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WORKING PAPER NO. 596

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January 2021

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ISSN: 2240-9696

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Maria De Paola*, Roberto Nisticò† and Vincenzo Scoppa‡

Abstract
We study the effect of a reduction in employment protection on fertility decisions. Using data from the Italian Labor Force Survey for the years 2013-2018, we analyze how the propensity to have a child has been affected by the 2015 Labor Market Reform, the so-called "Jobs Act", which has essentially reduced the employment protection for large-firm employees and leaved largely unchanged that for small-firm ones. We employ a Difference-in-Differences identification strategy and compare the change over time in fertility decisions of women employed in large firms with the change experienced by women employed in small firms. We find that women exposed to the reduction in employment protection have a 1.4 percentage points lower probability of having a child than unexposed women. A battery of robustness checks confirms this finding. We document large heterogeneous effects by age, marital status, parity, geographic areas as well as by the level of education and wage. Our findings help understand the potential unintended consequences that labor market reforms introducing greater flexibility have on fertility decisions by increasing insecurity on career prospects.

Keywords: Fertility, Employment Protection Legislation, Labor Market Reform, Difference-in-Differences.

JEL Classification: J13, J65, J41, M51, C31.

Acknowledgements: We would like to thank the Editor, Ghazala Azmat, and two anonymous referees for very useful comments. We also thank Giuseppe Bertola, Marco Bertoni, Giorgio Brunello, Daniela Del Boca, Maria Laura Di Tommaso, Tommaso Oliviero, Lia Pacelli, Lorenzo Pandolfi, Giovanni Pica, Lorenzo Rocco, Annalisa Scognamiglio, Giovanni Sulis, Roberto Torrini, Daniela Vuri as well as conference and seminar participants at the 2020 Conference of the Italian Association of Labor Economists (17-18 September), the 2019 meeting of the Italian Economic Association (Palermo, 24-26 October), the 2019 Padova Applied Economics Workshop, the Department of Economics and Statistics, "Cognetti De Martiis", Turin, for useful comments and suggestions. Roberto Nisticò acknowledges funding from the EIEF Research Grant. The usual disclaimer applies.

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1. Introduction

Fertility rates have become a matter of concern in many advanced countries. The current total fertility rate is below the population replacement rate of 2.1 in almost all OECD countries.[1] This is the result of a long-term decline occurring since the 1970s.[2] After reaching a low peak in the early 2000s (values had fallen below 1.3 in many European countries), some countries have experienced a recovery – though the increase has generally been moderate – while most Southern European countries are still plagued by very low fertility rates (values in 2016 are still below 1.4 for Greece, Italy, Portugal and Spain).

One of the causes of the observed decline in total fertility rates has been a tendency by women to postpone their decisions to have children until a later age. The mean age of women at first childbirth has increased dramatically in most OECD countries: from 24.1 years in 1970 to 30 years in 2017.[3] The postponement of childbearing affects completed fertility because of the limited time interval left for second or higher order births. In addition, also because of health-related problems associated to age, delaying entry into motherhood can also lead to involuntary childlessness (Beaujouan and Berghammer, 2019; te Velde et al., 2012).

The reduction of fertility rates and the postponement of fertility might also be related to increased market insecurity (Goldstein et al., 2013; Kreyenfeld and Andersson, 2014; McDonald, 2006; Sobotka et al., 2011). Contemporary labor markets are often characterized by employment instability that intensifies difficulties experienced by the young in their transition to adulthood. Since individuals are typically risk-averse, an increase in the uncertainty about future economic conditions might push them to defer family formation until full integration into the labor market or to decrease the number of children in order to reduce risks (Ranjan, 1999).

The empirical evidence on how economic uncertainty affects fertility is still very limited. Some studies have analyzed the impact of unemployment[4] and temporary contracts[5] on fertility, while a small

[1] The total fertility rate in each year is defined as the mean number of children that would be born alive to a woman during her lifetime if she were to pass through her childbearing years conforming to the age-specific fertility rates of a given year.

[2] The economic literature has long investigated the factors that determine fertility and how they change across countries and over time: the effect of the division of work within households (Del Boca et al., 2003; Ichino and Sanz de Galdeano, 2003), of the availability of childcare services (Chiuri, 2000; Del Boca, 2002; Marenzi and Pagani, 2008), of cultural variables (Friedlander et al. 1991; Hacker and Roberts, 2017; Kohler, 2000).

[3] In Italy this figure has reached 32 years in 2018.

[4] Studies looking at aggregate unemployment typically show a negative association with fertility (Adsera, 2005; Adsera, 2010; Adsera and Menendez, 2011; Currie and Schwandt, 2014; Inanc, 2015; Meron and Widmer 2002). However, they do not provide evidence of a causal effect, which is instead the aim of a number of works looking at individual unemployment and exploiting firm closure as a source of exogenous variation in unemployment. They document a strong negative effect which seems to be mainly related to the career shock rather than to the income shock induced by unemployment (Del Bono et al., 2015, 2012; Huttunen and Kellokumpu, 2016; Lindo, 2010). De Paola, Nisticò and Scoppa (2019) show that improved career prospects increase fertility among Italian university professors.

[5] While most of these papers find that fixed term contracts delay entry in motherhood and reduce fertility (Ahn and Mira, 2001; De La Rica and Iza, 2005; Modena et al., 2013; Modena and Sabatini, 2012; Vignoli et al., 2012), Santarelli (2011) finds no effect for the type of employment contract. Nevertheless, also in these studies the identification of causal effects is hampered by endogeneity and reverse causality issues. For instance, women planning to have a child in the near future might be more likely to accept a temporary job as they seek less demanding jobs and careers. On the other hand, it could also be that women who plan to start a family might seek more stable careers and job security.

literature has focused on the impact of Employment Protection Legislation (EPL) – see e.g., Adsera (2004), Bratti et al. (2005), Fahlen and Olah (2018) and Prifti and Vuri (2013).

Strictly linked to the latter works, this paper addresses how job instability affects fertility decisions by examining an exogenous change in EPL. We exploit the Italian "Jobs Act" Reform of 2015, which has substantially reduced firing costs for new hires with open-ended contract in large firms (with more than 15 employees) by phasing out the compulsory reinstatement of wrongfully discharged employees and mandating that firms have to compensate unfair dismissals exclusively by disbursing an amount of money predetermined by law and proportional to job tenure. The Reform has essentially reduced employment protection for large-firm employees hired on a permanent contract basis after March 7th, 2015, while it has left largely unchanged employment protection for small-firm employees, for whom the reinstatement clause did not exist. The fact that the Jobs Act has mainly increased flexibility in large firms has been documented by Boeri and Garibaldi (2019), who also show that right after the reform total firings from open ended contracts increased by more than 50 percent in large firms while remaining relatively stable in small ones.

This unique feature of the Reform allows us to employ a Difference-in-Differences approach and estimate the causal effect of EPL on fertility exploiting the structure of our data and comparing the difference in the likelihood to have a child between women who have been affected by the reform – new hires with open-ended contract in large firms – and women employed in large firms and hired before the reform who have not been affected by it; this difference is then compared with the analogous difference in fertility between women newly hired in small firms and women hired in small firms before the reform. A similar estimation strategy has been previously adopted by Prifti and Vuri (2013), who have analyzed the impact of an Italian reform that in 1990 has increased employment protection for firms with less than 15 employees and has left it unchanged for larger firms.

In our empirical analysis we use data from the Italian Labor Force Survey that provides quarterly information for the period 2013-2018 on a large sample of the Italian population. Our results show that a reduction in job security significantly lowers a women's propensity to have a child. More specifically, we document that women hired in large firms after the approval of the "Jobs Act" face a 1.4 percentage points lower probability of having a child compared to the change experienced by women employed in small firms. This translates into a decrease of the probability of having a child by 38% of the mean.[6] The estimated effect is larger for younger women, for unmarried ones, and for those with no kids, consistent with the prediction that younger women, by facing lower time pressure to make a family, have greater incentives to postpone childbearing in the interest of pursuing a professional career. Moreover, we find the effect to be larger for women working in the South of Italy, for women with lower education, and for women earning lower wages, therefore suggesting that the impact of job insecurity is likely mediated by income uncertainty and, more generally, by the expectations on future career prospects.[7]

[6] In terms of standard deviation, this corresponds to a decrease in the average probability of having a child by 7 percent of one standard deviation.

[7] The economic literature has given great attention to the fertility effects of improved job opportunities that drive more women into employment and lead to an increase in income. On the one side, better employment opportunities, by

It should be noted that, besides the abolition of the reinstatement clause for large firms, the Jobs Act has also introduced two other important changes for all firms (both small and large): i) a subsidy for new hirings with open ended contract, and ii) a new labor contract based on graded security for all new open ended jobs. While there is evidence that firms have reacted to the subsidy by substantially increasing open ended hirings and transforming fixed term into permanent contracts (see Boeri and Garibaldi, 2019, and Sestito and Viviano, 2018), both changes could positively affect fertility. Therefore, it is important to note that the negative effect on fertility that we find is an average difference between large- and small-firm employees with different working histories, and it could well be that the reform led to an increase in fertility for some specific populations of workers, such as those previously employed through fixed term contracts or those previously unemployed.

This paper contributes to the existing research on economic insecurity and fertility decisions in that it focuses on the impact of EPL, which has been poorly explored to date. There are just a few studies examining the relationship between EPL and fertility and their results are mixed. Indeed, while Bratti et al. (2005) find a positive association, Adsera (2004) finds a negative correlation. Moreover, Fahlen and Olah (2018) find that macro-level changes in EPL influences first childbearing intentions only for men and not for women.

Our work is closely related to that in Prifti and Vuri (2013), who find that reduced economic insecurity following a strengthening of the EPL regime has a positive and sizable effect on fertility decisions of Italian working women. In contrast to Prifti and Vuri (2013), we focus on a reform that has reduced job security, that from a theoretical viewpoint might generate a different reaction if individuals show loss aversion. In addition, the way individuals respond to variations in employment protection might have changed over time. Since the reform considered by Prifti and Vuri was implemented in 1990, our analysis also allows to investigate whether individuals have become accustomed to the increased uncertainty that has started to characterize the Italian labor market since the beginning of the new millennium.

Furthermore, this paper complements the large literature on the impact of employment protection on either side of the labor market, such as total factor productivity, job flow dynamics, innovation etc.[8] Our paper also extends the literature on the economic effects of a specific aspect of EPL: wrongful discharge protection. Prior studies have investigated the economic effects of wrongful discharge laws on employment

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increasing opportunity costs, reduce fertility (Cigno, 1991; Del Boca and Sauer, 2009; Del Boca et al., 2009; Francesconi, 2002; Hotz and Miller, 1988; Moffitt, 1984; Rosenzweig and Wolpin, 1980). On the other side, fertility rates may rise due to income effects. The ambiguity of this relationship (depending on whether the income effect prevails over the substitution effect) is confirmed by the fact that the correlation between female labor market participation and fertility, which has been negative for several years across countries, has recently turned out to be positive at least for Northern European countries and some Continental countries (Ahn and Mira, 2002).

[8] On the labor demand side, previous studies have examined the impact of EPL on labor and total factor productivity (Autor et al., 2007; Bassanini et al., 2009; Bjuggren, 2018; Cingano et al., 2016, 2010), job flow dynamics (Boeri and Garibaldi, 2019, 2007; Messina and Vallanti, 2007; Sestito and Viviano, 2018), innovation and firm creation (Griffith and Macartney, 2014; Schivardi and Torrini, 2008), investment on training (Bratti et al., 2019), use of temporary contracts (Hijzen et al., 2017), and wage (Bertola, 1990; Leonardi and Pica, 2013). On the labor supply side, prior contributions have investigated the effect of EPL on workers' probability of being dismissed (Boeri and Jimeno, 2005), workers' effort (Ichino and Riphahn, 2005), workers' welfare (Belot et al., 2007), and workers' initial mortgage conditions (Mistrulli et al., 2020).

and wages (Autor, Donohue and Schwab, 2006, 2004; MacLeod and Nakavachara, 200;) as well as on firms' capital market structure (Serfling, 2016), innovation (Acharya et al., 2014) and profitability (Bird and Knopf, 2009). This paper instead looks at the unintended consequences that changes in wrongful discharge laws may have on fertility.

The results of the present analysis have important policy implications in that they shed light on the potential unintended consequences that labor market reforms, aimed at dealing with high levels of unemployment by means of greater flexibility, may have on fertility decisions by increasing insecurity on career prospects. On this regard, our analysis might be relevant for the policy-making of most Southern European countries that have both fertility rates and labor markets similar to the Italian ones.

The paper proceeds as follows. Section 2 presents an overview of the institutional setting. Section 3 describes the data used and provides some descriptive statistics. In Section 4 we illustrate the econometric methodology implemented in the empirical analysis, present the main results and the robustness checks. Section 5 investigates possible heterogeneity in responses to the Reform. In Section 6 we provide some evidence to support the common trend assumption and try to deal with the self-selection of workers into small and large firms. Section 7 offers some concluding remarks.

2. The Institutional Setting

The Italian labor market has traditionally been characterized by a strict regime of EPL. Hiring and firing procedures, minimum wages, workplace safety and many other aspects of the employer-employee relationship were regulated by the Charter of workers' rights adopted in 1970.

According to "Article 18" of this Charter, dismissals of workers were allowed only in case of "just cause" (worker misbehavior or firms' need to reduce or reorganize its workforce). In contrast, in case of unfair dismissals the costs for firms could be rather high. More precisely, firms with more than 15 employees in case of dismissal were required to give to the employee a term of notice whose length is related to tenure. In addition, in case a judge finds that the dismissal was not motivated by a just cause the worker had the possibility to choose between reinstatement in the old job, plus a severance package equal to foregone earnings between the date of the dismissal and the date of the sentence, or a severance package consisting of 15 months of salary and the foregone earnings. Since the existence of a "just cause" was deemed by judges, the most critical aspect of this regime was the uncertainty in both the timing and contents of the judges' decisions, which have varied greatly across cases, labor markets and over time (Ichino, Polo and Rettore, 2003).

These rules were applied to firms with more than 15 employees, while firms with less than 15 employees were not mentioned by the Charter and were initially exempted from the EPL. Firing costs for small firms' employees were changed by Law No. 108, that in May 1990 established a number of dismissal restrictions also for workers holding jobs in firms with less than 15 employees. Small firms had to respect a term of notice and if a judge found the dismissal unfair the firm could choose to either pay the worker a

severance package between 2.5 and 6 months of salary or reinstate him/her in the old job (see Scoppa, 2010). In stark contrast with the provision for large firms, the firm could choose its preferred option and a maximum amount was established ex-ante as severance pay.

Despite the changes made in 1990, the degree of protection enjoyed by employees working in firms with more than 15 employees was greater. These workers, if unfairly dismissed, could ask to be reinstated and receive forgone wages and the health and social security contributions (for a minimum of 5 months) related to the period between the dismissal and the sentence. In addition, the unfairly dismissed employee had the right to choose between reinstatement and a severance payment amounting to 15 months' salary. In contrast, as said before, for employees in firms with fewer than 15 employees, the choice was up to the employer who could decide whether to reinstate the unfairly dismissed worker or make a severance payment.

At the end of the Nineties, to increase labor market flexibility and face the high unemployment rate, governments progressively introduced different types of fixed-term contracts (Malgarini et al., 2013). These policy interventions have led to a heavily segmented labor market, contraposing over-protected and under-protected categories, permanent and fixed-term workers, the former enjoying full employment protection and the latter facing high job instability.

An attempt to reduce firing cost was made by the Fornero Reform, adopted in June 2012 (Law No. 92/2012), which has weakened workers' protection in case of layoffs deemed as "unfair" by the Court. Restrictions on firing were reduced only for firms above the 15-employee cut-off by: i) limiting the possibility to choose between reinstatement and a monetary compensation in case of unfair dismissal, and ii) reducing the amount of severance payments and the duration of litigations, which, for these firms, were particularly high and uncertain (Gianfreda and Vallanti, 2017). However, in many situations the obligation of workers' reinstatement in case of unjust layoff has been preserved.

For this reason, in 2015 the Renzi government made a second attempt to reduce labor market segmentation by reducing firing costs for permanent workers with the so called "Jobs Act", that further limited the possibility of reinstatement, allowing it for discriminatory dismissals and for a few specific cases of disciplinary dismissals, and mandating, as a general rule, that unfair dismissals be compensated by disbursing an amount of money predetermined by law and proportional to job tenure (from a minimum of 4 times the monthly pay to a maximum of 24 times, i.e. 2-months' pay for every year of seniority).[9]

As discussed by Boeri and Garibaldi (2019), the Jobs Act is a structural reform that in order to reduce the dual structure of the Italian labor market operates on two fronts: on the one hand, by phasing out the reinstatement clause applied until March 2015 to unfair dismissals, it reduces the employment protection enjoyed by worker in open ended contracts; on the other hand, introducing a hiring subsidy for any new job opened on a permanent basis[10], it aims at increasing job stability for temporary and atypical workers. While the hiring subsidy applies to all new open ended hirings, the rules reducing employment protection regards

[9] This monetary compensation may be halved if the worker agrees to end any pending litigation about the nature of the dismissal, and the worker is exempted from paying taxes on the compensation received.

[10] Employers were exempted from paying social security contributions up to a 8,060 Euros cap per year and worker for the three years following the hiring (workers with an open ended contract in the previous six months and with an open ended contract with the same firm in the 3 months before Dec 2014 were excluded).

only firms above the 15-employees threshold. More precisely, these rules apply to all new hires in firms with more than 15 employees with a permanent contract signed after March 7th, 2015, when the new Law came into force, while they do not apply to previously hired workers in firms with more than 15 employees, who are still covered by the reinstatement clause.[11] Nonetheless, firms with a workforce below the threshold did not face significant changes, since, as explained, the reinstatement clause was not applied before the Jobs Act. All in all, the new regime reduces both the expected firing costs and, most significantly, the uncertainty surrounding them for firms over the 15-employees threshold, with no substantial changes for those below the threshold.

Following a number of papers using the 15-employees threshold to identify the effects of employment protection in the Italian labor market (Boeri and Garibaldi, 2019; Oliviero et al., 2020; Sestito and Viviano, 2016), we rely on this discontinuity and estimate the impact of a reduction in job security on fertility. Our hypothesis is that when firing costs are reduced, job security declines, and this could affect fertility decisions in two different ways. On the one hand, this could discourage women to have (more) children because their income flow becomes more uncertain. On the other side, firms could prefer women without children because they are more flexible and devote more time to work. If firms can (more) easily replace workers, women could feel threatened and, as a result, postpone fertility. The results we find in our empirical analysis lend support to this hypothesis.

It is important to recall that, together with the new rules concerning the reinstatement clause, in January 2015, the government introduced a sizeable hiring subsidy for new hires with open ended contracts, which was applied uniformly in large and small firms. This subsidy has induced firms to transform temporary contracts in open ended positions. Arguably, by increasing individual employment opportunities or stability, this has a positive influence on fertility. Even if the subsidy was available to all firms, Boeri and Garibaldi (2018, Table 3) show that the reform has produced an increase in open ended hiring as transformation from fixed term contracts of about 6.7 percentage points for small firms and of about 100% more (13.4 percentage points) for firms with more than 15 employees.

The differentiated effect for large and small firms is probably due to the fact that the reform, having suppressed the reinstatement clause for large firms, has reduced for them the effects deriving from the transformation from fixed term contracts to open ended ones (before the reform firms above the 15-employees threshold might fear the stricter employment protection enjoyed by open ended contracts and be discouraged to transform fixed term contracts in open ended position, while this discouraging effect was not in place for firms below the threshold). As a consequence, we cannot exclude that for this specific group of workers the reform has induced an increase in fertility. For this reason, it is worth emphasizing that the negative effect of the Jobs Act on fertility we document in this empirical investigation captures an average effect on employees with different working histories.

[11] Workers hired after the reform by a small (large) firm that becomes large (small) in subsequent years will be covered by the new rules. However, this does not imply any relevant change for workers as after the Jobs Act reform EPL is very similar for workers employed in the two types of firms.

3. Data and Descriptive Statistics

The Italian Labor Force Survey (LFS) is a dataset collected by ISTAT, the Italian National Institute of Statistics, providing quarterly information on the labor market status and other socio-economic characteristics of a representative sample of the Italian population (about 95,000 observations per quarter). We use data from the first quarter of 2013 to the fourth quarter of 2018 for a total of almost 2 million obs.[12]

As the Jobs Act rules apply to open-ended contract workers in large firms, we exclude from our sample self-employed, part-time and non-permanent employees. This also allows us to avoid self-selection problems: women planning to have a child might be more likely to accept these types of contracts since they might prefer less demanding jobs and careers. In addition, as the number of new hires in the public sector in recent years has been very small due to public finance limitations, we do not consider these workers.

Due to the features of our data set that only provides information on maternity leaves, we focus exclusively on employed women aged 16-46. Finally, since for maternity episodes taking place from April 2015 to December 2015 for women hired under the Jobs Act, we cannot be sure if the fertility decision has been effectively taken before or after the Jobs Act, we exclude these observations from our sample. Using all these sample selection criteria, we are left with 54,629 observations.

In order to build our dependent variable, we use two questions included in the LFS and inquiring participants about the reasons that have led to no working time or reduced working time during the reference week. More precisely, the first and the second question, proposed respectively to employees declaring no and reduced working time respectively, were formulated as follows: "What is the main reason why you did not work last week?"; "What is the main reason why you worked less than usual?". For both questions, among the possible answers there was one pointing to "Compulsory Maternity Leave",[13] which in Italy typically covers the two months before the date of childbirth and the three months following the birth.[14]

According to the Italian legislation during the period of compulsory abstention from work, the pregnant woman is entitled to retain her job and to receive a maternity allowance.[15] Using this information, we build the dependent variable *Maternity Leave* equal to one for women declaring as a reason for not

[12] Data since 2008 are also available but they are not perfectly homogeneous with subsequent years. In our main analysis we decided to use data starting from 2013 for two reasons. First, for years previous to 2013, the variable age, which is a fundamental determinant of fertility, is only available in 6 categories rather than being continuous. Second, extending the analysis to the years before 2013 would exacerbate the difference in terms of tenure between the group of women hired before the Jobs Act and the group of those hired after 2015, i.e., under the Jobs Act regime, which is not ideal in our thought experiment. However, we do use all the data in a robustness check analysis and we find almost identical results (see Section 4 and Tables A1 and A2 in the Appendix).

[13] The other possible answers for both questions were: "Under Earning Integration Fund (Cassa Integrazione Guadagni); Reduced activity for economic or technical reasons; Work disputes; Bad weather; Sickness, Holidays; Bank holidays, Flexible time schedule; Part-time; Study and training activity; Compulsory maternity leave; Voluntary parental leave; Leave for family reasons (excluding compulsory and maternity leave); Lack of work opportunities; New job or job change during the week; Work contract just expired; Occasional or seasonal job; Other".

[14] The worker has the possibility to put off her maternity leave until one month before the expected date of confinement and then continue it up to four months after the birth of her child.

[15] The Italian Law also allows women to apply for an early maternity leave (Astensione anticipata per gravidanza a rischio) for reasons related to health and safety during pregnancy. A medical certificate that certifies pregnancy at risk is required. Unfortunately, the LFS does not include a specific item for this condition.

working or working a reduced amount of time during the week before the interview "Compulsory maternity leave" (and zero otherwise).

We distinguish between small and large firms using the 15-employees threshold. We build a variable *Large Firm* that is equal to one if the number of employees is greater than 15 (16-49 employees; 20-49; 50-249; >250) and equal to zero if the number of employees is equal or below 15.[16]

In constructing our data set we had to deal with the fact that while information provided by the LFS refers to a reference week (the week before the interview), the exact period covered by this week is not released by ISTAT. The information that instead is made publicly available is the quarter of the interview, and then we impute the reference week in the middle of the quarter, for example, mid-February for the first quarter or mid-May for the second quarter, etc. As we calculate the date of beginning of the current job as the difference between the reference week and the number of months of *Tenure* (the number of months since the interviewed person has started the current job), this potentially introduces a measurement error in identifying among individuals hired around the threshold of March 7th, 2015 those affected by the Jobs Act reform.[17]

We define the dummy variable *Jobs Act* equal to one if a worker has been hired after the Jobs Act Reform, that is, after March, 7, 2015 (and zero otherwise) and calculate the variable *Time* (in days) as the difference between the date of hiring and March, 7, 2015.

In our dataset we have available the following variables: age, educational attainments (10 levels), number of children aged 2 or more, 20 regions of residence or 5 geographical areas, married, immigrant (no Italian citizenship), tenure (in years), 10 industry dummies,[18] job position,[19] quarter dummies and year dummies.

As shown in Table 1, 3.7% of women in our sample are on *Maternity Leave*. The percentage of women employed in large firms is 48% while in our sample women hired after the Jobs Act is equal to 7%. Women are relatively young (about 36 years old), with a medium level of education (about 12.7 years), 9.1 years of tenure and an average monthly wage of about 1,278 euros. About 56% of them are married, while on average they have 0.9 children aged 2 or older. 57% are white-collars, 39% are blue-collars and 4% have a managerial position. About 64% live in the Northern regions, 21% in the Center and about 15% in the South.

[16] We impute a missing value to *Large Firm* if the employee declares that "she does not know the exact number of employees, but this number is greater than 10", as in this case we are not able to understand whether the number of workers employed in the firm reaches the cutoff of 15 employees. Conversely, we impute 0 to *Large Firm* if the employee declares that "she does not know the exact number of employees, but this number is smaller than 10".

[17] We try to deal with this problem in a robustness check (see Table 5).

[18] Agriculture; Manufacturing; Building; Commerce; Hotel & Restaurants; Transports; Communications; Finance and Insurance; Housing and professional activities; Education, Health, Social Services.

[19] Manager, Cadre/Junior manager, White-Collar; Blue-Collar.

Table 1. Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max	Obs.
Maternity Leave	0.037	0.189	0	1	54,629
Large Firm	0.480	0.500	0	1	54,629
Jobs Act	0.070	0.255	0	1	54,629
Jobs Act*Large Firm	0.025	0.156	0	1	54,629
Age	36.648	6.523	16	46	54,629
Education (yrs.)	12.669	3.356	3	18	54,629
# Children (Age≥2)	0.904	0.903	0	10	54,629
Tenure	9.132	6.470	0	33	54,629
Immigrant	0.146	0.353	0	1	54,629
Married	0.560	0.496	0	1	54,629
Wage	1278.26	394.47	250	3000	54,629
Blue collar	0.387	0.487	0	1	54,629
White collar	0.569	0.495	0	1	54,629
Manager	0.043	0.203	0	1	54,629
North Est	0.356	0.479	0	1	54,629
North West	0.284	0.451	0	1	54,629
Center	0.215	0.410	0	1	54,629
South	0.094	0.292	0	1	54,629
Islands	0.052	0.222	0	1	54,629

Dataset: Italian Labor Force Survey (2013-2018), ISTAT. Sample: women employee (not self-employed), aged 16-46, with permanent job (no part-time), in private sector.

Preliminarily, in Table 2, we look at simple descriptive statistics in a standard 2x2 Table and consider whether large- and small-firm employees have differentially reacted to the introduction of the Jobs Act changing fertility decisions. For women hired before the introduction of the Jobs Act, Maternity Leave is on average 4.2% for employees of large firms and 3.4% for small-firm employees. In contrast, for women hired after the Jobs Act, the probability of being on Maternity Leave is equal to about 2% both in large and small firms. Our evidence shows that the fertility rate has declined following the Jobs Act, and that in the subsequent period the significant difference between large- and small-firm employees has completely disappeared.

Table 2. Maternity Leave Rates in Small and Large Firms Before and After the Jobs Act

	Hired Before Jobs Act	Hired After Jobs Act	After-Before Difference
Small Firms	0.034	0.021	-0.014***
	(0.001)	(0.003)	(0.003)
Large Firms	0.042	0.020	-0.022***
	(0.001)	(0.004)	(0.004)
Large-Small Difference	0.008***	-0.001	-0.008*
	(0.002)	(0.005)	(0.005)

Notes: Standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

4. Results from a Difference-in-Differences Approach

To provide evidence on the impact produced by the Jobs Act on fertility, we use a Difference in Differences approach considering employees of large firms as treated and small-firm employees as controls. We compare fertility rates of workers hired in small and large firms before and after March 2015, when the Jobs Act was introduced.

Following most of the papers in the literature, we use a linear estimator to estimate several specifications of the following model:

[1] $$Maternity\ Leave_{it} = \alpha + \beta Large_{it} + \gamma Jobs\ Act_{it} + \delta Large_{it} * Jobs\ Act_{it} + \lambda X_{it} + \mu_t + \varepsilon_{it}$$

The dependent variable *Maternity Leave*$_{it}$ represents our measure of fertility and takes value equal to one if woman *i* at time *t* was on Compulsory Maternity Leave during the reference week; *Large*$_{it}$ is a dummy for employees working in large firms (with more than 15 employees) and the coefficient β measures the difference in fertility rates between large and small firms' employees hired before the introduction of Jobs Act; *Jobs Act*$_{it}$ is a dummy taking the value of 1 for employees hired after 7 March 2015 and zero otherwise: γ represents the difference in fertility rates between small firms employees if hired before and after the Jobs Act; *Large*$_{it}$**Jobs Act*$_{it}$ is the interaction term whose coefficient δ measures the treatment effect of our interest; X_{it} is a vector of individual characteristics that could affect fertility decisions (age, age squared, education, marital status, immigrant, # children, tenure, region of residence, etc.), μ_t are year-quarter dummies, ε_{it} is an error term.

Estimates using an OLS estimator are presented in Table 3. In all specifications, standard errors are clustered at Large Firm*Year-Quarter level to take into account within Treatment*Time level correlation of the error terms (Bertrand, Duflo, and Mullainathan, 2004).[20]

In the first column of Table 3 we report the results of a very basic regression in which we only use as regressors *Large*$_{it}$ and *Jobs Act*$_{it}$ and the interaction term *Large*$_{it}$**Jobs Act*$_{it}$ without other controls. We find that women employed in large firms used to have a higher fertility rate of about 0.8 percentage points with respect to small firms' employees; women hired in small firms after the introduction of the Jobs Act reduced their fertility rates of 1.4 percentage points; more importantly, the introduction of the Jobs Act has reduced fertility for women hired in large firms by 0.8 percentage points with respect to small-firm employees (with the effect being statistically significant at the 5 percent level).

In column (2) we control for some important individual characteristics to avoid unbalanced comparisons between treated and control individuals. We control for *Age*, *Age Squared*, years of *Education*, *Married* and *Immigrant*. We find that women in large firms hired under the Jobs Act regime have significantly reduced their propensity to have a child by 1.2 percentage points (the coefficient is significant at 1 percent level). As regards controls variables, our findings are consistent with the previous literature: age and fertility are related by a concave relationship (with a maximum at age 29.7), education has a positive

[20] We obtain very similar results if we only correct standard errors for heteroskedasticity (estimates not reported).

impact on fertility (+0.7 percentage points for 5 years of education), married women are 5.2 percentage points more likely to have a child while female immigrants are less likely to have a child.

Table 3. The Effect of the Jobs Act on Fertility. Difference-in-Differences Estimates

	(1)	(2)	(3)	(4)	(5)
Large Firm	0.008***	0.003**	0.003**	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Jobs Act	-0.014***	-0.012***	-0.010***	-0.010***	-0.010***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Large Firm*Jobs Act	-0.008**	-0.012***	-0.014***	-0.014***	-0.014***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Age		0.018***	0.012***	0.012***	0.012***
		(0.001)	(0.001)	(0.001)	(0.001)
Age Sq.		-0.000***	-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)	(0.000)
Education, years		0.001***	0.001***	0.001***	0.001***
		(0.000)	(0.000)	(0.000)	(0.000)
Married		0.052***	0.054***	0.054***	0.054***
		(0.002)	(0.002)	(0.002)	(0.002)
Immigrant		-0.002	-0.008***	-0.008***	-0.008***
		(0.003)	(0.003)	(0.003)	(0.003)
# Children			-0.020***	-0.021***	-0.021***
			(0.001)	(0.001)	(0.001)
Tenure			-0.000	-0.000	-0.000
			(0.000)	(0.000)	(0.000)
Regional Dummies	NO	NO	NO	YES	YES
Year-Quarter Dummies	NO	NO	NO	NO	YES
Observations	54629	54629	54629	54629	54629
Adjusted R^2	0.001	0.036	0.045	0.045	0.045

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

In column (3) we control in addition for the number of children aged 2 or more[21] and the years of tenure. We find that the number of children tends to reduce the probability to have a child by 2.1 percentage points while tenure (controlling for age) has not impact on fertility. When controlling for these new variables, the impact of *Jobs Act* is slightly larger (-1.4 percentage points) and remains strongly significant. In columns (4) and (5) we progressively add region and year-quarter dummies to control for time-invariant unobserved heterogeneity at the geographic level and for time fixed effects, respectively. Reassuringly, we find almost identical results.

Considering that fertility rates (births per woman) in Italy are quite low and do not change much over time (according to the Italian National Institute of Statistics, the fertility rate was equal to 1.39 in 2011, 1.34 in 2014 and 1.29 in 2018, respectively) the impact we detect is relevant in magnitude. To give a clearer idea, considering that the probability to have a child in our sample is 3.7 percentage points, the effect found in our estimates corresponds to a decrease of about 38% of the average.

[21] The LFS dataset reports age of children only in categories.

The magnitude of the effect we find is slightly higher than that documented by Prifti and Vuri (2013), who show an increase of about 1 percentage point in the fertility of treated women due to the higher job security following the 1990 Reform, therefore suggesting that changes in EPL produce symmetric effects. However, it has to be considered that the Jobs Act has been implemented 25 years later and as a consequence, due to changes in economic and social factors that might have mediated its impact, comparisons have to be taken with care.

In Table 4, we check the sensitivity of our results by including additional controls to our main specification. In column (1) we control for job position dummies (5 categories), while in column (2) we include sector of activity dummies (10 categories) and results are unchanged with respect to those reported in column (5) of Table 3. Next, in column (3) we also control for the monthly (log) wage.[22] The estimated coefficient remains significant at the 1 percent level and slightly reduces in magnitude. Finally, in columns (4) we add region-specific time (linear) trends and we find again very similar results.[23]

Table 4. Difference-in-Differences Estimates. Additional Controls

	(1)	(2)	(3)	(4)
Large Firm	0.004***	0.004***	0.006***	0.006***
	(0.001)	(0.001)	(0.002)	(0.002)
Jobs Act	-0.010***	-0.010***	-0.011***	-0.011***
	(0.002)	(0.002)	(0.002)	(0.002)
Large Firm*Jobs Act	-0.014***	-0.014***	-0.013***	-0.011***
	(0.003)	(0.003)	(0.003)	(0.003)
Job position dummies	YES	YES	YES	YES
Sector of activity dummies	NO	YES	YES	YES
Wage (in log)	NO	NO	YES	YES
Region-specific trends	NO	NO	NO	YES
Observations	54629	54629	54629	54629
Adjusted R^2	0.045	0.045	0.047	0.047

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. Baseline controls (as in Table 3, column 5) are included in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

In Table 5 we report estimation results when using alternative time windows as regards the date of hiring of our sample women. We show results using the controls in specification (5) of Table 3 (our main specification). In column (1) we deal with the measurement error deriving from the fact that we have imputed the date of the interview of the respondent (to the mid-quarter) and the real interview could have taken place 45 days before or later. This implies that we could have erroneously calculated the time of hiring and so the *Jobs Act* dummy. To be on the safe side, we exclude from our sample 564 individuals (about 1% of the sample) who have been hired very near the threshold (that is we exclude $-45 \leq Time \leq 45$), a so-called "donut window". We find a slightly larger impact to that found above (-1.6 percentage points), consistent with a measurement error biasing towards zero the coefficient ("attenuation bias") of our previous estimates.

[22] We find a strong negative effect of wages on fertility, but this has to be taken with care since a reverse causality problem could seriously bias the estimates.

[23] All our findings are confirmed if – instead of a Linear Probability Estimator – we use a Probit estimator (estimates not reported).

In column (2) of Table 5 we restrict our sample to individuals who were hired close to the Jobs Act Reform choosing a symmetric window of 1350 days[24] around the threshold (difference between the date of hiring and March 7th, 2015). In this way we exclude women hired much before the cutoff date, that is, those hired before July 2011, focusing on a sample of about 14,000 observations. Also in this case our results remain qualitatively the same. This is reassuring as one possible concern in our estimates is that women hired after the Jobs Act are very different from women hired before the Jobs Act, in particular, in terms of tenure. In fact, tenure is on average 9.7 years for women hired pre-reform vs. 1.37 years for those hired post-reform.

Table 5. Difference-in-Differences Estimates. Alternative Time Windows

	(1) Donut Window	(2) Symmetric Window	(3) Symmetric with Donut	(4) 2 years Window with Donut
Large Firm	0.004***	0.005	0.006*	0.003
	(0.001)	(0.003)	(0.003)	(0.004)
Jobs Act	-0.010***	0.013**	0.017**	0.009
	(0.003)	(0.006)	(0.006)	(0.009)
Large Firm*Jobs Act	-0.016***	-0.012**	-0.015***	-0.011**
	(0.004)	(0.005)	(0.005)	(0.006)
Observations	54091	14345	13807	7418
Adjusted R^2	0.045	0.046	0.046	0.035

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. Baseline controls (as in Table 3, column 5) are included in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

In column (3) we jointly apply the two restrictions used in columns (1) and (2), obtaining results that are similar to those presented above (-1.5 percentage points). Finally, in column (4) we focus on a symmetric window of two years from hiring before and after the threshold and we exclude individuals hired very near the threshold (-45<=*Time*<=45). Results change only slightly.[25]

Since we have assigned employees to treatment and control groups on the base of their answers to the question regarding the total number of employees in the firm, a possible concern is that employees do not know (or do not remember) the exact number. To tackle this issue, we estimate our main specification on alternative samples that exclude some firm size categories that could originate miscalculations. Results are reported in Table 6. More precisely, in column (1) we first exclude observations in which the worker does not know the exact number of employees, but she believes that such number is below 10 or above 10. In column (2), starting from the sample used in column (1), we classify as "Small Firms" only firms with 10 employees or less and as "Large Firms" only firms with 20 or more employees, hence excluding the categories of 11-15 employees and 16-19 employees. In column (3) we additionally exclude the category 20-49 and therefore we re-classify as "Large Firms" only firms with a number of employees greater or equal 50.

[24] 1350 days is the highest tenure we observe in our dataset for individuals hired after the introduction of the Jobs Act.
[25] We obtain similar results considering a three years' symmetric window.

Notably, results in Table 6 indicate that the effect of the Jobs Act on fertility is very similar to that estimated in our main analysis.

Table 6. Difference-in-Differences Estimates. Excluding some Firm Size Categories

	(1) Excluding Not sure but <10 or >10	(2) Excluding also 11-15 & 16-19	(3) Excluding also 20-49
Large Firm	0.004***	0.004***	0.002
	(0.001)	(0.001)	(0.002)
Jobs Act	-0.011***	-0.009***	-0.009***
	(0.002)	(0.003)	(0.003)
Large Firm*Jobs Act	-0.013***	-0.016***	-0.016***
	(0.003)	(0.004)	(0.005)
Observations	53080	44625	36372
Adjusted R^2	0.045	0.046	0.042

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. Baseline controls (as in Table 3, column 5) are included in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

As a further robustness check, in Table 7 we exploit the fact that in our particular setting we are able to consider the behavior of women employed in large firms observed in the same period but hired both before and after the Jobs Act, for whom different dismissal rules apply. Therefore, we use only the sample of large firms' employees in the years 2016-2018 and verify if women hired under the Jobs Act have lower fertility rates. Using as control variables those in the specifications (1) to (5) of Table 3, we show that women hired under the new EPL regime are about 2.2 percentage points less likely to have a child with respect to their colleagues hired in large firms before the reform.

Table 7. Time-Differences Estimates. Only Large-Firm Employees

	(1)	(2)	(3)	(4)	(5)
Jobs Act	-0.021***	-0.024***	-0.022***	-0.022***	-0.024***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Observations	11496	11496	11496	11496	11496
Adjusted R^2	0.001	0.035	0.043	0.043	0.043

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. Baseline controls (as in Table 3, column 5) are included in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

We have also experimented by including in the sample individuals with temporary contracts. As the Jobs Act reform has also introduced a hiring subsidy (applied uniformly to large and small firms), firms were induced to transform temporary contracts in open ended positions and for individuals who have benefited from these transformations there has been, arguably, an increase in job stability and probably an increase in fertility. Considering that – as shown by Boeri and Garibaldi (2019) – the transformation of temporary contracts in open ended positions has interested especially large firms' employees, by including temporary contracts in the sample we would expect a reduction in the magnitude of the effect previously shown.

The estimates – replicating the specifications in Table 3 – are shown in Table 8. From these estimates two relevant findings emerge. First of all, we find that temporary workers have a considerable lower probability to have a child – around 1.8-2 percentage points less (*t*-stat around -10) – than workers on permanent contracts. This represents suggestive evidence of the impact of type of contract, although this effect cannot be considered completely causal since other differences between temporary and permanent workers could affect the estimates. Secondly, consistently with our expectations, we find that when including temporary workers in the sample the impact of the Jobs Act on fertility reduces from about 1.3-1.4 to about 1 percentage point, that nonetheless remains highly significant (*t*-stat around -4).

Table 8. The Effect of the Jobs Act on Fertility. Difference-in-Differences Estimates. Sample including Workers on Temporary Contracts

	(1)	(2)	(3)	(4)	(5)
Large Firm	0.007***	0.004***	0.004***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Jobs Act	-0.011***	-0.009***	-0.008***	-0.008***	-0.010***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Large Firm*Jobs Act	-0.007**	-0.009***	-0.010***	-0.010***	-0.010***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Temporary Workers	-0.019***	-0.020***	-0.017***	-0.017***	-0.019***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)
Observations	67994	67994	67891	67891	67891
Adjusted R^2	0.004	0.036	0.043	0.043	0.043

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Finally, we also check whether the results are robust to extending the analysis to the years before 2013. In particular, we re-estimate all specifications in Table 3 and Table 4 using data from 2008 to 2018 and obtain qualitatively similar results, with a slightly higher magnitude (see Table A1 and A2 in the Appendix of the paper).

5. Heterogeneous Responses

In this section we investigate whether the response of fertility to the reduction in employment protection differs in relation to a number of individual characteristics.

We first focus on age and split our sample in two different groups, that is, below and above the median age (i.e. 38). As shown in Table 9, the labor market reform we are examining has an impact on fertility for both age groups, but this is larger for the younger one (-1.7 percentage points). This result is consistent with the hypothesis that younger women face lower time pressure in the family formation decision-making, and thus have potentially greater incentives to postpone childbearing until professional integration or to reduce the number of children to lower child-penalty risks.

Table 9. Heterogeneity by Age (below/above median)

	(1) Age≤38	(2) Age>38
Large Firm	0.007***	0.000
	(0.002)	(0.001)
Jobs Act	-0.008*	-0.004
	(0.004)	(0.003)
Large Firm*Jobs Act	-0.017***	-0.011***
	(0.004)	(0.004)
Observations	30163	24466
Adjusted R^2	0.045	0.018

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. Baseline controls (as in Table 3, column 5) are included in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

In Table 10 we look at heterogeneous effects according to marital status, employment status of the partner and parity. In columns (1) and (2) we estimate the effect on unmarried women by distinguishing between singles and divorced/separated or widows and find that the reduction in employment protection has affected mainly single women. This indicates that the impact might be driven by an income effect.

In columns (3) and (4) we investigate the effect on married women. For the subsample of married women, we have built a variable *Husband Employed* indicating if the husband has a job or not. Interestingly, in column (3) we find that the effect of Jobs Act on a woman's fertility is smaller (0.8 percentage points) and not significant if her husband is employed - perhaps because the increased job insecurity is less problematic in this case – whereas the negative effect is much larger (-4.2 percentage points), although imprecisely estimated (p-value=0.14), when the husband is unemployed. This seems to suggest that the increased job insecurity is overwhelming if the household earns only one source of income, therefore confirming that the estimated effect might be mediated by an income effect.

Table 10. Heterogeneity by Marital Status, Husband's Employment Status and Parity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Unmarried		Married		Parity		
	Single	Divorced /widow	Husband employed	Husband not employed	0	1	2+
Large Firm	0.001	0.004*	0.004*	0.020***	0.006**	0.002	0.000
	(0.001)	(0.002)	(0.002)	(0.007)	(0.003)	(0.002)	(0.001)
Jobs Act	0.000	0.005	-0.026***	0.001	-0.010	-0.020***	0.002
	(0.003)	(0.008)	(0.007)	(0.024)	(0.007)	(0.004)	(0.003)
Large Firm*Jobs Act	-0.008***	-0.004	-0.008	-0.042	-0.029***	-0.003	-0.003
	(0.003)	(0.013)	(0.008)	(0.029)	(0.007)	(0.008)	(0.006)
Observations	13867	9475	27500	2463	22078	18236	14315
Adjusted R^2	0.007	0.007	0.045	0.043	0.044	0.032	0.005

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. Baseline controls (as in Table 3, column 5) are included in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

As regards parity, we find a strong effect for women without children in column (5) and almost no effect for women with one or more children - columns (6) and (7). Given the decline in fertility rate observed

in Italy in the last decades, this could derive from the fact that after the first child the probability of having additional children is extremely low and not particularly affected by economic conditions. Importantly, this finding lends further support to the hypothesis that younger women have greater incentives to defer family formation to pursue a professional career.

We also investigate whether there are statistically significant differences between women with different levels of education and between the North and the South part of the country. The reason for looking at differentiated effects according to education is that a higher level of education is expected to increase workers' re-employment rates (see for instance, Riddel and Song, 2011). Similarly, re-employment success is likely to depend on general economic conditions. Given the substantial and persistent gap in economic performance and living standards between Italian northern and southern regions and the high unemployment rates faced by people living in the South, it would be difficult for the latter to find employment after losing a job, which might imply a different reaction to changes in EPL.

In the first two columns of Table 11 we run separate regressions for women without and with a College Degree, respectively. We find that the effect is mainly concentrated on women with no tertiary education while there is no effect for women with a College Degree. This could be due to the fact that individuals who have acquired tertiary education have higher re-employment probabilities and also typically come from wealthier families and thus are less affected by job insecurity.[26]

Table 11. Heterogeneity by Education and Geographic Area

	(1) No Tertiary education	(2) Tertiary education	(3) North	(4) Center	(5) South
Large Firm	0.005***	-0.005	0.004**	-0.001	0.010**
	(0.002)	(0.004)	(0.002)	(0.002)	(0.004)
Jobs Act	-0.003	-0.036***	-0.017***	-0.012	0.003
	(0.003)	(0.007)	(0.004)	(0.008)	(0.008)
Large Firm*Jobs Act	-0.013*	0.004	-0.009*	-0.002	-0.031**
	(0.007)	(0.008)	(0.005)	(0.013)	(0.013)
Observations	42738	11891	34924	11719	7986
Adjusted R^2	0.042	0.052	0.046	0.041	0.053

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. Baseline controls (as in Table 3, column 5) are included in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

In columns (3) to (5) of Table 11 we run separate regressions for individuals living in the northern, center and southern regions, respectively. We find that the negative effect of the Jobs Act on fertility is mainly driven by women living in the South (3.1 percentage points). The effect for those living in the North is lower in magnitude (-0.9 percentage points) and significant at the 10% level. This result might be interpreted as evidence in support of the hypothesis that job instability could affect fertility through different expectations on future career prospects: as women working in the South have, *ceteris paribus*, lower employment opportunities than their counterparts in the Center-North - and these prospects did not change

[26] The strong impact that family background produces on the probability of acquiring a College Degree in Italy is documented by several papers, see for instance Checchi et al. (2012) and Pronzato (2012).

much in response to the Jobs Act reform - they might have lower expectations about their future protection, and as a result, feel more discouraged to have children by an increase in job insecurity.

Another interesting issue is whether women at different levels of the wage distribution reacted differently to the introduction of the Jobs Act. To this purpose in columns (1) and (2) of Table 12 we run separate regressions for women with wage below and above the median, respectively. Results show that the reduction in fertility rates in concentrated among women with wage lower than the median (-1.7 percentage points), while a statistically insignificant effect is found for women receiving wages above the median. Results are qualitatively similar when we distinguish according to job position (Blue collar, White collar and Manager). As shown in columns (3) and (4) of Table 12, fertility of treated women in blue- and white-collar positions has reduced following the introduction of the Jobs Act,[27] while the estimates in column (5) show that there is no impact for women in managerial positions (only 2,349 observations).

Table 12. Heterogeneity by Wage Level and Job Position

	(1)	(2)	(3)	(4)	(5)
	Wage<Median	Wage≥Median	Blue-Collar	White-Collar	Manager
Large Firm	0.009***	0.001	0.010***	0.000	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.008)
Jobs Act	-0.010***	-0.017***	-0.009**	-0.012***	-0.040***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.014)
Large Firm*Jobs Act	-0.017**	-0.006	-0.014	-0.013**	0.016
	(0.008)	(0.005)	(0.011)	(0.005)	(0.020)
Observations	27361	27268	21139	31102	2349
Adjusted R^2	0.051	0.041	0.041	0.047	0.039

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. Baseline controls (as in Table 3, column 5) are included in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Taken together, these results indicate that the salience of the effect of job insecurity on fertility crucially hinges upon the income level, therefore suggesting that income uncertainty is an important channel whereby employment protection affects fertility.

6. Common Trend Assumption, Placebo Tests, and Sorting Issues

A crucial assumption in the Difference-in-Differences estimation strategy is the hypothesis of common trend, that is, treated and control subjects – in the absence of the treatment – would have followed similar trends. While we cannot directly test this assumption since the counterfactual is not observable, in this Section we can verify if before the introduction of the Jobs Act large and small firms' employees were following similar trends in fertility.

To do so, using data over the period 2008-2018, we regress our measure of fertility on the dummy *Large Firm* and on the full set of interactions between *Large Firm* and the year dummies. We estimate specification 5 of Table 3 including the full set of controls and using as reference category different years

[27] Nowadays, many low-income workers are employed in white-collar jobs.

(2008, 2011, 2012 and 2015). Results are reported in Table 13. Intuitively, if some of the interaction terms were statistically significant, this would imply that, before treatment, fertility decisions of treated women followed a different pattern compared to controls, thus casting some doubts on our analysis.

Table 13. Parallel Trends for Large and Small Firms' Employees in the Pre-Jobs Act Period

	(1)	(2)	(3)	(4)
Large Firm	0.009***	0.008***	0.008**	0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Large Firm*y2008		0.007	0.007	0.010**
		(0.005)	(0.005)	(0.005)
Large Firm*y2009	-0.005	-0.005	-0.004	-0.001
	(0.005)	(0.005)	(0.005)	(0.005)
Large Firm*y2010	-0.004	-0.003	-0.003	0.001
	(0.005)	(0.005)	(0.005)	(0.005)
Large Firm*y2011	0.005		0.005	0.009*
	(0.005)		(0.005)	(0.005)
Large Firm*y2012	0.005	0.005		0.009
	(0.005)	(0.006)		(0.006)
Large Firm*y2013	-0.003	-0.003	-0.003	0.001
	(0.005)	(0.005)	(0.006)	(0.006)
Large Firm*y2014	0.006	0.006	0.006	0.010
	(0.006)	(0.006)	(0.006)	(0.006)
Large Firm*y2015	-0.009	-0.008	-0.008	
	(0.006)	(0.006)	(0.006)	
Large Firm*y2016	-0.035***	-0.034***	-0.034***	-0.032***
	(0.010)	(0.010)	(0.010)	(0.010)
Large Firm*y2017	-0.039***	-0.039***	-0.039***	-0.037***
	(0.007)	(0.007)	(0.007)	(0.007)
Large Firm*y2018	-0.015	-0.015	-0.015	-0.013
	(0.010)	(0.010)	(0.010)	(0.010)
Observations	131195	131195	131195	131195
Adjusted R^2	0.037	0.037	0.037	0.037
F-test	1.01	1.21	1.25	1.49
p-value	0.419	0.295	0.276	0.164

Notes: Each column reports estimates from OLS regression. The sample is from 2008Q1 to 2018Q4. The dependent variable is *Maternity Leave*. Robust standard errors are reported in parentheses. Baseline controls (as in Table 3, column 5) are included in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

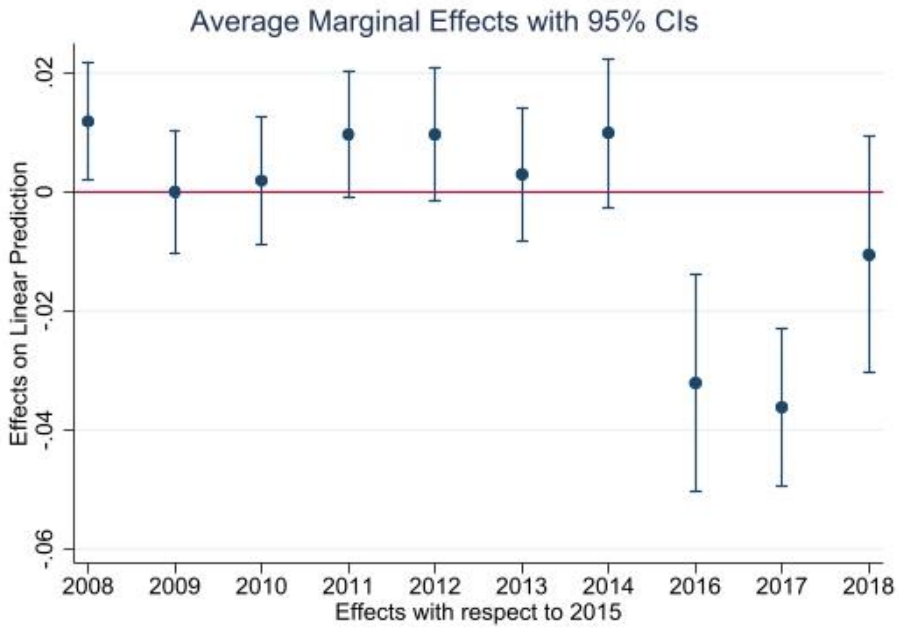
As shown in Table 13, large-firm employees have a higher fertility rate of about 0.9 percentage points in 2008, which decreases to 0.8 in 2011 and 2012. Yet, along time there is no evidence of different trends for women employed in large and in small firms as the pre-reform (i.e. before 2015) yearly dummies do not generally differ between groups. This holds true also for the year following the introduction of the Fornero reform (June 2012) that has made a first attempt to reduce firing cost for large firms.[28] Although, when we use as reference category the year 2015 some interactions turn out to be significant, on the whole we cannot reject the null hypothesis for the joint significance of all interaction terms (the *p*-value of the *F*-

[28] The negative coefficients associated with the interactions between Large Firm and the year dummies for 2013 and 2015 suggest a negative but not statistically significant effect.

test ranges from 0.16 to 0.42 in the various specifications). These results reassure us that the assumption of parallel trends between employees in treated and control groups is appropriate for our analysis.

For visual inspection, in Figure 1 we plot the estimated coefficients from specification 4 of Table 13. Despite the fertility rate being higher for women employed in large firms, the trend followed by large-firm employees during the seven years prior to 2015 (i.e. the Jobs Act year) is typically not statistically different from that emerging for small-firm employees. The figure shows clearly that treated and control groups start diverging in their fertility rates immediately after the implementation of the Jobs Act Reform, with the differential effect being negative and statistically significant for the years 2016 and 2017 (and remaining negative though not significant also in 2018). This suggests that the decrease in fertility observed for large-firm employees since 2016 is likely attributable to the reduction in their employment protection brought about by the 2015 Reform.

Figure 1. Estimated differences in large-small firms' employees before and after the Jobs Act



Notes: The graph plots the estimated coefficients from specification 4 of Table 13.

As a further robustness check, considering data for the period before the introduction of the Jobs Act, we run five placebo tests, and assume that a fictitious reform was introduced, in turn, in the first quarter of 2010, 2011, 2012, 2013 and 2014. We build five dummy variables, *Fake Jobs Act 2010*, *Fake Jobs Act 2011*, *Fake Jobs Act 2012*, *Fake Jobs Act 2013*, *Fake Jobs Act 2014* and the respective interactions with *Large Firm*. In Table 14 we run five regressions for the five fictitious reforms, respectively. Results in columns (1) to (5) show that the interaction terms are not far from zero and are never statistically significant. This again confirms that our estimated effect of the Jobs Act Reform on fertility is not a spurious correlation. Moreover, results reported in column 4 where we use *Fake Jobs Act 2013* are informative of the impact

produced by the Fornero Reform in the two years after its introduction: these estimates do not show any significant impact on fertility following the reform.

Table 14. Placebo Tests. Verifying the Impact of Five Fictitious Reforms

	(1)	(2)	(3)	(4)	(5)
Large Firm	0.009***	0.008***	0.009***	0.010***	0.009***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Fake Jobs Act 2010	0.007***				
	(0.002)				
Large Firm*Fake Jobs Act 2010	0.001				
	(0.003)				
Fake Jobs Act 2011		0.004			
		(0.002)			
Large Firm*Fake Jobs Act 2011		0.004			
		(0.003)			
Fake Jobs Act 2012			0.005**		
			(0.002)		
Large Firm*Fake Jobs Act 2012			0.002		
			(0.003)		
Fake Jobs Act 2013				0.004*	
				(0.003)	
Large Firm*Fake Jobs Act 2013				0.000	
				(0.004)	
Fake Jobs Act 2014					0.002
					(0.004)
Large Firm*Fake Jobs Act 2014					0.005
					(0.005)
Observations	98832	98832	98832	98832	98832
Adjusted R^2	0.037	0.037	0.037	0.037	0.037

Notes: Each column reports estimates from OLS regression. The sample is from 2008Q1 to 2014Q4. The dependent variable is *Maternity Leave*. Robust standard errors are reported in parentheses. Baseline controls (as in Table 3, column 5) are included in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Finally, we deal with the potential issues deriving from workers' sorting. The bias deriving from the endogenous sorting of women into firms with different employment protection is related to the fact that sorting might vary in the pre- and post-reform period, leading to a bias that cannot be differentiated away through the Difference-in-Differences approach. This implies that the decrease in fertility we find for large firms might be due to an increase in the hiring of women with an already lower fertility rate and/or a decrease in the hiring of women with an already higher fertility rate.

To try to handle this issue, we adopt an instrumental variable strategy and use as an instrument for *Large Firm* the percentage of employees in large firms (with more than 49 employees) operating in the previous year in the same sector and in the same region in which the worker is currently employed (*Perc. Large Firms Employees*).[29] Similarly, since in our model we use the interaction *Large Firm*Jobs Act*, we need to instrument this variable with the interaction between *Perc. Large Firms Employees* and *Jobs Act*.

[29] Unfortunately, since we do not have longitudinal data, we cannot follow the approach of Prifti and Vuri (2013) that instrument a worker's employment in small firms using the lagged employment status of the same worker.

The data we use are from the ISTAT ASIA Archive ("Archivio Statistico delle Imprese Attive") for years 2012-2018.

The idea behind our instrument is that whether a worker ends up working in a small or in a large firm is also related to the productive structure of the area in which she lives, proxied by the fraction of large firms' employees that in year *t-1* were active in the region and in the sector in which the worker is employed in year *t*. The exclusion restriction is based on the assumption that the pre-reform regional-industrial share of large firms should not affect directly fertility decisions.

Table A3 in the Appendix reports the 2SLS estimates. First stage results show that our instruments are always strongly significant in explaining firm size choice (F-statistics for the test that the coefficients of the instruments in the First Stage are jointly zero are always well above 100), therefore indicating that our instrument is a strong predictor of Large Firm.

Second stage results confirm a negative effect of the Jobs Act on fertility for large firms' employees. However, the estimated coefficients are large and not statistically significant at conventional levels (*p*-values are around 0.14). The larger magnitude and standard errors indicate that they are rather imprecise with respect to the estimates obtained by OLS. This is certainly not due to a weak-instrument issue, but it could be related to the fact that our instrument relies on the assumption of low mobility across regions and sectors, which might not be true for all categories of workers. Nonetheless, the IV estimates are suggestive that our previous estimates are not biased towards finding an effect when a null effect exists in reality. This result lends further credibility to our main results presented in Table 3.

We note also that given the nature of the institutional change under examination - a decrease in the EPL in large firms while maintaining constant the protection in small firms and making the degree of protection very similar in the two types of firms – the incentives for women to move across types of firms are not so high.

7. Concluding remarks

In many advanced countries the fertility rates have declined over time and are now dramatically low. In Italy, the fertility rate is around 1.4 births per woman, well below the population replacement rate of 2.1. The decline in fertility rates could be associated with increased participation rates of women in the labor market coupled with a growing instability of jobs and a marked decline of job security.

In this paper we investigate a specific aspect of this tendency, that is, we explore the consequences of a reduction in the degree of EPL on the fertility decisions of women. To this aim, we exploit a natural experiment represented by a recent reform in the Italian Labor Market ("Jobs Act") that has substantially reduced the employment protection enjoyed by new hires in large firms (abolishing the reinstatement clause for unfair dismissals) but left largely unchanged the protection for small firms' employees.

We employ a Difference-in-Differences estimation strategy and compare the variations over time in fertility rates of women employed in large firms with the analogous variations for small firms' employees.

We find that the fertility rate of treated women has reduced of about 1.4 percentage points more than that of women hired in small firms. This result remains robust when in our regressions we control for a large set of predetermined individual characteristics and when we restrict the sample to compare women with more similar characteristics in terms of tenure. Furthermore, the result holds true when we focus on different symmetric windows, in terms of employees' hiring date, around the time of introduction of the reform.

We also document large heterogeneous effects by age, marital status, parity and geographic residence, as well as by the level of education and wage. All in all, this evidence shows stronger effects for younger and for more vulnerable individuals, i.e., for women employed in low paying jobs and with a low level of education. Larger effects are also found for singles and for women whose husbands are unemployed. Furthermore, in order to reassure about the internal validity of our estimation strategy we carry out an analysis of the common trend assumption considering the fertility decisions of women in small and large firms since 2008 and conduct some placebo tests. Finally, we try to address sorting issues by means of an IV approach and we find support for our main results.

One potential caveat of our analysis is that, since we do not observe individuals' working histories, we are not able to disentangle the overall impact of the reform on some specific sub-groups of workers (such as those previously employed through fixed term contracts or unemployed) who might have instead benefited from other changes that have been simultaneously introduced by the Jobs Act, namely the subsidy for new hirings with open ended contract and the new labor contract with graded security. Nevertheless, our Difference-in-Differences estimates provide an average effect of the reform on workers' fertility.

Our results are in line with the findings of Modena et al. (2013) that show that most of the Italian couples were discouraged to have (more) children because of employment instability and the related income insecurity.

Overall, these results shed light on the unintended consequences that labor market reforms introducing greater flexibility may have on fertility by increasing insecurity on career prospects and suggest that policies aimed at increasing fertility should be coupled with adequate labor market policies. Clearly, our findings are relevant especially for Southern European countries that have both fertility rates and labor markets similar to the Italian ones.

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Appendix

Table A1. The Effect of the Jobs Act on Fertility. Difference-in-Differences Estimates. Data from 2008Q1 to 2018Q4.

	(1)	(2)	(3)	(4)	(5)
Large Firm	0.012***	0.007***	0.006***	0.006***	0.006***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
Jobs Act	-0.014***	-0.006**	-0.006**	-0.006**	-0.012***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)
Large Firm*Jobs Act	-0.013***	-0.016***	-0.016***	-0.016***	-0.016***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Age		0.018***	0.018***	0.018***	0.018***
		(0.001)	(0.001)	(0.001)	(0.001)
Age Sq.		-0.000***	-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)	(0.000)
Education, years		0.001***	0.001***	0.001***	0.001***
		(0.000)	(0.000)	(0.000)	(0.000)
Married		0.061***	0.061***	0.061***	0.061***
		(0.001)	(0.001)	(0.001)	(0.001)
Immigrant		-0.004***	-0.004**	-0.004**	-0.005***
		(0.002)	(0.002)	(0.002)	(0.002)
# Children			-0.002	-0.002	-0.001
			(0.001)	(0.001)	(0.001)
Tenure			0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)
Regional Dummies	NO	NO	NO	YES	YES
Year-Quarter Dummies	NO	NO	NO	NO	YES
Observations	131195	131195	127767	127767	127767
Adjusted R^2	0.001	0.037	0.037	0.038	0.038

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Since age is in categories in data before 2013, we build *Age* imputing the average value of each class for these data. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A2. Difference-in-Differences Estimates. Additional Controls. Data from 2008Q1 to 2018Q4.

	(1)	(2)	(3)	(4)
Large Firm	0.006***	0.007***	0.009***	0.009***
	(0.001)	(0.001)	(0.001)	(0.001)
Jobs Act	-0.011***	-0.012***	-0.012***	-0.012***
	(0.002)	(0.002)	(0.002)	(0.002)
Large Firm*Jobs Act	-0.016***	-0.016***	-0.015***	-0.014***
	(0.003)	(0.003)	(0.003)	(0.003)
Job position dummies	YES	YES	YES	YES
Sector of activity dummies	NO	YES	YES	YES
Wage (in log)	NO	NO	YES	YES
Region-specific trends	NO	NO	NO	YES
Observations	127767	127767	127767	127767
Adjusted R^2	0.038	0.038	0.040	0.040

Notes: Each column reports estimates from OLS regression. The dependent variable is *Maternity Leave*. Since age is in categories in data before 2013, we build *Age* imputing the average value of each class for these data. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. Baseline controls (as in Table 3, column 5) are included in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A3. Two Stages Least Squares Estimates.

	(1)	(2)	(3)	(4)	(5)
Second Stage					
Large Firm*Jobs Act	-0.089	-0.089	-0.089	-0.089	-0.080
	(0.057)	(0.059)	(0.059)	(0.058)	(0.064)
Basic Controls	YES	YES	YES	YES	YES
Regional Dummies	YES	YES	YES	YES	YES
Year-Quarter Dummies	NO	YES	YES	YES	YES
Job position dummies	NO	NO	YES	YES	YES
Wage (in log)	NO	NO	NO	YES	YES
Region-specific trends	NO	NO	NO	NO	YES
First Stage for Large Firm					
Perc. Large Firms Employees	0.009***	0.009***	0.009***	0.008***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Perc. Large Firms Employees*	0.001	0.001	0.001	0.001	0.001
Jobs Act	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
F-Stat	142.89	172.17	139.22	136.36	136.05
Observations	54079	54079	54079	54079	54079
Adjusted R^2	0.106	0.106	0.110	0.137	0.137

Notes: The Table reports TSLS estimates. The dependent variable is *Maternity Leave*. Robust standard errors are reported in parentheses. Controls included as in Table 3 (column 5) and Table 4. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.