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All about the money ? The gendered effect of education on industrial and occupational sorting

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Résumé : En utilisant la réforme du système éducatif de 1972 au Royaume-Uni comme une expérience naturelle, cet article isole l'effet de l'éducation sur les choix de secteurs et de métiers à l'âge adulte. Nous montrons que les probabilités de travailler dans l'administration publique ou d'avoir un métier non-manuel augmentent avec le niveau d'éducation pour les hommes. Pour les femmes, plus d'éducation se traduit par de plus grandes chances de travailler dans les secteurs de l'éducation et de la santé. Nous expliquons les plus grandes probabilités de travailler dans les secteurs de l'administration publique, de la santé et de l'éducation par un changement des préférences des travailleurs : lorsque le niveau d'éducation des travailleurs augmente, ces derniers attachent une plus grande importance à des caractéristiques non-pécuniaires de l'emploi telles que le sentiment d'être utile pour la société.

Mots-clés : Réforme de 1972, retour de l'investissement éducatif, sélection des travailleurs, choix de carrière, préférences non-monétaires

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Abstract : Using the UK 1972 compulsory education reform as a natural experiment, we isolate the effect of education on occupational and industrial sorting. More education leads to greater probabilities of working in the public administration and non-manual occupations for men and in the health and education industries for women. We find that men may shift towards non-manual occupations to work in high-paying jobs. In contrast, men may relocate into the public administration and women into the health and education industries because more educated workers place more importance into non-pecuniary job dimensions. These gender differences may be widening the gender wage gap.

Keywords : 1972 reform, returns to education, worker sorting, career choices, non-pecuniary preferences

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1 Introduction

The number of years of education that individuals complete has important implications for a wide range of later-life outcomes such as civic behaviour (Milligan et al., 2004), health (Groot and Van Den Brink, 2007; Plotnikov et al., 2020), criminality (Machin et al., 2011) or social capital (Huang et al., 2009). The educational level of individuals can also have important effects on their labour market outcomes, and most evidence has focused on the study of the impact of schooling on wages. For example, prior evidence has shown that a higher number of years of education raises earnings (Angrist and Krueger, 1991; Oreopoulos, 2006; Devereux and Hart, 2010; Grenet, 2013), and decreases earnings volatility (Delaney and Devereux, 2019). The extensive focus on the importance of education for wages could be because the only job characteristic that matters in the standard models of labour supply is the wage rate, and probably for the same reason, little is still known on whether education matters for job characteristics other than earnings.

But are wages the only job characteristic that matters for workers? Although they differ on many aspects, past contributions that investigated this research question have always come to the same conclusion: not only the wage is not the sole characteristic that matters for workers, but it is neither the most important. According to Clark (2010), 50 to 60% of individuals consider that job security and having an interesting job are "very important" job characteristics, while having a high income job only matters for 20% of individuals. In a similar vein, the most important job characteristic for British workers in Clark (2001) is job security; and while after job security, men give importance to the pay, women still attach more importance to independence, hours of work and the work itself than to wages. Consistent results are found by Leontaridi and Sloane (2004), who show that job content is the most important characteristic for individuals. Lastly, Lepinteur (2019) has assessed the influence of exogenous working time reductions and wage rate rises following labour market reforms in Portugal and France on job satisfaction, showing that only the former had a significant impact. Overall, job dimensions other than wages, such as job security, flexibility or job content, appear to be more important for workers than the pay.

This paper identifies the impact of education on industrial and occupational sorting, and provides evidence on the type of worker preferences driving these effects. To do so, we use the 1972 education reform in the UK as a natural experiment in a regression discontinuity design. This reform, which took place in September of 1972, increased the minimum school-leaving age from 15 to 16 years old. Subsequently, individuals who had been born before September of 1957 had the obligation to remain at school until the age of 15 years old, while those who had been born after had to stay at school until the age of 16. Previous evidence has shown that the 1972 education reform in the UK increased the age at which individuals left education, their earnings (Grenet, 2013), and decreased earnings volatility (Delaney and Devereux, 2019).

Our empirical analysis is based on two different datasets. The first is a large dataset of more than a quarter million individual–quarter observations that provides rich information on labour and socio-demographic characteristics and which we assemble from 32 Quarterly Labour Force surveys (QLFS) in the UK between 1993 and 2000. This dataset allows us identifying the effect of the 1972 education reform on industrial and occupational sorting. The second dataset, the International Social Survey Programme (ISSP), provides unique information on individual preferences in terms of job characteristics and working conditions. We use the four waves of the survey available that were fielded between 1989 and 2015.

We first show that the 1972 education reform in the UK led to an increase of 23 percentage points in the probability of leaving school after the age of 15. In turn, our results suggest that this exogenous increase in education affected industrial and occupational sorting. More specifically, we find that the workers who received more education following the 1972 education reform were more likely to work in the public administration, in health and education industries and in non-manual occupations. The effects we find are sizeable since they represent increases of respectively 12%, 6% and 3% of the baseline probabilities. The greater sorting into public administration, and health and education industries mirrors a greater likelihood to work in the public sector. These average effects also hide strong gender differences: the greater likelihood of sorting into the health and education industries is only found among women while only men report a higher probability of working in the public administration and in non-manual occupations. Our results are robust to using different (i) estimators, (ii) regression discontinuity parameters, (iii) bandwidths and (iv) units of analysis, and (v) they vanish when we implement placebo tests.

Although finding that greater education affects industrial and occupational sorting is an interesting fact *per se*, one may claim that these changes in sorting are motivated by pecuniary motives (as in Fischer et al., 2019). In such case, our findings and the extant literature documenting the positive impact of education on earnings would be the two sides of the same coin. We argue that it is not the case for several reasons. First, we do not find a positive effect of education on earnings for women in our estimation sample. Consequently, the shift we observe towards the health end education industries has to be motivated by a different reason. Our analysis of workers preferences reveals that more educated women give less importance to earnings and value more the social dimensions of their job (such as being useful for society and being able to help others). Those changes in workers preferences are consistent with the effects on industrial sorting of women. As for men, we do find a positive impact of education on earnings and the selection into non-manual occupations (that are also high-paying jobs on average) explains to a certain extent this increase in earnings. However, we show that the selection of men into the public administration does not contribute to greater earnings, and therefore, we postulate that this impact may be driven by non-pecuniary motives. Our analysis of workers preferences confirms our hypothesis: more educated men are more likely to say that job security and being useful to society are important job features. We conclude that changes in non-pecuniary preferences induced by a greater level of education may partly explain the shift of more educated individuals across industries and occupations.

Our paper contributes to two main strands of the literature. First, it adds to the literature on the effect of education on the labour market. The first studies of this kind showed that more education leads to higher earnings using the quarter of birth of individuals (Angrist and Krueger, 1991), changes in compulsory education laws (Harmon and Walker, 1995), distance to college (Card, 1993), and gender of siblings (Butcher and Case, 1994) as instruments for education, as well as using samples of twins and family relatives to control for family characteristics (Ashenfelter and Krueger, 1994; Ashenfelter and Zimmerman, 1997). More recent studies have also compared countries where the proportion of individuals affected by compulsory education reforms differs to evaluate the validity of these laws as instrumental variables and provided causal evidence on a positive impact of education on earnings using compulsory education reforms in a regression discontinuity approach (Oreopoulos, 2006; Devereux and Hart, 2010; Grenet, 2013). In addition, recent studies have measured more accurately instrumental variables methods based on compulsory schooling reforms (Dolton and Sandi, 2017) and used new instrumental variables such as early smoking (Dickson, 2013) to explore returns to education. Finally, Delaney and Devereux (2019) show that more education also decreases earnings volatility. Our paper contributes to this literature by showing that returns to education can go beyond earnings. In particular, we consider the interplay between education, worker sorting across multiple dimensions (industry and occupation) and workers preferences regarding job characteristics.

Second, we contribute to the literature that evaluates the effect of public policies aiming to improve the educational system and future labour outcomes. For example, previous evidence has shown that an increase in school resources and spending (Garces et al., 2002; Deming, 2009; Johnson, 2011; Jackson et al., 2016; Carruthers and Wanamaker, 2017; Baker, 2019; Johnson and Jackson, 2019; Schmick and Shertzer, 2019), rise in the number of high value-added teachers (Chetty et al., 2011, 2014), unrestricted school choices programmes (Lavy, 2015), higher school accountability (Deming et al., 2016), smaller class sizes (Card and Krueger, 1992; Chetty et al., 2011; Fredriksson et al., 2013), teacher performance pay (Bond and Mumford, 2018; Lavy, 2020), financial aid (Bettinger et al., 2019) and longer academic terms (Card and Krueger, 1992; Fischer et al., 2020) improve educational attainment and lead to higher future earnings. In contrast, teaching bargaining laws reduce future earnings (Lovenheim and Willén, 2019). We contribute to this literature by showing that reforms that increase the number of compulsory schooling years are not only important determinants of workers' earnings but also of career choices.

The remainder of the paper is organised as follows. Section 2 explains the identification strategy and Section 3 presents the QLFS and ISSP datasets, which we use in the analysis. Section 4 discusses the results on the effect of education on worker sorting. Plausible mechanisms are discussed in Section 5. Section 6 concludes.

2 Institutional Context and Empirical Strategy

In the UK, children aged 5 or older need to attend compulsory education for a number of academic years, which start at the beginning September and end at the end of June. The number of compulsory schooling years has varied over the last decades, and one of the biggest changes took place in 1972, when an important education reform came into place simultaneously in England, Scotland and Wales, which increased the minimum school-leaving age for children from 15 to 16 years old. The reform, which had been in preparation since 1964, was enacted on the 1^{st} of September of 1972, requiring students to take 6 years of compulsory primary education, and at least 5 years of secondary compulsory education, after which they would be aged 16 and able to voluntarily choose whether to stay in education or not. Before 1972, the compulsory school age had remained constant over a long period, as the last time when it had been changed was in 1947, when it was raised from 14 to 15.

The main implication of the 1972 Raising of the School Leaving Age (RoSLA) reform was therefore that children who had been born before the 1^{st} of September of 1957 needed to attend school at least until the age of 15 years old, while children who had been born after needed to attend school until the age of 16. This generates a convincing quasi-natural experiment, as individuals who were born right before or after the 1st of September of 1957 should have similar socio-demographic characteristics on average, but are subject to very different compulsory education ages.

We accordingly use the 1972 RoSLA reform in the UK as a quasi-natural experiment to isolate the effect of an exogenous increase in education on worker sorting. To do so, we take advantage of the date-of-birth discontinuity generated by the reform using the following Regression Discontinuity Design (RDD):

$$Y_{i,t} = \alpha Treat_i + \beta_1 f(C_i - c) + \beta_2 Treat_i * f(C_i - c) + \Omega X_{i,t} + g(t) + \epsilon_{i,t}$$

where $Y_{i,t}$ is the outcome of interest of individual i at quarter t. First, we explore the effect of the reform on a dummy that takes a value of one if the individual leaves school after the age of 15, and zero otherwise. This allows us confirming that our empirical design is accurate in isolating the effect of the 1972 RoSLA reform. Then, we examine the effect of the compulsory education reform on industrial and occupational sorting. In this article, the definitions of industries and occupations are based respectively on the Standard Industrial Classification 92 (SIC92) and the Standard Occupational classifications 90 (SOC90). In Appendix A, we report the marginal effects of $Treat_i$ from a multinomial logit model that uses the ten mutually exclusive categories of industries (agriculture and fishing, energy and water, manufacturing, construction, distribution, hotels and restauration, transport and communication, banking, finance and insurance, public administration, education and health, and other services) as dependent variables. We do the same for the occupations. In our main analysis, we focus on the three following outcomes: working in the public administration, working in the health and education industries, and working in a non-manual occupation. They all take the form of a dummy in our analysis. These three outcomes have a particular importance. A greater likelihood of working in the public administration or in health and education industries may reveal that more educated workers are not only seeking higher wages but also industries where they can find a feeling of fulfilment and contribute actively to the societal welfare. Careers in these industries may also bring a greater job security because of the preponderance of the public sector. As for non-manual jobs, although they likely are high-paying occupations, one may also argue that they are jobs that are less physically-demanding and potentially more diverse, with a greater sense of independence. Overall, looking at those three particular aspects of industrial and occupational sorting gives the chance to assess whether more educated workers tend to favour non-pecuniary job characteristics.

Our explanatory variable of interest, $Treat_i$, is a treatment dummy that takes value 1 if individual i was born after September 1957, and 0 otherwise. $f(C_i - c)$ is a first order polynomial of the date of birth C_i of individual i

centered at the date of the reform was effective c (i.e. September 1957). In our sensitivity analysis, we show that the estimates are qualitatively similar when we use polynomials of higher orders. $X_{i,t}$ is a set of exogenous individual controls made of gender, a second order polynomial in age and a dummy for white respondents. $g(t)$ is a set of time fixed effects that includes year, quarter and individuals' questionnaire number dummies. Finally, $\epsilon_{i,t}$ is a time-varying error at the individual level.

$\hat{\beta}_1$ captures the effect of the distance between the date of birth and the date of the reform (i.e. September 1957) and $\hat{\beta}_2$ allows this effect to differ between the treated and control group. As with any RDD design, $Treat_i$ acts as an intercept and shows the effect of the discontinuity in compulsory schooling age created by the 1972 RoSLA reform. In other words, $\hat{\alpha}$ – our estimate of interest – is the treatment effect.

The identifying assumption of our RDD is that the respondents on both sides of the discontinuity are arguably similar, except for their compulsory ages of education. In such framework, the size of the bandwidth, i.e. the number of years before and after the cut-off, is key. The smaller the bandwidth, the more similar are the workers on both sides of the discontinuity. However, we need the bandwidth to be large enough to provide sufficient statistical power. We here appeal to Calonico et al. (2014) to determine the bandwidth to use in our main analysis. The optimal bandwidth choice procedures suggest to use a bandwidth of roughly 10 years, going from September 1952 to August 1962 (5 years before and 5 years after the cut-off - September 1957). We show in the robustness checks that we find similar conclusions when we use different bandwidths.

Our main model is estimated using Ordinary Least Squares (OLS). We use a tent-shaped edge kernel centered around September 1957 to give a larger weight to observations close to the cut-off. In our robustness checks, we demonstrate that our conclusions are not sensitive to the choice of the estimator and the weighting procedure. Last, standard errors are clustered at the individual level.

3 Data

3.1 Quarterly Labour Force Survey

We use data from 32 UK Quarterly Labour Force Surveys (QLFS) that were carried out between 1993 and 2000. The QLFS is the largest household survey in the UK, and provides detailed information on the employment, education and other socio-demographic characteristics of individuals who are generally interviewed in 5 consecutive quarters. Regarding educational outcomes, the QLFS contains data on the age when adults completed education as well as on their educational level. Regarding employment outcomes, the QLFS dataset provides information on labour earnings, labour market participation, the occupation (as measured by the SOC90) and the industry (as measured by the SIC92) of workers, among other characteristics. Using the SOC90 classification, the data providers derived a variable indicating whether an individual works in a manual or non-manual job. Although we use the major occupation groups as outcome variables in Appendix A, the "non-manual occupation" dummy is the main variable we use to measure occupational sorting in our analysis. We report in Table A3 the occupations that the data producers labelled as "manual" and "non-manual". As with the major occupational groups, we also use the major industry sectors of SIC92 as dependent variables in Appendix A and focus more specifically on the public administration, education and health industries in the main analysis. Finally, the QLFS provides data on socio-demographic characteristics of individuals such as their age, gender, ethnicity, marital status and number of children, among others.

Our sample consists of individuals born in England and Wales between September 1952 and August 1962 and for whom we can observe their education and labour market outcomes. As the analysis studies the effect of education on worker sorting, we focus on the sample of employed individuals with a valid date of birth. The month and year of birth in the UK QLFS being publicly available only from 1934 to 2000, this leaves us with a sample of more than 272,000 individual-quarter observations.

Table 1 presents summary statistics of our sample. In regard to labour characteristics, approximately 30% of individuals work in the health and education industries, and individuals are more likely to work in non-manual occupations and the private sector.[1] As to socio-demographic characteristics, there are as many men as women in our sample, individuals are 39.5 years old on average and 83% of the sample leaves school after the age of 15. Lastly, it is important to note that 53% of our sample is subject to the 1972 education reform as they were born after September 1957.

[1] Appendix C also presents summary statistics for our main outcome variables over the period of analysis. As shown, the age when individuals complete their full-time education, and their probabilities of working in the public administration, health or education industries, and in a non-manual occupation are stable over the period of analysis.

3.2 International Social Survey Programme

The second dataset we use in the analysis is based on the International Social Survey Programme (ISSP), which is a cross-national survey that has been ran on an annual basis since 1984. In 1989, 1997, 2005 and 2015, respondents were asked multiple questions regarding job preferences and attitudes towards work to compile information for the work orientations' modules of the dataset. These surveys therefore provide unique information on the job characteristics that workers consider more important, as well as on their actual job conditions, previous work experiences and job satisfaction, among other labour features. The modules also provide precise information on individuals' socio-demographic characteristics such as age, gender, country of origin, number of years of education, legal partnership status, household size, religious activity and health condition, among others. We do not impose any sample restriction in the part of the analysis where we use these 4 ISSP survey datasets other than basing the analysis on individuals living in the UK with valid dependent and independent variables to run our RDD regressions. This leaves us with a sample of more than 1,500 individual-year observations.

4 Main Results

4.1 Graphical Analysis

We start by providing graphical evidence on the discontinuities on the school leaving age and labour market outcomes between treated and untreated individuals using our estimation sample based on QLFS data. Figure 1 shows the linear fits on each side of the cut-off with 6-months-of-birth bins separately for men and women.[2] There is a sharp increase in the probability of leaving school after the age of 15 for those born from September 1957 onwards. This discontinuity has been already extensively documented by the literature (Grenet, 2013; Delaney and Devereux, 2019) and it confirms that the 1972 compulsory education reform that the UK undertook was effective. Unsurprisingly, there is no gender difference in Figure 1.

Figures 2 and 3 consider the probability of working in the public administration and in the sector of health or education, respectively. We observe gender differences here. Men born after August 1957 are more likely to work in the public administration while women are more likely to have a job in the health or education sectors. Moreover, Figure 4 shows that men whose compulsory schooling age is 16 years old are more likely to sort into non-manual occupations.

4.2 Regression Results

To complement and verify the robustness of the graphical findings above, we now provide a regression analysis of the effect of the 1972 RoSLA reform on the probability of leaving school after the age of 15 and workers' industrial and occupational sorting based on our RDD equation. The main results are displayed in Table 2. There are three panels: one for the whole sample, one for men only and one for women only.

In columns (1) and (2), we report the treatment effects on education (respectively without and with the exogenous controls). Our estimates suggest that the reform increased the probability of leaving school after the age of 15 between 21.3 and 24.6 percentage points. The estimates are statistically

[2]Bins of different sizes produce similar results. Figures are available upon request.

significant at the 1% confidence level. These are large effects since they are approximately equal to an increase of 33% of the average baseline probability of leaving school after the age of 15. The effects are somewhat larger for women: this is because the average baseline probability for the untreated women was lower than that of untreated men.

Looking now at columns (3)–(8) and top panel of Table 2, it appears that the 1972 RoSLA reform increased significantly the probabilities of individuals sorting into the public administration, the health and education sectors and non-manual occupations. All the estimates are statistically different from zero at the 5% level at least. The magnitude of the treatment effect varies across outcomes. It is equal to an increase of roughly 12%, 6% and 3% of the baseline probabilities of working in the public administration, in the health and education sectors, and in a non-manual occupation, respectively.

Although the inclusion of the exogenous controls has no effect on the treatment effects in the first two columns of Table 2, we notice that the treatment effect somehow diminishes in columns (6) and (8). The exogenous control that is responsible for these changes is our *'female'* dummy, suggesting that the treatment effect is not orthogonal to gender. This is why we re-estimate the treatment effects separately by gender in the middle and bottom panels of Table 2. The controls have no longer an effect on the main estimates, confirming the arguably randomness of the treatment. The results are in line with what we observed in the graphical analysis: patterns of industrial and occupational sorting in response to greater education are different across gender. More educated men are more likely to work in the public administration and non-manual occupations while more educated women have a greater likelihood of working in the health and education sectors.[3]

4.3 Robustness

We now implement a battery of sensitivity tests in order to provide further evidence on the robustness of our identification strategy. Given that the effect of the 1972 RoSLA reform on education has been extensively documented in prior studies and to ease the readability of our sensitivity results, we focus our robustness analysis on labour market outcomes. Yet, we performed all the robustness tests using the probability of leaving school after the age of 15 as dependent variable and the results are available upon request.

The choice of the bandwidth in a RDD is key and it can significantly affect the treatment effect. We check the sensitivity of our results by using different time windows. Results when we use windows ranging from three to seven years around the discontinuity are reported for each outcome in Figure 5. All the treatment effects that are significant in our baseline framework (five-years window) are also significant with the other bandwidths. More importantly, the magnitudes of the estimates remain remarkably stable across specifications. We do not report the results when we use windows of one, two, eight, nine and ten years to ease the reading but the estimates we obtain when using these other bandwidths are similar to those in Figure 5.

[3] Rather than splitting the estimation sample on the basis of gender, we could have used the whole sample and interact our treatment effect with a *'female'* dummy. When we do so, the interaction terms are always significant at least at the 10% level. These significant interaction terms confirm that the changes in industrial and occupational sorting following an exogenous increase in education differ across gender. A further interesting consideration is whether the baseline estimates originate from the 1972 education reform providing incentives to individuals who would otherwise be unemployed or inactive to take jobs in particular industries or occupations. We explore this possibility in Appendix D, which suggests that this is not the case.

Figure 6 displays the treatment effects when we use different polynomial orders for $f(C_i - c)$. In our baseline analysis, we use a polynomial of order one. The results remain qualitatively similar when we use a polynomial of order two and three. Using polynomials of orders higher than three produces estimates that are also similar to our baseline results although less precisely estimated.

We next assume that the 1972 education reform was implemented 5 years before. In other words, we run a placebo test where we consider that the children born after August 1952, instead of August 1957, had to stay at school until the age of 16. Table 3 show the results. As expected, none of the estimates is statistically different from zero at conventional levels. More importantly, the absence of statistical significance mostly comes from the magnitude of the estimates that are all close to zero.

Table 4 reports the results for a further set of robustness tests. We present the baseline estimate in the first column. In column (2), we use a rectangular kernel rather than a tent-shaped kernel. In column (3), we use only one observation per individual, and in column (4) we report marginal effects from a logit regression. None of the aforementioned specifications yields to estimates that are significantly different from the baseline ones.[4]

Lastly, the 1972 compulsory education reform can be used as an instrumental variable for education in a Two-Stage Least Squares (2SLS) regression. The last column of Table 4 shows the causal effect of the instrumented probability of leaving school after the age of 15 on every labour market out-

[4]As an additional robustness test, we examine whether the 1972 education reform changes pre-determined characteristics that should not be affected by it. We present the results in Appendix E, and show that pre-determined characteristics were unaffected by the reform.

come. In line with our baseline estimates, column (5) suggests that more educated men sort into public administration and non-manual occupations while more educated women sort into health and education industries.

5 The role of education and workers' preferences

5.1 Is It All about Money?

Overall, our main results show that a higher number of years of education has an important effect on industrial and occupational sorting in that it increases the probabilities of working in the health and education industries for women and in the public administration and non-manual occupations for men. The objective of this section is to understand why education affects industrial and occupational sorting in the first place, and why it does so differently across gender.

As revealed by our descriptive statistics in Table 1, men are more likely to have a manual occupation. This could be explained, among other things, by the fact that manual occupations are more physically demanding. "Manual" jobs (following the definition of the QLFS) also require less education and are, on average, less well-paid. Observing a shift of more educated men towards non-manual occupations is then not surprising: they may simultaneously value the higher wages, the lower exposure to physically demanding tasks and the match between their education and the skills required by their job. Focusing more particularly on wages as outcome variable, we show that the estimate of the treatment effect of the 1972 compulsory education reform presented in columns (1) and (4) of Table 5 is only positive and statistically significant for men (an increase of roughly 5%). This confirms that the occupational sorting we observe for more educated men might somewhat be motivated by the possibility of getting higher wages.

But is money the key component of the sorting choices of more educated workers? The industrial sorting we observe in Table 2 may reveal different motives. Careers in the public administration and the health and education industries are almost synonymous of careers in the public sector. In our estimation sample, the correlation between working in one of the three aforementioned industries and in the public sector is of 0.74. If we re-estimate our baseline regressions using "working in the public sector" as dependent variable, we find positive and statistically significant treatment effects for both men and women. Working in the public administration or in the health and education sectors does not come with better wages. Coming back to Table 5, we see that more educated women do not get a better hourly wage. To understand whether the positive treatment effect of education on wages for men is due to industrial or occupational sorting, we use the decomposition of Gelbach (2016). The results in columns (2) and (3) confirm that around half of the greater hourly wages received by more educated men comes from the choice to have a "non-manual" job. In contrast, working in the public administration does not contribute to the wage effect.

5.2 Looking for the Non-Pecuniary Dimensions of a Job?

The greater likelihood of more educated individuals working in the public administration and the health and education industries can be due to two potential explanations. The first comes from the demand side of the labour market: employers in those industries may need workers with relatively high levels of education and, hence, the 1972 RoSLA reform would met their demand. The second potential explanation comes from the supply side and, more specifically, changes in workers' preferences in terms of job characteristics in response to greater levels of education. Standard microeconomics models consider that the wage is the only characteristic that defines a job. However, an extensive literature already demonstrated that the pay is not the most important job dimension for workers (Clark, 2001; Leontaridi and Sloane, 2004; Clark, 2010; Lepinteur, 2019). The rankings vary across datasets, countries, periods and gender but, overall, job security and job content are often more important characteristics for workers than the wage. If a higher educational level increases the weight that workers give to non-pecuniary job characteristics, this could also explain the increase in the probability of more educated individuals working in the public administration and health and education industries that we observed in our main results.

Under the assumption that there is no gender discrimination (which is likely the case since we are considering jobs that are mostly in the public sector) and provided that the rise in education was somewhat the same for men and women, the demand-side explanation is not sufficient to explain the differences we find across gender in terms of worker sorting. And therefore, given the similar level of education among the treated workers, why are women more likely to choose jobs in the education and health industries, and why do men choose a job in the public administration more frequently?

The QLFS does not contain information on workers' preferences. This is why we turn to ISSP data to address these questions.[5] We replicate our main analysis using various worker's preferences as dependent variables and report the results in Table 7. The top panel of the table reveals that these preferences are affected by education. More educated workers are less likely to report that a *"job is just for the money"* and are more likely to respond that job security, independence, helping other people and being useful for society are *"very important"* job characteristics. This is very consistent with the shift in industrial sorting displayed in Table 2: the public administration, health and education industries arguably bring feelings of fulfilment, and of being useful and helpful for society. Since these industries also belong to the realm of public sector jobs, they also provide job security.

The middle and bottom panels of Table 7 display gender heterogeneity. For men, more education only increases the importance they allocate to job security and being useful for society. For women, we observe a different pattern: more educated women consider that money is less important and that independence, helping other people and being useful for society are relatively more important in a job. These differences across men and women may therefore also partly explain the gender differences we found in our baseline results on worker sorting. Men are on average more likely to say that money matters and this gap seems to widen with education. This is

[5] Descriptive statistics of this estimation sample are shown in Table 6.

consistent with the fact that only more educated men work in occupations where they can earn a greater hourly wage. By also choosing career in the public administration, men can meet their greater demand for job security and being useful for society. The increase in the importance that workers place into being useful for society due to the 1972 RoSLA reform is the same for women. Nevertheless, women put on average more importance on this job dimension. Moreover, the importance of having a job that helps other people and of being independent increases with education only for women: this certainly explains to a certain extent why more education only leads to women choosing careers in the education and health industries more frequently.

6 Conclusions

This paper examines the causal effect of education on industrial and occupational worker sorting using as a natural experiment the compulsory schooling reform that took place in the UK in 1972 in a regression discontinuity approach. This reform increased the minimum age at which students could leave compulsory education from 15 to 16 years old. Subsequently, individuals who were born before September 1957 had to attend school until the age of 15 at least, while individuals who were born after September 1957 had to remain at school until the age of 16 at least.

We combine this information with two datasets to implement our analysis. First, we use a large dataset of a quarter million individual–quarter observations based on 32 Quarterly Labour Force Surveys (QLFS) which provide detailed individual labour information. Second, we use a dataset based on 4 surveys of the International Social Survey Programme (ISSP), which provide unique information on workers' attitudes and preferences.

Using the QLFS dataset, we show that the 1972 compulsory schooling reform increased the probability of leaving school after the age of 15 by 23 percentage points. This exogenous increase in education had an important effect on industrial and occupational worker sorting. In particular, we show that the reform increased the probability of the treated cohorts to work in the public administration, in the health and education industries and in non-manual occupations. We also find important gender differences in these effects: a greater level of education for men increases their probability of working for the public administration and in non-manual jobs while more educated women are more likely to sort into the health and education industries only.

We then explore whether our findings on occupational sorting originate from pecuniary motives. First, we provide evidence on more education leading to higher hourly wages for men but not for women, which suggests that, if any, our findings on worker sorting may be driven by the pay only for men. We provide further evidence on whether this is the case using a Gelbach decomposition. Here, we show that we cannot reject that the sorting of men into non-manual occupations is motivated by greater wages. However, we find that the changes in industrial sorting (public administration for men and health and education industries for women) cannot be explained by pecuniary motives. Having shown that money can at best explain part of the worker sorting decisions following an increase in the number of years of education, we turn to explore the role played by non-pecuniary motives. To do so, we use the ISSP, and exploit its rich information on workers' attitudes and preferences. We show that more educated male workers value more job security as well as the feeling of being useful for society, which may explain why they choose more frequently careers in the public administration. As for women, we show that a greater level of education reduces significantly the importance they give to wages and increases the importance they place into non-pecuniary job characteristics such as helping others or being useful for society through their job. This is consistent with the fact that women affected by the 1972 education reform are more likely to sort into the health and education industries and that their hourly wage is similar to that of untreated women.

Our results have several important implications. First, they show that earnings should not be the only outcome to be considered to measure the quality of a job. Second, they show that workers' pecuniary *as well as* non-pecuniary preferences change with education, which, in turn, has important implications for individual labour choices. Lastly, our conclusions may, at least partly, explain the co-existence of important gender wage gaps in societies where the number of years of education has been continuously rising over the last decades for both men and women. If education affects workers' preferences and sorting differently across gender, this may partially explain why gender inequality may remain in the labour market.

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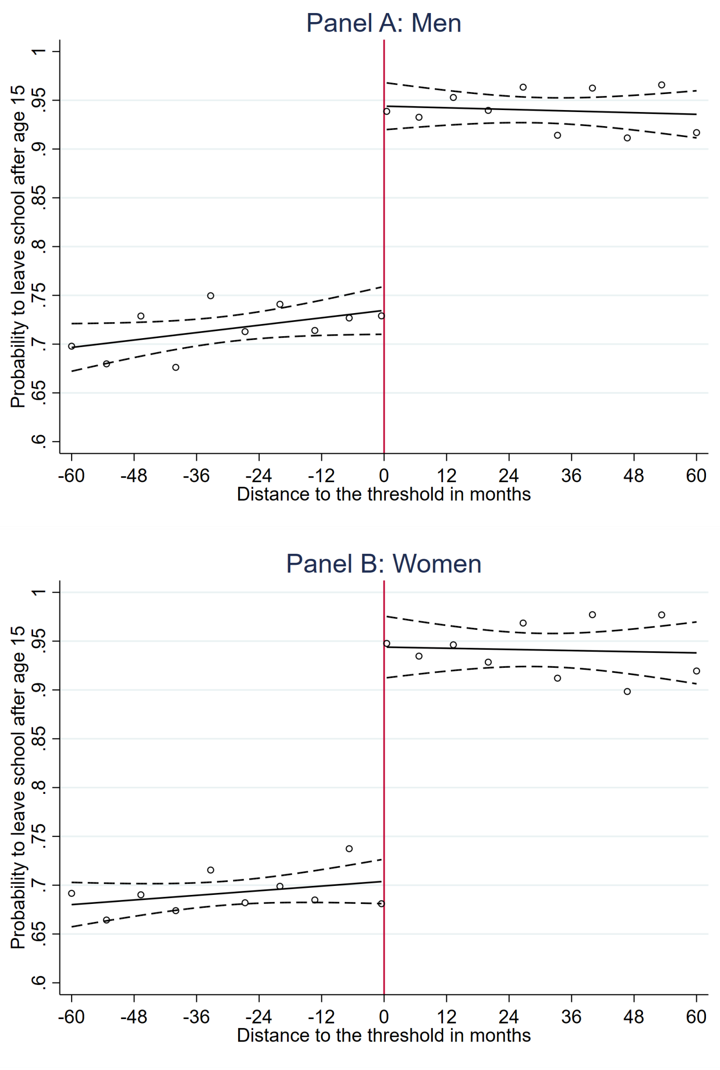
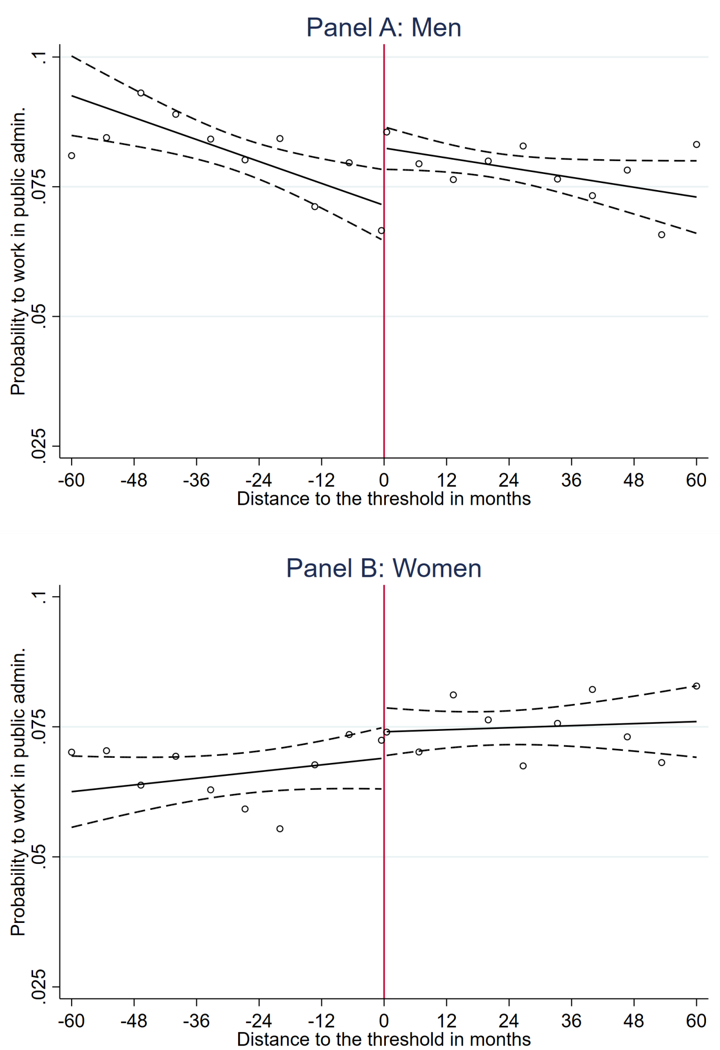
Figures

Figure 1: School-leaving age above 15 - RDD graph



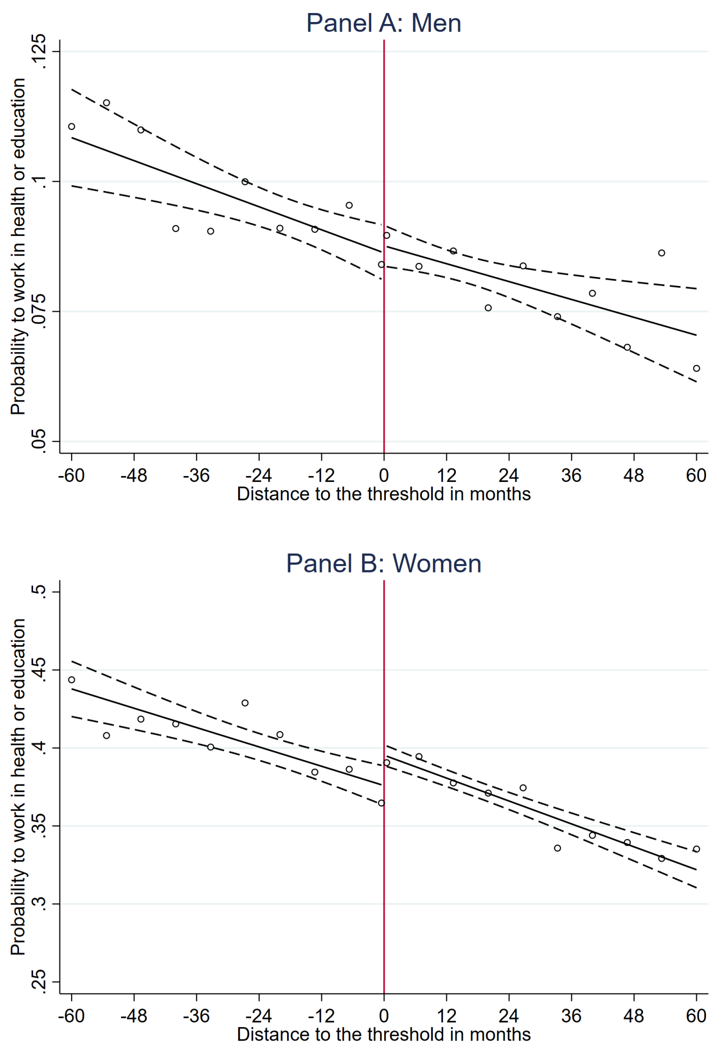
Notes: These figures refer to the wage earners born between September 1947 and August 1957. Panels A and B of the figure show linear fits on each side of the cut-off with 6-months-of-birth bins for men and women, respectively.

Figure 2: Working in public administration - RDD graph



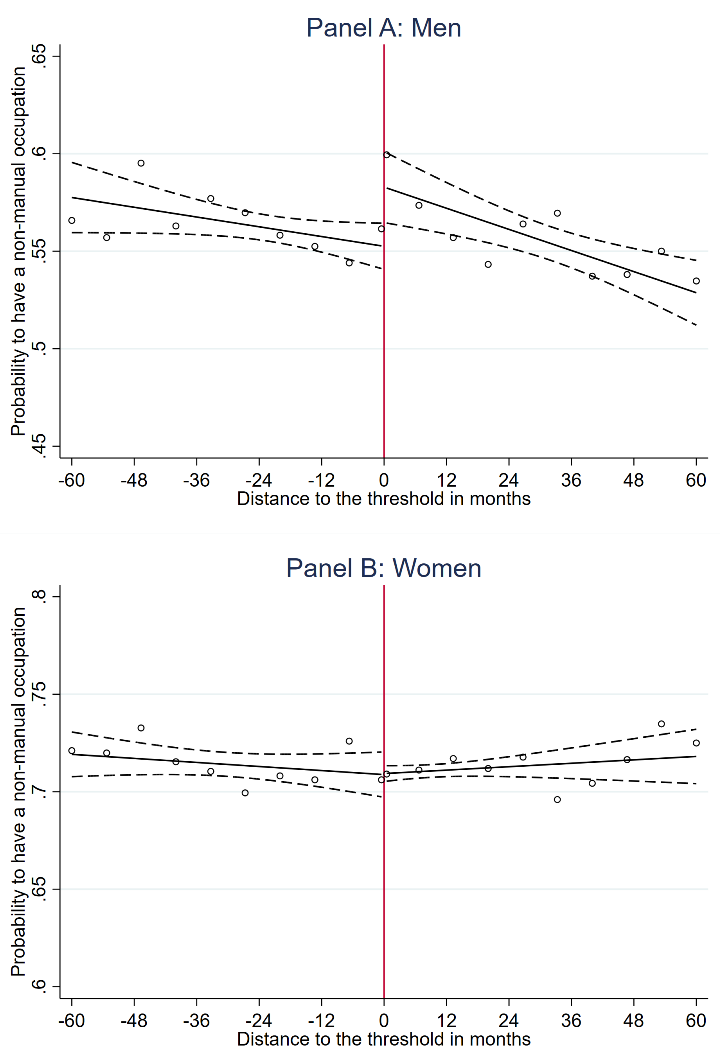
Notes: These figures refer to the wage earners born between September 1947 and August 1957. Panels A and B of the figure show linear fits on each side of the cut-off with 6-months-of-birth bins for men and women, respectively.

Figure 3: Working in health or education - RDD graph



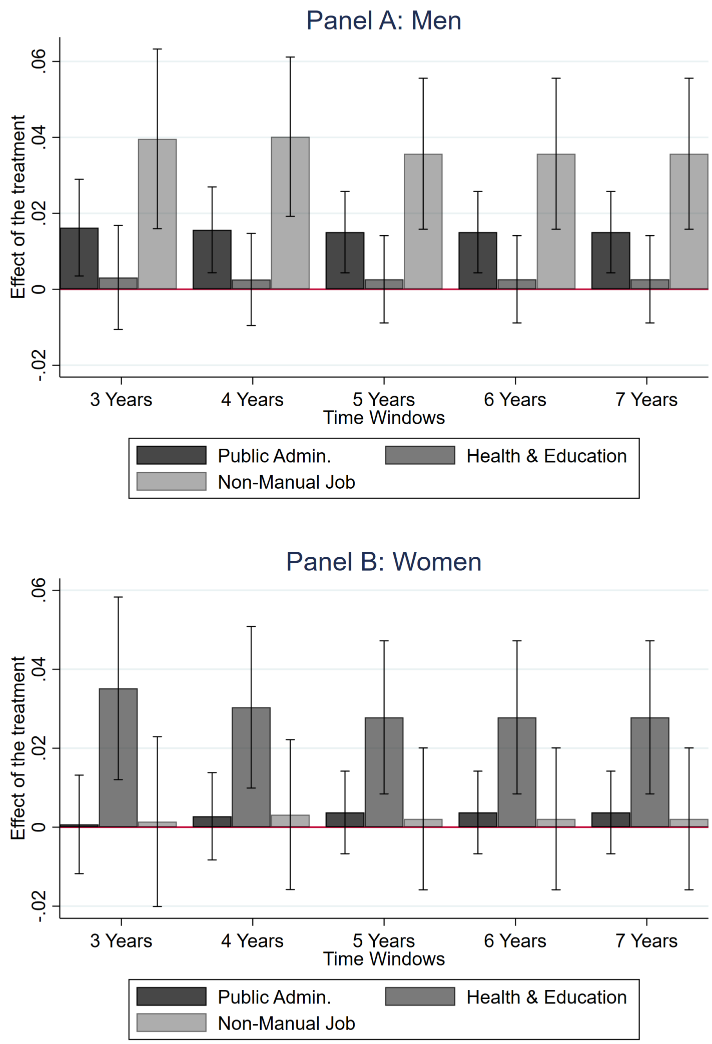
Notes: These figures refer to the wage earners born between September 1947 and August 1957. Panels A and B of the figure show linear fits on each side of the cut-off with 6-months-of-birth bins for men and women, respectively.

Figure 4: Having a non-manual job - RDD graph



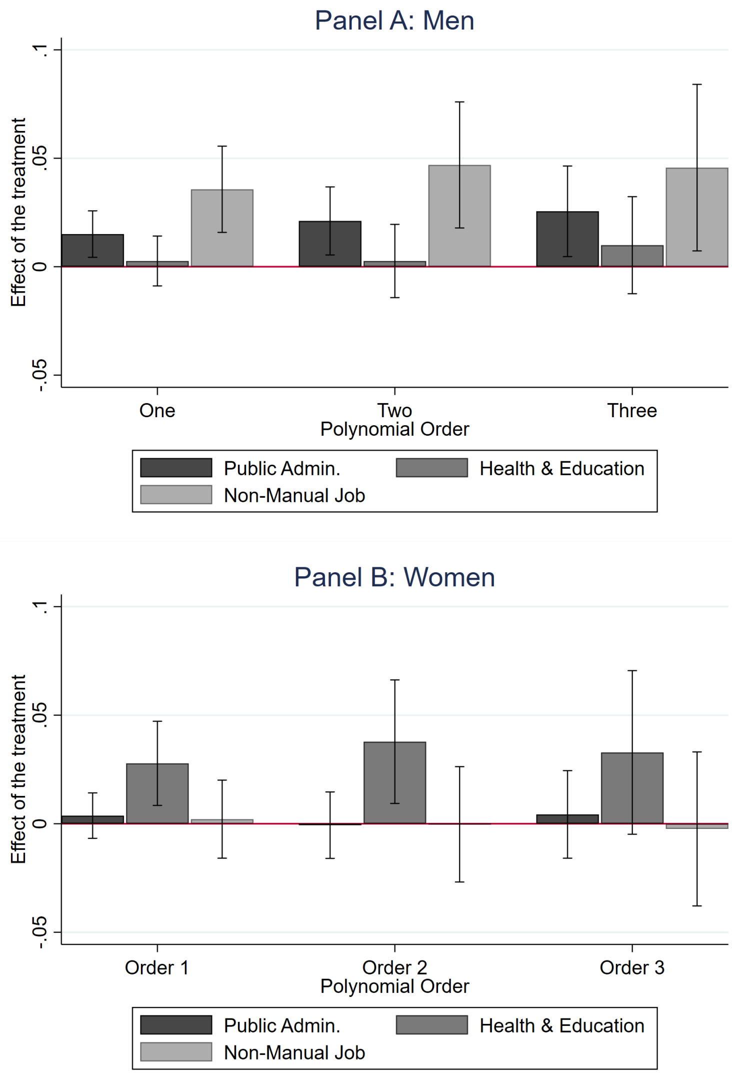
Notes: These figures refer to the wage earners born between September 1947 and August 1957. Panels A and B of the figure show linear fits on each side of the cut-off with 6-months-of-birth bins for men and women, respectively.

Figure 5: Treatment Effects with Polynomial of Different Bandwidths



Notes: These figures refer to the wage earners born between September 1947 and August 1957. We use a tent-shaped edge kernel centered around the date-of-birth cutoff and a first-order spline function of the date of birth. Controls are the age, the age squared, a female dummy, year, quarter and questionnaire number fixed-effects. Standard errors are clustered at the individual level.

Figure 6: Treatment Effects with Polynomial of Different Orders



Notes: These figures refer to the wage earners born between September 1947 and August 1957. We use a tent-shaped edge kernel centered around the date-of-birth cutoff and a first-order spline function of the date of birth. Controls are the age, the age squared, a female dummy, year, quarter and questionnaire number fixed-effects. Standard errors are clustered at the individual level.

Tables

Table 1: Descriptive statistics

	Whole Sample			
	Mean	SD	Min	Max
Treatment	0.53		0	1
School-leaving age greater than 15	0.83		0	1
Female	0.50		0	1
Age	39.46	3.44	32	48
White	0.99		0	1
Industry: Public Administration	0.08		0	1
Industry: Health and Education	0.24		0	1
Non-Manual Occupation	0.64		0	1
	Panel A: Men			
	Mean	SD	Min	Max
Treatment	0.53		0	1
School-leaving age greater than 15	0.83		0	1
Age	39.41	3.44	32	48
White	0.99		0	1
Industry: Public Administration	0.08		0	1
Industry: Health and Education	0.09		0	1
Non-Manual Occupation	0.56		0	1
	Panel B: Women			
	Mean	SD	Min	Max
Treatment	0.53		0	1
School-leaving age greater than 15	0.83		0	1
Female	0.53		0	1
Age	39.51	3.44	32	48
White	0.99		0	1
Industry: Public Administration	0.07		0	1
Industry: Health and Education	0.38		0	1
Non-Manual Occupation	0.71		0	1

Notes: These figures refer to the wage earners born between September 1947 and August 1957. “Treatment” is a dummy equal to one for individuals born after September 1957 and zero otherwise.

Table 2: Education, Industrial and Occupational sorting: RDD Results

	School-leaving Age above 15		Public Administration		Health and Education		Non-Manual Occupation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Whole Sample								
Treatment	0.229***	0.230***	0.009**	0.009**	0.020***	0.015**	0.021***	0.019**
	(0.006)	(0.006)	(0.005)	(0.005)	(0.007)	(0.007)	(0.008)	(0.008)
Observations	272012	272012	272012	272012	272012	272012	272012	272012
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R2	0.099	0.099	0.000	0.001	0.002	0.123	0.000	0.024
Average (untreated)	0.703	0.703	0.074	0.074	0.254	0.254	0.640	0.640
Panel A: Men								
Treatment	0.213***	0.213***	0.015**	0.015**	0.003	0.003	0.036***	0.036***
	(0.009)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)	(0.012)	(0.012)
Observations	134936	134936	134936	134936	134936	134936	134936	134936
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R2	0.090	0.090	0.000	0.000	0.001	0.001	0.001	0.001
Average (untreated)	0.715	0.715	0.081	0.081	0.098	0.098	0.564	0.564
Panel B: Women								
Treatment	0.246***	0.246***	0.004	0.004	0.028**	0.028**	0.002	0.002
	(0.009)	(0.009)	(0.006)	(0.006)	(0.012)	(0.012)	(0.011)	(0.011)
Observations	137076	137076	137076	137076	137076	137076	137076	137076
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R2	0.108	0.108	0.001	0.001	0.005	0.003	0.000	0.000
Average (untreated)	0.691	0.691	0.067	0.067	0.406	0.406	0.714	0.714

Notes: These figures refer to the wage earners born between September 1947 and August 1957. "Treatment" is a dummy equal to one for individuals born after September 1957 and zero otherwise. We use a tent-shaped edge kernel centered around the date-of-birth cutoff and a first-order spline function of the date of birth. Controls are the age, the age squared, a female dummy, year, quarter and questionnaire number fixed-effects. Standard errors in parentheses are clustered at the individual level. *, **, *** indicate significance at the 10%, 5% and 1% levels respectively.

Table 3: Education, Industrial and Occupational sorting: Placebo Test

	School-leaving Age above 15 (1)	Public Administration (2)	Health and Education (3)	Non-Manual Occupation (4)
Panel A: Men				
Placebo	0.005	0.004	-0.001	0.002
	(0.011)	(0.006)	(0.007)	(0.012)
Observations	128557	128557	128557	128557
Panel B: Women				
Placebo	0.012	-0.002	0.012	0.003
	(0.011)	(0.006)	(0.011)	(0.010)
Observations	135123	135123	135123	135123

Notes: These figures refer to the wage earners born between September 1942 and August 1952. "Placebo" is a dummy equal to one for individuals born after September 1952 and zero otherwise. We use a tent-shaped edge kernel centered around the date-of-birth cutoff and a first-order spline function of the date of birth. Controls are the age, the age squared, a female dummy, year, quarter and questionnaire number fixed-effects. Standard errors in parentheses are clustered at the individual level. *, **, *** indicate significance at the 10%, 5% and 1% levels respectively.

Table 4: Further Robustness Checks

	Panel A: Men				
	(1)	(2)	(3)	(4)	(5)
Outcome A: Public Administration					
Treatment	0.015**	0.010*	0.019***	0.015***	
	(0.007)	(0.006)	(0.007)	(0.007)	
School-leaving Age above 15					0.098***
					(0.031)
Outcome B: Health & Education					
Treatment	0.003	0.004	-0.001	0.003	
	(0.007)	(0.006)	(0.008)	(0.007)	
School-leaving Age above 15					0.012
					(0.032)
Outcome C: Non-Manual Occupation					
Treatment	0.036***	0.027**	0.038***	0.035***	
	(0.012)	(0.011)	(0.013)	(0.012)	
School-leaving Age above 15					0.168**
					(0.055)
Observations	134936	134936	26457	134936	134936
	Panel B: Women				
	(1)	(2)	(3)	(4)	(5)
Outcome A: Public Administration					
Treatment	0.004	0.007	0.003	0.003	
	(0.006)	(0.006)	(0.007)	(0.006)	
School-leaving Age above 15					0.015
					(0.026)
Outcome B: Health & Education					
Treatment	0.028**	0.020*	0.030**	0.028**	
	(0.012)	(0.011)	(0.013)	(0.012)	
School-leaving Age above 15					0.113**
					(0.048)
Outcome C: Non-Manual Occupation					
Treatment	0.002	0.000	-0.003	0.002	
	(0.011)	(0.010)	(0.012)	(0.011)	
School-leaving Age above 15					0.009
					(0.044)
Observations	137076	137076	26873	137076	137076

Notes: These figures refer to the wage earners born between September 1947 and August 1957. "Treatment" is a dummy equal to one for individuals born after September 1957 and zero otherwise. We use a tent-shaped edge kernel centered around the date-of-birth cutoff and a first-order spline function of the date of birth for the baseline estimates. Controls are the age, the age squared, a female dummy, year, quarter and questionnaire number fixed-effects. Standard errors in parentheses are clustered at the individual level. *, **, *** indicate significance at the 10%, 5% and 1% levels respectively.

Table 5: Gender Differences in Earnings - Gelbach Decomposition

	Men			Women		
	Base (1)	Full (2)	Expl. (3)	Base (4)	Full (5)	Expl. (6)
Treatment	0.052***	0.029*	0.023***	0.009	0.006	0.003
	(0.017)	(0.015)	(0.008)	(0.016)	(0.016)	(0.009)
Contributions						
Public Admin.			0.000			
			(0.000)			
Health and Education						0.003
						(0.009)
Non-manual Occupation			0.024***			
			(0.008)			
Observations		22746			23922	

Notes: These figures refer to the wage earners born between September 1947 and August 1957. "Treatment" is a dummy equal to one for individuals born after September 1957 and zero otherwise. We use a tent-shaped edge kernel centered around the date-of-birth cutoff and a first-order spline function of the date of birth for the baseline estimates. Controls are the age, the age squared, a female dummy, year, quarter and questionnaire number fixed-effects. Standard errors in parentheses are clustered at the individual level. *, **, *** indicate significance at the 10%, 5% and 1% levels respectively.

Table 6: Descriptive statistics - ISSP Estimation Sample

	Whole Sample			
	Mean	SD	Min	Max
Treatment	0.50		0	1
School-leaving age greater than 15	0.77		0	1
Female	0.53		0	1
Age	46.08	12.39	22	68
Job Just for Money	0.31		0	1
Is this Job Characteristic Very Important?				
High Income	0.16		0	1
Job Security	0.58		0	1
Interesting Job	0.45		0	1
Independence	0.22		0	1
Help other people	0.22		0	1
Useful for Society	0.19		0	1
Decide Time to Work	0.11		0	1
	Panel A: Men			
	Mean	SD	Min	Max
Treatment	0.50		0	1
School-leaving age greater than 15	0.79		0	1
Age	46.01	12.39	22	68
Job Just for Money	0.36		0	1
Is this Job Characteristic Very Important?				
High Income	0.17		0	1
Job Security	0.56		0	1
Interesting Job	0.43		0	1
Independence	0.22	0	1	
Help other people	0.17		0	1
Useful for Society	0.16		0	1
Decide Time to Work	0.10		0	1
	Panel B: Women			
	Mean	SD	Min	Max
Treatment	0.50		0	1
School-leaving age greater than 15	0.76		0	1
Age	46.15	12.39	22	68
Job Just for Money	0.26		0	1
Is this Job Characteristic Very Important?				
High Income	0.15		0	1
Job Security	0.59		0	1
Interesting Job	0.47		0	1
Independence	0.23		0	1
Help other people	0.25		0	1
Useful for Society	0.22		0	1
Decide Time to Work	0.13		0	1

Notes: These figures refer to the wage earners born between September 1947 and August 1957. “Treatment” is a dummy equal to one for individuals born after September 1957 and zero otherwise.

Table 7: Education and Preferences about Non-Monetary Job Charateristics: RDD Results - ISSP Estimation Sample

	Job Just	Is this Job Characteristic Very Important?						
	for Money	High Income	Job Security	Interesting Job	Independence	Help other people	Useful for Society	Decide Time to Work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Whole Sample								
Treatment	-0.122**	0.040	0.098*	0.072	0.146***	0.149***	0.152***	0.029
	(0.051)	(0.040)	(0.054)	(0.055)	(0.055)	(0.055)	(0.052)	(0.043)
Observations	1528	1562	1576	1569	1084	1086	1074	1080
Average (untreated)	0.341	0.156	0.608	0.436	0.205	0.204	0.188	0.106
Panel B: Men								
Treatment	-0.089	0.049	0.168**	0.029	0.013	0.087	0.147**	0.042
	(0.051)	(0.077)	(0.076)	(0.062)	(0.079)	(0.073)	(0.070)	(0.057)
Observations	734	740	745	741	520	517	515	516
Average (untreated)	0.371	0.172	0.580	0.430	0.211	0.162	0.137	0.083
Panel C: Women								
Treatment	-0.140**	0.034	0.030	0.109	0.266***	0.192**	0.148*	0.014
	(0.068)	(0.054)	(0.074)	(0.077)	(0.076)	(0.080)	(0.077)	(0.063)
Observations	794	822	831	828	564	569	559	564
Average (untreated)	0.313	0.143	0.632	0.441	0.199	0.245	0.238	0.129

Notes: These figures refer to the wage earners born between September 1947 and August 1957. "Treatment" is a dummy equal to one for individuals born after September 1957 and zero otherwise. We use a tent-shaped edge kernel centered around the date-of-birth cutoff and a first-order spline function of the date of birth for the baseline estimates. Controls are the age, the age squared, a female dummy, year, quarter and questionnaire number fixed-effects. Standard errors in parentheses are clustered at the individual level. *, **, *** indicate significance at the 10%, 5% and 1% levels respectively.

Appendices

A Worker Sorting Extended Analysis

Table A1: The Effect of Education on Industrial Sorting using th SIC92 Classification: RDD Results - Multinomial Logit

	Industrial Groups (SIC92)									
	AF	EW	M	C	DHR	TC	BFI	PA	EH	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Men										
Treatment	-0.000	-0.001	0.006	-0.003	-0.015*	-0.011	0.005	0.015**	0.003	0.002
	(0.003)	(0.004)	(0.011)	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)	(0.005)
Observations					134936					
Log pseudolikelihood					-135845.30					
Panel B: Women										
Treatment	-0.001	0.001	-0.007	0.004	-0.018*	-0.000	-0.013	0.003	0.028**	0.004
	(0.002)	(0.002)	(0.008)	(0.003)	(0.010)	(0.004)	(0.008)	(0.006)	(0.012)	(0.005)
Observations					137076					
Log pseudolikelihood					-120541.44					

Notes: These figures are marginal effects. The estimation sample is made of wage earners born between September 1947 and August 1957. SIC92 industrial groups are the following: "1 - Agriculture and fishing" (AF), "2 - Energy and water" (EW), "3 - Manufacturing" (M), "4 - Construction" (C), " 5 - Distribution, hotels and restaurants" (DHR), "6 - Transport and communication" (TC), "7 - Banking, finance and insurance" (BFI), "8 - Public administration" (PA), "9 - Education and health" (EH) and "10 - Other services" (Other). "Treatment" is a dummy equal to one for individuals born after September 1957 and zero otherwise. We use a tent-shaped edge kernel centered around the date-of-birth cutoff and a first-order spline function of the date of birth. Controls are the age, the age squared, a female dummy, year, quarter and questionnaire number fixed-effects. Standard errors in parentheses are clustered at the individual level. *, **, *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A2: The Effect of Education on Occupational Sorting using the SOC90 Classification: RDD Results - Multinomial Logit

	Occupational Groups (SOC90)								
	MA	PO	APO	CSO	CRO	PPSO	SO	PMO	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Men									
Treatment	0.018*	0.002	0.006	-0.003	-0.018*	-0.004	0.005	-0.011	0.004
	(0.010)	(0.008)	(0.007)	(0.006)	(0.009)	(0.006)	(0.005)	(0.009)	(0.005)
Observations					134936				
Log pseudolikelihood					-136848.19				
Panel B: Women									
Treatment	0.002	-0.003	0.006	0.016	-0.005	0.005	-0.013*	-0.004	-0.004
	(0.008)	(0.007)	(0.008)	(0.011)	(0.003)	(0.009)	(0.007)	(0.005)	(0.007)
Observations					137076				
Log pseudolikelihood					-138951.39				

Notes: These figures are marginal effects. The estimation sample is made of wage earners born between September 1947 and August 1957. SOC90 occupational groups are the following: "1 - Managers and administrators" (MA), "2 - Professional occupations" (PO), "3 - Associate professional occupations" (APO), "4 - Clerical and secretarial occupations" (CSO), "5 - Craft and related occupations" (CRO), "6 - Personal and protective service occupations" (PPSO), "7 - Sales occupations" (SO), "8 - Plant and machine operatives" (PMO) and "9 - Other occupations" (Other). "Treatment" is a dummy equal to one for individuals born after September 1957 and zero otherwise. We use a tent-shaped edge kernel centered around the date-of-birth cutoff and a first-order spline function of the date of birth. Controls are the age, the age squared, a female dummy, year, quarter and questionnaire number fixed-effects. Standard errors in parentheses are clustered at the individual level. *, **, *** indicate significance at the 10%, 5% and 1% levels respectively.

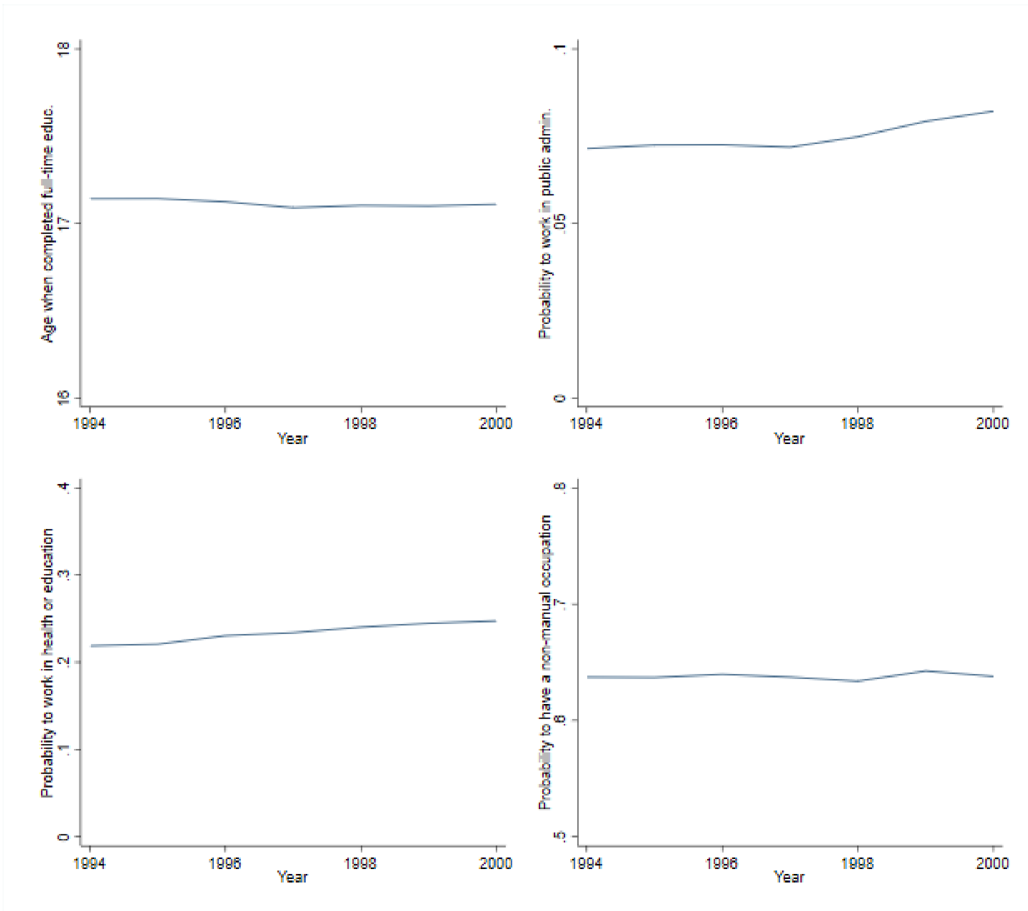
B Classification of occupations

Table A3: Manual and non-manual occupations according to the QLFS

1 - Managers and admini	Non-Manual Occupation	700 buyers (retail tr	Non-Manual Occupation
2 - Professional occupations	Non-Manual Occupation	701 buyers etc (non-r	Non-Manual Occupation
3 - Associate prof and te	Non-Manual Occupation	702 importers and expor	Non-Manual Occupation
400 civil service adm	Non-Manual Occupation	703 air,commodity and s	Non-Manual Occupation
401 local government	Non-Manual Occupation	710 technical and whole	Non-Manual Occupation
410 accounts clerks,b	Non-Manual Occupation	719 other sales repre	Non-Manual Occupation
411 counter clerks	Non-Manual Occupation	720 sales assistants	Non-Manual Occupation
412 debt,rent and other	Non-Manual Occupation	721 retail cash and che	Non-Manual Occupation
420 filing and record	Non-Manual Occupation	722 petrol pump forec	Non-Manual Occupation
421 library assistant	Non-Manual Occupation	730 collectors and cred	Non-Manual Occupation
430 clerks nes	Non-Manual Occupation	731 rounds and van sale	Manual Occupation
440 stores,control cl	Non-Manual Occupation	732 market,street tra	Non-Manual Occupation
441 storekeepers and wa	Manual Occupation	733 scrap dealers etc	Non-Manual Occupation
450 medical secretari	Non-Manual Occupation	790 merchandisers	Non-Manual Occupation
451 legal secretaries	Non-Manual Occupation	791 window dressers,	Non-Manual Occupation
452 typists and word pr	Non-Manual Occupation	792 telephone salespe	Non-Manual Occupation
459 other secretarial	Non-Manual Occupation	*8 plant and machine o*	Manual Occupation
460 receptionists	Non-Manual Occupation	900 farm workers	Manual Occupation
461 reception telepho	Non-Manual Occupation	901 farm machinery dr	Manual Occupation
462 telephone operato	Non-Manual Occupation	902 other related far	Manual Occupation
463 radio and telegraph	Non-Manual Occupation	903 fishing and related	Manual Occupation
490 computer etc oper	Non-Manual Occupation	904 forestry workers	Manual Occupation
491 tracers,drawing o	Non-Manual Occupation	910 coal mine laboure	Manual Occupation
5 - craft and related o	Manual Occupation	911 foundry labourers	Manual Occupation
610 police officers (	Non-Manual Occupation	912 engineering etc l	Manual Occupation
611 firemen (leading	Non-Manual Occupation	913 fitters mates (me	Manual Occupation
612 prison officers (	Non-Manual Occupation	919 making,processing	Manual Occupation
613 customs,immigrati	Non-Manual Occupation	920 woodworkers mates	Manual Occupation
614 traffic wardens	Manual Occupation	921 building trade ma	Manual Occupation
615 security guards e	Manual Occupation	922 rail construction	Manual Occupation
619 other security pe	Manual Occupation	923 road construction	Manual Occupation
620 chefs,cooks	Manual Occupation	924 paviours,kerb lay	Manual Occupation
621 waiters,waitresse	Manual Occupation	925 other building et	Manual Occupation
622 bar staff	Manual Occupation	930 stevedores,docker	Manual Occupation
630 travel and flight a	Manual Occupation	931 goods porters	Manual Occupation
631 railway station s	Manual Occupation	932 slingers	Manual Occupation
640 assistant nurses	Non-Manual Occupation	933 refuse and salvage	Manual Occupation
641 hospital ward ass	Manual Occupation	934 drivers mates	Manual Occupation
642 ambulance staff	Manual Occupation	940 postal workers,ma	Manual Occupation
643 dental nurses	Non-Manual Occupation	941 messengers,courie	Manual Occupation
644 care assistants and	Manual Occupation	950 hospital porters	Manual Occupation
650 nursery nurses	Manual Occupation	951 hotel porters	Manual Occupation
651 playgroup leaders	Non-Manual Occupation	952 kitchen porters	Manual Occupation
652 educational assis	Manual Occupation	953 catering assistan	Manual Occupation
659 other childcare o	Manual Occupation	954 shelf fillers	Non-Manual Occupation
660 hairdressers,barb	Manual Occupation	955 lift and car park a	Manual Occupation
661 beauticians and rel	Manual Occupation	956 window cleaners	Manual Occupation
670 domestic housekee	Manual Occupation	957 road sweepers	Manual Occupation
671 housekeepers (non	Manual Occupation	958 cleaners,domestic	Manual Occupation
672 caretakers	Manual Occupation	959 other sales,servi	Manual Occupation
673 launderers,dry cl	Manual Occupation	990 all other laboure	Manual Occupation
690 undertakers	Manual Occupation	999 all others (misce	Manual Occupation
691 bookmakers	Manual Occupation		
699 other personal se	Manual Occupation		

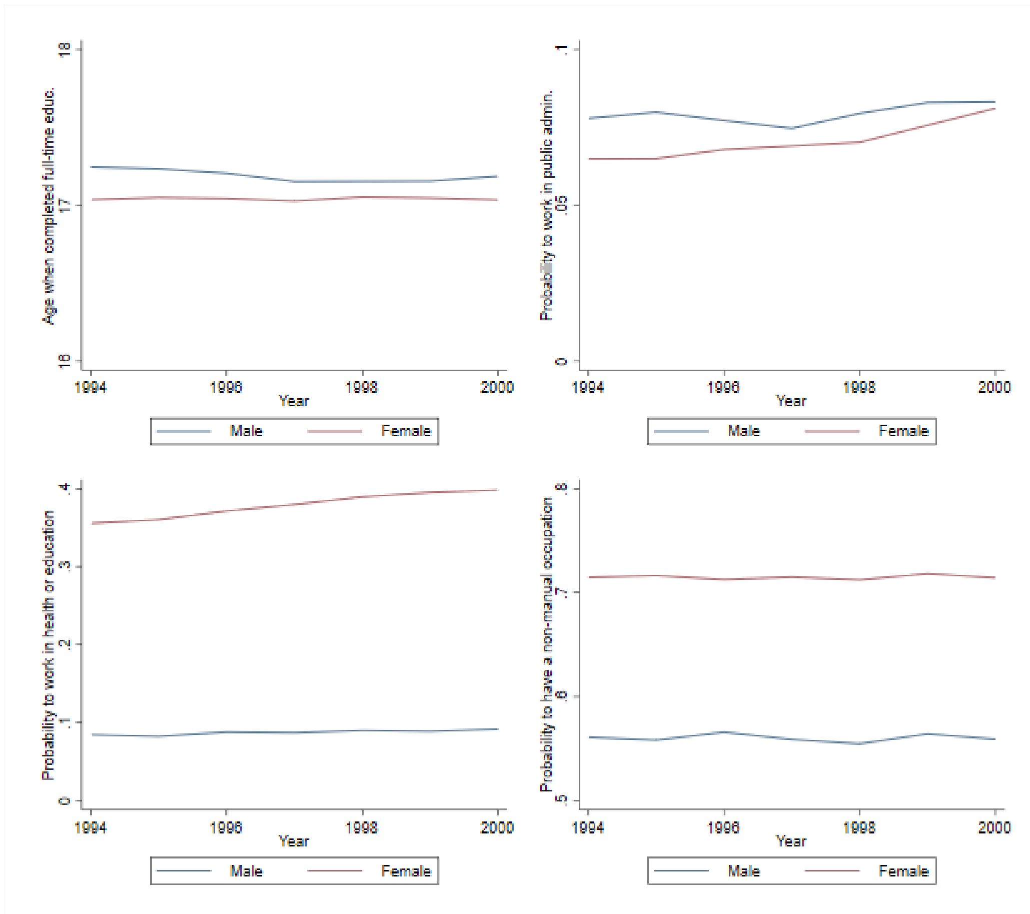
C Descriptive Statistics over Sample Period

Figure A1: School-leaving age above 15 - RDD graph



Notes: These figures refer to the wage earners born between September 1947 and August 1957. Panels A and B of the figure show linear fits on each side of the cut-off with 6-months-of-birth bins for men and women, respectively.

Figure A2: School-leaving age above 15 - RDD graph



Notes: These figures refer to the wage earners born between September 1947 and August 1957. Panels A and B of the figure show linear fits on each side of the cut-off with 6-months-of-birth bins for men and women, respectively.

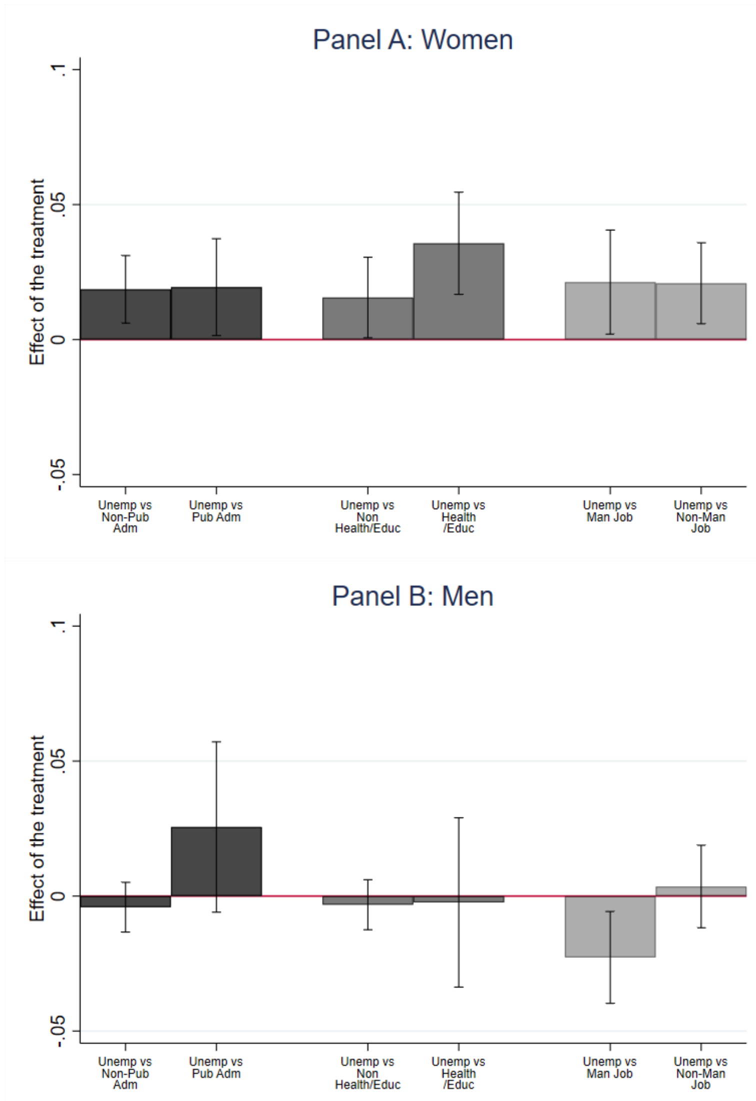
D Further Understanding of Baseline Estimates

This paper has shown that compulsory education reforms have important effects on industrial and occupational sorting and that these impacts differ by gender. In particular, we have shown that more education leads to a higher proportion of males working in the public administration and non-manual occupations and to a higher proportion of females working in the health-education industries.

An interesting question is whether these effects originate from education reforms providing incentives to individuals who would otherwise be unemployed or inactive to take jobs in particular industries or occupations, or whether the effects originate from individuals who would be employed with or without education reforms changing their labour decisions. We explore these possibilities by estimating our baseline specification and using as dependent variables dummies that take value 0 if the individual is unemployed or inactive and value 1 if the individual holds a: (i) non-public administration, (ii) public administration, (iii) non health or education industry, (iv) health or education industry, (v) manual and (vi) non-manual job, respectively.

We present the estimates in panels A and B of Figure A3 for females and males, respectively. As shown, the compulsory education reform incentives women to be more attached to the labour market, but in a similar way across different occupations and industries. For men, we find that the estimates of the effect of the reform on employment are not statistically different across different occupations and industries. In fact, the only estimate that is statistically significant shows that more education leads to unemployed individuals remaining unemployed instead of taking manual jobs, which cannot explain the increase in the proportion of males taking non-manual jobs that we find in our baseline results. Overall, these estimates suggest that our baseline results are not driven by the education reform leading to individuals who would be otherwise unemployed taking jobs in certain types of industries or occupations.

Figure A3: Further Understanding of Baseline Estimates



Notes: These figures refer to the wage earners born between September 1947 and August 1957. We use a tent-shaped edge kernel centered around the date-of-birth cutoff and a first-order spline function of the date of birth. Controls are the age, the age squared, a female dummy, year, quarter and questionnaire number fixed-effects. Standard errors are clustered at the individual level.

E Pre-determined Characteristics as Outcomes

Table A4: Pre-determined Characteristics as Outcomes

	(1) Age	(2) White	(3) UK citizen
Treatment	0.001	-0.001	0.001
	(0.002)	(0.002)	(0.001)
Observations	272012	272012	272012
Controls	No	No	No
Adjusted R2	0.988	0.002	0.000
Average (untreated)	42.152	0.996	0.999
Panel A: Men			
Treatment	0.002	-0.001	0.001
	(0.003)	(0.002)	(0.001)
Observations	134936	134936	134936
Controls	No	No	No
Adjusted R2	0.988	0.002	0.000
Average (untreated)	42.127	0.996	0.999
Panel B: Women			
Treatment	-0.000	-0.001	0.001
	(0.003)	(0.002)	(0.001)
Observations	137076	137076	137076
Controls	No	No	No
Adjusted R2	0.988	0.003	0.000
Average (untreated)	42.177	0.996	0.999

Notes: These figures refer to the wage earners born between September 1947 and August 1957. "Treatment" is a dummy equal to one for individuals born after September 1957 and zero otherwise. We use a tent-shaped edge kernel centered around the date-of-birth cutoff and a first-order spline function of the date of birth. Controls are year, quarter and questionnaire number fixed-effects. Standard errors in parentheses are clustered at the individual level. *, **, *** indicate significance at the 10%, 5% and 1% levels respectively.