



Temporary employment and fertility in Italy: The effect of two labor market reforms in the early 2000s[☆]

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ABSTRACT

This paper presents compelling evidence that the extensive use of temporary contracts in Italy affects fertility. Using work histories between 2003 and 2010 for specific reforms in the fixed-term and apprenticeship contracts that have been gradually applied since 2005, we examine the propensity of women to have children. Our findings point towards a significant negative impact on fertility outcomes, although the magnitude of these estimates is small. These results are heterogeneous dependent on the employment status of the women's partners, suggesting that partners' permanent contracts leave fertility behavior unaffected. Women with partners on temporary employment contracts have a lower chance of having children. We thoroughly explore the potential explanations for our results. Temporary contracts lead to more economic uncertainty, which we identify by the increase in wage volatility, the reduction in the probability to be in a couple, and – for the first child – childbearing postponement.

1. Introduction

Over the last few decades, the process of labor market deregulation, internationalization and the intensification of competition has led to rapid changes in the social and economic structures of modern societies. These changes have also favored the increase of widespread economic uncertainty, which is considered an individual risk factor in certain life stages, as regards unemployment and increasing forms of temporary work (Adsera, 2004; Alderotti et al., 2021).

A drastic transformation in fertility patterns over the life cycle of families has also been experienced in Western countries. In particular, Italy's total fertility rate (TFR) saw a long decreasing trend from the early 1970s, reaching its lowest level around 1995, when the average number of children per woman was 1.19 (Kohler et al., 2002). Nowadays, Italy remains one of the developed nations with the lowest fertility levels, with an average number of children per woman of 1.3 in 2020, representing a 50% reduction from the 1960s peak. According to the Second Demographic Transition theory, several arguments may be addressed to explain the low fertility in western countries. The transition to an "individualistic family model" weakened by the

increase of cohabitation and the dissolution of unions, the greater symmetry in sex roles, which favored female emancipation, and rising female education levels and economic autonomy were, among others, the main factors determining fertility decline below replacement level. In addition, other changes in value systems, such as contraceptives and the sexual revolution, the rise of "higher order" needs, like the assessment of individual autonomy and freedom of choice, women's self-actualization coming before family life and the rejection of authority, were also considered important ideological features for the new demographic regime (Lesthaeghe, 2020).

Our paper aims to investigate the effects of the labor market changes brought about by two different types of temporary employment contracts, specifically fixed-term and apprenticeship contracts, on workers' fertility behavior in Italy. Since 1997, Italy has attempted to introduce flexibility into the labor market by using temporary employment. However, the disciplines and employment protection for standard regular workers have been left unchanged. The so-called "Treu-Law" (No. 196/1997) introduced temporary work agencies and has given rise to the expansion of new forms of temporary contracts, as well as

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apprenticeship, seasonal and youth work-training contracts. At the turn of the new century, two labor market reforms increased job flexibility by introducing new forms of atypical and temporary contracts and by weakening the limitations on their use. The first labor market reform (Decree Law, no. 368 legislated in 2001) eased the use of fixed-term contracts, thereby allowing firms to use these types of contracts in order to face changing economic conditions for technical, organizational, production and replacement reasons. The second institutional change (Law no. 30/2003) mainly concerned apprenticeship contracts for young workers, increasing the age threshold up to 30 years of age and the duration of the contract. The last two labor market reforms, which became effective from 2005 onward, greatly contributed to the diffusion of temporary employment in Italy (Cappellari et al., 2012; d'Agostino et al., 2018).

Our paper adds to the growing microfounded literature on the relationship between job instability and fertility. Most of the previous research have investigated the objective economic uncertainties increased by job instabilities and estimated a negative impact of temporary employment on fertility (see, for example the review by Alderotti et al. 2021), although the findings produced were sometimes mixed, at least in European countries. For example, Wolbers (2007) found that workers hired with a temporary or a permanent contract have comparable probability of having “their first child”. Similarly, de Lange et al. (2014) found that the birth of the first child in the Netherlands was not affected by temporary employment, while (Gebel and Giesecke, 2009) found that fixed-term contracts in Germany did not affect fertility significantly. Differences in fertility behavior have been found to depend on a country’s institutional arrangements, which includes low levels of welfare. Barbieri et al. (2015) attribute the negative impact of temporary employment on demographic decisions to the inefficiencies of southern European welfare systems.¹

A further contribution this paper makes in explaining the effect of labor market reforms on fertility evidence is the couple-oriented approach that considers the job characteristics and employment (in)stability faced by both partners. As evidenced by Vignoli et al. (2019), the couple-level approach is rare in the literature, and it is only recently that a few studies have analyzed the interaction between employment instability and fertility/intentions from a couple’s perspective. We take advantage of the possibility to work on couple’s data by using the AD-SILC database from 2003 to 2010. It merges the “IT-SILC” dataset (2005) provided by the Italian Institute of Statistics (ISTAT) with the administrative archives of the Italian Institute of Social Security (INPS), which is a representative sample of Italian work histories. Thus, this source represents probably the most innovative feature of the paper and differentiates our study from recent similar attempts.

Another key aspect of our paper is the methodological approach devoted to solving endogeneity issues due to the sample selection and reverse causality. According to Balbo et al. (2013), a major obstacle in the line of research at the micro-level when studying the correlation between life course circumstances, which include temporary employment and fertility behavior, is “the challenge to establish their causality, or in other words, the ability to empirically determine whether these life course factors are actual determinants of fertility or whether certain life course factors and fertility behaviors are simultaneously affected by the presence of other common determinants”. In this paper, we provide causal evidence of the role played by the extension of temporary contracts in the labor market on fertility in Italy, identifying the underlying mechanisms at play through the gradual application of the two labor market reforms. We contribute to the economic literature and share

¹ Empirical evidence suggests that some measures, such as stronger legislation in employment protection for women, childcare facilities and parental leave benefits, may affect fertility and female labor market participation (Prifti and Vuri, 2013; Del Boca, 2002).

some methodological similarities with Cappellari et al. (2012), Picchio and Staffolani (2019) and d'Agostino et al. (2018), who have estimated the effect of labor market reforms in causal terms, although outcomes are directly addressed to employment and careers. We also add our results to the demographic literature on fertility, as in Vignoli et al. (2020), who use a causal approach to estimate the effect of entering the labor market with a temporary contract on first birth postponement in Italy, or (Barbieri et al., 2015), who relies on event history analysis and simultaneous equation models to test the impact of atypical contracts on the transition to motherhood in Italy.

While Section 2 summarizes the economic and demographic literature on the relationship between temporary contracts and fertility behavior and the underlying channels, Section 3 presents the empirical strategy adopted to estimate the effect of the new legislation on temporary contracts (i.e. fixed-term and apprenticeship contracts) on the probability of women having “any child” (or having “their first child”). The identification strategy exploits the exogenous source of variation determined by the gradual implementation of the new fixed-term and apprenticeship contracts that were conditional upon the renewal of collective agreements and specific regulations by each Italian region, respectively.

Section 4 provides some descriptive statistics extracted from the AD-SILC database to implement the reasoning behind our paper, while in Section 5 we estimate the difference-in-difference (DD) model, using a propensity score matching estimator (PSM) to cancel the bias due to differences in observable characteristics between women in the new regimes of temporary contracts and women not affected by the reforms (i.e., control groups). We show that the new labor market regime explains the reduction in the probability of having “any child” by about 1.5 percentage points, an impact that rises to around 3 percentage points for fixed-term contracts. The magnitude of our point estimates is slightly larger when we investigate the effect of having “their first child”, irrespective of whether we use temporary contracts or fixed-term contracts only. Our estimates are robust to a large number of sensitivity checks. We also examine the heterogeneous impact on childbearing by comparing the baseline results with those conditional to the employment status of the partner. We find a significant reduction in fertility when the partner has a temporary job, while the results are unaffected when the partner is involved in a permanent job. Consistently, a positive income effect of the partner seems to offset the negative impact of employment instability – and economic uncertainty onset – on fertility.

The final part of our analysis (Section 6) explores the underlying mechanisms behind our results. We find that temporary contracts became a strong trap for women by hindering their job-career trajectories, which generated an increase in economic uncertainty identified by the rise of wage volatility, the reduction in the probability of family formation and postponement of childbearing. Section 7 concludes by focusing on certain limitations within our analysis.

2. Temporary employment and fertility

The causes of a rise in the incidence of temporary contracts in Western countries have been largely discussed in the economic literature.² The most important need that temporary job contracts meet are changes in firms’ demand for flexibility of employment which, net of truncated working careers, offers lower levels of wages and protection on average, and may create more room for economic uncertainty (Schmitt, 2012). Furthermore, a person’s lack of permanent employment increases anxiety and fear and could adversely affect her quality of life and well-being. However, some advantages of temporary contracts have been stressed in the literature, namely that they increase job chances for active workers who would not have any chance of

² See, for example, ter Weel (2018).

entering the labor market or improve job satisfaction for highly skilled workers (Guest et al., 2006).

The increasing recourse to temporary jobs, which may generate job instability in some countries with inefficient institution arrangements, and the rise of economic uncertainty has affected the demographic sphere, determining a negative impact on family formation and, consequently, an expected decline in fertility.

Barbieri et al. (2015) show that, in some southern European countries, such as Spain and Italy, job instability contributes to postponing fertility choices. Again, Guner et al. (2019) used a life-cycle model to analyze the linkage between the labor market and fertility decisions of married women in Spain. Through this framework, they demonstrate that policy changes aimed at reducing duality in the labor market and job fragmentation can increase total fertility in college-educated women. In addition, these reforms reduce childbearing postponement. Furthermore, through micro-level high-quality retrospective data, Alderotti (2022) examines how the transition to parenthood is associated with employment status in Italian regions. He finds that unemployed women are more likely to become mothers in the South of Italy, and that temporary employment has a negative impact on the probability of having the first child in the North regions only.

The extension of temporary contracts caused penalties in wages and large volatility, which is often used to explain (negative) fertility behavior. According to the standard microeconomic model of fertility, the average wage difference between women employed in temporary and permanent jobs decreases the demand for children (income effect) (Schmitt, 2012). Repeated episodes of temporary employment decelerates wage progression and increases the likelihood of future unemployment for women, which, over the employment life cycle, leads to more economic uncertainty (Adsera, 2004; De La Rica and Iza, 2005). Using a policy change that occurred in Portugal in 2003, Lopes (2020) shows that fixed-term contracts make it less likely that women will have children. They find that job security affects fertility decisions about the first child, while income affects subsequent birth decisions more. On the other side, Clark and Lepinteur (2022) found an exogenous rise in job insecurity for younger workers in large private firms that paid the “French Delalande tax” when they laid off workers aged over 50. In turn, the probability of having an additional child was reduced by about 4 percentage points because of this rise of job insecurity. Reduced fertility is only found at the intensive margin, such as job insecurity reduces family size.

Theory also suggests that temporary contract effects should also be examined at the couple level, and not just considering the decisions of women, as this is where fertility decisions are in most cases taken. Matysiak and Vignoli (2008) suggest that the negative effect of women’s employment on fertility can be overestimated by omitting the partner’s occupation. For example, Vignoli et al. (2012) found that when both parents are employed with permanent contracts, fertility increases, while when they are employed on a temporary contract, the opposite effect is found. Instead, if only the partner is employed on an open-ended contract, total fertility does not necessarily decrease (Kreyenfeld, 2009). More specifically, employment instability from temporary contracts matters in regard to the likelihood of having “any child”, but in a gendered way: the man’s position in the labor market is generally found to be more important in South Europe and Italy than the women’s in informing a couple’s decision to have “their first child” (Bernardi et al., 2006) or having a second child (Mencarini and Tanturri, 2006).

The temporary contract reforms may also lead childless women to put off having children, with negative consequences on total fertility. Santarelli (2011) looked at how childless married couples transit to motherhood in Europe. She found that dual-earner couples postpone the decision of having “their first child” when compared to single-earner couples, although the type of contract seemed to be less important. Barbieri and Scherer (2009) show that a stable and secure job is a key factor for family formation in Italy, while temporary jobs

make it less likely that women will have “their first child”. Career-minded women especially, who invest more in their human capital and expect greater returns in terms of wages and economic security, will be more likely to put off their decisions on having children when they have temporary job contracts (Beaujouan and Berghammer, 2019). Auer and Danzer (2016) provide comprehensive evidence that women with fixed-term jobs tend to postpone their first child. By using the German Socio-Economic Panel, they show that fixed-term contracts not only significantly increase the probability of delay decisions about having their first child, but also reduce the number of children within the first 10 years after graduation. van Wijk et al. (2021) show that temporarily employed women delay having their first child by using data from a large-scale survey among Dutch employees. Investigating the role of the Job Act reform in Italy³, De Paola et al. (2020) show that temporary contracts may explain the probability of postponing childbearing, although (Modena et al., 2014) did not find a negative impact on childbearing decisions for less educated women.

3. The empirical model

3.1. Identification

We are interested in estimating the causal effect of the extension of temporary job contracts on the fertility behavior of working women in Italy. We focus on the average of this effect for women who were gradually exposed to fixed-term contracts following Italian Law 368/2001 or, to both fixed-term and apprenticeship contracts (D.L. 276/2003)⁴, exploiting their progressive implementations. We choose to investigate the effect of these reforms on women employed in new temporary contracts compared to all other types of jobs (CG_{all}) and not only for the specific pre-post effects in the temporary work law, because the specific changes to new temporary contracts have spillover effects on labor demand.

Collective agreements had to be renewed before the new fixed-term contracts could be adopted. The renewal process was gradual, and only textiles, wood products, chemicals, construction, transportation, retail trade, food products and telecommunications renewed collective agreements in 2005 and 2006. Metal Manufacturing and banking renewed their collective agreements during that period, but decided to postpone the implementation of the new fixed-term contract to a successive agreement in 2009. Similar approaches can be found for apprenticeship contracts as well. Only the governments’ intervention in 2005 stated that collective agreements at sector level could specify the training content of contracts, without regional regulations. This created two tracks before the new apprenticeship contract could be adopted: one covered regional guidelines, and the other was done by sector-specific collective agreements. The exposure to the reform of apprenticeship contracts occurred mostly through the staggered adoption of regional regulations implementing the national legislation (d’Agostino et al., 2018).⁵

³ The Job Act reform has essentially reduced employment protection for the employees of large firms and left the protections of small firm employees largely unchanged.

⁴ The so-called Biagi Law.

⁵ Besides Emilia-Romagna and Tuscany in 2005, Friuli, Marche, Sardinia, the autonomous province of Bolzano in 2006 and Latium in 2007 introduced specific regulations concerning the application of the apprenticeship contract. Other regions introduced only experimental regulations in certain sectors. Furthermore, besides some sector-specific collective agreements that introduced apprenticeships-specific regulations in 2005 (i.e. textiles, wood products, chemicals, construction, transportation, retail trade, banking and food products), metal-manufacturing and telecommunication collective agreements introduced them in 2006, while tourism and private insurance did so in 2007.

3.2. Estimation

We use a linear probability model to analyze the effect of the introduction of temporary contracts on fertility behavior and employ a propensity score matching estimator (PSM) in a DD framework.⁶ If female workers were exogenously assigned to these groups, the causal effect of the new apprenticeship or fixed-term contracts on fertility behaviors could be identified by least square estimation of the following specification:

$$Y_{it} = \alpha_B d_{it}^B + \delta_t + \sum_s (\zeta_s + \mu_{s,t}) Z_i^s + \sum_r (\zeta_r + \mu_{r,t}) Z_i^r + \tau' \mathbf{X}_{it} + \epsilon_{it}, \quad (1)$$

where Y_{it} is a fertility measure as a realization of the maternity event during year t . Our variable of interest is d_{it}^B , defined as a dummy taking the value “1” if woman i has a fixed-term contract or both fixed-term and apprenticeship contracts after the application of the labor market reforms, and “0” otherwise. Thus, the parameter, α_B , denotes whether fertility outcomes decrease for women in the new temporary job regime. In addition, δ_t is a time-fixed effect with $t = 2003, \dots, 2010$, Z_i^s and Z_i^r are dummy variables for regions and sectors, so that the ζ coefficients capture regional (r) and sectoral (s) fixed effects, while the μ coefficient vector captures region and sector-specific time trends, \mathbf{X}_{it} is a vector including control variables and a constant, and ϵ_{it} is an error term.

Without the reform, α_B would be zero. This means that, on average and conditional to \mathbf{X}_{it} , childbearing measures should have followed similar trends, regardless of whether they were affected by the reform or not, and the combination with the PSM estimator ensures that women affected by the labor market reforms are compared with women not affected, who are similar according to observable characteristics. Generally, women entering the labor market after the reforms are unequal in terms of the characteristics that may be related to fertility outcomes. For instance, our results may be driven by changes in the fertility rate in small sets of pregnant women with respect to the overall foreign population. To accommodate for the fact that there may be differences in fertility rates due to observed characteristics between the two groups before and after the labor market reforms, we weigh our estimates by using the PSM method.

Although this approach reduces the endogeneity issues, as the balanced large set of covariates accounts for fertility behavior and its variations between time periods, we cannot exclude some forms of reverse causality or self-selection of women into one of the temporary regimes created by the labor market reform. In this case, the presence of unobserved heterogeneity may cause a bias of our baseline estimates, comparing affected women in temporary contracts (i.e., treatment group: TG_{all}) with unaffected women (i.e., control group: CG_{all}). For instance, women who are planning of having a child in the near future may be more likely to accept a temporary job as they seek less demanding jobs and careers, therefore the significant presence of these women in TG_{all} may overestimate the (negative) impact of temporary contracts on fertility (downward bias).⁷ On the other hand, preferences for their careers and less willingness to assume parental duties of women in TG_{all} may underestimate the negative effect of the labor market reforms (upward bias). In order to take into consideration these issues, we created a new control group composed of women who were involved in the temporary contract reforms in successive years. Although the sample was reduced consistently, the fertility variation of

“next period” temporary contracts for women (CG_{next}) was considered to be the most appropriate control group for the fertility variation of TG_{all} , as they have the most similar unobservable characteristics according to future fertility behavior. Using the same framework, we carried out point estimates for the fixed-term contracts (i.e., treatment group TG_{ft}).

The proposed identification method may also be questionable for the anticipation effects of Law 247/2007, which introduced changes to the maximum duration (i.e., three years) for fixed-term contracts stipulated with each employer, which was applied on April 1, 2009. To address this issue, we will perform a robustness analysis, restricting the length of our baseline analyses up to the year 2008.

4. Data

We exploit the “AD-SILC” micro-level data by merging a longitudinal dataset drawn from the administrative records maintained by the Italian Social Security Institute (INPS) with the 2005 cross-section dataset “IT-SILC (2005)”, provided by the Italian Institute of Statistics (ISTAT). The “AD-SILC” dataset includes the working histories of around 56,000 workers from 1980 up to 2010 and provides, yearly, information on job attributes (such as national insurance contributions, contract duration and maternity leave) and some basic demographic information on workers’ characteristics (such as gender, year of birth, place of residence). Other demographic characteristics, available for 2005 only, include marital status, education level and household composition.⁸

To select the sample, we exclude self-employed and semi-subordinate female workers, since the dataset reports only partial information for these workers. When women had jobs in different firms during the same year, we use the information on social security contributions paid weekly and wages to isolate the main activity from the secondary job. We limit our dataset and consider the female population in the fertile age 15–49.

In order to build our fertility outcomes, we record from the dataset the number of consecutive weeks spent on maternity leave which, in Italy, typically covers the two months before the date of childbirth and the three months following the birth (i.e., compulsory maternity leave) and isolate successive parental leave periods after childbirth paid by INPS. This approach overcomes overestimation of fertility when women are not at work for a period longer than the compulsory maternity leave.⁹ We build the variable “maternity leave” equal to one for women on corrected compulsory maternity leave in each year (and zero otherwise). We then use the number of children recorded in 2005 from the “IT-SILC” dataset to construct two different fertility outcomes related to the transition to parenthood (women having “their first child”) and a more general measure of fertility as women having “any child”. While the short time span after the new rules in the labor market constrained a complete assessment of higher orders of births, heterogeneous point estimates between the two indicators may suggest different mechanisms in the decision of having their first child or an additional child. The final sample size includes 39,728 observations (4966 women between 2003 and 2010).

Then we recorded the employment status of women (and men), distinguishing between the unemployed, those on temporary contracts and those on permanent contracts, before and after the application of

⁶ Even though the outcome of interest is a limited dependent variable, as suggested by Angrist and Pischke (2009, page 69-73), we use the OLS estimator in the linear regression to approximate the conditional expectation function, which measures the effect of the policy change directly in terms of average treatment effects (ATE).

⁷ Such unobserved woman-specific heterogeneity could include positive preferences for entering into parenthood, in particular in the case of the first child.

⁸ The AD-SILC dataset shows a detailed picture of how working conditions have changed over time for a large group of people and looks at the whole working history of each person since their first job, but unfortunately does not give any information about the sample weights. A discussion of the sample design and the fiscal code procedure to merge the two datasets is given by Vignoli et al. (2019).

⁹ Employees have the option of putting their maternity leave off until one month before birth of the child, continuing four months after. Early maternity leave is available for reasons related to health and safety during pregnancy.

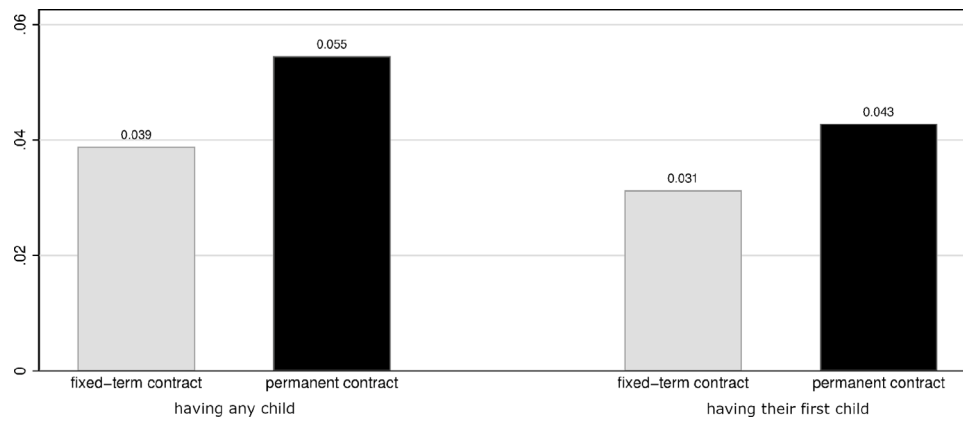


Fig. 1. Probability of having “any child” or “their first child” by women’s employment contract.

Table 1
Descriptive statistics.

		Any child			First child		
		<i>Cg_all</i>	<i>TG_all</i>	<i>TG_ft</i>	<i>Cg_all</i>	<i>TG_all</i>	<i>TG_ft</i>
Education level							
	Primary	4.96	3.51	3.46	4.44	3.84	4.99
	Secondary	86.42	92.94	92.27	85.88	92.60	91.69
	Tertiary	8.62	3.56	4.29	9.68	3.56	4.52
Marital status							
	Single	31.48	11.23	25.44	22.86	26.7	38.11
	Unmarried couple	23.79	73.52	48.08	6.34	17.95	48.58
	Married	44.40	15.11	27.05	70.33	54.72	20.57
	Divorced or widowed	0.33	0.13	0.24	0.46	0.64	0.05
Age classes							
	15–20	5.41	20.72	11.95	0.38	2.27	16.35
	21–25	13.09	35.75	24.32	2.98	16.97	30.81
	26–30	18.58	21.85	22.37	12.17	25.68	22.81
	31–35	21.65	10.29	18.27	26.83	23.05	14.46
	36–40	21.07	6.39	12.94	30.97	17.97	8.73
	41–45	15.96	3.96	8.03	21.62	11.80	5.06
	46–49	4.25	1.05	2.12	5.05	2.27	1.78
Italian citizenship		93.38	95.53	95.60	90.46	86.94	97.47
Illness leave		7.73	6.17	6.49	9.78	8.53	5.74
Child and family care		10.60	3.37	3.87	15.93	9.53	9.03
Part-time		15.50	9.76	14.29	20.60	15.98	12.63
Unemployed		7.55	12.20	14.84	6.90	18.26	13.26
First job seeker		3.25	6.91	5.77	0.85	3.20	6.80
Still in the educational system		12.75	5.96	9.34	18.06	13.33	6.41
Homemaker		13.74	29.81	23.90	2.27	6.30	31.81
Experience in the labor market		6.77	9.41	7.22	6.30	9.81	5.35
Women labor market conditions*							
	Not-employed	0.278			0.319		
	Temporary contract	0.175			0.208		
	Permanent contract	0.546			0.472		
Partner labor market conditions							
	Not-employed	14.05	21.33	15.64	9.03	12.03	13.08
	Temporary job	7.03	7.85	8.49	7.55	12.20	11.07
	Permanent job	78.92	70.82	75.87	83.42	75.76	75.86
Observations		37,967	1,761	697	15,596	403	194

Notes: *CG_all* are women not affected by the reforms. *TG_all* includes women affected by the fixed-term contract reform (Law 368/2001) or the apprenticeship reform (D.L. 276/2003), while *TG_ft* only includes women affected by the fixed-term contract reform (Law 368/2001).

the labor market reforms. For their large use, we recorded fixed-term contracts separately. To provide a first impression regarding the data used, in Fig. 1 we show the sample of the descriptive statistics for the two fertility outcomes, by disentangling between women in permanent and fixed-term contracts. On average, women with permanent contracts had a higher probability of having “any child” than women on temporary contracts (+1.6 percentage points); this result is in accordance with the frequencies related to having “their first child” even though, in the latter case, the magnitude is slightly smaller (+1.2 percentage points).

Table 1 lists the descriptive statistics of the time-varying and time-invariant covariates used in the estimation to control for observable

heterogeneity, which distinguishes between women who were affected or unaffected by the temporary contract reforms, or only by the new fixed-term contracts. Women involved with the new labor market regulations have different frequencies in many covariates with respect to unaffected women, irrespective of the fertility outcome. For example, women affected by the temporary contract reforms are younger, less educated and living as a couple outside of marriage. As argued above, sample differences between women affected by the new temporary contract law, compared to women who are not affected, are preliminary weighted by the inverse propensity score matching.

Table 2
Probability of having “any child” (or “their first child”) by women’s employment contract.

	Cg_{all}	TG_{all}	t_{test}	P_{value}
Probability of having “their first child”				
Temporary contracts	0.041	0.024	3.452	0.00
Fixed-term contracts	0.040	0.013	3.347	0.00
Probability of having “any child”				
Temporary contracts	0.058	0.045	1.614	0.05
Fixed-term contracts	0.058	0.030	1.928	0.03

Notes: Temporary contracts include fixed-term and apprenticeship contracts. Cg_{all} : includes women not affected by the reforms; TG_{all} : includes the women affected by the fixed-term contract reforms.

5. Results

5.1. Baseline estimations

We present the main findings of the relationship between temporary jobs and fertility outcomes by using the model specification described in the previous section. As a preliminary, Table 2 presents the descriptive statistics for fertility changes in women who were affected by the labor market reforms and those who were not affected, distinguishing between women having “their first child” or more generally, having “any child”. What we find is that women who are not affected by the labor market reforms show a higher probability of having “any child” (or “their first child”) with respect to those affected by the new temporary contracts.

All the tables report the results when both the fixed-term and apprenticeship contracts of women are accounted for, as well as for fixed-term contracts only. The left part of Table 3 shows *DD* estimates of having “any child” for women affected by the labor market reforms, with the corresponding number of observations; the right part lists estimates for having “their first child”. The reported error terms are clustered at the individual level.

The estimates indicate that the exposure to the new temporary contract laws decreases the probability of having “any child” by about

1.5 percentage points, which falls to almost 3 percentage points when we consider only women affected by the new fixed-term contract [−0.029; s.e. (0.006)]. The estimated effect for women in the new temporary jobs who are having “their first child” is also larger, decreasing by more than 4 percentage points [−0.042; s.e. = 0.015]. Similarly, having “their first child” after the new fixed-term contract appears to be less likely, although the point estimate reveals a statistical significance at the 90 percent confidence level (−0.040; s.e. = 0.021). This result leaves open the possibility that heterogeneous groups of affected women may have different significant effects.

Table 4 shows the estimated coefficients obtained using CG_{next} as the control group, which has women in temporary jobs with similar initial conditions to those of current workers affected by the reforms. From the Table, we note that the difference with respect to our baseline results is quite limited. Only when we consider the fertility outcome “having their first child”, these point estimates seem to lose significance, likely due to a strong restriction of the sample. Thus, we conclude that the bias correction eventually induced by other unobservable variables appears to be limited.

5.2. Robustness checks

Next, we compare our results against several robustness exercises, falsification tests and threats to the quasi-experimental design.

5.2.1. Expectation effects of other labor market reforms

The first issue is the threat to the quasi-experimental design of the labor market reforms on fertility outcomes due to Law 247/2007. This new regime of fixed-term contracts, which only came into force on April 1, 2009, may affect our baseline results because it reduced the maximum duration for fixed-term contracts stipulated with each employer by three years. This reduction in duration may have led firms to anticipate contracts with a longer duration between 2007 and 2009 and, in turn, this effect may have determined a reduction in uncertainty and a positive effect on childbearing.

Table 5 lists the estimates for fixed-term contracts of the restricted sample (2003–2008). The results suggest that the effects of the sample’s

Table 3
Effects of labor market reforms on the probability of women having “any child” and “their first child” (benchmark model).

	Probability of having “any child”		Probability of having “their first child”	
Temporary contract	−0.015 (0.005)	***	−0.042 (0.015)	***
Fixed-term contract	−0.029 (0.006)	***	−0.040 (0.021)	*
R^2	0.049	0.049	0.109	0.109
Observations	26,604	25,444	11,282	11,156

Notes: Temporary contracts include fixed-term and apprenticeship contracts. Robust standard errors are in parentheses. The asterisks stand for the *p-value* significance levels (* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$). Estimates are performed including the set of covariates X_{it} described in Table 1.

Table 4
Effects of labor market reforms on the probability of women having “any child” and “their first child”, using “next period” temporary contracts for women (CG_{next}) as the control group.

	Probability of having “any child”		Probability of having “their first child”	
Temporary contract	−0.017 (0.006)	***	−0.043 (0.024)	*
Fixed-term contract	−0.029 (0.008)	***	−0.036 (0.020)	*
R^2	0.080	0.081	0.228	0.225
Observations	4,303	4,115	891	844

Notes: Temporary contracts include fixed-term and apprenticeship contracts. Robust standard errors are in parentheses. The asterisks stand for the *p-value* significance levels (* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$). Estimates are performed including the set of covariates X_{it} described in Table 1.

Table 5
Robustness checks. Estimates of the benchmark model in the restricted sample 2003–2008.

	Probability of having “any child”		Probability of having “their first child”	
Temporary contract	−0.012 ** (0.006)		−0.049 ** (0.019)	
Fixed-term contract		−0.023 *** (0.007)		−0.039 * (0.023)
R ²	0.058	0.059	0.405	0.144
Observations	19,649	19,443	8,264	8,091

Notes: Temporary contracts include fixed-term and apprenticeship contracts. Robust standard errors are in parentheses. The asterisks stand for the *p*-value significance levels (* *p* < 0.1; ** *p* < 0.05; *** *p* < 0.01). Estimates are performed including the set of covariates X_{it} described in Table 1.

restriction do not affect the probability of having “any child” compared to the baseline estimations in different ways. Also, the point estimate of the probability of having “their first child” is in accordance with the baseline estimations, denoting that the further reduction of the timing for fixed-term contracts of Law 247/2007 did not reveal any role in changing the fertility decisions of childless women.¹⁰

5.2.2. Placebo regressions and implementation of temporary contract reforms

The estimated results may be affected by the choice of the control group. While women not influenced by the temporary contract reforms represent a natural control group for those involved in the new labor market regulation, we want to make sure that the plausible composition effect over the time of the analysis does not drive the results.

For this reason, we randomly draw 10,000 samples of women not involved in the reforms (control group) and re-run the estimates, pretending that each of these placebo samples is the true group of women involved in the new temporary contracts (or only for fixed-term contracts). We extract these samples for the two fertility outcomes and the relative women sample size (1761 for the probability of having “any child” and 289 for the probability of having “their first child”).

Fig. 2 shows that the densities of the placebo effects are centered more or less around zero, although we note a slight asymmetry on the left. This suggests that, although not statistically relevant, a small reduction in the fertility outcomes experienced by the control groups took place after the labor market reforms. The estimated effect for the true treated group (vertical line) is significantly different from zero for “their first child” and “any child” outcomes, as the confidence intervals does not overlap the zero of the control group distribution.

We also run a series of falsification tests at different placebo dates, so as to exclude that the *DD* point estimates capture the effects of other events besides the labor market reforms of temporary contracts. Above, we have discussed how the labor market reforms of fixed-term and apprenticeship contracts came into force gradually from the year 2005, involving different sectors and regions in the successive two years. Thus, we performed the baseline estimates choosing a lagged or forwarded year, as the change in the date of its introduction should lead to non-significant estimates of the probability of having “their first child” or having “any child”. That is, if we do not find that the estimated reduction in fertility outcomes is highest when the threshold is set at the true date of introduction in which women are affected by

¹⁰ Point estimates in the benchmark model may be biased since our sample incidentally overlaps with the onset of the Great Recession and economic difficulty for families. However, the negative effects in the Italian real economy started in 2009, such that our restriction used in this robustness analysis is sufficient to show that the results are in line with the prediction of the benchmark. Following the suggestions of a reviewer, we have also extended the model to include dummy variables for the last three years. Again, the point estimates are close to those of the benchmark model, although the standard errors are a little larger. These estimates are available upon request from the authors.

Table 6
Estimates of pre-reform trends in fertility outcomes of affected and unaffected women.

	Probability of having “any child”	Probability of having “their first child”
$\hat{\beta}_3$: temporary contract	0.000 (0.003)	−0.001 (0.015)
R ²	0.057	0.120
Observations	23,719	10,731
$\hat{\beta}_3$: fixed-term contract	0.002 (0.004)	0.002 (0.018)
R ²	0.056	0.121
Observations	22,685	10,619

Notes: Temporary contracts include fixed-term and apprenticeship contracts. Robust standard errors are in parentheses. The asterisks stand for the *p*-value significance levels (* *p* < 0.1; ** *p* < 0.05; *** *p* < 0.01). Estimates are performed including the set of covariates X_{it} described in Table 1.

the temporary reform, other unobservable variables may be affecting fertility patterns, casting doubt on the effectiveness of the identification strategy.

Fig. 3 shows the estimated parameters associated with temporary contracts on fertility outcomes (Eq. (1)), by lagging and forwarding the date of introduction of the labor market reforms. We find that, consistent with our expectations, the only significant change in the key variables of our model appears precisely in the true coefficient of the fertility impact, the time in which the reform was introduced.

5.2.3. Parallel pre-trends in fertility outcomes

In this robustness section, we checked for the presence of a common trend in the pre-reform period, as formalized in Eq. (2). The robustness test is implemented by estimating a linear time-trend model which uses only women from the pre-reform period and allows for the interaction of the linear trend with the exposed women. We can conclude that both the affected women and the control groups were following the same trend before the reforms came into force, if no significant differences are found. We can briefly write:

$$Y_{it} = \beta_1 * pre\ reform_i + \beta_2 * trend_t + \beta_3(pre\ reform * trend_t)_{it} + \mu_{it} \quad (2)$$

where β_1 are women from the pre-reform period, indicating “1” those that successively have a treatment with the new reforms and “0” the others; β_2 measures the effect of the linear trend and β_3 measures the interaction between the linear trend and women successively affected by the reforms, directly measuring differences in specific treatment and control trends.

The results are listed in Table 6. For each of the fertility indicators, we do not find evidence of a significant trend in the pre-reform period between the two groups of “affected” and “unaffected” women. The baseline assumption of a common pattern is not rejected and allows us to conclude in favor of our identification strategy or at least not in contrast to it.

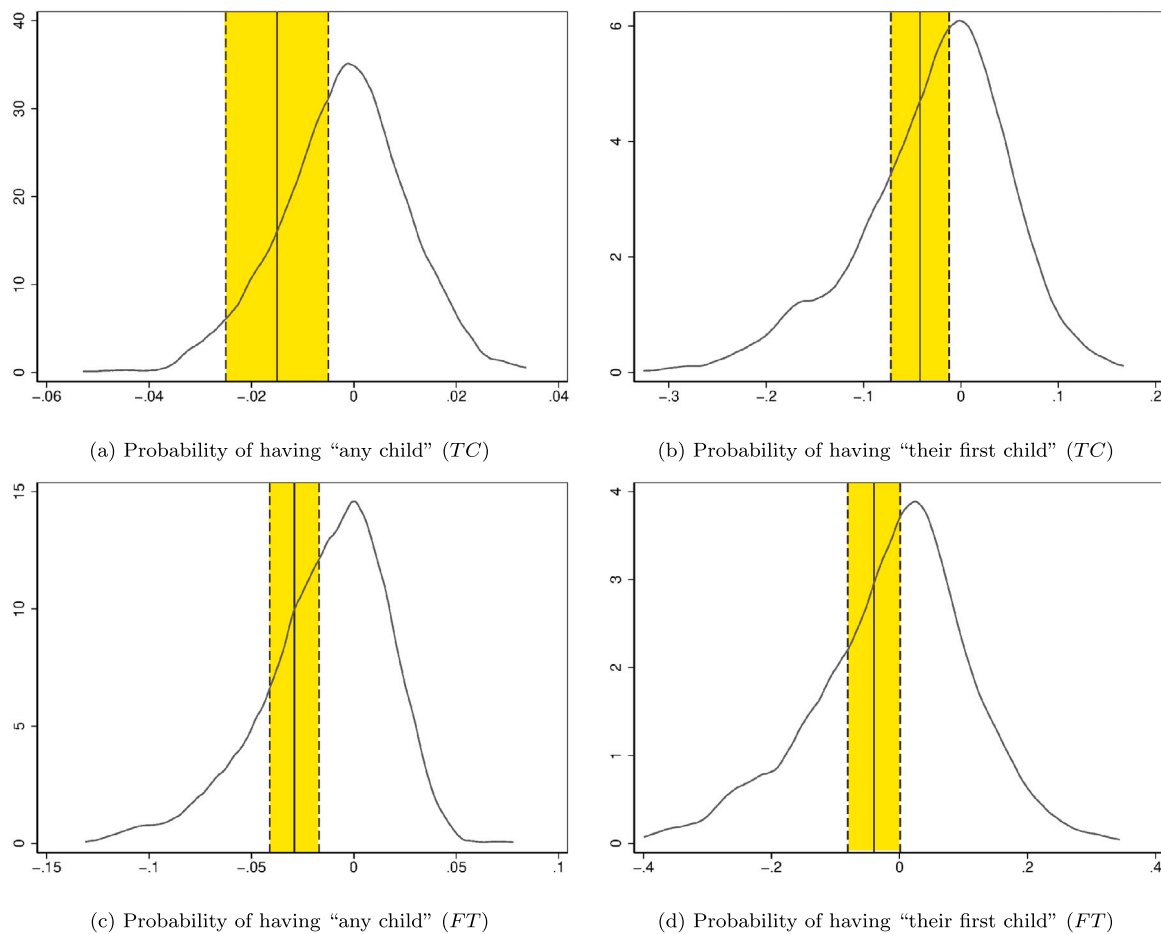


Fig. 2. The density of placebo effects.

Notes: Placebo samples for temporary contracts (TC) and for fixed-term contracts only (FT). The shaded areas are the confidence intervals.

Table 7
Robustness checks: Estimates of the benchmark model in the restricted sample of mothers aged 15–40.

	Probability of having “any child”		Probability of having “their first child”	
Temporary contract	-0.014 ***	(0.005)	-0.040 **	(0.016)
Fixed-term contract	-0.025 ***	(0.006)	-0.036	(0.024)
R ²	0.052	0.052	0.125	0.124
Observation	21,597	20,655	8,335	8,244

Notes: Temporary contracts include fixed-term and apprenticeship contracts. Robust standard errors are in parentheses. The asterisks stand for the *p*-value significance levels (* *p* < 0.1; ** *p* < 0.05; *** *p* < 0.01). Estimates are performed including the set of covariates X_{it} described in Table 1.

5.2.4. Sensitivity analysis of mother’s age

In order to identify the number of children in the household, we use the household survey (*IT – SILC*). As a limitation, the SILC dataset only contains information on household members and therefore children are only observed if they have not left the household yet. Thus, we could expect that for older women (i.e., those close to 49) children who had already exited from the household were not assigned because we did not observe them.

In this respect, in Table 7 we report estimates of the benchmark model in a restricted sample of mothers aged 15–40 to show that the results presented in Table 3 are not driven by measurement errors related to a misreporting of the number of children in the household. Table 7 shows that all the estimated parameters are close to the benchmark model. The only relevant difference concerns the fixed-term

contract effect on the probability of having a first child, which loses significance.¹¹

5.3. Heterogeneous effect of the labor market reforms on fertility: the partner’s employment

In this subsection, we extend point estimates of our baseline specification to the probability of having “any child” or “their first child”, considering the employment status of the women’s partners. The *DD* model shown in Eq. (1) is extended to define three subgroups of the

¹¹ The significance value of the same parameter in the benchmark model (Table 3) was very close to a 10% significance level.

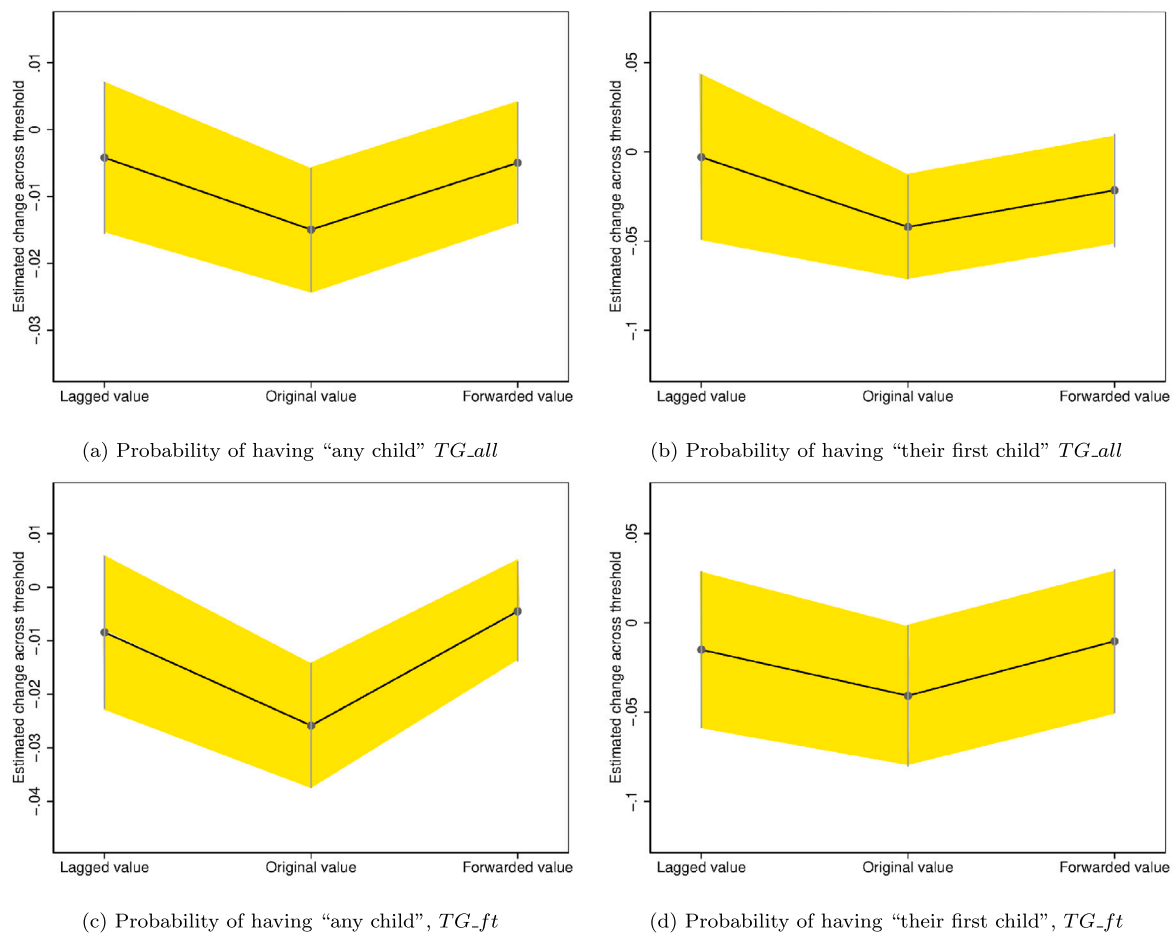


Fig. 3. Placebo tests on the identification.

Notes: TG_{all} : Samples from temporary contracts on different dates; TG_{ft} : Samples from fixed-term contracts on different dates. The shaded areas are the confidence intervals.

population. The first subgroup includes exposed women on temporary contracts or fixed-term contracts only, whose partner is unemployed; the second subgroup includes exposed women whose partner is employed on a temporary contract and, the third, with a permanent contract.

Table 8 shows the results. Looking at the effect of temporary jobs on women with unemployed partners, point estimates of the probability of having “any child” are generally significant, close to the baseline estimates. In addition, having an unemployed partner reduces the probability of women having “their first child”, with a point estimate that changes from -4 to -5.5 percentage points for fixed-term contracts. Compared with mothers who have partners employed on temporary contracts, the negative effect is significant and stable in regard to the probability of having “any child”, while this effect becomes non-significant when we measure the effect on the probability of having “their first child”. Clearly, the employment of both mother and father improves job expectations, reduces income uncertainty, and does not delay fertility. This result is strengthened for the subsample of mothers with partners in a permanent job. The positive gradient of job and income security for women with a partner in a permanent job does not negatively affect fertility outcomes when women are involved in temporary jobs, irrespective of whether we consider childless women or not.

Overall, this result affects women differently. It is worth noting that different fertility behavior arises from the choice of having “any child” or “their first child” in the estimations. As expected, when the partner is not employed, the reduction of having children, irrespective of the child order, is large compared to the benchmark model and statistically significant. Instead, temporary contracts for both women and their

partners, while reducing the probability of having “any child”, do not decrease the probability of having “their first child”. That is to say that doubling the income earners of the family allows for the couple to plan and for childless women to achieve their desire to become a mother, as the economic risk is shared within the couple.

6. Disentangling the channels of influence

Which plausible channels could be underlying the relationship between temporary contracts and fertility? The labor economics-fertility literature discussed in the second section suggests some potential mechanisms for explaining the negative impacts that we will summarize here, which may affect childless women differently with respect to those who have already had at least one child: the increase in wage volatility, the detrimental effects on family formation and, more specifically for “their first child”, postponement of parenthood.¹² However, the magnitude of these recognized non-contextual channels depends on how the new labor market regimes are linked with the country institutional setting. For example, in Nordic countries, the welfare system largely assists workers who have lost their job or women who become pregnant, favoring a speedy return to employment, also through an efficient system of employment offices. In this case, job instability from temporary employment is not expected to increase (or makes less significant) economic uncertainty. On the other hand, countries

¹² Clearly, the proposed channels are not exhaustive of the potential mechanisms behind the detrimental effects of temporary jobs on fertility. See, for example, the effect of house prices on fertility (Creina and Ross, 2016).

Table 8
Effects of labor market reforms on the probability of having “any child” and “their first child”, conditional upon the partner’s employment contract.

	Probability of having “any child”		Probability of having “their first child”	
Not-employed partner				
Temporary contract	−0.014 (0.006)	**	−0.049 (0.020)	**
Fixed-term contract		−0.029 (0.008)	***	−0.055 (0.021)
R^2	0.055		0.206	0.206
Observations	14,151	13,534	4,327	4,270
Partner with a temporary job				
Temporary contract	−0.020 (0.010)	**	−0.019 (0.031)	
Fixed-term contract		−0.041 (0.021)	**	0.004 (0.030)
R^2	0.220	0.220	0.397	0.397
Observations	2,601	2,488	1,050	1,011
Partner with a permanent job				
Temporary contract	−0.002 (0.012)		−0.037 (0.033)	
Fixed-term contract		−0.024 (0.015)	*	−0.017 (0.048)
R^2	0.099	0.099	0.180	0.180
Observations	9,852	9,422	5,905	5,834

Notes: Temporary contracts include fixed-term and apprenticeship contracts. Robust standard errors are in parentheses. The asterisks stand for the p -value significance levels (* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$). Estimates are performed including the set of covariates X_i , described in Table 1.

with a weak institutional setting associated with the extensive use of temporary contracts for employed women (or those searching for a job after maternity), generated less job opportunities and actually penalized them, with a reduction in contract duration and transition from temporary to permanent employment. Thus, a preliminary condition for identifying the channels through which economic uncertainty increases and explaining the detrimental effects of fertility behavior is to show that the new regimes of contracts in Italy increased the chance of remaining in a temporary job (i.e., job instability).

6.1. Estimating the probability of employment transition

The extension of the temporary contracts is assumed to have changed the transition of women in employment. Here, we use a marginal structural model (MSM) to analyze the probability of the transitions of women subjected to the two Italian labor market reforms. The relationship between the potential outcome $y^{(s)}$ and the treatment indicator s can be formulated as:

$$E[(y(t+1)|y(t))^{(s)}] = \gamma_0 + (s)\gamma_1, \quad (3)$$

where γ_0 is the potential outcome mean, γ_1 is the average treatment effect, and s is the exposure of a subject at time t to changes in temporary contracts or to fixed-term contracts only. We apply Eq. (3) to estimate a *three-state* model, which includes two different initial states t , *not-employed* and *temporary job* and three final states $t+1$, *not-employed*, *temporary job*, and *permanent job*.¹³ A linear probability model (LPM), which rationalizes the dynamic matching employment rules of *three-state* models, is then estimated.

Table 9 lists the marginal effects of the transition probabilities in a different work status for not-employed women exposed to the labor market reforms. Panel (a) of Table 9 shows that temporary contract

¹³ The effects of the labor market reforms are estimated using equation-by-equation methods and the parameters and variables are those reported in Eq. (3). According to the model specifications, although the reforms directly affect unemployed or temporary workers, they only have an indirect effect on permanent contracts.

reforms increase the hiring of not employed women by about 7 percentage points, which is larger in magnitude for fixed-term contracts (18 percentage points). Panel (b) lists the estimates of the transition probabilities for women that are already in temporary employment before the labor market reforms. Women exposed to the reforms increased the probability of remaining in this type of contract by 27.6 percentage points. For fixed-term contracts, the probability of remaining in the same contract increases by 38 percentage points. The reduction in permanent employment probability is offset by the 31 percentage-point increase in temporary employment probability. Using the new rules in temporary contracts, firms have found it convenient to replace permanent contracts with fixed-term or apprenticeship contracts. This implies an increase in the risk for female employees to be trapped in positions of temporary work in the long run (Modena et al., 2014).

The reduction in permanent employment probability is offset by the 31 percentage-point increase in temporary employment probability.

6.2. Mechanisms

A potential mechanism investigates whether temporary contracts directly lead to more economic uncertainty. Considering the peculiar characteristic of the Italian labor market, namely that it is affected by a high unemployment rate, this reduces the probability of people having a job once a temporary contract has expired. Following (Bonin et al., 2007), we estimate the degree of uncertainty in wages attached to fixed-term employment as a measure of volatility.¹⁴ That is, the variance of the residual part of a Mincer wage regression is estimated using individual monthly and annual wages of women. If the variance

¹⁴ We have excluded apprenticeship contracts from the analysis since apprentices’ wages are not comparable to the ones of a permanent contract in several respects. First of all, the apprentices contribute to training expenses by taking a wage lower than the wage floor defined by collective bargaining agreements. Secondly, the 2003 reform, liberalizing the training content of the apprenticeship contract, has introduced a minimum floor for apprentices’ wages, stipulated in collective bargaining agreements, to compensate apprentices for the removal of the investment in external training by firms.

Table 9
Effect of temporary contracts on the employment transition probabilities of women.

	Not – employed _(t+1)		Temporary job _(t+1)		Permanent job _(t+1)	
	(1)	(2)	(1)	(2)	(1)	(2)
Not – employed_(t)						
	-0.076 *	-0.102	0.069 **	0.183 ***	0.007	-0.081
	(0.041)	(0.067)	(0.029)	(0.054)	(0.036)	(0.052)
R ²	0.423	0.423	0.359	0.366	0.259	0.262
Observations	2,192	2,096	2,192	2,096	2,192	2,096
Temporary job_(t)						
	-0.065 *	-0.066	0.276 ***	0.381 ***	-0.211 ***	-0.315 ***
	(0.034)	(0.053)	(0.053)	(0.087)	(0.047)	(0.081)
R ²	0.379	0.379	0.319	0.323	0.315	0.320
Observations	1,969	1,883	1,969	1,883	1,969	1,883

Notes: Temporary contracts include the fixed-term and apprenticeship contracts. Robust standard errors are in parentheses. The asterisks stand for the *p*-value significance levels (* *p* < 0.1; ** *p* < 0.05; *** *p* < 0.01). Estimates are performed including the set of covariates *X_{it}* described in Table 1. Specification (1) uses a sample including women in temporary contracts, while specification (2) restrict the sample to women in fixed-term contracts.

Table 10
Variance ratio test for unexplained part of wages, by employment contract.

Statistics	Type of contract	Annual wages	Monthly wages
Mean values			
	Permanent	11,743	1,133
	Fixed-term	8,080	992
Variance ratio			
	Permanent	0.08	0.09
	Fixed-term	0.12	0.12
Variance ratio test			
	<i>F</i> statistic	1.774	1.715
	<i>P</i> value	0.000	0.000
	Observations	26,283	26,283

Notes: The table reports variances, test statistics, and p-values for the variance ratio test.

Table 11
Temporary contract and the probability of being in couple (or married).

	Couples	Married
Temporary contract	-0.022**	-0.026**
	(0.011)	(0.013)
Fixed-term contract	-0.025**	-0.027*
	(0.012)	(0.014)
R ²	0.272	0.662
Observations	16,755	7, 983

Notes: We define our dependent variable obtained from the couple (or marital) histories, by a dummy taking value “1” if the individual is involved in a couple (or married) in a given year, and “0” otherwise. The asterisks stand for the *p*-value significance levels (* *p* < 0.1; ** *p* < 0.05; *** *p* < 0.01). Estimates are performed including the set of covariates *X_{it}* described in Table 1.

of the unexplained part for women in temporary employment exceeds that for women in permanent contracts, uncertainty is higher for the former. Table 10 shows variances, test statistics, and p-values for the variance ratio test. On average, wages of fixed-term employed women are lower than those of their colleagues with permanent contracts. In addition, wages are more volatile and, therefore, more uncertain for workers in fixed-term contracts. The formal test confirms this result since the F-statistic leads to a rejection of equal variances. In sum, women with a fixed-term contract experience significantly higher wage uncertainty compared to individuals in permanent jobs.

Job instability that has arisen from the extension of temporary job contract reforms may also affect fertility through its potential (negative) effects on couple formation or marriage (van Wijk et al., 2021). As a potential mechanism, we construct our dependent variable obtained from the couple (or marital) histories, by a dummy taking value ‘1’ if the individual is involved in a couple (or married) in a given year, or ‘0’ if single. Then, we estimate the effects of women involved in temporary contracts, within the new labor market reforms, with respect to the control group of women in permanent and temporary jobs not affected by the reforms. We find a negative and significant relationship between the extension of temporary job contracts by new labor market reforms and the likelihood of forming a traditional couple, although the magnitude of this effect is not large (see column (1) of Table 11). We also test the marriage channel using the dichotomous variable of getting married. Irrespective of the chosen outcome, the results remain mostly unchanged. Reassuringly, estimates of benchmark specification show that the effects are only slightly sensitive to the exclusion of couple (or marital) status from the set of controls, suggesting that, while temporary jobs may have an impact on couple formation or marriage, this effect partially accounts for our main results on fertility.

Another potential mechanism in an attempt to explain the reduction in fertility behavior due to temporary employment is that the massive

introduction of these contracts – and the direct and indirect increase of economic uncertainty – may have played an important role in the postponement of having their first child (Santarelli, 2011; Vignoli et al., 2020), particularly for women with high career prospects. To construct our dependent variable, we exploit information concerning newborns, as well as the age of the mother at each birth, and build a dummy taking value “1” (or “0” otherwise) if the woman had “any child” in a given year over the mean sample. Thus, we can take the percentage of women that have a first birth for each year, by education and by type of contract, and see whether the timing of these first births is affected by their type of contract or not. Table 12 presents these percentages. We can see that the timing of the first births of women with only primary or secondary education is affected marginally by the type of contract – which confirms the findings by Modena et al. (2014) – whereas women with a fixed-term contract and university education tend to delay their first birth more than their counterparts with a permanent contract. Once again, it is confirmed that the opportunity cost of dis-investment in human capital due to the interrupted careers and increasing economic uncertainty for highly educated women may partly explain the reduction of fertility.¹⁵

7. Conclusions

This paper provides compelling evidence that the use of temporary contracts decreases fertility. We used a dataset on the work histories

¹⁵ One strand of this specific theory explains a delay and a decline in fertility, mediated by a rise in educational attainment, through the role of asymmetric information in the job market (Wang and Yin-Chi, 2020).

Table 12
Percentage of women at “their first child” by employment contract and education.

	Low education		High education	
	Early stage	Late stage	Early stage	late stage
Permanent contract	1.117	1.565	1.379	3.103
Fixed-term contract	3.120	3.802	2.144	5.756

Notes: We define early stage as a dummy variable taking value “1” (or “0” otherwise) if the woman delivered “any child” before the sample average age. The high education sample includes workers with a tertiary education degree, while low education includes workers with primary or secondary education degrees. The sample average age of giving birth is 31.9 for low-educated women and 34.2 for high-educated ones.

in Italy during the period 2003–2010 and exploited the progressive implementation of fixed-term and apprenticeship contract reforms in various sectors and regions of Italy to compare fertility behaviors between women who were exposed to the reforms and those that were not. We find that holding temporary contracts in the new labor market regimes constrains fertility behavior by 1.5 percentage points and this impact reaches 4 percentage points in childless women.

In regard to the partner’s employment status, we identified the heterogeneous effects on why granting temporary job contracts in the new labor market regime decreases fertility. The most positive gradient for the fertility of women who are exposed to the new labor market reforms is recorded when the partner is employed on a permanent contract. Instead, for women with partners on temporary employment contracts, there is no reduction in the probability of them having “their first child”; however, it reduces significantly the probability of them having “any child”.

The increase of job instability and the rise of economic uncertainty may explain why the extension of temporary employment reduces the probability of having “their first child” or having “any child”. We identified in the increase of the probability of remaining in temporary job contracts after the Italian reforms – largely more relevant for women – the stylized fact that explains these mechanisms. In a broader perspective regarding the economic uncertainty of women, we found that both job instability and wage volatility increased, as well as the fall in the probability of getting married and postponement of childbearing, as some significant underlying channels explaining the detrimental effects of more flexible jobs.

This paper strengthens the discussion of the effects that more labor flexibility have had on discouraging women to have children in Italy. We have focused our attention on how the extension of temporary contracts had an influence on working women. In according with (Vignoli et al., 2022), we conclude that the rising uncertainty in the labor market through temporary employment explains the persistently low Italian fertility levels only partially, with a small effect in the number of children “not born”. Nonetheless, even if our results are robust across different econometric strategies, we concede that there are certain limitations to the analysis presented in the study. First, we cannot cover all potential sources of job instability. Informal jobs, on-call jobs, and involuntary part-time contracts are also important channels of job instability but we are unable to estimate the effects on fertility of these work arrangements because of their limited number. Second, not all jobs are covered by collective agreements and, therefore, for a great number of employees or labor market entrants this treatment may have begun in 2001 or 2003. Thus, we only show a part of the picture concerning the impact temporary contracts have had on fertility, although recent estimates by Pedersini (2019) suggest that collective agreements cover approximately 80% of employment contracts. Third, we are aware that our study is not able to capture women who are temporarily unemployed, who are also more likely to be individuals with a discontinuous career and, in turn, more likely to have temporary positions. While we cannot exclude that women used interruptions to have children, with consequent measurement errors, our estimates involving women’s partners in their fertility behavior indirectly suggest

that periods of unemployment may be not good times of having a child, at least in Southern Italy. Following the argument of these limitations, our results most likely underestimate the impact of temporary contracts on fertility and offer a set of conservative estimates.

From a theoretical perspective, our study only refers to an objective economic uncertainty, which includes concerns by labor market reforms, as in Hofmann and Hohmeyer (2013) and Clark and Lepinteur (2022), even if, at the individual level, fertility has also been shown to fall with subjective measures of insecurity, as perceived job security or worries about own economic situation (Kreyenfeld et al., 2012). Although studies that consider subjective measures of employment uncertainty represent an avenue for future research, our dataset does not contain specific variables for subjective uncertainty. However, there are two reasons we are confident with our empirical approach. First, the timespan used (2003–2010) is a period that was relatively stable in the fundamentals of the Italian economy, except the last year of the sample. The Great Recession and the consequent increase in economic uncertainty discussed by Vignoli et al. (2012) had its effect mainly in the last decade, with an evident slowdown in births. Second, subjective measures are correlated with objective measures of economic uncertainty. The increase of wage volatility that we find as an explanation for the negative fertility results of the extension of temporary job in Italy is likely correlated with a negative expectation about the future of women who, under the new legislation of temporary job, are more insecure about maintaining a job in the long run. Combining individual-level data with regional indicators of precarious employment and subjective indicators of job uncertainty, Ayllón (2019) shows that objective indicators are more strongly linked to fertility than subjective indicators. This allows us to maintain confidence with the underlying explanation of temporary job-fertility nexus in this study.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The authors do not have permission to share data.

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