The Effect of Incrementally Raising the Compulsory Education Age on Medium-Term Labour Market Outcomes: England's Reforms

EC331: Research in Applied Economics
Department of Economics, University of Warwick
20 April 2025

Student: Samuel Salin
Supervisor: Dr. Jeisson Cardenas-Rubio[1]

Word Count: 4999

Abstract

As set out in the 2008 Education and Skills Act, England incrementally raised the compulsory education age from 16 to 17 in 2013 and 18 in 2015, aiming to improve labour market outcomes. Motivated by the limited literature exploring the incremental implementation of such policy, this paper investigates impacts on income and unemployment in the 7 years following the final reform. Using matched sample Difference-in-Differences and Event Study estimation on individual-level repeated cross-sectional data from APS, the empirical results suggest income significantly increased in the medium-term for those affected by the policy, while impacts on unemployment were insignificant. Therefore, policymakers should consider the effectiveness of compulsory schooling reforms on specific labour market outcomes before adopting similar policies to England.

---

[1] I would like to thank Dr. Jeisson Cardenas-Rubio for his invaluable feedback and guidance throughout this project, and am grateful for the methodology inspiration from Dr. Neil Lloyd and Dr. Dita Eckardt.

Table of Contents

1. Introduction

The 2008 Education and Skills Act represented a major reform in England's education policy, mandating an incremental rise in the compulsory schooling age from 16 to 17 in 2013, followed by 17 to 18 in 2015 (Woodin, 2013). The objective of the reform was to improve labour market outcomes over the medium- to long- run, including increasing earnings, lowering unemployment, and lowering the rate of young people not in education, employment, or training (Maguire, 2021).

There was strong justification for the reform through economic theory. Education is an investment in which individuals allocate time and effort to develop knowledge, skills, and productivity, collectively forming human capital. In the long-run this investment results in greater employment and earnings prospects for the individual (Becker, 1964). In the labour market, a rise in human capital drives economic growth and productivity, thus benefiting society at large (Woodin, 2013). Therefore, by extending the compulsory schooling age, governments can theoretically increase human capital, thereby improving labour market outcomes, specifically employment and earnings potential. However, such reforms potentially come with trade-offs, such as delayed workforce entry (Card, 1999) and an increased drop-out risk for low performing students (Hall, 2016), which have the potential to hinder the same labour market outcomes. The existing literature on compulsory schooling reforms reflects this trade-off, with mixed findings on earnings and employment effects.

This investigation of compulsory schooling age policy is motivated by global relevance as general trends have seen increases in the upper age limit set (Juusola, 2023). Such policies have been implemented in Sweden and Germany in the 1970s (Fischer, 2020), around the US and Canada (Oreopoulos, 2007), and in Finland as recently as 2021 (Juusola, 2023). By assessing the effects of England's reform, this paper provides valuable evidence to inform international policymakers of the effectiveness of similar future policies in enhancing medium-term earnings and employment elsewhere.

The aim of this paper is to determine if England's incremental compulsory schooling age reform has succeeded in improving medium-run employment and earnings outcomes. The medium-run is defined as 7 years following the final reform increment, as generally accepted in labour economics (Cedefop, 2023). Investigating the effects on both earnings and employment is important, as both are key indicators of individual economic well-being and broader labour market performance, and both were explicitly targeted by the reform. The incremental approach adopted by England is particularly interesting, as existing literature on such an approach remains extremely limited.

To estimate the causal effects of England's reform on earnings and employment, this paper uses matched Difference-in-Differences (DiD) and Event Study models applied to repeated cross-sectional data. The following hypotheses guide the analysis, defining the meaning of success for the reform.

Hypothesis I: Raising the compulsory schooling age will increase average earnings for affected individuals in the medium-run.

Hypothesis II: Raising the compulsory schooling age will decrease average unemployment for affected individuals in the medium-run.

2. Literature Review

The effects of extending compulsory education on long-term labour market outcomes, particularly income and unemployment, have been extensively studied, yet notable gaps persist in the literature. Prior research has overwhelmingly concentrated on single-step or historical reforms, with limited exploration of incrementally implemented policies, such as England's two-phase compulsory schooling reform in 2013 and 2015. Additionally, much of the literature draws from older cohorts and institutional contexts, potentially limiting relevance for contemporary policymaking. This paper will contribute by evaluating recent, incrementally applied reforms using a DiD framework modified to account for the incremental adjustment of the reform.

Seminal work by Angrist and Krueger (1991) laid the empirical foundation by using quarter of birth as an instrumental variable to estimate the local average treatment effect of additional schooling in the US. Their 2SLS approach suggested a 5.5% earnings gain per additional year of schooling for compliers. However, their identification strategy only captured marginal changes in entry and leaving ages, offering limited generalizability to incremental schooling reforms.

Oreopoulos (2007) expanded the methodological frontier by adopting a DiD approach across historical reforms in the US, UK, and Canada. His study, which used census and household survey data from 1915 to 2001, found a 15% increase in lifetime wealth from compulsory schooling, a much stronger effect than in other UK-specific literature. However, unobserved regional heterogeneity and long-run cultural shifts could have biased his cross-country estimates.

UK-specific evidence has largely focused on the 1947 and 1972 compulsory education reforms. Devereux and Hart (2010), applying a regression discontinuity design to the 1947 reform, found a 4-7% wage increase for men and no significant effect for women. Buscha (2012), using UKHLS data, found that individuals affected by the 1972 reform experienced a 5.5% increase in hourly wages even decades later, with slightly stronger effects for women. Complementing this, Demirel-Derebasoglu and Okten (2022) reported that extending compulsory schooling in Turkey disproportionately benefited women, suggesting such reforms may help reduce gender inequality in labour market outcomes.

When shifting focus to unemployment, findings remain mixed. Hall (2016) evaluated a Swedish reform extending vocational upper secondary programs, applying a DiD strategy covering the 2008-2010 recession. Rather than reducing unemployment risk, the policy disproportionately hurt low academic performers, raising concerns over unintended labour market consequences. Fischer (2020) distinguished between extending school term length and compulsory schooling in Sweden using district-level variation. While term extension significantly improved earnings and unemployment outcomes, compulsory schooling itself had a smaller 2% income effect, and no measurable impact on unemployment.

Overall, the literature yields inconsistent conclusions on the effects of compulsory schooling, especially on unemployment outcomes, and lacks investigations into incremental reforms or their medium-term effects on labour market outcomes. The existing literature is underdeveloped with regards to insight into staggered or more recent reforms. This paper addresses this gap by analysing the 2013 and 2015 reforms in England, applying a DiD model with two interaction terms, one for each reform increment, enabling separate identification of each policy change's effects. Along with an event study model to capture dynamic effects, this methodological approach will offer a more nuanced understanding of how incremental education reforms unfold over time.

3. Data

This paper uses cross-sectional data from the Annual Population Survey (APS) collected by the Office for National Statistics (ONS). A harmonized dataset was constructed by pooling annual data between 2004 and 2023, resulting in a repeated cross-sectional dataset covering demographic and labour market characteristics across countries in the UK. The harmonized dataset includes approximately 1,482,000 individual-level observations, averaging around 75,000 per year (2004 to 2023). After sample restrictions (further detailed in Methodology), including controlling for spillover effects and restricting birth cohort range for comparability, the analytical dataset used for modelling includes approximately 87,000 observations, averaging 4,800 per year (2006 to 2023).

The first dependent variable is ln_GRSSWK, which is the natural log of an individual's gross weekly pay, representing earnings. The log form of weekly pay is used to reduce right-skewness in the earnings distribution and to allow for the interpretation of coefficients in percentage terms. Gross weekly pay is reported before tax in pounds. Observations missing earnings data for employed individuals were dropped.

The second dependent variable is UNEMP, which is a binary indicator equal to 1 if the individual is unemployed. This variable is constructed based on the International Labour Organization (ILO) definition of unemployment, meaning individuals without a job who are actively seeking work and ready to start in the next two weeks.

The variable COUNTRY is a binary indicator equal to 1 if residing in England, the treated group, and 0 if residing in Wales, Scotland, or Northern Ireland. Together, these countries form a suitable control due to shared institutional frameworks, similar labour market structures, and comparable education systems to England. This variable reflects the geography of policy implementation, as the reform affected England but not the other UK countries. To capture exposure to the reform, treatment is further defined by birth cohort, with only individuals born in 1997 or later considered affected by the policy, as they turned 16 in 2013. Accordingly, to observe those exposed to the policy in the post-reform period, the binary variable TREATED is equal to 1 if residing in England, born in 1997 or later, and observed in 2013 or later.

Table 3.1 presents summary statistics for all key variables and covariates from the analytical dataset. Variable descriptions can be found in Appendix 3.

Table 3.1

Observations	86959			
Variable	Mean	S.D.	Min	Max
ln_GRSSWK	5.27	0.99	.6931472	9.718061
UNEMP	0.22	0.41	0	1
COUNTRY	0.64	0.48	0	1
TREATED	0.14	0.34	0	1
birthyear	1993.90	3.55	1990	2005
fileyear	2015.37	4.75	2006	2023
AGE	21.47	4.28	16	33
SEX	0.50	0.50	0	1
MARRIED	0.06	0.23	0	1
NATIDE11	0.58	0.47	0	1
LIMITK	0.07	0.27	0	1
BENFTS	0.19	0.34	0	1
FTPT	1.55	0.47	0	2
INDE07M	5.14	2.14	0	9
SC10MMJ	2.89	2.30	0	9
GRSSWK	219.01	228.05	-9	16615
ENROLL	1.67	0.51	0	2
edu	1.93	0.80	0	3

4. Methodology

DiD and Event Study models are used to estimate causal effects of England's compulsory education reform on earnings and employment. For these models to produce valid results, both require no-spillover effects and parallel trend assumptions to hold.

Spillover effects arise when the control group is indirectly affected by the treatment, biasing estimates. This can happen if individuals move between countries and are misclassified based on residence, despite not being exposed to the local education system. To address cross-border migration spillover and minimize misclassification based on education exposure, the sample is restricted to English nationals residing in England and non-English nationals residing in the control group.

The parallel trends assumption is critical for ensuring observed post-reform differences can be attributed to the policy and not pre-existing divergence. It assumes that without the reform England and the control group would have followed similar trends in earnings and unemployment. Figures 4.1 and 4.2 display these trends prior to the reform, suggesting similar pre-treatment patterns.

Figure 4.1

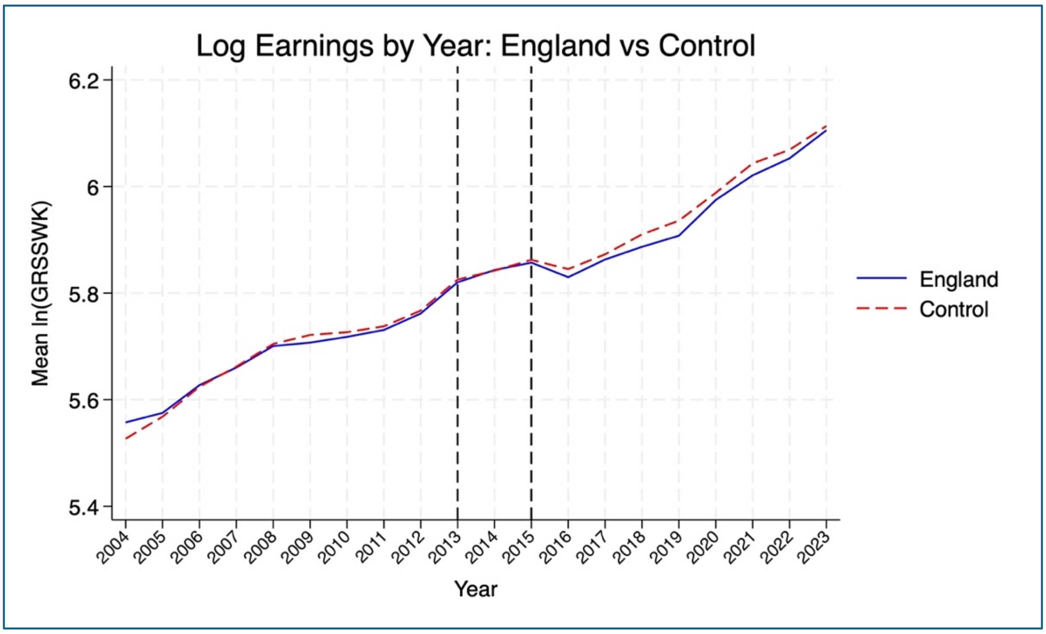
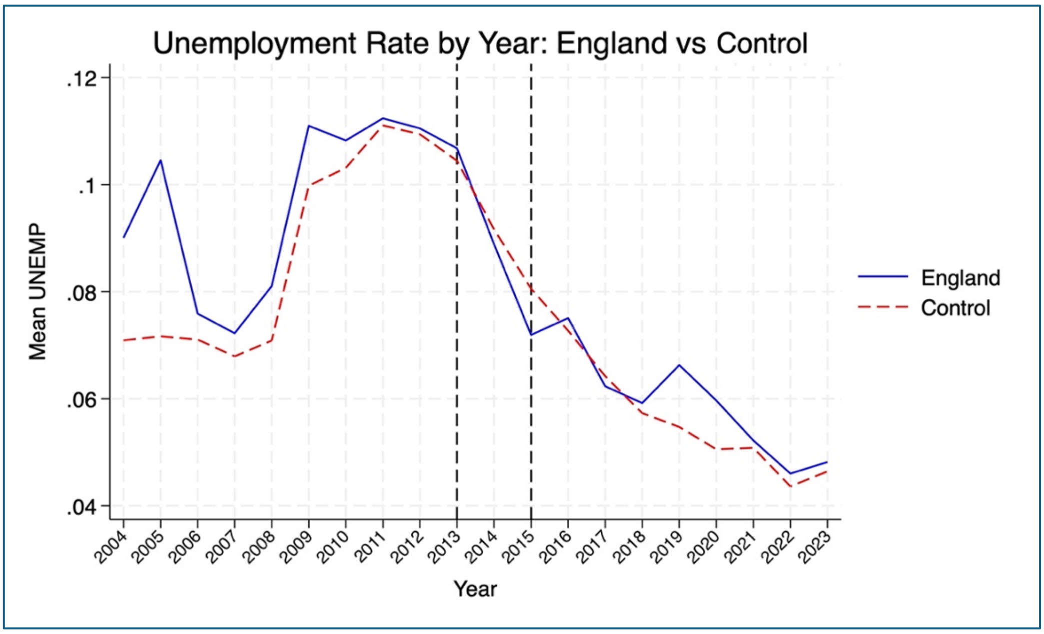


Figure 4.2



For both earnings and unemployment, a formal parallel trends test estimated a pre-trend regression with COUNTRY and fileyear interaction terms in the pre-reform period. A joint significance test rejected the null hypothesis of parallel trends for both outcomes, with an F-statistic of 4.79 and 6.16 respectively (see Appendix 4). Figures 4.3 and 4.4 illustrate the parallel trends violations, with statistically significant interaction term coefficients in multiple years, indicating systematic differences in pre-treatment trends between England and the control.

Figure 4.3

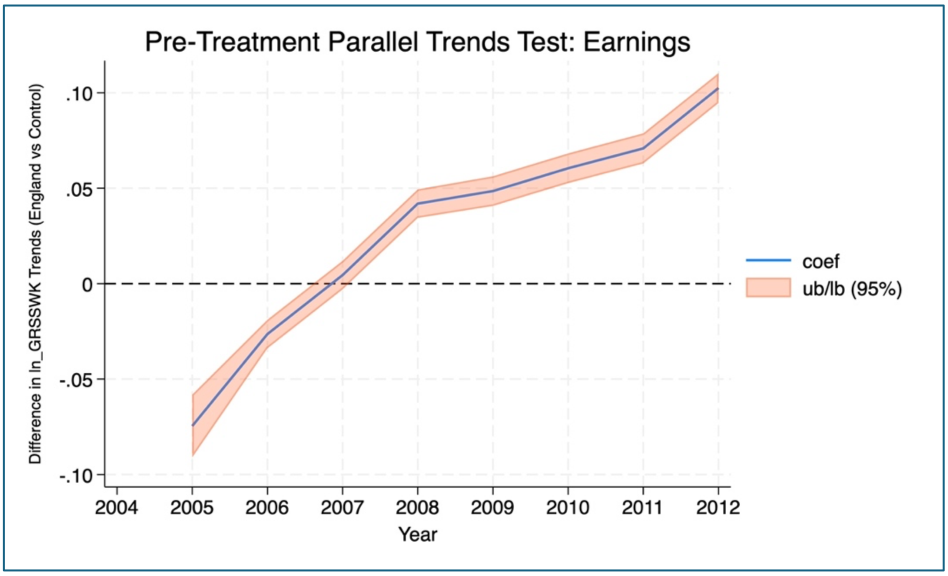
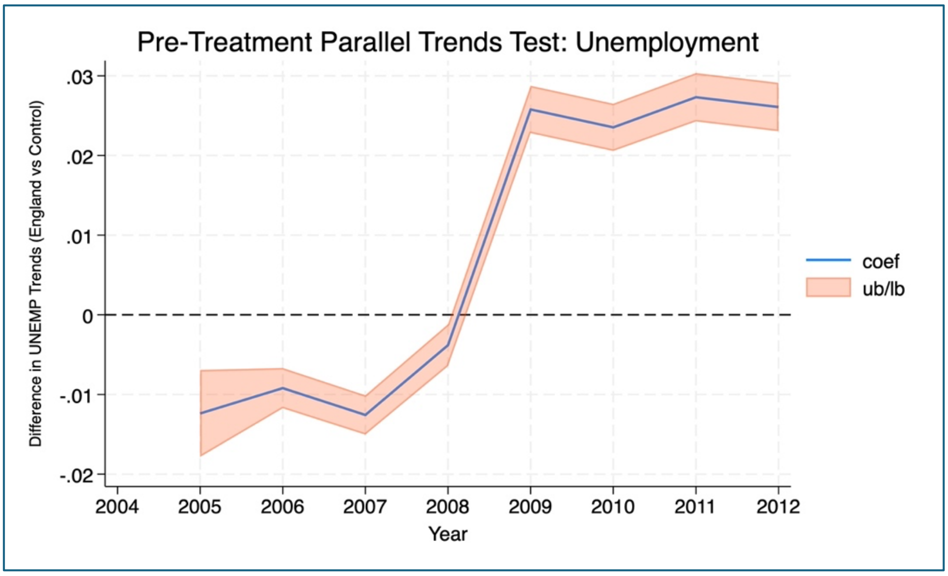


Figure 4.4



Given the parallel trends violation, the analysis proceeds with a matching model to improve covariate balance between the treated and control groups. Parallel trends will be re-assessed on the matched sample before estimating treatment effects using matched DiD and Event Study models.

4.1. Propensity Score Matching

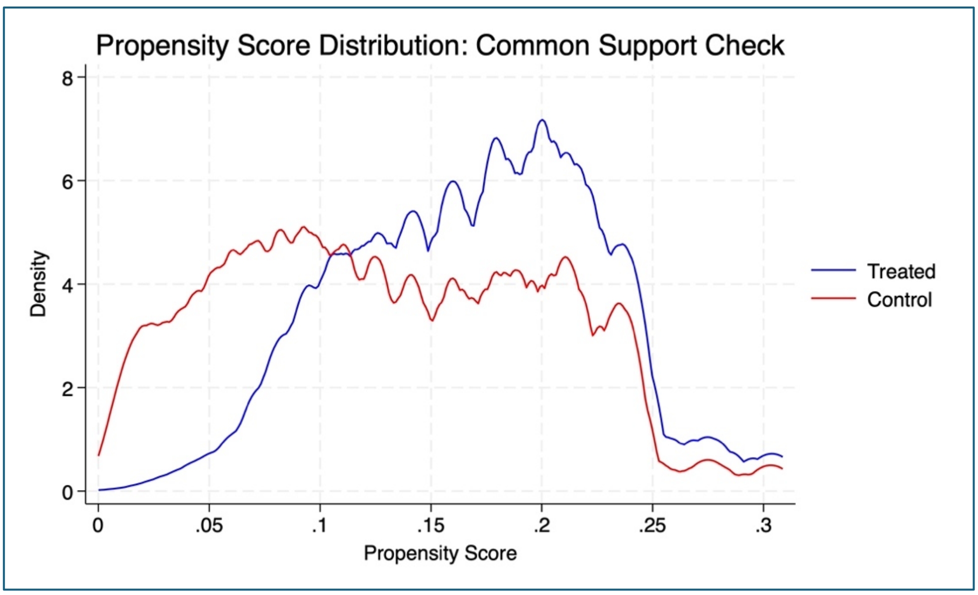
Propensity Score Matching (PSM) was implemented to improve comparability between treatment groups. The aim is to construct a matched sample where treated and control individuals are similar with respect to observed covariates, reducing treatment effect estimation bias. Hence, the sample is restricted to individuals born between 1990 and 2005, capturing an even range of cohorts before and after 1997, the first treated birth year cohort. This restriction improves comparability by removing much older cohorts, reducing bias from generational differences. The following logistic regression estimates propensity scores:

$$\Pr(TREATED_i = 1) = \Lambda(\beta_0 + \beta_1 AGE_i + \beta_2 SEX_i + \beta_3 MARRIED_i + \beta_4 LIMITK_i + \beta_5 BENFTS_i)$$

In this equation, TREATED includes those exposed to the policy in the post-reform period and $\Lambda(\cdot)$ is the logistic function. Stable covariates predictive of earnings and unemployment were selected, with LIMITK indicating health limitations and BENFTS, receiving social benefits, proxying for coming from a lower socioeconomic background. Variables such as education, industry, and sector were excluded to avoid post-treatment bias, as they are likely affected by the treatment itself. A 1-to-1 nearest-neighbour matching algorithm with a caliper of 0.05 and no replacement was used.

A kernel density plot of propensity score distributions shows strong overlap, indicating the common support assumption is satisfied (Figure 4.5).

Figure 4.5



The final matched sample from this PSM will be used in subsequent DiD and Event Study models. In addition to this, the matched sample also allows for a direct estimation of the Average Treatment effect of the Treated (ATT):

$$ATT = \frac{1}{N_T}\sum_{i \in T}(Y_i - Y_{i\prime})$$

By comparing outcomes (Y) between matched treated (i) and control (i') individuals, the ATT estimate offers an initial assessment of the reform's causal impact.

4.2. Difference-in-Differences Model

Prior to setting up a matched DiD model, another parallel trends test was conducted, now with the matched sample (see Appendix 4). Figures 4.6 and 4.7 illustrate that the assumption holds for earnings prior to the policy announcement in 2008, but remains violated for unemployment. This implies that estimates for unemployment must be interpreted with caution, as they may reflect underlying pre-treatment differences in trends rather than causal effects of the education reform. As previously discussed, the no-spillover effects assumption was already addressed by restricting the sample to reduce risk of mobility misclassification.

Figure 4.6

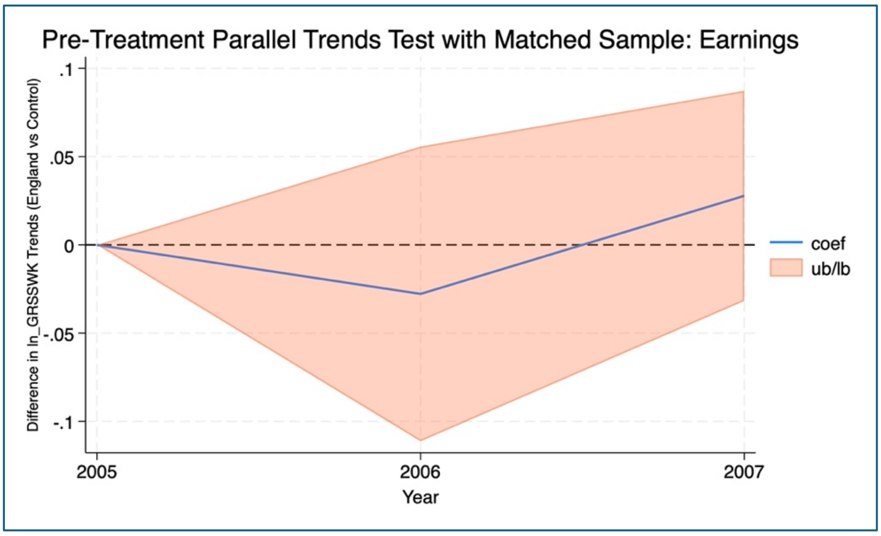
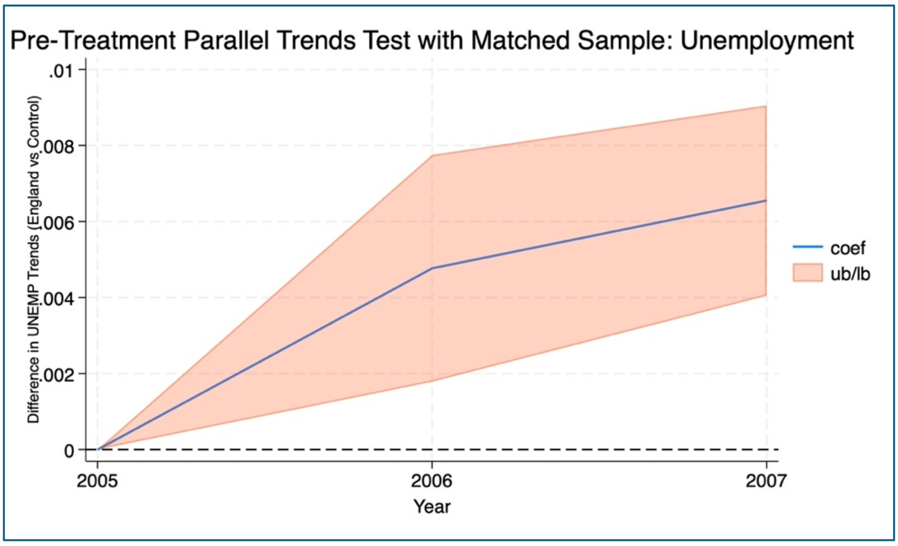


Figure 4.7



A matched DiD model is used to simultaneously estimate the causal effects of the 2013 and 2015 compulsory education changes in England on earnings and unemployment outcomes, using the matched sample obtained from PSM. This approach compares changes in outcomes over time between treated and control groups, thereby controlling for time-invariant differences between groups and common time shocks. The matched DiD model equation is:

$$Y_{it} = \alpha + \beta_1(COUNTRY_i * post2013_t) + \beta_2(COUNTRY_i * post2015_t) + \boldsymbol{X'}_{it}\theta + \delta_t + \varepsilon_{it}$$

In this model, Y is the outcome variable, either log earnings or unemployment. Being a continuous variable, log earnings is estimated with OLS. As a binary variable, unemployment is estimated with LPM, which was chosen over nonlinear models for ease of interpretation.

The interaction terms, $(COUNTRY_i * post2013_t)$ and $(COUNTRY_i * post2015_t)$, capture the causal effects of the 2013 and 2015 England schooling reforms. They estimate the difference in outcomes for those in England relative to the control, before and after each reform, thereby isolating the impact of the policy. Thus, the coefficients on these interactions, β_1 and β_2, represent the ATT for the 2013 and 2015 reforms. These can be interpreted as changes in outcome induced by the reform relative to what would have occurred in absence of the policy change. By separately estimating the 2013 and 2015 reform changes through two DiD interaction terms, this analysis contributes to the existing literature by offering a novel model modification to evaluate the incremental impact of a staggered increase in compulsory schooling age, an aspect that remains underexplored in current literature.

The transposed vector of individual-level covariates, $\boldsymbol{X'}_{it}$, includes the same variables justified in PSM. These account for observable differences that may influence earnings and unemployment independently of policy exposure, leaving out variables that may be susceptible to post-treatment bias. Year fixed effects, δ_t, control for time-specific shocks that affect all individuals helping ensure treatment effect estimates are not confounded by unrelated temporal trends. Robust standard errors are used to account for potential heteroscedasticity in the residual, ε_{it}.

4.3. Event Study Model

With parallel trends and spillover effect assumptions previously accounted for, a matched Event Study model was estimated to trace dynamic effects of the 2013 and 2015 compulsory schooling reform on earnings and unemployment outcomes. The model is estimated on the PSM matched sample. This approach allows the treatment effect to vary over time, providing a detailed view of how the incremental reforms together impacted outcomes after implementation. To define event time relative to the reform, 2012 is set as the base year as it is the final year prior to the reform taking effect 2013. Thus, 2012 is omitted from the model as the reference year.

The formal model specification follows as:

$$Y_{it} = \alpha + \sum_{k=-3, k\neq 0}^{10} \beta_k (COUNTRY_i * EventYear_k) + \boldsymbol{X'}_{it}\theta + \varepsilon_{it}$$

This model shares many similarities with the matched DiD set up. Y is still the outcome variable, being earnings or unemployment, and $\boldsymbol{X'}_{it}$ is the same transposed vector of covariates. Also, Robust standard errors are again used to account for potential heteroscedasticity in the residual, ε_{it}.

This analysis estimates dynamic treatment effects using a set of interaction terms between treatment status and event-year indicators, $(COUNTRY_i * EventYear_k)$. Thus, the coefficients, β_k, capture the annual differences in outcomes between the treated and control groups relative to the base year 2012. They reflect estimated changes in outcomes for treated individuals in each year compared to their expected outcomes in absence of the reform.

5. Results

This section presents and interprets the empirical findings of the matched DiD and Event Study models, as well as PSM ATT estimation. Results are evaluated in relation to the previously defined hypotheses and contextualized within the wider literature, concluding with discussion of future policy implications.

5.1. Model Findings

Incrementally increasing the compulsory education age in England had a statistically significant positive effect on average weekly earnings in the medium-run.

The PSM ATT estimate indicates treated individuals had log weekly earnings 0.266 higher than matched controls (SE=0.014), corresponding to an approximate 30.5% increase in earnings from extended compulsory education (see Appendix 5).

Table 5.1

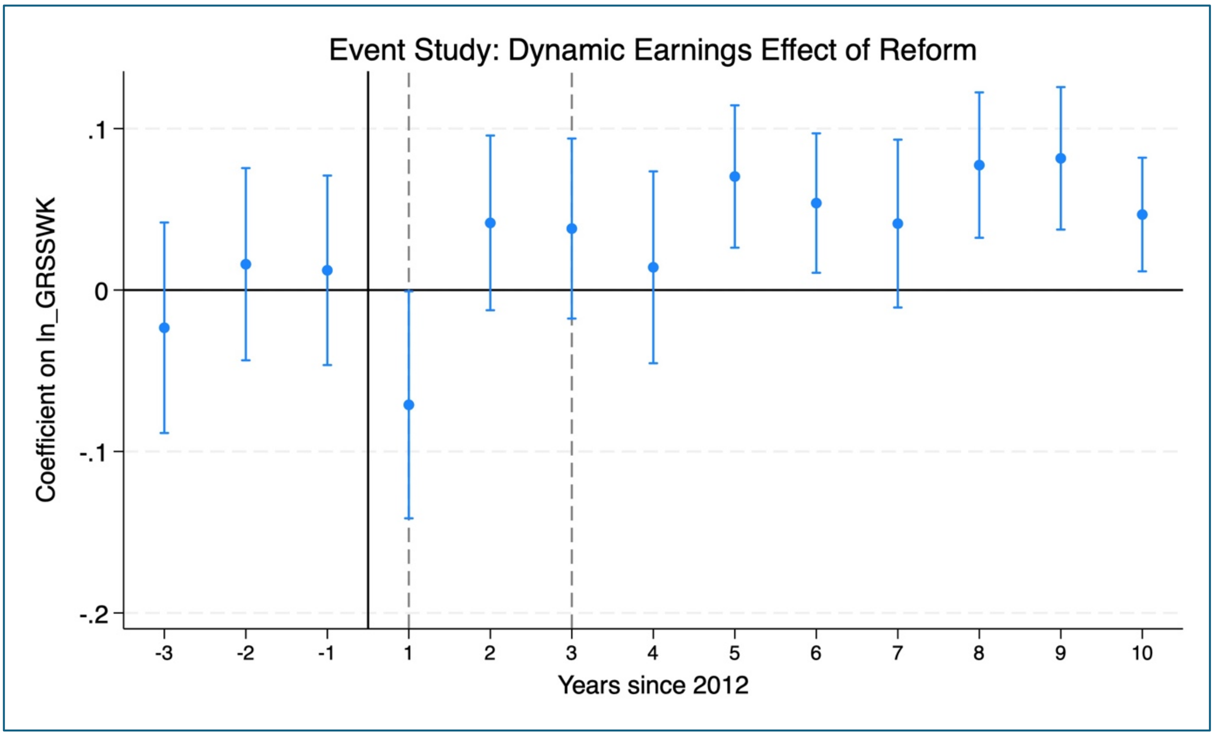
Matched DiD Model Estimates for Earnings (1) and Unemployment (2)

	(1) ln_GRSSWK	(2) UNEMP
1.post2013	1.303***	-0.161***
	(0.039)	(0.018)
1.COUNTRY#1.post2013	0.008	0.003
	(0.025)	(0.011)
1.post2015	0.858***	-0.167***
	(0.022)	(0.009)
1.COUNTRY#1.post2015	0.057**	-0.003
	(0.023)	(0.009)
SEX	-0.227***	-0.083***
	(0.006)	(0.003)
MARRIED	0.410***	-0.136***
	(0.009)	(0.004)
LIMITK	-0.005***	0.004***
	(0.001)	(0.000)
BENFTS	0.208***	-0.257***
	(0.012)	(0.010)
_cons	3.722***	1.046***
	(0.043)	(0.026)
N	68017	86959

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The matched DiD model for earnings in Table 5.1 further supports this finding. While the 2013 reform effect was statistically insignificant (β_1=0.008, SE=0.025), the 2015 reform showed a significant earnings increase of 5.9% (β_2=0.057, SE=0.023, $p<0.05$), implying the incremental age extension to 18 was particularly impactful.

Figure 5.1

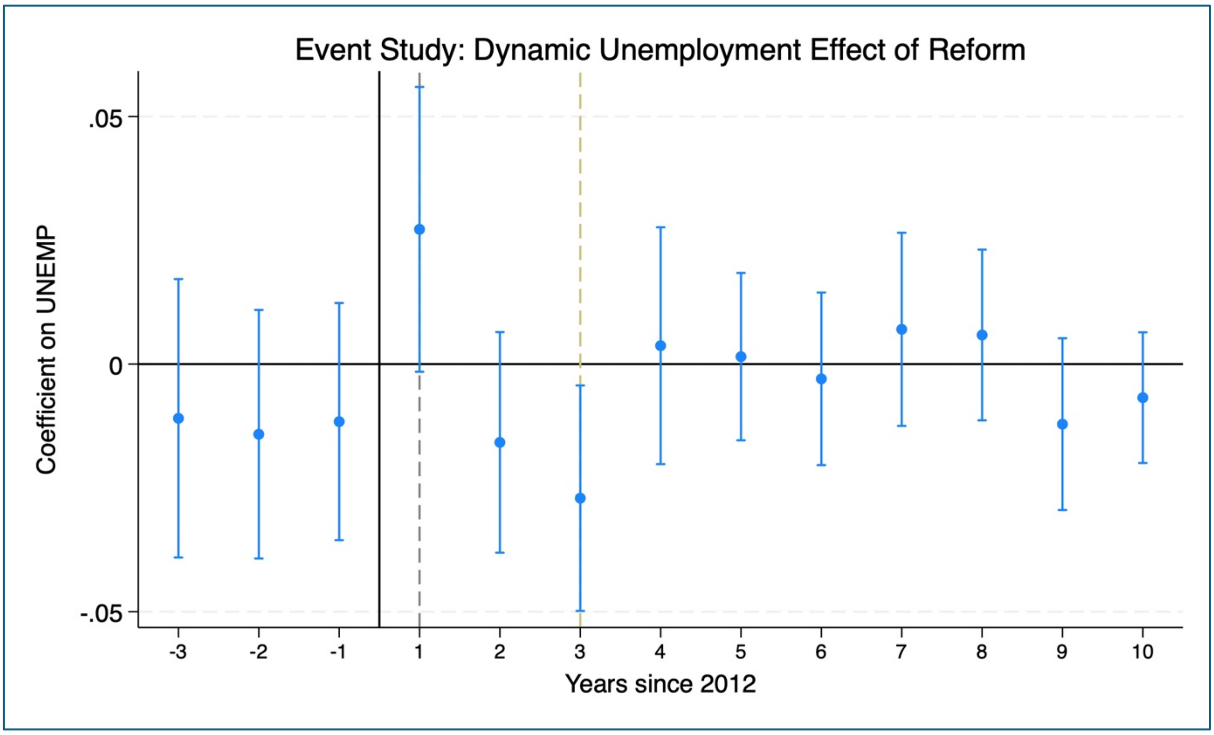


The matched Event Study in Figure 5.1 provides a detailed view of earnings dynamics over time. Despite earnings effects being slightly negative immediately post-reform (+1), they turned significantly positive from 2017 (+5) onwards, peaking at 8.5% in 2021 (+9) (β_9=0.0815, SE=0.022) (see Appendix 5). This implied the reform's benefits gradually strengthened as individuals progressed in employment.

Taken together, the evidence robustly supports **Hypothesis I**: raising the compulsory schooling age increased average earnings for affected individuals in the medium-run.

The reform's impact on unemployment is far less conclusive. While PSM ATT found a statistically significant 5.2 percentage point decrease in unemployment (SE=0.006) (see Appendix 5), the matched DiD found no statistically significant difference caused by the reforms. The 2013 reform effect (β_1=0.003, SE=0.011) and 2015 reform effect (β_2=-0.003, SE=0.009) were both indistinguishable from zero (Table 5.1). Matched Event Study results in Figure 5.2 further confirm this pattern with coefficients fluctuating around zero and wide confidence intervals throughout the medium-term post-reform period. Despite observing a statistically significant negative coefficient in 2015 (+3), this effect does not persist meaning no trend was established. Moreover, the parallel trends assumption was persistently violated for the unemployment outcome, reducing confidence in any causal interpretation regardless. Hence, **Hypothesis II** is not supported. Results do not provide robust evidence that raising the compulsory schooling age reduced average unemployment in the medium-run.

Figure 5.2



5.2. Discussion

This paper's findings on earnings are consistent with the existing literature. Oreopoulos (2007) found large and sustained wealth gains from compulsory schooling using cross-country data. Similarly, Buscha (2012) estimated long-run earnings gains of 5.5% from the 1972 UK reform. Our estimated 5.9% medium-run earnings gain from the 2015 reform aligns closely with Buscha's figure. A policy evaluation by Fischer (2020) reported slight earnings gains from extended schooling reform in Sweden but cautioned that no impact on unemployment was observed. Both of Fischer's results are analogous to this findings in England. Hall (2016) also found that extending compulsory schooling did not decrease unemployment.

With these comparisons, this paper's present findings can be contextualized. While reforms raising the compulsory schooling age reliably increase income, their impact on employment is more ambiguous. This paper contributes to the literature by analysing recent incremental changes in compulsory schooling on medium-term outcomes for earnings and unemployment. The novel staggered DiD specification captures heterogeneity between the 2013 and 2015 increments of the greater educational reform, revealing the latter extension to be more impactful. The analysis of a relatively recent policy increases relevance for policymaking.

This paper's findings offer encouraging evidence for policymakers considering similar reforms with the goal of increasing income. Extending compulsory schooling to age 18 appears to result in medium-run earnings gains, justifying its use as a tool to enhance labour market outcomes. Countries that have recently implemented such policies, such as Finland in 2021, may guide expectations from the case in England. Policymakers can anticipate delayed but significant effects on income. However, caution is warranted if the policy is proposed to improve unemployment outcomes, as no significant effects were established.

6. Robustness Checks and Limitations

This section discusses the robustness of the empirical strategies employed in the analysis, as well as limitations to be cautious of when considering the interpretation of results.

This paper's analysis is based on individual-level cross-sectional data from the APS, offering large, nationally representative samples. The survey consistently collects extensive information on key demographic and labour market variables. By harmonizing surveys from across multiple years into one repeated cross-sectional dataset, these variables can be tracked over time. The dataset's wide timespan, from 2004 to 2023, allows for analysis of more recent reforms with enough historical depth to effectively examine pre-policy trends, making it well-suited for analysing the given policy's medium-term labour market effects.

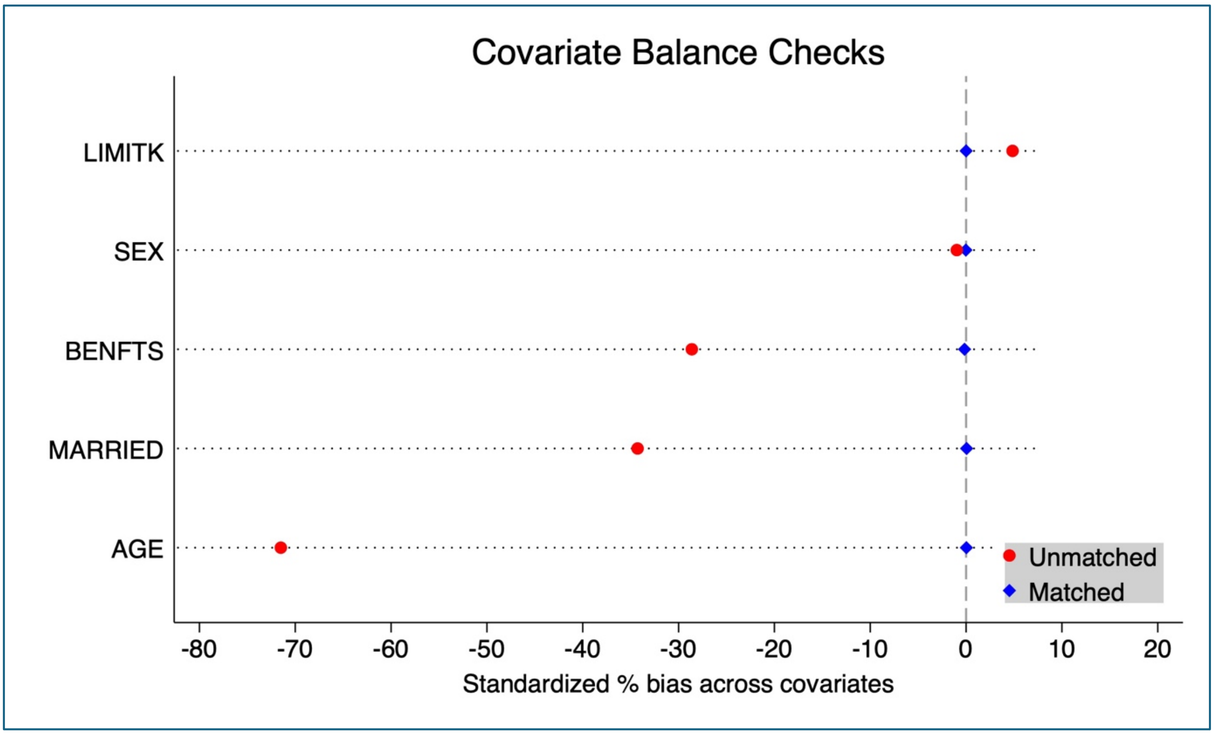
However, the repeated cross-sectional structure comes with notable limitations. Unlike panel data, tracking individuals over time is not possible, restricting the ability to investigate individual-specific dynamic responses to policy exposure or control for unobserved time-invariant individual heterogeneity. This prevents the use of more advanced fixed-effects strategies that rely on within-individual variation. Additionally, the nature of survey data leaves it susceptible to response bias.

6.1. Propensity Score Matching

The PSM model rests on the assumption of selection on observables, meaning all factors influencing treatment assignment and outcome are observed. This essentially aims to make treatment assignment as good as random conditional on observed covariates.

Covariate balance checks (Figure 6.1) demonstrate that the matching procedure effectively reduced differences between treated and control units, with all covariates exhibiting standardized biases well below the conventional 10% threshold after matching.

Figure 6.1



This suggests improved comparability and lends support to the robustness of the matched sample, providing some justification for the assumption of selection on observables. However, as with all matching methods, results remain sensitive to unobserved confounders that may still bias estimates of treatment effects.

6.2. Difference-in-Differences Model

As previously mentioned, the validity of the matched DiD model in estimating treatment effects hinges on the assumptions of parallel trends and no-spillover.

Initially, the parallel trends assumption was violated for both earnings and unemployment. After matching, the assumption held for earnings when examined in the pre-treatment period prior to policy announcement. However, the assumption remained violated for unemployment, raising concerns about the robustness of estimated treatment effects on unemployment, as trend differences potentially biasing results could not be ruled out.

To reduce spillover effects from cross-border movement, the sample was restricted to English nationals in England and non-English nationals in the control. While this mitigates treatment misclassification from migration, it cannot fully rule out other forms of spillover.

To further assess robustness, a placebo test on earnings was conducted with 2009 and 2011 as pseudo-treatment years to test for spurious treatment effects. The test results (see Appendix 6) support the robustness of the matched DiD estimation strategy as both placebo interaction terms were statistically insignificant, suggesting true treatment effect estimates are not spurious.

6.3. Event Study Model

Similarly to the matched DiD model, the validity of the matched Event Study model also depends on parallel trends and no-spillover assumptions. Therefore, the robustness considerations discussed above apply equally here.

As an additional robustness check, an anticipation effects test assessed pre-treatment dynamics up to 5 years before the reform. The test yielded a p-value of 0.492 (see Appendix 6), indicating no statistically significant pre-trends, strengthening the credibility of the matched Event Study estimates for earnings.

7. Conclusion

This paper set out to investigate the effect of England's incremental increase in compulsory schooling age, from 16 to 17 in 2013 and then to 18 in 2015, on medium-term labour market outcomes, specifically earnings and unemployment. Guided by human capital theory and empirical literature on compulsory schooling policy, this analysis used PSM to create a matched DiD model and matched Event Study model framework. These methodologies were applied to repeated cross-sectional data from the APS spanning 2004 to 2023 to isolate the causal effects of the reform on earnings and unemployment over the medium-term.

The empirical findings indicate that the 2015 reform, extending compulsory schooling to 18, significantly increased earnings for individuals exposed to the policy in the medium-term. This aligns with the human capital theory justification that further investment in education improves skills and productivity, which together enhance individual income prospects. The estimated 5.9% earnings rise reflects findings from earlier UK-based studies such as Buscha (2012), suggesting a continued relevance of compulsory schooling as a tool to enhance economic well-being and labour market outcomes. In contrast, no robust evidence was established to support a reduction in unemployment rates following the reform, which also aligns with findings from existing literature. In this case, violation of the parallel trends assumption weakened the robustness of unemployment outcome estimation.

This paper contributes to the existing literature by examining a recent and incremental policy change in the compulsory education age on medium-term labour market outcomes, addressing a gap where most previous studies focus on older reforms to investigate long-term outcomes. Additionally, limited literature exists exploring the implications of incremental adaptation of such educational policy, which this paper addresses using a matched DiD model specified with interaction terms for both policy increments.

However, several limitations must be acknowledged. Reliance on repeated cross-sectional data inhibits the analysis from capturing individual-level dynamics. Additionally, the inability to satisfy the parallel trends assumption for unemployment severely limits the ability to draw causal interpretations from its estimations.

The policymaking implications are cautiously optimistic. Raising the school leaving age can significantly improve earnings outcomes for those affected over the medium-term, supporting such reforms as a strategy for boosting economic well-being. However, policymakers should moderate expectations regarding unemployment effects and consider alternative policies if this is the key labour market outcome at hand.

To further develop this topic, future research should examine heterogeneous effects by sex, ethnicity, and background, as well as changes in educational attainment following the policy. Additionally, the use of panel data could improve robustness, particularly for the estimation of effects on unemployment, by controlling for individual fixed effects.

In summary, this paper finds that incremental increases in the schooling participation age in England have yielded positive earnings returns in the medium-term, with limited evidence of changes to unemployment outcomes. These findings reinforce the economic value of extending time in education with regards to income, while highlighting the importance of complementary policies to address labour market outcomes more broadly.

8. Bibliography

Angrist, J.D. and Krueger, A.B., 1991. Does compulsory school attendance affect schooling and earnings? *The Quarterly Journal of Economics*, 106(4), pp.979–1014.

Becker, G.S., 1964. *Human capital: A theoretical and empirical analysis, with special reference to education*. Chicago: University of Chicago Press.

Buscha, F., 2012. The raising of the school leaving age: Returns in later life. *Economics Letters*, 117(2), pp.389–393.

Card, D., 1999. The causal effect of education on earnings. In: O. Ashenfelter and D. Card, eds. *Handbook of Labor Economics*. 3rd ed. Amsterdam: Elsevier, pp.1801–1863.

Cedefop, 2023. *Medium and long-term labour market forecasts*. Available at: https://www.cedefop.europa.eu/en/tools/matching-skills/all-instruments/medium-and-long-term-labour-market-forecasts[Accessed 20 Apr. 2025].

Demirel-Derebasoglu, M. and Okten, C., 2022. Gender gap in intergenerational educational persistence: Can compulsory schooling reduce it? *Population Research and Policy Review*, 41(5), pp.2037–2083.

Devereux, P.J. and Hart, R.A., 2010. Forced to be rich? Returns to compulsory schooling in Britain. *The Economic Journal*, 120(549), pp.1345–1364.

Fischer, M., 2020. The long-term effects of long terms – Compulsory schooling reforms in Sweden. *Journal of the European Economic Association*, 18(6), pp.2776–2824.

Hall, C., 2016. Does more general education reduce the risk of future unemployment? Evidence from an expansion of vocational upper secondary education. *Economics of Education Review*, 52, pp.251–271.

Juusola, H., 2023. What do young people think about the extension of compulsory education? *Educational Research*, 65(4), pp.478–495. Available at: https://doi.org/10.1080/00131881.2023.2263885 [Accessed 20 Apr. 2025].Maguire, S., 2021. *Raising the Age of Participation to 18*. Cardiff: Wales Centre for Public Policy. Available at: https://www.wcpp.org.uk/wp-content/uploads/2021/02/Raising-the-Age-of-Participation-to-18.pdf [Accessed 20 Apr. 2025].

Office for National Statistics, 2024. *Annual Population Survey, January - December, 2023*. [data collection]. 2nd ed. UK Data Service. SN: 9248. Available at: http://doi.org/10.5255/UKDA-SN-9248-2 [Accessed 25 Nov. 2024].

Office for National Statistics, Social Survey Division, 2017. *Annual Population Survey Three-Year Pooled Dataset, January 2013 - December 2015*. [data collection]. 2nd ed. UK Data Service. SN: 8173. Available at: http://doi.org/10.5255/UKDA-SN-8173-2 [Accessed 25 Nov. 2024].

Office for National Statistics, Social Survey Division, 2024. *Annual Population Survey, January - December, 2011*. [data collection]. 7th ed. UK Data Service. SN: 7059. Available at: http://doi.org/10.5255/UKDA-SN-7059-7 [Accessed 25 Nov. 2024].

Oreopoulos, P., 2007. Do dropouts drop out too soon? Wealth, health and happiness from compulsory schooling. *Journal of Public Economics*, 91(11–12), pp.2213–2229.

Woodin, T., McCulloch, G. and Cowan, S., 2013. Raising the participation age in historical perspective: Policy learning from the past? *British Educational Research Journal*, 39(4), pp.635–653.

9. Appendix

3 - Data

Variable Labels

Description	Variable
Log gross weekly pay	ln_GRSSWK
Unemployment Indicator (1 = Unemployed, 0 = Employed)	UNEMP
Binary Indicator for Country within UK (1 = England, 0 = Wales, Scotland, Northern Ireland)	COUNTRY
Binary Indicator, 1 if (birthyear >= 1997 & COUNTRY == 1 & fileyear >= 2013)	TREATED
Year of birth	birthyear
Year of observation	fileyear
Age	AGE
Sex	SEX
Marriage Indicator (1 = married, 0 = otherwise)	MARRIED
English National Identity	NATIDE11
Whether health problem affects kind of work can do	LIMITK
Whether claiming (other) State Benefits	BENFTS
Whether working full or part Time	FTPT
Industry sector in main job	INDE07M
SOC2010 Main Job Major Group	SC10MMJ
Gross weekly pay in main job	GRSSWK
Enrolled on Full-Time/Part-Time education course (excluding leisure)	ENROLL
Categorical variable for highest level of educational qualification (0 = below lower secondary, 1 = lower secondary, 2 = upper secondary, 3 = undergraduate and above)	edu

4 – Methodology

Parallel Trends Test Output - Earnings

```
. reg ln_GRSSWK int_treat_2005-int_treat_2012 i.COUNTRY i.fileyear AGE SEX MARRIED, robust

Linear regression                               Number of obs     =    554,881
                                                F(20, 554860)     =    4602.73
                                                Prob > F          =     0.0000
                                                R-squared         =     0.1395
                                                Root MSE          =     .72917

------------------------------------------------------------------------------
             |               Robust
   ln_GRSSWK | Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
int_treat_2005 |  -.0148462   .0139695    -1.06   0.288     -.042226    .0125335
int_treat_2006 |  -.0191922   .0076926    -2.49   0.013    -.0342693    -.004115
int_treat_2007 |  -.0263746   .0077237    -3.41   0.001    -.0415129   -.0112364
int_treat_2008 |  -.0301573   .0077705    -3.88   0.000    -.0453873   -.0149273
int_treat_2009 |  -.0413762   .0079584    -5.20   0.000    -.0569744   -.0257781
int_treat_2010 |  -.0352852   .0079905    -4.42   0.000    -.0509464   -.0196241
int_treat_2011 |   -.035102   .0081778    -4.29   0.000    -.0511302   -.0190738
int_treat_2012 |  -.0312306   .0080732    -3.87   0.000    -.0470538   -.0154075
             |
     COUNTRY |
    England  |   .0205759   .0054118     3.80   0.000     .0099689    .0311829
             |
    fileyear |
       2005  |   .0394468   .0107337     3.68   0.000     .0184091    .0604846
       2006  |   .0921155   .0059219    15.56   0.000     .0805088    .1037221
       2007  |   .1302376   .0059662    21.83   0.000     .1185442    .1419311
       2008  |    .171556   .0060285    28.46   0.000     .1597403    .1833717
       2009  |   .1893983   .0061756    30.67   0.000     .1772943    .2015023
       2010  |   .1953302   .0062102    31.45   0.000     .1831585     .207502
       2011  |   .2056281   .0064194    32.03   0.000     .1930464    .2182099
       2012  |    .233355   .0063027    37.02   0.000     .2210019    .2457082
             |
         AGE |   .0049202   .0001007    48.88   0.000     .0047229    .0051175
         SEX |  -.5097334   .0019521  -261.11   0.000    -.5135596   -.5059073
     MARRIED |   .1612424   .0020499    78.66   0.000     .1572247    .1652601
       _cons |   6.017192   .0065213   922.70   0.000     6.004411    6.029974
------------------------------------------------------------------------------

. test int_treat_2005 int_treat_2006 int_treat_2007 int_treat_2008 int_treat_2009 int_treat_2010 int_treat_2011 int_treat_2012

 ( 1)  int_treat_2005 = 0
 ( 2)  int_treat_2006 = 0
 ( 3)  int_treat_2007 = 0
 ( 4)  int_treat_2008 = 0
 ( 5)  int_treat_2009 = 0
 ( 6)  int_treat_2010 = 0
 ( 7)  int_treat_2011 = 0
 ( 8)  int_treat_2012 = 0

       F(  8,554860) =    4.79
            Prob > F =    0.0000
```

Parallel Trends Test Output - Unemployment

```
. reg UNEMP int_treat_2005-int_treat_2012 AGE SEX MARRIED i.fileyear i.COUNTRY, robust

Linear regression                               Number of obs     =    609,012
                                                F(20, 608991)     =    1329.32
                                                Prob > F          =     0.0000
                                                R-squared         =     0.0549
                                                Root MSE          =     .27666

------------------------------------------------------------------------------
             |               Robust
       UNEMP | Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
int_treat_2005 |   .0081326   .0046723     1.74   0.082     -.001025    .0172903
int_treat_2006 |   .0101592   .0025621     3.97   0.000     .0051375     .015181
int_treat_2007 |   .0101302   .0025377     3.99   0.000     .0051563    .0151042
int_treat_2008 |   .0152078   .0026177     5.81   0.000     .0100772    .0203383
int_treat_2009 |   .0160853   .0028751     5.59   0.000     .0104502    .0217204
int_treat_2010 |   .0108585   .0028812     3.77   0.000     .0052114    .0165056
int_treat_2011 |   .0073841   .0030003     2.46   0.014     .0015036    .0132646
int_treat_2012 |   .0062812   .0029763     2.11   0.035     .0004478    .0121146
         AGE |  -.0025153   .0000329   -76.37   0.000    -.0025799   -.0024508
         SEX |  -.0375307   .0007139   -52.57   0.000    -.0389299   -.0361315
     MARRIED |  -.0786029   .0007309  -107.54   0.000    -.0800354   -.0771703
             |
    fileyear |
       2005  |   .0013614   .0036314     0.37   0.708    -.0057561    .0084788
       2006  |   .0025447   .0020053     1.27   0.204    -.0013857     .006475
       2007  |  -.0007885   .0019931    -0.40   0.692    -.0046949    .0031178
       2008  |   .0029133   .0020426     1.43   0.154    -.0010902    .0069167
       2009  |   .0316342   .0022551    14.03   0.000     .0272142    .0360542
       2010  |   .0346256   .0022656    15.28   0.000     .0301851    .0390662
       2011  |   .0418924   .0023934    17.50   0.000     .0372015    .0465833
       2012  |   .0417618   .0023625    17.68   0.000     .0371315    .0463922
             |
     COUNTRY |
    England  |  -.0093665   .0017574    -5.33   0.000    -.0128108   -.0059221
       _cons |   .2710529   .0022788   118.95   0.000     .2665866    .2755192
------------------------------------------------------------------------------

. test int_treat_2005 int_treat_2006 int_treat_2007 int_treat_2008 int_treat_2009 int_treat_2010 int_treat_2011 int_treat_2012

 ( 1)  int_treat_2005 = 0
 ( 2)  int_treat_2006 = 0
 ( 3)  int_treat_2007 = 0
 ( 4)  int_treat_2008 = 0
 ( 5)  int_treat_2009 = 0
 ( 6)  int_treat_2010 = 0
 ( 7)  int_treat_2011 = 0
 ( 8)  int_treat_2012 = 0

       F(  8,608991) =    6.16
            Prob > F =    0.0000
```

Matched Sample Parallel Trends Test Output - Earnings

```
. reg ln_GRSSWK int_treat_2005-int_treat_2007 AGE SEX MARRIED LIMITK BENFTS FTPT, robust
note: int_treat_2005 omitted because of collinearity.
note: MARRIED omitted because of collinearity.

Linear regression                               Number of obs     =      2,191
                                                F(7, 2183)        =     228.96
                                                Prob > F          =     0.0000
                                                R-squared         =     0.3926
                                                Root MSE          =     .63174

------------------------------------------------------------------------------
             |               Robust
   ln_GRSSWK | Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
int_treat_2005 |        0  (omitted)
int_treat_2006 |  -.0277721   .0426037    -0.65   0.515    -.1113202    .0557759
int_treat_2007 |   .0277745   .0303785     0.91   0.361    -.0317993    .0873483
         AGE |   .2542008   .0300504     8.46   0.000     .1952704    .3131311
         SEX |  -.0430523   .0284972    -1.51   0.131    -.0989368    .0128321
     MARRIED |          0  (omitted)
      LIMITK |   .0029818   .0041965     0.71   0.477    -.0052478    .0112114
      BENFTS |     .04459   .0402893     1.11   0.269    -.0344195    .1235995
        FTPT |  -1.068824   .0306429   -34.88   0.000    -1.128916   -1.008731
       _cons |   1.639454   .5083182     3.23   0.001     .6426155    2.636292
------------------------------------------------------------------------------

. test int_treat_2005 int_treat_2006 int_treat_2007

 ( 1)  o.int_treat_2005 = 0
 ( 2)  int_treat_2006 = 0
 ( 3)  int_treat_2007 = 0
       Constraint 1 dropped

       F(  2,  2183) =    0.89
            Prob > F =    0.4103
```

Matched Sample Parallel Trends Test Output - Unemployment

```
. reg UNEMP int_treat_2005-int_treat_2007 AGE SEX MARRIED LIMITK BENFTS FTPT, robust
note: int_treat_2005 omitted because of collinearity.
note: MARRIED omitted because of collinearity.

Linear regression                               Number of obs     =      3,375
                                                F(7, 3367)        >   99999.00
                                                Prob > F          =     0.0000
                                                R-squared         =     0.9955
                                                Root MSE          =     .03201

------------------------------------------------------------------------------
             |               Robust
       UNEMP | Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
int_treat_2005 |        0  (omitted)
int_treat_2006 |   .0047693   .0015221     3.13   0.002      .001785    .0077536
int_treat_2007 |   .0065546    .001278     5.13   0.000      .004049    .0090603
         AGE |  -.0064846   .0012858    -5.04   0.000    -.0090056   -.0039635
         SEX |   .0107873   .0011283     9.56   0.000     .0085752    .0129994
     MARRIED |          0  (omitted)
      LIMITK |   .0000757   .0001418     0.53   0.594    -.0002024    .0003538
      BENFTS |  -.0086606   .0021533    -4.02   0.000    -.0128825   -.0044387
        FTPT |  -.0926811   .0000745 -1244.32   0.000    -.0928271   -.0925351
       _cons |   .2663266   .0221499    12.02   0.000     .2228979    .3097553
------------------------------------------------------------------------------

. test int_treat_2005 int_treat_2006 int_treat_2007

 ( 1)  o.int_treat_2005 = 0
 ( 2)  int_treat_2006 = 0
 ( 3)  int_treat_2007 = 0
       Constraint 1 dropped

       F(  2,  3367) =   13.65
            Prob > F =    0.0000
```

5 - Results

PSM ATT Estimation on Earnings

Variable	Sample	Treated	Controls	Difference	S.E.
ln_GRSSWK	Unmatched	5.13489594	5.28948822	-.154592274	.011096674
	ATT	5.13489594	4.86927303	.265622912	.014432585

PSM ATT Estimation on Unemployment

Variable	Sample	Treated	Controls	Difference	S.E.
UNEMP	Unmatched	.22480686	.216733174	.008073686	.004090292
	ATT	.22480686	.277188216	-.052381357	.005639776

Event Study Regression Output - Earnings

```
. reg ln_GRSSWK e2013_event_* i.fileyear SEX MARRIED LIMITK BENFTS if matched_wage == 1, robust

Linear regression                               Number of obs     =     68,017
                                                F(34, 67982)      =    1301.26
                                                Prob > F          =     0.0000
                                                R-squared         =     0.3465
                                                Root MSE          =     .79891

------------------------------------------------------------------------------
             |               Robust
   ln_GRSSWK | Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
e2013_event_m3 |  -.0233749   .0332577    -0.70   0.482     -.08856    .0418101
e2013_event_m2 |   .0159909   .0303693     0.53   0.599    -.043533    .0755147
e2013_event_m1 |    .012226   .0299403     0.41   0.683   -.0464571     .070909
e2013_event_p1 |  -.0710996   .0358689    -1.98   0.047   -.1414027   -.0007965
e2013_event_p2 |   .0415841   .0276089     1.51   0.132   -.0125292    .0956975
e2013_event_p3 |   .0380768   .0284384     1.34   0.181   -.0176624    .0938161
e2013_event_p4 |   .0140778   .0303118     0.46   0.642   -.0453333    .0734889
e2013_event_p5 |   .0703034   .0224932     3.13   0.002    .0262168    .1143901
e2013_event_p6 |   .0538613   .0220291     2.45   0.014    .0106843    .0970383
e2013_event_p7 |   .0411721   .0265254     1.55   0.121   -.0108175    .0931618
e2013_event_p8 |   .0773734   .0229727     3.37   0.001    .0323469       .1224
e2013_event_p9 |   .0815739    .022513     3.62   0.000    .0374485    .1256993
e2013_event_p10 |  .0467613    .017959     2.60   0.009    .0115617    .0819608
             |
    fileyear |
       2007  |   .2220951   .0379378     5.85   0.000     .147737    .2964532
       2008  |   .5336599   .0362479    14.72   0.000    .4626141    .6047058
       2009  |   .6427064   .0417589    15.39   0.000    .5608591    .7245538
       2010  |    .714806   .0402219    17.77   0.000    .6359712    .7936408
       2011  |   .8643075   .0405338    21.32   0.000    .7848614    .9437537
       2012  |   1.032961   .0349286    29.57   0.000    .9645016    1.101421
       2013  |   1.172007    .042211    27.77   0.000    1.089273     1.25474
       2014  |   1.278655   .0394416    32.42   0.000     1.20135    1.355961
       2015  |   1.390852   .0397277    35.01   0.000    1.312986    1.468719
       2016  |   1.446811   .0401225    36.06   0.000    1.368171    1.525451
       2017  |   1.571157   .0372381    42.19   0.000     1.49817    1.644144
       2018  |    1.69108   .0367988    45.95   0.000    1.618954    1.763206
       2019  |    1.72795   .0381501    45.29   0.000    1.653176    1.802724
       2020  |   1.827509   .0374289    48.83   0.000    1.754148    1.900869
       2021  |   1.943004   .0371615    52.29   0.000    1.870168    2.015841
       2022  |   2.041039   .0352841    57.85   0.000    1.971882    2.110196
       2023  |   2.197878    .033644    65.33   0.000    2.131935     2.26382
             |
         SEX |  -.2271629   .0063815   -35.60   0.000   -.2396706   -.2146552
     MARRIED |   .4099921   .0085618    47.89   0.000     .393211    .4267733
      LIMITK |   -.004544    .000727    -6.25   0.000   -.0059689   -.0031192
      BENFTS |   .2089997   .0116737    17.90   0.000    .1861193    .2318802
       _cons |    3.71591   .0426764    87.07   0.000    3.632265    3.799556
------------------------------------------------------------------------------
```

Event Study Regression Output - Unemployment

```
. reg UNEMP e2013_event_* i.fileyear SEX MARRIED LIMITK BENFTS if matched_wage == 1, robust

Linear regression                               Number of obs     =     86,959
                                                F(34, 86924)      =     301.31
                                                Prob > F          =     0.0000
                                                R-squared         =     0.1468
                                                Root MSE          =     .38136

------------------------------------------------------------------------------
             |               Robust
       UNEMP | Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
e2013_event_m3 |  -.0109482    .014344    -0.76   0.445    -.0390623    .0171658
e2013_event_m2 |  -.0141709   .0128054    -1.11   0.268    -.0392694    .0109276
e2013_event_m1 |  -.0116167   .0122175    -0.95   0.342     -.035563    .0123296
e2013_event_p1 |   .0272097   .0146708     1.85   0.064     -.001545    .0559644
e2013_event_p2 |  -.0158172   .0113654    -1.39   0.164    -.0380932    .0064588
e2013_event_p3 |  -.0270806   .0116167    -2.33   0.020    -.0498492    -.004312
e2013_event_p4 |   .0037132   .0122024     0.30   0.761    -.0202034    .0276298
e2013_event_p5 |   .0015029   .0086224     0.17   0.862    -.0153969    .0184027
e2013_event_p6 |  -.0029806   .0088895    -0.34   0.737     -.020404    .0144427
e2013_event_p7 |   .0070183   .0099497     0.71   0.481    -.0124829    .0265196
e2013_event_p8 |   .0058757    .008794     0.67   0.504    -.0113604    .0231118
e2013_event_p9 |  -.0121353   .0088502    -1.37   0.170    -.0294816     .005211
e2013_event_p10 |  -.006778   .0067343    -1.01   0.314    -.0199772    .0064211
             |
    fileyear |
       2007  |  -.0676506   .0183611    -3.68   0.000    -.1036382    -.031663
       2008  |  -.0960539   .0174452    -5.51   0.000    -.1302464   -.0618614
       2009  |  -.0629481    .019487    -3.23   0.001    -.1011424   -.0247537
       2010  |  -.0766295   .0186803    -4.10   0.000    -.1132426   -.0400163
       2011  |  -.0608754   .0185901    -3.27   0.001    -.0973119   -.0244389
       2012  |  -.0809169   .0165817    -4.88   0.000    -.1134169    -.048417
       2013  |  -.1361306   .0195321    -6.97   0.000    -.1744133   -.0978478
       2014  |   -.148361   .0182808    -8.12   0.000    -.1841913   -.1125308
       2015  |  -.1976504   .0183733   -10.76   0.000    -.2336619   -.1616389
       2016  |  -.2271637   .0184558   -12.31   0.000    -.2633368   -.1909905
       2017  |  -.2627637    .017168   -15.31   0.000    -.2964129   -.2291145
       2018  |  -.2772899   .0172053   -16.12   0.000    -.3110121   -.2435678
       2019  |  -.2810694   .0173204   -16.23   0.000    -.3150173   -.2471215
       2020  |  -.2855408   .0171677   -16.63   0.000    -.3191894   -.2518922
       2021  |  -.3013658   .0172455   -17.47   0.000    -.3351669   -.2675646
       2022  |  -.3199154   .0165275   -19.36   0.000    -.3523092   -.2875216
       2023  |  -.3283861   .0161696   -20.31   0.000    -.3600785   -.2966938
             |
         SEX |  -.0831504   .0026736   -31.10   0.000    -.0883907   -.0779101
     MARRIED |  -.1356504   .0037364   -36.30   0.000    -.1429738    -.128327
      LIMITK |   .0042673   .0003203    13.32   0.000     .0036394    .0048952
      BENFTS |  -.2565089   .0097602   -26.28   0.000    -.2756388   -.2373791
       _cons |   1.044861   .0255553    40.89   0.000     .9947726    1.094949
------------------------------------------------------------------------------
```

6 – Robustness Checks and Limitations

DiD Placebo Test

```
. reg ln_GRSSWK COUNTRY##placebo_post1 COUNTRY##placebo_post2 i.fileyear SEX MARRIED LIMITK BENFTS if matched_wage == 1, robust
note: 2009.fileyear omitted because of collinearity.
note: 2011.fileyear omitted because of collinearity.

Linear regression                               Number of obs     =     68,017
                                                F(24, 67992)      =    1843.87
                                                Prob > F          =     0.0000
                                                R-squared         =     0.3463
                                                Root MSE          =       .799

------------------------------------------------------------------------------
                       |               Robust
             ln_GRSSWK | Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-----------------------+------------------------------------------------------------------
               COUNTRY |
              England  |   .0356198   .0065846     5.41   0.000     .0227139    .0485257
       1.placebo_post1 |   .6641362   .0419792    15.82   0.000      .581857    .7464155
                       |
COUNTRY#placebo_post1  |
            England#1  |  -.0590007   .0338992    -1.74   0.082     -.125443    .0074416
                       |
       1.placebo_post2 |   .8857321   .0407615    21.73   0.000     .8058397    .9656245
                       |
COUNTRY#placebo_post2  |
            England#1  |  -.0233935   .0306525    -0.76   0.445    -.0834724    .0366853
                       |
              fileyear |
                 2007  |   .2214859       .038     5.83   0.000     .1470059    .2959659
                 2008  |   .5331158   .0363023    14.69   0.000     .4619633    .6042683
                 2009  |          0  (omitted)
                 2010  |   .7236335   .0356121    20.32   0.000     .6538338    .7934332
                 2011  |          0  (omitted)
                 2012  |   1.031082   .0349811    29.48   0.000     .9625191    1.099645
                 2013  |   1.126479   .0370178    30.43   0.000     1.053925    1.199034
                 2014  |   1.304192   .0349352    37.33   0.000     1.235719    1.372665
                 2015  |   1.413907   .0351292    40.25   0.000     1.345054    1.482761
                 2016  |   1.454729   .0356555    40.80   0.000     1.384844    1.524614
                 2017  |   1.615564   .0341333    47.33   0.000     1.548663    1.682466
                 2018  |    1.72411   .0341618    50.47   0.000     1.657153    1.791067
                 2019  |   1.752567   .0350277    50.03   0.000     1.683913    1.821221
                 2020  |   1.875777     .03426    54.75   0.000     1.808628    1.942927
                 2021  |   1.993979   .0342132    58.28   0.000     1.926921    2.061037
                 2022  |   2.069065   .0336964    61.40   0.000      2.00302     2.13511
                 2023  |   2.199203   .0336943    65.27   0.000     2.133162    2.265243
                       |
                   SEX |  -.2273227   .0063819   -35.62   0.000    -.2398312   -.2148141
               MARRIED |   .4097857   .0085662    47.84   0.000      .392996    .4265753
                LIMITK |   -.004541   .0007269    -6.25   0.000    -.0059656   -.0031164
                BENFTS |   .2088796   .0116733    17.89   0.000         .186    .2317593
                 _cons |   3.694984   .0429333    86.06   0.000     3.610834    3.779133
------------------------------------------------------------------------------
```

Anticipation Effects Test

```
. testparm 5.event_time_shift#1.COUNTRY 6.event_time_shift#1.COUNTRY 7.event_time_shift#1.COUNTRY 8.event_time_shift#1.COUNTRY 9.event_time_shift#1.COUNTRY

 ( 1)  5.event_time_shift#1.COUNTRY = 0
 ( 2)  6.event_time_shift#1.COUNTRY = 0
 ( 3)  7.event_time_shift#1.COUNTRY = 0
 ( 4)  8.event_time_shift#1.COUNTRY = 0
 ( 5)  9.event_time_shift#1.COUNTRY = 0

       F(  5, 86920) =    0.88
            Prob > F =    0.4920
```