GENDER DIFFERENCES IN THE LIFECYCLE BENEFITS OF COMPULSORY SCHOOLING POLICIES

Sonja C. de New[1,2], Stefanie Schurer[3,2], Dominique Sulzmaier[4]

1. Centre for Health Economics, Monash University, Australia
2. IZA Institute of Labor Economics, Germany
3. School of Economics, The University of Sydney, Australia
4. Friedrich-Alexander University Erlangen-Nürnberg, Germany

The final version of this manuscript can be found at the journal website here: https://doi.org/10.1016/j.euroecorev.2021.103910

Please cite this article as below:

de New, S.C., Schurer, S., Sulzmaier, D. (2021), Gender differences in the lifecycle benefits of compulsory schooling policies, *European Economic Review*, Volume 140,103910, https://doi.org/10.1016/j.euroecorev.2021.103910.

Australian Government

Australian Research Council

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Sonja C. de New

Centre for Health Economics, Monash University and Institute for the Study of Labor (IZA)

Stefanie Schurer

School of Economics, The University of Sydney and Institute for the Study of Labor (IZA)

Dominique Sulzmaier

Friedrich-Alexander University Erlangen-Nürnberg

October 15, 2021

We estimate the lifecycle benefits of policies that raise the minimum school leaving age (MSLA). Using a difference-in-differences method, we estimate the causal impact of two adjacent Australian state reforms that extended the MSLA from 14 to 15 in mid 1960. Important gender and state differences emerge in how the reforms affected secondary and postsecondary education outcomes. The biggest winners were women in Victoria, for whom the reform increased postsecondary education, while the reform lifted only minimum schooling qualifications in South Australia. As a consequence, the Victorian reform improved the lifecycle capital accumulation process especially for women, while few benefits were observed for South Australians. Victorian women entered higher-skilled occupations, were more likely to own homes, to be still married and satisfied with family life in pre-retirement age. Victorian men also gained, but the gains were limited to better cognitive and non-cognitive skills, health, and satisfaction with (family) life. Yet, all groups benefitted from delayed and reduced fertility, and a happier family life. We conclude that raising education levels for individuals at the lower end of the education spectrum produces lifecycle benefits that exceed market-return considerations, but major benefits occur only if the reform impacts education outcomes beyond minimum schooling.

Keywords: Minimum school leaving age, education reform, lifecycle capital accumulation, non-cognitive skills, cognitive skills, marital quality, wealth, health.

JEL Codes: I20, I26, J24

Correspondence: Stefanie Schurer, School of Economics, The University of Sydney, Sydney, Australia, Tel: 61-(0)2-935 185 32, Email: stefanie.schurer@sydney.edu.au

The authors acknowledge support from an Australian Research Council Discovery Grant (DP190102765) and an Australian Research Council Discovery Early Career Award (DE140100463). We are grateful for generous comments provided by two anonymous referees, Regina Riphahn, participants of the Annual Conferences of the European Society for Population Economics 2018 (Antwerp), the German Economic Association 2018 (Freiburg), and the European Economic Association 2018 (Cologne).

1 Introduction

Governments of many countries have reformed their compulsory schooling policies to increase the quantity of schooling of marginalized groups. Lifting the minimum school leaving age forces some children to stay in school longer than they would have stayed in the absence of such legislation. Although paternalistic in nature and costly to implement, restricting the choice set of children and their families is often justified by the objective of reducing social inequalities and harm associated with lower levels of education (see Harmon, 2017, for a discussion).

The minimum school leaving age (MSLA) has been continuously raised since the 1940s in Western economies. Today, the debate and policy practice around the MSLA is still very topical. In most recent cases, the MSLA was raised to age 17 or even 18, obliging students to be either in education or training until the MSLA is reached. In the past 20 years, the MSLA has been raised in more than one third of all states in the United States (Diffey & Steffes, 2017; Stillman & Blank, 2000), in all Australian states (Australian Curriculum, Assessment and Reporting Authority, 2009), in the United Kingdom (Government of the United Kingdom, 2008) and several other European countries (UNESCO Institute for Statistics, 2018).

Raising the MSLA comes at a high cost to society because it requires the provision of additional teaching capacity and an ability of the school system to absorb potentially unmotivated and disruptive students (Harmon, 2017).[1] Moreover, not everyone agrees with the usefulness of their policy objectives. Some call it a "futile" attempt to force children at the margin to stay longer in school, considering the high opportunity cost of foregone learning on the job (McCulloch, 2014). In light of the frequent application of this policy tool around the world, it is critical to understand its opportunity costs and wider consequences for society.

In this study, we analyze the wider consequences to society of increasing the MSLA. We quantify the lifecycle benefits of two policy changes that raised the MSLA by one year from age 14 to 15 in mid 1960 in South Australia and Victoria, two comparable states located in the South of Australia. These reforms are of high scientific value. First, affected individuals have reached retirement age today. Thus, we are able to evaluate the impact of this policy beyond

[1] For instance, the 2010 Australian MSLA reform in New South Wales increased annual expenditures for the Department of Education by 100 million Australian dollars, equivalent to 1% of permanent expenditures (Harmon, 2017).

its immediate impact on education and shorter-term labor market outcomes. Second, the two reforms affected one in five pupils, a non-negligible fraction of Australian youth. Third, the two reforms occurred during a time window of relatively little other changes in the education system in comparison to other Australian states. They were responses to a large cohort of students entering the education system due to the baby-booming years triggered by large immigration inflows in the 1950s (Connell, 1993). As the reforms were introduced quickly, their exact dates of introduction provide a relatively clean natural experiment to identify causal impact estimates (see Section 2).

We focus our analysis on estimating both the market (e.g., wages, labour supply, wealth) and non-market returns (e.g., cognitive and non-cognitive skills, marital quality, fertility, health) of the reform. As outcome measures we consider an individual's full portfolio of labour market histories, older-age skills, wealth, health, and family relationships, an observable summary measure of success in life. We refer to this as the *diversified capital stock* observed at the end of productive life. This diversified capital stock is the outcome of a complex human capital accumulation process over the lifecourse. MSLA reforms may permanently alter the path of this accumulation process.

The analysis is conducted with high-quality, nationally representative data sourced from the Household, Income and Labour Dynamics in Australia survey (referred to as *HILDA*). The advantage of our data is that we do not only observe a large number of later-life outcomes in all domains of life, but the state in which the individual graduated from high school and her exact birth date. Both pieces of information are important because they are needed to correctly and exactly assign an individual to the treatment or control group.

Our empirical strategy relies on a difference-in-differences (DiD) model which identifies, at least in theory, the average treatment effect on the treated (ATT) (Athey & Imbens, 2006, p. 436). In practice however, the effect is the intention-to-treat estimate, because a small proportion of the population did not comply with the reform.[2] One advantage of this approach over other approaches often used in this literature is that the ATT takes into account potential changes in the whole education distribution rather than just locally around the minimum

[2] Non-compliance is common in compulsory schooling reforms. Non-compliance in the Australian case is no different from non-compliance in MSLA reforms in European countries (Harmon, 2017).

schooling threshold. This acknowledges that the reform may not only have impacted those who were at the margin of leaving school in the mid-1960s – the so-called compliers – but also those students, who would have continued school, even in the absence of the reform. These so-called always-takers are potentially affected by the reform through larger class size, lower teaching quality, and a different rank order within the class.[3] The differences in outcomes between reform-affected and -unaffected cohorts are compared against the differences between the same birth cohorts in two comparable states – New South Wales and the Australian Capital Territory – where such reforms were already implemented in the early 1940s. We discuss and test carefully the assumptions under which our DiD approach yields causal impact estimates of the MSLA reforms. For comparisons, we also provide estimates of the local average treatment effect, following previous studies in the Australian (Leigh & Ryan, 2008; Li & Powdthavee, 2015) or international context (e.g. Clark & Royer, 2013; Kemptner et al., 2011). Our estimates are robust to alternative modelling assumptions.

An extensive literature exists that exploits MSLA reforms to study the causal impact of education in general on a variety of outcomes. Studies on the returns to education generally find that an exogenous increase in education caused by MSLA reforms raises labor-market income (e.g., Aakvik et al., 2010; Angrist & Krueger, 1991; Brunello et al., 2009; Card, 2001; Devereux & Hart, 2010; Grenet, 2013; Harmon & Walker, 1995; Kamhöfer & Schmitz, 2016; Leigh & Ryan, 2008; Meghir & Palme, 2005; Oosterbeek & Webbink, 2007; Oreopoulos, 2006a; Pischke & von Wachter, 2008) and other financial outcomes (Cole et al., 2014). It also reduces unemployment (Li, 2006) and labor mobility (Machin et al., 2012). There is a particularly large body of research on the impact of education on physical health (e.g., Mazzonna, 2014; Chatterji, 2014; Clark & Royer, 2013; Kemptner et al., 2011; Lleras-Muney, 2005; Oreopoulos, 2007), which includes also broader outcomes such as health behaviors (Li & Powdthavee, 2015; Kemptner et al., 2011), fertility (Black et al., 2005; Cygan-Rehm & Mäder, 2013), mental health (Crespo

[3] Anecdotal evidence in Australia suggests that "...the raising of the minimum school leaving age meant that the proportion of uninterested adolescents in secondary schools passed the critical point and the problems of the traditional type of secondary education were intensified. Some children drift through school, a Victorian teacher wrote, their age being the sole qualification for promotion. 'Some of these children accepted this state quietly enough, but not a few rebelled, their rebellion taking the form of opposition to authority... This behavior was not confined to the junior forms, but was felt very strongly in Forms Three and Four'" (Barcan, 1980, p.314).

et al., 2014; Oreopoulos, 2007; Courtin et al., 2019), and cognition (Courtin et al., 2019; Crespo et al., 2014; Schneeweis et al., 2014). Finally, some studies provide evidence that higher population education levels reduce crime (Lochner & Moretti, 2004; Machin et al., 2011) and slow down the transmission of disadvantage across generations (e.g. Black & Devereux, 2011; Black et al., 2005, 2008; Oreopoulos et al., 2006).

Despite a mature development in this literature, some critical questions have remained unanswered. First, little is known about who benefits or is harmed most by compulsory schooling reforms. Oreopoulos & Salvanes (2011) highlight that "it's worth remembering that the relationships between schooling and life outcomes ... are averaged over some individuals who benefit more and some less. This makes assessing potential returns to schooling for subgroups complicated" (p. 179). Thus, providing analyses by subgroups, for instance by gender which seems to be "under-researched" (see Kemptner et al., 2011, p. 352), is useful to improve our understanding of the broader benefits of MSLA reforms. Furthermore, it is unclear where in the lifecycle benefits occur. It cannot be assumed that compulsory schooling policies generate positive returns at all ages. Bhuller et al. (2011), for example, showed for Norway that wage returns are maximised at middle-age. Potential lifecycle heterogeneity may also be problematic from a methodological perspective. Buscha & Dickson (2015) demonstrate that differences in accumulated experience over the lifecycle, if left unaccounted, are likely to lead to severe under-estimates of the wage returns for the compliers of MSLA reforms. This could explain the variation in results presented in the literature, typically with respect to the wage returns of education (see Buscha & Dickson, 2012, 2015 and the references therein). On the other hand, most previous work has focused almost exclusively on one single outcome that was changed as a consequence of compulsory schooling reforms. A notable exception is Oreopoulos & Salvanes (2011) who document the effect of MSLA reforms on both market and non-market outcomes.[4] The authors suggest that "future work on nonpecuniary returns to schooling should aim to bring together the broad array of evidence...The possibility that schooling affects preferences, we believe, is a particularly worthy avenue for future research" (Oreopoulos & Salvanes, 2011, p. 162).

[4] The study focuses on a wide array of outcomes including wages, job characteristics, time spent in jail or mental institution, voting behaviour, satisfaction with life, fertility and mortality.

Taking into account the trade-offs between various outcomes is a key concern. For example, while MSLA reforms may improve labour market opportunities, they may come at the cost of strained family relationships and psychological pressure. Forcing low-ability or unmotivated pupils to stay in school increases pressure associated with testing and sitting still in class. Such pressures in adolescence could have long-term consequences on mental health, interacting with other aspects of people's lives, such as the ability to maintain relationships. Avendano et al. (2017) showed for example that the 1972 British compulsory schooling reform increased the prevalence of depression in adulthood. In a comprehensive review on what we have learnt from compulsory school leaving reforms, Harmon (2017) concludes that the usefulness of MSLA reforms depends on "the wider benefits of the increase in schooling" (p. 1).

We contribute to this previous literature by providing a comprehensive view on the wider benefits of increasing schooling in the context of two Australians MSLA reforms. Where we add to the literature is a focus on both the market and non-market benefits as they are observed at the end of productive life. We are not the first to study outcomes at the end of productive life (see Schneeweis et al., 2014; Crespo et al., 2014, for other examples) or the differential impacts on men and women (Meghir & Palme, 2005; Kemptner et al., 2011; Fischer et al., 2020). But we are the first to study heterogeneity in the accumulated capital across five domains at the end of productive life. As we study two reforms that were implemented around the same time in two neighbouring and comparable Australian states, we are able to compare the effectiveness of the reform across the two jurisdictions, allowing for heterogeneity in the education system to deal with more pupils and labour markets to absorb a workforce with more schooling. We depart from the perspective that the immediate effect of an additional year of schooling for those at the margin is likely to build pupils' human capital in the broadest sense. By human capital we do not only mean educational qualification but the formation of labour-market relevant cognitive and non-cognitive skills. Previous research has demonstrated that high-school and tertiary education shapes non-cognitive skill development of youth both at the intensive and extensive margin (see Schurer, 2017; Kassenboehmer et al., 2018). Better skills in combination with better formal training are likely to impact lifetime outcomes multidimensionally. They may alter occupational and family formation choice, and thus may result in higher wages, wealth, and health and human

capital in older age. While the literature has studied the impact of MSLA reforms on cognitive decline in old age (Crespo et al., 2014; Schneeweis et al., 2014), we are the first to provide estimates on the non-cognitive returns measured in older age.[5]

Our findings reveal that the reform benefitted women more than men in the long-run. The biggest winners were women in Victoria, the larger of the two states. The reason for the beneficial effects is that the reform increased not only years spent in school but also the total number of years in education, disproportionately so for women, in Victoria. In Victoria the reform helped a larger share of women to shift away from minimum schooling (Year 10) into completing high school (Year 12). Completion of Year 12 is an education marker that allows entry into higher-skilled vocational training and university education. Thus, in Victoria, we observe a larger share of women to transition from no post-secondary training (e.g. just complete Year 11 or 12) into obtaining post-secondary schooling qualifications, in particular university training. As a consequence, women in Victoria were disproportionately more likely to enter professional occupations and exit manual labour occupations. These women were disproportionately more likely to own a house at the end of productive life and to be wealthy in general, and the only group to be more likely to still be married in pre-retirement age.

The reform also benefitted men in Victoria, who faced the same labour market conditions as women in Victoria, but in different aspects of their lives. The reform did not affect their labour market outcomes or wealth, but it boosted their cognitive and non-cognitive skills, brought them better educated wives, and improved their health and life satisfaction. The main losers of the reform were men in South Australia. This is the only group that did not experience a significant increase in total years of education. The MSLA reform lifted minimum schooling for these men and thus the years spent in high school. The reform harmed men in South Australia in terms of their non-cognitive skill development and labour market outcomes. This is particularly interesting, as women in South Australia, who faced the same labour market conditions, were not harmed in the same way. They experienced boosts to their years spent in education, their wealth and probability to work in a professional occupation.

[5] The only paper we know of currently is Lager et al. (2016), who estimated the impact of a Swedish compulsory schooling reform on late adolescent intelligence and emotional control, a non-cognitive measure collected for military conscription.

Finally, our heterogeneity analysis reveals that the reform affected fertility decisions for all groups in similar ways. It both delayed and reduced fertility. All groups were older at first marriage and at first child, and they all had fewer children. This is a positive finding as it suggests that an additional year spent in school – even if it did not lead to better qualifications – does have the effect to delay child birth, potentially at young age. All four groups were also more satisfied with their children at retirement age. Although not a lot of evidence exists on the differential impacts of MSLA reforms by gender, some of our findings are in line with the limited previous literature, others are unique. In Section 8 we discuss how our findings relate to the broader literature.

The remainder of the paper proceeds as follows. Section 2 provides an overview of the Australian education system, the details of the MSLA reforms and a literature review on how such reforms may impact the lifecycle capital accumulation process. Section 3 describes the empirical strategy and discusses the identification assumptions. In Section 4 we describe the HILDA data and relevant variables. All results are presented in Section 5. Section 6 discusses and tests the validity of the identifying assumptions and shows various robustness checks, such as the sensitivity of the results of the baseline model to alternative specifications. We discuss our results in Section 8. An Appendix provides supplementary material.

2 Institutional background and literature

2.1 Australian education system in the 1960s

Australia is a federated country, divided into six states and two territories. Schooling reforms are legislated, implemented, and administered at the state level. Despite this decentralized system of education, the education systems across states are similar in nature. In the 1960s, the schooling system offered twelve years of education in all states and territories. In most states, students attended primary school for the first seven years of their schooling career followed by up to five years in secondary school.[6] At the time, schooling was compulsory from the age of six to the age of 14 to 16, depending on the state and territory. Students could voluntarily continue secondary

[6] In New South Wales and the Australian Capital Territory, the first year of primary school was called kindergarten. Victoria and Tasmania had six years of primary school followed by six years of secondary school.

education up to grade 12, which they would complete around the age of 18 and which would allow them to study at university (Connell, 1993). It was however not common to complete secondary school training in the 1960s. A high share of students left shortly after reaching the MSLA. Most students would only complete Year 10, the year level that was implied by the MSLA. Only a small fraction would complete Year 12 and most of them would have come from economically privileged families (Karmel et al., 1985).

Because of the baby-booming years and high levels of immigration, the Australian education system faced a large inflow of students from the 1950s onward (Campbell & Proctor, 2014, p.179). To help prevent a potentially larger share of baby-booming students from entering unemployment, a number of states increased the MSLA to improve their human capital (Connell, 1993).

2.2 Minimum school leaving age reforms

In almost all countries worldwide, school attendance is not voluntary, but legally prescribed. Compulsory school attendance laws set the maximum age by which children must start school and the minimum age at which they may drop out. Between the 1940s and the 1960s, the minimum school leaving age was raised from 14 to 15 years in all Australian states and territories. These changes meant that individuals born 14 or less years before the proclamation date were required to remain in school for one extra year relative to those born more than 14 years before the reform. For example, the proclamation date in the state of Victoria was February 4, 1964, so all children born on or after February 4, 1950 would be affected by the reform and would be required to receive an additional year of schooling relative to those born before February 4, 1950. In practice, the reform implied that pupils had to stay until the end of Year 9 or 10, depending on the age they had entered school.

In Table 1 we present detailed information on the relevant MSLA reforms for all states. We focus our analysis on four states: Victoria (VIC) and South Australia (SA), which we consider as treatment states, and New South Wales (NSW) and the Australian Capital Territory (ACT), which we consider as control states. SA and VIC implemented MSLA reforms in the 1960s, raising their MSLA from 14 to 15 in 1963 and 1964, respectively. NSW and the ACT, whose

schools were under the NSW system over the reform period, had increased the MSLA from 14 to 15 between 1941 and 1943. Thus, when VIC and SA increased their MSLA in the 1960s, NSW and ACT had such policies already in place 20 years prior. Consequently, in our analysis we measure the impact of an extra year of schooling for children who were affected by the 1960s reforms in SA and VIC, bench-marking them against children in NSW and ACT, who had already benefited from such reforms.[7]

Choosing SA and VIC as treatment states has many advantages. Both are located next to each other in the Southern part of the continent. Both states have had similar settlement histories[8] and have had similar socioeconomic and demographic compositions in the 1960s. The MSLA reforms also occurred around the same time and were similar in nature. Victoria is the larger of the two states with a population of almost 3 million in 1960, while South Australia had a population of less than 1 million.[9] What we do not know is whether police in the two treatment states enforced the reforms and school attendance differently. However, already the South Australian Education Act 1915 and Victorian Education Act 1958 explicitly state in similar wording that parents face a penalty if their child misses school. Similarly, both Education Acts state that employers face a penalty if they employ a child that is required to be enrolled in school during school hours. This suggests that both states experienced similar attitudes and legal foundations for law enforcement.

[7] We exclude from the analysis other states and territories for the following reasons. Tasmania (TAS) is excluded because it raised its MSLA from 14 to 16 (rather than 15) in 1946, and there were a large number of granted individual exemptions (Barcan, 1980). We excluded Northern Territory (NT) schools because the NT was at the time too sparsely populated with just 26,000 residents and a too small number of students available in our data (data sourced from the Australian Bureau of Statistics, accessed on 20 July 2020 at https://www.abs.gov.au/ausstats/abs@.nsf/Lookup/by%20Subject/1301.0~2012~Main%20Features~Population%20size%20and%20growth~47). When the NT lifted its MSLA from 14 to 15 in 1965, only a couple of permanent high schools existed. We excluded Queensland (QLD) from our analysis because its 1965 MSLA reform took place in the midst of other changes in the education system, making the relative effect of the MSLA reform difficult to isolate. For example, automatic progression into high school was implemented in 1963 and the age of transfer from primary to secondary school was decreased from 14 to 13 at the same time as the compulsory attendance age increased from 14 to 15 (Campbell & Proctor, 2014). Additionally, QLD was in the process of transitioning to a comprehensive schooling system during the time of the compulsory attendance reforms, a conversion that was completed by the end of the 1960s. Finally, we exclude Western Australia because its MSLA was increased in stages between 1963 and 1966. The reform first changed the MSLA to the end of the school year in which the student turned 14. Hence, students in WA were no longer able to leave school on their 14th birthday. In a second step the reform changed the MSLA to the end of the year in which the student turned 15.

[8] Both states were settled in the mid 1830s, mainly by farmers and traders. Victoria was initially also a penal colony, while South Australia never harboured convicts.

[9] These statistics are sourced from the Australian Bureau of Statistics, accessed on 20 July 2020 at https://www.abs.gov.au/AUSSTATS/abs@.nsf/mf/3105.0.65.001.

Combining NSW and the ACT, which is located within NSW, into a control group against which VIC and SA can be compared against also has many advantages. NSW and the ACT border VIC to the east. Hence, all three states and the territory lie within the same geographic area and climate zone of what is otherwise a continent of multiple climatic zones and geographic isolation. The population in NSW was about 3.8 million, while the ACT has always been small with 52,0000 in 1960. Thus, the combined population of about 4 million was about the same as in the two treatment states. Both Melbourne and Sydney, the two capital cities of VIC and NSW, respectively, became prosperous competing against each other for wealth and human capital. All four states and the territory experienced an enormous inflow of pupils during the 1940s and 1950s as a result of post WWII migration streams and the baby booming years.

Yet, there were differences across the treatment and control states regarding education policy and responses to the challenges of a large inflow of pupils into the system (see Barcan, 1980, for an extensive overview). NSW was the first state to experiment with progressive education systems, mainly to keep up education quality and standards. Most importantly, NSW was the first to transition away from a selective schooling system to a comprehensive schooling system. Reforms were practically implemented by 1957, but they were broadened further in 1963, during a time when Victoria and South Australia increased their MSLA. Victoria was the last state to implement comprehensive schooling reforms in the early 1980s. This difference could be considered as problematic for our experiment. The reason is that although comprehensive schooling systems make education more democratic and accessible, it may harm the high-ability pupils. This possibility was explicitly acknowledged by the NSW education reformers. Thus, NSW allowed 16 high schools to proceed as selective high schools to cater for the needs of the high-ability students. The schools' class structures also allowed for different ability streams within the comprehensive school, teaching different curricula.

Fears of declining education standards also forced VIC and SA to broaden their curricula and give schools more power to design their own curricula. VIC implemented already during the 1950s reforms to improve teacher quality and retention in the primary school system. SA started to broaden the curriculum by the end of the 1960s in all schools, but implemented comprehensive schools only in 1975. Importantly, all states had abolished high school entry exams by the

mid to late 1940s. Decisions on who can enter specific schools were based on school grades, teacher assessments and in some states on intelligence tests. Thus, access to postsecondary education was significantly broadened in all states. The states also changed the way school leaving certificates were obtained. All states reduced the number of exams that were state-wide. Tuition fees were abolished everywhere in the 1940s.

This educational set up allows us to study the long-term impact of MSLA reforms that were implemented around the same time in neighbouring states, and compare their effect against the outcomes of pupils who live in a neighbouring state, which has already implemented comprehensive education reforms by the time our experiment starts. In the empirical section we will demonstrate that differential trends in education reform are not likely to confound our estimation results.

2.3 Literature: How do MSLA reforms impact capital accumulation over the life course?

What impacts could we expect from the two MSLA reforms in Australia? MSLA policies are paternalistic in nature as they restrict the choice set of adolescents and their parents. They are justified on the grounds of improving social welfare by boosting the human capital of adolescents at risk of dropping out from school early and at risk of unemployment. But not everyone argues that MSLA policies improve social welfare. Some previous work suggests that compulsory schooling reforms have zero returns in the labour market (Pischke & von Wachter, 2008) and do not improve health (Clark & Royer, 2013).[10] Making potential drop-outs stay on in school may penalize the students who would have stayed on to complete high school even in the absence of the reform, and add little to the learning effects for unmotivated students. MSLA reforms imply larger class rooms, and thus pupils operate in a more crowded class room. Thus, MSLA reforms may only be expensive tools to park youth in schools without benefits.

Yet, many previous studies have shown that such policies have labor market benefits (see Harmon, 2017, for a review), that may transmit to the next generation (Lindeboom et al., 2009;

[10] Zero returns may have been the result of unconsidered institutional details, as shown in Cygam-Rehm (2018) who estimates a return to one additional year of schooling of 6-8 percent.

Lundborg & Rooth, 2014). Recent work suggested that the returns to education could vary over the lifecycle, which could explain the variation in results found by the previous literature, typically with respect to wage returns (see Buscha & Dickson, 2012, 2015 and the references therein). It therefore cannot be assumed that compulsory schooling policies generate positive returns at all ages or for all groups. Bhuller et al. (2011) for example showed for Norway that wage returns are maximised at middle-age. We are argue that MSLA reforms are likely to have important non-market benefits (Oreopoulos & Salvanes, 2011), that affect individuals' lives in and outside the labor market. Specifically, more schooling is likely to impact upon whether and how individuals invest in their financial wellbeing and health, and how to decide over the quantity and quality of children and family relationships. Ultimately, such decisions will affect how happy individuals will be.

Judging whether more schooling directly impacts on non-market outcomes, or whether it simply produces them indirectly through better labor market outcomes, is one of the key challenges in this new literature (see Oreopoulos & Salvanes, 2011, p. 160). We approach this challenge from the perspective that both market and non-market benefits of MSLA reforms can be quantified at the end of the lifecycle, assuming that these benefits accumulate over the life course in an interconnected fashion. Individual choices about education depend on individual preferences over family formation, yet fertility decisions depend on health, human and financial capital. Fertility in turn will impact upon labor market outcomes, financial capital, and health capital accumulation. It is thus a complex matter how the impact of MSLA reforms can be isolated.

Lifting the MSLA increases the quantity of education demanded by students who are at risk of dropping out of school early. These students are forced into a continuation of their daily school routine. Staying on means an additional year of exposure to knowledge, reading, sitting exams, and interaction with peers of the same age. Adolescents who leave school are supervised less, work with older individuals or are unemployed having little or no daily routine at all. Increasing the MSLA therefore aims to improve human capital. Human capital involves many things, but usually it refers to occupation-specific, formal training or the acquisition of qualifications. Being forced to stay on for an additional year may also change educational aspirations

and expectations. This may increase the likelihood of staying on more than one additional year to complete high school. Completing high school may then lead to the acquisition of further qualifications either through university education or through occupational training.

At the same time, the MSLA may improve human capital more broadly, including a boost in cognitive and non-cognitive abilities (Almlund et al., 2011; Lundberg, 2018; Todd & Zhang, 2020). MSLA reforms target young people during adolescence, which is a period of growth where the human brain develops rapidly (Casey et al., 2008). Thus, having to stay one extra year in school may impact not only on the willingness to acquire further qualifications but also on the skill growth trajectory of adolescents. Non-cognitive skills are shaped early in life, but adolescence is an important window of opportunity in which these skills can be boosted (Elkins et al., 2017; Kassenboehmer et al., 2018; Schurer, 2017). The additional year spent at school could help bridge momentary distractions associated with sexual maturation, willingness to engage in high-risk behaviors during adolescence, and fertility decisions (Black et al., 2008; Cygan-Rehm & Mäder, 2013). Thus, increasing the MSLA by one year may build additional cognitive and non-cognitive abilities, motivation for further education, and shape occupational choice decisions in young adulthood. Non-cognitive skills may also play an important role in determining household bargaining weights and offered wages (Flinn et al., 2018; Todd & Zhang, 2020).

On the other hand, there could be unintended negative consequences on mental health and wellbeing for example, especially for low achieving pupils who are forced to remain in school against their will as shown in Avendano et al. (2017). Being exposed to a competitive academic environment could be a very stressful experience for these young adolescents, which might have long-term consequences on their mental health and wellbeing, which may also impact on their realized returns in other domains of life such as productivity in the labor market or the ability to maintain good relationships.

Better human capital in the broadest sense is likely to affect a series of important decisions that an individual needs to make when transitioning from young adulthood into middle age. These decisions are with respect to labor market participation productivity (labor market entry and duration, wages), financial decisions (home ownerships, other assets), family formation

(quality and quantity of partner(s) and children), and demand for health. Human capital is a critical determinant of these decisions as it affects the way individuals think about risk, the future, ambiguity, and social relationships.

At the end of productive life, individuals are left with a capital stock which they can deplete during retirement. Hence, we consider end-of-productive life capital stock in each of the five domains (skills, labor market, finance, family, and health). This diversified capital stock is a practical summary measure for evaluating success in life. Because of the cumulative process of capital development over the life course, we posit that MSLA reforms impact upon capital formation through complex channels that cannot be separately identified. We thus focus our analysis on the capital stock at the end of productive life, which is the outcome of complex dynamics that were triggered through a MSLA reform before the start of productive life.

3 Empirical strategy

3.1 Model

Our empirical analysis exploits exogenous variation in the number of years a pupil is required to spend at school. This exogenous variation comes from two MSLA reforms from Victoria and South Australia during the 1960s. We estimate the causal impact of the MSLA reform

using different Difference-in-Differences (DiD) specifications.[11] Our main regression equation is given by:

$$Y_i = \alpha Reform_i + \beta Post_i + \gamma Reform_i \times Post_i + X_i'\delta + \theta_c + \epsilon_i, \quad (1)$$

where Y_i represents the outcome variable for individual i. We employ several outcome variables which provide information on formal education, skills (cognitive and non-cognitive), labour market outcomes, financial capital, family capital and health capital. Section 4.2 explains the construction of all outcome variables in detail.

The indicator variable $Reform_i$ takes the value 1 if the individual i completed schooling in a MSLA state (Victoria, South Australia), and 0 otherwise (New South Wales, Australian Capital Territory). The indicator variable $Post_i$ takes the value 1, if the individual was born in a year that would have made her eligible for the reform (birth cohorts born in 1949 or after).[12] The interaction $Reform_i \times Post_i$ captures the post-treatment time period for treated individuals. Of main interest is the estimate of γ, which measures the treatment effect of the reform.

In an extension to the baseline model, we allow for interaction effects between the MSLA reform dummy and gender (e.g. $Reform_i \times Post_i \times male_i$) or treatment state (e.g. $Reform_i \times$

[11] Different methods have been used in the previous literature (see Oreopoulos, 2006b, for discussion), but DiD approaches have been used frequently in evaluations of compulsory school-leaving reforms in the Nordic countries (e.g. Fischer et al., 2020; Meghir & Palme, 2005; Black et al., 2008). Other studies have used so-called fuzzy regression discontinuity designs (e.g. Courtin et al., 2019; Clark & Royer, 2013; Oreopoulos, 2006b) or instrumental variable approaches (e.g. Kemptner et al., 2011; Leigh & Ryan, 2008; Oreopoulos & Salvanes, 2011; Li & Powdthavee, 2015; Schneeweis et al., 2014), both of which identify a local average treatment effect (LATE). Both IV-based and DiD methods bring along their methodological strengths and weaknesses (e.g. Oreopoulos, 2006b; Buscha & Dickson, 2015; Stephens & Yang, 2014). We see three advantages of a DiD approach in our particular setting. First, aiming to understand the broader benefits of MSLA reforms across the education distribution and for the full population, Fuzzy RDD and IV approaches usually identify effects at the lower end of the education distribution. The reform may not only affect individuals who are forced to stay in school for an extra year as a result of the reform, but potentially also those who would have stayed in school longer in the absence of the reform. These always-takers are affected because of e.g. a change in peer groups, in class size and in the own rank within the class. Second, even though the reform was implemented at one specific day, reinforcement potentially increased over time. As opposed to a RDD strategy, which identifies the treatment effect for individuals born just around the birth date cutoff, a DiD design gives equal weight to all cohorts in the sample. Yet, our DiD approach may be violated if the common trend assumption breaks down. We thus present in a separate robustness check estimates derived from an IV approach and a fuzzy RD design. These are presented in Section 6.

[12] For Victoria, the post reform cohorts start in 1950, although there are 5 individuals born before 04/02 in 1950 that are not part of the post reform cohorts due to their exact birth dates. For South Australia, the post reform cohorts start in 1949, although there are 6 individuals born before 04/04 in 1949 that are not part of the post reform cohorts due to their exact birth dates. Individuals from the control states NSW and ACT are part of the post cohorts starting from the birth year 1949.

$Post_i \times SA_i$), or treatment state and gender (e.g. $Reform_i \times Post_i \times SA_i \times male$). These interaction terms allow us to test whether women and men were affected differently by the reform, whether the reform was more effective in Victoria than in South Australia, and whether men and women were affected differently in South Australia or Victoria.

We model cohort trends θ_c in a linear fashion in the main specification. Alternative specifications are estimated allowing for non-linearities or state-specific cohort trends (Section 6, Robustness checks). The vector X_i includes a set of control variables such as gender, state, low socioeconomic status (0, 1), mother employed at age 14 (0, 1), father employed at age 14 (0, 1), at least one parent born abroad (0, 1), oldest child in the household (0, 1), number of siblings (0, 1), and grew up with single parent (0, 1).[13] By construction, the individual-level covariates should not affect the identification of the reform effect but may produce more efficiently estimated standard errors. In a robustness check, we test whether the covariates are balanced across cohorts between treatment and control group. All remaining unobserved factors are captured by the error term ϵ_i. Standard errors are clustered by state where the individual completed secondary education.

In our set up, the statistical inference may be biased because we test for a large number of hypotheses and the analysis is based on a small number of state clusters (see e.g. Bertrand et al., 2004). We therefore rigorously test for the sensitivity of our conclusions to multiple hypothesis testing and alternative standard error clustering. With 38 outcomes in total, we will find at least two significant effects by chance, assuming critical values of 0.05. We adjust for multiple hypotheses by implementing the efficient step-down approach developed in Romano & Wolf (2005). Following Cameron & Miller (2015), we consider three alternative clustering methods that correct the critical values from which p-values are calculated to account for small number of clusters. In the baseline specification, we use critical values based on a T-distribution, adjusted by the number of clusters (G) minus one degree of freedom ($G-1$). In two robustness checks we apply a more conservative approach, adjusting the critical values by $G-2$ degrees

[13] The measure for socioeconomic status is based on the father's occupational status scale developed by McMillan et al. (2009). The variable runs from 0 (low) to 100 (high) with mean 0.636 and standard deviation 0.481. Socioeconomic status is defined as low if the value of the continuous variable is below the mean value.

of freedom or the wild cluster bootstrap method with a six-point distribution recommended by Webb (2013).[14]

3.2 Identification assumptions

The DiD parameter estimate of γ measures the difference in outcomes between treated and non-treated cohort members in Victoria and South Australia (VIC/SA), relative to the differences in outcomes between the same cohorts in New South Wales and the Australian Capital Territory (NSW/ACT), where the same reform had already been implemented by 1943. Theoretically, the parameter γ captures the average treatment effect on the treated. However, because of a small number of non-compliers (around 5 percent), the treatment effect needs to be interpreted as the intention-to-treat effect.[15] Four assumptions are made to interpret γ as a causal effect.

First, in the absence of treatment, cohort trends would have been the same across treatment and control states. This counterfactual cannot be observed, but as suggested in the literature (see Angrist & Pischke, 2008; Wing et al., 2018, for reviews), one can test for the common trend assumption by an approximation test. In an extensive robustness check, we will document graphically cohort trends in all outcomes for both treatment and control groups leading up to the MSLA reform dates. We will furthermore test rigorously for the presence of differential cohort trends and for treatment effect sensitivity to the inclusion of treatment-state-specific cohort trends.

Second, individuals do not change their behavior in anticipation of the reform. Since the assignment into treatment is based on cohort affiliation and not on grade-level completion, an individual could not have affected treatment status (e.g. by grade retention or acceleration). However, students could have influenced treatment by moving across states. By 1966, all Australian states had implemented an MSLA of at least 15, leaving pupils in VIC/SA no alternative state for school avoidance. On the other hand, it could have been that cohort members from our comparison states – NSW/ACT – strategically moved to VIC/SA to avoid staying in school until

[14] The six point distribution accounts for the fact that the number of possible t-statistics in a bootstrap environment is small in a context with few clusters (Webb, 2013).

[15] Unlike reforms in the UK or Sweden, where over half of the pupil population was affected by the reform (e.g. Oreopoulos, 2006b; Fischer et al., 2020), only about one in five pupils were affected in Australia. Thus, we cannot interpret γ as an approximation of the average treatment effect in the population.

age 15. Our DiD estimates would then be biased because the pre-treatment cohorts in the treated states would consist of a larger share of unmotivated students, while the pre-treatment cohorts in the non-treated states would consist of more motivated students. This scenario is however highly unlikely, because NSW/ACT introduced their reforms between 1941-1943. Apart from the fact that it sounds implausible that a large number of families would and could move across borders in response to MSLA reforms, all affected students would have already completed high school by the time VIC/SA introduced their own MSLA reforms.

Third, no other policy changes occurred during the sample period that may have affected outcomes of pre- and post-cohort individuals differently. As outlined in Section 2, NSW and the ACT both implemented a comprehensive schooling system by 1957, which they broadened through new legislation in 1963. The comprehensive school system created progressive learning environments for pupils from all ability backgrounds, but it broadened more the educational opportunities for children from disadvantaged backgrounds. To cater for the needs of high-ability students, NSW legislation from 1957 allowed 16 high schools to continue to operate as selective schools. The two treatment states did not implement comprehensive schools until 1975 (South Australia) and 1980 (Victoria) (Barcan, 2007), but embarked on a series of reforms during the 1950s to broaden the curriculum. As shown elsewhere, comprehensive schooling systems improved wages and educational attainment for pupils from less advantaged backgrounds (Meghir & Palme, 2005; Pekkala Kerr et al., 2013). We conclude that some of the post-reform cohorts from the two non-treated states may have been better off than expected because they benefitted from access to better schooling since 1957. If this had been the case, we would underestimate the treatment effect of MSLA reforms.[16]

Finally, the composition of both treatment and control groups did not change over time, which is particularly important as we use cross-sectional cohort data. In a robustness check, we test this assumption by estimating the same DiD model as described in Eq. (1), but replace the outcome variable with measures that capture an individual's family background.

[16] Further, in Appendix Table C7, we use a Regression Discontinuity Design for VIC and SA as an alternative identification strategy that estimates the impact of an additional year of education and does not rely on the assumption that no other policy changes occurred during the sample period that affected outcomes of pre- and post-cohort individuals differently.

4 Data

4.1 Data and estimation sample

We use data from the Household, Income and Labour Dynamics in Australia (HILDA) survey which is a nationally representative household panel study with annual data collection since 2001. All adult household members (aged 15 years and above) answer the continuing or new-person questionnaire which is conducted by an interviewer. In addition, each member is asked to fill out a self-completion questionnaire (SCQ) without the help of the interviewer. The completed SCQ is then either collected on the same day or at a later date by the interviewer, or returned by mail. We use the in-confidence version of the HILDA survey which provides us with the exact birth date of each survey member and the state in which they completed schooling as a child (available in wave 12 and 16). From 2001 to 2010, approximately 13,000 individuals were interviewed annually. A top-up sample has increased the respondent number to around 17,500 since 2011 (Summerfield et al., 2017). The estimation sample varies by outcome, because some outcomes were measured only in a few waves (e.g. cognitive and non-cognitive abilities), while others were collected or updated every year (e.g. educational degree, family status).

Our main sample consists of individuals who turned 15 within a 7.5-year window to the left and the right of the MSLA reform date, including birth cohorts from 1942 to 1957. To identify who is affected by the reform, we use data available in HILDA on the exact birth date (day, month, year) and the state in which the individual completed high school education. The birth cohort members are between 58 to 74 years old in 2016 (wave 16). This means that most of our sample members are either close to retirement age or have already retired. The advantage of observing individuals at the end of their lifecycle is that many of the outcomes which we consider are fixed. It is for instance highly unlikely that older-age adults still change their educational degree, number of children or their marital status. We therefore use wave 16 as our baseline wave. Other outcomes however, such as cognition, softer skills and health, are more dynamic during this older age. To avoid temporal fluctuations in these outcomes, we therefore construct summary measures across several waves to reduce measurement error (e.g Cobb-Clark et al., 2014, for a similar strategy). A detailed description on how we measure the

diversified capital stock follows below. Table 2 summarizes all outcomes, explains when they were measured and presents sample sizes and summary statistics.[17]

4.2 Outcome measures

4.2.1 Human capital 1: Formal education

We consider several measures of educational attainment. On the one hand, we construct two continuous measures for the total number of years an individual spent in school (excluding post-secondary training) or in education overall (including post-secondary training). On the other hand, we construct binary indicator variables for different levels of educational attainment. First, we generate a binary variable that captures whether the individual has left school by the age of 14. At a minimum, the MSLA reform should have affected this binary indicator. Second, we construct a series of binary variables from the categorical variable of the highest year of secondary school completed. Each constructed binary variable is equal to 1 if the individual completed the respective school year and 0 otherwise (Year 8, Year 9, Year 10, Year 11, Year 12). An individual who completed Year 10 automatically also completed Year 8 and Year 9. Third, we construct binary indicators for the highest post-secondary education attained. Each binary indicator takes the value 1 if the individual achieved a certain level of post-secondary education, and 0 otherwise (Low level of vocational training: Certificate III or IV; Higher level of vocational training: diploma; Undergraduate degree: Bachelors/Honours; Postgraduate degree). An individual with a postgraduate degree would have automatically also completed an undergraduate degree. This way of coding the education outcomes allows us to consider shifts in the educational qualification distribution, in the same way as Clark & Royer (2013).

4.2.2 Human capital 2: Skills

We use a summary measure of cognitive ability which is constructed from three items of ability that were collected both in 2012 and 2016. The three items are the (1) Backward Digits Span (BDS) test, (2) National Adult Reading (NART) test, and (3) Symbol-Digit Modalities (SDM). These items were collected by the interviewer in 2012 and 2016. Participation rates were high

[17] Appendix Table A1 shows corresponding summary statistics for the pre-reform cohorts only.

(>93% in each test) (Wooden, 2013). The BDS measures working memory span and is a traditional sub-component of intelligence tests. The interviewer reads out a string of digits which the respondent has to repeat in reverse order. NART measures pre-morbid intelligence. Respondents are shown 25 irregularly spelled words which they have to read out loud and pronounce correctly. SDM was originally developed to detect cerebral dysfunction but is now a recognized test for divided attention, visual scanning and motor speed. Respondents have to match symbols to numbers according to a printed key that is given to them. As commonly used in the literature, we construct a combined measure of cognitive ability by conducting a factor analysis on all three measures and predicting its first factor. To furthermore reduce measurement error, we average this measure for each individual across the 2012 and 2016 waves. This measure of cognitive ability is standardized to mean 0 and standard deviation 1.

Non-cognitive ability is measured with the Big Five personality traits and locus of control. HILDA collected an inventory of the Big-Five personality traits based on Saucier (1994) that can be used to construct measures for extraversion, agreeableness, conscientiousness, emotional stability, and openness to experience. To construct a summary measure for each trait, we use the 28 items used to measure personality on the Big-5 and conduct a factor analysis (see Cobb-Clark & Schurer, 2012).

A measure of internal locus of control is derived from seven available items from the Psychological Coping Resources Component of the Mastery Module developed by Pearlin & Schooler (1978). Mastery refers to the extent to which an individual believes that outcomes in life are under her own control. Respondents were asked to report the extent to which they agree with each of seven statements related to the perception of control and the importance of fate. We construct a continuous measure increasing in internal locus of control using factor analysis (see Cobb-Clark & Schurer, 2013; Cobb-Clark et al., 2014).

To minimize measurement error in our constructs of non-cognitive ability, we average personality scores across all available waves as in Cobb-Clark et al. (2014). All measures are standardized to mean 0 and SD 1.

4.2.3 Labour market capital

A large fraction of our sample members are already retired and are no longer attached to the labor market. In our sample, the oldest cohort members are 74 years old. Assuming they have participated in the HILDA survey since the first wave, they were 59 years old at the time when the HILDA survey commenced – thus being close to retirement age. In contrast, the youngest individuals are 58 years old in wave 16. To make wages comparable and to avoid picking up the effect of differences in work experiences or a reduction of hours worked close to retirement, we calculate the average of the weekly gross wages of all current full-time jobs between ages 50 and 60. We exclude unreasonable results at the top and bottom 3% of the distribution to avoid bias from measurement error. We furthermore proxy cohort members' historical labor force attachment by calculating their accumulated unemployment experiences since leaving full-time education as measured by age 59. We also construct variables which indicate whether the last observed occupation was either a position as a (a) manager, (b) professional, (c) advanced or intermediate clerical, sales and service worker (which we label as "Service and Clerks") or (d) tradesperson, production worker, transport worker, labourers or other elementary worker (which we label as "Manual Labour").

4.2.4 Financial capital

Measuring wealth is complex, because households may systematically underreport wealth or because assets and capital is difficult to classify. We follow previous approaches to measure wealth in our data (see Cobb-Clark et al., 2016, for a review and applications with HILDA). We use house ownership and wealth portfolio as measures for financial capital. The first measure is a binary indicator for whether an individual owns a house or is currently paying off a mortgage. The second measure is a continuous measure of log household wealth, constructed from information on real estate assets, business equity, net financial assets, superannuation and vehicle value. This measure is averaged over all years in which wealth information is available to allow comparability between individuals of different ages. Wealth information is only available at the household level.

4.2.5 Family capital

We proxy family capital with measures on marital status, partner quality, age at first marriage and age at first birth, number of children, and satisfaction with family life. As family formation processes can be assumed completed by the age 59, we measure these variables in wave 16.[18] Marital status is measured with two binary variables where the first indicates being married or in a de facto relationship and the second indicates being separated or divorced. Partner quality is proxied with information on whether the partner completed year 12. Satisfaction with family life is proxied with subjective scores on how satisfied an individual is with his or her partner or children. Both indices are scaled from zero to ten. We average this information over all available waves in which the individual is observed. These satisfaction measures are regarded as summary measures, averaging out fluctuations due to altering circumstances.

4.2.6 Health capital

We proxy health with continuous measures of physical and mental health, and life satisfaction. The health measures are derived from the SF-36 inventory, a reliable self-completion questionnaire with 36 questions that was developed in Ware et al. (2000). Using the individual responses, a summary score is constructed, ranging between 0 and 100. As both physical and mental health depend on age and may fluctuate randomly, we construct an average health measure over all survey waves. A measure on overall life satisfaction is also taken from the self-completion questionnaire, in which participants are asked to rate their overall satisfaction with life on a scale between 0 (lowest level) to 10 (highest level). Again, as this measure may fluctuate randomly over time, we construct an average score across all survey waves.

5 Estimation results

We begin with reporting the estimated effects of the MSLA reforms on educational attainment and then discuss the estimated effects of the reforms on the diversified capital stock observed at the end of productive life. In principle, we discuss treatment effects as statistically significant

[18] Only 0.5 percent of all individuals in the HILDA survey change their family status after they turn 58 years old.

at the 5%-level or better. Some treatment effects are only significant at the 10%-level. We will highlight these only if the treatment effect is economically meaningful.

5.1 Reform effects on secondary and postsecondary education

We start out by graphically presenting evidence that the compulsory school-leaving age reform in the 1960s in Victoria and South Australia indeed increased schooling in those states. Figure 1a shows that the policy change resulted in a drop of 15 percentage points (ppt) in the probability to leave school by age 14, implying a drop of 75 percent. After the reform was implemented, 5 percent of each birth cohort would not comply with the MSLA, leaving school still by age 14.[19] The dotted line shows that the probability of having left school by age 14 in the control states, where the MSLA was already 15 in the 1960s, was stable at 10 percent around the reform date.

Figure 1b shows that the total number of years of schooling increased by a quarter of a year on average, which is comparable to what was commonly observed in post World War II MSLA reforms.[20] Again, the average years of schooling in the control states (dotted line) seem unaffected by the reform. Figures 1c-1f illustrate the treatment effects of the reform on schooling by gender. Females were more strongly affected by the reform as their probability to leave school by age of 14 was higher than the probability of males (above 20 percent versus 18 percent). After the reform, both sexes had a probability to leave school by age 14 of around 5 percent. The reform had a marginally larger effect on the total number of years of schooling for women (0.6 years) than for men (0.3 years). Again, women started at a lower level pre-reform (10.1 years) than men (10.6 years).

The DiD results from estimating Eq. (1) broadly support these graphical findings, although they are interpreted in a slightly different way. The DiD effects are interpreted as the difference in outcomes between birth cohorts affected and not affected by the reform, relative to the differences in outcomes between the same birth cohorts in the control states, where no reform took place. Figure 2 presents the estimated coefficients and their 95 percent confidence intervals,

[19] Due to exceptions to the compliance with the law, a small number of students was still allowed to leave school before the legal minimum school leaving age.

[20] On average, a change in compulsory schooling translated into 0.3 to 0.4 years of additional education for individuals at the lower end of the distribution, and 0.1 years on average for the rest of the population, see Harmon (2017, p. 3).

separately for secondary and post-secondary schooling outcomes. Estimation results of relevant coefficients are reported in Table B2 and full estimation results for one outcome – probability of leaving school by age 14 – are reported in Table B1 (Online Appendix). To make the results more comparable, we standardized all continuous outcomes to mean 0 and standard deviation 1 (SD).[21]

The reform was effective in keeping students in school beyond their 14th birthday. It reduced the probability of leaving school by age 14 by around 12 ppt (62 percent reduction relative to the pre-reform mean of 19.4 percent). The reform was also successful in keeping students in school to complete at least Year 9 or Year 10, the two year levels affected most by the reform as students are aged 14-15. The increases in these effects were large in magnitude. For instance, students were 17.7 ppt more likely to have completed at least Year 10, which is a 25 percent increase relative to the pre-treatment mean of 71 percent.

The reform also increased the probability to complete at least Year 11 or Year 12, two critical graduation markers for students who seek to continue with vocational training through the certificate system (CERT III/IV) or diploma system or to enter university. For instance, the reform increased the probability of completing high school (Year 12) by 11.3 ppt (39 percent relative to the pre-reform mean of 28 percent). It is thus not surprising that the reform significantly increased average years spent in school by 0.48 SD or 0.62 years.

The reform also shifted the distribution of post-secondary qualification. The reform increased the proportion of individuals who obtained at least vocational training in the diploma system by 7.5 ppt (or 22.7 percent). Diplomas offer practical courses that take around one year to complete. The reform also increased the share of individuals who obtained some form of university education, both at the undergraduate (7.2 ppt or 32 percent) and postgraduate (3.0 ppt or 111 percent) level. The reform therefore increased years of education by 0.23 SD or 0.56 years, but this, as most other estimates on postsecondary education, is noisily estimated.

[21] We do this in all Figures 2-7. The appendix tables show the corresponding regression results for the unstandardised outcomes. The cognitive and non-cognitive skills are always standardised for all models.

5.2 Reform effects on long-run outcomes

Estimation results of the long-run impact of the reform on the capital accumulation process are described in Figure 3. Coefficient estimates underlying this figure are reported in Table B3 (Online Appendix). Long-run outcomes are measured up until 2016, when the sample members were aged between 58 and 74 years. Some outcome measures are aggregated over a decade when sample members were aged 50 to 60 years (e.g. weekly wages) or evaluated at age 59 (e.g. cumulative unemployment experience).

In the long-run, the reform had a significant positive impact on cognitive skills (0.09 SD), but no significant impact on non-cognitive skills. Although the treatment effects are comparable in magnitude to the effect size of cognitive skills – Agreeableness increases by 0.13 SD and Openness to Experience by 0.14 SD – the confidence intervals are too wide to conclude that these effects are not produced by randomness or heterogeneity in the sample.

The reform had no strong impact on the labour market outcomes in terms of wages and unemployment experiences. We find a small, significant reduction in cumulative unemployment experience, but the effect size of 0.01 SD translates only into a difference in cumulative unemployment experience of 8.5 days. However, the reform affected the occupational composition of the workforce in the long-run. It increased the probability of working in a Professional occupation by 10.5 ppt (28 percent relative to pre-reform mean of 38 percent, significant at the 10%-level), and significantly reduced the probability of working as Manual Labourer (-3.8 ppt, 16 percent relative to the pre-reform sample mean of 0.24).

The reform also unambiguously improved the financial capital of affected households. The reform increased the probability of home ownership by 2.5 ppt, which is a small increase of 3 percent relative to the pre-reform mean of 86 percent. It also increased household wealth – a summary measure of all financial and capital assets in the family – by 0.072 SD (not significant), which translates into a 4.8 log percent increase.

The strongest effects of the reform are observed on family capital formation. The reform significantly delayed age at first marriage by 0.17 SD or by 1 year, and age at first birth by 0.3 SD or 1.7 years. The reform also reduced the number of children by 0.18 SD, which translates to 0.25 fewer children per family. Unsurprisingly, the reform boosted satisfaction levels with both

partner and children by 0.1 SD and 0.2 SD, respectively. The reform did not significantly affect whether the individual was still married in pre-retirement age and the quality of the partner. The reform also did not significantly impact physical or mental health or life satisfaction.

6 Robustness checks

We conducted a series of robustness checks to our baseline models, as outlined below. Most of our conclusions remain robust. We highlight the few exceptions.

6.1 Allowing for non-linear cohort trends and different cohort bandwidths

Instead of modelling the cohort trend linearly, we alternatively include birth-cohort dummies for each birth year, with the 1942 birth cohort as the base. This is a more flexible, non-parametric specification. The estimations results are very close to the ones reported above for each model considered (see Table C1, Online Appendix).

We also re-estimated the baseline model allowing for wider (+/- 8.5 years) and narrower windows (+/- 6.5 years) for defining sample birth cohorts (base line +/- 7.5 years). Our conclusions remain the same (see Table C2, Online Appendix).

6.2 Validity of DiD model

The DiD model would yield over-estimates of the effect of the reform if control states were on an upward (downward) trend pre-reform date, while treatment states were on a constant or declining (increasing) trend. We thus need to show that pre-treatment trends were approximately equal or, if different, that the control group was at least on a steeper trajectory than treatment groups.

To test for the common-trend assumption in the context of our main estimation model, we regressed each outcome variable at the individual-level on the reform indicator and a linear birth cohort trend, including the interaction of the two for the pre-treatment cohorts (1942-1950). We then graphed the trends of all outcome variables, separately for treatment and control states

(see Figures C1-C4).[22] Eye-balling the figures, we find steeper cohort trends for the control group than for the treatment group in 8 out of 38 outcome measures. The majority of these violations are found in the "highest degree obtained" category. For each of these cases in which the common trend assumption appears to be violated, we would under-estimate the treatment effect of the reform. In 4 out of 38 cases, we find that the treatment group was on a steeper trajectory than the control group. These cases are Completed at least Year 9, Agreeableness, and Physical and Mental Health. In these cases we would over-estimate the treatment effect.

However, when conducting formal hypothesis testing, in which we test for the statistical significance of the interaction effect between the reform indicator and a linear birth cohort trend, we find that only for two out of 38 outcomes we violate the common trend assumption. Each figure reports the p-value of the test of equality of trends between treatment and control group. In line with the decision criteria used in the estimation result section, we consider the common trend assumption as violated, if the p-value of the interaction effect is 0.05 or smaller.

We find statistically significant diverging trends for lower-quality vocational training (certificate) ($p<0.05$) and postgraduate degree ($p<0.01$). In both cases, the control states were on an increasing trend while treatment states experienced a less steep upward trend. For these two cases, we would under-estimate the true treatment effect of the reform.

Another approach to test for the validity of the DiD model is to demonstrate that adding treatment state-specific cohort trends to the baseline model does not significantly affect the estimated treatment effects. We therefore re-estimated our models as augmented versions of the baseline model, allowing for linear or quadratic cohort trends that are interacted with the treatment state indicator. Table C3 reports the estimated treatment effects under different cohort-trend specifications and the associated p-values for the hypothesis test that the interaction effects are statistically insignificant. Our conclusions remain largely the same in terms of sign and size of the treatment effects. In 12/38 outcome variables, we find a statistically significant ($p<0.05$)

[22] The equivalent analysis allowing for non-linearities is shown in Online Appendix Figures C5 - C8.

interaction effect between the cohort trend and treatment state indicator. Yet, the estimated treatment effects in these cases tend to remain unchanged.[23]

We also conducted balance-of-covariate tests to investigate whether the composition of the cohorts has been different or changed in different ways across cohorts between the treatment and control states. We fail to reject balanced cohort trends in all but one out of seven cases (see Table C4, Online Appendix).[24] We thus conclude that the sample composition has not changed differentially between treatment and control states, which could have explained differential capital accumulation pathways over the life course.

6.3 Standard error adjustments

Because of the high dimensionality of our outcome variable vector (38 outcomes), it may be the case that we find statistically significant effects by chance. We find that all estimated treatment effects reported in the pooled model (column (1) in Tables B2 and B3), that are statistically significant at the 5 percent level or better (17 out of 38), are still statistically significant at the same levels once we adjust standard errors for multiple hypothesis testing (see Table C5, Online Appendix).

We also assess the robustness of our results to alternative methods of standard error clustering. Following Cameron & Miller (2015), we consider three different clustering methods that correct the critical values from which p-values are calculated. First, we use critical values based on a T-distribution, adjusted by the number of clusters (G) minus one degree of freedom (G-1), or, more conservatively, adjusted by G-2 degrees of freedom. Second, we use the wild cluster bootstrap method with a six-point distribution recommended by Webb (2013) in the context of few clusters. We consider a treatment effect as robustly significant, if the p-value of the

[23] In 6/38 cases we find statistically significant treatment effects once allowing for treatment-state specific cohort trends. These are "At least undergraduate degree", "Years of education", "Emotional Stability", "Openness to experience", "Partner completed Year 12", "Overall life satisfaction". In 5/38 cases, a statistically significant treatment effect is either no longer statistically significant and/or changes sign. These are "Managers", "Professional" and "Service/Clerk" occupations, "Number of children', and "Age at first child".

[24] Our control variables used in the model are: Low socioeconomic status, Mother employed at age 14, Father employed at age 14 , At least one parent born abroad, Oldest child in the household, Number of siblings, Grew up with single parent.

test-statistic is p<0.05 in two out of the three methods. Our conclusions do not change when allowing for alternative standard error clustering (see Table C6, Online Appendix).[25]

6.4 Alternative estimation methods

A final sanity check to our estimation results is to use a different estimation method to identify the causal impact of the reform.[26] We re-estimated our baseline specification using both an instrumental variable approach (between 834 and 1,806 observations), following Li & Powdthavee (2015) and Kemptner et al. (2011), and a fuzzy RDD approach using local linear regression, following Clark & Royer (2013) using only Victorian and South Australian data (between 512 and 999 observations). Both methods use the MSLA reform as an instrument for the total years of education to identify the causal impact of education on later-life outcomes. Unlike the DiD approach, these methods identify a local average treatment effect (LATE) for the individuals who received an extra year of schooling as a consequence of the reform. This approach is analogous to conducting an RCT on a population of students aged 14 who are planning to leave school as soon as the minimum school leaving legislation allows them to leave. Hence, strictly speaking the findings are not comparable to our main model.

The estimation results from the IV approach demonstrate that the MSLA reform is a strong predictor of total years of education with a median F-statistic across 26 long-term outcomes in the first stage of 14.8 and an estimated increase in education by between 0.3 and 0.5 years.[27] The fuzzy RDD approach yields similar results, with an increase in years of education by between 0.2 and 0.7 years. However, in almost half of all outcomes, the F-statistic of the first stage is too small (<10) to consider their second-stage results as reliable. Overall, the estimated treatment effects on long-term outcomes are similar as in our DiD setting in terms of sign and significance. The effect sizes are however larger than our DiD baseline model. This is as expected, because the IV approach identifies the effect for a population at the lower end of the education and

[25] In 17 out of 38 outcomes, we had found a statistically significant treatment effect at the 5 percent level or better. Using alternative clustering methods yields statistically significant treatment effects in 14 out of these 17 treatment effects when using a strict p-value cutoff at 0.05. For the remaining three outcomes, we would also find significant treatment effects in 2 out of 3 clustering methods if we had applied a p-value cutoff at 0.1. These outcomes are "Years of unemployment", "Manager occupations" and "Home ownership".

[26] We thank an anonymous referee for this suggestion.

[27] We estimate 26 separate regressions. The first stage differs slightly across all outcomes, because of sample size variability.

skill distribution. For this group, we would expect the reform to have a larger impact, as the marginal gains from more education are higher (see Table C7, Online Appendix for details on the specification and the results).

7 Who benefitted most from the reform?

7.1 Heterogeneity in the effect of the reform on long-run-outcomes

Who benefitted most from the reform? Were women and men equally affected? Was the reform equally successful in changing lifecycle trajectories on both states? We present in this section interaction effects of the "Reform × Post" indicator with gender and reform state dummy variables.[28] Estimates of key interest and their 95 percent confidence intervals are documented in Figures 4, 5, and 6. Table B4 reports the underlying coefficients and standard errors (Online Appendix).[29]

The reform had different effects for men and women and across the two treatment states. Overall, we can say that the reform was more effective in improving lifecycle outcomes in Victoria than in South Australia. The most prominent finding is that the reform improved labour market, occupational choice, and wealth outcomes most strongly for women in Victoria in comparison to any other group. The second most important finding is that the reform also benefitted men in Victoria, but mainly in the area of soft factors (cognitive and non-cognitive skills), family life, health and life satisfaction. The third most prominent finding is that men in South Australia were predominantly negatively affected by the reform, while women in South Australia benefitted from higher levels of wealth in the pre-retirement years. Finally, the reform equally benefitted all groups by delaying the age at first marriage and age at first child, reducing the number of children, and by lifting satisfaction levels with children.

[28] To make the treatment effects comparable across the four resulting groups – women in Victoria, men in Victoria, women in South Australia, and men in South Australia – we use as base group the average levels of outcomes.

[29] Again, we standardised all continuous outcome variables to be able to plot the coefficients of all models in one graph. The appendix tables show the results using the corresponding unstandardised variables. The cognitive and non-cognitive skills are always standardised in the graphs and corresponding result tables. Additionally, Table B3 shows results for separate models where the "Reform × Post" indicator is interacted separately with gender and reform state dummy variables.

Women in Victoria improved significantly in their occupational opportunities: They were 18 ppt more likely to be in a Professional occupation, and shifted significantly away from Service/Clerk and Manual Labour occupations by 11 ppt and 7 ppt, respectively. Although better occupational opportunities did not result in significantly higher averages wages in the decade before age 60, accumulated wealth in retirement age was significantly improved for women in Victoria. For instance, the probability of home ownership increased by 6 ppt and wealth (which includes all assets plus housing) increased by 0.17 SD.

One reason for why women in Victoria had such better outcomes at the end of their productive life, is that they seem to have found better marital matches and invested in higher quality family life. Women in Victoria is the only group that is more likely to be still married by age 59, with a significant probability increase by about 7 ppt. We find no statistically significant effect of the reform on any other group. All groups delayed age at first marriage and age at first child, but women in Victoria experienced the largest decrease in the number of children by 0.24 SD, while the effect is significantly lower for all other groups (0.10 SD-0.18 SD). As a consequence, women in Victoria affected by the reform are more satisfied with their partners (0.14 SD) and children (0.23 SD). They are also in better mental health, although the effect size is relatively small (by 0.07 SD). Although women in Victoria did not benefit from improved cognitive skills, they scored higher on Agreeableness (0.16 SD) and Openness to Experience (0.20 SD).

The reform also benefitted men in Victoria, but in different ways. Men in Victoria benefitted profoundly in their cognitive and non-cognitive skills. Their cognitive skills increased significantly by 0.21 SD, so did their Agreeableness scores, their Openness to Experience, and their internal Locus of Control. For most other groups, there was no impact of the reform or the reform had negative impacts on skills. The labour market returns to the MSLA reform were very different for men in Victoria than for women in Victoria, who were exposed to the same labour markets, or men in South Australia. The reform helped men in Victoria to reduce cumulative unemployment experienced over the lifecycle by 0.06 SD. They shifted out of Manager and Service/Clerks occupations into Professional occupations, a significant increase by 8.2 ppt. The reform had no impact on their wages, home ownership or wealth, but significantly improved

their health and happiness. Men in Victoria are the only group that has significantly higher levels of physical health (0.27 SD), mental health (0.18 SD) and life satisfaction (0.10 SD) relative to the control group.

In stark contrast, the reform harmed men in South Australia in many ways. Men in South Australia are the only group for which the reform reduced wages in the decade before retirement age by around 0.17 SD (significant at 10%-level). The reform also reduced their probability of working in Manual Labour occupations, but it also shifted them out of Professional occupations and into Service/Clerk and Managerial occupations. Men in South Australia is the only group for which the reform significantly reduced accumulated wealth by almost 0.25 SD. It is thus not surprising that the reform significantly reduced their physical and mental health, and life satisfaction by between 0.10 and 0.17 SD. It also reduced their sociability (Extraversion: -0.30 SD) and their sense of control (Internal Locus of Control: -0.10 SD). The only benefit that men in South Australia had as a consequence of the reform, was that it improved their family life through delayed and reduced fertility and their cognitive skills in older age.

Finally, women in South Australia both benefitted and were harmed by the reform. The reform harmed their non-cognitive skills, with significantly reduced scores on Conscientiousness (-0.11 SD), Extraversion (-0.41 SD), and Openness to Experience (-0.07 SD, $p<0.10$). The financial and labour market experience effects of the reform on women in South Australia are estimated more imprecisely, but a clear pattern emerges that they benefitted through the reform financially. The reform increased wages in the decade before retirement (0.07 SD, not statistically significant) and led to higher probabilities of home ownership (3.3 ppt, significant at 10 percent level) and levels of wealth (0.19 SD). The reform also increased the probability of working in Professional occupations with a large effect size of 5.7 ppt, but the effect is not statistically significant. Hence, the MSLA reform affected the labor market experiences and financial capital of men and women in South Australia in opposite directions.

Although women in South Australia experienced the same benefits from delayed and reduced fertility, and a higher level of satisfaction with their current partner and children, they are the only group for which the reform reduced the probability of being matched with a better educated partner (Partner completed Year 12, -12.1 ppt). Similar to men in South Australia, they

are also less satisfied with their lives, although the effect size is only a third of the effect size for men in South Australia (-0.04 SD, significant at the 10 percent level).

7.2 Why do we observe heterogeneous long-run effects?

We posit that the mechanisms that drive heterogeneous long-run effects of the MSLA reform by gender and state are operating through heterogeneous reform effects on young-adulthood educational outcomes. In Figure 7 we document this heterogeneity. The figure shows the effects of the reform on education outcomes separately for women in Victoria, men in Victoria, women in South Australia, and men in South Australia (in descending order). Table B5 reports the underlying coefficients and their confidence intervals (Online Appendix).

First, the reform had the strongest educational benefits for women in Victoria. It did not only significantly prolong the average time women in Victoria spent in high school by 0.52 SD (or 0.67 years), it also significantly increased the years of total education by 0.35 SD, which translates into 0.84 years of education. Women in Victoria were significantly more likely to complete Year 12 as a consequence of the reform (18.0 ppt or 60 percent increase relative to the sample mean), which is the entry requirement into most post-secondary qualifications, while all other groups experienced significantly smaller increases (increases for the other groups were between 4.9 and 11.3 ppt). Women in Victoria were also more likely to complete post-secondary education than any other group as a consequence of the reform. For instance, the reform increased the share of women with at least some vocational training – both certificate and diploma qualifications – by 5.0 ppt (9 percent) and 11.0 ppt (32 percent), respectively. For all other groups there was either a smaller increase in the share of individuals with at least vocational training or a negative shift.

The reform significantly increased the probability to complete some form of university education for women in Victoria by 11.4 ppt (52 percent), a significantly larger impact than for any other group. All other groups had either smaller gains in the probability to obtain a university degree (e.g. 6.6 ppt for men in Victoria or 2.4 ppt for women in South Australia) or no gains (e.g. 0.1 ppt for men in South Australia).

For all other groups, the reform boosted only minimum levels of schooling, as can be seen by a significant reduction in the probability of leaving school by age 14 and an increase in the number of years spent in the primary and secondary education sector. Effect sizes range between 0.4 SD (0.52 years) for men in Victoria and 0.61 SD (0.79 years) for women in South Australia. Figure 7 makes clear why men in South Australia experienced no benefits in long-run outcomes as a consequence of the reform. It only increased their probabilities to complete at least Year 10 or Year 11 of high school. Hence, the reform impacted years of schooling, but it had overall no impact on the total years of education. Men in South Australia improved their post-secondary education outcomes by moving away from low-level vocational training (certificate) to higher levels of vocational training (diploma). They are the only group for which the reform did not improve access to university education.

We conclude that the MSLA reform benefitted most women in Victoria because it boosted most their post-secondary educational qualifications. It benefitted least men in South Australia, because it just increased time spent in high school without significantly increasing qualifications.

8 Discussion and conclusion

Our findings suggest that the two Australian MSLA reforms dramatically shifted the educational-attainment distribution during the 1960s. The reforms reduced young Australian's probability of leaving school at age 14 by 62 percent relative to the base probability. A small number of pupils (around 5 percent of each birth cohort) continued to leave school at 14, relying on legal exemption opportunities. On average, the reform added about half a year extra spent in full-time education, which is larger than what was achieved by the many European MSLA reforms (Harmon, 2017).

The MSLA reform was particularly effective in improving educational attainment for women in Victoria, the larger of the two states. The Victorian reform increased not only total years of schooling through higher retention rates into Year 12, but also total years of education. Women in Victoria were disproportionately more likely to obtain post-secondary vocational training or university education. Men in Victoria also benefitted from the reform, but this led to smaller

improvements in retention rates into Year 12 and completion of university education relative to women. In South Australia, the reform only boosted minimum schooling outcomes. It had no effects on the educational attainment distribution beyond Year 10. Not surprisingly, most of the long-term benefits of the reform are concentrated among women in Victoria, including substantial transitions out of low-skilled into high-skilled occupations, higher levels of wealth, and a higher probability of still being married at age 60. Both women and men in Victoria had better non-cognitive skill outcomes. Men in Victoria benefitted mostly from slowed cognitive decline, better non-cognitive skills and boosted health and happiness. Men in South Australia were mostly harmed by the reform with no labour market or wealth benefits and lower non-cognitive skills, health, and happiness, while women in South Australia experienced both benefits and losses.

Despite the heterogeneous treatment effects that the two MSLA reforms had on men and women, lifting the MSLA delayed age at marriage and birth, reduced the number of children and boosted satisfaction with children equally for all groups. This suggests that although MSLA reforms can backfire, they delay and reduce fertility decisions, which are associated with more satisfaction with family life.

Our findings have to be interpreted in light of a very broad but often conflicting evidence base. Where we differ most from the previous literature is that we find strong benefits for women. Existing research in the health domain finds no gender differences (Clark & Royer, 2013) or more beneficial effects for men than for women (Mazzonna, 2014; Kemptner et al., 2011). Previous studies had explained the health benefits of MSLA reforms for men by changes in occupational choice, who transitioned away from blue to white collar jobs. Better jobs were likely to reduce physical strain and improve working conditions. Despite these differences, our findings on the large cognitive gains of men in older age are almost identical both in terms of sign and size to the findings in previous work on older men in Europe (Schneeweis et al., 2014) and on younger men in Sweden (Lager et al., 2016). Our finding that the MSLA reform boosted non-cognitive skills for both men and women in Victoria is also consistent with the literature. We find that the reform improved Agreeableness, Openness to Experience, and internal Locus of Control (for men only), traits associated with altruism, educational opportunities, and

labour market success, respectively. It is not unlikely that these benefits were mediated through improved post-secondary education. Previous research has shown that university education in Australia (Kassenboehmer et al., 2018) and the United States (Heckman et al., 2006) can boost these skills.

We can only speculate on why men and women were affected in such different ways by the reform. One explanation is that the reform affected different channels through which higher levels of education, that were induced by the reform, affected capital development. The reform clearly boosted educational opportunities for women in Victoria, who were now more likely to complete secondary education which also increased their probabilities to obtain vocational or university training. For women, the additional years spent in high school and higher completion rates led to a transition away from manual labour jobs to more professional occupations. This finding is in line with evidence from Sweden, where women benefitted more than men in both education and labour market outcomes (Meghir & Palme, 2005; Fischer et al., 2020). What is surprising however, is that women did not experience better cognitive outcomes in older age. It has been suggested elsewhere that cognitively demanding jobs might protect individuals from cognitive decline in older age (Pool et al., 2016; Fisher et al., 2014).

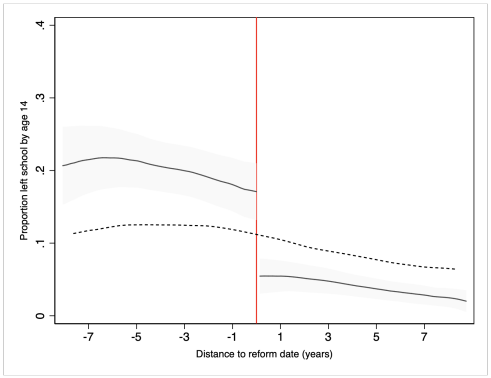
Finally, all men and women tended to be more satisfied with their family life as a consequence of the reform. The reason may be because the reform increased the age at which men and women married and had children for the first time. It also reduced the total number of children that reform-affected individuals had. The combined effects of better education, more experience when founding a family, and potentially better marital matches may explain why most groups had lower divorce rates. The strongest effect was observed for men in Victoria. It is thus not surprising that for these men the reform caused the highest gains in life satisfaction overall. These happiness-related, non-market benefits of education are consistent with findings in Oreopoulos & Salvanes (2011), Oreopoulos (2007), and Geruso & Royer (2018).

Although tentative, our findings suggest that the non-market benefits may outweigh the high costs of forcing children at the margin to stay longer in school in some institutional settings. As in the case of Victoria, MSLA reforms may have supported female empowerment during a time when women were expected to be the homemaker or work in low-skilled professions. These

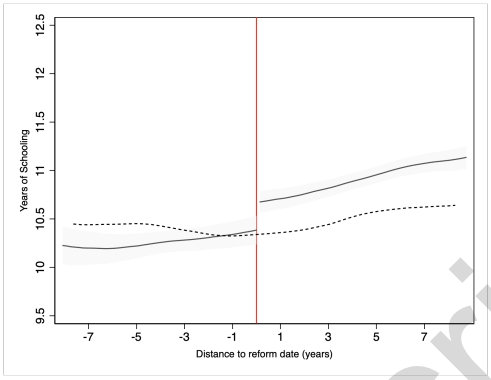
findings are highly policy relevant and important in today's cultural context. Similar to many other countries worldwide, Australian states and territories have been increasing the compulsory school leaving age continuously from age 15 to age 17 since 2006 with the aim to improve the labor-market prospects of adolescents. The long-term effects of such policy changes cannot be evaluated yet, because the affected children have been in the labor market for less than ten years. A comprehensive evaluation of the 1963 and 1964 reforms, may shed light on the longer-term benefits of more recent reforms. Such insights are critical in light of recent discussions on whether more years of education lead to more learning (Angrist et al., 2019).

Figures and Tables

Panel A: Pooled sample

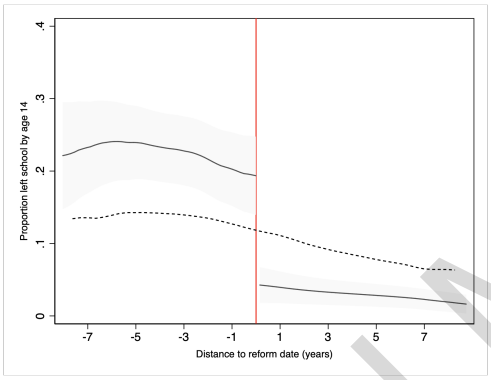


(a) Left school by age 14

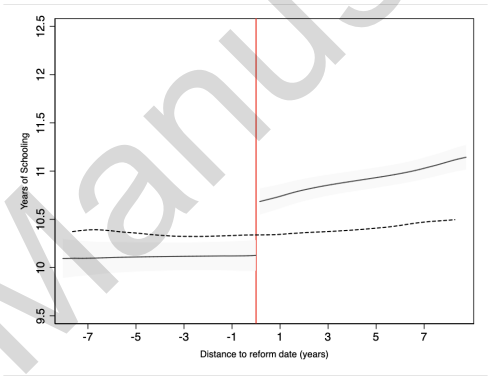


(b) Average years of schooling

Panel B: Female sample

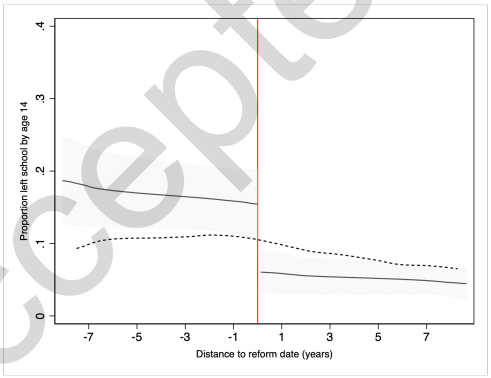


(c) Left school by age 14

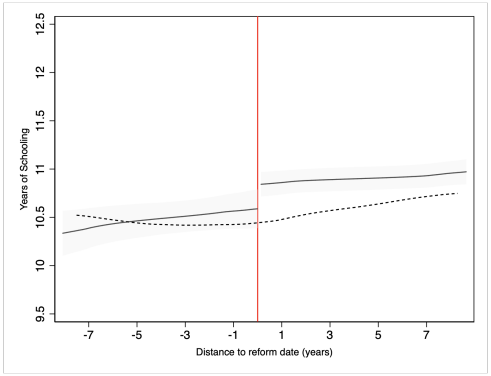


(d) Average years of schooling

Panel C: Male sample



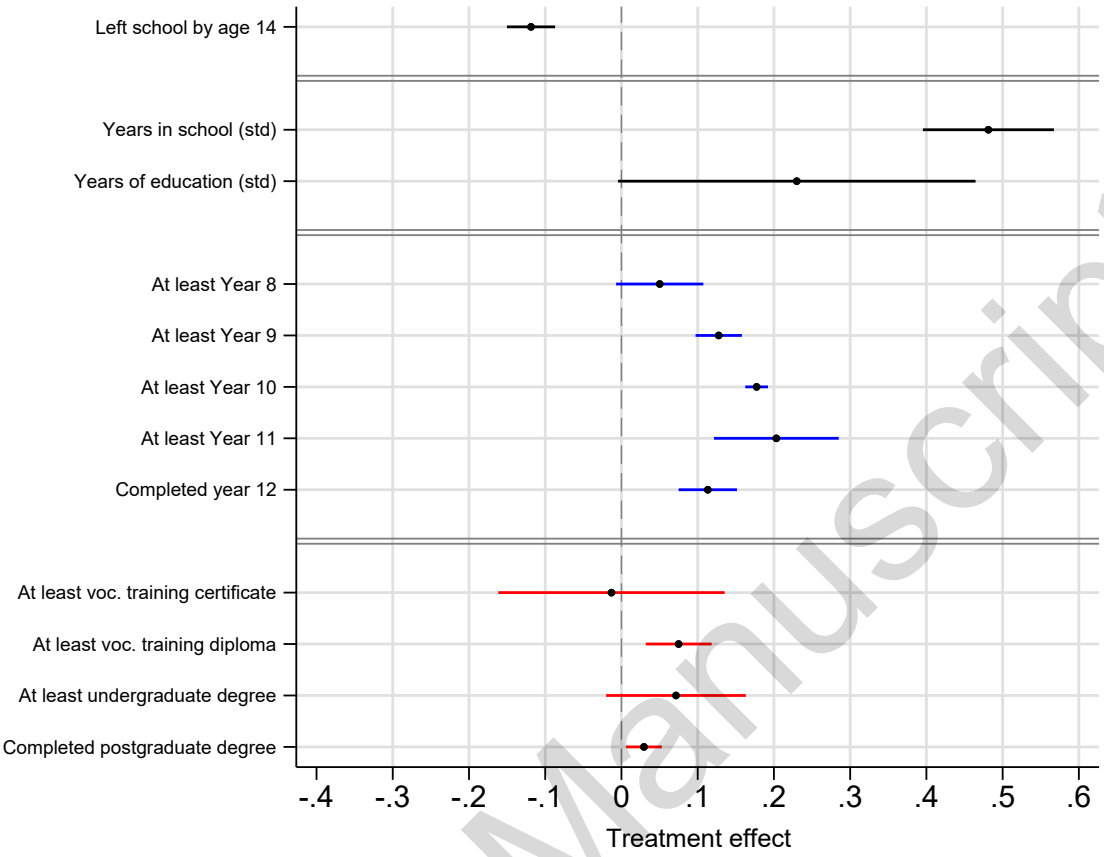
(e) Left school by age 14



(f) Average years of schooling

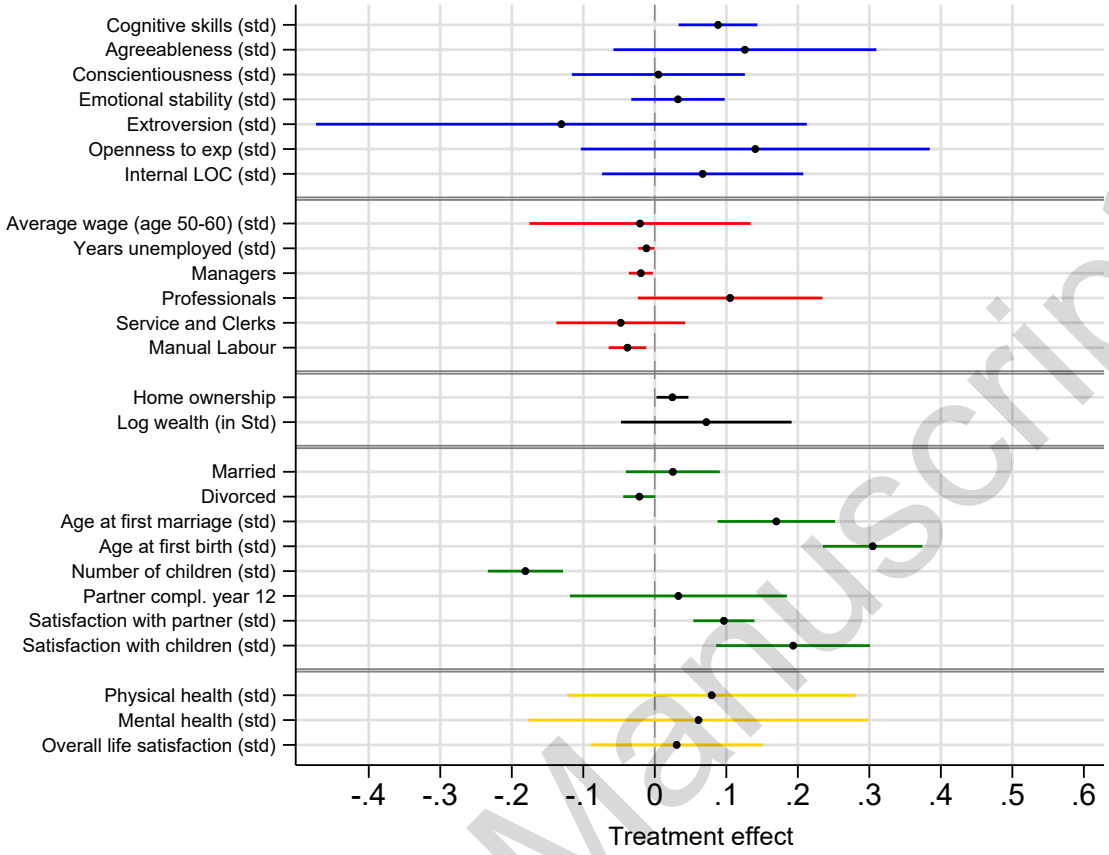
Notes: Graphs are based on kernel-weighted local polynomial smoothing with kernel bandwidth=3 and degree of the polynomial smooth=0. Sample size: Panel A=1806, Panel B=961, Panel C=845.

Figure 1: Reform effect on formal education



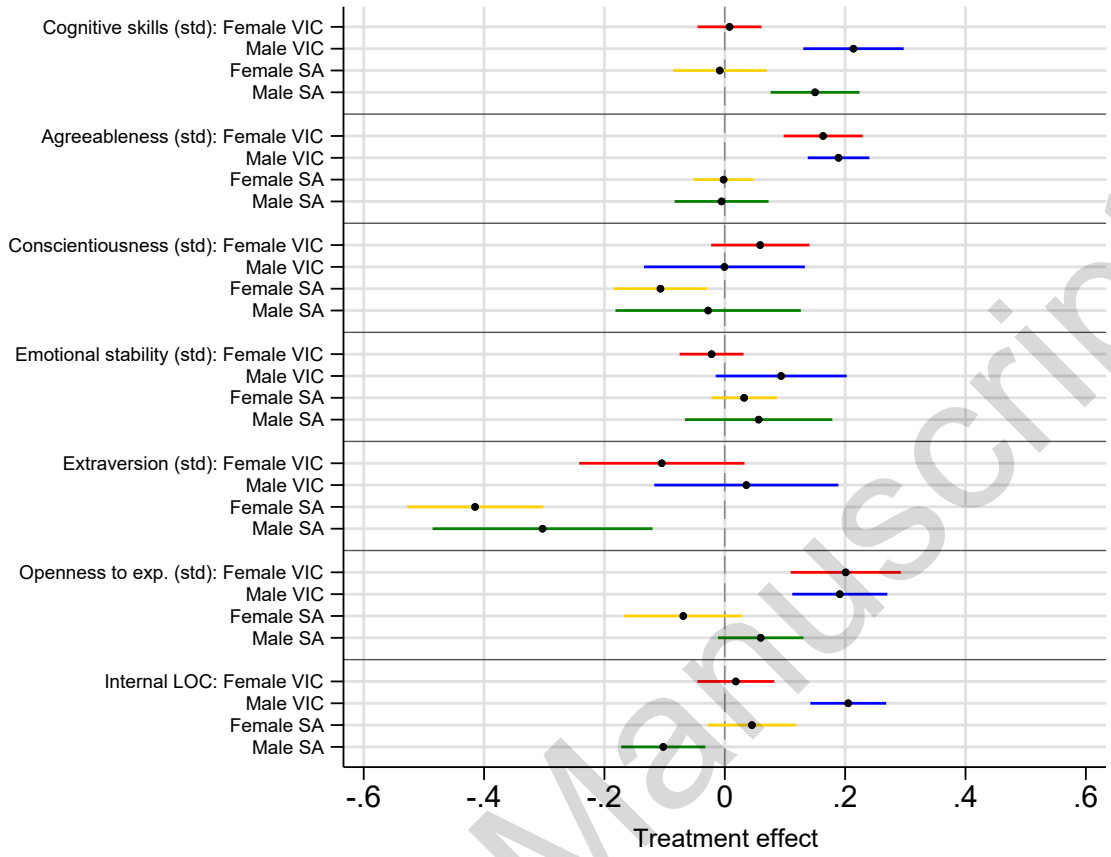
Notes: Reported are the estimates of the impact of the MSLA reform on education outcomes and 95% confidence intervals. Sample size: N=1806.

Figure 2: Reform effect on education



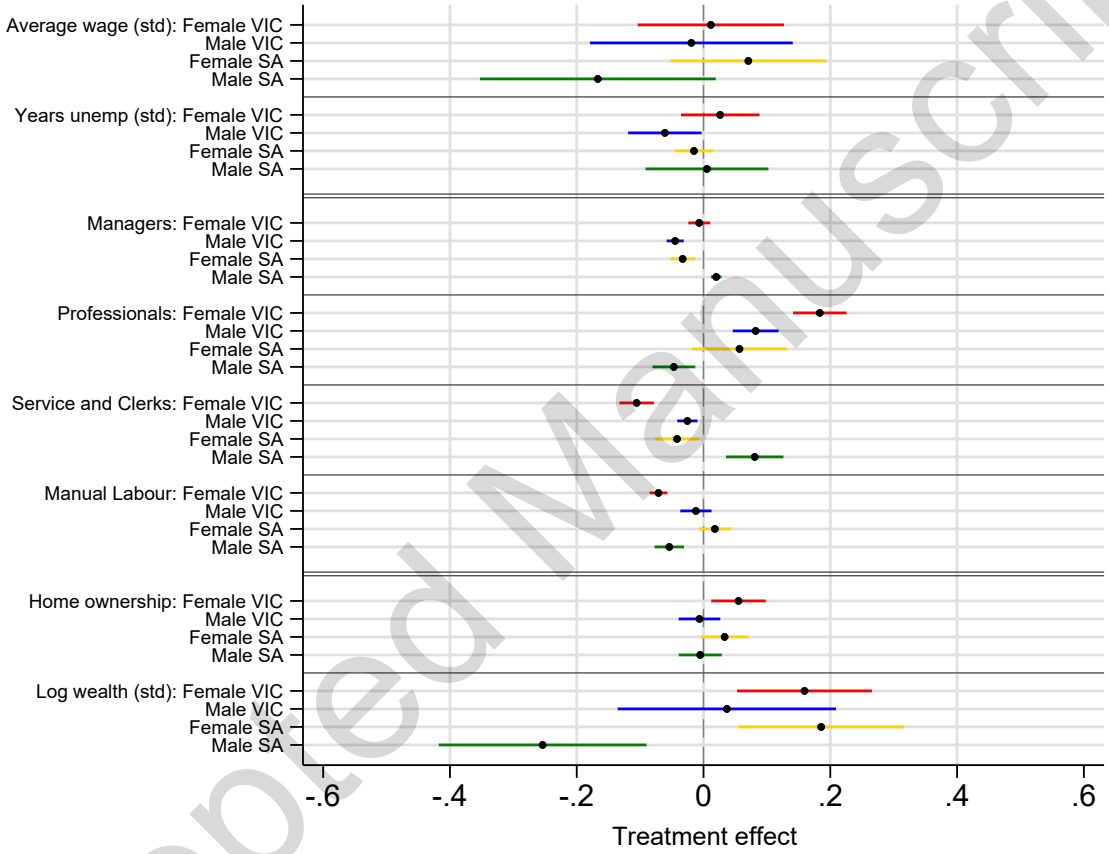
Notes: Reported are the estimates of the impact of the MSLA reform on education outcomes. Capped lines represent 95% confidence intervals. Sample size: N=1806.

Figure 3: Reform effect on long-term outcomes



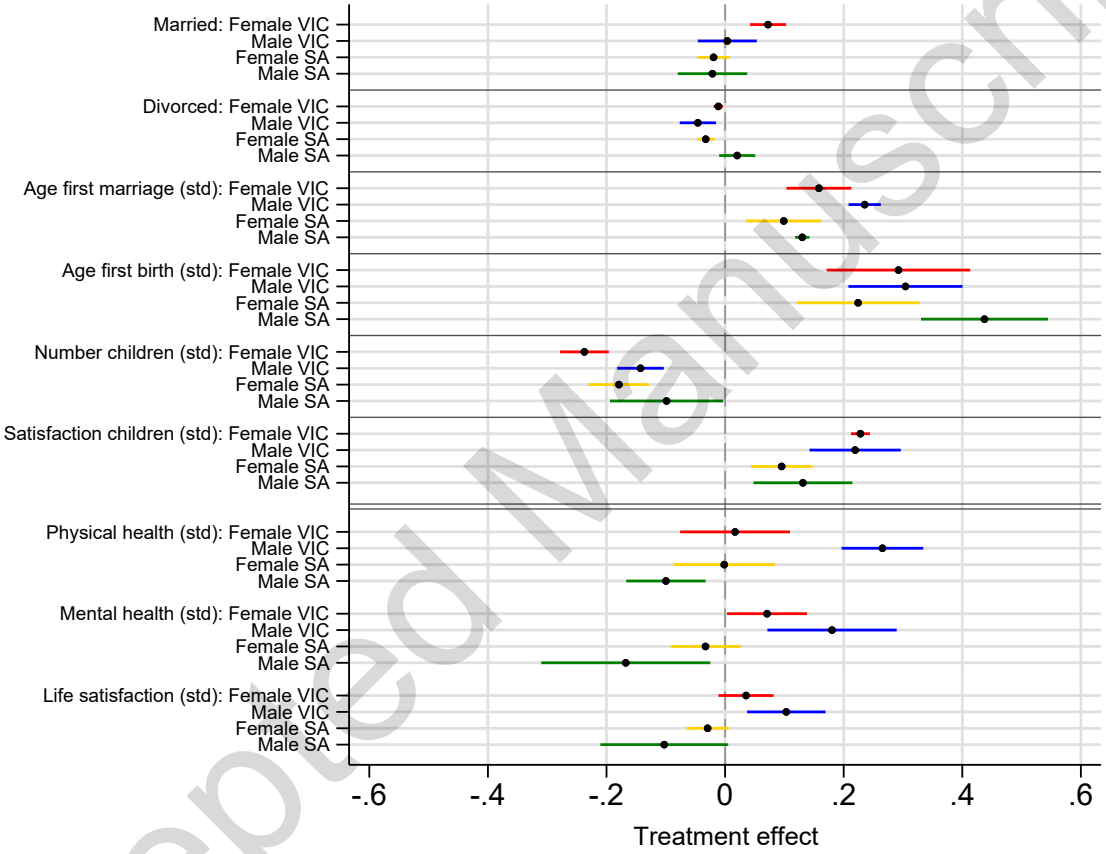
Notes: Reported are the estimates of the impact of the MSLA reform on long-term outcomes, separately for females in Victoria, males in Victoria, females in South Australia, and males in South Australia. Capped lines represent 95% confidence intervals. Sample size: N=1806.

Figure 4: Reform effect on skills by gender and state



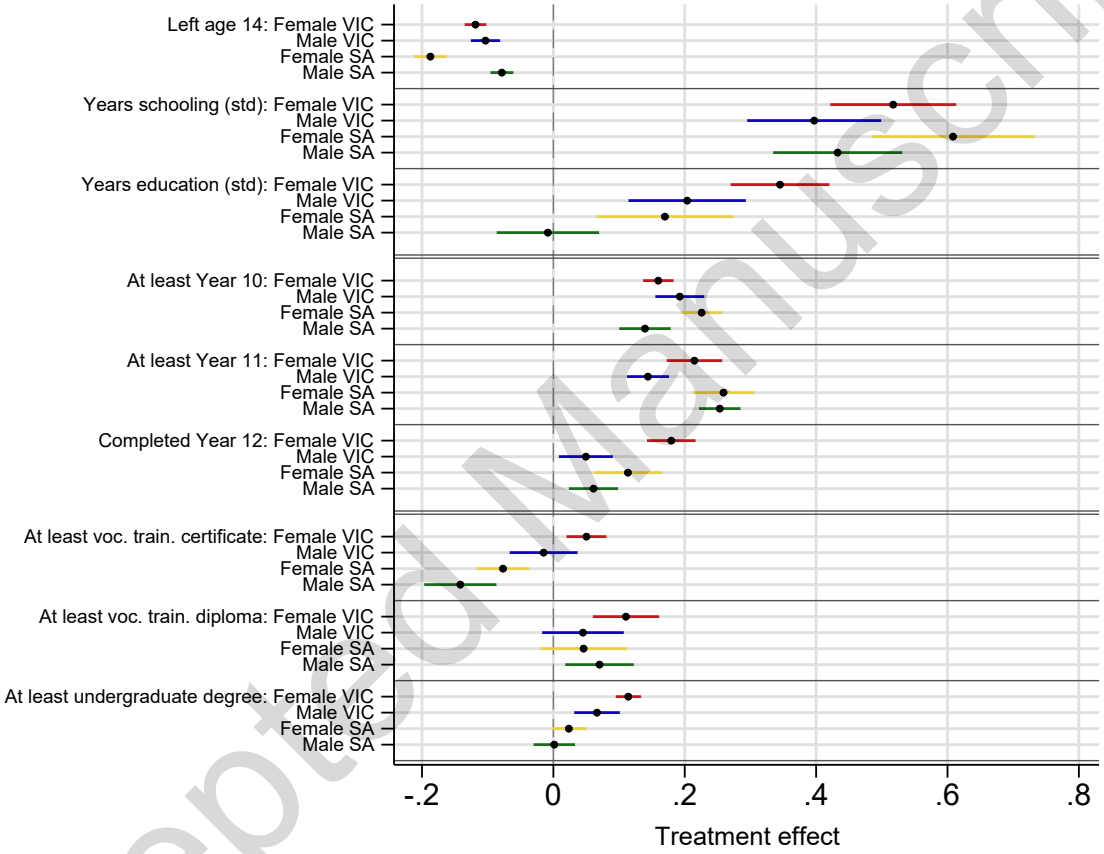
Notes: Reported are the estimates of the impact of the MSLA reform on long-term outcomes, separately for females in Victoria, males in Victoria, females in South Australia, and males in South Australia. Capped lines represent 95% confidence intervals. Sample size: N=1806.

Figure 5: Reform effect on labour market outcomes and financial capital by gender and state



Notes: Reported are the estimates of the impact of the MSLA reform on long-term outcomes, separately for females in Victoria, males in Victoria, females in South Australia, and males in South Australia. Capped lines represent 95% confidence intervals. Sample size: N=1806.

Figure 6: Reform effect on family and health capital by gender and state



Notes: Reported are the estimates of the impact of the MSLA reform on long-term outcomes, separately for females in Victoria, males in Victoria, females in South Australia, and males in South Australia. Capped lines represent 95% confidence intervals. Sample size: N=1806.

Figure 7: Reform effect on education by gender and state

Table 1: Australian minimum school leaving age reforms

Date of proclamation	State	Minimum school leaving age
01/01/1943*	Australian Capital Territory (ACT)	15th birthday
01/01/1943*	New South Wales (NSW)	15th birthday
01/02/1946	Tasmania (TAS)	16th birthday
04/04/1963	South Australia (SA)	15th birthday
04/02/1964	Victoria (VIC)	15th birthday
24/12/1964**	Queensland (QLD)	15th birthday
17/12/1965**	Northern Territory (NT)	15th birthday
01/01/1966*	Western Australia (WA)	End of the year child turned 15

Notes: * Gradual increase of the MSLA (in NSW and ACT: 1 Jan 1941: 14 years 4 months, 1 Jan. 1942: 14 years 8 months; in WA: 8 Nov 1962: end of year child turned 14. ** Date at which the Ordinance/Act was assented to. Source: All dates from the state government gazettes. Minimum school leaving age before proclamation 14th birthday.

Table 2: Summary statistics of main outcome variables

	Mean	SD	Min.	Max.	N	Wave
Formal education						
Panel A: Secondary education						
Left school by age 14	0.102	0.303	0.000	1.000	1806	16
At least Year 8	0.968	0.175	0.000	1.000	1806	16
At least Year 9	0.914	0.280	0.000	1.000	1806	16
At least Year 10	0.810	0.393	0.000	1.000	1806	16
At least Year 11	0.491	0.500	0.000	1.000	1806	16
Completed Year 12	0.348	0.476	0.000	1.000	1806	16
Years of schooling	10.563	1.291	8.000	12.000	1806	16
Panel B: Postsecondary education						
At least voc. training: certificate	0.577	0.494	0.000	1.000	1806	16
At least voc. training: diploma	0.342	0.475	0.000	1.000	1806	16
At least undergraduate degree	0.238	0.426	0.000	1.000	1806	16
Completed postgraduate degree	0.047	0.211	0.000	1.000	1806	16
Years of education	12.029	2.432	8.000	17.000	1806	16
Long-run capital effects						
Panel C: Skills						
Cognitive skills	0.000	1.000	-3.077	2.712	1650	12, 16
Agreeableness	5.498	0.802	1.000	7.000	1714	5, 9, 13
Conscientiousness	5.235	0.956	1.667	7.000	1714	5, 9, 13
Emotional stability	5.432	0.963	1.333	7.000	1714	5, 9, 13
Extraversion	4.359	1.044	1.333	7.000	1714	5, 9, 13
Openness to experience	4.157	0.996	1.000	7.000	1714	5,9, 13
Internal locus of control	5.353	0.966	1.214	7.000	1775	3, 4, 7, 11, 15
Panel D: Labour capital						
Wage (age 50-60)	1174.028	690.480	43.000	7942.455	934	1-16
Years unemployed	0.648	1.997	0.000	29.000	1449	1-16
Managers	0.108	0.310	0.000	1.000	1152	1-6
Professional	0.374	0.484	0.000	1.000	1152	1-6
Service and Clerks	0.282	0.450	0.000	1.000	1152	1-6
Manual Labour	0.236	0.425	0.000	1.000	1152	1-6
Panel E: Financial capital						
Home ownership	0.851	0.356	0.000	1.000	1804	16
Log wealth	13.211	1.593	1.498	16.834	1754	2, 6, 10, 14
Panel F: Family capital						
Married	0.708	0.455	0.000	1.000	1806	16
Divorced	0.164	0.370	0.000	1.000	1806	16
Partner compl. year 12	0.363	0.481	0.000	1.000	1201	16
Age at first marriage	24.143	5.994	14.000	66.000	1668	16
Age at first birth	26.225	5.657	11.000	52.000	1587	16
Number of children	2.373	1.412	0.000	10.000	1805	16

Continued on next page

Table 2 – *Continued from previous page*

Satisfaction with partner	8.124	1.847	0.000	10.000	1577	1-16
Satisfaction with children	8.243	1.547	0.000	10.000	1621	1-16
Panel G: Health capital						
Physical health	78.179	19.882	0.000	100.000	1804	1-16
Mental health	75.803	14.282	16.267	100.000	1803	1-16
Overall life satisfaction	8.211	1.344	0.000	10.000	1806	1-16

Notes: Summary statistics based on main sample. In the estimations, all skill variables are standardized with mean zero and standard deviation one. If more than one wave is named, an individual's value is calculated as the mean of all waves mentioned. Source: Hilda survey waves 2001-2016, own calculation. Significance level: *<0.1, **<0.05, ***<0.01. Source: HILDA survey waves 2001-2016, own calculation.

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Online Appendix

A-Summary statistics

Table A1: Summary statistics pre-reform period

	VIC		SA		NSW/ACT	
	Mean	SD	Mean	SD	Mean	SD
Formal education						
Panel A: Secondary education						
Left school by age 14	0.182	0.386	0.237	0.428	0.126	0.333
At least Year 8	0.948	0.221	0.887	0.319	0.970	0.170
At least Year 9	0.855	0.353	0.814	0.391	0.918	0.275
At least Year 10	0.703	0.458	0.732	0.445	0.825	0.380
At least Year 11	0.467	0.500	0.392	0.491	0.390	0.489
Completed Year 12	0.297	0.458	0.227	0.421	0.327	0.470
Years of schooling	10.321	1.412	10.165	1.374	10.461	1.259
Panel B: Postsecondary education						
At least voc. training: certificate	0.530	0.500	0.567	0.498	0.513	0.501
At least voc. training: diploma	0.336	0.473	0.299	0.460	0.338	0.474
At least undergraduate degree	0.218	0.414	0.216	0.414	0.245	0.431
Completed postgraduate degree	0.027	0.163	0.021	0.143	0.059	0.237
Years of education	11.706	2.481	11.742	2.522	11.896	2.549
Long-run outcomes						
Panel C: Skills						
Cognitive skills	-0.132	1.052	-0.154	0.930	-0.173	1.025
Agreeableness	5.489	0.806	5.617	0.787	5.511	0.854
Conscientiousness	5.185	1.001	5.414	0.887	5.226	0.995
Emotional stability	5.511	1.001	5.606	0.830	5.526	0.986
Extraversion	4.391	1.021	4.658	1.047	4.267	0.982
Openness to experience	4.040	1.003	4.135	0.950	4.120	1.016
Internal locus of control	5.346	1.022	5.499	1.087	5.394	0.936
Panel D: Labour capital						
Weekly wage (age 50-60)	1060.697	680.370	1036.312	762.183	1057.683	567.671
Years unemployed	0.470	1.902	0.311	1.221	0.615	2.158
Managers	0.123	0.330	0.091	0.290	0.122	0.329
Professional	0.356	0.480	0.455	0.503	0.395	0.490
Service and Clerks	0.269	0.445	0.255	0.440	0.245	0.431
Manual Labour	0.251	0.435	0.200	0.404	0.238	0.427
Panel E: Financial capital						
Home ownership	0.863	0.344	0.866	0.342	0.836	0.371
Log wealth	13.341	1.506	13.162	1.726	13.300	1.635
Panel F: Family capital						

Continued on next page

Table A1 – *Continued from previous page*

Married	0.697	0.460	0.742	0.440	0.684	0.466
Divorced	0.170	0.376	0.134	0.342	0.167	0.374
Partner compl. year 12	0.270	0.445	0.343	0.478	0.308	0.463
Age at first marriage	23.624	5.259	23.186	4.275	23.972	6.027
Age at first birth	25.217	4.598	25.189	4.836	25.754	4.989
Number of children	2.582	1.442	2.412	1.152	2.383	1.275
Satisfaction with partner	8.217	2.008	8.646	1.368	8.436	1.717
Satisfaction with children	8.331	1.612	8.532	1.316	8.510	1.344
Panel G: Health capital						
Physical health	75.764	20.344	75.123	21.167	75.567	20.006
Mental health	76.626	14.511	78.548	14.467	77.811	13.906
Overall life satisfaction	8.436	1.354	8.443	1.338	8.398	1.285

Notes: Summary statistics based on all pre-reform cohorts. In the estimations, all skill variables are standardised with mean 0 and standard deviation 1. Source: HILDA survey waves 2001-2016, own calculation.

B-Additional estimation results

Table B1: Full estimation results for left school by age 14

	Pooled (1)	**By gender** (2)	**By state** (3)
Reform × Post	-0.119***	-0.140**	-0.112***
	(0.010)	(0.025)	(0.004)
Reform × Post× male		0.047	
		(0.035)	
Reform × Post× SA			-0.023***
			(0.003)
Reform	0.104***	0.102***	0.116***
	(0.008)	(0.007)	(0.004)
Post	-0.005	-0.006	-0.004
	(0.014)	(0.014)	(0.014)
Linear cohort trend	-0.005**	-0.005**	-0.005**
	(0.002)	(0.002)	(0.002)
Victoria	-0.039***	-0.038***	-0.053***
	(0.002)	(0.002)	(0.000)
ACT	-0.031**	-0.030**	-0.030**
	(0.008)	(0.008)	(0.008)
Female	0.011	0.026*	0.011
	(0.010)	(0.010)	(0.011)
Low socioeconomic status	0.079**	0.078**	0.079**
	(0.015)	(0.014)	(0.016)
Mother employed at age 14	-0.010	-0.009	-0.010
	(0.014)	(0.014)	(0.014)
Father employed at age 14	-0.057	-0.058	-0.057
	(0.030)	(0.030)	(0.030)
At least one parent born abroad	-0.003	-0.000	-0.003
	(0.012)	(0.013)	(0.012)
Oldest child in the household	-0.068**	-0.069**	-0.067**
	(0.015)	(0.015)	(0.015)
Number of siblings	0.055***	0.057***	0.055***
	(0.007)	(0.007)	(0.007)
Grew up with single parent	0.026	0.026	0.026
	(0.026)	(0.026)	(0.026)
Constant	0.114**	0.106**	0.114**
	(0.033)	(0.032)	(0.033)

Notes: Standard errors are reported in parentheses. Significance level: *<0.1, **<0.05, ***<0.01.
Source: HILDA survey waves 2001-2016, own calculation. N=1,806.

Table B2: Impact of the MSLA reform on secondary and postsecondary education

	Pooled	**By gender**		**By state**	
	Reform × Post	Reform × Post	Reform × Post × Male	Reform × Post	Reform × Post × SA
	(1)	(2)	(3)	(4)	(5)
Panel A: Secondary education					
Left school by age 14	-0.119***	-0.140**	0.047	-0.112***	-0.023***
	(0.010)	(0.025)	(0.035)	(0.004)	(0.003)
At least Year 8	0.050*	0.050*	0.001	0.035***	0.056***
	(0.018)	(0.020)	(0.005)	(0.002)	(0.001)
At least Year 9	0.128***	0.136***	-0.020	0.120***	0.027***
	(0.010)	(0.022)	(0.031)	(0.003)	(0.003)
At least Year 10	0.177***	0.181***	-0.008	0.175***	0.009**
	(0.005)	(0.019)	(0.035)	(0.003)	(0.002)
At least Year 11	0.203***	0.225***	-0.048	0.183***	0.075***
	(0.026)	(0.017)	(0.030)	(0.011)	(0.001)
Completed Year 12	0.113***	0.160**	-0.104*	0.121***	-0.030**
	(0.012)	(0.028)	(0.041)	(0.007)	(0.005)
Years in school	0.621***	0.702***	-0.179*	0.599***	0.082***
	(0.035)	(0.046)	(0.063)	(0.023)	(0.011)
Panel B: Postsecondary education					
At least voc. training: certificate	-0.013	0.016	-0.066***	0.021	-0.128***
	(0.047)	(0.043)	(0.008)	(0.012)	(0.006)
At least voc. training: diploma	0.075**	0.091*	-0.035	0.081***	-0.022**
	(0.014)	(0.029)	(0.044)	(0.010)	(0.005)
At least undergraduate degree	0.072*	0.089*	-0.039**	0.093***	-0.079***
	(0.029)	(0.028)	(0.009)	(0.004)	(0.003)
Completed postgraduate degree	0.030**	0.028	0.004	0.027**	0.010**
	(0.007)	(0.013)	(0.020)	(0.006)	(0.002)
Years of education	0.559*	0.728**	-0.377**	0.687***	-0.473***
	(0.179)	(0.151)	(0.097)	(0.051)	(0.027)

Notes: Reported are estimated coefficients from the benchmark Diff-in-Diff model, as outlined in Eq. (1). Column (1) refers to the average effect of the reform in both Victoria and South Australia. Columns (2) and (3) report the effect of the reform and the interaction of the reform with male, respectively. Columns (4) and (5) show the reform effect and the interaction of the reform with South Australia, respectively. Panel B: Reported are degrees as outlined in the Australian Qualifications Framework which classifies degrees by the level of skill that a worker/employee acquires. Certificates I-IV comprise basic qualifications and prepare candidates for both employment and further education and training. Entry into Certificate III and IV courses requires the completion of Year 10 or Year 11 high school education respectively. Certificates III and IV replace the previous system of trade certificates and provide training in more advanced skills and knowledge. These courses are usually delivered by TAFE colleges, community education centres and registered private training providers. Courses at Diploma, Advanced Diploma and Associate degree level take between one and three years to complete, and are generally considered to be equivalent to one to two years of study at university degree level. Entry into Diploma and Advanced Diploma courses requires the completion of Year 12 education. Standard errors are reported in parentheses. Significance level: *<0.1, **<0.05, ***<0.01. Source: HILDA survey waves 2001-2016, own calculation.

Table B3: Impact of the MSLA reform on long-term outcomes

	Pooled	**By gender**		**By state**	
	Reform × Post	Reform × Post	Reform × Post × Male	Reform × Post	Reform × Post × SA
	(1)	(2)	(3)	(4)	(5)
Panel A: Skills					
Cognitive skills	0.088**	0.005	0.189***	0.099***	-0.037*
	(0.017)	(0.020)	(0.021)	(0.016)	(0.014)
Agreeableness	0.126	0.120*	0.014	0.175***	-0.179***
	(0.058)	(0.042)	(0.038)	(0.013)	(0.017)
Conscientiousness	0.005	0.010	-0.011	0.032**	-0.100***
	(0.038)	(0.043)	(0.049)	(0.007)	(0.007)
Emotional stability	0.032	-0.004	0.083	0.029	0.011*
	(0.021)	(0.020)	(0.053)	(0.022)	(0.004)
Extraversion	-0.131	-0.188	0.128	-0.043*	-0.322***
	(0.108)	(0.088)	(0.096)	(0.015)	(0.007)
Openness to experience	0.141	0.124	0.038	0.196***	-0.203***
	(0.077)	(0.095)	(0.042)	(0.027)	(0.001)
Internal locus of control	0.067	0.036*	0.070	0.103***	-0.132***
	(0.044)	(0.013)	(0.101)	(0.006)	(0.008)
Panel B: Labour Capital					
Wage (age 50-60)	-14.252	23.265	-68.167	-3.890	-39.340*
	(33.548)	(26.504)	(48.942)	(29.326)	(13.710)
Years unemployed	-0.023**	0.023**	-0.099**	-0.029**	0.021
	(0.007)	(0.006)	(0.020)	(0.006)	(0.031)
Managers	-0.019**	-0.016	-0.008	-0.024***	0.020***
	(0.005)	(0.010)	(0.029)	(0.002)	(0.001)
Professionals	0.105*	0.153**	-0.104***	0.137***	-0.131**
	(0.041)	(0.041)	(0.010)	(0.011)	(0.025)
Service and Clerks	-0.048	-0.092**	0.095**	-0.069***	0.089**
	(0.028)	(0.019)	(0.021)	(0.006)	(0.019)
Manual Labour	-0.038**	-0.046	0.017	-0.044***	0.022**
	(0.008)	(0.029)	(0.049)	(0.006)	(0.006)
Panel C: Financial Capital					
Home ownership	0.025**	0.049*	-0.054	0.028**	-0.011***
	(0.007)	(0.017)	(0.026)	(0.006)	(0.001)
Log wealth	0.115	0.280**	-0.374	0.170***	-0.203***
	(0.060)	(0.058)	(0.211)	(0.014)	(0.018)
Panel D: Family Capital					
Married (incl. de facto)	0.025	0.046	-0.046	0.042***	-0.060***
	(0.021)	(0.035)	(0.038)	(0.004)	(0.002)
Divorced	-0.022*	-0.020	-0.004	-0.027**	0.021***
	(0.007)	(0.010)	(0.033)	(0.005)	(0.003)
Partner compl. year 12	0.033	-0.018	0.103***	0.076**	-0.153***
	(0.048)	(0.049)	(0.015)	(0.022)	(0.010)
Age at first marriage	1.019***	0.857***	0.363	1.152***	-0.479***
	(0.155)	(0.117)	(0.201)	(0.058)	(0.040)
Age at first birth	1.724***	1.521***	0.454	1.678***	0.170**

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Table B3 – *Continued from previous page*

	(0.124)	(0.214)	(0.377)	(0.125)	(0.053)
Number of children	-0.255***	-0.312***	0.127**	-0.275***	0.074
	(0.023)	(0.031)	(0.035)	(0.008)	(0.032)
Satisfaction with partner	0.179***	0.292***	-0.244*	0.196***	-0.066**
	(0.025)	(0.024)	(0.087)	(0.018)	(0.015)
Satisfaction with children	0.299**	0.296**	0.008	0.347***	-0.172***
	(0.052)	(0.071)	(0.059)	(0.018)	(0.018)
Panel E: Health Capital					
Physical health	1.582	0.434	2.566	2.571**	-3.655***
	(1.262)	(0.554)	(2.175)	(0.476)	(0.088)
Mental health	0.873	0.713	0.358	1.728***	-3.156***
	(1.066)	(0.624)	(1.262)	(0.187)	(0.135)
Overall life satisfaction	0.041	0.030	0.026	0.089***	-0.178***
	(0.051)	(0.038)	(0.047)	(0.007)	(0.019)

Notes: Reported are estimated coefficients from the benchmark Diff-in-Diff model, as outlined in Eq. (1). Column (1) refers to the average effect of the reform in both Victoria and South Australia. Columns (2) and (3) report the effect of the reform and the interaction of the reform with male, respectively. Columns (4) and (5) show the reform effect and the interaction of the reform with South Australia, respectively. All skill measures in Panel (A) are standardised to mean 0 and standard deviation of 1. Standard errors are reported in parentheses. Significance level: *<0.1, **<0.05, ***<0.01. Source: HILDA survey waves 2001-2016, own calculation.

Table B4: Reform effect by gender within treatment state (Victoria versus South Australia)

	Reform × Post	Reform × Post × **VIC** × **male**	Reform × Post × **SA** × **female**	Reform × Post × **SA** × **male**
	(1)	(2)	(3)	(4)
Panel A: Skills				
Cognitive skills	0.008	0.206***	-0.016	0.142**
	(0.017)	(0.033)	(0.023)	(0.032)
Agreeableness	0.163***	0.026	-0.165***	-0.169**
	(0.021)	(0.028)	(0.021)	(0.040)
Conscientiousness	0.059	-0.060	-0.166***	-0.087
	(0.026)	(0.067)	(0.006)	(0.073)
Emotional stability	-0.022	0.116**	0.054***	0.078*
	(0.017)	(0.026)	(0.003)	(0.033)
Extraversion	-0.105*	0.141	-0.310***	-0.198
	(0.043)	(0.086)	(0.008)	(0.096)
Openness to experience	0.201***	-0.010	-0.270***	-0.141***
	(0.029)	(0.012)	(0.003)	(0.012)
Internal locus of control	0.018	0.187**	0.027	-0.121*
	(0.020)	(0.038)	(0.013)	(0.040)
Panel B: Labor capital				
Weekly wage (age 50-60)	8.006	-21.264	40.820**	-123.111**
	(25.034)	(15.554)	(10.231)	(22.074)
Years unemployed	0.052	-0.174	-0.082*	-0.042
	(0.039)	(0.075)	(0.028)	(0.098)
Managers	-0.007	-0.038**	-0.026***	0.027**
	(0.005)	(0.009)	(0.002)	(0.008)
Professionals	0.183***	-0.101***	-0.127**	-0.230***
	(0.013)	(0.010)	(0.027)	(0.019)
Service and Clerks	-0.105***	0.080***	0.064**	0.186***
	(0.009)	(0.007)	(0.018)	(0.022)
Manual Labour	-0.071***	0.059***	0.089***	0.017*
	(0.004)	(0.005)	(0.009)	(0.006)
Panel C: Financial capital				
Home ownership	0.055**	-0.062*	-0.022***	-0.060*
	(0.014)	(0.022)	(0.002)	(0.022)
Log wealth	0.254**	-0.195	0.042**	-0.658**
	(0.053)	(0.137)	(0.013)	(0.132)
Panel D: Family capital				
Married	0.072***	-0.069*	-0.092***	-0.094**
	(0.010)	(0.024)	(0.002)	(0.027)
Divorced	-0.011**	-0.035**	-0.021**	0.032**
	(0.003)	(0.008)	(0.004)	(0.008)
Partner compl. year 12	0.019	0.115***	-0.140***	-0.054
	(0.019)	(0.019)	(0.007)	(0.029)
Age at first marriage	0.949***	0.463**	-0.356***	-0.169
	(0.103)	(0.124)	(0.036)	(0.085)
Age at first birth	1.654***	0.067	-0.385***	0.821*
	(0.215)	(0.308)	(0.050)	(0.340)

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Table B4 – *Continued from previous page*

Number of children	-0.335***	0.134**	0.082*	0.196**
	(0.018)	(0.032)	(0.033)	(0.058)
Satisfaction with partner	0.259***	-0.142	0.099**	-0.344**
	(0.037)	(0.105)	(0.019)	(0.089)
Satisfaction with children	0.353***	-0.014	-0.206***	-0.150**
	(0.008)	(0.037)	(0.026)	(0.041)
Panel E: Health capital				
Physical health	0.334	4.942***	-0.362**	-2.319***
	(0.579)	(0.394)	(0.102)	(0.382)
Mental health	1.011**	1.565	-1.482***	-3.403**
	(0.303)	(0.702)	(0.109)	(0.837)
Overall life satisfaction	0.048*	0.091	-0.087**	-0.185*
	(0.020)	(0.046)	(0.018)	(0.065)

Notes: Standard errors are reported in parentheses. Significance level: *<0.1, **<0.05, ***<0.01. Source: HILDA survey waves 2001-2016, own calculation.

Table B5: Reform effect on secondary and postsecondary education, by gender within treatment state (Victoria versus South Australia)

	Reform × Post	Reform × Post × **VIC** × **male**	Reform × Post × **SA** × **female**	Reform × Post × **SA** × **male**
	(1)	(2)	(3)	(4)
Panel A: Secondary education				
Left school by age 14	-0.119***	0.015	-0.069***	0.040**
	(0.005)	(0.009)	(0.003)	(0.007)
At least Year 8	0.032***	0.007	0.065***	0.053***
	(0.004)	(0.005)	(0.002)	(0.004)
At least Year 9	0.114***	0.013	0.073***	-0.009
	(0.008)	(0.019)	(0.004)	(0.018)
At least Year 10	0.160***	0.033	0.066***	-0.020
	(0.007)	(0.018)	(0.003)	(0.019)
At least Year 11	0.215***	-0.071***	0.044***	0.039**
	(0.013)	(0.010)	(0.002)	(0.011)
Completed Year 12	0.179***	-0.130***	-0.066***	-0.118***
	(0.012)	(0.020)	(0.006)	(0.018)
Total years in school	0.668***	-0.155	0.118***	-0.109
	(0.039)	(0.066)	(0.015)	(0.065)
Panel B: Postsecondary education				
At least voc. training: certificate	0.050**	-0.065***	-0.127***	-0.192***
	(0.010)	(0.008)	(0.005)	(0.008)
At least voc. training: diploma	0.110***	-0.065	-0.064***	-0.040
	(0.016)	(0.030)	(0.006)	(0.026)
At least undergraduate degree	0.114***	-0.048*	-0.091***	-0.113***
	(0.006)	(0.015)	(0.002)	(0.014)
Completed postgraduate degree	0.020*	0.015	0.026***	0.008
	(0.008)	(0.012)	(0.003)	(0.014)
Total years of education	0.839***	-0.344**	-0.426***	-0.860***
	(0.058)	(0.072)	(0.028)	(0.056)

Notes: Standard errors are reported in parentheses. Significance level: *<0.1, **<0.05, ***<0.01. Source: HILDA survey waves 2001-2016, own calculation.

C-Testing the validity of the Diff-in-Diff approach and robustness checks

Table C1: Baseline model (BL) versus models with non-linear cohort trends (NL)

	Pooled		By Gender				By State			
	BL	NL	BL		NL		BL		NL	
	Reform × Post	Reform × Post	Reform × Post	Reform × Post × Male	Reform × Post	Reform × Post × Male	Reform × Post	Reform × Post × SA	Reform × Post	Reform × Post × SA
Panel A: Secondary education										
Left school by age 14	-0.119***	-0.117***	-0.140**	0.047	-0.139***	0.048	-0.112***	-0.023***	-0.112***	-0.020
	(0.010)	(0.008)	(0.025)	(0.035)	(0.019)	(0.036)	(0.004)	(0.003)	(0.011)	(0.009)
At least Year 8	0.050*	0.045**	0.050*	0.001	0.045**	-0.000	0.035***	0.056***	0.028*	0.062***
	(0.018)	(0.011)	(0.020)	(0.005)	(0.014)	(0.005)	(0.002)	(0.001)	(0.011)	(0.007)
At least Year 9	0.128***	0.126***	0.136***	-0.020	0.136***	-0.020	0.120***	0.027***	0.118***	0.029**
	(0.010)	(0.005)	(0.022)	(0.031)	(0.018)	(0.032)	(0.003)	(0.003)	(0.008)	(0.008)
At least Year 10	0.177***	0.180***	0.181***	-0.008	0.184***	-0.010	0.175***	0.009**	0.179***	0.003
	(0.005)	(0.009)	(0.019)	(0.035)	(0.013)	(0.035)	(0.003)	(0.002)	(0.012)	(0.007)
At least Year 11	0.203***	0.196***	0.225***	-0.048	0.216***	-0.044	0.183***	0.075***	0.174***	0.077***
	(0.026)	(0.022)	(0.017)	(0.030)	(0.018)	(0.029)	(0.011)	(0.001)	(0.018)	(0.003)
Completed Year 12	0.113***	0.098***	0.160**	-0.104*	0.141**	-0.095*	0.121***	-0.030**	0.105***	-0.026*
	(0.012)	(0.016)	(0.028)	(0.041)	(0.031)	(0.038)	(0.007)	(0.005)	(0.009)	(0.009)
Years in school	0.621***	0.600***	0.702***	-0.179*	0.676***	-0.170*	0.599***	0.082***	0.576***	0.083**
	(0.035)	(0.029)	(0.046)	(0.063)	(0.047)	(0.059)	(0.023)	(0.011)	(0.043)	(0.020)
Panel B: Postsecondary education										
At least voc. training: certificate	-0.013	-0.026	0.016	-0.066***	0.004	-0.068***	0.021	-0.128***	0.008	-0.120***
	(0.047)	(0.042)	(0.043)	(0.008)	(0.039)	(0.007)	(0.012)	(0.006)	(0.009)	(0.008)
At least voc. training: diploma	0.075**	0.066**	0.091*	-0.035	0.082*	-0.035	0.081***	-0.022**	0.072***	-0.019**
	(0.014)	(0.015)	(0.029)	(0.044)	(0.033)	(0.044)	(0.010)	(0.005)	(0.010)	(0.004)
At least undergraduate degree	0.072*	0.062	0.089*	-0.039**	0.078*	-0.035**	0.093***	-0.079***	0.083***	-0.073***
	(0.029)	(0.027)	(0.028)	(0.009)	(0.026)	(0.009)	(0.004)	(0.003)	(0.004)	(0.002)

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Table C1 – *Continued from previous page*

Completed postgraduate degree	0.030**	0.025	0.028	0.004	0.023	0.005	0.027**	0.010**	0.021	0.015**
	(0.007)	(0.012)	(0.013)	(0.020)	(0.019)	(0.019)	(0.006)	(0.002)	(0.010)	(0.003)
Years of education	0.559*	0.489*	0.728**	-0.377**	0.651**	-0.362**	0.687***	-0.473***	0.612***	-0.433***
	(0.179)	(0.173)	(0.151)	(0.097)	(0.146)	(0.102)	(0.051)	(0.027)	(0.061)	(0.019)
Panel C: Skills										
Cognitive skills	0.088**	0.080***	0.005	0.189***	-0.004	0.188***	0.099***	-0.037*	0.085***	-0.018
	(0.017)	(0.013)	(0.020)	(0.021)	(0.017)	(0.017)	(0.016)	(0.014)	(0.013)	(0.012)
Agreeableness	0.126	0.105	0.120*	0.014	0.105*	-0.001	0.175***	-0.179***	0.153***	-0.168***
	(0.058)	(0.049)	(0.042)	(0.038)	(0.035)	(0.032)	(0.013)	(0.017)	(0.020)	(0.020)
Conscientiousness	0.005	0.022	0.010	-0.011	0.024	-0.004	0.032**	-0.100***	0.054**	-0.111***
	(0.038)	(0.045)	(0.043)	(0.049)	(0.048)	(0.039)	(0.007)	(0.007)	(0.013)	(0.010)
Emotional stability	0.032	0.044	-0.004	0.083	0.009	0.078	0.029	0.011*	0.037	0.022
	(0.021)	(0.030)	(0.020)	(0.053)	(0.023)	(0.054)	(0.022)	(0.004)	(0.035)	(0.010)
Extraversion	-0.131	-0.129	-0.188	0.128	-0.186	0.128	-0.043*	-0.322***	-0.037	-0.323***
	(0.108)	(0.109)	(0.088)	(0.096)	(0.089)	(0.103)	(0.015)	(0.007)	(0.022)	(0.010)
Openness to experience	0.141	0.140	0.124	0.038	0.125	0.033	0.196***	-0.203***	0.199***	-0.205***
	(0.077)	(0.063)	(0.095)	(0.042)	(0.081)	(0.042)	(0.027)	(0.001)	(0.029)	(0.009)
Internal locus of control	0.067	0.102	0.036*	0.070	0.076**	0.058	0.103***	-0.132***	0.148***	-0.161***
	(0.044)	(0.059)	(0.013)	(0.101)	(0.018)	(0.106)	(0.006)	(0.008)	(0.013)	(0.010)
Panel D: Labour capital										
Wage (age 50-60)	-14.252	-26.277	23.265	-68.167	5.577	-58.756	-3.890	-39.340*	-8.916	-59.393**
	(33.548)	(31.525)	(26.504)	(48.942)	(23.164)	(50.127)	(29.326)	(13.710)	(26.297)	(13.831)
Years unemployed	-0.023**	-0.093**	0.023**	-0.099**	-0.049	-0.095**	-0.029**	0.021	-0.099**	0.022
	(0.007)	(0.022)	(0.006)	(0.020)	(0.028)	(0.017)	(0.006)	(0.031)	(0.022)	(0.041)
Managers	-0.019**	-0.033**	-0.016	-0.008	-0.028	-0.012	-0.024***	0.020***	-0.039***	0.021***
	(0.005)	(0.007)	(0.010)	(0.029)	(0.012)	(0.034)	(0.002)	(0.001)	(0.004)	(0.003)
Professionals	0.105*	0.121*	0.153**	-0.104***	0.174**	-0.116***	0.137***	-0.131**	0.151***	-0.117**
	(0.041)	(0.043)	(0.041)	(0.010)	(0.045)	(0.012)	(0.011)	(0.025)	(0.016)	(0.028)
Service and Clerks	-0.048	-0.048	-0.092**	0.095**	-0.096**	0.106***	-0.069***	0.089**	-0.070***	0.087**
	(0.028)	(0.026)	(0.019)	(0.021)	(0.020)	(0.011)	(0.006)	(0.019)	(0.006)	(0.017)
Manual Labour	-0.038**	-0.040**	-0.046	0.017	-0.050	0.022	-0.044***	0.022**	-0.042**	0.008

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Table C1 – *Continued from previous page*

	(0.008)	(0.011)	(0.029)	(0.049)	(0.033)	(0.052)	(0.006)	(0.006)	(0.011)	(0.008)
Panel E: Financial capital										
Home ownership	0.025**	0.028*	0.049*	-0.054	0.053*	-0.056	0.028**	-0.011***	0.031*	-0.010**
	(0.007)	(0.010)	(0.017)	(0.026)	(0.019)	(0.026)	(0.006)	(0.001)	(0.010)	(0.002)
Log wealth	0.115	0.133**	0.280**	-0.374	0.299**	-0.374	0.170***	-0.203***	0.187***	-0.188***
	(0.060)	(0.041)	(0.058)	(0.211)	(0.068)	(0.223)	(0.014)	(0.018)	(0.026)	(0.008)
Panel F: Family capital										
Married	0.025	0.030	0.046	-0.046	0.053	-0.050	0.042***	-0.060***	0.049***	-0.064***
	(0.021)	(0.022)	(0.035)	(0.038)	(0.037)	(0.039)	(0.004)	(0.002)	(0.004)	(0.002)
Divorced	-0.022*	-0.022	-0.020	-0.004	-0.023*	0.003	-0.027**	0.021***	-0.029**	0.023***
	(0.007)	(0.010)	(0.010)	(0.033)	(0.008)	(0.032)	(0.005)	(0.003)	(0.006)	(0.003)
Partner compl. year 12	0.033	0.003	-0.018	0.103***	-0.046	0.100**	0.076**	-0.153***	0.050**	-0.160***
	(0.048)	(0.052)	(0.049)	(0.015)	(0.063)	(0.025)	(0.022)	(0.010)	(0.010)	(0.007)
Age at first marriage	1.019***	0.738***	0.857***	0.363	0.556**	0.409	1.152***	-0.479***	0.850***	-0.384**
	(0.155)	(0.095)	(0.117)	(0.201)	(0.108)	(0.244)	(0.058)	(0.040)	(0.086)	(0.077)
Age at first birth	1.724***	1.683***	1.521***	0.454	1.472***	0.467	1.678***	0.170**	1.637***	0.158
	(0.124)	(0.141)	(0.214)	(0.377)	(0.161)	(0.369)	(0.125)	(0.053)	(0.125)	(0.110)
Number of children	-0.255***	-0.301***	-0.312***	0.127**	-0.361***	0.133**	-0.275***	0.074	-0.327***	0.090
	(0.023)	(0.016)	(0.031)	(0.035)	(0.024)	(0.037)	(0.008)	(0.032)	(0.042)	(0.042)
Satisfaction with partner	0.179***	0.223**	0.292***	-0.244*	0.346***	-0.259*	0.196***	-0.066**	0.243***	-0.070**
	(0.025)	(0.046)	(0.024)	(0.087)	(0.051)	(0.107)	(0.018)	(0.015)	(0.019)	(0.017)
Satisfaction with children	0.299**	0.322**	0.296**	0.008	0.317**	0.010	0.347***	-0.172***	0.372***	-0.176***
	(0.052)	(0.072)	(0.071)	(0.059)	(0.079)	(0.044)	(0.018)	(0.018)	(0.025)	(0.018)
Panel G: Health capital										
Physical health	1.582	1.797	0.434	2.566	0.608	2.642	2.571**	-3.655***	2.894*	-3.856***
	(1.262)	(1.442)	(0.554)	(2.175)	(0.984)	(2.304)	(0.476)	(0.088)	(0.961)	(0.254)
Mental health	0.873	1.402	0.713	0.358	1.269	0.295	1.728***	-3.156***	2.408***	-3.533***
	(1.066)	(1.312)	(0.624)	(1.262)	(0.716)	(1.416)	(0.187)	(0.135)	(0.383)	(0.090)
Overall life satisfaction	0.041	0.052	0.030	0.026	0.044	0.017	0.089***	-0.178***	0.108**	-0.198**
	(0.051)	(0.043)	(0.038)	(0.047)	(0.036)	(0.059)	(0.007)	(0.019)	(0.031)	(0.035)

Notes: Standard errors are reported in parentheses. Significance level: *<0.1, **<0.05, ***<0.01.

Table C2: Variation of included cohorts

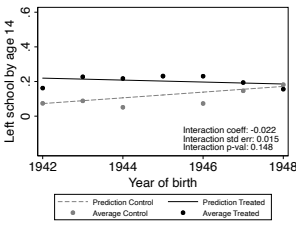
	$+/-8.5$		$+/-7.5$		$+/-6.5$	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
Panel A: Secondary education						
Left school by age 14	-0.135***	(0.006)	-0.119***	(0.010)	-0.114***	(0.006)
At least Year 8	0.056*	(0.019)	0.050*	(0.018)	0.052**	(0.014)
At least Year 9	0.151***	(0.010)	0.128***	(0.010)	0.124***	(0.008)
At least Year 10	0.189***	(0.007)	0.177***	(0.005)	0.188***	(0.003)
At least Year 11	0.205***	(0.033)	0.203***	(0.026)	0.194**	(0.034)
Completed Year 12	0.101***	(0.012)	0.113***	(0.012)	0.088***	(0.009)
Years in school	0.646***	(0.050)	0.621***	(0.035)	0.594***	(0.050)
Panel B: Postsecondary education						
At least voc. training: certificate	-0.015	(0.051)	-0.013	(0.047)	-0.044	(0.045)
At least voc. training: diploma	0.066**	(0.020)	0.075**	(0.014)	0.071**	(0.012)
At least undergraduate degree	0.070	(0.032)	0.072*	(0.029)	0.067*	(0.025)
Completed postgraduate degree	0.022**	(0.006)	0.030**	(0.007)	0.031*	(0.010)
Years of education	0.549*	(0.182)	0.559*	(0.179)	0.492*	(0.164)
Panel C: Skills						
Cognitive ability	0.065**	(0.015)	0.088**	(0.017)	0.097*	(0.041)
Agreeableness	0.123	(0.069)	0.126	(0.058)	0.124**	(0.027)
Conscientiousness	0.031	(0.040)	0.005	(0.038)	0.011	(0.054)
Emotional stability	0.031	(0.022)	0.032	(0.021)	0.021	(0.023)
Extraversion	-0.072	(0.096)	-0.131	(0.108)	-0.108	(0.116)
Openness to experience	0.137	(0.069)	0.141	(0.077)	0.169*	(0.055)
Locus of control	0.008	(0.055)	0.067	(0.044)	0.039	(0.045)
Panel D: Labour capital						
Wage (age 50-60)	-16.048	(27.942)	-14.252	(33.548)	-41.431	(39.195)
Years unemployed	-0.023	(0.016)	-0.023**	(0.007)	-0.042	(0.034)
Managers	-0.023***	(0.003)	-0.019**	(0.005)	-0.002	(0.001)
Professionals	0.086	(0.042)	0.105*	(0.041)	0.068	(0.042)
Service and Clerks	-0.040	(0.023)	-0.048	(0.028)	-0.018	(0.023)
Manual Labour	-0.022	(0.019)	-0.038**	(0.008)	-0.047*	(0.019)
Panel E: Financial capital						
Home ownership	0.024***	(0.004)	0.025**	(0.007)	0.026**	(0.005)
Log wealth	0.147***	(0.023)	0.115	(0.060)	0.193	(0.105)
Panel F: Family capital						
Married	0.024	(0.013)	0.025	(0.021)	0.039	(0.027)
Divorced	-0.024**	(0.007)	-0.022*	(0.007)	-0.034*	(0.013)
Partner compl. year 12	0.052	(0.051)	0.033	(0.048)	0.074	(0.067)
Age at first marriage	0.904***	(0.082)	1.019***	(0.155)	0.928***	(0.147)
Age at first birth	1.531***	(0.153)	1.724***	(0.124)	1.368***	(0.174)
Number of children	-0.327***	(0.033)	-0.255***	(0.023)	-0.146**	(0.032)
Satisfaction with partner	0.021	(0.037)	0.179***	(0.025)	0.229***	(0.021)
Satisfaction with children	0.234**	(0.055)	0.299**	(0.052)	0.317***	(0.042)
Panel G: Health capital						
Physical health	1.450	(1.206)	1.582	(1.262)	0.697	(1.409)
Mental health	0.935	(1.032)	0.873	(1.066)	0.046	(1.542)
Overall life satisfaction	-0.002	(0.064)	0.041	(0.051)	0.062	(0.061)

Continued on next page

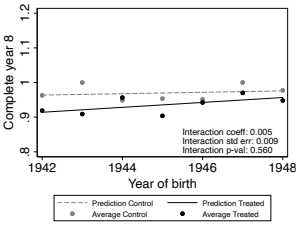
Table C2 – *Continued from previous page*

Notes: Table shows reform effect for different outcomes. The estimations include individuals between 8.5, 7.5 and 6.5 years before and after the reform date. Standard errors are reported in parentheses. Significance level: *<0.1, **<0.05, ***<0.01. Source: HILDA survey waves 2001-2016, own calculation.

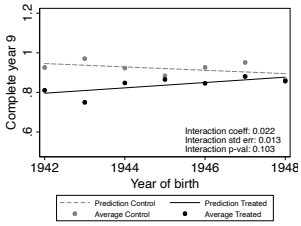
(1) Schooling



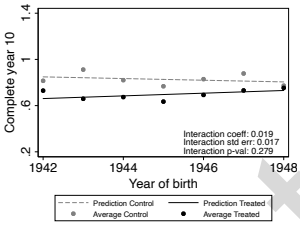
(a) Left school by age 14



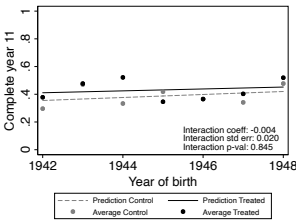
(b) At least Year 8



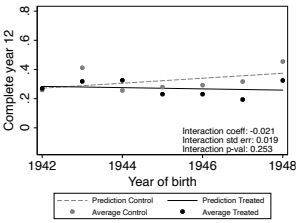
(c) At least Year 9



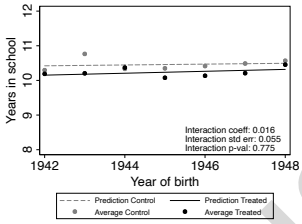
(d) At least Year 10



(e) At least Year 11

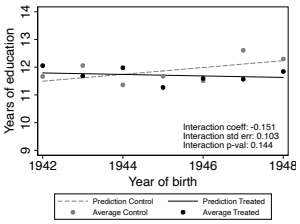


(f) Completed year 12

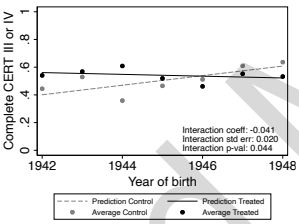


(g) Years of schooling

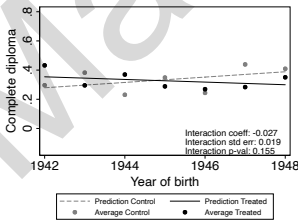
(2) Highest degree obtained



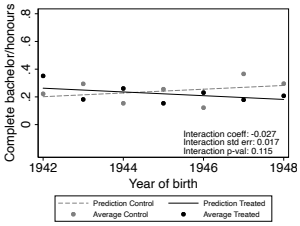
(h) Years of education



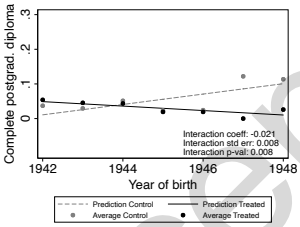
(i) Voc. train.: certif.



(j) Voc. training: Diploma



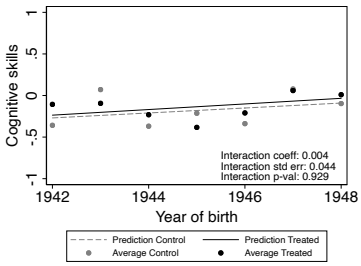
(k) Undergrad. degree



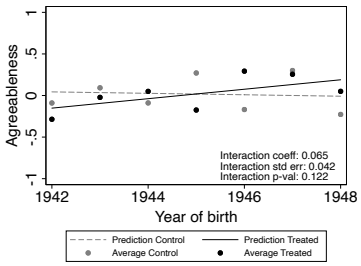
(l) Postgrad. degree

Notes: Graphs show the predictive outcomes for treatment (solid line) and control group (dashed line) over birth cohorts from a regression model at the individual level of each outcome regressed on linear birth cohort trends, reform dummy and interaction between the two. The dots show the means of treatment (black dots) and control group (grey dots) for each outcome variable and birth cohort. Each graph lists the estimated interaction term and its corresponding p-value. A significant interaction term would indicate different pre-treatment trends between treatment and control group.

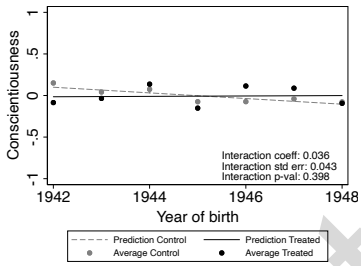
Figure C1: Pre-reform trends - Formal education



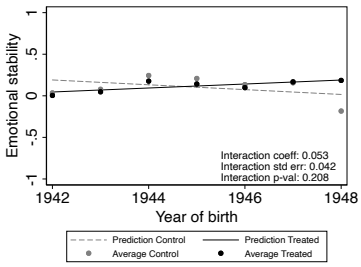
(a) Cognitive skills



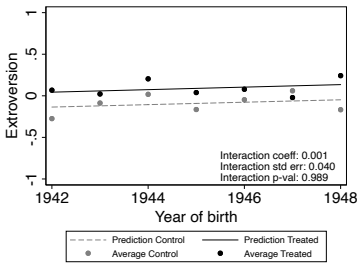
(b) Agreeableness



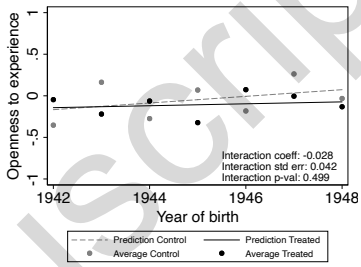
(c) Conscientiousness



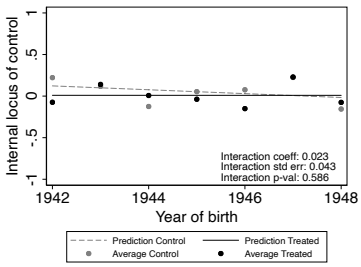
(d) Emotional stability



(e) Extraversion



(f) Openness to exp.

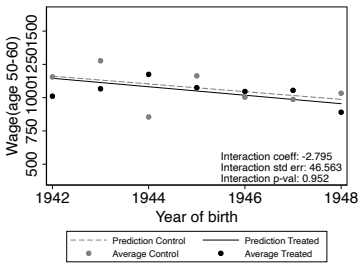


(g) Internal LOC

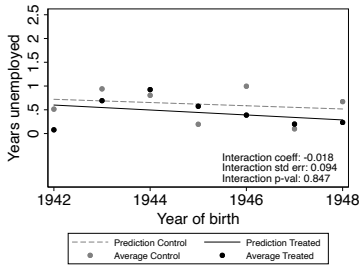
Notes: See Notes Figure C1.

Figure C2: Pre-reform trends - Skills

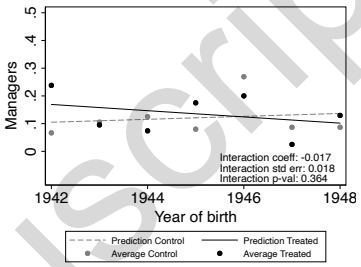
(1) Labor capital



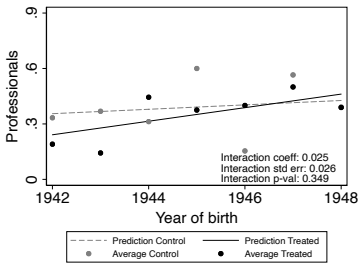
(a) Wage



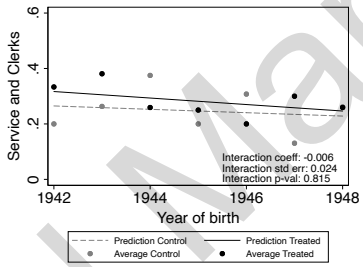
(b) Years unemployed



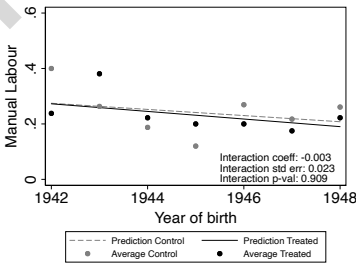
(c) Managers



(d) Professionals

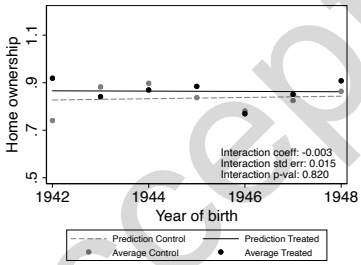


(e) Service and Clerks

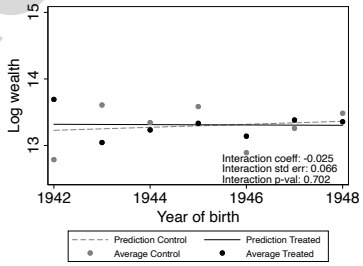


(f) Manual Labour

(2) Financial capital



(g) Home ownership

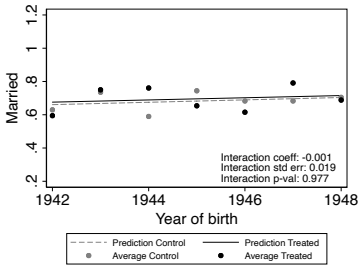


(h) Log wealth

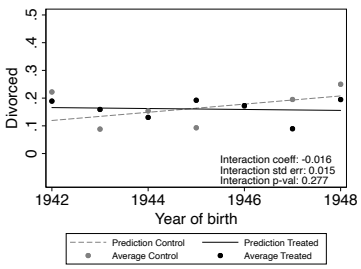
Notes: See Notes Figure C1.

Figure C3: Pre-reform trends - Labor and financial capital

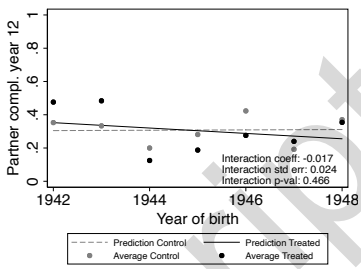
(1) Family capital



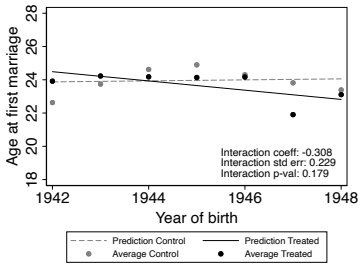
(a) Married



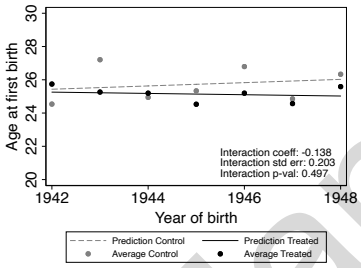
(b) Divorced



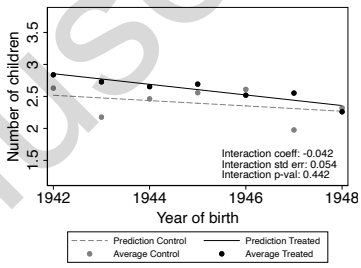
(c) Partner compl. year 12



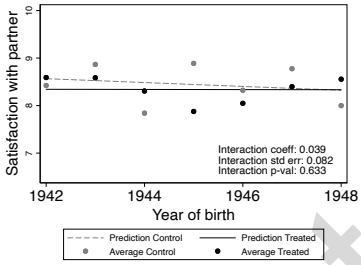
(d) Age at first marriage



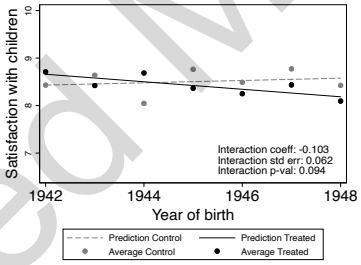
(e) Age at first birth



(f) Number of kids

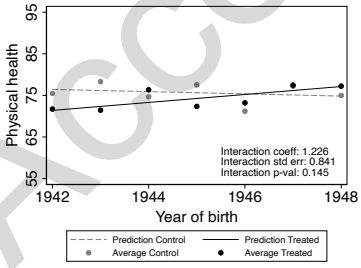


(g) Satisfaction with partner

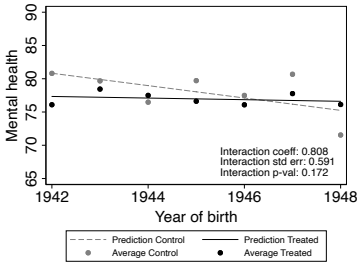


(h) Satisfaction with children

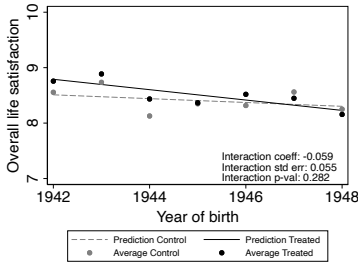
(2) Health capital



(i) Physical health



(j) Mental health

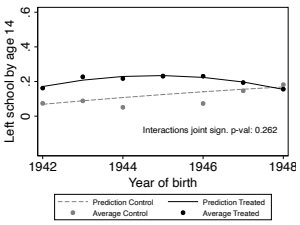


(k) Overall life satisfaction

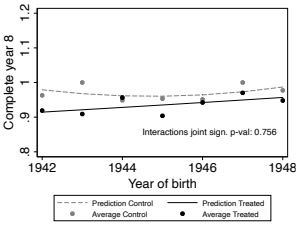
Notes: See Notes Figure C1.

Figure C4: Pre-reform trends - Family and health capital

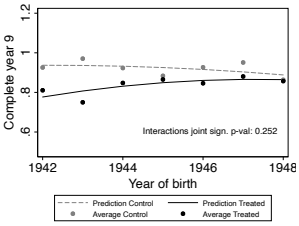
(1) Schooling



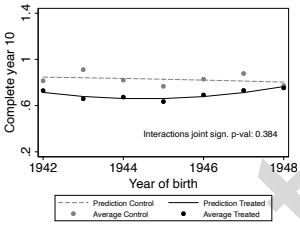
(a) Left school by age 14



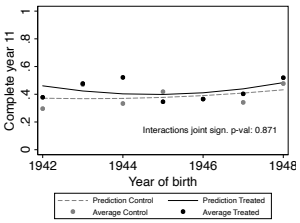
(b) At least Year 8



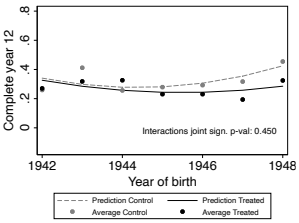
(c) At least Year 9



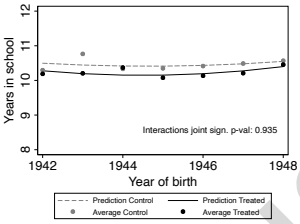
(d) At least Year 10



(e) At least Year 11

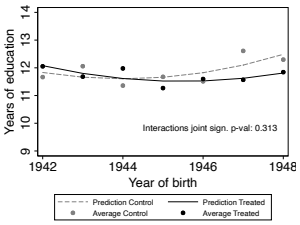


(f) Completed Year 12

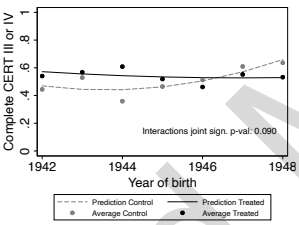


(g) Years of schooling

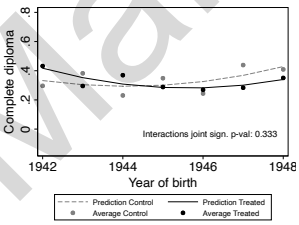
(2) Highest degree obtained



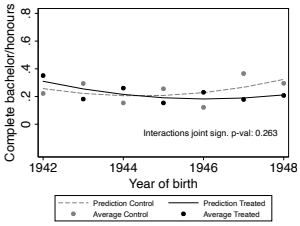
(h) Years of education



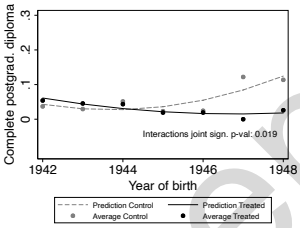
(i) Voc. train.: certif.



(j) Voc. train.g: diploma



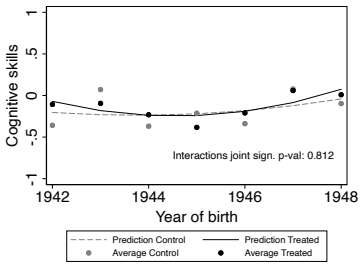
(k) Undergrad. degree



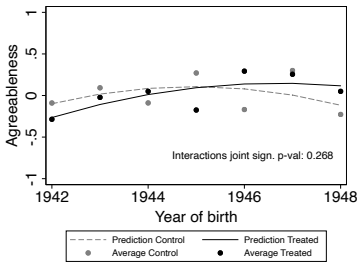
(l) Postgrad. degree

Notes: Graphs show the predictive outcomes for treatment (solid line) and control group (dashed line) over birth cohorts from a regression model at the individual level of each outcome regressed on linear birth cohort trends, squared cohort trends, reform dummy and interaction between the reform dummy and all trends. The dots show the means of treatment (black dots) and control group (grey dots) for each outcome variable and birth cohort. Each graph lists the p-value of joint significance of the interaction terms. Joint significance of the interaction effects would indicate different pre-treatment trends between treatment and control group.

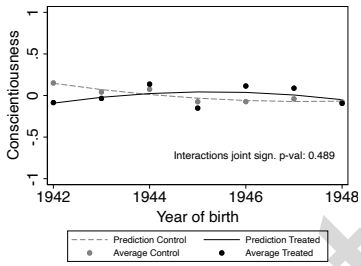
Figure C5: Squared pre-reform trends - Formal education



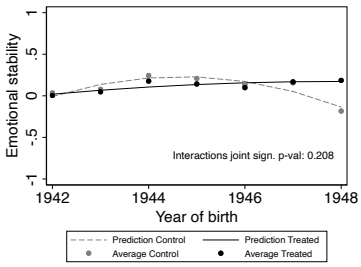
(a) Cognitive skills



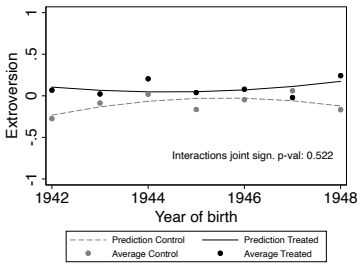
(b) Agreeableness



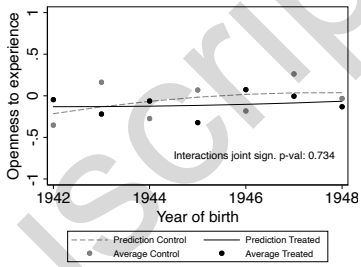
(c) Conscientiousness



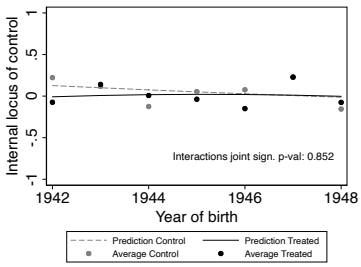
(d) Emotional stability



(e) Extraversion



(f) Openness to exp.

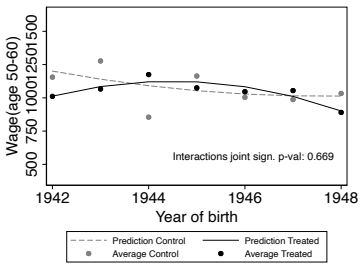


(g) Internal LOC

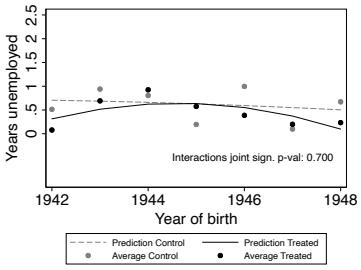
Notes: See Notes Figure C5.

Figure C6: Squared pre-reform trends: Long run capital - skills

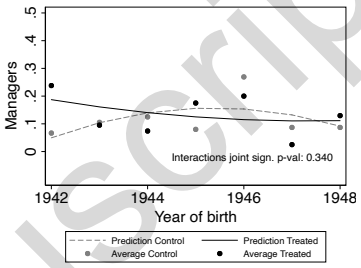
(1) Labor capital



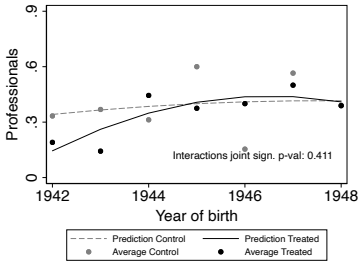
(a) Wage



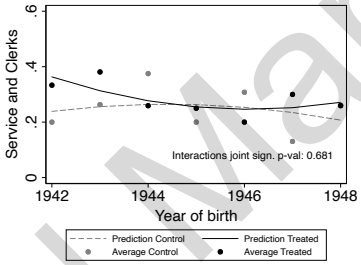
(b) Years unemployed



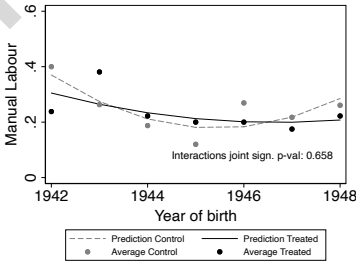
(c) Managers



(d) Professionals

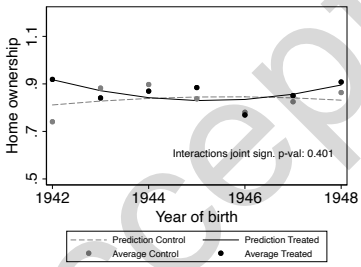


(e) Service and Clerks

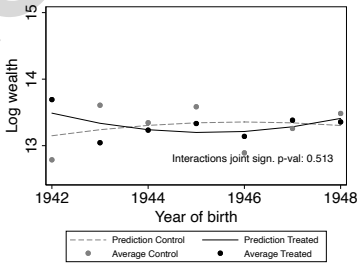


(f) Manual Labour

(2) Financial capital



(g) Home ownership

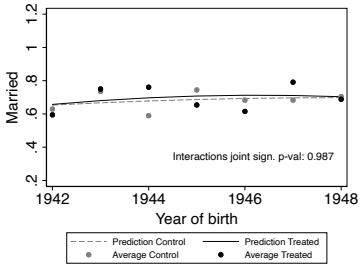


(h) Log wealth

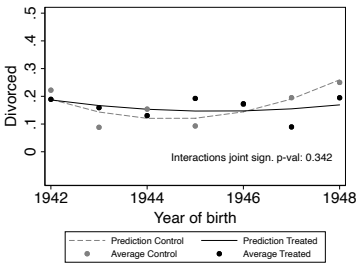
Notes: See Notes Figure C5.

Figure C7: Squared pre-reform trends: Long run capital - Labor and financial capital

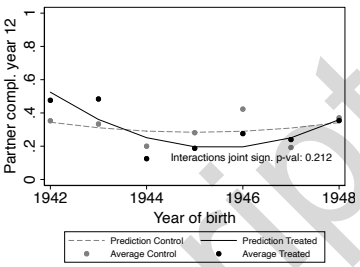
(1) Family capital



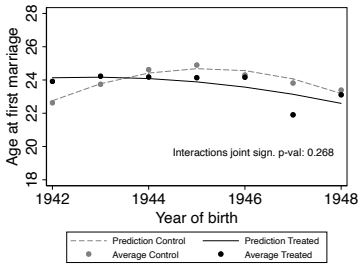
(a) Married



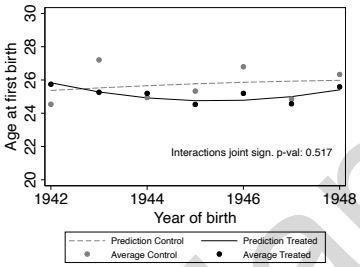
(b) Divorced



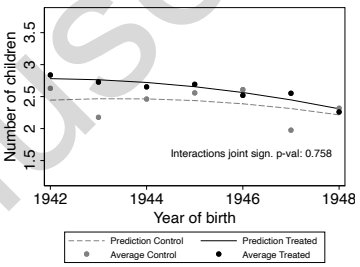
(c) Partner compl. year 12



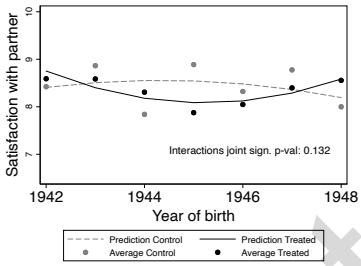
(d) Age at first marriage



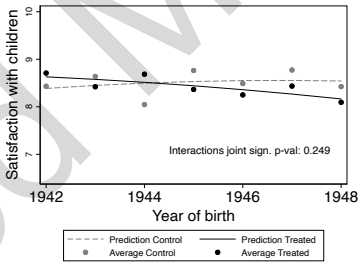
(e) Age at first birth



(f) Number of kids

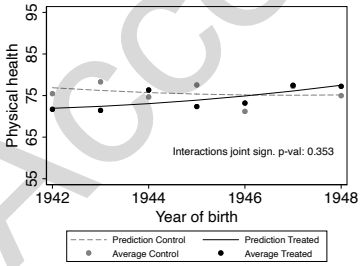


(g) Satisfaction with partner

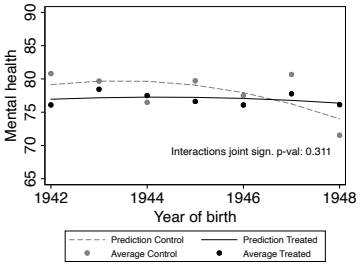


(h) Satisfaction with children

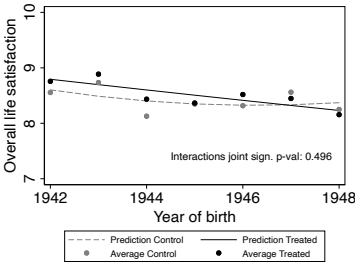
(2) Health capital



(i) Physical health



(j) Mental health



(k) Life satisfaction

Notes: See Notes Figure C5.

Figure C8: Squared pre-reform trends: Long run capital - family and health

Table C3: Treatment state specific cohort trends

	Baseline R × P (1)	Linear R × P (2)	Quadratic R × P (3)	p-value test Trend × Reform =0 Linear (4)	Quad (5)
Panel A: Secondary education					
Left school by age 14	-0.119***	-0.078***	-0.042***	0.023	0.011
	(0.010)	(0.009)	(0.005)		
At least Year 8	0.050*	0.080	0.073	0.266	0.001
	(0.018)	(0.039)	(0.037)		
At least Year 9	0.128***	0.083*	0.056	0.153	0.000
	(0.010)	(0.031)	(0.025)		
At least Year 10	0.177***	0.152**	0.112**	0.522	0.000
	(0.005)	(0.038)	(0.034)		
At least Year 11	0.203***	0.183*	0.165	0.695	0.426
	(0.026)	(0.073)	(0.081)		
Completed Year 12	0.113***	0.044*	0.054	0.013	0.000
	(0.012)	(0.016)	(0.026)		
Years in school	0.621***	0.462**	0.387*	0.232	0.116
	(0.035)	(0.135)	(0.157)		
Panel B: Postsecondary education					
At least voc. training: certification	-0.013	-0.087	-0.007	0.061	0.001
	(0.047)	(0.068)	(0.054)		
At least voc. training: diploma	0.075**	0.089***	0.114**	0.565	0.003
	(0.014)	(0.014)	(0.031)		
At least undergraduate degree	0.072*	0.052***	0.076***	0.438	0.005
	(0.029)	(0.007)	(0.013)		
Completed postgraduate degree	0.030**	0.062*	0.087**	0.151	0.007
	(0.007)	(0.022)	(0.027)		
Years of education	0.559*	0.403***	0.597***	0.290	0.006
	(0.179)	(0.055)	(0.057)		
Panel C: Skills					
Cognitive skills	0.088**	0.048	0.054	0.343	0.302
	(0.017)	(0.048)	(0.042)		
Agreeableness	0.126	0.128***	0.119**	0.982	0.001
	(0.058)	(0.012)	(0.037)		
Conscientiousness	0.005	0.005	-0.022	0.997	0.000
	(0.038)	(0.023)	(0.029)		
Emotional stability	0.032	-0.285***	-0.226**	0.002	0.001
	(0.021)	(0.043)	(0.049)		
Extraversion	-0.131	-0.281	-0.265	0.020	0.025
	(0.108)	(0.128)	(0.127)		
Openness to experience	0.141	0.245***	0.292**	0.126	0.004
	(0.077)	(0.035)	(0.058)		
Internal locus of control	0.067	-0.060	-0.058	0.122	0.025
	(0.044)	(0.048)	(0.049)		
Panel D: Labour capital					
Wage (age 50-60)	-14.252	-49.734	-115.369	0.741	0.006
	(33.548)	(117.822)	(140.208)		

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Table C3 – *Continued from previous page*

Years unemployed	-0.023**	0.035	-0.158	0.255	0.031
	(0.007)	(0.041)	(0.094)		
Managers	-0.019**	0.007	0.040**	0.326	0.006
	(0.005)	(0.018)	(0.007)		
Professionals	0.105*	-0.121	-0.100	0.011	0.005
	(0.041)	(0.077)	(0.086)		
Service and Clerks	-0.048	0.159**	0.108*	0.001	0.000
	(0.028)	(0.037)	(0.039)		
Manual Labour	-0.038**	-0.046	-0.049	0.892	0.967
	(0.008)	(0.059)	(0.051)		
Panel E: Financial capital					
Home ownership	0.025**	-0.009***	0.025***	0.017	0.000
	(0.007)	(0.001)	(0.001)		
Log wealth	0.115	-0.034	0.133	0.541	0.003
	(0.060)	(0.271)	(0.296)		
Panel F: Family capital					
Married	0.025	0.055*	0.054	0.001	0.000
	(0.021)	(0.020)	(0.027)		
Divorced	-0.022*	0.024	0.037	0.106	0.000
	(0.007)	(0.024)	(0.023)		
Partner compl. year 12	0.033	0.257*	0.215*	0.012	0.001
	(0.048)	(0.090)	(0.089)		
Age at first marriage	1.019***	1.041	1.168***	0.944	0.006
	(0.155)	(0.443)	(0.139)		
Age at first birth	1.724***	-0.038	0.332	0.065	0.002
	(0.124)	(0.596)	(0.336)		
Number of children	-0.255***	0.355**	0.206	0.007	0.023
	(0.023)	(0.095)	(0.093)		
Satisfaction with partner	0.179***	0.105	0.158	0.663	0.495
	(0.025)	(0.141)	(0.107)		
Satisfaction with children	0.299**	0.392**	0.544***	0.206	0.017
	(0.052)	(0.094)	(0.071)		
Panel G: Health capital					
Physical health	1.582	-0.698	-1.960	0.137	0.043
	(1.262)	(1.226)	(1.507)		
Mental health	0.873	-2.789	-3.218	0.012	0.022
	(1.066)	(1.379)	(1.558)		
Overall life satisfaction	0.041	0.382*	0.376*	0.019	0.013
	(0.051)	(0.123)	(0.123)		

Notes: Reported in Columns (1)-(3) are the coefficients for the Reform × Post interaction (R × P). Column (1) refers to the baseline model that includes a linear cohort trend. Columns (2) and (3) report augmented Diff-in-Diff models which include treatment state specific linear and quadratic cohort trends, respectively. Standard errors are reported in parentheses. Significance level: *<0.1, **<0.05, ***<0.01. Source: HILDA survey waves 2001-2016, own calculation.

Table C4: Balance-of-covariates test

	Summary statistics		Balancing covariates	
	Mean	**Std. dev.**	**Coef.**	**Std. Error**
Low socioeconomic status	0.712	0.453	-0.063	0.030
Mother employed at age 14	0.395	0.489	-0.012	0.010
Father employed at age 14	0.950	0.218	-0.008	0.017
At least one parent born abroad	0.203	0.403	-0.031	0.017
Oldest child in the household	0.327	0.469	-0.119**	0.035
Number of siblings	0.954	0.209	-0.018	0.012
Grew up with single parent	0.063	0.243	0.022	0.013

Notes: Balancing test is based on a regression model as defined in Eq. 1 excluding the covariate vector X. Each coefficient is based on a separate regression. Standard errors are cluster robust on the state level. Sample size is 1806 observations. Significance level: *<0.1, **<0.05, ***<0.01. Source: HILDA survey waves 2001-2016, own calculation.

Table C5: Adjustment of p-values for multiple hypothesis testing

	Baseline	Adjusted
Panel A: Secondary education		
Left school by age 14	0.0013	0.0000
At least Year 8	0.0683	0.0559
At least Year 9	0.0009	0.0000
At least Year 10	0.0000	0.0000
At least Year 11	0.0042	0.0000
Completed Year 12	0.0025	0.0000
Years in school	0.0004	0.0000
Panel B: Postsecondary education		
At least voc. training: certificate	0.7976	0.9540
At least voc. training: diploma	0.0117	0.0000
At least undergraduate degree	0.0888	0.0559
Completed postgraduate degree	0.0276	0.0030
Years of education	0.0525	0.0220
Panel C: Skills		
Cognitive skills	0.0145	0.0000
Agreeableness	0.1173	0.1259
Conscientiousness	0.9042	0.9540
Emotional stability	0.2120	0.3227
Extraversion	0.3125	0.5764
Openness to experience	0.1641	0.2208
Locus of control	0.2269	0.3227
Panel D: Labour capital		
Wage (age 50-60)	0.6996	0.8442
Years unemployed	0.0453	0.0220
Manager	0.0344	0.0000
Professional	0.0806	0.0559
Service and Clerks	0.1909	0.3227
Manual Labour	0.0189	0.0000
Panel E: Financial capital		
Home ownership	0.0393	0.0000
Log wealth	0.1502	0.1778
Panel F: Family capital		
Married	0.3076	0.5485
Divorced	0.0554	0.0220
Partner compl. year 12	0.5386	0.8442
Age at first marriage	0.0071	0.0000
Age at first birth	0.0008	0.0000
Number of children	0.0016	0.0000
Satisfaction with partner	0.0055	0.0000
Satisfaction with children	0.0106	0.0000
Panel G: Health capital		
Physical health	0.2987	0.4286
Mental health	0.4728	0.7063

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Table C5 – *Continued from previous page*

Overall life satisfaction	0.4780	0.7063

Notes: Table reports the p-values of a test of statistical significance of the treatment effect. Column (1) reports the p-values of the baseline model, while column (2) reports the p-values adjusted for multiple hypothesis testing using the Romano-Wolff step-down procedure.

Table C6: Alternative methods to calculate standard errors allowing for few clusters

	Baseline $T(G-1)$	$T(G-2)$	Webb bootstrap
Panel A: Secondary education			
Left school by age 14	0.001	0.007	0.066
At least Year 8	0.068	0.108	0.136
At least Year 9	0.001	0.006	0.068
At least Year 10	0.000	0.001	0.026
At least Year 11	0.004	0.016	0.078
Completed Year 12	0.003	0.011	0.044
Years in school	0.000	0.003	0.054
Panel B: Postsecondary education			
At least voc. training: certificate	0.798	0.806	0.814
At least voc. training: diploma	0.012	0.031	0.052
At least undergraduate degree	0.089	0.131	0.412
Completed postgraduate degree	0.028	0.057	0.052
Years of education	0.052	0.089	0.344
Panel C: Skills			
Cognitive skills	0.015	0.036	0.112
Agreeableness	0.117	0.161	0.526
Conscientiousness	0.904	0.908	0.794
Emotional stability	0.212	0.255	0.180
Extraversion	0.312	0.349	0.332
Openness to experience	0.164	0.208	0.408
Locus of control	0.227	0.269	0.556
Panel D: Labour capital			
Weekly wage (age 50-60)	0.700	0.712	0.734
Years unemployed	0.045	0.080	0.340
Manager	0.034	0.066	0.358
Professional	0.081	0.122	0.378
Service and Clerks	0.191	0.234	0.538
Manual Labour	0.019	0.043	0.214
Panel E: Financial capital			
Home ownership	0.039	0.073	0.070
Log wealth	0.150	0.194	0.434
Panel F: Family capital			
Married	0.308	0.345	0.556
Divorced	0.055	0.093	0.188
Partner compl. year 12	0.539	0.560	0.748
Age at first marriage	0.007	0.022	0.140
Age at first birth	0.001	0.005	0.130
Number of children	0.002	0.008	0.086
Satisfaction with partner	0.006	0.019	0.082
Satisfaction with children	0.011	0.029	0.318
Panel G: Health capital			
Physical health	0.299	0.337	0.570

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Table C6 – *Continued from previous page*

Mental health	0.473	0.499	0.622
Overall life satisfaction	0.478	0.504	0.642

Notes: Table reports p-values from different adjustments for small clusters in the difference-in-difference approach. We use critical values based on a T-distribution, adjusted by the number of clusters (G) minus one degree of freedom (G-1), or, more conservatively, adjusted by G-2 degrees of freedom, and the wild cluster bootstrap method with a six-point distribution recommended by Webb (2013) in the context of few clusters.

Table C7: Baseline versus IV and Fuzzy RDD Models

	Baseline	IV	Fuzzy-RDD
Panel A: Skills			
Cognitive skills	0.088**	0.289***	0.906***
	(0.017)	(0.086)	(0.083)
Agreeableness	0.126	0.255***	-0.060
	(0.058)	(0.076)	(0.544)
Conscientiousness	0.005	-0.014	-0.350
	(0.038)	(0.057)	(0.495)
Emotional stability	0.032	0.247**	0.613***
	(0.021)	(0.112)	(0.006)
Extraversion	-0.131	-0.315	-0.767
	(0.108)	(0.302)	(0.563)
Openness to experience	0.141	0.367***	0.104
	(0.077)	(0.043)	(0.590)
Internal locus of control	0.067	0.155***	-0.133
	(0.044)	(0.046)	(0.124)
Panel B: Labour capital			
Wage (age 50-60)	-14.252	-98.012*	- [1]
	(33.548)	(52.707)	
Years unemployed	-0.023**	-0.250***	-0.600***
	(0.007)	(0.070)	(0.090)
Managers	-0.019**	-0.014	0.510***
	(0.005)	(0.014)	(0.173)
Professionals	0.105*	0.183***	-0.242
	(0.041)	(0.062)	(1.128)
Service and Clerks	-0.048	-0.036	0.037
	(0.028)	(0.054)	(0.882)
Manual Labour	-0.038**	-0.133***	-0.305***
	(0.008)	(0.012)	(0.073)
Panel C: Financial capital			
Home ownership	0.025**	0.066***	0.033
	(0.007)	(0.021)	(0.067)
Log wealth	0.115	0.408***	0.324***
	(0.060)	(0.053)	(0.122)
Panel D: Family capital			
Married	0.025	0.040	0.149
	(0.021)	(0.036)	(0.204)
Divorced	-0.022*	-0.069***	-0.089
	(0.007)	(0.016)	(0.170)
Partner compl. year 12	0.033	0.003	0.318*
	(0.048)	(0.124)	(0.176)
Age at first marriage	1.019***	1.561***	0.246
	(0.155)	(0.202)	(0.670)
Age at first birth	1.724***	4.277***	1.113
	(0.124)	(1.221)	(2.036)

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Table C7 – *Continued from previous page*

Number of children	-0.255***	-0.706***	-0.014***
	(0.023)	(0.104)	(0.002)
Satisfaction with partner	0.179***	0.621***	0.180
	(0.025)	(0.115)	(0.113)
Satisfaction with children	0.299**	0.642***	-0.057
	(0.052)	(0.115)	(0.192)
Panel E: Health capital			
Physical health	1.582	0.337	3.824***
	(1.262)	(1.927)	(0.398)
Mental health	0.873	3.098**	3.813**
	(1.066)	(1.312)	(1.764)
Overall life satisfaction	0.041	0.309**	1.350***
	(0.051)	(0.134)	(0.504)
Panel F: IV-regression first stage			
Minimum: First stage Years of Education		0.294***	0.193***
		(0.046)	(0.051)
Maximum: First stage Years of Education		0.504***	0.670***
		(0.173)	(0.103)
Minimum: F-statistic first stage		2.78	3.51
Maximum: F-statistic first stage		46.04	31150.80
Median: F-statistic first stage		14.82	13.28
F-statistic >10		24	14
F-statistic <10		2	11
Max number of observations	1806	1806	999

Notes: The IV estimation is specified as in Li & Powdthavee (2015), where we use all relevant states (NSW, ACT, VIC and SA) which had compulsory schooling reforms at various points in time as outlined in Table 1). The instrument for years of education is a dummy variable that varies by states and birth cohorts indicating whether someone was subject to the reform, i.e. having to stay at least until age 15. We also control for the full set of control variables as well as nonlinear cohort trends. The fuzzy RDD specification follows Clark & Royer (2013) and focuses on Victoria and South Australia using local linear regression and the full set of control variables. As in Clark & Royer (2013), we estimate the effect via two-stage least squares where we instrument years of education in Victoria and South Australia with a dummy variable indicating the birth cohorts that were subjected to the reform in each state, i.e. having to stay at least until age 15. [1] The first stage regression for the sample, that restricts on having a wage between ages 50-60, leads to a very small F-statistic of <1, and therefore the second stage results are deemed unreliable and suppressed. Standard errors are reported in parentheses. Significance level: *<0.1, **<0.05, ***<0.01. Source: HILDA survey waves 2001-2016, own calculation.