Returning to Returns: Revisiting the Education Evidence

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This paper revisits the question of what is the rate of return to education. We make four important contributions. Firstly we re-assess the robustness of the papers by Harmon and Walker (1995), Oreopoulos (2006) and Devereux and Hart (2010) to equation specification and data set sensitivity. Secondly we generalize the IV approach of the previous papers by using the month of birth in the calculation of a more accurate IV. Thirdly, we uniquely compare each of the three UK Raising of the School Leaving Age (ROSLA) reforms in 1947, 1962 and 1972. Finally, we compare the parametric estimates obtained by the IV with the alternative of non-parametric bounds analysis. Our results provide a robust case for a 6% (Average Treatment Effect) return to education which is coherent across different datasets, estimation methods and specifications for men.

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I. Introduction

The rate of return to education (RoRtE) is widely regarded as the most important estimable parameter in labour economics. This is attested by the large associated economic literature. It is most disturbing that this single parameter – on which so much higher education policy is predicated – should be so widely and differently estimated. At one extreme the estimate is 0.0, at the other it is 0.15 (e.g., Harmon and Walker 1995). So much is riding on the estimate of this parameter that it is difficult to understate its importance. Specifically if the RoRtE is zero then parents, education authorities and governments should not encourage staying on at school or going to university. On the other hand if the RoRtE is 15% then a continued policy of high level incentives to acquire more education is obviously justified.

The core problem, of course, is that the decision to acquire more education, or stay on at school, is not one which is independent of potential future anticipated earnings associated with this extra educational acquisition. More specifically, we expect that the unobservables in an earnings equation (namely motivation, energy, stamina, determination, personality) can and do affect both education and wages. This endogeneity problem has plagued the estimation of any RoRtE parameter in regression studies, which means there is considerable uncertainty about the derived point estimates which may be conditional on the unrealistic assumptions of Ordinary Least Squares (OLS) estimation or the specific instrumental variable (IV) identification strategy used. In much of the RoRtE literature, researchers have relied on IV estimation to retrieve unbiased and consistent treatment effects of education (e.g., Angrist and Krueger, 1991; Card, 1995; Harmon and Walker, 1995; Acemoglu and Angrist, 2001). This IV identification strategy relies on the assumption that the chosen IV reflects on the decision of an individual to attend school, but does not directly influence future earnings (i.e., the outcome of interest); if this assumption is satisfied, use of IV can eliminate the estimation bias that derives from the endogeneity of the educational choices of individuals in the determination of their professional trajectories and, ultimately, of their future earnings. As pointed out by Imbens and Angrist (1994), the parameter identified by this procedure is the local average treatment effect (LATE), i.e., the average treatment effect among the compliers. In the particular case in which the decision to react to the instrument is not based on the same factors that also affect treatment gains, the LATE also coincides with the average treatment effect (ATE) among the individuals exposed to the treatment (Heckman, 1997). The contention of Oreopoulos (2006) that the LATE of a ROSLA reform is likely to be closer to an ATE of the return to education, the larger is the fraction of people affected by the ROSLA is likely to be true – but this must depend on using a first stage of the RDD/IV design which fully nets out for underlying trends and extraneous influences on the schooling decision.

Two-stage IV estimation strategies have been employed in recent years by a growing body of the applied economics literature that exploits the enforcement of discontinuous rules in settings where the treatment is determined partly by whether the assignment variable crosses a cutoff point. In these settings, commonly known as "fuzzy" regression discontinuity (RD) designs, the probability of treatment jumps at the threshold. The usual assumption required in this approach is that, conditional on the covariates, the treatment (i.e., the instrument) is as good as randomized; if there is local random assignment, then all the assumptions and interpretation rules of the IV strategy can be applied in order to retrieve an unbiased parameter of the treatment effect (Lee and Lemieux, 2010). Using Lee and Lemieux's (2010) terminology, Y is the outcome variable of interest, D the binary treatment indicator and X the observed assignment variable. In a binary treatment-binary instrument context with unrestricted heterogeneity in treatment effects, the IV estimand is interpreted as the LATE; in the fuzzy RD setting, the estimand can be interpreted as a weighted LATE, where the weights reflect the ex-ante probability of the observation's X being near the threshold. In both cases, the exclusion restriction and monotonicity condition are still required. Analogously, in the continuous treatment-binary instrument case, the local random assignment still applies and facilitates IV estimation. This means we can interpret the RD estimand as in Angrist and Krueger (1999), except for the fact that all averages need to be weighted by the ex-ante relative probability that the observation's X is near the threshold.

Lee and Lemieux (2010) offer a comprehensive survey of the applied economic research that have applied RD designs; they describe how both sharp and fuzzy RD designs have been employed in the analysis of labour markets (e.g., DiNardo and Lee, 2004; Black, Galdo and Smith, 2007; Chen and van der Klaauw, 2008)[2]. Educational outcomes are also among the most frequent applications of the RD design; Angrist and Lavy (1999), Clark (2009) and Urquiola and Verhoogen (2009) are only few of the studies that assess the impact on schooling outcomes of a number of discontinuous rules that give rise to an RD design.

In this paper we focus on a particular application of the RD design, namely the literature that investigates the rate of return to education. To be precise, we exploit the law changes in the school-leaving ages that took place in 1947, 1962 and 1972 in Great Britain to present new evidence on the

[2] RD methods have also been applied to: health economics (e.g., Card, Dobkin and Maestas, 2008; Card, Dobkin and Maestas, 2009; more recently, Clark and Royer, 2013); political economy (e.g., Lee, 2008; Ferreira and Gyourko 2009), to the analysis of crime (e.g., Berk and DeLeeuw, 1999) and environmental outcomes (Davis, 2008).

returns to compulsory education. In 1947 the minimum school leaving age was raised from 14 to 15. In 1962 the actual school leaving dates were modified for pupils born in certain months of the calendar year. In 1972 the age of compulsory schooling increased from 15 to 16. Due to the historically high dropout rates in Great Britain, and the remarkable effect of these policies on overall schooling attainment, this constitutes a particularly interesting context for the analysis of this parameter. We apply both parametric and non-parametric estimators to data from the General Household Survey (GHS) and the Family Expenditure Survey (FES); these datasets have been previously analyzed by Harmon and Walker (1995), Oreopoulos (2006) and Devereux and Hart (2010). Unlike compulsory schooling changes in the United States, that affected 5 percent of the relevant cohorts (Lleras-Muney 2005)[3], the high fraction of population affected by the SLA changes in Great Britain arguably allows us to retrieve an estimate of the gains from schooling closer to the average treatment effect (ATE) for the entire population than any previously reported (Oreopoulos 2006), and not just the effects for smaller subpopulations, that may be of limited interest (Heckman 2010). Moreover, the relevance of these policies for educational attainment also implies that we avoid the problems of weak instruments often encountered in this literature[4].

This paper makes four important contributions: firstly we re-assess the most important contributions to the debate by examining the robustness of the papers by Harmon and Walker (1995), Oreopoulos (2006) and Devereux and Hart (2010). We do this by using all the available data and examining the sensitivity of the results to the specification of the relevant earnings function and the sample used. We seek the optimal specification of the relevant earnings function using Akaike's criterion. We also test the robustness of previous results to the use of nonparametric estimation techniques in the presence of an RD design. Our replication analysis provides the means whereby the widely varying results of these three papers can be reconciled. Secondly we generalize the IV approach of the previous papers by using the month of birth in conjunction with the ROSLA in the calculation of a more accurate IV. In contrast with the previous literature, our IV also incorporates the additional variation within year-of-birth cohorts in the school leaving dates introduced by the 1962 reform. Therefore, our IV reflects the *de-facto* additional amount of compulsory schooling implied by all ROSLA reforms. We show that this approach provides more consistency in the results. Thirdly, we compare each of the three Raising of the School Leaving Age (ROSLA) reforms and carefully explain what was different about each reform. Finally, being cognicent of the limitations of the IV

[3] The effects in other countries—e.g., Norway, Canada, and France—are similarly small (Albouy and Lequien 2009; Black, Devereux, and Salvanes 2008; Lleras-Muney 2005; Oreopoulos 2006).
[4] See for example Clark and Royer 2013.

strategies we compare the parametric estimates obtained by the IV identification strategy with the alternative of non-parametric bounds analysis. Again we find that our results are very consistent with our previous results. In our conclusion, we then reflect on the general applicability of our results for the interpretation of the rate of return estimator as a 'causal' parameter.

Our results suggest that estimates of the returns to compulsory education found in some previous literature are very sensitive to both the particular analysis sample used in the estimation and the functional form chosen. Since the relevant compulsory schooling changes depended on month-year-of-birth, we do not use year-of-birth comparisons. Instead, we use the available information from the 1983-2000 GHS survey years on the month of birth of individuals; by doing so, not only do we increase precision in the description of the effects of the 1947, 1962 and 1972 SLA changes, but also we redefine our instrument in a way that better captures the exposure of the additional schooling time implied by these policy interventions. The use of our new instrument, along with an innovative identification strategy, lead to the derivation of more robust results. Finally, we extend our analysis by using the non-parametric bounds method of Manski (1997) and Manski and Pepper (2000) and test our parametric results against an alternative, non-parametric approach[5]. Given the high fraction of population affected by the SLA changes in Great Britain, and the availability of large datasets, we believe that this constitutes a particularly relevant context in which to apply non-parametric bounds analysis and compare these results to those from a parametric IV approach. The findings from our bounds analysis are consistent with the evidence from our parametric analysis.

In the next section, we examine carefully the compulsory schooling policies implemented in Britain from the World War II onwards. In section three we review the existing literature on the returns to compulsory education in Britain. In section four we describe the strategy employed in our replication analysis and show the specification sensitivity of some previously reported estimates of the returns to education. In section five we present our proposed parametric alternative, with a redefined instrumental variable and an innovative identification strategy for our parametric IV analysis of the returns to compulsory schooling. In section six, we describe the strategy and present the results from our bounds analysis of the returns to compulsory education. In section seven we conclude and we reflect on the meaning and external validity of our estimation results.

[5] Apart from Manski and Pepper (2000), at the time of writing only Giustinelli (2011) has applied non-parametric bounds to the analysis of returns to education, presenting evidence from Italy.

II. Analysing the Compulsory Schooling Law Changes

The first major change to the age of compulsory schooling examined in this study took place in Britain in 1947. The post-war society had changed sufficiently that 14 years old were considered too young to leave school and start working, and in response to the claim that secondary education needed to become available for all, the Education Act 1944 was passed. As a result of this, the minimum school leaving age increased from 14 to 15 on April 1st 1947. At that time, pupils were assessed with an exam at age 11 (the 11 plus exam). On the basis of this exam, pupils were assigned to one of the pathways of the tripartite system of post-primary education, namely Grammar schools, Technical schools and Secondary Modern schools. Following the 1944 Education Act, all schooling pathways were free of charge. The highest-scoring pupils were selected for Grammar schools[6], and the majority of the remaining students attended secondary modern schools. Since the provisions in the 1944 Education Act were only implemented on April 1st 1947, this implies that the relevant cutoff date of birth is April 1st 1933. Those born just before this date were not exposed to the new law regime, whereas the opposite is true for those born in its immediate aftermath. This gives rise to the first discontinuity assessed in this study.

The 1944 Education Act also gave power to the Minister of Education to raise the age of compulsory schooling from 15 to 16, at the earliest possible convenience. Through the Statutory Instrument No. 444, the Education Minister did so in March 1972, and the age of compulsory schooling was raised to 16 on September 1st 1972. This, in turn, implies that the relevant cutoff date of birth for the second policy change investigated in this paper is September 1st 1957; persons born before this date faced a minimum school leaving age of 15, and persons born from September 1st 1957 onwards faced a minimum age of 16. Figure 1 and Figure 2 were calculated on the entire British-born sample available from the 1979-2006 GHS survey years. They illustrate the impact of these policies on the educational attainment of British pupils, both for the pooled sample, and for the male sample only; however, returns to schooling are likely to differ across gender, and in the core section of the paper the discussion will focus exclusively on males. Results for the pooled sample are presented in the appendices.

Two things appear from Figure 1 and Figure 2; first, both laws had a strong and clear impact on school leaving behaviour. Both in 1947 and 1972, the fraction of pupils leaving school before the

[6] It should be noted that there was effectively a different pass mark for different Local Education Authorities (LEAs) and for boys and girls. It should also be noted that some pupils who passed the 11-Plus either did not go to Grammar schools or went to Technical schools. Technical schools did not constitute a large fraction of schools and were not available on a countrywide basis.

(new) minimum school leaving age dropped sharply; the fraction leaving school before age 15 fell from approximately 60 percent for the cohort that turned 14 just before April 1947 to approximately 10 percent for the cohort that turned 14 immediately after the cutoff point. In 1972, for pupils that turned 15 at around the cutoff point, the change in minimum school leaving age appears to have decreased the proportion of pupils leaving school by 15 years of age from 35 percent to less than 10 percent. The second fact of relevance that should be observed from Figure 1 and Figure 2 is that, in 1947, the proportion of pupils that left school by age 15 was not affected by the policy intervention. Similarly, the 1972 policy intervention did not seem to influence the fraction of pupils that left school by the age of 16. This, in turn, implies that both policies resulted in British pupils still leaving education at the earliest possible convenience. These findings are in line with the existing evidence from Britain (e.g., Oreopoulos, 2006; Devereux and Hart, 2010; Clark and Royer, 2013).

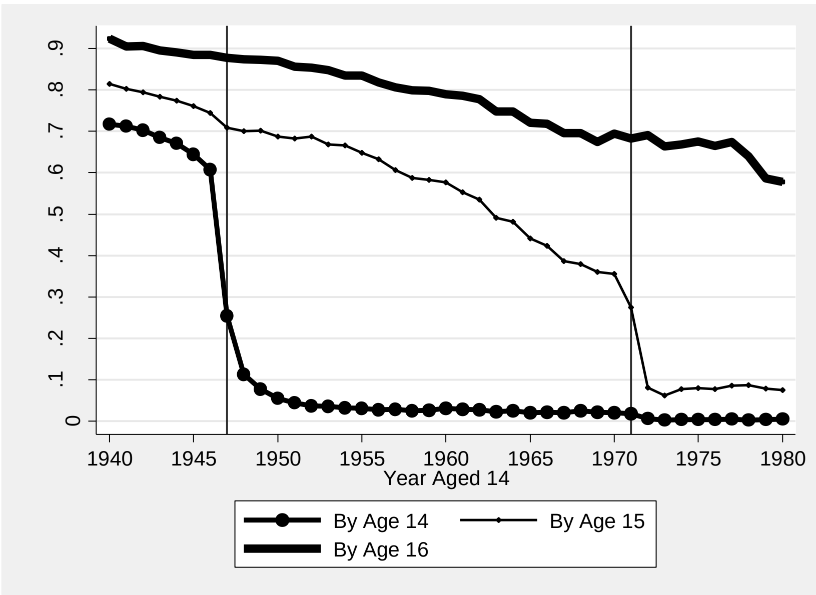

FIGURE 1. FRACTION LEFT FULL-TIME EDUCATION BY YEAR AGED 14, 15 AND 16

(Pooled Sample)

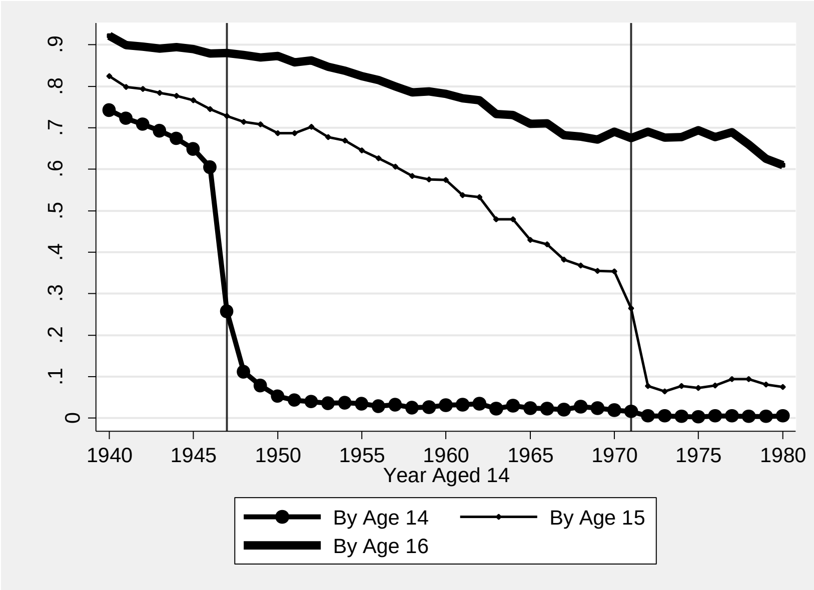

FIGURE 2. FRACTION LEFT FULL-TIME EDUCATION BY YEAR AGED 14, 15 AND 16

(Male Sample)

However, a careful analysis of the evolution of the regulations that determined the period of compulsory schooling in Great Britain over the post-war period reveals that the exposure to compulsory full-time education did not only vary as a result of these two reforms, but also as a result of another, and that variation occurred both between- and within-year-of-birth cohorts. In fact, the British education system has three terms that run September-December, January-April, and April-July, with precise dates varying by school and Local Education Authority (LEA). Until 1962, students had to stay in school until the end of the term in which they obtained the minimum school leaving age. As discussed also in Clark and Royer (2013), this implies that pupils born in September to December had to attend school until Christmas, pupils born in January to March had to attend school until Easter, while their peers born in April to August had to attend school until July. In 1962 a new Education Act was passed; as a result of this, after 1962 these laws changed, and pupils born September-January had to attend school until Easter, whereas those born February-August had to attend school until June[7]. Figure 3 offers a stylized description of the implications of these law changes for actual compulsory school attendance.

[7] The 1996 Education Act further modified this, introducing a new unique date (set as the school leaving date for any calendar year in the school year in which the pupil turns 16); since this came into force from 1998 onwards, however, our sample is not affected by the provision in the 1996 Education Act.

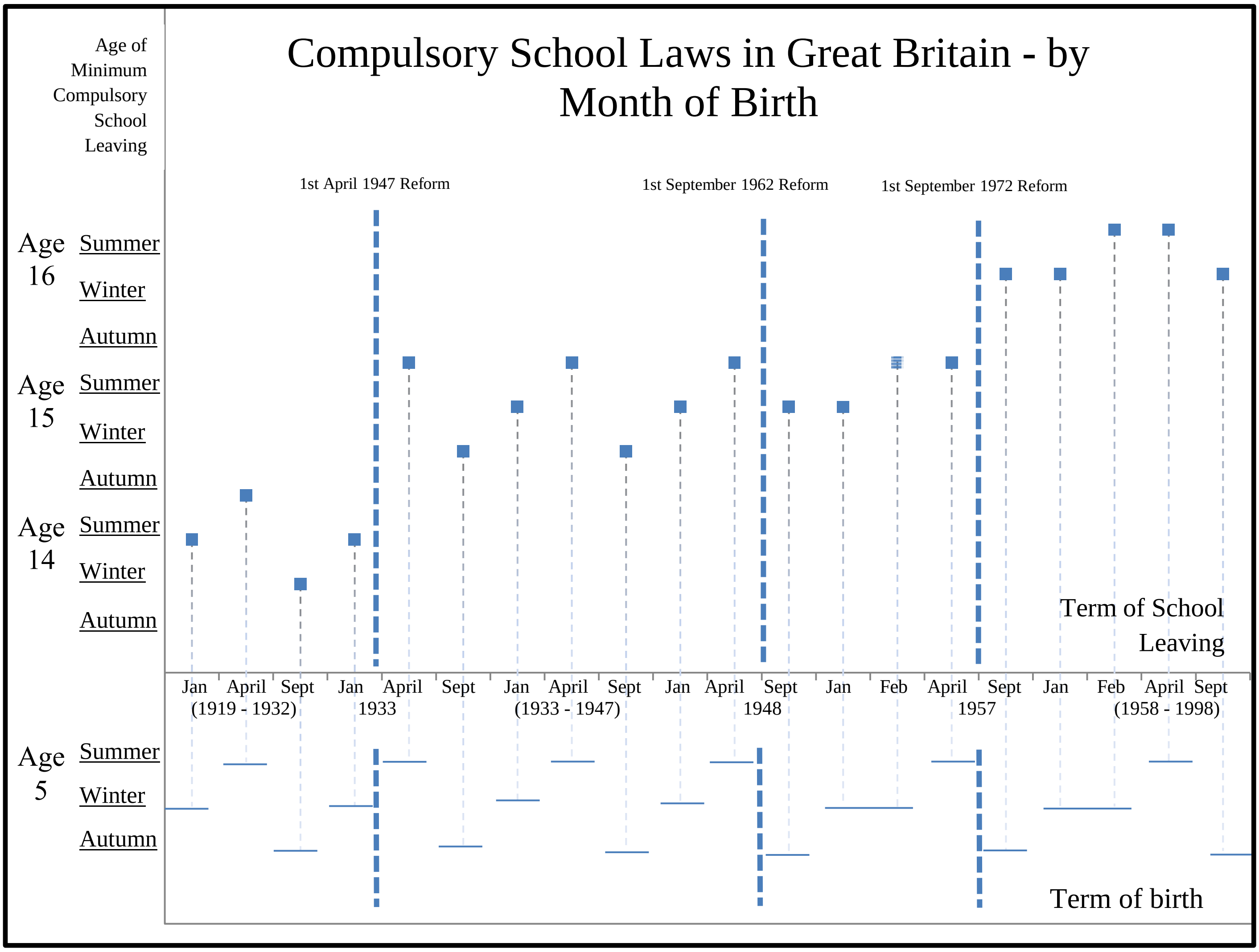

FIGURE 3. STYLIZED DESCRIPTION OF THE EFFECTS OF COMPULSORY SCHOOL LAWS[8]

In comparison with the cohort of pupils born before April 1st 1933, the 1947 reform resulted in 12 additional months of compulsory schooling for pupils born from April 1st 1933 up till August 31st 1948. However, starting from those born on September 1st 1948, the 1947 reform and 1962 reform implied 15 additional months of compulsory schooling for the cohorts born September-December and February-March (i.e., one more year, plus one term). Starting from September 1st 1957, 12 additional months of compulsory schooling ought to be added to this. In other words, starting from those born on September 1st 1957, the 1962 reform and 1972 reform implied 24 additional months of compulsory school for the cohorts born in January and April-August; for the cohorts born September-December and February-March, they implied 27 additional months of compulsory school (i.e., two more years, plus one term). Finally, starting from 1998, the 1996 Education Act

[8] Clark and Royer (2013) also note that not all local schools authorities adhered to the same school entry rules: some admitted all students at the beginning of the academic year in which they reached the age of five (i.e., in September); others had two rather than three entry points.

implied 27 additional months of compulsory school for those born January-March, 24 additional months for those born April-August, and 30 additional months for those born September-December (i.e., two more years, plus two terms). In all these figures the comparison is with the birth cohorts prior to April 1st 1933.

Hence, for example, a pupil born in September 1932 could leave school by December 1946, aged 14; as a result of the 1947 reform, a pupil born in September 1933 could only leave school by December 1948, aged 15 and having attended one additional year. As a result of the 1962 reform, a pupil born in September 1948, could only leave school by April 1964, aged 15 and having attended one additional year, plus one additional school term. Also a pupil born in January 1949 could only leave school by April 1964 - aged 15 and having attended one additional year, but, unlike the peer born in September 1948, no further additional school term. A pupil born in February 1949 instead could only leave school by July 1964, aged 15 and having attended one additional year and one additional school term. A pupil born in September 1956 could only leave school by April 1972, aged 15 and having attended one additional year, plus one additional school term. As a result of the 1972 reform, a pupil born in September 1957 could only leave school by April 1974, aged 16 and having attended two additional years, plus one additional school term. A pupil born in January 1958, could only leave school by April 1974, aged 16 and having attended two additional years, but not an additional school term. A pupil born in February 1958 instead could only leave school by July 1974, aged 16 and having attended two additional years, plus one additional school term. A pupil born in September 1982 could only leave school by July 1999, aged 16 and having attended two additional years, plus two additional school terms. Finally, it is important to notice that those born in late June, July or August (i.e., before the start of the new academic year) were treated similarly to those born in the last term of the previous academic year. This is valid for all aforementioned policy changes, and it helps explain why, in Figure 1 and Figure 2, some pupils were still found to leave school at, strictly speaking, less than the minimum school leaving age[9].

Bearing these considerations in mind, it becomes evident that variation in the compulsory schooling regime occurred both across year-of-birth cohorts and across month-of-birth cohorts, i.e., both between- and within-year-of-birth cohorts. This, in turn, implies that the analysis of the returns to schooling implied by these reforms ought to be analyzed and estimated with month-of-birth comparisons. Also, this further implies that a comprehensive analysis of the returns to compulsory

[9] Nickell (1993) and Halsey et al. (1980) report further possible explanations for this apparent imperfect compliance to the law, such as overcrowding of schools and labour-market shortages.

education in Britain exploiting the observed policy changes ought to take into account the increasing number of months of compulsory schooling that future generations faced, in comparison with the cohorts born before April 1933.

III. Previous Estimates of the Compulsory Schooling Law Changes

Five studies have analyzed the effect of the 1947 and 1972 reforms on the earnings returns to education: Harmon and Walker (1995), Oreopoulos (2006), Devereux and Hart (2010), Grenet (2013) and Clark and Royer (2013). None of them has modeled the effect of the 1962 Education Act.

Harmon and Walker (1995) constitutes the first attempt to capture the effect of the 1947 and 1972 reforms on future earnings. It also constitutes the only attempt to jointly exploit both policies in order to calculate the returns to compulsory education in Britain. They use data from the Family Expenditure Survey (FES) for the survey years 1978-86, focusing on males aged 18-64. They adopt an IV methodology; using the cohorts of males born before 1933 as omitted category, they define one dummy variable for pupils who entered their 14th year between 1947 and 1971 (therefore facing a compulsory school age of 15), and one for pupils entering their 14th year after 1971, who therefore faced a compulsory school age of 16. They control for a quadratic of age, for the administrative region and for survey year, without imposing any further restrictions to the sample used in the estimation. Their IV estimates suggest that the effect of an additional year of compulsory education on log earnings is 0.15, much larger than the OLS estimate of 0.06. As no controls for cohort were included in the specification, Harmon and Walker (1995) were criticised by Card (1999). As cohort effects were omitted, they did not allow for systematic inter-cohort changes in educational attainment and did not account for positive trends in earnings[10].

Oreopoulos (2006) uses the 1983–98 survey years from the General Household Survey (GHS) and includes in the analysis all British-born individuals from 1921-51. He focuses on the 1947 reform; he newly uses a regression discontinuity (RD) approach and includes in the specification a fourth-order polynomial in year of birth. By the inclusion of the quartic of year of birth, he attempts to control for cohort trends. His results are consistent with Harmon and Walker (1995), since he finds very large IV estimates - ranging from an annual gain in earnings between 10 to 14 percent -, irrespective of the proportion of population affected by the compulsory school policies and whether the result is calculated for men or for women.

[10] However, since they control for survey year, the linear age variable is in fact a linear cohort variable (Devereux and Hart 2010).

Devereux and Hart (2010) also focus on the 1947 reform; they follow Oreopoulos (2006) by estimating monetary returns to compulsory schooling including a quartic of year of birth and using similar estimation samples. Similarly to Oreopoulos (2006), they include in the analysis individuals who were born between 1921 and 1951 and are aged between 28 and 64. The major element of innovation in their work lies in the quality and size of their data; they complement the GHS with the New Earnings Survey Panel Dataset (NESPD), which offers a large sample of high-quality administrative earnings data. Moreover, since the 1947 reform came into force since April 1st, they define the instrument as being equal to zero for persons born before 1933, 0.75 for persons born in 1933, and one for persons born after 1933. Devereux and Hart (2010) do not find any significant returns to compulsory schooling for the pooled sample, whereas they find IV estimates for men that are much closer to the conventional OLS estimates.

Grenet (2013) focuses on the 1972 reform and compares the effects for future earnings of the 1972 reform in England and Wales with the 1967 Berthoin reform in France. He uses large samples from the UK Labour Force Survey (LFS) and, similarly to previous studies, he includes in the specification a fourth-order polynomial of year of birth. He concludes that, unlike the 1967 Berthoin reform in France, the ROSLA intervention in England and Wales resulted in significant increases in future earnings for pupils forced to stay in school. He attributes this discrepancy mostly to the fact that the new school-leaving age implied the obtainment of a certificate in Britain, whereas the same was not true in France.

Finally, Clark and Royer (2013) investigate the health returns to compulsory schooling. Focusing on both law changes, they fail to find any significant impact of the compulsory schooling reforms on health outcomes; however, they conclude that these policies affected positively and significantly the future earnings of affected pupils. They follow Devereux and Hart (2010) in that they estimate monetary returns to compulsory schooling using similar estimation samples. Unlike previous studies, they use month-of-birth comparisons and include a quartic of month-year of birth. However, in the calculation of their IV, they do not take into account the within-year-of-birth variation in the *de-facto* amount of additional compulsory schooling implied by the 1962 reform.

All reviewed studies choose a parametric functional form, but only Grenet (2013) and Clark and Royer (2013) test it against an alternative specification, such as a local linear regression. Consistent with the suggestion in Lee and Lemieux (2010), in the next section we test their specification against more flexible specifications; we show that parametric estimates previously found using datasets in Harmon and Walker (1995), Oreopoulos (2006) and Devereux and Hart (2010) are very sensitive to both the particular analysis sample used in the estimation and to the order of the polynomial in the parametric functional form chosen. Moreover, with the exception of Clark and Royer (2013), all reviewed studies make year-of-birth comparisons. Given that both the 1947 and 1972 law changes were introduced part way through the year, and that the 1962 Education Act modified further the actual implications of these law changes, models at the month-year-of-birth level may actually increase precision in the estimates. In this paper, we estimate models at the month-year of birth level, which capture the heterogeneity in compulsory schooling regulations both between- and within-year-of-birth cohorts.

Our substantive contribution is twofold. First, we implement an innovative IV strategy. Using information at the month-year of birth level, we construct an instrument that incorporates all the information (and precise implications) of the education acts implemented in Britain starting from the World War II. In the calculation of the IV, this is the first study to incorporate the additional variation within year-of-birth (across months of birth) in the school leaving dates introduced by the 1962 reform. This should allow us to make a comprehensive analysis of both the compulsory education policies and the changes in school leaving dates implemented in Britain. In addition to this, we also modify the identification strategy found in the reviewed literature. Specifically, we control for a quadratic of age both in the 1st stage and in the 2SLS estimates; in contrast, we do not control for a polynomial of year of birth[11] in all stages of the estimation; rather, we only control for a polynomial of month-year of birth in the 1st stage. Therefore, our identification strategy becomes that the SLA laws modified schooling attainment after taking into account time trends in schooling attainment. Having taken time trends into consideration in the 1st stage of the estimation, then the 2SLS estimate can be obtained with a traditional human capital model (i.e., including a quadratic of age) and with the instrumented schooling attainment. Our strategy departs from the literature in terms of this rigorous modeling of the first stage as well as its use of the month of birth data. Secondly, we propose an alternative, fully non-parametric approach to calculate the returns to schooling based on the contribution of Manski (1997) and Manski and Pepper (2000); this approach only requires weak assumptions about monotonicity, that are partly testable and that are consistent with economic models of schooling choice and production of human capital through schooling. In the context of compulsory schooling laws, we are not aware of any study that has done this in the past.

[11] Or month of birth, in the case of Clark and Royer, 2013

IV. Empirical Specification and Findings from the Replication Analysis

The results presented in this section attest to the sensitivity of the results reported in Harmon and Walker (1995), Oreopoulos (2006) and Devereux and Hart (2010) to two equally important factors: namely, their sensitivity across alternative and equally plausible specifications and across comparable sampling datasets. For this replication analysis we use both the GHS and the FES data, since these datasets have been used by these authors. From the GHS, we use four separate subsets of data: namely, the 1979-2006 GHS survey years (our new, larger dataset), the GHS 1979-1998 survey years as in Devereux and Hart (2010), the GHS 1983-1998 survey years as in Oreopoulos (2006), and the GHS 1979-1986, similarly to Harmon and Walker (1995)[12]. Because the changes in the compulsory schooling laws took place, respectively, in 1947 and in 1972 (i.e., 25 years apart), two different birth cohorts were used in the analysis of each of these policies. For the analysis of the 1947 reform only, we follow Oreopoulos (2006) and Devereux and Hart (2010) by including British-born individuals who were born between 1921 and 1951 and are aged between 28 and 64. For the analysis of the 1972 reform only, we use a similar window either sides of the reform by including British-born individuals who were born between 1935 and 1965 and are aged between 28 and 64. For the joint analysis of both reforms, we use both cohorts of data. In contrast, when we do the analysis on the GHS 1979-1986 in order to produce estimates comparable to Harmon and Walker (1995), we follow these authors, i.e., no cohort restrictions are imposed in this case, and we include all available individuals aged 18-64. This is valid for the analysis of all reforms, both separately and jointly. Table A.7, Table A.8 and Table A.9 in Appendix A report the descriptive statistics from the GHS on the key variables used in the econometric analysis for each of these datasets. Table A.10 in Appendix A reports descriptive statistics from the FES 1978-86 survey years, similarly to those reported in Harmon and Walker (1995). Finally, results are presented and discussed in this section for the male subsample. Appendix D and Appendix E report results for the pooled sample.

Since the subsets of the GHS appear fairly similar (at least within each of the tables), there is no obvious reason why estimates should differ across the different subsamples of the GHS – although we would expect our larger dataset to increase coefficient estimates' precision. In the econometric analysis, we perform the IV analysis of the returns to compulsory education exploiting both the

[12] We also use the FES 1978-86 survey years in order to replicate exactly Harmon and Walker's (1995) results. The results of this exercise are discussed later in this section.

1947 reform and the 1972 reform; we analyze the effect of these policies both separately and jointly. Hence, in our main specification, the first stage equation can be written as follows:

(1) $$EdAge_i = g_0 + g_1 Law_i + f(YoB_i)^n + \varepsilon_i,$$

where i indexes individuals, $EdAge_i$ represents age left school, Law is a dummy variable indicating if the ROSLA has changed, and $f(YoB_i)$ is a polynomial function of order n of year of birth. In our main specification we follow Devereux and Hart (2010), and in the reduced form specification we model log weekly earnings on the Law variable and a polynomial function of order n of year of birth. Formally,

(2) $$ln\, Y_i = v_0 + v_1 Law_i + g(YoB_i)^n + \theta_i,$$

where i indexes individuals, Y_i represents weekly earnings and $g(YoB_i)$ is a polynomial function of order n of year of birth. Finally, 2SLS estimates are derived in order to retrieve the impact of compulsory schooling on earnings. The only exception to our main specification applies to the analysis of the GHS 1979-86 survey years; since this is done in order to produce estimates comparable to Harmon and Walker (1995), similarly to these authors, regional dummies and survey year dummies are also added to the specifications above.

In our specifications, following Devereux and Hart (2010), we also include a gender dummy when both men and women are included in the analysis. Also, with the only exception of the analysis of the GHS 1979-86 survey years, we set to missing cases for which hourly wage observations are less than £1 or more than £150 (in December 2001 pounds), we exclude cases where weekly hours are greater than 84, less than 1, or missing, and we estimate robust standard errors, allowing for clustering by birth cohort.

All specifications also include controls for age; similarly to Oreopoulos (2006) and Devereux and Hart (2010), some specifications include a quartic function of age, while some include age dummies. Similar to Harmon and Walker (1995), and in line with the conclusion from Heckman and Polachek (1974), we also estimate the analysis including a quadratic of age. Unlike previous parametric RD estimates of the returns to compulsory schooling, we do not restrict the polynomial function of year of birth to be a quartic, i.e., a fourth-order polynomial. Rather, as suggested in Lee and Lemieux (2010), we perform the IV analysis for a variety of orders n of the polynomial, therefore allowing for increasing flexibility in the underlying trends in earnings and educational attainment across birth cohorts. Pursuing this logic, we also estimate a 'fully-saturated' model replacing the polynomial of year of birth with a set of dummies for year of birth, thus allowing for flexible heterogeneity in earnings and educational attainment at the year-level.

Table 1 and Table 2 report the IV estimates that result from our replication analysis using the GHS data for the male sample. Table D.21 in Appendix D reports results for the pooled sample. Table 1 presents the IV estimates from our parametric analysis; Table 2 presents the estimates from our local regression approach, where only observations close to the cut-off are used. The complete set of parametric results, inclusive of all the first-stage and reduced-form estimates implemented, are reported in Appendix E[13]. In our parametric analysis, using the GHS 1979-1998 survey years and including a quartic function of year-of-birth in the analysis, we manage to replicate exactly the analysis of Devereux and Hart (2010). Using the 1983-98 GHS survey years as in Oreopoulos (2006) and controlling for a quartic function of year-of-birth, we can replicate exactly the results reported in Devereux and Hart (2010) when using Oreopoulos' (2006) sample data[14]. Using the 1979-86 GHS survey years, we implement the analysis using the same econometric strategy of Harmon and Walker (1995), aiming to produce a meaningful setting in which to test the robustness of their findings to the use of different sample data.

Several things emerge from the estimates in Table 1. Omitting to control for cohort trends generally appears to have serious consequences on the estimated impact of schooling on earnings. In fact, the estimates derived without inclusion of controls for year of birth appear markedly different from those in which birth cohort is controlled for, and from those calculated using other datasets. This provides support for Card's (1999) criticism of Harmon and Walker (1995) for not adequately controlling for systematic inter-cohort changes in educational attainment and earnings. Overall, our estimates for the male sample appear very unstable; for both the 1947 reform and 1972 reform, the choice of the order of the polynomial appears crucial in the determination of the estimated results. For example, starting from the quartic function chosen by the previous literature, relatively modest deviations from the chosen order of the polynomial seem to give substantially different answers. This applies to all datasets considered - including our own. When both reforms are analyzed together, the estimated impact of schooling on earnings appears less sensitive to the choice of the

[13] Exact replications of estimates found in previous studies are in bold.
[14] We do not report directly the replication of the results in Oreopoulos (2006) because this author, after collaborating with Devereux and Hart, wrote a corrigendum for Oreopoulos (2006).

TABLE 1 - 2SLS EFFECTS OF ROSLA LAWS ON LOG WEEKLY EARNINGS – MALE SAMPLE (GHS Data)

	Order YoB	GHS 1979-2006			GHS 1979-1998 (DH)			GHS 1983-1998 (OR)			GHS 1979-1986 (HW)		
	Polynomial	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1947 Reform	Zero	0.127***	0.129***	0.129***	0.153***	0.154***	0.155***	0.046***	0.049***	0.045**	0.034*	0.110***	0.110***
		(0.005)	(0.005)	(0.005)	(0.011)	(0.010)	(0.010)	(0.015)	(0.015)	(0.017)	(0.018)	(0.021)	(0.026)
	One	-0.075*	-0.057	-0.054	-0.020	-0.017	-0.017	-0.042	-0.041	-0.054	0.032*	0.107***	0.106***
		(0.043)	(0.039)	(0.039)	(0.026)	(0.027)	(0.027)	(0.034)	(0.037)	(0.040)	(0.018)	(0.021)	(0.026)
	Two	0.029*	0.035**	0.040*	0.031*	0.034*	0.039*	0.005	0.012	0.006	0.002	0.066***	0.046*
		(0.017)	(0.017)	(0.022)	(0.017)	(0.017)	(0.022)	(0.033)	(0.032)	(0.035)	(0.019)	(0.021)	(0.027)
	Three	0.039**	0.040**	0.043*	0.019	0.020	0.023	0.006	0.007	0.003	-0.120***	0.041*	0.007
		(0.018)	(0.018)	(0.022)	(0.021)	(0.021)	(0.026)	(0.033)	(0.033)	(0.035)	(0.023)	(0.022)	(0.029)
	Four	0.069***	0.067**	0.072**	0.063**	**0.061****	**0.067****	0.062	0.060	0.064	0.032	0.065***	0.040
		(0.024)	(0.024)	(0.029)	(0.026)	**(0.026)**	**(0.031)**	(0.051)	(0.052)	(0.054)	(0.023)	(0.022)	(0.029)
	Five	0.068**	0.066**	0.072**	0.061**	0.060**	0.067**	0.064	0.062	0.069	0.018	0.042*	0.035
		(0.025)	(0.026)	(0.030)	(0.028)	(0.029)	(0.033)	(0.048)	(0.049)	(0.049)	(0.025)	(0.025)	(0.030)
	Six	0.098**	0.092**	0.101**	0.096***	0.095***	0.102***	0.116*	0.116*	0.124**	0.021	0.012	0.027
		(0.036)	(0.034)	(0.037)	(0.034)	(0.034)	(0.036)	(0.057)	(0.058)	(0.057)	(0.029)	(0.029)	(0.033)
	Seven	0.091***	0.091***	0.120*	0.086***	0.087**	0.098**	0.128	0.092*	0.152*	0.001	0.007	0.000
		(0.031)	(0.032)	(0.063)	(0.031)	(0.033)	(0.036)	(0.084)	(0.048)	(0.082)	(0.027)	(0.027)	(0.000)
	Eight	0.100**	0.091***	0.099***	0.092***	0.090***	0.077	0.137	0.100*	0.116**	-0.009	0.000	0.000
		(0.043)	(0.031)	(0.035)	(0.031)	(0.029)	(0.075)	(0.093)	(0.055)	(0.053)	(0.042)	(0.000)	(0.000)
	Dum-Mies	0.161***	0.132	0.158***	0.229***	0.459	0.226***	0.102***	-0.374	0.107***	0.257	1.972	0.375*
		(0.007)	(0.156)	(0.008)	(0.008)	(0.676)	(0.010)	(0.012)	(0.869)	(0.017)	(0.190)	(266.922)	(0.196)
1972 Reform	Zero	0.148***	0.149***	0.151***	0.252***	0.246***	0.245***	0.193***	0.183***	0.186***	-37.000	0.108*	-0.130
		(0.008)	(0.008)	(0.008)	(0.024)	(0.023)	(0.023)	(0.037)	(0.036)	(0.035)	(282.176)	(0.058)	(0.100)
	One	-4.997	-3.063	-3.214	-0.446	-0.492	-0.479	0.208	0.093	0.105	85.191	0.099*	-0.159
		(33.662)	(13.826)	(15.500)	(0.641)	(0.659)	(0.635)	(0.292)	(0.268)	(0.273)	(1,466.6)	(0.060)	(0.109)
	Two	-0.020	0.099	0.169	0.143	0.109	0.097	0.213*	0.189*	0.197*	-5.401	0.261***	0.137**
		(0.421)	(0.238)	(0.204)	(0.128)	(0.135)	(0.143)	(0.112)	(0.109)	(0.111)	(7.183)	(0.052)	(0.067)
	Three	0.102	0.149	0.192	0.181**	0.173**	0.169**	0.209***	0.197**	0.202**	0.150***	0.296***	0.205***
		(0.151)	(0.122)	(0.114)	(0.074)	(0.073)	(0.075)	(0.076)	(0.078)	(0.079)	(0.041)	(0.048)	(0.057)
	Four	0.173*	0.167*	0.198**	0.208**	0.217**	0.223**	0.186*	0.204**	0.207**	0.138***	0.258***	0.159***
		(0.087)	(0.089)	(0.089)	(0.100)	(0.101)	(0.105)	(0.097)	(0.096)	(0.095)	(0.040)	(0.042)	(0.050)
	Five	0.137***	0.134**	0.134**	0.070	0.069	0.077	0.071	0.074	0.071	0.130**	0.263***	0.148**
		(0.049)	(0.050)	(0.051)	(0.094)	(0.094)	(0.091)	(0.092)	(0.092)	(0.094)	(0.063)	(0.066)	(0.064)
	Six	0.126**	0.128**	0.120**	0.046	0.076	0.035	0.072	0.074	0.076	0.311*	0.425**	0.179**
		(0.049)	(0.047)	(0.050)	(0.091)	(0.085)	(0.094)	(0.084)	(0.086)	(0.085)	(0.160)	(0.175)	(0.090)
	Seven	0.127**	0.137**	0.134**	0.099	0.098	0.083	0.083	0.068	0.064	0.008	0.022	0.055
		(0.048)	(0.056)	(0.053)	(0.081)	(0.076)	(0.078)	(0.080)	(0.085)	(0.089)	(0.036)	(0.034)	(0.045)
	Eight	0.083	0.104*	0.158	0.007	0.038	0.051	0.270	0.036	0.059	0.002	0.000	0.043
		(0.137)	(0.052)	(0.543)	(0.161)	(0.170)	(0.087)	(1.481)	(0.095)	(0.272)	(0.033)	(0.000)	(0.039)
	Dum-Mies	0.153***	2.058	0.154***	0.280***	-8.118	0.115***	0.163***	-1.133	0.080***	0.257	0.295	0.375*
		(0.004)	(14.310)	(0.005)	(0.012)	(54.060)	(0.004)	(0.016)	(1.679)	(0.006)	(0.190)	(0.183)	(0.196)
1947 & 1972 Reforms	Zero	0.133***	0.134***	0.134***	0.163***	0.166***	0.167***	0.069***	0.071***	0.069***	0.025	0.102***	0.082***
		(0.005)	(0.004)	(0.004)	(0.012)	(0.012)	(0.012)	(0.018)	(0.019)	(0.020)	(0.019)	(0.021)	(0.027)
	One	0.014	0.024	0.023	0.009	0.013	0.014	-0.040	-0.045	-0.054	0.024	0.099***	0.077***
		(0.037)	(0.036)	(0.037)	(0.025)	(0.026)	(0.026)	(0.031)	(0.036)	(0.039)	(0.019)	(0.022)	(0.028)
	Two	0.015	0.027	0.028	-0.024	-0.022	-0.022	-0.030	-0.038	-0.048	-0.004	0.076***	0.039
		(0.034)	(0.033)	(0.034)	(0.037)	(0.037)	(0.039)	(0.046)	(0.050)	(0.054)	(0.020)	(0.022)	(0.029)
	Three	0.001	0.012	0.014	0.013	0.016	0.019	0.015	0.013	0.007	-0.101***	0.071***	0.028
		(0.024)	(0.023)	(0.026)	(0.021)	(0.021)	(0.026)	(0.039)	(0.040)	(0.042)	(0.023)	(0.022)	(0.030)
	Four	0.018	0.025	0.026	0.033	0.032	0.033	0.038	0.031	0.024	0.042*	0.091***	0.055*
		(0.027)	(0.027)	(0.029)	(0.022)	(0.022)	(0.026)	(0.038)	(0.039)	(0.040)	(0.023)	(0.023)	(0.030)
	Five	0.038	0.040	0.037	0.038	0.035	0.035	0.028	0.026	0.030	0.017	0.050*	0.036
		(0.035)	(0.035)	(0.036)	(0.036)	(0.036)	(0.041)	(0.065)	(0.065)	(0.067)	(0.027)	(0.026)	(0.033)
	Six	0.036	0.040	0.037	0.043	0.040	0.040	0.049	0.047	0.054	0.017	0.013	0.024
		(0.034)	(0.033)	(0.035)	(0.032)	(0.032)	(0.038)	(0.063)	(0.062)	(0.063)	(0.031)	(0.031)	(0.037)
	Seven	0.037	0.299	0.156***	0.054	0.580	0.048	0.055	0.050	0.095	0.007	0.014	0.026
		(0.035)	(6.034)	(0.045)	(0.042)	(1.489)	(0.036)	(0.094)	(0.059)	(0.077)	(0.028)	(0.028)	(0.032)
	Eight	0.017	0.028	0.049	0.040	0.024	0.003	0.000	-0.013	0.218	0.006	0.000	0.000
		(0.022)	(0.031)	(0.141)	(0.032)	(0.030)	(0.029)	(0.000)	(0.036)	(0.248)	(0.027)	(0.000)	(0.000)
	Dum-Mies	0.094***	-0.067**	-0.060**	0.046	0.051	0.094	2.143	2.117	2.620	-0.197	-0.205	-0.214
		(0.027)	(0.027)	(0.026)	(0.292)	(0.295)	(0.326)	(4.716)	(5.084)	(8.508)	(0.356)	(0.360)	(0.370)
	Age Contr.	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

order of the polynomial. However, when our dataset is used, and the reforms are analyzed together, the inclusion of birth cohort-dummies results in a significant modification of the estimates[15].

In light of these results, it would seem logical to retrieve the Akaike Information Criterion (AIC) for each of these models and use this information to let the data tell us what model we should prefer. Table D.19 in Appendix D reports the AIC calculated for each of our estimated IV models on the male sample[16]. If one model looked clearly superior to the others, then the choice of one specification (and in turn, one estimated RoRtE) over the others would seem justified. However, the conclusion we draw from the results in Table D.19 is that considerable uncertainty still remains on this issue; having investigated the AIC across different specifications, no model appears clearly superior to others. Starting again from the quartic function chosen by the previous literature, relatively modest deviations from the chosen order of the polynomial seemingly give different answers in the IV estimates, but not in the AIC values. This, in turn, leads us to the (rather discouraging) conclusion that we do not really know what estimated RoRtE we should believe.

Table 2 shows the Local Wald estimates from our local regression analysis; for this, heteroskedasticity-robust standard errors were clustered by year-of-birth, and 1,000 bootstrap replications were conducted for inference. Starting from an optimal bandwidth (in the sense of Imbens and Kalyanaraman 2009), results are also shown for observations within half the optimal bandwidth and observations within twice the optimal bandwidth. Local regression estimates do not appear robust to these plausible modifications of the chosen bandwidth. Moreover, substantially different conclusions are drawn using different datasets. In fact, for observations close to the 1947 reform, the estimates for the RoRtE range from zero to a statistically significant 0.20 percent. For the 1972 reform, the range of retrieved parameters appears even wider, and rather uninformative. Table D.22 in Appendix D shows the results for the pooled sample. Table D.23 and Table D.24 in Appendix D report the same analysis, but without any clustering (1,000 bootstrap replications still being conducted on heteroskedasticity-robust standard errors). Respectively, Table D.23 shows results for the male sample and Table D.24 for the pooled sample. These results are noteworthy, in that they show that the calculated results also critically depend on the decision to cluster the standard errors by year-of-birth. When no clustering is applied, estimated coefficients on the effects of the 1947 reform tend to lose significance and go to zero.

[15] The estimates on the pooled sample appear more stable across varying orders of the polynomial of year of birth. However, this is not true when dummy variables for birth cohorts, allowing for separate means for each birth cohort, are included in the analysis.

[16] Table D.20 in Appendix D reports the AIC calculated for the IV regressions on the pooled sample. Similar conclusions apply.

TABLE 2. LOCAL AVERAGE RD EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS – MALE SAMPLE

		(Our Own)	(D & H)	(Oreop)	(H & W)
1947 Reform	Red. form – optimal bandw.	0.022 (0.019)	0.022 (0.018)	0.055 (0.045)	-0.013 (0.014)
	1st stage – optimal bandw.	0.328*** (0.074)	0.328*** (0.070)	0.271*** (0.079)	0.404*** (0.017)
	Wald – optimal bandw.	**0.066**** **(0.031)**	**0.066**** **(0.031)**	**0.202**** **(0.102)**	**-0.033** **(0.034)**
	Red. form – 0.5 * optimal bandw.	0.019 (0.023)	0.019 (0.023)	0.000 (0.000)	0.000 (0.000)
	1st stage – 0.5 * optimal bandw.	0.390*** (0.077)	0.390*** (0.076)	0.000 (0.000)	0.000 (0.000)
	Wald – 0.5 * optimal bandw.	**0.050** **(0.035)**	**0.050** **(0.035)**	**0.000** **(0.000)**	**0.000** **(0.000)**
	Red. form – 2 * optimal bandw.	0.023 (0.019)	0.023 (0.018)	0.038 (0.028)	-0.022 (0.020)
	1st stage – 2 * optimal bandw.	0.341*** (0.048)	0.343*** (0.051)	0.300*** (0.072)	0.384*** (0.029)
	Wald – 2 * optimal bandw.	**0.068*** **(0.040)**	**0.066*** **(0.037)**	**0.128** **(0.080)**	**-0.058** **(0.049)**
1972 Reform	Red. form – optimal bandw.	0.066*** (0.009)	0.099*** (0.014)	0.111*** (0.017)	0.024 (0.032)
	1st stage – optimal bandw.	0.201*** (0.036)	0.216*** (0.052)	0.218*** (0.049)	0.161** (0.082)
	Wald – optimal bandw.	**0.326***** **(0.071)**	**0.459***** **(0.107)**	**0.508***** **(0.116)**	**0.151** **(0.137)**
	Red. form – 0.5 * optimal bandw.	0.068*** (0.008)	0.079*** (0.013)	0.079*** (0.013)	-0.017 (0.026)
	1st stage – 0.5 * optimal bandw.	0.203*** (0.053)	0.176*** (0.063)	0.176*** (0.063)	0.135* (0.075)
	Wald – 0.5 * optimal bandw.	**0.335***** **(0.083)**	**0.446***** **(0.134)**	**0.446***** **(0.134)**	**-0.122**** **(0.061)**
	Red. form – 2 * optimal bandw.	0.076*** (0.011)	0.083*** (0.014)	0.095*** (0.012)	0.023 (0.014)
	1st stage – 2 * optimal bandw.	0.209*** (0.035)	0.214*** (0.063)	0.224*** (0.062)	0.173** (0.082)
	Wald – 2 * optimal bandw.	**0.362***** **(0.076)**	**0.387***** **(0.108)**	**0.425***** **(0.109)**	**0.134**** **(0.061)**

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Finally, using the FES 1978-86 survey years, we replicate exactly Harmon and Walker's (1995) results; in addition to that, we also move progressively towards the specification in Devereux and Hart (2010), to test the stability of their results not only across sampling datasets, but also across different specifications. In the analysis of the FES data, we follow Harmon and Walker (1995) and we use hourly earnings (rather than weekly earnings) to measure the returns to schooling. Since they use a quadratic of age and only focus on the male sample, an exact replication of their estimate is reported in Table 3, row 1, columns (1) and (7).

However, as can be seen from both the pooled sample (for which results are still presented in Appendix D) and from the male sample, their result critically depends on the age controls they use, and, even more crucially, on the presence of controls for the birth cohort. In fact, when the specification of Devereux and Hart (2010) is applied to the FES 1978-86 survey data, the results change significantly; once again, the parameters for the male sample appear to be very sensitive to the chosen functional form.

TABLE 3 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG HOURLY EARNINGS - MALE SAMPLE (FES DATA)

	1st Stage: Schooling			Reduced Form: Hourly Earnings			2SLS: Hourly Earnings		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Row 1	0.539***	0.673***	0.675***	0.002	0.057***	0.053***	0.154***	0.091***	0.087***
H&W	(0.055)	(0.064)	(0.073)	(0.011)	(0.013)	(0.014)	(0.016)	(0.018)	(0.020)
1995	0.109	0.649***	0.687***	-0.148***	0.016	-0.005			
	(0.077)	(0.089)	(0.099)	(0.015)	(0.017)	(0.019)			
Row 2	0.539***	0.673***	0.675***	0.002	0.057***	0.053***	0.154***	0.091***	0.087***
Clustered	(0.105)	(0.132)	(0.142)	(0.021)	(0.017)	(0.017)	(0.030)	(0.018)	(0.016)
Stnd Errs	0.109	0.649***	0.687***	-0.148***	0.016	-0.005			
	(0.180)	(0.152)	(0.164)	(0.043)	(0.027)	(0.026)			
Row 3	0.607***	0.665***	0.655***	0.010	0.029	0.022	0.019	0.044*	0.031
Quartic	(0.170)	(0.169)	(0.169)	(0.022)	(0.021)	(0.021)	(0.032)	(0.022)	(0.027)
of Year	0.504***	0.654***	0.693***	-0.011	0.037	0.013			
of Birth	(0.163)	(0.163)	(0.173)	(0.025)	(0.024)	(0.023)			
Row 4	0.609***	0.668***	0.641***	0.014	0.033	0.022	0.024	0.052**	0.033
Regional	(0.173)	(0.175)	(0.173)	(0.022)	(0.021)	(0.022)	(0.031)	(0.021)	(0.026)
& Year	0.531***	0.687***	0.714***	-0.001	0.047*	0.022			
Dummies	(0.168)	(0.167)	(0.174)	(0.025)	(0.024)	(0.024)			
Omitted									
Age Controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Taking all these results into consideration, we see two limitations with the previous literature; first, no previous study takes into account the actual implications of the entire set of reforms that took place in Great Britain since the Education Act 1944, i.e., the reforms in 1947, 1962 and 1972. We believe this is relevant because these reforms did not act in isolation, but rather they all contributed to reshaping the school trajectories of British pupils at the time when they were implemented. The discussion in Section 2 highlighted the need to do so through month-of-birth comparisons, rather than year-of-birth comparisons[17]. However, unlike any previous study, we also model all these policies as exogenous shocks to the educational choice of British pupils. In particular, we are the first to take into account the within-year variation in school leaving dates introduced by the 1962 reform. In turn, this implies that our instrument is more accurate in that it incorporates all variation implied by the school-leaving dates discussed above.

[17] Clark and Royer (2013) is the first study to do so, although their focus is on health outcomes.

Secondly, given that the chosen specification plays an important role in the determination of the result of interest, and because the true underlying function is unknown, we calculate this parameter without making any assumption on the underlying functional form. In other words, building on the contribution of Manski (1989, 2000), we calculate this parameter using non-parametric methods.

V. Parametric Analysis of the Impact of Compulsory Education on Earnings – Using Our New IV Strategy

In light of the discussion in Section 2, our proposed IV attempts to better capture the actual amount of additional compulsory schooling that future generations received, in comparison with the cohorts of pupils born in Britain before April 1st 1933. Specifically we sought to measure the precise number of extra year of schooling involved in changing the SLA – as measured in years and fractions of a year and not just discrete whole years. As a consequence, our instrument takes the value 0 for pupils born before April 1st 1933 (i.e., our control group); it takes up value 1 for pupils born from April 1st 1933 until August 31st 1948, since these pupils were forced to attend one more year of school, compared to the control group. For pupils born from September 1st 1948 up until August 31st 1957, the instrument takes the value 1.33 for those born September-December, as well as for those born February-March; instead, it still takes up value 1 for those born in the remainder of the calendar year. This is because, as a result of the 1947 reform and the Education Act 1962, pupils born September-December and pupils born February-March were forced to attend one more year of school plus one term, in comparison with the control group. On the contrary, pupils born in the remainder of the calendar year were not directly affected by the Education Act 1962. Starting from September 1st 1957, our instrument takes the value 2; however, for those born September-December, as well as for those born February-March, the instrument takes the value 2.33; this is because, as a result of the 1972 reform and the Education Act 1962, these pupils were forced to attend two more years of school plus one term, in comparison with the control group. Figure 4 plots our instrument against those used in Harmon and Walker (1995), Oreopoulos (2006) and Devereux and Hart (2010). The logic of our alternatives to the IV for the ROSLA reforms is simple. The IV variable is directly measuring the *de-facto* amount of compulsory schooling implied by these reforms. It is not a binary integer variable, of who is forced to take an extra year of schooling – or not - but more accurately, who is getting exactly how many terms of extra schooling as a result of these reforms. This new variable has the distinct advantage of introducing a lot more variability into the measured treatment variable. This is directly reflected in the stability of the estimated effects. Our new metric on the IV is also a much more accurate measurement of exactly what is at stake – namely the exact amount of extra education received as a result of the reform. In this sense measuring the IV in the way previous authors have done constitutes a potential measurement error. We now rehearse the details of this assertion.

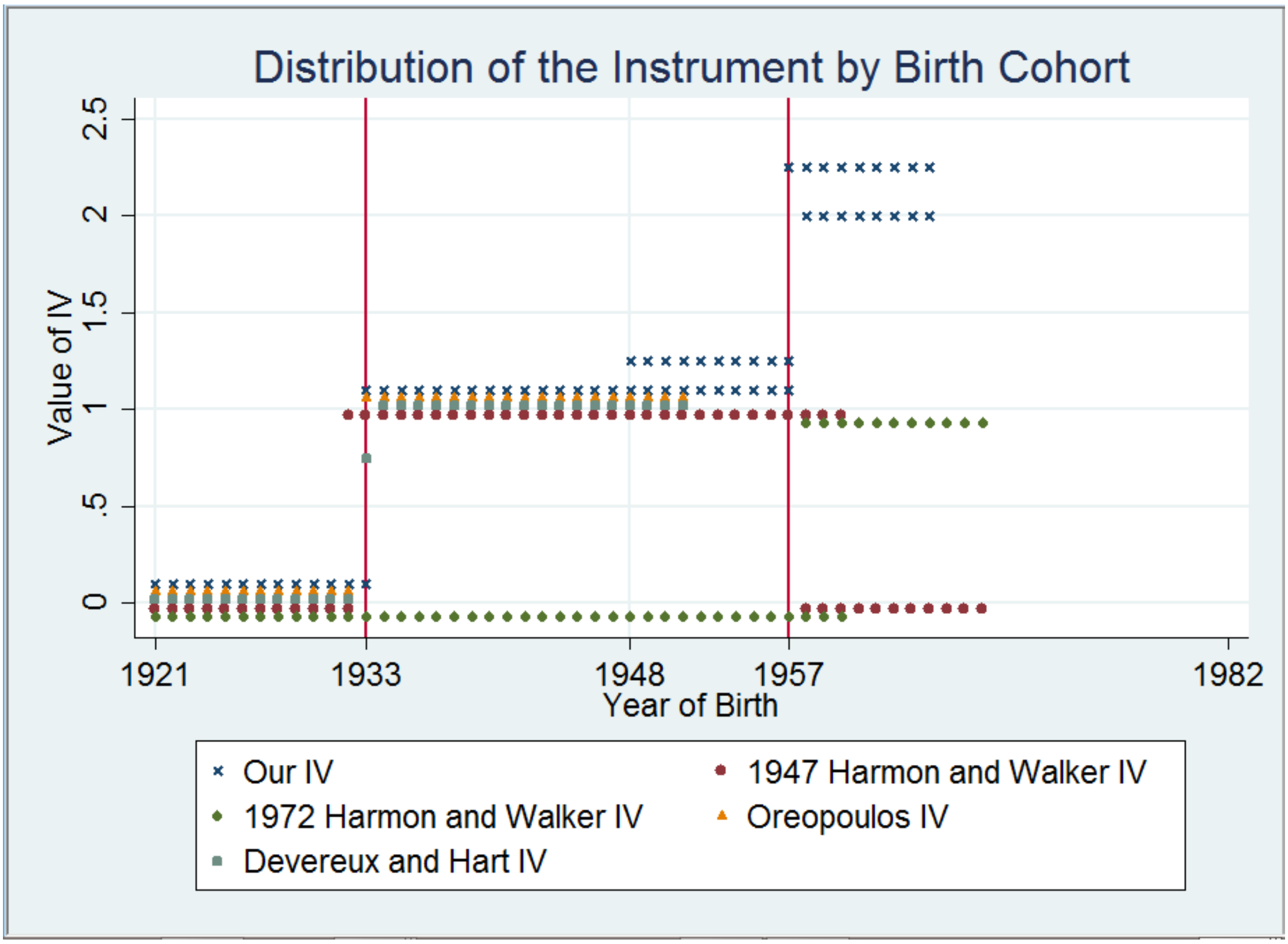

FIGURE 4. STYLIZED DESCRIPTION OF OUR PROPOSED INSTRUMENT VIS-À-VIS INSTRUMENTS USED IN REVIEWED LITERATURE

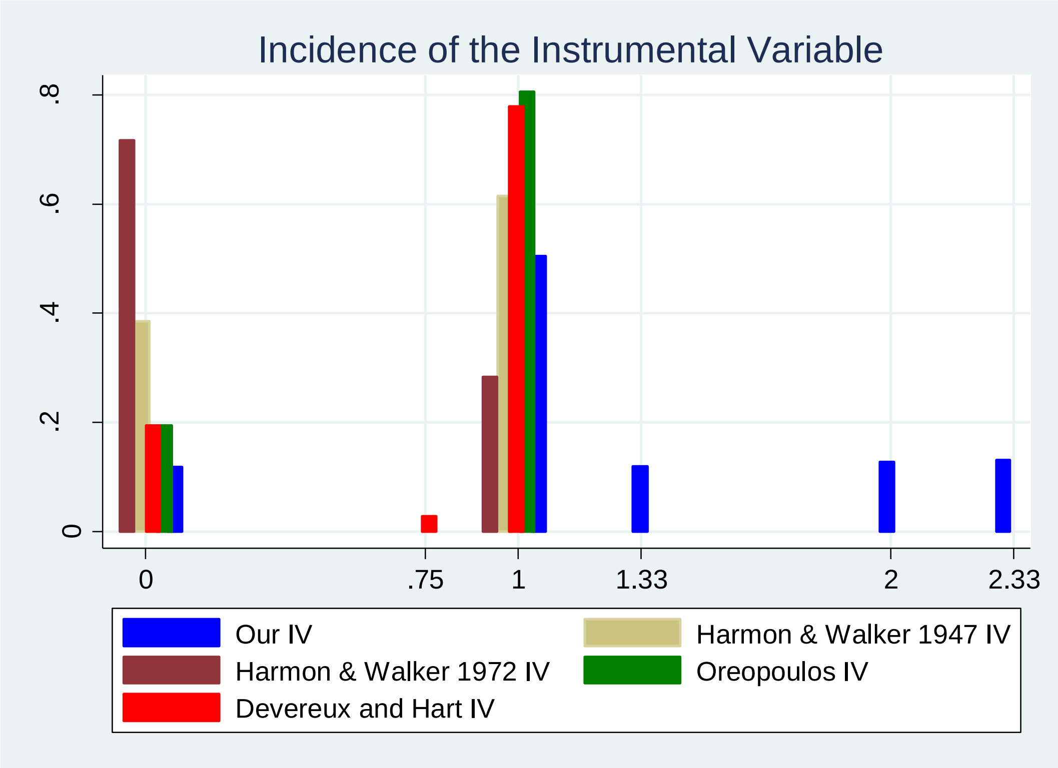

FIGURE 5. FREQUENCY OF OUR PROPOSED INSTRUMENT VIS-À-VIS INSTRUMENTS USED IN REVIEWED LITERATURE

Oreopoulos (2006) and Devereux and Hart (2010) only focus on the 1947 reform, and only include in the analysis the cohorts born in 1921-51. The only difference between their respective instruments lies in the fact that Devereux and Hart's (2010) instrument takes the value 0.75 for pupils born in 1933; Oreopoulos' (2006) instrument, instead, takes the value 1 for this cohort of pupils. Harmon and Walker (1995) use both law changes as instruments, captured by two dummies; the first, that captures the effect of the 1947 reform, takes the value 1 for the 1933-1957 cohorts; the second, that captures the effect of the 1972 reform, takes the value 1 for the 1957-1968 cohorts. Some overlapping is observed close to the threshold years because of the inclusion of the Scottish sample in Harmon and Walker's (1995) analysis; since Scotland came under a different regulation, and the 1947 and 1972 policies were implemented, respectively, in 1946 and 1976, Harmon and Walker (1995) attribute the policy changes to 1946 and 1976 for the Scottish sample.

There is no significant difference between our instrument and those of Oreopoulos (2006) and Devereux and Hart (2010) for the 1921-1947 birth cohorts; however, for the 1948-1951 cohorts, neither Oreopoulos (2006) nor Devereux and Hart (2010) take into account the effects of the 1962 Education Act – since we do, for these years our own instrument diverges from theirs. After the

1951 cohort, a comparison between our instrument and their instrument is not possible since they do not include later cohorts in the analysis. Harmon and Walker (1995) capture the changes in compulsory schooling with two dummies; by doing so, they omit to incorporate in their instruments the effects of the 1962 Education Act too. Also, since the 1947 reform and the 1972 reform implied two different treatments (in comparison with the control group), capturing both reforms with a dummy may have introduced measurement error. Our instrument rectifies these inaccuracies.

The histogram of the relative frequency on our IV values is presented in Figure 5. From this figure it can be seen that there is a significant fraction of the sample with all the different values of the IV. This illustrates the diversity of the *de facto* extra years and terms (expressed as fractions of a school year) of schooling which are forced (via the various ROSLA reforms) on pupils purely as a result of their month and year of birth. At this juncture it should be emphasized that we are not, *per se,* using the month of birth as our IV – rather our IV is derived by the conjunction of a month and year of birth and exactly what ROSLA reform may have affected each individual[18].

Since information on month of birth was not available from the FES, we only use the GHS data to calculate these results. More precisely, we use the 1983-2000 GHS survey years, since information on month of birth is only available from these survey years of the GHS. This results in a sample of 84,010 individuals; this number of individuals still allows to calculate estimates with precision. In our econometric analysis, we perform the IV analysis of the returns to compulsory education exploiting all changes in school leaving dates attributable to the reforms in 1947, 1962 and 1972; we analyze simultaneously the effect of all these policies. Hence, the first stage equation can be written as follows:

(3) $$EdAge_i = \gamma_0 + \gamma_1 Laws_i + f(MoB_i)^n + u_i,$$

where i indexes individuals, $EdAge_i$ represents age left school and $f(MoB_i)$ is a polynomial function of order n of month-year of birth; a quadratic of age is also included among the covariates. We estimate the first stage for a variety of orders n of the polynomial function of month-year of birth, therefore allowing for increasing flexibility in the underlying trends in educational attainment across month-year birth cohorts. Finally, we also estimate a fully-saturated model replacing the

[18] Following Angrist and Krueger (1991) and Acemoglu and Angrist (2001) there is a large literature now which uses month of birth directly as an IV in education studies as well as other fields. The logical potential endogeneity problem with this literature is that there may be non-random seasonality in this month of birth which induces effects on education outcomes or even on earnings directly. See Buckles and Hungerman (2010). Despite the fact that we do not use the month of birth directly as an IV, we show in Appendix C that our data does not exhibit this seasonality.

polynomial function of month-year of birth with a set of dummies for month-year of birth, thus allowing for heterogeneity in educational attainment at the month-year of birth level.

In the 2SLS specification we model log weekly earnings on the instrumented age left school and a quadratic of age, therefore estimating a simple human capital model once the endogeneity of the schooling decision is removed through the IV procedure. Our identification strategy, therefore, hinges on the assumption that returns to education were substantially constant over time in Great Britain, once time trends and the introduction of the SLA policies have been controlled for in the determination of educational attainments. The logical corollary of this is that returns to schooling can be estimated with a traditional human capital model, in which weekly earnings are determined by the instrumented level of schooling and a quadratic of age. Formally:

$$(4) \qquad ln\, Y_i = \delta_0 + \delta_1(EdAge_i = Laws_i) + w_i,$$

where i indexes individuals, Y_i represents weekly earnings and $EdAge_i = Laws_i$ represents the instrumented level of education. In our specifications, following Devereux and Hart (2010), we also include a gender dummy when both men and women are included in the analysis. Also, we still set to missing cases for which hourly wage observations are less than £1 or more than £150 (in December 2001 pounds), we exclude cases where weekly hours are greater than 84, less than 1, or missing, and we estimate robust standard errors, allowing for clustering by month-year of birth.

Table 4 reports the IV estimates that result from our analysis for the male sample; results for the pooled sample are reported in Table F.46 in Appendix F. In the rows of this table we respectively report the estimations using the old dichotomous IV from the previous literature and our own new IV which takes account of the number of terms of extra schooling a pupil receives. Both for the pooled sample and for males, the estimates appear robust across different specifications; in fact, no major differences emerge in the estimates across alternative orders n of the polynomial function of month-year of birth, and the IV estimates of the returns to education no longer seem to be as sensitive to the order of the polynomial of month-year of birth chosen. What appears to be clear is that most of the consistency of the RoRtE estimates is coming from the more careful specification of the first stage regression rather than from the precise definition of the IV.

When we limit the analysis to the cohorts born in the pre-World War II period and we use the 1947 reform for identification, we find a consistent RoRtE of 4% for males. When we focus on the later cohorts, i.e., individuals born in the years of the war or later, we use the 1972 reform for identification. The 2SLS estimates still appear robust across specifications and the RoRtE appears consistently centred around 9%. Finally, when we estimate the RoRtE for the entire sample, we use both reforms for identification, and the estimates appear to cluster around 6.5%. The introduction of our new IV, that also incorporates the effect of the 1962 reform, appears to have a modest effect on the 6.5% estimate of the RoRtE. The estimated impact of one additional year of schooling that we find is consistent with the IV results from Devereux and Hart (2010) and Clark and Royer (2013). However, unlike previous studies, our result also appears robust to alternative assumptions about the distribution of educational attainments over time. This holds true also when dummy variables at the month-year of birth level are included in the analysis.

TABLE 4 - 2SLS EFFECTS OF ROSLA LAWS ON LOG WEEKLY EARNINGS - MALE SAMPLE

	2SLS Estimates – Order of Month-Year of Birth Polynomial									
	0	1	2	3	4	5	6	7	8	Dummies
1947 Ref.	0.030*	0.044***	0.045***	0.045***	0.043***	0.043***	0.043***	0.043***	0.045***	0.061***
N=24,294	(0.017)	(0.013)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	(0.011)
1972 Ref.	0.098***	0.087***	0.087***	0.090***	0.090***	0.089***	0.089***	0.089***	0.091***	0.094***
N=35,334	(0.018)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.011)	(0.009)
1947 & 1972 Ref. – Old IV	0.042***	0.064***	0.065***	0.066***	0.067***	0.067***	0.065***	0.066***	0.066***	0.075***
N=40,100	(0.015)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)
1947 & 1972 Ref. – New IV	0.056***	0.067***	0.062***	0.065***	0.065***	0.065***	0.063***	0.063***	0.064***	0.075***
N=40,100	(0.012)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

At this juncture, it should be re-iterated that the estimation of these models for pooled samples in Appendix F was only presented because the convention was accepted in the previous papers we have re-estimated in detail. Regardless of the underlying functional form chosen, the estimated coefficients on returns to education for the pooled sample always exceed 0.22. We do not regard these pooled coefficients as meaningful as the inclusion of women in the pooled sample abstracts from all the other problems of women selecting themselves into work or making dynamic life-cycle participation decisions. The only two things we can confidently say about these results are that: introducing our methodology into the frame provides us with very stable coefficients which is a further vindication of our strategy. We now have a robust parameter, that is calculated by inclusion of all ROSLA policies in the country since the post-World War II period. Secondly, the returns to education for women who participate in the labour market – being a self-selected group – are much higher than the 6% we observe for men. How much of our estimated 26% is due to education per se, rather than selection, is a moot point.

To conclude, our replication analysis attests to the crucial importance of the chosen functional form in the determination of the results previously reported. Since the true function is not known in reality, we argue that this is not an element of secondary importance. In this regard, our results support the argument in Lee and Lemieux (2010) that *"it is essential to explore how RD estimates are robust to the inclusion of higher order polynomial terms"* (Lee and Lemieux 2010, pp. 318). By redefining the instrument with a higher degree of precision, and by estimating a traditional human capital model in the second stage of our 2SLS procedure, we find estimates that are consistent (especially for males) with some previous literature; our estimates are also robust to the inclusion of alternative polynomial terms in the determination of the level of schooling.

In the next section, we complement the previous analysis by utilizing the non-parametric partial identification strategy of Manski (1997) and Manski and Pepper (2000). We compare the results from our parametric IV analysis with the evidence from this non-parametric approach to the calculation of the RoRtE.

VI. Non-Parametric Analysis of the Rate of Return to Education – Using Bounds

In order to retrieve an estimate of the return to schooling, instrumental variables were used in all the reviewed and replicated papers examined above. However, a careful analysis of the causal impact of education on wages has shown that this approach can easily give inconsistent findings: in some cases a large positive (and statistically significant) return of up to 15% is found, while in some others the statistical significance of the coefficient goes to zero. This is true both across alternative specifications that use the same data, and across the same specification applied to different samples. Since in this literature, as well as in our replication analysis, similar identification strategies are adopted across different specifications, we suggest that the different results occur as a consequence of different first stage specifications in the IV estimation. Another contributory explanation for these divergent findings is that different samples – with different cohorts - are actually estimating different local average treatment effects, rather than the average treatment effect. The different studies (as well as our own replication analysis) rely on subsamples that differ in their characteristics from the overall population. The estimates based on these subsamples will retrieve the average return to education (henceforth, the ATE) only if such treatment effect is linear and homogeneous. In fact, linearity is often assumed for returns to education without any strong justification (Manski and Pepper 2000); violations of these assumptions may explain the diverging findings observed in the analysis of this parameter.

The rationale for using the additional bounds partial identification strategy in this section is fourfold. Firstly, conventional estimation methods like OLS or IV are demonstrably erratic when the functional form of the relationship (or its first stage) is unclear. Specifically if we are unsure what time trends, linearity or non-linearity assumptions are justified then the resulting estimates in the RoRtE literature vary widely. No such functional form assumptions are placed on bounds estimation. Secondly, bounds estimates do not require that conditional mean unobserved heterogeneity is independent of the observed covariates. This exogeneity assumption which is a very strict requirement for OLS is not an explicit assumption in bounds analysis. Alternatively if using IV we require the IV to be correlated with education but independent of the earnings equation error term – again bounds does not require this restrictive assumption. Thirdly, when identification from RDD is used it is clear that the RoRtE estimation is a LATE based on the ROSLA and it applies strictly only to those individuals who are affected by the reform. Bounds, in contrast provide a way of obtaining the ATE by considering the average upper bound estimate over the whole domain of the years of education. This non-parametric measure of the ATE of the RoRtE could be extremely useful in assessing the general effect of education on earnings – rather than the localized effect of an extra year of education for a specific cohort of pupils. The final rationale for the use of bounds, is that, as a minimum it is another form of robustness check on the estimates we obtain from IV or OLS estimation. As such we would wish to see a concordance of the size of estimate we are getting from bounds with that obtained from more conventional estimation procedures. If there is some such concordance this would give us confidence that we have a reasonable RoRtE – if not – then we would wish to further explore the source of this difference.

This justification for the use of bounds – in addition – to conventional estimation methods requires us to be aware of the assumptions necessary for the validity of bounds for our problem. Respectively we need to be reasonably sure that: the MTR assumption that any individuals earnings cannot be lower if they received more education; the MTS assumption that mean income in the sample is higher for more educated people; and finally the MIV assumption that any chosen covariate needs to exhibit higher mean earnings for those with a higher value of the covariate. Logically we would expect that these assumptions are reasonable, and in general weaker than those of OLS or IV estimation. So, in conclusion the justification for seeking to compare the IV/RDD estimates in our paper with those from an alternative estimation procedure are compelling and provide the best form of robustness check for those in search of an answer to the question of what is the ATE of the RoRtE.

Although an increasing number of studies has applied this technique in recent years (for example, Blundell et al. 2007 and DeHaan 2011), non-parametric bounds have not been applied yet to estimating the RoRtE or indeed the ROSLA reforms in Great Britain. This provides a particularly interesting context for the study of the returns to education. In turn this also implies that the application of this technique to this context could be particularly informative for both the returns to education and the more general methodological comparison between parametric IV analysis and non-parametric bounds analysis.

Following the notation in Manski and Pepper (2000) and deHaan (2011), we first describe the process used to retrieve informative bounds. In our empirical specification, for an individual i we observe the realized level of schooling s_i and the realized wage $y_i \equiv y_i(s_i)$, while we do not observe the potential outcomes $y_i(z)$ for $z \neq s_i$. For simplicity of exposition, the subscript i will be omitted for the remainder of this section. We are still interested in the average treatment effect of a marginal increase in the level of conjectured schooling, that is

(5) $$\Delta(t,z) = E[y(z)] - E[y(t)]$$

This *ATE* is derived from the difference in the average wage if all individuals had left school at age z ($[y(z)]$) and the average wage if all individuals had left school at age t ($[y(t)]$). We start the presentation of our analysis by focusing on the case in which all individuals have the same level of schooling; at the end of this section we describe how it is possible to measure the effect of an increase in an individual's level of schooling by one year on the individual's own wage.

If all individuals have the same level of schooling, by using the law of iterated expectations and the fact that $E[y(z)|s = z] = E[y|s = z]$, we can write

(6) $$E[y(z)] = E[y|s = z] * P(s = z) + E[y(z)|s \neq z] * P(s \neq z)$$

While we can observe the average wages for individuals with a level of education $s = z$, as well as the proportion of individuals with a level of education $s = z$, we cannot observe the counterfactual wage that individuals with a level of education $s \neq z$ would have experienced had their level of education not been $s = z$. In other words, we cannot observe $E[y(z)|s \neq z]$. However, one way to investigate this is by augmenting what is observed with assumptions. As shown by Manski (1989), if the support of the dependent variable is bounded, as it is the case for weekly wages, then it is possible to identify bounds on $E[y(z)]$ without any further assumptions. A lower bound and an upper bound on $E[y(z)]$ are obtained, respectively, by replacing $E[y(z)|s \neq z]$ with the lowest possible level of weekly wages y_{min} and with the highest possible level of weekly wages y_{max}. This, in turn, retrieves Manski's (1989) no-assumption (NOAS) bounds:

$$
\begin{aligned}
&E[y|s=z] * P(s=z) + y_{min} * P(s \neq z) \\
&\leq E[y(z)] \leq \\
&E[y|s=z] * P(s=z) + y_{max} * P(s \neq z)
\end{aligned} \tag{7}
$$

Since these bounds can be very wide, they may not be very informative. Therefore, we also added some non-parametric assumptions to make the NOAS bounds tighter. Similarly to Manski and Pepper (2000), we added the monotone treatment selection (MTS) assumption and the monotone treatment response (MTR) assumption. In this application, assuming the MRT assumption is equivalent to assuming that an individual's wage would not have been lower if they themselves had had a higher level of education[19]. Likewise the MTS assumption is equivalent to assuming that individuals that left school at older ages have weakly higher mean wage functions than do those who left school earlier. Several economic models of educational choice and wage determination predict that individuals with higher ability have higher mean wage functions and choose higher levels of education than do individuals with lower ability. As suggested in Manski and Pepper (2000), the MTS assumption is consistent with these models. The MTR assumption, instead, implies that, *ceteris paribus*, wage rises as a function of conjectured years of schooling. In other words, the MTR assumption is consistent with economic models of production of human capital through schooling. The MTS and MTR assumptions do not violate any conventional theories of human capital accumulation. By combining the MTS and MTR assumptions, we derive the MTR-MTS bounds[20]:

$$
\begin{aligned}
&E[y|s<z] * P(s<z) + E[y|s=z] * P(s=z) + E[y|s=z] * P(s>z) \\
&\leq E[y(z)] \leq \\
&E[y|s=z] * P(s<z) + E[y|s=z] * P(s=z) + E[y|s>z] * P(s>z)
\end{aligned} \tag{8}
$$

The combined MTR-MTS assumption can be tested. Under this assumption, the average wage of

[19] Note that this precludes the possibility that individuals undertake extra education in the knowledge that their earnings may actually be lower as a result. For example this rules out the possibility of a negative rate of return to PhD study (which is sometimes observed) and implies that education may not be undertaken for purely consumption reasons.

[20] A complete derivation of the MTR and MTR-MTS bounds is presented in Manski (1997) and Manski and Pepper (2000).

individuals ought to be weakly increasing in their own realized level of schooling. If this is not the case, then the MTR-MTS assumption should be rejected. In fact, the required monotonicity for the MTR-MTS assumption to be valid seems to be observed in this application. Table 5 reports the average level of weekly wages for all individuals, grouped by their level of education; estimates are reported for both the pooled sample and males only since the analysis will be carried out for both the pooled sample and the male subsample only. Weekly wages generally appear to be weakly increasing in the age at which individuals left school.

TABLE 5 - MEAN WEEKLY WAGE BY AGE LEFT SCHOOLING

Age Left Schooling	Weekly Earnings Pooled Sample	Weekly Earnings Male Sample
14	5.145	5.591
15	5.189	5.752
16	5.395	5.886
17	5.611	6.088
18	5.860	6.254
19	5.992	6.205

Since we observe additional variables of relevance in our data, we can use this information in our analysis; for simplicity of exposition, we call this informative variable v. Using this information, we can split our sample into subsamples, one for each level of v, and obtain lower and upper bounds on the returns to education for each subsample. Since some bounds may be relatively tighter than others for some subsamples of our data, we can exploit this variation in the bounds over the subsamples if v satisfies the monotone instrumental variable (MIV) assumption, a weaker version of the instrumental variable (IV) assumption (Manski and Pepper 2000)[21]. A variable v satisfies the MIV assumption, in the sense of mean monotonicity, if it holds that for all values of the instrument $m \in M$

(9) $$m_1 \leq m \leq m_2 \rightarrow E[y(z)|v = m_1] \leq E[y(z)|v = m] \leq E[y(z)|v = m_2]$$

Therefore, the MIV assumption requires a weakly monotone relationship between the variable v and the mean wage equation of the individuals (Manski and Pepper 2000). This, in turn, implies that $E[y(z)|v = m]$ is no lower than the lower bound on $E[y(z)|v = m_1]$ and it is no higher than the upper bound on $E[y(z)|v = m_2]$. For the subsample in which $v = m$, we can estimate a new lower

[21] Note that this Manski terminology is confusing in the sense that this is not an IV in the sense we mean in 2SLS. Specifically we are not seeking a Z which correlates with X but not u. Rather we are seeking a covariate which is monotone in y, i.e., the outcome variable. In this example we happen to use the ROSLA as a convenient covariate which just happens to be the IV in our RDD/IV analysis.

bound, by taking the largest lower bound over all the subsamples in which $v \leq m$. In a similar way, we can estimate a new upper bound, by taking the smallest upper bound over all subsamples in which $v \geq m$. By repeating this for all values of v, we derive the MIV bounds; by taking the weighted average of the MIV bounds over v, the aggregate MIV bounds are obtained as follows:

$$\sum_{m \in M} P(v = m) * \left[max_{m_1 \leq m} LB_{E[y(z)|v=m_1]}\right] \leq E[y(z)] \leq \sum_{m \in M} P(v = m) * \left[min_{m_2 \geq m} UB_{E[y(z)|v=m_2]}\right] \tag{10}$$

The MIV used in this article reflects the period in which the individual was born, according to the introduction of ROSLA laws in Great Britain; in other words, our MIV happens to group individuals in the same way as our IV in our RDD/IV analysis. Therefore, our MIV takes the value 1 if the individual was born before April 1st 1933. For the cohorts born in between April 1st 1933 and August 30th 1957, our MIV takes the value 2 if the individual was born in January, or April-August, and it takes the value 3 if the individual was born in February, March, or September-December. For the cohorts born starting from September 1st 1957, our MIV takes the value 4 if the individual was born in January, or April-August, and it takes the value 5 if the individual was born in February, March, or September-December. Therefore, this MIV reflects the monotonic increase of compulsory schooling faced by these cohorts of British pupils. By using this variable as an MIV, we assume that the mean wage function of an individual is monotonically increasing (or nondecreasing) in the period in which the individual was born. In other words, supposing that the level of education was the same in all subsamples, the MIV assumption in this application states that the average wage level we would observe for individuals born in later cohorts would be weakly higher than the average wage level we would observe for individuals born in earlier cohorts. We believe this assumption is plausible, given the positive trends in wages that took place in Great Britain over time in the period of observation, and given that cohorts with an associated higher value of our MIV faced higher minimum-school-leaving ages. We test this assumption in Appendix G[22].

Finally, as stated at the beginning of this exposition, what we are really interested in is the average

[22] Table G.47 shows that monotonicity is not observed for the two highest values of our MIV; we attribute this simply to the fact that such unconditional estimates have not been de-trended over time. However, we believe the assumption of positive growth in mean wages over time in Great Britain is still plausible.

treatment effect of a marginal increase in the level of conjectured schooling, that is $\Delta(t,z) = E[y(z)] - E[y(t)]$. Therefore, in order to obtain bounds on this *ATE*, we will subtract the lower (upper) bound on $E[y(t)]$ from the upper (lower) bound on $E[y(z)]$ to retrieve the upper (lower) bound[23].

We use again all observations available from the 1983-2000 GHS survey years, from which information on month of birth is available. Similarly to what we did in previous sections, we include in the analysis all individuals born from 1921-1965 and aged 28-64 at the time of the survey; moreover, we still set to missing cases for which hourly wage observations are less than £1 or more than £150 (in December 2001 pounds) and we exclude cases where weekly hours are greater than 84, less than 1, or missing. This results in a sample of 84,010 individuals; by substituting into the formulas in the equations presented in this section the sample means and the empirical probabilities from our data, we obtain the NOAS, the MTR, MTR-MTS, and MTR-MTS-MIV bounds. For inference, we perform 1,000 bootstrap replications in each case.

Table 6 reports the results of our analysis for the male sample. Results are reported for the pooled sample in Table G.48 in Appendix G. In these tables, we compare the findings from our bounds analysis with the results of using an exogenous treatment selection (ETS) assumption; this assumption implies that $E[y|s=z] = E[y(z)|s \neq z]$, and therefore that the educational choice is unrelated to the unobservables of the individual. This approach yields point estimates, and it is equivalent to an OLS regression of wages on a dummy for each level of schooling. Table 6 also reports the estimates and the 0.90 bootstrap quantiles of the MTR-MTS bounds and the MTR-MTS-MIV bounds on $\Delta(z-1,z)$, $z = 15, \ldots, 19$, followed by the bounds on $\Delta(14,19)$, that constitute the maximum and the minimum age at which individuals left school in our sample.

The evidence presented in Table 6 allows a direct comparison with the results of Manski and Pepper (2000), as well as most of the previous literature. Our MTR-MTS-MIV upper bound estimate for $\Delta(14,15)$ is 0.186, which is smaller than the estimates of Manski and Pepper (2000); interestingly, these authors use data from the National Longitudinal Survey of Youth (NLSY), and they find an upper bounds estimate of 0.202 for $\Delta(14,15)$. Our MTR-MTS-MIV upper bound estimate of 0.150 for $\Delta(15,16)$ and of 0.107 for $\Delta(18,19)$ are smaller than the estimated returns by Harmon and Walker (1995), but they are consistent with the estimates in Card (1993), Ashenfelter and Krueger (1994) and Oreopoulos (2006).

[23] Under the MTR assumption, the effect of an increase in years of schooling on wages cannot be negative; therefore, the lower bound on $\Delta(z,s)$ is never below zero. The MIV bounds instead do not use this assumption to obtain bounds on $\Delta(z,s)$.

These bounds may still seem relatively wide; therefore, one may conclude that the MTR-MTS-MIV assumption does not have sufficient identifying power in this application. However, a different conclusion arises if we calculate summary effects of additional education and, similarly to Manski and Pepper (2000), we calculate the return to all possible five years of additional education in our sample. The upper bound estimate for $\Delta(14, 19)$ reported in Table 6 is 0.317, which implies that leaving school at the age of 19 yields at most an increase of 0.317 in mean ln(wage), relative to leaving school at 14. This, assuming that returns to an extra year of education are additive, implies that the average value of the five year-by-year treatment effects $\Delta(14, 15)$, $\Delta(15, 16)$, $\Delta(16, 17)$, $\Delta(17, 18)$ and $\Delta(18, 19)$ is at most 0.063; this estimate is well below the point estimates retrieved in most of the reviewed literature, and it is in line with the results of our own parametric analysis in the previous section; interestingly, this evidence is also consistent with the findings of Devereux and Hart (2010) and Clark and Royer (2013)[24].

One major motivation for using bounds is that this approach provides a way of obtaining the ATE by considering the average upper bound estimate over different domains of the years of education. Given that identification from RDD based on the ROSLA reforms retrieves a LATE of the RoRtE and it applies strictly only to those individuals who are affected by the reforms, this non-parametric measure of the ATE of the RoRtE could be extremely useful in assessing the general effect of education on earnings – rather than the localized effect of an extra year of education for a specific cohort of pupils. Pursuing this logic, Table 6 also reports separately the upper bound estimates for $\Delta(14, 16)$ and for $\Delta(16, 19)$. In the context of the ROSLA reforms studied here, the former captures the year-by-year treatment effect of additional education for those who were affected by the ROSLA policies; the latter, instead, captures the year-by-year treatment effect of those who decided to study for longer, i.e., who acquired additional years of education not as a direct consequence of the ROSLA reforms. The results suggest that the average value of the two year-by-year treatment effects $\Delta(14, 15)$ and $\Delta(15, 16)$ is at most 0.10; in contrast, the average value of the three year-by-year treatment effects $\Delta(16, 17)$, $\Delta(17, 18)$ and $\Delta(18, 19)$ is at most 0.059. We interpret these as different LATE of education, and not as the ATE of education, because these results are calculated used different subsamples of our population. However, it is interesting to notice that the year-by-year treatment effects for $\Delta(14, 16)$ are higher than the treatment effects for $\Delta(16, 19)$. Given that, by and large, the former captures the year-by-year treatment effect of education for the individuals who complied to the ROSLA reforms, while the effects for $\Delta(16, 19)$

[24] The same exercise on the pooled sample returns an upper bound estimate of 11.99, in line with the estimates in Card (1993), Ashenfelter and Krueger (1994) and Oreopoulos (2006).

are, *de facto*, the returns to voluntary education, this evidence is consistent with the large literature who finds IV estimates of the RoRtE greater than OLS estimates. A common justification for this in the literature is that the LATE RoRtE may be higher for the compliers than for a more general population (e.g., Card, 2001; and Lang, 1993); the results presented here provide supportive evidence to this explanation.

TABLE 6 - ETS POINT ESTIMATES AND NON-PARAMETRIC BOUNDS ON RETURNS TO EDUCATION

Male Sample	ETS β	MTR-MTS		MTR-MTS-MIV	
		Lower Bound	Upper Bound	Lower Bound	Upper Bound
Δ(14, 15)	0.161 (0.143, 0.179)	0 (0	0.311 0.328)	0 (0	0 .186 0.234)
Δ(15, 16)	0.134 (0.123, 0.146)	0 (0	0.221 0.232)	0 (0	0.150 0.165)
Δ(16, 17)	0.202 (0.183, 0.220)	0 (0	0.298 0.313)	0 (0	0.172 0.202)
Δ(17, 18)	0.166 (0.145, 0.188)	0 (0	0.389 0.404)	0 (0	0.276 0.307)
Δ(18, 19)	-0.049 (-0.086, -0.011)	0 (0	0.316 0.351)	0 (0	0.107 0.197)
Summary Effects:					
Δ(14, 16)	0.295 (0.277, 0.314)	0 (0	0.370 0.388)	0 (0	0. 200 0. 248)
Δ(16, 19)	0.319 (0.283, 0.356)	0 (0	0.391 0.427)	0 (0	0.177 0.267)
Δ(14, 19)	0.615 (0.575, 0.654)	0 (0	0.615 0.654)	0 (0	0.317 0.417)

NOTE – Dependent variable is ln(weekly wage). Numbers between parentheses are Imbens-Manski 90% confidence intervals. Number of observations is 39,548.

VII. Conclusion

This paper has revisited the empirical literature on the returns to education in the UK. Courtesy of exogenous UK government school leaving reforms - this is a particularly interesting setting to investigate this important parameter. Not surprisingly, a rich literature has flourished in this context.

This paper shows that the estimates previously reported depend crucially on some key specification assumptions that have little rigorous justification in the economic literature. The key contribution of this paper lies in the rigorous examination of the appropriate first stage estimation and the adoption of an innovative IV strategy, as well as using the non-parametric bounds approach that only makes mild, testable assumptions of the data to provide consistent conclusions.

The key contributions to the literature on the rate of return to education are open to four criticisms. Firstly, the early contributions unjustifiably wanted to infer the estimate of the ATE of the RoRtE from a simple earnings function. We are now much more aware of the problems of endogeneity bias to accept this interpretation. Secondly, the contributions which used the IV LATE strategy of the ROSLA natural experiment wish to make general conclusions about the ATE of the RoRtE. This is not possible based on only a comparison of the compliers to the reform with those not subject to the reform. Thirdly the most important contributions to the debate are replete with different equation specifications and different data sets. Not surprisingly – in this context they have obtained widely different estimates of the RoRtE – from 0.0 to over 0.15 – depending on the data and the equation estimated. Finally, the literature has treated this parameter estimate as if it were the answer to not just the (P1) policy problem of wishing to estimate the ATT effect of a historical policy – in the terms of Heckman and Pinto (2013) - but also as if it were the answer to the underlying problem of seeking to estimate the ATE impact of acquiring education on earnings.

Given the hugely different results in the literature from various contributions then if one wants to make sense of the whole debate one must return to the data – standardize it, replicate the results of the key contributions and examine their robustness to different equation specifications using the same estimation methods. Then one must improve on these estimates if one can – in our case by improving on the IV – by - in this case – being very careful about what this IV is measuring – not just an extra year of education – but conditional on when a birthday falls this could be as little as .66 of a year or as much as 2.33 of a year of extra schooling – we address this issue and find it to be relevant. Next, one should address the important question of how these conclusions might differ if alternative estimation techniques are employed. Finally, one should then see if the results obtained are different if one uses less restrictive estimation techniques on the same data. This last step in our case involved using non-parametric bounds estimation.

So the contribution of this research to the debate relating to the rate of return to education is fourfold. Firstly we examined the robustness of the papers by Harmon and Walker (1995), Oreopoulos (2006) and Devereux and Hart (2010). We do this by using all the available data and examining the sensitivity of the results to the specification of the relevant first stage and the earnings function and the precise sample used. This provides the means whereby the widely varying results of these three papers can be reconciled. Secondly, we generalize the IV approach of the previous papers by using the month of birth in conjunction with the ROSLA in the calculation of a more accurate IV. We show that our approach, in turn provides more consistency in the results. Thirdly, we compare each of the three SLA regimes with the Raising of the School Leaving Age (ROSLA) reforms in 1947, 1962 and 1972 and carefully explain what was different about each reform. Accordingly, we then reflect on the general applicability of our results for the interpretation of the rate of return estimator as a 'causal' parameter. Finally, being aware of the limitations of the IV strategies we compare the parametric estimates obtained by the IV identification strategy with the alternative of non-parametric bounds analysis. Again we find that our results are very consistent with our previous conclusions.

The crucial question which must be addressed to our results in context is what does this LATE IV estimate actually mean? Quite literally our estimate is formally the ATT estimate of the rate of return to education for those who were forced to stay on at school for an extra year or more. This is really rather a special parameter. It addresses – in the terms of Heckman and Pinto (2013) - P1 type problem of wishing to estimate the ATT effect of a historical policy – namely ROSLA - on the extra marginal value of education. This parameter will not necessarily allow us to say anything on the P2 problem of: forecasting the impact of ROSLA on other environments (other times, countries or settings).

Perhaps, more disturbing is the realization that this identification strategy will not allow us to uncover the much more fundamental policy parameter of what the general effect of extra education is on earnings – outside of the situation when pupils are forced to stay on at school another year. This is the most fundamental policy parameter of all. In many respects this general parameter is much more fundamental – as it represents an estimate of the ATE impact of acquiring education on earnings. This policy parameter is of the utmost importance to governments who may not wish to raise the school leaving age but wish to know, at the margin, what is the effect of inducing people to acquire more education.

The conclusions of our empirical research is that the RoRtE based on the LATE IV using the ROSLA policy in the UK is .06 for males based on a standard specification and that this is invariant to which data set is used. The estimates which one can get by using different specifications using different data – as done by Harmon and Walker (1995) *et al* is that the estimates can vary as widely as 0.0 to 0.15. Hence our standardization of the data and estimated specification reaps a considerable dividend. We also find that the RoRtE for the pooled sample is much larger but still quite consistent. However we suggest that this is not a sensible sample to use to achieve stable results. The inherent difficulty of including the women into the analysis without explicitly modelling the selectivity of women into work and the problem of fertility and the dynamics of female labour supply makes this problem intractable for a pooled sample.

Our analysis based on the use of non-parametric estimation revealed reassuringly similar conclusions to the estimation based on parametric methods. Namely that on average a RoRtE of around .06 is also found when the estimated bounds estimates for each level of the year of schooling are averaged over the entire school year range. Finally, reflecting on the policy import of a RoRtE estimate of 6% we suggest that – relative to the literature this is not as large as we were led to believe from early contributions to this debate. Such an estimate is also well short of the 15% and higher estimates which so influenced the Blair Labour government's policy of having a target cohort higher education participation rate of 50%.

VIII. References

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APPENDICES

Appendix A. Descriptive Statistics

Table A.7 – GHS Descriptive Statistics for the study of 1947 SLA Reform

1947 SLA Reform	**GHS 1979-2006**					**GHS 1979-1998**					**GHS 1983-1998**					**GHS 1979-1986**				
Variable	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**
Survey Year	96,549	88.13	7.24	79	106	85,766	86.29	5.30	79	98	53,502	89.57	3.90	84	98	71,388	82.26	2.32	79	86
Cohort	96,549	20.20	8.06	1	31	85,766	19.42	8.13	1	31	53,502	20.90	7.30	1	31	71,388	23.23	12.89	-6	49
Female	96,549	0.45	0.50	0	1	85,766	0.45	0.50	0	1	53,502	0.46	0.50	0	1	71,388	0.44	0.50	0	1
Age	96,549	47.59	8.57	28	64	85,766	46.47	8.35	28	64	53,502	48.34	7.42	28	64	71,388	38.53	12.60	18	64
Employee	96,549	0.90	0.30	0	1	85,766	0.91	0.29	0	1	53,502	0.89	0.31	0	1	71,388	0.94	0.24	0	1
Hours Worked	96,549	34.85	12.97	1	84	85,766	34.82	12.88	1	84	53,502	35.03	13.33	1	84	71,388	35.18	11.53	1	84
Log (Hourly Wage)	96,549	1.91	0.59	0.00	5.01	85,766	1.88	0.58	0.00	5.01	53,502	1.94	0.60	0.00	5.01	71,388	1.77	0.52	0.00	4.93
Log (Weekly Earnings)	96,549	5.35	0.87	0.08	9.15	85,766	5.33	0.87	0.08	9.14	53,502	5.39	0.88	1.05	9.14	71,388	5.24	0.79	0.08	8.99
Age Left School	96,549	15.58	1.43	10	24	85,766	15.46	1.22	10	22	53,502	15.58	1.21	10	22	71,388	15.56	1.24	10	22
Left School by Age 14	96,549	0.17	0.37	0	1	85,766	0.19	0.39	0	1	53,502	0.13	0.34	0	1	71,388	0.19	0.40	0	1
Left School by Age 15	96,549	0.62	0.49	0	1	85,766	0.63	0.48	0	1	53,502	0.60	0.49	0	1	71,388	0.54	0.50	0	1
Left School by Age 16	96,549	0.81	0.39	0	1	85,766	0.82	0.38	0	1	53,502	0.81	0.40	0	1	71,388	0.81	0.39	0	1
Law mandates school until 15	96,549	0.79	0.40	0	1	85,766	0.77	0.42	0	1	53,502	0.84	0.36	0	1	71,388	0.75	0.43	0	1
Law mandates school until 16	96,549	0	0	0	0	85,766	0	0	0	0	53,502	0	0	0	0	71,388	0.17	0.37	0	1
Law mandates school until 15 - Different Calc for Scotland	96,549	0.79	0.40	0	1	85,766	0.77	0.42	0	1	53,502	0.84	0.36	0	1	71,388	0.75	0.43	0	1
Law mandates school until 15 - Different Calc for Scotland																				
Law mandates school until 16 - Different Calc for Scotland	96,549	0	0	0	0	85,766	0	0	0	0	53,502	0	0	0	0	71,388	0.16	0.36	0	1
Law mandates school until 16 - Different Calc for Scotland																				

Table A.8 – GHS Descriptive Statistics for the study of 1972 SLA Reform

1972 SLA Reform	GHS 1979-2006					GHS 1979-1998					GHS 1983-1998					GHS 1979-1986				
Variable	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**
Survey Year	121,614	91.97	7.80	79	106	92,730	88.55	5.41	79	98	71,943	90.74	3.97	84	98	71,388	82.26	2.32	79	86
Cohort	121,614	29.67	8.05	15	45	92,730	28.25	7.79	15	45	71,943	29.49	7.96	15	45	71,388	23.23	12.89	-6	49
Female	121,614	0.47	0.50	0	1	92,730	0.46	0.50	0	1	71,943	0.47	0.50	0	1	71,388	0.44	0.50	0	1
Age	121,614	42.07	8.42	28	64	92,730	39.96	7.60	28	64	71,943	40.96	7.88	28	64	71,388	38.53	12.60	18	64
Employee	121,614	0.90	0.30	0	1	92,730	0.90	0.29	0	1	71,943	0.90	0.31	0	1	71,388	0.94	0.24	0	1
Hours Worked	121,614	35.41	13.15	1	84	92,730	35.16	13.14	1	84	71,943	35.40	13.34	1	84	71,388	35.18	11.53	1	84
Log (Hourly Wage)	121,614	1.99	0.60	0.00	5.01	92,730	1.94	0.58	0.00	5.01	71,943	1.98	0.59	0.00	5.01	71,388	1.77	0.52	0.00	4.93
Log (Weekly Earnings)	121,614	5.46	0.87	0.08	9.15	92,730	5.40	0.87	0.08	8.89	71,943	5.44	0.87	0.15	8.89	71,388	5.24	0.79	0.08	8.99
Age Left School	121,614	16.17	1.58	10	24	92,730	15.92	1.13	10	22	71,943	15.98	1.14	10	22	71,388	15.56	1.24	10	22
Left School by Age 14	121,614	0.02	0.14	0	1	92,730	0.02	0.15	0	1	71,943	0.02	0.14	0	1	71,388	0.19	0.40	0	1
Left School by Age 15	121,614	0.42	0.49	0	1	92,730	0.46	0.50	0	1	71,943	0.42	0.49	0	1	71,388	0.54	0.50	0	1
Left School by Age 16	121,614	0.73	0.45	0	1	92,730	0.76	0.43	0	1	71,943	0.74	0.44	0	1	71,388	0.81	0.39	0	1
Law mandates school until 15	121,614	1	0	1	1	92,730	1	0	1	1	71,943	1	0	1	1	71,388	0.75	0.43	0	1
Law mandates school until 16	121,614	0.21	0.40	0	1	92,730	0.16	0.35	0	1	71,943	0.20	0.39	0	1	71,388	0.17	0.37	0	1
Law mandates school until 15 - Different Calc for Scotland	121,614	1	0	1	1	92,730	1	0	1	1	71,943	1	0	1	1	71,388	0.75	0.43	0	1
Law mandates school until 16 - Different Calc for Scotland	121,614	0.16	0.36	0	1	92,730	0.14	0.34	0	1	71,943	0.18	0.37	0	1	71,388	0.16	0.36	0	1

Table A.9 – GHS Descriptive Statistics for the joint study of 1947 & 1972 SLA Reforms

1947 & 1972 SLA Reforms	GHS 1979-2006					GHS 1979-1998					GHS 1983-1998					GHS 1979-1986				
Variable	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**
Survey Year	145,928	90.65	7.90	79	106	117,044	87.62	5.50	79	98	83,236	90.34	3.99	84	98	71,388	82.26	2.32	79	86
Cohort	145,928	26.17	10.85	1	45	117,044	24.18	10.69	1	45	83,236	26.81	10.11	1	45	71,388	23.23	12.89	-6	49
Female	145,928	0.46	0.50	0	1	117,044	0.46	0.50	0	1	83,236	0.46	0.50	0	1	71,388	0.44	0.50	0	1
Age	145,928	44.22	9.26	28	64	117,044	43.08	9.35	28	64	83,236	43.23	9.41	28	64	71,388	38.53	12.60	18	64
Employee	145,928	0.90	0.30	0	1	117,044	0.91	0.29	0	1	83,236	0.90	0.31	0	1	71,388	0.94	0.24	0	1
Hours Worked	145,928	35.25	13.02	1	84	117,044	35.02	12.98	1	84	83,236	35.23	13.33	1	84	71,388	35.18	11.53	1	84
Log (Hourly Wage)	145,928	1.96	0.59	0.00	5.01	117,044	1.91	0.58	0.00	5.01	83,236	1.96	0.60	0.00	5.01	71,388	1.77	0.52	0.00	4.93
Log (Weekly Earnings)	145,928	5.42	0.87	0.08	9.15	117,044	5.37	0.87	0.08	9.14	83,236	5.42	0.88	0.15	9.14	71,388	5.24	0.79	0.08	8.99
Age Left School	145,928	15.94	1.61	10	24	117,044	15.68	1.24	10	22	83,236	15.84	1.21	10	22	71,388	15.56	1.24	10	22
Left School by Age 14	145,928	0.11	0.32	0	1	117,044	0.14	0.35	0	1	83,236	0.09	0.29	0	1	71,388	0.19	0.40	0	1
Left School by Age 15	145,928	0.47	0.50	0	1	117,044	0.53	0.50	0	1	83,236	0.46	0.50	0	1	71,388	0.54	0.50	0	1
Left School by Age 16	145,928	0.75	0.43	0	1	117,044	0.79	0.41	0	1	83,236	0.76	0.43	0	1	71,388	0.81	0.39	0	1
Law mandates school until 15	145,928	0.86	0.34	0	1	117,044	0.83	0.37	0	1	83,236	0.89	0.30	0	1	71,388	0.75	0.43	0	1
Law mandates school until 16	145,928	0.18	0.38	0	1	117,044	0.12	0.32	0	1	83,236	0.17	0.37	0	1	71,388	0.17	0.37	0	1
Law mandates school until 15 - Different Calc for Scotland	145,928	0.87	0.34	0	1	117,044	0.84	0.37	0	1	83,236	0.90	0.30	0	1	71,388	0.75	0.43	0	1
Law mandates school until 16 - Different Calc for Scotland	145,928	0.13	0.34	0	1	117,044	0.11	0.31	0	1	83,236	0.15	0.36	0	1	71,388	0.16	0.36	0	1

Table A.10 – FES Descriptive Statistics for the joint study of 1947 & 1972 SLA Reforms

	Pooled Sample					Male Sample				
Variable	**Obs**	**Mean**	**Std Dev**	**Min**	**Max**	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Max**
Ln(wage)	61,019	1.751	0.345	-0.769	3.199	34,335	1.931	0.198	-0.211	3.199
Years of Schooling	61,019	16.159	2.136	14	23	34,335	16.159	2.203	14	23
Age	61,019	38.447	12.562	18	64	34,335	38.739	12.667	18	64
Yorkshire	61,019	0.088	0.283	0	1	34,335	0.088	0.283	0	1
Northwest	61,019	0.112	0.315	0	1	34,335	0.110	0.312	0	1
East Midlands	61,019	0.073	0.261	0	1	34,335	0.075	0.264	0	1
West Midlands	61,019	0.098	0.297	0	1	34,335	0.099	0.298	0	1
East Anglia	61,019	0.036	0.186	0	1	34,335	0.037	0.188	0	1
Southeast	61,019	0.310	0.463	0	1	34,335	0.306	0.461	0	1
Southwest	61,019	0.072	0.259	0	1	34,335	0.074	0.262	0	1
Scotland	61,019	0.090	0.286	0	1	34,335	0.089	0.285	0	1
Northern Ireland	61,019	0.014	0.118	0	1	34,335	0.014	0.115	0	1
Wales	61,019	0.049	0.217	0	1	34,335	0.051	0.219	0	1
Year = 1979	61,019	0.114	0.318	0	1	34,335	0.116	0.321	0	1
Year = 1980	61,019	0.116	0.320	0	1	34,335	0.116	0.321	0	1
Year = 1981	61,019	0.120	0.325	0	1	34,335	0.121	0.326	0	1
Year = 1982	61,019	0.116	0.320	0	1	34,335	0.117	0.322	0	1
Year = 1983	61,019	0.102	0.302	0	1	34,335	0.101	0.301	0	1
Year = 1984	61,019	0.107	0.310	0	1	34,335	0.104	0.306	0	1
Year = 1985	61,019	0.103	0.303	0	1	34,335	0.101	0.302	0	1
Year = 1986	61,019	0.105	0.307	0	1	34,335	0.102	0.303	0	1

Appendix B. Distribution of Age left Education and Earnings – By Month of Birth

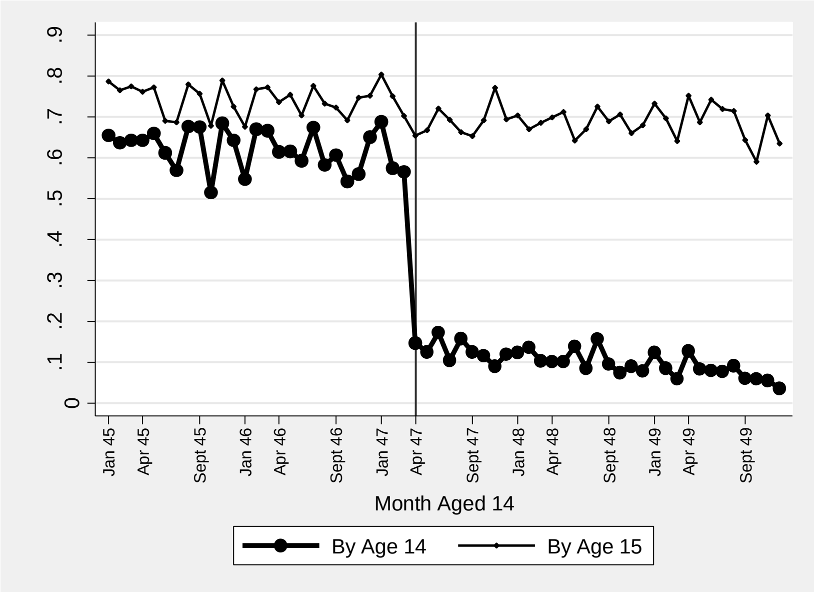

FIGURE B.6. FRACTION LEFT FULL-TIME EDUCATION BY MONTH AGED 14 AND 15
(*1931-35 Birth Cohorts - Pooled Sample*)

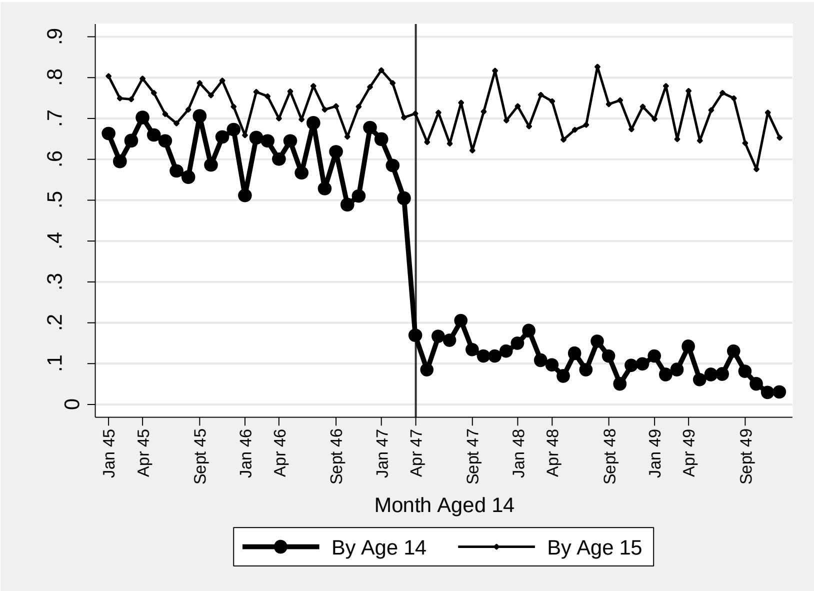

FIGURE B.7. FRACTION LEFT FULL-TIME EDUCATION BY MONTH AGED 14 AND 15
(*1931-35 Birth Cohorts - Male Sample*)

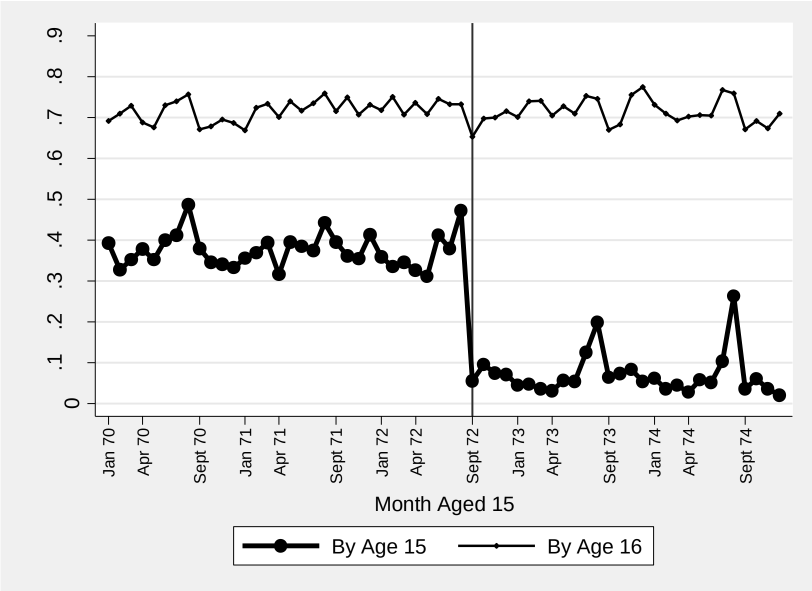

FIGURE B.8. FRACTION LEFT FULL-TIME EDUCATION BY MONTH AGED 15 AND 16

(1955-59 Birth Cohorts - Pooled Sample)

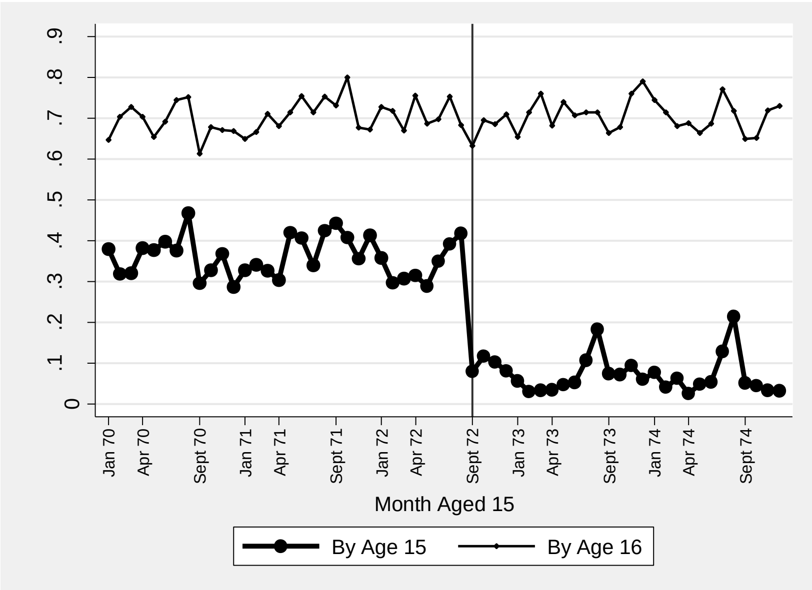

FIGURE B.9. FRACTION LEFT FULL-TIME EDUCATION BY MONTH AGED 15 AND 16

(*1955-59 Birth Cohorts - Male Sample*)

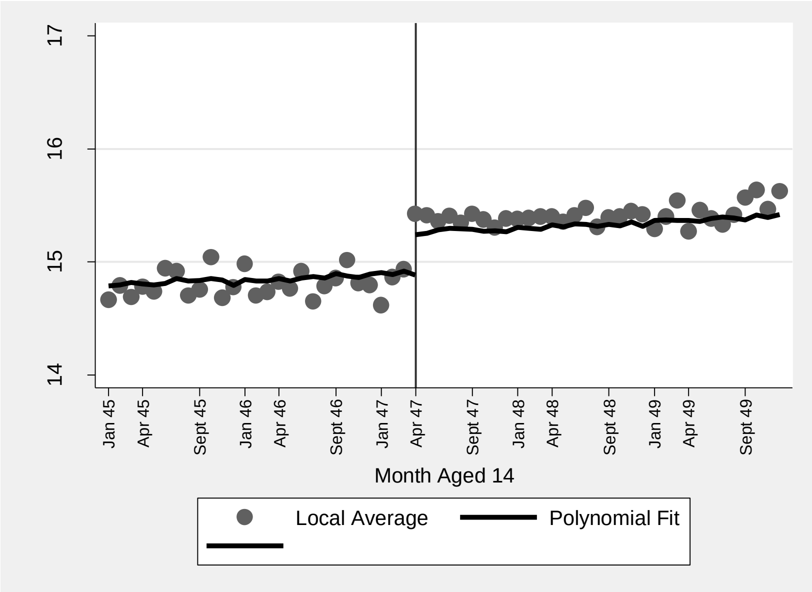

FIGURE B.10. AVERAGE AGE LEFT FULL-TIME EDUCATION BY MONTH AGED 14

(1931-35 Birth Cohorts - Pooled Sample)

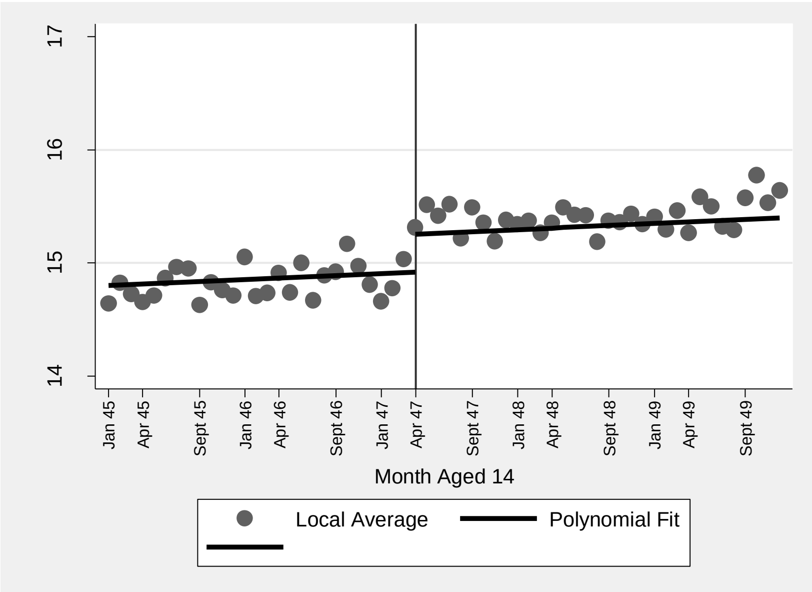

FIGURE B.11. AVERAGE AGE LEFT FULL-TIME EDUCATION BY MONTH AGED 14

(1931-35 Birth Cohorts - Male Sample)

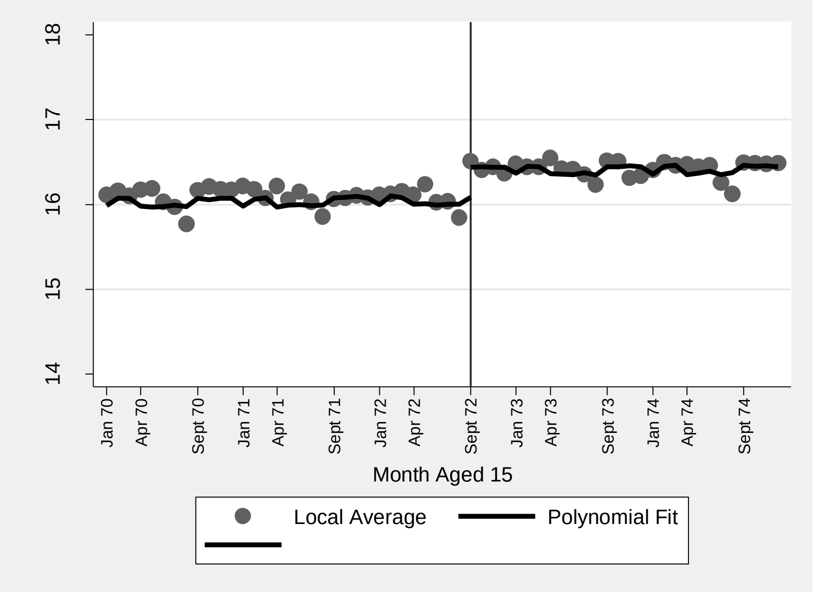

FIGURE B.12. AVERAGE AGE LEFT FULL-TIME EDUCATION BY MONTH AGED 15

(1955-59 Birth Cohorts - Pooled Sample)

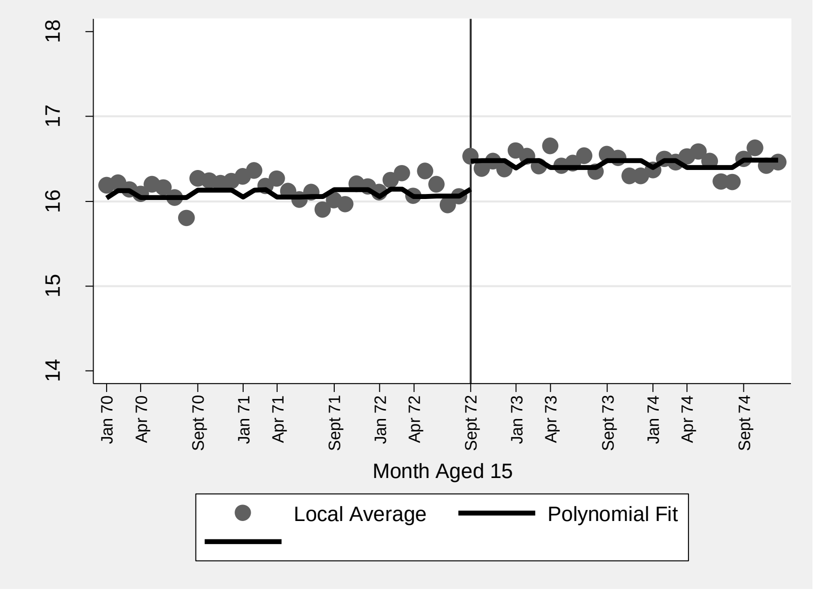

FIGURE B.13. AVERAGE AGE LEFT FULL-TIME EDUCATION BY MONTH AGED 15

(1955-59 Birth Cohorts - Male Sample)

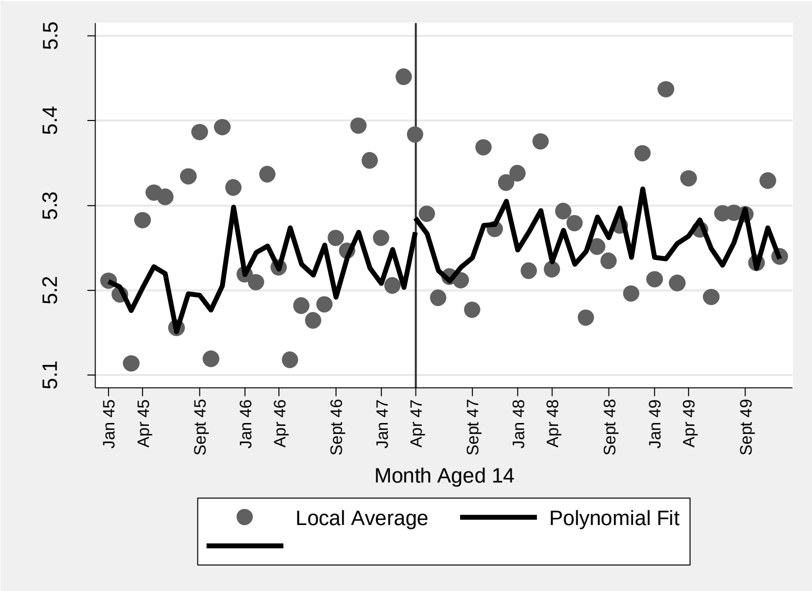

FIGURE B.14. AVERAGE ANNUAL LOG EARNINGS BY MONTH AGED 14

(1931-35 Birth Cohorts - Pooled Sample)

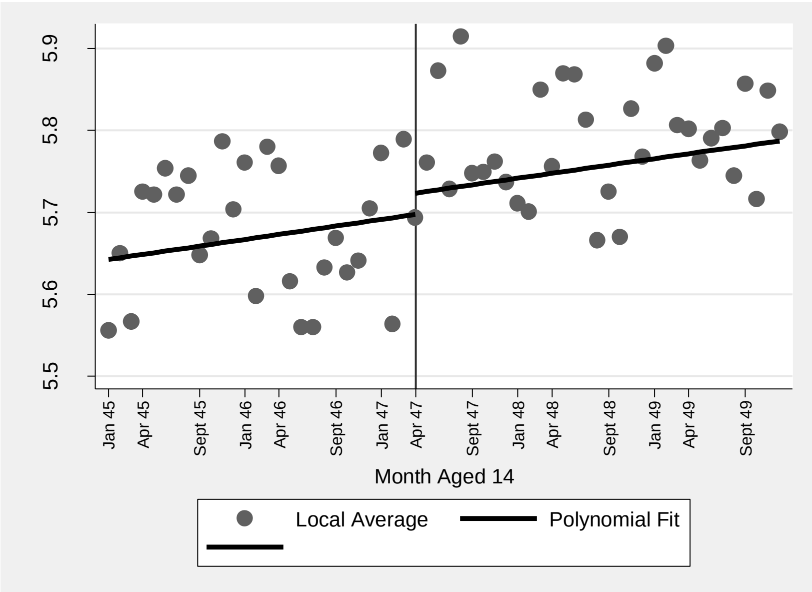

FIGURE B.15. AVERAGE ANNUAL LOG EARNINGS BY MONTH AGED 14

(1931-35 Birth Cohorts - Male Sample)

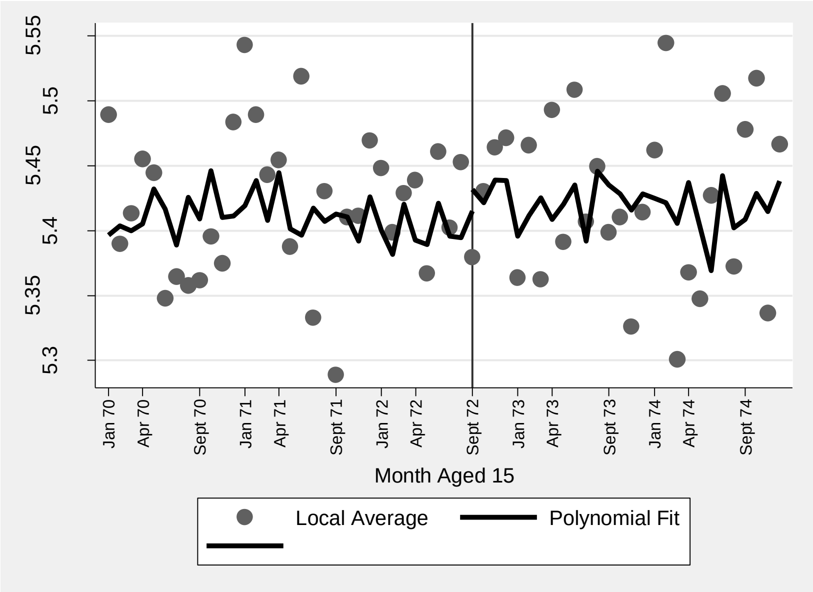

FIGURE B.16. AVERAGE ANNUAL LOG EARNINGS BY MONTH AGED 15

(1955-59 Birth Cohorts - Pooled Sample)

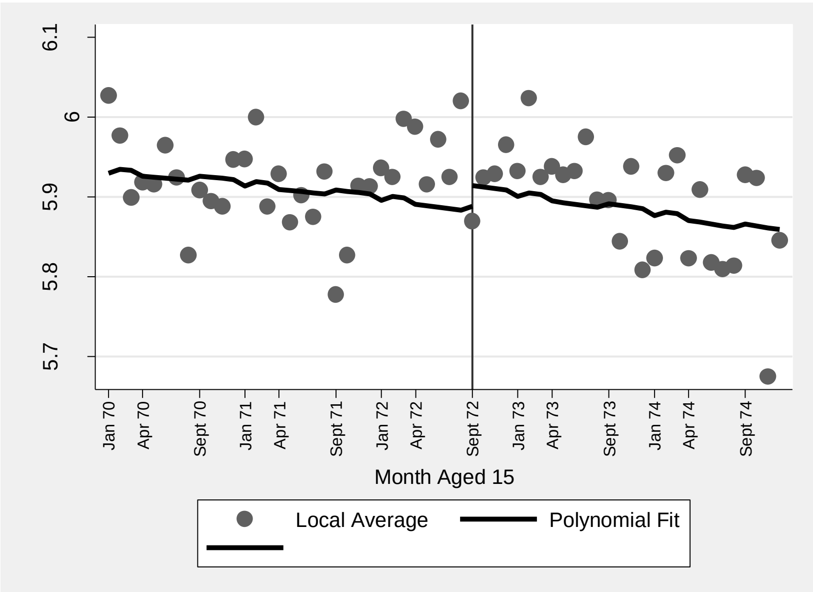

FIGURE B.17. AVERAGE ANNUAL LOG EARNINGS BY MONTH AGED 15

(1955-59 Birth Cohorts - Male Sample)

Appendix C. Analysis of Presence of Seasonality – by Month of Birth

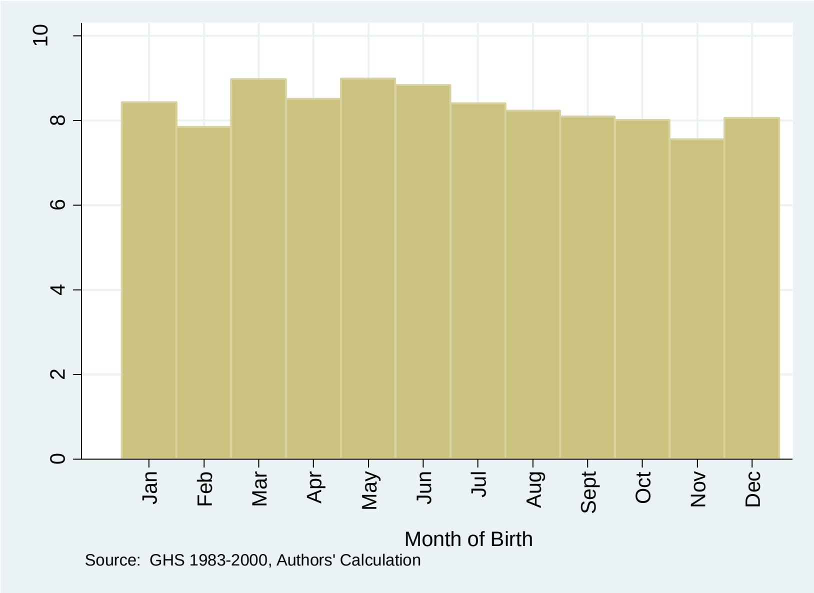

FIGURE C.18. OCCURRENCE OF BIRTHS ACROSS MONTHS - 1921-83 BIRTH COHORTS

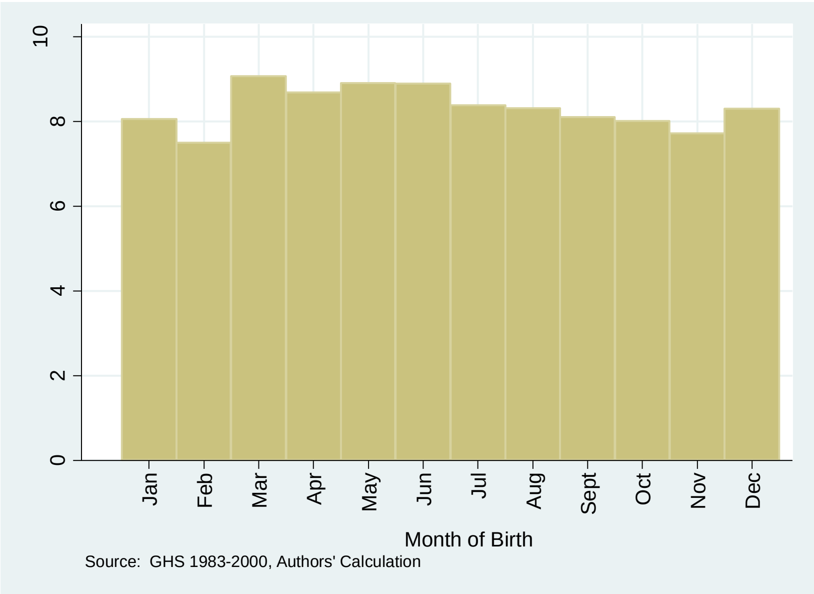

FIGURE C.19. OCCURRENCE OF BIRTHS ACROSS MONTHS - 1928-38 BIRTH COHORTS

FIGURE C.20. OCCURRENCE OF BIRTHS ACROSS MONTHS, BY YEAR - 1928-38 BIRTH COHORTS

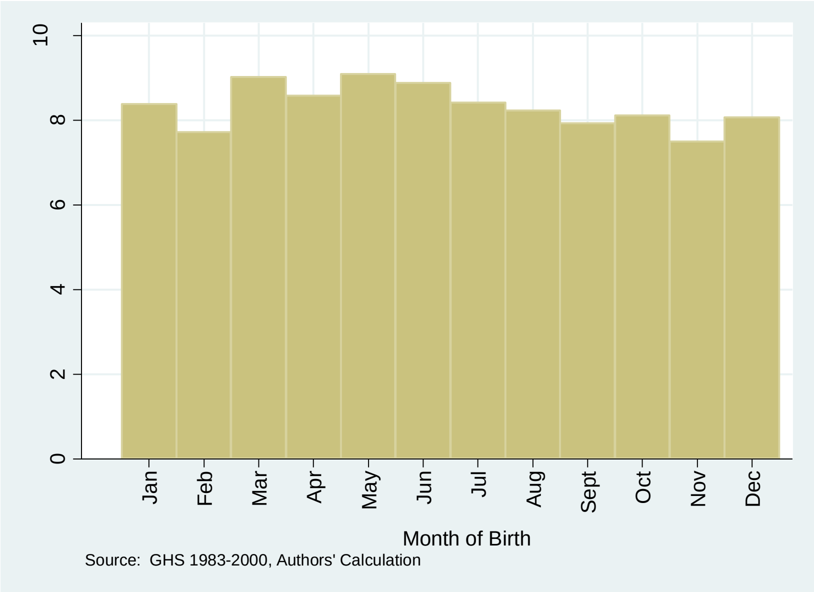

FIGURE C.21. OCCURRENCE OF BIRTHS ACROSS MONTHS, 1953-63 BIRTH COHORTS

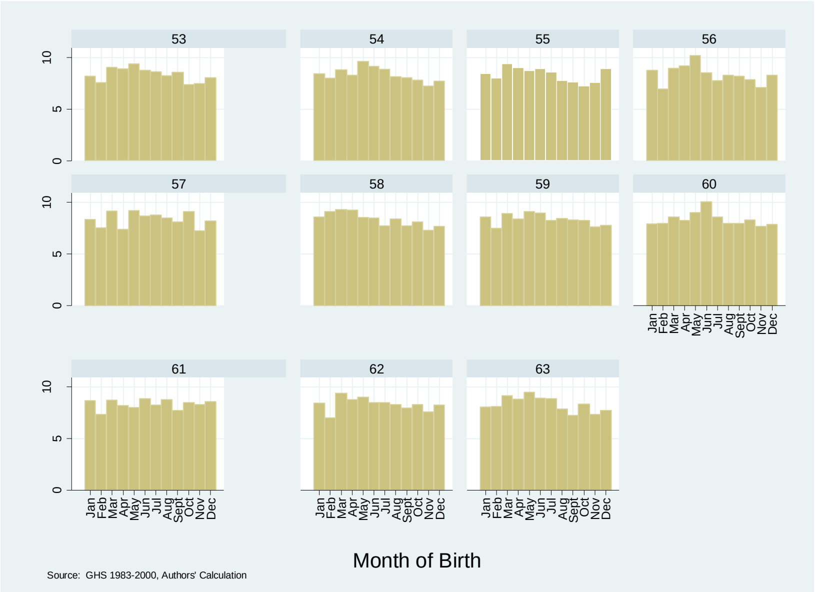

FIGURE C.22. OCCURRENCE OF BIRTHS ACROSS MONTHS, BY YEAR - 1953-63 BIRTH COHORTS

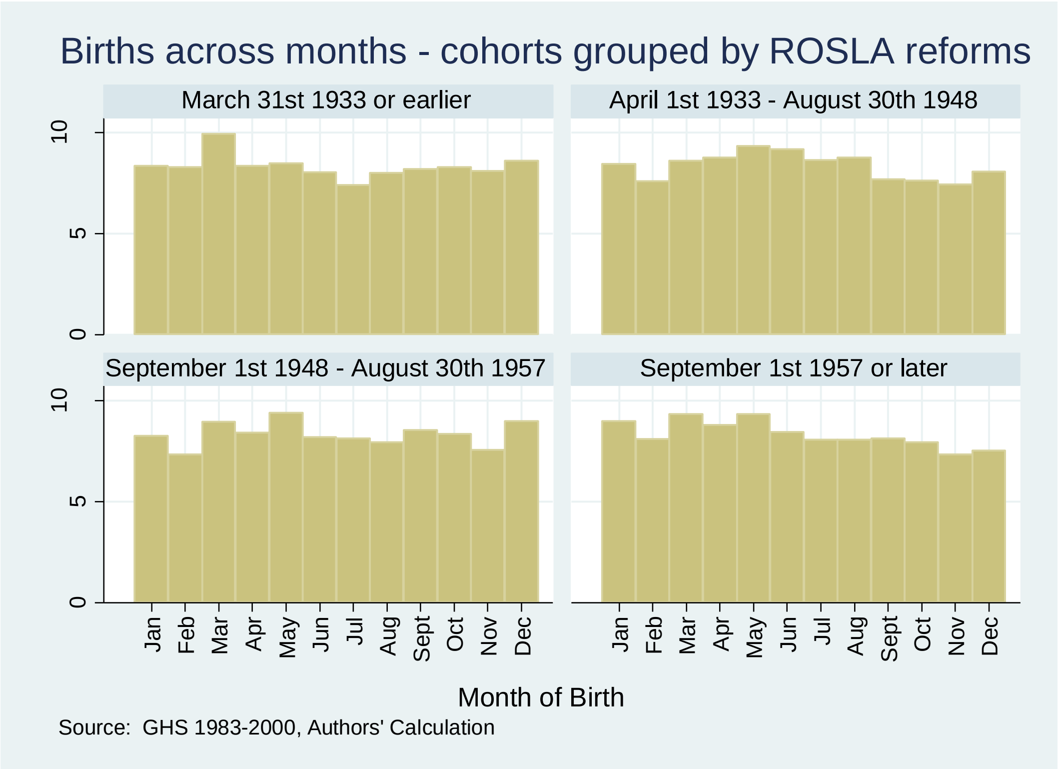

FIGURE C.23. OCCURRENCE OF BIRTHS ACROSS MONTHS, BY YEAR - 1953-63 BIRTH COHORTS. MALE SAMPLE

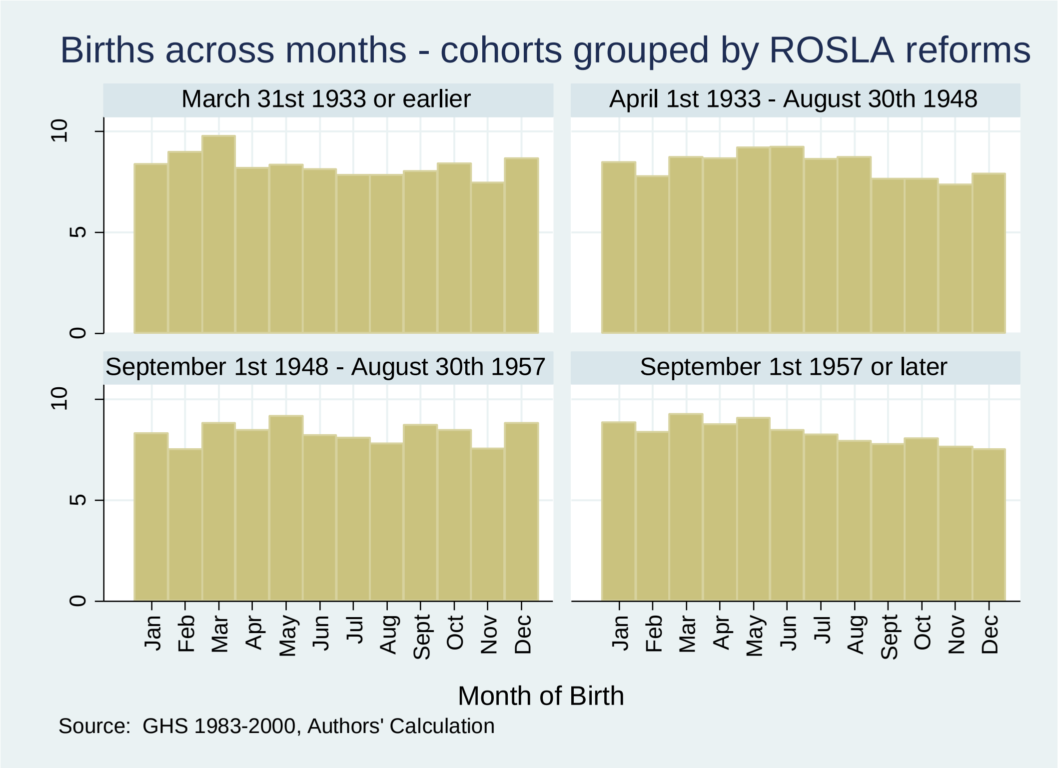

FIGURE C.24. OCCURRENCE OF BIRTHS ACROSS MONTHS, BY YEAR - 1953-63 BIRTH COHORTS. POOLED SAMPLE

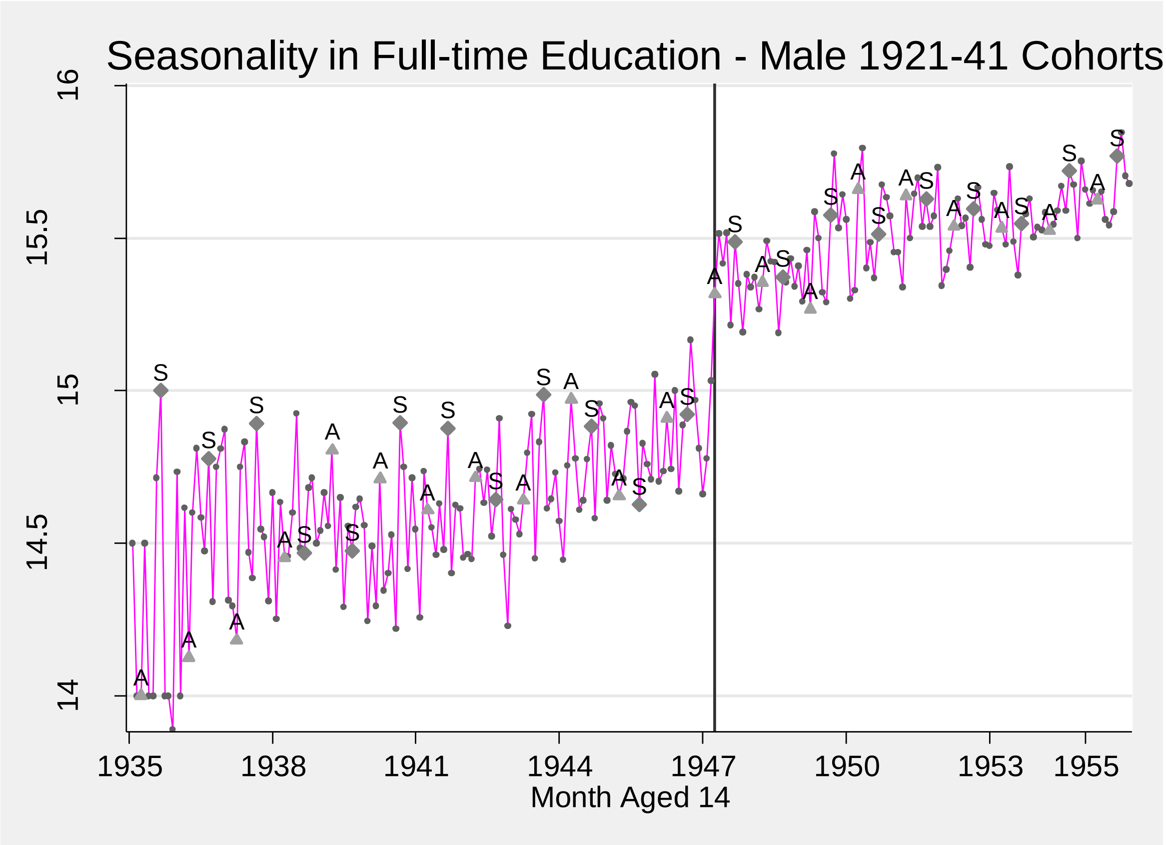

FIGURE C.25. SEASONALITY IN AGE LEFT FULL-TIME EDUCATION FOR COHORTS BORN IN 1921-41. MALE SAMPLE

TABLE C.11 – T-TESTS FOR SIGNIFICANCE OF DIFFERENCES IN AGE LEFT FULL-TIME EDUCATION FOR COHORTS BORN IN 1921-41. MALE SAMPLE

Month of Birth (vs all other months)	t	Pr(|T| > |t|)
January	-0.1863	0.8538
February	-1.2587	0.2206
March	-0.7751	0.4459
April	-0.2124	0.8336
May	0.3839	0.7045
June	0.4001	0.6927
July	-0.0572	0.9549
August	-0.4485	0.6577
September	1.5248	0.1398
October	0.4686	0.6438
November	0.2202	0.8276
December	0.0597	0.9529

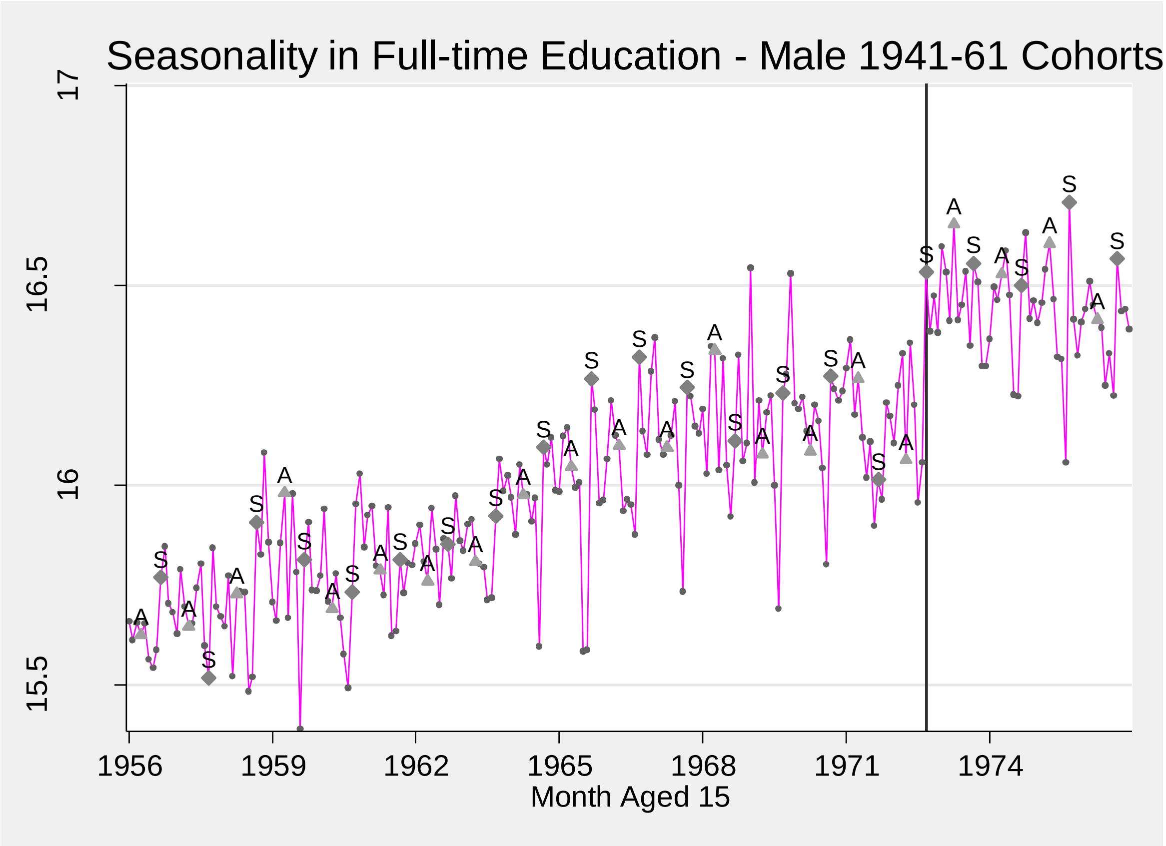

FIGURE C.26. SEASONALITY IN AGE LEFT FULL-TIME EDUCATION FOR COHORTS BORN IN 1941-61. MALE SAMPLE

TABLE C.12 – T-TESTS FOR SIGNIFICANCE OF DIFFERENCES IN AGE LEFT FULL-TIME EDUCATION FOR COHORTS BORN IN 1941-61. MALE SAMPLE

Month of Birth (vs all other months)	t	Pr(|T| > |t|)
January	0.4811	0.6349
February	0.6565	0.5177
March	0.3925	0.6982
April	0.2685	0.7907
May	-0.124	0.9023
June	-0.1137	0.9103
July	-2.0626	0.0502*
August	-4.3662	0.0002***
September	1.2942	0.2085
October	1.5603	0.1314
November	1.2265	0.2313
December	0.5328	0.5988

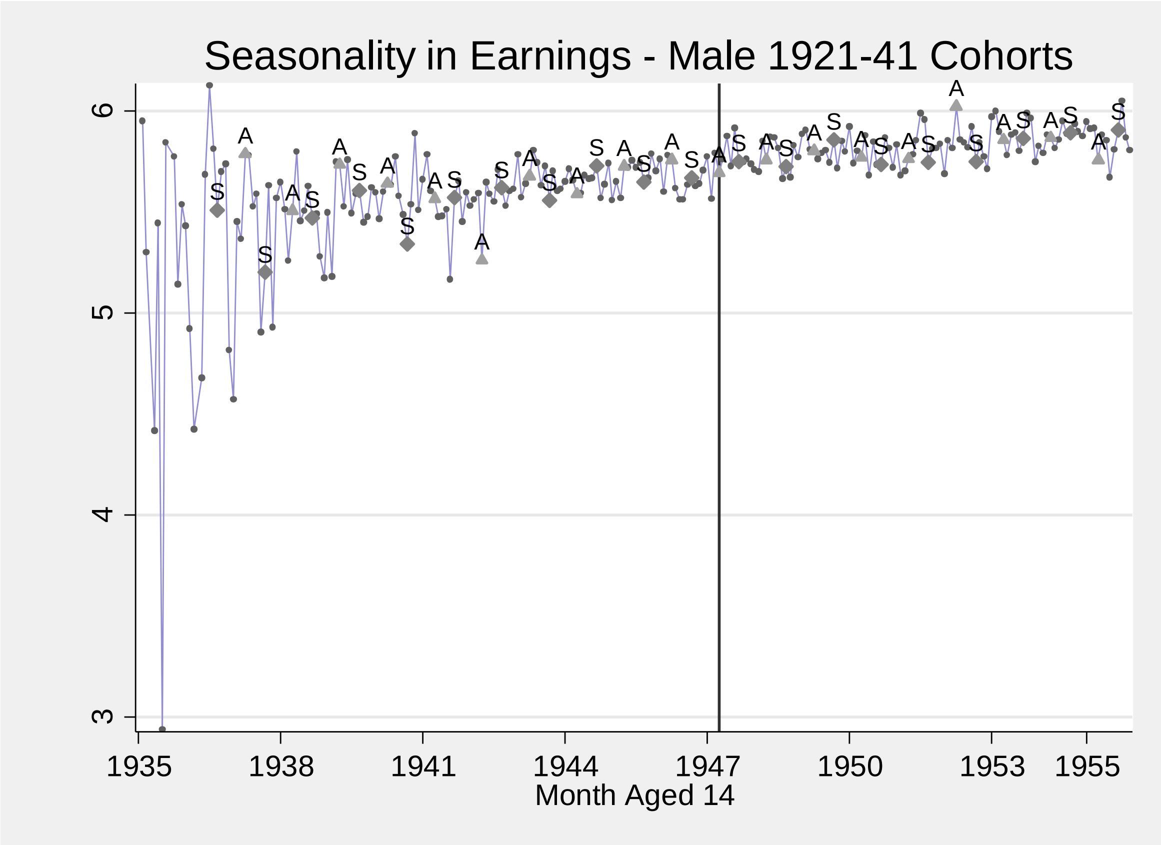

FIGURE C.27. SEASONALITY IN EARNINGS FOR COHORTS BORN IN 1921-41. MALE SAMPLE

TABLE C.13 – T-TESTS FOR SIGNIFICANCE OF DIFFERENCES IN EARNINGS FOR COHORTS BORN IN 1921-41. MALE SAMPLE

Month of Birth (vs all other months)	t	Pr(|T| > |t|)
January	0.0694	0.9453
February	-0.2628	0.7948
March	-0.5636	0.5785
April	1.4061	0.1697
May	-0.4469	0.6593
June	1.5529	0.1285
July	-0.5374	0.5967
August	0.4879	0.6297
September	-0.0177	0.986
October	1.4757	0.1488
November	-0.0524	0.9586
December	-0.5668	0.5757

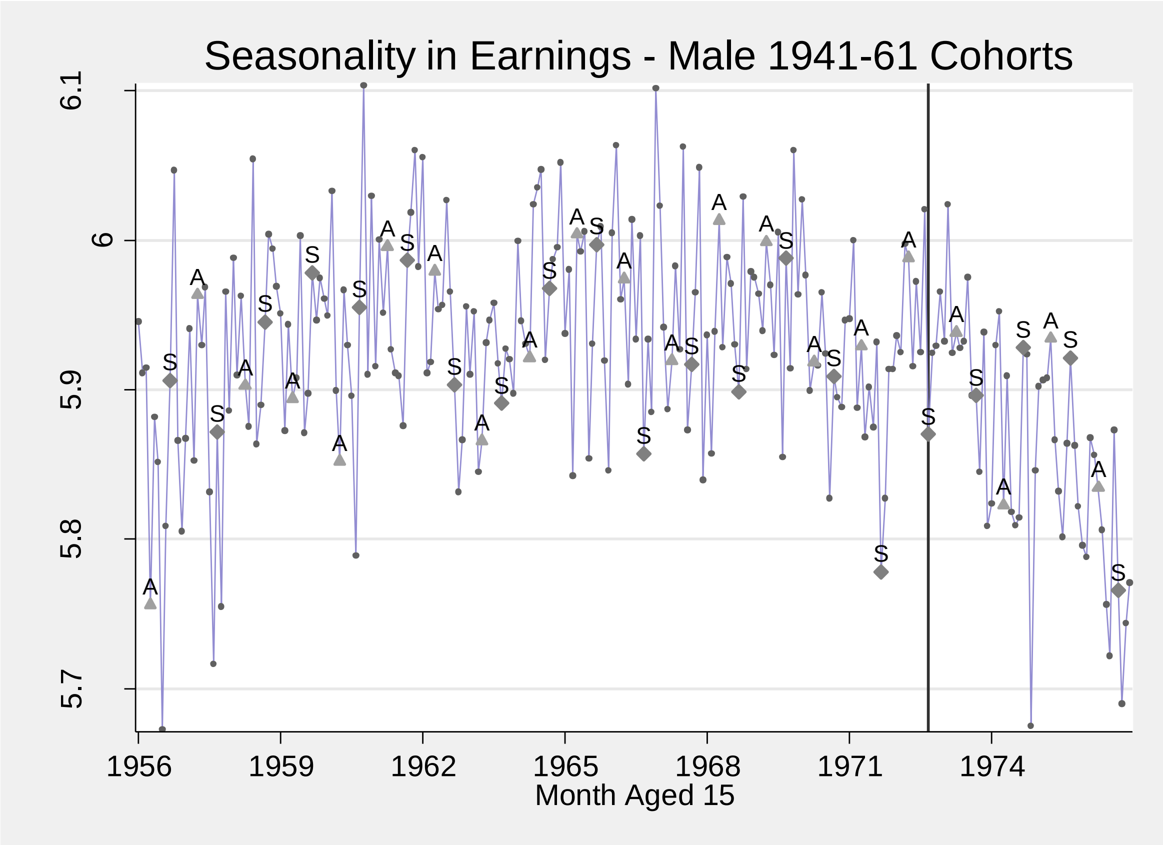

FIGURE C.28. SEASONALITY IN EARNINGS FOR COHORTS BORN IN 1941-61. MALE SAMPLE

TABLE C.14 – T-TESTS FOR SIGNIFICANCE OF DIFFERENCES IN EARNINGS FOR COHORTS BORN IN 1941-61. MALE SAMPLE

Month of Birth (vs all other months)	t	Pr(|T| > |t|)
January	1.6338	0.1147
February	2.2625	0.0318**
March	-0.654	0.5175
April	0.2028	0.841
May	0.176	0.8615
June	1.061	0.2993
July	-0.9438	0.3555
August	-2.3765	0.0257**
September	-0.7699	0.4483
October	0.2183	0.8292
November	-0.1348	0.894
December	-0.0754	0.9406

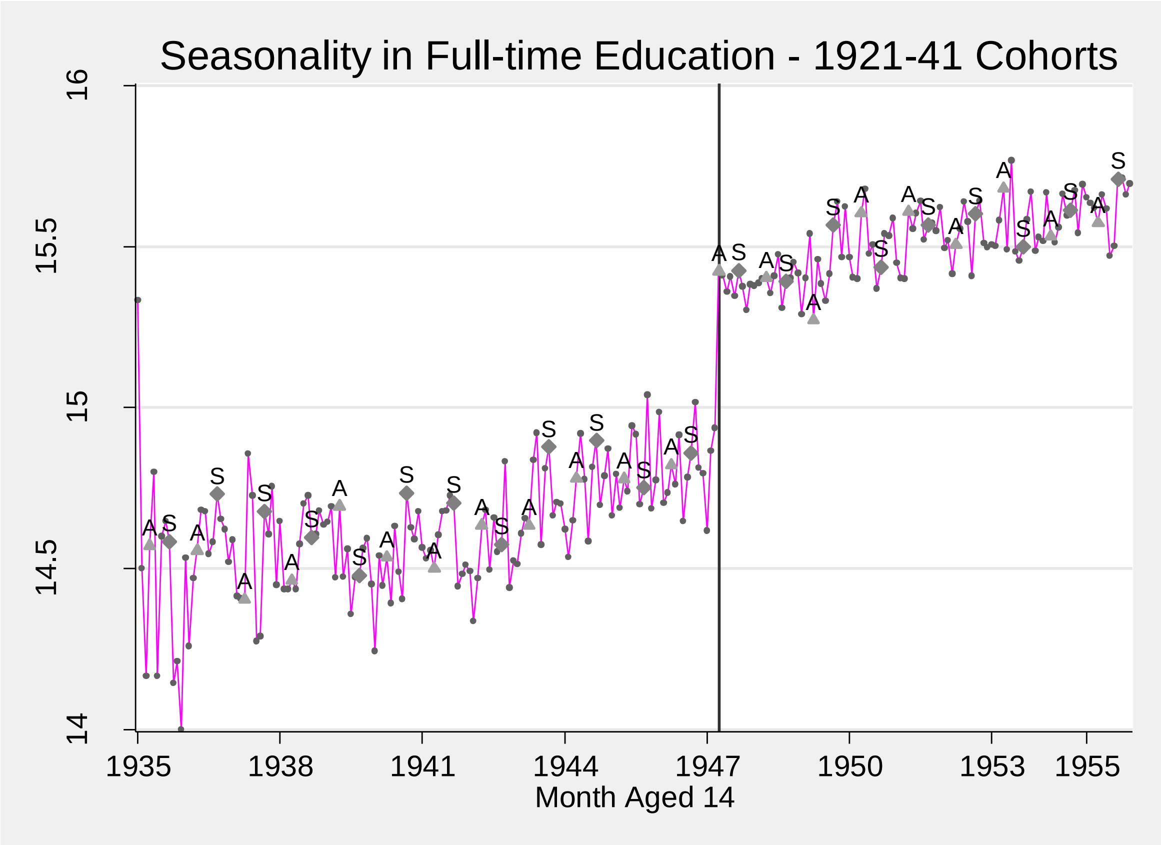

FIGURE C.29. SEASONALITY IN AGE LEFT FULL-TIME EDUCATION FOR COHORTS BORN IN 1921-41. POOLED SAMPLE

TABLE C.15 – T-TESTS FOR SIGNIFICANCE OF DIFFERENCES IN AGE LEFT FULL-TIME EDUCATION FOR COHORTS BORN IN 1921-41. POOLED SAMPLE

Month of Birth (vs all other months)	t	Pr(|T| > |t|)
January	-0.3198	0.7519
February	-0.899	0.3777
March	-0.7933	0.4356
April	0.0496	0.9609
May	0.5072	0.6166
June	0.4968	0.6239
July	-0.1299	0.8978
August	-0.2222	0.826
September	0.7313	0.4715
October	0.5376	0.596
November	0.0919	0.9275
December	0.0236	0.9814

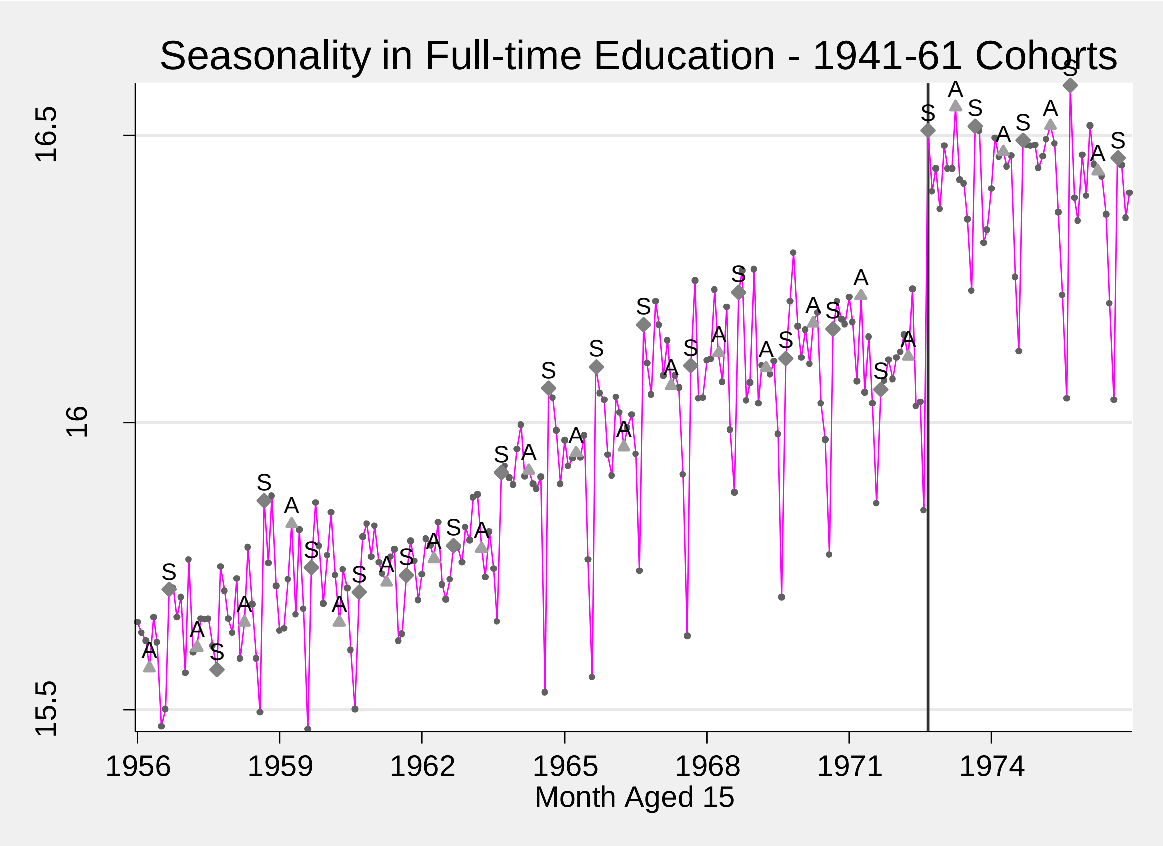

FIGURE C.30. SEASONALITY IN AGE LEFT FULL-TIME EDUCATION FOR COHORTS BORN IN 1941-61. POOLED SAMPLE

TABLE C.16 – T-TESTS FOR SIGNIFICANCE OF DIFFERENCES IN AGE LEFT FULL-TIME EDUCATION FOR COHORTS BORN IN 1941-61. POOLED SAMPLE

Month of Birth (vs all other months)	t	Pr(|T| > |t|)
January	0.2401	0.8124
February	0.6171	0.543
March	0.2574	0.7991
April	0.2355	0.8159
May	0.2525	0.8028
June	0.0087	0.9931
July	-2.0205	0.0541*
August	-5.3225	0.00***
September	1.2839	0.2119
October	1.6586	0.1101
November	0.9815	0.3358
December	0.5772	0.5692

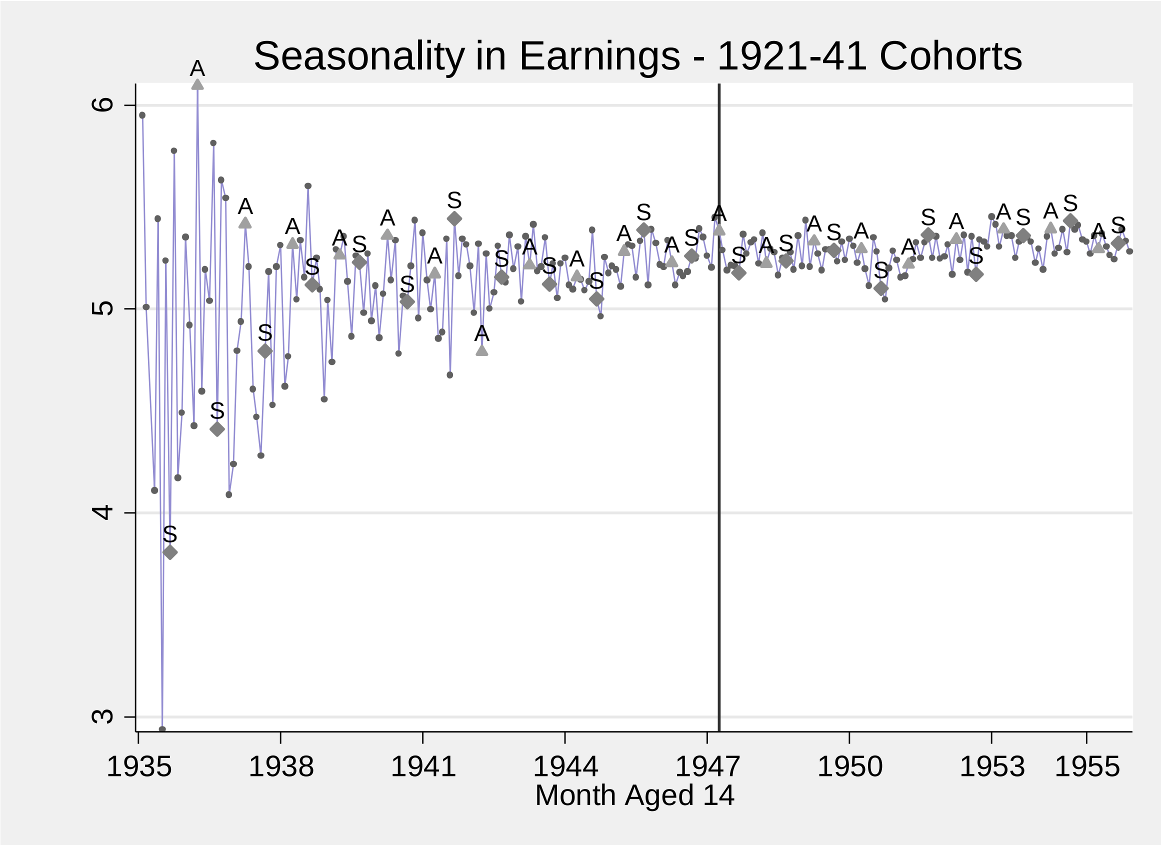

FIGURE C.31. SEASONALITY IN EARNINGS FOR COHORTS BORN IN 1921-41. POOLED SAMPLE

TABLE C.17 – T-TESTS FOR SIGNIFICANCE OF DIFFERENCES IN EARNINGS FOR COHORTS BORN IN 1921-41. POOLED SAMPLE

Month of Birth (vs all other months)	t	Pr(|T| > |t|)
January	0.7071	0.4862
February	-0.5311	0.6001
March	-0.3937	0.6969
April	2.5371	0.0176**
May	-0.4918	0.6273
June	0.4268	0.6724
July	-1.3745	0.1837
August	0.9792	0.3372
September	-0.9135	0.3707
October	1.9651	0.0583*
November	0.6131	0.5456
December	-0.9057	0.3744

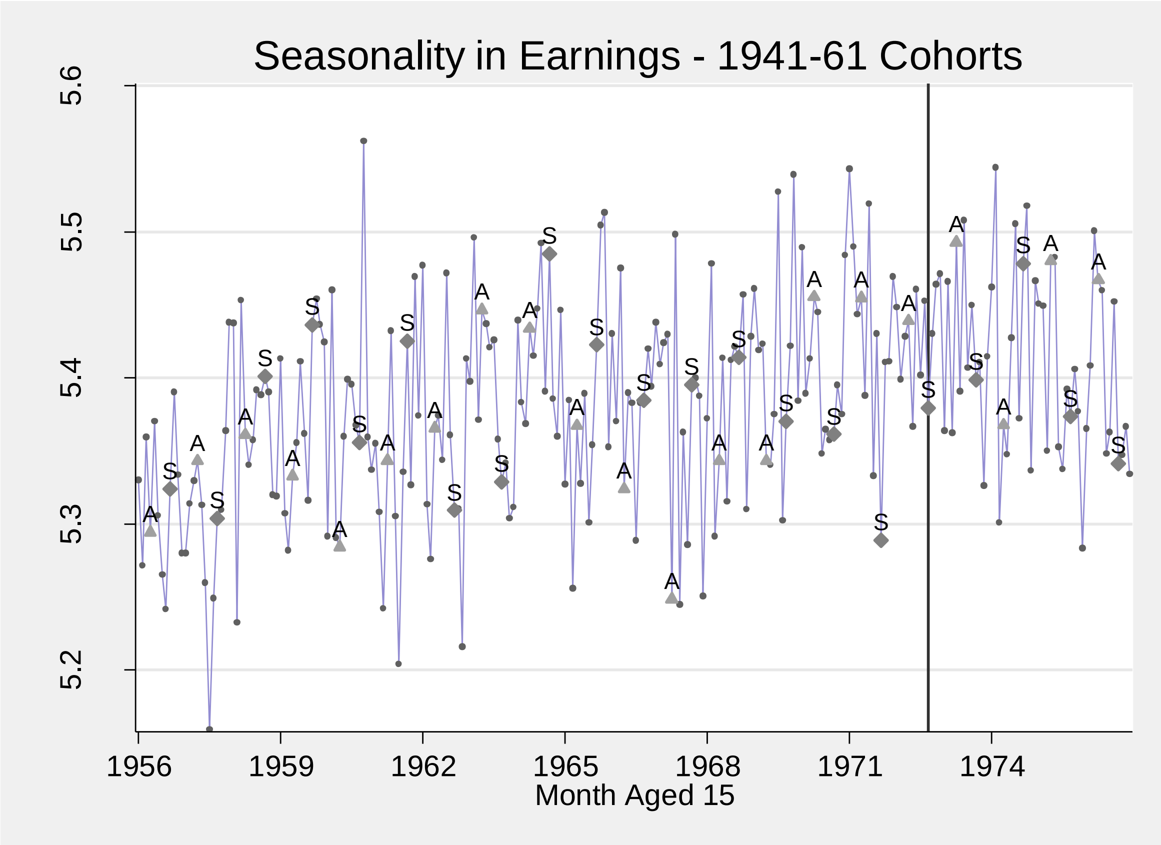

FIGURE C.32. SEASONALITY IN EARNINGS FOR COHORTS BORN IN 1941-61. POOLED SAMPLE

TABLE C.18 – T-TESTS FOR SIGNIFICANCE OF DIFFERENCES IN EARNINGS FOR COHORTS BORN IN 1941-61. POOLED SAMPLE

Month of Birth (vs all other months)	t	Pr(|T| > |t|)
January	1.6232	0.1175
February	0.6909	0.4966
March	-1.2456	0.2254
April	-0.2439	0.8094
May	0.7907	0.4358
June	-0.441	0.6632
July	-0.7215	0.4782
August	-1.4706	0.1538
September	-0.3507	0.7285
October	1.8291	0.0796*
November	-0.2991	0.7675
December	0.1888	0.8518

Appendix D. Replication Analysis – 2SLS Estimates and AIC

TABLE D.19. AIC CALCULATED FOR IV MODELS – MALE SAMPLE

Male Sample		Our Own Dataset			Devereux and Hart (2010)			Oreopoulos (2006)			Harmon and Walker (1995)		
Order of Polynomial		AIC			AIC			AIC			AIC		
1947 Reform	0	81157	81155	81169	68205	68205	68218	49002	48937	49048	47186	45112	45086
	1	93491	91446	91158	74844	74648	74611	52277	52204	52841	47282	45105	45078
	2	84048	83651	83419	71475	71288	71077	50322	50192	50310	48550	45467	45974
	3	83470	83372	83212	72183	72140	71946	50298	50267	50430	56935	46084	47466
	4	82010	82066	81893	69893	**69965**	**69716**	48540	48650	48510	46476	45423	46068
	5	82059	82170	81923	69973	70045	69712	48485	48590	48378	47058	46028	46256
	6	81054	81015	81005	68863	68824	68289	47701	47269	47387	46980	47380	46594
	7	81351	81430	81200	68273	68851	69194	47921	47873	47627	47506	48000	46652
	8	81351	81430	81200	68273	68851	69194	47921	47873	47627	117667	48997	106438
	Dumm	86551	80985	101403	70015	68296	78601	48090	66268	53165	47355	49979	52832
	Age controls	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm
1972 Reform	0	102893	102915	102972	75172	74876	74871	59763	59673	59702	412739	45109	57811
	1	448109	391364	393174	124159	129275	127657	59958	60408	60153	48767	45118	60517
	2	110674	102219	103810	72755	73181	73463	60057	59714	59814	259288	51038	45273
	3	102211	102755	105466	72926	72796	72761	59993	59808	59869	45825	53669	47521
	4	104103	103675	105945	73495	73684	73913	59702	59892	59946	45343	50732	45692
	5	102315	148989	103043	73493	74454	73458	60672	60708	61511	45222	51131	45421
	6	104637	137756	102742	73248	81664	87130	62041	77036	60099	48686	87245	46959
	7	104637	137756	102742	73248	81664	87130	62041	77036	60099	47572	46124	45848
	8	109964	182058	725643	72983	95713	119725	61708	61243	68278	48793	49991	45926
	Dumm	112727	102104	125609	74851	75197	81476	60593	61453	65099	47355	49980	52832
	Age controls	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm
1947 & 1972 Reforms	0	123166	123185	123218	93598	93615	93653	73113	73029	73126	47561	45112	45240
	1	128728	127682	127778	99390	98998	98973	78698	79037	79648	47605	45120	45311
	2	128588	127303	127258	102336	102142	102184	78037	78586	79297	48902	45277	46183
	3	130203	128882	128664	98976	98733	98516	75372	75481	75853	55185	45300	46550
	4	128286	127523	127399	97478	97557	97524	74249	74577	74951	46125	45035	45633
	5	126272	126066	126342	97162	97311	97374	74733	74806	74657	47132	45774	46211
	6	126388	126102	126337	96841	96929	97055	74004	73757	73471	47122	47270	46709
	7	126749	126204	126548	97576	96267	96910	75461	73781	71559	47321	48332	46601
	8	125380	160417	364018	96822	100727	111936	83127	72671	83682	51493	109655	47511
	Dumm	145300	140383	139251	96547	95859	94146	202240	230172	220674	64185	65148	65842
	Age controls	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm

Nb. Figures in bold are replicates of the models of these authors

TABLE D.20. AIC CALCULATED FOR IV MODELS – POOLED SAMPLE

Pooled Sample		Our Own Dataset			Devereux and Hart (2010)			Oreopoulos (2006)			Harmon and Walker (1995)		
Order of Polynomial		AIC			AIC			AIC			AIC		
1947 Reform	0	197346	197328	197318	171588	171474	171474	112044	112110	112164	147834	132668	135541
	1	225237	218080	217773	186225	185539	185609	120498	121739	122390	148151	132696	135787
	2	204671	202163	202346	178734	177375	177512	117183	116990	117310	149327	132791	136833
	3	200948	201277	201436	176321	176959	177109	116037	116602	116907	153472	133044	138744
	4	200397	200975	200712	176632	**176760**	**176354**	115137	**115236**	**115053**	132762	132460	134857
	5	200457	201103	200770	176958	177064	176514	114960	115066	114753	134377	133085	135420
	6	200460	200693	199564	175625	175456	174572	113024	111435	112803	134659	134756	136373
	7	199607	199494	199758	175981	175597	181083	112207	114867	111022	133352	139069	135605
	8	199607	199494	199758	175981	175597	181083	112207	114867	111022	132632	136702	135132
	Dum	196831	222352	214002	170995	179345	179062	111014	113559	113658	134502	230242	137576
	Age controls	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm
1972 Reform	0	264698	264491	264533	204437	203459	203589	156885	155523	155777	523890	161781	135442
	1	355936	347265	344009	196259	205606	204166	151833	156577	155728	137050	161993	134803
	2	559155	394114	364374	207442	210136	210237	151867	158160	157333	282856	156338	135517
	3	357421	302382	285438	200556	197331	197087	153232	155372	154938	147942	156841	137837
	4	286362	270392	265303	203055	197274	195680	154214	152855	152475	140992	147400	132770
	5	264980	260276	452367	202964	200338	197407	160049	156856	156090	132869	139010	132279
	6	263018	255584	259015	194906	199030	198760	154123	169137	157794	132948	135884	132447
	7	263018	255584	259015	194906	199030	198760	154123	169137	157794	132494	136561	132296
	8	264311	667114	291604	191426	205594	325906	714329	161934	200306	139122	132365	134034
	Dum	265192	284766	278894	191950	290518	196892	151688	330151	155186	134502	138387	137576
	Age controls	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm
1947 & 1972 Reforms	0	307440	307350	307354	239860	239531	239614	175136	175411	175391	147864	132550	135635
	1	359310	356767	355979	259096	261147	260986	182903	194386	195018	148149	132564	135931
	2	318046	314377	314991	250172	253898	253933	179475	191242	191608	149531	132593	136685
	3	317704	314280	314690	249938	249651	249657	184007	186998	187069	149594	132580	137271
	4	311866	312095	312281	247249	247590	247613	182984	185493	185699	132613	132560	134330
	5	307375	308709	309093	246062	246990	246978	185398	184507	184117	134834	133280	135585
	6	309782	309682	310464	246130	246961	246940	184894	184071	183646	135216	135241	136844
	7	313724	308592	310672	245364	246738	248554	175287	183743	183741	133774	189338	135395
	8	318013	306719	328800	247962	254165	250215	175287	183743	183741	133666	317864	135364
	Dum	322286	315943	317099	247051	249372	253488	175981	180771	179205	160074	159804	162244
	Age controls	Quadr	Quartic	Dum	Quadr	Quartic	Dum	Quadr	Quartic	Dum	Quadr	Quartic	Dum

Nb. Figures in bold are replicates of the models of these authors

TABLE D.21 - 2SLS EFFECTS OF ROSLA LAWS ON LOG WEEKLY EARNINGS – POOLED SAMPLE (GHS DATA)

	Order of YoB polynomial	GHS 1979-2006			GHS 1979-1998 (DH)			GHS 1983-1998 (OR)			GHS 1979-1986 (HW)		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1947 Reform	Zero	.184***	.190***	.190***	.183***	.186***	.187***	.101***	.099***	.097***	-.129***	.121***	.023
		(.009)	(.007)	(.006)	(.013)	(.013)	(.012)	(.016)	(.015)	(.016)	(.022)	(.021)	(.028)
	One	-.158***	-.114***	-.112***	-.082***	-.077**	-.077**	-.083**	-.102**	-.111**	-.132***	.118***	.018
		(.049)	(.040)	(.038)	(.026)	(.030)	(.029)	(.034)	(.039)	(.042)	(.022)	(.021)	(.028)
	Two	-.011	.013	.011	-.004	.011	.009	-.029	-.027	-.032	-.144***	.111***	-.001
		(.024)	(.014)	(.017)	(.019)	(.014)	(.017)	(.037)	(.034)	(.035)	(.023)	(.022)	(.029)
	Three	.029*	.023	.021	.027	.017	.015	-.008	-.019	-.025	-.184***	.096***	-.031
		(.016)	(.015)	(.017)	(.018)	(.018)	(.021)	(.031)	(.034)	(.034)	(.026)	(.022)	(.031)
	Four	.036	.027	.029	.023	**.019**	**.025**	.010	**.009**	**.012**	.117***	.145***	.035
		(.023)	(.022)	(.026)	(.023)	**(.023)**	**(.027)**	(.049)	**(.050)**	**(.048)**	(.023)	(.022)	(.031)
	Five	.035	.025	.029	.019	.015	.023	.014	.011	.018	.051**	.087***	.023
		(.024)	(.025)	(.027)	(.027)	(.028)	(.030)	(.048)	(.049)	(.048)	(.026)	(.025)	(.032)
	Six	.034	.030	.041	.038	.039	.052	.086	.085	.080	.044	.039	.003
		(.035)	(.033)	(.034)	(.035)	(.036)	(.035)	(.058)	(.058)	(.058)	(0.028)	(0.028)	(.034)
	Seven	.031	.024	.042	.031	.034	.093	.042	.062	.039	.056**	.058**	.015
		(.031)	(.033)	(.046)	(.032)	(.033)	(.061)	(.054)	(.060)	(.052)	(0.026)	(0.027)	(.033)
	Eight	.053	.046	.042	.033	.043	.042	.051	.047	.048	.112***	.074***	.000
		(.036)	(.036)	(.040)	(.032)	(.033)	(.031)	(.054)	(.053)	(.054)	(0.027)	(0.026)	(.000)
	Dum-mies	.254***	-.172	.244***	.331***	.317***	.318***	.252***	.083	.244***	0.000	1.874***	.585*
		(.006)	(.138)	(.006)	(.009)	(.007)	(.008)	(.010)	(.568)	(.015)	(0.000)	(0.473)	(0.334)
1972 Reform	Zero	.267***	.268***	.268***	.442***	.434***	.435***	.386***	.365***	.370***	8.866	.551***	.265**
		(.011)	(.009)	(.009)	(.041)	(.033)	(.033)	(.068)	(.049)	(.049)	(35.546)	(.089)	(.130)
	One	-.393	-.370	-.348	.037	-.070	-.056	.272	.018	.032	40.790	.553***	.251*
		(.395)	(.332)	(.299)	(.129)	(.139)	(.129)	(.237)	(.122)	(.118)	(795.291)	(.091)	(.137)
	Two	-1.421	-.579	-.427	-.084	-.108	-.110	.128	-.006	.006	1.666	.504***	.266***
		(4.376)	(1.522)	(1.032)	(.161)	(.195)	(.197)	(.112)	(.113)	(.108)	(1.092)	(.067)	(.088)
	Three	-.399	-.184	-.114	-.016	.020	.022	.086	.039	.047	.419***	.508***	.306***
		(.644)	(.415)	(.295)	(.082)	(.075)	(.073)	(.067)	(.081)	(.076)	(.056)	(.060)	(.076)
	Four	-.116	-.033	-.004	-.043	.019	.041	.064	.093	.103	.347***	.421***	.192***
		(.229)	(.180)	(.157)	(.129)	(.108)	(.100)	(.089)	(.096)	(.091)	(.051)	(.051)	(.062)
	Five	.040	.032	.034	-.017	-.012	.003	-.029	.004	.010	.103	.322***	.138
		(.086)	(.083)	(.082)	(.088)	(.085)	(.081)	(.080)	(.081)	(.079)	(.103)	(.096)	(.087)
	Six	-.087	.048	.051	.001	.003	.001	-.013	-.011	.016	.143	.248	.106
		(.263)	(.074)	(.070)	(.082)	(.077)	(.077)	(.073)	(.081)	(.075)	(.315)	(.263)	(.121)
	Seven	.016	.039	.041	.016	-.003	.025	-.019	.016	-.007	.126***	.124***	.033
		(.090)	(.076)	(.074)	(.073)	(.083)	(.079)	(.071)	(.078)	(.081)	(.038)	(.036)	(.044)
	Eight	.000	.000	.033	.000	.000	-.005	-.014	.005	-.009	.327***	-.064*	.046
		(.000)	(.000)	(.076)	(.000)	(.000)	(.093)	(.065)	(.072)	(.090)	(.042)	(.037)	(.039)
	Dum-mies	.268***	.276***	.276***	.525***	.502***	.131***	.446***	.430***	.106***	1.007	.000	-.201**
		(.007)	(.006)	(.006)	(.019)	(.011)	(.005)	(.029)	(.017)	(.007)	(1.033)	(.000)	(.083)
1947 & 1972 Reforms	Zero	.215***	.218***	.218***	.232***	.229***	.231***	.198***	.152***	.155***	-.129***	.136***	.021
		(.009)	(.007)	(.007)	(.024)	(.019)	(.019)	(.032)	(.023)	(.024)	(.023)	(.022)	(.029)
	One	-.189***	-.182***	-.179***	-.084***	-.100***	-.098***	-.006	-.130***	-.135***	-.132***	.132***	.015
		(.061)	(.063)	(.060)	(.024)	(.031)	(.029)	(.035)	(.038)	(.042)	(.023)	(.022)	(.029)
	Two	-.019	-.001	-.004	-.015	-.048	-.048*	.044	-.100**	-.104**	-.146***	.128***	.002
		(.023)	(.028)	(.027)	(.020)	(.029)	(.028)	(.032)	(.041)	(.044)	(.024)	(.022)	(.030)
	Three	-.018	-.001	-.003	-.013	-.013	-.013	-.020	-.057*	-.057	-.147***	.127***	-.009
		(.027)	(.019)	(.021)	(.017)	(.017)	(.019)	(.031)	(.034)	(.035)	(.025)	(.023)	(.031)
	Four	.018	.013	.012	.012	.006	.006	-.007	-.040	-.042	.133***	.167***	.047
		(.021)	(.019)	(.020)	(.019)	(.019)	(.020)	(.031)	(.032)	(.033)	(.023)	(.023)	(.032)
	Five	.054*	.039	.036	.024	.012	.012	-.037	-.029	-.023	.040	.080***	.020
		(.030)	(.028)	(.028)	(.028)	(.028)	(.031)	(.051)	(.050)	(.049)	(.028)	(.027)	(.034)
	Six	.035	.031	.025	.024	.012	.013	-.030	-.025	-.014	.032	.028	-.002
		(.026)	(.025)	(.026)	(.028)	(.027)	(.030)	(.054)	(.052)	(.051)	(.030)	(.030)	(.037)
	Seven	.019	.030	.012	.042	.000	.015	-.020	-.027	-.010	.058**	.060**	.024
		(.040)	(.025)	(.033)	(.060)	(.000)	(.027)	(.074)	(.054)	(.051)	(.029)	(.028)	(.033)
	Eight	.037	.010	.183**	.000	.010	.000	-.088	-.152	-.052	.079***	.408***	.018
		(.025)	(.022)	(.081)	(.000)	(.019)	(.000)	(.069)	(.221)	(.115)	(.026)	(.038)	(.032)
	Dum-mies	-.042	-.010	-.017	.014	-.038	-.045	.119**	.115	.045	-.248	-.247	-.268
		(.048)	(.048)	(.049)	(.050)	(.023)	(.046)	(.046)	(.081)	(.109)	(.316)	(.314)	(.318)
	Age Controls	Quadr	Quartic	Dum	Quadr	Quartic	Dum	Quadr	Quartic	Dum	Quadr	Quartic	Dum

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE D.22. LOCAL AVERAGE RD EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS – POOLED SAMPLE

		(Our Own)	(D & H)	(Oreop)	(H & W)
1947 Reform	Red. form – optimal bandw.	-0.011 (0.013)	-0.011 (0.013)	0.035* (0.018)	-0.071** (0.030)
	1st stage – optimal bandw.	0.321*** (0.079)	0.322*** (0.085)	0.276*** (0.071)	0.320*** (0.090)
	Wald – optimal bandw.	**-0.033** **(0.032)**	**-0.033** **(0.033)**	**0.128***** **(0.047)**	**-0.223***** **(0.065)**
	Red. form – 0.5 * optimal bandw.	-0.010 (0.019)	-0.010 (0.019)	0.010 (0.020)	0.000 (0.000)
	1st stage – 0.5 * optimal bandw.	0.402*** (0.078)	0.402*** (0.079)	0.353*** (0.069)	0.000 (0.000)
	Wald – 0.5 * optimal bandw.	**-0.024** **(0.036)**	**-0.024** **(0.036)**	**0.028** **(0.035)**	**0.000** **(0.000)**
	Red. form – 2 * optimal bandw.	-0.014 (0.018)	-0.014 (0.017)	0.005 (0.023)	-0.053 (0.034)
	1st stage – 2 * optimal bandw.	0.357*** (0.053)	0.360*** (0.056)	0.304*** (0.049)	0.375*** (0.073)
	Wald – 2 * optimal bandw.	**-0.039** **(0.048)**	**-0.038** **(0.046)**	**0.015** **(0.067)**	**-0.140** **(0.095)**
1972 Reform	Red. form – optimal bandw.	0.028*** (0.005)	0.040* (0.020)	0.042** (0.020)	-0.012 (0.019)
	1st stage – optimal bandw.	0.124*** (0.011)	0.151** (0.069)	0.154** (0.068)	0.142* (0.083)
	Wald – optimal bandw.	**0.223***** **(0.015)**	**0.262**** **(0.133)**	**0.273**** **(0.133)**	**-0.085** **(0.082)**
	Red. form – 0.5 * optimal bandw.	0.000 (0.000)	0.030*** (0.007)	0.030*** (0.007)	-0.017*** (0.003)
	1st stage – 0.5 * optimal bandw.	0.000 (0.000)	0.125 (0.085)	0.125 (0.084)	0.114 (0.076)
	Wald – 0.5 * optimal bandw.	**0.000** **(0.000)**	**0.242***** **(0.093)**	**0.242***** **(0.092)**	**-0.147***** **(0.029)**
	Red. form – 2 * optimal bandw.	0.014** (0.006)	0.021 (0.014)	0.024** (0.011)	-0.004 (0.020)
	1st stage – 2 * optimal bandw.	0.099*** (0.018)	0.164** (0.078)	0.171** (0.077)	0.143 (0.088)
	Wald – 2 * optimal bandw.	**0.144**** **(0.064)**	**0.125** **(0.083)**	**0.143*** **(0.077)**	**-0.031** **(0.118)**

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

TABLE D.23. LOCAL AVERAGE RD EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS – MALE SAMPLE

		(Our Own)	(D & H)	(Oreop)	(H & W)
1947 Reform	Red. form – optimal bandw.	0.022	0.022	0.055	-0.013
		(0.036)	(0.036)	(0.045)	(0.026)
	1st stage – optimal bandw.	0.328***	0.328***	0.271**	0.404***
		(0.085)	(0.083)	(0.106)	(0.067)
	Wald – optimal bandw.	**0.066**	**0.066**	**0.202**	**-0.033**
		(0.128)	**(0.118)**	**(3.804)**	**(0.081)**
	Red. form – 0.5 * optimal bandw.	0.019	0.019	0.000	0.000
		(0.021)	(0.021)	(0.000)	(0.000)
	1st stage – 0.5 * optimal bandw.	0.390***	0.390***	0.000	0.000
		(0.048)	(0.046)	(0.000)	(0.000)
	Wald – 0.5 * optimal bandw.	**0.050**	**0.050**	**0.000**	**0.000**
		(0.055)	**(0.053)**	**(0.000)**	**(0.000)**
	Red. form – 2 * optimal bandw.	0.023	0.023	0.038	-0.022
		(0.025)	(0.024)	(0.045)	(0.031)
	1st stage – 2 * optimal bandw.	0.341***	0.343***	0.300***	0.384***
		(0.055)	(0.054)	(0.086)	(0.088)
	Wald – 2 * optimal bandw.	**0.068**	**0.066**	**0.128**	**-0.058**
		(0.073)	**(0.070)**	**(0.468)**	**(0.102)**
1972 Reform	Red. form – optimal bandw.	0.066**	0.099***	0.111***	0.024
		(0.031)	(0.034)	(0.034)	(0.031)
	1st stage – optimal bandw.	0.201**	0.216***	0.218***	0.161*
		(0.089)	(0.075)	(0.070)	(0.086)
	Wald – optimal bandw.	**0.326**	**0.459**	**0.508**	**0.151**
		(6.549)	**(1.954)**	**(0.328)**	**(1.484)**
	Red. form – 0.5 * optimal bandw.	0.068***	0.079***	0.079***	-0.017
		(0.018)	(0.020)	(0.019)	(0.018)
	1st stage – 0.5 * optimal bandw.	0.203***	0.176***	0.176***	0.135***
		(0.054)	(0.042)	(0.041)	(0.050)
	Wald – 0.5 * optimal bandw.	**0.335****	**0.446*****	**0.446*****	**-0.122**
		(0.142)	**(0.146)**	**(0.144)**	**(0.269)**
	Red. form – 2 * optimal bandw.	0.076***	0.083***	0.095***	0.023
		(0.021)	(0.023)	(0.023)	(0.021)
	1st stage – 2 * optimal bandw.	0.209***	0.214***	0.224***	0.173***
		(0.064)	(0.050)	(0.048)	(0.058)
	Wald – 2 * optimal bandw.	**0.362**	**0.387*****	**0.425*****	**0.134**
		(0.223)	**(0.126)**	**(0.123)**	**(0.328)**

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

TABLE D.24. LOCAL AVERAGE RD EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS – POOLED SAMPLE

		(Our Own)	(D & H)	(Oreop)	(H & W)
1947 Reform	Red. form – optimal bandw.	-0.011	-0.011	0.035	-0.071
		(0.040)	(0.042)	(0.053)	(0.050)
	1st stage – optimal bandw.	0.321***	0.322***	0.276***	0.320***
		(0.067)	(0.065)	(0.082)	(0.074)
	Wald – optimal bandw.	**-0.033**	**-0.033**	**0.128**	**-0.223**
		(0.136)	**(0.140)**	**(0.376)**	**(0.198)**
	Red. form – 0.5 * optimal bandw.	-0.010	-0.010	0.010	0.000
		(0.024)	(0.025)	(0.032)	(0.000)
	1st stage – 0.5 * optimal bandw.	0.402***	0.402***	0.353***	0.000
		(0.037)	(0.036)	(0.046)	(0.000)
	Wald – 0.5 * optimal bandw.	**-0.024**	**-0.024**	**0.028**	**0.000**
		(0.062)	**(0.062)**	**(0.095)**	**(0.000)**
	Red. form – 2 * optimal bandw.	-0.014	-0.014	0.005	-0.053
		(0.028)	(0.028)	(0.038)	(0.043)
	1st stage – 2 * optimal bandw.	0.357***	0.360***	0.304***	0.375***
		(0.043)	(0.042)	(0.055)	(0.049)
	Wald – 2 * optimal bandw.	**-0.039**	**-0.038**	**0.015**	**-0.140**
		(0.080)	**(0.080)**	**(0.135)**	**(0.135)**
1972 Reform	Red. form – optimal bandw.	0.028	0.040	0.042	-0.012
		(0.020)	(0.041)	(0.040)	(0.038)
	1st stage – optimal bandw.	0.124***	0.151***	0.154***	0.142**
		(0.040)	(0.052)	(0.054)	(0.068)
	Wald – optimal bandw.	**0.223**	**0.262**	**0.273**	**-0.085**
		(0.640)	**(0.357)**	**(0.350)**	**(4.366)**
	Red. form – 0.5 * optimal bandw.	0.000	0.030	0.030	-0.017
		(0.000)	(0.024)	(0.024)	(0.022)
	1st stage – 0.5 * optimal bandw.	0.000	0.125***	0.125***	0.114***
		(0.000)	(0.031)	(0.032)	(0.038)
	Wald – 0.5 * optimal bandw.	**0.000**	**0.242**	**0.242**	**-0.147**
		(0.000)	**(0.210)**	**(0.218)**	**(0.449)**
	Red. form – 2 * optimal bandw.	0.014	0.021	0.024	-0.004
		(0.028)	(0.028)	(0.027)	(0.027)
	1st stage – 2 * optimal bandw.	0.099*	0.164***	0.171***	0.143***
		(0.054)	(0.036)	(0.036)	(0.046)
	Wald – 2 * optimal bandw.	**0.144**	**0.125**	**0.143**	**-0.031**
		(5.238)	**(0.171)**	**(0.162)**	**(0.264)**

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

TABLE D.25 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG HOURLY EARNINGS – POOLED SAMPLE (FES DATA)

	1st Stage: Schooling			Reduced Form: Hourly Earnings			2SLS: Hourly Earnings		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Row 1	0.487***	0.738***	0.672***	-0.030***	0.061***	0.040***	0.089***	0.084***	0.065***
H&W	(0.040)	(0.046)	(0.052)	(0.009)	(0.010)	(0.011)	(0.012)	(0.013)	(0.016)
1995	0.088	0.728***	0.682***	-0.140***	0.049***	0.010			
	(0.055)	(0.063)	(0.071)	(0.012)	(0.014)	(0.015)			
Row 2	0.487***	0.738***	0.672***	-0.030	0.061***	0.040**	0.089**	0.084***	0.065***
Clustered	(0.096)	(0.115)	(0.115)	(0.021)	(0.017)	(0.018)	(0.034)	(0.016)	(0.019)
Stnd Errs	0.088	0.728***	0.682***	-0.140***	0.049*	0.010			
	(0.182)	(0.143)	(0.146)	(0.044)	(0.025)	(0.024)			
Row 3	0.696***	0.762***	0.680***	0.030	0.051**	0.027	0.044*	0.069***	0.040
Quartic	(0.156)	(0.153)	(0.145)	(0.022)	(0.021)	(0.022)	(0.024)	(0.016)	(0.025)
of Year	0.574***	0.753***	0.709***	0.015	0.069***	0.029			
of Birth	(0.159)	(0.153)	(0.154)	(0.025)	(0.022)	(0.023)			
Row 4	0.691***	0.762***	0.668***	0.032	0.053**	0.026	0.046*	0.075***	0.042*
Regional	(0.160)	(0.158)	(0.150)	(0.022)	(0.021)	(0.022)	(0.023)	(0.016)	(0.024)
& Year	0.602***	0.792***	0.742***	0.022	0.079***	0.036			
Dummies	(0.164)	(0.157)	(0.155)	(0.025)	(0.022)	(0.023)			
Omitted									
Age Controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix E. Replication Analysis – 1st Stage Estimates and 2SLS Estimates

TABLE E.26 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS. POOLED SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	1.325***	1.347***	1.351***	0.244***	0.256***	0.257***	0.184***	0.190***	0.190***
	N=96,549	(0.109)	(0.112)	(0.113)	(0.030)	(0.028)	(0.028)	(0.009)	(0.007)	(0.006)
	79-00 GHS	0.979***	0.973***	0.975***	0.179***	0.181***	0.183***	0.183***	0.186***	0.187***
	N=85,766	(0.049)	(0.047)	(0.045)	(0.019)	(0.017)	(0.017)	(0.013)	(0.013)	(0.012)
	84-98 GHS	0.815***	0.810***	0.814***	0.083***	0.080***	0.079***	0.101***	0.099***	0.097***
	N=53,502	(0.054)	(0.053)	(0.053)	(0.014)	(0.012)	(0.013)	(0.016)	(0.015)	(0.016)
	79-86 GHS	0.572***	0.636***	0.572***	-0.073***	0.077***	0.013	-0.129***	0.121***	0.023
	N=71,639	(0.020)	(0.024)	(0.027)	(0.012)	(0.014)	(0.016)	(0.022)	(0.021)	(0.028)
1972 Reform	79-06 GHS	1.035***	1.039***	1.044***	0.277***	0.278***	0.280***	0.267***	0.268***	0.268***
	N=121,614	(0.083)	(0.083)	(0.079)	(0.028)	(0.027)	(0.026)	(0.011)	(0.009)	(0.009)
	79-00 GHS	0.517***	0.517***	0.518***	0.228***	0.225***	0.226***	0.442***	0.434***	0.435***
	N=92,730	(0.045)	(0.045)	(0.044)	(0.031)	(0.027)	(0.027)	(0.041)	(0.033)	(0.033)
	84-98 GHS	0.377***	0.384***	0.384***	0.146***	0.140***	0.142***	0.386***	0.365***	0.370***
	N=71,943	(0.034)	(0.031)	(0.029)	(0.024)	(0.020)	(0.020)	(0.068)	(0.049)	(0.049)
	79-86 GHS	0.005	0.184***	0.113***	0.043***	0.101***	0.030**	8.866	0.551***	0.265**
	N=71,639	(0.020)	(0.023)	(0.025)	(0.012)	(0.014)	(0.015)	(35.546)	(0.089)	(0.130)
1947 & 1972 Reform	79-06 GHS	1.427***	1.422***	1.424***	0.266***	0.269***	0.270***	0.215***	0.218***	0.218***
	N=145,928	(0.108)	(0.111)	(0.111)	(0.030)	(0.028)	(0.028)	(0.009)	(0.007)	(0.007)
		2.311***	2.307***	2.308***	0.513***	0.517***	0.517***			
		(0.134)	(0.140)	(0.141)	(0.039)	(0.038)	(0.037)			
	79-00 GHS	0.927***	0.926***	0.930***	0.167***	0.166***	0.169***	0.232***	0.229***	0.231***
	N=117,044	(0.054)	(0.056)	(0.055)	(0.022)	(0.018)	(0.018)	(0.024)	(0.019)	(0.019)
		1.374***	1.385***	1.394***	0.376***	0.366***	0.372***			
		(0.080)	(0.078)	(0.075)	(0.045)	(0.037)	(0.036)			
	84-98 GHS	0.731***	0.763***	0.763***	0.093***	0.066***	0.067***	0.198***	0.152***	0.155***
	N=83,236	(0.059)	(0.064)	(0.063)	(0.022)	(0.015)	(0.013)	(0.032)	(0.023)	(0.024)
		1.033***	1.080***	1.083***	0.222***	0.183***	0.187***			
		(0.072)	(0.071)	(0.070)	(0.037)	(0.027)	(0.026)			
	79-86 GHS	0.532***	0.582***	0.510***	-0.068***	0.070***	0.009	-0.129***	0.136***	0.021
	N=71,639	(0.019)	(0.022)	(0.025)	(0.011)	(0.013)	(0.015)	(0.023)	(0.022)	(0.029)
		0.497***	0.684***	0.586***	-0.059***	0.139***	0.030			
		(0.027)	(0.031)	(0.035)	(0.016)	(0.019)	(0.021)			
	Age Controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies
	Degree YoB Polynomial	Zero	Zero	Zero	Zero	Zero	Zero	Zero	Zero	Zero

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

TABLE E.27 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

POOLED SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.461***	0.487***	0.493***	-0.073***	-0.056***	-0.055***	-0.158***	-0.114***	-0.112***
	N=96,549	(0.037)	(0.034)	(0.033)	(0.019)	(0.018)	(0.018)	(0.049)	(0.040)	(0.038)
	79-00 GHS	0.559***	0.559***	0.560***	-0.046***	-0.043**	-0.043**	-0.082***	-0.077**	-0.077**
	N=85,766	(0.027)	(0.027)	(0.027)	(0.015)	(0.017)	(0.016)	(0.026)	(0.030)	(0.029)
	84-98 GHS	0.532***	0.518***	0.518***	-0.044**	-0.053**	-0.058**	-0.083**	-0.102**	-0.111**
	N=53,502	(0.038)	(0.037)	(0.039)	(0.018)	(0.021)	(0.022)	(0.034)	(0.039)	(0.042)
	79-86 GHS	0.570***	0.634***	0.569***	-0.075***	0.075***	0.010	-0.132***	0.118***	0.018
	N=71,639	(0.020)	(0.024)	(0.027)	(0.012)	(0.014)	(0.016)	(0.022)	(0.021)	(0.028)
1972 Reform	79-06 GHS	0.063	0.062*	0.065*	-0.025*	-0.023**	-0.022**	-0.393	-0.370	-0.348
	N=121,614	(0.038)	(0.035)	(0.033)	(0.012)	(0.011)	(0.011)	(0.395)	(0.332)	(0.299)
	79-00 GHS	0.091**	0.091**	0.092**	0.003	-0.006	-0.005	0.037	-0.070	-0.056
	N=92,730	(0.042)	(0.042)	(0.042)	(0.012)	(0.011)	(0.011)	(0.129)	(0.139)	(0.129)
	84-98 GHS	0.088*	0.090*	0.088*	0.024	0.002	0.003	0.272	0.018	0.032
	N=71,943	(0.045)	(0.045)	(0.045)	(0.015)	(0.011)	(0.011)	(0.237)	(0.122)	(0.118)
	79-86 GHS	0.001	0.179***	0.107***	0.042***	0.099***	0.027*	40.790	0.553***	0.251*
	N=71,639	(0.020)	(0.023)	(0.025)	(0.012)	(0.014)	(0.015)	(795.291)	(0.091)	(0.137)
1947 & 1972 Reforms	79-06 GHS	0.282***	0.280***	0.281***	-0.067***	-0.066***	-0.066***	-0.189***	-0.182***	-0.179***
	N=145,928	(0.043)	(0.044)	(0.044)	(0.014)	(0.015)	(0.014)	(0.061)	(0.063)	(0.060)
		0.248***	0.245***	0.245***	-0.088***	-0.088***	-0.089***			
		(0.073)	(0.073)	(0.074)	(0.020)	(0.020)	(0.020)			
	79-00 GHS	0.495***	0.494***	0.495***	-0.037***	-0.049***	-0.048***	-0.084***	-0.100***	-0.098***
	N=117,044	(0.035)	(0.035)	(0.035)	(0.011)	(0.015)	(0.014)	(0.024)	(0.031)	(0.029)
		0.546***	0.544***	0.548***	-0.016	-0.055**	-0.052**			
		(0.057)	(0.057)	(0.058)	(0.018)	(0.021)	(0.021)			
	84-98 GHS	0.478***	0.469***	0.463***	-0.004	-0.061***	-0.062***	-0.006	-0.130***	-0.135***
	N=83,236	(0.043)	(0.045)	(0.046)	(0.016)	(0.018)	(0.020)	(0.035)	(0.038)	(0.042)
		0.529***	0.513***	0.504***	0.030	-0.063**	-0.062**			
		(0.060)	(0.063)	(0.066)	(0.020)	(0.024)	(0.025)			
	79-86 GHS	0.530***	0.580***	0.507***	-0.070***	0.069***	0.006	-0.132***	0.132***	0.015
	N=71,639	(0.019)	(0.022)	(0.026)	(0.011)	(0.013)	(0.015)	(0.023)	(0.022)	(0.029)
		0.494***	0.680***	0.581***	-0.062***	0.136***	0.025			
		(0.027)	(0.031)	(0.035)	(0.016)	(0.019)	(0.021)			
	Age Controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies
	Degree YoB Polynomial	One	One	One	One	One	One	One	One	One

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

TABLE E.28 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

POOLED SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.555***	0.573***	0.584***	-0.006	0.007	0.006	-0.011	0.013	0.011
	N=96,549	(0.042)	(0.042)	(0.046)	(0.013)	(0.008)	(0.010)	(0.024)	(0.014)	(0.017)
	79-00 GHS	0.568***	0.569***	0.572***	-0.002	0.006	0.005	-0.004	0.011	0.009
	N=85,766	(0.037)	(0.037)	(0.037)	(0.011)	(0.008)	(0.010)	(0.019)	(0.014)	(0.017)
	84-98 GHS	0.539***	0.531***	0.531***	-0.016	-0.014	-0.017	-0.029	-0.027	-0.032
	N=53,502	(0.054)	(0.053)	(0.053)	(0.020)	(0.018)	(0.019)	(0.037)	(0.034)	(0.035)
	79-86 GHS	0.577***	0.645***	0.578***	-0.083***	0.072***	-0.000	-0.144***	0.111***	-0.001
	N=71,639	(0.020)	(0.024)	(0.028)	(0.012)	(0.014)	(0.017)	(0.023)	(0.022)	(0.029)
1972 Reform	79-06 GHS	0.022	0.029	0.035	-0.032	-0.017	-0.015	-1.421	-0.579	-0.427
	N=121,614	(0.057)	(0.055)	(0.053)	(0.020)	(0.016)	(0.016)	(4.376)	(1.522)	(1.032)
	79-00 GHS	0.118*	0.118*	0.116*	-0.010	-0.013	-0.013	-0.084	-0.108	-0.110
	N=92,730	(0.067)	(0.067)	(0.067)	(0.015)	(0.017)	(0.017)	(0.161)	(0.195)	(0.197)
	84-98 GHS	0.140**	0.141**	0.139**	0.018	-0.001	0.001	0.128	-0.006	0.006
	N=71,943	(0.061)	(0.060)	(0.060)	(0.014)	(0.016)	(0.015)	(0.112)	(0.113)	(0.108)
	79-86 GHS	0.031	0.252***	0.182***	0.052***	0.127***	0.048***	1.666	0.504***	0.266***
	N=71,639	(0.021)	(0.025)	(0.028)	(0.012)	(0.015)	(0.016)	(1.092)	(0.067)	(0.088)
1947 & 1972 Reforms	79-06 GHS	0.493***	0.495***	0.500***	-0.026**	-0.019	-0.020	-0.019	-0.001	-0.004
	N=145,928	(0.053)	(0.054)	(0.057)	(0.012)	(0.013)	(0.013)	(0.023)	(0.028)	(0.027)
		0.305***	0.304***	0.307***	-0.077***	-0.075***	-0.076***			
		(0.060)	(0.059)	(0.060)	(0.018)	(0.018)	(0.018)			
	79-00 GHS	0.494***	0.493***	0.494***	-0.005	-0.011	-0.013	-0.015	-0.048	-0.048*
	N=117,044	(0.052)	(0.052)	(0.053)	(0.009)	(0.012)	(0.013)	(0.020)	(0.029)	(0.028)
		0.546***	0.544***	0.547***	-0.013	-0.052***	-0.049**			
		(0.058)	(0.058)	(0.059)	(0.016)	(0.019)	(0.019)			
	84-98 GHS	0.454***	0.447***	0.439***	0.008	-0.039**	-0.040**	0.044	-0.100**	-0.104**
	N=83,236	(0.059)	(0.058)	(0.059)	(0.016)	(0.018)	(0.019)	(0.032)	(0.041)	(0.044)
		0.523***	0.509***	0.500***	0.033*	-0.059**	-0.058**			
		(0.059)	(0.061)	(0.064)	(0.019)	(0.022)	(0.023)			
	79-86 GHS	0.539***	0.579***	0.503***	-0.078***	0.059***	-0.006	-0.146***	0.128***	0.002
	N=71,639	(0.020)	(0.023)	(0.026)	(0.012)	(0.014)	(0.016)	(0.024)	(0.022)	(0.030)
		0.491***	0.681***	0.582***	-0.060***	0.141***	0.029			
		(0.027)	(0.032)	(0.035)	(0.016)	(0.019)	(0.021)			
	Age Controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies
	Degree YoB Polynomial	Two	Two	Two	Two	Two	Two	Two	Two	Two

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

TABLE E.29 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

POOLED SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.615***	0.623***	0.629***	0.018*	0.014	0.013	0.029*	0.023	0.021
	N=96,549	(0.041)	(0.045)	(0.047)	(0.010)	(0.009)	(0.011)	(0.016)	(0.015)	(0.017)
	79-00 GHS	0.599***	0.599***	0.602***	0.016	0.010	0.009	0.027	0.017	0.015
	N=85,766	(0.040)	(0.040)	(0.040)	(0.011)	(0.011)	(0.012)	(0.018)	(0.018)	(0.021)
	84-98 GHS	0.563***	0.551***	0.551***	-0.005	-0.011	-0.014	-0.008	-0.019	-0.025
	N=53,502	(0.048)	(0.048)	(0.047)	(0.018)	(0.019)	(0.019)	(0.031)	(0.034)	(0.034)
	79-86 GHS	0.535***	0.633***	0.559***	-0.098***	0.061***	-0.017	-0.184***	0.096***	-0.031
	N=71,639	(0.021)	(0.024)	(0.029)	(0.012)	(0.014)	(0.017)	(0.026)	(0.022)	(0.031)
1972 Reform	79-06 GHS	0.051	0.054	0.064	-0.020	-0.010	-0.007	-0.399	-0.184	-0.114
	N=121,614	(0.049)	(0.050)	(0.046)	(0.016)	(0.016)	(0.015)	(0.644)	(0.415)	(0.295)
	79-00 GHS	0.176***	0.178***	0.176***	-0.003	0.004	0.004	-0.016	0.020	0.022
	N=92,730	(0.038)	(0.038)	(0.038)	(0.014)	(0.014)	(0.013)	(0.082)	(0.075)	(0.073)
	84-98 GHS	0.179***	0.177***	0.173***	0.015	0.007	0.008	0.086	0.039	0.047
	N=71,943	(0.038)	(0.039)	(0.040)	(0.013)	(0.015)	(0.014)	(0.067)	(0.081)	(0.076)
	79-86 GHS	0.279***	0.283***	0.219***	0.117***	0.144***	0.067***	0.419***	0.508***	0.306***
	N=71,639	(0.024)	(0.025)	(0.028)	(0.014)	(0.015)	(0.016)	(0.056)	(0.060)	(0.076)
1947 & 1972 Reforms	79-06 GHS	0.496***	0.501***	0.507***	-0.018	-0.006	-0.007	-0.018	-0.001	-0.003
	N=145,928	(0.052)	(0.053)	(0.056)	(0.012)	(0.008)	(0.010)	(0.027)	(0.019)	(0.021)
		0.315***	0.327***	0.333***	-0.045**	-0.027*	-0.028*			
		(0.073)	(0.076)	(0.077)	(0.018)	(0.015)	(0.016)			
	79-00 GHS	0.502***	0.502***	0.504***	-0.005	-0.005	-0.006	-0.013	-0.013	-0.013
	N=117,044	(0.055)	(0.055)	(0.056)	(0.009)	(0.008)	(0.010)	(0.017)	(0.017)	(0.019)
		0.597***	0.597***	0.602***	-0.013	-0.012	-0.010			
		(0.063)	(0.063)	(0.064)	(0.018)	(0.017)	(0.018)			
	84-98 GHS	0.481***	0.478***	0.470***	-0.011	-0.027	-0.028	-0.020	-0.057*	-0.057
	N=83,236	(0.069)	(0.069)	(0.070)	(0.016)	(0.016)	(0.017)	(0.031)	(0.034)	(0.035)
		0.580***	0.577***	0.566***	-0.009	-0.033	-0.031			
		(0.078)	(0.077)	(0.079)	(0.022)	(0.022)	(0.022)			
	79-86 GHS	0.489***	0.565***	0.484***	-0.097***	0.047***	-0.020	-0.147***	0.127***	-0.009
	N=71,639	(0.020)	(0.023)	(0.027)	(0.012)	(0.014)	(0.016)	(0.025)	(0.023)	(0.031)
		0.589***	0.690***	0.590***	-0.021	0.148***	0.034			
		(0.028)	(0.032)	(0.036)	(0.017)	(0.019)	(0.021)			
	Age Controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies
	Degree YoB Polynomial	Three	Three	Three	Three	Three	Three	Three	Three	Three

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

TABLE E.30 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

POOLED SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.520***	0.512***	0.512***	0.019	0.014	0.015	0.036	0.027	0.029
	N=96,549	(0.030)	(0.028)	(0.031)	(0.012)	(0.012)	(0.013)	(0.023)	(0.022)	(0.026)
	79-00 GHS	0.506***	0.506***	0.506***	0.012	0.010	0.013	0.023	0.019	0.025
	N=85,766	(0.030)	(0.030)	(0.029)	(0.012)	(0.012)	(0.014)	(0.023)	(0.023)	(0.027)
	84-98 GHS	0.451***	0.452***	0.456***	0.005	0.004	0.005	0.010	0.009	0.012
	N=53,502	(0.037)	(0.037)	(0.039)	(0.022)	(0.023)	(0.022)	(0.049)	(0.050)	(0.048)
	79-86 GHS	0.649***	0.664***	0.591***	0.076***	0.096***	0.021	0.117***	0.145***	0.035
	N=71,639	(0.026)	(0.026)	(0.031)	(0.015)	(0.015)	(0.018)	(0.023)	(0.022)	(0.031)
1972 Reform	79-06 GHS	0.085**	0.090**	0.099**	-0.010	-0.003	-0.000	-0.116	-0.033	-0.004
	N=121,614	(0.041)	(0.044)	(0.041)	(0.016)	(0.015)	(0.015)	(0.229)	(0.180)	(0.157)
	79-00 GHS	0.147***	0.149***	0.150***	-0.006	0.003	0.006	-0.043	0.019	0.041
	N=92,730	(0.050)	(0.050)	(0.051)	(0.018)	(0.017)	(0.016)	(0.129)	(0.108)	(0.100)
	84-98 GHS	0.166***	0.167***	0.165***	0.011	0.016	0.017	0.064	0.093	0.103
	N=71,943	(0.049)	(0.049)	(0.050)	(0.016)	(0.018)	(0.017)	(0.089)	(0.096)	(0.091)
	79-86 GHS	0.292***	0.320***	0.262***	0.101***	0.135***	0.050***	0.347***	0.421***	0.192***
	N=71,639	(0.025)	(0.026)	(0.028)	(0.014)	(0.015)	(0.017)	(0.051)	(0.051)	(0.062)
1947 & 1972 Reforms	79-06 GHS	0.519***	0.514***	0.521***	0.004	0.001	0.001	0.018	0.013	0.012
	N=145,928	(0.065)	(0.065)	(0.068)	(0.012)	(0.010)	(0.011)	(0.021)	(0.019)	(0.020)
		0.349***	0.347***	0.354***	-0.013	-0.017	-0.015			
		(0.084)	(0.085)	(0.087)	(0.022)	(0.019)	(0.019)			
	79-00 GHS	0.545***	0.545***	0.545***	0.008	0.004	0.003	0.012	0.006	0.006
	N=117,044	(0.071)	(0.071)	(0.072)	(0.011)	(0.011)	(0.012)	(0.019)	(0.019)	(0.020)
		0.657***	0.657***	0.659***	0.004	0.001	0.003			
		(0.076)	(0.076)	(0.077)	(0.017)	(0.017)	(0.018)			
	84-98 GHS	0.519***	0.514***	0.505***	-0.006	-0.020	-0.022	-0.007	-0.040	-0.042
	N=83,236	(0.075)	(0.074)	(0.075)	(0.017)	(0.017)	(0.018)	(0.031)	(0.032)	(0.033)
		0.628***	0.620***	0.608***	-0.001	-0.025	-0.024			
		(0.079)	(0.079)	(0.081)	(0.022)	(0.022)	(0.022)			
	79-86 GHS	0.568***	0.579***	0.493***	0.058***	0.074***	0.009	0.133***	0.167***	0.047
	N=71,639	(0.024)	(0.024)	(0.029)	(0.014)	(0.014)	(0.017)	(0.023)	(0.023)	(0.032)
		0.661***	0.699***	0.596***	0.122***	0.163***	0.053**			
		(0.031)	(0.032)	(0.036)	(0.018)	(0.019)	(0.021)			
	Age Controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies
	Degree YoB Polynomial	Four	Four	Four	Four	Four	Four	Four	Four	Four

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

TABLE E.31 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

POOLED SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.520***	0.512***	0.512***	0.018	0.013	0.015	0.035	0.025	0.029
	N=96,549	(0.030)	(0.028)	(0.030)	(0.013)	(0.013)	(0.014)	(0.024)	(0.025)	(0.027)
	79-00 GHS	0.505***	0.504***	0.505***	0.009	0.008	0.011	0.019	0.015	0.023
	N=85,766	(0.031)	(0.031)	(0.030)	(0.014)	(0.014)	(0.015)	(0.027)	(0.028)	(0.030)
	84-98 GHS	0.449***	0.450***	0.455***	0.006	0.005	0.008	0.014	0.011	0.018
	N=53,502	(0.037)	(0.037)	(0.039)	(0.022)	(0.022)	(0.022)	(0.048)	(0.049)	(0.048)
	79-86 GHS	0.592***	0.603***	0.572***	0.030*	0.053***	0.013	0.051**	0.087***	0.023
	N=71,639	(0.026)	(0.027)	(0.031)	(0.016)	(0.016)	(0.018)	(0.026)	(0.025)	(0.032)
1972 Reform	79-06 GHS	0.240***	0.240***	0.243***	0.009	0.008	0.009	0.040	0.032	0.034
	N=121,614	(0.056)	(0.055)	(0.056)	(0.023)	(0.021)	(0.020)	(0.086)	(0.083)	(0.082)
	79-00 GHS	0.267***	0.267***	0.270***	-0.005	-0.003	0.000	-0.017	-0.012	0.003
	N=92,730	(0.042)	(0.042)	(0.042)	(0.023)	(0.022)	(0.022)	(0.088)	(0.085)	(0.081)
	84-98 GHS	0.277***	0.280***	0.278***	-0.008	0.001	0.003	-0.029	0.004	0.010
	N=71,943	(0.043)	(0.044)	(0.045)	(0.021)	(0.023)	(0.022)	(0.080)	(0.081)	(0.079)
	79-86 GHS	0.155***	0.179***	0.207***	0.016	0.058***	0.029	0.103	0.322***	0.138
	N=71,639	(0.028)	(0.029)	(0.032)	(0.016)	(0.017)	(0.019)	(0.103)	(0.096)	(0.087)
1947 & 1972 Reforms	79-06 GHS	0.464***	0.457***	0.457***	0.025	0.012	0.011	0.054*	0.039	0.036
	N=145,928	(0.072)	(0.071)	(0.073)	(0.016)	(0.014)	(0.014)	(0.030)	(0.028)	(0.028)
		0.279***	0.272**	0.270**	0.014	-0.002	-0.002			
		(0.103)	(0.107)	(0.109)	(0.022)	(0.019)	(0.019)			
	79-00 GHS	0.449***	0.449***	0.446***	0.011	0.005	0.005	0.024	0.012	0.012
	N=117,044	(0.068)	(0.068)	(0.069)	(0.014)	(0.013)	(0.014)	(0.028)	(0.028)	(0.031)
		0.517***	0.517***	0.515***	0.009	0.002	0.006			
		(0.085)	(0.085)	(0.087)	(0.021)	(0.021)	(0.022)			
	84-98 GHS	0.425***	0.426***	0.428***	-0.016	-0.012	-0.011	-0.037	-0.029	-0.023
	N=83,236	(0.075)	(0.076)	(0.077)	(0.022)	(0.021)	(0.021)	(0.051)	(0.050)	(0.049)
		0.493***	0.494***	0.498***	-0.016	-0.013	-0.007			
		(0.093)	(0.094)	(0.096)	(0.029)	(0.028)	(0.028)			
	79-86 GHS	0.523***	0.530***	0.480***	0.020	0.038**	0.004	0.040	0.080***	0.020
	N=71,639	(0.025)	(0.025)	(0.029)	(0.015)	(0.015)	(0.017)	(0.028)	(0.027)	(0.034)
		0.553***	0.583***	0.546***	0.030	0.078***	0.033			
		(0.034)	(0.035)	(0.038)	(0.020)	(0.020)	(0.022)			
	Age Controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies
	Degree YoB Polynomial	Five	Five	Five	Five	Five	Five	Five	Five	Five

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

TABLE E.32 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS POOLED SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.486***	0.480***	0.487***	0.017	0.014	0.020	0.034	0.030	0.041
	N=96,549	(0.031)	(0.031)	(0.033)	(0.017)	(0.017)	(0.017)	(0.035)	(0.033)	(0.034)
	79-00 GHS	0.483***	0.483***	0.487***	0.019	0.019	0.025	0.038	0.039	0.052
	N=85,766	(0.037)	(0.037)	(0.036)	(0.018)	(0.018)	(0.018)	(0.035)	(0.036)	(0.035)
	84-98 GHS	0.442***	0.442***	0.449***	0.036	0.036	0.036	0.086	0.085	0.080
	N=53,502	(0.044)	(0.044)	(0.047)	(0.025)	(0.026)	(0.027)	(0.058)	(0.058)	(0.058)
	79-86 GHS	0.613***	0.607***	0.578***	0.027	0.024	0.002	0.044	0.039	0.003
	N=71,639	(0.029)	(0.029)	(0.033)	(0.017)	(0.017)	(0.020)	(0.028)	(0.028)	(0.034)
1972 Reform	79-06 GHS	0.241***	0.242***	0.245***	0.009	0.009	0.010	-0.087	0.048	0.051
	N=121,614	(0.059)	(0.059)	(0.059)	(0.024)	(0.023)	(0.023)	(0.263)	(0.074)	(0.070)
	79-00 GHS	0.278***	0.279***	0.280***	-0.001	0.002	0.004	0.001	0.003	0.001
	N=92,730	(0.048)	(0.048)	(0.048)	(0.025)	(0.024)	(0.024)	(0.082)	(0.077)	(0.077)
	84-98 GHS	0.283***	0.285***	0.284***	-0.004	0.004	0.006	-0.013	-0.011	0.016
	N=71,943	(0.049)	(0.050)	(0.050)	(0.023)	(0.024)	(0.024)	(0.073)	(0.081)	(0.075)
	79-86 GHS	0.061*	0.078**	0.165***	0.008	0.020	0.018	0.143	0.248	0.106
	N=71,639	(0.033)	(0.033)	(0.034)	(0.019)	(0.019)	(0.020)	(0.315)	(0.263)	(0.121)
1947 & 1972 Reforms	79-06 GHS	0.450***	0.446***	0.444***	0.018	0.010	0.008	0.035	0.031	0.025
	N=145,928	(0.065)	(0.064)	(0.065)	(0.014)	(0.013)	(0.013)	(0.026)	(0.025)	(0.026)
		0.286***	0.278**	0.276**	0.017	-0.001	-0.001			
		(0.104)	(0.108)	(0.110)	(0.020)	(0.019)	(0.019)			
	79-00 GHS	0.443***	0.443***	0.439***	0.011	0.007	0.006	0.024	0.012	0.013
	N=117,044	(0.064)	(0.064)	(0.065)	(0.013)	(0.012)	(0.013)	(0.028)	(0.027)	(0.030)
		0.535***	0.534***	0.536***	0.010	-0.001	0.004			
		(0.084)	(0.084)	(0.085)	(0.023)	(0.022)	(0.024)			
	84-98 GHS	0.385***	0.385***	0.389***	-0.010	-0.009	-0.004	-0.030	-0.025	-0.014
	N=83,236	(0.064)	(0.064)	(0.066)	(0.020)	(0.020)	(0.020)	(0.054)	(0.052)	(0.051)
		0.496***	0.498***	0.501***	-0.017	-0.014	-0.008			
		(0.080)	(0.081)	(0.083)	(0.029)	(0.027)	(0.027)			
	79-86 GHS	0.527***	0.523***	0.481***	0.017	0.016	-0.003	0.032	0.028	-0.002
	N=71,639	(0.027)	(0.027)	(0.030)	(0.016)	(0.016)	(0.018)	(0.030)	(0.030)	(0.037)
		0.562***	0.568***	0.546***	0.023	0.031	0.019			
		(0.041)	(0.041)	(0.042)	(0.024)	(0.024)	(0.025)			
	Age Controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies
	Degree YoB Polynomial	Six	Six	Six	Six	Six	Six	Six	Six	Six

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

TABLE E.33 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

POOLED SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.474***	0.469***	0.474***	0.019	0.017	0.022	0.031	0.024	0.042
	N=96,549	(0.024)	(0.025)	(0.025)	(0.016)	(0.016)	(0.016)	(0.031)	(0.033)	(0.046)
	79-00 GHS	0.472***	0.472***	0.477***	0.020	0.020	0.025	0.031	0.034	0.093
	N=85,766	(0.029)	(0.029)	(0.030)	(0.017)	(0.017)	(0.017)	(0.032)	(0.033)	(0.061)
	84-98 GHS	0.418***	0.418***	0.424***	0.048*	0.047*	0.046*	0.042	0.062	0.039
	N=53,502	(0.039)	(0.039)	(0.042)	(0.025)	(0.025)	(0.027)	(0.054)	(0.060)	(0.052)
	79-86 GHS	0.516***	0.512***	0.512***	0.003	0.002	-0.007	0.056**	0.058**	0.015
	N=71,639	(0.033)	(0.033)	(0.035)	(0.019)	(0.019)	(0.021)	(0.026)	(0.027)	(0.033)
1972 Reform	79-06 GHS	0.244***	0.244***	0.247***	0.009	0.008	0.008	0.016	0.039	0.041
	N=121,614	(0.059)	(0.059)	(0.059)	(0.023)	(0.023)	(0.023)	(0.090)	(0.076)	(0.074)
	79-00 GHS	0.277***	0.277***	0.279***	-0.002	-0.000	0.003	0.016	-0.003	0.025
	N=92,730	(0.046)	(0.046)	(0.046)	(0.025)	(0.024)	(0.024)	(0.073)	(0.083)	(0.079)
	84-98 GHS	0.284***	0.286***	0.285***	-0.007	0.001	0.003	-0.019	0.016	-0.007
	N=71,943	(0.047)	(0.048)	(0.049)	(0.024)	(0.025)	(0.024)	(0.071)	(0.078)	(0.081)
	79-86 GHS	0.138***	0.153***	0.172***	0.017	0.029	0.018	0.126***	0.124***	0.033
	N=71,639	(0.033)	(0.033)	(0.034)	(0.019)	(0.019)	(0.020)	(0.038)	(0.036)	(0.044)
1947 & 1972 Reforms	79-06 GHS	0.426***	0.421***	0.422***	0.015	0.006	0.004	0.019	0.030	0.012
	N=145,928	(0.071)	(0.070)	(0.071)	(0.016)	(0.014)	(0.015)	(0.040)	(0.025)	(0.033)
		0.279**	0.271**	0.270**	0.016	-0.002	-0.002			
		(0.106)	(0.110)	(0.112)	(0.020)	(0.019)	(0.019)			
	79-00 GHS	0.384***	0.384***	0.383***	0.005	0.004	0.005	0.042	0.000	0.015
	N=117,044	(0.063)	(0.063)	(0.064)	(0.014)	(0.014)	(0.015)	(0.060)	(0.000)	(0.027)
		0.530***	0.530***	0.530***	0.009	-0.001	0.004			
		(0.079)	(0.079)	(0.080)	(0.022)	(0.022)	(0.024)			
	84-98 GHS	0.354***	0.356***	0.359***	-0.010	-0.005	-0.002	-0.020	-0.027	-0.010
	N=83,236	(0.063)	(0.063)	(0.065)	(0.022)	(0.022)	(0.022)	(0.074)	(0.054)	(0.051)
		0.495***	0.497***	0.501***	-0.017	-0.014	-0.008			
		(0.079)	(0.080)	(0.082)	(0.028)	(0.028)	(0.028)			
	79-86 GHS	0.427***	0.424***	0.415***	-0.006	-0.006	-0.012	0.058**	0.060**	0.024
	N=71,639	(0.030)	(0.030)	(0.032)	(0.018)	(0.018)	(0.019)	(0.029)	(0.028)	(0.033)
		0.497***	0.504***	0.496***	0.007	0.017	0.012			
		(0.041)	(0.041)	(0.043)	(0.025)	(0.025)	(0.025)			
	Age Controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies
	Degree YoB Polynomial	Seven	Seven	Seven	Seven	Seven	Seven	Seven	Seven	Seven

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

TABLE E.34 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

POOLED SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.470***	0.466***	0.469***	0.021	0.018	0.023	0.053	0.046	0.042
	N=96,549	(0.027)	(0.028)	(0.028)	(0.017)	(0.017)	(0.017)	(0.036)	(0.036)	(0.040)
	79-00 GHS	0.470***	0.470***	0.476***	0.022	0.021	0.026	0.033	0.043	0.042
	N=85,766	(0.034)	(0.034)	(0.035)	(0.018)	(0.018)	(0.018)	(0.032)	(0.033)	(0.031)
	84-98 GHS	0.426***	0.425***	0.429***	0.049*	0.048*	0.047	0.051	0.047	0.048
	N=53,502	(0.046)	(0.046)	(0.049)	(0.026)	(0.027)	(0.028)	(0.054)	(0.053)	(0.054)
	79-86 GHS	0.508***	0.504***	0.506***	0.000	-0.001	-0.003	0.112***	0.074***	0.000
	N=71,639	(0.033)	(0.033)	(0.035)	(0.020)	(0.019)	(0.021)	(0.027)	(0.026)	(0.000)
1972 Reform	79-06 GHS	0.239***	0.237***	0.236***	0.011	0.008	0.007	0.000	0.000	0.033
	N=121,614	(0.069)	(0.069)	(0.068)	(0.028)	(0.028)	(0.027)	(0.000)	(0.000)	(0.076)
	79-00 GHS	0.341***	0.341***	0.342***	0.015	0.013	0.013	0.000	0.000	-0.005
	N=92,730	(0.052)	(0.052)	(0.052)	(0.030)	(0.030)	(0.029)	(0.000)	(0.000)	(0.093)
	84-98 GHS	0.357***	0.360***	0.361***	-0.006	0.004	0.007	-0.014	0.005	-0.009
	N=71,943	(0.056)	(0.057)	(0.058)	(0.029)	(0.030)	(0.029)	(0.065)	(0.072)	(0.090)
	79-86 GHS	0.110***	0.124***	0.152***	0.011	0.021	0.034	0.327***	-0.064*	0.046
	N=71,639	(0.035)	(0.035)	(0.036)	(0.020)	(0.020)	(0.021)	(0.042)	(0.037)	(0.039)
1947 & 1972 Reforms	79-06 GHS	0.378***	0.378***	0.381***	0.009	0.008	0.008	0.037	0.010	0.183**
	N=145,928	(0.069)	(0.069)	(0.072)	(0.017)	(0.016)	(0.016)	(0.025)	(0.022)	(0.081)
		0.224**	0.222*	0.223*	0.010	0.001	0.003			
		(0.106)	(0.114)	(0.117)	(0.021)	(0.020)	(0.020)			
	79-00 GHS	0.346***	0.347***	0.346***	0.004	0.007	0.010	0.000	0.010	0.000
	N=117,044	(0.062)	(0.063)	(0.063)	(0.015)	(0.015)	(0.016)	(0.000)	(0.019)	(0.000)
		0.491***	0.491***	0.493***	0.008	0.002	0.009			
		(0.073)	(0.073)	(0.074)	(0.022)	(0.021)	(0.023)			
	84-98 GHS	0.309***	0.309***	0.314***	0.013	0.014	0.015	-0.088	-0.152	-0.052
	N=83,236	(0.064)	(0.064)	(0.067)	(0.026)	(0.026)	(0.026)	(0.069)	(0.221)	(0.115)
		0.454***	0.454***	0.460***	0.005	0.003	0.008			
		(0.074)	(0.074)	(0.077)	(0.029)	(0.030)	(0.030)			
	79-86 GHS	0.423***	0.420***	0.411***	-0.007	-0.007	-0.009	0.079***	0.408***	0.018
	N=71,639	(0.030)	(0.030)	(0.032)	(0.018)	(0.018)	(0.019)	(0.026)	(0.038)	(0.032)
		0.479***	0.486***	0.481***	0.003	0.012	0.021			
		(0.042)	(0.042)	(0.043)	(0.025)	(0.025)	(0.026)			
	Age Controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies
	Degree YoB Polynomial	Eight	Eight	Eight	Eight	Eight	Eight	Eight	Eight	Eight

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

TABLE E.35 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

POOLED SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	1.135***	1.179***	1.169***	0.194***	0.196***	0.201***	0.254***	-0.172	0.244***
	N=96,549	(0.031)	(0.029)	(0.030)	(0.008)	(0.007)	(0.009)	(0.006)	(0.138)	(0.006)
	79-00 GHS	1.004***	1.005***	1.025***	0.203***	0.184***	0.208***	0.331***	0.317***	0.318***
	N=85,766	(0.010)	(0.011)	(0.013)	(0.007)	(0.008)	(0.012)	(0.009)	(0.007)	(0.008)
	84-98 GHS	0.825***	1.056***	0.876***	0.144***	0.110***	0.124***	0.252***	0.083	0.244***
	N=53,502	(0.023)	(0.030)	(0.026)	(0.013)	(0.021)	(0.019)	(0.010)	(0.568)	(0.015)
	79-86 GHS	0.404*	0.512**	0.868*	0.280**	0.195	0.440	0.000	1.874***	0.585*
	N=71,639	(0.233)	(0.236)	(0.467)	(0.138)	(0.139)	(0.276)	(0.000)	(0.473)	(0.334)
1972 Reform	79-06 GHS	2.316***	2.323***	2.310***	0.621***	0.640***	0.637***	0.268***	0.276***	0.276***
	N=121,614	(0.037)	(0.042)	(0.045)	(0.009)	(0.005)	(0.005)	(0.007)	(0.006)	(0.006)
	79-00 GHS	0.377***	0.373***	0.356***	0.170***	0.144***	0.146***	0.525***	0.502***	0.131***
	N=92,730	(0.004)	(0.005)	(0.005)	(0.009)	(0.005)	(0.007)	(0.019)	(0.011)	(0.005)
	84-98 GHS	1.149***	1.143***	1.148***	0.512***	0.491***	0.494***	0.446***	0.430***	0.106***
	N=71,943	(0.035)	(0.033)	(0.034)	(0.037)	(0.021)	(0.021)	(0.029)	(0.017)	(0.007)
	79-86 GHS	-1.608	-15.281	-1.789	-0.785	-27.332	-0.496	1.007	0	-0.201**
	N=71,639	(20,361)	(20,352)	(20,344)	(12,048)	(12,034)	(12,017)	(1.033)	(0)	(0.083)
1947 & 1972 Reforms	79-06 GHS	-0.114***	-0.113***	-0.112***	-0.003*	-0.000	0.001	-0.042	-0.010	-0.017
	N=145,928	(0.001)	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.048)	(0.048)	(0.049)
		-0.374***	-0.376***	-0.383***	0.008	0.002	0.006			
		(0.064)	(0.076)	(0.077)	(0.014)	(0.013)	(0.013)			
	79-00 GHS	-0.114***	-0.114***	-0.114***	0.003**	0.005***	0.008***	0.014	-0.038	-0.045
	N=117,044	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.050)	(0.023)	(0.046)
		-0.109***	-0.111***	-0.108***	0.031*	0.015	0.022			
		(0.033)	(0.034)	(0.034)	(0.016)	(0.017)	(0.019)			
	84-98 GHS	-0.078***	-0.078***	-0.078***	-0.012***	-0.009***	-0.005**	0.119**	0.115	0.045
	N=83,236	(0.001)	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.046)	(0.081)	(0.109)
		-0.080**	-0.084**	-0.084**	0.004	-0.009	-0.002			
		(0.033)	(0.034)	(0.036)	(0.017)	(0.017)	(0.019)			
	79-86 GHS	-0.069	-0.070	-0.070	-0.038	-0.038	-0.039	-0.248	-0.247	-0.268
	N=71,639	(0.089)	(0.089)	(0.089)	(0.053)	(0.052)	(0.052)	(0.316)	(0.314)	(0.318)
		-0.166*	-0.167*	-0.167*	-0.002	-0.002	-0.000			
		(0.099)	(0.099)	(0.099)	(0.059)	(0.059)	(0.059)			
	Age controls	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies	Quadratic	Quartic	Dummies
	Degree YoB Polynomial	Dummies	Dummies	Dummies	Dummies	Dummies	Dummies	Dummies	Dummies	Dummies

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

TABLE E.36 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

MALE SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	1.353***	1.377***	1.381***	0.172***	0.178***	0.179***	0.127***	0.129***	0.129***
	N=52,714	(0.117)	(0.121)	(0.121)	(0.018)	(0.017)	(0.016)	(0.005)	(0.005)	(0.005)
	79-00 GHS	0.985***	0.978***	0.979***	0.150***	0.151***	0.152***	0.153***	0.154***	0.155***
	N=46,995	(0.051)	(0.048)	(0.046)	(0.015)	(0.013)	(0.013)	(0.011)	(0.010)	(0.010)
	84-98 GHS	0.806***	0.802***	0.806***	0.037***	0.039***	0.036***	0.046***	0.049***	0.045**
	N=28,890	(0.056)	(0.055)	(0.055)	(0.011)	(0.011)	(0.012)	(0.015)	(0.015)	(0.017)
	79-86 GHS	0.601***	0.610***	0.569***	0.021*	0.067***	0.063***	0.034*	0.110***	0.110***
	N=40,267	(0.027)	(0.033)	(0.038)	(0.011)	(0.013)	(0.015)	(0.018)	(0.021)	(0.026)
1972 Reform	79-06 GHS	0.986***	0.991***	0.995***	0.146***	0.148***	0.150***	0.148***	0.149***	0.151***
	N=64,460	(0.076)	(0.076)	(0.072)	(0.015)	(0.014)	(0.014)	(0.008)	(0.008)	(0.008)
	79-00 GHS	0.482***	0.483***	0.484***	0.121***	0.119***	0.119***	0.252***	0.246***	0.245***
	N=49,843	(0.043)	(0.043)	(0.042)	(0.015)	(0.015)	(0.015)	(0.024)	(0.023)	(0.023)
	84-98 GHS	0.343***	0.352***	0.351***	0.066***	0.065***	0.065***	0.193***	0.183***	0.186***
	N=38,003	(0.036)	(0.032)	(0.031)	(0.012)	(0.012)	(0.012)	(0.037)	(0.036)	(0.035)
	79-86 GHS	0.004	0.212***	0.156***	-0.131***	0.023*	-0.020	-37.000	0.108*	-0.130
	N=40,267	(0.027)	(0.032)	(0.034)	(0.011)	(0.013)	(0.014)	(282.176)	(0.058)	(0.100)
1947 & 1972 Reform	79-06 GHS	1.444***	1.443***	1.445***	0.181***	0.184***	0.184***	0.133***	0.134***	0.134***
	N=78,363	(0.114)	(0.117)	(0.118)	(0.018)	(0.017)	(0.017)	(0.005)	(0.004)	(0.004)
		2.272***	2.271***	2.272***	0.308***	0.311***	0.311***			
		(0.134)	(0.141)	(0.142)	(0.020)	(0.020)	(0.020)			
	79-00 GHS	0.934***	0.931***	0.935***	0.138***	0.140***	0.142***	0.163***	0.166***	0.167***
	N=63,746	(0.054)	(0.055)	(0.054)	(0.017)	(0.015)	(0.015)	(0.012)	(0.012)	(0.012)
		1.345***	1.358***	1.365***	0.239***	0.243***	0.246***			
		(0.078)	(0.078)	(0.074)	(0.026)	(0.025)	(0.024)			
	84-98 GHS	0.725***	0.763***	0.763***	0.030**	0.035**	0.033**	0.069***	0.071***	0.069***
	N=44,682	(0.055)	(0.062)	(0.060)	(0.014)	(0.014)	(0.015)	(0.018)	(0.019)	(0.020)
		0.991***	1.048***	1.048***	0.079***	0.086***	0.085***			
		(0.071)	(0.073)	(0.071)	(0.021)	(0.022)	(0.023)			
	79-86 GHS	0.552***	0.551***	0.500***	0.004	0.056***	0.046***	0.025	0.102***	0.082***
	N=40,267	(0.027)	(0.031)	(0.036)	(0.011)	(0.013)	(0.014)	(0.019)	(0.021)	(0.027)
		0.532***	0.684***	0.612***	-0.123***	0.070***	0.032			
		(0.037)	(0.043)	(0.049)	(0.015)	(0.017)	(0.020)			
	Age Controls	Quadr	Quart	Dumm	Quadr	Quart	Dumm	Quadr	Quart	Dumm
	Degree YoB Polynomial	Zero	Zero	Zero	Zero	Zero	Zero	Zero	Zero	Zero

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

TABLE E.37 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

MALE SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.433***	0.461***	0.466***	-0.032*	-0.026	-0.025	-0.075*	-0.057	-0.054
	N=52,714	(0.036)	(0.033)	(0.033)	(0.017)	(0.017)	(0.018)	(0.043)	(0.039)	(0.039)
	79-00 GHS	0.546***	0.545***	0.546***	-0.011	-0.009	-0.009	-0.020	-0.017	-0.017
	N=46,995	(0.028)	(0.028)	(0.028)	(0.014)	(0.015)	(0.015)	(0.026)	(0.027)	(0.027)
	84-98 GHS	0.496***	0.485***	0.484***	-0.021	-0.020	-0.026	-0.042	-0.041	-0.054
	N=28,890	(0.042)	(0.041)	(0.042)	(0.018)	(0.019)	(0.020)	(0.034)	(0.037)	(0.040)
	79-86 GHS	0.598***	0.606***	0.563***	0.019*	0.065***	0.060***	0.032*	0.107***	0.106***
	N=40,267	(0.027)	(0.033)	(0.038)	(0.011)	(0.013)	(0.015)	(0.018)	(0.021)	(0.026)
1972 Reform	79-06 GHS	0.007	0.010	0.009	-0.035**	-0.031**	-0.029*	-4.997	-3.063	-3.214
	N=64,460	(0.045)	(0.042)	(0.040)	(0.016)	(0.015)	(0.015)	(33.662)	(13.826)	(15.500)
	79-00 GHS	0.055	0.057	0.057	-0.025	-0.028	-0.028	-0.446	-0.492	-0.479
	N=49,843	(0.049)	(0.049)	(0.049)	(0.017)	(0.017)	(0.017)	(0.641)	(0.659)	(0.635)
	84-98 GHS	0.048	0.053	0.050	0.010	0.005	0.005	0.208	0.093	0.105
	N=38,003	(0.055)	(0.054)	(0.055)	(0.016)	(0.016)	(0.016)	(0.292)	(0.268)	(0.273)
	79-86 GHS	-0.002	0.206***	0.148***	-0.133***	0.020	-0.024*	85.191	0.099*	-0.159
	N=40,267	(0.027)	(0.032)	(0.034)	(0.011)	(0.013)	(0.014)	(1,466)	(0.060)	(0.109)
1947 & 1972 Reform	79-06 GHS	0.301***	0.303***	0.303***	-0.020	-0.017	-0.017	0.014	0.024	0.023
	N=78,363	(0.038)	(0.040)	(0.040)	(0.012)	(0.012)	(0.013)	(0.037)	(0.036)	(0.037)
		0.201***	0.202***	0.202***	-0.057**	-0.054**	-0.054**			
		(0.064)	(0.065)	(0.065)	(0.022)	(0.021)	(0.021)			
	79-00 GHS	0.496***	0.498***	0.498***	-0.003	-0.001	-0.001	0.009	0.013	0.014
	N=63,746	(0.032)	(0.033)	(0.033)	(0.012)	(0.013)	(0.014)	(0.025)	(0.026)	(0.026)
		0.501***	0.510***	0.511***	-0.033	-0.034	-0.033			
		(0.064)	(0.064)	(0.065)	(0.025)	(0.026)	(0.026)			
	84-98 GHS	0.462***	0.463***	0.456***	-0.018	-0.021	-0.025	-0.040	-0.045	-0.054
	N=44,682	(0.039)	(0.041)	(0.042)	(0.015)	(0.018)	(0.020)	(0.031)	(0.036)	(0.039)
		0.469***	0.469***	0.458***	-0.016	-0.021	-0.025			
		(0.065)	(0.070)	(0.073)	(0.026)	(0.030)	(0.031)			
	79-86 GHS	0.549***	0.547***	0.494***	0.002	0.054***	0.043***	0.024	0.099***	0.077***
	N=40,267	(0.027)	(0.031)	(0.036)	(0.011)	(0.013)	(0.014)	(0.019)	(0.022)	(0.028)
		0.526***	0.677***	0.602***	-0.127***	0.066***	0.026			
		(0.037)	(0.043)	(0.049)	(0.015)	(0.018)	(0.020)			
	Age Controls Degree	Quadr	Quart	Dumm	Quadr	Quart	Dumm	Quadr	Quart	Dumm
	YoB Polynom.	One	One	One	One	One	One	One	One	One

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

TABLE E.38 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

MALE SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.535***	0.553***	0.563***	0.016*	0.020**	0.022*	0.029*	0.035**	0.040*
	N=52,714	(0.040)	(0.042)	(0.047)	(0.009)	(0.009)	(0.012)	(0.017)	(0.017)	(0.022)
	79-00 GHS	0.553***	0.551***	0.553***	0.017*	0.019**	0.021*	0.031*	0.034*	0.039*
	N=46,995	(0.037)	(0.037)	(0.038)	(0.009)	(0.009)	(0.012)	(0.017)	(0.017)	(0.022)
	84-98 GHS	0.500***	0.493***	0.492***	0.003	0.006	0.003	0.005	0.012	0.006
	N=28,890	(0.058)	(0.057)	(0.057)	(0.016)	(0.016)	(0.017)	(0.033)	(0.032)	(0.035)
	79-86 GHS	0.607***	0.617***	0.576***	0.001	0.041***	0.026*	0.002	0.066***	0.046*
	N=40,267	(0.028)	(0.034)	(0.039)	(0.011)	(0.014)	(0.016)	(0.019)	(0.021)	(0.027)
1972 Reform	79-06 GHS	0.047	0.055	0.055	-0.001	0.005	0.009	-0.020	0.099	0.169
	N=64,460	(0.061)	(0.060)	(0.058)	(0.019)	(0.017)	(0.017)	(0.421)	(0.238)	(0.204)
	79-00 GHS	0.120	0.122	0.120	0.017	0.013	0.012	0.143	0.109	0.097
	N=49,843	(0.073)	(0.073)	(0.072)	(0.020)	(0.021)	(0.021)	(0.128)	(0.135)	(0.143)
	84-98 GHS	0.133*	0.138*	0.134*	0.028	0.026	0.027	0.213*	0.189*	0.197*
	N=38,003	(0.070)	(0.068)	(0.068)	(0.018)	(0.019)	(0.019)	(0.112)	(0.109)	(0.111)
	79-86 GHS	0.022	0.274***	0.219***	-0.117***	0.072***	0.030**	-5.401	0.261***	0.137**
	N=40,267	(0.028)	(0.034)	(0.038)	(0.011)	(0.014)	(0.015)	(7.183)	(0.052)	(0.067)
1947 & 1972 Reform	79-06 GHS	0.462***	0.468***	0.469***	-0.012	-0.006	-0.005	0.015	0.027	0.028
	N=78,363	(0.045)	(0.047)	(0.050)	(0.016)	(0.016)	(0.017)	(0.034)	(0.033)	(0.034)
		0.243***	0.245***	0.246***	-0.054**	-0.051**	-0.051**			
		(0.058)	(0.058)	(0.058)	(0.021)	(0.021)	(0.020)			
	79-00 GHS	0.475***	0.476***	0.475***	-0.004	-0.002	-0.002	-0.024	-0.022	-0.022
	N=63,746	(0.047)	(0.047)	(0.049)	(0.016)	(0.017)	(0.018)	(0.037)	(0.037)	(0.039)
		0.499***	0.509***	0.510***	-0.033	-0.034	-0.033			
		(0.062)	(0.061)	(0.062)	(0.025)	(0.026)	(0.026)			
	84-98 GHS	0.416***	0.418***	0.409***	-0.012	-0.015	-0.018	-0.030	-0.038	-0.048
	N=44,682	(0.052)	(0.051)	(0.052)	(0.021)	(0.022)	(0.024)	(0.046)	(0.050)	(0.054)
		0.459***	0.462***	0.449***	-0.014	-0.020	-0.024			
		(0.060)	(0.064)	(0.066)	(0.026)	(0.029)	(0.030)			
	79-86 GHS	0.558***	0.544***	0.490***	-0.008	0.029**	0.014	-0.004	0.076***	0.039
	N=40,267	(0.027)	(0.032)	(0.037)	(0.011)	(0.013)	(0.015)	(0.020)	(0.022)	(0.029)
		0.522***	0.679***	0.604***	-0.123***	0.081***	0.037*			
		(0.037)	(0.044)	(0.049)	(0.015)	(0.018)	(0.020)			
	Age Controls	Quadr	Quartic	Dummies	Quadr	Quartic	Dummies	Quadr	Quartic	Dummies
	Degree YoB Polynomial	Two	Two	Two	Two	Two	Two	Two	Two	Two

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

TABLE E.39 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

MALE SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.584**	0.597***	0.601***	0.023**	0.024**	0.026**	0.039**	0.040**	0.043*
	N=52,714	(0.040)	(0.044)	(0.046)	(0.010)	(0.010)	(0.012)	(0.018)	(0.018)	(0.022)
	79-00 GHS	0.572**	0.575***	0.577***	0.011	0.011	0.013	0.019	0.020	0.023
	N=46,995	(0.039)	(0.040)	(0.040)	(0.012)	(0.012)	(0.015)	(0.021)	(0.021)	(0.026)
	84-98 GHS	0.535**	0.526***	0.525***	0.003	0.004	0.001	0.006	0.007	0.003
	N=28,890	(0.051)	(0.051)	(0.050)	(0.017)	(0.017)	(0.018)	(0.033)	(0.033)	(0.035)
	79-86 GHS	0.550**	0.601***	0.552***	-0.066***	0.025*	0.004	-0.120***	0.041*	0.007
	N=40,267	(0.029)	(0.034)	(0.040)	(0.012)	(0.014)	(0.016)	(0.023)	(0.022)	(0.029)
1972 Reform	79-06 GHS	0.079	0.084	0.089*	0.008	0.012	0.017	0.102	0.149	0.192
	N=64,460	(0.054)	(0.055)	(0.051)	(0.016)	(0.015)	(0.014)	(0.151)	(0.122)	(0.114)
	79-00 GHS	0.190**	0.191***	0.189***	0.034**	0.033**	0.032**	0.181**	0.173**	0.169**
	N=49,843	(0.040)	(0.040)	(0.041)	(0.015)	(0.014)	(0.015)	(0.074)	(0.073)	(0.075)
	84-98 GHS	0.183**	0.182***	0.178***	0.038**	0.036**	0.036**	0.209***	0.197**	0.202**
	N=38,003	(0.042)	(0.043)	(0.045)	(0.015)	(0.015)	(0.015)	(0.076)	(0.078)	(0.079)
	79-86 GHS	0.321**	0.314***	0.266***	0.048***	0.093***	0.055***	0.150***	0.296***	0.205***
	N=40,267	(0.034)	(0.035)	(0.038)	(0.013)	(0.014)	(0.015)	(0.041)	(0.048)	(0.057)
1947 & 1972 Reform	79-06 GHS	0.474**	0.481***	0.483***	-0.000	0.005	0.006	0.001	0.012	0.014
	N=78,363	(0.047)	(0.048)	(0.051)	(0.010)	(0.010)	(0.012)	(0.024)	(0.023)	(0.026)
		0.294**	0.304***	0.308***	-0.003	0.000	0.002			
		(0.070)	(0.074)	(0.075)	(0.020)	(0.019)	(0.020)			
	79-00 GHS	0.484**	0.484***	0.485***	0.002	0.004	0.005	0.013	0.016	0.019
	N=63,746	(0.051)	(0.051)	(0.053)	(0.010)	(0.010)	(0.013)	(0.021)	(0.021)	(0.026)
		0.582**	0.582***	0.584***	0.020	0.022	0.024			
		(0.067)	(0.067)	(0.068)	(0.024)	(0.023)	(0.025)			
	84-98 GHS	0.451**	0.450***	0.441***	0.001	0.000	-0.003	0.015	0.013	0.007
	N=44,682	(0.063)	(0.063)	(0.063)	(0.018)	(0.018)	(0.019)	(0.039)	(0.040)	(0.042)
		0.543**	0.543***	0.529***	0.019	0.018	0.015			
		(0.079)	(0.079)	(0.081)	(0.028)	(0.028)	(0.029)			
	79-86 GHS	0.487**	0.524***	0.463***	-0.069***	0.014	-0.006	-0.101***	0.071***	0.028
	N=40,267	(0.028)	(0.032)	(0.037)	(0.011)	(0.013)	(0.015)	(0.023)	(0.022)	(0.030)
		0.632**	0.689***	0.611***	-0.028*	0.088***	0.042**			
		(0.039)	(0.044)	(0.049)	(0.016)	(0.018)	(0.020)			
	Age Controls	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm
	Degree YoB Polynom	Three	Three	Three	Three	Three	Three	Three	Three	Three

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

TABLE E.40 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

MALE SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.490***	0.482***	0.477***	0.034***	0.032***	0.035**	0.069***	0.067**	0.072**
	N=52,714	(0.028)	(0.025)	(0.027)	(0.011)	(0.011)	(0.013)	(0.024)	(0.024)	(0.029)
	79-00 GHS	0.470***	0.471***	0.469***	0.030**	0.029**	0.032**	0.063**	0.061**	0.067**
	N=46,995	(0.025)	(0.024)	(0.024)	(0.012)	(0.012)	(0.014)	(0.026)	(0.026)	(0.031)
	84-98 GHS	0.427***	0.429***	0.433***	0.027	0.026	0.028	0.062	0.060	0.064
	N=28,890	(0.043)	(0.043)	(0.045)	(0.021)	(0.021)	(0.022)	(0.051)	(0.052)	(0.054)
	79-86 GHS	0.620***	0.626***	0.580***	0.020	0.041***	0.023	0.032	0.065***	0.040
	N=40,267	(0.036)	(0.036)	(0.043)	(0.014)	(0.015)	(0.017)	(0.023)	(0.022)	(0.029)
1972 Reform	79-06 GHS	0.146***	0.147***	0.151***	0.025	0.025	0.030*	0.173*	0.167*	0.198**
	N=64,460	(0.046)	(0.050)	(0.047)	(0.016)	(0.016)	(0.016)	(0.087)	(0.089)	(0.089)
	79-00 GHS	0.164***	0.164***	0.163***	0.034*	0.035*	0.036*	0.208**	0.217**	0.223**
	N=49,843	(0.053)	(0.053)	(0.055)	(0.019)	(0.019)	(0.019)	(0.100)	(0.101)	(0.105)
	84-98 GHS	0.181***	0.182***	0.180***	0.034	0.037*	0.037*	0.186*	0.204**	0.207**
	N=38,003	(0.053)	(0.053)	(0.054)	(0.021)	(0.021)	(0.022)	(0.097)	(0.096)	(0.095)
	79-86 GHS	0.327***	0.348***	0.306***	0.045***	0.090***	0.049***	0.138***	0.258***	0.159***
	N=40,267	(0.034)	(0.035)	(0.039)	(0.013)	(0.014)	(0.016)	(0.040)	(0.042)	(0.050)
1947 & 1972 Reform	79-06 GHS	0.483***	0.482***	0.486***	0.011	0.013	0.014	0.018	0.025	0.026
	N=78,363	(0.059)	(0.060)	(0.062)	(0.012)	(0.012)	(0.013)	(0.027)	(0.027)	(0.029)
		0.309***	0.306***	0.312***	0.013	0.012	0.014			
		(0.077)	(0.078)	(0.078)	(0.022)	(0.021)	(0.021)			
	79-00 GHS	0.526***	0.527***	0.526***	0.013	0.013	0.013	0.033	0.032	0.033
	N=63,746	(0.067)	(0.067)	(0.069)	(0.013)	(0.013)	(0.015)	(0.022)	(0.022)	(0.026)
		0.641***	0.642***	0.642***	0.036*	0.034*	0.035			
		(0.076)	(0.076)	(0.078)	(0.020)	(0.020)	(0.021)			
	84-98 GHS	0.503***	0.501***	0.490***	0.014	0.010	0.006	0.038	0.031	0.024
	N=44,682	(0.067)	(0.066)	(0.066)	(0.020)	(0.021)	(0.021)	(0.038)	(0.039)	(0.040)
		0.608***	0.603***	0.588***	0.034	0.029	0.026			
		(0.076)	(0.076)	(0.078)	(0.025)	(0.026)	(0.026)			
	79-86 GHS	0.524***	0.532***	0.466***	0.008	0.024*	0.007	0.042*	0.091***	0.055*
	N=40,267	(0.034)	(0.034)	(0.040)	(0.014)	(0.014)	(0.016)	(0.023)	(0.023)	(0.030)
		0.667***	0.693***	0.613***	0.046***	0.095***	0.051**			
		(0.043)	(0.044)	(0.050)	(0.017)	(0.018)	(0.020)			
	Age Controls Degree	Quadr	Quart	Dumm	Quadr	Quart	Dumm	Quadratic	Quart	Dumm
	YoB Polynom.	Four	Four	Four	Four	Four	Four	Four	Four	Four

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

TABLE E.41 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

MALE SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.494***	0.485***	0.482***	0.034***	0.032**	0.035**	0.068**	0.066**	0.072**
	N=52,714	(0.024)	(0.023)	(0.023)	(0.012)	(0.012)	(0.014)	(0.025)	(0.026)	(0.030)
	79-00 GHS	0.471***	0.471***	0.469***	0.029**	0.028**	0.032**	0.061**	0.060**	0.067**
	N=46,995	(0.024)	(0.024)	(0.023)	(0.013)	(0.014)	(0.015)	(0.028)	(0.029)	(0.033)
	84-98 GHS	0.425***	0.427***	0.432***	0.027	0.026	0.030	0.064	0.062	0.069
	N=28,890	(0.041)	(0.040)	(0.043)	(0.019)	(0.020)	(0.020)	(0.048)	(0.049)	(0.049)
	79-86 GHS	0.577***	0.579***	0.563***	0.010	0.024*	0.020	0.018	0.042*	0.035
	N=40,267	(0.036)	(0.037)	(0.043)	(0.015)	(0.015)	(0.017)	(0.025)	(0.025)	(0.030)
1972 Reform	79-06 GHS	0.296***	0.295***	0.294***	0.040**	0.039**	0.039**	0.137***	0.134**	0.134**
	N=64,460	(0.052)	(0.051)	(0.053)	(0.017)	(0.017)	(0.017)	(0.049)	(0.050)	(0.051)
	79-00 GHS	0.260***	0.260***	0.263***	0.018	0.018	0.020	0.070	0.069	0.077
	N=49,843	(0.051)	(0.051)	(0.052)	(0.026)	(0.026)	(0.026)	(0.094)	(0.094)	(0.091)
	84-98 GHS	0.280***	0.282***	0.278***	0.020	0.021	0.020	0.071	0.074	0.071
	N=38,003	(0.051)	(0.052)	(0.053)	(0.028)	(0.027)	(0.028)	(0.092)	(0.092)	(0.094)
	79-86 GHS	0.236***	0.246***	0.262***	0.031**	0.065***	0.039**	0.130**	0.263***	0.148**
	N=40,267	(0.038)	(0.039)	(0.043)	(0.015)	(0.016)	(0.017)	(0.063)	(0.066)	(0.064)
1947 & 1972 Reform	79-06 GHS	0.433***	0.426***	0.422***	0.028*	0.026*	0.024	0.038	0.040	0.037
	N=78,363	(0.062)	(0.060)	(0.062)	(0.014)	(0.014)	(0.015)	(0.035)	(0.035)	(0.036)
		0.243**	0.232**	0.228**	0.036	0.029	0.026			
		(0.093)	(0.097)	(0.099)	(0.024)	(0.024)	(0.024)			
	79-00 GHS	0.419***	0.420***	0.416***	0.015	0.014	0.013	0.038	0.035	0.035
	N=63,746	(0.056)	(0.056)	(0.059)	(0.015)	(0.015)	(0.017)	(0.036)	(0.036)	(0.041)
		0.484***	0.485***	0.482***	0.038	0.036	0.036			
		(0.078)	(0.078)	(0.081)	(0.029)	(0.029)	(0.030)			
	84-98 GHS	0.422***	0.424***	0.426***	0.010	0.009	0.011	0.028	0.026	0.030
	N=44,682	(0.065)	(0.066)	(0.068)	(0.027)	(0.027)	(0.028)	(0.065)	(0.065)	(0.067)
		0.491***	0.492***	0.495***	0.029	0.028	0.032			
		(0.088)	(0.088)	(0.091)	(0.039)	(0.039)	(0.040)			
	79-86 GHS	0.495***	0.497***	0.456***	0.001	0.012	0.005	0.017	0.050*	0.036
	N=40,267	(0.034)	(0.035)	(0.040)	(0.014)	(0.014)	(0.016)	(0.027)	(0.026)	(0.033)
		0.591***	0.607***	0.567***	0.029	0.066***	0.042**			
		(0.046)	(0.047)	(0.053)	(0.019)	(0.019)	(0.021)			
	Age Controls	Quadr	Quart	Dumm	Quadr	Quart	Dumm	Quadr	Quart	Dumm
	Degree YoB Polynomial	Five	Five	Five	Five	Five	Five	Five	Five	Five

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

TABLE E.42 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

MALE SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.454***	0.444***	0.451***	0.044***	0.042**	0.046***	0.098**	0.092**	0.101**
	N=52,714	(0.023)	(0.023)	(0.025)	(0.015)	(0.015)	(0.017)	(0.036)	(0.034)	(0.037)
	79-00 GHS	0.459***	0.458***	0.462***	0.044***	0.044***	0.047***	0.096***	0.095***	0.102***
	N=46,995	(0.032)	(0.031)	(0.031)	(0.016)	(0.016)	(0.016)	(0.034)	(0.034)	(0.036)
	84-98 GHS	0.423***	0.424***	0.434***	0.050**	0.049**	0.054**	0.116*	0.116*	0.124**
	N=28,890	(0.050)	(0.050)	(0.053)	(0.022)	(0.022)	(0.023)	(0.057)	(0.058)	(0.057)
	79-86 GHS	0.562***	0.556***	0.550***	0.012	0.006	0.015	0.021	0.012	0.027
	N=40,267	(0.040)	(0.040)	(0.046)	(0.016)	(0.016)	(0.019)	(0.029)	(0.029)	(0.033)
1972 Reform	79-06 GHS	0.298***	0.297***	0.296***	0.041**	0.040**	0.040**	0.126**	0.128**	0.120**
	N=64,460	(0.055)	(0.054)	(0.056)	(0.017)	(0.017)	(0.017)	(0.049)	(0.047)	(0.050)
	79-00 GHS	0.280***	0.280***	0.280***	0.021	0.021	0.021	0.046	0.076	0.035
	N=49,843	(0.057)	(0.057)	(0.057)	(0.025)	(0.025)	(0.025)	(0.091)	(0.085)	(0.094)
	84-98 GHS	0.290***	0.291***	0.288***	0.022	0.023	0.021	0.072	0.074	0.076
	N=38,003	(0.059)	(0.060)	(0.061)	(0.027)	(0.027)	(0.027)	(0.084)	(0.086)	(0.085)
	79-86 GHS	0.115***	0.129***	0.206***	0.040**	0.054***	0.037*	0.311*	0.425**	0.179**
	N=40,267	(0.044)	(0.045)	(0.047)	(0.018)	(0.018)	(0.019)	(0.160)	(0.175)	(0.090)
1947 & 1972 Reform	79-06 GHS	0.422***	0.417***	0.411***	0.026*	0.025*	0.022	0.036	0.040	0.037
	N=78,363	(0.054)	(0.053)	(0.054)	(0.013)	(0.013)	(0.015)	(0.034)	(0.033)	(0.035)
		0.253**	0.241**	0.237**	0.038	0.030	0.027			
		(0.097)	(0.100)	(0.102)	(0.024)	(0.024)	(0.024)			
	79-00 GHS	0.415***	0.416***	0.411***	0.017	0.016	0.015	0.043	0.040	0.040
	N=63,746	(0.053)	(0.053)	(0.055)	(0.013)	(0.013)	(0.015)	(0.032)	(0.032)	(0.038)
		0.504***	0.506***	0.505***	0.029	0.026	0.027			
		(0.080)	(0.080)	(0.082)	(0.031)	(0.031)	(0.033)			
	84-98 GHS	0.388***	0.390***	0.393***	0.018	0.018	0.022	0.049	0.047	0.054
	N=44,682	(0.054)	(0.054)	(0.056)	(0.024)	(0.024)	(0.024)	(0.063)	(0.062)	(0.063)
		0.502***	0.505***	0.505***	0.026	0.025	0.028			
		(0.076)	(0.076)	(0.080)	(0.037)	(0.036)	(0.037)			
	79-86 GHS	0.480***	0.476***	0.446***	0.007	0.004	0.004	0.017	0.013	0.024
	N=40,267	(0.037)	(0.037)	(0.042)	(0.015)	(0.015)	(0.017)	(0.031)	(0.031)	(0.037)
		0.559***	0.563***	0.545***	0.042*	0.049**	0.040*			
		(0.056)	(0.056)	(0.058)	(0.023)	(0.023)	(0.024)			
	Age Controls	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm
	Degree YoB Polynomial	Six	Six	Six	Six	Six	Six	Six	Six	Six

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

TABLE E.43 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

MALE SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.444***	0.437***	0.439***	0.041***	0.040***	0.043***	0.091***	0.091***	0.120*
	N=52,714	(0.019)	(0.020)	(0.021)	(0.014)	(0.014)	(0.015)	(0.031)	(0.032)	(0.063)
	79-00 GHS	0.446***	0.446***	0.449***	0.042***	0.041***	0.044***	0.086***	0.087**	0.098**
	N=46,995	(0.022)	(0.022)	(0.023)	(0.014)	(0.014)	(0.015)	(0.031)	(0.033)	(0.036)
	84-98 GHS	0.393***	0.394***	0.403***	0.050**	0.049**	0.054***	0.128	0.092*	0.152*
	N=28,890	(0.040)	(0.041)	(0.043)	(0.019)	(0.019)	(0.019)	(0.084)	(0.048)	(0.082)
	79-86 GHS	0.476***	0.472***	0.481***	0.021	0.018	0.016	0.001	0.007	0.000
	N=40,267	(0.045)	(0.045)	(0.049)	(0.018)	(0.018)	(0.020)	(0.027)	(0.027)	(0.000)
1972 Reform	79-06 GHS	0.300***	0.299***	0.298***	0.040**	0.039**	0.039**	0.127**	0.137**	0.134**
	N=64,460	(0.055)	(0.055)	(0.057)	(0.018)	(0.019)	(0.019)	(0.048)	(0.056)	(0.053)
	79-00 GHS	0.275***	0.275***	0.276***	0.019	0.019	0.020	0.099	0.098	0.083
	N=49,843	(0.057)	(0.057)	(0.057)	(0.026)	(0.026)	(0.026)	(0.081)	(0.076)	(0.078)
	84-98 GHS	0.287***	0.289***	0.286***	0.020	0.021	0.019	0.083	0.068	0.064
	N=38,003	(0.058)	(0.058)	(0.060)	(0.028)	(0.028)	(0.029)	(0.080)	(0.085)	(0.089)
	79-86 GHS	0.203***	0.214***	0.226***	0.039**	0.052***	0.037*	0.008	0.022	0.055
	N=40,267	(0.045)	(0.045)	(0.047)	(0.018)	(0.018)	(0.019)	(0.036)	(0.034)	(0.045)
1947 & 1972 Reform	79-06 GHS	0.414***	0.409***	0.407***	0.023	0.021	0.021	0.037	0.299	0.156***
	N=78,363	(0.064)	(0.062)	(0.063)	(0.016)	(0.016)	(0.018)	(0.035)	(6.034)	(0.045)
		0.251**	0.239**	0.236**	0.037	0.029	0.027			
		(0.098)	(0.101)	(0.103)	(0.024)	(0.024)	(0.025)			
	79-00 GHS	0.360***	0.361***	0.358***	0.019	0.018	0.019	0.054	0.580	0.048
	N=63,746	(0.052)	(0.052)	(0.053)	(0.017)	(0.017)	(0.018)	(0.042)	(1.489)	(0.036)
		0.500***	0.502***	0.501***	0.029	0.027	0.027			
		(0.073)	(0.072)	(0.075)	(0.031)	(0.031)	(0.033)			
	84-98 GHS	0.359***	0.361***	0.365***	0.020	0.020	0.024	0.055	0.050	0.095
	N=44,682	(0.052)	(0.053)	(0.055)	(0.026)	(0.026)	(0.026)	(0.094)	(0.059)	(0.077)
		0.500***	0.502***	0.504***	0.026	0.025	0.029			
		(0.074)	(0.074)	(0.078)	(0.037)	(0.037)	(0.038)			
	79-86 GHS	0.384***	0.381***	0.374***	0.010	0.009	0.003	0.007	0.014	0.026
	N=40,267	(0.041)	(0.041)	(0.044)	(0.017)	(0.017)	(0.018)	(0.028)	(0.028)	(0.032)
		0.502***	0.507***	0.496***	0.044*	0.051**	0.039			
		(0.057)	(0.057)	(0.059)	(0.023)	(0.023)	(0.024)			
	Age Controls	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm
	Degree YoB Polynomial	Seven	Seven	Seven	Seven	Seven	Seven	Seven	Seven	Seven

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

TABLE E.44 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

MALE SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	0.445***	0.439***	0.439***	0.035**	0.034**	0.037**	0.100**	0.091**	0.099***
	N=52,714	(0.022)	(0.022)	(0.022)	(0.015)	(0.015)	(0.016)	(0.043)	(0.031)	(0.035)
	79-00 GHS	0.446***	0.446***	0.449***	0.037**	0.037**	0.039**	0.092***	0.090***	0.077
	N=46,995	(0.025)	(0.025)	(0.026)	(0.015)	(0.015)	(0.016)	(0.031)	(0.029)	(0.075)
	84-98 GHS	0.401***	0.401***	0.409***	0.045*	0.044*	0.050**	0.137	0.100*	0.116**
	N=28,890	(0.049)	(0.049)	(0.051)	(0.023)	(0.023)	(0.022)	(0.093)	(0.055)	(0.053)
	79-86 GHS	0.463***	0.459***	0.467***	0.020	0.016	0.017	-0.009	0.000	0.000
	N=40,267	(0.046)	(0.046)	(0.049)	(0.018)	(0.018)	(0.020)	(0.042)	(0.000)	(0.000)
1972 Reform	79-06 GHS	0.298***	0.298***	0.293***	0.033	0.033	0.030	0.083	0.104*	0.158
	N=64,460	(0.069)	(0.070)	(0.070)	(0.026)	(0.026)	(0.025)	(0.137)	(0.052)	(0.543)
	79-00 GHS	0.333***	0.333***	0.333***	0.014	0.014	0.012	0.007	0.038	0.051
	N=49,843	(0.064)	(0.064)	(0.064)	(0.035)	(0.035)	(0.035)	(0.161)	(0.170)	(0.087)
	84-98 GHS	0.371***	0.374***	0.372***	0.013	0.013	0.013	0.270	0.036	0.059
	N=38,003	(0.067)	(0.068)	(0.070)	(0.038)	(0.038)	(0.039)	(1.481)	(0.095)	(0.272)
	79-86 GHS	0.173***	0.184***	0.195***	0.041**	0.052***	0.043**	0.002	0.000	0.043
	N=40,267	(0.047)	(0.047)	(0.050)	(0.019)	(0.019)	(0.020)	(0.033)	(0.000)	(0.039)
1947 & 1972 Reform	79-06 GHS	0.371***	0.370***	0.369***	0.031	0.029	0.029	0.017	0.028	0.049
	N=78,363	(0.059)	(0.059)	(0.061)	(0.018)	(0.018)	(0.020)	(0.022)	(0.031)	(0.141)
		0.202**	0.194*	0.192*	0.045*	0.038	0.036			
		(0.098)	(0.105)	(0.108)	(0.024)	(0.024)	(0.024)			
	79-00 GHS	0.336***	0.336***	0.333***	0.027	0.027	0.027	0.040	0.024	0.003
	N=63,746	(0.051)	(0.051)	(0.052)	(0.017)	(0.017)	(0.019)	(0.032)	(0.030)	(0.029)
		0.475***	0.476***	0.475***	0.038	0.036	0.035			
		(0.066)	(0.065)	(0.068)	(0.027)	(0.027)	(0.029)			
	84-98 GHS	0.325***	0.325***	0.331***	0.038	0.039	0.042	0.000	-0.013	0.218
	N=44,682	(0.052)	(0.052)	(0.056)	(0.028)	(0.028)	(0.028)	(0.000)	(0.036)	(0.248)
		0.468***	0.469***	0.472***	0.043	0.043	0.045			
		(0.066)	(0.066)	(0.070)	(0.034)	(0.034)	(0.034)			
	79-86 GHS	0.376***	0.373***	0.365***	0.010	0.008	0.004	0.006	0.000	0.000
	N=40,267	(0.041)	(0.041)	(0.044)	(0.017)	(0.017)	(0.018)	(0.027)	(0.000)	(0.000)
		0.482***	0.486***	0.472***	0.043*	0.050**	0.042*			
		(0.058)	(0.058)	(0.060)	(0.023)	(0.023)	(0.024)			
	Age Controls	Quadr	Quartic	Dumm	Quadr	Quartic	Dumm	Quadr	Quart	Dumm
	Degree YoB Polynomial	Eight	Eight	Eight	Eight	Eight	Eight	Eight	Eight	Eight

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

TABLE E.45 – REDUCED FORM AND 2SLS EFFECTS OF ROSLA LAWS ON SCHOOLING AND LOG WEEKLY EARNINGS

MALE SAMPLE

		1st Stage: Schooling			Reduced Form: Weekly Earnings			2SLS: Weekly Earnings		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1947 Reform	79-06 GHS	1.160***	1.171***	1.156***	0.160***	0.160***	0.160***	0.161***	0.132	0.158***
	N=52,714	(0.035)	(0.031)	(0.031)	(0.010)	(0.012)	(0.014)	(0.007)	(0.156)	(0.008)
	79-00 GHS	1.730***	1.741***	1.009***	0.445***	0.447***	0.189***	0.229***	0.459	0.226***
	N=46,995	(0.023)	(0.022)	(0.014)	(0.014)	(0.015)	(0.015)	(0.008)	(0.670)	(0.010)
	84-98 GHS	1.587***	1.101***	0.923***	0.187***	0.038	0.057**	0.102***	-0.374	0.107***
	N=28,890	(0.040)	(0.031)	(0.027)	(0.024)	(0.028)	(0.025)	(0.012)	(0.869)	(0.017)
	79-86 GHS	0.607**	0.725**	1.509**	0.315***	0.386***	0.687***	0.257	1.972	0.375*
	N=40,267	(0.298)	(0.301)	(0.612)	(0.120)	(0.121)	(0.246)	(0.190)	(266.922)	(0.196)
1972 Reform	79-06 GHS	2.153***	2.159***	2.144***	0.354***	0.360***	0.355***	0.153***	2.058	0.154***
	N=64,460	(0.036)	(0.042)	(0.045)	(0.007)	(0.006)	(0.008)	(0.004)	(12.244)	(0.005)
	79-00 GHS	0.276***	0.278***	0.280***	0.045***	0.038***	0.039***	0.280***	-8.118	0.115***
	N=49,843	(0.007)	(0.010)	(0.011)	(0.005)	(0.006)	(0.006)	(0.012)	(52.398)	(0.004)
	84-98 GHS	0.298***	0.257***	1.100***	0.044***	0.023**	0.178***	0.163***	-1.133	0.080***
	N=38,003	(0.010)	(0.015)	(0.032)	(0.006)	(0.009)	(0.020)	(0.016)	(1.678)	(0.006)
	79-86 GHS	1.383**	1.489**	1.523**	0.355	0.439*	0.572**	0.257	0.295	0.375*
	N=40,267	(0.628)	(0.629)	(0.632)	(0.253)	(0.253)	(0.254)	(0.190)	(0.183)	(0.196)
1947 & 1972 Reform	79-06 GHS	-0.094***	-0.093***	-0.096***	0.023***	0.024***	0.024***	-0.094***	-0.067**	-0.060**
	N=78,363	(0.000)	(0.001)	(0.003)	(0.000)	(0.000)	(0.001)	(0.027)	(0.027)	(0.026)
		-0.374***	-0.382***	-0.390***	0.049***	0.043***	0.041***			
		(0.058)	(0.069)	(0.072)	(0.009)	(0.010)	(0.009)			
	79-00 GHS	-0.096***	-0.096***	-0.098***	0.023***	0.023***	0.024***	0.046	0.051	0.094
	N=63,746	(0.000)	(0.000)	(0.003)	(0.000)	(0.000)	(0.001)	(0.292)	(0.295)	(0.326)
		-0.078**	-0.075**	-0.073**	0.060***	0.057***	0.059***			
		(0.030)	(0.029)	(0.028)	(0.009)	(0.009)	(0.010)			
	84-98 GHS	0.003***	0.003***	0.000	0.090***	0.091***	0.093***	2.143	2.117	2.620
	N=44,682	(0.001)	(0.001)	(0.004)	(0.000)	(0.001)	(0.003)	(4.716)	(5.084)	(8.508)
		0.017	0.016	0.010	0.120***	0.117***	0.119***			
		(0.029)	(0.029)	(0.030)	(0.010)	(0.009)	(0.011)			
	79-86 GHS	-0.131	-0.131	-0.128	-0.042	-0.040	-0.042	-0.197	-0.205	-0.214
	N=40,267	(0.133)	(0.133)	(0.133)	(0.054)	(0.054)	(0.054)	(0.356)	(0.360)	(0.370)
		-0.194	-0.194	-0.190	0.005	0.007	0.007			
		(0.147)	(0.147)	(0.147)	(0.059)	(0.059)	(0.059)			
	Age Controls Degree	Quadr	Quart	Dumm	Quadr	Quart	Dumm	Quadr	Quart	Dumm
	YoB Polynomial	Dumm	Dumm	Dumm	Dumm	Dumm	Dumm	Dumm	Dumm	Dumm

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

Appendix F. New Parametric Results - Pooled Sample

TABLE F.46 - 2SLS EFFECTS OF ROSLA LAWS ON LOG WEEKLY EARNINGS - POOLED SAMPLE

	2SLS Estimates – Order of Month-Year of Birth Polynomial									
	0	1	2	3	4	5	6	7	8	Dummies
1947 Ref. N=49,863	0.100*** (0.015)	0.214*** (0.012)	0.216*** (0.012)	0.218*** (0.012)	0.215*** (0.012)	0.215*** (0.012)	0.215*** (0.012)	0.215*** (0.012)	0.222*** (0.012)	0.196*** (0.012)
1972 Ref. N=72,906	0.296*** (0.021)	0.323*** (0.012)	0.322*** (0.012)	0.317*** (0.012)	0.317*** (0.012)	0.313*** (0.012)	0.314*** (0.012)	0.314*** (0.012)	0.310*** (0.012)	0.275*** (0.010)
1947 & 1972 Ref. – Old IV N=81,041	0.221*** (0.015)	0.276*** (0.010)	0.276*** (0.010)	0.275*** (0.010)	0.275*** (0.010)	0.274*** (0.010)	0.273*** (0.010)	0.273*** (0.010)	0.270*** (0.010)	0.240*** (0.009)
1947 & 1972 Ref. – New IV N=81,041	0.222*** (0.012)	0.275*** (0.010)	0.267*** (0.010)	0.266*** (0.010)	0.266*** (0.010)	0.266*** (0.010)	0.260*** (0.009)	0.260*** (0.009)	0.257*** (0.009)	0.241*** (0.009)

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix G. Bounds Analysis – test of MIV assumption and Pooled Sample results

TABLE G.47 – MEAN WEEKLY WAGE BY VALUE OF IV

Date of Birth	Weekly Earnings Pooled Sample	Weekly Earnings Male Sample
IV=0	5.242	5.659
IV=1	5.369	5.911
IV=1.33	5.423	5.944
IV=2	5.497	5.890
IV=2.33	5.475	5.892

TABLE G.48 - ETS POINT ESTIMATES AND NON-PARAMETRIC BOUNDS ON RETURNS TO EDUCATION

Pooled Sample	ETS β	MTR-MTS		MTR-MTS-MIV	
		Lower Bound	Upper Bound	Lower Bound	Upper Bound
Δ(14, 15)	0.043	0	0.248	0	0.196
	(0.023, 0.064)	(0	0.268)	(0	0.246)
Δ(15, 16)	0.207	0	0.300	0	0.274
	(0.194, 0.219)	(0	0.310)	(0	0.295)
Δ(16, 17)	0.216	0	0.348	0	0.337
	(0.196, 0.235)	(0	0.364)	(0	0.358)
Δ(17, 18)	0.248	0	0.508	0	0.483
	(0.227, 0.27)	(0	0.523)	(0	0.508)
Δ(18, 19)	0.133	0	0.604	0	0.409
	(0.092, 0.174)	(0	0.643)	(0	0.508)
Summary Effects:					
Δ(14, 16)	0.250	0	0.340	0	0.276
	(0.228, 0.272)	(0	0.361)	(0	0.327)
Δ(16, 19)	0.597	0	0.692	0	0.494
	(0.557, 0.637)	(0	0.731)	(0	0.593)
Δ(14, 19)	0.847	0	0.847	0	0.5995
	(0.805, 0.890)	(0	0.890)	(0	0.711)

NOTE – Dependent variable is ln(weekly wage). Numbers between parentheses are Imbens-Manski 90% confidence intervals. Number of observations is 80,114.