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Gender Differences in the Lifecycle Benefits of Compulsory Schooling Policies

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NON-TECHNICAL SUMMARY

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 (MSLA) 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.

In this study, we analyze the wider consequences to society of increasing the MSLA using high quality nationally representative survey data – the Household, Income and Labour Dynamics in Australia Survey. 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 reforms that raised MSLA in South Australia and Victoria during the 1960s.

Our findings suggest that these two MSLA reforms dramatically shifted the educational-attainment distribution during the 1960s. The reforms were particularly effective in improving educational attainment for women. They helped many girls to complete high school, and thus stay an additional three years in school. For boys, the effects were more mixed but the reform mostly kept them in school to complete year 10. In the long run, the reforms improved women's older-age cognition, their wages, and their lifetime financial assets. Women also experienced higher quality marital matches, as measured by their lower divorce probabilities and better educated partners. For men, on the other hand, the reforms improved mainly their non-cognitive skills. Males who were forced to stay a year longer in school were more pro-social and developed stronger beliefs about their own control in life, and they were happier. Yet, their improved soft skills and happiness did not translate into higher wages and wealth.

Our research is highly policy relevant. We are the first to provide an overview of the long-term benefits to society of raising the MSLA of yet another year and how these affect men and women in different ways. Although tentative, our findings suggest that the non-market benefits may outweigh the high costs of forcing children at the margin, who would otherwise have left, to stay longer in school. MSLA reforms may also have led to female empowerment during a time when women were expected to be the homemaker or work in low-skilled professions.

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ABSTRACT

We estimate the lifecycle benefits of raising minimum school leaving age (MSLA) policies. Using a difference-in-differences method, we estimate the causal impact of two Australian state reforms that extended the MSLA from 14 to 15 during the 1960s. Important gender differences emerge in how the reforms affected the lifecycle capital accumulation process. While raising the MSLA improved women's wages and wealth, the reforms improved men's non-cognitive skills and satisfaction with life. The differential outcomes may be explained by heterogeneous impacts of the reform on educational attainment and occupational choice. The reforms increased women's but not men's probability to complete high school, without affecting access to tertiary education. Women were disproportionately more likely to enter professional occupations, achieved higher quality marital matches and experienced better family relationships. Raising education levels for individuals at the lower end of the education spectrum produces lifecycle benefits that exceed market-return considerations.

Keywords: minimum school leaving age; education reform; lifecycle capital accumulation; non-cognitive skills; cognitive skills; marital quality; wealth; health; Australia

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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 an overview of these issues).

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.

[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).

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 during the 1960s 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 its immediate impact on education and shorter-term labor market outcomes. Second, the two reforms were introduced during a time when most students would leave high school at the MSLA. Thus, the reforms affected a relatively large fraction of Australian youth. Third, the two reforms occurred during a time of relatively little other educational changes, unlike the reforms in 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 it as *diversified capital stock* observed at the end of productive life. As we will argue in Section 3, 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 and thus the diversified capital stock.

The analysis is conducted with high-quality, nationally representative survey data from Australia, the Household, Income and Labour Dynamics in Australia Survey (referred to as *HILDA*). Our empirical strategy relies on a difference-in-differences (DiD) model which identifies the average treatment effect on the treated (ATT) (Athey & Imbens, 2006, p. 436). One advantage of this approach over other approaches often used

in this literature which produce local average treatment effects (LATE, e.g. fuzzy regression discontinuity approaches) is that the ATT takes into account potential changes in the whole education distribution rather than just locally around the minimum 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.[2] 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 during the 1940s. We discuss and test carefully the assumptions under which our difference-in-difference approach yields causal impact estimates of the MSLA reforms.

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, 2006; 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 also a large body of research on the impact of education on physical health (e.g., Chatterji, 2014; Clark & Royer, 2013; Lleras-Muney, 2005; Oreopoulos, 2007), health behaviors (Li & Powdthavee, 2015), fertility (Black et al., 2005;

[2] 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).

Cygan-Rehm & Mäder, 2013), and mental health (Crespo et al., 2014; Oreopoulos, 2007). Finally, some studies provide evidence that higher population education levels may reduce crime (Lochner & Moretti, 2004; Machin et al., 2011) and may have important impacts on the intergenerational transmission of disadvantage (e.g. Black & Devereux, 2011; Black et al., 2005, 2008; Oreopoulos et al., 2006). 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 about the lifetime benefits in the context of one single reform. We depart from the perspective that the immediate effect of an additional year of schooling for those at the margin is likely to build students' human capital in the broadest sense. By human capital we do not only mean educational qualification but the formation of labor-market relevant cognitive and non-cognitive skills. Previous research has demonstrated that at both the intensive and extensive margin high-school education does impact non-cognitive skill development (see Schurer, 2017, for an overview of this literature). Better skills in combination with better formal training is 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. The impact of MSLA reforms on cognitive and non-cognitive skill development has not been studied in this context before.

Our research is highly policy relevant. We are the first to provide an overview of the long-term benefits to society of raising the MSLA of yet another year and how these affect men and women in different ways. Australian states and territories have been increasing the compulsory school leaving age 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 reforms 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).

The remainder of the paper proceeds as follows. Section 2 provides an overview of the Australian education system and the details of the MSLA reforms. In Section 3 we outline our hypotheses on how raising the MSLA provides broader benefits to society by acknowledging that education impacts permanent outcomes through building human capital at the broadest sense. Section 4 describes the empirical strategy. In Section 5 we describe the HILDA and relevant variables. All results are presented in Section 6. We discuss our results in Section 7. An Appendix provides supplementary material.

2 Institutional background

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.[3] 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 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. By the early 1970s, most students (80 percent) would only complete Year 10, the year level that was

[3] 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.

implied by the MSLA. Only a small fraction would completed 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 MSLA reforms

In most countries, 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 the 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 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 in three stages from 1941 to 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, benchmarking them against children in NSW and ACT, who had already benefited from such reforms.

Importantly, we exclude from the analysis all other states for various reasons.[4] Tasmania (TAS), which raised its MSLA from 14 to 16 (rather than 15) in 1946, is excluded because of the large number of exemptions that were applied for and granted immediately following what was perceived as an unpopular reform (Barcan, 1980).[5] Northern Territory (NT) schools were under the responsibility of South Australia during the 1960s. NT schools are exempted because of large differences in the settlement histories compared to southeast Australia. Additionally, at the time when its MSLA was increased from 14 to 15 in 1965, only a couple of permanent high schools existed in the state. Queensland (QLD) is excluded from our analysis because its 1965 MSLA reform took place in the midst of other dramatic changes to 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 from 14 years old to the end of the year at which children turned 15, rather than on their 15th birthday. This means that students born earlier in the year could leave school at a considerably earlier age than students born late in the year.

[4] The results are mostly robust to the inclusion of all states but Queensland. We decide against including the other states to preserve a clean identification strategy. Results are available on request.

[5] Rural families considered their boys who remained in school until 16 to be at a disadvantage relative to those who obtained exemption, given that they could begin apprenticeships from age 15 . This large proportion of exemptions meant that the average MSLA was not substantially higher compared to that of other states. In 1948, the premier responded to this high proportion of exemptions by relaxing the MSLA requirements.

Choosing SA and VIC as treatment states has many advantages. Both states have had similar settlement histories 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. 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 who is required to be enrolled in school during school hours. This suggests that both states experienced similar attitudes and legal foundations for law enforcement.

3 Theory: Capital formation over the life course

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. Although some argue that MSLA policies do not improve social welfare[6] and are thus expensive tools to simply keep children in school, many previous studies have shown that such policies have labor market benefits (see Harmon, 2017, for a review), that may even transmit to the next generation (Lindeboom et al., 2009; Lundborg & Rooth, 2014). Additionally, MSLA reforms are likely to have important non-market benefits (Oreopoulos & Salvanes, 2011), that may affect individuals' lives in and outside the labor market. Specifically, more schooling is likely to impact upon whether and

[6] Some studies find zero labor market returns to compulsory schooling reforms (Pischke & von Wachter, 2008) and no positive health returns (Clark & Royer, 2013). One explanation for this result is that lifting the MSLA may not lead to more learning or additional qualifications. Another explanation is that 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. This group is now operating in a more crowded class room and may need to engage with less motivated peers, which may lead to disruptions in the class-room or a reduction in the motivation of the more capable students. It should be noted here, however, that the findings of zero wage returns of additional school as presented in Pischke & von Wachter (2008) may be a statistical artefact. 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.

how individuals invest in their financial wellbeing and health, and how to decide over the quantity and quality of children and family relationships.

Judging whether more schooling directly improves 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. In this section, we carefully describe the dynamics between market and non-market decisions that may be shifted through MSLA policies over the lifecycle. The decision-making process is illustrated graphically in Figure 1.

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 build human capital more broadly, including a boost in cognitive and non-cognitive abilities (Almlund et al., 2011; Lundberg, 2018; Todd & Zhang, 2018). MSLA reforms target young people at a time when the human brain develops rapidly. 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). Non-cognitive skills may also play an important role in determining household bargaining weights and offered wages (Flinn et al., 2018; Todd & Zhang, 2018). 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.

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 (1) labor market participation productivity (labor market entry and duration, wages), (2) financial decisions (home ownerships, other assets), (3) family formation (quality and quantity of partner(s) and children), and (4) 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. Human capital continues to be shaped over the life course.

Of course these areas are not independent from each other. Family formation decisions are likely to impact decisions around finance, health, and the labor market (and vice versa). Investments in each area will build capital in each dimension until reaching retirement age. Hence, different forms of capital are dynamic concepts that are built or depleted depending on the decisions that individuals make. The same holds for human

capital which is a dynamic concept over the lifecycle. It is built from birth into young adulthood, then it stays relatively constant during the productive years (Cobb-Clark & Schurer, 2012; Cobb-Clark et al., 2013; Elkins & Schurer, 2018)

At the end of productive life, individuals are left with a capital stock which they can deplete during retirement and which is likely to determine life expectancy. 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.

4 Empirical strategy

4.1 Model

This paper analyses the effect of a one year increase in the compulsory school leaving age on later life outcomes. We use a difference-in-differences (DiD) approach to identify an average treatment effect on the treated (ATT). This approach is in contrast to previous

studies in a similar context, which have mainly used a fuzzy regression discontinuity design (RDD) to identify a local average treatment effect (LATE).[7] We see three advantages of a DiD approach in our setting: First, the aim is to identify the overall effect for the full population affected by the treatment. That is, the reform does not only affect those individuals who must stay in school for an extra year as a result of the reform, but potentially also those who would have stayed in school longer anyways. 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. Third, a RDD approach is very data intensive and our survey sample is not large enough to meet this obligation. However, survey data is needed as many of the outcomes we are interested in are typically not captured in administrative data.

We obtain the effect of an increase in the compulsory school leaving age by one year by taking the difference of two differences: firstly, the difference between treated and non-treated states and secondly, the difference between pre- and post-reform cohorts. We estimate the following econometric model:

$$y_i = state\_educ_i'\beta + YOB_i'\gamma + \delta reform_i + x_i'\alpha + \epsilon_i, \tag{1}$$

[7] Two previous studies analyzed the causal returns to MSLA reforms in the Australian context using a fuzzy RDD approach and data on all Australian state policies. Leigh & Ryan (2008) find a positive effect of the reform on annual income, which is however only significant at the 10% level. Li & Powdthavee (2015) find that one additional year of schooling improves health behaviors such as diet and regular exercising, but does not affect the probability of smoking. We refrain from using the same identification strategy as in Leigh & Ryan (2008) and Li & Powdthavee (2015), as it is unclear from their empirical findings why MSLA performs poorly as an instrument (F-statistic < 6 in Leigh & Ryan (2008), Table 4; F-statistic < 14 in Li & Powdthavee (2015), Table 1) while the interaction between MSLA and birth year, which is used in the benchmark specification, achieves an excessively high F-statistic that lies above 8 billion in Leigh & Ryan (2008) (Table 4) and 39 million in Li & Powdthavee (2015) (Table 1). The treatment effect of interest in both papers is only statistically and economically significant in specifications where the F-statistic exceeds such extreme values. In contrast to those studies, we use the exact reform dates, a tighter sample window and exclude states with fuzzy reform dates or other reforms happening at the same time. See Section 2 for an overview of the education reform landscape.

where y_i represents the outcome variable of interest for individual i. We employ several outcome variables which provide information on a) human capital including cognitive and non-cognitive skills, b) labor market histories, c) financial capital, d) family capital and e) health capital.[8]

The vector $state\_educ$ includes a set of indicator variables which are equal to one if an individual completed the highest degree of education in a particular state. The state fixed effects (vector β) pick up potential time invariant differences between states. The vector YOB comprises indicator variables for each year of birth. We thereby control for any time trends which are not state-specific (vector γ). The binary variable $reform$ is equal to one, if an individual completed school in Victoria or South Australia and was affected by the rise in the compulsory school leaving age. De facto, $reform$ is an interaction term between $state\_educ$ and YOB. Consequently, the coefficient δ is the main coefficient of interest.

The vector x includes a constant and individual level covariates for sex, information on parents (employment status at the individual's age of fourteen, marital status, migration background) and siblings (number of siblings and whether the individual is the firstborn child). The summary statistics for these variables are presented in Table A1. By construction, the individual level covariates should not affect the identification of the reform effect but may produce more efficiently estimated standard errors. All remaining unobserved factors are captured by the error term ϵ.

We consider a large number of outcomes to capture the diversified capital stock. With 34 outcomes in total, we will find at least 1.5 significant effects by chance, assuming critical values of $\alpha = 0.05$. To address the consequences of multiple hypothesis testing, we implement the efficient step-down approach developed in Romano & Wolf (2005).

Furthermore, we adjust for the small number of state clusters (4) which may downward bias our standard errors and therefore lead to misleading statistical inference (Bertrand et al., 2004). Following Cameron & Miller (2015), we consider three different clustering

[8] Section 5.2 explains the construction of these variables in detail.

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.[9]

4.2 Identification assumptions

The DiD parameter estimate of δ measures the difference in outcomes between treated and non-treated cohort members in SA/VIC, relative to the differences in outcomes between the same cohorts in NSW/ACT (where no reform took place). It yields a causal impact of the MSLA reform under four critical assumptions.

First, in the absence of treatment, treatment and control groups would have developed in parallel. While the parallel trends assumption is impossible to test, we can approximate a test by graphically examining the trends in outcomes for both treatment and control groups before the reform dates. We illustrate the pre-reform date trends in 34 outcomes in Figures A1 and A2 (Appendix A). The graph shows that in most cases the fitted line for the control states lies within the confidence interval of the line for the treatment states. If the line for the control states does not lie within the confidence interval of the line for the treatment states, both confidence intervals still at least overlap or the trends seems to evolve in parallel. Only for "postgraduation level" the trend slightly deviates between treatment status. However, the trends are suggestive of underestimation rather than overestimation. It is important to note that we would only overestimate the treatment effect, if there was a state-specific cohort trend such that in the treated states earlier cohorts naturally had better outcomes than later cohorts, which is the exact opposite to what we observe in Figure A1 (k).

[9] 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).

Second, we assume that 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 a MSLA of at least 15, leaving SA/VIC pupils no alternative state for school avoidance. On the other hand, it could have been that cohort members from our comparison states – NSW and ACT – strategically moved to VIC or SA to avoid staying in school until age 15. Our DiD estimates would then be biased because the pre-treatment cohorts in the treated states would consist of a larger portion of unmotivated students, while the pre-treatment cohorts in the non-treated states would consist of more motivated students. We consider this scenario as highly unlikely, because this option may only be feasible for families living close to the boarder. The majority of the Australian population is concentrated in eight major coastal cities, spread across the continent. It seems implausible that a large share of families moved long distance to a different state to benefit from a lower MSLA.

Third, our identification strategy requires that no other policy changes occurred during the sample period that may have affected outcomes of pre- and post-cohort individuals differently. We are only aware of one other relevant reform. In 1963, the control states – NSW and ACT – both implemented a comprehensive schooling system, opening up educational opportunities for children from all skill and knowledge backgrounds. The two treatment states did not implement comprehensive schools until 1975 (South Australia) and 1980s (Victoria) (Barcan, 2007). As previous studies have shown that comprehensive schooling systems improved wages and educational attainment for students 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 be better off than expected because they benefitted from access to better schooling. If this conclusion is true, we will underestimate the treatment effect of MSLA reforms. We therefore consider our DiD estimates as lower bounds.

Finally, we assume that the composition of both treatment and control groups did not change over time, which is particularly important as we use cross-sectional cohort data. To test this assumption, we apply a balance-of-covariates test on all available control variables. We run a series of regression models similar to Eq. (1) in which each control variable is considered separately as an outcome variable (all control variables are omitted). Table A1 (column 3, Appendix A) provides evidence in support of the claim that the cohorts did not change their composition in terms of observable characteristics. All estimated reform effects are statistically and economically insignificant.

5 Data

5.1 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 conducted yearly 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 high school as a child (available in wave 12 and 16). From 2001 to 2010, approximately 13,000 individuals are 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 are measured only in a few waves (e.g. cognitive and non-cognitive abilities), while others are collected and updated every year (e.g. educational degree, family status).

Our main sample consists of individuals that turned 15 in a 15 year window around the reform date. These individuals are between 59 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.

5.2 Outcome measures

5.2.1 Human capital 1: educational attainment

We consider two measures of educational attainment. First, we generate a binary variable that captures whether the individual has left school by the age of 14. 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 one if the individual completed the respective school year (and 0 otherwise). An individual who completed year ten automatically also completed year eight and nine.

5.2.2 Human capital 2: cognitive and non-cognitive 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 (>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.

To construct measures of non-cognitive skills, we use data on the Big-Five personality traits and locus of control. HILDA collected an inventory of the Big-Five personality traits based on Saucier (1994). The five dimensions: extroversion, agreeableness, conscientiousness, emotional stability, and openness to experience represent personality at the broadest level of abstraction. The survey measure collected 30 of the original 40 items suggested in Saucier (1994) plus six items that were added by the survey methodology team. Ultimately, 28 of these 36 items are used to construct five aggregate scores of personality, all of which have a high degree of reliability (see Cobb-Clark & Schurer (2012) for a justification). Each trait is scored from 1 to 7, with a high score indicating that the personality trait describes the individual very well. As is standard in the literature, we use factor analysis to derive a weighted index for each of the five traits (e.g., Cobb-Clark et al. (2014) and Cobb-Clark & Schurer (2012)). All of these measures are standardized to mean zero and standard deviation one.

5.2.3 Labor market history

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 59 years old in wave 16. To make wages comparable and to avoid to pick up 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 jobs between ages 50 and 59. 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.

5.2.4 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 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 are only available on the household level. To avoid identifying the effect of living as a couple, we condition wealth on being married.

5.2.5 Family capital

We proxy family capital with measures on marital status, number of children, partner quality, and satisfaction with family life. As family formation processes can be assumed

completed by the age 59, we measure these variables in wave 16.[10] Marital status is measured with two binary variables that take the value 1 if married or divorced, respectively (and 0 otherwise). The base category is being separated, widowed, never married or in a de facto relationship. Partner quality is proxied with information on the highest educational degree of the partner. 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.

5.2.6 Health capital

We proxy health with continuous measures of physical and mental health, and life satisfaction. Both of our health measures are derived from the SF-36 inventory, a reliable self-completion questionnaire developed in Ware et al. (2000). Based on the answers on 36 questions, a score between zero and one hundred is constructed for each individual. 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.

6 Estimation results

We begin with reporting the estimated effects of the MSLA reform on educational attainment and then discuss the estimated effects of the reform on the diversified capital stock

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

observed at the end of productive life. We report our results separately for males and females, but present also the pooled results to illustrate the importance of considering the heterogeneity in the treatment effect by gender.

6.1 Reform effect on educational attainment

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 2 (a) shows that the policy change resulted in a drop of more than 15 percentage points (abbreviated as $p.p.$) in the probability of having left school by the age of 14 in the treated states (solid black line). Before the reform, around 20 percent of individuals in the treated states left 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, as they leave school at age 14.[11] 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 2 (b) shows that the total number of years of schooling increased by half a year on average, which is slightly higher than what was commonly observed in post World War II MSLA reforms.[12] Again, the average years of schooling in the control states (dotted line) seem unaffected by the reform. Figures 2 (c)-(f) 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 before the age of 14 was higher than the probability of males (23 percent versus 18 percent). After the reform, both sexes had a probability to leave school before the age of 14 of around 5 percent. The reform had a similar effect on the total number of years of schooling for both males and females while again, female students started at a lower level before the reform.

[11] 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.

[12] 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)

The difference-in-differences (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 differences in outcomes between birth cohorts affected and non-affected by the reform, relative to the differences in outcomes between the same birth cohorts in the control states, where no reform took place. Columns (1) and (2) of Table 3 show the estimated reform effect and its significance level for various educational outcomes for the whole sample (Panel A) and separately by females (Panel B) and males (Panel C).[13] Columns (3) to (5) show significance levels adjusted to multiple hypothesis testing and few clusters. We refer to *robust effects* if they are robust to multiple hypothesis testing and to at least one of the two methods that account for few clusters to calculate the standard errors.

The DiD estimates show that the reform reduced the probability to leave school by age 14 by 10 p.p.. It is evident that the reform also changed the likelihood of staying in school for more years than the MSLA. We find a *robust* 12 percentage-point increase in the probability to stay in school at least until year 9 and a 14 percentage-point increase in the probability to stay at least until year 10. The results suggest that the reform led some students to stay in school more than just the one additional year. Alternatively, higher-achieving students may have increased their years of schooling, possibly to differentiate themselves from weaker students who now receive more years of schooling through the reform. The MSLA indeed led some students to obtain higher levels of qualification, increasing the probability to obtain a bachelor or honours degree by 6 p.p., an increase of 25 percent from the base probability (24 percent).

Important gender differences emerge on how the MSLA reform impacted educational qualifications. Females were more likely to complete high-school by finishing year 12 (by 14.3 p.p.), while males were only significantly more likely to complete year 10, which would be expected with a MSLA of 15 (by 12.3 p.p.), and some were even less likely

[13] Full estimation results, including sample sizes for each outcomes and standard errors are reported in Table B1, Appendix B

to complete Year 12.[14] This suggests that the reform added for females not only one additional year of schooling, but on average three additional years (Year 10, year 11, and year 12). However, some males were also more likely to obtain a university degree if they managed to stay on to complete high school. This treatment effect is significant and robust at 9.4 p.p..

6.2 Long-run reform effects

6.2.1 Skills

We find robust treatment effects of the MSLA reform on cognitive skills and some non-cognitive skills, and important gender differences in the treatment, see Panel A (1) in Table 4.[15] Cognitive skills increase on average by 0.15 standard deviations (abbreviated as SD). This suggests that the MSLA reform improved cognitive functioning in older age for reform-affected cohorts. However, this robust improvement was observed for women only (see Panel B, Table 4).

The MSLA reform also shaped some non-cognitive skills, but this softer-skill improvement was only observed for males (see Panels A(1)-C(1), Table 4). We find for males large, significant and robust increases in agreeableness, a trait associated with prosociality (0.29 SD increase) and internal locus of control, a trait associated with a strong belief about self-control (0.21 SD increase). The only notable effect of the MSLA reform on softer skills for females is on locus of control, which was reduced by 0.12 SD. This

[14] It is no straight forward to explain why the reform had some negative impact on male students to complete high school. One explanation is that the reform had heterogeneous impacts. As male students were forced to stay on an extra year to Year 10, they may have been more discouraged to stay on longer as a consequence, possibly because of poorer learning environments. More research is needed to explain this finding.

[15] Table B2 in the Appendix shows the full results including standard errors and sample sizes.

indicates that the reform boosted males perception of their own ability to influence the outcomes of their life, while it tempered this perception for females.[16]

6.2.2 Labor and financial capital

We find no robust impact of the reform on labor market histories or financial capital accumulation as observed at the end of productive life for the full sample (see Table 4, Panels A (2)-(3)). The reason is that the potential effects of the MSLA reform on the diversified capital stock are masked due to heterogenous treatment effects by gender. Indeed, the MSLA reform significantly improved labor and financial capital of females and tentatively worsened long-term financial outcomes for males. For instance, the reform significantly increased women's wages by 128$ per week, wealth by 24 (log) percent, and the probability to own a house by 7.7. p.p.. These impact estimates are robust. For males, we find no or negative impact estimates on wages, wealth and house ownership, although the negative effects are not consistently robust. The only exception is that reform-affected males are 6.2 p.p. less likely to own a house in older age.

To explain this heterogeneous impact of the reform by gender, we furthermore explored how the reform impacted occupational choice (see Table 5). Both reform-affected males and females were significantly less likely to work as tradespersons by 4.3 and 1.9 p.p., respectively. Males were also less likely to work in laborer occupations by 3.6 p.p..

[16] It is not clear why we observe such differential effects by gender. Most of the existing evidence on the association between education and internal locus of control would suggest a positive link. For instance, Heckman et al. (2006) demonstrate that individuals with university education score over 0.3 SD higher on internal locus of control than individuals who completed year 12 only. Elkins & Schurer (2018) also demonstrated that parental engagement with daughters' schooling increased their internal locus of control scores by mid age. As we have shown, the MSLA reform did not raise tertiary qualifications for females but helped them complete high school education. Yet other reforms such as an increase in the teaching intensity in German high schools, or schooling programs targeting disadvantaged children, such as the Promise Academy, had no demonstrable impacts on internal locus of control development (see Schurer (2017) for an overview of existing causal impact of secondary or tertiary education or interventions. Another channel through which this finding could be explained is that for some reform-affected males, the MSLA reform boosted tertiary education). Kassenboehmer et al. (2018) demonstrated that tertiary education improves some non-cognitive skills of males from socioeconomically disadvantaged backgrounds, but not for females. Although this study did not explore locus of control development, this could be one explanation why non-cognitive skills are not positively affected for females in our sample.

Males shifted predominantly into intermediate clerical, sales and service worker occupations (increase by 4.4 p.p.) and some moved into professional occupations. Females on the other hand disproportionately moved into professional occupations, with a staggering increase of 15.7 p.p. in this likelihood. This finding suggests that reform-affected females flocked into professional occupations where they earned more income and accumulated more wealth, while reform-affected males predominantly shifted away from laborer jobs into service-sector occupations, in which they did not earn higher incomes but had less physically strenuous jobs.

6.2.3 Family and health capital

Last but not least, we evaluate the impact of the reform on family (Panel A (4), Table 4) and health capital (Panel A (5), Table 4). Reform-affected individuals were less likely to experience divorce by age 59 (by 3.7 p.p.), they reported higher satisfaction levels with their partners (by 0.22 points), and they were more likely to be married with partners who completed at least Year 11 (by 11 p.p.). At the same time, the reform reduced the number of children – although the impact estimate is not robust – and increased satisfaction scores with children (by 0.32 points). As shown in Panel B (4), the treatment effects of the MSLA reform on marital quality are observed for females only, with one important exception. Both men and women were happier with their family life, as measured by their satisfaction with their children. Reform-affected males are also happier with their lives in general by 0.26 points (Panel A(5)). There is no significant impact of the MSLA reform on health capital (Panel A(5)).

6.2.4 Further robustness checks

To rule out the possibility that our estimation framework picks up unobserved differences in the trends of cohorts between treatment and control states, we set an arbitrary reform date to two years before the true MSLA reform date and repeated our analysis. This robustness check uses only a sample of pre-reform cohorts, but coming from all

four states. We are able to demonstrate that an arbitrary reform date does not produce the same treatment effects as in our benchmark specification (see Table A2, Appendix A). In some cases, the treatment effects are of opposite signs, although they are not statistically significant.

Our benchmark results reported in Tables 3 and 4 were based on an estimation sample that included all cohort members who turned 14 between 7.5 years before and 7.5 years after the reform date. The inclusion of two additional cohorts – 8.5 years before and after the reform – or the exclusion of two cohorts – 6.5 years before and after the reform – does not notably change the results (see Table A3, Appendix A). Reaching the limits of the data, we cannot further reduce the sample size.

7 Discussion and conclusion

Governments worldwide use minimum school leaving age (MSLA) policies to reduce economic inequalities that are associated with insufficient training for the labor market. Such policy is paternalistic in nature, as it forces youth to stay, and parents to support their children, for another year in education. Thus, lifting the MSLA entails individual and household welfare costs including financial opportunity costs and psychic costs, in addition to the financial costs accrued by governments to fund additional training capacity. Recently, researchers have argued that despite increasing levels of education worldwide, many students may be left without additional learning (see Angrist et al., 2019). It is for these reasons that the benefits of raising the MSLA need to be carefully assessed. A large body of literature exists on the market returns and health effects of MSLA reforms (see Oreopoulos & Salvanes, 2011; Harmon, 2017, for overviews).

We contribute to this extensive literature by studying how MSLA reforms affect the capital accumulation process over the lifecourse and gender differences in this process. A specific benefit of our study is that we consider a broad range of outcomes, which may have resulted from better human capital (including cognitive and non-cognitive skills) that

was produced by the MSLA reforms. We focus our analysis on two specific MSLA policy changes that occurred in Australia during the 1960s. Both South Australia and Victoria, two comparable states located in the South of Australia, raised the MSLA by one year from 14 to 15 in 1963 and 1964, respectively. We provide an estimate of the *overall life-cycle benefits* of the Australian MSLA policies. We consider both market and non-market returns of forcing school-age pupils to stay for one additional year in secondary education. We explore whether the returns to additional schooling differ for men and women, who had very different educational opportunities during the 1960s. We zoom in on what we call the *diversified capital stock* as observed at the end of productive life, measuring capital stock in terms of skills, labor market histories, wealth, family, and health capital.

These two MSLA reforms dramatically shifted the educational-attainment distribution during the 1960s. The reforms reduced adolescents' probability of leaving school at age 14 by 75 percent relative to the base probability, although about 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 reforms were particularly effective in improving educational attainment for women. They helped many women to complete high school, and thus stay an additional three years in school. For boys, the impacts were more heterogeneous. For a large fraction of boys the reform kept them in school to complete year 10, a year level implied by the new MSLA. For a small fraction however, the reform led to tertiary education.

Following the affected cohort members for over 50 years, we were able to establish a battery of stylised facts for the Australian MSLA reform experience. On the one hand, the Australian reform had no tangible impacts on health, a finding consistent with previous evidence from Britain (e.g. Clark & Royer, 2013). On the other hand, men and women were affected very differently by the reform in the long run. For females, the

reform improved older-age cognition, their wages, and their lifetime financial assets, including home ownership. They also experienced higher quality marital matches, as measured by their lower divorce probabilities and better educated partners. This latter finding is in line with for instance Oreopoulos & Salvanes (2011) who demonstrate that MSLA reforms decrease the probability of divorce, and Geruso & Royer (2018) who find that MSLA reforms lead to better-educated partner matches. For males, the reform improved mainly their non-cognitive skills. Males who were forced to stay a year longer in school were more pro-social and open to new cultural experiences and developed stronger beliefs about their own control in life. Yet, their improved soft skills did not translate into higher wages and wealth.

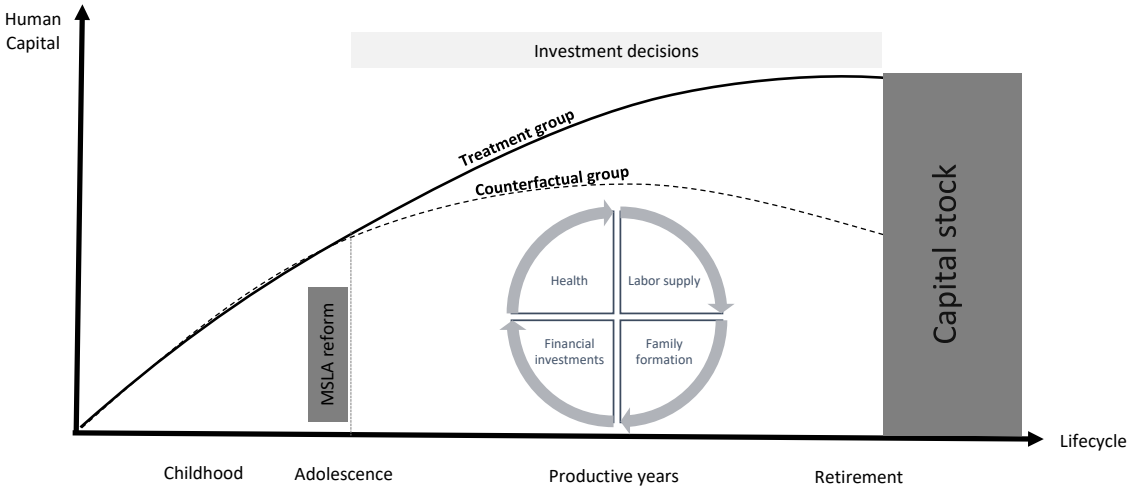
We can only speculate on why men and women were affected in such different ways by the reform. One explanation is that the reform affected different channels through which higher levels of education, that were induced by the reform, affected capital development. The reform clearly boosted educational opportunities for females, who were now more likely to complete secondary education. Yet, this increased probability did not translate into tertiary education, although the reform increased university education for some males. Hence, the differential impacts for men and women may be explained by differential access to tertiary education. Recent evidence from Australia suggested that university education boosts non-cognitive skill development, especially for males from disadvantaged backgrounds (Kassenboehmer et al., 2018). Our findings that the MSLA reform boosted non-cognitive skills of men is highly consistent with this evidence.

For women, the additional years spent in high school and higher completion rates led to increased access to professional occupations, a shift away from trade occupations. Although men were also more likely to access professional occupations, they tended to select more strongly into sales and service-sector occupations. This may explain why the reform benefitted women's wages. In combination with better marital matches and lower divorce rates, this may also explain why reform-affected women accumulated more wealth over the lifecycle. Access to less strenuous jobs may also explain why women

had better cognitive health 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, and maybe most interestingly, both men and women tended to be more satisfied with their family life, and men were also more happy with their lives as a consequence of the MSLA reform. These happiness-related, non-market benefits of education were also found in Oreopoulos & Salvanes (2011) and Oreopoulos (2007). 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. MSLA reforms may also have led to female empowerment during a time when women were expected to be the homemaker or work in low-skilled professions.

Figures and tables



Notes: Figure shows the potential effects of a minimum school leaving age reform on human capital formation and end-of-productive years capital stock. Better human capital affects a series of investment decisions concerning labor capital, financial capital, family capital, and health capital during the productive years of life. All of these factors influence the capital stock at retirement age.

Figure 1: Lifecycle capital formation

Table 1: Australian minimum school leaving age reforms in the 1940s to 1960s

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

* 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 the Ordinance/Act was assented to.

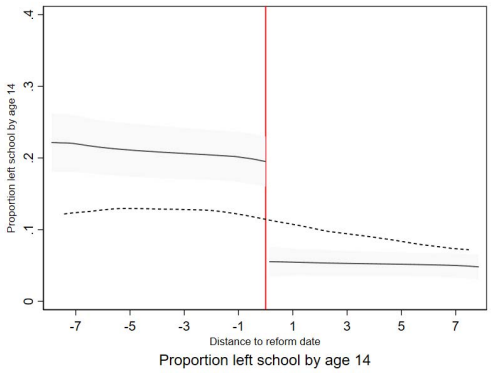
Notes: All dates from the state government gazettes. Minimum school leaving age before proclamation 14th birthday.

Table 2: Summary statistics of main outcome variables

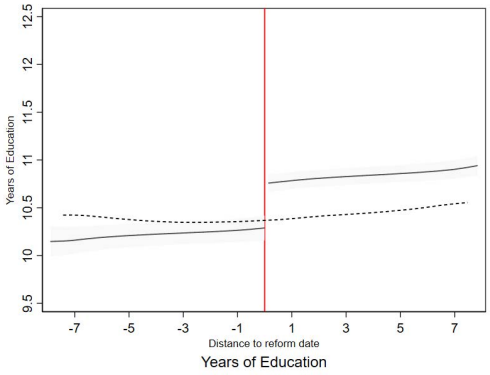
	Mean	SD	Min.	Max.	N	Wave
Formal education						
(1) Schooling						
Left school by age 14	0.108	0.310	0	1	1603	16
Complete at least year 8	0.966	0.180	0	1	1603	16
Complete at least year 9	0.908	0.290	0	1	1603	16
Complete at least year 10	0.802	0.399	0	1	1603	16
Complete at least year 11	0.479	0.500	0	1	1603	16
Complete year 12	0.336	0.472	0	1	1603	16
(2) Educational degree obtained						
Complete CERT III or IV	0.576	0.494	0	1	1603	16
Complete diploma	0.337	0.473	0	1	1603	16
Complete bachelor/honours degree	0.234	0.423	0	1	1603	16
Complete grad. diploma	0.132	0.339	0	1	1603	16
Complete postgrad. level	0.048	0.214	0	1	1603	16
Long-run capital effects						
(1) Skills						
Cognitive skills	0.010	0.997	-3.051	2.459	1458	12,16
Agreeableness	5.488	0.811	1.000	7.000	1522	5,9,13
Conscientiousness	5.232	0.962	1.667	7.000	1522	5,9,13
Emotional stability	5.434	0.975	1.333	7.000	1522	5,9,13
Extroversion	4.343	1.039	1.333	7.000	1522	5,9,13
Openness to experience	4.143	1.006	1.000	7,000	1522	5,9,13
Internal locus of control	5.345	0.978	1.214	7,000	1576	3,4,7,11,15
(2) Labor capital						
Wage (age 50-60)	1169.891	715.968	43.000	7942.455	812	1-16
Years unemployed (age 59)	0.666	2.056	0.000	29.000	1336	1-16
(3) Financial capital						
Home ownership	0.846	0.361	0	1	1600	16
Log wealth	13.172	1.651	1.498	16.746	1558	2,6,10,14
(4) Family capital						
Married	0.630	0.483	0	1	1603	16
Divorced	0.132	0.338	0	1	1603	16
Partner compl. at least year 11	0.509	0.500	0	1	1054	16
Partner completed year 12	0.361	0.480	0	1	1054	16
Number of kids	2.367	1.405	0	10	1601	16
Satisfaction with partner	8.129	1.863	0.000	10.000	1392	1-16
Satisfaction with children	8.228	1.556	0.000	10.000	1438	1-16
(5) Health capital						
Physical health	77.529	20.319	0.000	100.000	1601	1-16
Mental health	75.829	14.349	13.500	100.000	1600	1-16
Overall life satisfaction	8.206	1.360	0.000	10.000	1602	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.

Panel A: Pooled sample

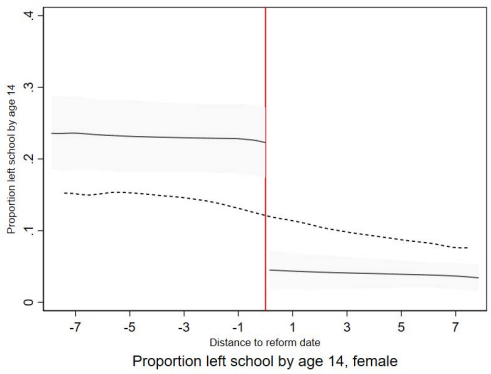


(a) Left school by age 14

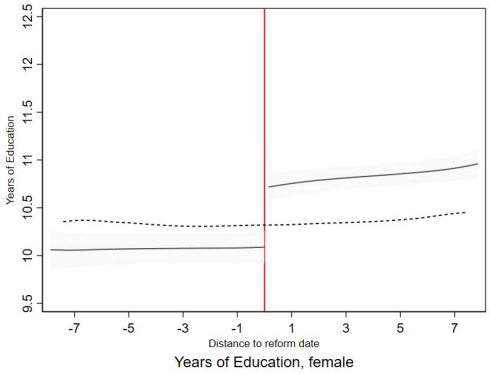


(b) Average years of schooling

Panel B: Female sample

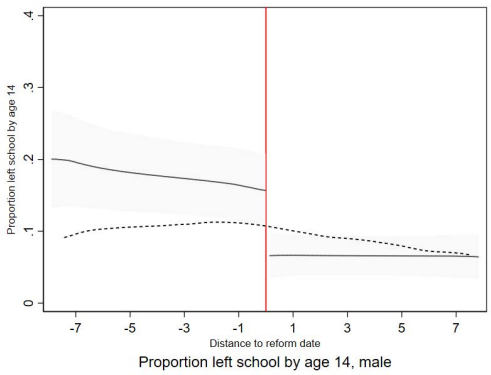


(c) Left school by age 14

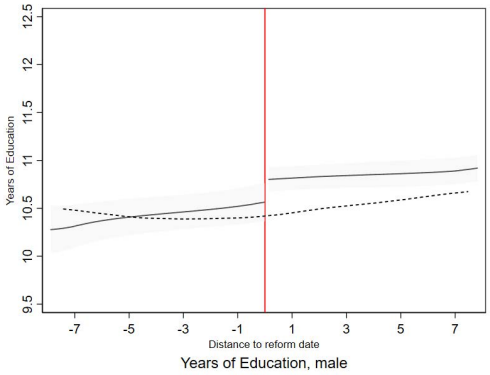


(d) Average years of schooling

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=1603, Panel B=856, Panel C=747.

Figure 2: Reform effect on formal education

Table 3: Difference-in-differences estimation: Formal education

	Main results		Accounting for multiple testing	Accounting for few clusters	
	Coeff.	**Sign.**	**Romano-Wolf**	**T(G-2)**	**Webb adj.**
	(1)	(2)	(3)	(4)	(5)
Panel A: Pooled sample					
(1) Schooling					
Left school by age 14	-0.096	**	***	**	*
Complete year 8	0.056				
Complete year 9	0.115	**		**	*
Complete year 10	0.142	**	***	**	*
Complete year 11	0.151	*			*
Complete year 12	0.047	*			*
(2) Educational degree obtained					
Complete CERT III or IV	-0.048				
Complete diploma	0.061	**	***	**	
Complete bachelor/honours	0.067	***	***	***	*
Complete grad. diploma	0.019				
Complete postgrad. level	0.022				*
Panel B: Female sample					
(1) Schooling					
Left school by age 14	-0.094	**			
Complete year 8	0.045	*			
Complete year 9	0.143	**	***	*	
Complete year 10	0.145	**	***	*	
Complete year 11	0.227	**	***	*	*
Complete year 12	0.143	**	***	*	
(2) Highest degree obtained					
Complete CERT III or IV	-0.058	*			**
Complete diploma	0.063	*			
Complete bachelor/honours	0.039	*			
Complete grad. diploma	-0.013				
Complete postgrad. level	-0.008				
Panel C: Male sample					
(1) Schooling					
Left school by age 14	-0.090	***	***	**	
Complete year 8	0.064				
Complete year 9	0.066	***	***	***	
Complete ear 10	0.123	***	***	***	*
Complete year 11	0.073				*
Complete year 12	-0.060	**	***	**	

Continued on next page

Table 3 – *Continued from previous page*

(2) Highest degree obtained				
Complete at least CERT III or IV	-0.051			
Complete at least diploma	0.038			
Complete at least bachelor/honours	0.094	**	***	**
Complete at least grad. diploma	0.049			
Complete at least postgrad. level	0.055			

Notes: Table shows reform effect for different outcomes. For each outcome variable (separate regressions), the table presents the reform effect in column (1) and corresponding significance levels based on clustered standard errors (at the state level) in column (2). Column (3) shows significance levels adjusted for multiple hypothesis testing (Romano-Wolf method) accounting for all estimations presented in Tables 3-6, separately for panel A, B, and C. The remaining columns correct standard errors for few clusters (T(G-2)(4) and Webb adjustment (5)). All regressions include state and cohort fixed effects and controls for gender, low socioeconomic-status, separated parents, parental employment status at age 14, parents' migration background, number of siblings, and a firstborn identifier. Sample size: Panel A=1603, Panel B=856, Panel C=747. Full main results including standard errors and sample sizes are shown in appendix Table B1. Significance level: *<0.1, **<0.05, ***<0.01.
Source: Hilda survey waves 2001-2016, own calculation.

Table 4: Difference-in-differences estimation: Long-run capital effects

	Main results		Accounting for multiple testing	Accounting for few clusters	
	Coeff.	**Sign.**	**Romano-Wolf**	**T(G-2)**	**Webb adj.**
	(1)	(2)	(3)	(4)	(5)
Panel A: Pooled sample					
(1) Skills					
Cognitive skills	0.145	**	***	**	
Agreeableness	0.136	**	***	**	
Conscientiousness	0.014				
Emotional stability	0.011				
Extroversion	-0.089				
Openness to experience	0.121	**	***	*	*
Internal locus of control	0.055	**	***	*	*
(2) Labor capital					
Wage (age 50-60)	-31.138				
Years unemployed (age 59)	-0.078	*			
(3) Financial capital					
Home ownership	0.021				**
Log wealth	-0.018				
(4) Family capital					
Married	0.028				
Divorced	-0.037	***	***	***	**
Partner completed year 11	0.109	**	***	*	
Partner completed year 12	0.039				
Number of kids	-0.094	*			
Satisfaction with partner	0.216	**	***	**	**
Satisfaction with children	0.317	**	***	**	
(5) Health capital					
Physical health	0.401				
Mental health	1.373				
Overall life satisfaction	0.152	*			
Panel B: Female sample					
(1) Skills					
Cognitive skills	0.144	**	***	**	
Agreeableness	-0.030				
Conscientiousness	-0.053				
Emotional stability	-0.063				*
Extroversion	-0.253				
Openness to experience	0.077				
Internal locus of control	-0.115	**	**	*	
(2) Labor capital					

Continued on next page

Table 4 – *Continued from previous page*

Wage (age 50-60)	127.766	**	***	*	*
Years unemployed (age 59)	-0.087				
(3) Financial capital					
Home ownership	0.077	***	***	***	**
Log wealth	0.242	**	***	**	
(4) Family capital					
Married	0.051				
Divorced	-0.037	*	*	*	
Partner completed year 11	0.140	**	***	*	
Partner completed year 12	-0.012				
Number of kids	-0.020				
Satisfaction with partner	0.324				
Satisfaction with children	0.179	***	***	**	
(5) Health capital					
Physical health	-0.043				
Mental health	0.632				
Overall life satisfaction	0.069				
Panel C: Male sample					
(1) Skills					
Cognitive skills	0.152				
Agreeableness	0.290	***	***	***	*
Conscientiousness	0.063				
Emotional stability	0.080				
Extroversion	0.076				
Openness to experience	0.127				
Internal locus of control	0.209	**	***	**	
(2) Labor capital					
Wage (age 50-60)	-102.162	*			
Years unemployed (age 59)	-0.111				
(3) Financial capital					
Home ownership	-0.062	**	***	*	
Log wealth	-0.290	*			
(4) Family capital					
Married	-0.016				
Divorced	-0.030				
Partner completed year 11	0.050				
Partner completed year 12	0.061				
Number of kids	-0.103				
Satisfaction with partner	0.101				
Satisfaction with children	0.309	**	***	**	*
(5) Health capital					
Physical health	0.852				

Continued on next page

Table 4 – *Continued from previous page*

Mental health	1.469			
Overall life satisfaction	0.257	**	***	*

Notes: Table shows reform effect for different outcomes. For each outcome variable (separate regressions), the table presents the reform effect in column (1) and corresponding significance levels based on clustered standard errors at the state level in column (2). Column (3) shows significance levels adjusted for multiple hypothesis testing (Romano-Wolf method) accounting for all estimations presented in Tables 3-6, separately for panel A, B, and C. The remaining columns correct standard errors for few clusters (T(G-2)(4) and Webb adjustment (5)). Sample size: All min. N=812, max. N=1603; Female min. N=341 max. N=856; Male min. N=448, max. N=747. All regressions include state and cohort fixed effects and controls for gender, low socioeconomic-status, separated parents, parental employment status at age 14, parents' migration background, number of siblings, and a firstborn identifier. Full main results including standard errors and sample sizes are shown in Appendix Table B1. Significance level: *<0.1, **<0.05, ***<0.01.
Source: Hilda survey waves 2001-2016, own calculation.

Table 5: Difference-in-differences estimation: Labor capital, detailed occupations

	All	Female	Male
Managers and administrators	-0.007	0.018	-0.030*
	(0.013)	(0.015)	(0.011)
Professionals	0.118**	0.157*	0.074**
	(0.025)	(0.051)	(0.022)
Romano-Wolf adj.	***	*	**
T(G-2) adj.	**	*	*
Webb adj.	*	*	
Associate professionals	-0.023	-0.023*	0.000
	(0.020)	(0.008)	(0.033)
Romano-Wolf adj.		*	
T(G-2) adj.		*	
Tradepersons and related workers	-0.026**	-0.019**	-0.043***
	(0.006)	(0.005)	(0.007)
Romano-Wolf adj.	***	***	***
T(G-2) adj.	**	*	**
Webb adj.		*	
Advanced clerical ans service workers	0.030	0.038	0.024*
	(0.017)	(0.036)	(0.010)
Intermediate clerical, sales and service workers	-0.047	-0.149	0.044***
	(0.037)	(0.075)	(0.006)
Romano-Wolf adj.		***	
T(G-2) adj.			**
Webb adj.			**
Intermediate production and transport workers	-0.040	-0.000	-0.072
	(0.021)	(0.008)	(0.031)
Elementary clerical, sales and service workers	0.005	-0.046	0.038
	(0.019)	(0.027)	(0.020)
Labourers and related workers	-0.010	0.025	-0.036**
	(0.018)	(0.045)	(0.007)
Romano-Wolf adj.			***
T(G-2) adj.			**

Notes: Table shows reform effect for different outcomes separately for the pooled sample (N=1020), female sample (N=526) and male sample (N=494). For each outcome variable (separate regressions), the table presents the reform effect, standard errors in parenthesis and the corresponding significance level based on clustered standard errors at the state level. Significance levels adjusted for multiple hypothesis testing (Romano-Wolf method, accounting for all estimations in Tables 3-6, separately for panel A, B, and C)) and few clusters (T(G-2) and Webb adjustment) are shown, if significant for at least one of the three samples. All regressions include state and cohort fixed effects and controls for gender, low socioeconomic-status, separated parents, parental employment status at age 14, parents' migration background, number of siblings, and a firstborn identifier. Significance level: *<0.1, **<0.05, ***<0.01.
Source: HILDA survey waves 2001-2016, own calculation.

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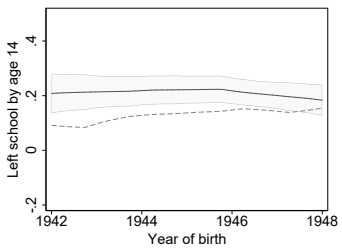
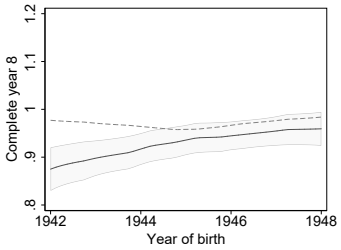
Appendix

A Identification assumptions

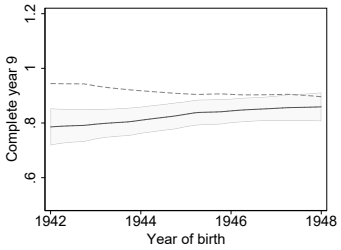
(1) Schooling



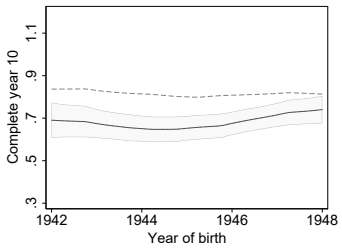
(a) Left school by age 14



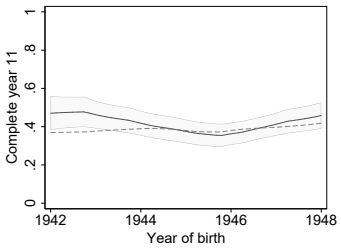
(b) *Complete year 8



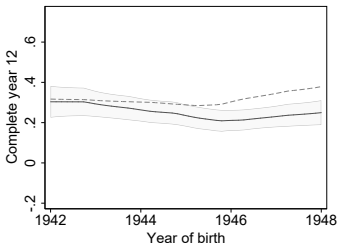
(c) *Complete year 9



(d) Complete year 10

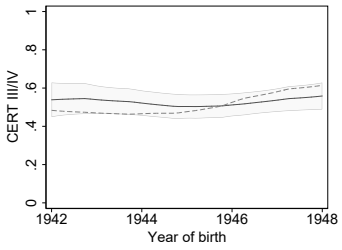


(e) Complete year 11

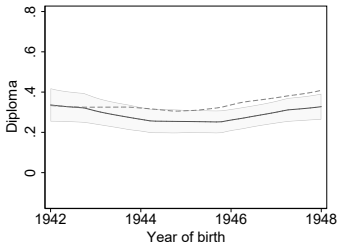


(f) Complete year 12

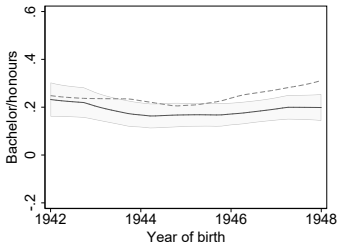
(2) Highest degree obtained



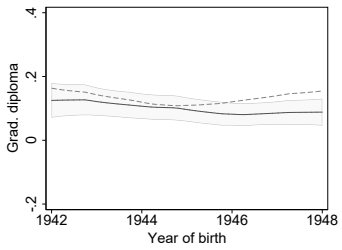
(g) CERT III/IV



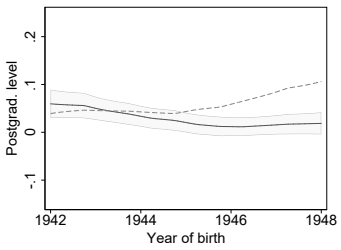
(h) Diploma



(i) Bachelor/honours



(j) Grad. diploma

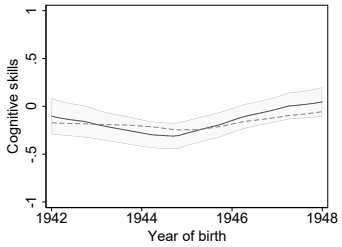


(k) Postgrad. level

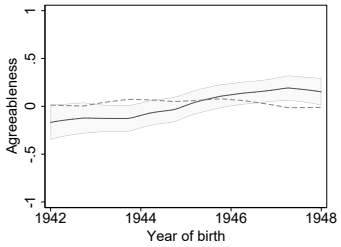
Notes: Sample size: 1603. Solid black line represents time trend for treated states surrounded by a 95% confidence interval, dashed line represents time trend for non-treated states. Figures are based on a kernel-weighted local polynomial regression with bandwidth 1 for the pre-reform cohorts (1942-1948).

Figure A1: Pre-reform trend - Formal education

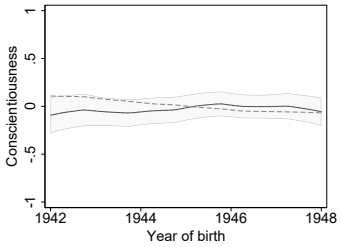
(1) Skills



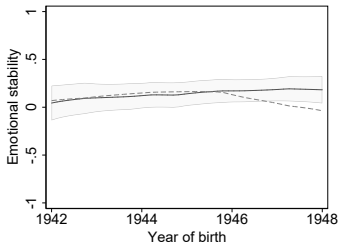
(a) Cognitive skills



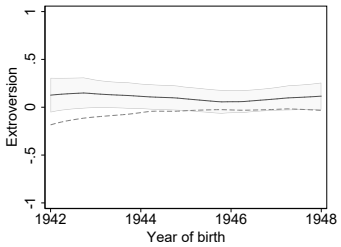
(b) *Agreeableness



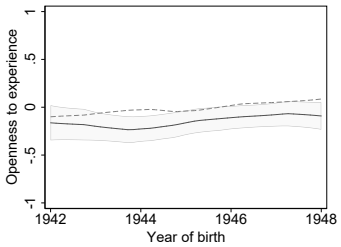
(c) Conscientiousness



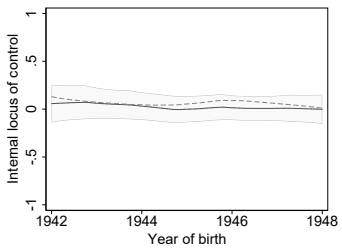
(d) Emotional stability



(e) Extroversion

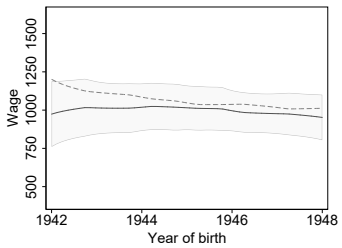


(f) Openness to experience

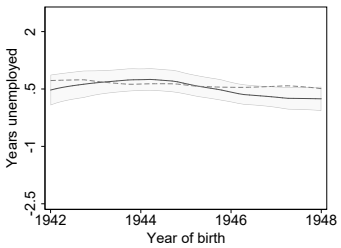


(g) Internal locus of control

(2) Labor capital

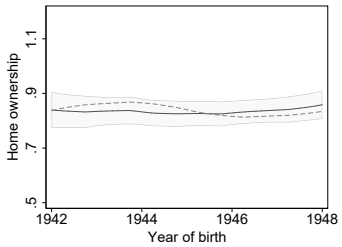


(h) Wage

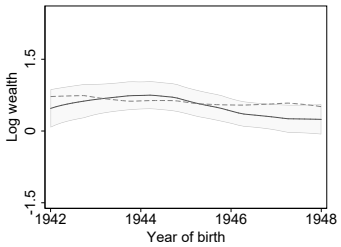


(i) Years unemployed

(3) Financial capital

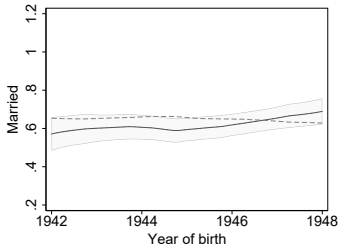


(j) Home ownership

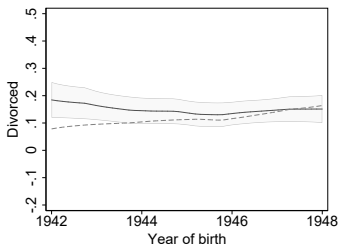


(k) Log wealth

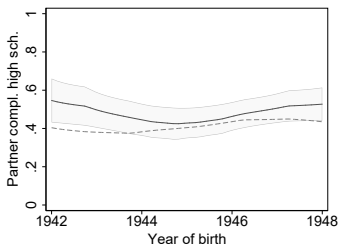
(4) Family capital



(l) Married

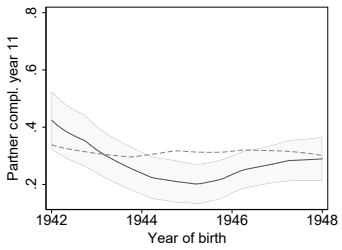


(m) Divorced

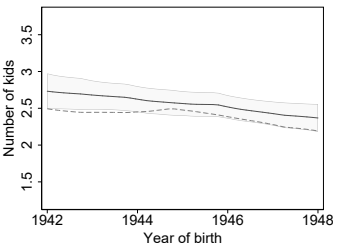


(n) Partner compl. year 11

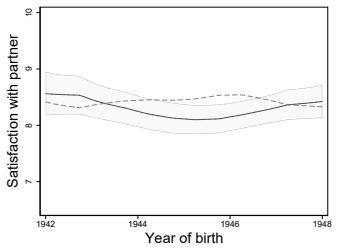
]



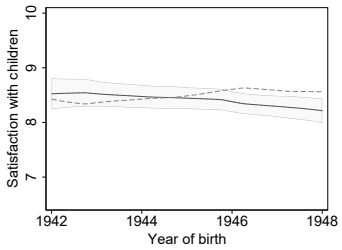
(o) *Partner compl. year 12



(p) Number of kids

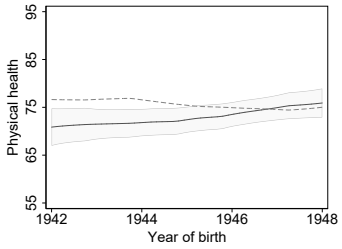


(q) Satisfaction with partner

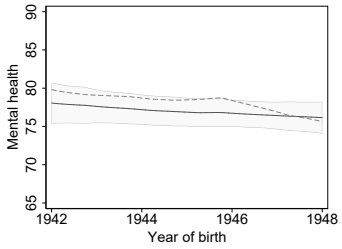


(r) Satisfaction with children

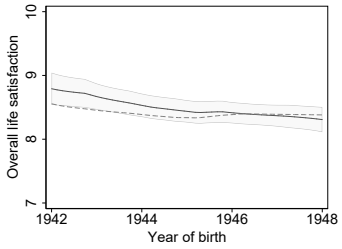
(5) Health capital



(s) *Physical health



(t) Mental health



(u) Overall life satisfaction

Notes: Sample size: Min. N=812, max. N=1603. Solid black line represents time trend for treated states surrounded by a 95% confidence interval, dashed line represents time trend for non-treated states. Figures are based on a kernel-weighted local polynomial regression with bandwidth 1 for the pre-reform cohorts (1942-1948). * indicates that common trend assumption may be violated and presented treatment effect may be over-estimated.

Figure A2: Pre-reform trend: Long run capital

Table A1: Covariates

	Summary statistics		Balancing covariates	
	Mean	**Std. dev.**	**Coef.**	**Std. Error**
Female	0.534	0.499	-0.110	(0.048)
Low socioeconomic status	0.731	0.444	-0.047	(0.029)
Mother employed at age 14	0.382	0.486	0.056*	(0.023)
Father employed at age 14	0.928	0.258	0.000	(0.023)
At least one parent born abroad	0.151	0.358	0.027	(0.029)
Oldest child in the household	0.319	0.466	-0.077	(0.040)
Number ob siblings	0.951	0.217	-0.019	(0.012)
Grew up with single parent	0.079	0.269	0.025	(0.013)

Notes: Sample size: 1603. Balancing test is based on a regression equation like eq. 1, excluding the vector x. Each coefficient is based on a separate regression. Standard errors are cluster robust on the state level. Significance level: *<0.1, **<0.05, ***<0.01.
Source: Hilda survey waves 2001-2016, own calculation.

Table A2: Difference-in-differences estimation: Placebo reform

	Main results		Accounting for multiple testing	Accounting for few clusters	
	Coeff.	**Sign.**	**Romano-Wolf**	**T(G-2)**	**Webb adj.**
	(1)	(2)	(3)	(4)	(5)
Formal education					
(1) Schooling					
Left school by age 14	-0.107				
Complete year 8	0.019				
Complete year 9	0.058				
Complete year 10	0.081				
Complete year 11	0.078				
Complete year 12	-0.028				
(2) Highest degree obtained					
Complete CERT III or IV	-0.060				
Complete diploma	0.068	**		*	
Complete bachelor/honours degree	0.043				
Complete grad. diploma	0.028				
Complete postgrad. level	-0.048	**		**	**
Long-run capital effects					
(1) Skills					
Cognitive skills	0.185				
Agreeableness	0.017				
Conscientiousness	-0.119				
Emotional stability	0.040				
Extroversion	0.012				
Openness to experience	-0.056				
Internal locus of control	-0.058				
(2) Labor capital					
Wage (age 50-60)	-14.118				
Years unemployed (age 59)	-0.266	**		*	
(3) Financial capital					
Home ownership	-0.019				
Log wealth	-0.120				
(4) Family capital					
Married	0.068				
Divorced	-0.031				
Partner completed year 11	0.533				
Partner completed year 12	0.089				
Number of kids	-0.213				
Satisfaction with partner	0.280				
Satisfaction with children	-0.395	**		*	

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Table A2 – *Continued from previous page*

(5) Health capital	
Physical health	3.294
Mental health	2.507
Overall life satisfaction	-0.219

Notes: Table shows the effect for a placebo reform for different outcomes. The sample includes only individuals in pre-reform cohorts (N not larger than 594). We set the placebo reform to two years before the real reform occurred. For each outcome variable (separate regressions), the table presents the reform effect in column (1) and corresponding significance levels based on clustered standard errors (at the state level) in column (2). Remaining columns show significance levels adjusted for multiple hypothesis testing (Romano-Wolf method) (3) as well as few clusters (T(G-2) (4) and Webb adjustment (5)). All regressions include state and cohort fixed effects and controls for gender, low socioeconomic-status, separated parents, parental employment status at age 14, parents' migration background, number of siblings, and a firstborn identifier. Significance level: *<0.1, **<0.05, ***<0.01.
Source: Hilda survey waves 2001-2016, own calculation.

Table A3: Variation of included cohorts

	8.5 years before/after reform			6.5 years before/after reform		
	All	**Female**	**Male**	**All**	**Female**	**Male**
Formal education						
(1) Schooling						
Left school by age 14	-0.116***	-0.116**	-0.102***	-0.091***	-0.093**	-0.074*
	(0.016)	(0.031)	(0.013)	(0.011)	(0.028)	(0.027)
	1815	967	848	1412	751	661
Complete year 8	0.056*	0.044*	0.067	0.047**	0.042*	0.044*
	(0.021)	(0.014)	(0.036)	(0.014)	(0.014)	(0.017)
	1815	967	848	1412	751	661
Complete year 9	0.137***	0.163**	0.094***	0.113***	0.149**	0.048*
	(0.022)	(0.032)	(0.005)	(0.016)	(0.033)	(0.020)
	1815	967	848	1412	751	661
Complete year 10	0.155***	0.154**	0.139***	0.152***	0.162**	0.120***
	(0.019)	(0.027)	(0.007)	(0.014)	(0.030)	(0.016)
	1815	967	848	1412	751	661
Complete year 11	0.155*	0.249**	0.063	0.147*	0.241**	0.057
	(0.051)	(0.048)	(0.050)	(0.059)	(0.062)	(0.046)
	1815	967	848	1412	751	661
Complete year 12	0.048**	0.156**	-0.070***	0.028	0.133**	-0.085***
	(0.011)	(0.028)	(0.012)	(0.020)	(0.036)	(0.013)
	1815	967	848	1412	751	661
(2) Educational degree obtained						
Complete CERT III IV	-0.020	-0.030	-0.019	-0.062	-0.067	-0.080
	(0.034)	(0.021)	(0.072)	(0.034)	(0.029)	(0.057)
	1815	967	848	1412	751	661
Complete diploma	0.076**	0.096**	0.042	0.056**	0.062	0.024
	(0.013)	(0.023)	(0.026)	(0.011)	(0.027)	(0.027)
	1815	967	848	1412	751	661
Complete bachelor/honours	0.077**	0.076**	0.080*	0.057**	0.029	0.084**
	(0.014)	(0.020)	(0.027)	(0.010)	(0.022)	(0.023)
	1815	967	848	1412	751	661
Complete grad. diploma	0.026	0.013	0.042	-0.011	-0.032*	0.007
	(0.022)	(0.018)	(0.037)	(0.019)	(0.012)	(0.036)
	1815	967	848	1412	751	661
Complete postgrad. Level	0.017	-0.004	0.043	0.017	-0.021	0.058
	(0.015)	(0.010)	(0.025)	(0.020)	(0.017)	(0.031)
	1815	967	848	1412	751	661
Long-run capital effects						
(1) Skills						
Cognitive skills	0.104*	0.153***	0.084	0.141*	0.174*	0.094
	(0.037)	(0.020)	(0.089)	(0.049)	(0.068)	(0.100)
	1658	879	779	1281	678	603
Agreeableness	0.107**	0.002	0.212***	0.109*	-0.058	0.260***
	(0.033)	(0.061)	(0.017)	(0.035)	(0.061)	(0.018)
	1721	919	802	1338	713	625
Conscientiousness	0.045**	-0.014	0.061	0.016	-0.053	0.037
	(0.009)	(0.035)	(0.032)	(0.021)	(0.038)	(0.030)
	1721	919	802	1338	713	625
Emotional stability	0.032	-0.026	0.093	0.004	-0.068	0.058
	(0.039)	(0.033)	(0.051)	(0.048)	(0.032)	(0.058)
	1721	919	802	1338	713	625
Extroversion	-0.045	-0.207	0.106	-0.042	-0.190	0.094
	(0.106)	(0.124)	(0.077)	(0.127)	(0.140)	(0.078)
	1721	919	802	1338	713	625
Openness to exper.	0.129**	0.111	0.123	0.157**	0.120	0.141**
	(0.038)	(0.072)	(0.063)	(0.036)	(0.093)	(0.041)
	1721	919	802	1338	713	625
Internal locus of control	0.028*	-0.080*	0.121**	0.036**	-0.088	0.132**
	(0.010)	(0.025)	(0.025)	(0.011)	(0.037)	(0.041)
	1784	953	831	1390	741	649

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Table A3 – *Continued from previous page*

(2) Labor capital						
Wage (age 50-60)	2.483	161.738**	-75.595	-56.784	103.819**	-130.102*
	(21.685)	(32.590)	(57.585)	(24.821)	(31.515)	(43.900)
	928	391	537	712	297	415
	910	440	470	759	365	394
Years unemployed (age 59)	-0.032	0.030	-0.149	-0.095	0.067	-0.290**
	(0.019)	(0.100)	(0.089)	(0.053)	(0.159)	(0.086)
	1421	767	654	1163	623	540
(3) Financial capital						
Home ownership	0.039***	0.114***	-0.059***	0.029**	0.099***	-0.067**
	(0.004)	(0.007)	(0.008)	(0.008)	(0.013)	(0.020)
	1812	965	847	1409	749	660
Log wealth	-0.019	0.267**	-0.312**	-0.001	0.340***	-0.353**
	(0.016)	(0.058)	(0.098)	(0.025)	(0.035)	(0.071)
	1114	551	563	874	429	445
(4) Family capital						
Married	0.024	0.078	-0.050	0.020	0.020	0.005
	(0.023)	(0.036)	(0.042)	(0.038)	(0.052)	(0.052)
	1815	967	848	1412	751	661
Divorced	-0.046***	-0.076***	-0.013	-0.036*	-0.037***	-0.036
	(0.003)	(0.009)	(0.011)	(0.012)	(0.005)	(0.015)
	1815	967	848	1412	751	661
Partner compl. year 11	0.112***	0.123**	0.066	0.105	0.178**	0.017
	(0.019)	(0.030)	(0.029)	(0.048)	(0.051)	(0.070)
	1195	571	624	924	441	483
Partner compl. year 12	0.048	-0.002	0.070*	0.063	0.045	0.064
	(0.042)	(0.077)	(0.028)	(0.053)	(0.088)	(0.045)
	1195	571	624	924	441	483
Number of kids	-0.232***	-0.173**	-0.224**	-0.082	-0.038	-0.048
	(0.038)	(0.032)	(0.044)	(0.043)	(0.071)	(0.037)
	1813	965	848	1411	750	661
Satisfaction with partner	0.101*	0.335**	-0.111	0.220***	0.214	0.198
	(0.043)	(0.102)	(0.132)	(0.033)	(0.144)	(0.151)
	1585	805	780	1222	619	603
Satisfaction with children	0.270***	0.197**	0.200*	0.323***	0.178**	0.316**
	(0.042)	(0.037)	(0.076)	(0.023)	(0.050)	(0.057)
	1626	870	756	1267	679	588
(5) Health capital						
Physical health	1.128	0.909	1.309	-0.399	-1.051	0.380
	(0.588)	(0.746)	(1.008)	(0.651)	(0.458)	(1.324)
	1813	966	847	1410	750	660
Mental health	1.129	0.680**	1.040*	0.576	-0.285	0.662
	(0.509)	(0.213)	(0.434)	(1.255)	(0.991)	(0.968)
	1812	965	847	1409	749	660
Overall life satisfaction	0.116	0.058	0.191**	0.147	0.060	0.232**
	(0.056)	(0.090)	(0.052)	(0.084)	(0.111)	(0.056)
	1814	966	848	1411	750	661

Notes: Table shows reform effect for different outcomes. The estimations include individuals between 8.5 (6.5) years before after the reform date. For each outcome variable (separate regressions), the table presents the reform effect, standard errors in parenthesis, the corresponding significance level based on clustered standard errors at the state level and the sample size. All regressions include state and cohort fixed effects and controls for gender, low socioeconomic-status, separated parents, parental employment status at age 14, parents' migration background, number of siblings, and a firstborn identifier. Full main results including standard errors and sample sizes are shown in Appendix Table B1. Significance level: *<0.1, **<0.05, ***<0.01. Source: Hilda survey waves 2001-2016, own calculation.

B Full estimation results

Table B1: Full DiD results: Formal education

	All	Female	Male
(1) Schooling			
Left school by age 14	-0.096**	-0.094*	-0.090***
	(0.022)	(0.034)	(0.011)
	1603	856	747
Complete year 8	0.056	0.045*	0.064
	(0.024)	(0.018)	(0.035)
	1603	856	747
Complete year 9	0.115**	0.143**	0.066***
	(0.025)	(0.036)	(0.006)
	1603	856	747
Complete year 10	0.142**	0.145**	0.123***
	(0.025)	(0.035)	(0.008)
	1603	856	747
Complete year 11	0.151*	0.227**	0.073
	(0.059)	(0.054)	(0.060)
	1603	856	747
Complete year 12	0.047*	0.143**	-0.060**
	(0.018)	(0.037)	(0.013)
	1603	856	747
(2) Educational degree obtained			
Complete CERT III or IV	-0.048	-0.058*	-0.051
	(0.025)	(0.021)	(0.053)
	1603	856	747
Complete diploma	0.061**	0.063*	0.038
	(0.012)	(0.024)	(0.028)
	1603	856	747
Complete bachelor or honours	0.067***	0.039*	0.094**
	(0.006)	(0.016)	(0.021)
	1603	856	747
Complete grad. diploma	0.019	-0.013	0.049
	(0.021)	(0.018)	(0.036)
	1603	856	747
Complete postgrad. level	0.022	-0.008	0.055
	(0.017)	(0.013)	(0.029)
	1603	856	747

Notes: Table shows reform effect presented in table 3, the corresponding standard errors in parenthesis and the respective sample size. For more details see notes to table 3.

Table B2: Full DiD results: Long-run capital effects

	All	Female	Male
(1) Skills			
Cognitive skills	0.145**	0.144**	0.152
	(0.032)	(0.031)	(0.093)
	1458	776	682
Agreeableness	0.136**	-0.030	0.290***
	(0.026)	(0.041)	(0.023)
	1522	813	709
Conscientiousness	0.014	-0.053	0.063
	(0.018)	(0.034)	(0.042)
	1522	813	709
Emotional stability	0.011	-0.063	0.080
	(0.049)	(0.032)	(0.056)
	1522	813	709
Extroversion	-0.089	-0.253	0.076
	(0.127)	(0.149)	(0.079)
	1522	813	709
Openness to experience	0.121**	0.077	0.127
	(0.034)	(0.064)	(0.056)
	1522	813	709
Internal locus of control	0.055**	-0.115**	0.209**
	(0.016)	(0.032)	(0.044)
	1576	844	732
(2) Labor capital			
Wage (age 50-60)	-31.138	127.766**	-102.162*
	(23.024)	(27.791)	(43.045)
	812	341	471
Occupational status (age 59)	0.468	-3.020	3.117*
	(0.417)	(2.202)	(1.295)
	859	411	448
Years unemployed (age 59)	-0.078*	-0.087	-0.111
	(0.028)	(0.134)	(0.081)
	1336	719	617
(3) Financial capital			
Home ownership	0.021	0.077***	-0.062**
	(0.011)	(0.006)	(0.015)
	1600	854	746
Log wealth	-0.018	0.242**	-0.290*
	(0.010)	(0.048)	(0.114)
	990	488	502
(4): Family capital			
Married	0.028	0.051	-0.016
	(0.032)	(0.046)	(0.047)
	1603	856	747

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Table B2 – *Continued from previous page*

Divorced	-0.037***	-0.037*	-0.030
	(0.002)	(0.012)	(0.014)
	1603	856	747
Partner completed year 11	0.109**	0.140**	0.050
	(0.031)	(0.039)	(0.046)
	1054	505	549
Partner completed year 12	0.039	-0.012	0.061
	(0.047)	(0.070)	(0.038)
	1054	505	549
Number of kids	-0.094*	-0.020	-0.103
	(0.033)	(0.020)	(0.044)
	1601	854	747
Satisfaction with partner	0.216***	0.324	0.101
	(0.029)	(0.153)	(0.125)
	1392	708	684
Satisfaction with children	0.317***	0.179***	0.309**
	(0.037)	(0.027)	(0.068)
	1438	772	666
(5) Health capital			
Physical health	0.401	-0.043	0.852
	(0.572)	(0.479)	(1.218)
	1601	855	746
Mental health	1.373	0.632	1.469
	(1.004)	(0.700)	(0.844)
	1600	854	746
Overall life satisfaction	0.120*	0.063	0.186***
	(0.045)	(0.070)	(0.021)
	1603	856	747

Notes: Table shows reform effect presented in table 4, the corresponding standard errors in parenthesis and the respective sample size. For more details see notes to tables 4.