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Schooling and Self-Control

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Research Summary

Why was the research done?

Researchers have repeatedly demonstrated the importance of self-control for people's life outcomes. Those with more self-control have healthier lifestyles, higher educational attainment, more labor market success, enhanced financial well-being, and higher levels of life satisfaction. While the positive association between self-control and many favorable outcomes is well-established, the direction of causality is unclear. We do not fully understand whether self-control is a cause or a consequence of people's life success.

What were the key findings?

We make a contribution to resolving this issue by exploiting a series of Australian and German educational reforms that increased minimum education requirements as a source of exogenous variation in education levels. Instrumental variables estimates suggest that, for people affected by the reforms, an additional year of schooling has no effect on self-control.

What does this mean for policy and practice?

Our take-away is that, while enhancing self-control through school-based interventions is feasible, long-term success is likely to rely on specifically tailored curricula and pedagogical choices. Simply increasing the length of time students are required to spend in formal education does not seem to be enough to increase the capacity for self-control.

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Schooling and Self-Control*

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Abstract: While there is an established positive relationship between self-control and education, the direction of causality remains a matter of debate. We make a contribution to resolving this issue by exploiting a series of Australian and German educational reforms that increased minimum education requirements as a source of exogenous variation in education levels. Instrumental variables estimates suggest that, for people affected by the reforms, an additional year of schooling has no effect on self-control.

Keywords: self-control; quasi-experiments; compulsory schooling reforms; Brief Self-Control Scale

JEL Classifications: *D90, I26, C26*

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1 Introduction

Researchers have repeatedly demonstrated the importance of self-control for people's life outcomes. Those with more self-control have healthier lifestyles, higher educational attainment, more labor market success, enhanced financial well-being, and higher levels of life satisfaction (see, e.g., Cobb-Clark et al., 2022; Duckworth and Seligman, 2005; Kaur et al., 2015; Moffitt et al., 2011; Tangney et al., 2004). While the positive association between self-control and many favorable outcomes is well-established, the direction of causality is unclear. We do not fully understand whether self-control is a cause or a consequence of people's life success.

Our work makes a contribution by exploiting educational reforms in both Australia and Germany to assess whether people's self-control is shaped by the amount of education they have received. Although people's capacity for self-control is no doubt a key driver of their educational attainment, it is also possible that a longer exposure to the school environment—in which there are high returns to discipline (being on time, not skipping class, doing homework)—strengthens students' self-control. Addressing this type of reverse causality is empirically challenging. We are able to make progress, however, by analyzing a series of structural reforms of the Australian and German school systems. The minimum school leaving age in all Australian states and territories was raised from age 14 to age 15 at some point between 1943 and 1966.[1] Between 1956 and 1969, West German states implemented similar reforms that introduced a mandatory ninth grade. This allows us to exploit arguably exogenous variation in the time people spend in compulsory schooling to study the effects of education on self-control.

Compulsory schooling reforms are a common feature of the education systems in many industrialized countries (see, e.g., Harmon, 2017, for a review). In this paper, we focus on Australia and Germany—two countries with similar educational reforms that also have population-representative, validated self-control measures available. In Australia, educational policy is determined by the states and territories. Schooling is provided up to grade 12—typically divided into six or seven years in primary school and five or six years in secondary school. Before the reforms, however, students were only required to remain in school until they turned 14 years old. Subsequently, the minimum school-leaving age was raised to age 15 (or 16) across all states and territories. Australia experienced a large inflow of students with the baby-boomers and

[1] In Tasmania the minimum school leaving age was raised to age 16, while the changes in Western Australia mandated students to stay in school until the end of the year they turned 15.

high immigration from the 1950s onwards. Consequently, these reforms were targeted at improving human capital among these large cohorts to avoid a potentially large number of school leavers entering unemployment (see de New et al., 2021, and the references therein, for a more extensive discussion of the reforms). We exploit the introduction of the reforms in the states of South Australia and Victoria for students born on or after April 4, 1949 (South Australia) and February 4, 1950 (Victoria), using students in the Australian Capital Territory and New South Wales—where the school leaving age had been raised more than two decades prior—as a comparison group.

In Germany, like in Australia, education policy is determined by the governments of the federal states, not the national federal government. Following typically four years in primary school, students are tracked into three secondary school types.[2] Before the reforms, students attending basic school (*Hauptschule*) graduated after a total of eight years of education: four years of elementary schooling (ages 6 to 10) and four years in basic school. Subsequently, students affected by the compulsory schooling reforms undertook a total of nine years of schooling. Consideration of mandatory ninth grade for basic school students in Germany began in the immediate aftermath of World War II. Weak labor market conditions and inadequate skills of the workforce to cope with new production technologies were the main reasons cited for extending the time spent in school (see Pischke and von Wachter, 2008, for a more extensive discussion of the reforms). We exploit the staggered introduction of the reforms across the federal states in an instrumental variables approach.

Despite differences in the educational setting, the reforms in both countries targeted similar students—those leaving school as soon as they were permitted to do so. In both cases, we can therefore estimate local average treatment effects for those at the lower end of the educational distribution, enabling us to provide consistent evidence across two different countries.

The combination of exogenous variation in years spent in school and novel, population-representative data on self-control measured consistently across both countries allows us to add to a very limited number of studies analyzing the effects of institutional change on population-level self-control.[3] Studies evaluating targeted educational interventions, especially those de-

[2]The three secondary school types (listed in ascending order by educational attainment) are basic schools (*Hauptschule*), intermediate schools (*Realschule*), and academic schools (*Gymnasium*).

[3]See also Cobb-Clark et al. (2024) who analyze the post-WWII division of Germany to provide evidence that former citizens of the communist German Democratic Republic (East Germany) have higher levels of self-control than their West German counterparts.

signed to improve socio-emotional skills, demonstrate that self-control is malleable (Alan et al., 2019; Alan and Ertac, 2018; Breitkopf et al., 2024; Piquero et al., 2016; Sorrenti et al., 2024). The effect of broader educational reforms on population-level self-control, however, has not previously been investigated.

2 Data

Our analysis draws on data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey (Watson and Wooden, 2021) and from the Innovation Sample of the German Socio-Economic Panel (SOEP-IS) (Goebel et al., 2019; Richter and Schupp, 2015). The HILDA Survey is a rich annual household survey that is representative of the Australian population, while SOEP-IS data—like the core SOEP—are both rich and representative of the German population. As SOEP-IS was designed to support innovations in survey design, researchers are able to propose new batteries of questions to be surveyed. Following a competitive tender process, in 2017, we were granted the opportunity to administer the 13-item Brief Self-Control Scale (BSCS) for the first time in a major, nationally-representative household survey. In 2019, the BSCS was included for the first time in the HILDA Survey.

Measuring Self-Control. Originally proposed by Tangney et al. (2004), the 13-item BSCS is highly correlated with the full 36-item version (Tangney et al., 2004), has high predictive validity across remarkably diverse domains (de Ridder et al., 2012), is internally consistent (Bertrams and Dickhäuser, 2009; Tangney et al., 2004), and has a high test–retest reliability both after three (0.87, Tangney et al., 2004) and seven weeks (0.82, Bertrams and Dickhäuser, 2009). Table 1 presents all items of the BSCS, which are answered on a scale from 1 to 5. For comparability across items, we first standardize each item to have mean 0 with standard deviation 1. We then compute an individual's self-control by taking an average of all 13 items and standardizing the resulting score again to have mean 0 with standard deviation 1. While we measure self-control long after school completion, BSCS scores appear to be stable in adulthood, at least in the medium-run. In particular, Cobb-Clark et al. (2023) demonstrate a high degree of mean-level, rank-order, and individual-level stability in Germans' BSCS scores over a three year horizon.

—Insert Table 1 here—

Australian Sample. Starting with all respondents of the HILDA Survey who completed the self-completion questionnaire containing the BSCS in 2019, we retain individuals in the final estimation sample if they (i) answered all BSCS items; (ii) reported the state in which they obtained their highest school degree; (iii) completed school in either the Australian Capital Territory, New South Wales, South Australia, or Victoria;[4] and (iv) were born between 1942 and 1957. These restrictions follow de New et al. (2021) and provide us with a sample in which all individuals completing school in the Australian Capital Territory or New South Wales were required to stay in school until age 15, while individuals in South Australia and Victoria were subject to the increased minimum school leaving age only if born in 1949/1950 or later. These selection rules result in an estimation sample of 1,710 respondents.

German Sample. Starting with all respondents in SOEP-IS who were administered the BSCS in 2017, we retain individuals in the final estimation sample if they (i) answered all BSCS items; (ii) were born ±15 years around the first cohort affected by the reform in their state; (iii) were born between 1940 and 1970; (iv) live in the West German states of Baden-Württemberg, Bavaria, Hesse, Lower Saxony, North Rhine-Westphalia, or Rhineland-Palatinate as the reforms took place in West Germany only;[5] and (v) attended a basic school (*Hauptschule*) or an intermediate school (*Realschule*)—the two tracks that were either directly or indirectly affected, as some students' response to the reforms may have been to opt for a more prestigious intermediate school degree awarded after 10 years of schooling in total. Students in academically more demanding *Gymnasium* schools were not affected by the reforms and are dropped from the analysis. These criteria ensure a homogeneous estimation sample where some individuals are affected by the compulsory schooling reforms, while others—born prior to their introductions—are not. These selection rules result in an estimation sample of 415 respondents.

Table 2 presents descriptive statistics for both main estimation samples. Importantly, raw self-control scores—i.e., without standardizing the individual items or the resulting score—are very similar. The slightly higher scores for Australians (47.63) than for Germans (46.43) can likely to be attributed to the four year age difference in the samples, as self-control is found

[4] We do not include the states of Tasmania and Western Australia (where the minimum school leaving age was raised beyond students' 15$^{\text{th}}$ birthday), Queensland (with other simultaneous educational changes taking place), and the Northern Territory (given its small population and hence few observations in our data) in our preferred estimation, see Table 3. However, our results are robust to their inclusion, see Section 4.1.

[5] We exclude smaller states for which we have too few observations, and therefore no variation in treatment, and city states as their small geographical size and a high share of out-of-state commuting make a mis-assignment of the state where people obtained their schooling degree more likely.

to increase with age (Cobb-Clark et al., 2024). Both samples have slightly more women than men. By construction, average years of schooling are lower in our German sample (9.1)—which is restricted to graduates from basic and intermediate schools—than in our Australian sample (10.6).

—Insert Table 2 here—

3 Empirical Strategy

We are interested in the relationship between education and self-control. Formally, this can be modeled as:

$$\text{Self-Control}_{ist} = \beta_0 + \beta_1 S_{ist} + \beta_2 F_{ist} + \boldsymbol{\gamma}_s + \boldsymbol{\omega}_t + \epsilon_{ist}, \tag{1}$$

where $\text{Self-Control}_{ist}$ is the standardized mean of the 13 BSCS items for individual i in state s and year t. Moreover, S_{ist} is years of schooling, making β_1 the parameter of interest in our investigation. We control for gender using an indicator if i is female (F_{ist}) as well as state ($\boldsymbol{\gamma}_s$) and year ($\boldsymbol{\omega}_t$) fixed effects, while ϵ_{ist} is the error term.

Ordinary least squares (OLS) estimation of equation (1) does not yield the causal effect of education on self-control given the potential for omitted confounders and reverse causality. We address this by implementing a two-stage least squares (2SLS) instrumental variables estimation strategy. The instrument that exogenously shifts individuals' education is the compulsory schooling reform Z_{st} in state s which raises the minimum school leaving age from 14 to 15 years (Australia) or adds a ninth grade to basic schools (Germany), respectively. Despite the institutional difference, econometrically both reforms can be expressed through a binary indicator. Australian students in state s who are unaffected by the relevant state-level reform may leave on their 14$^{\text{th}}$ birthday, while unaffected German students can leave school after a total of eight years of education ($Z_{st} = 0$). Individuals born after the introduction cohort, however, are legally required to remain in school until their 15$^{\text{th}}$ birthday in Australia or for at least nine years in Germany ($Z_{st} = 1$).[6] Tables 3 and 4 show the year reforms were introduced in each state in both countries.

—Insert Tables 3 and 4 here—

We exploit this variation in the first stage of a 2SLS model to estimate years of schooling (S_{ist}):

$$\mathrm{S}_{ist} = \alpha_0 + \alpha_1 \mathrm{Z}_{ist} + \alpha_2 \mathrm{F}_{ist} + \boldsymbol{\gamma}_s + \boldsymbol{\omega}_t + \xi_{ist}. \tag{2}$$

The second stage is calculated by inserting the fitted years of schooling ($\hat{S}_{ist}$) into equation (1), instead of the actual (endogenous) years of schooling.

The 2SLS approach yields the causal effect of education on self-control for people affected by the compulsory schooling reforms (those at the left-hand side of the educational distribution), if our instrument, the compulsory schooling reforms, meets the following requirements: they (i) were not introduced earlier in some states because political decision-makers were worried about a decline in self-control or related personality traits; (ii) affect self-control only through years of schooling, as opposed to any other channel; and (iii) significantly increased years of schooling. Neither set of reforms was introduced to specifically target non-cognitive skills. In Australia, raising the minimum school leaving age was aimed at promoting human capital development more broadly as means of preventing unemployment. In Germany, the curriculum of the ninth grade was designed to provide a more in-depth general education and improve students' occupational maturity, with a special emphasis on political upbringing in many states (see Margaryan et al., 2021). Thus, the first assumption, independence, seems reasonable. Moreover, previous studies have not identified other contemporaneous reforms or structural changes that might also have affected people's life outcomes—hence justifying the second assumption, the exclusion

[6] For Australia, we have direct information on the state in which people obtained their highest school leaving degree. For Germany, we use the current state of residence as a proxy. As noted by Pischke and von Wachter (2008), people with basic schooling have a lower tendency to move out of their home state relative to those with higher levels of education.

restriction.[7] Finally, the third requirement, the instrument's power, is an empirical question reflected in the first stage, which we consider below.

4 Results

4.1 Australia

Our 2SLS results are presented in panel (a) of Table 5. People affected by the increased minimum school leaving age have, on average, 0.597 additional years of schooling (column 1). The first-stage F-statistic is 27.873, implying that the Australian reforms were relevant for the years of schooling attained by students; this meets the third instrument requirement discussed in Section 3. Our second stage effect of years of schooling on self-control is close to zero (0.012, column 2). This is further confirmed by the reduced-form effect, capturing the direct effect of the increased minimum school leaving age on self-control (0.007, column 3).[8] The corresponding 95 percent confidence intervals range from -0.19 to 0.22 (IV) and from -0.12 to 0.13 (reduced form). Thus, the estimates are not overly precise, yet the confidence intervals are clearly centered around zero and we can rule out effects larger than 0.2 of a standard deviation. Overall, these results demonstrate that years of schooling do not seem to significantly increase self-control.

Robustness. We present the results of several robustness checks around both model specification and sample restrictions in the remaining panels of Table 5. In panel (b), we consider additional birth cohorts by including people born between 1934 and 1965. This doubles the window around the first cohort affected by the reforms in South Australia and Victoria. The point estimates of both the second stage and the reduced form remain insignificant and close to zero. In panels (c) and (d) we expand our sample by including individuals from all states. In panel (c), we use a simple binary indicator for individuals being affected by the reform, while in panel (d), we use the minimum school leaving age which varies between 14 and 16 years across

[7] An exception in Germany is the implementation of a so-called 'short school year', harmonizing the school year calendars across the states (Pischke, 2007). In some states, this overlapped with the introduction of the compulsory schooling reform (Cygan-Rehm, 2022). Our results are robust to considering this issue (see Section 4.2). In Australia, some states and territories implemented additional changes to the educational system at the same time as raising the school leaving age, which is why we exclude them. Our results, however, are robust to their inclusion (see Section 4.1).

[8] Given that the reforms instrument is binary, the reduced form allows for a difference-in-differences interpretation with South Australia and Victoria serving as the treatment and New South Wales and the Australian Capital Territory as the control group.

states and time. Both analyses reveal no significant second stage or reduced form effects. In panel (e), we add state-specific linear trends to the set of control variables (see Stephens and Yang, 2014). Instead of using variation in schooling across states over time, adding state-specific linear trends equates to comparing deviations from a linear trend in school attainment across states over time. This reduces our first-stage power, perhaps due to there being little variation between states given the setup of our sample. Finally in panel (f), we follow the approach by de New et al. (2021) and—instead of flexibly controlling for full sets of state and cohort fixed effects—control only for completing school in a treatment state as opposed to a control state, an indicator for being born after the implementation of the reform in the treatment states, an interaction of the two, and linear cohort trends. Again, we find no significant second stage nor reduced form effect. In sum, these sensitivity analyses confirm that our findings for Australia are robust and that there is no evidence for a beneficial effect of years of schooling on self-control.

—Insert Table 5 here—

4.2 Germany

Our 2SLS results are presented in panel (a) of Table 6. Individuals affected by the compulsory schooling reforms have, on average, 0.689 additional years of education (column 1).[9] The first stage F-statistic is 17.318, implying that the German reforms meet the relevance requirement discussed in Section 3 as well. Again, our second stage effect of years of schooling on self-control (column 2) is very close to zero (-0.001) and so is the reduced-form effect (-0.001, column 3), i.e., the direct effect of the reforms on self-control.[10] However, our German estimates are less precise given our small sample size ($n = 415$), such that we cannot rule out potentially sizable effects in either direction. Yet, the point estimates are remarkably close to zero. Overall, these results are consistent with our findings for Australia (see Section 4.1) and confirm that there is no evidence that the additional years of schooling increase self-control.

[9]The reforms should have had no effect on students who attended an intermediate school independent of the reforms (always-takers). Instead, the reforms typically increased years of schooling for basic school students by one year or, in a few cases, by two years if a student decided to attend an intermediate instead of basic school because of the reforms (the compliers of the reforms were potentially among both groups of students).

[10]In the German context, the reduced form allows for a staggered difference-in-differences interpretation, where individuals in states that had not yet introduced the compulsory schooling reform serve as the control group for individuals in states where the ninth grade was already mandatory.

Robustness. Again, we conduct several robustness checks. In panel (b) of Table of 6, we add a control variable for the one-off implementation of 'short school year' reforms that harmonized school year calendars across the states, see Pischke (2007). Our estimates remain very similar to those in our preferred specification. In panel (c), we consider additional birth cohorts by increasing the time window around the first cohort affected by the reforms to ±20 years. This increases the sample size, but has the consequence that the earlier and later cohorts are now more likely to differ in ways other than being affected by the compulsory schooling reforms. The point estimates of the second stage and reduced form remain very close to zero and are insignificant. In panel (d), like in the Australian estimations, we also control for state-specific linear trends. While the second stage and the reduced form estimates increase, they are still small and not statistically significant. Taken together, the results of these robustness checks are in line with those of our main analyses: estimated effects are statistically insignificant and economically small.

—Insert Table 6 here—

5 Conclusions

Our results lead us to conclude that additional time spent in school does not increase self-control. This is consistent with previous evidence that within-individual changes in self-control over time predict subsequent changes in students' grade point averages, but not the reverse (Duckworth et al., 2010). Thus, the well-established relationship between self-control and education does not seem to be the result of (reverse) causality running from education to self-control, but rather that educational achievement is a consequence of self-control.

Of course, the lack of an effect of an additional year of education on self-control does not rule out the possibility that other aspects of the school experience can enhance self-control. Importantly, the nature of the reforms we consider implies that our results reflect the long-term effect of additional schooling for students who were affected by the reforms, i.e., those who would have left school as soon as they got the chance to do so (Imbens and Angrist, 1994). In the German context, students attending basic school made up about three-quarters of the German population attending school (see Table 4) and we cannot definitively rule out the possibility that additional intermediate, academic-track, or higher education enhances the self-control of other students.[11] Similarly, changes to the funding arrangements underpinning Australian universities in the 1980s lead to a dramatic increase in the numbers of students accessing tertiary education (see Chapman and Ryan, 2002) potentially raising levels of self-control. Perez-Arce (2017), for example, finds that university education has a causal effect in increasing patience, pointing to a relationship between education and time preferences more generally. Experimental evaluations of early childhood and school-based programs also demonstrate the potential for targeted interventions to enhance children and adolescents' self-control (Alan et al., 2019; Alan and Ertac, 2018; Breitkopf et al., 2024; Heckman et al., 2024; Piquero et al., 2016; Sorrenti et al., 2024).

Our take-away is that, while enhancing self-control through school-based interventions is feasible, long-term success is likely to rely on specifically tailored curricula and pedagogical choices. Simply increasing the length of time students are required to spend in formal education does not seem to be enough to increase the capacity for self-control.

[11] While basic schools were the most important secondary school track in terms of students, the number of intermediate and academic schools increased in the period under review, see Jürges et al. (2011) and Kamhöfer and Schmitz (2016). Suggestive evidence indicates a reduced-form effect of the number of intermediate or academic schools per 1,000 km^2 in a given state on self-control of 0.000 (standard error 0.022) and 0.017 (standard error 0.039), respectively. This suggests that other margins of schooling may not have a large impact on self-control either. The number of observations in the SOEP-IS data prevents us from exploring this further, however.

Tables

Table 1: Brief Self-Control Scale

	Item
1.	I am good at resisting temptation.
2.	I have a hard time breaking bad habits. (reversed item)
3.	I am lazy. (reversed item)
4.	I say inappropriate things. (reversed item)
5.	I do certain things that are bad for me, if they are fun. (reversed item)
6.	I refuse things that are bad for me.
7.	I wish I had more self-discipline. (reversed item)
8.	People would say I have iron self-discipline.
9.	Pleasure and fun sometimes keep me from getting work done. (reversed item)
10.	I have trouble concentrating. (reversed item)
11.	I am able to work effectively towards long-term goals.
12.	Sometimes, I cannot stop myself from doing something, even if I know it is wrong. (reversed item)
13.	I often act without thinking through all the alternatives. (reversed item)

Notes: The Brief-Self-Control Scale is taken from Tangney et al. (2004). Respondents answer on a scale from 1 (“not at all”) to 5 (“very well”) how well the statements describe how they usually are. Questions marked as “reversed item” enter the final self-control score reversed.

Table 2: Descriptive Statistics

	(1) Mean	(2) Standard deviation	(3) Minimum	(4) Maximum
Australia				
BSCS score (raw)	47.63	7.83	23	65
Years of schooling	10.63	1.30	7	12
Female	0.54	0.50	0	1
Age	67.99	4.42	61	77
Germany				
BSCS score (raw)	46.43	7.14	27	65
Years of schooling	9.08	0.85	8	10
Female	0.57	0.50	0	1
Age	63.82	8.27	47	77

Notes: Descriptive statistics for estimation samples used in main specification for Australia (1,710 obs., HILDA) and Germany (415 obs., SOEP-IS). Raw BSCS score is calculated by adding up responses to all 13 BSCS items (each on a scale from 1 to 5).

Table 3: Australian minimum school leaving age reforms

State or Territory	(1) First effective date	(2) First birth cohort affected	(3) Minimum school leaving age	(4) Included in baseline specification?
Australian Capital Territory[a]	01/01/1943	01/01/1929	15$^{\text{th}}$ birthday	Yes
New South Wales[a]	01/01/1943	01/01/1929	15$^{\text{th}}$ birthday	Yes
Northern Territory	17/12/1965	17/12/1951	15$^{\text{th}}$ birthday	No, small population
Queensland	24/12/1964	24/12/1950	15$^{\text{th}}$ birthday	No, other simultaneous educational changes
South Australia	04/04/1963	04/04/1949	15$^{\text{th}}$ birthday	Yes
Tasmania	01/02/1946	01/02/1932	16$^{\text{th}}$ birthday	No, raise to age 16
Victoria	04/02/1964	04/02/1950	15$^{\text{th}}$ birthday	Yes
Western Australia[a]	01/01/1966	01/01/1952	End of year student turns 15	No, raise beyond age 15 + staggered intro.

Notes: Information is taken from de New et al. (2021) (Table 1); [a] indicates a staggered introduction (see de New et al., 2021, for details). The first column gives the effective date of the raised minimum school leaving age. Column 2 gives the corresponding date of birth of the first cohort affected, calculated from column 1. Column 3 states the new minimum school leaving age. We assign individuals as affected by the compulsory school law change if they completed their highest school degree in the state on the left of this table and were born on the date stated in column 2 or later. For calculating the minimum school leaving age (used in a sensitivity analysis), we take into account the staggered implementation in the Australian Capital Territory, New South Wales, and Western Australia. Column 4 indicates whether the state is included in the baseline specification.

Table 4: German introduction of the compulsory ninth grade for basic schools

State	(1) First graduation year affected	(2) First birth cohort affected	(3) Share of students in basic schools (in %)	(4) Included in baseline specification?
Baden-Württemberg	1967	1953	77.3	Yes
Bavaria	1969	1955	81.1	Yes
Bremen	1958	1943	73.4	No, city state
Hamburg	1949	1934	74.2	No, city state
Hesse	1967	1953	72.4	Yes
Lower Saxony	1962	1947	78.0	Yes
North Rhine-Westphalia	1967	1953	76.9	Yes
Rhineland-Palatinate	1967	1953	82.4	Yes
Saarland	1964	1949	83.1	No, no untreated obs.
Schleswig-Holstein	1956	1941	71.4	No, no untreated obs.

Notes: Own calculations. Information in columns 1 and 2 is taken from the working paper version of Pischke and von Wachter (2008) (Pischke and von Wachter, 2005, Table 1). Column 3 is calculated based on information from the German Federal Statistical Office (1967, p. 92). The first column gives the year in which the ninth grade was introduced. Column 2 gives the approximate corresponding birth cohort (=year of introduction−15 because of nine years of schooling with a school starting age of 6). Column 3 states the share of students in basic schools in 1964. We assign individuals as affected by the compulsory school law change if they currently live in the state on the left of this table and were born in the year stated in column 2 or later. Respondents that report to have lived outside West Germany in 1989 are excluded. Column 4 indicates whether the state is included in the baseline specification.

Table 5: Effect of years of schooling on self-control—Estimates for Australia

	(1)	(2)	(3)	(4)	(5)
	First stage	Second stage	Reduced form		
	Dependent variable:				
	Years of schooling	Self-Control	Self-Control	Number of observations	First-stage F-statistic
a) Main specification					
Coefficient	0.597***	0.012	0.007	1710	27.873
	(0.113)	(0.104)	(0.063)		
b) Including additional birth cohorts (born 1934-1965)					
Coefficient	0.558***	−0.008	−0.004	3212	36.710
	(0.092)	(0.079)	(0.045)		
c) Including all states, using reform dummy as instrument					
Coefficient	0.418***	−0.029	−0.012	2281	18.364
	(0.097)	(0.149)	(0.063)		
d) Including all states, using school leaving age as instrument					
Coefficient	0.395***	−0.055	−0.022	2281	19.252
	(0.090)	(0.154)	(0.061)		
e) Adding state-specific linear trends					
Coefficient	0.222	−0.448	−0.099	1710	0.914
	(0.232)	(0.572)	(0.115)		
f) Following de New et al. (2021)					
Coefficient	0.725***	−0.042	−0.030	1710	56.644
	(0.096)	(0.135)	(0.099)		

Notes: Own calculations based on HILDA (restricted release 21), wave 2019 augmented with information from other years. The first stage gives the effect of the compulsory schooling reforms on years spent in school. The second stage gives the effect of (instrumented) years of schooling on self-control. The reduced form gives the direct effect of the raised minimum school leaving age on self-control (standardized to mean 0 and standard deviation 1). All regressions in this table control for gender. Specifications in panels (a)-(e) control for state and cohort fixed effects and effect of reform dummy (a-c and e) or school leaving age (d) is captured. Specification in panel (f) controls for treatment state (South Australia/Victoria vs. Australian Capital Territory/New South Wales), a post-indicator, an interaction of the two, and linear cohort trends. The first-stage F-statistic refers to the F-statistic of the instrument. State-of-birth, cohort-clustered standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of years of schooling on self-control—Estimates for Germany

	(1)	(2)	(3)	(4)	(5)
	First stage	Second stage	Reduced form		
	Dependent variable:				
	Years of schooling	Self-Control	Self-Control	Number of observations	First-stage F-statistic
a) Main specification					
Coefficient	0.689***	−0.001	−0.001	415	17.318
	(0.165)	(0.529)	(0.383)		
b) Controlling for short school years					
Coefficient	0.613***	0.011	0.007	415	12.102
	(0.176)	(0.590)	(0.381)		
c) Including additional birth cohorts (reform ±20 years)					
Coefficient	0.629***	0.008	0.005	455	15.238
	(0.161)	(0.565)	(0.374)		
d) Adding state-specific linear trends					
Coefficient	0.696***	0.040	0.028	415	15.623
	(0.176)	(0.526)	(0.385)		

Notes: Own calculations based on SOEP-IS, wave 2017. The first stage gives the effect of the compulsory schooling reforms on years spent in school. The second stage gives the effect of (instrumented) years of schooling on self-control. The reduced form gives the direct effect of the introduction of a compulsory ninth grade for basic school students on self-control (standardized to mean 0 and standard deviation 1). The first-stage F-statistic refers to the F-statistic of the instrument. All regressions in this table include full sets of gender, year of birth, and state fixed effects. State-of-birth, cohort-clustered standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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