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# Employment protection legislation and household formation: evidence from Italy

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## Abstract

While many studies have investigated the determinants of household formation and fertility in young adults, only a few have focused on the impact of employment protection legislation (EPL) on these outcomes. Moreover, to the best of our knowledge, no one has investigated how changes in EPL may alter the intentions of young individuals even before their realization. In this paper, we study the differential impact of the reduction in EPL associated with Italy's 2015 Jobs Act on the household formation and fertility intentions of young Italians in various districts. We use data from a survey conducted on a sample of young people between the ages of 18-34 for the years 2012, 2015, 2016, and 2017. The identification strategy exploits local variation in the level of efficiency of the courts, measured in terms of the average duration of proceedings, to assess the existence of within-country and across-district heterogeneity in the reform's impact. Indeed, firing costs used to be relatively greater in districts characterized by a longer duration of labor trials. The Jobs Act, by reducing firing costs and modifying the autonomy of judges, should have had a larger impact in districts with less efficient courts. According to our results, the reform seems to have indirectly leveled out the fertility and household formation intentions of young Italians living in districts with more and less efficient courts. Heterogeneities indicate that the results are mainly driven by older individuals and graduates who live in less disadvantaged areas.

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## 1 Introduction

According to Eurostat data, across the European Union in 2020 the average age at which young people leave their parents' homes has been increasing. The average age at the European Union level is 26.4 years. However, there are significant differences between member states, with youths in southern countries tending to stay longer in their parents' homes with respect to their peers in Northern Europe.[1] The determinants of these disparities have been investigated in several papers. For example, Di Stefano (2019) finds that poor labor market conditions and high housing costs drive the choice of youth Italians to remain with their parents. Using data from eleven European countries, Stella (2017) analyzes the relation between parental retirement and son's decision to leave the parental home.[2]

As for fertility, rates have instead been decreasing. The total fertility rate at the European Union level decreased from 1.62 in 2010 to 1.50 in 2020, while in Italy the decline was from 1.46 to 1.24.[3]

Some papers have studied the relationship between employment protection legislation (EPL hereafter) and both household formation and fertility realizations, reporting that when a young worker receives an open-ended contract, this may influence her/his household formation and fertility decisions. For example, Prifti and Vuri (2013) analyzed an early reform in the 1990s that increased employment protection for small firms, finding that women who already had an open-ended contract showed an increase in fertility decisions for Italy. De Paola, Nisticò and Scoppa (2022), analyzing the "Jobs Act" reform (the one considered in this paper), and by comparing women with open ended contracts started before and after the reform, find a negative effect of the reduction in EPL on fertility realizations, measured as the number of maternity leaves taken.[4]

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[1] The average age at which young people leave their parental household is 30.2 years in Italy, 29.8 years in Spain, and 29.6 years in Greece, while it is 21.2 years in Denmark, 22 years in Finland, and 17.5 in Sweden. For further details, see: https://ec.europa.eu/eurostat/statistics-explained/index.php?title=Age_of_young_people_leaving_their_parental_household.

[2] Other examples are Ahn and Sánchez-Marcos (2017) and Aparicio-Fenoll and Oppedisano (2016) for Spain and Chiuri and Del Boca (2010) and Manacorda and Moretti (2006) for Italy.

[3] Source: https://ec.europa.eu/eurostat/statistics-explained/index.php?title=Fertility_statistics.

[4] This literature is related to that investigating the relationship between fertility and household formation and, more broadly, job (in)security. Regarding fertility decisions, see (among others) Adsera (2005) for OECD countries; Ayllón (2019) for Europe; Ahn and Mira (2002), De la Rica and Iza (2005), and Gutiérrez-Domènech (2008) for Spain; Bratti et al. (2005) for Italy; Andersson (2000) for Sweden; and Bhaumik and Nugent (2011) for Germany. Regarding household formation, see Paciorek (2016) for the US; Chiuri and Del Boca (2010) for Europe; Becker et al. (2010) for Italy; Martins and Villanueva (2009) for Portugal; and Martìnez-Granado and Ruiz-Castillo (2002), Aparicio-Fenoll and Oppedisano (2015), and García Ferreira and Villanueva López (2007) for Spain.

However, from the moment a young worker receives an open-ended contract, the realization of intentions may require months or years. In addition, the misalignment between intentions and realizations can be due to both unrealized intentions and unintended pregnancies. As noted by Billari, Philipov and Testa (2009) regarding fertility: "the timing of this proceptive behavior is frequently approximated by the date of start of a pregnancy or the birth of a child. This approximation is evidently problematic, not only because of the delay between the decision to interrupt contraception and actual conception, but also because conceptions include unintended pregnancies that may result in an induced abortion or in an unwanted birth."[5] Household formation can also be affected by difficulties searching for a house and obstacles in accessing a loan. This underscores how, from a policymaker perspective, employment protection legislation can only alter household and fertility intentions, while their future realization also depends on external factors that are not necessarily under policy control.

In addition to that, previous studies that compare individuals with open-ended contracts does not take into account the potential variation in the probability of receiving a permanent position due to the variation in EPL (see Boeri & Garibaldi, 2019).

In this paper, we explicitly address these issues in two ways. Firstly, by focusing our investigation on the short-term impacts of employment protection on both household formation and fertility *intentions* instead of realizations. Secondly, by comparing areas more exposed to the reform with areas less exposed, we are able to identify the overall effect of the reduction in EPL and not the effect given having received an open ended contract.

We exploit the variation provided by the introduction of the 2015 "Jobs Act" reform approved by the government led by Mr. Matteo Renzi, which eliminates the need to bring disputes to the courts in case of dismissal.[6] Our starting hypothesis is that firms have rationally adjusted their hiring strategies depending on the efficiency of the courts in their district. We expect that in districts where the courts are less efficient, the increase in open-ended contracts will be greater with respect to districts where courts are more efficient and where firing costs were thus already limited. Finally, this increase in open-ended contracts may have had an impact on both household formation and fertility intentions.[7]

We retrieve information about household and fertility intentions from a survey carried out by the *"Osservatorio Giovani"* (Youth Observatory), which is part of the

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[5] The difference between fertility intentions and realizations has been widely studied, in particular in the demographics literature. See, for example, Billari et al. (2009), Schoen et al. (1999), and Thomson (1997).

[6] Since the Jobs Act reform, the employee of a company with more than 15 employees (5 in the case of an agricultural company) is not entitled to reinstatement in the event of illegitimate dismissal, but only to compensation of an economic nature that increases with the length of service. The Jobs Act provides for full real protection of the worker in only three cases: null dismissals, discriminatory dismissals, and oral notice of dismissal. Full real protection entails the reemployment of the worker and compensation from the employer for the damage suffered by the worker and for their social security contributions related to that period. For further details, see Section 2.

[7] Bratti et al. (2021) and Boeri and Garibaldi (2019) document an overall increase in open-ended contracts in Italy due to the reduction in employment protection. Moreover, Kugler and Pica (2008) found that the increase in small firms' dismissal costs associated with Italy's 1990 Reform decreased new hires. On the other hand, Gianfreda and Vallanti (2017) report ex post firing costs equivalent to approximately 36 months of wages in Trento (with an average length of labor trials of 313 days) versus 160 months in Salerno (with an average length of labor trials of 1397 days).

Giuseppe Toniolo Institute of Higher Education. This survey was administered in 2012 and then again from 2015 to 2017 amongst young individuals aged 18 to 34. We complement this information with data on court efficiency computed at the district level.[8]

We estimate the effects of the Jobs Act on household formation and fertility decisions using a difference-in-differences strategy, comparing outcomes for individuals living in areas with more and less efficient courts before and after the implementation of the policy. Our results suggest that young individuals living in areas where the courts are less efficient show a greater increase in both their household formation and fertility intentions. This underlines how the positive effect generated by the higher probability of receiving an open-ended contract overcomes the potential negative effect of the reduction in EPL given having received a permanent contract. We also show that the effect is particularly relevant for graduates and older individuals and that our results are mainly driven by individuals living in less disadvantaged areas, who are more able to react to policy changes.

This work is also related to a strand of literature that analyses the effects of the Jobs Act. Among the works that have dealt with the effects of the Jobs Act, of particular importance are Sestito and Viviano (2016), Boeri and Garibaldi (2019), and De Paola et al. (2022).

The rest of the paper is organized as follows. Section 2 introduces the main changes in Italian labor market regulation initiated by the 2015 Jobs Act. In Section 3, we present the data. In Section 4, we describe the identification strategy, while in Section 5 we present the results, some robustness checks, and heterogeneous effects. Section 6 offers some concluding remarks.

## 2 Institutional background

Since the 1990s, there has been an awareness in Europe of the need to make work more flexible. Globalization and competition from companies in countries with very low labor costs have forced governments to find solutions to efficiently adapt work to market needs. In light of this need, in the last two decades the Italian labor market has undergone a profound change from a legislative point of view, as well as from a structural and social point of view.

From 1970, firms with more than 15 employees were only allowed to fire workers with a *justified reason*. For these firms, if a court decided that the layoff was illegitimate, the employer had to pay all wages for the period between the dismissal and the sentence, and then the employee could decide to be reinstated or to receive severance pay for 15 months. As noted by Boeri and Garibaldi (2019), the risk of a long trial that might end with the payment of all wages for the period between the dismissal and the sentence was the main deterrent to the creation of open-ended contracts in large firms. In this regard, Gianfreda and Vallanti (2017) argue that the slowness of judicial

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[8] In Italy, following the 2012 reform of the judicial districts and the subsequent suppression of 31 court seats, there are 140 ordinary court districts located throughout the national territory. The Italian territory is divided into 20 regions (NUTS-2) and 110 provinces (NUTS-3). Thus, on average, each region has 7 districts. The territorial organization into court districts is not based on and does not coincide with regional and provincial districts. Our data are representative of individuals from 136 districts out of 140.

proceedings relating to the labor market increases dismissal costs for employers, who are obliged to bear the cost of longer trials. They report ex post firing costs equivalent to approximately 36 months of wages in Trento (with an average length of labor trials of 313 days) versus 160 months in Salerno (with an average length of labor trials of 1397 days). According to this view, there would therefore be a difference in terms of "de facto" employment protection legislation, and the length of the processes in the matters under discussion would be an important part of the problem.

On the other hand, even in the case of *unjustified dismissal*, firms with a maximum of 15 employees could decide to pay compensation ranging from 2.5 to 14 months of salary, without even paying compensation for wages lost during the trial period (see Berton et al., 2017, 2012, for further details).

The first two reforms to take place subsequently were the so-called "Treu Law" (from the name of the Minister of Labor) in 1997 and the "Biagi Law" (which took its name from Prof. Marco Biagi, the main supporter of this reform) in 2003. Both of these laws tried to introduce more flexibility into the Italian labor market, mainly by introducing new atypical fixed-term employment contracts, while the legislation on open-ended contracts remained basically unchanged.

Later, in 2012, the government led by Prof. Mario Monti approved the law generally known as the "Fornero Law", based on the name of the Minister of Welfare, Prof. Elsa Fornero. Among other changes, this reform modified the condition of reinstatement but left to the courts the decision of whether the dismissal was justified or not.

In 2015, the government led by Mr. Matteo Renzi approved a labor market reform named the "*Jobs Act*", which introduced several changes in labor market regulation. The Jobs Act provided for the introduction of the "contratto a tutele crescenti" (*contract with increasing protection*, CTC hereafter). Under the new CTC, an employee of a company with more than 15 employees (5 in the case of an agricultural company) is not entitled to reinstatement in the event of illegitimate dismissal, but only to compensation of an economic nature that increases with the length of service.[9] However, the reform applies only to contracts beginning after March 7, 2015, or contracts started earlier for firms that reached the 15-employee requirement after that day. In the remaining cases, conditions provided by the "Fornero Law" still apply.

The Jobs Act provides for full real protection of the worker in three cases: null dismissals, discriminatory dismissals, and oral notice of dismissal. Full real protection entails the reemployment of the worker and the payment of compensation by the employer for the damage suffered by the worker and for their social security contributions related to that period. The reduced real protection always provides for reintegration into the workplace, but without compensation for damage. Rather, a value commensurate with the last reference salary is indicated for the calculation of the severance indemnity. This amount may not exceed twelve months' salary, minus what the worker received for other work activities and how much he could have received by accepting an appropriate offer of work. This allowance is provided only in the case of dismissal for a justified subjective reason and right cause, for which it is directly

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[9] In addition to reducing firing costs, the reform also introduced incentives for firms to hire permanent staff. However, while the incentives are the same in all areas of the country, the reduction in firing costs has a differential impact depending on the quality of the courts.

demonstrated in court that there is no material fact alleged against the worker and with respect to which there is no assessment of the disproportionate nature of the dismissal. In cases of unjustified dismissal, only an indemnity is granted (not less than 4 and not more than 24 months of salary).

It is therefore clear that the important difference with the Fornero Law is the role played by the judges. If before the Jobs Act judges still had an important role in verifying whether the conditions for reintegration were met, they no longer have that role. As we have seen, even if the employer decides to dismiss the worker, the CTC already provides for months of salary to be paid to the worker without having to go to court.

## 3 Data

Our main data source is a survey conducted by the *"Osservatorio Giovani"* (Youth Observatory), which is part of the Giuseppe Toniolo Institute of Higher Education.[10] The objective of the survey is to provide a comprehensive and detailed source of information on the new Italian generations and their connections to the transformations taking place in society. For the purposes of our research, we use the survey waves of 2012, 2015, 2016, and 2017.

Our baseline (i.e., pre-reform) information relies on the survey conducted in 2012. These data cover 9,087 individuals representative of the universe of young people aged 19 to 31 years residing in Italy in 2012. In 2015, the sample was completely renewed through interviews of 9,358 young people between 18 and 33 years of age, still representative of the same population. In the next two years, the sample was again the universe of respondents of the 2015 survey; however, each year the attrition rate was about one-third of the original sample.[11] For this reason, we consider our data as a repeated cross-section for a total of 27,180 observations. In order to not inflate our estimates, we have decided to include in our analysis only young adults 25 years of age or over, eliminating 8,426 individuals.[12] The main reason is that younger individuals are less likely to leave the family of origin or decide to have a child compared to older ones.

We use this data to retrieve information on household formation intentions and fertility intentions, as well as contextual indicators such as age, gender, region and town of residence, educational qualifications, parents' qualifications, size of the household, and the sampling weights attributed to individuals. While information about fertility intentions is available for all individuals, 8,695 individuals had

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[10] In 2012, the Giuseppe Toniolo Institute founded the "Osservatorio Giovani" with the aim of investigating the needs and problems of the youth in Italy. The analyzed survey is the main source of information for a yearly report produced by the observatory. This report is the most extensive research on the world of youth in Italy. It is promoted by the Giuseppe Toniolo Institute of Higher Education in collaboration with the Catholic University of the Sacred Heart, Milan, with the support of the Cariplo Foundation and Intesa San Paolo, and it is carried out by Ipsos.

[11] In 2016, 6,172 young people completed the interview, with a retention rate of 66% from 2015 to 2016. In 2017, 3,034 young people participated in the survey, with a redemption rate of 49% from 2016 to 2017.

[12] Some robustness checks on the age threshold choice are provided in Table 10.

**Table 1** Descriptive statistics

| Variables | Mean | Std. Dev. | Min. | Max. |
|---|---|---|---|---|
| Household formation intentions | 0.346 | 0.476 | 0 | 1 |
| Fertility intentions | 0.293 | 0.455 | 0 | 1 |
| Female | 0.599 | 0.490 | 0 | 1 |
| Employed | 0.541 | 0.498 | 0 | 1 |
| Age | 28.527 | 2.652 | 25 | 35 |
| Has siblings | 0.805 | 0.396 | 0 | 1 |
| Education: lower secondary or lower | 0.037 | 0.188 | 0 | 1 |
| Education: higher secondary | 0.438 | 0.496 | 0 | 1 |
| Education: tertiary | 0.525 | 0.499 | 0 | 1 |
| Father's education: lower secondary or lower | 0.353 | 0.478 | 0 | 1 |
| Father's education: higher secondary | 0.491 | 0.500 | 0 | 1 |
| Father's education: tertiary | 0.156 | 0.363 | 0 | 1 |
| Mother's education: lower secondary or lower | 0.389 | 0.488 | 0 | 1 |
| Mother's education: higher secondary | 0.435 | 0.496 | 0 | 1 |
| Mother's education: tertiary | 0.176 | 0.381 | 0 | 1 |
| Youth unemployment rate | 0.307 | 0.121 | 0.060 | 0.720 |
| Sampling weights | 0.887 | 0.845 | 0.126 | 11.525 |
| Obs. | 10,059 | | | |

Dataset: Osservatorio Giovani (2012, 2015, 2016, 2017), Ipsos, Giuseppe Toniolo Institute of Higher Education. Sample: young adults at least 25 years of age

already left their parental home and we exclude them in order to construct a homogeneous sample.[13] The final sample is composed of 10,059 individuals.

The outcomes were constructed by harmonizing questions across different survey waves. Further details regarding the harmonization of the outcomes across survey waves and the robustness of the results to different outcomes can be found in Section A in the Online Appendix. In our preferred specification, we also control for the youth unemployment rate at the provincial level. This information is obtained from the Italian National Bureau of Statistics (Istat hereafter). Descriptive statistics for the final sample are reported in Table 1.

We retrieve information on the inefficiency of courts provided by the Italian Ministry of Justice using the indicator proposed by Istat. Regarding the topic of interest, we refer to "Work and social security", with a particular focus on "Dismissal (individual/collective)" in the private sector. We made this choice in light of the fact that the interest in terms of the performance of the courts relates to a issue and not to

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[13] Table 9 in the Online Appendix reports estimates regarding fertility intentions for the full sample of individuals for which information was available.
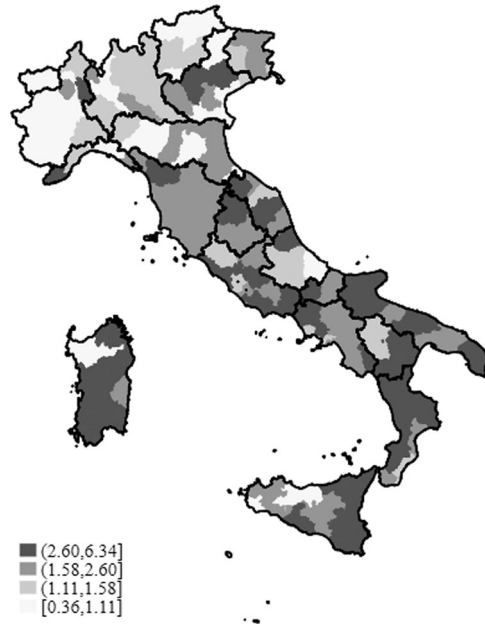
**Fig. 1** Average length of judicial proceedings, 2013–2014. Note: The polygons in the map correspond to Italian districts. To each Italian municipality is attributed an indicator relating to the judicial district to which it belongs. The average length of judicial proceedings is calculated by an index, as explained in Section 3. Source: authors' calculations based on Istat and Ministry of Justice data

all procedures (e.g., ordinary civil, special summary proceedings, voluntary jurisdiction, and so on). The measure is the following:

$$CourtIneff_t = \frac{(P_{t-1} + P_t)}{(F_t + C_t)},$$

where $P_{t-1}$ and $P_t$ are, respectively, the number of cases pending at the beginning and the end of the year. $F_t$ is the number of new cases filed during the year, and $C_t$ is the number of cases that ended with a judicial decision or were withdrawn by the parties during the year. The higher the indicator, the more cases the court accumulated, and vice versa. The indicator can be interpreted as the average duration of court proceedings expressed in years. The data relating to the performance of the courts are those relating to 2014, i.e., the last year before the Jobs Act came into force. The indicator ranges from 0.36 (district of Rovereto, Trentino Alto Adige) to 6.34 (district of Nuoro, Sardinia). The average of our indicator is 2.15. The entire distribution is summarized in Fig. 1.

In Table 7 in the Online Appendix, we show that our indicator is negatively correlated with some economic characteristics such as GDP and the number of firms. This highlights how courts are more inefficient in more economically disadvantaged areas. Correlations with demographic characteristics also show that courts are more inefficient in less populated areas, those with lower population density, and where the average age is lower. Interestingly, we do not find any relevant correlation to the fertility rate.

# 4 Identification strategy

Our identification strategy takes advantage of two sources of variation. The first is determined by the temporal variability of the law. The second is given by the different characteristics of the districts that make them more exposed to the nationwide reform. According to our hypothesis, in the districts where there was a greater degree of employment protection legislation due to the poorer performance of the courts, the labor market reform should have had a greater impact, at least in terms of the number of new open-ended contracts with respect to the pre-reform period.

If the reform has a direct impact on firms, and therefore on employers' choices regarding hiring and firing, this in turn affects the employment opportunities of young people, and consequently, their life choices.

As explained in Section 2, in order to be directly exposed to the Jobs Act, a young person must have received a permanent contract in a firm with at least 15 employees after March 7, 2022. Unfortunately, our data does not allow us to estimate the effective take-up[14], and our results have to be interpreted as reduced-form estimates. However, it is also true that young adults' intentions may also change in an indirect way. As an example, the household formation intentions of a young adult may also change after her/his partner gets a permanent job.

The objective is to verify whether in the post-reform period there is a difference in family formation and fertility intentions between places where court efficiency is notoriously low and those where it is high. To do so, we use a difference-in-differences estimation strategy that assumes the following form:

$$Y_{ict} = \alpha + \beta(CourtIneff_c \times Post_t) + \delta X_{ict} + \mu_c + \tau_t + \epsilon_{ict}, \tag{1}$$

where the coefficient of interest is $\beta$, which is the coefficient associated with the interaction between the indicator of court inefficiency ($CourtIneff_c$) and the post-treatment dummy ($Post_t$). This coefficient provides the effect on the outcome that results from an increase in treatment intensity. Therefore, looking at $\beta$ we can see the differential effect of the reform for those who live in a district with a very efficient court compared to those who live in a district with an inefficient court. $X_{ict}$ controls for observable individual characteristics such as gender, age, employment status, presence of siblings, educational qualifications, and parents' educational qualifications, as well as youth unemployment on a provincial basis in order to capture macroeconomic characteristics of the place in which they live. $\mu_c$ and $\tau_t$ are district and time fixed effects, respectively. Standard errors are clustered at the court level. We weight each observation by the corresponding sampling weight. Coefficients related to the variables $CourtIneff_c$ and $Post_t$ cannot be separately identified because they are absorbed by district and time fixed effects, respectively. Only in some specifications in which we control for regional fixed effects instead of district fixed effects (higher level of granularity) the coefficient related to $CourtIneff_c$ can be identified. We report these estimates accordingly.

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[14] To the best of our knowledge, no data on the 15-employee threshold are available. Indeed, the companies for which individuals work are not declared in the survey and we do not know when contracts begin. The only available information is the rate of firms with at least 10 employees at the provincial level.

Two potential concerns may invalidate our identification strategy: (i) other changes stemming from the Jobs Act that are not directly related to the reduction in EPL (e.g., the hiring incentives that were provided in the initial phase); (ii) districts with different levels of court inefficiency may not show a common trend in terms of both household formation and fertility intentions in the pre-reform period. Regarding the first concern, all other changes introduced by the Jobs Act were applied in the same way in all of Italy and do not depend on court inefficiency. For this reason, we can assume that these changes are absorbed by time fixed effects.

Regarding the common trend assumption, unfortunately, our data do not allow us to directly test whether different districts showed a common trend before the intervention because data are only available for one year in the pre-reform period. We address this issue in three different ways. First, we show that regions with different levels of court inefficiency showed common trends in terms of both household and fertility realizations before the Jobs Act. Then, we randomly allocate the court inefficiency indicator across districts 1,000 times and report the distribution of the estimated coefficients, showing that no mechanical effects are in place. Third, we construct a fake difference-in-difference between the years 2016 and 2017 (post-reform years only), which shows no differential effects. Finally, we test the balancing of the covariates by running a series of the regressions using the main specification described in Eq. (1) but putting one covariate at a time in the left side, in the spirit of the test proposed by Pei, Pischke and Schwandt (2019). All results are reported in Section 5.1.[15]

## 5 Results

Table 2 reports our main results, divided into two panels: Panel A includes results regarding household intentions, while Panel B is about fertility intentions. All specifications are for the sample of individuals at least 25 years of age for which we have been able to retrieve information about outcomes and controls. Table 2 includes regressions with regional fixed effects (columns 1 and 2 of both panels) and with district fixed effects (columns 3 and 4). In both cases, results are shown without (columns 1 and 3) and with (columns 2 and 4) additional controls. Controls include gender, age, employment status, presence of siblings, educational qualifications, and parents' educational qualifications, as well as the provincial youth unemployment rate. In all specifications, standard errors are clustered at the district level. Each observation is weighted by the corresponding sampling weight.

The first row is related to the coefficient of interest, which is the interaction term between the post-reform dummy and the court inefficiency indicator. Both panels show a positive relationship with the outcome of interest, indicating that the introduction of the Jobs Act has altered household formation and fertility intentions more in areas with less efficient courts. The positive sign of the interaction term can be interpreted as the reform having had a positive impact on household formation intentions. In our preferred specification (column 4), the result regarding household

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**Table 2** Main results

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Panel A: Household formation intentions | | | | |
| CourtIneff × Post | 0.0437* | 0.0431* | 0.0396* | 0.0383* |
| | (0.0263) | (0.0222) | (0.0228) | (0.0203) |
| CourtIneff | −0.0428* | −0.0431** | | |
| | (0.0225) | (0.0186) | | |
| Time = 2015 | −0.1296 | −0.1401* | −0.1060 | −0.1143* |
| | (0.0832) | (0.0728) | (0.0701) | (0.0641) |
| Time = 2016 | 0.0534 | 0.0364 | 0.0802 | 0.0689 |
| | (0.0833) | (0.0717) | (0.0703) | (0.0620) |
| Time = 2017 | 0.0148 | −0.0066 | 0.0400 | 0.0269 |
| | (0.0876) | (0.0756) | (0.0757) | (0.0662) |
| Observations | 10,059 | 10,059 | 10,059 | 10,059 |
| R-squared | 0.0384 | 0.0539 | 0.0952 | 0.1026 |
| Panel B: Fertility intentions | | | | |
| CourtIneff × Post | 0.0524** | 0.0460** | 0.0440** | 0.0406** |
| | (0.0259) | (0.0206) | (0.0220) | (0.0184) |
| CourtIneff | −0.0373* | −0.0314* | | |
| | (0.0219) | (0.0172) | | |
| Time = 2015 | −0.0327 | −0.0428 | −0.0212 | −0.0339 |
| | (0.0849) | (0.0646) | (0.0692) | (0.0587) |
| Time = 2016 | −0.0111 | −0.0217 | 0.0019 | −0.0128 |
| | (0.0864) | (0.0652) | (0.0716) | (0.0589) |
| Time = 2017 | −0.0167 | −0.0333 | −0.0018 | −0.0242 |
| | (0.0889) | (0.0676) | (0.0752) | (0.0612) |
| Observations | 10,059 | 10,059 | 10,059 | 10,059 |
| R-squared | 0.0187 | 0.0906 | 0.0902 | 0.1444 |
| Age | ≥25 | ≥25 | ≥25 | ≥25 |
| Region FE | √ | √ | | |
| District FE | | | √ | √ |
| Controls | | √ | | √ |
| Cluster | District | District | District | District |

Note: In Panel A, the dependent variable is the dummy related to household formation intentions. In Panel B, the dependent variable is the dummy related to fertility intentions. The variable *CourtIneff*Post* is the difference-in-differences interaction term between the indicator of court inefficiency and the year dummy. The variable *CourtIneff* represents the indicator of court inefficiency. *Time* represents the time dummies for 2015, 2016 and 2017 survey (baseline 2012). Columns (1) and (2) control for regional fixed effects (FE), while columns (3) and (4) control for district FE. Columns (2) and (4) include additional controls, such as gender, age, employment status, presence of siblings, educational qualifications, and parents' educational qualifications, as well as the provincial youth unemployment rate. The sample weights are applied. Standard errors are clustered at the district level

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

formation intentions is statistically significant at the 10% level, while for fertility intentions it is significant at the 5% level. The size of the coefficients means that an increase of one year in the average length of disputes generates increases in the share of youths with positive household formation and fertility intentions of 3.83% and 4.06%, respectively. Table 2 also reports the coefficient related to the court inefficiency indicator in two columns in which it is not absorbed by district fixed effects.

The negative and statistically significant (for the case of household formation) value underscores how in places with greater court inefficiency household formation and fertility intentions were much lower in the pre-reform period. This may suggest that the reform has balanced previous disparities in some sense. Finally, Table 2 also reports coefficients related to time dummies, although no relevant trends emerge.

## 5.1 Placebo tests and robustness checks

To validate our empirical approach, we run a battery of placebo and robustness checks. Here we present only the main ones, while the Online Appendix contains further robustness checks. The difference-in-differences estimator is valid under the assumption of a common trend between treatment and comparison groups in the absence of the reform. Since we only have one year of survey available in the pre-reform period, we cannot investigate the common trend assumption directly. Thus, to provide evidence supporting the validity of the design, we perform some placebo tests.

First, we take data from Istat on household formation and fertility *realizations* and check whether places with different levels of court inefficiency were on similar paths (i.e., common trends) in the pre-reform period. The indicator regarding household formation is constructed by taking regional data (NUTS-2 level) on the share of individuals still living with their parents and taking the complement, while the fertility realizations are constructed by taking provincial data (NUTS-3 level) on the total fertility rate per 1,000 inhabitants. Finally, the observations are divided into 3 groups: (i) the places where the courts are the most inefficient (top 25%), (ii) the mid-level-inefficiency courts (between 25% and 75%), and (iii) the least inefficient courts (bottom 25%). Results are reported in Fig. 2. The left graph reports results regarding household formation, while the one on the right refers to fertility realizations. Results show that the household formation indicator is quite stable across time, while the one for fertility decreases across time. However, even though regions show different averages of both indicators, the changes across time are perfectly comparable, supporting the common trend assumption.

The second placebo test is related to the policy in question and focuses on the identification strategy adopted. Our identification strategy is based on the idea that the Jobs Act was more likely to have a deeper impact in areas characterized by a relatively higher level of court inefficiency. In order to control for the fact that our indicator could be spuriously correlated with the outcome of interest, we created a new court efficiency indicator by keeping the true values of the indicator and randomly shuffling them across districts. At this point, we randomly distribute our "placebo" indicator to the various districts, and therefore to the young people living in the various districts. We replicated this random allocation 1,000 times. Results are reported in Fig. 3.

The left graph shows the distribution of the results of the random allocation for household formation intentions, while the right one shows the same distribution for fertility intentions. The independent variable, *CourtIneff\*Post (placebo)*, is the variable of interaction between the fake treatment indicator and the time dummy. In both figures, the average of the estimated coefficients is centered at zero, and this shows that the relationship is not mechanical and automatic. However, the number of districts is low and, therefore, in the various simulations it can happen that by chance some combinations are very similar to the true one, making the true effect (reported as a black line) fall back inside the range.
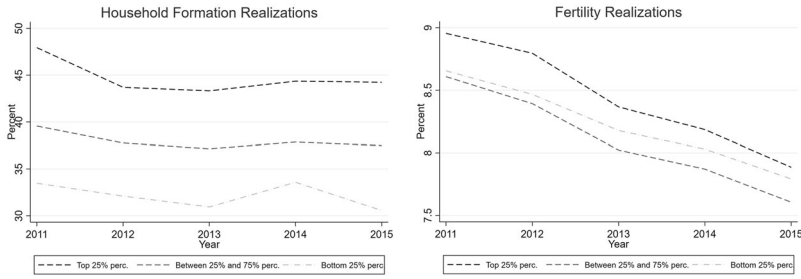
**Fig. 2** Evolution of household formation and fertility realizations in the five years before the Jobs Act. Note: We use data produced by Istat on household formation and fertility realizations in the five years before the reform. We plot the average of each indicator in the most inefficient courts (top 25%), in the mid-level-inefficiency courts (between 25% and 75%), and in the most efficient courts (bottom 25%). The left graph refers to household formation and the right graph to fertility realizations
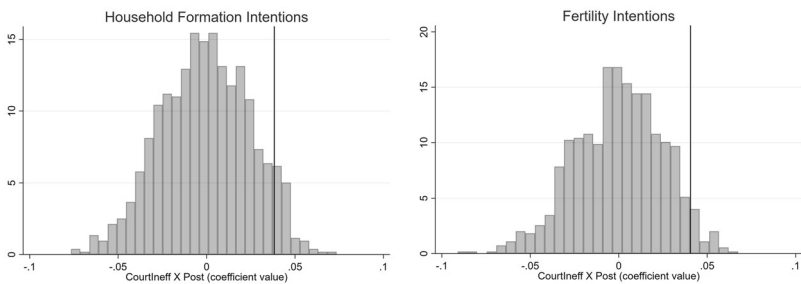


**Fig. 3** Random allocation of the court efficiency indicator. Note: We randomly allocate the indicator of court inefficiency across districts 1,000 times, and we estimate the specifications proposed in column (4) of Table 2. The plots shows the distribution of the coefficient related to the variable *CourtIneff*Post*. The left graph is related to household formation, while the right one refers to fertility intentions. The vertical black line corresponds to the true estimated value of the coefficient, reported respectively in column (4) Panel A (0.0383*) and Panel B (0.0406**) of Table 2

In the same spirit, Fig. 4 reports the results of the test proposed by Pei et al. (2019). The test is computed by progressively excluding an *X* variable from the right side of Eq. (1) and using it as a placebo outcome. This test should highlight potential sources of unobservable bias by capturing an imbalance of these variables.[16]

Reassuringly, almost all coefficients show no significant relationship with the dependent variable *CourtIneff*Post* (only two coefficients are slightly significant).

In the last placebo, we shift the pre-reform period to a post-reform year in order to see whether significant paths emerge between the two time periods in which the same legislative conditions applies. We use 2016 as the (placebo) pre-treatment period and 2017 as the post-treatment period. In this way, the dummy *Post* takes a value of 0 in 2016 and a value of 1 in 2017. Results are reported in columns (2) and (4) of Table 3, while columns (1) and (3) report results from column (4) of Table 2 for comparison. As can be seen from the magnitude and significance of the coefficients, no significant results were found here either.

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[16] The full set of results is available in Table 8 in the Online Appendix.
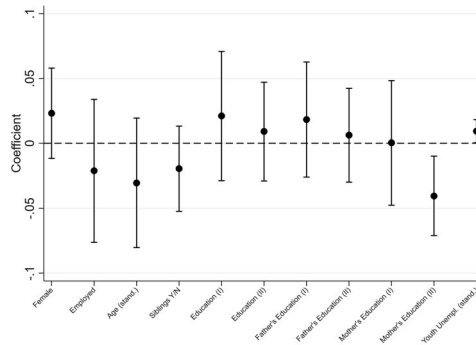
**Fig. 4** Test of covariate balance. Note: The figure reports the estimates of the coefficients related to the variable *CourtIneff\*Post* as per Eq. (1). Each regression is computed removing one covariate and using it as the dependent variable, as suggested by Pei et al. (2019). Dots refer to point estimates, while vertical lines represent 95% confidence intervals

Besides the validation tests of our design, we also perform robustness checks to test for incorrect specifications; these can be found in the Online Appendix. Sensitivity to different dependent variables (aggregating the variables of the questionnaire differently) are reported in Table 6. In Table 10, we provide an indication of sensitivity to the selected age threshold. In the first specification, we include individuals at least 24 years of age and find that the estimated coefficients are lower with respect to our main specification. In the second specification, we include individuals at least 26 years of age, which leads to results of a higher magnitude. This supports our assumption that the outcomes of younger individuals may be unaffected. Further robustness checks investigate possible links between the dependent variables and aspects of the macro-economic scenario, such as the youth unemployment rate. Table 11 provides a check of robustness to polynomial order, excluding any non-linear relation between the outcomes and our variable of interest. Finally, since the post-reform surveys (2015, 2016, and 2017) were conducted starting from the same sample, in Table 12 we exclude the survey conducted in 2017 in order to avoid that our results be mainly driven by the potential self-selection of respondents. The results are substantially unchanged.

## 5.2 Heterogeneous effects

It is interesting to investigate the possible existence of heterogeneous effects. The lines of heterogeneity that we decided to investigate are essentially related to the characteristics of individuals and the environment in which they live. Table 4 reports the first set of results. Panel A refers to household formation, while Panel B presents the results regarding fertility intentions.

The first two columns show the coefficients of interest calculated on the sub-samples of men (column (1)) and women only (column (2)). It is interesting to note that the results do not show any particular differences between men and women. Columns (3) and (4) refer to individuals between 25 and 26 years of age and to individuals at least 27 years of age, respectively. The results show that the effect is entirely driven by older individuals, underscoring how younger individuals' intentions may not be affected by an open-ended contract. In columns (5) and (6), we split

**Table 3** Shift in the year of implementation of the reform

|  | Household formation intentions | | Fertility intentions | |
|---|---|---|---|---|
|  | (1) | (2) | (3) | (4) |
| *CourtIneff × Post* | 0.0383* |  | 0.0406** |  |
|  | (0.0203) |  | (0.0184) |  |
| *CourtIneff × Post* (placebo) |  | −0.0005 |  | −0.0042 |
|  |  | (0.0173) |  | (0.0134) |
| Observations | 10,059 | 4,053 | 10,059 | 4,053 |
| R-squared | 0.1026 | 0.1259 | 0.1444 | 0.1660 |
| Years | All | 2016–2017 | All | 2016–2017 |
| District FE | √ | √ | √ | √ |
| Time FE | √ | √ | √ | √ |
| Controls | √ | √ | √ | √ |
| Cluster | District | District | District | District |

Note: In column (1) to column (2), the dependent variable is the dummy related to household formation intentions, while in column (3) to column (4) the dependent variable is the dummy related to fertility intentions. The main results are reported in columns (1) and (3) (as reported in Table 2, column (4), Panel A and Panel B, respectively) for comparison purposes. The variable *CourtIneff*Post* is the interaction term as in equation (1), while *CourtIneff*Post* (placebo) is the same interaction but shifted (it takes a value of 0 in 2016 and 1 in 2017). The specification is as in column (4) of Table 2

\*\*\*$p < 0.01$; \*\*$p < 0.05$; \*$p < 0.1$

the sample between the sub-populations of graduates and non-graduates. The only significant relationships emerge when we consider the subsample of graduated individuals. This is probably due to the fact that these individuals enter into the labor market at an older age. The last two columns show the results for individuals who are employed or not employed. The results show that the effects are entirely driven by employed individuals. This may suggest that the effect can be driven by the individuals that have received an open-ended contract thanks to the reduction in EPL. Unfortunately, our data do not allow us to directly test this mechanism.

Table 5 investigates potential heterogeneous effects according to a battery of characteristics relative to the territory in which people live. Panel A shows results regarding household formation intentions, while Panel B refers to fertility intentions.

Columns (1), (2), and (3) show the results for different geographical areas: Northern, Central, and Southern Italy. The results show that our main estimates are entirely driven by the subsample of individuals from Northern and Central Italy, while no relationships are found when we consider Southern Italy. This is not surprising, as Southern Italy is characterized by a higher youth unemployment rate, lower female labor market participation, and stronger family ties (see, among others, Battistin et al., 2014). In columns (4) and (5), we divide the sample according to the youth unemployment rate at the provincial level, using the median value as a threshold. We find that the reform impacted only those who live in territories with a youth unemployment rate lower than the median value. This is in line with the relationship demonstrated in the first three columns: where unemployment is high and the socioeconomic characteristics of the territory are negative, the

**Table 4** Heterogeneous effects - individual characteristics

| | (1) Men | (2) Women | (3) Youngest | (4) Oldest | (5) Graduate | (6) Non-graduate | (7) Employed | (8) Not employed |
|---|---|---|---|---|---|---|---|---|
| **Panel A: Household formation intentions** | | | | | | | | |
| $Courtheff \times Post$ | 0.0393** | 0.0275 | −0.0001 | 0.0627** | 0.0491** | 0.0083 | 0.0522** | 0.0025 |
| | (0.0196) | (0.0232) | (0.0198) | (0.0255) | (0.0201) | (0.0189) | (0.0202) | (0.0196) |
| Observations | 4,030 | 6,026 | 2,824 | 7,232 | 5,284 | 4,775 | 5,438 | 4,621 |
| R-squared | 0.1230 | 0.1605 | 0.1699 | 0.1290 | 0.2189 | 0.1107 | 0.1759 | 0.1217 |
| **Panel B: Fertility intentions** | | | | | | | | |
| $Courtheff \times Post$ | 0.0233 | 0.0359 | −0.0093 | 0.0614** | 0.0436*** | 0.0198 | 0.0693*** | 0.0055 |
| | (0.0167) | (0.0223) | (0.0151) | (0.0262) | (0.0166) | (0.0152) | (0.0247) | (0.0164) |
| Observations | 4,030 | 6,026 | 2,824 | 7,232 | 5,284 | 4,775 | 5,438 | 4,621 |
| R-squared | 0.1518 | 0.1772 | 0.2106 | 0.1527 | 0.2255 | 0.1636 | 0.1678 | 0.1521 |
| age | ≥25 | ≥25 | ≥25 and ≤26 | ≥27 | ≥25 | ≥25 | ≥25 | ≥25 |
| District FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Cluster | District | District | District | District | District | District | District | District |

The table shows the results of the same specification as in column (4) of Table 2 for different subsamples. Column (1) includes only males and column (2) only females; column (3) only includes individuals between 25 and 26 years of age; column (4) refers to individuals at least 27 years of age; column (5) includes only graduates; and column (6) only includes non-graduates. Panel A refers to household formation intentions. Panel B refers to fertility intentions

Results in the overall specifications (for comparison purposes): household formation intentions $= Courtheff*Post = 0.0383*$; fertility intentions $= Courtheff*Post = 0.0406**$

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

**Table 5** Heterogeneous effects - territorial characteristics

| | (1) Northern Italy | (2) Central Italy | (3) Southern Italy | (4) Yunemp(high) | (5) Yunemp(low) | (6) %firms(low) | (7) %firms(high) |
|---|---|---|---|---|---|---|---|
| **Panel A: Household formation intentions** | | | | | | | |
| $Courtneff \times Post$ | 0.0617* | 0.0555 | −0.0013 | 0.0096 | 0.0706*** | 0.0107 | 0.0529** |
| | (0.0332) | (0.0357) | (0.0175) | (0.0216) | (0.0242) | (0.0205) | (0.0237) |
| Observations | 3,861 | 1,546 | 4,652 | 5,506 | 4,551 | 3,289 | 6,769 |
| R-squared | 0.1382 | 0.0976 | 0.0932 | 0.1179 | 0.1338 | 0.1034 | 0.1117 |
| **Panel B: Fertility intentions** | | | | | | | |
| $Courtneff \times Post$ | 0.0350 | 0.0495 | −0.0021 | −0.0280* | 0.0546* | 0.0015 | 0.0465* |
| | (0.0288) | (0.0306) | (0.0156) | (0.0167) | (0.0304) | (0.0133) | (0.0237) |
| Observations | 3,861 | 1,546 | 4,652 | 5,506 | 4,551 | 3,289 | 6,769 |
| R-squared | 0.1808 | 0.1597 | 0.1333 | 0.1367 | 0.1784 | 0.1469 | 0.1522 |
| District FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Cluster | District | District | District | District | District | District | District |

The table shows the results of the same specification as in column (4) of Table 2 for different subsamples. Column (1) includes only individuals from Northern Italy, column (2) only from Central Italy, and column (3) only from Southern Italy. Columns (4) and (5) refer to subsamples of individuals who live in provinces with levels of youth unemployment above and below the median, respectively. Columns (4) and (5) are obtained by dividing the entire sample by the percentage of firms above the threshold of 10 employees (below and above the median level, respectively). Panel A refers to household formation intentions. Panel B refers to fertility intentions

Results in the overall specifications (for comparison purposes): household formation intentions = $Courtneff*Post = 0.0383*$; fertility intentions = $Courtneff*Post = 0.0406**$

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

reform does not seem to have had an impact. Instead, it seems to have had an impact where the conditions were not as negative in terms of labor market opportunities. Finally, in columns (6) and (7) we divide the sample according to the number of firms above the 10-employee threshold out of the total number of firms in the province. Although the 10-employee threshold is used as a proxy, the Jobs Act only impacted those working in a company with more than 15 employees. Also in this case, we note that the reform seems to particularly have had an impact in provinces with a greater number of firms above the threshold. As we would have expected, in provinces with few firms above the threshold, the coefficient is not statistically significant. This result is consistent with our hypothesis regarding the mechanism of indirect transmission between the law, courts, employers, and the youth. Where there is a low presence of big firms, the reform does not impact young people's intentions because they do not perceive a positive indication in terms of increased chances of finding a job—no matter how efficient the courts are.

## 6 Concluding remarks

Using a representative sample, this paper examines the differential impact of the EPL reduction resulting from the 2015 Italian Jobs Act on the household formation and fertility intentions of young Italians.

As a prerequisite for this analysis, we started from the fact that in Italy there was within-country variation in terms of de facto firing costs. This difference is mainly due to the different degrees of efficiency of the courts located in the various districts. Since in the pre-reform period it was up to the judge to decide matters relating to individual and collective dismissals, the timing of justice was an important factor for the employer in terms of choices about hiring and of the type of contract. The Jobs Act, with its contraction of protections, no longer provides for dismissals to be decided by a judge, and therefore changes the institutional setting faced by entrepreneurs.

We find that in the post-reform period, the household formation and fertility intentions of young adults improved more in places where the courts were less efficient in the pre-reform period than in places where the courts were more efficient. Studying the existence of heterogeneous effects, we further show that the reform impacted above all the intentions regarding fertility and household formation of older youths and those with tertiary education who have graduated. Moreover, the effect is mainly due to individuals living in northern and central regions, which have a lower youth unemployment rate and a higher concentration of firms with more than 10 employees.[17]

These results raise the question of an indirect effect of the reform on young adults' perceptions of security. The reform may have had an impact on the choices of entrepreneurs in areas with less efficient courts, i.e., those that have benefited most from the change in legislation. As a consequence, this may have influenced the perceptions of young adults. While many studies have investigated the determinants

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[17] We used the 10-employee threshold as a proxy for the 15-employee threshold, which would be the correct threshold but for which no data is available. This result is in line with the provisions of the Jobs Act. In fact, as can be seen in the section discussing the institutional background, the Jobs Act has changed the dismissal regulations for companies with more than 15 employees.

of household formation and fertility for young adults, to the best of our knowledge no one had focused on the differential effects of labor market reform on their household formation and fertility intentions.

From a policy perspective, these results suggest that the increase in open-ended contracts due to the reduction of employment protection may also generate positive and unexpected externalities, such as modifying household formation and fertility choices of youths.

**Compliance with ethical standard**

**Conflict of interest** The authors declare no competing interests.

**Consent for publication** The Authors hereby consent to publication of the Work. The Authors warrant that the Work has not been published before in any form, that the Work is not being concurrently submitted to and is not under consideration by another publisher, that the persons listed above are listed in the proper order and that no author entitled to credit has been omitted.

**Ethics approval** All procedures performed in studies involving human participants were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards.

# 7 Appendix

## 7.1 Outcome construction

For both household formation intentions and fertility intentions, in some years the survey asked young adults to answer questions based on a period of one year if they gave a positive answer to questions about a timeframe of three years. In addition, in some years there were three possible answers, while in others there were four. Before starting the analysis, we thus homogenized the questions and answers. The following is a list of the individual questions and explanations of how they have been harmonized.

**Household formation intentions - 2012 survey**
**Q1: In the next 3 years, do you plan to live alone or with friends?**

A. Almost certainly.
B. I believe so.
C. I believe not.
D. I exclude it.

**Q2 [If Q1 = A or B] Do you plan to live alone (or with friends) within a year?**

A. Yes, it is very likely.
B. Probably not.
C. Definitely not, certainly later.

In this case, we code the answer as 0 if Q1 = C or D or Q2 = B or C, 1 if Q2 = A.

**Household formation intentions – 2015 survey**
**Q1: Do you plan to live alone (or with friends) within a year?**

A. Yes, it is very likely.
B. Probably not.
C. Definitely not, certainly later.

In this case, we code the answer as 0 if Q1 = B or C, 1 if Q1 = A.

**Household formation intentions – 2016 survey**
**Q1: Do you plan to move out of the parental home to live alone (or with friends) within the next year?**

A. I exclude it.
B. I believe not.
C. I believe so.
D. Almost certainly.

In this case, we code the answer as 0 if Q1 = A or B, 1 if Q1 = C or D.

**Household formation intentions – 2017 survey**

**Q1: Do you plan to leave the family home to move out on your own, with friends, or to move in with a partner within the next 3 years?**

A. I exclude it.
B. I believe not.
C. I believe so.
D. Almost certainly.

**Q2 [If Q1 = A or B] Do you plan to move out of the family home within the next year?**

A. I exclude it.
B. I believe not.
C. I believe so.
D. Almost certainly.

In this case, we code the answer as 0 if Q1 = A or B or Q2 = A or B, 1 if Q2 = C or D.

**Fertility intentions – 2012 survey**

**Q1: In the next 3 years, do you plan to have (another) child?**

A. Almost certainly.
B. I believe so.
C. I believe not.
D. I exclude it.

In this case, we code the answer as 0 if Q1 = C or D, 1 if Q1 = A or B.

**Fertility intentions – 2015, 2016, and 2017 surveys**

**Q1: In the next 3 years, do you plan to have (another) child?**

A. I exclude it,
B. I believe not.
C. I believe so.
D. Almost certainly.

In this case, we code the answer as 0 if Q1 = A or B, 1 if Q1 = C or D.

In Table 6, we report the robustness of the results to the choice of the outcome variable. More specifically, we use the same rules explained above, with the difference that we also code as 1 the answers "Probably not" and "I believe not" with respect to household formation intentions and the answer "I believe not" with respect to fertility intentions. In this way, we isolate only strongly negative responses. In both cases, the results lose statistical significance, and in the case of fertility intentions, the size of the coefficient also shrinks to zero. This highlights how our main effects are driven by individuals with strong preferences.

**Table 6** Robustness to different specifications of the outcomes

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Household formation intentions | | Fertility intentions | |
| *CourtIneff* × *Post* | 0.0383* | 0.0409 | 0.0406** | 0.0068 |
| | (0.0203) | (0.0257) | (0.0184) | (0.0202) |
| Observations | 10,059 | 10,059 | 10,059 | 10,059 |
| R-squared | 0.1026 | 0.0917 | 0.1444 | 0.1116 |
| District FE | √ | √ | √ | √ |
| Time FE | √ | √ | √ | √ |
| Controls | √ | √ | √ | √ |
| Cluster | District | District | District | District |

In this table, we consider alternative definitions of the outcome variables. Columns (1) and (2) refer to household formation, while columns (3) and (4) are about fertility intentions. The main results are reported in columns (1) and (3) for comparison purposes, as reported in Table 2, column (4), Panel A and Panel B, respectively. Columns (2) and (4) consider the alternative definitions. The specification is as in column (4) of Table 2

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

# 8 Additional robustness checks

Tables 7, 8, 9, 10, 11, and 12.

**Table 7** Correlations with the *CourtIneff* indicator

| GDP | Number of active firms | Fertility rate |
|---|---|---|
| −0.542 | −0.192 | −0.012 |
| Population | Pop. density | Pop. age |
| −0.145 | −0.213 | −0.266 |

This table reports the correlations of some demographic and economic characteristics to our main independent variable, i.e., *CourtIneff*. All variables refer to 2014 and are collected at the NUTS-3 level (provinces) by Eurostat (GDP, number of active firms, population, population density, and population age) and Istat (fertility rate)

**Table 8** Full set of coefficients of Fig. 4

|  | Female | Employment | Age (stand.) | Has siblings |
|---|---|---|---|---|
| *CourtIneff × Post* | 0.0231 | −0.0212 | −0.0304 | −0.0196 |
|  | (0.0178) | (0.0281) | (0.0254) | (0.0167) |
| Observations | 10,059 | 10,059 | 10,059 | 10,059 |
| R-squared | 0.1354 | 0.1909 | 0.2155 | 0.1354 |
| District FE | √ | √ | √ | √ |
| Time FE | √ | √ | √ | √ |
| Controls | √ | √ | √ | √ |
| Cluster | District | District | District | District |

|  | Education | | Father's education | |
|---|---|---|---|---|
|  | Higher sec. | Tertiary | Higher sec. | Tertiary |
| *CourtIneff × Post* | 0.0211 | 0.0091 | 0.0183 | 0.0063 |
|  | (0.0254) | (0.0194) | (0.0227) | (0.0184) |
| Observations | 10,059 | 10,059 | 10,059 | 10,059 |
| R-squared | 0.1231 | 0.1807 | 0.2038 | 0.3192 |
| District FE | √ | √ | √ | √ |
| Time FE | √ | √ | √ | √ |
| Controls | √ | √ | √ | √ |
| Cluster | District | District | District | District |

|  | Mother's education | | |
|---|---|---|---|
|  | Higher sec. | Tertiary | Youth unempl. |
| *CourtIneff × Post* | 0.0003 | −0.0405** | 0.0093** |
|  | (0.0245) | (0.0156) | (0.0046) |
| Observations | 10,059 | 10,059 | 10,059 |
| R-squared | 0.1945 | 0.2953 | 0.9165 |
| District FE | √ | √ | √ |
| Time FE | √ | √ | √ |
| Controls | √ | √ | √ |
| Cluster | District | District | District |

This table shows all the estimates reported in Fig. 4. The specification is as in column (4) of Table 2

**Table 9** Fertility intentions – full sample estimates

| | Fertility intentions | |
|---|---|---|
| | (1) | (2) |
| CourtIneff × Post | 0.0406** | 0.0278* |
| | (0.0184) | (0.0146) |
| Observations | 10,059 | 18,754 |
| R-squared | 0.1444 | 0.1273 |
| District FE | √ | √ |
| Time FE | √ | √ |
| Controls | √ | √ |
| Cluster | District | District |

This table reports the estimates as in Panel A of Table 2 on fertility intentions. Column (1) reports the results as in column (4) of Panel B in Table 2 for comparison purposes. In column (2), we include all individuals for whom information about fertility intentions were available and not only individuals who still live with their parents

\*\*\*$p < 0.01$; \*\*$p < 0.05$; \*$p < 0.1$

**Table 10** Robustness test on age threshold

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Household formation intentions | | | Fertility intentions | | |
| CourtIneff × Post | 0.0383* | 0.0326* | 0.0577*** | 0.0406** | 0.0313* | 0.0570*** |
| | (0.0203) | (0.0171) | (0.0218) | (0.0184) | (0.0163) | (0.0217) |
| Observations | 10,059 | 11,560 | 8,568 | 10,059 | 11,560 | 8,568 |
| R-squared | 0.1026 | 0.1007 | 0.1125 | 0.1444 | 0.1460 | 0.1501 |
| District FE | √ | √ | √ | √ | √ | √ |
| Time FE | √ | √ | √ | √ | √ | √ |
| Controls | √ | √ | √ | √ | √ | √ |
| Cluster | District | District | District | District | District | District |

In this table, we provide some robustness checks regarding the chosen age threshold. Columns (1), (2), and (3) refer to household formation, while columns (4), (5), and (6) refer to fertility intentions. For comparison purposes, the main results as reported in Table 2, column (4), Panel A and Panel B, are reported in columns (1) and (3), respectively. Columns (2) and (4) include individuals at least 24 years of age, while columns (3) and (6) include those at least 26 years of age. The specification is as in column (4) of Table 2

\*\*\*$p < 0.01$; \*\*$p < 0.05$; \*$p < 0.1$

**Table 11** Robustness to polynomial order

|  | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
|  | Household formation intentions | | Fertility intentions | |
| $CourtIneff \times Post$ | 0.0383* | 0.0392 | 0.0406** | 0.0733 |
|  | (0.0203) | (0.0678) | (0.0184) | (0.0610) |
| $CourtIneff^2 \times Post$ |  | −0.000188 |  | −0.00669 |
|  |  | (0.0118) |  | (0.0104) |
| Observations | 10,059 | 10,059 | 10,059 | 10,059 |
| R-squared | 0.103 | 0.103 | 0.144 | 0.144 |
| District FE | √ | √ | √ | √ |
| Time FE | √ | √ | √ | √ |
| Controls | √ | √ | √ | √ |
| Cluster | District | District | District | District |

In this table, we provide some robustness checks regarding the polynomial order. Columns (1) and (2) refer to household formation, while columns (3) and (4) refer to fertility intentions. For comparison purposes, the main results as reported in Table 2, column (4), Panel A and Panel B, are reported in columns (1) and (3), respectively. Columns (2) and (4) include the second-order polynomial of *CourtIneff* interacted with *Post*. The specification is as in column (4) of Table 2

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

**Table 12** Robustness excluding 2017

|  | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
|  | Household formation intentions | | Fertility intentions | |
| $CourtIneff \times Post$ | 0.0383* | 0.0348* | 0.0406** | 0.0411** |
|  | (0.0203) | (0.0185) | (0.0184) | (0.0163) |
| Observations | 10,059 | 8,626 | 10,059 | 8,626 |
| R-squared | 0.1026 | 0.1181 | 0.1444 | 0.1529 |
| District FE | √ | √ | √ | √ |
| Time FE | √ | √ | √ | √ |
| Controls | √ | √ | √ | √ |
| Cluster | District | District | District | District |

In this table, we provide some robustness checks regarding the polynomial order. Columns (1) and (2) refer to household formation, while columns (3) and (4) refer to fertility intentions. For comparison purposes, the main results as reported in Table 2, column (4), Panel A and Panel B, are reported in columns (1) and (3), respectively. Columns (2) and (4) are obtained dropping observations from the 2017 survey. The specification is as in column (4) of Table 2

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

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