Essays in Applied Economics

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Abstract

This thesis consists of two research articles. The first paper shows that permanent employment contracts are important drivers of the fertility decision. I provide causal evidence by using variation in subsidies to employers transforming temporary jobs into permanent in Spain as instrument for permanent employment. The amount of the subsidy markedly varies across regions, with the age and gender of the workers whose contract is converted, and within these three dimensions over time. Using a large administrative individual-level panel dataset, I show that permanent contracts have a positive impact on the fertility decision of men, but no effect for women. Several findings suggest that the effect declines when individuals are more likely to have a higher opportunity cost of having a child. I find that permanent employment has a lower impact on fertility when individuals are young, have a high wage growth, or do not cohabit with a partner. Based on a different regional level dataset, I replicate the findings from the main analysis and quantify the aggregate effects of the subsidies, which raised fertility by 2% at a cost of 13,300 euros per child.

The second paper studies the impact of television on academic performance and educational inequality, and explores whether the previous effects depend on the educational value of the alternative activities that television crowds out. I identify a causal effect by using a natural experiment that consists in the staggered introduction of the digital television signal in the British television market. The digital switchover leads to an increase in the time that individuals watch television and a drop in the viewing shares of the traditional channels. I find that the digital switchover increases academic performance, contributing to human capital formation, and that the effect is larger in schools at the bottom

of the scores distribution, reducing educational inequality. I also find that the digital switchover decreases the probability of children taking part in detrimental activities such as alcohol drinking, and their frequency. This suggests that the impact of television on education depends on the educational value of the out-of-school activities that television crowds out.

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Chapter 1

Introduction

The presence of children and their human capital level are crucial for future economic well-being. Consequently, understanding the determinants of fertility and education has always received considerable attention and raised important concerns. On the one hand, fertility rates have plunged in most of the developed countries during the last decades, frequently even falling below the replacement level of a population. This is worrisome because as a consequence, their population is rapidly ageing, which may be harmful for future economic growth, the sustainability of the pension systems or future fiscal balance. On the other hand, digital technologies, which are one of the most important activities in the daily life of children, have been subject to enormous changes in previous years, which may have had an important effect on education. This is important as education is one of the main determinants of future labour market performance. This thesis is split into two independent papers that respectively provide new causal evidence on the determinants of fertility and education.

Chapter 2 examines the effect of permanent employment on the fertility decision. Temporary employment is widely used in many countries, probably due to its fixed duration and low severance payment, which makes it very attractive for employers. Given its low job security, temporary employment may have a negative impact on hard to reverse decisions, and it has been reported that this is the case in outcomes such as fertility (De La Rica and Iza, 2005; Gutiérrez-

Domènech, 2008; Modena et al., 2014). However, when studying the later impact, exogenous variation has been hard to find, which raises identification concerns. For example, the type of contract could be correlated with unobservable determinants of fertility, or individuals might select their labour status in anticipation of childbearing (Malo and Muñoz-Bullón, 2008). The main contribution of my paper is to provide evidence on the causal impact of permanent employment on fertility. I do so by using variation in subsidies given by the regional authorities in Spain to employers transforming temporary contracts into permanent as an instrument for permanent employment. Previous literature has studied the impact of these subsidies on the labour market (García Pérez and Rebollo Sanz, 2009a,b), or adopted them to explore the impact of job security on wealth accumulation (Barceló and Villanueva, 2016). The amount of the subsidy markedly varies across regions, with the age and gender of the workers whose contract is converted, and within these three dimensions over time. The strong variation in the reform at four different levels allows to identify the impact of subsidies on permanent employment. The fact that the policy solely changes the predisposition of employers to use permanent employment suggest that the exclusion restriction is satisfied.

The analysis is conducted estimating a 2SLS model on a panel dataset of 2.4 million of observations that contains yearly information on the labour and fertility outcomes of approximately half a million individuals followed during the period 1997-2004. Once I account for endogeneity, I provide a novel set of results. My findings show that permanent employment has a positive impact on the fertility decision of men, but a negligible effect for women. I show that the probability of males having a new child in the next year increases by 8.74% when holding a permanent contract. This is a sizable effect, which is not surprising given the enormous difference between permanent and temporary contracts. The reason why the estimates differ by gender may be that permanent employment not only increases job security but also career development (Arulampalam and Booth, 1998; Bentolila et al., 1994; Booth et al., 2002; García-Serrano and Malo, 2013; Jimeno and Toharia, 1993), which increases the opportunity cost of having

a child. As having a child reduces more the labour force participation, number of hours worked and wages of women relative to men (Angelov et al., 2016; Angrist and Evans, 1998; Bridges and Mumford, 2001; Cools et al., 2017; Kleven et al., 2018; Lundberg and Rose, 2000; Michaud and Tatsiramos, 2011), permanent employment may increase the opportunity cost of having a child for females relative to males. The paper explores the opportunity cost of having a child as a plausible mechanism behind the baseline estimates, by studying whether the effect of permanent employment on fertility declines when individuals are more likely to have a higher opportunity cost of having a child. Several findings suggest that this is the case. I find that permanent employment has a lower impact on fertility when individuals are young, have a high wage growth, or do not cohabit with a partner.

The paper also estimates a separate aggregate analysis that uses a different aggregate panel dataset. The aggregate analysis makes four important contributions to the main one. First, the aggregate analysis uses a dataset that contains information on the total number of labour contracts and births that occur in each municipality in Spain. This allows to quantify the impact of the subsidies on the labour market and fertility rates. Second, the dataset provides information on the marriage rate of each municipality, which is an interesting outcome variable that I could not observe in the individual dataset. Third, it contains information on the amount provided of a number of public services at the municipality level, such as healthcare or childcare services, for which it is important to control. I do not control for these in the individual analysis because the MCVL does not provide information about the municipality of residence of an important part of my sample. Finally, I provide further evidence on the causal impact of permanent employment on fertility. Using the aforementioned subsidies as an instrument for the number of permanent contracts created per year, I show that permanent employment has a positive impact on fertility and marriage rates. This replicates the findings from the main analysis. Given that the aggregate analysis is based on universal data, I also use the estimates to quantify the aggregate effects of the subsidies. I calculate that the policy increased the number of births by 2%

each year it was in place in Spain, costing 13,300 euros per child.

My second paper (chapter 3) explores the importance of digital technologies on education. I do so by studying the causal impact of television on academic achievement and educational inequality. The paper also explores whether the previous effect depends on the educational value of the out-of-school activities that television replaces. All the previous is important as television is one of the main activities in the everyday life of most of the children in many countries. Most of the literature has found that television is bad for the academic achievement and behaviour of children (Beentjes and Van der Voort, 1988; Christakis et al., 2004; Hancox et al., 2005; Huesmann et al., 2003; Johnson et al., 2004; Timmer et al., 1985; Walberg and Ling Tsai, 1985). However, causal evidence has been hard to find, which raises important endogeneity concerns. For example, television could be correlated with unobserved characteristics that determine education, such as preferences.

Chapter 3 contributes to the previous literature by providing evidence on the causal impact of television on education, and exploring the mechanism behind this effect. I do so by exploiting the digital switchover that occurred in UK between 2008 and 2012 as a natural experiment. This process consisted in the staggered adaptation of every TV transmitter in UK to stop transmitting analogue signal and start transmitting high power digital signal. The digital switchover brought important changes to the television market. The most important one is that at the day when the digital switchover occurred in a region, the number of TV channels that could be watched multiplied by eight, which increased the average television viewing time by 34 minutes per day.

This natural experiment has two methodological strengths. First, TV transmitters, which are located in different regions in UK, were transformed at different times. Consequently, the switchover process affected the different geographical areas at different years, which created strong exogenous variation. Second, the government delegated the digital switchover to Ofcom, the official communications regulator in UK, and DigitalUK, an independent non-profit organisation funded by the public-service broadcasters and operators. The ob-

jective of these organisations was to implement the digital switchover on time, based on the physical features of the TV transmitters, which had been built in the 1960s and 1970s. Hence, it is hard to believe that the switchover process was correlated with unobserved determinants of education.

I web-scrape the DigitalUK website to collect information on when the digital switchover occured in all the postcodes in England. This allows me to use variation in my natural experiment at a very accurate geographical unit. In the paper, I also use a large panel administrative dataset that contains yearly information on the test scores of every school in England, and a survey panel dataset at the individual level that provides yearly information on the activities in which children get involved.

Using a difference-in-difference model, I compare scores of schools located in regions that got access to digital television at different years, and also grades within schools before and after the switchover. I find that an increase in television viewing time improves academic performance, and that the impact is higher for schools at the bottom of the score distribution, reducing educational inequality. The estimates show that television increases test scores by 0.238 standard deviations. The magnitude of the estimated effect is higher than the estimates provided in previous studies. The paper next explores a plausible mechanism behind my baseline results. To do so, I estimate a separate difference-in-difference approach and use the individual-level panel dataset on the activities at which children get involved. I show that television reduces the probability and the frequency with which children take part in out-of-school activities that may harm their academic performance, such as alcohol drinking. Overall, the estimates suggest that the effect of television on academic achievement depends on the educational value of the alternative activities that television replaces.

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Chapter 2

Permanent Employment and the Fertility Decision

2.1 Introduction

During the last decades, most of the developed countries have experienced fertility rates below the replacement level of a population.[1] Consequently, their population is rapidly ageing, which raises concerns about future economic growth. The labour market is likely to be one of the factors behind low fertility rates. Most of the literature on the link between labour outcomes and fertility has focused on the role of unemployment and job loss (Adsera, 2005; Ahn and Mira, 2001; Del Bono et al., 2015).[2] However, the limited response of fertility rates to macroeconomic shocks and the marked differences in the birth rate across developed countries with similar unemployment levels suggest that other labour determinants may be playing a role.

Temporary employment is widely used in many countries. The difference between temporary and permanent contracts is that the former have a limited duration and generally much lower severance payment. Prior literature has reported

[1] Figures A.1-A.2 respectively show the Total Fertility Rates in the EU and Spain during the last three decades. Also see `https://data.worldbank.org/indicator/sp.dyn.tfrt.in`.

[2] Previous literature has highlighted the importance of socioeconomic attributes on the impact that unemployment has on the fertility decision (Ananat et al., 2013; Andersson, 2000; Andersen and Ozcan, 2011; Cazzola et al., 2016; Kreyenfeld and Andersson, 2014).

that temporary contracts have a negative impact on fertility relative to permanent contracts (De La Rica and Iza, 2005; Gutiérrez-Domènech, 2008; Modena et al., 2014). Yet, no causal evidence has been given until date, which raises identification concerns. For example, the type of contract may be correlated with unobservable determinants of fertility (Adserà, 2004; Blau and Kahn, 2017; Panel on Youth, 1974), or individuals may select their labour status in anticipation of childbearing (Malo and Muñoz-Bullón, 2008).[3]

This paper provides evidence on the causal impact of permanent employment on the fertility decision of males and females by exploiting exogenous variation in subsidies to open-ended labour contracts in Spain.[4] These subsidies were given by the different regional governments to any employer which transformed a temporary contract into an open-ended one. The introduction, size, and eligibility conditions for subsidies were decided by the regional authorities, and frequently changed on a yearly basis. Consequently, the amount of the subsidy markedly varied across regions, with the age and gender of the workers whose contract was converted, and within these three dimensions over time. I use this variation at four different levels as an instrument for permanent employment. According to the Spanish regional governments' subsidies publications, the only goal of the policy was to incentivize employers to use permanent employment. This suggests that the exclusion restriction is satisfied. Previous literature has studied the impact of these subsidies on the labour market (García Pérez and Rebollo Sanz, 2009a,b), or used them to examine the effect of job security on wealth accumulation (Barceló and Villanueva, 2016).[5]

[3]The direction of the bias is unclear. On the one hand, employers may give less firm-specific training or dead-end jobs to workers who might become parents soon (Blau and Kahn, 2017). Moreover, workers who are highly attached to the labour market might be more likely to hold permanent contracts, but less likely to have a child (Adserà, 2004). On the other hand, employees may search for stable contracts if they want to have a child. Furthermore, social transitions characterizing the passage to adulthood may increase fertility and have a positive effect on the labour status of individuals (Panel on Youth, 1974).

[4]This type of subsidies was only given in Spain, and so, I perform the analysis using data based on this country. However, the estimates obtained provide causal evidence on the impact of permanent employment on fertility. This shows the importance of taking into account externalities when evaluating labour policies.

[5]Previous literature has used this type of exogenous variation to study the effect of holding an open-ended contract on household formation (García-Ferreira and Villanueva, 2007), finding that an increase in job security has no impact on the probability of forming a new household.

I conduct the analysis using an individual-level administrative dataset that contains retrospective labour market and sociodemographic information on 1.1 million individuals randomly selected from the Spanish Social Security records. Using this information, I construct a large panel dataset on the labour and fertility history of each individual during the period 1997-2004. The use of administrative data avoids sample selection and self-reported biases.

My findings provide a novel set of results after accounting for endogeneity. I show that permanent employment has a positive impact on the fertility decision of men, but a negligible effect for women. For males, the probability of having a new child in the next year increases by 8.74% when holding a permanent contract. This is a sizable impact, which is not surprising due to the marked difference between temporary and permanent contracts. Permanent employees receive 45 days of paid salary per year worked in the case of unfair dismissal, whereas temporary workers do not obtain any kind of compensation. Importantly, I find that permanent contracts only increase the probability of males entering parenthood, and the effect vanishes for the second and third child.[6] These results are relevant from a demographic point of view, as an earlier start in childbearing has been shown to have a positive impact on the completed fertility of individuals (Kohler et al., 2002; Buckles, 2008). The baseline estimates are robust to: (i) restricting the sample to individuals that never move across regions to test for selection, (ii) changes in sample composition, (iii) using different definitions of the instrument, (iv) placebo tests, (v) estimating alternative specifications and including additional covariates, and (vi) using alternative dependent varaibles.

A further set of results explores a plausible mechanism underlying the baseline estimates. Permanent employment increases job security but also career prospects and wages (Arulampalam and Booth, 1998; Bentolila et al., 1994; Booth et al., 2002; García-Serrano and Malo, 2013; Jimeno and Toharia, 1993). Decreases in economic uncertainty have a positive impact on the fertility of

[6]Prior evidence has reported that the career penalties of having a child may differ by birth order (Berger and Waldfogel, 2004; Duvander, 2006).

women (Prifti and Vuri, 2013).[7] Increases in wages have two offseting effects for parents (Becker, 1965). On the one hand, a higher salary leads to a higher fertility due to an income effect (Becker, 1960; Borg, 1989; Black et al., 2013). On the other, better career prospects increase the value of the time of workers in the labour market. As taking care of children is time intensive, a higher wage rises the opportunity cost of children. This is the substitution effect, and it decreases fertility (Schultz, 1985). Because parenthood reduces more labour force participation, hours of work, experience, and wage rates for mothers than for fathers (Angelov et al., 2016; Angrist and Evans, 1998; Bridges and Mumford, 2001; Cools et al., 2017; Kleven et al., 2018; Lundberg and Rose, 2000; Michaud and Tatsiramos, 2011), the substitution effect may larger for women. [8] This may explain the gender differences I find in the impact of permanent employment on fertility. I further explore this as a plausible mechanism. I test whether the effect of permanent employment on fertility declines when individuals are more likely to have a higher substitution effect. I find that permanent employment has a lower impact on fertility when workers are young, experience a high wage growth, or do not cohabit with a partner.[9]

This paper also estimates a separate analysis based on a different administrative dataset that comes from the Spanish Public State Employment Service and the National Statistical Institute.[10] The data contains yearly information on the total number of labour contracts and births in each of the 7,447 municipalities in Spain. This aggregate analysis contributes to the main one in several important ways. First, it is based on a dataset that contains information on every labour contract and birth that occurs in Spain during the period of analysis. This allows to quantify the magnitude of the total impact of the subsidies policy

[7] Prifti and Vuri (2013) provide evidence of a strengthening of the employment protection legislation for small firms having a positive effect on the fertility decision of Italian working women.

[8] Previous evidence shows that higher wages may have a positive impact on fertility for men but negative for females (Heckman and Walker, 1990; O'Donoghue et al., 2011; Schaller, 2016).

[9] I explore heterogeneity in age as previous literature has reported that the fertility behaviour responds to changes in the career prospects during the life cycle (Auer et al., 2013; Kohler et al., 2002; Ward and Butz, 1980).

[10] In Spanish Servicio Público de Empleo Estatal (SEPE) and Instituto Nacional de Estadística (INE).

on the labour market and fertility rates. The estimates are based on the whole population, so they also add representativeness to the main analysis. Second, it contains information on an important outcome variable that I could not observe in my individual dataset, which is marriage rates. Third, the aggregate dataset includes annual information on the extent of a number of public services that are provided at the municipality level and which may be correlated with fertility, such as healthcare or childcare services. The reason for not including these in the individual analysis is that the MCVL only provides information on the province of residence of individuals, which is too aggregate.[11] Finally, the analysis constitutes a second set of evidence on the causal impact of permanent employment on fertility. The empirical strategy uses the aforementioned subsidies as an instrument for permanent employment. I show that the policy increases permanent employment and reduces the use of temporary jobs, which boosts fertility rates in the following years. Using the aggregate estimates, I calculate that subsidies respectively increased the total number of permanent contracts and births in Spain by 30,096 and 8,216 per year over the period 1997-2004. Given that the average number of births in Spain was of 404,000 per year during the period of analysis, and the total cost of the subsidies has been estimated in 874 million of euros (García Pérez and Rebollo Sanz, 2007), the policy increased the number of births by 2.03% each year it was in place at the cost of 13,300 euros per child.[12] I also show that permanent employment has a positive impact on marriage rates. This is in line with previous evidence on economic stress increasing separations of partnerships (Bridges and Disney, 2016). The aggregate estimates are consistent with those obtained in the main analysis.

This paper primarily contributes to two literatures. First, this paper belongs to the literature that studies the impact of permanent employment on fertility (De La Rica and Iza, 2005; Gutiérrez-Domènech, 2008; Modena et al., 2014). I contribute to this literature by providing causal evidence on the effect of

[11] More precisely, the MCVL contains information about the municipality of residence of individuals as long as they live in municipalities with a population higher than 40,000. However, 47.37% of my sample lives in municipalities where population is lower than 40,000 inhabitants.

[12] All the monetary amounts are measured in constant euro of 2002.

permanent contracts on the fertility decision. After accounting for endogeneity, I provide a novel set of results. I find that permanent employment has a positive impact on fertility for males, but no effect for females. The paper also provides some preliminary evidence on the mechanism that might operate behind the gender differences. If the substitution effect that arises due to the higher career prospects of permanent employment is large, permanent employment may have no impact on fertility. Second, this paper belongs to the literature that evaluates labour market policies aimed at the encouragement of job security (García Pérez and Rebollo Sanz, 2009a, Kugler et al., 2003; Kugler and Pica, 2008; Mendez, 2013; Jimeno et al., 2015). Labour market regulation has been frequently evaluated based solely on labour outcomes. This paper emphasizes and quantifies the importance of accounting for externalities for the correct evaluation of public policies.

The remainder of the article is organized as follows. Section 2.2 explains the subsidies to promote the use of open-ended contracts. Section 2.3 describes the data, section 2.4 discusses the empirical strategy, and section 2.5 presents the results of the main analysis. Section 2.6 shows the data, methodology, and estimates of the aggregate analysis. Section 2.7 concludes.

2.2 Labour Market Context

Fixed-term employment has been widely used in Spain since its introduction in the Worker's Act (Estatuto de los Trabajadores) in 1984. The difference between temporary and permanent contracts is that the former have a fixed duration and much lower severance payment in the case that the dismissal is declared unfair at court. Whereas a permanent employee would be compensated with 45 days of paid salary per year worked in the case of unfair dismissal (with a limit duration of 42 months of indemnification), a temporary employee would not receive any kind of compensation. As a response, temporary employment is an attractive source of flexibility for employers, which may explain its rapid expansion after 1984. According to OECD data, the proportion of employees holding a

temporary contract in Spain increased from 15.6% in 1987 to 33.64% in 1997.[13] Two national labour market reforms were approved in 1997 and 2001 to halt this expansion. These significantly decreased unfair dismissal costs and payroll taxes of new open-ended contracts. Simultaneously, some regional governments introduced subsidies to open-ended contracts. The proportion of temporary employees stabilized, and fell from 33.64% in 1997 to 32.24% in 2004. Yet, this is a high level in international standards. For example, the proportion of temporary employees in the EU-28 was 10.1% in 2004.

2.2.1 Regional Subsidies

In 1997, some of the 17 Spanish regional governments started providing subsidies to the employers that converted a temporary contract into an open-ended one.[14] According to the Spanish regional governments' subsidies publications, the only objetive of subsidies was to incentivize employers to use permanent employment. The incentives were generally made as a lump-sum payment, but in some cases took the form of a reduction in the payroll tax. The provision of subsidies was managed by the regional authorities and the amounts given were frequently changed in a yearly basis. This generated a strong variation in the amount of the subsidy across regions and within regions over time. Figure 2.1 shows the subsidy in thousands of euros that regional authorities gave each year during the period 1996-2004. As shown, regional governments did not introduce incentives to permanent employment at the same time. Whereas only three of the 17 regional governments provided subsidies by 1997, the number increased to 14 by 2000. This provision faded away in some regions after 2000, and some others (Navarra and Catalonia) never gave a subsidy. Figure 2.1 also shows that there was a high variation in the amount of the subsidy provided across regions and within regions over time. Subsidies reduced the labour costs of the first year of permanent contracts from 9% to 60% depending on the region and year, and the

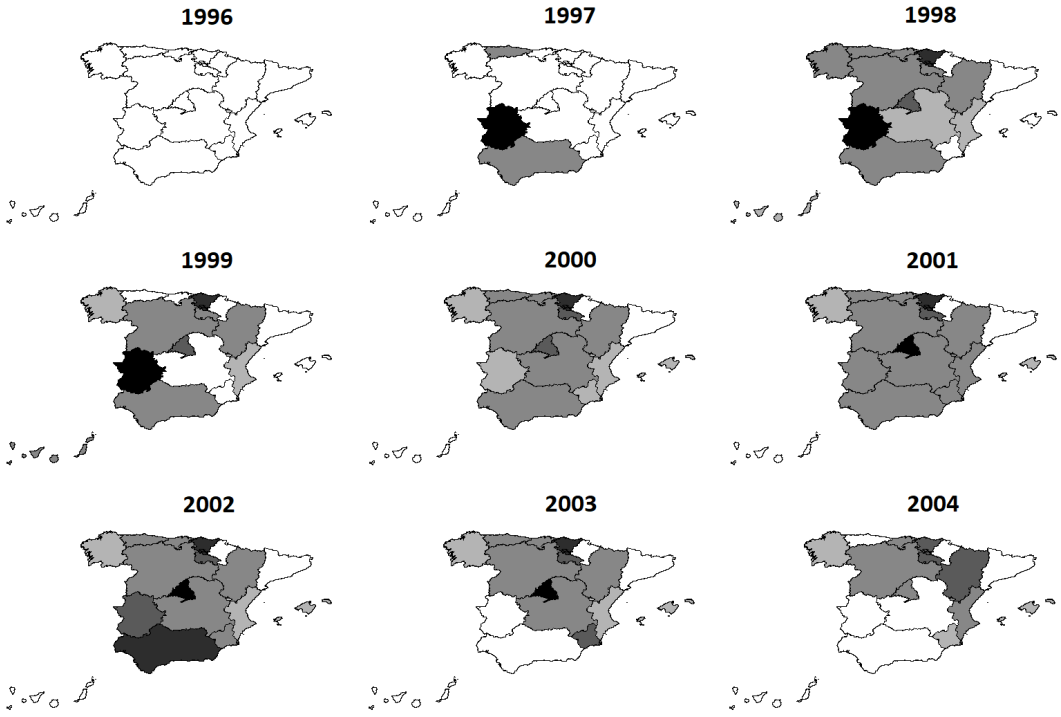
[13] See https://data.oecd.org/emp/temporary-employment.htm.

[14] An alternative form of subsidies was given to the employers that hired an unemployed individual as a permanent employee, but this form of subsidies was much less used in practice (Barceló and Villanueva, 2016).

average subsidy reduced them by 24% (García Pérez and Rebollo Sanz, 2009a).

As shown in Appendix A.2, regional authorities also frequently provided a different amount of the subsidy depending on the age and gender of the employees whose contract was transformed. In particular, subsidies were frequently lower for the contract conversion of male or middle-aged workers. The age and gender eligibility conditions and the amount of the subsidy provided attending to these were exclusively administered by the regional authorities, and often varied over time. All together, the amount of the subsidy for which employers were eligible markedly varied in the region, age and gender of the worker whose contract was converted, and within these three dimensions over time. I exploit exogenous variation in these subsidies to study the causal impact of permanent employment on the fertility decision.

Figure 2.1: Average Regional Subsidies in Euros by CCAA and Year



The figure presents the amounts of the subsidies that were given by region and year. I group the amounts of the subsidies in six different ranges: no subsidy, subsidy between 0 and 2,500 euros, subsidy between 2,500 and 5,000 euros, subsidy between 5,000 and 7,500 euros, subsidy between 7,500 and 10,000 euros, and subsidy higher than 10,000 euros. I assign a colour to each of the previous groups, with darker colours referring to higher amounts of the subsidies.

2.3 Data

I use data from the Muestra Continua de Vidas Laborales (MCVL). This is a large administrative dataset that contains information on the complete labour market history of approximately 1.1 million individuals randomly selected from the Spanish Social Security records in 2006. This represents 4% of the total number of employed, self-employed, unemployed, and retired individuals in Spain. The MCVL includes labour information of individuals on their earnings, occupation, type of contract (e.g. whether it is open-ended or fixed-term, or whether it is full-time or part-time), and date of start and end of the labour spell. The dataset also contains sociodemographic information on the age, gender, education, nationality and province of residence of the individual. Finally, the MCVL gives information on the employer, such as its sector of activity, whether it is private or public, and the province where the job is established. Administrative data eliminates the problems of missing information and measurement error coming from recall biases and self-reporting.

The analysis is based on employees and their type of contract. Therefore, it does not include other labour market spells such as self-employment, unemployment, and inactivity. Due to data availability, I restrict the sample to the period 1997-2004. The MCVL has been matched with the Spanish Registry of Inhabitants, which contains information on the date of birth and gender of the members of the households where the sampled individuals live. I use this information to construct the fertility history of individuals. However, the MCVL does not provide information about the family relationship among the members of a household. Following Bonet et al. (2013), I restrict my analysis to individuals aged 20-45 living in households with five or fewer members to minimise the probability of adults cohabiting with children that are not their offspring.[15] I also restrict the sample to individuals that are living with one or no adult individual

[15] Bonet et al. (2013) compare the MCVL with census data and argue that the probability of adults cohabiting with children that are not their offspring augments with the number of members of the household and with the age of the head of the household. Bonet et al. (2013) calculate that the proportion of households where adults only cohabit with children who are their offspring is above 95% when there are five or fewer members in the household and the head of the household is between 20 and 45 years old.

of a similar age for the same reason.[16] Overall, this sample selection results in an unbalanced panel of more than 2.4 million observations that contains yearly information on the labour and fertility outcomes of approximately 460,000 individuals.

It is important to note that even though this dataset is a representative sample of the labour market in 2006, it is not in the previous years (Domínguez Fabián et al., 2012). For example, individuals strongly attached to the labour market are more likely to be present in the data in the years before 2006. Although this may constitute a source of bias, it is minimised by the fact that my sample period does not include years far back in time. In particular, more than 80% of my sample is present in every year of the period under analysis, and only 2.8% of the sample is present in three or fewer years.

2.3.1 Descriptive Analysis

Table 2.1 presents summary statistics of temporary and permanent employees separately by gender. The table also reports the p-values of tests that compare the means of observable characteristics between temporary and permanent employees. On average, and independently of gender, individuals holding an open-ended contract are less likely to become unemployed, earn almost double, have worked longer for their employer, are more experienced and have a higher probability of holding a full-time position, than temporary employees. Moreover, permanent employees are more qualified, older, and more likely to be natives. Regarding my outcome variables, permanent employees live in households with approximately the same number of members as temporary employees, but are more likely to have a child and to have had a child in the previous five years.

It is also important to mention the differences in observable characteristics between males and females. Men are less likely to become unemployed, earn more, are more experienced, and less qualified than women. Moreover, males have had more children than females in the previous five years, but are less likely

[16] I define that two individuals have a similar age if their age difference is below 15. The estimates of the analysis are robust to using different age thresholds.

to have a new child at year t. This may be because men are older than women.

The descriptive analysis suggests that the effect of permanent employment on fertility is unclear. On the one hand, permanent employees have a lower probability of transiting into unemployment than temporary workers. Consequently, holding an open-ended contract entails a lower economic uncertainty, which may encourage the fertility decision. On the other, permanent employees earn a higher wage. This entails an income and a substitution effect. The former increases fertility. The latter increases the value of the time of individuals in the labour market. Children are time intensive, and so, a higher wage raises the opportunity cost of having a child. This may decrease fertility.

2.4 Empirical Strategy

This paper studies the effect of permanent employment on the fertility decision. I estimate, separately for males and females, the following baseline specification:

$$F_{i,t+1} = \alpha + \beta PE_{i,t} + \phi X_{i,t} + \lambda_t + \delta_i + \varepsilon_{i,t} \tag{2.1}$$

where $F_{i,t+1}$ is a binary variable that equals 1 if individual i has a new child at year $t+1$, and zero otherwise. I use as dependent variable the probability of having a child in the next year to allow for the nine months pregnancy period.[17] $PE_{i,t}$ is a dummy that equals 1 if individual i holds an open-ended contract at time t, and 0 otherwise.[18] $X_{i,t}$ is a vector of time-varying covariates at the individual level, which includes age and age squared to control for non-linearities of fertility in age, the logarithm of wage, tenure, tenure squared, and a dummy that indicates whether the individual holds a full-time contract at time t. $X_{i,t}$ also

[17] Appendix A.10 also estimates the baseline specification using as dependent variable the probability of having a new child two years after, to study the impact of permanent employment on the fertility decision in the short run. As shown, I find similar estimates to the ones I obtain when I use $F_{i,t+1}$ as dependent variable.

[18] I choose as independent variable of interest the probability of holding a permanent contract rather than the probability of transiting from temporary to permanent employment. The reason is that 5.3% of permanent employees transit to temporary employment every year, and this would not be taken into account in the latter variable. This can be seen in the transition matrix of Appendix A.3.

contains a set of two-digit industry dummies, and a set of binary variables that indicate the region where individual i lives at year t. Finally, some regional governments gave grants to the women that gave birth in some of the years within the period of analysis. I account for this by including a dummy that takes value 1 if individual i lives in a region where incentives to fertility are present at year t, and 0 otherwise. θ_t are year fixed effects that control for flexible trends in the dependent variable common across individuals, and μ_i is a set of individual fixed effects dummies that capture individual time-invariant characteristics. Finally, $\varepsilon_{i,t}$ is an individual and time specific level error term. I allow for an arbitrary correlation of residuals at the individual and region-year level.[19]

The coefficient of interest, β, is the impact that holding a permanent contract at t has on the probability of having a new child at $t+1$. Estimating this using OLS specification (2.1) may lead to biased estimates of permanent employment for multiple reasons. For example, the labour status may be correlated with the proclivity of workers to have children in the future (Malo and Muñoz-Bullón, 2008). On the one hand, employees may look for more stable contracts if they are planning to have a child in the short-term. On the other, employers may discriminate against workers who are likely to have a child soon, by giving them less firm-specific training or dead-end jobs (Blau and Kahn, 2017). Another possible source of bias are preferences. If individuals become more highly attached to the labour market they might be more likely to obtain a permanent contract, but less likely to have a child (Adserà, 2004). Alternatively, social transitions characterizing the passage to adulthood may have a positive effect on the labour status and the parenthood decision of individuals (Panel on Youth, 1974). To address these concerns, I use exogenous variation in subsidies to open-ended contracts at three different dimensions (across regions, over time and by age) as an instrument for permanent employment.

I estimate a Two-Stage Least Squares model to study the causal impact of

[19] Appendix A.5 shows that the baseline estimates are robust to not including time-varying labour covariates as controls in the regression. As shown in Appendix A.6, the baseline estimates are also robust to controlling for additional covariates such as occupation. Finally, Appendix A.4 finds that the baseline estimates are robust to using alternative cluster levels of the standard errors, such as the individual and region level.

Table 2.1: Descriptive Statistics

	Males			Females		
	Temp Emp	Perm Emp	p-val	Temp Emp	Perm Emp	p-val
	(1)	(2)	(3)	(4)	(5)	(6)
Prob Unemp$_{t+1}$	0.251	0.075	0.000	0.270	0.114	0.000
	(0.434)	(0.263)		(0.444)	(0.318)	
Monthly Wage	749.123	1365.375	0.000	578.795	1095.554	0.000
	(509.819)	(636.427)		(510.947)	(600.164)	
Tenure	2.405	5.810	0.000	2.382	5.419	0.000
	(1.710)	(4.574)		(1.851)	(4.577)	
Full-time	0.914	0.961	0.000	0.743	0.845	0.000
	(0.281)	(0.193)		(0.447)	(0.362)	
Experience	9.804	14.117	0.000	7.320	11.635	0.000
	(6.895)	(7.154)		(5.402)	(6.681)	
White-collar	0.140	0.311	0.000	0.250	0.373	0.000
	(0.347)	(0.463)		(0.433)	(0.484)	
Age	30.221	33.925	0.000	30.094	32.851	0.000
	(7.033)	(6.599)		(6.992)	(6.630)	
Native	0.976	0.985	0.000	0.978	0.985	0.000
	(0.182)	(0.123)		(0.146)	(0.122)	
Prob having child	0.032	0.042	0.000	0.033	0.046	0.000
	(0.175)	(0.201)		(0.178)	(0.210)	
Child past 5 years	0.125	0.182	0.000	0.127	0.152	0.000
	(0.331)	(0.386)		(0.333)	(0.359)	
Number of members	2.598	2.686	0.000	2.613	2.581	0.000
	(1.110)	(1.110)		(1.074)	(1.062)	
N	632,307	804,571		483,827	526,017	

The sample contains employees aged 20-45. The table displays the means of the observable covariates, and the standard deviations in parentheses.

an endogenous dummy regressor on a binary outcome variable. The specification firstly exploits exogenous variation in subsidies as an instrument for permanent employment, and then estimates the causal impact of permanent employment on the fertility decision. This gives raise to the following first-stage:

$$PE_{i,t} = \omega + \pi S_{j,a,t} + \eta X_{i,t} + \theta_t + \mu_i + \nu_{i,t} \tag{2.2}$$

where $S_{j,a,t}$ is the subsidy for which an employer located in region j at year t is eligible if it converts a temporary employee of age a into a permanent worker. There are three dimensions of variation of $S_{j,a,t}$: (i) different amounts of the

subsidy were given across the different regions, (ii) the amount of the subsidy strongly varied within regions over time, and (iii) subsidies varied depending on the age of the workers whose contract was converted. I construct $S_{j,a,t}$ following the information contained in Barceló and Villanueva (2016), and measure it in terms of percentage of labour costs saved during the first year of new open-ended contracts.[20] The first-stage includes the same set of controls as the second-stage specification (2.1), and $\nu_{i,t}$ is an individual and time specific level error term. Overall, the first stage estimates show whether subsidies increase the presence of permanent employment. The coefficient of interest, π, assesses whether a reduction in the labour costs of open-ended contracts has an impact on the probability of individuals holding a permanent job.

Using the previous 2SLS specification allows me to estimate the causal impact of permanent employment on the fertility decision. The validity of my identification strategy relies on $S_{j,a,t}$ being correlated with $PE_{i,t}$, and orthogonal to the error term $\varepsilon_{i,t}$. First, there are reasons to believe that $S_{j,a,t}$ is correlated with $PE_{i,t}$, as subsidies encourage the use of permanent employment. Second, and according to the Spanish regional governments' subsidies publications, the only purpose of subsidies was to incentivize permanent employment. To do so, employers were only eligible for subsidies when they transformed a temporary contract into a permanent one. This leads to the policy only changing the predisposition of employers to use permanent employment. Therefore, after controlling for the levels of variation of the subsidies, the instrument is unlikely to be correlated with unobservable determinants of fertility. The fact that subsidies were approved and announced almost simultaneously in a yearly basis, and the strong temporal variability in the eligibility conditions avoided workers anticipatedly changing their behaviour.[21]

[20] I therefore divide the amount of the subsidy for which an employer located in region j at time t is eligible when it transforms the temporary contract of an employee of age a into an open-ended contract, by the average total labour costs of region j at time t. I accordingly measure $S_{j,a,t}$ in terms of percentage of labour cost saved during the first year of contract, instead of measuring it in thousands of euros.

[21] When subsidies were given in region j at year t, for the contract conversion of an employee of age a and gender g, any employer was eligible to receive a subsidy. Applying for subsidies was an easy process. Employers only needed to fill a short application form and send some

A possible concern is that subsidies might have had an impact on the spouse's probability of holding a permanent contract, which might have had an effect on the fertility decision of individuals. Although my administrative dataset does not contain information on the spouse's labour market history, it is important to note that the previous channel would still operate through the permanent employment mechanism. I estimate the analysis separately by gender and cohabitation status, which provides evidence on the impact that the spouse's type of contract may have on the fertility decision of individuals. As I will show later in the paper, my estimates are a lower bound of the impact of permanent employment on fertility, if any.

2.5 Empirical Results

2.5.1 The Effect of Subsidies on the Labour Market

Column 1 of Table 2.2 estimates the first-stage specification to study the impact of subsidies on the probability of a male individual i holding an open-ended contract at time t, when individual fixed effects are not included in the analysis. As shown, eligibility for a decrease of 10% in the labour costs of the first year of a permanent contract increases the probability of a male holding an open-ended contract by 0.54%. The estimate is significant at the 1% confidence level. Given that the average subsidy reduced the labour costs of the first year of new open-ended contracts by 24% (García Pérez and Rebollo Sanz, 2009a), the policy increased the probability of a male holding an open-ended contract by 1.3%. The magnitude of the estimate is similar to those obtained in previous studies (García Pérez and Rebollo Sanz, 2009a, Barceló and Villanueva, 2016).[22] Column 2 includes individual fixed effects to the previous specification. The

documentation, such as the labour contract of the employee. The application form is shown in Appendix A.8. I also describe the rest of the documentation that was needed to apply for subsidies.

[22]For example, an increase in the average subsidy by 1,000 euros has been shown to raise the probability of workers holding an open-ended contract by 1.1% (Barceló and Villanueva, 2016). Similarly, an increase in the average subsidy by 4,800 euros has been shown to boost the conversion of temporary contracts of middle-aged female workers into permanent ones by 0.44% every quarter (García Pérez and Rebollo Sanz, 2009a).

estimate of the subsidies is similar to the coefficient obtained in column 1, and significant at the 1% confidence level. Columns 3-4 repeat the analysis for the sample of females. The estimates of the subsidies are positive, slightly lower in magnitude than those found for males, and significant at the 1% confidence level. Overall, I conclude that the policy increased the presence of permanent contracts, and that male individuals benefited slightly more from the subsidies. Panel B of Table 2.2 reports the F-statistics of the instrument, which are always higher than the threshold of 10 proposed by Stock et al. (2002).

2.5.2 Permanent Employment and the Fertility Decision

Table 2.3 studies the causal impact of permanent employment on the fertility decision of males. Column 1 presents the OLS estimates of equation (2.1), when individual fixed effects are not included in the analysis. Holding a permanent contract increases the probability of males having a new child in the following period, and the estimate is significant at the 1% confidence level. As previously explained, the OLS estimate might be subject to an important bias. To address this concern, column 2 uses the subsidies to permanent contracts as an instrument for permanent employment. After accounting for endogeneity, I find that permanent employment increases the probability of males having a new child at $t+1$ by 8.74%. This is a substantial impact, which is not surprising due to the high firing costs of permanent employees relative to temporary workers. The estimate is significant at the 1% confidence level. Columns 3-4 show that the estimates of permanent employment remain almost unchanged after controlling for individual fixed effects. Estimates are significant at the 1% and 5% confidence level.

Table 2.4 is identical to Table 2.3, but for the sample of females. As shown in column 1, OLS estimates suggest that permanent employment increases the probability of females giving birth in the following year by 0.70%. The estimate is significant at the 1% confidence level and higher in magnitude than the OLS coefficient found for males. However, after accounting for endogeneity, the estimate becomes not statistically significant. Including individual fixed effects in

columns 3-4 does not alter the findings obtained in columns 1-2.[23] A possible explanation for the gender differences is that permanent employment not only increases job security but also career prospects and wages (Arulampalam and Booth, 1998; Bentolila et al., 1994; Booth et al., 2002; García-Serrano and Malo, 2013; Jimeno and Toharia, 1993). An increase in job security has a positive impact on fertility (Prifti and Vuri, 2013), but higher wages lead to two offseting outcomes (Becker, 1965). First, an income effect that has a positive impact on fertility (Becker, 1960; Borg, 1989; Black et al., 2013). Second, a substitution effect because better career prospects raise the value of the time that workers dedicate to the labour market. As taking care of children is time intensive, a higher wage rises the opportunity cost of children. This decreases fertility (Schultz, 1985). Importantly, parenthood reduces more labour force participation, hours of work, experience, and wage rates for mothers than for fathers (Angelov et al., 2016; Angrist and Evans, 1998; Bridges and Mumford, 2001; Cools et al., 2017; Kleven et al., 2018; Lundberg and Rose, 2000; Michaud and Tatsiramos, 2011), and so, the substitution effect may larger for women.[24] Consequently, the fertility decision of females may respond less to the increase in job security implicit in permanent employment relative to men. I explore this as a plausible mechanism later in the paper.

It is also relevant to comment on the 2SLS estimates of the rest of the covariates of Tables 2.3 and 2.4. First, fertility is concave in age, with the estimates being significant at the 1% confidence level. Second, full-time employment increases the probability of females having a child in the following year. Whereas part-time contracts may improve the work-family balance, the choice of this type of employment may not be voluntary in the Spanish labour market (Buddelmeyer et al., 2005). Finally, the fertility behaviour is independent of the number of years that an employee has been working for the same employer.

This paper also studies whether the impact of permanent employment on

[23]The baseline results are also robust to using alternative dependent variables (adopting the probability of having a new child at $t+2$), and estimation methods (estimating a bivariate probit model). I present these sensitivity checks in detail in Appendices A.10-A.11.

[24]Previous evidence shows that higher wages may have a positive impact on fertility for men but negative for females (Heckman and Walker, 1990; O'Donoghue et al., 2011; Schaller, 2016).

the fertility decision occurs at the time when employees acquire a permanent position or whether the effect is persistent in the following years of permanent contract. Estimates are presented in Appendix A.9. As shown, the impact of permanent employment on fertility for males is positive and significant during the first six years that employees hold an open-ended contract. Estimates are positive but not statistically significant for females.

Table 2.2: OLS Estimates First Stage

Panel A:	Dep var: Perm Emp			
	Males		Females	
	(1)	(2)	(3)	(4)
Subsidy	0.00054***	0.00056***	0.00042***	0.00041***
	(0.00008)	(0.00009)	(0.00011)	(0.00010)
Age	0.01678***	0.04159***	0.01519***	0.04427***
	(0.00060)	(0.00377)	(0.00071)	(0.00363)
Age^2	-0.00036***	-0.00097***	-0.00035***	-0.00100***
	(0.00001)	(0.00004)	(0.00002)	(0.00005)
Logwage	0.11986***	0.06796***	0.11915***	0.07433***
	(0.00308)	(0.00290)	(0.00276)	(0.00255)
Full-time	0.01223	0.03460***	-0.00402	0.00563
	(0.01284)	(0.00908)	(0.01137)	(0.00749)
Fert Grant	0.02843***	0.02685***	0.02955***	0.02506***
	(0.00847)	(0.00644)	(0.00822)	(0.00677)
Tenure	0.08504***	0.05873***	0.07906***	0.05965***
	(0.00159)	(0.00110)	(0.00132)	(0.00135)
$Tenure^2$	-0.00301***	-0.00268***	-0.00247***	-0.00264***
	(0.00006)	(0.00005)	(0.00005)	(0.00006)
Individual FE	No	Yes	No	Yes
Region FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Panel B:		Tests		
Weak id F-stat	42.740	36.74	14.315	17.837
Observations	1,436,878	1,436,878	1,009,844	1,009,844

* p<0.10, ** p<0.05, *** p<0.01
Standard errors in parentheses. The sample contains employees aged 20-45. Weak id F-stat refers to the F statistic of the instrument, and I use it to test whether my instrument is weak. I cluster standard errors at the individual and region-year level.

Table 2.3: OLS & 2SLS Estimates Second Stage for Males

	OLS	2SLS	OLS	2SLS
Depvar: Fert_{t+1}	(1)	(2)	(3)	(4)
Perm Emp	0.0042***	0.0874***	0.0021***	0.0939**
	(0.0004)	(0.0332)	(0.0007)	(0.0429)
Age	0.0135***	0.0122***	0.0126***	0.0090***
	(0.0002)	(0.0006)	(0.0009)	(0.0019)
Age^2	-0.0004***	-0.0004***	-0.0004***	-0.0003***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Logwage	0.0045***	-0.0055	-0.0001	-0.0064**
	(0.0002)	(0.0040)	(0.0003)	(0.0030)
Full-time	0.0013*	0.0003	0.0009	-0.0023
	(0.0007)	(0.0015)	(0.0008)	(0.0019)
Fert Grant	0.0014***	-0.0006	0.0019***	-0.0002
	(0.0005)	(0.0011)	(0.0007)	(0.0013)
Tenure	0.0016***	-0.0054*	0.0009***	-0.0045*
	(0.0001)	(0.0028)	(0.0002)	(0.0025)
Tenure^2	-0.0001***	0.0002	-0.0001***	0.0002
	(0.0000)	(0.0001)	(0.0000)	(0.0001)
Individual FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Observations	1,436,878	1,436,878	1,436,878	1,436,878

* p<0.10, ** p<0.05, *** p<0.01

Standard errors in parentheses. The sample contains employees aged 20-45. I cluster standard errors at the individual and region-year level.

Table 2.4: OLS & 2SLS Estimates Second Stage for Females

	OLS	2SLS	OLS	2SLS
Depvar: Fert_{t+1}	(1)	(2)	(3)	(4)
Perm Emp	0.0070***	0.0409	0.0081***	0.0082
	(0.0004)	(0.0456)	(0.0007)	(0.0720)
Age	0.0132***	0.0127***	0.0136***	0.0136***
	(0.0003)	(0.0007)	(0.0014)	(0.0038)
Age^2	-0.0004***	-0.0004***	-0.0004***	-0.0004***
	(0.0000)	(0.0000)	(0.0000)	(0.0001)
Logwage	0.0026***	-0.0014	0.0032***	0.0032
	(0.0002)	(0.0055)	(0.0004)	(0.0054)
Full-time	-0.0008	-0.0007	0.0054***	0.0054***
	(0.0005)	(0.0007)	(0.0008)	(0.0010)
Fert Grant	0.0002	-0.0007	0.0005	0.0005
	(0.0009)	(0.0016)	(0.0014)	(0.0021)
Tenure	0.0031***	0.0004	0.0031***	0.0031
	(0.0002)	(0.0036)	(0.0003)	(0.0044)
Tenure^2	-0.0001***	-0.0000	-0.0002***	-0.0002
	(0.0000)	(0.0001)	(0.0000)	(0.0002)
Individual FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Observations	1,009,844	1,009,844	1,009,844	1,009,844

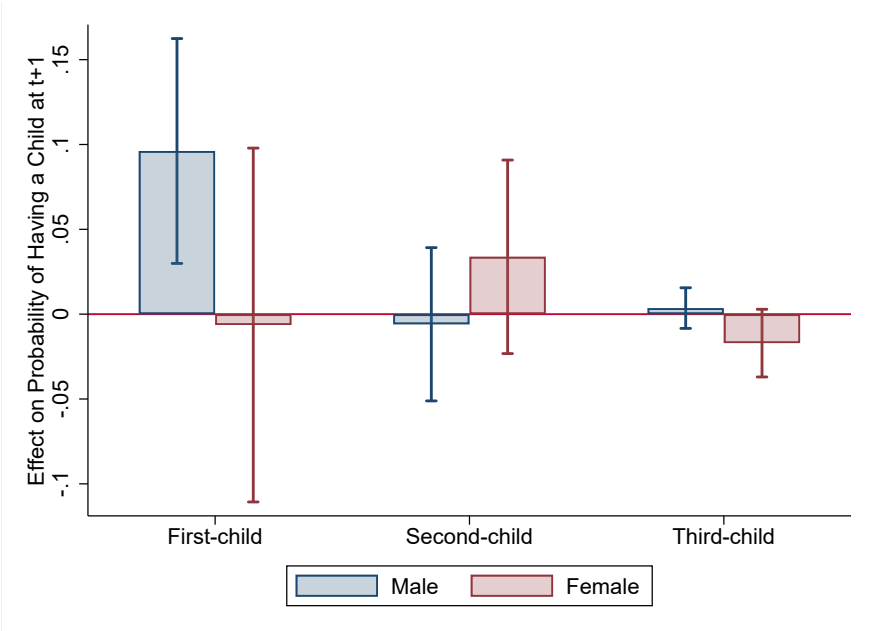
* p<0.10, ** p<0.05, *** p<0.01

Standard errors in parentheses. The sample contains employees aged 20-45. I cluster standard errors at the individual and region-year level.

2.5.3 Impact on the First, Second, and Third Child

I have so far studied the average impact of permanent employment on the probability of having a new child in the following year. However, it is also relevant to examine whether the impact of permanent contracts on fertility differs by birth order. Figure 2.2 shows the estimates of separate regressions similar to my baseline model but that respectively use as dependent variable the probability of having the first, second, and third child in the following year. As shown, holding an open-ended contract has a positive impact on the probability of males having their first child in the following year, but no impact on their likelihood of having subsequent births. Permanent employment has no effect on the fertility decision of females regardless of birth order. Therefore, Figure 2.2 reveals that permanent employment only matters for the decision of entering parenthood, and that the disparities found in my baseline estimates by gender are due to differences in the impact that permanent employment has on the probability of males and females entering parenthood.

Figure 2.2: Estimates for the First, Second, and Third Child



The figure estimates six regressions similar to the baseline specification but that respectively use the probability of having the first, second and third child as dependent variable.

2.5.4 Sensitivity Analysis

In this section, I implement a number of robustness tests to provide additional evidence supporting the identification strategy and to study whether the baseline estimates are robust to the use of different specifications.

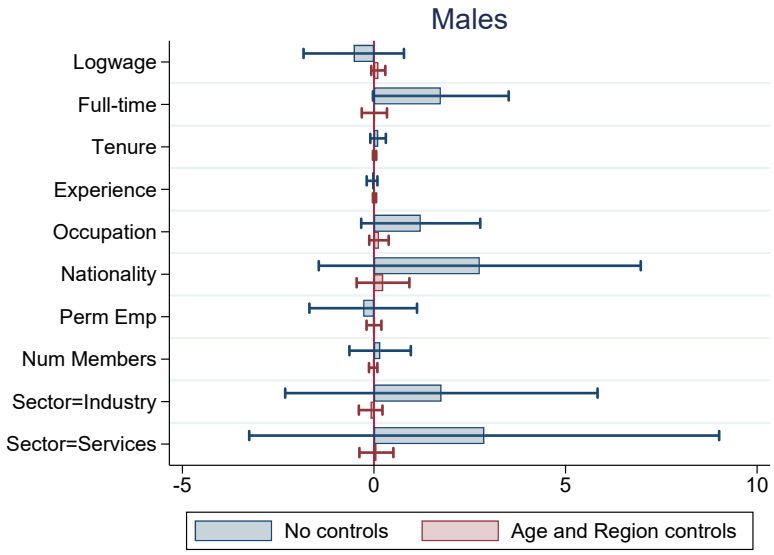
2.5.4.1 Allocation of Subsidies and Predetermined Characteristics

I next provide some evidence supporting the validity of my identifying assumption. I do so by estimating an OLS regression that studies whether a number of predetermined characteristics in the year before the subsidies started to be implemented predict the average subsidy for which employers were eligible during the period of analysis. Formally, I estimate the following specification:

$$AvS_{97-04} = \alpha + \beta X_{i,96} + Age_{i,96} + Age_{i,96}^2 + \delta_{j,96} + \varepsilon_i \tag{2.3}$$

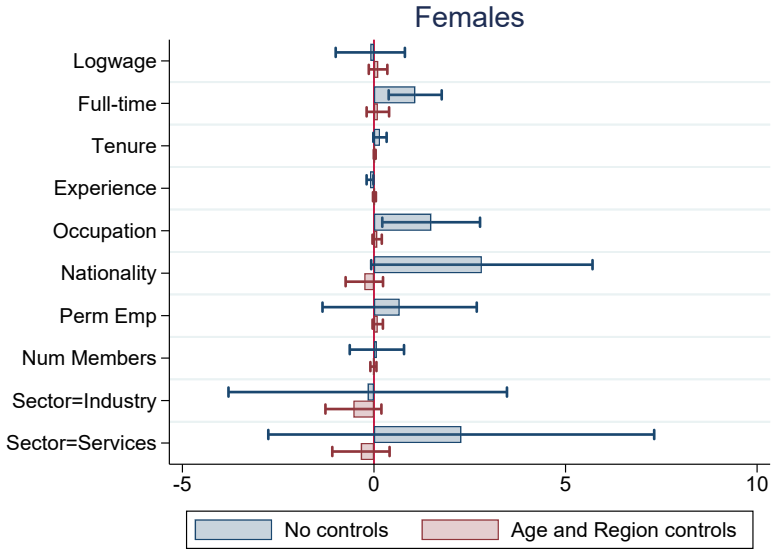
where AvS_{97-04} is the average subsidy for which a company is eligible during the period 1997-2004 if it converts the labour contract of temporary employee i into permanent. $X_{i,96}$ is a set of predetermined characteristics previous to the introduction of the subsidies. To illustrate the importance of controlling for the levels at which the subsidies varied, Figures 2.3 and 2.4 report the estimates of specification (2.3) with and without controlling for region (denoted by $\delta_{j,96}$) and age. I do not control for the two additional levels of variation of the subsidies (gender and year) because I estimate equation (2.3) separately by gender and because the controls included in this regression are not time-varying. Figures 2.3 and 2.4 respectively show the 95% confidence interval of the estimates for males and females. I find that several predetermined characteristics are unconditionally correlated with the subsidies for which employers were eligible. However, none of the estimates are statistically significant after controlling for age and region, and the magnitude of every coefficient becomes extremely small. Overall, the results of this subsection are consistent with the subsidies having been allocated exogenously, once I control for the levels of variation of the policy.

Figure 2.3: Correlation between Predetermined Characteristics and Subsidies for Males



The figure estimates a regression that uses as dependent variable the subsidies for which individuals were eligible during the period of analysis. The independent variables are a set of predetermined characteristics the year before the subsidies commenced to be implemented.

Figure 2.4: Correlation between Predetermined Characteristics and Subsidies for Females



The figure estimates a regression that uses as dependent variable the subsidies for which individuals were eligible during the period of analysis. The independent variables are a set of predetermined characteristics the year before the subsidies commenced to be implemented.

2.5.4.2 Selection and Sample Composition

A potential concern is that individuals may have selectively moved to regions where higher subsidies were being provided. As previously discussed, this is an unlikely event as subsidies were approved and announced almost at the same time, and given that there was a strong variation in the eligibility conditions over time. However, it is convenient to provide some evidence supporting this argument. Columns 1 and 2 of Table 2.5 estimate the baseline specification but restricting the sample to individuals that never moved across regions during the period of analysis. As shown, the estimates are very similar to the baseline results in terms of magnitude and significance, suggesting that the effect was not driven by those who moved across regions. Another important check is to test whether the baseline estimates are robust to changes in the sample composition. To evaluate this, columns 3 and 4 estimate the baseline specification separately by gender and restricting the sample to ages between 25 and 40. As shown, the estimate of permanent employment is positive and significant at the 1% confidence level for males, but not statistically significant for females. Moreover, the estimate of permanent employment increases in magnitude when I restrict the analysis to ages at which individuals are more likely to have a child. Overall, Table 2.5 suggests that the baseline estimates are robust to selection and changes in sample composition.

Table 2.5: Selection and Sample Composition

	Never moved		Sample 25-40	
	Male	Female	Male	Female
Depvar: Fertility$_{t+1}$	(1)	(2)	(3)	(4)
Perm Emp	0.0877**	-0.0005	0.2337***	0.1232
	(0.0430)	(0.0757)	(0.0507)	(0.1105)
Time-varying covariates	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Observations	1,282,389	913,235	944,681	662,024

* p<0.10, ** p<0.05, *** p<0.01
Standard errors in parentheses. All the specifications include (not shown in the table) the same time-varying covariates than the previous analysis.

2.5.4.3 Placebo Tests

This subsection adopts several placebo tests to evaluate whether holding an open-ended contract has an impact on the probability of having a new child in the following year, when an incorrect timing of the subsidies is used as an instrument. Columns 1 and 2 of Table 2.6 respectively estimate the baseline 2SLS specification for the sample of males and females, but assigning subsidies at $t+1$ as an instrument for permanent employment at t. Columns 3 and 4 are similar to the previous two columns, but use subsidies at $t+2$ as an instrument for permanent employment at t. As shown, the estimates in Table 2.6 do not support the presence of an impact of permanent employment on the fertility decision when I use an incorrect timing of the subsidies as instrument.

Table 2.6: Placebo Tests

	Subsidy$_{t+1}$ as IV		Subsidy$_{t+2}$ as IV	
	Male	Female	Male	Female
Depvar: Fertility$_{t+1}$	(1)	(2)	(3)	(4)
Perm Emp	-0.0461	-0.0180	-0.1658	-0.1706
	(0.0827)	(0.0815)	(0.1260)	(0.1290)
Time-varying covariates	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Observations	1,210,177	825,959	1,006,896	673,047

* p<0.10, ** p<0.05, *** p<0.01
Standard errors in parentheses. All the specifications include (not shown in the table) the following time-varying covariates: age, age^2, tenure, tenure^2, logarithm of the wage, regional fertility grants and a dummy that indicates whether the individual works under a full-time contract.

2.5.4.4 Alternative Definitions of the Instrument

I next estimate the baseline specification using alternative definitions of my instrument. First, columns 1-2 of Table 2.7 use the subsidies measured in percentage of labour costs saved during the first year of contract, and the square of this variable, as instruments. This specification accounts for the fact that subsidies may have a non-linear effect on the probability of an individual holding an open-ended contract. The use of two instruments for one endogenous variable also leads to an overidentified specification, which allows to test whether the excluded instruments are correlated with the error process. Second, columns 3-4 use as instrument a dummy that takes value 1 when an employer located in region j at time t is eligible for a subsidy if it converts a temporary employee of age a into a permanent worker, and 0 otherwise. This recognizes the possibility that employers may aim at receiving the subsidy (extensive margin) rather than at its quantity (intensive margin) when converting a temporary contract into a permanent one. Finally, columns 5-6 estimate my baseline specification using as instrument the subsidies measured in thousands of euros. The reason is that employers may have targeted the monetary value of the subsidies rather than the labour costs they save. As shown in Table 2.7, the previous specifications give similar estimates than the ones obtained in the main analysis. I also fail to reject the null hypothesis of the overidentifying restrictions being valid, which suggests that the excluded instruments are independent of the error process.

2.5.4.5 Alternative Specifications

A potential concern is that individuals of different ages might have followed dissimilar fertility trends during the period of analysis or had distinct fertility patterns depending on the region where they live. To address the previous matter, column 1 of Table 2.8 estimates a 2SLS specification similar to the baseline model but that controls for age using a set of dummies of intervals of five years (e.g. a dummy for age 20-24, another for 25-29, and so on), and column 2 includes second-order interactions between the age dummies and year and region

dummies as additional controls. Another possible concern is that subsidies may have been correlated with unobserved firm characteristics that determine fertility. This is unlikely as any company was eligible when subsidies were provided. However, to mitigate further this concern, column 3 includes employer fixed effects to account for unobserved time-invariant firm characteristics.

The results suggest that permanent employment has a positive impact on the probability of males having a new child in the following year. Estimates are higher in magnitude than those obtained using the baseline specification, and significant at the 1% and 5% confidence level. Columns 4-6 are identical to columns 1-3, but for the sample of females. Estimates of the effect of holding an open-ended contract at t on the probability of females having a new child at $t+1$ are not statistically significant. This is consistent with the baseline estimates.

2.5.5 Mechanisms

The previous results show that permanent employment has a positive causal impact on the fertility decision of males, but no effect for females. As previously explained, a possible explanation is that permanent employment not only increases job security but also career prospects and wages. A higher job security has a positive impact on fertility. Greater wages lead to two offseting effects. On the one hand, an income effect that increases fertility. On the other, a substitution effect that increases the value of the time of individuals in the labour market. As taking care of children requires time, the substitution effect increases the opportunity cost of having a child, and decreases fertility. Parenthood has a negative impact on the labour outcomes of mothers relative to fathers. Subsequently, the substitution effect is likely to be larger for women. I next provide evidence on this mechanism by exploring whether the impact of permanent employment on fertility is lower for individuals whose substitution effect is likely to be larger. More precisely, I study whether the estimates vary by age, wage growth and cohabitation status.

Table 2.7: Alternative Instruments

A.ESTIMATES						
	Subsidy and Subsidy^2		Subsidy Dummy		Subsidy Euros	
	Males	Females	Males	Females	Males	Females
Depvar: Fert$_{t+1}$	(1)	(2)	(3)	(4)	(5)	(6)
Perm Emp	0.0972**	0.0517	0.1155**	-0.0755	0.1518***	0.0429
	(0.0420)	(0.0585)	(0.0506)	(0.2012)	(0.0407)	(0.0792)
Time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
B.TESTS						
Over-identif	1.693	1.900				
P-value	0.1931	0.1681				
Observations	1,436,878	1,009,844	1,436,878	1,009,844	1,436,878	1,009,844

* p<0.10, ** p<0.05, *** p<0.01

Standard errors in parentheses. All the specifications include (not shown in the table) the following time-varying covariates: age, age^2, tenure, tenure^2, logarithm of the wage, regional fertility grants and a dummy that indicates whether the individual works under a full-time contract.

Table 2.8: Alternative Specifications

	Males			Females		
Depvar: Fert$_{t+1}$	(1)	(2)	(3)	(4)	(5)	(6)
Perm Emp	0.1305***	0.1596***	0.2735**	0.0129	-0.0219	-0.0019
	(0.0455)	(0.0516)	(0.1347)	(0.0730)	(0.0837)	(0.1176)
Time-varying cov	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry dumm	Yes	Yes	Yes	Yes	Yes	Yes
Age dum	Yes	Yes	Yes	Yes	Yes	Yes
Age dum*Reg FE	No	Yes	Yes	No	Yes	Yes
Age dum*Year FE	No	Yes	Yes	No	Yes	Yes
Firm FE	No	No	Yes	No	No	Yes
Observations	1,436,878	1,436,878	1,312,626	1,009,844	1,009,844	937,764

* p<0.10, ** p<0.05, *** p<0.01

Standard errors in parentheses. Columns 1-4 include a set of age dummies of intervals of five years instead of controlling for age and age^2. Columns 2 and 4 include second-order interactions between the age dummies, and the year and regional dummies. All the specifications include (not shown in the table) the following time-varying covariates: tenure, tenure^2, logarithm of the wage, regional fertility grants and a dummy that indicates whether the individual works under a full-time contract.

2.5.5.1 Age

I next study whether the effect of permanent employment on the fertility decision is heterogeneous in age. I do so by estimating a specification similar to the baseline model but that includes an interaction between the probability of holding a permanent contract and age as additional control.[25] I use an interaction between the subsidies and age as instrument for the latter variable. Figure 2.5 displays the estimates. As shown, holding an open-ended contract increases the probability of older males having a new child in the following year, but has a negligible impact on the fertility decision of mature females. Estimates of the effect of permanent employment on fertility are negative and significant for young individuals. A possible explanation is that permanent employment increases career prospects, and so, the value of the time of individuals in the labour market. This may be especially relevant for young individuals, who are more likely to invest in career development than older workers. The higher wage implicit in permanent employment, together with the ability of young employees to have a child later in life, may lead to a delay in their fertility decision.[26]

2.5.5.2 Wage Growth

It is also important to study whether the impact of permanent employment on fertility varies by wage growth. A high wage growth increases the value of the time of individuals in the labour market, and so, the opportunity cost of childbearing. This substitution effect may decrease fertility. Figure 2.6 estimates the baseline specification separately for employees who experience a wage growth above and below the median during the period of analysis. Although the estimates obtained in this subsection are not necessarily causal, as wage growth is an endogenous variable, it is useful to examine whether the estimates of perma-

[25] I also estimated a specification that includes an interaction between the probability of holding a permanent contract and the second-order polynomial in age as additional controls. I used an interaction between the subsidies and the second-order polynomial in age as instruments. However, these interactions were not statistically significant.

[26] It is important to show the probability of men and women having a child at different ages in Spain. As shown in Appendix A.12, it is frequent for males and females to have a child at a late age.

nent employment are lower when the opportunity cost of childbearing is high. I find that permanent employment has a higher impact on the fertility decision of individuals experiencing a low growth in their earnings relative to those who have a high wage growth. The latter holds independently of gender. Overall, the estimates suggest that fertility is invariant to an increase in job security when the opportunity cost of having a child is high.

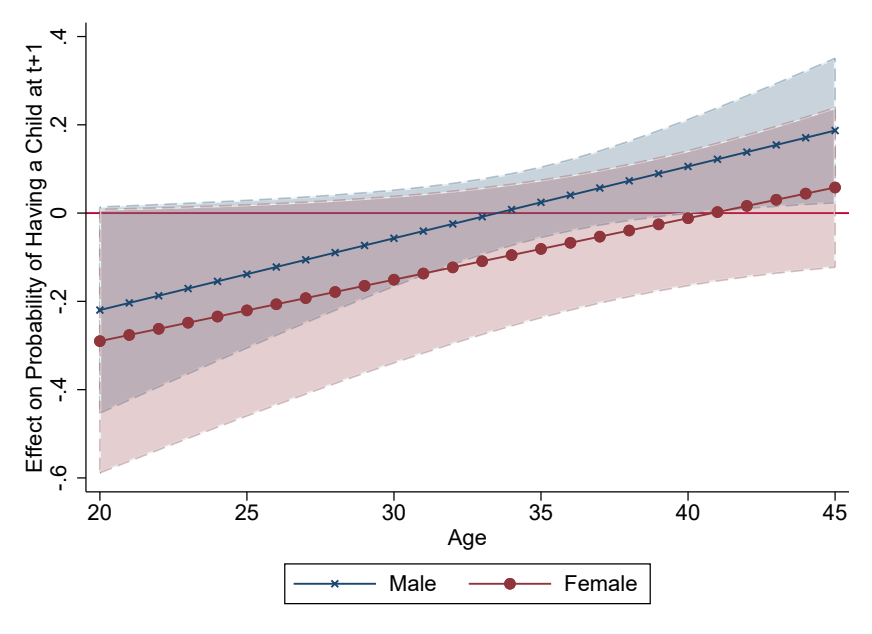
2.5.5.3 Cohabitation Status

This subsection studies whether the impact of permanent employment on fertility varies by cohabitation status. Three caveats must be considered when interpreting the results. First, the dataset does not contain information on the family link among household members. Nonetheless, the sample does not include adult individuals living with two or more people of a similar age. Previous literature has reported that when exactly two adults are living in a household their likelihood of being a couple is high (Carpenter et al., 2018). I therefore assume that two adult individuals are a couple if they live in the same household and their age difference is below 15. The estimates presented in this section are robust to using alternative definitions of a couple, as shown in Appendix A.13. Second, I only have information on whether an individual is cohabiting with a partner the last year when the individual is observed. Therefore, cohabitation status is not a time-varying covariate. Finally, cohabitation status is an endogenous variable, and I do not exploit any exogenous variation as an instrument for it. Nevertheless, it is relevant to study whether the estimates of permanent employment vary by cohabitation status. Non-cohabiting individuals are less likely to be able to share the costs of children with a partner. Yet, the higher wages implicit in permanent employment increase the value of their time in the labour market. As children are time intensive, the higher career prospects of permanent employment may lead to a higher substitution effect for non-cohabiting individuals.

Figure 2.7 estimates 2SLS specification (2.1) separately for individuals who are cohabiting with a partner and for those who are not. The estimates are also presented separately by gender. As shown, permanent employment has a posi-

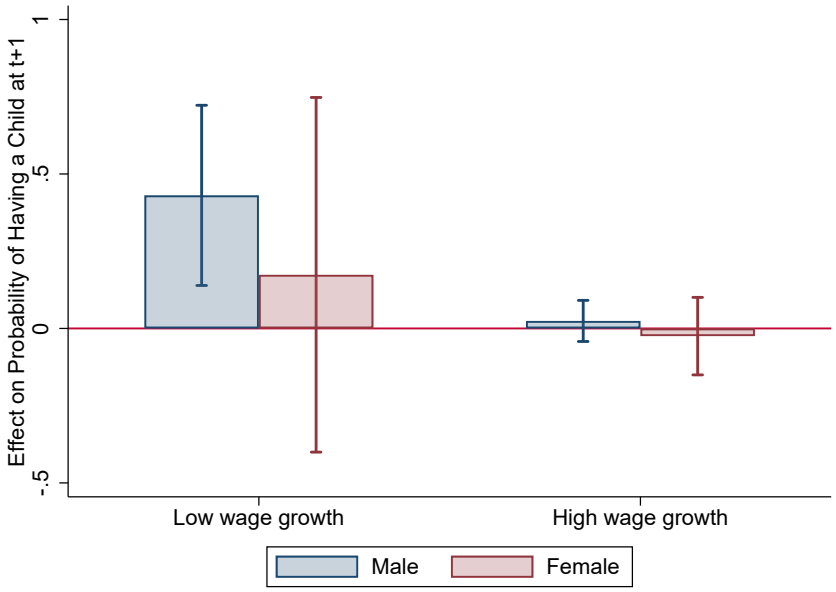
tive impact on the probability of males having a new child in the following year independently of cohabitation status. The estimate of permanent employment is also positive but not statistically significant for females cohabiting with a partner, and negative and significant for women who do not live with a couple. Overall, the magnitude of the effect of permanent employment is lower, or even negative, when individuals do not live with a couple. Finally, it is important to note that the estimates of permanent employment are positive for cohabiting males and females. This suggests that when the spouse holds a permanent contract, the probability of individuals having a child in the following year increases, if any.

Figure 2.5: Estimated Coefficients by Age



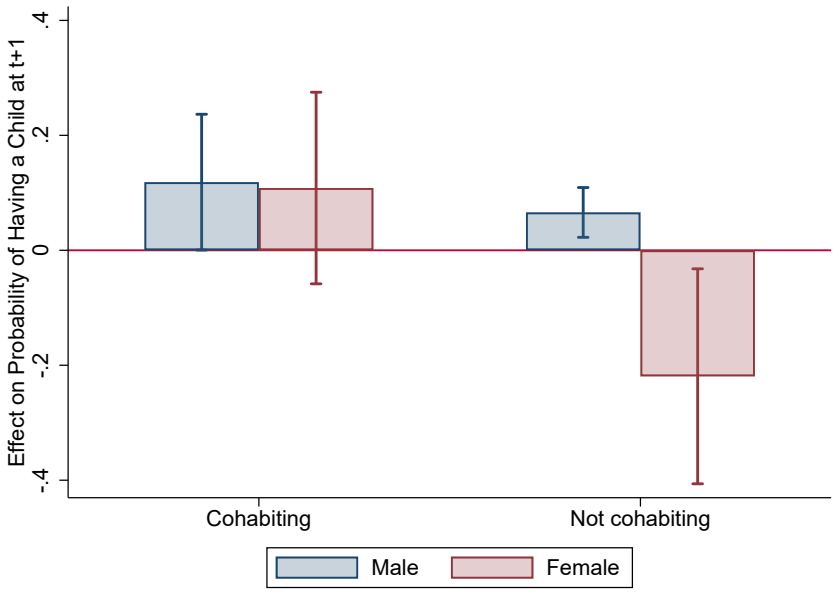
The figure estimates a specification similar to the baseline one but including an interaction between the probability of holding an open-ended contract and age. I display the average marginal effects of permanent employment for the different ages and gender of individuals.

Figure 2.6: Estimated Coefficients by Wage Growth



The figure estimates a regression similar to the baseline specification but respectively for the sample of individuals that are experiencing a high and a low growth in their wage, and separately by gender.

Figure 2.7: Estimated Coefficients by Cohabitation Status



The figure estimates a regression similar to the baseline specification but respectively for the sample of individuals that are cohabiting and not cohabiting with a couple, and separately by gender.

2.6 Aggregate Analysis

This section of the paper estimates a separate aggregate analysis using a different administrative dataset at the regional level. The aggregate analysis contributes to the individual one in several relevant ways. First, it is based on data that contains information on all the labour contracts and births that occur in Spain during the period of analysis. This allows to quantify the magnitude of the total effect of the subsidies on the labour market and fertility rates. Given that the data covers the whole population, it also adds representativeness to the previous analysis. Second, the dataset contains information on marriage rates, which is a relevant outcome variable that I could not observe in my individual dataset. Third, it also provides information on the extent of a number of public services at the municipality level, such as childcare or healthcare services. The reason why I did not control for these in the individual analysis is that the MCVL only provides information on the province of residence of individuals, which is too aggregate.[27] Finally, the aggregate analysis serves as a second set of evidence on the causal effect of permanent employment on fertility rates.

2.6.1 Aggregate Data

The dataset that I use in the aggregate analysis contains yearly information on the total number of births and labour contracts that take place at the municipality level in Spain. I construct this dataset from three different sources. First, data on employment was provided by the Spanish Public State Employment Service (SEPE). It contains information on the number of labour contracts created by type (temporary or permanent) and for the different groups of the population classified by age and gender. Second, fertility and sociodemographic data was provided by the National Statistical Institute (INE), and contains information on the number of births, marriages, deaths, fetal deaths, and proportion of immigrants at the municipality level. This data also provides information on the

[27] As previously explained, the MCVL only gives information about the municipality of residence of individuals if municipalities have a population higher than 40,000. However, 47.37% of the sample resides in municipalities where this is not the case.

female labour market participation rate, unemployment rate, and growth of the GDP at the province level.[28] Finally, I obtain data on the school attendance rate of children aged between 0 and 2 years old from the Spanish Ministry of Education.

I exploit exogenous variation in subsidies during the period 1997-2004, and so restrict the sample to these years. This results in a panel that contains 7,447 municipalities and 57,277 observations. Figures 2.8 and 2.9 respectively display the average number of permanent contracts and births per 1,000 childbearing population of the different municipalities during the period of analysis.[29] As shown, permanent employment is more prevalent at the centre and east of Spain, whereas fertility is higher at the central and southern regions of Spain. I present summary statistics of labour and sociodemographic characteristics of the different Spanish regions in Appendix A.14.

2.6.2 Aggregate Analysis Empirical Strategy

Using my aggregate dataset, I re-estimate the causal impact of permanent employment on the fertility decision by adopting the following 2SLS specification:

$$PE_{m,t} = \omega + \pi S_{m,t} + \eta X_{m,t} + \tau Y_{p,t} + \psi Z_{j,t} + \theta_t + \mu_m + \nu_{m,t} \tag{2.4}$$

$$F_{m,t+1} = \alpha + \beta PE_{m,t} + \phi X_{m,t} + \gamma Y_{p,t} + \zeta Z_{j,t} + \lambda_t + \delta_m + \varepsilon_{m,t} \tag{2.5}$$

where $F_{m,t+1}$ is the number of births per 1,000 childbearing population in municipality m at year $t+1$. $PE_{m,t}$ is the number of open-ended contracts created per 1,000 childbearing population in municipality m at year t. $S_{m,t}$ is the average subsidy for which an employer is eligible in municipality m at year t. As in the main analysis, it is measured in terms of percentage of labour costs saved during the first year of open-ended contracts. Appendix A.15 shows the details of how

[28] There are 52 provinces in Spain.

[29] Childbearing population is the population aged 16-45

I have constructed this variable. $S_{m,t}$ varies across municipalities and year of hire. $X_{m,t}$, $Y_{p,t}$ and $Z_{j,t}$ are vectors of time-varying covariates at the municipal, provincial, and regional level, respectively. The reason for controlling for time-varying covariates at three different geographical levels is that data is not always available at the municipality level. θ_t are year fixed effects, μ_m are municipality fixed effects to capture time-invariant characteristics, and $\nu_{m,t}$ is a municipality and time specific level error term. I allow for an arbitrary correlation of the error term at the regional level.[30]

The identifying assumption is that, conditional on observable time-varying covariates, municipality, and year fixed effects, my instrument $S_{m,t}$ is not correlated with $\varepsilon_{m,t}$. A first concern is that subsidies might have been correlated with the provision of some public services that have an effect on the fertility decision. For example, regional governments subsidizing permanent employment might have provided a different amount of free pre-school education,[31] maternity services at hospitals, or subsidies to childbearing. Following the regional governments' subsidies publications, this is an unlikely scenario. The reason is that the only goal of regional governments when giving subsidies was to incentivise permanent employment. Yet, it is worth to account for the previous public services. I take them into consideration by controlling for the proportion of children aged 0-2 attending nursery school in province p at year t,[32] the fetal death rate in municipality m at year $t+1$,[33] and a dummy that takes value 1 when a municipality is within a region that subsidizes fertility at year t. A second concern is that fertility rates and the provision of subsidies may have been driven by economic growth, or may have been correlated with sociodemographic characteristics of the population. I address this by controlling for the GDP growth of province p

[30] There are 17 regions in Spain.

[31] Previous research has shown the importance of welfare state support to reconciliate the work-family balance (Brodmann et al., 2007), with female participation in the labour market being higher as the proportion of 3-year-old children enrolled in public childcare increases (Nollenberger and Rodríguez-Planas, 2011).

[32] The proportion of children aged 0-2 attending nursery school is calculated as the number of children aged 0-2 attending nursery school divided by the total number of children aged 0-2 in province p at time t

[33] The fetal death rate is calculated as the number of babies that die at birth divided by the total number of births in municipality m at time $t+1$

at year t, and the proportion of immigrant population in municipality m at year t.[34]

2.6.3 Quantifying the Magnitude of the Impact of the Policy

Table 2.9 presents the estimates of the aggregate analysis. Columns 1 and 2 study the impact of the subsidies on the labour market. As shown, a decrease of 10% in the labour costs of the first year of open-ended contracts reduces (increases) by 9.35 (0.66) the number of temporary (permanent) contracts created per 1,000 population of childbearing age. The estimates are respectively significant at the 5% and 1% confidence level, and the F-statistic of the instrument is 13.1. Taking into account that the average subsidy reduced the labour costs of the first year of open-ended contracts by 24% (García Pérez and Rebollo Sanz, 2009a), and that there was approximately a 19 million population of childbearing age in Spain during the period of analysis, I calculate that the policy created 30,096 permanent contracts per year. Overall, the policy increased the creation of open-ended contracts, at the cost of reducing the use of temporary employment.

Columns 3-4 respectively show the OLS and 2SLS estimates of equation (2.5). The OLS estimate suggests that the creation of 1,000 additional permanent contracts increases the number of births by 7.20 in the following year. The estimate is significant at the 5% confidence level. Using the subsidies to open-ended contracts as an instrument for the permanent employment creation rate increases both the magnitude and significance of the estimate. In particular, the 2SLS estimate becomes 272.6 for the same rise in permanent employment, and it is significant at the 1% confidence level. As in my individual analysis, the higher magnitude of the 2SLS estimate suggests the presence of a strong bias in the OLS estimate of the impact of permanent employment on fertility. Possible causes of the bias have been already discussed. Using the 2SLS estimate, I calculate that the policy increased the number of births in Spain by 8,216 per

[34]The proportion of immigrant population is calculated as the number of foreign individuals divided by the total population of municipality m at time t.

year. The average number of births in Spain during the period of analysis was of 404,000 per year, and so, the policy increased this quantity by 2.03% each year it was in place. Moreover, the total public spending on subsidies was approximately 874 million of euros, and so, the implicit cost of the policy was 13,300 euros per child born. However, it is important to bear in mind that the only purpose of the policy was to increase permanent employment. Column 5 shows that permanent employment has a positive impact on marriage rates. The creation of 1,000 open-ended contracts increases the number of marriages by 138.1 in the following year.[35]

It is also important to interpret the estimates of the rest of the covariates in column 4. Not surprisingly, a better health system increases fertility rates, with the estimate being significant at the 1% confidence level. A better economic performance, however, has a negative impact on the fertility decision. This may be explained by economic expansions having a positive impact on the career prospects of individuals. This increases the value of the time of individuals in the labour market, which raises their opportunity cost of childbearing. Estimates of the provision of childcare and the proportion of immigrant population are not statistically significant. Overall, the policy increased the use of permanent employment, which had a positive effect on fertility rates.

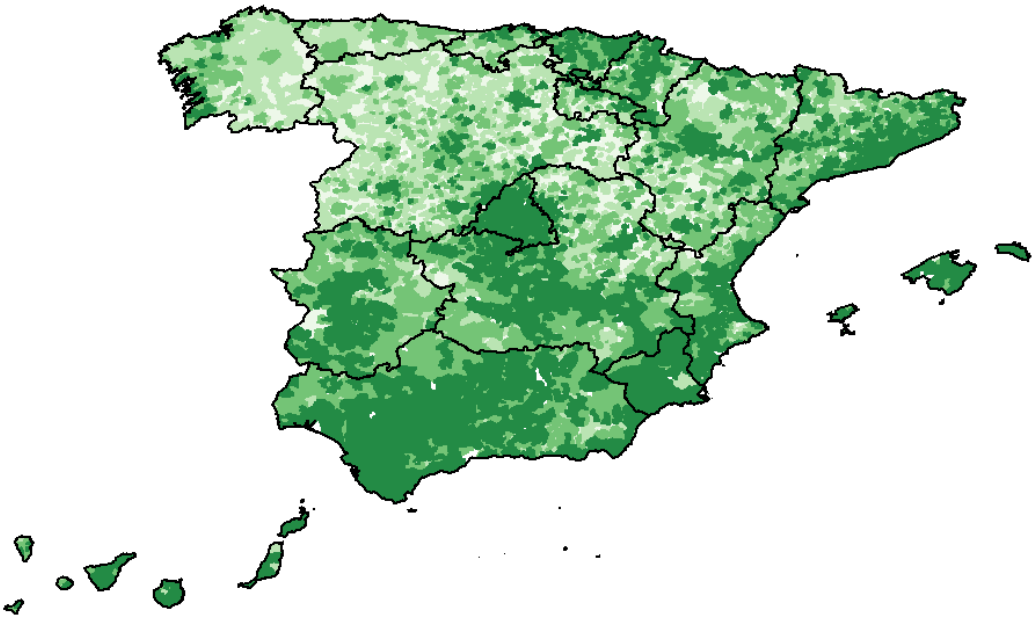
[35]As shown in Table A.9, using the fertility and marriage rates of municipality m at year $t+2$ as dependent variable leads to similar estimates than the ones obtained in this section.

Figure 2.8: Permanent Employment Rates by Municipality



The map presents the average permanent employment rate during the period 1997-2004 for each municipality in Spain. I group permanent employment rates by quartiles: lower or equal than 6.45, between 6.45 and 11.76, between 11.76 and 21.76, and higher than 21.76. I assign a colour to each of these categories, with darker colours referring to higher permanent employment rates.

Figure 2.9: Fertility Rates by Municipality



The map presents the average fertility rate during the period 1997-2004 for each municipality in Spain. I group fertility rates by quartiles: lower or equal than 3.33, between 3.33 and 5.99, between 5.99 and 8.95, and higher than 8.95. I assign a colour to each of these categories, with darker colours referring to higher fertility rates.

Table 2.9: Estimates of the First and Second Stages of the Aggregate Analysis

		First-stage	Second-stage		
			OLS	2SLS	
Depvar:	Temp Emp	Perm Emp	Fert $_{t+1}$	Fert $_{t+1}$	Marriage $_{t+1}$
	(1)	(2)	(3)	(4)	(5)
Subsidy	-0.9347**	0.0664***			
	(0.3656)	(0.0183)			
Perm Emp			0.0072**	0.2726***	0.1381**
			(0.0036)	(0.1002)	(0.0582)
Fert Death $_{t+1}$	0.0270	-0.0024	-0.0078***	-0.0072***	-0.0016
	(0.0342)	(0.0021)	(0.0018)	(0.0020)	(0.0011)
Fert Grant	-37.7615**	2.5033	0.3077	-0.3669	-0.5576
	(16.6463)	(2.2515)	(0.4909)	(0.5130)	(0.4430)
School Attend	1.4968	0.0476	0.0553	0.0277	-0.0162
	(1.7018)	(0.0739)	(0.0379)	(0.0447)	(0.0136)
Foreign Pop	-0.6012***	-0.0109	-0.0031	-0.0013	-0.0068**
	(0.1700)	(0.0165)	(0.0053)	(0.0054)	(0.0027)
Growth GDP	0.9928	-0.0976	-0.1045***	-0.0771***	-0.0196
	(1.4681)	(0.0841)	(0.0189)	(0.0239)	(0.0191)
Municip FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Panel B:			Tests		
F-stat instr				13.104	13.104
Observations	57,277	57,277	57,277	57,277	57,277

* p<0.10, ** p<0.05, *** p<0.01

Standard errors in parentheses. The sample contains yearly information on every labour contract and birth episode that occurred in Spain at the municipality level. I use the F-test of instrument to test whether my instrument is weak. I cluster standard errors at the regional level.

2.7 Conclusions

This paper studies the impact of permanent employment on the fertility decision. To identify a causal effect, I exploit exogenous variation in subsidies to use open-ended contracts at three different dimensions: (i) across regions, (ii) over time within regions, and (iii) by age of the employees. Using a Spanish administrative panel dataset at the individual level of 2.4 million observations, and the subsidies as an instrument for permanent employment, I estimate the causal impact of permanent employment on the fertility decision.

I find that permanent employment has a positive impact on the fertility decision of males, but no effect for women. I also show that permanent contracts only increase the probability of males entering parenthood, and that the effect vanishes for the second and third child. The effect may differ by gender because permanent employment increases job security and career prospects. A higher job security has a positive impact on fertility. An increase in wages leads to an income and a substitution effect that offset each other. The income effect increases fertility. The substitution effect arises because a higher wage increases the value of the time of individuals in the labour market. As taking care of children requires time, an increase in salaries raises the opportunity cost of having a child. This reduces fertility. However, the substitution effect may be larger for women. The reason is that parenthood reduces more labour force participation, wages and hours of work for females relative to males. I explore this as a plausible mechanism underlying the gender differences that I find in my baseline estimates. I do so by estimating whether the impact of permanent employment on fertility is lower when individuals are more likely to have a higher opportunity cost of having a child. Several findings suggest that this is the case. I find that permanent employment has a lower impact on fertility when individuals are young, have a high wage growth, or are non-cohabiting. Young individuals normally invest more in their career development and can easily delay the fertility decision. High-wage growth individuals experience a great career growth. Non-cohabiting individuals are less able to share the career penalties of children

with a partner.

This paper also estimates a separate analysis based on a different aggregate panel administrative dataset that contains yearly information on the total number of labour contracts and births that occur in each of the 7,447 municipalities in Spain. This analysis serves to quantify the incidence of subsidies on the labour market and fertility rates, to estimate the causal impact of permanent employment on marriage rates, and as a second set of evidence on the causal effect of permanent employment on fertility. The aggregate estimates are consistent with the results obtained in the main analysis, and I find that permanent employment also increases marriage rates. I calculate that subsidies raised the number of births in Spain by 2% every year the policy was in place at a cost of 13,300 euros per child. However, it is important to bear in mind that the only purpose of the policy was to increase permanent employment.

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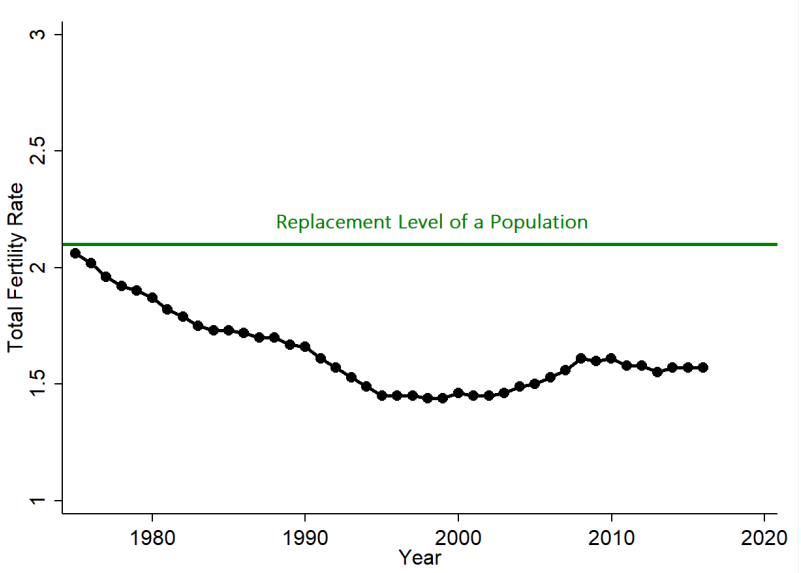
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A Appendix to Chapter 2

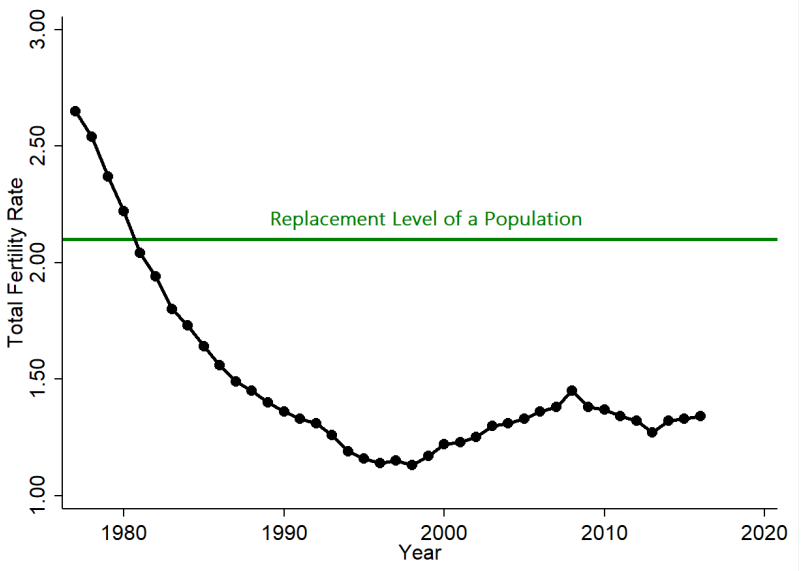
A.1 Total Fertility Rate

Figure A.1: Total Fertility Rate in the European Union



Source: World Bank Data. The figure presents the Total Fertility Rate of the European Union during the period from 1975 until present.

Figure A.2: Total Fertility Rate in Spain

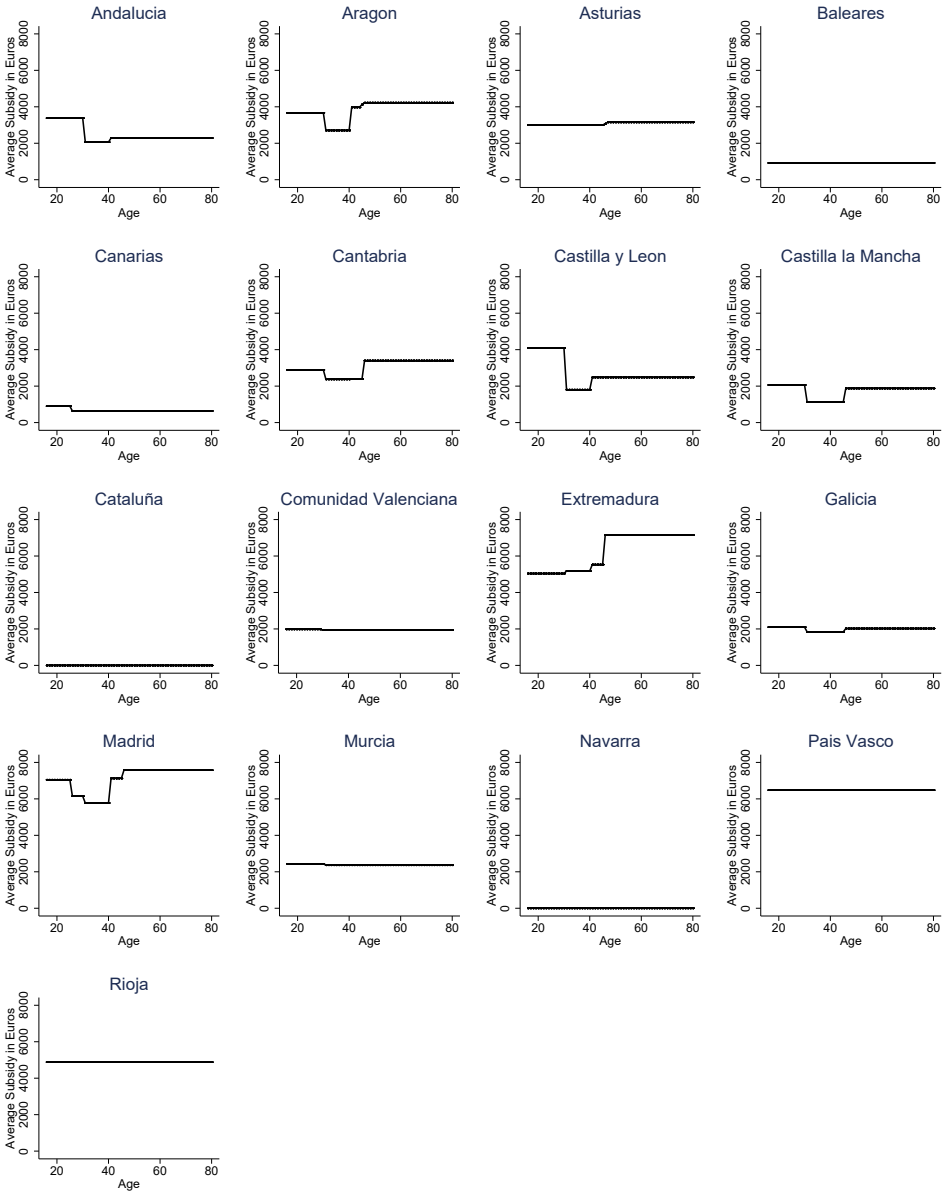


Source: World Bank Data. The figure presents the Total Fertility Rate of Spain during the period from 1975 until present.

A.2 Variation in Age and Gender

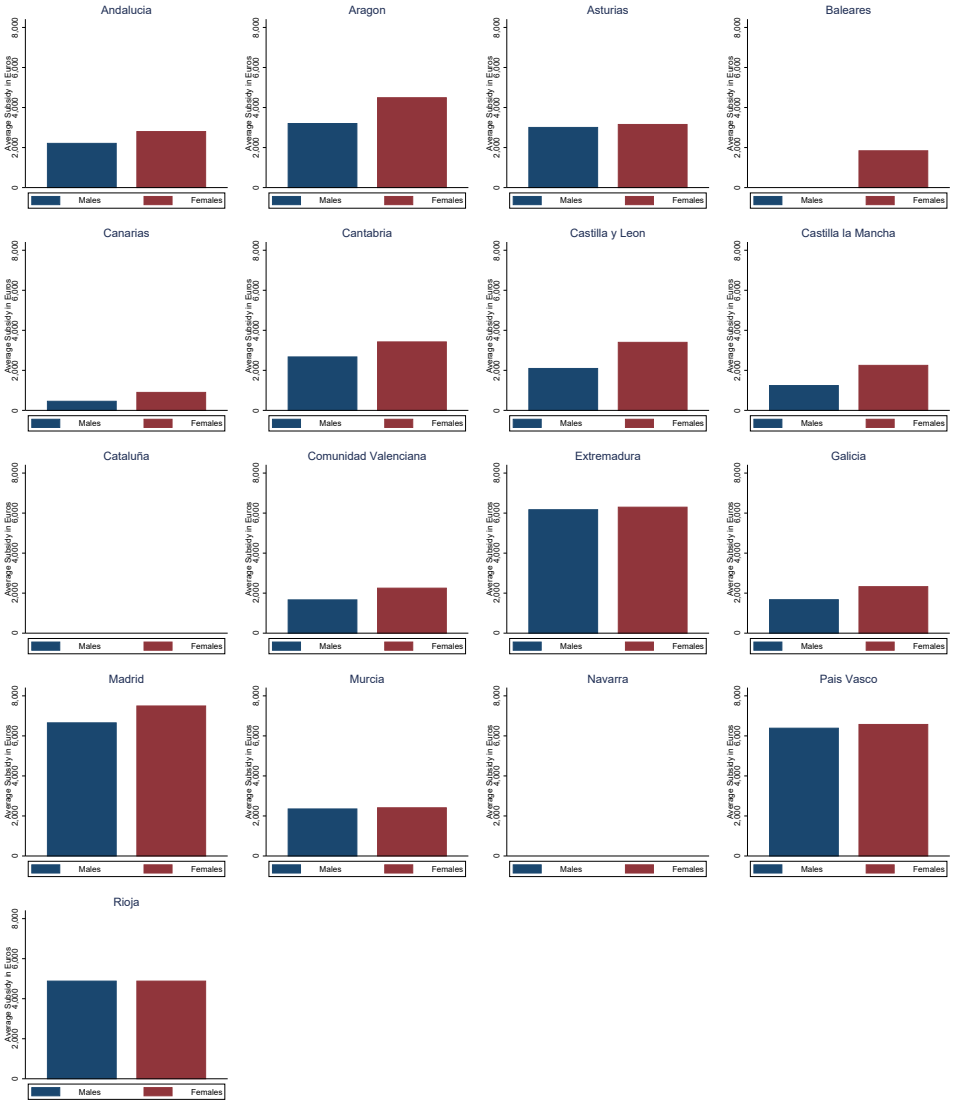
In this section, I present how subsidies varied in age and gender across regions. To do so, Figures A.3 and A.4 respectively show the average subsidy that was given by age and gender across the different regions during the period of analysis. However, it is important to note that the amount of the subsidy that was given within region-gender and region-age varied over the years.

Figure A.3: Average Subsidy by Age and CCAA



This figure shows the average subsidy that was given by age across the different regions during the period of analysis.

Figure A.4: Average Subsidy by Age and CCAA



This figure shows the average subsidy that was given by gender across the different regions during the period of analysis.

A.3 Transition Matrices

Table A.1 shows the probability of permanent and temporary employees holding a permanent or a temporary contract the year after. As shown, most of temporary and permanent employees do not change of type of contract in the next year. However, 17.1% of temporary employees hold a permanent contract at $t+1$, and 5.3% of permanent employees hold a temporary contract the next year. Table A.2 separates the transition probabilities by gender. As shown, male temporary employees are less likely to hold a permanent contract at year $t+1$ relative to female temporary employees. Male permanent employees are also less likely to hold a temporary contract at year $t+1$ relative to female permanent employees.

Table A.1: Transition Matrix

	Temp Emp	Perm Emp
Prob Temp Emp_{t+1}	0.829	0.053
	(0.377)	(0.224)
Prob Perm Emp_{t+1}	0.171	0.947
	(0.377)	(0.224)
Observations	1,017,463	1,298,127

Table A.2: Transition Matrix by Gender

	Males		Females	
	Temp Emp	Perm Emp	Temp Emp	Perm Emp
Prob Temp Emp_{t+1}	0.838	0.051	0.816	0.055
	(0.368)	(0.221)	(0.388)	(0.229)
Prob Perm Emp_{t+1}	0.162	0.949	0.184	0.945
	(0.368)	(0.221)	(0.388)	(0.229)
Observations	585,181	787,266	432,282	510,861

Table A.3 compares the descriptive statistics of three different groups: (i) employees who transit from temporary to permanent employment, (ii) workers who transit from permanent to temporary employment and (iii) employees who never change of contract. The table also reports the p-values of tests that compare the means of observable characteristics between switchers and those who never change of contract. As shown, switchers from temporary to permanent

employment earn more and are less likely to become unemployed than those who transit from permanent to temporary employment. Switchers from temporary to permanent employment have also worked longer for their current employer, are younger and have less experience. The table also shows that non-switchers are the employees with better labour outcomes. Non-switchers earn more, are less likely to become unemployed, have more experience and have worked longer for their current employer than any of the two switcher groups. Non-switchers are also older, more likely to have a child, and more likely to have had a child than the other two groups.

Table A.3: Summary Statistics

	Switcher Temp to Perm	Non-Switcher	p-val	Switcher Perm to Temp	Non-Switcher	p-val
	(1)	(2)	(3)	(4)	(5)	(6)
Prob Unemp$_{t+1}$	0.17	0.17	0.084	0.27	0.17	0.000
	(0.37)	(0.37)		(0.44)	(0.37)	
Monthly Wage	909.62	1065.45	0.000	834.46	1065.45	0.000
	(569.30)	(710.73)		(513.31)	(710.73)	
Tenure	3.12	5.06	0.000	2.81	5.06	0.000
	(2.10)	(4.80)		(2.16)	(4.80)	
Full-time	0.86	0.90	0.000	(0.88	0.90	0.000
	(0.35)	(0.30)		(0.32)	(0.30)	
Experience	9.21	12.62	0.000	10.17	12.62	0.000
	(5.95)	(7.59)		(6.16)	(7.59)	
White-collar	0.44	0.43	0.000	0.38	0.43	0.000
	(0.50)	(0.49)		(0.48)	(0.49)	
Age	29.96	33.57	0.000	30.33	33.57	0.000
	(6.43)	(7.06)		(6.51)	(7.06)	
Native	0.98	0.98	0.000	0.98	0.98	0.948
	(0.14)	(0.15)		(0.15)	(0.15)	
Prob having child	0.03	0.04	0.000	0.03	0.04	0.000
	(0.18)	(0.19)		(0.18)	(0.19)	
Child past 5 years	0.09	0.12	0.000	0.09	0.12	0.000
	(0.28)	(0.33)		(0.29)	(0.33)	
N of members	2.49	2.74	0.000	2.48	2.74	0.000
	(1.06)	(1.11)		(1.07)	(1.11)	
Observations	1,054,777	1,312,383		386,899	1,312,383	

A.4 Alternative Cluster Levels

Tables A.4 and A.5 are respectively similar to tables 2.3 and 2.4, but cluster standard errors at the individual and region level.

Table A.4: Cluster at the Individual and Region Level for Males

	OLS	2SLS	OLS	2SLS
Depvar: Fert_{t+1}	(1)	(2)	(3)	(4)
Perm Emp	0.0042***	0.0874**	0.0021**	0.0939**
	(0.0005)	(0.0402)	(0.0010)	(0.0397)
Individual FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Time-varying controls?	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Observations	1,436,878	1,436,878	1,436,878	1,436,878

* p<0.10, ** p<0.05, *** p<0.01.
Standard errors clustered at the individual and region level.

Table A.5: Cluster at the Individual and Region Level for Females

	OLS	2SLS	OLS	2SLS
Depvar: Fert_{t+1}	(1)	(2)	(3)	(4)
Perm Emp	0.0070***	0.0409	0.0081***	0.0082
	(0.0005)	(0.0524)	(0.0007)	(0.1006)
Individual FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Time-varying controls?	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Observations	1,009,844	1,009,844	1,009,844	1,009,844

* p<0.10, ** p<0.05, *** p<0.01.
Standard errors clustered at the individual and region level.

A.5 Using Different Set of Controls

Tables A.6 and A.7 estimate my baseline specification, but only controlling for truly exogenous time-varying covariates such as age, age^2 and fertility grants. I therefore do not control for other time-varying labour covariates. It is important to control for labour covariates, as doing so increases the precision of the estimate of permanent employment. However, this robustness check avoids the concern of including labour covariates that are outcomes of the subsidies.

Table A.6: Impact of Permanent Employment on Fertility for Males

	OLS	2SLS	OLS	2SLS
Depvar: Fert$_{t+1}$	(1)	(2)	(3)	(4)
Perm Emp	0.0078***	0.1137***	0.0027***	0.1260**
	(0.0004)	(0.0439)	(0.0006)	(0.0608)
Individual FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Time-varying controls?	Yes	Yes	Yes	Yes
Observations	1,473,619	1,473,619	1,473,619	1,473,619

* p<0.10, ** p<0.05, *** p<0.01
Standard errors in parentheses. I cluster standard errors at the individual and region-year level. In the specifications presented in the table, I only control for age, age^2, and fertility grants as time-varying covariates.

Table A.7: Impact of Permanent Employment on Fertility for Females

	OLS	2SLS	OLS	2SLS
Depvar: Fert$_{t+1}$	(1)	(2)	(3)	(4)
Perm Emp	0.0115***	0.0173	0.0108***	0.0162
	(0.0005)	(0.0300)	(0.0007)	(0.0679)
Individual FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Time-varying controls?	Yes	Yes	Yes	Yes
Observations	1,037,423	1,037,423	1,037,423	1,037,423

* p<0.10, ** p<0.05, *** p<0.01
Standard errors in parentheses. I cluster standard errors at the individual and region-year level. In the specifications presented in the table, I only control for age, age^2, and fertility grants as time-varying covariates.

A.6 Controlling for Occupation

In the baseline specification, I did not control for occupation as it is highly correlated with wage, which might generate a problem of collinearity. Moreover, both occupation and wage measure career development. This section tests whether my estimates on permanent employment are robust to the inclusion of occupation as a covariate. As shown in Tables A.8 and A.9, this is the case.

Table A.8: Impact of Permanent Employment on Fertility for Males

	OLS	2SLS	OLS	2SLS
Depvar: Fert_{t+1}	(1)	(2)	(3)	(4)
Perm Emp	0.0039***	0.0879**	0.0020***	0.0952**
	(0.0004)	(0.0341)	(0.0007)	(0.0435)
Individual FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Time-varying controls?	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Observations	1,436,515	1,436,515	1,436,515	1,436,515

* p<0.10, ** p<0.05, *** p<0.01
Standard errors in parentheses. I cluster standard errors at the individual and region-year level. In the specifications presented in the table, I control for age, age^2, fertility grants, the logarithm of the wage, whether the individual holds a full-time contract, tenure, tenure^2, and whether the individual is a white-collar worker, as time-varying covariates.

Table A.9: Impact of Permanent Employment on Fertility for Females

	OLS	2SLS	OLS	2SLS
Depvar: Fert_{t+1}	(1)	(2)	(3)	(4)
Perm Emp	0.0070***	0.0423	0.0081***	0.0096
	(0.0004)	(0.0453)	(0.0007)	(0.0725)
Individual FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Time-varying controls?	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Observations	1,009,581	1,009,581	1,009,581	1,009,581

* p<0.10, ** p<0.05, *** p<0.01
Standard errors in parentheses. I cluster standard errors at the individual and region-year level. In the specifications presented in the table, I control for age, age^2, fertility grants, the logarithm of the wage, whether the individual holds a full-time contract, tenure, tenure^2, and whether the individual is a white-collar worker, as time-varying covariates.

A.7 Goodness of Fit

This section shows the share of correctly predicted outcomes by the models that I use in columns 2 and 4 of Tables 2.3 and 2.4. The indicator variable of fertility takes value 0 with a frequency of 96.7%. I therefore choose as a cut-off the unconditional mean of fertility, which is 0.33. The models predict better outcome 1.

Table A.10: Goodness of Fit Baseline Estimates Males

	2SLS without FE $Fert_{t+1}$		
$\widehat{Fert}_{t+1}$	**0**	**1**	
% Correctly Predicted Values	46.5%	69.4%	
N observations	1,382,980	53,898	1,436,878

	2SLS with FE $Fert_{t+1}$		
$\widehat{Fert}_{t+1}$	**0**	**1**	
% Correctly Predicted Values	49.1%	61.9%	
N observations	1,382,980	53,898	1,436,878

Table A.11: Goodness of Fit Baseline Estimates Females

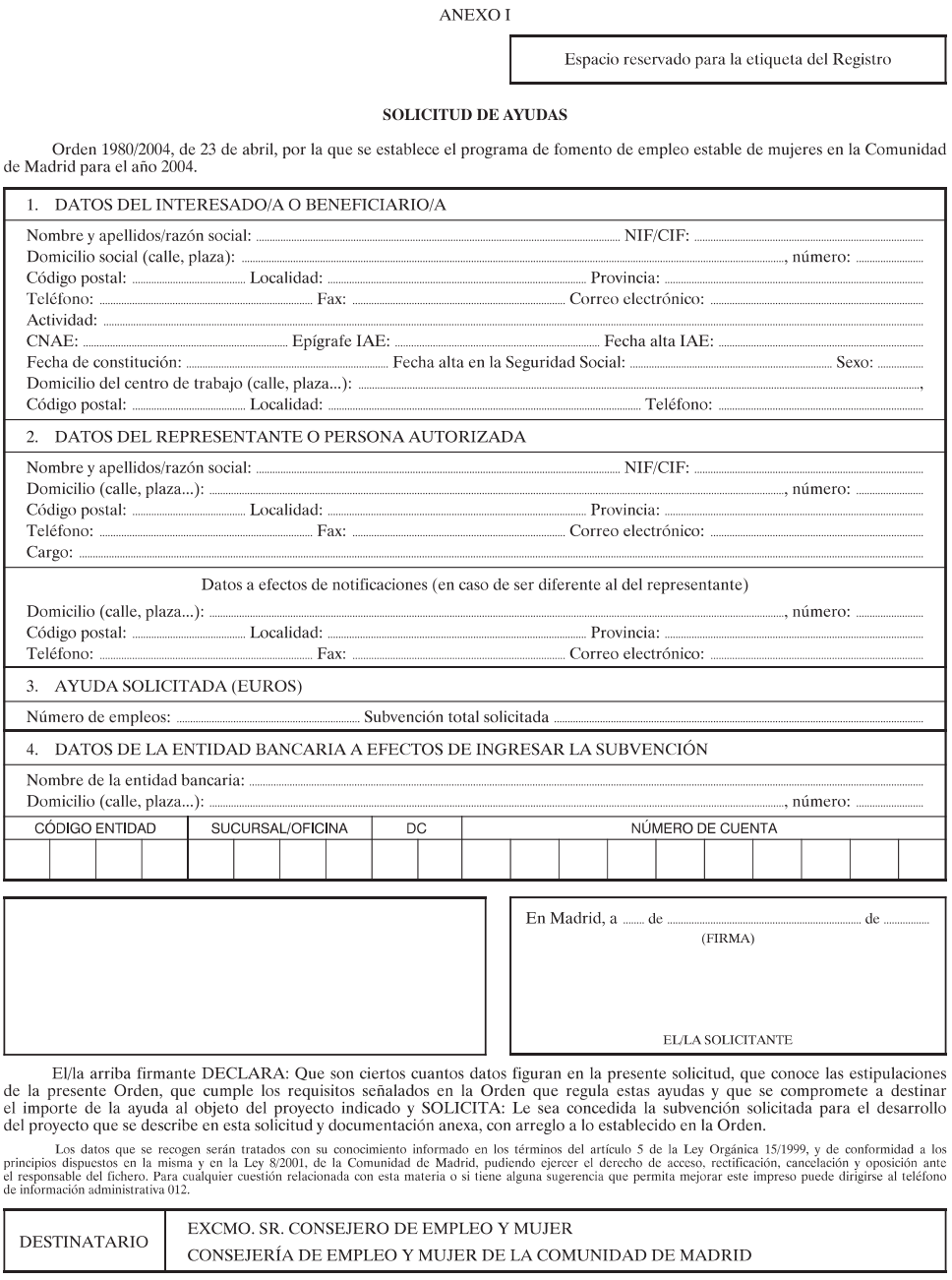
	2SLS without FE $Fert_{t+1}$		
$\widehat{Fert}_{t+1}$	**0**	**1**	
% Correctly Predicted Values	41.7%	82.6%	
N observations	969,552	40,292	1,009,844

	2SLS with FE $Fert_{t+1}$		
$\widehat{Fert}_{t+1}$	**0**	**1**	
% Correctly Predicted Values	0%	100%	
N observations	969,552	40,292	1,009,844

A.8 Application Form and Documentation

Figures A.5 and A.6 show the application form that employers needed to complete when applying for a subsidy for the contract conversion of a temporary employee into a permanent one. As shown, this was not a long application, and so, when employers were eligible for subsidies it was easy for them to apply. In addition to the application form, employers needed to send several documents to the regional authority in order to receive the subsidy. In particular, employers needed to send the following documents: ID number of the representative of the company, registration form of the employer within the companies' registry, Social Security registration of the company, tax declaration of the company, labour contract of the employee and the labour history of the employee from the Social Security Records. Applying or not for subsidies, employers need to have this documentation, and so, they only had to send these documents together with the application form to receive the subsidy.

Figure A.5: Application Form



ANEXO I

Espacio reservado para la etiqueta del Registro

SOLICITUD DE AYUDAS

Orden 1980/2004, de 23 de abril, por la que se establece el programa de fomento de empleo estable de mujeres en la Comunidad de Madrid para el año 2004.

1. DATOS DEL INTERESADO/A O BENEFICIARIO/A

Nombre y apellidos/razón social: ..... NIF/CIF: .....
Domicilio social (calle, plaza): ....., número: .....
Código postal: ..... Localidad: ..... Provincia: .....
Teléfono: ..... Fax: ..... Correo electrónico: .....
Actividad: .....
CNAE: ..... Epígrafe IAE: ..... Fecha alta IAE: .....
Fecha de constitución: ..... Fecha alta en la Seguridad Social: ..... Sexo: .....
Domicilio del centro de trabajo (calle, plaza...): .....,
Código postal: ..... Localidad: ..... Teléfono: .....

2. DATOS DEL REPRESENTANTE O PERSONA AUTORIZADA

Nombre y apellidos/razón social: ..... NIF/CIF: .....
Domicilio (calle, plaza...): ....., número: .....
Código postal: ..... Localidad: ..... Provincia: .....
Teléfono: ..... Fax: ..... Correo electrónico: .....
Cargo: .....

Datos a efectos de notificaciones (en caso de ser diferente al del representante)

Domicilio (calle, plaza...): ....., número: .....
Código postal: ..... Localidad: ..... Provincia: .....
Teléfono: ..... Fax: ..... Correo electrónico: .....

3. AYUDA SOLICITADA (EUROS)

Número de empleos: ..... Subvención total solicitada .....

4. DATOS DE LA ENTIDAD BANCARIA A EFECTOS DE INGRESAR LA SUBVENCIÓN

Nombre de la entidad bancaria: .....
Domicilio (calle, plaza...): ....., número: .....

CÓDIGO ENTIDAD				SUCURSAL/OFICINA				DC		NÚMERO DE CUENTA									

En Madrid, a ..... de ..... de .....

(FIRMA)

EL/LA SOLICITANTE

El/la arriba firmante DECLARA: Que son ciertos cuantos datos figuran en la presente solicitud, que conoce las estipulaciones de la presente Orden, que cumple los requisitos señalados en la Orden que regula estas ayudas y que se compromete a destinar el importe de la ayuda al objeto del proyecto indicado y SOLICITA: Le sea concedida la subvención solicitada para el desarrollo del proyecto que se describe en esta solicitud y documentación anexa, con arreglo a lo establecido en la Orden.

Los datos que se recogen serán tratados con su conocimiento informado en los términos del artículo 5 de la Ley Orgánica 15/1999, y de conformidad a los principios dispuestos en la misma y en la Ley 8/2001, de la Comunidad de Madrid, pudiendo ejercer el derecho de acceso, rectificación, cancelación y oposición ante el responsable del fichero. Para cualquier cuestión relacionada con esta materia o si tiene alguna sugerencia que permita mejorar este impreso puede dirigirse al teléfono de información administrativa 012.

DESTINATARIO	EXCMO. SR. CONSEJERO DE EMPLEO Y MUJER CONSEJERÍA DE EMPLEO Y MUJER DE LA COMUNIDAD DE MADRID

The figure presents a standard application form that employers needed to fill to apply for subsidies.

Figure A.6: Application Form

CONSENTIMIENTO DE CESIÓN DE DATOS PERSONALES

N.º Expte: ..... /04

DATOS DE LA ENTIDAD
Nombre de la entidad .....
NIF/CIF .....
DATOS DE LA OTORGANTE DEL CONSENTIMIENTO DE CESIÓN DE DATOS PERSONALES
D. .....
con NIF ....., en su calidad de .....
de la entidad .....
domiciliada en ..... calle .....
..... presenta la siguiente
AUTORIZACIÓN
El/la firmante cuyos datos figuran en este escrito consiente en la cesión, por parte de la Agencia Estatal de Administración Tributaria (AEAT) a la Dirección General de Empleo de la Consejería de Empleo y Mujer, de sus datos de carácter tributario[1], a efectos de la tramitación y seguimiento del expediente número ....., gestionado al amparo de la Orden ..... (BOLETÍN OFICIAL DE LA COMUNIDAD DE MADRID de .....), por la que se establece el programa de Fomento de Empleo Estable en la Comunidad de Madrid para el año 2004. La presente autorización se otorga al amparo de lo establecido en el artículo 11 de la Ley Orgánica 15/1999, de 13 de diciembre, de Protección de Datos de Carácter Personal ("Boletín Oficial del Estado" número 298, de 14 de diciembre). La cesión de estos datos tiene un carácter facultativo y revocable, de acuerdo con lo estipulado en la Ley Orgánica 15/1999, de 13 de diciembre. Dado el carácter facultativo de esta cesión, el no consentimiento a la misma supone la obligación por parte del/de el/la interesado/a de aportar por sí mismo/a la documentación exigida en la Orden de convocatoria de ayudas que sirve de base para la tramitación y seguimiento de este expediente. Para cualquier cuestión relacionada con esta materia puede dirigir su consulta a la Agencia de Protección de Datos de la Comunidad de Madrid a través del teléfono de información administrativa 012.

En Madrid, a ..... de ..... de 2004

(FIRMA Y SELLO DE LA EMPRESA)

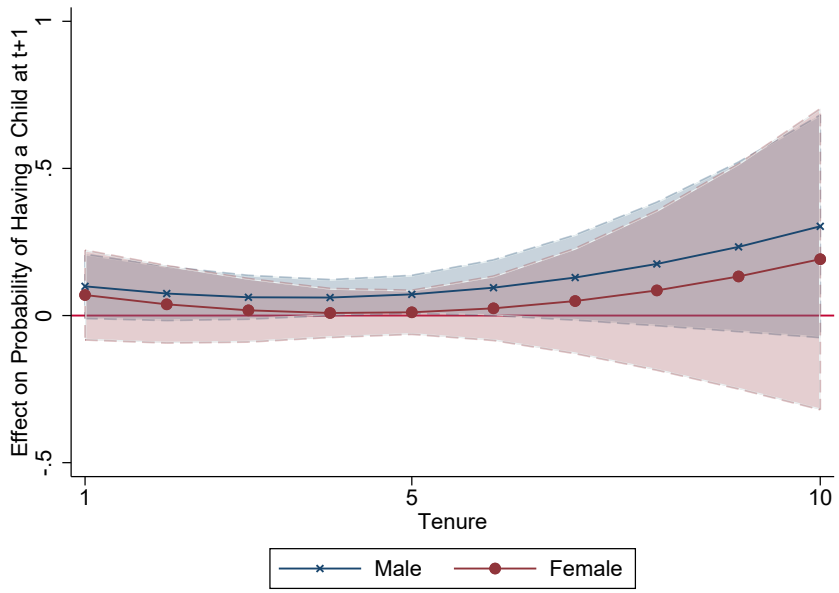
[1] Este Anexo de consentimiento de cesión de datos personales se refiere a los datos tributarios de la empresa solicitante, para lo cual deberá ser cumplimentado y firmado por la empresaria individual, o en el caso de las Pymes, por el socio o socios de la misma o persona con capacidad para ello.

The figure presents a standard application form that employers needed to fill to apply for subsidies.

A.9 Persistence of the Effect

Figure A.7 displays the estimates of a model similar to the baseline one but that includes an interaction between the probability of holding an open-ended contract and a second-order polynomial in the number of years that an employee has worked for the same employer. I instrument the latter using an interaction between the subsidies and the second-order polynomial in the tenure of the labour contract. I find that the impact of permanent employment on the fertility decision of males is positive and U-shaped in the number of years that they have held a permanent position. The estimates of the first six years of tenure of the labour contract are significant at the 5% confidence level. Figure A.7 shows that the effect is positive and U-shaped for females too, albeit the estimates are not statistically significant.

Figure A.7: Estimated Coefficients by Tenure



The figure estimates a regression similar to the baseline specification but that also includes an interaction between the probability of holding an open-ended contract and a second-order polynomial in tenure. I display the average marginal effects of permanent employment for the different years that an employee has held a permanent position and separately by gender.

A.10 Using Alternative Dependent Variables

Tables A.12 and A.13 report the estimates of the baseline specification when I use as dependent variable the probability of having a new child two years after.

Table A.12: OLS & 2SLS Estimates Second Stage for Males

	OLS	2SLS	OLS	2SLS
Depvar: Fert_{t+2}	(1)	(2)	(3)	(4)
Perm Emp	0.0051***	0.1418***	0.0035***	0.0802**
	(0.0004)	(0.0312)	(0.0006)	(0.0382)
Time-varying covariates	Yes	Yes	Yes	Yes
Individual FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Observations	1,436,878	1,436,878	1,436,878	1,436,878

* p<0.10, ** p<0.05, *** p<0.01
Standard errors in parentheses. The sample contains employees aged 20-45. All the specifications include (not shown in the table) the following time-varying covariates: tenure, tenure^2, logarithm of the wage, regional fertility grants and a dummy that indicates whether the individual works under a full-time contract. I cluster standard errors at the individual and region-year level.

Table A.13: OLS & 2SLS Estimates Second Stage for Females

	OLS	2SLS	OLS	2SLS
Depvar: Fert_{t+2}	(1)	(2)	(3)	(4)
Perm Emp	0.0079***	0.0580	0.0089***	0.0283
	(0.0004)	(0.0446)	(0.0007)	(0.0640)
Time-varying covariates	Yes	Yes	Yes	Yes
Individual FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Observations	1,009,844	1,009,844	1,009,844	1,009,844

* p<0.10, ** p<0.05, *** p<0.01
Standard errors in parentheses. The sample contains employees aged 20-45. All the specifications include (not shown in the table) the following time-varying covariates: tenure, tenure^2, logarithm of the wage, regional fertility grants and a dummy that indicates whether the individual works under a full-time contract. I cluster standard errors at the individual and region-year level.

A.11 Bivariate Probit Model

Following Angrist and Pischke (2009), it is also possible to use a bivariate probit model to estimate the causal effect of an endogenous binary variable on a dummy outcome variable, although 2SLS is more robust. I therefore adopt the following bivariate probit model as an alternative to my baseline specification to study the impact of permanent employment on the fertility decision:

$$prob[F_{i,t+1} = 1] = prob[\alpha + \beta PE_{i,t} + \delta X_{i,t} + \varepsilon_{i,t} > 0] = \Phi[\alpha + \beta PE_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}] \tag{2.6}$$

$$prob[PE_{i,t} = 1] = prob[\omega + \pi S_{j,g,a,t} + \eta X_{i,t} + \nu_{i,t} > 0] = \Phi[\omega + \pi S_{j,g,a,t} + \eta X_{i,t} + \nu_{i,t}] \tag{2.7}$$

where $\Phi[.]$ is the cumulative distribution function of the standard normal distribution. The dependent and explanatory variables are the same than the ones included in my baseline specification. Year dummies are included in $X_{i,t}$. I do not control for individual fixed effects in my bivariate probit model as their inclusion would lead to the incidental parameters problem, and so to inconsistent estimates of my variables of interest. $\varepsilon_{i,t}$ and $\nu_{i,t}$ are individual and time specific level error terms. I perform the analysis separately for males and females.

The maximum likelihood estimates for the sample of males are presented in Table A.14. As shown in column 1, the probit estimates indicate that permanent employment has a positive impact on the fertility decision of males. However, column 3 shows that the correlation between the residuals in equations (2.6) and (2.7) is negative and significant, suggesting that the fertility decision is endogenous to the type of contract. Column 3 therefore estimates (2.6) and (2.7) simultaneously, assuming that $\varepsilon_{i,t}$ and $\nu_{i,t}$ follow a bivariate normal distribution. The bivariate probit estimate of permanent employment is higher in magnitude than the probit coefficient, and significant at the 1% confidence level. The average marginal effect is displayed in column 4, and I use the delta method to calculate its variance. As shown, holding a permanent contract increases the probabil-

ity of male employees having a child in the following period by 2.26%. Table A.15 is identical to Table A.14, but for the sample of females. As shown, the probit estimate suggests that permanent employment has a positive impact on the fertility decision of females. However, the estimate and the average marginal effect become not statistically significant after allowing for the possibility of the unobserved determinants of fertility and the type of contract being correlated. These results are consistent with my baseline estimates.

Table A.14: Maximum Likelihood Estimates Second Stage for Males

	Probit	AME	Biprobit	AME
Depvar: Fert_{t+1}	(1)	(2)	(3)	(4)
Perm Emp	0.0463***	0.0036***	0.2834***	0.0226***
	(0.0051)	(0.0004)	(0.0551)	(0.0046)
ρ			-0.1441***	
			(0.0340)	
Time-varying covariates	Yes		Yes	
Region FE	Yes		Yes	
Year FE	Yes		Yes	
Industry dummies	Yes		Yes	
Observations	1,436,878		1,436,878	

* p<0.10, ** p<0.05, *** p<0.01
Standard errors in parentheses. All the specifications include (not shown in the table) the following time-varying covariates: age, age^2, tenure, tenure^2, logarithm of the wage, regional fertility grants and a dummy that indicates whether the individual works under a full-time contract.

Table A.15: Maximum Likelihood Estimates Second Stage for Females

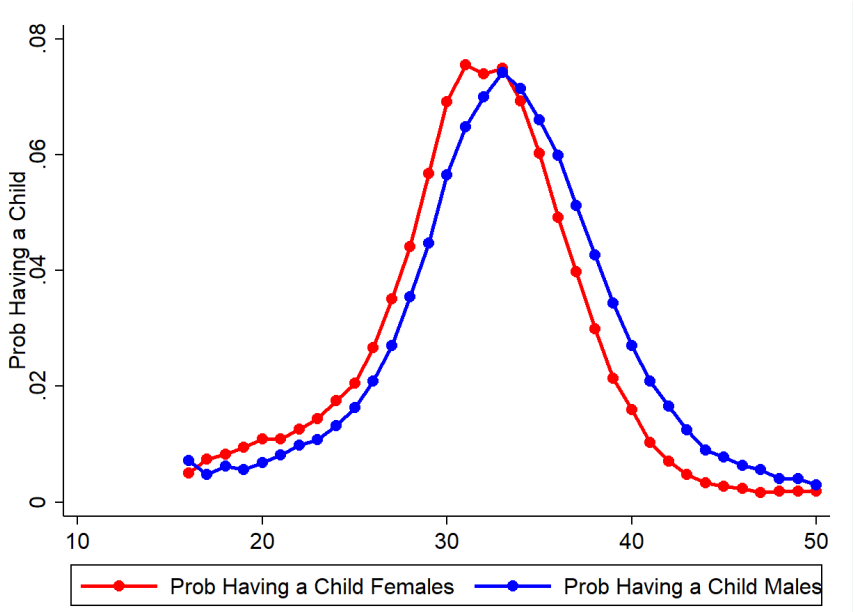
	Probit	AME	Biprobit	AME
Depvar: Fert_{t+1}	(1)	(2)	(3)	(4)
Perm Emp	0.0754***	0.0061***	-0.0270	-0.0023
	(0.0054)	(0.0004)	(0.0667)	(0.0057)
ρ			0.0680*	
			(0.0401)	
Time-varying covariates	Yes		Yes	
Region FE	Yes		Yes	
Year FE	Yes		Yes	
Industry dummies	Yes		Yes	
Observations	1,009,844		1,009,844	

* p<0.10, ** p<0.05, *** p<0.01
Standard errors in parentheses. All the specifications include (not shown in the table) the following time-varying covariates: age, age^2, tenure, tenure^2, logarithm of the wage, regional fertility grants and a dummy that indicates whether the individual works under a full-time contract.

A.12 Fertility Distribution in Age

Figure A.8 shows the probability of men and women having a child at different ages in Spain. As displayed, it is frequent for males and females to have a child at a late age.

Figure A.8: Fertility Distribution in Age

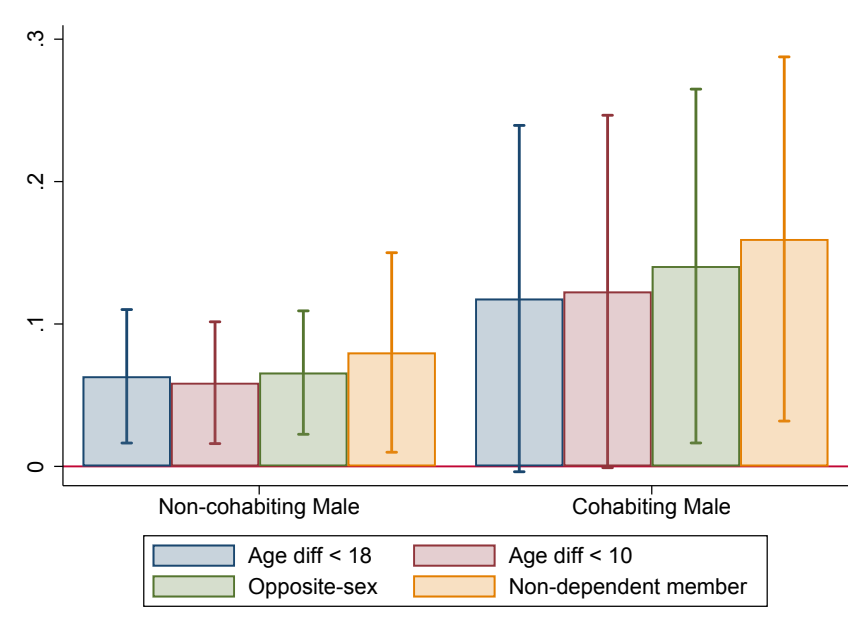


The figure shows the probability of males and females having a child at different ages.

A.13 Different Definitions of Cohabitation Status

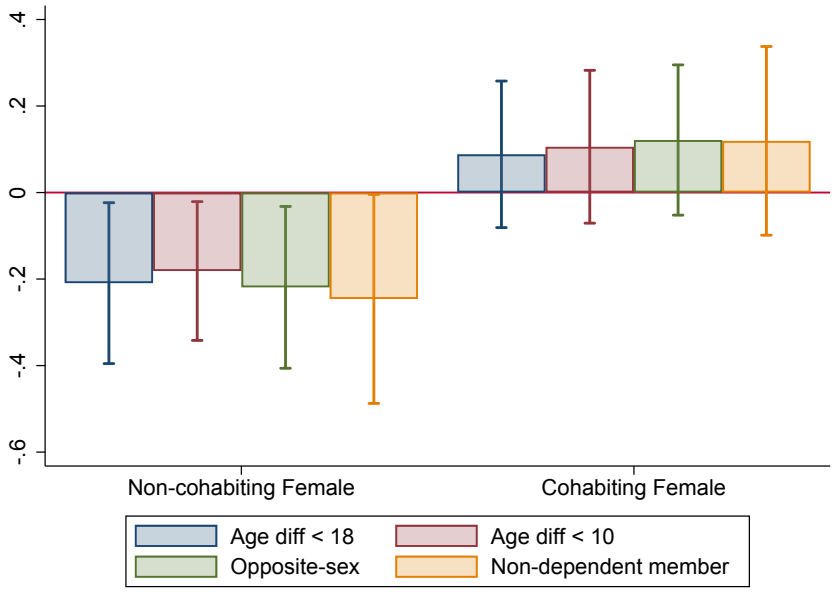
This section tests whether the cohabitation status estimates displayed earlier in the paper are robust to the use of different definitions of couple. As in the main analysis, all the specifications in Figures A.9 and A.10 exclude households where there is more than one adult individual that could be the couple of the sampled individuals. I use four definitions of couple different than the one used in the main analysis. First, I define a couple as two adult individuals that live in the same household as long as their age difference is below 18. Second, I restrict the age difference to 10 years. Third, I set the age difference to 15 years (as in the main analysis) but add the restriction of the two adults being of opposite-sex. Finally, I restrict the analysis to households where the sampled individuals do not cohabit with adults other than their couple. This excludes households where there are old-age dependent individuals. As shown in Figures A.9 and A.10, the estimates are robust to the use of different definitions of couple.

Figure A.9: Estimated Coefficients for Males by Cohabitation Status



The figure estimates a regression similar to the baseline specification but respectively for the sample of males that are cohabiting and not cohabiting with a couple, using different definitions of couple.

Figure A.10: Estimated Coefficients for Females by Cohabitation Status



The figure estimates a regression similar to the baseline specification but respectively for the sample of females that are cohabiting and not cohabiting with a couple, using different definitions of couple.

A.14 Descriptive Statistics Aggregate Analysis

Table A.16: Descriptive Statistics Aggregate Analysis

	Andalucia	Aragon	Asturias	Baleares	Canarias	Cantabria	Castilla y Leon	Castilla la Mancha
N births per 1,000	21.0	12.4	12.7	22.7	19.8	15.9	10.8	15.6
childbearing pop	(8.5)	(19.8)	(5.0)	(5.9)	(6.0)	(7.8)	(14.6)	(26.2)
N dead babies per 1,000 births	4.0	2.0	3.1	3.7	3.3	3.9	2.3	2.3
	(25.7)	(28.9)	(16.5)	(12.6)	(14.4)	(40.4)	(36.1)	(30.6)
N marriages per 1,000	9.7	5.9	8.6	10.1	7.7	9.7	5.6	7.5
childbearing pop	(5.4)	(10.1)	(4.1)	(4.1)	(3.3)	(7.9)	(10.7)	(16.2)
N deaths per 1,000 pop	10.2	14.2	14.3	9.6	7.2	11.7	13.9	14.0
	(4.3)	(11.2)	(4.1)	(3.4)	(2.5)	(4.9)	(11.0)	(12.5)
N immigrants per 1,000 pop	18.5	17.3	7.4	93.8	73.6	8.9	8.8	17.7
	(45.4)	(30.5)	(7.9)	(65.2)	(86.9)	(10.4)	(21.6)	(31.0)
Population	9665.8	1658.1	13834.0	12961.0	20284.6	5282.5	1108.3	1929.6
	(38223.8)	(22921.9)	(38739.8)	(41519.1)	(46742.7)	(19052.7)	(9439.7)	(7619.8)
N Births - N deaths	26.7	-3.9	-71.2	35.6	77.7	-8.3	-3.5	0.0
	(114.8)	(16.1)	(149.5)	(128.3)	(156.6)	(45.6)	(12.7)	(30.2)
N Perm Contracts Created per	10.4	22.0	14.8	32.4	35.9	19.0	16.9	18.6
1,000 Childbearing Pop	(12.6)	(41.5)	(13.4)	(24.0)	(35.8)	(31.6)	(104.8)	(35.0)
N Temp Contracts Created per	588.2	206.6	180.9	275.0	316.7	232.9	181.6	233.2
1,000 Childbearing Pop	(461.9)	(319.8)	(134.8)	(245.2)	(300.9)	(223.8)	(1430.6)	(530.9)
Female Labour Part Rate	38.1	37.2	33.9	48.4	44.1	37.5	36.1	34.2
	(3.7)	(4.6)	(2.3)	(3.6)	(3.2)	(3.4)	(5.4)	(5.4)
Unemp Rate	21.9	8.1	14.5	9.1	14.6	13.9	13.0	11.9
	(7.1)	(3.5)	(4.8)	(2.3)	(3.9)	(5.1)	(5.1)	(3.7)
GDP per Capita	11824.4	16551.3	13479.6	18735.8	14713.2	14956.6	14551.2	12979.2
	(2851.8)	(3182.5)	(2713.5)	(3078.5)	(2647.8)	(3278.6)	(3528.8)	(2306.9)
Growth GDP	6.6	6.1	6.3	5.4	5.7	6.9	6.1	5.4
	(2.7)	(1.9)	(2.0)	(1.9)	(1.5)	(1.4)	(2.9)	(3.4)
School Attendance Rate	4.4	19.6	4.4	11.1	3.0	8.5	11.1	3.8
Age 0-2	(2.2)	(19.6)	(2.8)	(2.0)	(2.2)	(7.8)	(7.3)	(0.5)
N	8466	8026	858	737	957	1122	24722	10088

Table A.17: Descriptive Statistics Aggregate Analysis

	Catalua	Comunidad Valenciana	Extremadura	Galicia	Madrid	Murcia	Navarra	Pais Vasco	Rioja
N births per 1,000	19.1	18.3	17.9	13.2	24.4	24.6	16.9	19.5	12.5
childbearing pop	(11.6)	(11.9)	(9.7)	(5.1)	(11.7)	(4.6)	(14.5)	(10.0)	(16.7)
N dead babies per 1,000 births	2.8	2.1	3.7	2.1	2.5	5.1	2.1	3.6	2.5
	(27.8)	(19.3)	(33.9)	(10.4)	(15.4)	(14.0)	(25.6)	(21.6)	(33.0)
N marriages per 1,000	9.4	10.3	8.1	8.7	12.5	10.9	8.3	11.0	6.5
childbearing pop	(8.6)	(8.6)	(6.0)	(3.8)	(14.3)	(2.9)	(10.2)	(7.7)	(10.6)
N deaths per 1,000 pop	10.7	11.9	12.5	13.5	8.9	8.4	11.1	10.1	12.0
	(6.4)	(7.0)	(6.0)	(4.3)	(8.0)	(2.0)	(8.2)	(5.0)	(11.7)
N immigrants per 1,000 pop	35.6	48.1	7.1	11.2	54.7	47.0	20.0	12.0	22.6
	(41.7)	(89.4)	(21.0)	(15.5)	(50.6)	(60.4)	(33.1)	(12.8)	(43.3)
Population	6791.5	7850.5	2805.7	8702.4	30116.6	26480.0	2037.2	8422.5	1584.1
	(52681.3)	(37396.7)	(9132.2)	(23849.7)	(223183.1)	(59278.4)	(11345.5)	(30331.4)	(10211.8)
N Births - N deaths	8.6	8.5	-1.5	-29.5	109.2	118.7	2.2	-3.1	-0.8
	(128.9)	(64.9)	(34.4)	(49.2)	(360.2)	(291.6)	(30.9)	(65.3)	(18.1)
N Perm Contracts Created per	44.1	38.1	10.5	14.5	96.4	35.2	31.1	34.9	25.8
1,000 Childbearing Pop	(68.3)	(70.5)	(13.1)	(16.6)	(487.4)	(30.6)	(46.6)	(48.7)	(50.2)
N Temp Contracts Created per	291.7	321.6	483.1	169.5	711.2	470.4	272.2	439.1	335.2
1,000 Childbearing Pop	(425.0)	(414.9)	(490.5)	(171.3)	(4070.4)	(381.0)	(393.3)	(964.1)	(476.7)
Female Labour Part Rate	46.9	42.9	36.0	41.1	45.7	40.4	42.7	43.6	38.1
	(4.3)	(3.4)	(2.7)	(3.0)	(4.4)	(3.1)	(3.9)	(3.5)	(4.4)
Unemp Rate	9.8	12.6	21.4	13.8	11.5	13.8	7.3	12.5	8.3
	(3.6)	(5.0)	(6.1)	(3.8)	(4.9)	(4.6)	(2.3)	(4.9)	(2.9)
GDP per Capita	19396.2	15443.9	10265.6	12243.9	20956.1	13212.2	19921.0	19625.7	17616.8
	(3605.2)	(3130.3)	(2213.9)	(2521.3)	(4020.0)	(2661.3)	(3814.2)	(4349.3)	(3054.7)
Growth GDP	5.9	6.0	6.6	6.2	6.3	6.5	6.3	6.9	5.7
	(2.1)	(2.3)	(3.4)	(2.4)	(1.5)	(1.1)	(0.9)	(1.8)	(1.3)
School Attendance Rate	49.5	12.3	2.8	12.3	29.1	17.1	8.2	64.2	4.3
Age 0-2	(2.7)	(4.2)	(0.8)	(12.4)	(10.7)	(3.6)	(16.5)	(13.9)	(1.0)
N	10398	5950	4207	3462	1969	495	2992	2750	1914

A.15 Instrument of the Aggregate Analysis

Regional governments frequently provided higher subsidies to individuals who were younger than 30, older than 45, or females. I therefore follow several steps to construct the variable $S_{m,t}$. First, I collect data on the population of the following sociodemographic groups in municipality m at year t: males younger than 30, males aged 30-45, females younger than 30 and females aged 30-45. Second, I assign to these groups the amount of the subsidy for which they were respectively eligible within region j at year t. Third, I calculate $S_{m,t}$ as a weighted average of the amounts of the subsidies that were given to these groups in region j at time t. The weights used to calculate this average are the ratios between the number of individuals in each of these groups and the total population younger than 45 in municipality m at year 1996. I use the populations in 1996 as this was the last year before the subsidies started to be implemented, and so, the distribution of the population of the municipalities in 1996 is exogenous to the policy. More formally, I calculate $S_{m,t}$ using the following equation:

$$S_{m,t} = \sum_{g=1}^{4} (Pop_{g,m,96}/TotchildbearingPop_{m,96}) * Subsidy_{g,j,t} \tag{2.8}$$

where g refers to sociodemographic group, m to municipality, j to region and t to year. Finally, I divide the amount obtained using equation (2.8) by the average labour costs per annum of region j at year t. I do so to measure variable $S_{m,t}$ in terms of the percentage of the labour cost saved during the first year of new open-ended contracts.

A.16 Using Alternative Dependent Variables

Table A.18 reports the baseline estimates of the aggregate analysis when I use as dependent variable the fertility rates two years after.

Table A.18: Estimates of the First and Second Stages of the Aggregate Analysis

	OLS	2SLS	
Depvar:	Fert $_{t+2}$	Fert $_{t+2}$	Marriage $_{t+2}$
	(1)	(2)	(3)
Perm Emp	0.0003	0.2670***	0.1635***
	(0.0027)	(0.0913)	(0.0488)
Municip FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Panel B:	Tests		
F-stat instr		13.104	13.104
Observations	57,277	57,277	57,277

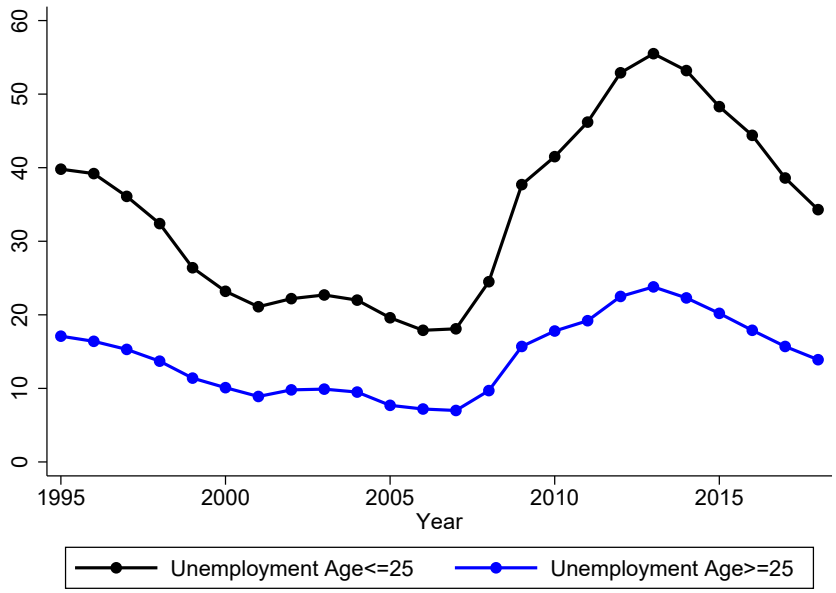
* p<0.10, ** p<0.05, *** p<0.01

Standard errors in parentheses. The sample contains yearly information on every labour contract and birth episode that occurred in Spain at the municipality level. I use the F-test of instrument to test whether my instrument is weak. I include in all specifications the following controls (not shown): the fetal death rate of municipality m at year $t+2$, the fertility grants that were given in region j at year t, the school attendance rate of province p at year t, the proportion of foreign population in municipality m at year t, and the growth of the GDP of province p at year t. I cluster standard errors at the regional level.

A.17 Unemployment Rate and External Validity

I have argued that permanent employment has an overall positive impact on fertility due to a higher job security. However, the value that individuals give to permanent employment may depend on the unemployment rate. For example, if unemployment is high, it will be harder for individuals to find a job if they get fired from their current position. Therefore, individuals might give more importance to holding a permanent contract when the level of unemployment is high, as it is harder to be dismissed. If this is the case, permanent employment may have a higher impact on fertility during recessions. Figure A.11 shows the unemployment rate and the youth unemployment rate in Spain during the period 1996-2018. As shown, the period of analysis was characterized by a low level of unemployment relative to the years after it. This suggests that the impact of permanent employment on fertility might have been even higher during more recent years.

Figure A.11: Unemployment Rate 1996-2018



The figure shows the total unemployment rate and the youth unemployment rate (for individuals younger than 25) during the period 1996-2018.

Chapter 3

Television, Time Use and Education: Evidence from a Natural Experiment

3.1 Introduction

Television viewing is one of the main daily activities for most of children. A large body of empirical research has investigated the impact of television on academic achievement. Most of studies have reported a negative effect (Beentjes and Van der Voort, 1988; Christakis et al., 2004; Hancox et al., 2005; Rice, 1983; Timmer et al., 1985; Walberg and Ling Tsai, 1985; Zimmerman and Christakis, 2005), but recent research has shown that preschool television raises adolescent test scores (Gentzkow and Shapiro, 2008). Besides the mixed evidence, little is known about the mechanism that operates behind the effect of television on education.

This paper provides two important sets of causal estimates. First, I provide causal evidence on the short-run impact of television on academic performance and educational inequality. The relevance of this set of results lies in the importance of cognitive development for future labour market performance (Angrist and Keueger, 1991). Second, I explore a mechanism behind my base-

line estimates on education: the effect of television on the use time of children. Addressing these research questions introduces identification concerns. For example, academic achievement may determine the time that children dedicate to television. Alternatively, television viewing may be correlated with unobserved characteristics, such as preferences, that also determine education and habits. To confront potential endogeneity problems, I use a natural experiment that consists in the digital television switchover that took place in UK between 2008 and 2012. During this period, UK adapted in stages all its television transmitters to stop broadcasting analogue television and strengthen the power of the digital television signal that they provided.

There are two main methodological advantages of using the digital switchover as a natural experiment. First, the decentralized location of the television transmitters induced substantial geographical variation in the timing of the switchover process. Neighbouring postcode units obtained access to digital television at different years, which allows me to exploit exogenous variation in my natural experiment at the postcode level. Second, the government delegated the digital switchover process to Ofcom, which is the official communications regulator in UK, and DigitalUK, which is an independent non-profit organization funded by the public-service broadcasters and operators. The main goal of these two institutions was to implement the switchover process on time attending to the physical characteristics of the television transmitters. These had been built in the 1960s and 1970s. Therefore, it is unlikely that the switchover is correlated with unobserved characteristics that determine education or behaviour.

The switchover process was an important revolution in the British television market. It increased the number of television channels from 5 to 40 for more than 10 million of television viewers for the first time in their lives. It also introduced the possibility of watching television in different languages. According to data from the Broadcasters Audience Research Board in UK, these changes increased the average television viewing time by 34 minutes per day. As the daily average television viewing time in 2008 was 224.7 minutes, the digital switchover

raised this number by 15.1%.[1] The digital switchover also decreased the audience share of the five traditional channels by 12%.[2] However, the television contents that people watch remained unchanged during the period of analysis.

Using web scraping, I extract information on the date when the digital switchover takes place in every postcode unit in UK. Given that there are about 1.7 million of postcode units in UK, I exploit exogenous variation in my natural experiment at a very precise geographical level. I use two additional datasets to conduct the analysis. The first is a panel administrative dataset that contains yearly information on the Key Stage 2 (KS2) test results of each of the 16,000 schools in England during the period 1991-2016. This dataset also contains annual sociodemographic information on schools. I use KS2 exam data as this test is taken when students are pre-adolescents, which is when they start spending time with friends and may begin to get involved in detrimental activities (Duncan et al., 2006; Milton et al., 2007). The dataset comes from the UK Department for Education. The use of external assessed exam information avoids sample selection and self-reported biases. My second dataset comes from the youth questionnaire of the Understanding Society Survey in UK. This is a panel survey dataset at the individual level that contains yearly information on the activities at which children get involved.

Regarding my empirical strategy, I use a difference-in-difference approach. This not only compares the academic performance of schools that are affected by the digital switchover at different years, but also the test scores within schools before and after the switchover takes place. I find that the digital switchover increases KS2 test scores by 0.028 standard deviations. This is not a small impact. The digital switchover increases television viewing time by 34 minutes. However, an average individual watches television for 224.7 minutes per day. Subsequently, the average television viewing time raises academic performance by 0.185 standard deviations. This is in line with the estimates provided in recent

[1] See https://www.barb.co.uk/trendspotting/data/average-weekly-viewing/ I calculate this number by subtracting the average television viewing time in the last week of 2008, which is right before the beginning of the switchover, to the average TV viewing time in the final week of 2012, which is just after the end of the switchover.

[2] See https://www.barb.co.uk/trendspotting/data/channel-viewing-share/.

literature (Gentzkow and Shapiro, 2008), but higher in magnitude. The effect lasts for three years, which substantially contributes to human capital formation. Importantly, the magnitude of the impact depends on the academic performance of schools previous to the introduction of the digital switchover. The effect is positive for schools below the median of the score distribution previous to the switchover process, and negligible for schools above it. This reduces educational inequality. The baseline estimates on education are robust to: (i) checking for pre-trends in grades, (ii) controlling for school individual trends, (iii) performing placebo tests, (iv) using alternative dependent variables, and (v) using the number of students in schools as weights.

My second set of estimates explores a plausible mechanism behind my results on education. I examine whether obtaining access to digital television signal changes the time use of children. I show that the digital switchover reduces the probability and the frequency with which children get involved in detrimental activities, such as alcohol drinking. Importantly, the impact is driven by economically disadvantaged, male and non-white British children. I also study heterogeneity in age, as the KS2 exam is taken at the age 10-11. My findings show that children aged 10-11 get involved in detrimental activities, and that the impact of television on time use is driven by them. I also check for alternative mechanisms, such as whether the digital switchover changes behaviour or the amount of time that children dedicate to homework. My estimates show that this is not the case. Taken together, the results suggest that the effect of out-of-school activities on academic performance does not depend on their absolute educational value, but on their relative one.

This paper primarily contributes to three literatures. First, my paper belongs to the literature that studies the impact of television on cognitive development. A large number of studies have reported a negative effect (Beentjes and Van der Voort, 1988; Christakis et al., 2004; Hancox et al., 2005; Rice, 1983; Timmer et al., 1985; Walberg and Ling Tsai, 1985), but recent research has shown that preschool television raises adolescent test scores (Gentzkow and Shapiro, 2008). My paper contributes to this literature by shedding light on: (i)

the mechanism that operates behind the effect of television on academic achievement, (ii) the causal impact of television on education in the short-run, (iii) the effect of television on educational inequality, and (iv) the dynamics of the previous effects. I make these contributions using the staggered introduction of the digital television signal in the British television market as a natural experiment. As the family background is an important determinant of academic achievement (Blanden and Gregg, 2004), I also explore heterogeneity of the previous effects in the proportion of disadvantaged, non-native, and Special Educational Needs students. Second, this paper adds to previous literature on the importance of television on behaviour and habits formation (Wright et al., 2001). Previous evidence has reported that exposure to entertainment and violent content lowers academic achievement (Kirkorian et al., 2008), increases aggressiveness at an adult age (Huesmann et al., 2003) and raises the frequency of harmful habits (Robinson et al., 1998).[3] Yet, the previous effects may be due to the type of TV programs that children watch (Huston et al., 1999), and so, alternative television contents may contribute towards the development of children. For example, prior evidence has shown that age appropriate educational programs have a positive impact on cognitive development (Fisch, 2014; Calvert and Kotler, 2003). I contribute to this literature by providing causal evidence on the impact of television on time use and behaviour. Furthermore, I examine heterogeneity of the previous effects in age, gender and ethnicity. As children from disadvantaged households spend more time in unstructured activities (Rokicki and McGovern, 2017), I also study heterogeneity in income. Finally, my paper belongs to the literature on the importance of television on social attitudes, perceptions and behaviour (Jensen and Oster, 2009; La Ferrara et al., 2012; Mastrorocco and Minale, 2018; Gentzkow, 2006; DellaVigna and Kaplan, 2007). This paper contributes to this literature from a different, but relevant, policy point of view. I show that the true determinant of academic achievement is the relative educational value of out-of-school activities, rather than their absolute one.

[3]Watching television has been also associated with causing sleep patterns among children (Johnson et al., 2004)

The remainder of the paper is organized as follows. Section 3.2 describes the digital television switchover and its introduction in the British television market. Section 3.3 presents the data. Section 3.4 contains the empirical strategy and section 3.5 presents the results on academic performance. Section 3.6 explores a mechanism behind the baseline results and presents the estimates on time use. Section 3.7 concludes.

3.2 Digital Switchover Process

Digital television has been the first revolution in the television industry since colour television was introduced in the 1950s. Digital television offers several important advantages relative to its analogue predecessor, such as the possibility of watching television in different languages, additional services related to multimedia, a higher definition, and multiplexing, which is the ability of having more than one program on the same channel. However, the crucial feature of digital television is that it increased the number of television channels that could be watched from 5 to 40.

The television signal in UK is provided by 1,235 television transmitters that can be classified in two different groups: 81 principal transmitters and 1,154 relay transmitters which receive television signal from the former and repeat it to the households that are unable to receive television signal from the principal ones. In 2004, the British Government appointed Ofcom and DigitalUK and delegated to them the task of adapting all the television transmitters in UK so that they stopped transmitting analogue television signal and started providing high-power digital signal. Ofcom is the communications regulator in UK, and DigitalUK is an independent non-profit organisation funded by the public-service broadcasters and operators. Ofcom amended the broadcast licences required for the delivery of the analogue switch-off to the different television transmitters according to a specific timetable. DigitalUK organised the timetable of the digital switchover, managed the upgrade of the television transmitters, and ensured that individuals were supported and understood the process.

The digital switchover started at the end of 2008 and ended in 2012. During this process, every television transmitter in UK was adapted to cease broadcasting analogue television signal and to strengthen the power of their digital signal. The digital switchover was done in stages and in a transmitter group basis.[4] Figure 3.1 presents a mapping of the digital switchover process, and shows that there was a strong geographical variation in its timing.

Figure 3.2 explains in detail the switchover definition that I use in the analysis. In England, every postcode unit receives television signal from different television transmitters. To see this, I set as example a hypothetical postcode unit that receives television signal from transmitters located in different regions. There are two types of transmitters: the principal transmitters (A, B, C and E), which broadcast television signal, and the relay transmitters (D), which receive television signal from the principal ones and repeat it to other geographical areas. Given that television transmitters have different switchover dates, the postcode unit is affected by the analogue switch-off at the earliest of the switchover dates of its television transmitters.

In summary, I define the digital switchover date of a particular postcode unit as the earliest of the switchover dates of the television transmitters that provide television signal to that postcode. The decentralized location of television transmitters lead to a strong geographical variation in the timing of the digital switchover. Neighbouring postcode units got access to digital television signal at different years. I exploit exogenous variation in my natural experiment at the postcode unit level.

3.2.1 Importance of the Digital Switchover

The digital switchover process made the digital television signal available to more than 10 million of individuals for the first time in their lives. Additionally, there were millions of households who had access to digital signal before the

[4] A transmitter group is a bunch of transmitters, one principal transmitter and several relay transmitters, each of which provides television signal. The former broadcasts television signal to individuals and to a number of relay transmitters, which repeat the television signal to many other individuals who cannot receive television signal from the principal transmitters.

analogue switch-off, but that waited to adapt their television device to be able to watch digital television until they could no longer watch analogue television (at the time when the switchover occurred). Overall, and according to a DigitalUK report, more than 75% of households were only able to watch analogue television signal on at least one television three years before the switchover process start.[5]

Figure 3.3 presents the weekly average television viewing time in England during the period of analysis. The two red vertical lines respectively represent the start and completion date of the switchover process. As shown, the digital switchover significantly increased the average time that individuals dedicate to watch television. After 2012, the year when the switchover process finished, there was a continuous decrease in the traditional television-set viewing time. Two possible explanations are the growing popularity of non-broadcast services such as Netflix and Amazon Prime Instant Video, and the growth in the adoption of smartphones and tablets.

As shown in Figure 3.4, the digital switchover also decreased the total audience share of the five traditional channels in UK from 63.5% in 2007 to 51% in 2013.[6] This reduction was compensated by an increase in the audience share of the newly available digital channels. However, the fall in the relative importance of the main traditional channels did not translate into a change in the television contents that people watch. Figure 3.5 presents the proportion of the total television viewing time by genre during the period of analysis. I classify television contents in eight different groups: entertainment, soap operas, documentaries, current affairs, news, educational television programmes, children contents, and other. The latter category includes television channels such as music or movie channels. As shown in Figure 3.5, the digital switchover does not change the television contents that people watch. Appendix B.2 shows a Figure similar to 3.5, but that uses a more disaggregated classification of television contents.

[5]See "Digital TV switchover 2008-2012 Final Report".

[6]The five traditional channels that could be watch through analogue television signal were BBC One, BBC Two, ITV, Channel 4 and Channel 5.

3.3 Data

3.3.1 Digital Television Data

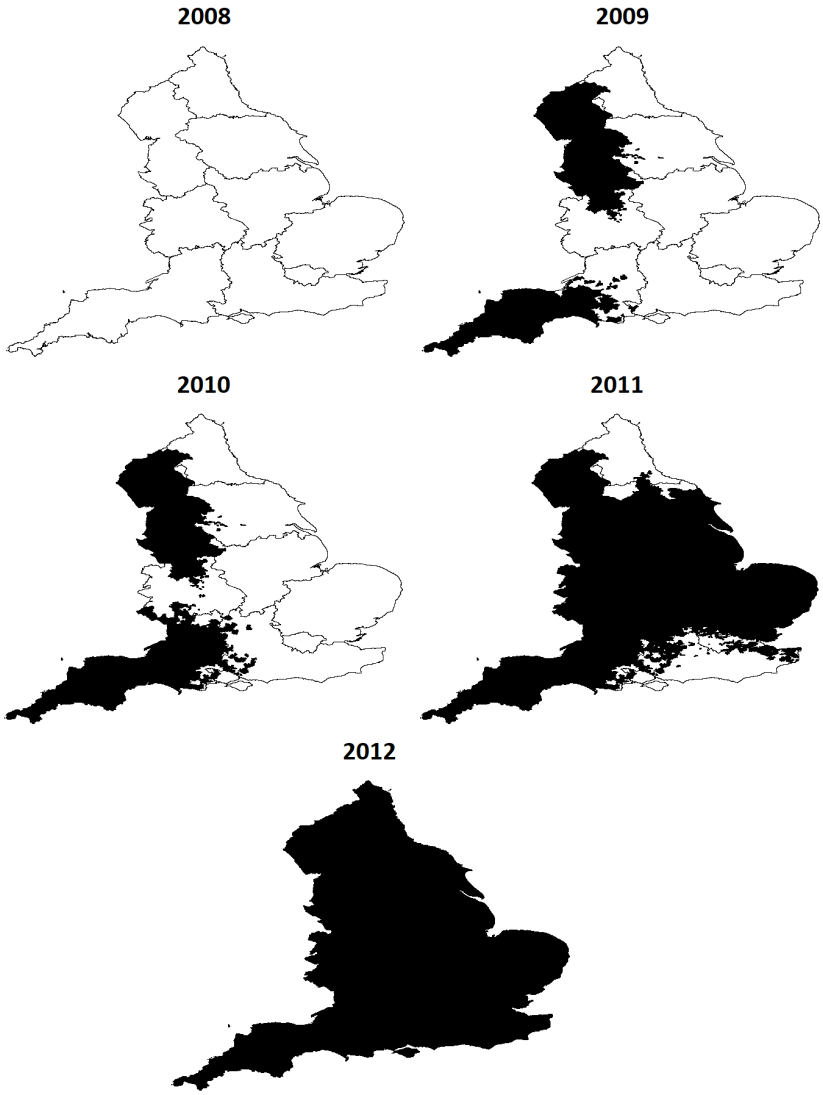
Using web-scraping on the DigitalUK organisation's webpage, I collect information on the dates when the digital switchover took place at every postcode unit in England. As there are 1.7 million of postcode units in England, this allows me to exploit exogenous variation in my natural experiment at a very precise geographical level. The dataset also contains information on the name of the transmitter that first provided digital television signal to each postcode, and on their type (whether they are principal or relay transmitters). Finally, this dataset contains information on the quality of the television signal that each postcode unit receives.

3.3.2 Schooling Data

I use a panel administrative dataset that contains yearly information on the KS2 test score of each of the 16,000 schools in England during the period 1991-2016. I obtain this data from the UK Department for Education. The KS2 test is an annual external assessed exam taken by students at the later stage of primary education. This is an important stage of life as it coincides with pre-adolescence, when students start to spend time with friends and may begin to get involved in detrimental activities (Duncan et al., 2006; Milton et al., 2007). The KS2 exam consists of several sections respectively on mathematics, English grammar, writing, punctuation and spelling. The KS2 test score ranges from 0 to 33 points, albeit the average grade of most of schools is close to the national average score, which is 27.[7] Figure 3.6 shows the score distribution of schools for each year during the period of analysis. As shown, KS2 scores have increased over time and converged to the mean of the distribution.

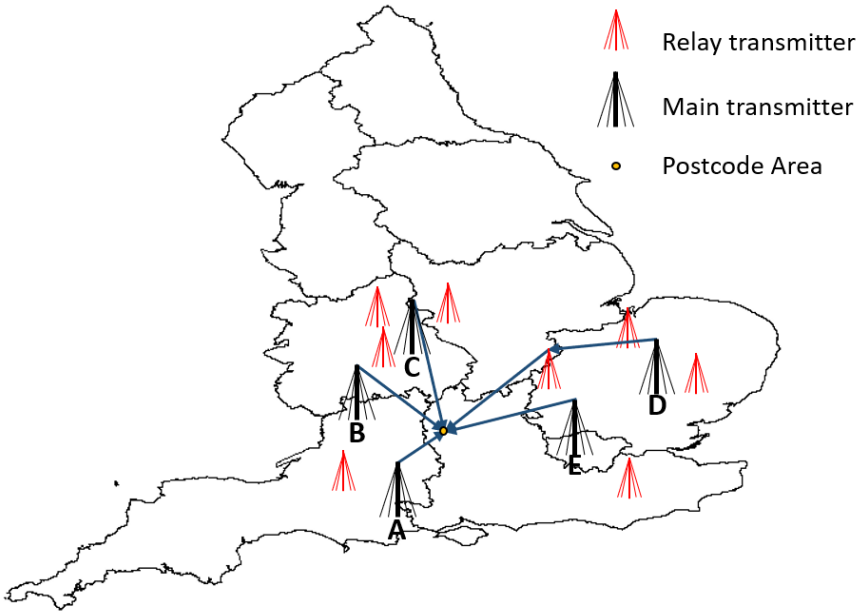
[7] A mapping of the different test levels and their respective point scores is presented in Appendix B.1.

Figure 3.1: Stages of the Digital Switchover in England



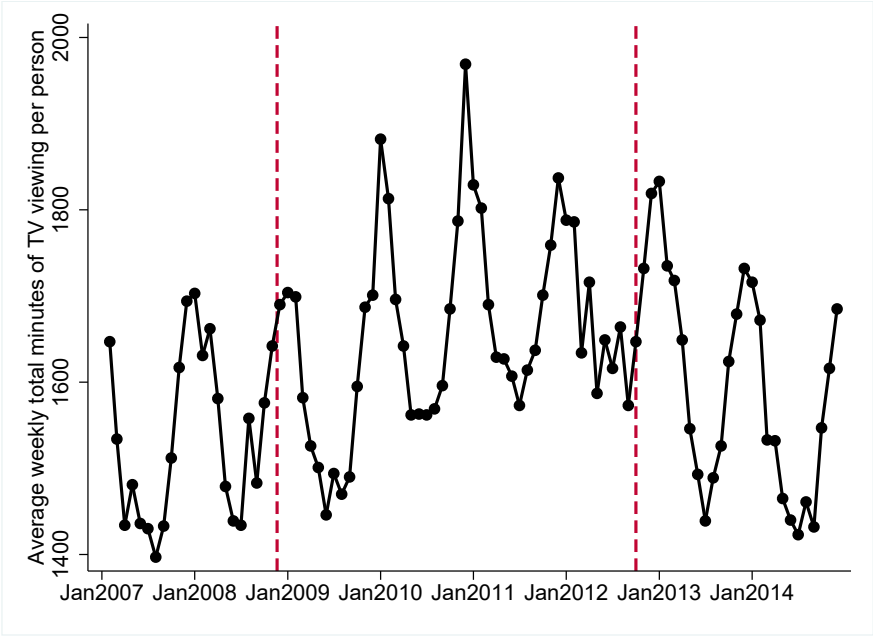
The figure shows a mapping of the digital switchover process in England. Every postcode unit receives television signal from different television transmitters, which have different switchover dates. Therefore, neighbouring postcode units got access to digital signal at different years.

Figure 3.2: Explanation of Digital Switchover



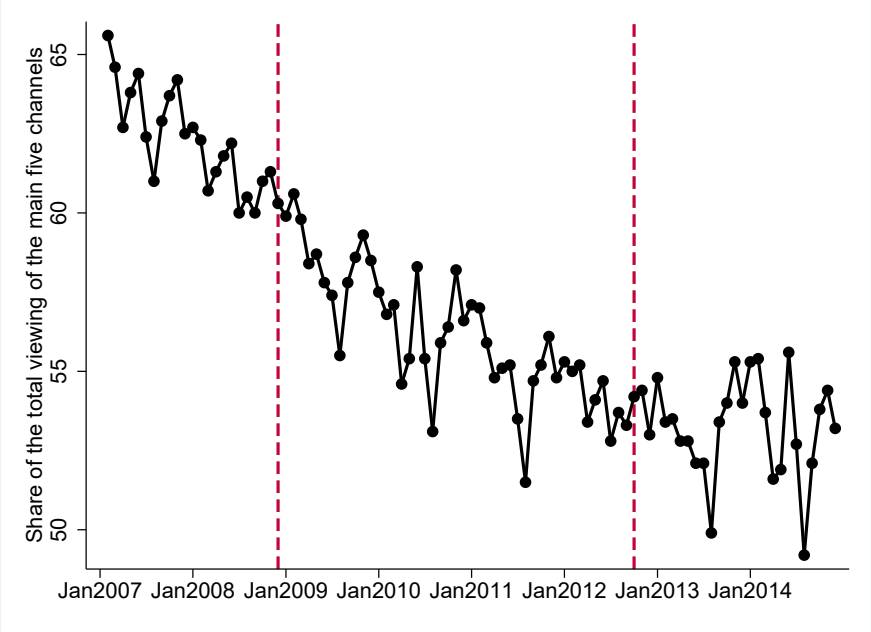
The figure explains the switchover date definition that I use in the analysis. As shown, every postcode unit receives television signal from transmitters located in different regions in UK. As television transmitters had different switchover dates, I define the switchover date for each postcode unit as the earliest of the switchover dates of its television transmitters.

Figure 3.3: Weekly Average Television Viewing Time



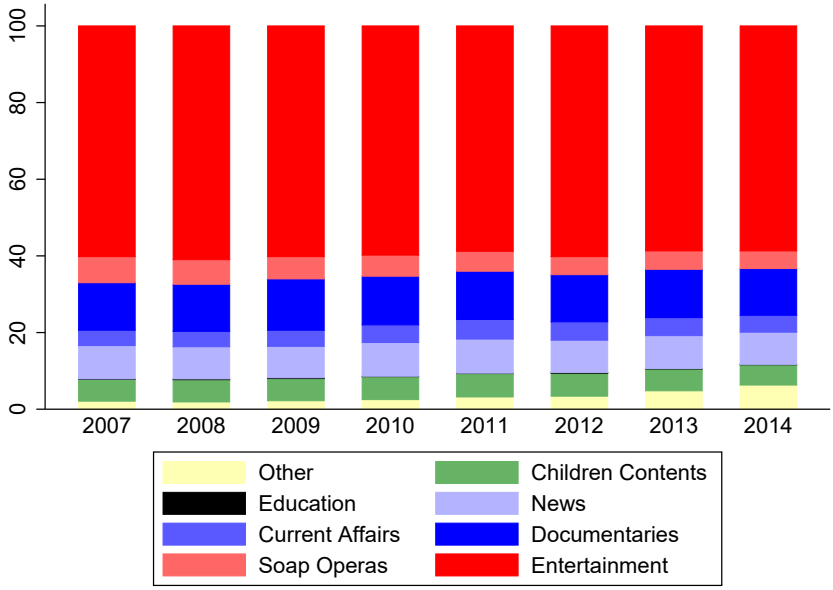
The figure presents the weekly average television viewing time during the period of analysis in UK. The two red vertical lines represent the start date and the completion date of the switchover process.

Figure 3.4: Total Audience Share of Five Traditional Channels



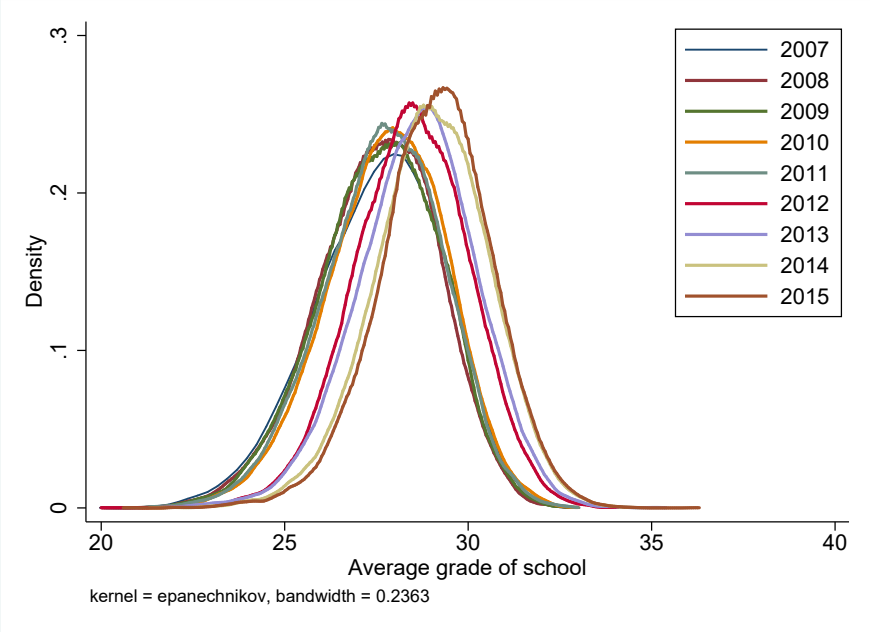
The figure presents the total audience share of the five traditional channels in UK during the period of analysis. The two red vertical lines represent the start date and the completion date of the switchover process.

Figure 3.5: Proportion of Television Viewing Time by Genre



The figure presents the proportion of television viewing time by genre. I classify television contents in entertainment, soap operas, documentaries, current affairs, news, educative television programmes, children contents, and other.

Figure 3.6: Yearly Average Grade Distribution



The figure presents the yearly average score distribution of schools during the period 2007 - 2015. As shown, the mean of the score distribution increased during the period of analysis.

This dataset includes information on several academic performance indicators such as the average test score of schools or the percentage of students with a score of 29 or higher. The data also contains annual sociodemographic information at the school level, such as the total number of students, the percentage of pupils whose mother language is not English, and the percentage of economically disadvantaged students.

All the schools included in the dataset are state-funded schools. The reason is that independent schools are not required to ask their pupils to take part in the KS2 test, and so, the UK Department for Education does not publish their KS2 results. Moreover, the UK Department for Education does not publish data on schools where the number of students that participate in the KS2 exam is below 6. Finally, I exclude from my sample Community Special Schools and Foundation Special Schools, as these have only operated since 2010.

Within the sample of schools that I use in the analysis, every student in the final year of primary education is eligible to take the KS2 test. The UK Department for Education includes the score of every pupil eligible for the KS2 test

in its calculations of the schools' performance. Students can only be excluded from the schools' performance indicators when they have a limited English and have arrived from a non-English speaking country within the last two years.

I match my datasets on education and television using the postcodes of schools. Students attend a school near where they live due to two main reasons. First, most of schools in UK receive more applications than vacancies available, and one of the main criteria used to select students is the distance from the child's home address to the school. Second, it is unlikely that children attend schools far away from their home due to the high travelling costs they would incur. Subsequently, the digital switchover takes place in the postcode unit where students live at the same time than it occurs at the postcode unit of the school they attend. After merging my television and education datasets, I am left with a panel of 160,776 observations.

It is important to note that there have been two important reforms in the KS2 exam during the last decade. First, the test was initially based on an English, Maths and Science part. However, the Science section ended in 2009 and only the average scores from 2007 until 2009 were recalculated by the UK Department for Education based on the English and Maths sections of the test. I therefore do not include the years before 2007 in the analysis. Second, the highest grade that could be obtained in the KS2 exam increased from 35 to 39 in 2012, and the writing section started to be based on teacher assessment rather than being externally evaluated in the same year. The inclusion of year fixed effects and school trends as controls in the analysis accounts for these changes.

It is also possible to use to use the National Pupil Dataset to perform the analysis. This individual-level data provides information on the academic performance and sociodemographic characteristics of all the students in England. However, the KS2 exam is only taken by students once in life and my natural experiment varies at the postcode level. Therefore, individual data only allows me to compare the average scores of postcodes that were affected by the switchover at different years, and the average scores within postcodes before and after the switchover takes place. To simplify the differences-in-differences approach, and

to have a panel-setting dataset, I use the UK Department for Education dataset on the yearly academic performance of every school in England. As this is a large panel dataset that contains yearly academic and sociodemographic information of every school in England, I obtain very precise estimates in the analysis.

3.3.3 Understanding Society Survey Data

The third dataset I use in the analysis contains yearly information on the time use of children, and it is based on the youth questionnaire of the Understanding Society Survey (UKHLS, 2017). The Understanding Society Survey is a longitudinal survey that has followed the members of 40,000 households in the United Kingdom every year since 2009. Among these individuals, there are approximately 3,000 children aged 10-15 that annually respond to the youth questionnaire of the Understanding Society Survey. This section contains self-completion questions about their lives, experiences, habits, and the social activities in which they get involved. For example, the youth questionnaire contains queries on how happy children are at school, the frequency with which they do homework, sport, use computer, attend extracurricular activities, play video games, go out at night, smoke, or drink alcohol, among others. The youth questionnaire also provides sociodemographic information on children such as their age, gender and family income. It also allows me to identify children over the years. Using this information, I construct an unbalanced panel of 15,069 observations. On average, children are present in 2.92 years during the period of analysis. Importantly, the Understanding Society Survey provides information on the Lower Layer Super Output Area (LSOA) where individuals live. These are very precise geographical units designed with a minimum population of 1,000 and a mean population of 1,500. The total number of LSOAs in England is 32,844, and each of them consists of a grouping of Output Areas, typically five. The Office for National Statistics in UK provides data on the postcode units that pertain to each of the LSOAs in England. Using this information, I match the Understanding Society Survey dataset with the data on when did the digital switchover occur at each of the postcode units in England.

3.4 Empirical Framework

3.4.1 Summary Statistics

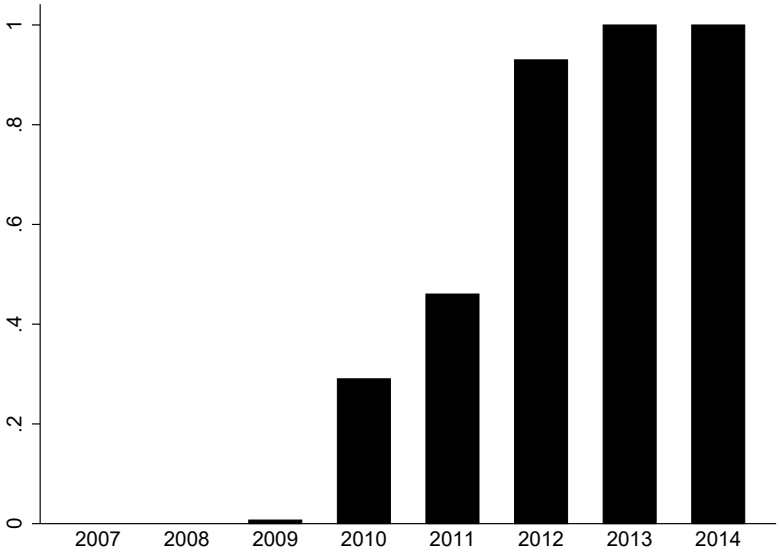
As the digital switchover was implemented in stages in England, it took place at different dates for the different postcode units of schools. Figure 3.7 displays the percentage of schools that had already received access to digital television in the different years during the period of analysis. As shown, the percentage of schools with access to digital television gradually increased from 2009 to 2013, and the conversion rate was the highest in 2012.

I next present some descriptive statistics of my sample, and study whether schools that are affected by the digital switchover at different years are different in observed characteristics that may explain discrepancies in their academic performance. Column 1 of Table 3.1 presents unweighted summary statistics of my sample. As shown, the average KS2 grade previous to the start of the switchover process is 27.66 points. This score remains almost unchanged in 2009, and increases after it. Columns 2-5 respectively assign schools into four different groups attending to the year when they are affected by the digital switchover. For example, column 2 presents the summary statistics of the schools that receive access to digital signal in 2009. As shown, schools with late access to digital television have more students, and these are slightly more likely to be socioeconomically disadvantaged or to have special needs. Importantly, schools with different switchover dates have similar average grades in the years before the start of the switchover process, and their average scores evolve in a parallel manner during the period of analysis.

3.4.2 Empirical Strategy

This paper identifies the causal impact of television on education by exploiting exogenous variation in the timing of the digital switchover in England at the postcode level. This allows me to implement a difference-in-difference approach that not only compares the average scores of schools that receive access to digital television at different years, but also the average scores within schools before and

Figure 3.7: Percentage of the Territory Affected by the Digital Switchover



The figure presents the percentage of schools that had already received access to digital television in the different years of the period of analysis. No school had received access to digital television by 2008, and all of them had done so by 2013.

after the switchover process takes place. More formally, I estimate the following specification:

$$y_{s,t} = \alpha + \beta DigitalSwitch_{s,t} + \phi DigitalTenure_{s,t} + \gamma X_{s,t} + \delta_s + \lambda_t + \delta_s t + \varepsilon_{s,t} \quad (3.1)$$

where $y_{s,t}$ is the average KS2 test score of school s at year t. $DigitalSwitch_{s,t}$ is a dummy variable that takes value 1 if the postcode unit of school s has already been affected by the digital switchover by year t, and 0 otherwise. Using information on my natural experiment at the postcode unit allows me to exploit strong exogenous variation at a very precise geographical level.

The impact of the digital switchover on education may be temporary or persist in time. I therefore control for $DigitalTenure_{s,t}$, which is the number of years that have passed since digital television was introduced in the postcode unit of school s at year t. More precisely, $DigitalTenure_{s,t}$ takes value 0 if the switchover process has not yet occurred in the postcode unit of school s by year t, 1 if it took place within the last 12 months, 2 if it occurred between 12 and 24

Table 3.1: Descriptive Statistics

	Total	2009	2010	2011	2012
	(1)	(2)	(3)	(4)	(5)
N of students	269.99	237.66	231.10	263.43	315.70
	(147.56)	(121.45)	(132.70)	(144.40)	(163.51)
N of students with SEN	3.55	3.03	2.71	3.48	4.28
	(3.37)	(2.91)	(2.94)	(3.33)	(3.72)
% of students free meals	0.27	0.27	0.20	0.25	0.30
	(0.21)	(0.21)	(0.18)	(0.20)	(0.22)
Average grade in 2007	27.66	27.69	27.77	27.62	27.67
	(1.75)	(1.72)	(1.72)	(1.74)	(1.80)
Average grade in 2008	27.62	27.68	27.70	27.58	27.62
	(1.66)	(1.61)	(1.72)	(1.67)	(1.65)
Average grade in 2009	27.65	27.70	27.67	27.58	27.71
	(1.68)	(1.63)	(1.63)	(1.70)	(1.70)
Average grade in 2010	27.78	27.88	27.84	27.66	27.89
	(1.62)	(1.57)	(1.62)	(1.66)	(1.59)
Average grade in 2011	27.82	27.89	27.90	27.71	27.93
	(1.61)	(1.59)	(1.63)	(1.62)	(1.59)
Average grade in 2012	28.56	28.57	28.56	28.46	28.70
	(1.56)	(1.52)	(1.56)	(1.59)	(1.54)
Average grade in 2013	28.69	28.70	28.71	28.57	28.88
	(1.61)	(1.55)	(1.69)	(1.65)	(1.57)
N	118,729	27,617	6,120	52,681	32,311

The table presents the means of a number of observable variables, and their standard deviations in parenthesis.

months earlier, and so on. δ_s are school fixed effects that capture school time-invariant characteristics that might be correlated with the timing of the digital switchover and the academic performance of students. λ_t are year fixed effects that allow for flexible trends in the average KS2 test score of schools over time. $\delta_s t$ are a set of variables that control for linear trends in the scores of the different schools over time. $X_{s,t}$ is a vector of time-varying covariates at the school level. This includes the total number of students and the number of students with special needs. $\varepsilon_{s,t}$ is a school and time specific level error term. Throughout the empirical analysis, I cluster standard errors at the regional level, which allows for an arbitrary correlation of residuals within postcodes. My identifying assumption is that the timing of the switchover process is not correlated with unobserved determinants of education, after having controlled for time-varying observable covariates, school and year fixed effects, and school individual trends.

3.5 Empirical Results

Column 1 of table 3.2 presents the estimates of specification (3.1) without controlling for time-varying covariates, $DigitalTenure_{s,t}$ and school trends. The estimates show that the digital switchover increases KS2 test scores by 0.036 standard deviations, and so, that an increase in television viewing time has a positive impact on educational performance. It is important to comment on the magnitude of the estimate. The digital switchover raises television viewing time by 15.1%. Therefore, the estimate points that total television viewing time increases academic performance by 0.238 standard deviations. This finding is in line with the estimates provided by recent research (Gentzkow and Shapiro, 2008), but higher in size. The estimate is statistically significant at the 1% confidence level. Column 2 includes time-varying controls at the school level and column 3 adds a set of linear school trends as additional covariates. As shown, their inclusion in the empirical specification lowers the magnitude of the estimate of the effect of television on education, but does not change its sign neither significance. Column 4 controls for the number of years that have passed since the introduction of the digital switchover in a particular postcode unit. I find that the estimate of the digital switchover is still positive and significant at the 5% confidence level, and that the impact of an additional year with access to digital television signal is negligible. This suggests that the positive impact of television on education persists in time. It is also important to comment on the estimates of the rest of the covariates. An increase in the number of students of a school or in the number of pupils with special needs have a negative impact on test scores.

Previous literature has pointed that television may be most beneficial for children who are relatively disadvantaged (Gentzkow and Shapiro, 2008). I next explore whether the impact of television on education depends on the academic background of students. To do so, columns 5-8 split my sample in quartiles attending to the position of schools in the score distribution of 2008, which is the year before the switchover process started. This allows me to study whether the

digital television transition has an impact on educational inequality. I find that the digital switchover has a positive effect on the average educational outcomes of schools at the bottom of the score distribution, but no effect on schools at the top of it. This reduces educational inequality. A possible explanation is that television changes the time use of children differently depending on their socioeconomic background. If television viewing time displaces activities with relatively lower educational value for bad students, an increase in television exposure may improve their academic performance. I test for this hypothesis later in the paper. The estimates in columns 5-8 also show that the positive impact of television on education is persistent in time for schools at the bottom of the score distribution in 2008, but temporary for schools at the second quartile of the distribution. I next study in more detail the timing of the impact of the digital switchover on education.

3.5.1 Timing of the Effect of Television on Education

The previous section shows that an increase in television viewing time has a positive impact on academic achievement and educational inequality. I next study the dynamics of the previous effect and test whether the digital switchover is correlated with previous trends in grades. To do so, I estimate the following event approach specification:

$$y_{s,t} = \alpha + \beta \sum_{r=-3}^{3} YearsinceSwitch_{s,t+r} + \gamma X_{s,t} + \delta_s + \lambda_t + \delta_s t + \varepsilon_{s,t} \qquad (3.2)$$

where the second term on the right-hand side is a set of dummies that indicate the years that are left or have passed at year t relative to the introduction of the digital switchover in the postcode unit of school s. For instance, the dummy with $r = 0$ takes value 1 the year when the postcode unit of school s is affected by the digital switchover, and 0 otherwise. Albeit I include year fixed effects in the analysis, I still have enough variation in the previous set of dummies as different postcode units obtained access to digital television at different years. The event time r ranges from -3 to 3. Including event dummies

Table 3.2: Effect of Digital Switchover on KS2 Test Scores

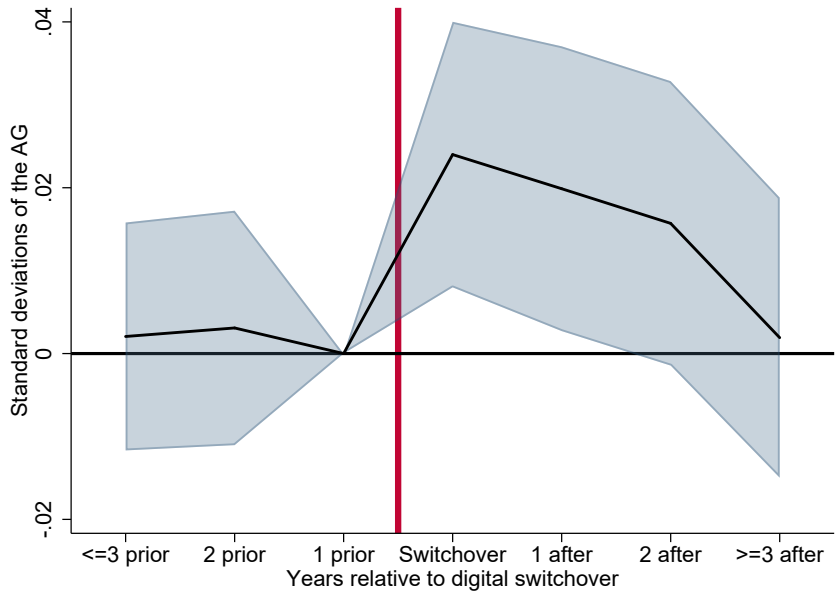
					Quartiles			
					Q1	Q2	Q3	Q4
Depvar: KS2 Standardized average score	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DigitalSwitch	0.0363***	0.0284***	0.0260**	0.0283**	0.0662**	0.0857***	-0.0120	-0.0270
	(0.0082)	(0.0080)	(0.0085)	(0.0102)	(0.0231)	(0.0204)	(0.0199)	(0.0188)
DigitalTenure				-0.0041	-0.0139	-0.0495**	0.0307	0.0204
				(0.0108)	(0.0244)	(0.0223)	(0.0216)	(0.0199)
% students SEN		-0.0245***	-0.0243***	-0.0243***	-0.0212***	-0.0234***	-0.0272***	-0.0271***
		(0.0004)	(0.0004)	(0.0004)	(0.0007)	(0.0008)	(0.0009)	(0.0008)
log N students		0.1366***	-0.6232***	-0.6233***	-0.5879***	-0.3983***	-0.3853***	-0.3874***
		(0.0287)	(0.0451)	(0.0450)	(0.0761)	(0.0913)	(0.0945)	(0.1052)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	118,729	117,899	117,899	117,899	27,287	27,703	26,419	27,354

* p<0.10, ** p<0.05, *** p<0.01

standard errors in parenthesis. I allow for an arbitrary correlation of standard errors at the regional level. The dependent variable is standardized to have a mean of zero and a standard deviation of unity.

with $r = -1$, $r = -2$ and $r = -3$ allows me to study whether the digital switchover was correlated with previous trends in grades. Were this the case, my empirical strategy would be flawed. As in my baseline specification, I control for school time-varying characteristics, year fixed effects, school fixed effects, and school trends. The results are displayed in Figure 3.8. As shown, the estimates of the effect of the years previous to the digital switchover on test scores are negligible and not statistically significant. However, the estimates become positive and significant for the years after the introduction of the digital television signal. The impact of television on education lasts for three years and then vanishes. A possible explanation is that children may get used to the availability of a higher number of channels over time. As the positive effect of television on education persists during three years, the introduction of the digital television signal notably increases human capital formation. Appendix B.3 shows the dynamics of the effect of television on education for each of the subsamples that I constructed attending to the position of schools in the score distribution of 2008.

Figure 3.8: Dynamic Impact of Television on Education



The figure estimates the event approach specification (2.3), and shows the dynamics of the effect that television has on education. More specifically, the figure presents the estimates of the set of dummies $\sum_{r=-3}^{3} YearsinceSwitch_{s,t+r}$.

3.5.2 Robustness Checks

This section implements a number of robustness tests that support the validity of my identification strategy and explore whether the baseline estimates are robust to the use of different specifications. Columns 1 and 2 of Table 3.3 estimate my benchmark specification but respectively using as dependent variable the percentage of students that obtain a score of 25 or higher in the Maths and English section of the KS2 test. This robustness test is important because of two reasons. First, it allows me to study whether the positive effect of television on academic performance is due to an enhancement in the verbal or mathematical skills of students. Second, it serves to examine whether the baseline estimates are robust to the use of alternative dependent variables. As shown, the estimates of the digital switchover are positive and significant in both sections of the test, albeit higher in magnitude for Maths. The estimates of an additional year with access to digital television are negative, but only significant in the English section of the test. As previously explained, a possible explanation is that the digital switchover allowed for the possibility of watching television in languages other than English. Non-native students may opt for watching television in their own language rather than English, and this may have a negative impact on their English language skills. Column 3 estimates the benchmark specification using the contextual value added (CVA) score as dependent variable. The CVA score is a statistical measure of the students and schools academic progress. It is calculated by the UK Department for Education using a statistical model that calculates the prediction of the national average score for each category of students, and then compares the score of each student against that prediction. As shown, the estimate of the impact of an increase in television viewing time on education is positive and significant at the 1% significance level.

Column 4 uses the number of students in each school as weights in my regression. The estimate of the digital switchover is positive, significant at the 1% confidence level, and similar in magnitude than my baseline estimate. Finally, columns 5-6 adopt a placebo test to evaluate whether television has an impact on

education when I use an incorrect timing of the digital switchover as a natural experiment. More specifically, columns 5 and 6 respectively estimate whether obtaining access to digital television signal at year $t+1$ and $t+2$ has an impact on academic performance at year t. As shown, the estimates do not support the presence of an effect of television on educational outcomes when I use an incorrect timing of the digital switchover. Overall, the estimates are robust across the different specifications.

3.5.3 Heterogeneity

This section explores whether the effect of television on education varies in the sociodemographic characteristics of schools. Figure 3.9 estimates a specification similar to the baseline model but includes an interaction between the digital switchover variable and a set of dummies indicating the quartile of school s in the distribution of the proportion of students eligible for free school meals. As shown, the estimate of the impact of the digital switchover on test scores is driven by schools at the top and bottom of the distribution. This points that students who benefited from television are not only economically disadvantaged students. Figure 3.10 is similar to Figure 3.9, but instead studies heterogeneity in the proportion of students who are not English native speakers. As shown, the impact of the digital switchover is positive and statistically significant when schools have a low and a high proportion of non-native students. The impact is higher in magnitude when the proportion of non-native students is low. A possible explanation is that the digital switchover allowed for the possibility of watching television in languages other than English. Non-native students may opt for watching television in their own language rather than English, and this may have a negative impact on their English language skills.

Figure 3.11 tests whether the effect of television on education depends on the proportion of students with Special Educational Needs in schools. Students are defined to have Special Educational Needs when they have a low educational attainment or any kind of behavioural or physical problem. As shown, the estimates of the digital switchover become higher as the number of students

Table 3.3: Robustness Checks

	Different Dependent Variables			Weights	Placebo Tests	
	Maths	English	CVA		Switch_{t+1}	Switch_{t+2}
	(1)	(2)	(3)	(4)	(5)	(6)
DigitalSwitch	0.0032**	0.0027*	0.0709***	0.0352***		
	(0.0016)	(0.0015)	(0.0147)	(0.0100)		
DigitalTenure	-0.0021	-0.0038**	-0.0377**	-0.0105		
	(0.0016)	(0.0015)	(0.0158)	(0.0106)		
$\text{DigitalSwitch}_{t+1}$					0.0031	
					(0.0087)	
$\text{DigitalSwitch}_{t+2}$						-0.0025
						(0.0090)
Time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School Trends	Yes	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	Yes	No	No
Observations	118,079	118,022	117,460	117,899	103,824	89,444

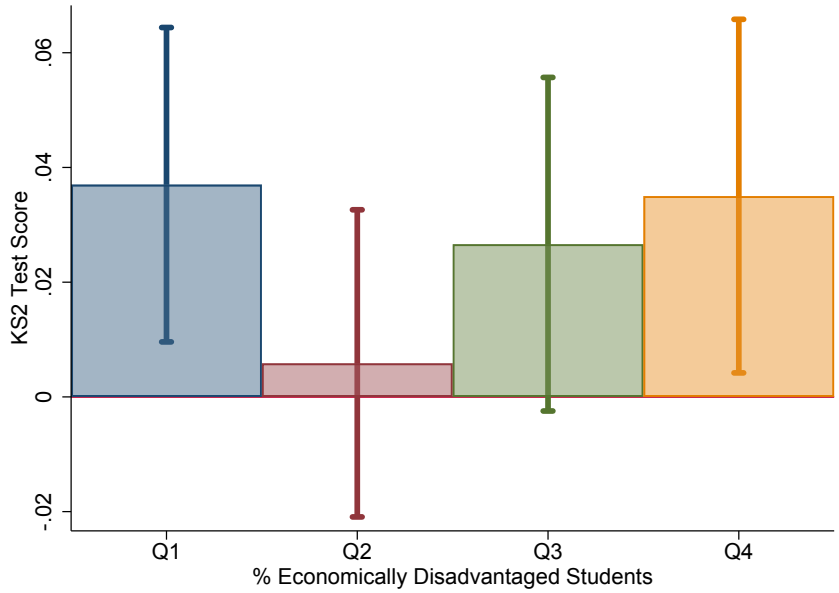
* p<0.10, ** p<0.05, *** p<0.01

standard errors in parenthesis. Standard errors clustered at the regional level. All dependent variables are standardized to have a mean of zero and a standard deviation of unity. I include (not shown in the table) as controls the logarithm of the number of students and the number of students with special needs.

with Special Educational Needs increases.Finally, Figure 3.12 studies the effect of television on education attending to the number of students of schools. The estimates of the impact of television on academic achievement are only positive and statistically significant for schools below the median size of schools.

The evidence provided up until now suggests that students who benefit the most from an increase in television viewing time are: (i) low achievers, (ii) economically disadvantaged or with a very high income, and (iii) Special Educational Needs students.[8] Low-achieving students are more likely to get involved in out-of-school activities that may lower their academic achievement. Subsequently, television may increase test scores of children if it reduces the frequency with which they get involved in detrimental activites. The following section tests for this hypothesis and so explores the underlying mechanism behind the baseline estimates.

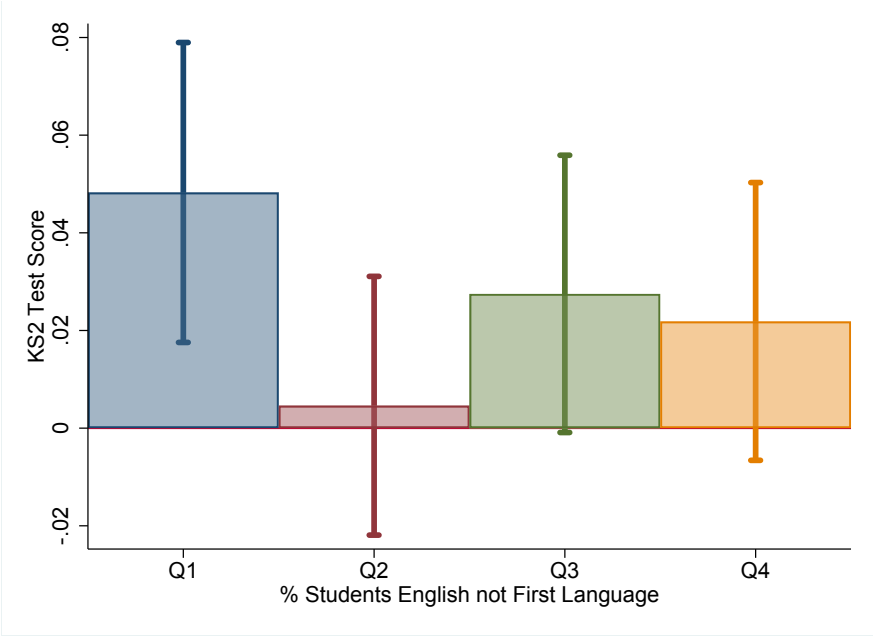
Figure 3.9: Heterogeneity in % of Economically Disadvantaged Students



The figure estimates a specification similar to the baseline one but controls for an interaction between the digital switchover and a set of dummies indicating the quartile of school s in the distribution of the proportion of students eligible for free school meals.

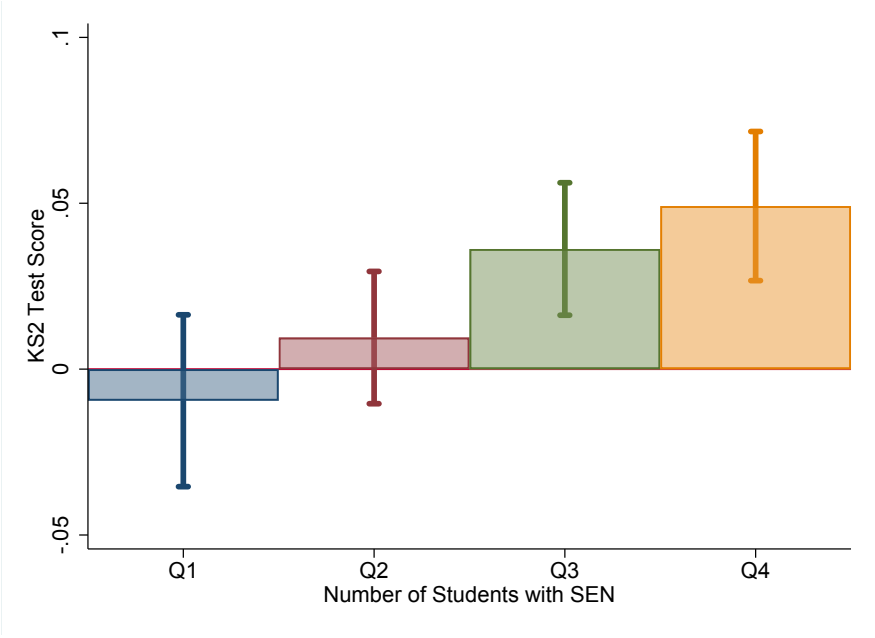
[8]Table B.2 of Appendix B.4 presents the proportion of disadvantaged, native and SEN students for each of the subsamples that I assigned attending to the position of schools in the score distribution of 2008. The Table also presents the average number of students and the proportion of religious schools for each of the quartiles.

Figure 3.10: Heterogeneity in % of Students who are not English Native Speakers



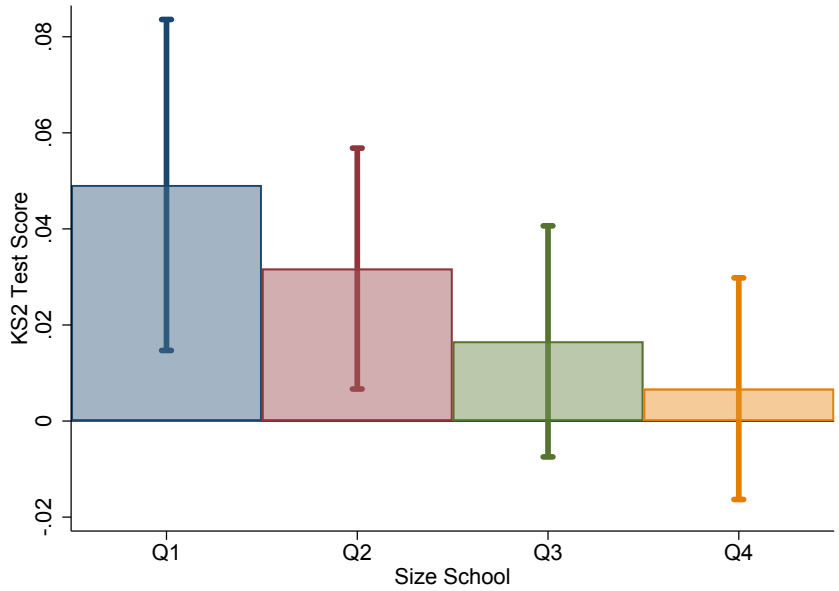
The figure estimates a specification similar to the baseline one but controls for an interaction between the digital switchover and a set of dummies indicating the quartile of school s in the distribution of the proportion of students who are not English Native Speakers.

Figure 3.11: Heterogeneity in % of Students with SEN



The figure estimates a specification similar to the baseline one but controls for an interaction between the digital switchover and a set of dummies indicating the quartile of school s in the distribution of the proportion of students with special educational needs.

Figure 3.12: Heterogeneity in the Number of Students



The figure estimates a specification similar to the baseline one but controls for an interaction between the digital switchover and a set of dummies indicating the quartile of school s in the distribution of the total number of students.

3.6 Impact of Digital Television on Habits

I previously showed that television has a positive impact on test scores and reduces educational inequality. I next explore a plausible mechanism behind the baseline estimates, which is whether television changes the amount of time that children dedicate to activities that may harm their academic performance. I do so by estimating the impact of the digital switchover on the time use of children, and on the frequency with which they get involved in detrimental activities.

3.6.1 Data and Summary Statistics

In this part of the analysis, I use a panel survey dataset at the individual level that contains yearly information on the time use of children. I have previously described this dataset, which comes from the youth questionnaire of the Understanding Society Survey in UK. The panel is unbalanced and contains 15,069 observations on approximately 5,000 children. I next present some de-

scriptive statistics of the sample that I use in the analysis. Column 1 of Table 3.4 displays the unweighted summary statistics of the whole sample, and columns 2-5 assign individuals into four different groups attending to the year when they got access to digital television for the first time. As shown, children have a similar age, and are equally likely to be female or native independently of the year when they were affected by the digital switchover. Households also have a similar number of members and children across the different subsamples. Regarding socioeconomic characteristics, the number of bedrooms, cars, and the gross income of households are approximately the same across the different subsamples, with the exception of households that got access to digital television in 2009, who have a lower income. Finally, children affected by the digital switchover at the beginning or at the end of the process are more likely to live in urban areas. I control for these observable characteristics in the analysis.

Table 3.4: Descriptive Statistics

	Sample	2009	2010	2011	2012
	(1)	(2)	(3)	(4)	(5)
Age	12.51	12.51	12.43	12.53	12.50
	(1.69)	(1.67)	(1.70)	(1.70)	(1.68)
Female	0.50	0.47	0.52	0.51	0.49
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Native	0.96	0.96	0.98	0.97	0.95
	(0.20)	(0.20)	(0.13)	(0.18)	(0.22)
N members	4.42	4.37	4.37	4.38	4.50
	(1.37)	(1.29)	(1.23)	(1.36)	(1.43)
N children	1.90	1.92	1.89	1.88	1.91
	(1.15)	(1.15)	(1.10)	(1.16)	(1.14)
Gross Income	4158.1	3805.0	4491.2	4114.7	4376.6
	(2767.8)	(2291.4)	(3223.0)	(2584.6)	(3123.1)
Rural Area	0.19	0.18	0.26	0.26	0.10
	(0.39)	(0.39)	(0.44)	(0.44)	(0.30)
N bedrooms	3.40	3.40	3.57	3.47	3.29
	(0.94)	(0.91)	(0.98)	(0.93)	(0.95)
N cars	1.39	1.42	1.71	1.46	1.25
	(0.88)	(0.83)	(0.86)	(0.85)	(0.92)
N	15,053	3,131	706	6,037	5,179

The table presents the means of a number of observable variables, and their standard deviations in parenthesis. I divide individuals in groups attending to the year when they were first affected by the digital switchover. This allows me to compare the summary statistics of the different groups.

3.6.2 Empirical Model

Similarly to the baseline specification, I follow a difference-in-difference approach that uses exogenous variation in the timing of the digital switchover as a natural experiment. More formally, I estimate the following specification:

$$a_{i,t} = \alpha + \beta DigitalSwitch_{s,t} + \gamma X_{i,t} + \delta_i + \lambda_t + \varepsilon_{i,t} \tag{3.3}$$

where $a_{i,t}$ is the frequency/probability with which child i gets involved in activity a at year t. $DigitalSwitch_{i,t}$ is a dummy that takes value 1 if the LSOA where child i lives has already been affected by the digital switchover by year t, and 0 otherwise.[9] As previously explained, the reason for using exogenous variation at the LSOA level is that this is the smallest geographical unit at which the Understanding Society Survey provides information. The total number of LSOAs in England is 32,844, and they have a mean population of 1,500. $X_{i,t}$ is a set of time-varying characteristics at the child level such as age, or whether the child lives in an urban area. $X_{i,t}$ also includes time-varying household characteristics such as the number of members, children, cars and bedrooms in the house. Finally, $X_{i,t}$ controls for the gross income of the household where child i lives at year t. δ_i are child fixed effects that control for unobserved time-invariant characteristics at the individual level, such as preferences. λ_t are year fixed effects, and $\varepsilon_{i,t}$ is a time-varying error term at the individual level. I allow for an arbitrary correlation of standard errors at the regional and child level.[10]

[9] In this part of the analysis, I do not control for $DigitalTenure_{i,t}$, which is the number of years that have passed by year t since child i got access to digital television. The reason is that the main purpose of this section is to explore the mechanism behind the baseline estimates, but not its dynamics. Moreover, $DigitalTenure_{i,t}$ is highly correlated with age, which generates an important multicollinearity problem when I include $DigitalTenure_{i,t}$ in the regression. Yet, the inclusion of $DigitalTenure_{i,t}$ as a covariate does not change the estimates presented in this part of the analysis.

[10] The reason why I use a two-way cluster is that some children move across regions during the period of analysis.

3.6.3 Results

Column 1 of Table 3.5 tests whether the digital switchover increases motivation of children towards school. This serves as a second set of evidence on the causal impact of television on education by using a different dataset than the one I adopted to estimate the baseline results. More specifically, column 1 estimates specification (3.3) using as dependent variable a categorical variable that measures how happy children are with their school work in a scale that ranges from 1 (not happy at all) to 7 (very happy). I find that digital switchover has a positive impact on the degree of happiness of children towards school work, and the estimate is significant at the 10% confidence level. This is in line with the baseline estimates of this paper.

Columns 2-7 study the causal impact of television on the probability and frequency with which children: (i) drink alcohol, (ii) smoke, and (iii) arrive late at home. I measure all the frequencies in a scale that ranges from 1 (never) to 5 (most days). These columns allow me to test whether the positive impact of television on education is due to television reducing the time that children dedicate to out-of-school activities that may harm their academic achievement. More specifically, column 2 of Table 3.5 estimates specification (3.3) using as dependent variable a dummy that takes value 1 if child i has ever drunk alcohol by year t, and 0 otherwise. As shown, the digital switchover reduces the probability of children having ever drunk alcohol by 2.8 percentage points. Given that the digital switchover raises television viewing time by 15.1%, the average television viewing time reduces the probability of children drinking alcohol by 18.5%. The estimate is significant at the 5% confidence level. Column 3 is similar to column 2, but uses as dependent variable the frequency with which child i drinks alcohol per week at year t. As shown, an increase in television viewing time reduces the frequency with which children drink alcohol. The estimate is significant at the 1% confidence level. Column 4 studies the impact of television on a binary variable that takes value 1 if child i has ever smoked by year t, and 0 otherwise. Column 5 is similar to column 4, but uses the frequency with which children

smoke per week as dependent variable. As shown, the digital switchover reduces the probability of having ever smoked and the frequency of smoking, albeit the estimates are not statistically significant. Finally, columns 6-7 respectively use as dependent variable the probability and frequency with which children have arrived late at home in the last month. I define arriving late as reaching home after 9:00 pm. The estimates of the digital switchover are negligible and not statistically significant. Overall, the results suggest that the positive impact of television on academic performance is at least partially due to television reducing the probability/frequency with which children get involved in detrimental activities.[11,12,13]

Another plausible explanation as to why television has an impact on education is that television may change the importance that children give to school grades, the time they dedicate to homework, or their interest in extra-curricular activities. As shown in Appendices B.5-B.6, this is not the case. It is also important to note that my baseline estimates on education are based on a sample of children aged 10-11, whereas the set of results of this section is based on children aged 10-15. Therefore, it is important to explore whether children aged 10-11 are likely to drink alcohol, smoke or arrive late at home. If they do not get involved in this type of activities, then the digital switchover cannot reduce the probability or frequency with which children aged 10-11 take part in them. As shown in Appendix B.9, it is not rare for children aged 10-11 to drink alcohol, smoke and arrive late at home. I next explore heterogeneity in the impact of television on time use by age, income, gender and ethnicity.

[11] Appendix B.7 also tests whether the digital switchover changes the probability and frequency with which children use social media and play video games. As shown, the digital switchover only reduces the time that children spend on social media.

[12] Appendix B.8 combines bad activities (drinking alcohol, smoking and arriving late at home) into one variable. It also studies the impact of the digital switchover on this variable.

[13] It is also possible to test whether the digital switchover has an effect on television viewing time. However, the data on television viewing time available in the Understanding Society Survey is subject to three important limitations. First, the variables that contain this information only take five possible values: no television viewing time, less than one hour, 1-3 hours, 4-6 hours, 7 hours or more. Second, 85% of observations fall in the second and third categories. Third, there only exists information on television viewing time during weekends in two waves. These limitations markedly reduce the variability in the information available on television viewing time. It is therefore not surprising that I find no effect of the digital switchover on television viewing time.

Table 3.5: Harmful Activities and Practices

	Happ school	Prob drunk alcohol	Freq drinks alcohol	Prob smoked	Freq smokes	Prob arrives late	Freq arrives late
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Digital Switch	0.056*	-0.031**	-0.058***	-0.004	-0.003	0.001	0.002
	(0.034)	(0.014)	(0.021)	(0.009)	(0.017)	(0.012)	(0.019)
Age	0.048	0.019	0.001	0.020	0.036	-0.032**	-0.033
	(0.047)	(0.020)	(0.032)	(0.012)	(0.023)	(0.016)	(0.026)
Gross Income	-0.006	-0.014	-0.019	-0.003	-0.005	0.012	0.016
	(0.023)	(0.011)	(0.016)	(0.006)	(0.011)	(0.008)	(0.012)
Rural Area	-0.061	0.022	-0.111	0.033	-0.022	-0.095**	-0.168**
	(0.165)	(0.054)	(0.108)	(0.044)	(0.112)	(0.047)	(0.076)
N bedrooms	-0.033	-0.016	-0.049*	-0.006	-0.018	-0.011	-0.003
	(0.049)	(0.016)	(0.026)	(0.011)	(0.018)	(0.012)	(0.020)
N members	-0.014	-0.004	0.021	-0.002	0.002	0.030***	0.023
	(0.034)	(0.014)	(0.020)	(0.009)	(0.020)	(0.012)	(0.019)
N children	0.02	-0.03**	-0.13***	-0.03***	-0.07***	-0.03***	-0.05***
	(0.025)	(0.011)	(0.017)	(0.007)	(0.016)	(0.009)	(0.014)
Children FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,182	12,200	12,153	12,237	11,989	12,252	12,252

* p<0.10, ** p<0.05, *** p<0.01

Standard Errors in parenthesis. I allow for an arbitrary correlation of standard errors at the regional and child level. The number of observations vary depending on the specification because sometimes information is missing depending on the dependent variable.

3.6.4 Heterogeneity in Age

I have so far studied the average effect of television on the probability and frequency with which children get involved in detrimental activities. I next examine whether this impact is heterogeneous in the age of children. This is relevant as my analysis on education is based on a sample of children whose age is 10-11, whereas my analysis on time use on children aged 10-15. This part of the analysis also allows me to evaluate whether children of different ages re-distribute their time allocation differently as a response of the digital switchover process. I study heterogeneity in age by estimating a specification similar than 3.3, but that includes an interaction between the digital switchover dummy and age.[14]

[14]To simplify the analysis, I do not control for $DigitalTenure_{i,t}$, which is the number of years that have passed by year t since child i got access to digital television. However, its

Figures 3.13-3.15 present the average marginal effects of television for the different ages, respectively using as dependent variable the probability of having ever drunk alcohol, smoked or arrived late at home in the last month. As shown, the digital switchover decreases the probability of children getting involved in any of these detrimental activities when they are younger, and the estimates are significant at the 5% confidence level. However, television increases these habits when children are older, albeit the estimates are only significant for the probability of smoking or arriving late at home. A possible explanation is that teenagers watch television contents that induce them to get involved in harmful activities. Appendix B.10 estimates an analysis similar to the one presented in this section, but uses as dependent variables the frequency of children drinking alcohol, smoking or arriving late at home. As shown, I obtain similar estimates than the ones presented in this section.

3.6.5 Heterogeneity in Income

This subsection studies heterogeneity in the impact of television on habits attending to the income of the household where children live. This is important because I previously found that the effect of television on education varies in the socioeconomic background of students. If children with poorer socioeconomic conditions are more likely to get involved in detrimental activities, an increase in television viewing time may benefit them more. I therefore explore whether the effect of the digital switchover on habits varies in the household income of children. I do so by estimating a model analogous to 3.3, but that includes an interaction between the digital switchover indicator and income quartiles.[15] Figure 3.16 presents the average marginal effects of the digital switchover when I use as dependent variable the probability of having ever had an alcoholic drink. As shown, the impact of the digital switchover on this habit is negative for every income quartile. However, the estimates are higher for children at the bottom

inclusion would lead to similar estimates than the ones presented in this section.

[15]Similarly than in the analysis where I study heterogeneity in age, I do not include $DigitalTenure_{i,t}$ in the model of this section to simplify the analysis. However, including it as a covariate gives similar estimates than the ones I show in this section.

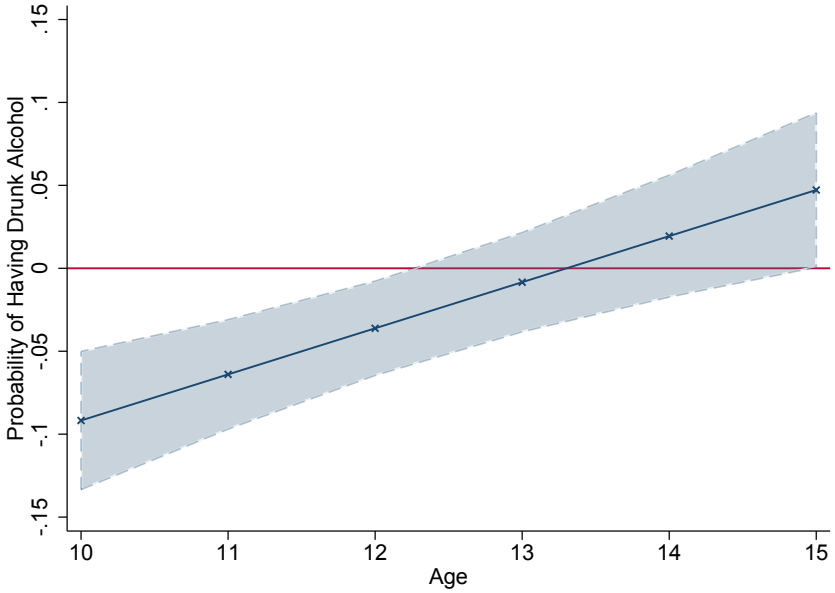
of the income distribution, and only statistically significant at the 5% confidence level for the first income quartile. Figure 3.17 shows the estimates when I use the probability of having ever smoked as dependent variable. Interestingly, the estimate of the digital switchover is only negative and significant at the 5% confidence level for children at the top of the income distribution. This provides further evidence on television reducing the probability of children getting involved in detrimental activities. This set of estimates also indicates that children who benefit from television are not only those with worse socioeconomic background. Finally, Figure 3.18 presents the estimates when I use the probability of arriving late at home in the last month as dependent variable. As shown, all the estimates are small and not statistically significant. In Appendix B.11, I perform an analysis similar to the one of this section, but respectively adopting as dependent variables the frequency of children drinking alcohol, smoking, or arriving late at home. As shown, I find analogous results to the ones presented in this section.

3.6.6 Heterogeneity in Gender and Ethnicity

It is also important to test whether the impact of television on time use is heterogeneous in gender and ethnicity. To do so, I first estimate specification 3.3, but controlling for an interaction between the digital switchover dummy and gender. I respectively use as dependent variables the probability of having ever had an alcoholic drink, smoked, or arrived late at home in the previous month. Figure 3.19 presents the average marginal effects of television for boys and girls. As shown, the impact of television on time use is driven by boys. Figure 3.20 is similar to figure 3.19, but instead explores heterogeneity in the ethnicity of children. I classify children into two different groups: white British and non-white British. As shown, the switchover reduces the time that non-white British children dedicate to detrimental activities, but has a negligible impact for white British children.[16]

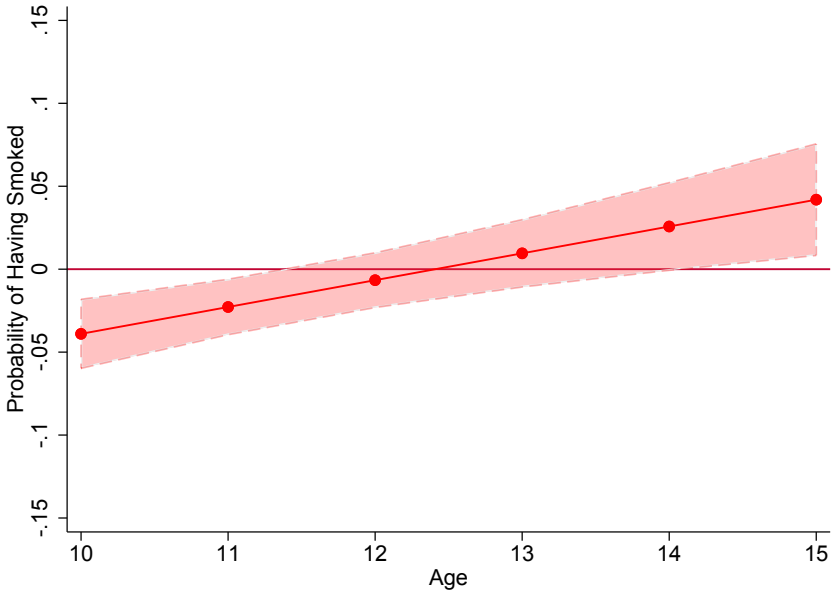
[16] Appendix B.12 studies whether the impact of the digital switchover on detrimental activities is heterogeneous in ethnicity, and uses a more disaggregated classification of race.

Figure 3.13: Impact on Probability of Having Ever Drunk Alcohol



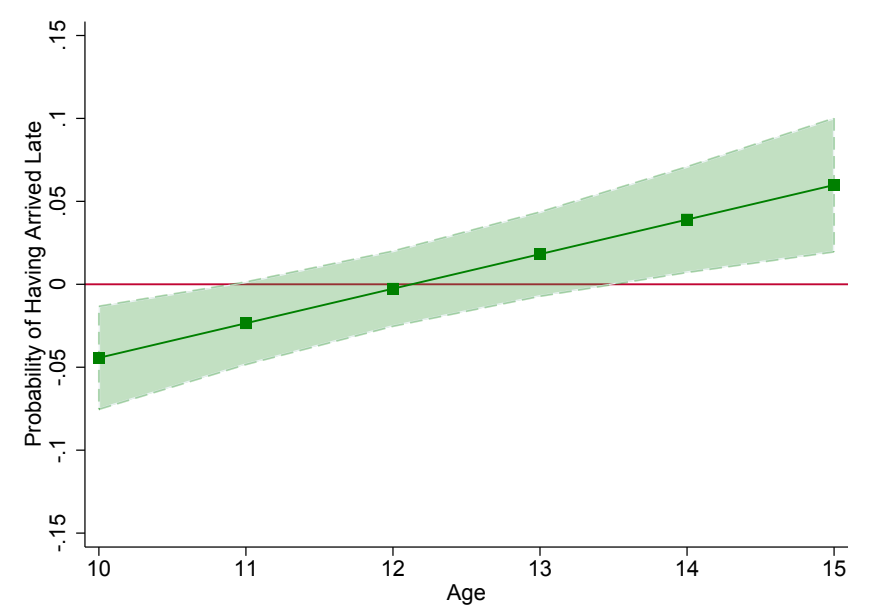
The figure estimates equation (3.3), including as control an interaction between the switchover dummy and age. I use as dependent variable the probability of having ever had an alcoholic drink. I show the average marginal effects of television for the different ages of children.

Figure 3.14: Impact on Probability of Having Ever Smoked



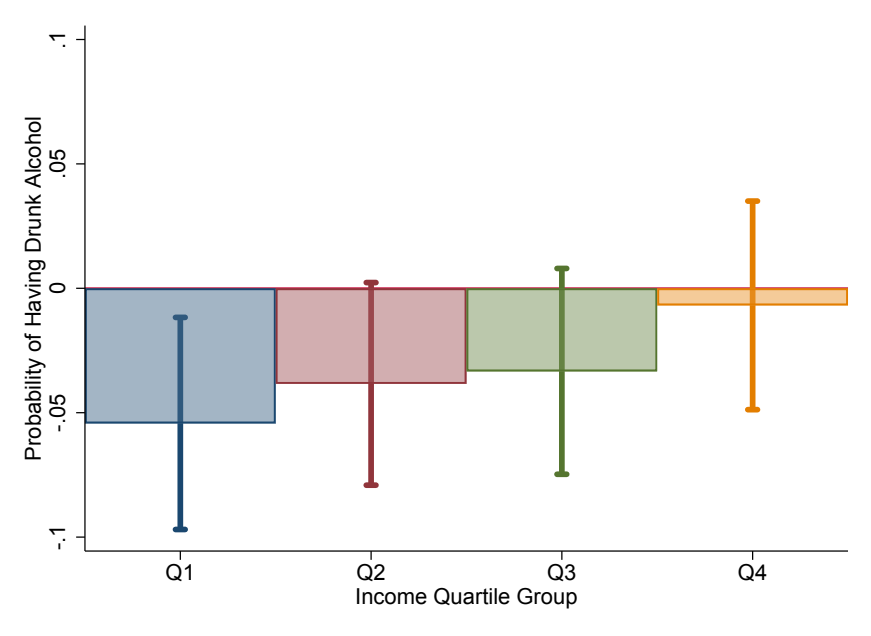
The figure estimates equation (3.3), including as control an interaction between the switchover dummy and age, and using as dependent variable the probability of having ever smoked. I show the average marginal effects of television for the different ages of children.

Figure 3.15: Impact on Probability of Having Arrived Home after 9pm



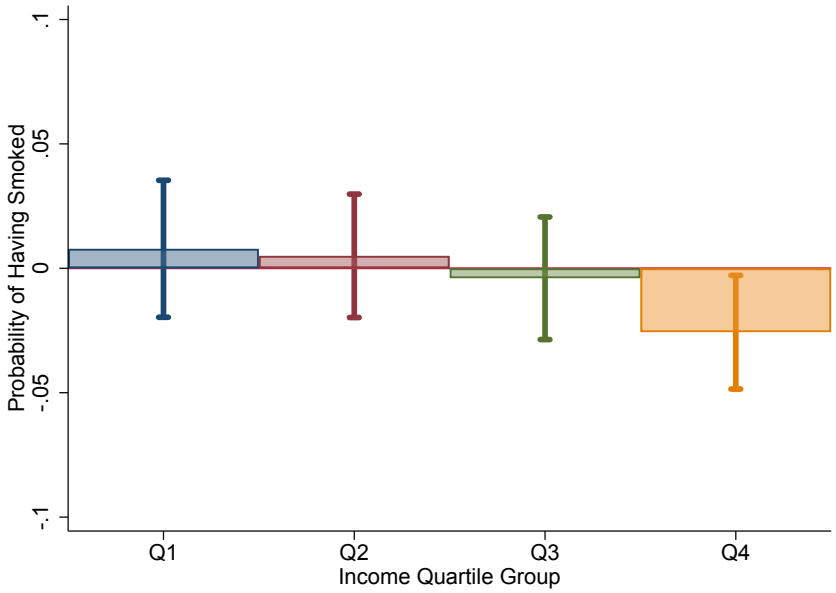
The figure estimates equation (3.3), including as control an interaction between the switchover dummy and age, and using as dependent variable the probability of having arrived late at home in the past month. I show the average marginal effects of television for the different ages of children.

Figure 3.16: Impact on Probability of Having Ever Drunk Alcohol



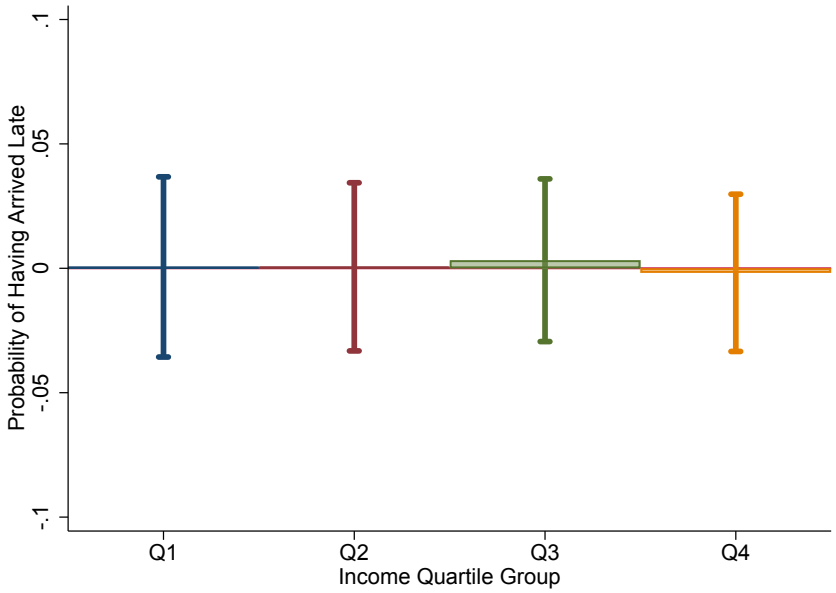
The figure estimates equation (3.3), but including as control an interaction between the digital switchover indicator and income quartiles, and using as dependent variable the probability of having ever drunk an alcoholic drink. I show the average marginal effects of television for the different income quartiles of the households where children live.

Figure 3.17: Impact on Probability of Having Ever Smoked



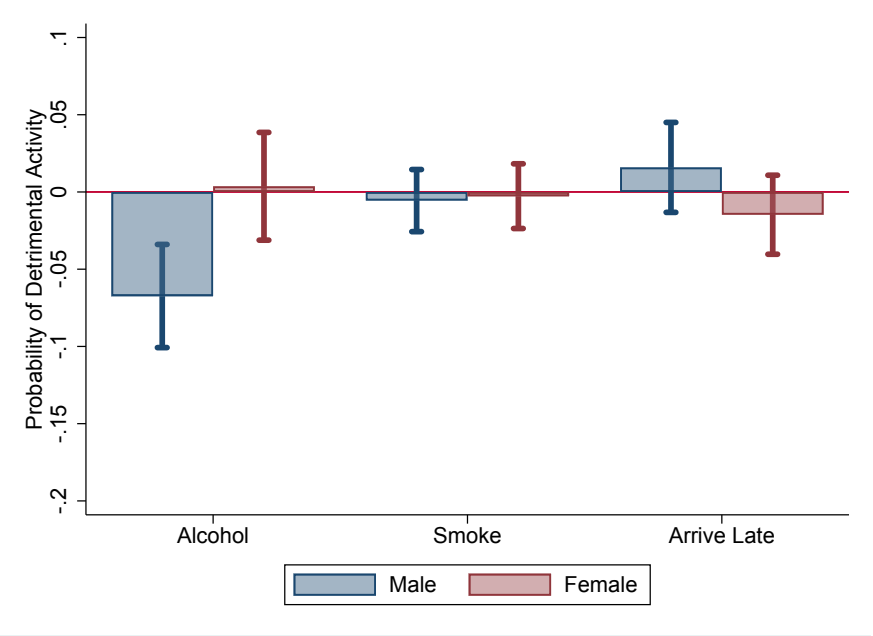
The figure estimates equation (3.3), but including as control an interaction between the digital switchover indicator and income quartiles, and using as dependent variable the probability of having ever smoked. I show the average marginal effects of television for the different income quartiles of the households where children live.

Figure 3.18: Impact on Probability of Having Arrived Home after 9pm



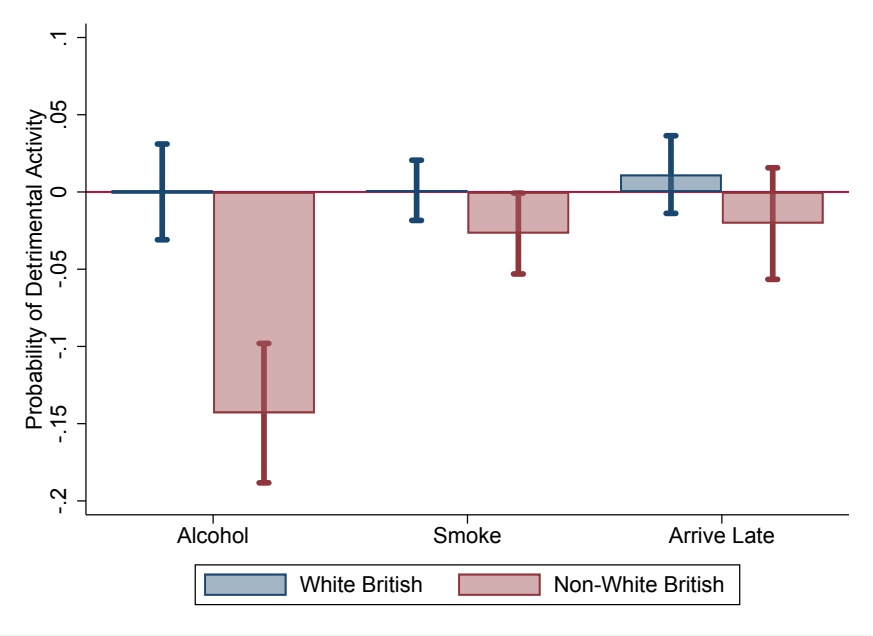
The figure estimates equation (3.3), but including as control an interaction between the digital switchover indicator and income quartiles, and using as dependent variable the probability of having arrived home after 9pm in the past month. I show the average marginal effects of television for the different income quartiles of the households where children live.

Figure 3.19: Impact on Probability of Detrimental Activity



The figure estimates equation (3.3), but including as control an interaction between the digital switchover dummy and gender. I respectively use as dependent variables the probability of having ever drunk an alcoholic drink, smoked, or arrived late at home in the last month. I show the average marginal effects of television for boys and girls.

Figure 3.20: Impact on Probability of Detrimental Activity

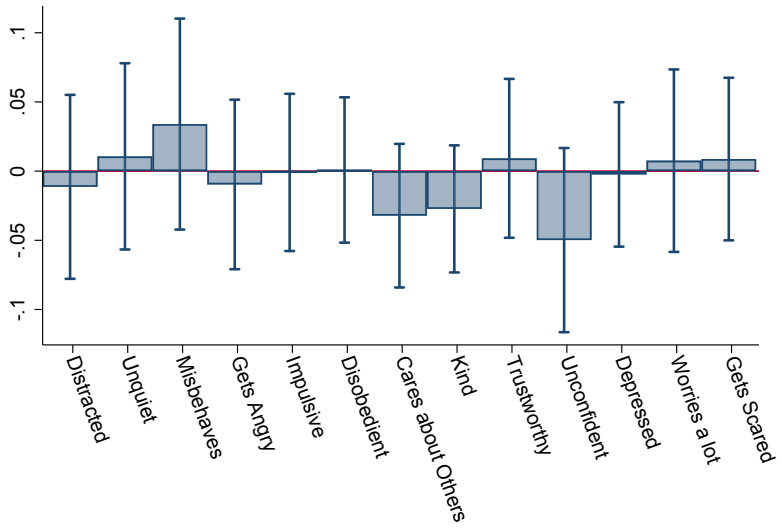


The figure estimates equation (3.3), but including as control an interaction between the digital switchover dummy and ethnicity. I respectively use as dependent variables the probability of having ever drunk an alcoholic drink, smoked, or arrived late at home in the last month. I show the average marginal effects of television for white British and non-white British children.

3.6.7 Exploring further Outcomes: Behaviour

This paper has shown that television has a positive impact on academic achievement and educational inequality. Moreover, I have found that television reduces the probability and frequency with which children get involved in detrimental out-of-school activities. I next explore whether the digital switchover also has an effect on the behaviour of children. To do so, I estimate specification (3.3), but using as dependent variable different measures of the behaviour and feelings of children. To simplify the analysis, I do not control for $DigitalTenure_{i,t}$. More specifically, I use as dependent variables the frequency with which, over the previous six months, a child: (i) got distracted, (ii) felt unquiet, (iii) misbehaved, (iv) got angry, (v) made decisions impulsively, (vi) was disobedient, (vii) cared about others, (viii) was kind, (ix) trustworthy, (x) unconfident, (xi) felt depressed, (xii) worried a lot, and (xiii) got scared. All these variables are measured with categorical scales that take three possible values: not true (0), somewhat true (1) and certainly true (2). As shown in Figure 3.21, no estimate is statistically significant at the 5% confidence level, indicating that the digital switchover has no impact on the behaviour of children.

Figure 3.21: Impact on Behaviour of Children



The figure estimates equation (3.3), but using as dependent variable different measures of the behaviour and feelings of children.

3.7 Conclusions

This paper studies the effect of television on academic performance and time use. I provide causal estimates by using the British digital television switchover as a natural experiment. This process occurred during the period 2008-2012, and at different years for each of the TV transmitters. These are located in different regions in UK. Consequently, the timing of the switchover varied at the postcode unit level. The switchover was delegated to Ofcom and DigitalUK, whose goal was to implement the process on time attending to the physical characteristics of the TV transmitters. Therefore, it is unlikely that the switchover is correlated with unobserved characteristics that determine education or habits. Using a difference-in-difference approach, I firstly estimate whether television has an impact on academic performance. I then explore a plausible mechanism behind the estimates on education: whether television changes the time use of children.

I find that the digital switchover increases KS2 test scores by 0.028 standard deviations. However, it is important to bear in mind that the digital switchover raises television viewing time by 15.1%. Subsequently, the average television viewing time increases scores by 0.185 standard deviations. This contributes to human capital formation. Moreover, the effect is driven by schools at the bottom of the test score distribution, which reduces educational inequality. The estimates are robust across different samples and specifications.

My second set of resuts explore a plausible mechanism behind my estimates on education: the impact of television on time use. I show that television reduces the probability and the frequency with which children get involved in detrimental activities, such as alcohol drinking. Taken together, the results suggest that the effect of out-of-school activities on academic performance does not depend on their absolute educational value, but on their relative one.

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B Appendix to Chapter 3

B.1 Mapping of Point Scores to Levels

Table B.1 presents a mapping between the point scores that students achieve at the Key Stage 2 exam and the levels to which these grades correspond. I collect this information from the UK Department of Education. Regarding the levels, letter "c" means that a child has scored at the lower end of the level, "b" means that the child is working comfortably at that level, and "a" means that the child's score is at the top end of the level. Column 3 of the Table describes what achieving a particular level means in terms of academic performance according to the UK Department of Education. As shown, having a point score of 23 or lower indicates a poor educational performance, and achieving a score of 37 or above reflects outstanding academic attainment.

Table B.1: Scores KS2 Test

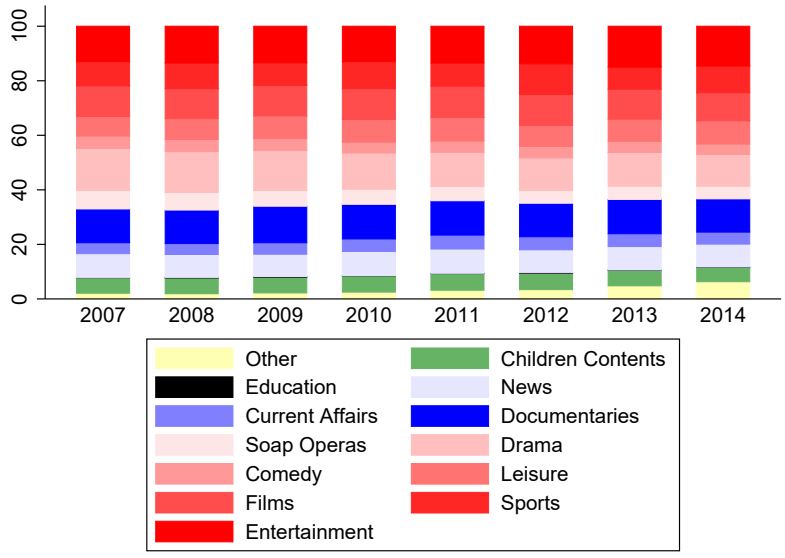
Level	Point Score	Description
(1)	(2)	(3)
3a	23	Below Average
4c	25	About Average
4b	27	About Average
4a	29	Above National Average
5c	31	Above National Average
5b	33	Well Above National Average
5a	35	Well Above National Average
6c	37	Exceptional
6b	39	Exceptional

The Table maps the point scores of the Key Stage 2 exam with their respective academic levels, and with a brief description of what achieving these levels means.

B.2 Proportion of Television Viewing Time by Genre

Figure B.1 presents the proportion of the total television viewing time by genre. This Figure is similar than Figure 3.5, but presents a more disaggregated classification of the different television contents that are available. More specifically, I classify television contents in thirteen different groups: entertainment, sports, films, leisure, comedy, drama, soap operas, documentaries, current affairs, news, educational television programmes, children contents, and other. Similar to the main analysis, the latter category includes television channels such as music programmes. This classification allows me to further explore whether television contents changed as a response to the digital switchover process. The Figure suggests that this was not the case.

Figure B.1: Proportion of Television Viewing Time by Genre

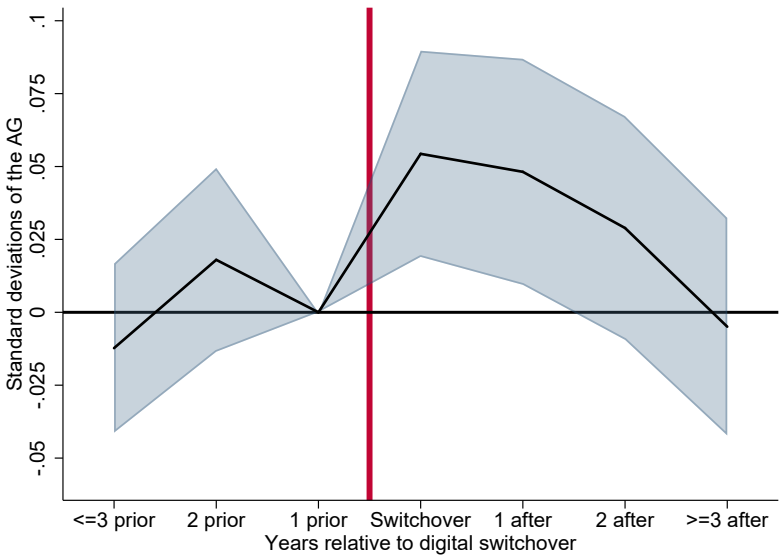


The figure presents the proportion of television viewing time by genre. I classify television contents in entertainment, sports, films, leisure, comedy, drama, soap operas, documentaries, current affairs, news, educational television programmes, children contents, and other.

B.3 Timing of the Effect of Television on Education

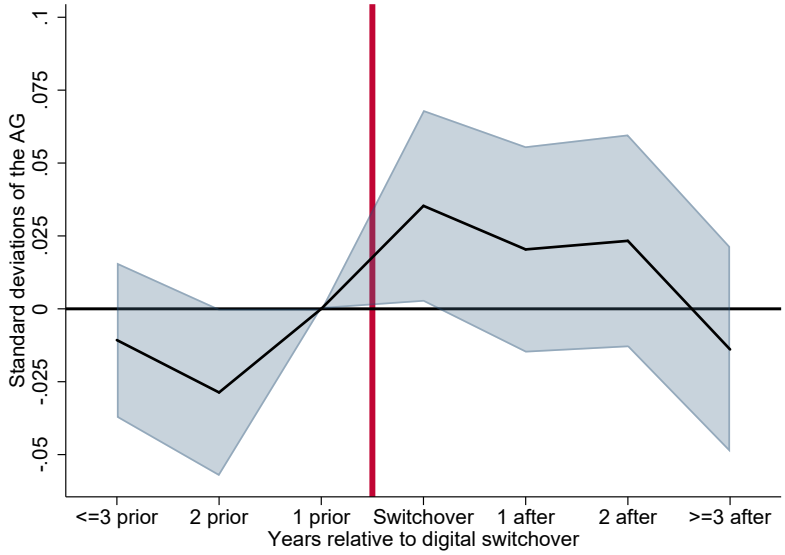
Figures B.2 - B.5 estimate the event approach specification (3.3) for each of the subsamples that I assigned attending to the position of schools in the score distribution of 2008. As previously explained, I select the grade distribution in 2008 because this is the year before the switchover process started. This subsection allows me to study whether there are differences in the dynamics of the impact of television on education depending on the performance of schools previous to the switchover. As shown, the estimates of the effect of the years previous to the digital switchover on academic scores are not statistically significant. The estimates become positive and significant in the years after the introduction of the digital television signal, and only for the schools that pertain to the first and second quartile of the score distribution in 2008. The switchover process has no impact on the academic scores of schools performing at the top of the score distribution in 2008. Similar to my baseline results, the estimates suggest that the impact of television on education is temporary.

Figure B.2: Dynamic Impact of Television on Education for the First Quartile



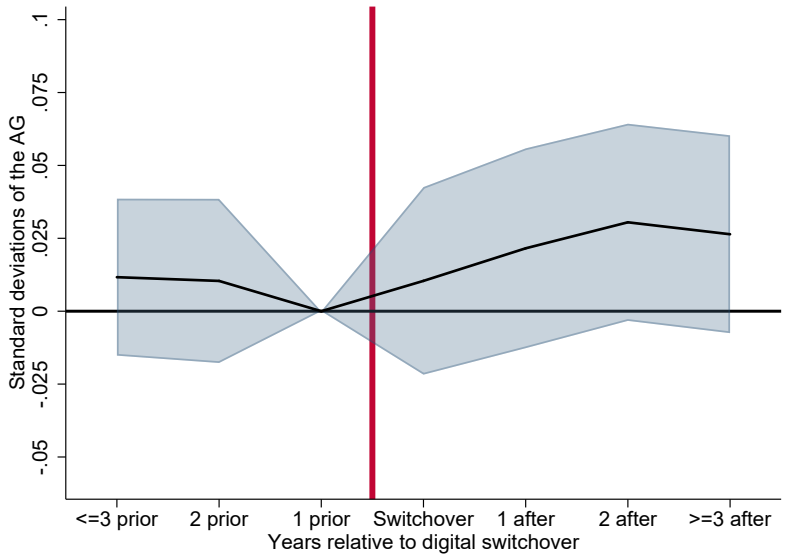
The figure estimates the event approach specification (3.2) for the first quartile of the score distribution in 2008, showing the dynamics of the effect of television on education.

Figure B.3: Dynamic Impact of Television on Education for the Second Quartile



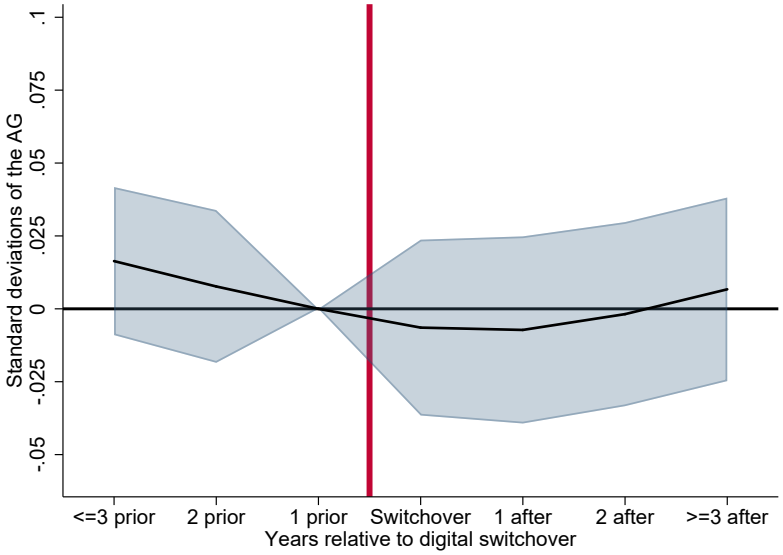
The figure estimates the event approach specification (3.2) for the second quartile of the score distribution in 2008, showing the dynamics of the effect of television on education.

Figure B.4: Dynamic Impact of Television on Education for the Third Quartile



The figure estimates the event approach specification (3.2) for the third quartile of the score distribution in 2008, showing the dynamics of the effect of television on education.

Figure B.5: Dynamic Impact of Television on Education for the Fourth Quartile



The figure estimates the event approach specification (3.2) for the fourth quartile of the score distribution in 2008, showing the dynamics of the effect of television on education.

B.4 Relationship between Predetermined Scores and Observable Characteristics

Table B.2 shows the summary statistics of the variables that I use in the heterogeneity analysis. I show their descriptive statistics for the whole sample and for each of the subsamples that I assigned attending to the position of schools in the score distribution of 2008. This helps to understand the similarity between the baseline estimates on educational inequality and the results obtained in the heterogeneity analysis. As shown in the Table, schools at the top of the score distribution have fewer economically disadvantaged students and are smaller in size. They also have a higher proportion of native students and are more likely to be religious. The proportion of students with special educational needs is independent of the position of schools in the score distribution of 2008.

Table B.2: Descriptive Statistics for Each Quartile

	Sample	Q1	Q2	Q3	Q4
	(1)	(2)	(3)	(4)	(5)
% disadvantaged students	0.27	0.45	0.30	0.20	0.13
	(0.21)	(0.20)	(0.19)	(0.16)	(0.12)
% native students	0.87	0.77	0.85	0.91	0.93
	(0.22)	(0.28)	(0.23)	(0.17)	(0.13)
% SEN students	0.02	0.03	0.02	0.02	0.02
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
N students	269.99	288.07	278.97	269.82	260.60
	(147.56)	(141.64)	(137.38)	(141.62)	(133.85)
% religious schools	0.38	0.22	0.34	0.41	0.52
	(0.48)	(0.41)	(0.48)	(0.49)	(0.50)
N	118,729	27,515	27,849	26,566	27,464

The Table displays the proportion of economically disadvantaged, native and SEN students for each of the subsamples that I assigned attending to the position of schools in the score distribution of 2008. The Table also shows the average number of students and the proportion of religious schools for each of the quartiles.

B.5 Impact on Dedication to School

This section tests an alternative mechanism behind my estimates on education, which is whether television increases the importance that children give to school, or the time they dedicate to homework. To do so, columns 1-6 of Table B.3 estimate my baseline specification respectively using as dependent variable: (i) a dummy that takes value 1 if the child wants to study after school and 0 otherwise, (ii) a dummy that takes value 1 if the child wants to go to University after school and 0 otherwise, (iii) a categorical variable that takes 4 possible values, where higher values indicate that child i gives more importance to grades, (iv) the number of evenings that child i does homework per week, (v) the number of hours that students dedicate to homework per week, and (vi) a categorical variable that contains information on the frequency with which child i uses computer for homework. As shown in Table B.3, the digital switchover has no effect on any of these variables, as the estimates are not statistically significant.

Table B.3: Impact on Dedication to School

	Study After School	University	GCSE Matters	Evenings Homework	Hours Homework	Freq PC Homework
Digital Switch	0.002	0.008	-0.010	0.116	-0.891	-0.002
	(0.014)	(0.008)	(0.016)	(0.113)	(1.166)	(0.056)
Time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes
Children FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,665	8,108	11,917	2,570	2,608	4,700

* p<0.10, ** p<0.05, *** p<0.01
Standard Errors in parenthesis. I allow for an arbitrary correlation of standard errors at the regional and child level, as children may move across regions. The number of observations vary depending on the specification because sometimes information is missing depending on the dependent variable. I control for the following time-varying covariates: age, household income, whether the individual lives in a rural area, and the number of bedrooms, members, and children in the household.

B.6 Impact on Extra-curricular Activities

I next test whether the digital switchover has an impact on the time that children dedicate to extra-curricular activities. To do so, columns 1-4 of Table B.4 estimate my baseline specification respectively using as dependent variable: a dummy that indicates whether child i gets involved in (i) music, (ii) art, (iii) dancing or (iv) tutorials for school subjects, when s/he is not at school. Columns 5-7 respectively use as dependent variable the number of books that child i reads per week, and the frequency with which the child gets involved in sports and youth clubs. As shown, the digital switchover has no impact on the time that children dedicate to extra-curricular activities, as no estimate is statistically significant at the 5% confidence level.

Table B.4: Impact on Extra-curricular Activities

	Music	Art	Dance	Tutorial School	Read	Sport	Youth Club
Digital Switch	-0.039*	-0.007	-0.010	0.007	0.415	-0.023	0.179
	(0.024)	(0.020)	(0.023)	(0.018)	(0.550)	(0.056)	(0.129)
Time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Children FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,514	2,514	2,514	2,514	2,660	6,302	2,792

* p<0.10, ** p<0.05, *** p<0.01

Standard Errors in parenthesis. I allow for an arbitrary correlation of standard errors at the regional and child level, as children may move across regions. The number of observations vary depending on the specification because sometimes information is missing depending on the dependent variable. I control for the following time-varying covariates: age, household income, whether the individual lives in a rural area, and the number of bedrooms, members, and children in the household.

B.7 Impact on Social Media and Videogames

This section tests whether the digital switchover reduces the time that children dedicate to alternative leisure activities at home. More specifically, columns 1-6 of Table B.5 estimate my baseline specification respectively using as dependent variable: (i) a dummy that takes value 1 if child i belongs to a social website, (ii) the hours that child i spends chatting on social media, (iii) the probability of child i having played multiplayer games online, (iv) the frequency with which child i uses computer for videogames, (v) whether the child has a video game console at home, and (vi) the hours that the child spends playing video games per day. As shown in Table B.5, the digital switchover reduces the probability of children belonging to a social website and the time they spend chatting with friends on social media.

Table B.5: Impact on Extra-curricular Activities

	Belongs Social Website	Hours Chatting	Has Played Multiplayer Games	Frequency Use PC Games	Has Game Console	Hours Video-games
Digital Switch	-0.025*	-0.093***	0.009	-0.049	-0.006	0.017
	(0.014)	(0.034)	(0.030)	(0.047)	(0.011)	(0.044)
Time-varying covariates	Yes	Yes	Yes	Yes	Yes	Yes
Children FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,172	8,389	2,706	4,698	4,976	4,497

* p<0.10, ** p<0.05, *** p<0.01

Standard Errors in parenthesis. I allow for an arbitrary correlation of standard errors at the regional and child level, as children may move across regions. The number of observations vary depending on the specification because sometimes information is missing depending on the dependent variable. I control for the following time-varying covariates: age, household income, whether the individual lives in a rural area, and the number of bedrooms, members, and children in the household.

B.8 Combined Bad Activities as Dependent Variable

This section studies whether the digital switchover has an impact on the probability and frequency with which children get involved in detrimental activities, after combining all the bad activities into a unique variable. Column 1 uses as dependent variable a dummy that takes value 1 if child i has ever drunk alcohol, smoked or arrived late at home in the last month. Column 2 uses as dependent variable the sum of the frequencies with which child i has drunk alcohol, smoked or arrived late at home in the past month. As shown, the estimates of the digital switchover are negative but not statistically significant. Albeit the digital switchover reduced the probability and the frequency with which children drink alcohol, it might be difficult for the model to find this effect after I combine this activity with other detrimental habits for which television has no effect. Another possibility is that different children get involved in distinct detrimental activities.

Table B.6: Combined Bad Activities as Dependent Variable

	Prob Bad Activity	Freq Bad Activity
Digital Switch	-0.036	-0.055
	(0.022)	(0.037)
Time-varying covariates	Yes	Yes
Children FE	Yes	Yes
Observations	12,030	11,725

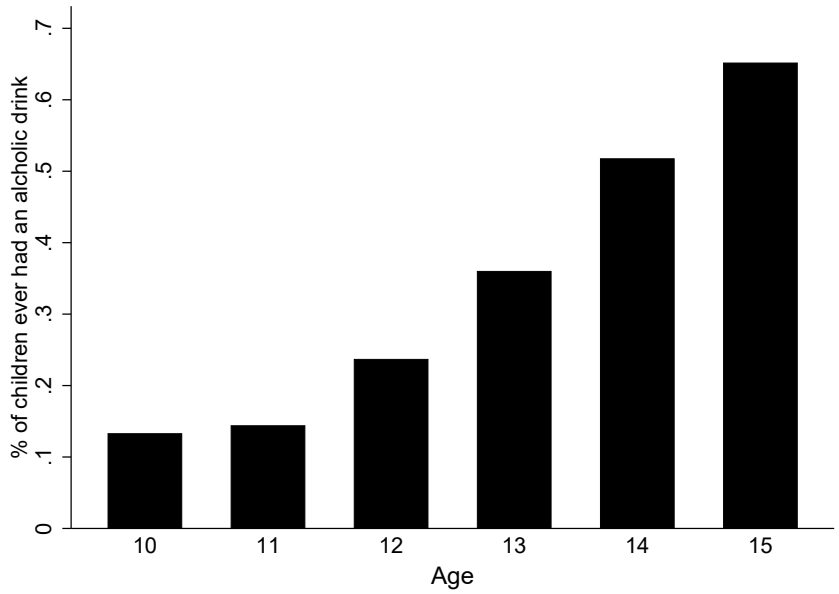
* p<0.10, ** p<0.05, *** p<0.01

Standard Errors in parenthesis. I allow for an arbitrary correlation of standard errors at the regional and child level, as children may move across regions. The number of observations vary depending on the specification because sometimes information is missing depending on the dependent variable. I control for the following time-varying covariates: age, household income, whether the individual lives in a rural area, and the number of bedrooms, members, and children in the household.

B.9 Children and Harmful Activities at Age 10-11

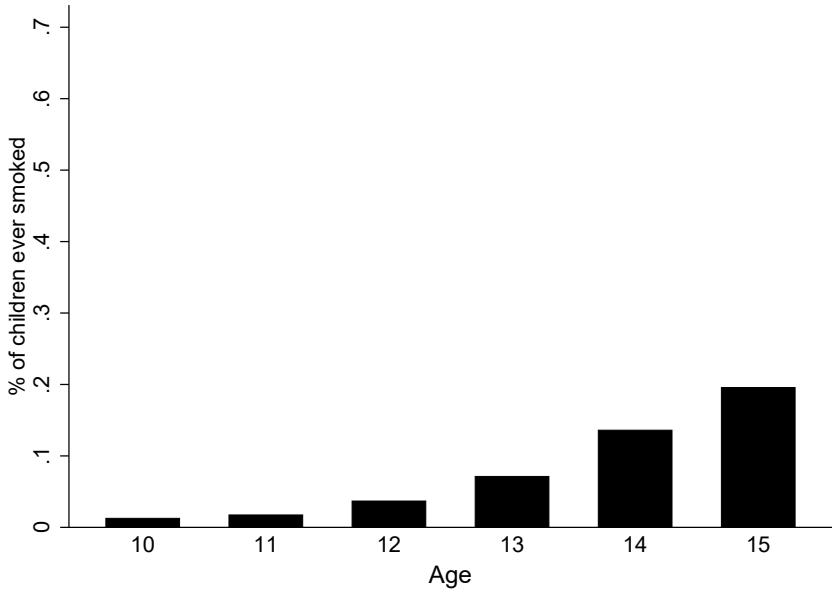
As shown in Figures B.6-B.8, children aged 10-11 are likely to have ever drunk alcohol, smoked, or arrived late at home in the past month. The probability of getting involved in this type of activities increases as children get older.

Figure B.6: Probability of Having Ever Drunk Alcohol



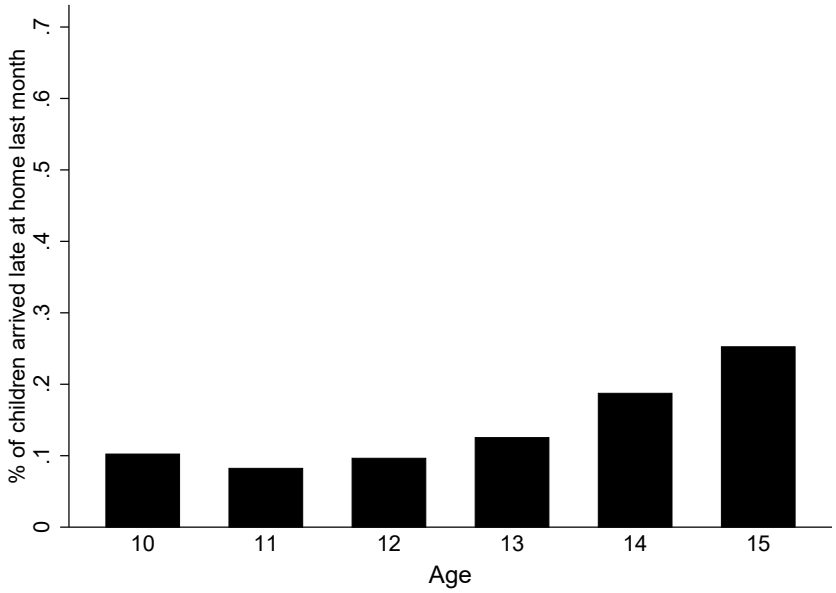
The figure shows the probability of children having ever drunk alcohol at different ages. I select the age range 10-15 as this is the age range of my sample.

Figure B.7: Probability of Having Ever Smoked



The figure shows the probability of children having ever smoked at different ages. I select the age range 10-15 as this is the age range of my sample.

Figure B.8: Probability of Having Arrived Late Last Month

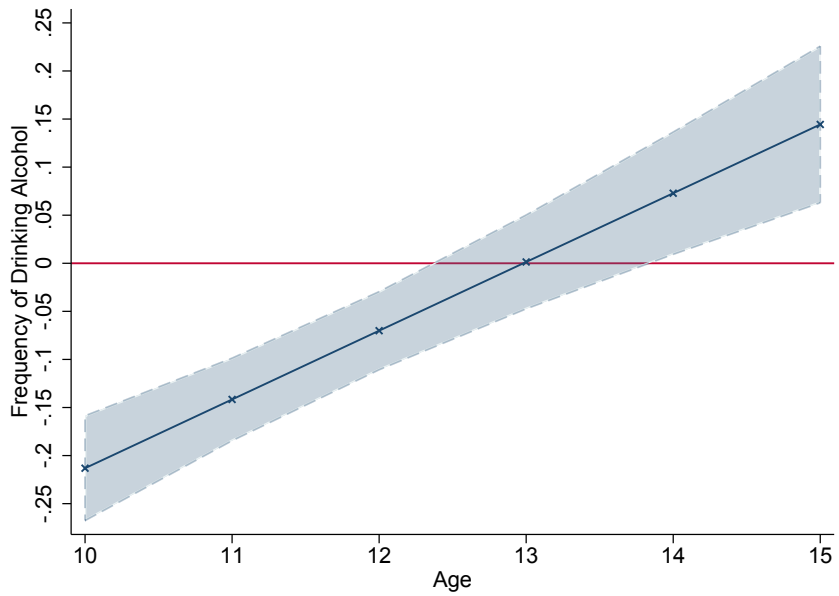


The figure shows the probability of children having arrived after 9pm at home in the past month at different ages. I select the age range 10-15 as this is the age range of my sample.

B.10 Heterogeneity in Age Using Frequencies as Dependent Variables

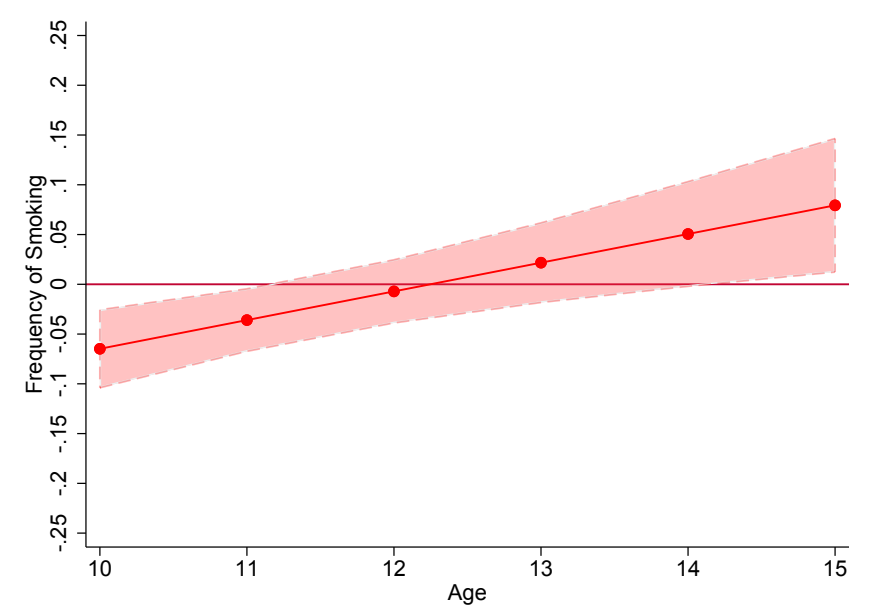
This section studies whether the impact of television on the frequency with which children get involved in detrimental activities is heterogenous in age. To do so, I estimate a model similar to (3.3), but controlling for an interaction between the switchover covariate and age. Figures B.9 - B.11 present the average marginal effects of the switchover process for the different ages of children, respectively using as dependent variable the frequency with which children drink alcohol, smoke, or arrive late at home. As shown, the digital switchover reduces all these frequencies when children are younger, and the estimates are significant at the 5% confidence level. The estimates of the digital switchover become positive when children are older, and these are also statistically significant.

Figure B.9: Impact on Frequency of Drinking Alcohol



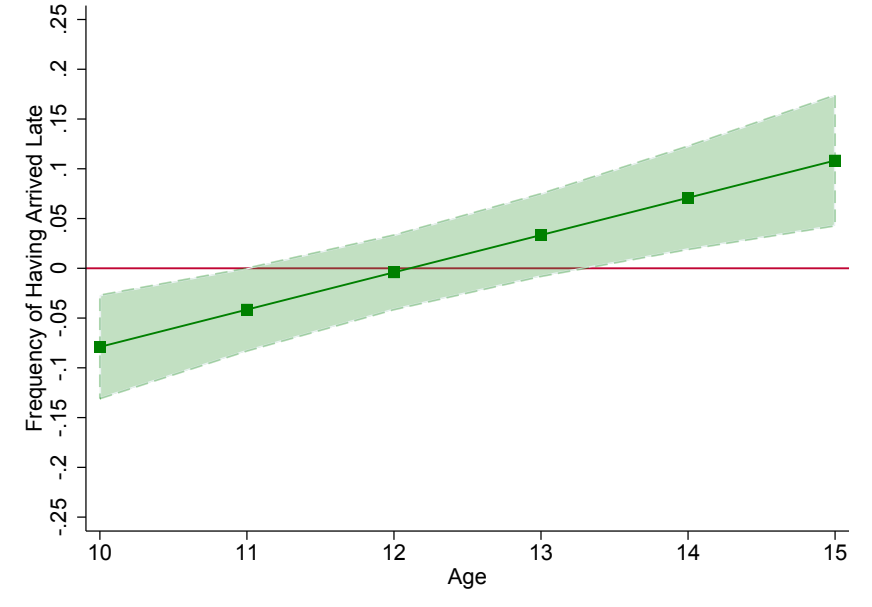
The figure estimates equation (3.3), but including as control an interaction between the digital switchover dummy and age. I use as dependent variable the frequency of drinking alcohol. I show the average marginal effects of television for the different ages of children.

Figure B.10: Impact on Frequency of Smoking



The figure estimates equation (3.3), but including as control an interaction between the digital switchover dummy and age. I use as dependent variable the frequency of smoking. I show the average marginal effects of television for the different ages of children.

Figure B.11: Impact on Frequency of Arriving after 9pm

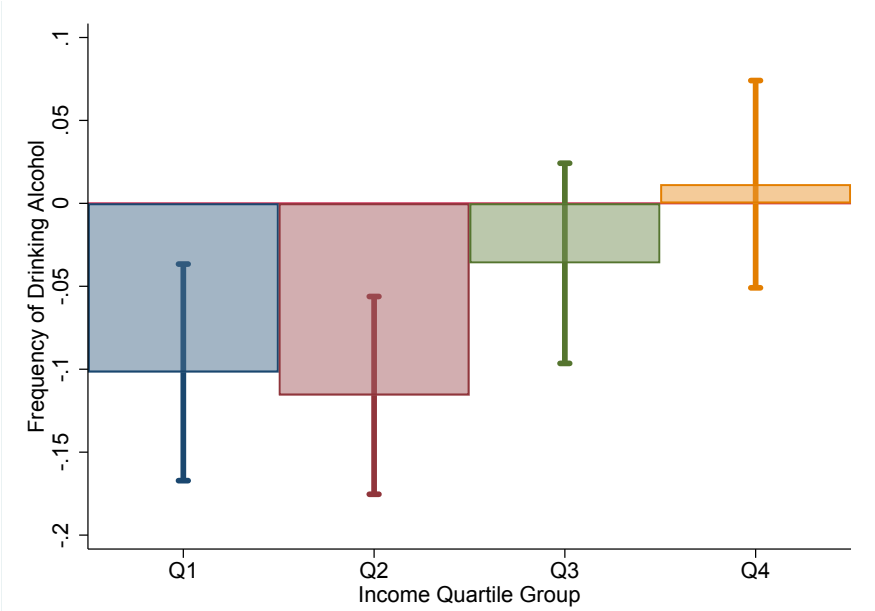


The figure estimates equation (3.3), but including as control an interaction between the digital switchover dummy and age. I use as dependent variable the frequency of arriving late at home in the last month. I show the average marginal effects of television for the different ages of children.

B.11 Heterogeneity in Income Using Frequencies as Dependent Variables

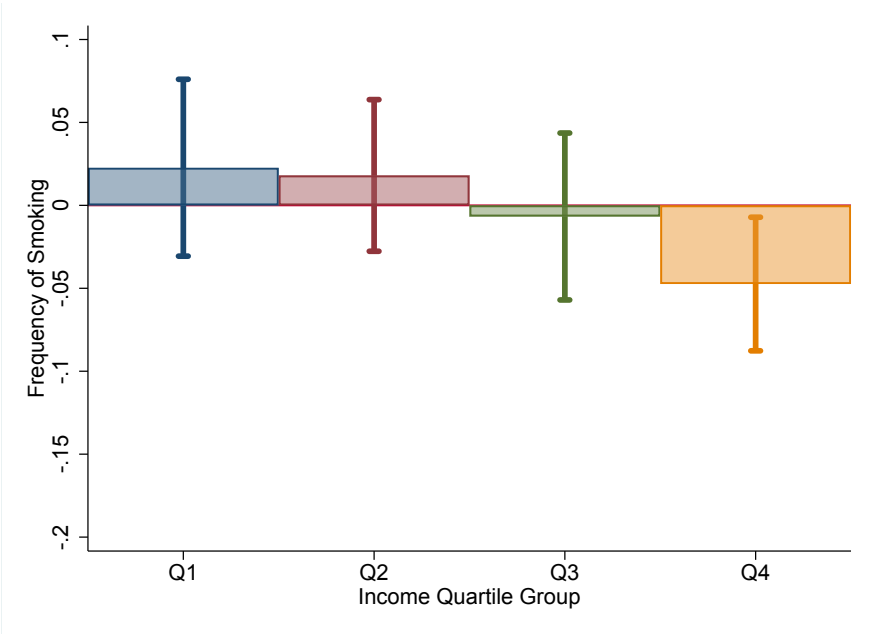
This section explores whether the effect of the digital switchover on the frequency with which children get involved in detrimental activities is heterogeneous in income. For this, I assign children in quartiles attending to their household income, and estimate a specification analogous to (3.3), where I also control for an interaction between the switchover indicator and the income quartiles. Figures B.12 - B.14 show the average marginal effects of television for the different income quartiles, respectively using as dependent variable the frequency with which children drink alcohol, smoke, or arrive late at home. First, I find that the digital switchover only reduces the frequency with which children drink alcohol when they have a poorer socioeconomic background. The estimates are significant at the 1% confidence level. Second, I show that the impact of television on the frequency with which children smoke is only negative when they live in richer households. The estimate is significant at the 5% confidence level for the fourth quartile of the income distribution. Finally, I show that television has no effect on the frequency with which children arrive late at home for any of the income quartiles.

Figure B.12: Impact on Frequency of Drinking Alcohol



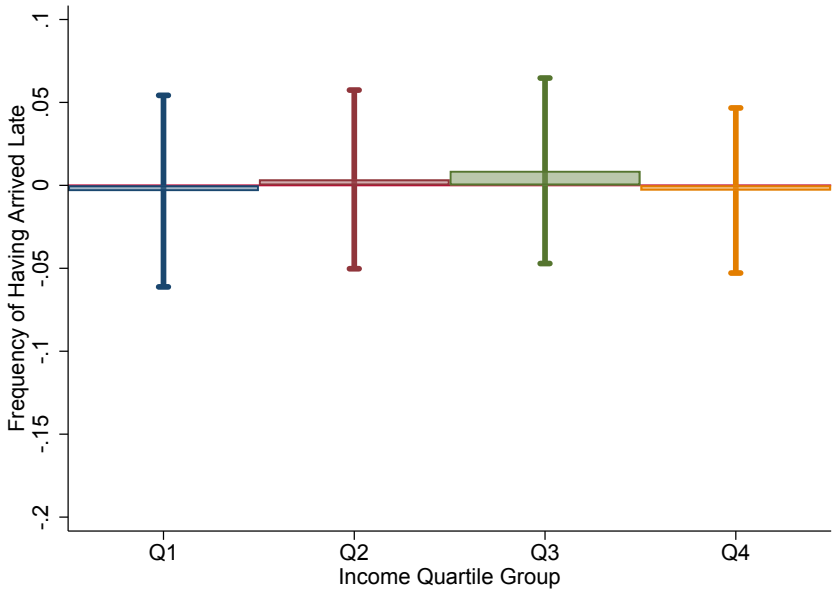
The figure estimates equation (3.3), but including as control an interaction between the digital switchover indicator and income quartiles, and using as dependent variable the frequency with which children drink alcohol. I show the average marginal effects of television for the different income quartiles of the households where children live.

Figure B.13: Impact on Frequency of Smoking



The figure estimates equation (3.3), but including as control an interaction between the digital switchover indicator and income quartiles, and using as dependent variable the frequency with which children smoke. I show the average marginal effects of television for the different income quartiles of the households where children live.

Figure B.14: Impact on Frequency of Arriving Late at Home

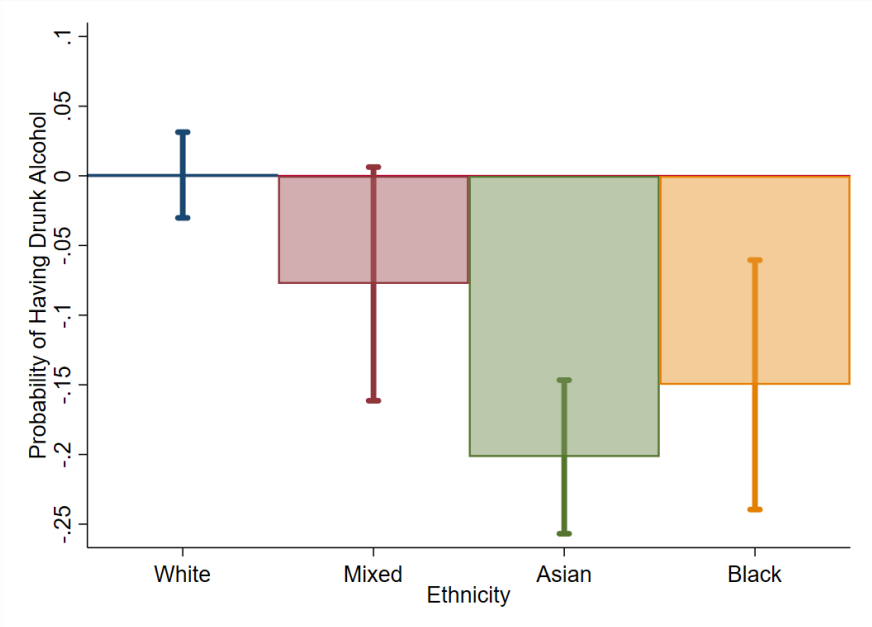


The figure estimates equation (3.3), but including as control an interaction between the digital switchover indicator and income quartiles, and using as dependent variable the frequency with which children arrived late at home in the last month. I show the average marginal effects of television for the different income quartiles of the households where children live.

B.12 Heterogeneity in Ethnicity

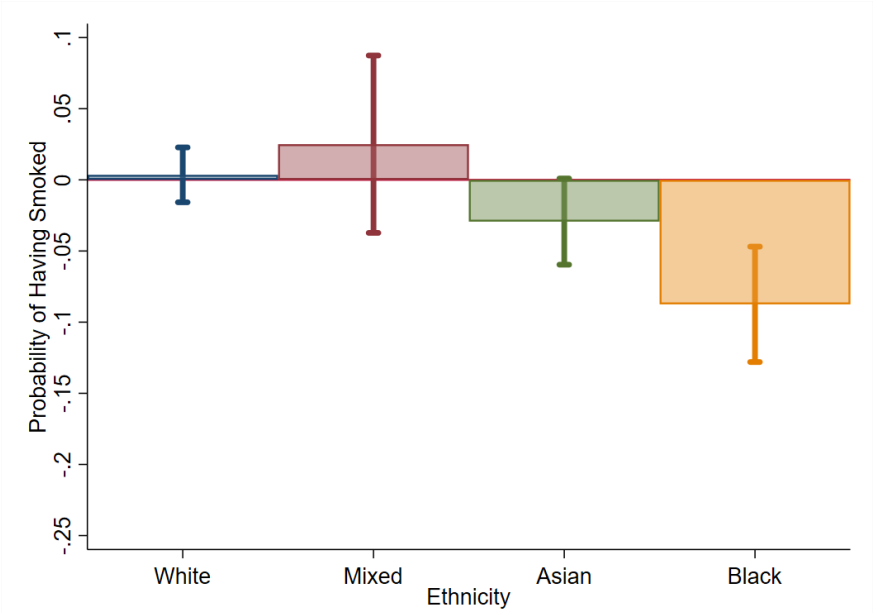
This section further explores whether the impact of the digital switchover on the probabability of getting involved in detrimental activities is heterogeneous in ethnicity. To do so, I first assign children into four groups: (i) white, (ii) mixed, (iii) asian and (iv) black. I then estimate my baseline specification but controlling for an interaction between the digital switchover variable and a set of dummies that indicate whether the child pertain to any of the previous four groups. Figures B.15-Figures B.17 respectively show the estimates when I use as dependent variable the probability of having drunk alcohol, smoked or arrived late at home in the last month. As shown, the effect of television on time use is driven by asian and black children.

Figure B.15: Impact on Probability of Having Drunk Alcohol



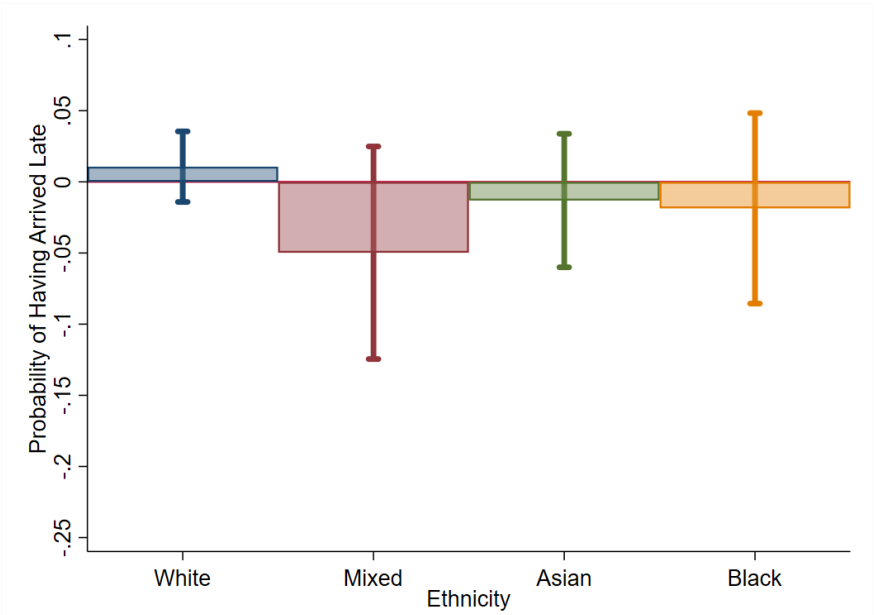
The figure estimates equation (3.3), but including as control an interaction between the digital switchover indicator and a set of dummies for the ethnicity of children. I use as dependent variable the probability of children having drunk alcohol. I show the average marginal effects of television for the different ethnicities.

Figure B.16: Impact on Probability of Having Smoked



The figure estimates equation (3.3), but including as control an interaction between the digital switchover indicator and a set of dummies for the ethnicity of children. I use as dependent variable the probability of children having drunk alcohol. I show the average marginal effects of television for the different ethnicities.

Figure B.17: Impact on Probability of Having Arrived Late at Home



The figure estimates equation (3.3), but including as control an interaction between the digital switchover indicator and a set of dummies for the ethnicity of children. I use as dependent variable the probability of children having drunk alcohol. I show the average marginal effects of television for the different ethnicities.

Chapter 4

Conclusions

This thesis has given new insights about the determinants of fertility and education by adopting causal inference analyses and large administrative datasets. In this chapter, I summarize my results, focusing on the contributions that this thesis makes to prior literature.

Fertility and education are two fundamental pillars for the prosperity of any society. This may explain the substantial research that has been done to study the determinants of these, and the concerns they have always raised. First, most of the developed countries have experienced low fertility rates during the last decades, raising concerns about future economic growth, the sustainability of their pension systems, or future fiscal balance. Understanding the drivers of low fertility is therefore essential. This thesis explores one of the possible factors having an impact on fertility, which is the presence of dual labour markets where outsiders suffer from low job security and career prospects, and insiders enjoy a high job protection and career training. Second, education is crucial for human capital accumulation, and so, for future economic growth. Many factors may play a role on the academic performance of children, and television is likely to be one of these. The reason is that television is one of the most important activities in the daily life of children. This thesis explores the importance of television as a determinant of educational performance, and whether this impact depends on

the educational value of the alternative activities that television replaces.

What are the main contributions of this thesis? The second chapter of this thesis explores the first of the aforementioned pillars, and gives evidence on the causal effect of permanent employment on fertility. To do so, I use variation in subsidies provided by the different regional governments in Spain to employers that transform a temporary contract into a permanent one as an instrument for permanent employment. The analysis is based on an administrative individual-level panel dataset containing yearly information on the labour and fertility outcomes of 1.1 million individuals. After accounting for endogeneity, I provide a novel set of results. I show that permanent employment has a positive impact on the fertility decision of males, but a negligible effect for females. I next explore a plausible mechanism as to why the estimates differ by gender, which is the opportunity cost of having a child. I do so by estimating whether the effect of permanent employment on fertility is lower when individuals are more likely to have a higher opportunity cost of having a child. Several findings suggest that this is the case. I find lower estimates of permanent employment when individuals who do not cohabit with a partner, have a high wage growth, or are young.

It is important to account for two caveats. First, the second paper of this thesis provides causal evidence on the impact of the labour market on fertility in the short-term. Changes in the immediate fertility behaviour of individuals may have a long-lasting effect on the number of children they end up having. However, this paper does not examine a long-term impact due to data limitations. A possible extension to this article is therefore to analyse the effect of permanent employment on the complete fertility of individuals. Second, I provide evidence on the impact of permanent contracts on fertility during the period 1997-2004 in Spain. Whereas the estimates may be valid in the context of Southern European countries, a possible extension to the analysis is to study whether the findings hold in a different labour market context or period. The third chapter of this thesis studies the impact of television on academic achievement and educational inequality, and whether the previous effects are due to the im-

pact that television has on the time use of children when they are not at school. I provide causal estimates by using a natural experiment that consists in the staggered introduction of the digital television signal in England. On the day when this process took place in a region, the number of channels that could be watched through terrestrial television multiplied by 8, and this increased the average television viewing time by 34 minutes per day. Using web scraping, I collect information on when did the digital switchover occur at the different post-code units. I adopt a difference-in-difference specification, and find that a rise in television viewing time has a positive impact on the academic performance of children. The effect is higher when schools are performing at the bottom of the score distribution or have a greater proportion of economically disadvantaged students. This reduces educational inequality. I next explore a possible mechanism behind my baseline estimates, and show that television reduces the time that children dedicate to out-of-school activities that may harm their academic performance, such as alcohol drinking.

The third paper of this thesis is subject to a number of possible extensions. First, the article studies the short-term impact of television on the academic outcomes of students. This is important for human capital formation and may have an effect on the future labour outcomes of children. However, the paper does not provide evidence on whether the impact on academic performance is long-lasting or whether it has further implications on the labour market. Further research may examine these. Second, the paper focuses on the effect of television on education for children aged 10-11. Possible extensions of this analysis may examine whether television also has an impact on education for students of different age ranges.

More generally, research on fertility and education can be delicate: Many are the factors that determine these outcomes, and these are frequently correlated. Providing causal estimates is therefore necessary, as otherwise bias is likely to be large. Recent evidence is increasingly helping to understand the true effect of the determinants of fertility and education. This thesis contributes towards this goal. I show causal estimates on the impact of permanent employment on

fertility and on the effect of television on education and time use. This, together with future evidence, may help policy makers to design labour and educational policies.