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Female labor Supply and Divorce: New Evidence from Ireland.*

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Abstract

If participation in the labor market helps to secure women's outside options in the case of divorce/separation, an increase in the perceived risk of marital dissolution may accelerate the increase in female labor supply. This simple prediction has been tested in the literature using time and/or spatial variation in divorce legislation (e.g., across US states), leading to mixed results. In this paper, we suggest testing this hypothesis by exploiting a more radical policy change, i.e., the legalization of divorce. In Ireland, the right to divorce was introduced in 1996, followed by an acceleration of marriage breakdown rates. We use this fundamental change in the Irish society as a natural experiment. We follow a difference-in-difference approach, using families for whom the dissolution risk is small as a control group. Our results suggest that the legalization of divorce contributed to a significant increase in female labor supply, mostly at the extensive margin. Results are not driven by selection and are robust to several specification checks, including the introduction of household fixed effects and an improved match between control and treatment groups using propensity score reweighting.

Key Words : divorce law, natural experiment, labor supply, fixed effects, propensity score.

JEL Classification : J12, J22, D10, D13, K36

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1 Introduction

If participation in the labor market helps to secure women's outside options in the case of divorce/separation, an increase in the perceived risk of marital dissolution can be expected to accelerate the increase in female labor supply. This simple prediction has been tested in the literature, notably by using cross-sectional variation in divorce laws (eg., across US states). In this paper, we suggest exploiting an even more radical change, the mere legalization of divorce, in order to test this hypothesis.

The right to divorce was introduced in Ireland in 1996. We first show that divorce legalization was followed by a sharp increase in marital breakdown rates (including both separations and newly allowed divorces). Then we use this fundamental change in the Irish society as a natural experiment.[1] Following a difference-in-difference approach, we focus on the effect of divorce legalization on female labor supply within intact couples. To account for other possible factors affecting labor supply over the period, we use families at a "low risk" of marital breakdown as a control group. The separation/divorce risk is proxied by a measure of religiosity based on church attendance or, alternatively, a direct estimation of the individual-specific probability of marital breakdown, i.e., a flexible function of individual characteristics and information on religiosity. We use the Living in Ireland Survey, which spans from 1994 to 2001 and hence provides data pre and post divorce legalization.

We show that female labor supply significantly increased as a result of the exogenous increase in the risk of marital dissolution, and that this response occurred mainly at the extensive margin. Thus, building outside options seems to depend crucially on keeping *some* attachment to the labor market. Results are robust to different specification checks. In particular, differences between the treatment and control groups are addressed by propensity score reweighting. Also, since non-random attrition from the survey may cause a selection issue, we account for (time-invariant) unobserved heterogeneity by estimating a household fixed-effects model. Further results show that increased female labor supply was not compensated by either a decrease in domestic time spent on childcare or an increase in childrearing by fathers. There is no compelling evidence that male labor supply has increased with divorce risk. Hence our results suggest that a decrease in specialization within households did not necessarily occur and that women who secured their outside options by increasing labor market participation may have done so, at least in the short-run, at the expense of their leisure time and welfare.

[1] González and Özcan (2008) use the same reform to examine the impact of the risk of divorce on the savings behavior of married couples in Ireland.

The outline of the paper is as follows. Section 2 briefly reviews the literature while section 3 presents the institutional background. Section 4 describes the empirical approach, the data and the definition of the control groups. Section 5 presents the main results and robustness checks. Section 6 concludes.

2 Literature

The impact of divorce laws has received a lot of attention. The first type of question studied in the literature was how divorce laws affect divorce rates, and notably the impact of unilateral divorce, which fundamentally changes the nature of the marriage contract by allowing either party to end it at will. Several authors have exploited time and/or spatial variation in legislation but evidence is mixed. Peters (1986, 1992), using a cross-section of data on women, finds no effect. Allen (1992) and Friedberg (1998) obtain the opposite result using an alternative model specification and panel data recording all the divorces by state and year respectively. Wolfers (2006) finds only a small long run effect of unilateral divorce regulations. González and Viitanen (2009) exploit time and cross-country variations in Europe and find that unilateral divorce had a sizeable effect on the divorce rate.

Closer to our concern, the literature has also examined the impact of divorce legislation on household behavior. Precisely, legal reforms leading to "easier divorce" and subsequent increases in divorce rates are suspected to affect the *perceived* risk of marital dissolution and therefore, potentially, household decisions.[2] In particular, specialization within households may have declined and female labor supply increased.[3] Previous evi-

[2] Several important outcomes have received some attention. Unilateral divorce laws have been shown to decrease domestic violence, spousal homicide, and suicide (Stevenson and Wolfers 2006), to affect fertility (Alesina and Giuliano, 2007) and marriage specific investments (Stevenson, 2007). Divorce also seems to have long-term adverse effects on children (Gruber 2004, González and Viitanen, 2008). Chiappori et al. (2002) find substantial evidence of a change in intrahousehold bargaining associated with a change in the laws.

[3] The traditional division of labor between husbands and wives is commonly argued to be an important gain associated with marriage. Spouses efficiently concentrate on activities in which each of them has a relative advantage so that family utility is maximized (Becker, 1973). The supposed female comparative advantage in domestic production is often attributed to the gender gap in market wages and – less consensually – to a productivity advantage in household activities (such as childcare). However, couples can engage in an efficient degree of specialization only if the relationship is stable and the working spouse can commit to compensate the partner in charge of domestic production. In effect, moving from cohabitation to marriage may lead to increased specialization, as shown by El Lahga and Moreau (2007). Inversely, an increase in the perceived risk of marital breakdown – or the mere possibility to divorce

dence tends to confirm this hypothesis. Using cross-sectional comparisons, Peters (1986) and Parkman (1992) suggest that unilateral divorce led to a two percentage point rise in female labor force participation in the US. These results were argued to be erroneous in Gray (1998) who found that unilateral divorce laws had very different effects depending on the underlying property division laws. Stevenson (2008) revisits the question by taking a long run perspective and adding important controls that were missing in previous studies. She finds that women seeking both insurance against divorce and greater bargaining power within the marriage are more likely to engage in market work when states allow unilateral divorce, irrespective of the underlying property division laws.

The direction of the relationship between women's work and divorce is ambiguous. The rise in women's labor force participation is often seen as responsible for increasing divorce rates (Becker, 1981). However, recent evidence points to the effect emphasized in the present paper. That is, women may take up a job as a form of insurance in case of divorce, or in anticipation of divorce. Evidence of anticipatory behavior has been found in sociological studies (see for instance Poortman, 2005). Recent economic studies also stress the importance of this effect. Using the Michigan Panel Study of Income Dynamics (PSID), Johnson and Skinner (1986) showed that, while the effect of wives' labor market participation on the divorce risk is insignificant, a rising probability of divorce faced by married women increases their labor supply, *whether they ultimately separate or not.* They estimate that up to one-third of the unexplained increase in female labor market participation in the U.S. between 1960–1980, a period during which divorce rates doubled, may be attributed to this effect. Lundberg and Rose (1999) also used the PSID and found that a higher divorce risk is associated with decreased specialization. Gray (1995) found that women's labor force participation increased two to three years prior to divorce.[4]

In this paper, we analyze the effect of an arguably stronger shock to the risk of divorce

– makes intertemporal commitment more problematic and is likely to reduce the level of specialization within marriage. Indeed, spouses who specialize in home production may be disadvantaged in the case of a divorce compared with their partners, and may want to secure their outside options by increasing labor market participation (see Lundberg, 2002, for an enlightening discussion).

[4] Further evidence is provided by alternative methodologies. Papps (2006) calculated divorce probability using a Cox proportional hazard model and data from the National Longitudinal Survey of Youth 1979 and found that married women work more when they face a higher probability of divorce. Using aggregated time series data, Bremmer and Kesselring (2004) found that an increase in the divorce rate results in a long-run increase in the participation rate. Note also that Sen (2000) found different patterns for older and younger cohorts. In the former, women who foresaw a high probability of divorce were likely to work more than their low divorce-risk counterparts; in the latter, labor supply patterns for high and low divorce-risk women were relatively similar.

than the introduction of unilateral divorce: the legalization of divorce in a setting where it was previously banned. We show that the legalization was followed by higher rates of marital breakdown, and exploit the heterogeneity in the risk of divorce across the population to analyze the effect of the reform on the labor supply of married women. We also consider the effect on men's labor supply, which has received less attention and for which the existing evidence is mixed. Among these studies, Kapan (2008) finds no change in husbands' labor supply in response to changes in the divorce law in the UK. Chiappori et al. (2002) argue that men would increase their labor supply only if the laws favor them, while Mueller (2005) finds an increase in the work hours of Canadian men in anticipation of divorce.

3 Institutional Background

The Republic of Ireland was one of the last Western countries not to have any legal provision for divorce, the Irish Constitution of 1937 having put a ban on the dissolution of marriage. A referendum on the subject took place in 1986 in which two-third of the electorate rejected a change in the law. In the wake of the referendum, however, legal separation was introduced;[5] by 1995, 75,000 Irish couples had become legally separated. On 25 November 1995, the question was again put to the Irish electorate. At the beginning of the referendum campaign opinion polls suggested that there would be a clear, if not comfortable, majority in favor of divorce. The margin declined as polling day approached and in the last month before the referendum, the Irish Government placed advertisements in favor of a yes vote in a large number of national and regional newspapers. The result was a very narrow majority (50.28 percent) in favor of the legislation of divorce. The turnout of eligible voters was 61.9 per cent compared to 59.6 per cent in the June 1986 referendum on the same issue. The narrowness of the 1995 vote necessitated a recount (Irish Times, 1995). Based on these facts, we argue that the result of the referendum was largely unexpected and that the introduction of divorce was unanticipated prior to November 1995.

The removal of the ban was incorporated into the Constitution in June 1996, and the new divorce law became effective in February 1997. Divorce in Ireland is not unilateral, i.e., even if the separation requirement is met a divorce is not automatically granted if one of the partners is opposed. The economic consequences of divorce for the spouses are broadly at the discretion of the courts. The law states the factors to be taken into

[5] Judicial separation was possible since 1989. An application can be made in case of adultery or if the spouses have lived apart or have not had a normal marital relationship for at least one year.

consideration, including the contributions made by the two spouses (both pecuniary and non-pecuniary), but there is no explicit policy of equal division of assets. The calculation of actual maintenance payments is up for the courts to decide and is based on the financial resources and needs of the spouses.

The top panel of Figure 1 shows the trend in the number of divorces and judicial separations since the late 1980s according to Census data. Obviously the number of divorces granted rose sharply immediately after it came in to law in 1997. This could simply be a reflection of a 'backlog' being cleared, i.e., separated couples who wished to divorce prior to 1996 were now availing of divorce as it became legally possible. Nonetheless, the number of separated persons did not decrease – even if it progressed less rapidly in the second half of the 1990s than in the first half, as some substitution with divorce may have occurred. The important aspect for our purpose is that the legalization of divorce increased the *overall* rate of marital dissolution (divorces, separations and remarriages).[6] Figure 1 (top panel) confirms that the stock of broken marriages rose sharply from around 40,000 in 1986 to 200,000 twenty years later, and that the progression is much more rapid following the legalization of divorce.[7] We show in what follows that these average figures hide contrasted patterns for different subgroups of the population.

4 Empirical Strategy

4.1 Difference-in-Difference Approach

The possibility of divorce and a rising rate of marital breakdown may encourage married women to increase their labor market participation and strengthen their outside options. We test this simple prediction using a difference-in-difference approach. Denote Y_i the outcome of interest for household i, and X_i a vector of controls. The sample comprises of married couples observed in the pre-divorce period ($Post_i = 0$) as well as following the introduction of divorce ($Post_i = 1$). The variable $Treat_i$ denotes the intensity of treatment

[6] It is noticeable that the number of separations had already started to increase prior to divorce legalization. Several authors discuss how the rise in divorce rates can occur before the introduction of new divorce laws due to a prior change in social norms (Fella et al., 2004, Allen, 1998, Hiller and Recoules, 2009).

[7] The number of married people is also rising over the period but not to the extent as to negate the increase in marital breakdown. According to Census information, a ratio of 14:1 married people to separated/divorced people existed in 1996. This ratio had dropped to 9:1 by 2002 and fell again to 8:1 by 2006. Note also that the legalization of divorce did not absorb previous marriage annulments (the annulment rate remained very small, around the 1% mark, over the whole period under consideration).

for household i, i.e., the degree of exposure to an increased risk of marital breakdown. As discussed in detail below, it is proxied by either the degree of non-religiosity or a direct, individual-level estimation of the probability of separation/divorce. When using religiosity as a binary variable, with $Treat = 0$ for a religious household (control group) and 1 otherwise (treatment group), the estimation goes as follows:

$$Y_i = \alpha Post_i + \beta Treat_i + \gamma Post_i \times Treat_i + \delta X_i + \eta Post_i \times X_i + \epsilon_i. \tag{1}$$

In the case of a binary treatment, the interpretation is standard. That is, α captures the time trend, i.e., the average difference in outcome Y_i between the pre- and post-treatment periods, as identified on the non-treated; β captures the average difference in outcome between the treated and the non-treated; γ is the coefficient of interest, i.e., the difference-in-difference estimator. Covariates X may improve the precision of the model but also control for the differences in observables between treated and control groups. Note that the treatment effect γ may be (wrongly) driven by differing trends in observables between the treated and control groups as captured by the $Treat_i$ variable. We purge the estimation of this effect by introducing interactions between the $Post$ variable and the controls X.[8]

The main outcome Y_i is married women's labor supply as a continuous variable, i.e., their weekly work hours, so that model (1) can be estimated by OLS. We consider two cases, with or without zeros, in order to verify if divorce had an effect at both the extensive and the intensive margin. Also, the participation decision can be estimated by a linear probability model, to ease the interpretation of the coefficients in difference-in-difference analyses, or by a logit model in which (1) represents the propensity to participate in the labor market. We also consider male labor supply in the last section, as well as time spent on childcare by both husbands and wives. Below, we present the data and discuss in detail two essential dimensions that may crucially affect the results: the definition of the pre and post-treatment periods and the choice of the control group.

4.2 Data

Our core results are based on samples drawn from the Living in Ireland Survey (LII). This is a longitudinal survey that was conducted on an annual basis between 1994 and 2001. It is based on a representative sample of the Irish population and contains information

[8] At this stage, we ignore the panel dimension and cluster standard errors at the individual level when estimating model (1) on data pooled over a number of years (see section 4.2). Accounting for selection on observables only and doing so in a linear way may not be enough. To improve on both accounts, we shall also allow for (time-invariant) unobservables using panel information and perform propensity score reweighting (see section 5.2).

on demographics, work duration, social situation, living standards and financial circumstances of Irish families. The original sample consisted of just over 4,000 households and nearly 15,000 individuals per year. For the main difference-in-difference estimations, we use (separately) the subsamples of married women and married men. Since the legalization of divorce may well affect the incentives to marry, we exclude couples whose marriages took place in 1996 or later in order to avoid potential selection into marriage effects (30 observations). Since retirement decisions may interfere with the labor supply response that we aim to capture, we exclude couples above 60 (26% of the initial sample).

Additionally, we use a sample of separated/divorced individuals together with married people to estimate the probability of divorce, as explained below. We also use the Irish Household Budget Survey (HBS) for one of our robustness checks. The HBS is carried out at five-year intervals and contains information on household income sources and expenditure as well as demographic and socio-economic variables. The sample size is around 8,000 households for each wave and the most recent data available are for the years 1987, 1994, 1999 and 2005.

4.3 Sensitivity to Timing

The definition of pre- and post-treatment periods may crucially affect the results and necessitates an extensive sensitivity check. For the main difference-in-difference analysis using the LII, we pool years 1994 and 1995 (until referendum day, the 25th of November 1995) to obtain the pre-divorce group. We make use of different post-divorce introduction periods. Once people knew that divorce was going to be introduced presently, they might have adjusted their behavior there and then. Hence the first "post" group is simply obtained by pooling observations from voting day until 2001. Since the first Irish divorce was passed in 1997 – with substantial media coverage – we use the period 1997-2001 as an alternative "post" group. As one may argue that it took time for the increased rate of divorce/separation to affect the perceived risk of marital breakdown, we also use a later period 1998-2001.

We also provide a "check" difference-in-difference estimation based on pre- and post-periods which do not surround the legalization of divorce, namely 1998-99 and 2000-01. This can be seen as a 'placebo' test, the aim of which is to verify whether the approach may be picking up a general trend rather than the effect of divorce introduction.

A specific issue is related to the fact that 1,515 households were added to the survey in 2000 because of some attrition over the life of the survey and to ensure that a representative sample was maintained. There may be bias in the original sample because of attrition, but the refreshment sample may also cause bias because of possible differences from the original sample. The fixed effects estimation presented below is a partial check on that issue. We also present results both with and without the refreshment sample for years 2000-2001.

Finally, when using the HBS, we simply choose the two available waves which most closely surround divorce legalization (1994 and 1999) and compare with LII estimations based on the very same years.

4.4 Control Groups

We suggest control groups that are subject to similar economic conditions as the treated but did not experience, or were much less affected by, the increase in the perceived risk of divorce following the law change. Firstly, we identify the risk of marital dissolution using the degree of religiosity (see also González and Özcan, 2008). We then carry out a direct, individual-level estimation of the risk of marital breakdown using the LII survey and a number of covariates.[9]

Religiosity

While most European countries had a legislative basis for divorce from the first half of the 20th century, three countries had a ban on divorce in place until relatively recently: Italy (divorce was legalized in 1970), Spain (1981) and Ireland (1996). These three countries are also predominantly Roman Catholic.[10] Since divorce is banned by the Catholic Church, it is plausible to think that religious couples would be less responsive to the legalization of divorce. Our first treatment variable therefore relies on proxies for the degree of (non-)religiosity.

Studies on the economics of religion typically use church attendance as a measure of religiosity at the individual level when self-reported religiosity is not available, as it is the case in our data (Iannaccone, 1998). In the LII survey, respondents are asked 'Apart from weddings, funerals and christenings, about how often do you attend religious services?'.

[9] Note that we refrain from using single individuals as a control group for several reasons. Firstly, there is possibly an important lack of "common support" between the two groups (especially with respect to age). Also, the labor supply behavior of singles is fundamentally different from the joint decision of partners in a couple. In our data, labor supply patterns of the two groups are very different, not only in level but also in trend. Finally, even though evidence for Ireland does not point to a radical change in the marriage rate, the decision to marry is potentially affected by the legalization of divorce (since there is a change in the value of marriage).

[10] In Ireland, out of $21,355$ marriages in 2005, 74.3% were celebrated as a Catholic marriage, 3.4% under other religious denominations (93% of which were Church of Ireland or Presbyterian) and 22.3% as civil marriages.

The response takes a value of between 1 (attends religious services more than once a week) and 7 (never attends religious services).[11] For the main results, we use the answer either as a continuous variable ($Treat$ is increasing with the degree of non-religiosity) or as binary, where the control group ($Treat = 0$) is composed of households where the wife *attends church at least once a week*. This threshold is found to be the relevant one as discussed in the estimations of the risk of marital breakdown below.

As in González and Özcan (2008), we believe that this is a robust control group for the difference-in-difference estimation. Firstly, and most importantly, there is clear evidence that "religious women", so defined, do have a much lower rate of marital dissolution (around 4 times less than non-religious couples prior to divorce legalization and around 6 times less by the end of the 1990s). This can be seen in the lower panel of Figure 1 where we plot the rates of separation and marital breakdown (separations plus divorces) for religious and non-religious households. We also point to the fact that religious couples were not affected by the new law: the rate of separation remains constant and the number of actual divorces is marginal. Secondly, we do not believe that church attendance reflects only a compliance with social norms in such a religious country as Ireland. The 2002 European Social Survey asks about both church attendance and self-reported religiosity (on a scale from 0 to 10). Around 89% of those who attend church at least once a week also report to be religious or very religious (values 5-10), versus 34% for those who attend less than once a week.[12] Finally, church attendance typically occurs at times where it does not interfere with work choices (Saturday evenings, Sunday mornings), and hence should not conflict with our estimates of female labor supply.

Nonetheless, it is important to check for potential differences between religious and non-religious women. This is taken care of by the inclusion of X_i and $Post_i \times X_i$ interactions terms in the regression. Moreover, we focus on married couples and may have to account also for spouses' religiosity. Clearly, the main treatment, as defined above, uses own religiosity, i.e., we use the church attendance of the wives (husbands) in the regressions on female (male) labor supply/participation. Yet we have also experimented with alternative measures based on both spouses' church attendance (e.g., $Treat = 0$ if both attend at least once a week) or constructed as a religiosity "score" based on both spouses' answers, as explained in the sub-section on robustness checks.

In addition, we use another question from the LII survey concerning confidence in the

[11] There is very little variation in reported church attendance over time but we nonetheless fix the response to this question equal to the response given the first time the individual appears in the survey.

[12] Inversely, among those who report to be very religious (values 8, 9 or 10), 82% also report attending church at least once a week.

church (answers are 1-great deal, 2-quite a lot, 3-not very much and 4-none at all) and the amount of donations to the church reported in the HBS (and calculated as a proportion of household total disposable income). Contrary to the question on church attendance, it is difficult to decide on a cut-off to create a binary treatment, so we simply use the level of donation as a continuous proxy for religiosity. Only for the purpose of reporting descriptive statistics (see below) do we create a binary variable where religious households are defined as those with positive amounts of donation to the church.

Risk of Marital Breakdown

The control groups previously defined require some assumptions, for instance the choice of a threshold for the binary variable, the cardinality assumption when using religiosity in a continuous way or the definition of particular scores. Alternatively, we can estimate and predict directly the individual probability of marital breakdown using church attendance and other controls, then use it as a continuous variable for the risk of marital dissolution ($Treat$) in the difference-in-difference estimation. This way, we "let the data speak" about the influence of the different church attendance levels on the propensity of marital breakdown. To do so, we run a probit regression on the sample of all women (married, separated or divorced) where the dependent variable takes a value of one if a woman is separated/divorced.

Estimates and marginal effects are reported in Table 1. The first specification includes a single dummy for religiosity and shows that attending church at least once a week is associated with a smaller risk of being divorced/separated. The magnitude (-4.2 percentage points) reduces the probability of being divorced/separated to almost zero compared to the average predicted probability for married women. The second estimation uses the complete set of dummies for the different answers to the church attendance question and is used to predict divorce probability for married women hereafter. Results with this flexible specification show that church attendance less than once a week increases significantly the probability of marital breakdown ("more than once a week" is the omitted category and the coefficient for "once a week" is not significant). This lends support to our choice of "at least once a week" as the relevant cutoff for the binary treatment variable previously defined.[13]

[13] Other controls show that age has an inversed U-shape effect on the risk of divorce/separation while the presence of young children and the number of children decrease it. Urban dwellers, those with low educational levels or with university degrees are more at risk. A third specification (not reported) includes political views but does not improve the fit much. It only shows that those close to the Workers' Party are also more at risk. A limitation of these estimations is that for those who are divorced or separated, information on their previous marriage is not available. That is, we cannot use information on their former

4.5 Descriptive Statistics

Before turning to the estimation results, we report the descriptive statistics of our sample of married women in Table 2. We first describe our full LII sample, then present statistics for both LII and HBS for the only two years available in the HBS (1994 and 1999). We distinguish between religious and non-religious couples using the church attendance definition ('at least once a week') for the LII, and using positive donations for the HBS. Interestingly, the two definitions give relatively similar proportions of religious families (76% and 72% in LII and HBS respectively, for the pre-divorce legalization period). Note that the proportion of religious persons is larger than the proportion of voters against divorce legalization, but religious people may well accept that others need to divorce.

Not surprisingly, both LII and HBS datasets show that religious couples are older, with less children (perhaps due to the difference in age structure), more highly concentrated in rural areas and have less university degree qualifications. The most likely reason for these differences is a cohort effect. Table 2 shows that religious women work less than non-religious ones in general. Again, this may reflect the slightly older makeup of religious women. Importantly, our estimations control for characteristics such as age and education in X_i and $Post \times X_i$ terms. In section 5.2, we control more specifically for the (observed) differences between treatment and control groups using propensity score reweighting. Notice that both religious and non-religious women increased their labor supply over the time period in question, which translates secular trend in increased participation and the more specific context of the "celtic tiger" economic upturn. The important observation is that non-religious women have increased their participation by a greater extent, i.e., we find a crude effect of around 5 points when using the whole LII sample to compare pre- and post-divorce legalization (see table Table 2). This can be visualized clearly in Figure 2, where the time trends in female labor market participation is depicted by religiosity group. Both groups show an increasing trend but the rise is more rapid for non-religious women. This is very suggestive of a positive effect of divorce legalization on the participation rate of women affected by the increased risk of marital breakdown. The rest of the paper aims to move beyond these average trends (and crude difference-in-difference measures) by controlling for individuals characteristics.

husband (e.g., their religiosity, the age gap, etc.) or marriage (age at marriage, length of marriage, etc.).

5 Results

5.1 Main Difference-in-Difference Estimations

We firstly present our difference-in-difference estimations based on the LII survey and for the three main treatment variables as explained above: (1) a binary variable taking a value of 1 if the wife attends church at least once a week, (2) the continuous religiosity variable based on the wife's church attendance and (3) the continuous risk of separation as predicted using the LII data. Several other treatment options have also been experimented with and are discussed below. The alternative pre- and post-divorce introduction periods and the different outcomes are those defined in the previous section. In the various tables described below, we simply report the coefficient on the $Post \times Treat$ variable, i.e., the average treatment effect on the treated.[14] The sign and significance of this coefficient γ is indeed the relevant information for all the models at use, including the probit of participation.[15]

Table 3 shows that coefficients are all significant for the participation model and for labor supply (work hours including zeros), for the four alternative "post" periods and the three main treatment variables. None of the estimates are significant for work hours excluding zeros. This indicates that the response to the introduction of divorce occurred at the extensive margin. That is, for those married women who already worked, there seem to be no significant response to the legalization of divorce. This provides an interesting insight into the bargaining mechanisms possibly at work within married couples. Precisely, what seems to matter for women who want to build up outside options is to keep *some* attachment to the labor market rather than to increase hours of work. Having a job, whether it is part- or full-time, may be enough to maintain human capital levels, access to a social network, access to a potential remarriage market, etc. Other studies, and in particular Johnson and Skinner (1988), confirm that women's increase in labor supply in

[14] The set of estimation tables for all the scenarios (different treatment definitions, pre and post periods definitions and outcomes) is not included due to lack of space but is available from the authors. Results are relatively standard concerning the determinants of female labor supply: the presence of young children and the number of children decrease female participation, as does the level of household income other than female labor income (capital income and husband's earnings); participation increases with education levels and varies with age according to an inverted U-shape. The R2 (for OLS estimations) and pseudo-R2 (for logit estimations) are at conventional levels for the work hour equation (including zeros) and the participation model, but small for the estimation of work hours excluding zeros.

[15] Concerns about the interpretation of interaction terms in non-linear models have been raised by Ai and Norton (2003). However, Puhani (2008) demonstrates that these concerns are not relevant for the treatment effect in non-linear difference-in-difference models.

anticipation of divorce is mostly on account of an increase in participation rather than in work hours.

Several checks have been performed. We find that excluding the $Post \times X_i$ interaction terms does not affect the estimates much (not reported). Also, omitting the refreshment sample (896 observations, 9% of our total sample) does not change the results fundamentally when 1997-2001 is used as the "post" period (see fourth rows in panels of table 3). Finally, it is reassuring to see that the coefficients obtained with our placebo test, i.e., when the "pre" and "post" periods follow the divorce introduction, are not significant (see fifth and sixth rows in panels of table 3). This conveys that the effect is not due to general differences in labor supply trends between religious and non-religious women.

We now look at the magnitude of the effect, first considering the participation model with treatment 1 (the binary variable for religiosity). When using a linear probability model, the effect is directly given by coefficient γ (top left panel of table 3, first row), ranging within a narrow $.07-.08$ interval over the different "post" scenarios. We have also calculated the marginal (rather, incremental) effect when using a logit for participation (top right panel of table 3). In that case, the treatment effect is slightly larger, around .10, but does not vary significantly between the different timing scenarios.[16] This means that, following the legalization of divorce, the participation rate of non-religious married women increased by around 10 percentage points, relative to religious married women. Expressed as a proportion of the average participation rate of non-religious women prior to divorce introduction (40%), this points to a 25% increase. Remaining with treatment 1, the coefficient for the work-hour model (including zeros) shows that post divorce introduction, the work duration of non-religious married women increased by around $2.2-2.7$ hours per week relative to religious married women (see bottom left panel of table 3). Using treatment 3, we obtain coefficients of around 25 in the case of work hours (incl. zeros). Dividing these coefficients by 100 gives an intuitive interpretation: a one percent increase in the risk of marital breakdown leads to an increase in labor supply of around 0.25 hours per week. It also leads to an increase in participation of around 1.1 point according to the logit model (top right panel of table 3) and .8 point according to the linear probability model (not reported).

[16] As said above, the concern raised by Ai and Nortn (2003) does not apply here, and this effect can be calculated simply as the incremental effect of the coefficient γ of the interaction term in the logit estimation (see Puhani, 2008).

5.2 Robustness Checks and Additional Results

The results described in the previous sub-section convey that female labor supply amongst groups at a higher risk of divorce has significantly increased following the legalization of divorce in Ireland. We now suggest several robustness checks and additional results.

Propensity Score Reweighting

Descriptive statistics (Table 2) have shown that religious and non-religious women have relatively different characteristics, likely indicative of a cohort effect. We have accounted in a linear way for observed differences in X, and how these characteristics affect labor supply in the post-divorce introduction period. It is possible, however, to use matching techniques to relax the linearity assumption and to check (or impose) common support. In the case where the treatment variable is binary (treatment 1 in our previous results), a simple approach consists in estimating the propensity of being treated and using the inverse propensity score to reweight the data. Denoting $\widehat{\lambda}_i = \widehat{P}(Treat_i = 1)$ as the estimated probability of treatment for observation i, we use the weights suggested by Firpo (2007) in a more general (non-linear) framework, that is $1/(1-\widehat{\lambda}_i)$ and $1/\widehat{\lambda}_i$ for non-treated and treated observations respectively.[17]

According to Table 4, results are relatively robust to this sensitivity check in terms of significance. The inclusion of the X_i and $Post \times X_i$ interaction terms in our regressions was already quite successful in controlling for differences in characteristics between the treated and control groups. Yet we can observe that coefficients of the participation logit are slightly larger when reweighting is used, and so are the standard errors. The coefficients of the linear model in hours (including zeros) increase by around a third (a half for the first timing scenario).

Selection and Fixed Effects Model

A potential bias in the preceding results stems from the fact that we focus on married couples. Yet it is possible that the stock of marriages that survive post-1996 may not be comparable to the pre-1996 ones, as the "worst marriages" may drop out of our selected sample upon divorce introduction, particularly for the non-religious. To deal with this issue, we have replicated our estimations while excluding all women that are observed getting separated or divorced at any point during the survey – that is, they are no longer in both our pre- and post-divorce samples. This excludes only 121 observations so that the results with the remaining "stable marriages" are not fundamentally different from the

[17] We have checked that the mean of each covariate in X, as well as the mean propensity score, is approximately equal across the treatment and control groups once these weights are used.

baseline estimates. In any case, this does not solve the problem of non-random attrition due to couples who disappear from the original dataset following a separation/divorce.

A traditional way to deal with these issues is to estimate a fixed effects model using the panel information in the LII data. The new model is written as follows:

$$y_{it} = \alpha_i + \beta_t + \gamma Post_{it} \times Treat_i + \delta Z_{it} + \eta Post_{it} \times Z_{it} + \epsilon_{it} \tag{2}$$

with Z_{it} a vector of time-varying control variables, α_i the individual fixed effect and ε_{it} an i.i.d. normally distributed stochastic term accounting for possible measurement error. As before, the coefficient γ captures the potential effect of the increased risk of marital dissolution on the outcome for the treated. The dummy $Post_{it}$ takes a value of 1 if household i is observed in year t which is posterior to the introduction of divorce, and 0 otherwise. It is only introduced through interaction terms since the time trend is already accounted for in β_t.

The selection problem would be solved if dropping those who separate/divorce post-1996 is equivalent, for the labor supply estimation, to taking out a random subgroup; that is, if the residual ϵ_{it} is not correlated with the propensity to separate/divorce. We control in a linear way for the *observed* characteristics that can affect this propensity (e.g., birth of a child), and the fixed effects α_i may well capture time-invariant unobservables that are correlated with the divorce risk.[18] A usual limit to this approach is that we ignore the possibility that time-varying unobservables (negative shocks like unemployment) affect both women's participation and their risk of divorce. We also estimate the fixed effects model where observations are reweighted by the inverse propensity score as explained above. According to Smith and Todd (2005), combining these two methods is more robust than traditional cross-section matching estimators, as it allows selection on observables as well as time-invariant selection on unobservables.

In table 5, we compare the different models for work hours including zeros and for treatment 1. Reassuringly, the treatment effect is significant for the different timing scenarios and insignificant for the placebo check. The simple fixed effects model gives much smaller estimates, between 35% and 45% of what we previously found using the reweighted difference-in-difference model. Results are very stable when adding interaction terms and/or reweighting (again with the exception of the first timing scenario, which gives slightly larger effects). As before, standard errors increase when reweighting is used. In

[18] For instance, the age at the beginning of the relationship (or the age at marriage), which is known to influence strongly the chances of marital breakdown and which is unfortunately not available in the data.

terms of work hours, the treatment effect is in the range $1.3 - 2$ over the different models and timing scenarios. Similar estimations for the participation decision (not reported) give a participation effect around 4 percentage points, which corresponds to an increase of 10% compared to the pre-divorce situation, to be compared to 10 points and 25% with the reweighted difference-in-difference model.

Alternative Treatment/Control Groups and Datasets

We have also checked that results are robust to the choice of the treatment variable. For instance, using the fixed-effects model with $Post_{it} \times Z_i$ interactions, we find that for all the "post" scenarios, the coefficient γ is significantly positive for treatments 2 and 3 as previously defined, but also for alternative binary variables of religiosity (wife's attendance: more than once a week; both wife and husband attend once a week; both wife and husband attend more than once a week; both wife and husband have a high degree of confidence in the church) and several continuous variables (wife and husband's additive and multiplicative scores for church attendance; wife's confidence in the church; wife and husband's additive score for confidence in the church).

A final robustness check is carried out using an alternative measure of religiosity based on donations to the church and drawn from the Household Budget Survey. Since the treatment in the LII analysis is the degree of non-religiosity (or the direct risk of marital breakdown), we compute the degree of non-religiosity in the HBS as either $1/exp$(church donation) or $1-$ (church donation), where church donation is expressed as a proportion of disposable income. Since the HBS only overlaps with the LII survey in years 1994 and 1999, we replicate the results based on the LII for these two years only, in order to improve the comparison. Results of the difference-in-difference estimations are described in table 6. Both measures confirm that participation and work hours (incl. zeros) increased between 1994 and 1999 as a likely response to the increased risk of marital breakdown.

Results for Men and Childcare Time

While we expected the higher risk of divorce to increase married women's labor supply, the expected effect on men's is more ambiguous. Men may want to work less in order to reduce expected maintenance payments, but they may want to work more in anticipation of the costs of potential separation and divorce.

We proceed with similar estimations on the labor supply of married men. The main findings are reported in table 7 for treatment 1 (using husband's church attendance) and work hours (including zeros). We find very weak evidence of an increase in male labor supply when using the difference-in-difference model with propensity score reweighting or the fixed effects model. Estimates of the treatment effect are significant for some, but not all, of the timing scenarios with the PS-reweighted fixed effects model. Yet the magnitude of the effect varies extremely from one timing scenario to the other, and coefficients are also significant (and of a similar or larger magnitude) in the 'placebo test', which casts serious doubt that what we are capturing here is the real effect of divorce laws on male labor supply. Results are similar when using either husband's or wife's attendance to the church. We conclude that the introduction of divorce did not increase married men's labor supply.

Finally, we have checked that our main results do not change significantly when focusing on families with children. For these, we have also used LII information on time spent by married women and men on childcare. An issue is that the definition of this variable has changed, from a discrete choice ("less than 2 hours per day", "two to four hours", "more than an hour") in 1994 to the exact number of hours of childcare per week from 1995 data onwards. Since it was difficult to reconcile these two pieces of information into a consistent variable, we have rerun our estimations using the second definition and for the years 1995-2001 only, which reduced the number of observations prior to divorce legalization. Therefore, results are probably less robust than for labor supply estimations. We focus on households with children only. In these, the average childcare time by fathers is 9.5 hours per week in 1995 and 10 hours in 2001 while it is respectively 63 and 58 hours for mothers. The right panel of table 7 shows in fact that childcare time has not change significantly for men or women in response to divorce legalization.

6 Conclusion

This paper exploits the recent legalization of divorce in Ireland as a natural experiment to analyze the effect of an increase in the risk of marital breakdown on spousal labor supply. Using a difference-in-difference approach, we show that the exogenous shock to the risk of marital breakdown brought about by the reform is responsible for a significant increase in female labor supply. The effect is found to be especially strong at the extensive margin. In other words, it seems that the increased risk of divorce led women to acquire insurance against the potential negative shock of divorce by participating more in the labor market.

We have shown that labor supply increased significantly also for the sub-group of women with children. It is tempting to go one step further and to suggest that divorce reduces specialization in marriage by accelerating the decline of traditional gender roles.

However, additional evidence shows that time spent on childcare by men has not significantly increased while childrearing by women has not significantly decreased. Further research is needed to check if this conclusion extends to other domains within the sphere of domestic production. In other words, it is possible that domestic activities performed by wives, and hence the production of public goods within the household, have declined. It may also be the case that married women with children have seen an increase in their total working time (domestic and market work) with the reform, i.e., a decrease in pure leisure, and a possible loss in welfare. This would be partly compensated if men undertook more of the other domestic tasks or if women were compensated by a larger consumption share (see Browning and Gørtz, 2006, for direct evidence on individual expenditure, domestic and market work). It is unclear, however, whether legalizing divorce may have strengthened or weakened wives' bargaining position within the marriage.[19] Further research could possibly evaluate the welfare effects of the reforms by using the subjective well-being information contained in the Living in Ireland Survey. In particular, it would be possible to follow Alessie et al. (2006) to recover the sharing rule consistent with spouses' individual welfare measures and check if the rule changed around the time of the reform.

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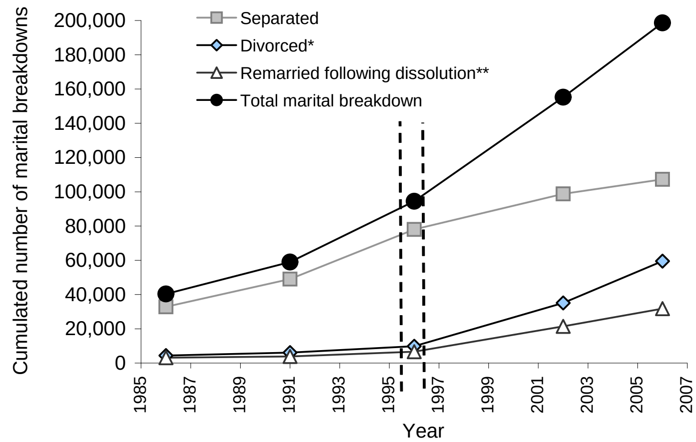
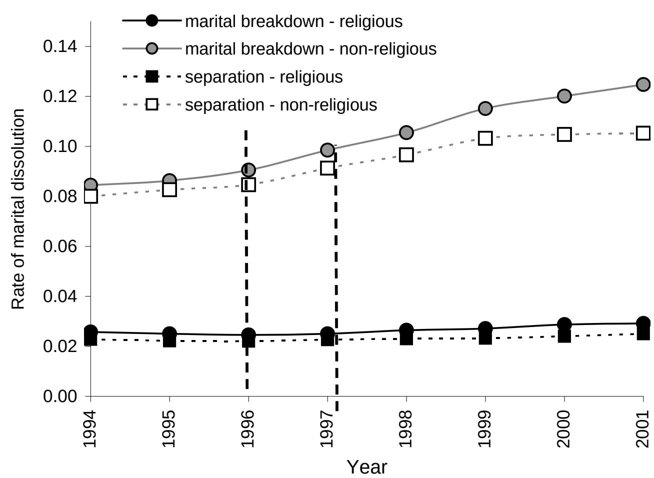
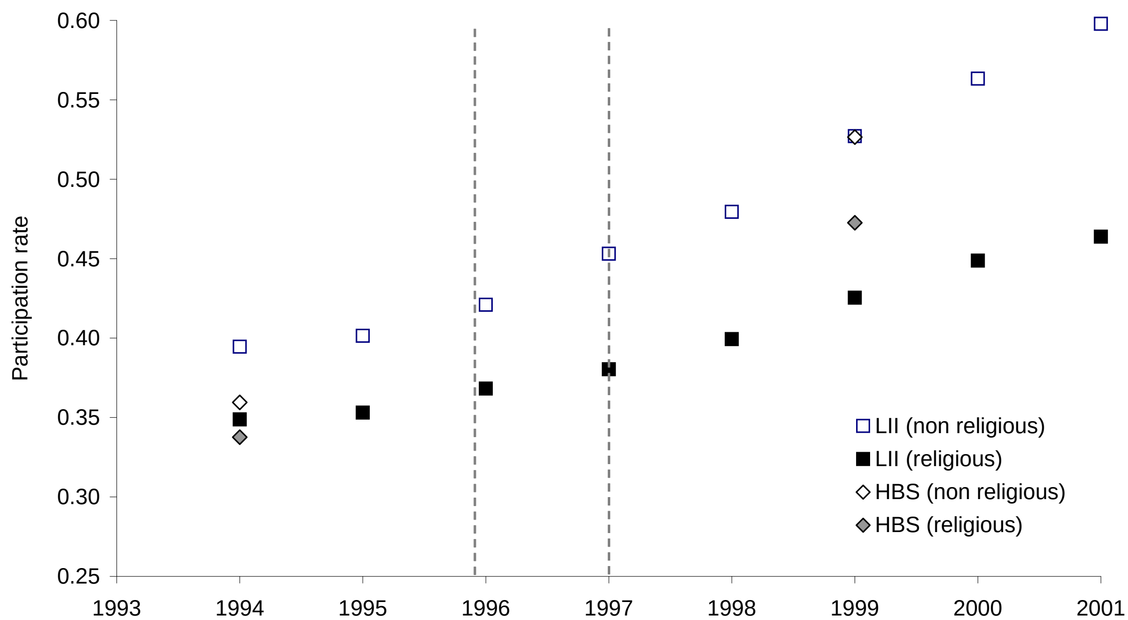


Source: Census
** Prior to 1997: divorced in other country as divorce was not legal in Ireland*
*** Remarriage includes only those who remarried after divorce, not following widowhood.*



Source: own calculation using the LII survey. Religious: defined as "attend church at least once a week". Marital breakdown: separation & divorce. Moving averages. Dashed lines indicated dates of referendum and of first divorce.

Figure 1: Trend in Marital Breakdown (Census and LII)



Source: Living in Ireland Survey (LII) and Irish Household Budget Survey (HBS); moving averages.

Figure 2: Time Trend in Female Participation: Religious vs. Non-religious

Table 1: Probability of Marital Breakdown: Estimates

	Coef.	Std. Err.		Marg. Eff.	Coef.	Std. Err.		Marg. Eff.
Age	0.13	0.04	***	.008	0.13	0.03	***	0.008
Age2 / 1000	-1.45	0.38	***	-.084	-1.43	0.38	***	-0.083
Young chidren	-0.17	0.10	*	-.009	-0.18	0.10	*	-0.009
No. of children < 18	-0.11	0.04	***	-.006	-0.11	0.04	***	-0.006
Urban	0.38	0.10	***	.022	0.38	0.10	***	0.022
Religiosity (church attendance)								
binary:								
at least once a week	-0.54	0.08	***	-.042				
detailed categories:§								
once a week					-0.07	0.09		-0.004
>= once a month					0.30	0.12	**	0.022
>= twice a year					0.42	0.14	***	0.035
>= once a year					0.64	0.14	***	0.066
once a year					0.68	0.18	***	0.074
never					0.74	0.15	***	0.081
Education §§								
Some 2nd level, no exams	-0.15	0.13		-.008	-0.15	0.13		-0.008
Group, Inter. and Junior Cert.	-0.01	0.11		-.001	-0.01	0.11		-0.001
Leaving Cert./Matric	-0.27	0.12	**	-.014	-0.25	0.12	**	-0.013
Diploma from University	-0.35	0.17	**	-.015	-0.34	0.17	**	-0.015
Primary Degree	-0.32	0.20		-.014	-0.33	0.20	*	-0.014
Higher degree	-0.31	0.21		-.014	-0.32	0.22		-0.014
Constant	-3.48	0.78	***		-3.93	0.80	***	
No. obs		15,682				15,682		
Pseudo-R2		0.173				0.181		

*Controls also include regions. Level of significance: *=10%, **=5%, ***=1%.*

§ Omitted variable: >once a week

§§ Omitted variable: no or primary educ.

Table 2: Descriptive Statistics: Married Women

	Living in Ireland Surveys								Household Budget Surveys			
Pre-divorce legalization	1994 & 1995 until 25/11				1994				1994			
Post	1996 - 2001				1999				1999			
	Religious*		Non religious		Religious*		Non religious		Religious**		Non religious	
	pre #	post	pre	post	pre #	post	pre	post	pre ##	post	pre	post
Age	42.8	44.8	38.1	41.3	42.5	45.3	38.0	42.3	41.4	42.2	36.9	38.2
Hours (incl. zeros)	11.7	12.6	13.9	16.4	11.7	12.6	13.9	15.5	9.0	12.9	10.6	15.1
Participation rate (%)	0.35	0.41	0.40	0.51	0.34	0.42	0.39	0.49	0.34	0.47	0.36	0.53
Increase in participation		0.06		0.11		0.08		0.10		0.13		0.17
Crude diff-in-diff				**0.05**				**0.02**				**0.03**
# of Children <18	1.8	1.7	1.7	1.8	1.8	1.7	1.7	1.8	2.2	2.0	2.0	1.7
Pre School Child (%)	0.21	0.16	0.30	0.23	0.22	0.14	0.32	0.19	0.30	0.30	0.43	0.36
Urban (%)	0.44	0.41	0.70	0.68	0.45	0.40	0.71	0.68	0.55	0.38	0.73	0.64
Primary educ. (%)	16.7	13.8	16.8	15.7	16.9	12.7	16.8	18.3	20.1	12.0	24.8	13.0
Lower sec. educ (%)	37.9	36.9	36.4	36.9	38.2	35.9	35.4	38.4	31.7	30.1	31.3	30.3
High sec. educ (%)	34.1	36.3	30.6	29.6	33.6	36.9	30.7	27.8	41.3	45.8	34.8	41.4
University degree (%)	11.2	13.1	16.2	17.9	11.2	14.6	17.1	15.6	6.9	12.1	9.1	15.4
N	2,420	4,757	764	1,876	1,264	597	381	263	2171	1936	1324	1339

** Wife attends church at least once a week*

*** Household gives some positive donation to the church*

Table 3: Difference-in-Difference Estimates: Female labor Supply

Treatment:		1#	1	2	3	1	2	3
Pre	Post		Participation (coefficient)			Participation (marginal effect)		
1994 - 24/11/1995	24/11/1995 - 2001	.07 ***	.37 ***	.20 ***	4.54 ***	.09 ***	.05 ***	1.09 ***
		(0.02)	(0.12)	(0.06)	(1.22)	(0.03)	(0.02)	(0.29)
1994 - 24/11/1995	1997 - 2001	.08 ***	.41 ***	.21 ***	4.62 ***	.10 ***	.05 ***	1.10 ***
		(0.02)	(0.12)	(0.06)	(1.14)	(0.03)	(0.02)	(0.27)
1994 - 24/11/1995	1998 - 2001	.08 ***	.41 ***	.21 ***	4.30 ***	.10 ***	.05 ***	1.03 ***
		(0.03)	(0.12)	(0.07)	(1.10)	(0.03)	(0.02)	(0.26)
1994 - 24/11/1995	1997 - 2001 @	.08 ***	.41 ***	.22 ***	4.95 ***	.10 ***	.05 ***	1.16 ***
		(0.03)	(0.13)	(0.07)	(1.13)	(0.03)	(0.02)	(0.27)
1998 - 99	2000 - 2001	.00	.02	.01	-1.22	.00	.00	-.29
		(0.02)	(0.12)	(0.06)	(1.11)	(0.03)	(0.02)	(0.26)
1998 - 99	2000 - 2001 @	.05	.20	.11	1.60	.05	.03	.40
		(0.03)	(0.16)	(0.09)	(1.13)	(0.04)	(0.02)	(0.28)
Range of R2 for the difference models:				0.14 to 0.18				
			Work hours (incl. zeros)			Work hours (excl. zeros)		
1994 - 24/11/1995	24/11/1995 - 2001		2.62 ***	1.39 ***	25.19 ***	.35	-.10	8.27
			(0.91)	(0.47)	(5.96)	(1.00)	(0.55)	(10.22)
1994 - 24/11/1995	1997 - 2001		2.26 **	1.15 **	25.01 ***	-1.25	-.71	3.26
			(0.89)	(0.46)	(5.92)	(0.92)	(0.52)	(10.45)
1994 - 24/11/1995	1998 - 2001		2.23 **	1.11 **	25.51 ***	-1.47	-.90 *	6.30
			(0.90)	(0.47)	(5.97)	(0.92)	(0.51)	(11.09)
1994 - 24/11/1995	1997 - 2001 @		2.39 **	1.34 ***	26.21 ***	-.85	-.36	.83
			(0.95)	(0.51)	(6.03)	(1.02)	(0.59)	(10.37)
1998 - 99	2000 - 2001		.63	.39	-2.64	1.59	.98	10.55
			(0.94)	(0.50)	(6.84)	(1.20)	(0.67)	(11.50)
1998 - 99	2000 - 2001 @		.98	.55	4.75	-1.37	-.97	-12.98
			(1.18)	(0.61)	(7.82)	(1.33)	(0.74)	(10.53)
Range of R2 for the difference models:				0.15 to 0.17			0.06 to 0.08	

*Standard erros in brackets. Level of significance: *=10%, **=5%, ***=1%. The participation model is estimated by logit, except first column indicated by # with coefficients from a linear probability model*

@ excluding the refreshment sample for 2000-2001

Treatment:

1: religiosity dummy =1 if wife's church attendance is high (at least once a week)

2: continuous religiosity variable based on wife's church attendance

3: continuous risk of marital breakdown

Table 4: D-in-D Estimates with Propensity Score Reweighting: Female labor Supply

		Participation			Hours (incl. zeros)		
Pre	Post	Post x controls interactions	Reweighted	Reweighted & interactions	Post x controls interactions	Reweighted	Reweighted & interactions
1994 - 24/11/1995	24/11/1995 - 2001	.37 ***	.45 ***	.44 ***	2.62 ***	3.88 ***	3.84 ***
		(0.12)	(0.14)	(0.14)	(0.91)	(1.23)	(1.21)
1994 - 24/11/1995	1997 - 2001	.41 ***	.45 ***	.45 ***	2.26 **	2.99 ***	2.93 ***
		(0.12)	(0.13)	(0.13)	(0.89)	(1.15)	(1.14)
1994 - 24/11/1995	1998 - 2001	.41 ***	.48 ***	.48 ***	2.23 **	3.00 ***	2.97 ***
		(0.12)	(0.13)	(0.13)	(0.90)	(1.15)	(1.15)
1994 - 24/11/1995	1997 - 2001 @	.41 ***	.46 ***	.46 ***	2.39 **	3.25 ***	3.20 ***
		(0.13)	(0.14)	(0.14)	(0.95)	(1.26)	(1.23)
1998 - 99	2000 - 2001	.02	.19	.18	.63	.14	.11
		(0.12)	(0.16)	(0.16)	(0.94)	(1.44)	(1.45)
1998 - 99	2000 - 2001 @	.20	.27	.25	.98	1.11	1.08
		(0.16)	(0.17)	(0.17)	(1.18)	(1.47)	(1.45)

*Standard erros in brackets. Level of significance: *=10%, **=5%, ***=1%.*

Treatment = 1 if wife's church attendance is high (at least once a week)

@ excluding the refreshment sample for 2000-2001

Table 5: Fixed-effects Estimates: Female Work Hours (incl. zeros)

		DD, reweighted & interactions	FE	FE, interactions	FE, reweighted	FE, reweighted & interactions
Pre	Post					
1994 - 24/11/1995	24/11/1995 - 2001	3.84 ***	1.35 ***	1.44 ***	1.90 ***	1.97 ***
		(1.21)	(0.41)	(0.42)	(0.57)	(0.58)
1994 - 24/11/1995	1997 - 2001	2.93 ***	1.30 ***	1.35 ***	1.33 **	1.38 **
		(1.14)	(0.43)	(0.44)	(0.63)	(0.64)
1994 - 24/11/1995	1998 - 2001	2.97 ***	1.33 ***	1.42 ***	1.31 *	1.40 **
		(1.15)	(0.47)	(0.48)	(0.67)	(0.69)
1994 - 24/11/1995	1997 - 2001 @	3.20 ***	1.34 ***	1.39 ***	1.36 **	1.41 **
		(1.23)	(0.45)	(0.46)	(0.64)	(0.65)
1998 - 99	2000 - 2001	.11	.94	1.07	.67	.61
		(1.45)	(0.64)	(0.66)	(0.83)	(0.86)
1998 - 99	2000 - 2001 @	1.08	1.04	1.19	1.36	1.41
		(1.45)	(0.70)	(0.72)	(0.88)	(0.91)

*Std. errors in brackets. Level of significance: *=10%, **=5%, ***=1%. DD: difference in difference on pooled data. FE: fixed-effects estimations. Treatment = 1 if wife's church attendance is high (at least once a week).*

@ excluding the refreshment sample for 2000-2001

Table 6: Comparison: Household Budget Survey and Living in Ireland Survey

Margin \ Treatment (Data)	A (HBS)		B (HBS)		C (LII)	
Participation	10.75	*	2.47	*	.18	*
	(5.49)		(1.35)		(0.09)	
Hours (incl. zeros)	75.54	**	17.07	**	1.07	**
	(33.02)		(7.99)		(0.54)	
Hours (excl. zeros)	40.54		11.14		.27	
	(52.31)		(12.70)		(0.84)	

*Pre: year 1994, Post: year 1999. Std. errors in brackets. Level of significance: *=10%, **=5%, ***=1%. Treatment (continuous var. for non- religiosity):*

A: 1 / exp(relative church donation)

B: 1 - (relative church donation)

C: wife's church attendance (scale 1-7, with 1 = very religious)

Note: in HBS, relative donation expressed in % of disposable income (hence measures A and B are in a [0-1] range, with 0= very religious)

Table 7: Additional Results

		Men's Hours (incl. zeros)			Weekly childcare # (FE, reweighting & interaction)	
		DD, reweighting & interaction	FE, interaction	FE, reweighting & interaction	Women	Men
Pre	Post					
1994 - 24/11/1995	24/11/1995 - 2001	1.62	.40	.92	1.54	-.03
		(1.12)	(0.49)	(0.57)	(1.99)	(0.78)
1994 - 24/11/1995	1997 - 2001	2.03 *	.97 *	1.36 **	.81	-.41
		(1.04)	(0.52)	(0.58)	(2.03)	(0.90)
1994 - 24/11/1995	1998 - 2001	2.14 **	.86	1.30 **	.66	-1.53
		(1.06)	(0.57)	(0.60)	(2.21)	(0.94)
1994 - 24/11/1995	1997 - 2001 @	1.62	.83	1.28 **	1.00	-.46
		(1.10)	(0.55)	(0.60)	(2.06)	(0.91)
1998 - 99	2000 - 2001	2.40 *	2.27 ***	1.95 **	2.80	.33
		(1.29)	(0.74)	(0.82)	(2.81)	(1.99)
1998 - 99	2000 - 2001 @	1.87	2.07 **	1.74 *	1.00	-.46
		(1.40)	(0.83)	(0.90)	(2.95)	(2.17)

*Std. errors in brackets. Level of significance: *=10%, **=5%, ***=1%. DD: difference in difference on pooled data. FE: fixed-effects estimations. Treatment = 1 if church attendance is high (at least once a week). # = estimation on households with children only.*

@ excluding the refreshment sample for 2000-2001