Employment protection and fertility: Evidence from the 1990 Italian reform

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Abstract

The aim of this paper is to investigate the short-term effects of Employment Protection Legislation (EPL) on fertility decisions of Italian working women using administrative data. We exploit a quasi-experimental setup to study the main hypothesis that increased EPL reduces future job insecurity feelings and positively affects a female worker's proneness to take childbearing decisions. We use linear and semi-parametric difference in difference (DID) models to control for possible sorting effects related to time-invariant unobservables and instrumental variable model (IV-DID) to account for sorting effects related to time-varying unobservables. We find that reduced economic insecurity following a strengthening of the EPL regime positively affects the propensity to childbearing for Italian working women. Disaggregating the effect according to individual observed characteristics we show that the reform benefited to a significantly greater extent the childbearing propensity of low-earnings women.

Keywords: Fertility; Employment protection; Difference-in-difference; Instrumental variables; Policy evaluations.

JEL Classification Numbers: J2, J13, J65.

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1 Introduction

Most European countries started to experience a decline in fertility levels since the late sixties. While North European nations had started to invert the downward trend in fertility by the early eighties, in Italy the decline in fertility has not stopped yet (Del Boca and Locatelli, 2006). Nowadays Italy is one of the countries in Europe experiencing the lowest fertility with 1.29 children per women lagging well behind the replacement level of 2.1 (Rosina, 2004). Persistent low fertility is likely to have a considerable negative effect on future labor supply, pension system and public health sustainability. McDonald and Kippen (2001) estimate that in Italy and Germany the labor force might reduce by 11 million units should the fertility levels of the late nineties persist in the next 50 years. Low fertility leads to future labor supply shortages of young skilled workers who are important for maintaining international economic competitiveness. Claims are made that as much as 80% of new technology becomes obsolete in ten years while 80% of workers have obtained their qualifications more than 10 years ago (McDonald, 2006). Moreover, by 1997 almost 45% of total social expenditures in the EU-15 area went to pensions for the elderly. In Italy the share of pensions to the elderly amounts to 79% of social expenditures and to 16% of GDP putting government budgets under severe stress and pointing to the economic non-sustainability of the mechanism of intergenerational transfers (De Santis and Livi Bacci, 2001; Del Boca et al., 2004). Besides, immigration is only a temporary and imperfect remedy for low fertility and integration has shown not to be an easy process (De Santis and Livi Bacci, 2001). For instance, the National Identity Survey carried out by the International Social Survey Programme in 2003 confirmed the existence of strong negative stereotypes that immigrants increase crime and reduce the number of jobs left for natives (Bianchi et al., 2008; Venturini, 2002).

All these issues point to the need for Italy and many developed countries to try to influence the age structure of their populations. As psychological and sociological determinants of fertility related to the prevailing set of values in the society can change only in the very long run, the economic determinants of fertility arise as the most suitable instrument to influence it.

Fertility decisions and female labor market behaviour have been studied jointly since the appearance of the first models of fertility economics (Becker, 1960; Barro and Becker, 1986). Several cross country studies investigate empirically the economic models based on the joint determination of fertility decisions and labor market participation. They highlight the role played by welfare state support (subsidized childcare, maternity leaves and child fiscal benefits) in reconciling women's role as mothers and workers and in putting an end to the fertility decline in North European countries (Del Boca et al., 2004).

Other studies focusing on the economic determinants of fertility have tried to explain the fertility decline in developed countries and the increase in the age at first child. The usual predictions state that increases in income raise the opportunity cost of time devoted to children in terms of foregone wages and career advancement, and hence reduce fertility rates (Becker, 1960; Hotz, 1997). Moreover, women with a higher human capital endowment at marriage and/or at the steeper part of the wage curve also tend to postpone childbearing decisions and eventually make fewer children. (Cigno and Ermisch, 1988). Del Bono et al.(2011) investigates the effects on fertility of career interruptions due to job loss following an exogenous firm closure. They use nonparametric treatment effects methods to show that career interruptions negatively influence fertility via two main channels: reduced probability of reentering the labor market after the maternity episode and a falling behind effect in the wage profile once reentering the labor market. The last channel acts through firm-specific human capital destruction due to the career interruption. Similarly Adsera (2005) exploiting cross country variation in labor market institutions in different OECD nations documents reduced fertility and postponement in the timing of births in countries that experienced acute unemployment. The mechanism at work here is the increase in women's investment in education and skill acquisition in order to minimize the risk of unemployment. Findings include a positive effect of secure public sector jobs based on permanent contracts and of part-time jobs that reduce women's uncertainty about reconciling work and family. She concludes that work and family are compatible only in those countries where institutions reduce the uncertainties connected with childbearing.

Only recently expectations of future labor market outcomes and the level of confidence and uncertainty of young workers have received attention as an important economic determinant of fertility. Ahn and Mira (2001) find that males' past unemployment episodes and temporary contracts are an important determinant of marriage delay and fertility decline in Spain. De La Rica and Iza (2005) focusing on the economic uncertainty-fertility nexus show that Spanish women holding fixed-term contracts and lacking stable employment prospects delay entry into motherhood compared to female workers holding open-ended contracts. Andersson (2000) finds that women in a stable employment and with decent levels of earnings in Sweden have much higher propensities to become mothers compared to women less attached to the labor market. Paihe and Solaz (2009) use similar measures of objective economic insecurity, like precarious fixed-term contracts, past unemployment spells and low current income and show that all have a depressing effect on French women's fertility decisions. Kreyenfeld (2009) and Bhaumik and Nugent (2002) study the impact on fertility of objective economic uncertainty along with subjective uncertainty measured by the feeling that the personal economic situation is insecure, using a panel of German women. While the former finds no empirical evidence for the economic uncertainty-fertility nexus, the latter confirms a negative correlation. The difference in results might be due to the difference in statistical methodologies and time period covered by their data. Finally, the study from Bratti et al. (2005) studies women's employment after the birth of the first child in Italy. They find that Italian women who enjoy a greater amount of employment protection have a higher incentive to return to the labor force in the three years following childbirth compared to female workers in less protected jobs. They conclude that women with highly protected and stable jobs find it easier to combine career and family, while those who are less sheltered by the legislation are more likely to withdraw from the labor market.

None of these studies takes a causal approach at the problem of measuring how conditions of economic insecurity, whether objective or perceived, might impact childbearing decisions of female workers. Moreover, the measures of objective economic insecurity that have been considered before include temporary contracts, unemployment history and current income. To the best of our knowledge no study has looked into the effects of employment protection as a measure of objective economic insecurity on fertility. The goal of this study is to investigate the impact on fertility decisions of

Italian female workers of a reduction in economic insecurity due to the strengthening of employment protection legislation (EPL henceforth). We exploit a reform in 1990 that extended employment protection to small firm workers who had been exempt up to that point. By changing the EPL regime of small firm workers and leaving unaltered protection for large firm workers the reform creates a quasi-experimental setup.

Economic insecurity has multiple determinants and it is a multidimensional concept itself, involving above all the perceived probability of losing the current job in the near future (Anderson and Pontusson, 2007). More generally it refers to the stability and continuity of one's employment in a given organization (Bernardi et al. 2008.) The connection between economic insecurity and EPL is made clear by Anderson and Pontusson (2007) in a cross-country study of the OECD area which finds that government legislation aimed at restricting the ability of employers to fire workers is a major determinant of the subjective economic insecurity (perceived probability of losing the job) of individuals.

Our study contributes to this strand of literature in two ways. First, we have a unique opportunity to study the relationship between economic insecurity and fertility decisions in a causal setup with representative individual data of the Italian labor market. We explicitly address the question of potential treatment endogeneity stemming from deliberate self-selection of workers into one of the EPL regimes created by the reform. We extensively discuss the identifying assumptions of our estimators and are able to isolate a distinct causal pathway from employment protection to childbearing decisions. Initially we use a linear difference-in-difference estimator which can account for endogeneity of the treatment status under the assumption of time-invariant unobserved heterogeneity. We then estimate a semi-parametric difference in difference model proposed by Abadie (2005) which improves on some of the shortcomings of the linear DID but maintains its fundamental identifying assumption. Finally we address the issue of possible correlation of the treatment status with time-varying unobserved heterogeneity via instrumental variables. Our results show that reducing job insecurity by mean of increasing EPL generates a positive effect on fertility.

Secondly, we investigate how the effects of economic uncertainty vary with observed heterogeneity in our sample of workers and find that the reduced insecurity due to the reform benefites to a greater extent low income workers.

The rest of the paper is organized as follows. Section 2 presents a brief picture of the institutional aspects of the Italian labor market that brought to the 1990 reform. Section 3 deals with the formal economic model relating economic insecurity to fertility decisions. In section 4 the econometric methods are discussed in depth while in section 5 we describe the data used in this study. Section 6 presents results and section 7 concludes.

2 Institutional background

Employment protection is primarily enforced to provide security and reduce income uncertainty by enhancing job and income stability (Barone, 2001). The Italian labor market has always been one of the strictest in Europe in terms of EPL and reached the peak of its rigidity by the late seventies

(Bertola, 1990). At the time, the position of Italy in the continuous rigidity-flexibility spectrum was determined by the relative bargaining power of the two main players when it comes to labor market regulation, namely labor unions and employers' association. Following widespread social unrest and intense strike activity in the late sixties the workers' unions could rely on strong popular support. In this context, the first EPL reform was introduced in Italy in 1966 with the law 604. This law established that the firm should reintegrate the worker or pay different amounts of firing costs depending on tenure and firm size should a Labor Tribunal recognize the dismissal as unfair or unjustified. The pro-working class climate culminated in the adoption of the Charter of workers' rights in 1970 that represents the most serious attempt to transversely regulate a large number of aspects ranging from hiring and firing procedures to minimum wages and workplace safety. Article 18 of this law established that firms with more than 15 employees had to face a higher cost of unfair dismissal as they were obliged to reintegrate the worker and compensate her for the forgone wages during the period going from dismissal to the sentence of the judge.[1] Nonetheless, firms with less then 15 employees were not mentioned by the Charter and were completely exempted from the employment protection regime for the following twenty years.

In May 1990 the law No.108 extended dismissal restrictions also to those workers holding an open-ended contract and employed in firms with less than 15 employees.[2] In particular, this law introduced costs for dismissals unmotivated by a 'fair cause' or a 'justified reason' in firms with less then 15 employees, while unfair dismissal costs in firms with more then 15 employees remained unchanged. In particular, if a Labor Tribunal recognizes the episode as unjustified dismissal, under the new regime a small firm has to choose whether to pay the worker between 2.5 and 6 months of salary as punitive damages or reintegrate her.

The introduction of this law creates a quasi-experimental context and naturally suggests a difference in difference approach for the identification and estimation of the above effect, by exploiting variation in time and among firms of the EPL regime. At the heart of the identification strategy lies the assumption that unobserved characteristics of workers working in large and small firms are time invariant and general macroeconomic time-specific shocks have identical effects on both groups of workers.

When by the early eighties the general social and political climate had started to change, workers' unions started to loose drift and legislative changes meant to increase liberalization and flexibility in the labor market were passed. In particular law 56/1987 liberalized the use of fixed term contracts that up to that time had been restricted to very special circumstances. The 1990 EPL reform applies only to workers holding open ended contracts. To avoid interference with this policy change that goes in the direction of reducing employment protection and to isolate the effects of a strengthening of the EPL regime on fertility, we focus on permanent workers and exclude from our sample temporary,

[1]Bertola and Ichino (1995) offer a detailed review of Italian labor market regulations and reforms up to the early 90-s.

[2]Article 19 of the Charter establishes another difference between firms on both sides of the 15 employees threshold. Workers employed in large firms are given the right to create and adhere to firm level unions in addition to the national workers' unions. Unfortunately, we do not have information in our data set on union membership. This should not be relevant as agreements reached at a national level by unions apply to all workers regardless of their membership to country level or firm level organizations.

seasonal and apprentice workers holding fixed-term contracts introduced by law 56/1987.[3] Moreover, in order to avoid capturing false policy effects, we looked at other policy changes occurring in the years surrounding the 1990 reform that might have had an effect on fertility. We are aware of two of these, namely immigration laws 943/1986 and 39/1990 which formally allowed family reunions and most importantly programed inflows of non-EU citizens to fulfill local labor demand. None of these reforms was intended to have differentiated effects on small and large firms. Nonetheless, they might be a threat to our identification strategy if working immigrants, who are known to strongly contribute to fertility, engage in different fertility behaviors according to firm size. To shed light on this hypothesis, we perform a robustness check on the subsample that excludes immigrants. A final legislative change approved by the Government in 1991 (law 223) is worth mentioning as it might put at risk our identification strategy. This norm was aimed at loosening the EPL regime and applied to firms with more than 15 employees by allowing collective dismissals of more than 5 workers. In particular, law 223/1991 "explicitly recognized that external flexibility is justifiable on economic grounds, and offered limited opportunities for trade unions to try and avoid employment reductions" (Bertola and Ichino, 1995, p. 388). While the 1990 reform reduced the gap of EPL provisions by pulling up protection for small firms, the 1991 law further narrows the gap by pushing down protection for workers in large firms. Should the two reforms interfere with each other than what we would be capturing are the joint effects of both an incremental change in the protection of small firm workers and a decremental change in the protection of large firm workers, with the latter reinforcing the former, i.e. we would observe a greater reduction of the fertility gap between large and small firms than what we would have observed without the 1991 reform. In order to investigate whether the effects of the 1990 reform were influenced in any way by the 1991 law a triple difference would be in place. By interacting the 1990 reform dummy with a dummy for large firms laying off more than five workers one could isolate differences in fertility between firms with more than 15 employees laying off more and less than five employees after 1990. Unluckily, this strategy is not feasible with the data at hand because they do not provide the reason a given worker leaves a firm, whether she chooses to leave or is fired. Therefore we have to interpret our results with this caveat in mind.

3 The theoretical framework

In this section we provide the theoretical framework underpinning our research hypothesis that we test empirically in the following sections. The model is due to Ranjan (1999) and it establishes that increases in uncertainty about future income make agents postpone childbearing decision.[4/5] Children here are seen as irreversible investments generating both benefits and costs in terms of direct out-of-pocket expenditures and opportunity-costs of time taken away from work and leisure.

[3]The presence of temporary workers however does not threaten our identification strategy as the law that liberalized their use was applied to all firms regardless of size.

[4]A similar model connecting fertility decisions with uninsurable earnings' risk is due to Vetechova (2008).

[5]We take economic uncertainty to mean risk of having a lower wage or even loosing the job. In our case this risk is referred to the working mother's labor income instead of household income as we do not observe the income of the spouse.

The agent lives only two periods and is risk averse ($U^{''}(\cdot) < 0$). She knows her first period income I, while the second period income could be either $I + \delta(\cdot)$ (good state) or $I - \delta(\cdot)$ (bad state) with probability 1/2.[6] The crucial variable of the model measuring uncertainty is the mean preserving spread of future labor income $\delta(\cdot)$ that we assume to be a function of the EPL regime which the worker is subject to. Workers employed in small firms after the reform were exposed to an increase in employment protection and we expect them to experience a reduction in the mean preserving spread of future income.

The agent decides at the beginning of period 1 whether to have a child in the first period or wait until the second period for the uncertainty surrounding income to be resolved and then take the childbearing decision. To make the waiting option valuable the model assumes that once the woman decides to postpone childbearing she will have a child in period two only in the good state of the world. She decides to postpone the childbearing decision if the expected lifetime utility of the waiting option ($F(I)$) is greater than the lifetime utility she would derive from having a baby immediately ($V(I)$).[7] The decision problem for the agent is $Max\,\{F(I), V(I)\}$. For the econometrician the observed choice is $y = \mathbf{1}\,[F(I) < V(I)]$. Depending on parameter values entering the utility functions, $F(I)$ can be greater or smaller than $V(I)$. However, we are interested in how their difference behaves when uncertainty about future income ($\delta(\cdot)$) changes. With regard to this, the main prediction of the model is

$$\frac{\partial(F(I) - V(I))}{\partial\delta(\cdot)} > 0 \tag{1}$$

for any diminishing marginal utility function ($U^{''}(\cdot) < 0$). Equation (1) implies that the higher the uncertainty the higher the relative attractiveness of the postponement option. The intuition behind this is that the greater the uncertainty, the greater the likelihood the individual will end up with a baby in a bad state of the world in the second period.[8]

Although the main focus of this model is the relationship between economic insecurity and fertility decisions, testable predictions are also made concerning the effects of income on fertility. In this model, the sign of the association between the two will depend on the relative importance of the two components of total costs generated by the decision to have a child, namely direct out-of-pocket monetary expenditures and opportunity costs of time dedicated to childrearing in terms of forgone wages. When wage rises these two components of cost move in opposite directions. A rise in the

[6] According to the magnitude of $\delta(\cdot)$ the model can allow for both reductions in labor income (small $\delta(\cdot)$) or job loss ($\delta(\cdot)$=I). The case with discrete probabilities is chosen for ease of exposition. The conclusions of the model remain valid if any continuous distribution is assumed for δ.

[7] General analytical expressions for the two utilities are given by
$V(I) = U(I - \gamma) + \frac{1}{2}\beta\,[U(I + \delta(\cdot) - \gamma) + U(I - \delta(\cdot) - \gamma)] + (1 + \beta)\phi$ and
$F(I) = U(I) + \frac{1}{2}\beta\,[U(I + \delta(\cdot) - \gamma) + U(I - \delta(\cdot))] + \frac{1}{2}\phi$
where γ is the per-period expenditure on the child, ϕ is the per-period benefit from the child and β is the discount rate. $U(\cdot)$ is any diminishing marginal utility function s.t $U^{''}(\cdot) < 0$. The difference between the two alternative utilities is $[U(I) - U(I - \gamma) - \phi] + \frac{1}{2}\beta\,[U(I - \delta(\cdot)) - U(I - \delta(\cdot) - \gamma) - \phi]$ where the first expression in brackets gives the utility loss of the women due to postponement and the second one gives the utility gain from the postponement.

[8] Formally, the reason for (1) is that the utility loss from postponing childbearing does not depend on δ, while the utility gain from postponement is increasing with uncertainty ($\delta(\cdot)$) as can be seen in the previous footnote.

wage makes the time devoted to the child more expensive. On the other hand, the higher labor income the lower the marginal utility of consumption and the lower the utility cost of direct monetary expenditures. We will investigate empirically the net effect of these two forces in section 6.[9]

In this model economic uncertainty has always a direct negative effect on fertility and an indirect threshold effect the sign of which depends on the relationship between income and fertility. The direct effect is given by (1). As for the indirect threshold effect we start by noticing that in case of a positive relationship between income and fertility women prefer having babies immediately as income increases and the postponement option becomes less valuable with further increases in income. Be that as it may, women will prefer postponing childbearing in case of negative effect of income on fertility. The model predicts that there exists some threshold level of income I^* for which the agent is just indifferent between postponing or not $(F(I^*) = V(I^*))$. Given a positive effect of income on fertility, women above the threshold will prefer having a child immediately. Should increases in income negatively influence fertility then women above the threshold will prefer postponing childbearing. The model also predicts that the threshold level of income I^* rises with uncertainty about future income $(\frac{\partial I^*}{\partial \delta} > 0)$ producing the above mentioned indirect effects of uncertainty on fertility. Hence a reduction in economic uncertainty while producing a direct and positive influence on fertility it also pushes down the income threshold and increases the number of individuals that find themselves above this threshold who, as a result, will make less children in case of a negative relationship between income and fertility. To sum up, the indirect threshold effects mitigate the direct effects of uncertainty on fertility if there is a negative association between income and fertility and amplify them if the sign of the association is positive.

The testable implication of this model is that the increased employment protection in small firms on the one side contributed to lowering future labor income uncertainty $(\delta(\cdot))$ for working mothers and on the other side it pushed down the income threshold. The former effect makes these women go for more children immediately instead of postponing childbearing. The latter effect increases the number of individuals who place themselves above the threshold. In case of a negative relation between income and fertility the net effect captured will depend on the interplay of the opposing effects of reduced insecurity and lowered threshold.

4 Econometric method

We are interested in estimating the effects of an increase in employment protection on the female workers' proneness to maternity. The 1990 reform provides a quasi-experimental framework because it increased the EPL in small-firm workers while maintaining employment protection unaltered in large firms. This creates variation of employment protection in time and among firms thus allowing us to identify the desired effect. Exploiting this quasi-experimental framework we can estimate the average treatment effect on the treated $ATT(X) = E(Y_1 - Y_0|X, D = 1)$ by using a difference-in-

[9]Traditional neoclassical economic models of fertility assert a negative relationship between income and fertility stemming from both the quality-quantity interaction and from the fact that opportunity cost of time devoted to children goes up when labor income increases (Hotz et al., 1997).

difference approach.[10] In our case the ATT parameter measures the effect of the EPL reform on proneness to maternity in individuals that actually experienced the reform relative to what would have happened had they not been exposed to it. We start with a linear DID, and afterwards we relax some of its restrictive assumptions by applying a semiparametric DID estimator and correcting for possible treatment endogeneity by IV.

Under the following assumption,

$$(A.1) \quad E(u_{00}|x, D=0) - E(u_{00}|x, D=1) = E(u_{01}|x, D=0) - E(u_{01}|x, D=1)$$

it can be shown that the ATT is identified by the DID estimator given by,

$$DID(x) = E[Y_{11}|x, D=1] - E[Y_{00}|x, D=1] - \{E[Y_{01}|x, D=0] - E[Y_{00}|x, D=0]\} \qquad (2)$$

where Y_{0t} denotes the potential maternity status at time $t = \{0, 1\}$ of an individual had she not been exposed to the reform (j=0) regardless of whether she did actually benefit from the reform or not, while Y_{1t} denotes potential maternity status of those women who benefit from the EPL reform. D is a dummy equal to 1 if the worker is subject to the reform and 0 otherwise (Heckman et al., 1998; Heckman and Smith, 1999).[11] Assumption A.1 states that the average unobservables for the treated and controls would have followed parallel trends in absence of the treatment.[12] This can also be interpreted in terms of time invariance of the selection bias. Indeed, the LHS of A.1 gives the selection bias for the ATT(x) in T=0, say $B_0(x)$, while the RHS represents the selection bias for the ATT(x) in T=1, call it $B_1(x)$ (Heckman et al. 2006). In other terms, the DID estimator allows for selection bias both before and after the treatment as long as they are of the same magnitude and cancel out ($B_0(x) = B_1(x)$).[13]

[10]Given the causal model of the potential outcome equations

$$Y_1 = \mu_1(X) + u_1 \qquad Y_0 = \mu_0(X) + u_0$$

and the assumption $E[u_0|X] = E[u_1|X] = 0$, the average treatment effect is defined as $ATT(X) = E(Y_1 - Y_0|X, D = 1) = \mu_1(X) - \mu_0(X) + E[u_1 - u_0|X, D = 1]$, which varies with x. The unconditional $ATT = E(Y_1 - Y_0|, D = 1)$ is obtained by integrating over the distribution of X for the treated. A simpler but more restrictive case is the *homogeneous and constant* treatment effect one, in which the difference between potential outcomes in the two states is constant and common to all $Y_1 - Y_0 = \alpha$. In this case individual treatment effects are assumed to be the same even among people with different x-s.

[11]PROOF:Adding and subtracting $E(Y_{01}|x, D = 1) - E(Y_{00}|x, D = 1)$ in (2) we get

$$\begin{aligned} DID(x) &= E[Y_{11}|x, D=1] - E[Y_{00}|x, D=1] - \{E[Y_{01}|x, D=0] - E[Y_{00}|x, D=0]\} \\ &+ \{E(Y_{01}|x, D=1) - E(Y_{00}|x, D=1)\} - \{E(Y_{01}|x, D=1) - E(Y_{00}|x, D=1)\} \end{aligned}$$

After some algebra it can be shown that
$DID(x) = ATT(x) + \underbrace{E(u_{01}|x, D = 1) - E(u_{00}|x, D = 1) - E(u_{01}|x, D = 0) + E(u_{00}|x, D = 0)}_{Bias}$ which gives $DID(x) = ATT(x)$ if (A.1) holds and the bias term is null.

[12]In graphical terms, we assume that in absence of the reform the small firms' fertility rate in Figure 1 would have faced a counterfactual downward trend too.

[13]Lee and Kang (2006) focusing specifically on the issue of what DID identifies, impose a stronger identifying assumption of zero selection bias in both pre- and post-treatment periods $B_0(x) = B_1(x) = 0$ which is sufficient but not necessary. They achieve the same result but at a higher price.

It is possible to show that the DID estimator can be obtained using the following linear probability model (Abadie, 2005).[14]

$$Y_{it} = m + \mathbf{x}'_{it}\beta + \tau D_i + \delta T_t + \alpha D_i \times T_t + \epsilon_{it} \tag{3}$$

Here Y_{it} and D_i are the observed maternity and treatment status respectively, while T_t is a dummy equal to 1 for the post reform period. The coefficient τ captures the effect on childbearing decisions of working in a small firm instead of a large firm irrespective of the time period when this takes place, i.e it represents permanent differences between the treated and controls. The coefficient δ captures the effect on Y of the simple passing of time, i.e. general changes in the economic and social context. The interaction coefficient α identifies exactly the constant ATT under A.1.[15] We control for observed characteristics by adding linearly the k vector $\mathbf{x}_{it}$ (Meyers, 1995).
In general, the coefficients in (3) can be estimated by OLS.[16] We assume throughout that T and X are exogenous, hence consistent estimation of (3) hinges on D being uncorrelated with ϵ. This condition is less restrictive than, and is implied by $E[u_{it}|x, D] = 0 \ \forall t$ which in turn is equivalent to $B_1(X) = B_0(X) = 0$ (Abadie 2005).

Specification (3) implies a *constant* and common to all homogeneous treatment effect $Y_{1i} - Y_{0i} = \alpha$, which might be inappropriate if the effect is different for different subgroups of the population defined by x. We allow for heterogeneous treatment effects that vary with covariates $\alpha(\mathbf{x}_k)$ in the LPM (3) by allowing interactions of D_iT_t with some of the covariates in $\mathbf{x}$. We do this by estimating the following specification of our LPM

$$Y_{it} = m + \mathbf{x}'_{it}\beta + \tau D_i + \delta T_t + \alpha_1 D_i T_t + \alpha_2 D_i \mathbf{x}_{kit} + \alpha_3 T_t \mathbf{x}_{kit} + \alpha_4 D_i T_t \mathbf{x}_{kit} + \epsilon_{it} \tag{4}$$

where $\mathbf{x}_{kit} \subseteq \mathbf{x}_{it}$ and α_4 captures how the treatment effects differ among groups of the population defined by $\mathbf{x}_k$.

[14]Our dependent variable is a dummy equal to 1 if a maternity episode is registered during the year and 0 otherwise. Nonetheless, we chose a Linear Probability Model instead of a probit as almost all our regressors are categorical and the model is almost saturated. A fully saturated LPM does not suffer from out of range predicted probabilities. We also correct standard errors for heteroschedasticity.

[15]The interpretation of the coefficients is made clearer in the following derivation of the linear DID due to Abadie (2005). Assume that the observed outcome is generated by a component of variance process as in $Y_{it} = \alpha D_{it} + v_{it} = \alpha D_{it} + \delta(t) + \eta_i + u_{it}$ where $E[u_{it}] = 0 \ \forall t$, $\delta(t)$ is a time specific component and η_i is an individual specific time-invariant component. With only two time periods before (0) and after (1), T_t is a dummy equal to 1 for the post reform period, and $D_i = 1$ if the workers is exposed to the treatment, than $D_{it} = 1_{[D_i=1,T_t=1]} = D_i T_t$. After some algebra one has

$$Y_{it} = m + \underbrace{E[\eta_i|D_i = 1, T = 1] - E[\eta_i|D_i = 0, T = 1]}_{\tau = \, permanent\ differences} D_i + \underbrace{(\delta(1) - \delta(0))}_{\delta = \, baseline\ time\ effect} T_t + \alpha D_{it} + \epsilon_{it}$$

where $m = \delta(0) + E[\eta_i|D_i = 0, T = 1]$ is an intercept and $\epsilon_{it} = \eta_i - E[\eta_i|D_i, T = 1] + u_{it}$.

[16]Ignoring within unit serial correlation might bias the estimated standard errors downwards, although coefficient estimates remain unbiased. Clustering on the individuals solves the problem.

4.1 Linear DID pitfalls and possible remedies

As we stated above the necessary and sufficient identifying assumption for the DID is time-invariance of selection bias $B_t(X)$ i.e. $B_1(X) = B_0(X)$. The bias in each period $t = \{0, 1\}$ can be broken down to three deeper sources $B_{1t}(X), B_{2t}(X), B_{3t}(X)$ (Heckman et al. 1998).[17] The first source of bias $B_{1t}(X)$ is due to the existence of non overlapping regions of the support of the pdf of the propensity score for the treated and controls.[18] In these regions subjects belonging to one group do not find their counterpart in the other group. The second $B_{2t}(X)$ is due to the **x**-s being distributed differently among treated and controls within the region of common support of the propensity score. The last source of bias $B_{3t}(X)$ can be attributed to unobservables, and is made up of differences in outcomes that remain even after controlling for observable differences that are at the base of the first two sources.

The linear DID in (3) (or in (4)) does not make any attempt to control the first two sources of bias. It does not impose a common support condition, and changes in the outcomes of the treated are compared with changes in the untreated outcomes of individuals that might have different distributions of covariates. Instead, it rests on the assumption that the algebraic sum of these three components in the before and after periods cancels out.

Abadie (2005) proposes a semiparametric DID estimator that improves on the first two sources of bias, while still relying on the assumption of time-invariance for the third component. This estimator is based on assumption (A.1) and the common support assumption

$$(A.2) \quad P(D = 1|X) < 1 \; with \; probability \; 1, \; and \; P(D = 1) > 0$$

which requires that no value of X should perfectly predict the participation to treatment. Under assumptions (A.1) and (A.2) the semiparametric DID identifies the unconditional treatment effect on the treated ATT,

$$E[Y_{11} - Y_{01}|D = 1] = E_M \left[\frac{P(D = 1|X)}{P(D = 1)} \, \omega \, Y \right] \tag{5}$$

where Y_{jt} is defined as before. $\omega = \frac{T-\lambda}{\lambda(1-\lambda)} \frac{D-P(D=1|X)}{P(D=1|X)P(D=0|X)}$ is a weighting factor, and $\lambda = \frac{n_1}{n_0+n_1} \in (0, 1)$ is the proportion of observations that are observed in the post-treatment period T=1.[19]

Assumption (A.2) takes care of the B_1 component of bias by excluding individuals in non-

[17] The three components of total selection bias computed by Heckman et al. (1998) are
$B_1 = \int_{S_{1X}/S_X} E[Y_0|X, D = 1]dF(X|D = 1) - \int_{S_{0X}/S_X} E[Y_0|X, D = 0]dF(X|D = 0)$
$B_2 = \int_{S_X} E[Y_0|X, D = 0]d(F(X|D = 1) - F(X|D = 0))$
$B_3 = P_X \overline{B}_{S_X}$
where S_{1X} is the support of X for D=1, S_{0X} is the support of X for D=0, $S_X = S_{1x} \cap S_{0x}$ is the region of overlap, $P_X = \int_{S_X} dF(X|D = 1)$ is the portion of the density of X given D=1 in the overlap region S_X and $\overline{B}_{S_X} = \frac{\int_{S_X} B(X)dF(X|D=1)}{\int_{S_X} dF(X|D=1)}$ is termed mean selection bias.

[18] The propensity score is usually defined as the probability of selection into treatment for a given subject, namely $P(D = 1|X)$.

[19] Equation (5) is an identification result. Estimation of the ATT(X) or the ATT is based on least squares approximations as the following result due to Abadie (2005) shows that:
$\beta_0 = argmin_{\beta \in B} = E_M[P(D = 1|X) \{\omega Y - g(\mathbf{x}_k, \beta)\}^2]$

overlapping regions of the PS support. Unlike ordinary regression in (4) here we don't extrapolate outside the range of data points observed in both groups.

The weighting factor ω addresses the B_2 bias due to the covariates being unequally distributed among treated and controls. The corrective reweighting is carried out by weighting up or down the distribution of Y among the untreated depending on whether covariates are under- or over-represented in that group. We will show in our descriptive analysis that half of the X-s are unequally distributed in the two groups, which is one of the reasons that motivated our recourse to this estimator.[20] In practice, outcome values corresponding to those observations for which covariates are overrepresented $((F(X|D=1) < F(X|D=0))$ will be down-weighted by ω, that in this case will have low $\frac{P(D=1|X)}{P(D=0|X)}$. On the contrary, when the disequilibrium has a negative sign $(F(X|D=1)) > F(X|D=0)$ the corresponding Y will be weighted up by a high value of $\frac{P(D=1|X)}{P(D=0|X)}$ in ω. By so doing, the same distribution of the covariates is imposed for treated and controls and non parallel dynamics in average outcomes possibly due to differences in observed characteristics but unrelated to the reform are avoided. In addition, differencing purges the estimates of the effects of time-invariant unobserved heterogeneity potentially correlated to D.

Although the semiparametric DID eliminates by construction the first two sources of bias, it still relies on (A.1) i.e. time invariance of the third component of bias due to unobservables ($B_{30} = B_{31}$). Treatment endogeneity due to correlation of D with time-varying unobserved heterogeneity in (4) would still cause (A.1) to break down. In our case endogeneity of D could be an issue, since other studies (Leonardi and Pica, 2010) focusing on the 1990 EPL reform give evidence of workers' self-selection into a particular EPL regime. In other words time-varying unobservables influencing fertility decisions might be correlated with the unobservables determining firm choice as would be the case if risk averse women more prone to childbearing selected themselves into the better protected firms both before and after the reform. These two circumstances, namely the *ex ante* self-selection of workers in small firms in anticipation of the reform, and their switching to small firms after, and due to, the reform cause changes in groups' composition. As for the former, we can safely assume there was no sorting of workers in small firms in anticipation of the reform. The first public news about the reform appeared in January 1990. Our results are not affected by this type of sorting as long as workers refrain from sorting in advance until March 1990, as also assumed by Kugler and Pica (2008). In this reasonable scenario all maternity decisions observed in 1990 do not suffer from anticipative sorting effects. The second of these circumstances could still cause total sorting bias $B_t(x)$ or only its third component $B_{3t}(x)$ to vary over time. Assumption (A.1) would break down and the ATT estimates produced by the OLS-DID and the SDID would be both biased. Applying instrumental variables in (4) is one way of handling endogeneity and hence restoring the validity of (A.1).

The main issue with IV in our case is finding a variable (exclusion restriction) that shows a strong enough asymptotic correlation with the endogenous policy dummy, and is uncorrelated with the unobservables influencing fertility decisions. We argue that a potential candidate for the exclusion

[20]Since exposure to treatment is not randomized we did expect both observed and unobserved characteristics to be unequally distributed in the two groups. Both circumstances are bound to give rise to pre-treatment non parallel dynamics of the outcomes in the two groups.

restriction is the proportion of small firms in each of the 95 (in 1990) Italian provinces (z). We show this variable to be a strong determinant of D, i.e. of the fact whether a woman works in a small or large firm. However, neither theory nor previous empirical studies provide guidance on whether the percentage of small firms at the provincial level is systematically correlated with unobserved determinants of maternity proneness. Nonetheless, there is little reason to believe that unobserved determinants of fertility vary systematically with the proportion of small firms in the province. Moreover, we do control for province of work and region of birth of the worker which account for all territorial factors influencing childbearing decisions and the choice of firm.[21] Variation in our exclusion restriction is at the provincial level unlike the rest of the variables that vary at the individual level. We take that circumstance into account and correct standard errors by allowing for two-way clustering when performing IV estimation which accommodates correlation at both the individual and the provincial level. We use the propensity score $P(D = 1|R)$ as an instrument, where R contains all the X in the outcome equation plus the exclusion restriction z. This generated instruments 2SLS method is robust to misspecifications of the chosen parametric model (e.g probit) for the propensity score, which is why we prefer it to ordinary 2SLS. Moreover, one doesn't have to correct standard errors for generated regressors as in a control function approach (Wooldridge 2002). Any regressor that is interacted with D_i has to be instrumented as well.

5 Data and descriptive evidence

We use administrative data from the Italian Social Security Institute (INPS). This dataset covers a 1:90 random sample of all employees working in the private sector in Italy. The dataset is an employer-employee unbalanced panel, that contains information on both individual and firm characteristics.[22] The data include information on individual characteristics such as sex and age of the worker, the initial and the final date of each employment spell, the total gross earnings accumulated over the period, the total number of days and weeks worked, an indicator for part-time status, an occupational qualification code and most importantly it records maternity episodes. Our dependent variable is a dummy equal to 1 if a maternity episode was registered during the year.

We also have a set of variables related to firm characteristics, such as the industry in which it operates, the geographic location, the average number of employees, and the initial and in some cases also the final year of activity.

Because of its administrative nature, the main drawback of the dataset is that there is very little demographic information on individual characteristics; in particular we do not know the level of education, the marital status and spouse's income.[23]

[21] We assume there is no sorting on unobserved gains, and responses to treatment are homogeneous conditional on the X. Only sorting on the level (i.e correlation of D and u) that can be solved by IV is allowed. In this setup IV identifies always the ATT=ATE, regardless of the particular instrument being used (Heckman et al. 2006).

[22] Only private sector workers are recorded. People who at some point leave the dataset do so because of one of the following causes: they move to the public sector, to self-employment, to the agricultural sector, to black market jobs, unemployment and retirement.

[23] We construct a proxy for education based on the individual's age at entry in the dataset.

We select all women aged between 18 and 46 in 1990 and 1992. The choice of this time window is motivated by the fact that the reform took place in May 1990. We assume the workers did not anticipate the reform passed in May 1990, therefore all fertility episodes registered in 1990 can be safely attributed to the pre-treatment period. As to the exact timing of birth, unfortunately the data set contains information only on the year when the fertility episode took place. Hence, the 1991 year was excluded from the sample as children born in that year were conceived both before and after the reform. Fertility episodes registered in 1992 are instead assigned to the post-treatment period. With a one-year window only short-term effects on fertility of the EPL reform can be investigated. Finally, when faced with multiple spells of the same worker during the year we keep the longest one.[24]

As to the selection of firms, we keep in our sample all firms with a yearly average number of employees greater than 5 and less than 50, hence forming an asymmetric window around the threshold value of 15. This enhances the comparability of treatment and control groups, as firms near the cut-off value should be similar in their fundamentals.[25] As a result, we define small all those firms that employ an average number of employees between 5 and 15 units both before and after the reform. Consequently, firms crossing the 15 employee threshold either upwards or downwards are excluded from our sample.

At the moment when the EPL reform took place, in Italy pregnant workers were entitled to a mandatory maternity leave of five months (usually two before delivery and three after it), during which pregnant workers were paid 80% of the original salary, according to law 1204/1971. All female workers were eligible for maternity leave, irrespective of the sector or the tenure they have matured in a given firm. The maternity protection legislation, also prohibits firms to lay off pregnant women, unless there is a 'fair cause' of dismissal. Hence, maternity protection legislation operates at a subordinate level compared to the EPL regime.

Figure 1 shows the fertility rate in small and large firms before and after the 1990 reform. We observe that before 1990 outcomes in both groups follow almost parallel paths, and after that they enter into non parallel trends.[26] In particular, between the end of 1990 and the end of 1992, fertility shows a downward trend in large firms, while it shows a slight increase in small firms.[27]

Tables 1 and 2 present raw means by firm size in the pre- and post-treatment period, for the dependent and independent variables respectively. Table 1 shows that fertility decreased by -0.0011

[24]As we mention in section 2, we refrain from using a wider time window as other changes of the labor market regulations took place shortly after 1990, whose effects could interfere in unpredictable ways with the EPL reform of 1990.

[25]Firms with at most 50 employees make up the economically homogeneous group of Small and Medium Enterprises (SME), which motivates our choice of the upper bound of the [5,50] window. Firms with less than 5 employees were excluded because they are usually family run. Following Kugler and Pica (2008) we perform robustness checks with another alternative window. We consider the [5,35] window which restricts the control group to those working in firms with more than 15 and less than 35 employees. Results are presented in section 6.2.

[26]As fertility levels before the reform were higher in the better protected large firms (see fig. 1), we expect a narrowing of the fertility gap between large and small firms following the narrowing of the protection gap between the two.

[27]Although we report the trend also for 1991, our post-treatment period is year 1992 only. As mentioned before, maternity episodes registered in 1991 refer to childbearing decisions taken both before and after the reform and are therefore excluded from the empirical analysis.

probability points from 1990 to 1992 in large firms. While fertility in small firms went up by 0.0011 units in the same period. The difference in the differences amounts to a significant 0.0122 increase in the probability of having a child in small firms compared to large firms after the reform. Overall these simple double differences of raw means suggest that, following the EPL reform, fertility in small firms increased compared to fertility in large firms. However, causal interpretation may be impaired by other confounders (workers' and firms' characteristics) that potentially influence fertility.

Table 2 presents descriptive statistics on individuals' and firms' characteristics by firms size, before and after the 1990 reform, and their differences. If exposure to treatment was randomized the means of all regressors would be equal in the two groups. However, as can be seen from columns 3 and 6, half of the differences in means between the the treated and controls in Table 2 are statistically significant. Since the workers are different in terms of their observable characteristics and choose which firm to work for, and hence the EPL regime, we need to control for covariates in the regression analysis in order to shed light on the causal relationship between EPL and fertility, as shown in the following sections.

6 Results

We now expand the descriptive analysis of section 5 to include regressors, which improve efficiency of estimation and offer interesting interpretations of competing economic and socio-cultural models of fertility behaviour. Table 3 reports marginal effects of the OLS-DID specified in equation (3) (columns 1-2) and (4) (column 3) with different sets of covariates. The first column displays estimation results for the case with only the treatment dummy, the time dummy and the interaction of the two. When no other regressors are included, the coefficient of the interaction term coincides with the raw double difference of the outcome presented in table 1. The regression estimates confirm a positive constant average treatment effect of the reform that amounts to 0.0122 probability points.

The second column presents results for the specification of equation (3) that adds the full set of conditioning variables containing information on women's and firms' characteristics. The estimated ATT provides evidence in support of the theoretical framework presented in section 3 on the economic insecurity-fertility nexus. Net of the effect of all other variables, the effect of the reform remains a significant 0.0137 evaluated at the mean of all other observables. This is the average treatment effect on the treated, that under the assumptions of equation (3) is constant and common to all individuals, regardless of their observed and unobserved characteristics. The result is in line with other findings of a negative correlation of perceived job instability and fertility (Kreyenfeld, 2005; Bernardi et al., 2008). We interpret this result also in the light of previous research that has studied the effects of the same reform on job accessions and separations. The suggested channel through which the increased EPL might exercise its influence on fertility decisions of working women is by reducing job and income insecurity. In fact, the study of Kugler and Pica (2008) suggests that the probability of loosing the job decreased by as much as 15% in small firms after the reform. Another possible channel transmitting the EPL effects through to fertility decisions might be due to wages. Leonardi and Pica (2010) provide empirical evidence that the 1990 reform produced a reduction in wages for small firm workers in line with theoretical predictions according to which firms discount higher future firing costs and transfer them to workers through tenure-wide lower wages. If we consider this jointly with our finding of a negative relationship between income and fertility it might suggest that the lower wages after the reform might have produced part of the positive effect on fertility that we capture.

Moving to the effects of the other covariates, we obtain a significant negative effect (-0.0417) of income on fertility. In terms of percentage impact this coefficient implies that a one percent increase in women's own labor income reduces her probability of having a child by as much as 2%. This confirms predictions of traditional models of fertility economics according to which a higher wage increases the cost of childbearing by increasing the opportunity cost of time dedicated to such activity in terms of foregone wages (Hotz et al. 1997). In terms of our model, it also highlights the prevalence of the opportunity cost component of the cost of children compared to the direct monetary component. The economic model of section 3 predicts that, on one hand, a reduction in economic insecurity stimulates immediate fertility decisions and on the other hand, it also pushes down the income threshold above which people find convenient to postpone fertility decisions. Hence, the former effect outweighs the latter and a net positive effect of reduced economic uncertainty on fertility emerges. We also find that highly educated women exhibit lower probability (-0.0058) of being observed in a maternity episode compared to a woman of middle education. Our data seem to indicate an inverse U-shaped relation between fertility and education of female workers, although only the coefficient of the low education dummy (-0.0129) is significantly different from zero. This is consistent with a human capital interpretation according to which women with a higher endowment of human capital tend to postpone childbearing (Cigno and Ermisch, 1988). The positive coefficient of tenure shows that women who are higher up in their career ladder have higher chances of taking a childbearing decision compared to otherwise equivalent less senior female colleagues. A one year increase in tenure increases the probability of having a child by a significant 0.0136 points. An interesting result is the one on the part-time work indicator. Theory predicts that the possibility to work part-time helps women to reconcile work and family commitments and lighten the double burden of the mother-worker (Del Boca et al. 2004; Adsera, 2005). Our estimates are inconclusive on this point as we find a negative but statistically insignificant effect of the part-time indicator in all specifications. A possible explanation for this could be that part time work was not very widespread at the time under analysis. Del Boca et al. (2004) using the European Commission Household Panel finds the same negative relationship for Italy. Furthermore, being a bluecollar worker reduces the probability of having a child by as much as -0.0168 probability points compared to white collar workers with the same average observed characteristics.

In order to investigate the hypothesis of geographically differentiated fertility behaviour, the twenty Italian quasi-federal regions are aggregated into three macro-regions, namely North, Center (excluded category) and South&Islands. Differently from other papers on the topic that study childbearing decisions of the entire Italian female population (Del Boca et al. 2004; Kertzer et al. 2007), here we restrict our focus to Italian female workers. Results show that the probability of having a child for female workers employed in the North is 0.0242 probability points higher compared to an observationally equivalent women working in central Italy. Furthermore, working in the South results in a reduced probability (-0.0083) of having a child compared to otherwise equivalent women who work in the Center. This could be due to an inadequate supply of child care facilities and local labor market characteristics in the South (Del Boca et al., 2004, Del Boca and Vuri, 2007). As a result, reconciling work and family life might be comparatively harder in southern Italy. Indeed, Kertzer et al. (2007) using similar cohorts as ours find that living in southern Italy and being employed are associated with declined propensity to marry and a longer waiting time to the first child. Both circumstances are known to contribute to a reduction in fertility rates. The macro-sector within which the firm operates also has some predictive power over a worker's decision to have a child. In fact, being employed in the financial and in the services sector (commercial, health, education, transport etc.) negatively affects fertility decisions in a significant way compared to the condition of being employed in the industrial sector.

The reaction to economic insecurity of individuals can be influenced and mediated by some shared characteristics and held values stemming from working environment, social class or the regional cultural context in general (Bernardi et al. 2008). For this reason we check whether the reform heterogeneously affected different subgroups of the population sharing certain (observed) characteristics $\mathbf{x}_k$. This is done by including third level interactions of D_iT_t with other regressors $\mathbf{x}_k$ as in equation 4. We choose income as the most relevant socioeconomic indicator among the available regressors to check for heterogeneous effects. Estimates of the ATT as a function of income are shown in column 3 of Table 4. We split continuous income into two categories, namely low and medium-high income individuals placing themselves respectively below or above the EURO 12,150 threshold.[28] The increase in employment protection benefited low income workers by a significant 0.0192 probability points more than otherwise equivalent medium-high income colleagues. This is in line with a human capital interpretation according to which unskilled workers are more likely to suffer economic insecurity. Those who benefit most are the workers with less human capital endowment and reduced probabilities of finding an equivalent job in the event of dismissal.

6.1 Selection issues

As we saw in section 4.1, failure of assumption (A.1) leads the OLS-DID to produce an estimated effect α of the reform that comprises the true effect plus the sorting bias. The SDID and the IV-DID are an attempt to purge the estimated effect from the sorting bias by addressing different possible drawbacks of the OLS-DID. The former corrects for non overlapping regions of the PS and pretreatment differences in observables although still relying on (A.1) in order to difference away time constant unobservables possibly correlated with D. However, nonrandom sorting of workers into small and large firms based on time-varying unobservables remains a potential source of bias. This issue can be addressed by the use of IV.

In this section we discuss the results obtained with the SDID and IV-DID estimators. Table 4 presents the results for the semi-parametric DID estimator. Care must be taken in side by side comparison of these estimates with those obtained with the linear DID. In fact, the estimate produced by the SDID is a direct linear least square approximation of the homogeneous (ATT) or heterogeneous

[28]This is the mean yearly wage of all workers in the years 1990-1992.

(ATT($\mathbf{x}_k$)) treatment effect depending on whether we choose to approximate these parameter by a constant or some observed characteristics, while the OLS-DID estimate of the ATT is the regression coefficient of the outcome of interest on D_{it} and the full set of observed characteristics. The constant ATT produced by the SDID, which corresponds to the interaction coefficient in a linear DID model that conditions on the full set of covariates but without third level interactions (column 2 in Table 3), is obtained by setting $\mathbf{x}_k$ equal to a constant (column of ones).[29] The linear least square approximation of this parameter yields a significant 0.0123 constant average treatment effect which is virtually equal in magnitude to the corresponding OLS-DID estimates in column 2 of table 3. Pre-treatment distributional differences between workers employed in small and large firms and non-overlapping supports of the corresponding pdf-s of the propensity score apparently did not influence the fertility dynamics in these two groups. The semi-parametric DID allows us also to investigate the hypothesis of heterogeneous response of fertility decisions to the reform by computing how the $ATT(\mathbf{x}_k)$ varies with one or more observed characteristics. Coherently with the specification of the linear DID in column 3 of table 3, we estimate linear least squares approximations of the $ATT(\mathbf{x}_k)$ as a function of income categories by setting $\mathbf{x}_k$ equal to this regressor. Results are shown in the second row of table 4. The SDID confirms what we found with the linear DID, i.e. the effect of the reform benefited comparatively more low-income workers by a significant 0.0172 probability points compared to the more well-off workers.

As mentioned above, in order to address nonrandom sorting of workers into small and large firms based on time-varying unobservables we apply instrumental variable estimation. We employ the percentage of small firms in the province as an exclusion restriction. This approach has been used by Newhouse and Beegle (2006) and Neal (1997) to study the effects of catholic school attendance on academic achievement. They use the local proportion of Catholic schools and of Catholic adherents as instruments for school choice. As to the direction of the selection bias, on one hand we expect unobserved proneness to having a child to be negatively correlated with economic insecurity (D) for risk averse individuals, and on the other to positively influence the decision to have a child. In this scenario omitted variable bias would be negative, and the OLS-DID would underestimate the effects of the reform (Angrist and Pischke, 2008). Indeed, as shown in table 5, the effects of the IV-DID are systematically higher than the OLS-DID and SDID estimates regardless of the specification of the conditioning set of regressors. Results in columns (1) of panel A in table 5 corresponding to the specifications with no regressors show that the effects of the reform become stronger when correcting for selection on (time-varying) unobservables although they are imprecisely estimated. Results for the specification that includes all regressors are presented in column 2. These coefficients confirm what we found in the OLS-DID estimates regarding the effects of income, tenure, age and all other regressors that are similar in magnitude to those obtained with the OLS-DID estimator. The effect of the reform in this specification is higher in magnitude (0.0532) than the one obtained in the corresponding specification of the OLS-DID although significant only at a 10% level of confidence, confirming a positive effect of reduced economic uncertainty on fertility. Column 3 confirms the OLS-DID result that the reform had a stronger positive impact on low income workers (0.0398) although the coefficient is not

[29]See footnote 19 for the estimating equation of the SDID.

statistically significant.

For the IV identification strategy to be valid it is necessary that our instrument is strongly correlated with firm choice but uncorrelated with ϵ, the shock in equation (4).[30] To gain insight into the empirical validity of using the proportion of small firms at the provincial level as an exclusion restriction we present in panel B of table 5 test statistics for the first stage for each of the three specifications of regressors. These statistics should reassure us on the consequences of having weak instruments. If the instrument is only weakly correlated with firm choice (D) then even the slightest correlation between the instrument and the residual of equation (4) might induce large inconsistency in the IV estimate possibly exceeding the inconsistency of the corresponding OLS-DID estimate (Wooldridge, 2003). Moreover, finite sample bias of the IV estimate might be unduly inflated if the correlation between the instrument and the choice of firm (D) nears zero. The first four rows of panel B in table 5 show F-statistics for the first stage regression of the endogenous regressors in each of the three specifications. The overall F-statistic of each regression is way above the critical level of 10 regardless of the specification (Staiger and Stock, 1997). We report in the fifth row of panel B the p-values of a Chi square test of under-identification, which is a test of the rank condition for IV identification. We always reject the null of reduced rank of the coefficients' matrix of excluded instruments in the first stage regression of all endogenous covariates on all instruments (included and excluded) (Wooldridge, 2002). The last but one row in panel B reports the HAC version of the Craig-Donald F statistic. The value of the F-statistic is always high enough to safely reject the null of weak instruments.[31] Table 6 shows the results for the first stage regressions of the endogenous variables in each specification on the excluded instruments and other regressors. The estimates in this table show that our instrument is a strong predictor of D thus reassuring us on the relevance of the chosen instrument. Overall we can be fairly confident that inconsistency and finite sample bias of the IV estimator are not an important concern in our case.

In summary, all estimates support the hypothesis that the reform caused an increase in the probability of having a child. If we compare the three estimators in pairs interesting conjectures can be drawn in relation to the bias of the estimated ATT. The pair of the OLS-DID and the S-DID estimators produced very similar coefficient estimates pointing to the marginal role played by the first two components of bias, which the SDID eliminates. Second, a simple comparison of the OLS and IV estimates leads us to conclude that omitted variable bias has a negative sign and that OLS underestimate the true effects of the reform. Both circumstances are the result of unobserved proneness to maternity being naturally positively correlated to fertility (Y) but negatively correlated to economic insecurity (D). Finally, comparing the SDID to the IV and under the assumption that the IV identifying conditions are met, it is tempting to infer that total selection bias or its third component are time-varying and are not differenced away in a DID approach. However, the weak significance of the IV estimates imposes caution in interpreting the results.

[30] We performed Hausman-like tests of endogeneity for the supposedly endogenous regressors and were able to refute the null hypothesis of consistent OLS estimates.

[31] When the Stock-Yogo critical values were not available (specifications 4) the 'rule of thumb' of Staiger and Stock was used that the F statistic should be at least 10 for the weak identification to be ignored.

6.2 Sensitivity analysis

Finally we perform several robustness checks. In the first one we restrict estimation to Italian workers, leaving out of the sample all immigrants, to avoid interference of other reforms as mentioned in section 2. The point estimates of the effects of the reform shown in table 7 change little regardless of the estimator and of the specification, except for the IV estimates of the heterogeneous ATT that turns out to be insignificant. We also re-estimate our models with a differently sized window around the 15 employee threshold in table 8. The magnitude and significance of the coefficients remain virtually unaltered when the window defining the treatment and control groups is changed to [5-15:16-35]. Again the effects of the reform remain qualitatively similar to those obtained with the [5-15,16-50] window used in the study, except for the IV estimate of the heterogeneous ATT which turns negative though not significantly different from zero.

7 Conclusions

This paper exploits a natural experiment to estimate the impacts on Italian working women's fertility decisions of an increase in employment protection. Despite intense public debate on both employment protection provisions and worring low fertility levels in Italy and other South European countries, to date we know little about how the propensity to childbearing of working women is affected by the level of protection of their job positions. To the best of our knowledge this paper is the first to bridge the gap between these two phenomena and to isolate a distinct causal pathway from EPL to fertility decisions.

We use individual level administrative data from the Work Histories Italian Panel (WHIP) covering the period 1990-1992. We exploit a reform occurred in 1990 in Italy which increased job protection for small firm workers while leaving the EPL regime of large firms unaltered, thus creating variation in EPL both across firms and in time. This allows us to use a difference-in-difference identification strategy. We confront results from three empirical models, a baseline linear DID, a semi-parametric DID and an instrumental variables approach which allow for increasing degrees of treatment endogeneity. All of these estimators produce a sizable effect of the increased EPL on fertility. We argue that the channel through which the increased EPL exercises its influence on fertility decisions of working women is by reducing job and income insecurity. Not surprisingly, we capture more pronounced effects of the reform on low income workers.

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Table 1: Fertility rates by timing of the reform and firm size

	Large firm	Small firm	DID
Pre-treatment (1990)	0.0555	0.0465	
Post-treatment (1992)	0.0444	0.0474	
Diff	-0.0111*	0.0011	0.0122*

NOTES Symbols: *** significant at 1%; **significant at 5%, *significant at 10%. The pre-treatment period refers to year 1990, and the post-reform period to year 1992. The sample contains female workers aged between 18 and 46 working in firms with more than 5 and less than 50 employees.

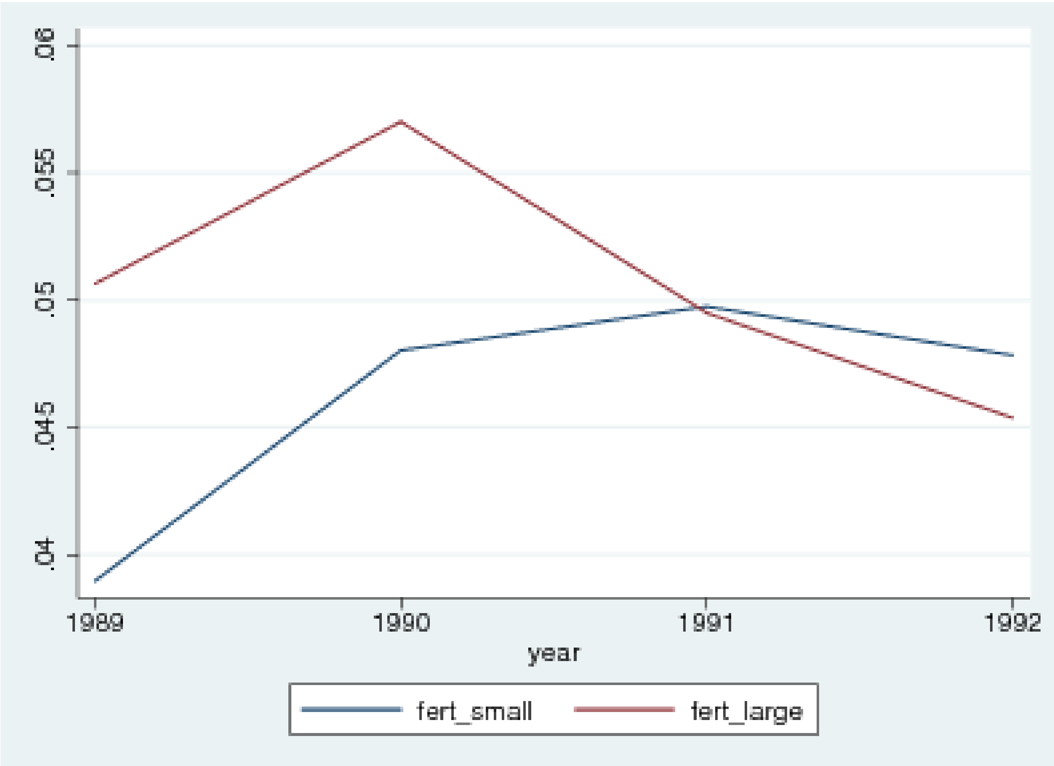

Figure 1: Fertility trends by firm size

Table 2: Descriptive statistics by firm sizes and timing of the reform

	Pre-treatment			Post-treatment		
Variables	Large firms	Small firms	Diff	Large firm	Small firms	Diff
Income	8230.4200	7382.1900	848.2300 * **	9470.9600	8573.5900	897.3600***
Age	30.4097	29.6070	0.8027 * **	30.6791	29.8580	0.8210***
Entry age	26.4911	25.8801	0.6110 * **	27.2343	26.7928	0.4423***
Tenure	43.0109	39.0861	3.9247 * **	48.8786	43.9095	4.9690***
Ptime	0.0728	0.1155	−0.0426 * **	0.0933	0.1473	−0.0539***
Bluecollar	0.6315	0.5691	0.0623 * **	0.6228	0.5500	0.0728***
Low edu	0.1179	0.1283	−0.0104	0.0850	0.0778	0.0071
Med edu	0.2484	0.2806	−0.0321 * **	0.2310	0.2566	−0.0255***
High edu	0.6336	0.5910	0.0426 * **	0.6838	0.6655	0.0183
Young firm	0.2914	0.3565	−0.0651 * **	0.2493	0.3107	−0.0614***
Med firm	0.3860	0.3884	−0.0024	0.4230	0.4295	−0.0064
Old firm	0.3224	0.2549	0.0675 * **	0.3276	0.2597	0.0679***
Ethnic	0.0394	0.0341	0.0052	0.0462	0.0410	0.0052
Born north	0.5556	0.5631	−0.0075	0.5523	0.5768	−0.0245
Born center	0.1735	0.1694	0.0040	0.1788	0.1660	0.0128
Born south	0.2305	0.2325	−0.0019	0.2203	0.2149	0.0054
Work north	0.6491	0.6505	−0.0013	0.6453	0.6561	−0.0108***
Work center	0.1961	0.1942	0.0019	0.2028	0.1882	0.0146
Work south	0.1544	0.1550	−0.0006	0.1517	0.1555	−0.0037
Mine&Fish	0.0012	0.0020	−0.0008	0.0015	0.0017	−0.0002
Industry	0.6745	0.5230	0.1515 * **	0.6515	0.5004	0.1510***
Finance	0.1023	0.1285	−0.0261 * **	0.1098	0.1472	−0.0373***
Services	0.2217	0.3463	−0.1245 * **	0.2373	0.3505	−0.1134***

NOTES Symbols: *** significant at 1%; **significant at 5%, *significant at 10%. The pre-treatment period refers to year 1990, and the post-reform period to year 1992. The sample contains female workers aged between 18 and 46 working in firms with more than 5 and less than 50 employees. Young firms are those created less than 5 years before 1990. Old firms are those created more than 15 years before 1990. Ethnic equals 1 if the worker was born abroad. Low edu, Med edu, High edu are proxy dummies for education built on the basis of the age the worker is observed for the first time in the dataset (Entry age).

Table 3: Effects of EPL on fertility: OLS-DID

	.(1)	.(2)	.(3)
T	−0.0112***	−0.0117***	−0.0116†
D	−0.0912**†	−0.0100 * *	−0.0069†
T x D *(ATT)*	0.0122**	0.0137 * *	−0.0436†
Log-income		−0.0417***	
Part-time		−0.0719†	−0.0423†
Ethnic		−0.0895†	0.0836†
Born south		0.0074†	0.0332†
Born center		0.0650†	0.0459†
Tenure		0.0136***	0.0103***
Age		0.0243***	0.0257***
Age_squared		−0.0041***†	−0.0004***
Bluecollar		−0.0168***	−0.0190***
Low edu		−0.0129 * *	−0.0177***
High edu		−0.0582†	−0.0583†
Young_firm		0.0682*†	0.0580†
Medium_firm		0.0225†	0.0002†
Work_north		0.0242***	0.0219***
Work_south		−0.0832†	−0.0826†
Mine&Fish		−0.0237	−0.0215
Financial		−0.0163***	−0.0835*†
Services		−0.0152***	−0.0702**†
Lowincome			0.0789***
Lowincome x T			−0.0584†
Lowincome x D			−0.0121
Lowincome x DT *ATT(income)*			0.0192 * *
Constant	0.0556***	−0.0357	−0.405 * **
N	21679		

NOTES Symbols * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. † Coefficient is multiplied x10. T is a dummy equal to 1 if the worker is observed after the reform. D is a dummy equal to 1 if the worker works in a small firm. Lowincome is dummy equal to 1 if the worker yearly income was less than EUR 12.150. Young firms are those created less than 5 years before 1990. Old firms are those created more than 15 years before 1990. Ethnic equals 1 if the worker was born abroad. Low edu, Med edu, High edu are proxy dummies for education built on the basis of the age the worker is observed for the first time in the dataset (Entry age). The first column shows OLS estimates of marginal effects of equation (3) when only D, T and their interaction are included in the RHS. The second column adds the full set of covariates in equation (3). The third column shows OLS estimates of equation (4). Huber-White heteroskedasticity robust standard errors are computed.

Table 4: Effects of EPL on fertility: SDID

A. HOMOGENEOUS EFFECTS	
ATT	0.0123***
B. HETEROGENEOUS EFFECTS ATT(X_k)	
ATT(low income)	0.0172***
N	23227

NOTES Symbols * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effects of EPL on fertility: IV-DID

A. Reduced forms	.(1)	.(2)	.(3)
T		$-0.0329**$	$-0.0898\dagger$
D	$-0.2700***$	$-0.1698***$	$-0.0887**$
T x D *(ATT)*	0.0719	$0.0532*$	0.0227
Log-income		$-0.0441***$	
Part-time		$0.0605\dagger$	$0.0667\dagger$
Ethnic		$-0.0219**$	$-0.0257\dagger$
Born south		$0.0002\dagger$	$0.0266\dagger$
Born center		$0.0350\dagger$	$0.0086\dagger$
Tenure		$0.0133***$	$0.0995***\dagger$
Age		$0.0249***$	$0.0261***$
Age_squared		$-0.0004***\dagger$	$-0.0004***\dagger$
Bluecollar		$-0.0227***$	$-0.0289***$
Low edu		$-0.0108*$	$-0.0428\dagger$
High edu		-0.0100	$-0.0227***$
Young_firm		$0.0210***$	$0.0174**$
Medium_firm		0.0107	$0.0647\dagger$
Work_north		$0.0239***$	$0.0209***$
Work_south		$-0.0578\dagger$	-0.0104
Mine&Fish		$-0.0174\dagger$	$-0.0459\dagger$
Financial		$-0.0374\dagger$	$0.0225\dagger$
Services		$0.0606\dagger$	$0.0116*$
Lowincome			$0.1357***$
Lowincome-x-T			-0.0237
Lowincome-x-D			$-0.0847***$
Lowincome-x-DT *ATT(income)*			0.0398
Constant		0.1322	$-0.379***$
B. First stage statistics			
D	38.62	38.00	18.49
DT	25.85	204.72	79.76
Lowincome x D			113.45
Lowincome x DT			77.49
Under-id (χ^2 p-value)	0.00	0.00	0.00
Weak-id (F-stat)$\star$	71.34	62.91	33.18
N	21679		

NOTES Symbols * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. † Coefficient is multiplied x10. The specifications in columns 1-3 are the same as those of the corresponding columns in table 3. Refere to the note of table 3. The first four rows of panel B, one for each endogenous variable, show F-statistics for the first stage regression. The fifth row of panel B reports the p-values of a Chi square test of under-identification, which is a test of the rank condition for IV identification. $\star$ Heteroschedasticity-robust version of the test statistic. Huber-White heteroskedasticity robust standard errors are computed.

Table 6: First stage regressions

	$.D$	$.DT$	$.D{*}Lowinc$	$.DT{*}Lowinc$
Specification 1				
$\hat{P}(D=1\|x,z)$	$1.06***$	0		
$\hat{P}(D=1\|x,z)$T	-0.12	$0.94***$		
Specification 2				
$\hat{P}(D=1\|x,z)$	$1.04***$	0.02		
$\hat{P}(D=1\|x,z)$T	-0.01	$1.01***$		
Specification 3				
$\hat{P}(D=1\|x,z)$	$1.42***$	0.03	$0.21*$	$0.12**$
$\hat{P}(D=1\|x,z)$T	-0.07	$1.28***$	-0.02	$-0.02**$
$\hat{P}(D=1\|x,z)$xlowinc	$-0.42***$	-0.02	$0.90***$	$-0.04***$
$\hat{P}(D=1\|x,z)$Txlowinc	0.05	$-0.34**$	-0.01	$0.95***$
N	21679			

NOTES Symbols * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Specification 1 corresponds to equation (3) when only D, T and DT are included but no other regressors (column 1 of table 5). Specification 2 has the full set of conditioning variables of column 2 in table 5. Specification 3 correspond to the last column in table 5. z is the exclusion restriction and $\hat{P}(\cdot)$ the estimated propesnity score. Huber-White heteroskedasticity robust standard errors are computed.

Table 7: Robustness checks (no foreigners)

	OLS-DID	S-DID	IV-DID
A.Homogeneous effects			
ATT	0.0136**	0.0127***	0.0469*
B.Heterogeneous effects ATT(X_k)			
ATT(low income)	0.0202**	0.0175***	0.0374
N	20814		

NOTES Symbols * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All three estimators are computed on a different subsample. Refere to the notes in table 3, 4 and 5 respectively for details on their specification. Huber-White heteroskedasticity robust standard errors are computed.

Table 8: Robustness checks ([5-15:16-35] window)

	OLS-DID	S-DID	IV-DID
A.Homogeneous effects			
ATT	0.0143**	0.0126***	0.0596*
B.Heterogeneous effects ATT(X_k)			
ATT(low income)	0.0186*	0.0173***	-0.0038
N	19245		

NOTES Symbols * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All three estimators are computed on a different subsample. Refere to the notes in table 3, 4 and 5 respectively for details on their specification. Huber-White heteroskedasticity robust standard errors are computed.