UNIVERSITÀ DEGLI STUDI FIRENZE

DISIA
DIPARTIMENTO DI STATISTICA, INFORMATICA, APPLICAZIONI "GIUSEPPE PARENTI"

The Causal Impact of Temporary Employment on First Births in Italy: An Update

Raffaele Guetto, Valentina Tocchioni, Daniele Vignoli

DISIA WORKING PAPER
2023/06

The Causal Impact of Temporary Employment on First Births in Italy: An Update

Raffaele Guetto[1], Valentina Tocchioni[2], Daniele Vignoli[3]

Abstract

Rising economic uncertainty is widely considered in the literature as one of the driving forces behind the postponement of childbearing and the reduction in fertility rates in contemporary Europe, especially following the Great Recession. Understanding whether employment instability causally and negatively impacts fertility decisions is of fundamental importance to providing clear recommendations to policymakers. To the best of our knowledge, the only study applying a counterfactual approach to the study of the causal impact of temporary employment for the transition to parenthood is a recent article by Vignoli, Tocchioni, and Mattei (2020). The present study replicates such a paper utilizing more recent data for Italy (2016, instead of 2009), thus covering a period encompassing the time of the Great Recession. We adopt the potential outcome approach to causal inference so as to quantify the net effect of having a first job with a temporary vs. permanent contract on the propensity to have a first child within the first five years of employment. Our findings confirm a clear-cut causal effect of temporary employment on first birth postponement. Even among men, we found negative causal effects of a first experience of temporary work, although less intense. These results largely overlap with those obtained by Vignoli and colleagues (2020), demonstrating how precarious work has by now become a structural factor discouraging the transition to parenthood among young Italians.

Keywords: Temporary employment; Fertility; First births; Potential outcome approach; Propensity score matching; Italy

[1] Università degli Studi di Firenze. Email: raffaele.guetto@unifi.it
[2] Università degli Studi di Firenze. Email: valentina.tocchioni@unifi.it
[3] Università degli Studi di Firenze. Email: daniele.vignoli@unifi.it

1. Introduction

In the 1980s, a new era of economic insecurity emerged, commonly attributed to various societal changes encompassed by the term “globalization”. These changes included a diminishing significance of national borders in economic transactions and heightened global interconnectedness through advancements in information and technology, accompanied by deregulation, privatization, and liberalization of domestic industries and markets. In particular, numerous European countries have implemented a series of reforms to enhance labour market flexibility. These deregulatory reforms primarily involved the gradual relaxation and endorsement of alternative forms of employment contracts, characterized by reduced bargaining power, diminished social protection, and generally lower wages (Barbieri and Cutuli, 2016). The proliferation of flexible work contracts has led to an increase in the instability of career paths. However, deregulation reforms took place “at the margin”, that is leaving the insider workforce institutionally sheltered by the reforms while burdening the younger cohorts of labour market entrants with all the demands for flexibility (Esping-Andersen and Regini, 2000). Extensive evidence indicates that the youth have become more susceptible to economic uncertainty and diminished job security, which can, in turn, influence their decisions regarding family formation (Alderotti et al., 2021).

Rising economic uncertainty is widely considered in the literature as one of the driving forces behind the postponement of childbearing and the reduction in fertility rates in contemporary Europe (Vignoli et al., 2020), especially following the Great Recession (Matysiak et al., 2021). However, concerns have been raised about the possibility of interpreting the negative “effects” of unemployment or temporary forms of employment on fertility in causal terms (Kreyenfeld, 2021). A first potential issue is that of *reverse causality*: for instance, more “family-oriented” women employed with a temporary contract may not strive to obtain a permanent contract in light of their desire to become mothers. A second issue, that of *unobserved heterogeneity*, stems from the fact that people are not assigned randomly to a certain type of contract: for instance, poorer subjective well-being and health status may simultaneously increase the chances of being in precarious employment and negatively influence fertility transitions. Understanding whether employment instability causally and negatively impacts fertility decisions is of fundamental importance to providing clear recommendations to policymakers.

To the best of our knowledge, the only study applying a counterfactual approach, based on propensity score matching, to the study of the causal impact of temporary employment for the transition to parenthood is a recent article by Vignoli, Tocchioni, and Mattei (2020). In this paper, which focuses on the Italian case, the authors analysed whether labour market entry through a temporary employment contract, rather than a permanent one, had a causal effect on the probability of conceiving the first child within five years of the first work experience. Based on retrospective data from the nationally representative 2009 Italian Multipurpose Household Survey on Family and Social Subjects – in short, FSS – Vignoli and colleagues (2020) showed a non-negligible first-birth postponement ascribable to a first temporary job. The study also provided us with an in-depth analysis of the heterogeneity of treatment effects by combinations of gender and level of education and found that tertiary educated women are more strongly affected by a first temporary job, whereas, among men, those with low and middle education are more likely to postpone the first birth.

Our replication study aims at assessing the robustness of these findings by using more recent data drawn from the 2016 version of the FSS, conducted by the Italian Institute of Statistics (ISTAT).

We replicate the analyses shown in Vignoli and colleagues (2020), based on the 2009 data, and investigate the impact of first labour market entry with a temporary versus a permanent job on potential first-birth postponement, comparing the two sets of analyses. In addition, our replication contributes to the existing study by focusing on younger cohorts of individuals, and by also considering employment spells that started after the onset of the Great Recession.

2. Data

The original analyses were based on data from the 2009 FSS survey. This is a large-scale, nationally-representative survey of approximately 24,000 households and 50,000 individuals, with a response rate of over 80%. The FSS survey is particularly suitable for our purposes because it provides retrospective information on fertility, work, partnership, and education histories, as well as information on several background characteristics.

For this replication, we draw on data from the 2016 edition of the FSS survey, conducted by ISTAT in 2016. This survey collected information on nearly 25,000 individuals aged 18 or older, with a response rate of 77.35%[1]. As in the 2009 edition, the 2016 survey contains retrospective information on fertility, work, partnership, and education histories on a monthly basis.

In order to investigate the impact of first labour market entry with a temporary versus a permanent job on potential first-birth postponement, we applied the same sample selection of the previous study. That is, we selected women and men aged 18-49 at the interview date. They had to be at least 18 and childless at the beginning of their first employment spell, which had to last at least one year. Individuals who had at least an employment spell longer than three months before turning 18 have been excluded from the sample, whereas people who had an employment (or several employment spells) before 18 that lasted at most three months were included. The reason behind these choices is that we did not want to enlarge the parameters of inclusion, being preferred for our kind of analysis to have a more homogeneous sample on first job-related characteristics. In this respect, a three-month period of employment may be considered seasonal employment, more common among full-time students and adolescents. Moreover, parenthood before 18 is very uncommon in Italy and may be considered mostly unintentional parenthood across all educational levels and both genders.

Overall, the sample consisted of 2,783 women and 3,178 men born between 1959 and 1991 for the 2009 survey, and of 1,819 women and 1,862 men born between 1967 and 1998 for the 2016 survey. In 2009, among women, 30.6% had a first temporary employment, and 5.5% found themselves in the least protected employment condition (i.e. project-based jobs); among men, 24.2% had a first temporary employment, and 3.0% had project-based jobs. In 2016, both types of temporary employment increased for the first labour market entry, both for women and men, with an increase of 4.6 percentage points (p.p.) of women having a first temporary employment (overall, 35.2%), and of 1.8 p.p. of women with a first project-based job. For men, the increase of first-time temporary workers is even higher and equal to 7.1 p.p. (overall, 31.3%), while first-time project-based workers increased by 1.0 p.p. It is also worth noting that job instability was higher among women than among men over the studied period.

Table 1: Sample by gender, edition of the survey, and type of contract. Absolute and column percentage values.

[1] In the 2016 survey, the sample size is halved compared to the 2009 edition because of its different sampling design.

	2009		2016	
	M	W	M	W
Permanent job	2,408 *(75.8%)*	1,931 *(69.4%)*	1,279 *(68.7%)*	1,179 *(64.8%)*
Fixed-term job	674 *(21.2%)*	699 (25.1%)	509 *(27.3%)*	507 *(27.9%)*
Project-based job	96 *(3.0%)*	153 *(5.5%)*	74 *(4.0%)*	133 *(7.3%)*
Total	**3,178**	**2,783**	**1,862**	**1,819**

3. Method

3.1 Causal Inference Framework

The analytical strategy of this paper replicates as similarly as possible the one originally applied by Vignoli and colleagues (2020). We report explicitly whenever the analytical strategy had to be adjusted because of differences in the data collection between the two surveys.

We are interested in estimating the effect of having a first temporary versus a permanent job contract on entering parenthood in the five years following job start, using retrospective (observational) data, where individuals with temporary and permanent jobs might systematically differ in their background characteristics. We faced this issue by using propensity score matching methods under the assumption of selection on observables (Imbens, 2003; Rosenbaum and Rubin, 1983), and we segmented the analysis by gender and survey year.

Our <u>treatment variable</u> was a binary indicator W for the type of employment, where $W_i = 1$ for individuals with a temporary job (treated individuals), and $W_i = 0$ for individuals with a permanent job (control individuals).

Our <u>outcome variable</u> was the conception of the first child[2]. Under the Stable Unit Treatment Value Assumption (SUTVA; Rubin, 1980), each individual *i* had two potential outcomes: s/he might conceive or not a child if s/he had a permanent job, $Y_i(0)$, or s/he might conceive or not a child if s/he had a temporary job, $Y_i(1)$ (where $Y=1$ if there was conception, $Y=0$ conversely). The outcome of interest was annually measured each year from the beginning of the first employment spell, up to five years or till the end of the first employment spell, whichever occurred first. Note that those who ended the first employment before five years (because of a career advancement – e.g., from employee to manager – or because of a change in occupational status – e.g., from a temporary work contract to a permanent one, or from a temporary work contract to not employment) were not excluded in the estimation of the outcome of interest for all the five years; thus, the sample did not change during the five years. But after the person changed his/her employment, we did not check whether s/he changed also his/her parenthood status any longer.

The causal estimand we aimed to estimate is the Average Treatment effect for the Treated (ATT; Imbens and Rubin, 2015):

$$ATT = E[Y_i(1)\text{-}Y_i(0) \mid W_i = 1] = Pr[Y_i(1)=1 \mid W_i=1] - Pr[Y_i(0)=1 \mid W_i=1]$$

where the second equality follows from the binary nature of the outcome.

[2] The questionnaire asks for the year and month of birth of each child. Thus, we computed the conception of the first child backdating by 9 months the date of his/her birth.

In our context, the ATT measures the difference between the proportion of first-child conceptions under temporary vs. permanent jobs among those who had a temporary job (the treated group; Imbens and Rubin, 2015).

Temporary and permanent jobs may include different employment contracts. For this reason, we run two separate analyses[3]:

(1) First, we focus on the effects of having a temporary job (that is, a fixed-term or a project-based job) versus having a permanent job. Fixed-term and project-based jobs both identify unstable forms of employment.
(2) Second, we focus on the effects of having a fixed-term versus a permanent job.

Since each person was only observed in either the treatment or control group, only one of the two potential outcomes was observed for each individual, and we need to estimate the missing outcomes. To this end, we rely on well-known assumptions of *unconfoundedness* and *overlap* (Rosenbaum and Rubin, 1983). Unconfoundedness requires that there are no unobserved confounders of the treatment-outcome relationship, and overlap implies that there are treated and control individuals for all values of the covariates. In our study, unconfoundedness might be violated due to the presence of latent (unobserved) variables, such as fertility intentions, family orientation and career ambitions, which are reasonably related to both the employment contract and the decision to conceive a child. Nevertheless, we have information on a large set of background variables (see Table A1 in the Appendix), some of which can also be viewed as proxies for important latent confounders. Therefore, we believe that most of the relevant variables are observed in our analyses; for this reason, we are confident that the unconfoundedness assumption may be reasonable. Nevertheless, we conducted a sensitivity analysis to verify how our results would be affected due to unobserved confounders (see Section 4.1 for a description of the test and results). In any event, it can be argued that possible bias due to unobserved confounders may produce an underestimation of the negative impact of temporary jobs on first-birth postponement. For instance, family-oriented women may respond to unfavourable employment prospects by choosing the "alternative career" of mothers (Friedman et al., 1994).

There exist various methods for drawing inference on average treatment effects under strong ignorability (see, e.g., Imbens, 2004; Imbens and Wooldridge, 2009). Here we use matching methods based on the propensity score (Imbens and Rubin, 2015; Stuart, 2010).

The analysis involves two steps. In the first step (*design phase*), the focus was on selecting a sub-sample of units where the distribution of the observed covariates was well-balanced between treated and control groups. Because we are interested in drawing inference on the ATT, our focus is on treated units. Therefore, in the design phase we used matching to find, for each treated person, one matched control person with similar background characteristics. In this phase, some slight adjustments in the propensity score estimation in the 2016 survey were implemented, because of differences in the collection of some covariates (see Section 3.2 for details). In the second step (*analysis phase*), we imputed the missing potential outcome for each treated unit i by using the

[3] In the original analyses on the 2009 sample, Vignoli and colleagues (2020) run an additional analysis that compared the effects of having a project-based vs. permanent job. Unfortunately, the 2016 sample is smaller compared to the 2009 sample because of the nearly halved sample of the most recent edition of the survey. For this reason, and for the sake of brevity, we did not implement a separate analysis of project-based workers.

outcome of her/his matched control, and estimated the unit-level causal effect, $Y_i(1) - Y_i(0)$, as difference between the observed outcome for that treated unit and her/his imputed outcome: $Y_i - Y_i(0)$.

3.2 Design phase: propensity score matching

Our matching procedure is based on the propensity score, which is defined as the probability of having a temporary job, given the observed covariates (Rosenbaum and Rubin, 1983): $Pr(W_i = 1|Z_i)$. The propensity score has been estimated by specifying a logit model for the treatment indicator W_i on the background variables. Our dataset comprised a rich set of relevant confounders of the relationship between employment contract and fertility decisions, that is, variables that are not affected by the treatment of interest, and that could reasonably influence both the conception of the first child and the type of contract at labour market entry (i.e., age, partnership). For those matching variables that may change over time (i.e. education, partnership), we fixed the values before the start of the observation period, i.e. before the first employment spell. See Table A1 in Appendix for the complete list of confounders we included in the models for the propensity score estimation, collected in both the 2009 and 2016 surveys[4].

Given the estimated propensity score, we selected a sub-sample of matched control units such that the covariate distribution in the matched control group was similar to the covariate distribution in the treated sample, adopting the same procedures already used by Vignoli and colleagues (2020) – e.g. discarding few control observations outside the common support range, using the one-to-one nearest neighbour matching algorithm without replacement with an exact match on age and education (e.g., Abadie and Imbens, 2002).

Table A1 in Appendix shows the distributions of the covariates for the control group and the treated group, before and after matching. In all cases, the matching procedure seems to perform well.

3.3 Analysis phase: ATT estimation

Given the sample of treated and matched-control individuals, for each treated individual *i* we imputed her/his missing potential outcome, $Y_i(0)$, using the outcome of her/his matched-control individual Y_i^C. Then, we estimated the Average Treatment effect for the Treated (ATT)[5]:

$$\widehat{ATT} = \frac{1}{N_t}\sum_{i:W_i=1}(Y_i^{obs} - Y_i^C) = \frac{1}{N_t}\sum_{i:W_i=1} Y_i^{obs} - \frac{1}{N_c}\sum_{i:W_i=0} Y_i^{obs}$$

where $N_t = \sum_{i=1}^{N} W_i$ is the number of treated individuals and $N_c = \sum_{i=1}^{N}(1 - W_i)$ is the number of matched-control subjects[6].

Thus, once the missing potential outcome for the treated person (i.e. the potential first-child conception under permanent job) is estimated using the observed outcome for the matched-control person (i.e. s/he has effectively conceived or not the first child), we estimated the percentage of potential postponement through the above formula of the $\widehat{ATT}$ (multiplied by 100) each year up to five years from the beginning of the first employment spell.

---

[4] The 2016 survey only collected the date in which the highest educational level has been reached; if the respondent entered the first employment spell before completing his/her education, we assigned him/her a lower educational level with respect to the highest reached.

[5] This formula differs with respect to the one presented in subsection 3.1 because that quantity was a theoretical one, which cannot be computed directly.

[6] Note that in our study Nc = Nt.

4. Results

Table 2 shows the ATTs on first-birth conception, by number of years after the beginning of the first employment spell, type of contract, survey year, and gender. The results for the 2009 survey are taken from Vignoli and colleagues (2020).

Comparing the results across surveys, the potential postponement effects of temporary employment on first-birth conceptions seems to be stronger in 2009 than in 2016, and the differences between the two surveys are larger for women than for men. For women, in the original work, the ATT estimates were statistically significant four years after the start of the first job, whereas in our replication they are statistically significant in the fifth year only. Moreover, the magnitudes of potential postponements of first-birth conceptions were higher in Vignoli and colleagues (2020): 7.5% of women who had a first temporary job would have had the first child within five years from the beginning of their first employment spell if they had had, instead, a permanent job, whereas the same figure drops to 4.7% in 2016. The gap between the two surveys is even larger comparing fixed-term versus permanent jobs, reaching 9.2% and 4.4% of first-birth potential postponement for women in 2009 and 2016, respectively.

As for men, potential postponement in the proportion of first-birth conceptions due to temporary/fixed-term employment in the five years following the entry into first employment is similar across the two surveys. Our findings thus confirm a relevant postponement of first-birth conceptions attributable to temporary employment: 4.7% of men in 2009 and 4.0% of men in 2016 who had a first temporary job would have had the first child within five years from the beginning of the first employment spell if they had had, instead, a permanent job. Again, the percentage of first-birth losses increases comparing fixed-term versus permanent jobs, reaching 5.7% and 4.7% of first-birth potential postponement in 2009 and in 2016, respectively.

Notwithstanding the differences, our replication confirms the overall finding of Vignoli and colleagues (2020) that entering the labour market with a temporary contract induces a potential postponement of first-birth conceptions, an effect that increases (almost) monotonically over time.

Table 2: Average Treatment effect for the Treated (ATT) on first-birth conception from propensity score matching, by the number of years after the beginning of the first employment spell, type of contract and survey year. Percentage values. Women and men

a) Women

Temporary vs permanent				
	2009		**2016**	
n treated	845		639	
# Years after employment start	ATT	*Confidence interval*	ATT	*Confidence interval*
1	-1.30	*[-2.90; 0.30]*	0.00	*[-1.55; 1.55]*
2	-1.78	*[-4.12; 0.57]*	0.47	*[-1.87; 2.81]*
3	-2.84	*[-5.73; 0.05]*	0.16	*[-2.61; 2.92]*
4	-4.85	*[-8.08; -1.62]*	-0.94	*[-4.11; 2.24]*
5	-7.46	*[-10.89; -4.02]*	-4.70	*[-8.24; -1.16]*

Fixed-term vs permanent				
	2009		2016	
n treated	694		502	
# Years after employment start	ATT	*Confidence interval*	ATT	*Confidence interval*
1	-0.58	*[-2.29; 1.14]*	-0.20	*[-2.05; 1.65]*
2	-0.86	*[-3.43; 1.70]*	0.20	*[-2.57; 2.97]*
3	-2.88	*[-6.13; 0.36]*	-1.00	*[-4.37; 2.37]*
4	-5.91	*[-9.55; -2.26]*	-1.59	*[-5.39; 2.21]*
5	-9.22	*[-13.12; -5.33]*	-4.38	*[-8.50; -0.27]*

b) Men

Temporary vs permanent				
	2009		2016	
n treated	769		583	
# Years after employment start	ATT	*Confidence interval*	ATT	*Confidence interval*
1	-0.26	*[-1.73; 1.21]*	0.17	*[-1.43; 1.77]*
2	-1.04	*[-3.04; 0.96]*	-0.52	*[-2.48; 1.45]*
3	-1.56	*[-3.95; 0.83]*	-2.41	*[-4.95; 0.13]*
4	-3.25	*[-6.04; -0.47]*	-2.93	*[-5.78; -0.07]*
5	-4.68	*[-7.71; -1.66]*	-3.96	*[-7.04; -0.88]*
Fixed-term vs permanent				
	2009		**2016**	
n treated	670		506	
# Years after employment start	ATT	*Confidence interval*	ATT	*Confidence interval*
1	0.30	*[-1.23; 1.83]*	0.00	*[-1.80; 1.80]*
2	-1.49	*[-3.71; 0.72]*	-1.19	*[-3.47; 1.10]*
3	-2.69	*[-5.39; 0.02]*	-3.56	*[-6.50; -0.61]*
4	-3.88	*[-6.86; -0.90]*	-4.15	*[-7.45; -0.85]*
5	-5.67	*[-8.95; -2.39]*	-4.74	*[-8.24; -1.25]*

Note: ATT on first-birth conception for the 2009 survey are taken from Vignoli et al. (2020).

When it comes to the heterogeneity of the effects of having a first temporary versus permanent job by educational level, we replicated the analysis separately for individuals with tertiary education, for those with upper-secondary education and for those with lower-secondary education at most. The ATTs by educational level, survey year, and gender are reported in Table 3.

For women, both in the original and in our analyses, the highest proportion of potential postponement of first-birth conceptions is recorded among the tertiary educated: 15.9% and 13.2% of them did not have a first child within five years from the beginning of the first employment spell because of the instability of their first job, respectively – confidence intervals are very large because of small numbers and superimposable between the two analyses. On the other hand, women who only achieved lower-secondary education at most show the smallest gap in potential postponement: In fact, while Vignoli and colleagues (2020) found virtually no impact of the type of contract for this group of women, using the 2016 edition of the FSS survey we found an accelerating effect of conceiving the first child while having a first temporary job, even if all ATT estimates are not statistically significant at conventional levels and with very large CIs. As far as women with upper-secondary education, ATT estimates are very similar across the two studies and show an important potential postponement of first-birth conceptions attributable to temporary employment.

Turning to men, ATT estimates are similar across different educational levels in both surveys, even if the smaller sample sizes increase the uncertainty around the estimates at the lowest and highest educational levels in 2016. That notwithstanding, in 2009, 7% of men with primary or lower-secondary education did not have a first child within five years from the beginning of the first employment spell because they had a temporary rather than a permanent job: A negative impact we could not detect in our replication using the 2016 data. For men with upper-secondary education, the size of potential postponement is sizable and evident starting from four years after the beginning of the employment spell, with a percentage of potential postponement of first-birth conceptions around 5% in both studies after five years since labour market entry.

Table 3: Average Treatment effect for the Treated (ATT) on first-birth conception from propensity score matching, by the number of years after the beginning of the first employment spell, educational level, and survey year. Percentage values. Women and men

a) Women

Primary/Lower-secondary education				
	2009		**2016**	
n treated	171		126	
# Years after employment start	ATT	*Confidence interval*	ATT	*Confidence interval*
1	-0.58	*[-4.32; 3.15]*	2.38	*[-1.67; 6.43]*
2	0.00	*[-5.62; 5.62]*	4.76	*[-0.86; 10.39]*
3	1.17	*[-5.49; 7.83]*	4.76	*[-1.89; 11.41]*
4	1.17	*[-6.06; 8.40]*	4.76	*[-2.73; 12.25]*
5	-1.75	*[-9.30; 5.79]*	3.97	*[-3.90; 11.83]*
Upper-secondary education				
	2009		**2016**	
n treated	542		393	
# Years after employment start	ATT	*Confidence interval*	ATT	*Confidence interval*
1	-1.29	*[-3.34; 0.75]*	-1.02	*[-3.22; 1.18]*
2	-1.85	*[-4.69; 1.00]*	-1.53	*[-4.74; 1.69]*
3	-3.14	*[-6.68; 0.41]*	-2.04	*[-5.63; 1.56]*
4	-5.17	*[-9.12; -1.21]*	-3.31	*[-7.36; 0.74]*
5	-7.01	*[-11.21; -2.81]*	-6.87	*[-11.36; -2.38]*
Tertiary education				
	2009		**2016**	
n treated	126		114	
# Years after employment start	ATT	*Confidence interval*	ATT	*Confidence interval*
1	-1.59	*[-4.67; 1.49]*	-2.63	*[-5.57; 0.307]*
2	0.79	*[-4.25; 5.84]*	-1.75	*[-6.53; 3.017]*
3	-4.76	*[-12.25; 2.73]*	-7.90	*[-15.65; -0.144]*
4	-12.70	*[-21.58; -3.81]*	-7.90	*[-16.48; 0.694]*
5	-15.87	*[-25.37; -6.38]*	-13.16	*[-22.58; -3.740]*

b) Men

Primary/Lower-secondary education				
	2009		2016	
n treated	256		195	
# Years after employment start	ATT	*Confidence interval*	ATT	*Confidence interval*
1	-2.34	*[-6.14; 1.45]*	0.51	*[-3.05; 4.08]*
2	-1.56	*[-6.21; 3.08]*	-1.54	*[-6.21; 3.14]*
3	-5.08	*[-10.75; 0.60]*	-4.10	*[-9.54; 1.33]*
4	-6.25	*[-12.39; -0.11]*	-3.08	*[-8.96; 2.80]*
5	-7.03	*[-13.48; -0.58]*	-2.56	*[-8.78; 3.65]*
Upper-secondary education				
	2009		2016	
n treated	411		338	
# Years after employment start	ATT	*Confidence interval*	ATT	*Confidence interval*
1	-1.97	*[-2.61; 0.67]*	0.00	*[-1.41; 1.41]*
2	-1.70	*[-3.95; 0.55]*	-0.59	*[-2.41; 1.23]*
3	-2.92	*[-5.78; -0.06]*	-2.07	*[-4.80; 0.66]*
4	-4.14	*[-7.36; -0.91]*	-4.44	*[-7.76; -1.11]*
5	-5.35	*[-8.89; -1.82]*	-5.33	*[-8.86; -1.79]*
Tertiary education				
	2009		2016	
n treated	94		47	
# Years after employment start	ATT	*Confidence interval*	ATT	*Confidence interval*
1	1.06	*[-2.52; 4.64]*	4.26	*[-1.52; 10.03]*
2	-3.19	*[-8.59; 2.20]*	-2.13	*[-11.19; 6.94]*
3	-5.32	*[-11.99; 1.35]*	-2.13	*[-12.73; 8.48]*
4	-6.38	*[-15.15; 2.38]*	-2.13	*[-14.02; 9.76]*
5	-6.38	*[-15.53; 2.76]*	-4.26	*[-16.69; 8.18]*

Note: ATT on first-birth conception for the 2009 survey are taken from Vignoli et al. (2020).

a. Sensitivity analyses

The results shown in the previous paragraph are based on the unconfoundedness and overlap assumptions (Rosenbaum and Rubin, 1983). In our study, despite the large set of background variables included in the design phase, unconfoundedness might be violated due to the presence of latent, unobserved variables, such as fertility intentions, family orientation and career ambitions. To verify this assumption, we conducted a sensitivity analysis specifically developed to assess if estimates after a matching procedure are robust to the possible presence of unobserved confounders (see Rosenbaum, 2002 for more details on this method).

Summarising, this method relies on the sensitivity parameter Γ that measures the degree of departure of treatment from random assignment. For example, in an observational study, if Γ=2 and two subjects are identical on all matched covariates, it means that one of the two might be twice as likely to be in the treatment group because of unobserved factors (Rosenbaum, 2005). Usually, most studies in social sciences are not robust to large values of Γ; consequently, values of Γ below 2 are usually chosen. For a binary outcome like ours, the sensitivity analysis is based on McNemar's test, which produces the lower and upper bounds of the p-value associated with the estimate of the ATT.

Table 4 shows lower and upper bounds for the p-values of ATTs on first-birth conception, computed five years after the beginning of the first employment spell, and Γ varies from 1.0 to 1.5. Recall that the ATT values when the number of years after the beginning of first employment is 5 were all statistically different from zero in all combinations by gender, type of contract and survey year (Table 2). As for women, Rosenbaum's sensitivity tests show how our results for the 2009 data are robust for a value of Gamma of 1.5, i.e. even if a woman was 50% more likely to be treated due to unobserved factors. Turning to the 2016 data, however, our inference could change if the odds of a woman being a temporary worker (or fixed-term worker) were 1.3 (or 1.2) times higher because of different values on unobserved covariates. Among men, our conclusions could change if the odds of a man being a temporary/fixed-term worker were 1.3 times higher (or 1.4 for 2009 fixed-term workers) due to unobserved factors.

Table 4: Rosenbaum's bounds on p-values for ATTs on first-birth conception from propensity score matching 5 years after the beginning of the first employment spell, by type of contract and survey year. Women and men.

a) Women

Temporary vs permanent				
	2009		**2016**	
n treated	845		639	
Gamma	Lower bound	Upper bound	Lower bound	Upper bound
1.0	0.000	0.000	0.003	0.003
1.1	0.000	0.000	0.001	0.015
1.2	0.000	0.000	0.000	0.048
1.3	0.000	0.000	0.000	0.113
1.4	0.000	0.002	0.000	0.214
1.5	0.000	0.009	0.000	0.343
Fixed-term vs permanent				
	2009		**2016**	
n treated	694		502	
Gamma	Lower bound	Upper bound	Lower bound	Upper bound
1.0	0.000	0.000	0.014	0.014
1.1	0.000	0.000	0.003	0.044
1.2	0.000	0.001	0.001	0.106
1.3	0.000	0.003	0.000	0.203
1.4	0.000	0.013	0.000	0.328
1.5	0.000	0.040	0.000	0.464

b) Men

Temporary vs permanent				
	2009		**2016**	
n treated	769		583	
Gamma	Lower bound	Upper bound	Lower bound	Upper bound
1.0	0.001	0.001	0.003	0.003
1.1	0.000	0.007	0.001	0.012
1.2	0.000	0.027	0.000	0.031
1.3	0.000	0.071	0.000	0.066
1.4	0.000	0.150	0.000	0.121
1.5	0.000	0.261	0.000	0.196
Fixed-term vs permanent				
	2009		**2016**	
n treated	670		506	
Gamma	Lower bound	Upper bound	Lower bound	Upper bound
1.0	0.001	0.001	0.004	0.004
1.1	0.000	0.003	0.001	0.013
1.2	0.000	0.012	0.000	0.034
1.3	0.000	0.033	0.000	0.072
1.4	0.000	0.075	0.000	0.132
1.5	0.000	0.142	0.000	0.211

To conclude, our findings seem to be fairly robust to possible hidden bias due to unobserved confounders, especially among women in the 2009 dataset (possibly due to the larger sample size).

5. Conclusions and discussion

The role of causality in population studies is currently receiving renewed interest and growing attention (Wunsch and Gourbin, 2020; Kreyenfeld, 2021). Papers adopting an experimental approach (e.g., Guetto et al., 2022; Lappegård et al., 2022; Vignoli et al., 2022), using a quasi-experimental design (e.g., Klüsener et al., 2013; Azzolini and Guetto, 2017; Comolli and Vignoli, 2021), or exogenous shocks to estimate "causal effects" (Ananat et al. 2013; Hofmann et al., 2017) have been increasingly published. Nonetheless, demographic research remains largely observational, characterized by the application of non-experimental methods that often highlight meaningful associations, rather than infer causation. We located only one study relying on a potential outcome approach to assess the causal impact of temporary employment for the transition to parenthood in Italy (Vignoli et al., 2020). The present study replicates such a paper by utilizing more recent data (2016, instead of 2009), thus covering a period encompassing the time of the Great Recession. In particular, the work studies whether a first temporary employment contract, rather than a permanent one, has a causal effect on the probability of conceiving the first child within five years of the first work experience. Also, it analyses the possible existence of heterogeneity in the effect according to gender and educational level.

The results, obtained through the application of propensity score matching techniques, indicate that women are affected the most by the type of contract, especially those with tertiary education. Among them, indeed, the probability of conceiving the first child within five years since the start of the first employment episode is reduced, ceteris paribus, by 13 percentage points in the case of a temporary contract, compared to a permanent one. Even among men, we found negative causal effects of a first experience of temporary work, although less intense. These results largely overlap with those obtained by Vignoli and colleagues (2020), demonstrating how precarious work has by now become a structural factor discouraging the transition to parenthood among young Italians. Tertiary-educated women experience stronger first-birth postponement because they are – at least in the Italian context – particularly exposed to higher opportunity-costs compared to the lesser educated (e.g., Adsera, 2004). Highly-educated women may accept a temporary contract to have the chance to progress in their careers and catch up on their initial fertility loss when their contract becomes permanent.

The results for lower-educated women, instead, differ from those of Vignoli and colleagues (2020), notwithstanding the high estimation uncertainty due to the small sample size. While the original analyses based on the 2009 FSS data found no effects of the type of first job contract, the new analyses based on the 2016 FSS survey suggest that lower-educated women with more uncertain contractual conditions tend to opt for parenthood earlier than their counterparts having a better contractual position. Lower-educated women, indeed, have the least to lose in case of maternity leave, because of lower wages and poorer career expectations. These results seem to align with an uncertainty reduction narrative (Friedman et al., 1994), according to which some women may decide to ‘focus’ primarily on family life if employment uncertainty reaches too high levels. This finding represents a novelty for the Italian context, while the accelerating effect of unemployment on the transition to parenthood has been emphasized in other contexts (like Germany and Denmark: Kreyenfeld, 2010; Kreyenfeld and Andersson, 2014).

Heterogeneity of treatment effects by level of education differs across the two surveys also among men. The original study highlighted a stronger first-birth postponement among men with the lowest educational qualifications. Our findings reveal that, in more recent years, differences by level of education between men tend to vanish. Our study encompasses the time of the Great Recession, and during negative economic conjunctures, highly-educated individuals may become more attached to the labour market, to keep career options open, and thus postpone childbearing. Irrespective of educational qualifications, hence, establishing a stable, secure, and more or less successful career is likely to be a chief goal among men, pointing to the importance of their “breadwinner qualities” in family formation options.

This paper replicates the only study we could locate in the literature utilizing techniques of causal analysis to estimate the potential first-birth postponement among men and women holding a temporary contract. Nonetheless, the causal language used in this paper is partly confined to the statistical literature used, and can be generalized to make substantive causal conclusions only keeping in mind the assumptions underlining an analysis of this kind. In order to draw inference on the causal effect of interest, in fact, we had to rely on the *unconfoundedness* assumption. Unconfoundedness is a strong and untestable assumption, which is violated under the presence of unobserved variables that might simultaneously affect the likelihood of experiencing the outcome and receiving the treatment. In our study, unconfoundedness might be violated due to the presence of latent (unobserved) variables, such as personality traits or family orientation, which can affect the relations between temporary employment and transition to parenthood, as well as – in the moderation analyses – between temporary employment, education, and first births. These variables can only be measured using longitudinal data, whereas we relied on retrospective data. Beyond taking into account a larger set of control variables related to personality traits and family preferences, future studies should adopt a couple approach. In fact, due to a lack of retrospective information on partners' characteristics, we could only implement separate analyses by sex, whereas it would be interesting to measure the causal effect of different combinations of both partners' employment situations. Nevertheless, our data offered information on a large set of background variables, some of which can also be viewed as proxies for important latent confounders, rendering unconfoundedness a reasonable approximation in our analysis. What is more, the omission of potential confounders is likely to make our first-birth postponement estimates rather "conservative". For instance, individuals with high family orientation or low career ambition – unobserved factors in our analysis – might have opted out of temporary employment and chosen family formation. Finally, given that men tend to become parents later than women but enter the labour market around the same age, the first five years of employment may be less relevant in terms of childbearing for them, which may explain – at least partly – the reduced effect of temporary employment that we found among men compared to women.

References

Abadie, A., & Imbens, G. W. (2002). *Simple and bias-corrected matching estimators. Technical report, Department of Economics.* Berkeley, US: University of California.

Adsera, A. (2004). Changing fertility rates in developed countries. The impact of labor market institutions. *Journal of Population Economics*, 17: 17–43.

Alderotti, G., Vignoli, D., Baccini, M., & Matysiak, A. (2021). Employment instability and fertility in Europe: A meta-analysis. *Demography*, 58: 871–900.

Ananat E.O., Gassman-Pines, A., and Gibson-Davis, C. (2013). Community-wide job loss and teenage fertility: evidence from North Carolina. *Demography*, 50: 2151–2171.

Azzolini, D., and Guetto, R. (2017). The impact of citizenship on intermarriage: Quasi-experimental evidence from two European Union Eastern enlargements. *Demographic Research*, 36 (43): 1299–1336.

Barbieri, P., and Cutuli, G. (2016). Employment Protection Legislation, Labour Market Dualism, and Inequality in Europe. *European Sociological Review*, 32 (4): 501–516.

Comolli, C.L., and Vignoli, D. (2021). Spread-ing Uncertainty, Shrinking Birth Rates: A Natural Experiment for Italy. *European Sociological Review*, 37(4): 555-570.

Esping-Andersen, G., and Regini, M. (2000). *Why Deregulate Labor Markets?* Oxford: Oxford University Press.

Friedman, D., Hechter, M., & Kanazawa, S. (1994). A theory of the value of children. *Demography*, 31: 375–401.

Guetto, R., Bazzani, G., and Vignoli, D. (2022). Narratives of the future and fertility decision-making in uncertain times. An application to the COVID-19 pandemic. *Vienna Yearbook of Population Research*, 20:1-38.

Hofmann B., Kreyenfeld M., and Uhlendorff A. (2017). Job displacement and first birth over the business cycle. *Demography*, 54: 933–959.

Imbens, G.W. (2003). Sensitivity to exogeneity assumptions in program evaluation. *The American Economic Review*, 93 (2).

Imbens, G. W. (2004). Nonparametric estimation of average treatment effects under exogeneity: A review. *Review of Economics and Statistics*, 86(1), 4–29.

Imbens, G.W., and Wooldridge, J.M. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature*, 47 (1): 5-86.

Imbens, G. W., & Rubin, D. B. (2015). *Causal inference in statistics, social, and biomedical sciences.* Cambridge, US: Cambridge University Press.

Klüsener, S., Neels, K., & Kreyenfeld, M. (2013). Family policies and the Western European fertility divide: Insights from a natural experiment in Belgium. *Population and Development Review*, 39(4): 587–610.

Kreyenfeld, M. (2021). Causal Modelling in Fertility Research: A Review of the Literature and an Application to a Parental Leave Policy Reform. *Comparative Population Studies,* 46.

Kreyenfeld, M. (2010). Uncertainties in female employment careers and the postponement of parenthood in Germany. *European Sociological Review*, 26: 351–366.

Kreyenfeld, M., & Andersson, G. (2014). Socioeconomic diferences in the unemployment and fertility nexus: Evidence from Denmark and Germany. *Advances in Life Course Research*, 21: 59–73.

Lappegård, L., Kristensen, A.P., Dommermuth, L., Minello, A., Vignoli, D. (2022). The Impact of Narratives of the Future on Fertility Intentions in Norway. *Journal of Marriage and Family*, on-line first.

Matysiak, A., Sobotka, T., and Vignoli, D. (2021). The great recession and fertility in Europe: A sub-national analysis. *European Journal of Population* 37(1): 29-64.

Rosenbaum, P.R., and Rubin, D.B. (1983). Assessing sensitivity to an unobserved binary covariate in an observational study with binary outcome. *Journal of the Royal Statistical Society Series B: Methodological*, 45 (2): 212-218.

Rosenbaum, P.R. (2005). Observational Study. In B.S. Everitt, & D.C. Howell (Eds.), *Encyclopedia of Statistics in Behavioral Science*, (vol. 3). New York: John Wiley and Sons.

Rubin, D. B. (1980). Bias Reduction Using Mahalanobis-Metric Matching. *Biometrics*, 36(2), 293–298.

Stuart, E. A. (2010). Matching methods for causal inference: A review and a look forward. *Statistical Science: A Review Journal of the Institute of Mathematical Statistics*, 25(1), 1.

Vignoli, D., Guetto, R., Bazzani, G., Pirani, E., and Minello, A. (2020). A reflection on economic uncertainty and fertility in Europe: The Narrative Framework. *Genus*, 76(1), 28.

Vignoli, D., Minello, A., Bazzani, G., Matera, C., and Rapallini, C. (2022). Narratives of the Future Affect Fertility: Evidence from a Laboratory Experiment. *European Journal of Population*, 38: 93-124.

Vignoli, D., Tocchioni, V., and Mattei, A. (2020). The impact of job uncertainty on first-birth postponement. *Advances in Life Course Research* 45, 100308.

Wunsch, G., and Gourbin, C. (2020). Causal assessment in demographic research. *Genus*, 76, 18.

Appendix

Note to the table: the primary goal here is to find an adequate specification for the propensity score, which leads to adequate balance between covariate distributions in treatment and control groups in our sample (Imbens and Rubin, 2015).

Table A1 – Set of background covariates among workers with a temporary job (treated), workers with a permanent job (control) and matched workers with a permanent job (matched control). Absolute and percentage values. Women and men. 2016

WOMEN	Control		Matched control		Treated	
	n	%	n	%	n	%
Total	1179		639		639	
In union twelve months before	71	6.0	27	4.23	30	4.7
In union one month before	103	8.7	37	5.8	43	6.7
Age						
18-19	303	25.7	157	24.6	157	24.6
20-21	280	23.8	137	21.4	137	21.4
22-23	204	17.3	117	18.3	117	18.3
24-26	193	16.4	113	17.7	113	17.7
27+	199	16.9	115	18.0	115	18.0
Highest educational level						
Primary/Lower-secondary	236	20.0	130	20.3	130	20.3
Upper-secondary	731	62.0	395	61.8	395	61.8
Tertiary	212	18.0	114	17.8	114	17.8
Still in education	220	18.7	170	26.6	197	30.8
Mother's education						
No education	36	3.1	26	4.1	26	4.1
Primary/Lower-secondary	757	65.0	372	58.2	371	58.1
Upper-secondary	302	25.9	193	30.2	199	31.1
Tertiary	70	6.0	42	6.6	39	6.1
Father's education						
No education	26	2.3	19	3.0	21	3.3
Primary/Lower-secondary	708	61.3	354	55.4	364	57.0
Upper-secondary	326	28.2	197	30.8	193	30.2
Tertiary	95	8.2	57	8.9	49	7.7
Respondent's social class						
Non-skilled manual worker	164	13.9	108	16.9	112	17.5
Skilled manual worker	63	5.3	30	4.7	30	4.7
Sales personnel	294	24.9	174	27.2	180	28.2
Intermediate professional	381	32.3	171	26.8	155	24.3
Professional and supervisor	226	19.2	130	20.3	132	20.7
Higher managerial staff	49	4.2	24	3.8	27	4.2
Mother's social class when respondent was 14						

No work	557	47.8	296	46.3	287	44.9
Manual worker	216	18.5	120	18.8	116	18.2
Non-manual employee	232	19.9	148	23.2	151	23.6
Self-employed	149	12.8	61	9.6	65	10.2
Professional and higher managerial staff	11	0.9	9	1.4	12	1.9
Father's social class when respondent was 14						
No work	41	3.6	26	4.1	29	4.5
Manual worker	453	39.3	232	36.3	225	35.2
Non-manual employee	300	26.0	187	29.3	178	27.9
Self-employed	323	28.0	162	25.4	172	26.9
Professional and higher managerial staff	36	3.1	18	2.8	20	3.1
Parents' separation when respondent was 14	105	9.0	64	10.2	64	10.2
Siblings						
No brothers/sisters	156	13.2	82	12.8	90	14.1
One brother/sister	543	46.1	295	46.2	288	45.1
Two or more brothers/sisters	480	40.7	262	41.0	261	40.9
Macroarea of residence[a]						
North-West	311	26.4	128	20.0	128	20.0
North-East	291	24.7	161	25.2	168	26.3
Centre	221	18.7	121	18.9	124	19.4
South/Islands	356	30.2	229	35.8	219	34.3
Left the parental home	206	17.5	117	18.3	124	19.4
Calendar period						
before 1994	297	25.2	109	17.1	99	15.5
1994-2003	480	40.7	226	35.4	218	34.1
2004-2009	242	20.5	164	25.7	150	23.5
after 2009	160	13.6	140	21.9	172	26.9

MEN	Control		Matched control		Treated	
	n	%	n	%	n	%
Total	1279		581		581	
In union twelve months before	34	2.7	12	2.1	12	2.1
In union one month before	54	4.2	20	3.4	21	3.6
Age						
18-19	367	28.7	194	33.4	194	33.4
20-21	280	21.9	129	22.2	129	22.2
22-23	223	17.4	98	16.9	98	16.9
24-26	194	15.2	87	15.0	87	15.0
27+	215	16.8	73	12.6	73	12.6
Highest educational level						
Primary/Lower-secondary	410	32.1	196	33.7	196	33.7

Upper-secondary	735	57.5	338	58.2	338	58.2
Tertiary	134	10.5	47	8.1	47	8.1
Still in education	197	15.4	128	23.8	150	25.8
Mother's education						
No education	68	5.4	37	6.4	40	6.9
Primary/Lower-secondary	832	66.4	341	58.7	333	57.3
Upper-secondary	290	23.1	149	25.7	153	26.3
Tertiary	64	5.1	41	7.1	46	7.9
Father's education						
No education	57	4.6	33	5.7	33	5.7
Primary/Lower-secondary	801	64.3	330	56.8	323	55.6
Upper-secondary	301	24.2	155	26.7	165	28.4
Tertiary	86	6.9	46	7.9	47	8.1
Respondent's social class						
Non-skilled manual worker	244	19.1	155	26.7	174	30.0
Skilled manual worker	411	32.1	169	29.1	156	26.9
Sales personnel	154	12.0	60	10.3	61	10.5
Intermediate professional	204	16.0	88	15.2	83	14.3
Professional and supervisor	204	16.0	79	13.6	79	13.6
Higher managerial staff	59	4.6	29	5.0	26	4.5
Mother's social class when respondent was 14						
No work	739	58.5	303	52.2	289	49.7
Manual worker	190	15.0	79	13.6	88	15.2
Non-manual employee	197	15.6	118	20.3	125	21.5
Self-employed	123	9.7	64	11.0	61	10.5
Professional and higher managerial staff	15	1.2	9	1.6	11	1.9
Father's social class when respondent was 14						
No work	50	4.0	12	2.1	12	2.1
Manual worker	535	42.7	242	41.7	245	42.2
Non-manual employee	316	25.2	143	24.6	142	24.4
Self-employed	305	24.3	141	24.3	139	23.9
Professional and higher managerial staff	47	3.8	29	5.0	27	4.7
Parents' separation when respondent was 14	100	7.9	48	8.3	55	9.5
Siblings						
No brothers/sisters	181	14.2	69	11.9	74	12.7
One brother/sister	553	43.2	267	46.0	259	44.6
Two or more brothers/sisters	545	42.6	245	42.2	248	42.7
Macroarea of residence[a]						
North-West	258	20.2	101	17.4	101	17.4
North-East	292	22.8	141	24.3	141	24.3

Centre	238	18.6	96	16.5	101	17.4
South/Islands	491	38.4	243	41.8	238	41.0
Left the parental home	168	13.1	73	12.6	75	12.9
Calendar period						
before 1994	344	26.9	134	23.1	127	21.9
1994-2003	512	40.0	186	32.0	176	30.3
2004-2009	257	20.1	140	24.1	142	24.4
after 2009	166	13.0	121	20.8	136	23.4

Source: own elaboration on survey data.

Note: the sum of the different categories is not always equal because of missing data.

[a] The area of residence was collected at the time of the interview. However, it is relatively trouble-free to use the macroarea of residence as a time-constant covariate because Italian internal mobility has been low over recent decades and mainly relegated within short distances only (Reynaud and Conti, 2011).