HCEO WORKING PAPER SERIES

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Sarah C. Dahmann
Silke Anger

Working Paper

2018-065

09/2018

HUMAN CAPITAL AND
ECONOMIC OPPORTUNITY
GLOBAL WORKING GROUP

The University of Chicago
1126 E. 59th Street Box 107
Chicago IL 60637

www.hceconomics.org

Cross-Fertilizing Gains or Crowding Out? Schooling Intensity and Noncognitive Skills*

Sarah C. Dahmann†
The University of Sydney, School of Economics;
ARC Centre of Excellence for Children and Families over the Life Course

Silke Anger
IAB Nuremberg; University of Bamberg; IZA

September 20, 2018

Abstract

This paper studies the impact of schooling intensity on students' noncognitive skills. It exploits a major school reform that reduced total years in high school but retained the high school curriculum, thereby increasing weekly school hours. The sharp, regionally staggered one-year reduction in high school duration allows us to identify causal effects. Our results show that higher schooling intensity decreases overall students' emotional stability but increases openness for disadvantaged students. Our finding that investments in cognitive skills can crowd out noncognitive skills is consistent with the predictions of our theoretical model, which imposes a per-period budget constraint for total investments in skill formation.

Keywords: Skill Formation, Noncognitive Skills, Big Five, Locus of Control, Cognitive Investment, High School Reform
JEL Classification: I21, I28, J24

*We would like to thank Alexandra Avdeenko, Simon Burgess, Colin Cameron, Deborah Cobb-Clark, Flávio Cunha, Thomas Dohmen, Nabanita Datta Gupta, Adrian Hille, Daniel Kamhöfer, Peter Kuhn, Philip Oreopoulos, Pia Pinger, Erik Plug, Mari Rege, David Richter, Stefanie Schurer, Thomas Siedler, and seminar participants at DIW Berlin, Maastricht University, NIW Hannover, University of Hannover, University of Konstanz, University of Nuremberg, Technical University of Darmstadt, and the Berlin Network of Labor Market Research for valuable discussions and suggestions. We also thank conference participants at the AMCIS Conference "Education Systems", the Spring Meeting of Young Economists, the Annual Conference of the Scottish Economic Society, the RWI Research Network Conference, the Annual Conference of the European Society for Population Economics, the Annual Conference of the German Economic Association, the International Workshop of Applied Economics of Education, the Annual Conference of the European Association of Labour Economists, the Essen Health Conference, the HCEO Summer School on Socioeconomic Inequality, and the HCEO Faculty Collaborative Seminar for helpful comments. Sarah Dahmann gratefully acknowledges funding from the German Academic Scholarship Foundation. A previous version of the paper was circulated under the title "The Impact of Education on Personality – Evidence from a German High School Reform".

†Correspondence to: Sarah Dahmann; School of Economics, The University of Sydney, New South Wales 2006, Australia; ✉: sarah.dahmann@sydney.edu.au.

1 Introduction

A vast literature has documented the importance of noncognitive skills—such as personality traits, preferences, and attitudes—for individual economic and social success.[1] As for the determinants of noncognitive skills, the literature has identified two main channels that shape these skills: *nature* (genetic inheritance) and *nurture*, which encompasses all environmental factors (e.g. culture, family background, and situational factors). Cunha and Heckman (2007) formalize a skill formation model that includes both channels and that emphasizes the role of critical and sensitive periods for optimal human capital investments. Two features of their model—*self-productivity* and *dynamic complementarity*—stress the importance of skill development early in life due to their multiplier effects.[2]

Human capital investments into noncognitive skills are thus most productive before adulthood and can be targeted (or not) at a specific skill. Empirical studies have documented the positive effects of targeted investments on noncognitive skills via intervention programs (for preschool and elementary school children, see, e.g., Schweinhart et al. 2005; Heckman et al. 2013; Bierman et al. 2010; Alan and Ertac 2018; Kosse et al. 2018; for adolescents, see, e.g., Oreopoulos et al. 2017; Martins 2010). Other studies have documented the noncognitive benefits of non-targeted investments, such as education in general (Heckman et al. 2006; Li and Powdthavee 2014; Kassenboehmer et al. 2018). However, only few studies on noncognitive skill formation differentiate between specific types of investments, e.g., through time allocated to particular activities (see, e.g., Fiorini and Keane 2014).

Yet human capital investments targeted exclusively at other skills can also be productive because the skill formation model implies that investments in one skill type are beneficial for other skill types through cross-productivity (*cross-fertilizing gains*). While Cunha and Heckman (2008), modeling cross-productivity, find that noncognitive skills promote the development of cognitive skills, no study has investigated the specificity of investments in different skill types in the context of cross-productivity. Potential cross-effects may arise not only from cross-fertilizing gains between cognitive and noncognitive skills but also from crowding out effects of skill-specific investments if total investments are limited.

This study is the first to analyze the cross effects of cognitive skill investments on noncognitive skills, both theoretically and empirically. We extend the Cunha and Heckman (2007) skill formation model by imposing a per-period budget constraint for total skill investments, and empirically investigate the impact of schooling intensity on students' noncognitive skills, exploiting a major school reform as a quasi-natural experiment. The reform retained the high school curriculum, while sharply shortening high school duration in most of Germany's federal states between 2001 and 2007. Consequently, weekly school hours increased by three hours on average (about 10 percent). Thus, unlike studies on education as a general investment, we can use the reform design of keeping earlier curriculum standards to narrow the treatment to cognitive investment.[3] Representative data covering adolescents who entered high school between 1993 and 2009 allows us to provide a detailed picture of the impact of cognitive investments on personality traits—essential skills that may benefit from higher schooling intensity through cross-fertilizing gains or suffer through crowding out from the reform.

The German high school reform offers an excellent setting for estimating the causal effect of

[1] For a detailed overview, see Almlund et al. (2011).

[2] The implication of their model—that investments are most productive in early stages in life—is consistent with the psychology literature, according to which personality traits develop mainly during childhood and adolescence, and remain relatively stable later in life (Costa and McCrae 1994).

[3] For a detailed description of the high school reform and the German education system in general, see Section 3.

cognitive skill investments on students' noncognitive skills. The nationwide educational reform shortened academic-track high school by one year for entering cohorts of high school students in a staggered manner, with different federal states adopting the new setting in different years. While sharply shortening high school duration, the reform did not change either the overall educational content or the total hours for obtaining a high school degree. Students, therefore, obtained their high school degree after 12 years, instead of 13, with a corresponding increase in weekly school hours by about 10 percent.

The purpose of our study is to assess whether this increase in schooling intensity affects adolescents' personality traits, measured by both the dimensions of the Big Five personality inventory (openness to experience, conscientiousness, agreeableness, extraversion, emotional stability) and locus of control. In addition, we examine heterogeneous effects by analyzing which students exhibit larger changes in personality following the increased schooling intensity. Moreover, by disentangling various potential channels of impact, we also investigate the underlying mechanisms of cross-fertilizing gains or crowding out effects.

We apply a generalized difference-in-differences framework to test whether students who studied under the higher schooling intensity show personality traits different from those of students who studied under lower schooling intensity. To derive hypotheses, we extend the Cunha and Heckman (2007) skill formation model by imposing a per-period budget constraint for total, i.e. cognitive and noncognitive, investments in skill formation. This framework allows us to study the potential positive and negative externalities of an investment that has been proven productive for cognitive skills (e.g., Lavy 2015, shows that instruction time improves test scores worldwide), but been neglected in analyses of the formation of noncognitive skills.

We find that the increase in schooling intensity following the shortening of high school duration leads to considerably lower emotional stability among students. We attribute this finding to students' lack of recreational time, adverse health conditions, lower school performance, and negative emotions after the change. Yet, we find no average effects for the other Big Five personality traits or for locus of control.

Moreover, our estimates point to important heterogeneous effects: The decrease in emotional stability is more pronounced among students in the former East German states. As these states had a shorter high school duration for many decades before the 1990 reunification, they were more experienced with a higher schooling intensity. However, as they implemented the new reform very quickly, we attribute the adverse effects in these states to this ad hoc implementation.

In contrast, positive effects appear for disadvantaged students from either non-intact or immigrant families. These students benefit substantially in terms of openness and extraversion, and openness and conscientiousness, respectively. While students report having time to daydream or do nothing significantly less often, the increase in schooling intensity does not affect more formal leisure activities: Neither the percentage of students who participate in sports or play a musical instrument nor the frequency of these activities changes with the intensified schooling. These findings imply that the positive effects of higher schooling intensity on the personality traits of disadvantaged students are driven by the stronger student-teacher and student-student interactions resulting from the longer school day.

Our findings are robust to a number of specification checks: considering age effects, correcting for measurement error, addressing multiple hypotheses testing, and controlling for both selection and for timing of the reform. These results provide evidence indicating that the estimated effects of the higher schooling intensity are not due to unobserved factors coinciding with the change or to general individual-specific factors, measurement error, or selectivity.

Our study shows that educational investments in cognitive skills play an important role

in shaping adolescents' personality traits. Whereas cognitive skill investments lead to cross-fertilizing gains of personality traits for disadvantaged students, they produce crowding out of noncognitive skills, where the reform was implemented ad hoc. These two findings imply that, to reach optimal skill development, policymakers have to carefully adjust investments in cognitive and noncognitive skills if total investments are limited.

This study is the first to explicitly address the potential crowding out effects of educational investments. Although distinguishing between cognitive and noncognitive investments is crucial for measuring cross effects, most studies on the effects of human capital investments on cognitive and noncognitive skills do not separate investments into specific skill types.[4] This paper also contributes to the small but rapidly growing literature on the effects of educational investments on the development of noncognitive skills. Most studies examine (early) childhood investments, to the neglect of investigating the malleability of personality traits in adolescence.[5] This study therefore extends the literature in the following three ways.

First, by using a school reform to document the effects of schooling intensity on noncognitive skills, we are able to isolate the investment into a specific skill type, cognitive skills. Doing so allows us to examine whether crowding out effects or cross-fertilizing gains of educational investments predominate. While the effects of these educational investments on cognitive skills are well documented (Huebener et al. 2017; Andrietti 2015; Dahmann 2017), the cross effects on noncognitive skills have not been previously investigated. However, if constraints such as limited capacity of skill formation or (time or monetary) budget restrictions in investment activities entail unintended crowding out effects, examining the effects of cognitive investment on other skill types is critical. By examining the malleability of personality in adolescence, we also complement previous studies on skill formation that focus primarily on young children. Although Cunha and Heckman (2008) demonstrate a stronger multiplier effect of early childhood investments, their empirical results reveal that the critical period of investment lasts longer in life for noncognitive skills than for cognitive skills.[6] Evidence on whether personality is malleable even in adolescence is crucial for policymakers who may want to target individuals who are too old for childhood intervention programs.

Second, we use representative data and, to examine the causal effect of schooling intensity on students' personalities, exploit a nationwide reform in the educational system as a quasi-natural experiment. While most studies on the impact of human capital investments on the malleability of personality traits in young children are small-scale, our treatment is large-scale and not locally restricted. Indeed, it affects students from almost an entire country. Analyzing reform effects per se is critical, as educational policymakers need to know the second-order effects of such far-reaching school reforms. Moreover, as the high school reform was imple-

[4] A noteworthy exception is Thiel et al. (2014), who do not find economically meaningful effects of the same high school reform on the personality traits of double cohort students in Saxony-Anhalt, an Eastern German federal state with a very special situation (it had extended high school shortly before the reform). The study considers the double cohort of graduates—the last cohort graduating from high school with 13 years of schooling and the first cohort graduating after 12 years—a cohort that undoubtedly features peculiarities that could lead to confounding effects, such as the increased competition for jobs and college places.

[5] Exceptions include Martins (2010) who shows that a program targeted at adolescents in Portugal improved student achievement by increasing motivation, self-esteem, and study skills. Moreover, the National Guard Youth ChalleNGe program in the U.S. increased discipline and emotional stability among high school dropouts (Bloom et al. 2009). For Australia, Li and Powdthavee (2014) find that an increase in compulsory school years raises individuals' conscientiousness and internal locus of control, and Kassenboehmer et al. (2018) show that tertiary education still plays a role in shaping noncognitive skills of young adults.

[6] Heckman et al.'s (2006) findings, based on the National Longitudinal Survey of Youth 1979 (NLSY79), show that locus of control is affected primarily by high school, not college, attendance and point towards a potential point of exhaustion of malleability in late adolescence.

mented nationwide but only gradually introduced into Germany's federal states, we can exploit this variation over time and across states. We thus are able to isolate the causal effect of schooling intensity from any other potential influential factors or policy changes. In addition, to reduce the risk that potential unobserved effects bias our estimates, we include students who had graduated several years both before and after the change, thereby enabling us to establish a long-lasting effect of schooling intensity rather than merely an artifact of the reform's implementation.

Third, our data provides rich information on individuals' socioeconomic backgrounds, allowing us to examine whether the personalities of particular groups of students are more malleable than those of others. Initial endowments of students, related to their individual and family background, may affect their ability to cope with the intensified schooling. Consequently, different students may react differently to the higher schooling intensity in terms of both the type of personality traits affected and the extent to which those personality traits change. Previous studies have shown that socioeconomic status is an important predictor of noncognitive skills, such as economic preferences (Deckers et al. 2017). Therefore, to capture the students' ability to cope with reform-induced changes, we include pre-reform characteristics of students and their families such as immigration background, parental education, and occupational status.

The paper proceeds as follows: Section 2 presents the theoretical framework of skill formation. Section 3 explains the reform and discusses potential effects on adolescents' personality traits. Section 4 describes the data and the empirical strategy, and Section 5 presents the results. Section 6 elaborates on several robustness checks. Section 7 concludes and discusses the implications of our findings.

2 Theoretical Framework

We develop our theoretical framework from the Cunha and Heckman (2007) model of skill formation, a model widely used for explaining the importance of early human capital investments. Previous literature has extended the original framework by, for example, including effort (Almlund et al. 2011; Thiel et al. 2014) and applying it to the prenatal context (Nilsson 2017).

2.1 Skill Formation Model (Cunha and Heckman 2007)

Personality traits are a significant part of an individual's noncognitive skills.[7] Both cognitive abilities and noncognitive skills constitute personal skills, which belong to an individual's overall human capital. In their model, Cunha and Heckman (2007) propose that an individual's skill stock depends on his or her past skill stock, previous investments, and parental characteristics, according to the following model:

$$\theta_{t+1} = f_t(\theta_t, I_t, h), \tag{1}$$

where a vector of skill stocks at age $t+1$, θ_{t+1}, depends in some functional form $f(\cdot)$ on the past vector of skill stocks (with initial endowment θ_1), on the investment in period t, I_t, and on parental characteristics h.

In this model, Cunha and Heckman (2007, 2008) propose and empirically verify a multiplier effect driven by two mechanisms. The first is *self-productivity* and *cross-productivity*: Skills persist such that higher skills in one period create higher skills in the subsequent period,

[7] Examples of other noncognitive skills are trust, time, and risk preferences (see, e.g., Almlund et al. 2011).

also across domains ($\partial f_t(\theta_t, I_t, h)/\partial\theta_t > 0$). The second is *dynamic complementarity*: The productivity of an investment increases with higher existing skills ($\partial^2 f_t(\theta_t, I_t, h)/\partial\theta_t\partial I_t' > 0$).

To distinguish between different skill types, Cunha and Heckman (2007) formulate equation (1) separately for cognitive and noncognitive skills, explicitly accounting for cross-productivity between the two:

$$\theta_{t+1}^k = f_t^k(\theta_t^C, \theta_t^N, I_t^k, h^C, h^N) \text{ for } k \in \{C, N\}, \tag{2}$$

where C denotes cognitive and N denotes noncognitive skills. Equation (2) illustrates cross-fertilizing gains between cognitive and noncognitive skills, as θ_{t+1}^k is a positive function of both types of existing skills. Even though any investment in cognitive skills (I_t^C) is not included in equation (2) for θ_{t+1}^N (and thus has no direct impact on noncognitive skills in the next period), it benefits noncognitive skills in period $t+2$ through the gains in cognitive skills in period $t+1$; thus, $\frac{\partial\theta_{t+2}^N}{\partial I_t^C} = \frac{\partial f_{t+1}^N}{\partial\theta_{t+1}^C} * \frac{\partial f_t^C}{\partial I_t^C} \geq 0$ (see Appendix for details). As skills are self- and cross-productive and increased cognitive and noncognitive skills 'beget' themselves and each other, this potential gain carries over to all later periods.[8] Therefore, under the standard assumptions, the Cunha and Heckman (2007) skill formation model implies that any cognitive investment benefits noncognitive skills or, at worst, if it fails to increase cognitive skills or if cross-productivity is low, has no impact. Thus, under these assumptions, cognitive investment can never be harmful for noncognitive skills.

2.2 Skill Formation with Investment Constraints

We build on the same framework, notation, and assumptions as in Cunha and Heckman (2007) but introduce a budget constraint on investments, suggesting that total investments (I_t) in one period are bounded. While Cunha and Heckman (2007) consider investment constraints only across periods of the life cycle, we extend their skill formation model by introducing restricted investments in each period. This extension more accurately reflects the real world, given that important inputs into the skill production function, such as monetary or time resources, are usually scarce, with credits often constrained.[9] As Cunha and Heckman (2007) point to evidence that credit constraints affect skill formation mostly during early childhood, we focus on the restriction of time investments.[10]

As the time available for investment is limited in each period, we have total investments $I_t^C + I_t^N = I_t$.[11] This budget constraint is determined, for example, by the maximum of 24 hours a day or by the number of weeks during one academic year. The constraint yields $I_t^N = I_t - I_t^C$, such that the cognitive investment (I_t^C) has an immediate negative impact on

[8] One channel is dynamic complementarity, which is included in the marginal effect of (non)cognitive skills on future (non)cognitive skills: Altering the level of skills changes the productivity of future investment, even if the level of future investment remains constant, because $\partial^2 f_t(\theta_t, I_t, h)/\partial\theta_t\partial I_t' > 0$.

[9] Alternatively, one may also consider of a constraint in terms of limited cognitive capacity. Even if the storage capacity for long-term memories is infinite, the capacity for working memories (from which the information is transferred to the long-term memories) is not, as "the ability to simultaneously process and maintain multiple pieces of information is limited" (Turi et al. 2018).

[10] Moreover, as monetary investments are mainly borne by the government, not by individuals or their families, time is the more relevant investment constraint in this paper. The literature also considers time constraints within periods, for example, in modeling parental investment in their children's human development (Cobb-Clark et al. 2018).

[11] Some investments may simultaneously foster cognitive and noncognitive skills (I_t^{C+N}), such that $\theta_{t+1}^k = f_t^k(\theta_t^C, \theta_t^N, I_t^k + I_t^{C+N}, h^C, h^N)$ for $k \in \{C, N\}$. In such a case, the budget constraint changes to $I_t^C + I_t^N + I_t^{C+N} = I_t$ and the following derivations can easily be adapted. However, as our focus is on an increase in purely cognitive investment, we assume (without loss of generality) I_t^{C+N} being constant, and thus not altering our derivations. For simplicity of illustration, we therefore do not include this term.

the development of noncognitive skills in period $t+1$ through the reduction it imposes on noncognitive investment (I_t^N), i.e. $\frac{\partial \theta_{t+1}^N}{\partial I_t^C} = -\frac{\partial f_t^N}{\partial I_t^N} \leq 0$. This adverse effect increases with a higher productivity of (foregone) noncognitive skill investments.

For noncognitive skills in period $t+2$, the effect of the cognitive investment is ambiguous:

$$\frac{\partial \theta_{t+2}^N}{\partial I_t^C} = \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^C} * \frac{\partial f_t^C}{\partial I_t^C} - \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^N} * \frac{\partial f_t^N}{\partial I_t^N} \begin{cases} > 0 & \text{if } \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^C} * \frac{\partial f_t^C}{\partial I_t^C} > \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^N} * \frac{\partial f_t^N}{\partial I_t^N} \\ = 0 & \text{if } \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^C} * \frac{\partial f_t^C}{\partial I_t^C} = \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^N} * \frac{\partial f_t^N}{\partial I_t^N} \\ < 0 & \text{if } \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^C} * \frac{\partial f_t^C}{\partial I_t^C} < \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^N} * \frac{\partial f_t^N}{\partial I_t^N}. \end{cases}$$

The left-hand side in the inequality conditions denotes the cross-fertilizing gains of the cognitive investment through the improvement in cognitive skills, while the right-hand side denotes the losses arising from the crowding out of the noncognitive investment. Whichever part predominates determines whether an increase in cognitive investment in period t is beneficial or detrimental for noncognitive skills in period $t+2$. Because skills are self- and cross-productive, this trade-off continues in all later periods (see Appendix for details). Thus, as an extension of the Cunha and Heckman (2007) model, we suggest that, as soon as total investment is constrained, cognitive investment *can* harm subsequent noncognitive skills, and we test this hypothesis empirically.[12] The lower (higher) the cross-productivity (self-productivity) and the lower (higher) the productivity of cognitive (noncognitive) skill investments, the more likely an adverse effect.

3 The German High School Reform

3.1 Institutional Background

In Germany, educational policy is the responsibility of the federal states (*Bundesländer*). In every state, children start elementary school at age six and usually continue to secondary school at age ten.[13] The German educational system comprises three basic types of secondary school: the lower (*Hauptschule*), the intermediate, (*Realschule*), and the higher track (*Gymnasium*). Only successful completion of *Gymnasium* (hereafter, "high school") leads to university entrance qualification (*Abitur*). Some states also have comprehensive schools, which combine all three secondary school types.

With one exception, high school in Germany lasted nine years in almost all federal states until 2001,[14] with a total of 13 years of schooling needed for a high school diploma.[15] This relatively long university preparatory track, compared to most Western countries, was considered a disadvantage for German graduates, who entered the labor market relatively late. By reducing overall schooling from 13 to 12 years, the reform gave high school graduates earlier

[12] We do not solve for the optimal allocation between cognitive and noncognitive investments, but instead analyze the impact of an increase in cognitive investment on noncognitive skills—keeping all else equal.

[13] Exceptions are the states of Berlin, Brandenburg, and Mecklenburg-West Pomerania, where elementary school is six, not four, years.

[14] In 1966 and 1967, the academic year was shortened to move its start date to the middle of the calendar year: The total time in school was reduced by two-thirds of a year for students then enrolled in school, while the basic curriculum was left unchanged.

[15] In the former East Germany, students finished secondary school after 12 years. Following the 1990 reunification, Brandenburg, Mecklenburg-West Pomerania, and Saxony-Anhalt adapted to West German standards increasing total years of schooling to 13 in the 1990s. Only Saxony and Thuringia had (and still have) 12 years of schooling to obtain a high school diploma.

labor market entry, which was attractive for three reasons.[16]

First, the reform aimed at enabling German high school graduates to be more competitive internationally. Second, by shortening high school duration, the reform intended lowering the costs per student and increasing efficiency of the German education system. Third, the reform tackled the challenges of demographic changes common to many Western countries. By expanding the labor force by one birth cohort, the reform aimed not only at reducing the shortage of skilled workers but also at solving the problem of an increasing disparity between a reduced group of young workers contributing to the pension plan and an aging population receiving pension benefits. Nonetheless, opponents of the reform feared that shortening the high school track would harm the quality of education.

The German high school reform was introduced in most of the federal states between 2001 and 2007.[17] Exceptions are Rhineland-Palatinate (where, to date, high school duration was reduced only in selected schools), Saxony, and Thuringia. The educational reform was first implemented in Saarland in the 2001/2002 school year. As the reform was mandatory for all high schools within a state, all students were involved.[18] Table 1 shows a detailed overview of the introduction of the reform and the first cohorts being affected, broken down by federal state.

Although the reform reduced the total duration of schooling, it led students to achieve earlier standards and did *not* reduce the overall curriculum. The total number of hours required for graduation between grade five and graduation remained the same: week hours per year needed to sum up to 265 over all years (KMK 2013). While individual schools could decide how to distribute the total school hours over the remaining grades, most of them allocated the increase in hours to grades seven to nine (student ages 13-16). As a result, students spent on average more than three hours more per week in school than earlier (33 hours compared to less than 30).[19] This increase in workload per period of about 10 percent on average led to a higher weekly schooling intensity,[20] including longer school days.[21] The reform thus corresponds to an exogenous increase in cognitive investment, which, according to the theoretical framework outlined in Section 2, can either benefit or harm students' personality traits.[22]

[16] In the states of Berlin, Brandenburg, and Mecklenburg-West Pomerania, where tracking takes place in grade seven, the reform reduced high school from seven to six years.

[17] Starting in 2015, some federal states returned to 13 years of schooling, and others intend to do so in reaction to political pressure, mainly from parental organizations. Currently, schools in some states can choose (by parental vote) whether to return to 13 years of schooling. Either way, this possibility is very new and therefore does not affect our sample of students (Section 4).

[18] See Section 6.5 for a discussion on non-compliance with the reform.

[19] An increase in school hours is accompanied by an increase in homework. Therefore, the true increase in workload per week is even higher than the pure increase in school hours.

[20] Some states minimally reduced the curriculum content by diminishing the school hours of elective courses. Thus the schooling intensity did not increase as much as it could have (12.5 percent), and we estimate lower bound effects of the reform.

[21] At the same time, there was a substantial increase in all-day high schools. By 2010, almost half of all high schools had become all-day schools, compared to only about 12 percent in 2002 (Autorengruppe Bildungsberichterstattung 2012). In addition to the compulsory curriculum, most of these all-day schools offer other, voluntary activities, as do more traditional schools to a lesser extent.

[22] In our context, we use the term "investment" for describing inputs in the skill production function (as is common in the literature), not for the national education budget. Nonetheless, the investment we refer to is allocated by the policymaker, not (as in Cunha and Heckman 2007), by parents.

TABLE 1
INTRODUCTION OF THE REFORM BY STATE

State	Implementation of the reform	High school entry of first cohort affected	Graduation of first cohort affected
Saxony	—	—	—
Thuringia	—	—	—
Saarland	2001	2001	2009
Hamburg	2002	2002	2010
Saxony-Anhalt	2003	1999	2007
Mecklenburg-West Pomerania	2004	2002	2008
Bavaria	2004	2003	2011
Lower Saxony	2004	2003	2011
Baden-Wuerttemberg	2004	2004	2012
Bremen	2004	2004	2012
Hesse[1]	2004	2004-2006	2012-2014
North Rhine-Westphalia	2005	2005	2013
Berlin	2006	2006	2012
Brandenburg	2006	2006	2012
Schleswig-Holstein	2007	2008	2016
Rhineland-Palatinate	2007	—	—

SOURCE.—Autorengruppe Bildungsberichterstattung (2010)
[1]Gradual introduction: school year 2004/05 (10 percent of all schools); 2005/06 (60 percent); 2006/07 (30 percent).

3.2 Anticipated Effects of the Increased Schooling Intensity on Personality

The reform-induced increase in schooling intensity through a greater number of hours per school year might have had various unintended consequences for students, such as an increase in grade repetition (Huebener and Marcus 2017) or a decrease in mental health, due to higher stress (Quis 2018). Similar changes in educational policy took place in other countries. For example, Krashinsky (2014) finds for students in Ontario, Canada, after a similar reform in 1999, that students with one year less high school receive significantly lower grades in college. However, the Ontario change effectively reduced both the curriculum and the number of courses available.

For the German 1966 and 1967 changes, Pischke (2007) finds no adverse effects of shorter school duration on earnings or employment later in life, concluding that the more recent high school reform (i.e., the one we use in this study) may not compromise the labor market success of the affected students. However, Pischke also finds that the shorter school years increase grade repetition in elementary schools and lead to fewer students attending higher secondary school tracks, thereby pointing to important heterogeneous effects: The most poorly performing students appear less likely to keep up with the increased schooling intensity. Furthermore, Pischke (2007) notes that the shorter school duration may induce costs by shifting students' time away from activities not directly linked to labor market outcomes (e.g., artistic activities or taking part in elections).

As the recent increase in schooling intensity represented a much greater change than the shortened academic years of the 1960s, the students' noncognitive skills affected may include

their personalities. In this study, we focus on the personality concept of the five factor model (McCrae and Costa 1999), which comprises five psychological dimensions also known as the *Big Five*: openness to experience, conscientiousness, extraversion, agreeableness, and emotional stability. "Openness to experience" entails an individual's creativity and imagination, and "conscientiousness" describes the propensity to work effectively, efficiently, and thoroughly. "Extraversion" is an individual's tendency to be outgoing, sociable, and communicative. "Agreeableness" entails being polite, forgiving, and kind, and "emotional stability" refers to whether an individual tends to cope well with stress and is generally relaxed, rather than prone to worry. In addition to the Big Five, we include the concept of "locus of control", which is an individual's perception of control over his or her life (Rotter 1966).

Each of these personality traits may be affected differently by the reform-induced increase in schooling intensity. As this increase was targeted primarily at cognitive skill development, it should have reduced school time that could otherwise be devoted to activities fostering socio-emotional skills within the classroom. In addition, the longer school days reduced students' time for after-school activities. Therefore, in line with the time budget constraint we impose on total investment in Section 2.2, we expect that the shift in investments induced by the higher schooling intensity affects personality traits in general. However, the direction and degree to which traits are affected depend on the type of activities that are crowded out and on the relationship, in particular the cross-productivity, between cognitive skills and the affected personality traits.

As the sensitive investment periods come earlier for cognitive skills and later for noncognitive ones (Cunha and Heckman 2008), the change affecting students only in adolescence may be more relevant for the development of noncognitive skills than for that of cognitive skills. Nonetheless, studies show that this particular reform increased cognitive skills and academic achievement among 15- and 17-year-olds respectively, at least for some groups of students (Dahmann 2017; Huebener et al. 2017; Andrietti 2015). Therefore, in line with the Cunha and Heckman (2007) skill formation model, we expect that the resulting improvement in cognitive skills extends to personality traits through cross-productivity—the extent of which may differ between traits.

However, while Cunha and Heckman (2008) find that noncognitive skills promote the development of cognitive skills, they find in most of their specifications that cognitive skills do not promote noncognitive skills. Thus we expect the reduction in noncognitive investment to predominate and thus deteriorate the traits that Cunha and Heckman (2008) studied, traits closely related to emotional stability and, to some extent, agreeableness.[23] The extent to which relevant noncognitive investment is crowded out depends on the type of activities that are altered and on the specific trait: While we expect that personality traits related to social behavior, such as extraversion and agreeableness, are primarily affected by joint activities involving interactions with others, emotional stability may depend more on individual emotional experiences.

In contrast, openness to experience and conscientiousness are the largest correlates of cognitive skills and academic achievement among the Big Five (Borghans et al. 2008; Almlund et al. 2011). Thus, given cross-fertilizing gains, we expect them being largest for these two traits. Nevertheless, whether these gains can compensate for the reduction in noncognitive investment—and thus which personality traits benefit or deteriorate with increased schooling intensity—remains an empirical question.

[23]Their noncognitive skill measure encompasses the following five dimensions: antisocial, anxious/depressed, headstrong, hyperactive, and peer problems (Cunha and Heckman 2008). According to Almlund et al. (2011), all five can be classified particularly under the Big Five dimensions of neuroticism (describing the reverse of emotional stability) and agreeableness.

4 Data and Methods

4.1 Sample of Adolescents and Young Adults

Our analysis is based on data from the German Socio-Economic Panel (SOEP) study, a representative household panel survey (Wagner et al. 2007) with about 30,000 individuals in almost 15,000 households in the 2013 wave.[24] In addition to rich information on family background and childhood environment characteristics, since 2005 the SOEP has provided self-ratings of personality traits for 17-year-old adolescents and adult respondents aged 18 and up. We use data from 2005 through 2015,[25] selecting all adolescents and young adult respondents (up to age 21) who are in high school at the time of the survey or had earned a high school diploma.[26]

To identify whether individuals are subject to higher schooling intensity, we determine whether they attend school before or after the reform, according to their year of school entry and state of residence or—in cases of school completion—the state where the high school diploma was obtained. The year of school entry, if missing, is imputed from the date of birth.[27] We assign students from Saxony and Thuringia as subject to higher schooling intensity, despite no educational change, because they kept their eight-year high school track after reunification.

We exclude students from Rhineland-Palatinate, where, to date, the reform has been introduced only in selected schools. In Hesse, there were double graduating cohorts in three consecutive years: 2012, 2013, and 2014. We therefore only include Hesse students who are not affected by the reform and who graduated in 2012 or earlier, and students who graduated in 2014 or later and thus are affected by the reform. To avoid noise from different schooling levels and intensities, we also exclude individuals who repeated one or more grades.[28] Finally, we include only individuals who successfully answered the items from all Big Five dimensions and provided valid information on family background and home environment.[29] The final sample consists of 1,467 individuals, 589 of whom are subject to higher schooling intensity.[30]

4.2 Outcome Measures and Background Variables

The SOEP provides self-ratings of personality traits in various waves since 2005 (Gerlitz and Schupp 2005). These cover personality measures related to the five factor model (McCrae and Costa 1999), which comprises the dimensions of the Big Five (openness to experience, con-

[24] This paper uses data from the Socio-Economic Panel (SOEP), data for 1984-2015, version 32, SOEP, 2016, doi:10.5684/soep.v32.

[25] We exclude data sampled as part of "Families in Germany" (FiD) or as part of the newest immigration samples because low-income single-parent households and immigrants are oversampled in these data.

[26] Students in the academic track at a comprehensive school are also affected by the reform. However, we exclude them because we cannot unambiguously identify which tracks they have been in after entering secondary school. Moreover, identifying these students' exposure to the reform would be difficult, as some states excepted comprehensive schools from the reform.

[27] The year of school entry is available for 40 percent of the full sample. For these students, the imputed year matches the actual year in 82.7 percent of all cases, and the assignment to treatment or control group is correct in 98.6 percent of all cases.

[28] Huebener and Marcus (2017) show that, following the German high school reform, class repetitions do not increase up to grade nine. Thus the composition of our sample of 17-year-olds (in grade 10 or 11) should be largely unaffected by the probability of grade repetition. We drop 84 students from our sample for repeating one grade (28 from the control group and 56 from the treatment group).

[29] These constitute more than 97 percent of all individuals in our sample of consideration, as only 1.2 percent did not report on all Big Five dimensions, 1.7 percent did not report on the background variables, and 0.1 percent did not report on either.

[30] The average size of a birth cohort surveyed at age 17 is 251 in the years under observation; on average 42 percent of a cohort are in the high school track.

scientiousness, agreeableness, extraversion, emotional stability). We measure each dimension with three items on a seven-point Likert-type scale.[31] Moreover, following the approach of Specht et al. (2013), we construct a measure for internal locus of control from seven items.

To construct measures for each personality trait, we average the items for each dimension and standardize them separately by gender at mean zero and variance one. While for adolescents these measures are included in the annual SOEP youth questionnaire from 2006 to 2015, adult respondents' Big Five traits were surveyed only in waves 2005, 2009, and 2013, and their locus of control only in waves 2005 and 2010. If an individual's personality was measured twice, we use the personality measures at the youngest possible age (17-21 years) to guarantee a largely homogeneous sample and to avoid biased estimates through possible age effects.

To account for potential age effects, we include age and age squared at the point of measurement in all estimations. We also control for several pre-reform individual characteristics in some specifications, including demographic and socioeconomic variables (e.g., gender, immigration background, whether the student lived primarily in a rural area during childhood). We incorporate students' previous educational performance, measured by whether the student's elementary school teacher explicitly recommended only the lower or intermediate track. We capture non-intact family circumstances by whether students lived with only one parent until age 15.

The model also accounts for parental characteristics such as education, father's occupational status, and mother's employment status when the student was ten years old.[32] For a description of all variables, see Table A.1 in the Appendix. The summary statistics of the personality traits and the individual characteristics appear in Tables A.4 and A.5 in the Appendix.

4.3 Empirical Strategy and Treatment

To measure a causal effect of schooling intensity on personality, we exploit the German high school reform introduced in almost all federal states from 2001 to 2007 as a quasi-natural experiment. We define the control group as students who entered high school before the reform and graduate after nine years of high school, i.e., facing a lower schooling intensity. In contrast, our treatment group comprises students who entered high school after the reform and graduate after only eight years of high school, i.e., facing a higher schooling intensity. Individuals are assigned to the treatment or control group according to their year of school entry and state of residence.[33] To isolate the effect of schooling intensity from other influential factors, we exploit the variation over time and across regions.

Thus we estimate the following model:

$$y_{ist,17} = \alpha \text{REFORM}_{st} + X_i\beta + \gamma_s + \delta_s * t + \tau_t + \varepsilon_{ist}, \tag{3}$$

where $y_{ist,17}$ is the personality measure at age 17 (or slightly older) of person i in state s who has entered high school in year t. Our prime variable of interest, $REFORM_{st}$, equals 1 if in state s students entering high school in year t are affected by the reform, and 0 otherwise. A set of fixed effects accounts for differences between states (γ_s) and between years of high school

[31] For details on all personality items, see Tables A.2 and A.3 in the Appendix.

[32] In an additional estimation, we include parental personality traits. Unfortunately, for most of the sample, parental personality is not observed pre-reform and therefore may entail spillover effects from children to parents. We thus exclude parental personality traits from our preferred specification. Nonetheless, our estimates are robust to including them.

[33] Individuals who already graduated from high school are assigned to the treatment or control group by the state where they graduated.

entry (τ_t), and state-specific linear trends in these years ($\delta_s * t$). X_i is a vector of individual characteristics, including age and age squared, gender, and a number of pre-reform characteristics that may be correlated with personality. These are immigration background, previous educational performance, father's occupational status, parental education, childhood environment, a dummy for non-intact family, and mother's employment status when the individual was ten years old.

We also include indicator dummies for the different SOEP sub-samples, and we cluster error terms at the state level.[34] When estimating equation (3), our main parameter of interest is α, which indicates the impact of the increased schooling intensity on each personality trait. Our analysis thus resembles an extended difference-in-differences setting, in which the key identifying assumption is that, in the absence of a reform, α will be zero, that is, the treated ($REFORM_{st} = 1$) and the non-treated ($REFORM_{st} = 0$) do not differ significantly in terms of their personality traits.

Next, we describe the exact treatment that we use in our analysis. Although the treatment group experiences a shorter high school track than that of the control group, we do not measure the effect of a decrease in years of overall school duration per se. The reason is that most of our sample was surveyed the year they turned 17, when still in high school. Instead, we focus on the increased schooling intensity and the way it shapes personality traits during adolescence. Thus especially between ages 13 and 16, the treatment implied by the reform is a higher workload in school, longer school days, and a higher level of learning achievement by age 17, all resulting from more cumulative hours of school.

4.4 Timing of the High School Reform and Common Trend Assumptions

The key identifying assumption of our analysis is that no other policy changes or regional shocks coincide with the implementation of the high school reform and affect students' personality traits at the same time. Thus identification requires that the personality traits of students who were not affected and students who were affected by the reform would have followed the same trend, had they studied under the same schooling intensity. In our case, this *common trend assumption* boils down to the assumption that—in the absence of the reform—the personality of high school students would have evolved similarly in the different federal states.

This assumption is not testable because the counterfactual observation in absence of the reform cannot be observed. Yet, we argue that the assumption is likely fulfilled because we focus exclusively on the homogeneous group of high school students and control for a potential time trend through year fixed effects. As the choice of secondary school track strongly correlates with initial ability, the evolution of one student's personality is likely to most closely resemble that of another student in the same school track (i.e., high school). This should hold even if the two students come from different states (as opposed to students from the same state but in different school tracks) given that the state of residence is largely predetermined due to low mobility across states.

To assess the plausibility of the common trend assumption, Figure 1 displays average pre-reform scores for each of the personality traits by high school entry cohort, differentiating between states with an early and a late adoption of the reform. The figure shows that the pre-

[34] As the number of clusters is very small (15), we follow the recommendation by Cameron and Miller (2015) and base the statistical inference on the critical values of the student's t-distribution with accordingly reduced degrees of freedom (15 – 1 = 14). Instead, one could use wild cluster bootstrapped standard errors (Cameron et al. 2008). However, our estimations show that the wild cluster bootstrap does not consistently yield more conservative inference. While we therefore report the usual standard errors without bootstrapping, we deal with this issue in Section 6.1.

reform traits appear to develop largely in parallel over cohorts in both groups, supporting a common trend. In addition, we regress each personality trait on the interaction of being in an early-adopter state with each year of high school entry. The resulting coefficients, which serve as a placebo test of pre-reform differentials, appear in Figure 2. They reveal that—for almost all personality traits—no single year marks a significant difference between the two groups before early adopting states gradually introduced the reform, starting with the 1998 high school entry cohort.[35] As we control for state fixed effects, we allow students from different federal states to differ in their levels of personality traits. Further, we include state-specific linear time trends, such that the slopes of personality development over time may differ across states.

Moreover, crucial to the interpretation of the reform effect as a causal impact of schooling intensity is that the variation in schooling is truly exogenous for serving as a quasi-natural experiment. This assumption would be violated if there was self-selection into either the treatment group or the control group, or if the assignment was not random. In our analysis, neither issue should be a concern. As the reform was introduced in an entire state at one time, the only possibility of avoiding the reform, i.e. self-selecting into the control group, was to move to a different state.[36] Given the high costs associated with moving an entire family to another state, this appears highly unlikely.[37] The descriptive statistics on individual characteristics of the control and the treatment group (see table A.5 in the Appendix) confirm that selection is not a problem. Mean comparisons of both groups show that no (pre-reform) variables exhibit severe differences on average, apart from *age*, *East*, and *working mother*. These differences can be attributed to the composition of the sample and the earlier introduction of the reform in the former East German states. The slightly reduced percentage of low-performing students in the treatment group can be explained by an increasing binding character of the teacher's recommendation in the transition to secondary school.

To avoid the overcrowding of universities, the federal states chose different dates for implementing the reform. Therefore, the timing of the high school reform may correlate with specific state characteristics. As we control for state fixed effects in our analysis, the independence between timing of the reform in each state and state characteristics is not necessary (see Black et al. 2005). Nevertheless, we investigate whether the timing of the implementation of the intensified schooling system followed some pattern: We run OLS and ordered probit regressions of the state specific timing (late implementation, year in which the reform was implemented) on various pre-reform state characteristics (see Table A.6). The analysis shows that no such pattern exists because the timing of the reform is independent of the considered state characteristics: the percentage of high school students in a state's population, the governing party, upcoming state elections in 2001/2002, and the state's GDP per capita. States with a higher median age of residents adopted the high school reform slightly earlier, an artifact caused by the older age of all residents in the former East German states.

[35]The only exception is the first coefficient for openness. As in Figure 1, the first two years show a slightly less parallel pattern for openness, extraversion, agreeableness, and locus of control. Therefore, we repeat the analysis, excluding students who entered high school before 1997, but the results do not differ qualitatively. Results are available from the authors upon request.

[36]The only exception is Hesse, from where we exclude students accordingly.

[37]See Section 6.5 for a more extensive discussion on selectivity and robustness checks.

5 Results

The results of our OLS estimates of equation (3) appear in Table 2.[38] For each trait, the first specification does not control for any individual characteristics apart from gender, and, to account for any potential age effects, age and age squared. The second specification additionally controls for several individual pre-reform characteristics. The parameter estimates of the higher schooling intensity are almost identical in both specifications, validating the use of the high school reform as a quasi-natural experiment: As individual characteristics that possibly correlate with personality do not appear to change after the reform, their omission does not bias the estimated impact. Therefore, in our main specification we control only for the more parsimonious set of individual characteristics, in addition to fixed effects for the state, the year of school entry, and the SOEP sample, and state specific linear time trends.

As coefficients are small in size and statistically insignificant, the impact of the reform-induced schooling intensity on openness, conscientiousness, extraversion, and locus of control in the overall population of high school students remains unclear. In contrast, the estimates show that the higher schooling intensity significantly decreases students' emotional stability—indeed, substantially so, as it decreases by one third of a standard deviation on average. Compared to the predictive power of adverse childhood conditions on adult personality, this effect is sizable. A decrease of 0.32 standard deviations in emotional stability as a consequence of more intense schooling is similar in magnitude to the effect of having caregivers neglecting their basic needs in childhood (0.25 to 0.32 standard deviations) and is only slightly lower than that of sexual abuse during childhood (0.39 to 0.58 standard deviations), as investigated by Fletcher and Schurer (2017) in different specifications of sibling-fixed effects estimations.

Using summary indexes in sensitivity analyses in Section 6.1, we show that this finding is not a coincidental result of multiple hypotheses testing. Instead, this decrease in emotional stability is in line with our expectations and can reflect the impact of the higher workload, through increased stress and pressure on all students and reduced leisure time. Investigating the effects of the higher schooling intensity on the individual items that emotional stability encompasses, we find that, in particular, an increase in students' worries and a decrease in their ability to cope with stress drive the effect (see Table 3).

Moreover, in quantile regressions we find that the more emotionally stable students reveal stronger effects with the largest significant reduction at the median cutoff and a large but marginally insignificant coefficient at the upper quartile cutoff (see Table 4). In contrast, the personality of those in the lowest quartile changes to a lesser extent and not significantly so. However, these weaker effects at the lower tail may also be attributed to a floor effect.[39]

Following the increase in schooling intensity, agreeableness reveals a change of 0.14 standard deviations. This effect, however, is not statistically significant.[40] Nevertheless, the positive sign reveals no crowding out effects on agreeableness; instead, agreeableness may benefit from the type and intensity of social interactions between students due to longer school days. Considered together, these findings emphasize that the increase in schooling intensity likely impacts

[38] While Table 3 provides results from unweighted regressions, weighted regressions deliver virtually the same estimated coefficients.

[39] We also investigate whether the reform effect on emotional stability differs by the timing of birth: Even though students born in the first half of the calendar year usually enter school at a younger age than those born in the second half, we do not find that they respond differently to the reform in terms of emotional stability. Results are available from the authors upon request.

[40] We pick the 10 percent level of significance as relevant, as the number of observations is not overly large. Yet statistical insignificance does not prove that no effect exists but only that we fail to reject it, possibly as an artifact of the sample size. Even if effects are imprecisely estimated, they allow us to interpret the results qualitatively.

TABLE 2
EFFECTS OF THE REFORM

	Outcome Variables: Personality Traits											
	Open. (1)	Open. (2)	Consc. (3)	Consc. (4)	Extrav. (5)	Extrav. (6)	Agree. (7)	Agree. (8)	Emot. (9)	Emot. (10)	LoC (11)	LoC (12)
Reform	-0.034	-0.028	-0.057	-0.072	-0.023	-0.033	0.137	0.136	-0.324**	-0.320**	0.022	0.024
	(0.131)	(0.136)	(0.156)	(0.149)	(0.114)	(0.108)	(0.124)	(0.121)	(0.129)	(0.128)	(0.092)	(0.091)
Female	0.018	0.020	-0.008	-0.018	-0.016	-0.018	-0.009	-0.008	-0.012	-0.001	-0.025	-0.023
	(0.054)	(0.057)	(0.062)	(0.061)	(0.068)	(0.066)	(0.049)	(0.051)	(0.065)	(0.066)	(0.053)	(0.050)
Age	-0.654	-0.866	0.909	1.262	0.305	0.301	0.424	0.548	-1.967**	-2.016**	-0.721	-0.791
	(1.341)	(1.299)	(1.175)	(1.282)	(1.205)	(1.225)	(0.846)	(0.900)	(0.846)	(0.857)	(1.302)	(1.330)
Age^2	0.016	0.022	-0.022	-0.031	-0.008	-0.008	-0.011	-0.014	0.052**	0.053**	0.017	0.019
	(0.035)	(0.034)	(0.031)	(0.034)	(0.032)	(0.032)	(0.022)	(0.024)	(0.022)	(0.022)	(0.034)	(0.035)
Rural area		-0.073		-0.032		-0.015		-0.026		-0.089**		-0.041
		(0.071)		(0.092)		(0.064)		(0.061)		(0.041)		(0.048)
Non-intact family		0.141***		-0.073		0.151*		-0.082		-0.048		-0.102
		(0.043)		(0.090)		(0.079)		(0.062)		(0.059)		(0.059)
Work.-class father		-0.240**		0.029		0.009		-0.063		-0.116		-0.006
		(0.082)		(0.069)		(0.075)		(0.075)		(0.089)		(0.051)
High par. educ.		0.067		-0.192***		-0.143**		-0.047		-0.039		0.012
		(0.041)		(0.040)		(0.066)		(0.057)		(0.039)		(0.057)
Working mother		0.058		-0.022		0.126*		-0.032		0.140***		-0.036
		(0.068)		(0.051)		(0.059)		(0.036)		(0.037)		(0.065)
Immigration backgr.		0.198***		0.005		-0.088		-0.041		0.002		0.029
		(0.065)		(0.073)		(0.063)		(0.083)		(0.089)		(0.054)
Low-perf. student		-0.035		-0.074		-0.242**		0.153*		-0.123**		-0.155
		(0.129)		(0.088)		(0.100)		(0.086)		(0.052)		(0.099)
R^2	0.031	0.048	0.055	0.064	0.050	0.065	0.040	0.044	0.068	0.077	0.062	0.066
Observations	1,467	1,467	1,467	1,467	1,467	1,467	1,467	1,467	1,467	1,467	1,351	1,351

NOTE.—SOEPv32 waves 2005 to 2015. OLS regressions. The following controls are included: state specific linear time trends, and a maximum set of state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. Standard errors, reported in parentheses, are clustered at the state level; to account for the small number of clusters, inference is based on the critical values of the t-distribution with 14 degrees of freedom. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

TABLE 3
INDIVIDUAL ITEMS OF EMOTIONAL STABILITY

	OUTCOME VARIABLES: I SEE MYSELF AS SOMEONE WHO…		
	worries a lot	gets nervous easily	is relaxed, handles stress well
	(1)	(2)	(3)
Reform	0.441**	0.253	-0.424**
	(0.153)	(0.164)	(0.192)
Observations	1,467	1,467	1,467

NOTE.—SOEPv32 waves 2005 to 2015. OLS regressions. The following controls are included: female, age, age squared, state specific linear time trends, and a maximum set of state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. Answers are measured on a 7-point Likert scale. Standard errors, reported in parentheses, are clustered at the state level; to account for the small number of clusters, inference is based on the critical values of the t-distribution with 14 degrees of freedom. * p<0.1, ** p<0.05, *** p<0.01.

TABLE 4
QUANTILE REGRESSIONS

	OUTCOME VARIABLES: PERSONALITY TRAITS					
	Open. (1)	Consc. (2)	Extrav. (3)	Agree. (4)	Emot. (5)	LoC (6)
	A. Cutoff: 0.25					
Reform	-0.162	0.136	0.074	0.172	-0.176	-0.157
	(0.271)	(0.202)	(0.218)	(0.187)	(0.188)	(0.163)
	B. Cutoff: 0.50					
Reform	-0.140	-0.083	0.191	0.202	-0.374**	0.048
	(0.170)	(0.175)	(0.141)	(0.169)	(0.157)	(0.174)
	C. Cutoff: 0.75					
Reform	-0.131	-0.087	0.088	0.028	-0.300	0.186*
	(0.167)	(0.125)	(0.163)	(0.147)	(0.174)	(0.101)
Observations	1,467	1,467	1,467	1,467	1,467	1,351

NOTE.—SOEPv32 waves 2005 to 2015. Quantile regressions at the 0.25, 0.5, and 0.75 cutoff thresholds. The following controls are included: female, age, age squared, state specific linear time trends, and a maximum set of state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. Standard errors, reported in parentheses, are clustered at the state level; to account for the small number of clusters, inference is based on the critical values of the t-distribution with 14 degrees of freedom. * p<0.1, ** p<0.05, *** p<0.01.

particular facets of personality, possibly via different channels.[41]

For the individual characteristics, we find only few meaningful driving forces.[42] Living

[41]For a more extensive discussion on the potential mechanisms at work, see Section 5.2.

[42]The gender-specific standardization offsets all differences in absolute levels of the scores on personality traits between male and female students. We therefore expect the coefficient of *female* being small and insignificant in

in a non-intact family appears related to several personality traits: Children living with only one parent appear more extroverted and open to new experiences, possibly for being more familiar with new and changing environments. Yet, given that they do not reveal substantially lower levels of emotional stability, they do not suffer from significantly more emotional stress. Thus we can put the impact of schooling intensity into context both in terms of magnitude and significance: The increase in intensity decreases emotional stability by almost seven times as much as being raised in a non-intact family.[43]

Moreover, students from working-class and less-educated families are on average less open, more conscientious, and more extroverted than students from higher socioeconomic backgrounds.[44] Students with an immigration background display a higher level of openness but no other significant differences in traits. Both students growing up mainly in a rural area and low-performing students show lower levels of emotional stability. In addition, low-performing students have significantly lower levels of extraversion.[45]

In an additional specification, we include the parental personality trait in each regression for a subsample of our students (see Table A.7 in the Appendix). The respective coefficients stand out in both magnitude and significance, pointing to a strong intergenerational transmission of personality.[46] However, their inclusion makes little difference in the estimated impact of schooling intensity.

5.1 Heterogeneous Effects

Thus far, the estimates show average effects for the overall population of high school students. Those with higher schooling intensity tend to be less emotionally stable, while some are also potentially more agreeable. However, to shed real light on the consequences of the increased schooling intensity, we also consider how the policy change differently affects particular student subgroups. This differentiation not only reveals whose personality traits are particularly susceptible to schooling intensity but also illustrates how the mechanisms possibly interact with initial endowment.

A natural distinction is by *gender*, as boys and girls of a given age differ both in their stage of physical and mental development and in their behavior. Moreover, Dahmann (2017) finds that cognitive skills improve only among male students, suggesting that potential cross-fertilizing gains may differ by gender. Table 5 shows that the decrease in emotional stability is larger for females who also become less conscientious and perceive themselves as less in control of their lives. However, none of these interaction effects exhibits statistical significance at conventional levels.

We now examine heterogeneous effects by family structure. Given stronger interactions

the estimation, as is indeed the case. However, the weak gender effect does not imply that no significant gender differences exist in the original scores on personality.

[43]These findings apply to the sample of high school students. Being raised in a non-intact family may have larger effects on emotional stability in the overall population.

[44]Deckers et al. (2017) show that socioeconomic status shapes a child's preferences in particular via the level of parental investment. However, our sample is special in that it comprises only high school students—in a presumably more advantaged situation than students in other school tracks. Thus the effects of the individual characteristics on personality are not representative for the average adolescent in Germany.

[45]In unreported estimations, we further consider family characteristics related to birth order and the number of siblings for the subsample of students with the relevant information. While the effects of schooling intensity prove stable, these estimates reveal that first-born individuals are more conscientious.

[46]The intergenerational effects are between 0.087 and 0.267 for the Big Five personality traits and 0.144 and 0.174 for locus of control. These results are largely in line with the intergenerational correlation coefficients reported by Anger (2012) for children of all school types.

TABLE 5
HETEROGENEOUS EFFECTS OF THE REFORM BY ENDOWMENT

	OUTCOME VARIABLES: PERSONALITY TRAITS					
	Open. (1)	Consc. (2)	Extrav. (3)	Agree. (4)	Emot. (5)	LoC (6)
	A. Female					
Reform	0.014	0.001	-0.024	0.140	-0.269*	0.120
	(0.158)	(0.140)	(0.119)	(0.150)	(0.150)	(0.094)
Interaction	-0.095	-0.112	0.002	-0.006	-0.107	-0.194
	(0.130)	(0.113)	(0.119)	(0.137)	(0.081)	(0.119)
	B. Non-intact family					
Reform	-0.103	-0.046	-0.126	0.143	-0.346**	0.025
	(0.131)	(0.147)	(0.129)	(0.133)	(0.134)	(0.093)
Interaction	0.270***	-0.033	0.421**	-0.010	0.103	0.002
	(0.079)	(0.134)	(0.142)	(0.085)	(0.187)	(0.146)
	C. High parental education					
Reform	-0.030	-0.091	0.047	0.044	-0.272**	0.004
	(0.203)	(0.124)	(0.183)	(0.130)	(0.120)	(0.159)
Interaction	0.006	0.035	-0.137	0.159*	-0.091	0.034
	(0.167)	(0.096)	(0.134)	(0.087)	(0.053)	(0.161)
	D. Immigration background					
Reform	-0.093	-0.120	-0.027	0.139	-0.330**	0.035
	(0.113)	(0.164)	(0.092)	(0.144)	(0.130)	(0.098)
Interaction	0.356*	0.392**	0.029	-0.011	0.037	-0.092
	(0.184)	(0.180)	(0.226)	(0.196)	(0.132)	(0.208)
	E. Low-performing student					
Reform	-0.035	-0.051	-0.032	0.129	-0.315**	0.008
	(0.137)	(0.153)	(0.114)	(0.123)	(0.133)	(0.092)
Interaction	0.007	-0.063	0.106	0.093	-0.091	0.155
	(0.197)	(0.194)	(0.308)	(0.125)	(0.165)	(0.164)
Observations	1,467	1,467	1,467	1,467	1,467	1,351

NOTE.—SOEPv32 waves 2005 to 2015. OLS regressions, separately for each interaction considered. Only the coefficients of *REFORM* and the respective interaction with *REFORM* are presented. The following controls are included: female, age, age squared, the respective control variables of interest (without interaction with reform), state specific linear time trends, and a maximum set of state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. Standard errors, reported in parentheses, are clustered at the state level; to account for the small number of clusters, inference is based on the critical values of the t-distribution with 14 degrees of freedom. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

with teachers as additional adult reference persons, the increase in school hours may give students from *non-intact families* a more stable environment. However, these students may also receive less support at home for coping with the increased learning. Our estimates indicate that students who live with only one parent during part of their childhood benefit from higher schooling intensity in terms of openness and extraversion. In contrast, we find no evidence that a potential lack of support at home gives these students a larger decrease in emotional stability than students who live with both parents.

Moreover, we want to know whether and, if so, how students from different socioeconomic backgrounds cope with the increased schooling intensity. Therefore, we investigate heterogeneous effects by *parental education*, which enters the skill production function both directly, as a parental characteristic, and indirectly, by affecting initial endowment. As students with a lower socioeconomic status are less likely to receive (adequate) support from their parents for meeting the increased school demands, the higher reform-induced workload can lead to a higher stress level for this group. However, we find no significant differences in the impact of the increased schooling intensity on emotional stability. Indeed, the estimates reveal that students with better educated parents drive the potential increase in agreeableness.

As students with an *immigration background* may respond differently to schooling intensity by spending more time at school, they may benefit from becoming better integrated into their peer groups and improving their language skills. If so, more intense schooling could help reduce initial and persistent differences between these two groups, and foster the integration and educational performance of students from immigrant families. In a cross-country analysis, Schneeweis (2011) finds that increased instructional time is correlated with narrowing the achievement gap between natives and immigrants.

In line with these results, we find substantial benefits of the increased schooling intensity on the personality traits of immigrant students: Openness and conscientiousness increase by more than 0.35 standard deviations in this group compared to non-immigrant students. Lundberg (2013) shows these two traits being important correlates of later educational achievement, particularly for groups of low socioeconomic background. Thus an increased schooling intensity could be vital for fostering equality of opportunity between individuals with and without immigration background.

A general concern is that *low-performing* students are particularly vulnerable to the increased intensity, as they are most at risk of being "the first to be left behind" by the higher requirements at school. Huebener et al. (2017) find that achievement scores improve primarily for the better performing students, suggesting that potential cross-fertilizing gains may be unevenly distributed. Thus we investigate whether students with low performance before high school show differential effects. We define as low-performers those students who, at the end of elementary school, received a teacher recommendation for the basic or the intermediate track, not for high school. However, the decrease in emotional stability following the change in schooling intensity is not statistically different for this group.[47]

5.2 Potential Mechanisms

Individual Level. Thus far, we find evidence of a causal effect of schooling intensity on adolescents' emotional stability, while the other traits change for specific subgroups. Yet the mechanisms behind the effect of the increased intensity on students' personalities remain largely

[47] However, the percentage of low-performing students, identified through the teacher's recommendation at the end of elementary school is very low among high school students, making it difficult for us clearly interpreting our results.

unknown because this educational change has a number of implications. Affected students face a higher schooling intensity, particularly between ages 13 and 16, and the higher number of cumulative school hours are aimed at the students achieving a higher level of learning by age 17. Consequently, as they also spend more time per day in school, the intensity of schooling could impact their personalities through various distinct channels. Given our theoretical framework, we discuss the role of additional factors and possible pathways, considering particularly (i) cross-fertilizing gains through changes in cognition and school performance, and (ii) potential crowding out through changes in time allocation.

Changes in cognition: The higher level of learning achievement by age 17 due to more cumulative school hours may impact personality traits through changes in cognition. As the great majority of schools increased hours between grades seven and nine (ages 13-16), the adolescents in our treatment group have typically accumulated a full year of additional learning more than the control group. This earlier learning may increase cognitive skills at a younger age. Therefore, Huebener et al. (2017), Andrietti (2015), and Dahmann (2017) investigate students still attending high school, between ages 15 and 17. While they observe effect differences by cognition and gender, all three studies find that cognitive skill measures increase from the cognitive investment in the form of the increased schooling intensity. This increase, in turn, may improve noncognitive skills through the cross-productivity suggested by Cunha and Heckman (2007) in their skill formation model.

However, these studies also show that the cognitive benefits do not universally hold for all students, with some students having greater difficulties in keeping up with the additional workload. Proctor et al. (1996) show that, in some cases, an increase in workload significantly relates to impaired performance in tests of attention and executive function. An increase in schooling intensity may therefore also decrease at-school attention and cognitive functioning among students, resulting in inferior school performance. Büttner and Thomsen (2015) show for the double cohort in the state of Saxony-Anhalt that an increase in schooling intensity indeed negatively affects students' grades in mathematics and English.[48] This negative effect, in turn, could increase feelings of pressure and stress due to fear of failure among students who are subject to more intense schooling.

Indeed, we find that raising schooling intensity decreases students' satisfaction with their school performance. The percentage of students who are very satisfied with their performance in German literature decreases by 10 percentage points (see Table 6).[49] Levels of satisfaction with mathematics and first foreign language are not significantly affected by schooling intensity. Therefore, for overall school performance, the drop in satisfaction is much lower and not significant. Moreover, we find evidence that the increased schooling intensity raises students' need for paid tutor lessons.[50]

Changes in time allocation: As students subject to more intense schooling spend more time per week in school, the change in the institutional framework may also affect students through the change in time allocation. As they not only attend additional classes, which often take place

[48] Unfortunately, we cannot investigate the effect of schooling intensity on school performance. Given the lack of central exams, students' grades are not comparable across states. Moreover, while the grades of students in our control group only count for their annual grade report, the grades of students in our treatment group already count towards their final high school grades and are thus much more relevant.

[49] We define students as being very satisfied with their educational performance if they rate their satisfaction either 9 or 10 on a scale from 0 (low) to 10 (high). Among the control group, 19 percent report very high satisfaction with school performance overall and 23 percent with literature.

[50] Under higher schooling intensity, the percentage of students attending paid tutor lessons in addition to their regular school attendance increases by 6.5 percentage points, although we cannot show statistical significance at conventional levels. These results are available from the authors upon request.

TABLE 6
EFFECTS OF THE REFORM: MECHANISMS

	Outcome Variables						
	Satisfaction with School Performance		Leisure Activities			Health & Emotions	
	School	Literature	Music	Sport	Relax	Health	Sadness
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reform	-0.037	-0.101**	0.033	-0.026	-0.115*	-0.041	0.170*
	(0.055)	(0.042)	(0.076)	(0.057)	(0.064)	(0.026)	(0.093)
Obs.	895	895	1,061	1,060	893	1,466	917

NOTE.—SOEPv32 waves 2005 to 2015. OLS regressions. The following controls are included: female, age, age squared, state specific linear time trends, and a maximum set of state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. Standard errors, reported in parentheses, are clustered at the state level; to account for the small number of clusters, inference is based on the critical values of the t-distribution with 14 degrees of freedom. * p<0.1, ** p<0.05, *** p<0.01.

in the afternoon, but also spend lunch together, which in most schools was introduced with the more intense schooling, students face stronger student-teacher or student-student interactions resulting from longer school days. Thus, the increased school time spent leads to more social interactions with teachers and other students, and fosters independence from the parents.[51] This stimulation of interpersonal behavior may positively affect openness, extraversion, and agreeableness, which increase among particular subgroups. This expectation is in line with the findings on heterogeneous effects: As mostly disadvantaged students from non-intact or immigrant families benefit in these traits, the more intense schooling appears to provide a more stimulating environment than the counterfactual.

Yet, students have less leisure time—time that could be a valuable source of noncognitive investment and in particular impact their personality traits related to social behavior and wellbeing.[52] Furthermore, openness could be reduced through less diversity in the activities in which students engage. To examine whether and, if so, how such crowding out through the intensified schooling affects students' time allocation, we investigate their participation in leisure activities (music and sports) and their frequency of daydreaming or doing nothing (see Table 6). We find no evidence for the higher schooling intensity affecting participation in either music or sports.[53] Both the treatment and the control group appear to engage equally in both. In contrast, we find a decrease of 12 percentage points in students reporting daydreaming or doing nothing at least once a day. These findings suggest that our results are more likely driven by an overall decrease in unstructured leisure time, that is, time that students have for themselves, than by a reduction in the uptake of specific activities.

Unreported regressions also reveal that the reform has no impact on the frequency with which students are with their best friend, their boy-/girlfriend, or with friends in general. Thus, increased school hours do not crowd out activities that are related more to social behavior or diversity, reflected, for example, in openness, agreeableness, and extraversion, which adds to

[51] Jackson (2018) shows that teachers' quality affects noncognitive skills, while Schneeweis (2011) finds more time in school being related to a better integration of immigrant students.

[52] Kosse et al. (2018) show that the social environment affects noncognitive skills of elementary school children.

[53] This finding holds true not only for *whether* students participate in these activities at all but also for *how often* they participate.

explaining why only emotional stability is negatively affected by the reform. In contrast, the reduced time for unwinding may constitute an important reason for students affected by the reform being less relaxed and less able to handle stress. Thus the reduction in this non-allocated leisure time demonstrates the crowding-out effect of the increased cognitive investment, specifically for emotional stability.

One important pathway of this crowding-out effect can be adverse health effects resulting from the combination of higher pressure and the lack of time for relaxing. Previous research on working conditions and health shows that an increase in workload negatively affects health-related outcomes (Proctor et al. 1996), which may be related to an individual's emotional stability.[54] To shed more light on this potential pathway and whether and, if so, how it amplifies the negative effects, we investigate whether the change affects students' health-related characteristics. We find evidence that the increased schooling intensity has adverse effects on students' perceived health status (see Table 6). The likelihood of reporting good or very good health decreases by 4.1 percentage points, which is almost statistically significant at the 10 percent level. This result is in line with findings on the double cohort in the state of Baden-Wuerttemberg by Quis (2018), who shows that the change in schooling intensity leads to more stress and more mental health-related symptoms, particularly among females.

Moreover, we find that the schooling intensity increases feelings of sadness. Following the reform, the probability of being sad at least "sometimes" in the previous four weeks (as opposed to "seldom" or "very seldom") rises by 17 percentage points among affected students. We interpret these results as evidence that negative consequences on health and emotions are a relevant transmission channel that is not counteracted because noncognitive investment is crowded out. Overall, we conclude that the increase in workload arising from the intensified schooling causes lower subjective school performance, worse health conditions, and emotional difficulties among students. While these changes explain the decrease in emotional stability, they are not reflected in changes in the other personality traits.

Institutional Level. To fully shed light on both the potential and the dangers that accompany increasing schooling intensity, we also investigate differences in the implementation of the reform and other institutional factors. This analysis reveals whether particular measures can help counteract some of the unintended negative outcomes while fostering the positive ones.

First, as the former East and West Germany have persistent differences in socioeconomic environment and educational policies, we investigate differential impacts of the higher schooling intensity in the two parts of Germany. The long-standing West German tradition of the 13-year school system does not exist in the former East: Those Eastern states that reduced high school by one year in the 2000s had prolonged high school by one year only one decade earlier. If thus fewer infrastructural adjustments were necessary for the switch back, the changes induced by the increased schooling intensity could be mitigated in the former East compared to West Germany.

However, contrary to this intuition, the decrease in emotional stability is significantly more pronounced among students in the former East (see Table 7).[55] One reasonable explanation is that in Eastern states, where students normally attend elementary school for their first six years of schooling, seventh-grade students face both the transition to high school and more intense schooling at the same time. In contrast, West German seventh-graders already have had two

[54]Taking automotive workers as an example, Proctor et al. (1996) find overtime work associated with increased feelings of depression, fatigue, and confusion.

[55]Adolescents in the former East and West Germany show identical levels of emotional stability before the increase in schooling intensity.

TABLE 7
HETEROGENEOUS EFFECTS OF THE REFORM BY INSTITUTIONAL FACTORS

	OUTCOME VARIABLES: PERSONALITY TRAITS					
	Open. (1)	Consc. (2)	Extrav. (3)	Agree. (4)	Emot. (5)	LoC (6)
	A. East vs. West Germany					
Reform	0.031	-0.003	-0.040	0.190	-0.221	0.056
	(0.142)	(0.145)	(0.147)	(0.125)	(0.150)	(0.124)
Reform*East	-0.259	-0.226	0.086	-0.212	-0.413*	-0.110
	(0.268)	(0.272)	(0.287)	(0.158)	(0.224)	(0.259)
	B. Heterogeneous Effects by Preparation Time[1]					
Reform	0.109	0.000	-0.189	0.213	-0.186	0.184
	(0.197)	(0.144)	(0.176)	(0.135)	(0.156)	(0.124)
Reform*Little time	0.049	0.123	0.477**	0.040	-0.103	-0.319***
	(0.171)	(0.212)	(0.196)	(0.127)	(0.424)	(0.095)
Reform*No time	-0.751**	-0.419	0.234	-0.411**	-0.542**	-0.377
	(0.300)	(0.465)	(0.363)	(0.153)	(0.216)	(0.333)
	C. Heterogeneous Effects by Time since Implementation[2]					
Reform	-0.034	-0.033	-0.072	0.139	-0.302**	0.016
	(0.169)	(0.164)	(0.136)	(0.115)	(0.138)	(0.102)
Cohort=2	0.005	-0.087	0.184	-0.015	-0.070	0.010
	(0.189)	(0.134)	(0.175)	(0.105)	(0.064)	(0.144)
Cohort≥3	0.102	-0.094	0.293	-0.139	0.034	0.418**
	(0.193)	(0.127)	(0.179)	(0.092)	(0.111)	(0.180)
Observations	1,467	1,467	1,467	1,467	1,467	1,351

NOTE.—SOEPv32 waves 2005 to 2015. OLS regressions, separately for each panel. The following controls are included: female, age, age squared, state specific linear time trends, and a maximum set of the respective control variable of interest (without interaction with reform), state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. Standard errors, reported in parentheses, are clustered at the state level; to account for the small number of clusters, inference is based on the critical values of the t-distribution with 14 degrees of freedom. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

[1]States with little preparation time are Bavaria and Lower Saxony, in which the first cohort affected by the reform entered grade six at the time of implementation. States with no preparation time are Mecklenburg-West Pomerania and Saxony-Anhalt, in which the first cohort affected by the reform entered grade nine at the time of implementation.

[2]Next to *REFORM*, dummies for the second cohort affected by the reform, and for the third cohort and higher are included in the estimation and presented. For Saxony and Thuringia, all individuals are determined to be in cohort three or higher.

years to acclimatize themselves to high school before being confronted with the higher intensity. Thus this finding may show the importance of gradually changing the school environment of students who may otherwise feel stressed if several features change at once.[56] To rule out the possibility that a single (former East German) state is driving these results, we run the regressions 15 times, each time excluding one state. The coefficients change merely in size and significance.[57]

Second, we analyze whether the effects differ by the preparation time, that is, whether states planned the change well in advance or not. Therefore, we separate states into three groups: *No preparation time* covers Mecklenburg-West Pomerania and Saxony-Anhalt, which implemented the reform so abruptly that their first affected cohort immediately entered grade nine. *Little preparation time* covers Bavaria and Lower Saxony, where the first cohort affected by the reform had already entered high school at the time of implementation, but had completed only grade five. *Adequate preparation time*, the comparison category, covers all other states, in which students were aware of the reform when entering high school and the additional workload could be allocated with the appropriate foresight. Our results reveal that the students from states where the reform was implemented most suddenly, with the least preparation time, show the strongest deterioration in personality with large declines in openness, agreeableness, and emotional stability (see Table 7). This finding suggests that unintended consequences may be particularly detrimental if an increase in schooling intensity is not well planned. Furthermore, as the Eastern states were among the first to raise intensity, this finding possibly explains the effect differences between the former East and West Germany.

Third, we investigate whether institutional learning helps mitigate effects, such that cohorts coming three or more years after the implementation are affected differently than the first and the second cohorts. Except for an increased locus of control, we find no such differences, nor does the decrease in emotional stability diminish with time elapsed since the implementation (see Table 7). This finding allays the concern that teachers who are affected by the reform and who adjust only slowly are driving the effects.

5.3 Cost-Benefit Calculations

To put the effects we identify into perspective, we quantify the costs and benefits in terms of personality traits that result from the increase in schooling intensity by about 10 percent and assess changes in prospective earnings based on the estimated returns to personality traits in the literature.[58] Table 8 presents the estimated changes in wage and lifetime earnings from a simple back-of-the-envelope calculation. Given that the analyses show heterogeneous effects of higher schooling intensity on personality, we consider different groups: average students showing significant decreases in emotional stability, students in the former East Germany showing stronger decreases in emotional stability, students from non-intact families becoming also more open

[56] An interaction model reveals that the point estimate for emotional stability is indeed larger in absolute magnitude for states starting high school in grade seven; however, this difference is not statistically significant. Results are available from the authors upon request.

[57] Results are available from the authors upon request.

[58] The assumption that effects are quantifiable is particularly strong when studying intangible effects like those on personality. First, whether an increase or decrease in personality traits is advantageous or disadvantageous may depend on the individual's initial level. For example, a personality trait with a positive connotation, such as conscientiousness, may be undesirable if the individual is already at the extreme. Second, certain personality traits, such as agreeableness, may be detrimental for one dimension, for example in the labor market, while beneficial for another, like for social life or health. Therefore, focusing on prospective earnings as one aspect only, naturally renders an incomplete description of overall costs and benefits. Nevertheless, it is of key interest, especially to economists.

TABLE 8
COST-BENEFIT CALCULATIONS OF PERSONALITY CHANGES ON LIFETIME EARNINGS

	Wage return of reform (1)	Change in Lifetime Earnings Absolute (EUR) (2)	Relative (%) (3)
	A. Men		
Men	-2.37%	1,038.37	0.06%
...in the former East Germany	-5.58%	-54,563.25	-3.23%
...from non-intact family	-2.07%	6,212.50	0.37%
...with immigration background	-2.67%	-4,236.17	-0.25%
	B. Women		
Women	-4.32%	-18,897.65	-1.62%
...in the former East Germany	-7.29%	-54,457.76	-4.67%
...from non-intact family	-2.41%	4,037.25	0.35%
...with immigration background	-2.76%	-207.91	-0.02%

NOTE.—Authors' calculations. The first column is obtained through multiplying the statistically significant changes in personality traits induced by the increased schooling intensity with wage returns to a one-standard deviation change in the trait for men (Openness -0.017, Conscientiousness +0.013, Extraversion +0.012, Emotional Stability +0.088) and women (Openness +0.023, Emotional Stability +0.115), respectively. To obtain columns 2 and 3, the resulting wage changes are used to compute lifetime earnings with earlier labor market entry (hourly wage*week hours*52 weeks*(working years+1)*(1+wage change)) and compare them to lifetime earnings with later labor market entry (hourly wage*week hours*52 weeks*working years). For simplification, we use average gross hourly wages (Men: EUR 20.20, Women: EUR 15.83) and the average length of working life (Men: 40.2 years, Women: 35.4 years) from Crößmann and Mischke (2016) and assume full-time employment of 40 working hours per week.

and extroverted, and students from immigrant families becoming also more open and conscientious.

As our sample is too young to estimate labor market effects of personality changes way into adulthood, we compute wage returns to the reform-induced changes based on estimates for Germany by Heineck and Anger (2010)[59] and for the Netherlands by Nyhus and Pons (2005).[60] Both studies also reveal that wage returns to personality may differ across gender: while openness, for example, is penalized for men, women are rewarded for higher levels of openness. For this reason, we distinguish our groups further by gender. Under the assumption that the changes in personality traits persist into adult working ages, we find that the increased schooling intensity in all groups leads to decreases in wages (column 1).[61] However, an important benefit of the increased schooling intensity is that the same level of schooling can be achieved in shorter time, thus enabling individuals earlier labor market entry by one year. Therefore,

[59]We take significant coefficients of openness, conscientiousness, and extraversion from OLS regressions as the most conservative estimates.

[60]We rely on estimates for emotional stability from Nyhus and Pons (2005), even though these are also included in Heineck and Anger (2010). In contrast to previous literature, Heineck and Anger (2010) do not find significant effects, attributing the absence of an effect to the inclusion of an individual's attitude towards reciprocal behavior, which is linked to emotional stability, in their model. As we do not account for changes in reciprocity, the estimates for emotional stability by Heineck and Anger (2010) do not apply to our context.

[61]These are aggregate effects: Negative effects of the decrease in emotional stability cancel out positive effects resulting from, for example, increases in extraversion and conscientiousness.

column 2 and 3 present changes in hypothetical lifetime earnings.[62]

These calculations reveal that, as the increased schooling intensity entails costs as well as benefits, the wage penalties for the personality changes are not necessarily translated into a decrease in total lifetime earnings. For some individuals, the earlier labor market entry—through the higher schooling intensity that shortens overall school duration—offsets any costs. Thus, the increased schooling intensity yields overall increases in lifetime earnings, for example, on average for men or individuals from non-intact families. For women in contrast, we find the strongest penalty in lifetime earnings of up to 4.7 percent in the former East Germany.[63] Yet, for West German high school students, the monetary benefits outweigh the costs. Even in a conservative calculation, in which we assume a statistically significant negative reform effect on Western students' emotional stability despite the marginally insignificant coefficient (see Table 7), the about 10 percent higher schooling intensity yields a 0.5 percent increase in lifetime earnings for males, and a 0.2 percent increase for females.

6 Sensitivity Analyses

For a causal interpretation of the effects of the increase in schooling intensity, we have to rule out the possibility that any other factors than those considered might drive our estimates. This section discusses several sensitivity analyses that we carry out, with the corresponding tables in the Appendix.

6.1 Estimation Model

As mentioned in Section 4.3, the wild cluster bootstrap that Cameron et al. (2008) propose to account for the small number of clusters does not consistently yield larger p-values in our case. As this method is thus not necessarily more conservative, we refrain from bootstrapping in our preferred estimation. Nonetheless, results using wild cluster bootstrap confirm our statistical inference and findings (see Table A.8).

Furthermore, as the Likert-type scale for the assessment of personality is ordinal, an ordered probit model may be more appropriate than OLS regressions, especially if changes of one unit have different implications according to their location on the scale (i.e., if the scale is non-linear). Therefore, we run an ordered probit regression, with the outcome categories being the standardized measures of each personality trait (see Table A.8). This procedure yields results very similar to our original findings, thereby confirming our earlier conclusions.

With multiple outcomes, the concern of multiple hypotheses testing arises. To ensure that our results are not coincidental as a result of us running several regressions, we construct a summary index for the personality inventory. Given that adding outcomes to the index does not increase the probability of a false rejection, this index is robust to overtesting (Anderson 2008). Doing so also allows us to estimate the effect on students' latent personality. Thus these summary indexes (i) are potentially more powerful than testing the individual variables, (ii) reduce measurement error, and (iii) provide an estimate of a more general effect of the change in schooling intensity (Anderson 2008). Therefore, they are more effective for our analysis than procedures maintaining the number of tests and only adjusting the p-values.

[62] The relative changes presented in column 3 are less sensitive to the choice of parameters (e.g., the choice between part time and full-time work) than the absolute changes presented in column 2.

[63] This analysis ignores any consequences of the increased schooling intensity on further outcomes and does not consider general equilibrium effects. It mainly serves putting effects into perspective in a simplified back-of-the-envelope calculation.

We construct two types of summary indexes for personality. We include all traits (i) in the direction we use throughout the study (so that we can jointly interpret them as stock of noncognitive skills) and (ii) in the direction of their main effect in Table 2 (so that we can estimate the general effect of higher schooling intensity without opposite effects on different traits offsetting each other). Summarizing the Big Five outcomes by weighting each trait to maximize the amount of information captured in the first index reveals no significant overall improvement or deterioration of the noncognitive skills (see Table A.8). However, the second index reveals a significant impact of higher schooling intensity in general of 0.13 standard deviations. If locus of control is added to these summary indexes, the general impact remains in both size and significance. This finding indicates that, although schooling intensity affects students' personality in general, the directions of effects in terms of skill stocks offset one another across traits.

6.2 Age Effects

Thus far, to account for potential age effects in personality, we include age and age squared in all estimations. An alternative way is regressing the original score of each personality trait (without standardization) on age and age squared, next to gender (Nyhus and Pons 2005). The resulting residuals are then age-free and enter the second stage as outcome measures. Our results are robust to this procedure (see Table A.9).

6.3 Measurement Issues

When analyzing personality traits, a major concern is potential measurement error. In particular, when including personality traits as *independent* variables, estimated coefficients are biased if the measure of personality traits suffers from a lack of precision. Therefore, existing studies usually correct for potential measurement error (see, for example, Heckman et al. 2013; Heineck and Anger 2010; Zumbühl et al. 2013). However, in this study the *dependent* variable may suffer from measurement error. In this case, estimates are unbiased, as long as the error occurs randomly, but the variance may increase.

We do not expect students who are subject to higher schooling intensity to systematically differ in their self-reporting of personality other than for differences induced by true reform effects. Thus, the assumption that potential measurement error is random, and thereby uncorrelated with the assignment of students to the treatment or control group, is reasonable in our study. In addition, our results prove robust to aggregating the personality traits into a single summary index (see Section 6.1), which reduces measurement error.

To take into account changes in personality after graduation, we re-estimate the impact of schooling intensity while controlling for individuals having graduated or not (see Table A.9).[64] While the coefficients point to some changes in personality after high school graduation, these effects are not precisely estimated. Importantly, the effects of the intensified schooling on the personality traits do not differ from the baseline specification. Another concern is that students who are affected by the reform may experience additional stress because of a closer proximity to the graduation exams at the time of the interview. Therefore, exploiting exogenous timing of the interview, we control for the time to graduation in months linearly and quadratically (see Table A.9). Again, our results remain robust.

Finally, the construction of the personality measures by simply averaging item scores along each dimension neglects measurement error. We do not expect that this way of constructing the

[64] Only 21 percent of the individuals in our sample have already graduated.

personality measures is correlated with the assignment of students to the treatment or control group. Nevertheless, to validate this approach, we conduct a factor analysis. We find that the personality items that we use in our analysis load on specific factors that correspond to the dimensions of the five factor model and to locus of control.[65]

In sum, we conclude that our study does not suffer from bias due to measurement error. Even in the presence of measurement error, the effects we find are unbiased and inference is still valid because the estimated standard errors provide an upper bound in this case.

6.4 Stability of Personality

Another concern is that the personality measures of the students in our sample may be a mere snapshot and not persist over time. Exploiting the panel character of the dataset, we use a subsample of students for whom a second self-rating of personality is available in the 2013 survey[66] and compare the two measurements: The rank-order correlation coefficients range from 0.42 to 0.60 for the Big Five dimensions.[67] These coefficients are perfectly in line with Specht et al. (2011) for this age group and are only slightly below the rank-order correlations found for adults between 0.64 and 0.75 depending on the trait (Specht et al. 2011). Thus we have no reason to suspect that the personality of our sample of high school students is exceptionally unstable.

6.5 Selectivity

As outlined in Section 4.3, we regard selection from the treatment into the control group or vice versa as highly unlikely given the relatively high moving costs involved. Indeed, for 92 percent of the individuals in our sample, we know that they have not changed residence since their childhood.

Nevertheless, considering that individuals living close to a state border may choose to attend school in a different state to avoid the higher schooling intensity, we define a subset of late-adopter states whose neighboring states have all adopted the reform earlier.[68] In these states, self-selection from the treatment into the control group by moving to (or attending school in) a neighboring state to avoid the reform is thus ruled out. We still find a large and significant coefficient on emotional stability (see Table A.10).

A more severe concern of selectivity is self-selection *out* of the sample, that is, students who originally would attend high school but, because of the increased intensity, follow a different secondary school track.[69] This type of selection could occur in two distinct ways: either directly through a different school track choice after elementary school, or through a change in high school dropout rates at a later stage. As Huebener and Marcus (2017) find that the reform does

[65] Results are available from the authors upon request.

[66] This is the case for 39 percent of our original sample. Naturally, all students observed in 2013 or later for the first time cannot be considered.

[67] These computations use the non-standardized measures of personality. Rank-order correlation coefficients are measured by Spearman's Rho. Information on locus of control is not surveyed in the wave 2013.

[68] This group contains all states where the first students subject to increased schooling intensity graduate in 2012 or later: Baden-Wuerttemberg, Bremen, Hesse, North Rhine-Westphalia, Berlin, Schleswig-Holstein, and Brandenburg.

[69] In the presence of self-selection based on personality traits, we would expect primarily the least emotionally stable students, who worry most about and fear being unable to cope with the increased intensity, to leave or not enter high school (i.e., drop out of the treatment group). In this case, our estimated decrease in emotional stability in Section 5 would represent a lower bound in absolute terms and provide a conservative estimate of the true negative effects of increased schooling intensity in terms of stress and emotional instability.

not affect high school entry rates and graduation rates, both ways are unlikely.

Nevertheless, the concern of track choice may be most applicable in the presence of comprehensive schools (*Gesamtschule*). As this track allows students to obtain the same university entrance qualification, comprehensive schools serve as the most viable alternative to high schools. However, comprehensive schools are not equally common to all states, and analyses restricting our sample to states with typically no comprehensive schools[70] reveal virtually the same coefficient on emotional stability (see Table A.10). Given the sharp reduction of the sample size the precision drops expectedly. Moreover, the increase in extraversion is even stronger in the states without comprehensive schools than on average, while conscientiousness decreases in this subsample. These results suggest a weakening of the effects of schooling intensity if students find a way to avoid the new system.

Lastly, both possible channels of self-selection—the change in school choice and the increase in high school dropout rates—would not only affect the composition of high school students but also the composition of students enrolled in the other types of secondary school that students have to attend instead of high school. However, our findings in Section 6.8 on reform effects for students from other school types support the assumption of no such changes in the composition of students.

6.6 Announcement Effects and Double Cohort

In some states, the pre-implementation discussions about the high school reform possibly led to its anticipation and may have caused announcement effects among student cohorts close to the implementation date. Yet post-reform students were highly unlikely to switch to the control group by skipping one grade because grade-skipping is not only virtually impossible but would also lead to ultimately graduating in the original graduation year. Likewise, pre-reform students were highly unlikely to switch to the treatment group by intentional grade repetition, as they would have caught up with their original cohort at the time of graduation. In contrast, it is more plausible that pre-reform high school students who were at risk of repeating a grade may have tried evading the reform by putting high effort in staying in their original grade (remaining in the control group).

However, these concerns apply only to the last cohort graduating after nine years of high school and to the first cohort with only eight years. Indeed, this double cohort features peculiarities possibly leading to confounding effects or offsetting true effects of the change in schooling intensity. Excluding this double cohort of graduates in an additional model reveals an even stronger decrease in emotional stability (see Table A.10) and thus supports the notion of (at least partially) offsetting effects in the double cohort of graduates.[71]

6.7 Other Institutional Changes

Another aspect of the German high school system—the standardized high school graduation examinations (*Zentralabitur*)— was also intensively discussed in public. While some federal states had standardized examinations in place since the 1990s or even earlier, most of the remaining states introduced those between 2005 and 2008. As our empirical strategy exploits

[70] We define states with typically no comprehensive schools as those states in which the percentage of students attending comprehensive schools was less than 10 percent between 2000 and 2013. These are Baden-Wuerttemberg, Bavaria, Lower Saxony, Mecklenburg-West Pommerania, Saxony, Saxony-Anhalt, and Thuringia (Autorengruppe Bildungsberichterstattung 2012, 2014).

[71] This finding may explain why Thiel et al. (2014) find smaller effects, restricting their analysis to the double cohort of graduates in Saxony-Anhalt.

the variation over time and across regions, we isolate the effects of the change in schooling intensity from any other policy change occuring at a different time, such as the introduction of the standardized graduation examinations.

Moreover, if newly introduced, standardized high school graduation exams affect both earlier cohorts (not affected by the more intense schooling) and the later cohorts (affected by the more intense schooling), i.e., almost our entire sample participates in the standardized examinations. Nonetheless, their introduction may affect students with different levels of schooling intensity differently. Thus, to rule out that the implementation of the standardized examinations drives our results, we consider a subsample of states with standardized exams that have been in place for a long time.[72] The still significant and even larger decrease in emotional stability following the reform (see Table A.10) confirms that the effect is not driven by the introduction of the standardized graduation examinations.

6.8 Placebo Estimation

Finally, to rule out that the observed changes in students' personalities are caused by any other factors than the increased schooling intensity, we run a Placebo regression. In this regression, we estimate the effect of the high school reform on the personality measures of students of other school types in the exact same manner. As the high school reform did not involve students at the lower secondary school (*Hauptschule*) and at the intermediate secondary school (*Realschule*), these students did not experience an increase in schooling intensity and therefore should not show any reform-induced changes in their personality traits.[73] Perfectly in line with this expectation, our Placebo estimation shows no reform effects (see Table A.11).

Moreover, these findings refute the concern of out-of-sample selectivity following the reform: If high school students with specific personality traits selected out of the sample in reaction to the reform, we would expect finding a change in average personality among students of the other school types. As this is clearly not the case, we can rule out that students who aimed at attending high school selected themselves into alternative school types due to the higher schooling intensity.

7 Summary and Conclusion

We examine the cross effects of cognitive investments on students' noncognitive skills using a sharp, staggered high school reform across states between 2001 and 2007 in Germany. The reform reduced total years in high school but retained the high school curriculum, thereby increasing weekly schooling by about three hours. Using this nationwide quasi-experiment allows us to investigate the causal impact of schooling intensity—as an investment particularly targeting cognitive skills—on personality traits. Based on representative survey data, we find that the increase in schooling intensity decreases overall students' emotional stability, but has neither average effects on the other Big Five personality traits nor on locus of control.

We show in a number of sensitivity analyses that these findings are unrelated to unobserved factors coinciding with the change, to general individual-specific factors, to measurement error,

[72] These states are Baden-Wuerttemberg, Bavaria, Mecklenburg-West Pomerania, Saarland, Saxony-Anhalt, Saxony, and Thuringia.

[73] Students from the lower and intermediate secondary school track may be indirectly affected by the reform, e.g., by facing increased competition for apprenticeships, as their age advantage over the better qualified high school graduates was reduced substantially after the reform. However, these effects should not be as strong as direct effects. Indeed, Mühlemann et al. (2018), analyzing the graduation of the double cohort students, do not find reduced probabilities for lower and middle track students to enter into an apprenticehip contract.

and to selectivity. Quantifying the effects through prospective earnings, we find an average lifetime earnings penalty for the 10 percent higher schooling intensity of 1.6 percent for women, whereas offsetting effects on average lead to a zero net effect for men. Yet, the cost-benefit calculation for the West German states yields exclusively premiums in lifetime earnings.

Surprisingly, the decrease in emotional stability is larger for students in the former East Germany, even though these states were more experienced with higher schooling intensity given that they had eight years of high school for many decades before the 1990 reunification. At the same time, positive effects appear for disadvantaged students from either non-intact or immigrant families who benefit substantially from higher openness and extraversion, and from higher openness and conscientiousness, respectively. These findings call for further research on the interaction of the family environment with cognitive and noncognitive investments (see Deckers et al. 2017, for the influence of socioeconomic status on cognitive and noncognitive skills). Lundberg (2013) finds openness to new experiences being particularly relevant in determining college graduation among disadvantaged students. Thus the improvement in this trait for students from non-intact and immigrant families has significant implications for reducing inequality at later educational stages.

Our findings reinforce that personality traits in adolescence are malleable through schooling (cf. Martins 2010; Bloom et al. 2009; Li and Powdthavee 2014; Kassenboehmer et al. 2018). As the high school reform did not affect the overall curriculum, the relevant mechanisms may underly the higher schooling intensity, such as the higher accumulated knowledge at a particular age, stronger student-teacher or student-student interactions given the longer school days, and the change in time allocation away from non-academic activities. By comparing how further outcomes change with the increase in intensity, we investigate which of these potential mechanisms are at work in shaping adolescents' personalities through schooling.

While the reform did not affect participation in specific leisure activities, such as music and sports, students have less non-structured time to relax. Our results point to adverse effects on contemporaneous measures of health, subjective school performance, and emotions, that may affect personality in the long run. These differential effects indicate that the intensified schooling crowded out only some noncognitive investments: opportunities that foster students' wellbeing decrease but activities related to, for example, social behavior remain unaffected—explaining why emotional stability deteriorates, while other traits do not. Future research should address student-teacher and student-student interactions in a setting of intensified schooling (see Jackson 2018, who studies the impact of teachers' quality on noncognitive skills).

The treatment investigated in this study affects only students enrolled in academic-track high school. We assume these students possessing more favorable noncognitive skills than students who attend the lower tracks. While the difference in initial skills between these groups calls the external validity of the precise estimated effects into question, it does not alter our conclusion that personality traits in adolescence are malleable through schooling. Yet the recent change in schooling intensity allows us to investigate only short-term effects. Although the considered personality traits prove being relatively stable once adolescents enter adulthood, future research should assess the long-term impacts of schooling intensity and the persistence of differences in students' personality subjected to different intensity levels.

We conclude that educational investments in cognitive skills not only play an essential role in shaping adolescents' personality traits but also have the potential of crowding out noncognitive skills. These findings are novel and consistent with the predictions of our theoretical model, which imposes a per-period budget constraint for total investments in skill formation.[74]

[74] An alternative theoretical interpretation of our results would be a shift in the skill production function causing

The results highlight that, as educational changes—such as an increase in schooling intensity and in instruction time (see, e.g., Lavy 2015; Dahmann 2017; Huebener et al. 2017; Andrietti 2015)—may benefit one skill dimension but come at the cost of another, investment externalities should be considered.

Although the focus of this study is on the increase in instruction hours that is accompanied by new learning contents (from higher school grades) at younger ages, the findings may potentially be generalized to any school reform that overemphasizes the focus on a single skill dimension by changing either learning contents or teaching methods.[75] In our context, we attribute the adverse effects largely to the quick ad hoc implementation of the higher schooling intensity, as occured particularly in the former East Germany: In the Eastern federal states, where the implementation of the reform has not been extensively prepared, additional cognitive skill investments crowd out noncognitive skills and deteriorate emotional stability. If, in contrast, the implementation of additional school hours is well planned and matches investments in noncognitive skills, cognitive skill investments have the potential to realize cross-fertilizing gains of personality traits. Hence, effective education policy should aim at preventing crowding out of noncognitive skills by adjusting school workload in a way that realizes optimal cognitive and noncognitive skill formation.

crowding out effects. Yet, we can show that even with an unmodified skill production function, extending the Cunha and Heckman model by a budget constraint can produce negative effects (consistent with our empirical findings).

[75] For example, a change to the "teaching to the test"-strategy, which is often adopted to increase performance on high-stakes exams, could similarly impact on noncognitive skills.

Appendix

A.1 Skill Formation Model

The Cunha and Heckman Model

Starting from $\theta_{t+1}^k = f_t^k(\theta_t^C, \theta_t^N, I_t^k, h^C, h^N)$ for $k \in \{C, N\}$, the formation of cognitive and noncognitive skills can be described as follows, by iterative insertion of the equations:

$$
\begin{aligned}
\theta_{t+1}^C =& f_t^C(\theta_t^C, \theta_t^N, I_t^C, h^C, h^N) \\
\theta_{t+1}^N =& f_t^N(\theta_t^C, \theta_t^N, I_t^N, h^C, h^N) \\
\theta_{t+2}^N =& f_{t+1}^N(\theta_{t+1}^C, \theta_{t+1}^N, I_{t+1}^N, h^C, h^N) \\
=& f_{t+1}^N\Big(f_t^C(\theta_t^C, \theta_t^N, I_t^C, h^C, h^N), f_t^N(\theta_t^C, \theta_t^N, I_t^N, h^C, h^N), I_{t+1}^N, h^C, h^N\Big) \\
\theta_{t+3}^N =& f_{t+2}^N(\theta_{t+2}^C, \theta_{t+2}^N, I_{t+2}^N, h^C, h^N) \\
=& f_{t+2}^N\Bigg(f_{t+1}^C\Big(f_t^C(\theta_t^C, \theta_t^N, I_t^C, h^C, h^N), f_t^N(\theta_t^C, \theta_t^N, I_t^N, h^C, h^N), I_{t+1}^C, h^C, h^N\Big), \\
& f_{t+1}^N\Big(f_t^C(\theta_t^C, \theta_t^N, I_t^C, h^C, h^N), f_t^N(\theta_t^C, \theta_t^N, I_t^N, h^C, h^N), I_{t+1}^N, h^C, h^N\Big), I_{t+2}^N, h^C, h^N\Bigg), \text{etc.}
\end{aligned}
$$

The impact of cognitive investment in period t on future noncognitive skill development can now be obtained by applying the chain rule for derivatives:

$$
\begin{aligned}
\frac{\partial \theta_{t+1}^N}{\partial I_t^C} &= 0 \\
\frac{\partial \theta_{t+2}^N}{\partial I_t^C} &= \underbrace{\frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^C}}_{\substack{\geq 0 \\ \text{(cross-} \\ \text{productivity)}}} * \underbrace{\frac{\partial f_t^C}{\partial I_t^C}}_{\substack{\geq 0 \\ \text{(productivity} \\ \text{of cognitive} \\ \text{investment)}}} \geq 0, \\
\frac{\partial \theta_{t+3}^N}{\partial I_t^C} &= \Bigg(\underbrace{\frac{\partial f_{t+2}^N}{\partial \theta_{t+2}^C}}_{\substack{\geq 0 \\ \text{(cross-} \\ \text{productivity)}}} * \underbrace{\frac{\partial f_{t+1}^C}{\partial \theta_{t+1}^C}}_{\substack{\geq 0 \\ \text{(self-} \\ \text{productivity)}}} + \underbrace{\frac{\partial f_{t+2}^N}{\partial \theta_{t+2}^N}}_{\substack{\geq 0 \\ \text{(self-} \\ \text{productivity)}}} * \underbrace{\frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^C}}_{\substack{\geq 0 \\ \text{(cross-} \\ \text{productivity)}}} \Bigg) \underbrace{\frac{\partial f_t^C}{\partial I_t^C}}_{\substack{\geq 0 \\ \text{(productivity} \\ \text{of cognitive} \\ \text{investment)}}} \geq 0, \text{etc.},
\end{aligned}
$$

such that $\frac{\partial \theta_{t+i}^N}{\partial I_t^C} \geq 0 \ \ \forall i \geq 2$.

Skill Formation with Investment Constraints

Introducing a budget constraint on total investment, $I_t^C + I_t^N = I_t$, the above description of skill formation changes to:

$$
\begin{aligned}
\theta_{t+1}^C =& f_t^C(\theta_t^C, \theta_t^N, I_t^C, h^C, h^N) \\
\theta_{t+1}^N =& f_t^N(\theta_t^C, \theta_t^N, I_t - I_t^C, h^C, h^N) \\
\theta_{t+2}^N =& f_{t+1}^N(\theta_{t+1}^C, \theta_{t+1}^N, I_{t+1}^N, h^C, h^N) \\
=& f_{t+1}^N\Big(f_t^C(\theta_t^C, \theta_t^N, I_t^C, h^C, h^N), f_t^N(\theta_t^C, \theta_t^N, I_t - I_t^C, h^C, h^N), I_{t+1}^N, h^C, h^N\Big) \\
\theta_{t+3}^N =& f_{t+2}^N(\theta_{t+2}^C, \theta_{t+2}^N, I_{t+2}^N, h^C, h^N) \\
=& f_{t+2}^N\Bigg(f_{t+1}^C\Big(f_t^C(\theta_t^C, \theta_t^N, I_t^C, h^C, h^N), f_t^N(\theta_t^C, \theta_t^N, I_t - I_t^C, h^C, h^N), I_{t+1}^C, h^C, h^N\Big), \\
& f_{t+1}^N\Big(f_t^C(\theta_t^C, \theta_t^N, I_t^C, h^C, h^N), f_t^N(\theta_t^C, \theta_t^N, I_t - I_t^C, h^C, h^N), I_{t+1}^N, h^C, h^N\Big), I_{t+2}^N, h^C, h^N\Bigg), \text{etc.}
\end{aligned}
$$

Analogously, the impact of cognitive investment in period t on future noncognitive skill development changes to:

$$
\frac{\partial \theta_{t+1}^N}{\partial I_t^C} = -\underbrace{\frac{\partial f_t^N}{\partial I_t^N}}_{\substack{\geq 0 \text{ (productivity of} \\ \text{cognitive investment)}}} \leq 0,
$$

$$
\frac{\partial \theta_{t+2}^N}{\partial I_t^C} = \underbrace{\frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^C}}_{\substack{\geq 0 \\ \text{(cross-} \\ \text{productivity)}}} * \underbrace{\frac{\partial f_t^C}{\partial I_t^C}}_{\substack{\geq 0 \\ \text{(productivity} \\ \text{of cognitive} \\ \text{investment)}}} - \underbrace{\frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^N}}_{\substack{\geq 0 \\ \text{(self-} \\ \text{productivity)}}} * \underbrace{\frac{\partial f_t^N}{\partial I_t^N}}_{\substack{\geq 0 \\ \text{(productivity} \\ \text{of noncognitive} \\ \text{investment)}}} \begin{cases} > 0 & \text{if } \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^C} * \frac{\partial f_t^C}{\partial I_t^C} > \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^N} * \frac{\partial f_t^N}{\partial I_t^N} \\ = 0 & \text{if } \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^C} * \frac{\partial f_t^C}{\partial I_t^C} = \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^N} * \frac{\partial f_t^N}{\partial I_t^N} \\ < 0 & \text{if } \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^C} * \frac{\partial f_t^C}{\partial I_t^C} < \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^N} * \frac{\partial f_t^N}{\partial I_t^N}. \end{cases}
$$

Similarly, for all subsequent periods, the trade-off continues to exist, as skills are self- and cross-productive. Thus, the benefits of the increase in cognitive investments and the costs of the foregone noncognitive investments will be multiplied by the respective combinations of cross- and self-productivity of both cognitive and noncognitive skills, e.g., for period $t+3$ we have:

$$
\frac{\partial \theta_{t+3}^N}{\partial I_t^C} = \underbrace{\frac{\partial f_t^C}{\partial I_t^C}\left(\frac{\partial f_{t+2}^N}{\partial \theta_{t+2}^C} * \frac{\partial f_{t+1}^C}{\partial \theta_{t+1}^C} + \frac{\partial f_{t+2}^N}{\partial \theta_{t+2}^N} * \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^C}\right)}_{\geq 0 \text{ (benefits from cognitive investment)}} \underbrace{- \frac{\partial f_t^N}{\partial I_t^N}\left(\frac{\partial f_{t+2}^N}{\partial \theta_{t+2}^C} * \frac{\partial f_{t+1}^C}{\partial \theta_{t+1}^N} + \frac{\partial f_{t+2}^N}{\partial \theta_{t+2}^N} * \frac{\partial f_{t+1}^N}{\partial \theta_{t+1}^N}\right)}_{\leq 0 \text{ (costs from foregone noncognitive investment)}},
$$

which can be > 0, $= 0$, or < 0, and analogously thereafter.

A.2 Variables

TABLE A.1
DESCRIPTION OF VARIABLES

Variable	Description
Openness (Open.)	Standardized Big Five measure for openness to experience
Conscientiousness (Consc.)	Standardized Big Five measure for conscientiousness
Extraversion (Extrav.)	Standardized Big Five measure for extraversion
Agreeableness (Agree.)	Standardized Big Five measure for agreeableness
Emotional stability (Emot.)	Standardized Big Five measure for emotional stability
Locus of control (LoC)	Standardized measure for internal locus of control
Age	Age (in years)
Female	Dummy for female
East	Dummy for East German states
Rural area	Dummy for living mostly in rural area until age 15
Non-intact family	Dummy for not having lived with *both* parents for the entire time up to age 15
Working-class father	Dummy for father having blue-collar occupation at student's age 15, reference category includes *all* others
High parental education	Dummy for at least one of an individual's parents having an upper secondary school degree or higher
Working mother	Dummy for working mother (both full-time and part-time) at student's age 10
Immigration background	Dummy for student with immigration background
Low-performing student	Dummy for having received a recommendation for a different type of secondary school, i.e. *other than high school*, after the fourth grade
High school diploma	Dummy for having graduated from high school
Health	Dummy for health status being good or very good (as opposed to satisfactory, not so good, bad)
Sadness	Dummy for frequency of being sad in the past 4 weeks being sometimes, often, or very often (as opposed to seldom or very seldom)
Satisfaction School	Dummy for satisfaction with overall school performance of at least 9 (on scale from 0, low, to 10, high)
Satisfaction Literature	Dummy for satisfaction with school performance in the subject German literature of at least 9 (on scale from 0, low, to 10, high)
Music	Dummy for being musically active
Sport	Dummy for doing sports
Relax	Dummy for doing nothing/relaxing/dreaming at least once a day

NOTE.—SOEPv32 waves 2005 to 2015.

TABLE A.2
BIG FIVE PERSONALITY TRAITS

I SEE MYSELF AS SOMEONE WHO…	
is original, comes up with new ideas	Openness to experience
values artistic experiences	Openness to experience
has an active imagination	Openness to experience
does a thorough job	Conscientiousness
does things effectively and efficiently	Conscientiousness
tends to be lazy (reversed)	Conscientiousness
is communicative, talkative	Extraversion
is outgoing, sociable	Extraversion
is reserved (reversed)	Extraversion
is sometimes somewhat rude to others (reversed)	Agreeableness
has a forgiving nature	Agreeableness
is considerate and kind to others	Agreeableness
worries a lot (reversed)	Emotional stability
gets nervous easily (reversed)	Emotional stability
is relaxed, handles stress well	Emotional stability

NOTE.—SOEPv32 waves 2005 to 2015, youth and adult questionnaires.

TABLE A.3
INTERNAL LOCUS OF CONTROL

How my life goes depends on me.
If I run up against difficulties in life, I often doubt my own abilities. (reversed)
Compared to other people, I have not achieved what I deserve. (reversed)
What a person achieves in life is above all a question of fate or luck. (reversed)
I frequently have the experience that other people have a controlling influence over my life. (reversed)
The opportunities that I have in life are determined by the social conditions. (reversed)
I have little control over the things that happen in my life. (reversed)

NOTE.—SOEPv32 waves 2005 to 2015, youth and adult questionnaires.

A.3 Summary Statistics

TABLE A.4
SUMMARY STATISTICS OF (NON-STANDARDIZED) PERSONALITY TRAITS

	Obs.	Mean	Std.Dev.	Mean		Eq. of Means
				Control	Treatment	t-stat
Openness	1,467	4.846	1.081	4.878	4.798	1.386
Conscientiousness	1,467	4.940	1.104	4.989	4.869	2.042
Extraversion	1,467	4.995	1.205	5.036	4.933	1.598
Agreeableness	1,467	5.397	0.885	5.391	5.406	-0.321
Emotional stab.	1,467	4.195	1.167	4.280	4.067	3.446
Locus of Control	1,351	5.017	0.805	5.012	5.026	-0.306

NOTE.—SOEPv32 waves 2005 to 2015, sample: adolescents and young adults aged 17 to 21 attending high school or with a high school diploma.

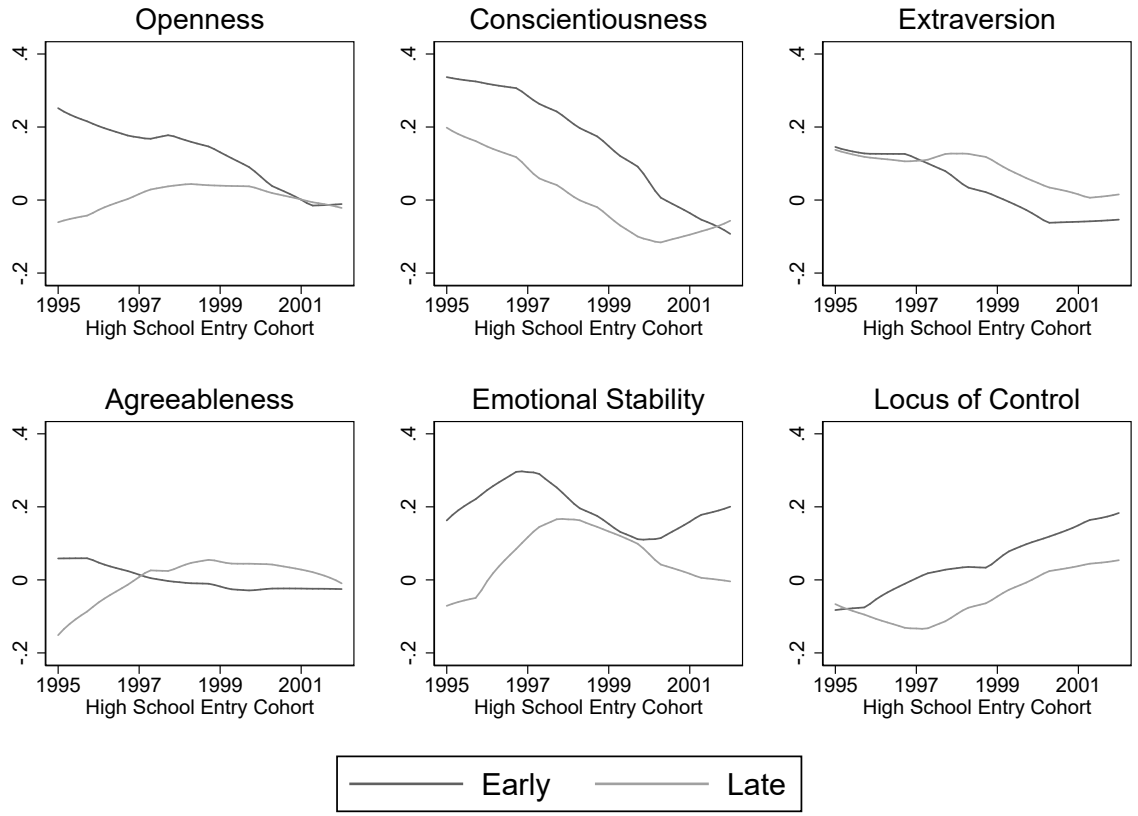
TABLE A.5
SUMMARY STATISTICS OF INDIVIDUAL CHARACTERISTICS

	Mean		Equality of Means
	Control	Treatment	t-stat
Age	18.124	17.353	10.938
Female	0.543	0.531	0.447
East	0.117	0.370	-12.026
Rural area	0.269	0.301	-1.324
Non-intact family	0.192	0.212	-0.926
Working-class father	0.199	0.205	-0.286
High parental education	0.579	0.574	0.180
Working mother	0.682	0.762	-3.339
Immigration background	0.174	0.158	0.822
Low-performing student	0.126	0.075	3.168
Observations	878	589	

NOTE.—SOEPv32 waves 2005 to 2015, sample: adolescents and young adults aged 17 to 21 attending high school or with a high school diploma.

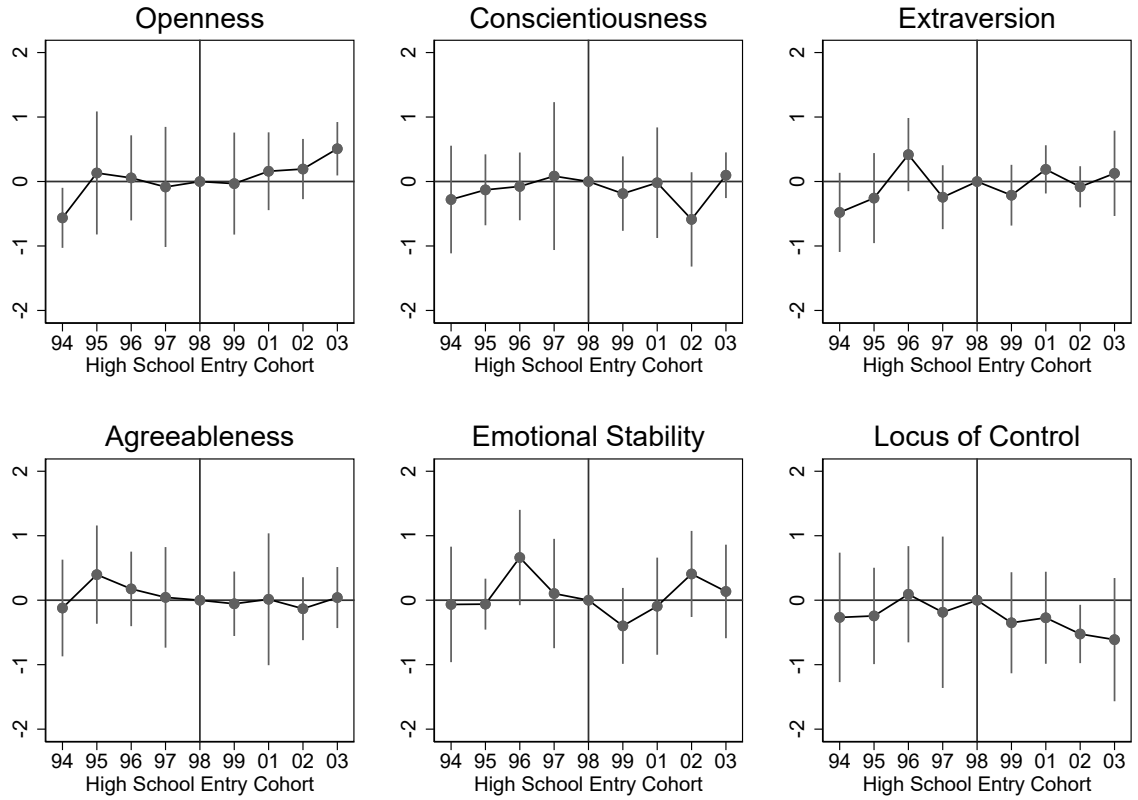
A.4 Figures

Figure 1: Pre-Reform Personality Traits over High School Entry Cohorts by Early and Late-Adopter States



NOTE.—SOEPv32 waves 2005 to 2015, including only pre-reform observations and cohort-state group combinations with at least 15 observations. Local polynomial fit with bandwidth 1.

Figure 2: Estimation of Pre-Reform Differentials between Early and Late-Adopter States



NOTE.—SOEPv32 waves 2005 to 2015, coefficients of early-adopter state interacted with the years of school entry between 1994 and 2003 from OLS regressions with 95% confidence intervals are displayed. In addition, the following controls are included: interactions for years of school entry 2004 to 2009, female, age, age squared, state specific linear time trends, and a maximum set of state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. Standard errors are clustered at the state level; to account for the small number of clusters, inference is based on the critical values of the t-distribution with 14 degrees of freedom.

A.5 Estimation Results

TABLE A.6
TIMING OF REFORM IMPLEMENTATION AND STATE CHARACTERISTICS

	OUTCOME VARIABLES	
	Late Implementation[1] (OLS) (1)	Year of Implementation[2] (Ordered Probit) (2)
Proportion of high school students	-0.035	-0.096
	(0.030)	(0.062)
Conservative government	0.100	-0.939
	(0.335)	(0.665)
Next Election in 2001 or 2002	-0.116	-0.309
	(0.327)	(0.640)
Median population age	-0.084	-1.042**
	(0.170)	(0.411)
GDP per capita	0.000	-0.000
	(0.000)	(0.000)
Constant	4.476	
	(7.037)	
R^2	0.219	
Pseudo R^2		0.230
Observations	15	15

NOTE.—The regressors are pre-reform state characteristics in the year 2000 from administrative data sources. Proportion of high school students (in percentage points)/median population age (in years)/GDP per capita (in euros): Federal Statistical Office; governing party/election dates: www.election.de. OLS, respectively ordered probit regressions. The indicator variable conservative government equals one if a state's prime minister is from the Christian Democratic Union (CDU), and zero otherwise. Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

[1]Dummy variable for late-adopter state (as defined in Section 6.5), that is, first cohort affected graduating in 2012 or later.

[2]Year of implementation of the reform ranging from 1 (2001) to 7 (2007). Saxony and Thuringia are coded as category 0.

TABLE A.7
EFFECTS OF THE REFORM (CONTROLLING FOR PARENTAL PERSONALITY)

	OUTCOME VARIABLES: PERSONALITY TRAITS					
	Open. (1)	Consc. (2)	Extrav. (3)	Agree. (4)	Emot. (5)	LoC (6)
Reform	-0.028	-0.085	0.032	0.102	-0.364***	-0.020
	(0.175)	(0.143)	(0.144)	(0.158)	(0.106)	(0.122)
Open. (Mother)	0.267***					
	(0.045)					
Open. (Father)	0.151***					
	(0.034)					
Consc. (Mother)		0.133***				
		(0.027)				
Consc. (Father)		0.177***				
		(0.030)				
Extrav. (Mother)			0.180***			
			(0.034)			
Extrav. (Father)			0.125***			
			(0.027)			
Agree. (Mother)				0.184***		
				(0.040)		
Agree. (Father)				0.089***		
				(0.026)		
Emot. (Mother)					0.226***	
					(0.040)	
Emot. (Father)					0.087**	
					(0.036)	
LoC (Mother)						0.174***
						(0.046)
LoC (Father)						0.144**
						(0.064)
Observations	1,175	1,178	1,177	1,177	1,177	1,027

NOTE.—SOEPv32 waves 2005 to 2015. OLS regressions. The following controls are included: female, age, age squared, state specific linear time trends, and a maximum set of state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. The respective personality traits of the parents are age-standardized and averaged over all available observations, 3 at maximum, to reduce measurement error. Standard errors, reported in parentheses, are clustered at the state level; to account for the small number of clusters, inference is based on the critical values of the t-distribution with 14 degrees of freedom. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

TABLE A.8
SENSITIVITY ANALYSES 6.1—ESTIMATION MODEL

	OUTCOME VARIABLES: PERSONALITY TRAITS					
	Open. (1)	Consc. (2)	Extrav. (3)	Agree. (4)	Emot. (5)	LoC (6)
	A. Wild cluster bootstrap[1,3]					
Reform	-0.034	-0.057	-0.023	0.137	-0.324*	0.022
	(0.744)	(0.676)	(0.860)	(0.408)	(0.052)	(0.780)
Observations	1,467	1,467	1,467	1,467	1,467	1,351
	B. Ordered Probit Estimation[2]					
Reform	-0.059	-0.070	-0.018	0.116	-0.351**	0.027
	(0.124)	(0.157)	(0.108)	(0.118)	(0.130)	(0.097)
Observations	1,467	1,467	1,467	1,467	1,467	1,351

	Outcome Variables: Overall Measure of Personality			
	Positive direction		Effect direction	
	Big 5	Big 5+LoC	Big 5	Big 5+LoC
	C. Accounting for the multiple hypotheses testing[1,4]			
Reform	-0.065	-0.059	0.125**	0.117***
	(0.088)	(0.074)	(0.056)	(0.038)
Observations	1,467	1,351	1,467	1,351

NOTE.—SOEPv32 waves 2005 to 2015. The following controls are included: female, age, age squared, state specific linear time trends, and a maximum set of state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. Standard errors, reported in parentheses, are clustered at the state level; to account for the small number of clusters in panels B and C, inference is based on the critical values of the t-distribution with 14 degrees of freedom. * p<0.1, ** p<0.05, *** p<0.01.

[1]OLS regressions.

[2]Ordered Probit estimation with the standardized personality measures as categorical outcomes.

[3]In parentheses p-values are reported instead of standard errors. These are obtained through wild cluster bootstrapping following Cameron et al. (2008).

[4]Overall personality measures obtained following Anderson (2008) to avoid the problem of multiple hypotheses testing. Positive direction includes all measures as displayed throughout this paper; effect direction reverses openness, conscientiousness, extraversion, and emotional stability according to the sign of the coefficients in the main estimation.

TABLE A.9
SENSITIVITY ANALYSES 6.2-6.3—AGE EFFECTS AND MEASUREMENT ISSUES

	OUTCOME VARIABLES: PERSONALITY TRAITS					
	Open. (1)	Consc. (2)	Extrav. (3)	Agree. (4)	Emot. (5)	LoC (6)
	A. Personality traits obtained as age-free residuals					
Reform	-0.034	-0.061	-0.026	0.122	-0.368**	0.017
	(0.141)	(0.169)	(0.139)	(0.110)	(0.141)	(0.074)
Observations	1,467	1,467	1,467	1,467	1,467	1,351
	B. Controlling for having graduated[1]					
Reform	-0.010	-0.069	-0.006	0.144	-0.318**	0.022
	(0.122)	(0.160)	(0.118)	(0.120)	(0.126)	(0.092)
High school diploma	-0.372	0.190	-0.252	-0.100	-0.087	0.097
	(0.227)	(0.137)	(0.162)	(0.156)	(0.167)	(0.176)
Observations	1,467	1,467	1,467	1,467	1,467	1,351
	C. Controlling for distance to graduation[1,2]					
Reform	0.010	-0.023	0.053	0.121	-0.233*	0.014
	(0.131)	(0.165)	(0.118)	(0.143)	(0.110)	(0.115)
Distance	-0.008	0.004	-0.012**	0.003	-0.005	0.006
	(0.008)	(0.006)	(0.005)	(0.006)	(0.008)	(0.007)
Distance^2	-0.000	0.000	-0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,467	1,467	1,467	1,467	1,467	1,351

NOTE.—SOEPv32 waves 2005 to 2015. OLS regressions. The following controls are included: female, state specific linear time trends, and a maximum set of state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. Standard errors, reported in parentheses, are clustered at the state level; to account for the small number of clusters, inference is based on the critical values of the t-distribution with 14 degrees of freedom. * p<0.1, ** p<0.05, *** p<0.01.

[1]In addition, we include age and age squared.

[2]Distance to graduation is measured in months, with negative values indicating an interview prior to graduation and positive values indicating an interview post graduation.

TABLE A.10
SENSITIVITY ANALYSES 6.5-6.7—SELECTIVITY, DOUBLE COHORT, AND INSTITUTIONAL CHANGES

	OUTCOME VARIABLES: PERSONALITY TRAITS					
	Open. (1)	Consc. (2)	Extrav. (3)	Agree. (4)	Emot. (5)	LoC (6)
	A. Subsample: Late-adopter states[1]					
Reform	0.063	0.114	-0.151	0.055	-0.277**	0.235
	(0.167)	(0.130)	(0.167)	(0.254)	(0.097)	(0.143)
Observations	835	835	835	835	835	769
	B. Subsample: States without comprehensive schools[2]					
Reform	0.030	-0.336*	0.285*	0.145	-0.370	-0.077
	(0.250)	(0.167)	(0.120)	(0.165)	(0.219)	(0.139)
Observations	783	783	783	783	783	726
	C. Subsample: Exclusion of double graduating cohort					
Reform	0.039	-0.235	0.059	-0.110	-0.388***	0.090
	(0.113)	(0.142)	(0.170)	(0.137)	(0.124)	(0.146)
Observations	1,238	1,238	1,238	1,238	1,238	1,161
	D. Subsample: States with standardized examinations established[3]					
Reform	0.029	-0.224	0.202	0.193	-0.474**	-0.067
	(0.275)	(0.142)	(0.107)	(0.166)	(0.193)	(0.116)
Observations	635	635	635	635	635	596
	E. Subsample: Exclusion of Saxony and Thuringia					
Reform	-0.057	-0.018	0.022	0.143	-0.302*	0.012
	(0.128)	(0.165)	(0.104)	(0.132)	(0.139)	(0.093)
Observations	1,333	1,333	1,333	1,333	1,333	1,226

NOTE.—SOEPv32 waves 2005 to 2015. OLS regressions. The following controls are included: female, age, age squared, state specific linear time trends, and a maximum set of state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. Standard errors, reported in parentheses, are clustered at the state level; to account for the small number of clusters, inference is based on the critical values of the t-distribution with 14 degrees of freedom. * p<0.1, ** p<0.05, *** p<0.01.

[1]These are states where the first students affected by the reform graduate in 2012 or later, that is, Baden-Wuerttemberg, Bremen, Hesse, North Rhine-Westphalia, Berlin, Schleswig-Holstein, and Brandenburg.

[2]The states where comprehensive schools typically do not exist are Baden-Wuerttemberg, Bavaria, Lower Saxony, Mecklenburg-West Pomerania, Saxony, Saxony-Anhalt, and Thuringia.

[3]These states with standardized high school graduation examinations long in place are Baden-Wuerttemberg, Bavaria, Mecklenburg-West Pomerania, Saarland, Saxony, Saxony-Anhalt, and Thuringia.

TABLE A.11
SENSITIVITY ANALYSES 6.8—PLACEBO ESTIMATION

	OUTCOME VARIABLES: PERSONALITY TRAITS					
	Open. (1)	Consc. (2)	Extrav. (3)	Agree. (4)	Emot. (5)	LoC (6)
	Sample: Students from other school tracks					
Reform	0.096	0.080	-0.100	0.072	0.051	-0.029
	(0.082)	(0.153)	(0.109)	(0.089)	(0.128)	(0.143)
Observations	1,808	1,808	1,808	1,808	1,808	1,621

NOTE.—SOEPv32 waves 2005 to 2015. OLS regressions. The following controls are included: female, age, age squared, state specific linear time trends, and a maximum set of state dummies, year of school entry dummies, dummies for the different SOEP samples, and a constant. Standard errors, reported in parentheses, are clustered at the state level; to account for the small number of clusters, inference is based on the critical values of the t-distribution with 14 degrees of freedom. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

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