20 and 49.

Figure A3: Average Perceived Job Security and the Probability of being Married by Treatment Group

Panel A: Perceived Job Security



Panel B: Probability of being married



Note: The dotted vertical line indicates the date at which the rise in the Delalande tax was announced, while the dashed vertical line shows the date of its implementation.

Table A1: The Delalande Tax and Marriage – Panel results with the full set of controls

	Whole Sample		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform Announcement	0.018		0.031		0.010	
	(0.012)		(0.020)		(0.016)	
Reform Implementation	0.015		0.047*		-0.007	
	(0.015)		(0.025)		(0.019)	
Total Effect of the reform		0.016		0.042**		-0.002
		(0.013)		(0.021)		(0.017)
Age: 26-30	0.109***	0.109***	0.090***	0.089***	0.124***	0.123***
	(0.020)	(0.020)	(0.031)	(0.031)	(0.027)	(0.027)
Age: 31-35	0.113***	0.113***	0.088**	0.087**	0.132***	0.132***
	(0.024)	(0.024)	(0.034)	(0.034)	(0.032)	(0.032)
Age: 36-40	0.097***	0.097***	0.063*	0.062*	0.122***	0.122***
	(0.026)	(0.026)	(0.036)	(0.036)	(0.035)	(0.035)
Age: 41-45	0.061**	0.061**	0.003	0.002	0.101***	0.100***
	(0.028)	(0.028)	(0.042)	(0.042)	(0.038)	(0.038)
Age: 46-49	0.006	0.006	-0.080	-0.081	0.065	0.064
	(0.032)	(0.032)	(0.051)	(0.050)	(0.042)	(0.042)
Monthly Wage (in log)	0.021	0.021	-0.007	-0.007	0.054**	0.054**
	(0.016)	(0.016)	(0.023)	(0.023)	(0.021)	(0.021)
Weekly Working Hours	-0.001**	-0.001**	-0.000	-0.000	-0.001**	-0.001**
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Self-Assessed Health [1-5]	-0.001	-0.001	-0.006	-0.006	0.004	0.004
	(0.004)	(0.004)	(0.007)	(0.007)	(0.005)	(0.005)
Number of Children in the Household (lagged)	0.054***	0.054***	0.057***	0.057***	0.052***	0.052***
	(0.007)	(0.007)	(0.012)	(0.012)	(0.009)	(0.009)

Notes: These are linear regressions. The sample here is permanent-contract private-sector workers in the French ECHP between the ages of 20 and 49. There are 10371 observations in the whole sample (2797 individuals), 4248 observations in the sample of women (1175 individuals) and 6123 observations in the sample of men (1622 individuals). The announcement effect of the reform refers to the treatment from the beginning of 1998, when the reform to the Delalande tax was announced, up to its implementation on January 1st 1999; the reform-implementation effect considers the implementation treatment starting on January 1st 1999. These two effects correspond to α_2 and α_3 in Equation (1). The total effect of the reform corresponds to β_2 in Equation (2). Standard errors in parentheses are clustered at the individual level. All of the regressions include individual fixed effects, as well as wave, occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A2: Differences in observable characteristics between the treatment and control group before and after the reform's announcement

	Before the announcement of the 1999 rise in the Delalande tax			After the announcement of the 1999 rise in the Delalande tax			Difference-in-Differences
	Treatment	Control	Difference	Treatment	Control	Difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	34.128	33.092	1.036***	36.140	35.143	0.997**	-0.040
	[6.082]	[6.517]	(0.175)	[7.024]	[7.302]	(0.199)	(0.266)
Female	0.386	0.441	-0.056***	0.380	0.425	-0.045***	0.011
	[0.487]	[0.497]	(0.014)	[0.486]	[0.494]	(0.014)	(0.019)
High Education	0.266	0.187	0.079***	0.298	0.217	0.081***	0.002
	[0.442]	[0.390]	(0.012)	[0.458]	[0.412]	(0.012)	(0.017)
No. Children in Household (lagged)	0.943	0.943	-0.000	0.958	0.959	-0.000	-0.000
	[0.984]	[0.969]	(0.027)	[0.970]	[0.954]	(0.027)	(0.038)
Health Status	3.901	3.916	-0.015	3.839	3.851	-0.013	0.002
	[0.740]	[0.752]	(0.021)	[0.706]	[0.673]	(0.019)	(0.028)
Weekly Working Hours	40.108	39.516	0.592***	38.851	39.097	-0.246	-0.838***
	[6.898]	[9.151]	(0.218)	[6.557]	[8.285]	(0.218)	(0.309)
Monthly Wage (log)	9.004	8.818	0.227***	9.143	8.930	0.214***	-0.013
	[0.440]	[0.471]	(0.013)	[0.472]	[0.482]	(0.013)	(0.018)

Notes: There are 10371 observations in the whole sample (2797 individuals). Standard errors are in parentheses and standard deviations are in square brackets. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A3: Differences in characteristics between the treatment and control group before and after the reform's announcement by gender

	Before the announcement of the 1999 rise in the Delalande tax			After the announcement of the 1999 rise in the Delalande tax			Difference-in-Differences
	Treatment	Control	Difference	Treatment	Control	Difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Women							
Age	33.925	32.902	1.024***	35.712	35.117	0.595*	-0.428
	[6.117]	[6.617]	(0.278)	[7.105]	[7.327]	(0.318)	(0.422)
High Education	0.312	0.219	0.093***	0.338	0.255	0.082***	-0.011
	[0.464]	[0.414]	(0.019)	[0.473]	[0.436]	(0.020)	(0.027)
No. Children in Household (lagged)	0.821	0.915	-0.093**	0.804	0.930	-0.126***	-0.033
	[0.886]	[0.859]	(0.038)	[0.85]	[0.873]	(0.038)	(0.054)
Health Status	3.888	3.876	0.012	3.801	3.835	-0.034	0.045
	[0.718]	[0.754]	(0.032)	[0.710]	[0.678]	(0.030)	(0.044)
Weekly Working Hours	37.701	35.646	2.054***	36.755	35.635	1.119***	-0.935*
	[6.725]	[9.122]	(0.355)	[6.220]	[8.023]	(0.319)	(0.478)
Monthly Wage (log)	8.901	8.625	0.277***	8.993	8.759	0.235***	-0.042
	[0.447]	[0.468]	(0.020)	[0.472]	[0.493]	(0.021)	(0.029)
Panel B: Men							
Age	34.257	33.243	1.014***	36.402	35.162	1.240***	0.226
	[6.058]	[6.434]	(0.227)	[6.963]	[7.286]	(0.256)	(0.343)
High Education	0.236	0.161	0.075***	0.274	0.189	0.085***	0.010
	[0.425]	[0.368]	(0.014)	[0.446]	[0.392]	(0.015)	(0.021)
No. Children in Household (lagged)	1.019	0.966	0.054	1.053	0.980	0.073**	0.020
	[1.034]	[1.047]	(0.037)	[1.022]	[1.010]	(0.037)	(0.053)
Health Status	3.910	3.948	-0.038	3.862	3.864	-0.002	0.036
	[0.753]	[0.748]	(0.026)	[0.703]	[0.669]	(0.026)	(0.037)
Weekly Working Hours	41.619	42.573	-0.954***	40.138	41.661	-1.123***	-0.569
	[6.571]	[7.940]	(0.260)	[6.428]	[7.508]	(0.257)	(0.365)
Monthly Wage (log)	9.134	8.970	0.164***	9.235	9.056	0.179***	0.015
	[0.411]	[0.414]	(0.015)	[0.448]	[0.434]	(0.016)	(0.022)

Notes: There are 4248 observations in the sample of women (1175 individuals) and 6123 observations in the sample of men (1622 individuals). Standard errors are in parentheses and standard deviations are in square brackets. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A4: The Delalande Tax and Other Types of Partnerships – Panel Results

Panel A: Whole Sample	Married	Partnered	Partnered but not married	Divorced or Separated	Never in a relationship
	(1)	(2)	(3)	(4)	(5)
Total Effect of the Reform	0.016	-0.023	-0.009	-0.004	0.013
	(0.013)	(0.015)	(0.013)	(0.007)	(0.011)
Panel B: Women	Married	Partnered	Partnered but not married	Divorced or Separated	Never in a relationship
	(1)	(2)	(3)	(4)	(5)
Total Effect of the Reform	0.042**	-0.013	-0.052**	0.006	0.010
	(0.021)	(0.023)	(0.025)	(0.013)	(0.020)
Panel C: Men	Married	Partnered	Partnered but not married	Divorced or Separated	Never in a relationship
	(1)	(2)	(3)	(4)	(5)
Total Effect of the Reform	-0.002	-0.007	-0.002	-0.009	0.013
	(0.017)	(0.014)	(0.018)	(0.009)	(0.012)

Notes: These are linear regressions. The sample here is permanent-contract private-sector workers in the French ECHP between the ages of 20 and 49. There are 10371 observations in the whole sample (2797 individuals), 4248 observations in the sample of women (1175 individuals) and 6123 observations in the sample of men (1622 individuals). The total effect of the reform corresponds to β_2 in Equation (2). Standard errors in parentheses are clustered at the individual level. All of the regressions include individual and year fixed effects, as well as age dummies (in five-year bands), the number of children in the household (lagged), health status, weekly working hours, the log of the monthly wage, and occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A5: The Rise in the Delalande Tax and Perceived Job Security – Robustness Checks

	20+ employees (1)	Spanish sample (2)	Italian sample (3)	Danish sample (4)	OLS (5)	BUC Ordered Logit (6)	Donut DiD (7)
Panel A: Whole Sample							
Total Effect of the reform	-0.167**	-0.018	-0.049	0.032	-0.110***	-0.475***	-0.142***
	(0.069)	(0.064)	(0.058)	(0.077)	(0.042)	(0.114)	(0.055)
Individual Time-Invariant Controls	.	.	.	.	Yes	.	.
Individual Fixed Effects	Yes	Yes	Yes	Yes	.	Yes	Yes
Observations	6657	8938	11373	4682	10371	16479	7523
Individuals	1740	3127	3411	1432	2797	2797	2067
Panel B: Women							
Total Effect of the reform	-0.202*	0.143	-0.065	0.167	-0.131*	-0.379**	-0.095
	(0.109)	(0.103)	(0.105)	(0.145)	(0.070)	(0.187)	(0.087)
Individual Time-Invariant Controls	.	.	.	.	Yes	.	.
Individual Fixed Effects	Yes	Yes	Yes	Yes	.	Yes	Yes
Observations	2599	3084	4265	1739	4248	6546	3018
Individuals	691	1134	1340	572	1175	1175	860
Panel C: Men							
Total Effect of the reform	-0.150*	-0.092	-0.037	-0.019	-0.103**	-0.549***	-0.192***
	(0.090)	(0.081)	(0.069)	(0.092)	(0.052)	(0.144)	(0.073)
Individual Time-Invariant Controls	.	.	.	.	Yes	.	.
Individual Fixed Effects	Yes	Yes	Yes	Yes	.	Yes	Yes
Observations	4058	5854	7108	2934	6123	9933	4505
Individuals	1049	1993	2071	860	1622	1622	1207

Notes: These are linear regressions, except in column (6). The sample here is permanent-contract private-sector workers in the French ECHP between the ages of 20 and 49 (except in columns (2), (3) and (4), where it is their Spanish, Italian and Danish counterparts). The total effect of the reform corresponds to β_2 in Equation (2). Standard errors in parentheses are clustered at the individual level. The BUC ordered logit coefficients in column (6) refer to the log of the odds ratio and the number of observations is artificially higher due to the estimation method. All of the regressions include year fixed effects, age dummies (in five-year bands), the number of children in the household (lagged), health status, weekly working hours, the log of the monthly wage, and occupation and region dummies. The individual time-invariant controls are gender and education dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A6: The Rise in the Delalande Tax and Perceived Job Security – Heterogeneity Analysis

	Women					Men				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Total Effect of the Reform	-0.150	-0.181**	-0.203	-0.036	-0.034	-0.248***	-0.219***	-0.302***	-0.137	-0.138
	(0.117)	(0.089)	(0.135)	(0.138)	(0.138)	(0.091)	(0.071)	(0.089)	(0.106)	(0.106)
Interacted with:										
Born after 1963	-0.018					0.015				
	(0.154)					(0.123)				
High Education		0.116					-0.061			
		(0.170)					(0.138)			
High Monthly Wage			0.071					0.194		
			(0.167)					(0.125)		
Parent beforehand				-0.163					-0.137	
				(0.165)					(0.129)	
1 child beforehand					-0.192					-0.148
					(0.193)					(0.156)
2 children beforehand					-0.036					-0.109
					(0.184)					(0.156)
3+ children beforehand					-0.688					-0.170
					(0.433)					(0.200)

Notes: These are linear regressions. The sample here is permanent-contract private-sector workers in the French ECHP between the ages of 20 and 49. There are 10371 observations in the whole sample (2797 individuals), 4248 observations in the sample of women (1175 individuals) and 6123 observations in the sample of men (1622 individuals). The total effect of the reform corresponds to β_2 in Equation (2). Standard errors in parentheses are clustered at the individual level. All of the regressions include individual and year fixed effects, as well as age dummies (in five-year bands), the number of children in the household (lagged), health status, weekly working hours, the log of the monthly wage, and occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

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Tel: +44 (0)20 7955 7673 Email info@cep.lse.ac.uk
Website: http://cep.lse.ac.uk Twitter: @CEP_LSE