Unintended Yet Effective: Evidence from Policy Reforms in Early Childhood Education and Care, Education and Environmental Economics

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Chapter 1

General Introduction

1.1 Motivation

"Faust: Nun gut wer bist du denn?
Mephistopheles: Ein Theil von jener Kraft,
Die stets das Böse will und stets das Gute schafft."[1]
Johann Wolfgang von Goethe: Faust - Der Tragödie erster Teil, 1808

In his influential work "The Unanticipated Consequences of Purposive Social Action" the American sociologist Merton (1936) coined the concept of unintended[2] consequences, consolidating a long history of thought by economic theorists such as John Locke, Adam Smith, and Max Weber to name only a few. Adam Smith's "invisible hand" stands metaphorically for the liberal economic principle that selfish behavior leads unintentionally to a beneficial public turnout, and thus, the case of unintended consequences with positive benefits. The opposite case with perverse outcomes is framed by Merton (1936) as the "paradox of social action - the

[1]The English translation according to https://www.poetryintranslation.com/PITBR/German/FaustIScenesItoIII.php (last accessed: 09/13/2022) is:

"Faust: Well, what are you then?
Mephistopheles: "Part of the Power that would Always wish Evil, and always works the Good."

[2]Unintended and unanticipated consequences are here used as synonyms.

'realization' of values may lead to their renunciation" (p.903). He thus converts Goethe's Mephistopheles to "Die Kraft, die stets das Gute will, und stets das Böse schafft."

Searching for use-cases leads to a battery of policy examples[3] from varying fields and scope of (mostly negative) consequences, which often come along large societal costs. For example, Short et al. (2018) assess the unintended negative result of providing mosquito nets in many parts of the developing world to combat malaria. Contrary to its designated purpose these nets have been largely used as fishing nets, leading to the occurrence of unsustainable fish stock due to dramatic over-fishing.

Merton (1936) points in his essay to five causes for the occurrence of unintended consequences.[4] First, they might be due to ignorance (1) or second, caused by errors in analyzing the problem (2). Third, unintended consequences might stem from short-term interests winning over long-term gains (3), as for example politicians whose primary goal is to be reelected are more interested in short-term effects and thereby potentially overlooking long-term consequences. Fourth, they might be caused by "basic values" that call for certain actions irrespective of potential (un-)favourable outcomes (4) or fifth, consequences might not occur due to the self-defeating prophecy[5] that by trying to tackle a certain problem before it even exists prevents it from happening (5).

The "methodological pitfall" behind this concept is well-known to the applied economist interested in causal inference, namely "the problem of causal imputation, the problem of ascertaining the extent to which 'consequences' may justifiably be attributed to certain actions." He further argues that "this ever-present difficulty of causal imputation must be solved for every empirical case which is studied" (Merton, 1936, p.897).

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[3] The Wikipedia article on unintended consequences compiles numerous examples, see `https://en.wikipedia.org/wiki/Unintended_consequences` for more.

[4] In Table 1.1, I use this framework to categorize my own work accordingly.

[5] Merton gives the example for the prophet's dilemma of Karl Marx's prediction of a growing wealth concentration which helped to organize labor in the 19th century and thus, slowing or even self-defeating his own prediction.

This dissertation collects three such empirical cases that detect for distinct policy areas arguably causal relationships by tying certain actions to various unintended outcomes. Although each article is self-contained, unintended consequences run like a common thread through all chapters along with the endeavor to approach the problem of causal imputation with the help of state-of-the-art micro-econometric techniques of the applied economics. For the applied economist in social science policy reforms are a key source for conducting (ex-post) causal inference[6]. By changing laws or regulations, policy reforms can help to form a treatment group consisting of individuals who are exposed to the change in regulation. Their outcomes can be then compared to a control group for which the assumption should hold that the latter mimics the unobserved counterfactual outcome of what would have happened without the intervention.[7]

In the following section, I give a brief overview of each chapter and discuss the extent to which my unintended, yet effective findings enrich the respective literature, while section 1.3 summarizes general implications.

1.2 Overview and discussion

This dissertation consists of three independent empirical research articles (see Figure 1-1 for a graphical overview). Table 1.1 summarizes for each article its research question, the applied data set, the methodological approach as well as the main findings. For the overarching discussion, I also propose a classification of the analyzed outcomes according to the delineated framework by Merton (1936).[8]

[6]Policy reforms that provide quasi-experimental settings form a suitable alternative to randomized controlled trials (RCT) which are predominantly conducted in medical research but come along high costs as well as ethical and other methodological concerns.

[7]This workhorse method of difference-in-differences analysis is applied in Chapters 2 and 3.

[8]Even though his framework is set to explain negative unanticipated consequences, it may also apply to positive consequences.

Table 1.1: Overview of different chapters

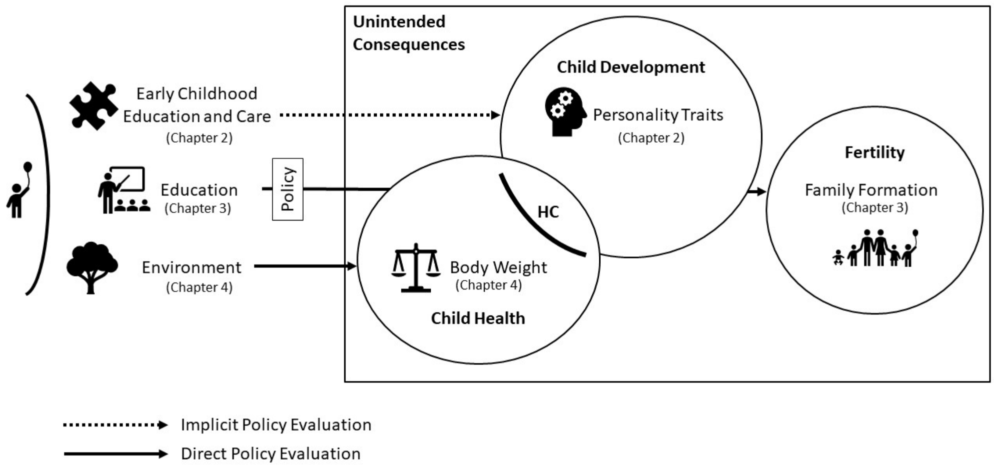
	Chapter 2	**Chapter 3**	**Chapter 4**
Title	Long Run Effects of Universal Childcare on Personality Traits	The Length of Schooling and the Timing of Family Formation	Green Cities, Healthier Children: The Effect of Urban Green Space on the Body Weight of Primary School Starters
Research question	Does an earlier entry into childcare affect children's personality traits in the long-run at adolescent age?	Does shortening of the education phase impact on the timing of marriage and childbearing?	Can expanding urban green space affect children's health in terms of body weight problems? Are impacts higher for high-obesity risk groups?
Data	NEPS, SOEP, various datasets from statistical offices	Micro Census, SOEP, DJI Family Survey, various datasets from statistical offices	ESU Data Berlin-TS, "Grün Berlin" park use data, various datasets from statistical offices
Method	Instrumental Variable	Difference-in-Differences	Difference-in-Differences, Event Study Techniques, Synthetic Control, Entropy Balancing
Main findings	We instrument an earlier entry in universal childcare with geographically varying supply of childcare places following the first roll-out reform of the German childcare system in 1996 and find that an earlier entry in universal childcare increases extroversion in adolescence at age 15.	We exploit the introduction of short school years in Germany in 1966-67, which compressed the education phase without affecting the curriculum, and find that exposure affects the timing of marriage for all secondary school tracks and shifts forward childbearing mainly for academic-track graduates.	I find that expanding urban green space in Germany's capital city decreases the probability to be overweight (BMI $>$ 90P) for children who live in close proximity to the park, mainly driven by girls and children from foreign cultural backgrounds. Children with lower maternal education have a higher probability to be obese (BMI $>$ 97P.)
Classification Merton (1936)	(1), (3)	(1), (4)	(1)

Source: Own compilation.

In Chapter 2, we ask the question whether an early entry in universal childcare compared to an entry one year later (at age three versus age four) has long-term consequences on child development, i.e. personality traits. In Figure 1-1, the relationship between early childcare influencing personality traits is illustrated by the dashed arrow. The policy framework that we exploit for causal inference in this study implicitly relates to the first roll-out law[9] of the German childcare system. As of 1996, the German government introduced a legal claim to a place in universal childcare for children aged 3-6. The cut-off rule that was imposed in reaction to the legal claim by municipalities with less abundant supply of childcare places implied that children turning three after the beginning of the school year had to wait almost another year to enter childcare. This particular institutional framework in combination with a high level of geographic variation in childcare supply in western Germany at the time lends itself to an instrumental variable (IV) approach. Thus, the local childcare supply, measured by the slot-child-ratio on the county level, is a highly relevant predictor of the probability to enter childcare early, but arguably exogenous to personality trait development. By using two representative data sets, the National Educational Panel Study (NEPS) and the Socio-Economic Panel (SOEP), we find that an earlier entry in childcare increases extroversion in adolescence. Borrowed from the neighbouring discipline of psychology, the item extroversion from the set of non-cognitive personality traits is an outcome of particular economic interest as it entails high predictive power for future labor market performance (Fletcher, 2013).

[9]The second significant follow-up regulation became effective in 2008 with the law on support for children ("Kinderförderungsgesetz"), committing German federal states to a gradual expansion of childcare supply for children below three years. This law also included the expansion of the legal claim to a subsidized childcare slot to the age group one to three years by August 2013.

Figure 1-1: Structural Visualization of Thesis



Notes: This figure visualizes outcome bubbles.

Viewed against the backdrop of Merton (1936)'s framework, I argue that the analyzed outcome can be labeled as unintended due to two reasons, ignorance (1) and in a wider sense short-term gains winning over long-term interests (3). First, policy involvement in the 1990s to roll-out the German childcare system was mainly intended to provide childbearing incentives after the Federal Constitutional Court had decided upon a revision of the abortion law in 1992[10]. Consequently, positive long-term effects on children's personality traits belong to the type of consequences that were clearly not foreseen at the time of policy action. The second classification is not as straightforward as positive consequences usually do not impose a trade-off between intended short-term gains and unintended long-term interests. However, not taking positive long-term consequences into account underestimates the effect policy-makers attribute to early education when only looking at short-term

[10]For the law text ("Schwangeren- und Familienhilfegesetz") see http://www.bgbl.de/xaver/bgbl/start.xav?startbk=Bundesanzeiger$_B$GBl&jumpTo=bgbl192s1398.pdf (last accessed: 09/22/2022).

gains in accordance with their intention to be reelected in the next election. This might help to explain, why investments in public early childhood education and care are still relatively low.[11] Hence, providing empirical evidence on long-term consequences allows policy-makers to more efficiently factor in the economic potential on early childhood investments for which Nobel laureate James Heckman[12] identified diminishing returns with respect to children's age (e.g. Heckman et al., 2006). In Figure 1-1, the downward-sloping concave Heckman curve (labeled with "HC") reflects this idea of child development as economic production process with highest economic returns stemming from the earliest investments in children. In sum, analyzing unintended long-term consequences and as in this paper detecting beneficial and economically relevant outcomes caused by an early exposure to universal childcare can be highly informative for future ambitions to increase public expenditure in this policy domain.

In chapter 3, we analyze with Micro Census data whether changing the duration of the education phase, which typically precedes family formation, impacts on the timing of the latter. We apply a different methodological approach by exploiting the exogenous nature of a policy intervention in the German educational system with difference-and-differences (DiD) techniques (marked with a solid arrow in Figure 1-1). The policy reform under study is the introduction of short school years (SSYs) in Germany in 1966-67[13] which shortened the education phase, but did not affect the curriculum. We find a duration effect that for treated individuals who were exposed to short school years the timing of marriage is shifted forward

[11] The OECD Family Database reports for Germany and the year 2017 an annual public expenditure rate of 0.67 percent of GDP, which is less but close to the OECD average of 0.74 percent of GDP. For the full data sheet, see
https://www.oecd.org/els/soc/PF3$_1$Public$_s$pending$_o$n$_c$hildcare$_a$nd$_e$arly$_e$ducation.pdf
(last accessed: 09/02/2022).

[12] See also a compilation of associated work on https://heckmanequation.org/ (last accessed 08/31/2022).

[13] Pischke (2007) and Braakmann (2010) have used this framework to study duration effects on human capital outcomes, such as wages and employment, and health outcomes and do not find any detrimental reform effects on either set of outcomes.

irrespective of the attended secondary school track. The timing of the first child, however, is only affected and moved forward for academic-track students.

From an economic point of view, our findings are positive consequences as they potentially lower social costs (e.g. Billari et al., 2006; Díaz-Giménez and Giolito, 2013; Gustafsson, 2001; Larsen and Vaupel, 1993) associated with later childbearing in industrialized countries. With respect to Merton (1936)'s thinking, this reasoning and the timing of family formation has clearly not been subject to debate in the KMK (1962)'s discussion on reforming the school year. Thus, the assessed outcomes classify as unintended due to lack of knowledge and foresight (1). In addition, during the debate of shortening school years in order to harmonize the German school system it can be argued that the goal of a joint start of the school year in Germany is considered a "basic value" that calls for policy reform irrespective of the potential consequences (4). This empirical case, however, provides evidence in favor of incorporating yet overlooked or ignored demographic consequences in current and future debates about altering the education phase.[14]

In chapter 4, I apply the same identification strategy (also visualized with a solid arrow in Figure 1-1) but a different quasi-experimental setting in the context of the urban built environment to test its potential mitigating effects on child health[15] measured in terms of being overweight or even obese. The policy reform is about transforming former airport grounds in Germany's capital city to a large urban green space, the so-called "Tempelhofer Feld". I shed light on a causal relationship yet overlooked in the literature by comparing several weight outcomes, based on the body mass index, of treated children living within close proximity to the park to children living further away before and after park transformation. I use new administrative panel data on the district level of Berlin from mandatory school entrance examinations covering the full universe of local school starters and objectively measured body height and weight outcomes. I find that park opening

[14]Examples for such discussions are the European Bologna reform (Hahm and Kluve, 2019) or the German G8 reform (Marcus and Zambre, 2019).

[15]There is evidence that the reasoning behind the Heckman Curve also applies to child health, as investing in child health early is more effective than later on (e.g. Belli and Appaix, 2003).

causally reduces the prevalence of overweight children, driven by girls, children from foreign cultural backgrounds and children with less childcare exposure. I also find small, but significant park-induced effects lowering the probability to be obese for children with lower maternal education background.

This research set-up adds a new economic perspective for environmental studies on public green space which have so far mainly looked at economic outcomes such as house prices (e.g. Diao et al., 2017; Baum-Snow and Kahn, 2000). To the best of my knowledge, the lengthy discussion around opening the former airport grounds to recreational use of citizens prior to transformation did not include potential positive consequences in terms of children's health, i.e. reduced weight problems. Hence, unintended consequences again accrue due to lack of knowledge (1) with respect to Merton (1936)'s framework. The results from this third empirical case bridge this knowledge gap and add another argument to the compilation by Brenck et al. (2021) of the societal value attributed to the Tempelhofer Feld. This might enable policy-makers in the future to better price in effective consequences of providing urban green space on child health.

1.3 Contribution

In sum, the delineated empirical cases of this thesis solve knowledge deficiencies in three important domains around children's growing of age. By looking at new and/or interdisciplinary outcome variables, I enrich the respective applied economics literature with my findings of overall positive unintended consequences causally tied to policy action in the German institutional context.

Collecting empirical evidence from Germany is of particular interest, as the institutional context matters for causal inference. For example, the first chapter expands the literature on child development mainly from the U.S. and targeted educational programs (e.g. Perry Preschool Program, Head Start), whose implications cannot be transferred to the German institutional context unconditionally.[16]

The applied researcher's challenge to establish causal inference by using the most credible identification strategy for the research question of interest with the best-suited available data is met by using a variety of microeconometric techniques (IV in chapter 2, DiD in chapters 3 and 4 as well as event studies and additional pre-processing techniques of entropy balancing and synthetic control groups in chapter 4) in combination with a wide range of data sets with different strengths and weaknesses outlined in more detail in each chapter (see Table 1.1 for an overview). The advantage of the applied methods is that conditional on certain assumptions they are all robust to selection on unobservable factors which is often a source of substantial bias in empirical work.

Applying the framework by Merton (1936) to think about the sign, nature and implications of unanticipated consequences from public policy action as conducted in this chapter is also a useful exercise, in particular since policy-making in modern democracies has become highly multidimensional and increasingly complex. His systematic approach to pin down the elements of unanticipated consequences is therefore not meant to "examine exhaustively the implications (...) for social prediction, control and planning" (Merton, 1936, p.904). But his reasoning goes in the same direction as the growing interest in more evidence-based policy-making in social policy calling for rigorous evaluation and continuous adjustment of policies (Baron, 2018).

Taken together, this dissertation provides three empirical use-cases from the German policy context that invite policy-makers to account for yet overlooked causal relationships with high policy relevance. Future research should continue to study relevant unintended, yet potentially effective consequences from various disciplines and policy actions to help striving for the ultimate policy goal "jener Kraft, die stets das Gute will und stets das Gute schafft."

[16] See Spiess (1998) for a detailed comparison between the U.S. and the German childcare system.

Chapter 2

Long Run Effects of Universal Childcare on Personality Traits[1]

Abstract

Although universal childcare has become an essential tool to support child development, few economic studies analyze its effects on non-cognitive skills and little is known about effects on these skills in the long run. In this paper we go beyond short run analyses and examine the long run effects of one additional year of universal childcare on students' personality traits in adolescence as part of their non-cognitive skills set. As of 1996, a legal entitlement to universal childcare applied to children of three years and older in Germany. However, severe shortages in the former-West meant that many children could not get a childcare place and had to wait a full year until the next entry date. Using data from the National Educational Panel Study (NEPS) we estimate effects of one additional year of childcare on personality traits exploiting geographical variation in the timing of care entry at age three and older in West-Germany. We complement our analyses with an additional nationwide and representative data set, the Socio-Economic Panel (SOEP). We find that an earlier entry in universal childcare increases extroversion in adolescence, which has been shown to be associated with favorable labor market outcomes.

2.1 Introduction

Universal provision of childcare has become an essential policy tool to both increase parental employment opportunities and support child development in many

[1]This chapter is joint work with Maximilian Bach and Frauke Peter. An earlier version of this paper circulated under the title 'Early Childcare Entrance and Personality Traits'.

European countries, and recently also in the U.S. with the announcement of a large-scale push for subsidized childcare. The effectiveness of such an early educational investment is often rationalized by the skill formation process of "dynamic complementarities" in which skills attained early beget skills later, and make human capital investment more productive (e.g., Cunha et al., 2006; Cunha and Heckman, 2007). Studying developmental effects of different exposures to stimulating learning environments is of particular interest, as children face many new challenges on account of globalization, technological changes, and demographic ageing. Given that childcare centers have become increasingly important for the development of skills outside the family environment (Kautz et al., 2014) attending childcare early on may makes a difference for children's skill development. Mastering these emerging challenges requires not only cognitive skills, but increasingly also a broad variety of skills beyond those measured with the typical cognitive tests (Heckman and Kautz, 2012; Kautz et al., 2014; Lechner et al., 2019).

In this study we build on and extend the existing literature of childcare effects on non-cognitive skills. Although studies show that non-cognitive skills impact cognitive skills, but not vice versa, and that these non-cognitive skills are as important as cognitive skills regarding school performance (Cunha and Heckman, 2007; Heckman et al., 2006), still little is known about how non-test score outcomes, such as personality traits, evolve later in life if children have access to universal childcare early on. Furthermore, Heckman et al. (2013) show for the Perry Preschool intervention in the U.S. that non-cognitive skills explain more of the variance in later outcomes than cognitive skills. The same is true for findings from another U.S. intervention, project STAR, a kindergarden class size experiment for children at age five, which also reveals a long run causal impact on non-cognitive skills (Chetty et al., 2011; Bietenbeck, 2020). All the same, these results cannot necessarily be transferred to universal childcare services as findings show from Norway and the Canadian province of Quebec (Havnes and Mogstad, 2011; Baker et al., 2019). In this paper, we bridge this gap in the economic literature and investigate the effect of attending universal childcare one year earlier on personality traits at age fifteen as important predictors of later educational achievements, health outcomes, and labor market success (e.g., Fletcher, 2013; Blanden et al., 2007; Brunello and Schlotter, 2011; Baron and Cobb-Clark, 2010; Heckman et al., 2013; Caliendo et al., 2015; Prevoo and ter Weel, 2015).

In order to identify causal effects on long-run personality development, we utilize geographical variation in childcare availability across counties in West-Germany. This variation stems from a reform in the mid-1990s, which led to an increase in the availability of childcare slots for children aged three and older. The period of rolling-out the childcare system along with the introduction of a legal claim to a childcare slot for children aged three and older translated into a substantial rise in childcare attendance rates for this age group up until today. In 1991, only 33% of three-year-olds attended childcare in the western German federal states (see Figure 2-1) compared to 94% in 2018 (Jessen et al., 2018). Although in our data framework we are constrained on exploiting geographical variation, we have detailed information on individual-level controls to provide evidence that the implementation of the reform is uncorrelated with local (employment) trends. Instrumenting childcare attendance using regional variation in childcare provision has been used before in this literature. For example, Datta Gupta and Simonsen (2010) exploit variations in supply of childcare provision across municipalities in Denmark to compare two types of childcare and their influence on non-cognitive skills at the age of eleven. For identification we exploit the institutional context of rationing in the mid-1990s in the German childcare market, where despite a legal claim even newly created slots were immediately filled due to excess demand. We are aware that two studies for Germany exploit differences in the timing of childcare expansion across municipalities to estimate heterogeneous short-run effects of childcare on children's school readiness just before they enter primary school (Felfe and Lalive, 2018; Cornelissen et al., 2018) using a marginal treatment effects approach. We argue that the estimates of overall local-average-treatment effects (LATE) documented in this study bear additional insights, since it is less clear looking at children's personality traits how childcare attendance might determine these in the long-run. In addition, by focusing on the LATE effects we estimate the likely effect of expanding universal public childcare. Furthermore, our data sets allow to control for a richer set of information on the individual family level.

This paper uses data from the German National Educational Panel Study (NEPS) (Blossfeld et al., 2011) and the German Socio-Economic Panel Study (SOEP) for long-run analyses on universal childcare provision, specifically early attendance, in West-Germany. We find that attending universal childcare earlier affects personality traits of children at age fifteen. Our results suggest a sizable increase of 0.37 SD in the personality trait extroversion for children who are induced to enter childcare one year earlier because of a larger supply of childcare places in their county. This result is robust against the inclusion of a rich set of family background and regional characteristics. For the other personality trait dimensions we observe a rather mixed pattern of results. We further cross validate our results with a comparable, representative sample of slightly older students from different birth cohorts from the SOEP. These data allow to control for an even more extensive set of family characteristics, including parents' personality traits, but include considerably fewer observations. Reassuringly, we find a similar effect on extroversion in the SOEP data, corroborating the results from the NEPS.

We confirm our main finding in a series of robustness analyses. The effect of attending childcare one year earlier remains significant when we use the slot-child-ratio at the county-level in 1994 instead of 1998. This is a measure of supply from the year prior to the birth of the fifteen year-olds who were mostly born in 1995 and 1996. In addition, we estimate the effect of early childcare entry on arguably predetermined outcomes of children. These are height at birth, height at adolescence, low birth weight, and premature birth. For all four outcomes we find no effects of entering childcare earlier. This makes us confident that the slot-child-ratio is 'exogenous' to any other factors influencing students' extroversion.

The paper contributes to the literature in two dimensions. First, we add to the vibrant strand of research that examines non-cognitive skills as relevant input factors of later life outcomes as well as output factor of educational investment (see for an overview Kautz et al., 2014; Lechner et al., 2019). Studies in the economic literature show an increase in the likelihood of transiting to secondary schooling, college graduation, and higher wages for individuals with trait sets 'emphasizing' openness, conscientiousness, or extroversion (Mueller and Plug, 2006; Borghans et al., 2008; Baron and Cobb-Clark, 2010; Heineck and Anger, 2010; Almlund et al., 2011; Lundberg, 2013; Fletcher, 2013). Fort et al. (2020) find negative impacts of one additional childcare month at age 0-2 on personality traits studying a relatively affluent population in Bologna, and thus a counterfactual home environment of potentially higher quality and more one-to-one interactions. In contrast, we study long-run outcomes of an older age group of children aged 3-4 in a universal access to subsidised childcare setting using representative data. Although, similar to other countries with universal childcare provision like Denmark (Heckman and Landersø, 2022), usage is far from universal, the counterfactual to early childcare attendance is usually later attendance, and not abstaining from childcare all together.[2]

Second, we contribute to the literature that examines the impact of early childhood education and care on children's skill development and educational success. So far, the economic literature has mainly shown short-run effects of childcare attendance on cognitive and non-cognitive skills (amongst others Magnuson et al., 2007; Loeb et al., 2007; Baker et al., 2008; Berlinski et al., 2009; Brilli et al., 2011; Datta Gupta and Simonsen, 2010, 2012; Drange et al., 2016; Peter et al., 2016; Gomajee et al., 2021; Felfe and Lalive, 2018; Cornelissen et al., 2018; Fort et al., 2020). Fewer studies analyze the causal effect of universal childcare on outcomes in the medium or long run, looking at educational attainment, income, or the need for social assistance (e.g. Dumas and Lefranc, 2010; Havnes and Mogstad, 2011; Apps et al., 2013; DeCicca and Smith, 2013; Fessler and Schneebaum, 2019; Baker et al., 2019; Gray-Lobe et al., 2021). One explanation for this gap in the literature is that analyzing long-run outcomes requires rich and long-run data sources. Consequently, the literature is still growing often combining analyses of survey and administrative data. Among these emerging literature one recent study that

[2]Existing evidence shows that even small advances in the timing of the start of early childcare, e.g., from age 19 months to age 15 months in Drange and Havnes (2019), have pronounced positive effects on child development.

looks at long-run returns to childcare attendance on personality traits is Kuehnle and Oberfichtner (2020). They do not find any effect of early childcare attendance on non-cognitive skills also using NEPS data with a fuzzy-regression discontinuity design. They exploit the fact that in regions with sufficiently many childcare slots, childcare centers often gave priority to children who had not turned three yet, but who would turn three within the calendar year. Therefore, Kuehnle and Oberfichtner (2020) compare children who enter even earlier than mandated by the legal claim to children who enter on time at age three. We argue based on the institutional landscape at that time of severe rationing of childcare places that our identification strategy captures effects that are more representative for the population at large. Our approach mostly compares children who enter childcare on time (in the year they turn three) with children who enter one year later due to a shortage of places. Hence, compliers in our setting are likely to be children of parents with a weaker preference for public childcare compared to Kuehnle and Oberfichtner (2020)'s group of compliers. Thus, by combining survey data with administrative data on childcare supply as well as on regional information across counties on demand side characteristics, such as female employment rates, unemployment rates, and share of conservative votes, we are able to establish that early childcare attendance affects personality traits at age fifteen. In particular, we find that students who attend childcare one year earlier are more likely to be communicative and assertive.

The remainder of the paper is structured as follows: Section 2.2 describes the institutional setting. Section 2.3 outlines the empirical strategy and Section 2.4 describes the data in more detail. This is followed by the results in Section 2.5 and further analyses in Section 2.6. Section 2.7 presents several robustness tests before Section 2.8 concludes.

2.2 Institutional background

In this paper, we examine the potential long-run effect of attending childcare for one additional year using the county-level variation in childcare slots during the mid-1990s. We restrict our analysis to the western federal states of Germany, since the institutional setting of former-East Germany differed with respect to childcare related norms and values compared to former-West German states. Former-East German counties guaranteed almost full provision of childcare for all children from age one until school start as a relic from the German Democratic Republic (GDR) regarding childcare as a key pillar of socialistic policy (see Figure 2-1 for a comparison of attendance rates between West and East Germany in the 1990s). We focus on children aged three years and older who attend universal childcare also known under the widespread term "Kindergarten".[3] Whereas kindergarden in the United States starts elementary school and aims at five year old children, the German "Kindergarten"[4] comprises both care and education in institutions for children aged three and older (until school starting age which most children enter at age six in Germany) in mainly age-mixed groups. In comparison, the German universal childcare system comprises on average higher quality care and nearly all childcare institutions are publicly funded, leaving little market power for private suppliers (Spiess, 1998; Kreyenfeld and Hank, 2000).

Public involvement in the childcare system in western Germany in the 1990s was mainly driven by educational motivations along with the ambition to provide equal opportunities for all children. However, childcare slots were severely rationed and free places were mainly granted to older children (Spiess, 2008; Alt et al., 2018). Thus, childcare attendance varied substantially by age, as shown in Figure 2-1. In 1991, only 33 percent of all three-year-old and 70 percent of all four-year-old children attended formal childcare in western Germany. One of the first "milestone" reforms to roll out the universal childcare system for all children aged

[3] In the 1970s, the "Kindergarten" was officially established as first stage of the German educational system (Deutscher Bildungsrat, 1970).

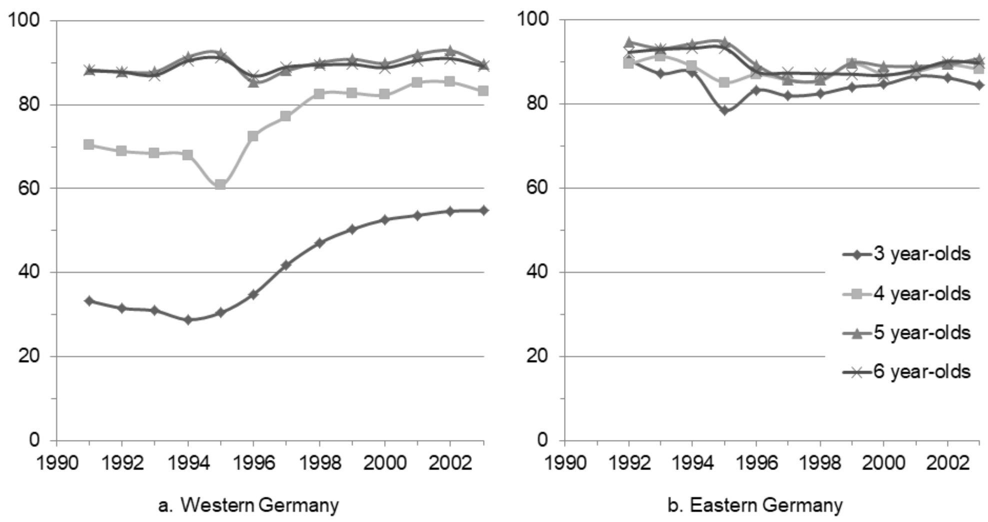
[4] The U.S. counterpart to the German "Kindergarten" would be higher quality pre-school instead.

three or older was implemented through the introduction of a legal right to a slot in highly subsidized half-day childcare.[5] The law became effective in January 1996.[6] Figure 2-1 shows that in the years after the reform childcare attendance rates for three-year-olds increased by more than 20 percentage points (pp) and around 15 pp for four-year-olds, while the attendance rates for older children remained relatively stable. Hence, the roll-out of the German childcare system mainly affected three and four year old children for whom excess demand had been largest.

[5] Bauernschuster and Schlotter (2015) find that the expansion of universal childcare coverage after the 1996 reform increased the labour market participation of mothers with eligible children by 6.4 percentage points. However, given that the reform covered only half-day care, the economic aspect of facilitating maternal labor supply played a subordinated role. It became a primary policy goal of the roll-out of the childcare system for one- and two-year-olds later on (Kreyenfeld and Hank, 2000; Spiess, 2008).

[6] The legal claim for children aged three or older is anchored in 24 SGB VIII ("Achtes Sozialgesetzbuch") and was later amended for children older than 12 months.

Figure 2-1: Childcare attendance rates in Germany (1991-2003)

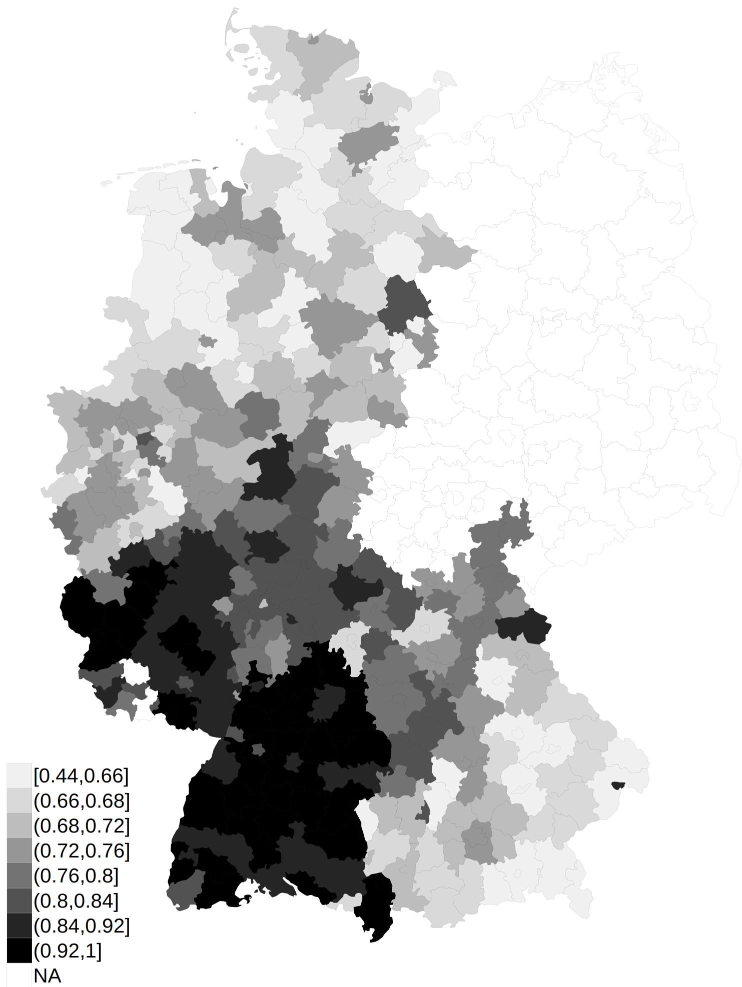


Notes: The left side shows the percentage share of children of the respective age group who attend childcare in the western German federal states (including former-West Berlin), while the right side displays childcare attendance rates in the former-Eastern German federal states (including former-East Berlin) between 1991 and 2003. Own graphical display.
Source: Statistical Offices, Bevölkerungsfortschreibung, Fachserie 1, Reihe 1.3, Micro Census, Calculations of the Dortmunder Arbeitsstelle Kinder- und Jugendhilfestatistik.

Whether or not parents of three year old children were able to claim their slot for which the 1996 reform had given them a legal right, depended on the local availability of childcare places in their county of residence. Figure 2-2 depicts the high level of regional variation in childcare supply in western Germany at the time. Some counties could provide slots for only 44 percent of the children in the relevant three to six years old age group (white areas in Fig. 2-2), while other counties had childcare provision rates exceeding 92 percent (black areas in Fig. 2-2). These large regional differences reflect the decentralized planning process of childcare provision in Germany. Under the subsidiarity principle, the smallest social unit is responsible for providing childcare services.[7] In the context of the 1996 reform, this meant that even though the legal mandate originated at the federal level, states were responsible for the financial implementation and passed roll-out obligations onto the counties and municipalities.

[7] See Spiess (2008) for a detailed synopsis on the composition and organization of the German childcare system.

Figure 2-2: Coverage map for childcare provision in West-Germany (slot-child-ratio at county level)



Notes: This figure shows the slot-child-ratio for children between age three and six and a half aggregated at the county level in western Germany measured in the year 1998. The map reveals a very heterogeneous level of childcare provision. In the institutional context of severe rationing, slots are all filled. Hence, the slot-child-ratio measures the supply as well as the regional attendance of childcare. In some counties, only 44 percent of children in the relevant three to six and a half years old age group attend childcare (light grey areas), while a few counties have a high level of childcare provision with more than 92 percent of children enrolled in childcare (black areas). Own graphical display. *Source:* RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1998

Since most counties were unable to meet the demand originating from the new law, children typically enrolled in childcare between August and September rather than around their third birthday. This is related to the systematic release of slots in childcare centers through school enrolment of school-aged children (typically at age six in Germany).[8] However, in many counties not enough children vacated places to enroll all children turning three who demanded a place under the legal claim.[9] Hence, in most of the counties in the former-West with a shortage of supply, especially in the early years between 1996 and 1998, children did not get one of these slots in the year that they turned three. Instead, they had to wait nearly one year until they could enter childcare in August/September the next year.

Our empirical strategy uses this particular institutional landscape by instrumenting attending childcare at age three (according to the legal right) with the respective supply of childcare places in the county of residence. In 1996 the likelihood of actually starting childcare at age three, the binding age of the legal entitlement, was constrained by the local availability of places. These shortages varied considerably across regions because of the highly decentralized childcare system and it is this variation that we exploit in our estimation strategy.

2.3 Empirical strategy

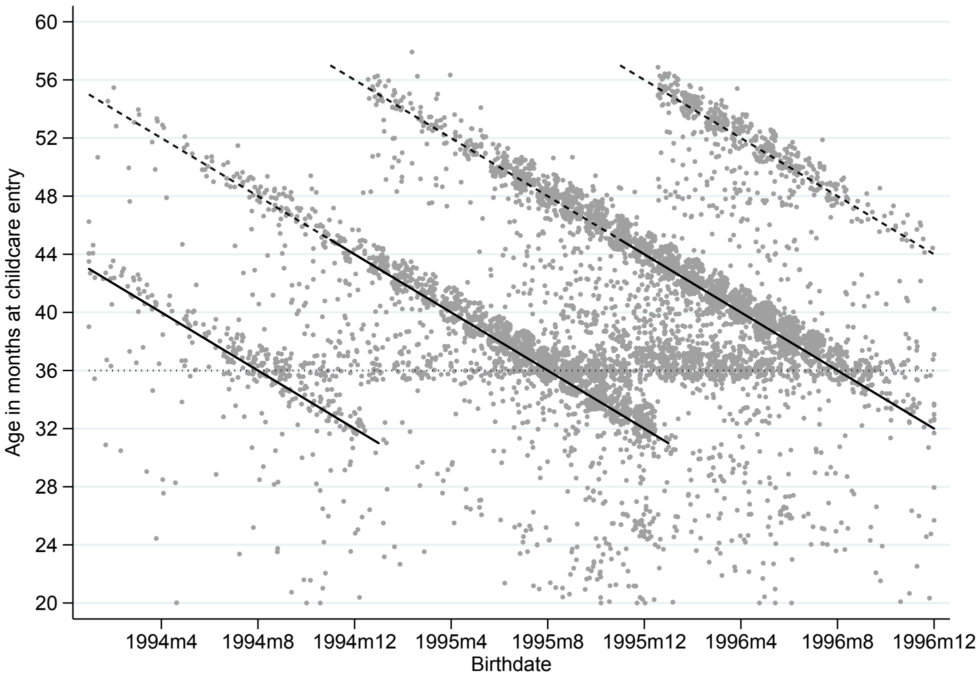
To estimate the long run effects of an earlier entry into childcare and the years spend in childcare, we compare children from the same birth cohorts who entered childcare approximately one year apart. Early entrance is defined to take place two months around the official school entry month (which falls between August and September) of the year a child turns three (the legal entitlement age for a universal childcare slot at the time). We refer to these children who enter early

[8] Up until today, children typically enroll in childcare between August and September as school enrolment still frees up most capacities for new cohorts.

[9] For more institutional details on this so-called decisive decision rule of counties, which is related to children's birth month, see Bauernschuster and Schlotter (2015).

as treatment group. Late entrance refers to children enrolling in childcare in the year they turn four (control group). Conditional on month of birth, children from these two groups differ by approximately one year in their childcare starting age. This is illustrated in Figure 2-3 where we plot childcare entry age against birth month for our sample.[10]

Figure 2-3: Childcare entry regimes of adolescents (NEPS)



Notes: This figure shows childcare entry for adolescents in the NEPS SC4 born between 1994 and 1996. On the vertical axis, children's age in month at childcare entry is depicted and on the horizontal axis their month of birth. Data points close to the solid line correspond to children who enter childcare in compliance with the legal claim of 1996 (in the year they turn three), while the data points close to the dashed line represent children who have to wait approximately one additional year before entering childcare due to a shortage of available childcare slots. Thus, these children are more likely to enter childcare at age four. Own calculations and graphical display.
Source: NEPS Data, Data Version SC4: 7.0.0 remote access.

[10]For a detailed description of the data, see Section 2.4.

Data points clustered around the black solid lines correspond to children in the treatment group, and thus, to children who enter childcare in August or September in the year they turn three. Data points close to the dashed lines correspond to children from the control group. Figure 2-3 clearly shows that children in our sample typically enter childcare between August and September, when first-graders vacate universal childcare slots each year. For example, children born in January are often either 43-44 months old when they enter childcare in the year they turn three or 55-56 months when they enter one year later. Our definition of early and late childcare entry excludes some of those observations who cluster around the horizontal line at the age of 36 months. These are children who entered childcare as soon as they turned three, which was only possible in regions with a sufficient supply of childcare slots. Excluding these children from the analyses allows us to maintain a one-year-interval in childcare starting age between treatment and control groups.[11]

We will first estimate the following linear model that links personality traits in adolescence with early childcare entry:

$$Y_i = \beta C_i + X_i'\delta + \epsilon_i \tag{2.1}$$

where Y_i is the personality trait measure of the i'th individual, the main explanatory variable C_i is equal to one if child i enters childcare within two months of the official school entry period (August-September) in the year that she turns three and zero if entry occurs approximately twelve months later, and X is a vector of controls that includes, among other things, birth month fixed effects, maternal employment status, household income, parental education.

However, a simple comparison of personality traits across treatment and control groups may not yield the causal effect for early childcare entry because of potential

[11]Results including those children are qualitatively similar to our main results, but with somewhat smaller effects and increased standard errors because of a reduced difference in the average age at childcare entry between treatment and control groups.

selection effects. The primary concern is that age at childcare start is not only determined by the availability of slots, but may depend to a large extent on family background characteristics. These characteristics are known to be important determinants of children's personality traits (for example, see Cunha and Heckman, 2007, 2008). In Germany, several family characteristics are associated with the timing of childcare entry, e.g. maternal employment status, single parenthood, or parental education.[12] For instance, Büchel et al. (2002) point out that in the mid-1990s to early 2000s children from low socio-economic backgrounds with working mothers were prioritized, if childcare centers had insufficient places for three-year-old children. Whether these factors cause an upward or downward bias is ex ante unclear. Thus, our analysis includes a rich set of family background characteristics, among others, parental education, household income, maternal employment, household size, and single parenthood, to control for systematic differences across treatment and control groups.[13] Nevertheless other unobserved factors might exist that influence the timing of childcare entry and personality traits in adolescence. There is, for example, the possibility of reverse causality, i.e. a child's pre-existing personality traits (which we do not observe) might affect the timing of childcare entry. In that case OLS estimates of β might be biased. Thus, we apply an instrumental variable approach that exploits regional differences in the supply of childcare. The rationale behind our strategy is that the supply of childcare places in Germany varies considerably across regions and at the time the children in our sample became eligible for children was the main determinant for whether a child could enter childcare in the year she turns three or one year later. Since the specific planning process of municipalities in Germany leaves little room for parents to shape the regional supply of childcare (Kreyenfeld et al., 2001), we are

[12] Büchner and Spiess (2007) find that maternal employment status is an important predictor of starting childcare at the age of three rather than at later ages. Labor supply of women is generally found to be positively affected by their level of education (Schultz, 1990). On the other hand, maternal employment can result from poor employment opportunities for the husband (Juhn and Murphy, 1997) or single parenthood.

[13] See also Schober and Spiess (2013); Spiess and Büchner (2009) and Wrohlich (2008) for further research on determinants of childcare entry in Germany.

less concerned that a higher provision of childcare in certain regions is potentially correlated with other factors affecting adolescent outcomes. In addition, regional mobility in Germany is low, reducing concerns that parents selectively move to counties with a higher childcare supply, which in turn could potentially bias our results. Regional net mobility patterns for the year 1996 among federal states also appear to be unrelated to childcare availability (see Figure 2-5 in the Appendix). Felfe and Lalive (2012) and Cornelissen et al. (2018) provide additional evidence that mobility of parents in Germany is very low and unrelated to the number of childcare slots per county.

Using the conditional quasi-random assignment of available childcare slots we estimate a 2SLS model consisting of the following first and second stage:

$$C_i = \gamma_1 Z_i + X_i' \gamma_2 + \epsilon_i \tag{2.2}$$

$$Y_i = \delta_1 \hat{C}_i + X_i' \delta_2 + \eta_i \tag{2.3}$$

where the instrument Z_i is the local supply of childcare slots measured by the slot-child-ratio for children between age three and six and a half on the county level, $\hat{C}_i$ is the instrumented early childcare indicator based on the first stage, and all other variables are as defined in (2.1). X_i further includes several regional characteristics that predict local childcare demand.[14] To mitigate potential bias from misspecification, we rely on dummy variables for each category for all included variables in our regressions. This also allows us to deal with missing information in a straightforward way by simply including separate dummies for missing values.

Estimates for δ_1 in Equation (2.3) can be interpreted as the causal effect of early childcare entry under the assumption that conditional on our set of family characteristics, the local availability of childcare places is unrelated to pre-existing child outcomes or unobserved family background characteristics affecting student outcomes. Naturally, the local childcare supply is also affected by demand for

[14] We describe the set of controls used in the analysis in more detail in Section 2.4.

childcare which in turn might be correlated with other unobserved factors that influence child outcomes. While the rich set of family background variables should alleviate concerns about remaining unobserved confounders, we additionally include several predictors of local childcare demand in X_i from administrative data (German Federal Statistical Office, 2016; German Federal Office for Building and Regional Planning, 2018). These are measured at the county level and include income (per capita), the unemployment rate, the fraction of foreigners, the population density, the political vote share for conservative parties and the female employment share. In Section 2.5 we present evidence that, even after controlling for a set of demand factors, the first stage relationship remains strong. This supports the assumption that in our institutional setting regional differences in slot-child-ratios are to a large extent driven by differences in the supply of childcare places rather than the demand for them. We hypothesize that the rest of the unexplained variation in supply differences is likely to be random and can be attributed to decentralized budget planning and political bargaining at the county level. Still, any of these remaining differences across counties that are correlated with the supply of childcare places and adolescents' personality traits could lead to bias in the IV estimates. We provide some placebo tests to address these concerns in Section 2.7.

2.4 Data

For the empirical analyses we draw on four different data sets: (1) the German National Educational Panel Study (NEPS), administrative county level data[15] on (2) slot-child-ratios and (3) regional controls, and (4) the German Socio-Economic Panel Study (SOEP).[16] The main analysis is based on the NEPS, a nationwide and representative multi-cohort panel study (see Blossfeld et al., 2011). The NEPS covers six age groups ranging from newborn children to adults. In total, it cov-

[15]We use data from the years 1994 and 1998 (German Federal Statistical Office, 2016; German Federal Office for Building and Regional Planning, 2018).

[16]We describe the SOEP data in Section 2.6.

ers educational trajectories of more than 60,000 individuals. We focus on the starting cohort 4 (SC4), which is a sample of students who were first sampled in the school year 2010/2011 when they attended ninth grade. The full SC4 consists of approximately 15,000 students mainly born between 1994 and 1996. The data set contains comprehensive information regarding the students' skill development, learning environments, educational decisions and parental backgrounds.[17] It furthermore provides retrospective information on childcare entry dates from which we infer the age in months at childcare entrance and classify children into treatment and control groups. For the IV approach, we merge these data to administrative data on county-level slot-child-ratios defined as the ratio of available childcare places to the number of children in that age group. In our preferred specification we use the slot-child-ratio for children between age three and six for the year 1998.[18] These data come from the statistics of children and youth welfare (*'Kinder- und Jugendhilfestatistik'*) and were provided by the research data center (RDC) of the federal statistical office and the statistical offices of the federal states (German Federal Statistical Office, 2013).

2.4.1 Main sample restrictions

We restrict our sample to students born between 1994 and 1996 who attend ninth grade in 2010 and for whom we have information on the exact starting date of childcare attendance. Further, we focus on students living in the former-West of Germany (as discussed in Section 2.2). In addition, we discard all students without valid personality trait measures. These restrictions reduce the sample size from 15,000 to 6,813. Finally, to ensure that we measure the effect of entering childcare for students approximately twelve months apart, we focus on students who entered childcare within two months of the official school entry month (in August or September) in the year that they turn three or four. Our final sample

[17] For more information on the SC4 see LIfBi (2016).

[18] In Section 2.7, we use the slot-child-ratio for the year 1994 as an alternative instrument for early childcare entry.

consists of 4,579 students. Although these restrictions reduce the sample size quite a bit, we are confident that this does not affect the outcome of our study, as the personality traits measured in the overall sample and in the final sample do not differ (see Section 2.4.2).

Table 2.1: Summary statistics of relevant characteristics by treatment status (NEPS)

	Control	**Treatment**	**Diff**
Child characteristics			
Age at childcare entry in months	48.87	38.20	10.67***
Age at school entry in years	6.63	6.53	0.10***
Age at interview in years	15.16	15.06	0.10***
Migration Background	0.23	0.26	-0.03*
Male	0.54	0.51	0.02
Parental characteristics			
Mother working	0.80	0.82	-0.02
Single parent	0.07	0.09	-0.02*
Mother's years of education	13.35	13.46	-0.11
Father's years of education	13.74	13.92	-0.18*
Mother's age at birth	29.62	29.75	-0.13
Household characteristics			
Household Income:			
First quartile	0.18	0.19	-0.01
Second quartile	0.23	0.19	0.04**
Third quartile	0.22	0.22	-0.00
Fourth quartile	0.17	0.21	-0.04**
Missing	0.20	0.19	0.01
Household size	4.16	4.06	0.10**
Regional characteristics			
Slot-child-ratio in 1998	0.73	0.78	-0.05***
Conservative vote share	45.68	44.80	0.87***
Female employment share	41.30	41.87	-0.58***
N	1,325	3,254	4,579

Notes: This table depicts summary statistics. Panel A shows mean differences of the outcome variables (Big Five personality traits) measured at age 15 in the NEPS by treatment status, i.e. compares adolescents who enter childcare at age three (treatment group) to those entering childcare approximately one year later at age four (control group). Panel B shows mean differences of the included control variables. Own calculations. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.
Source: NEPS Data, Data Version SC4: 7.0.0 remote access, RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1998, and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018).

The mean comparison of socio-demographic characteristics of adolescents in treatment and control groups shows that students in the treatment group are more likely to live in a single parent household, to have fathers with more years of schooling, and to live in higher income households (see Table 2.1). Table 2.1 further reveals that our instrumental variable, the regional slot-child-ratio, varies significantly between treatment and control groups. Thus, children in the treatment group reside, on average, in counties with a less restricted supply of childcare than children in the control group. This already points to the relevance of our instrument.

Furthermore, we also merge the following regional characteristics measured in 1998 at the county-level to our final sample and use them in our preferred specification as additional control variables: population density, per capita GDP, unemployment rate and the share of foreigners. These data are also provided by the German Federal Statistical Office (German Federal Statistical Office, 2016). Moreover, we use two additional regional covariates of interest from the INKAR database (German Federal Office for Building and Regional Planning, 2018). First, we use the female employment rate at the county level in 1998.[19] Second, we use data from official electoral vote share statistics for the election of the German Federal Parliament in 1998, which allows us to include a measure of regional political attitudes and values as an additional covariate in our regressions.[20]

[19]This measure captures only female employees in Germany who are subject to mandatory social insurance contributions, hence it does not include self-employed individuals, or women working for the public sector. However, it covers roughly 70 percent of all women employed and is therefore most frequently used as measure for female labor supply.

[20]We construct an indicator of the county average share of valid party votes for either the Christian Democratic Union/Christian Social Union in Bavaria (CDU/CSU) or the Free Democratic Party (FDP). All three parties represent the more conservative German political spectrum. Therefore, a higher share of more conservative votes is used as an indicator for more conservative views on average per county. A smaller share of conservative votes represents higher shares of more liberal party votes for either the Social Democratic Party of Germany (SPD), the Left Party or the Green Party.

2.4.2 Personality traits

The NEPS SC4 data include measures of the Big Five personality traits, which comprise five basic psychological dimensions: openness to experience, conscientiousness, extroversion, agreeableness, and neuroticism (see also McCrae and Costa, 1996, 1999).[21] Compared to the original Big Five inventory, the personality traits in the NESP SC4 are measured with a validated short scale based on 10 items ("BFI-10") provided by Rammstedt and John (2007).[22] Students in the SC4 self-rate their personality traits on a five-point scale (from 1 "disagree strongly" to 5 "agree strongly") for each item. For example, the dimension extroversion captures two items, for which students self-rate to which degree they regard themselves as outgoing or sociable and reserved. Table 2.2 shows the summary score of each personality trait dimension for the full NEPS sample in column 1 and our restricted estimation sample in column 2. It can be seen that despite our restrictive sample selection, observations in our estimation sample do not differ from the full sample in terms of their Big Five personality traits. For the analyses, we take the average over all relevant items for each trait and standardize each measure to have a mean of zero and a standard deviation of one. To account for gender differences in personality traits, we standardize separately by gender.

[21] Table 2.9 in the Appendix includes the definitions of all Big Five personality traits.

[22] See also Table 2.10 in the Appendix for the questionnaire items used in NEPS to measure the Big Five.

Table 2.2: Summary statistics of personality traits in adolescence (NEPS)

	Full sample SC4	**Estimation sample**			
	Mean	Mean	Std. Dev.	Min.	Max.
Openness	3.47	3.49	0.97	1	5
Conscientiousness	3.15	3.15	0.87	1	5
Extroversion	3.43	3.44	0.88	1	5
Agreeableness	3.45	3.47	0.67	1	5
Neuroticism	2.77	2.76	0.85	1	5
N	14,206	4,579	4,579	4,579	4,579

Notes: This table shows summary statistics of adolescents' personality traits at age 15 in the NEPS. Column (1) displays the mean for the unrestricted NEPS SC4 sample, while columns (2)-(5) show summary statistics for the estimation sample used in our analyses. The scores of the personality traits range from 1 (disagree strongly) to 5 (strongly agree) in the NEPS. A higher value represents a higher score on the respective personality dimension. Own calculations.
Source: NEPS Data, Data Version SC4: 7.0.0 remote access.

2.5 Results

We start with the main results of the empirical model in Equation (2.1). The first four columns in Table 2.3 report OLS estimates of entering childcare one year earlier on the respective Big Five personality trait and step-wise adding more control variables. The first specification in column 1 only controls for birth month fixed effects while column 2 adds control variables for migration background, mother's age at childbirth, gender of the student, single parenthood, and mother's years of education. In column 3, we further include dummies for mothers' employment status, household income quartiles, and household size. The last specification shown in column 4 additionally includes regional characteristics. The estimates in column 1 suggest that children who enter childcare one year earlier are more likely to have higher levels of extroversion (around 0.12 SD) and openness (around 0.07 SD), and lower levels of conscientiousness (around -0.09 SD) at the age of fifteen.

These estimates are robust to the inclusion of further control variables.[23] The OLS results for conscientiousness and extroversion are statistically significantly different from zero at the one percent level across all specifications. Those on openness are significant at the five percent level and decrease in significance level to being marginally significant in columns 3 and 4.

We next turn to demonstrating the relevance of our instrument. Table 2.4 reports results for the first stage where we regress early childcare attendance at the age of three (C_i) on the slot-child-ratio in 1998 (Z_i) at the county-level. Columns 1 to 4 show that the slot-child-ratio is a strong predictor of starting childcare one year earlier; in all specification the F-Test is larger than 120. The size of the first stage coefficients changes only slightly when adding additional control variables. In our preferred specification in column 4 of Table 2.4, which includes proxy variables for the regional demand for childcare places, the coefficient increases to 0.93 from 0.85 in Column 1. This indicates almost a one-to-one relationship between the supply of early childcare and early entry; as the share of children for which a childcare slot is available increases by one pp, the probability of early entry into childcare also increases by one pp. This is exactly what we would expect in a setting with an under-provision of childcare slots where every additional slot is immediately filled.

[23]For the sake of brevity, we only show the coefficient of main interest, early childcare entry, even though some additional variables are statistically significantly correlated with personality traits. See Table 2.14 for the full results. For example, household income is significantly and positively correlated with extroversion. Furthermore, children of employed mothers are also more likely to have a higher level of extroversion.

Table 2.3: Estimation of one additional year of childcare on adolescents' personality traits (NEPS)

	OLS				**2SLS**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Openness	0.071**	0.060**	0.056*	0.059*	0.025	-0.040	-0.020	0.026
	(0.030)	(0.030)	(0.031)	(0.031)	(0.170)	(0.168)	(0.165)	(0.173)
Conscientiousness	-0.093***	-0.092***	-0.088***	-0.088***	0.261	0.273*	0.269	0.251
	(0.033)	(0.033)	(0.033)	(0.033)	(0.172)	(0.166)	(0.164)	(0.170)
Extroversion	0.124***	0.118***	0.108***	0.107***	0.312**	0.308**	0.298**	0.367**
	(0.032)	(0.031)	(0.032)	(0.032)	(0.156)	(0.151)	(0.147)	(0.171)
Agreeableness	-0.033	-0.032	-0.029	-0.040	0.312	0.290	0.290	-0.014
	(0.034)	(0.034)	(0.034)	(0.034)	(0.194)	(0.192)	(0.191)	(0.192)
Neuroticism	-0.011	-0.010	-0.000	0.011	0.006	-0.001	-0.007	-0.026
	(0.034)	(0.035)	(0.035)	(0.035)	(0.176)	(0.174)	(0.174)	(0.172)
Birth month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Full controls	No	No	Yes	Yes	No	No	Yes	Yes
Regional controls	No	No	No	Yes	No	No	No	Yes

Notes: The table shows OLS and 2SLS estimates for the main estimation sample (N=4,579) where the county-level slot-child-ratio for 1998 is used as an instrument on one additional year of childcare. Standard errors are clustered at the county level and are given in parentheses. The sample for the models in columns (1) through (6) consists of all children who enter childcare within 2 months of the start of the school year in which they turn three (treatment group) or four (control group) in western Germany. Individual controls include migration background, mother's age at birth, sex of the child, a dummy for being a single parent and dummies for mother's years of education. Full controls include mother's employment status, dummies for household income and size. Regional controls include quintile dummies for the county population density, unemployment rate, share of foreigners, per capita GDP, as well as county level conservative vote share, and female employment share. Own calculations. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.
Source: NEPS Data, Data Version SC4: 7.0.0 remote access, RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1998 and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018).

Table 2.4: First stage estimation of slot-child-ratio instrument on one additional year of childcare (NEPS)

	First stage IV estimation			
	(1)	(2)	(3)	(4)
Slot-child-ratio 1998	0.854***	0.867***	0.867***	0.931***
	(0.078)	(0.077)	(0.076)	(0.084)
Birth month FE	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	Yes
Full controls	No	No	Yes	Yes
Regional controls	No	No	No	Yes
R^2	0.042	0.077	0.084	0.100
First-stage F-test	120.22	125.75	129.57	142.94

Notes: The table shows first stages estimates for the main estimation sample (N=4,579) where the county level slot-child-ratio for 1998 is used as an instrument on one additional year of childcare. Standard errors are clustered at the county level and are given in parentheses. The sample for the models in columns (1) through (4) consists of all children who enter childcare within two months of the start of the school year in which they turn three (treatment group) or four (control group). Individual controls include migration background, mother's age at birth, sex of the child, a dummy for being a single parent and dummies for mother's years of education. Full controls further include mother's employment status, dummies for household income and size. Regional controls include quintile dummies for the county population density, unemployment rate, share of foreigners, per capita GDP, as well as county level conservative vote share, and female employment share. Own calculations. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.
Source: NEPS Data, Data Version SC4: 7.0.0 remote access, RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1998 and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018).

Columns 5-8 in Table 2.3 report the result of the instrumental variable approach (IV) using the slot-child-ratio at the county level as instrument for early entrance in universal childcare as formulated in Equations (2.2) and (2.3). Again, we step-wise include further control variables. Effects are less precisely estimated in these specifications; standard errors increase five-fold compared to the OLS estimates. Hence, the IV results are less statistically significant albeit larger in size compared to the OLS results. Most importantly, the effect on extroversion remains positively significant and increases substantially in size to 0.37 SD. This effect is significant at the five percent level across all specifications and changes only slightly when including further control variables from 0.31 SD to 0.37 SD. This suggests that earlier childcare entry induced by a higher supply of childcare slots causally increases children's level of extroversion at age fifteen by 37 percent of a standard deviation. This is a substantial effect size.[24] Compared to interventions, such as the Perry Preschool Program, it is of similar magnitude and ranks at the lower end of reported effect sizes by Heckman et al. (2013) between 0.34 and 0.64 of a standard deviation. To ensure that statistical significance is not the result of multiple hypothesis testing, we also computed p-values based on the Romano-Wolf correction (Romano and Wolf, 2005). These lead to similar significance levels for the extroversion estimate. The effect on conscientiousness switches sign compared to the OLS results. For children entering childcare earlier in the mid-1990s in counties with higher supply in places column 8 indicates an increase in conscientiousness of 0.25 SD by age fifteen. Albeit significant in size, the effect is not statistically significant at conventional levels (p-value 0.14). For the remaining three personality traits openness, agreeableness, and neuroticism, we cannot conclude that entering childcare earlier has a statistically significant impact on them.

The results in Table 2.3 raise the question of what explains the differences between the OLS and IV estimates for conscientiousness and extroversion. One explanation for these differences could be effect heterogeneity. Our IV estimates only capture the LATE of entering childcare approximately one year earlier because of a larger supply of childcare places in children's respective county. The effect for these complier children might differ from the average treatment effect. To test whether complier children differ from the rest of our sample, we characterize the complier population in terms of several background characteristics in Table

[24] For example, it is larger than the effect of moving from the first to the fourth quartile of household income (0.22 SD, see Table 2.14 in the Appendix)

2.5. In this analysis Z is defined as a binary variable equal to zero if the slot-child-ratio is below the 33^{rd} slot-child-ratio percentile and equal to one if it is above the 66^{th} percentile. C_{1i} indicates early childcare entry when $Z = 1$ and C_{0i} when $Z = 1$. The ratios in column 3 of Table 2.5 give the relative likelihood of compliers having characteristics shown in the utmost left column of this table. We see that complier children are more likely to be from disadvantaged backgrounds; they are more likely to live in single parent households and have mothers without post-secondary education. Cornelissen et al. (2018) provide evidence that children from disadvantaged backgrounds benefit more from childcare because of worse outcomes when not enrolled in childcare. This heterogeneity could explain why our IV estimates are considerably larger than the corresponding OLS results.

Table 2.5: Complier characteristics for slot-child-ratio instrument (NEPS)

	$P(X_i = 1)$	$P(X_i = 1 \mid C_{1i} > C_{0i})$	$\frac{P(X_i=1 \mid C_{1i}>C_{0i})}{P(X_i=1)}$
Complier characteristics			
Migration background	0.253	0.248	0.977
Single parent household	0.085	0.097	1.140
Mother working at age 15 of child	0.794	0.790	0.994
Mother has no postsecondary educational qualification	0.105	0.113	1.078

Notes: The table reports an analysis of complier characteristics for the slot-child-ratio instrument. Z is defined as a binary variable taking the value 0 if the slot-child-ratio is below the 33^{rd} slot-child-ratio percentile and the value 1 if it is above the 66^{th} percentile. C_{1i} indicates early childcare entry when $Z = 1$ and C_{0i} when $Z = 1$. The ratios in column 3 give the relative likelihood that compliers have the characteristics indicated on the left hand side. Own calculations. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.
Source: NEPS Data, Data Version SC4: 7.0.0 remote access, RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1998 and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018).

2.6 Further analysis

To check whether our previous findings are the results of multiple testing, we replicate the main analyses in Table 2.3 with data from the German Socio-Economic

Panel Study (SOEP). In addition to cross validating our results, the SOEP allows to further assess the validity of our IV approach because it contains additional information, in particular Big Five measures for parents. These are potentially important controls, because childcare choices are in part explained by parental personality traits (for example, see Bjerre et al., 2011) and personality traits have been shown to be transmitted from parents to children (Anger, 2011). Hence, controlling for parental personality traits should be informative about potential omitted variable bias.

The SOEP has been carried out since 1984 and is an annual nationwide random German household panel survey with more than 30,000 individuals in approximately 17,000 households participating in 2017 (see Wagner et al., 2007). Similar to the NEPS data the longitudinal design allows to examine long run effects of attending universal childcare earlier on personality traits. Although the SOEP has advantages due to its household sampling approach compared to the NEPS, we are left with a rather small sample after imposing the same sample restrictions as in our main sample (see Section 2.4). The final SOEP sample comprises 631 seventeen-year-olds from initially 3,525 adolescents[25] and covers a wider range of birth cohorts, i.e. children born between 1990 and 1998.[26] For 631 adolescents we have information on personality traits, childcare attendance at age three, month of birth, county of residence, parental personality traits, as well as maternal, household and regional characteristics from the years when the adolescents turned three. Similar to the NEPS Big Five inventory, personality traits in the SOEP are also measured using a modified version (Dehne and Schupp, 2007) of the Five Factor Model by McCrae and Costa (1996, 1999). In the SOEP, adolescents self-rate their personality traits based on a set of 16 statements (see Table 2.10 in the Appendix for an overview). All questions are answered on a seven-point Likert type scale

[25] The surveying of personality traits was only introduced in 2006 in the youth questionnaire. Restricting the sample to observations with Big Five measures and valid information on childcare attendance leads to a large drop in sample size, as nearly 40 percent of adolescents stem from a large refreshment sample in 2000. For those students we do not have any information on childcare attendance in the mid-1990s.

[26] See Table 2.13 in the Appendix for descriptive statistics of this sample.

(from 1 "does not apply to me at all" to 7 "applies to me perfectly"). Again, we sum the relevant items determining each dimension of the five personality traits with scores ranging from 1 to 7, and standardize these measures by gender, to have zero mean and a standard deviation of one.

Table 2.6: Estimation of one additional year of childcare on adolescents' personality traits (SOEP)

	OLS			**2SLS**		
	(1)	(2)	(3)	(4)	(5)	(6)
Openness	0.272***	0.263***	0.290***	0.260	0.292	0.324
	(0.097)	(0.091)	(0.097)	(0.282)	(0.275)	(0.259)
Conscientiousness	0.117	0.124	0.106	0.302	0.310	0.167
	(0.095)	(0.090)	(0.092)	(0.285)	(0.287)	(0.288)
Extroversion	0.186**	0.180**	0.212***	0.614**	0.606**	0.594**
	(0.083)	(0.080)	(0.080)	(0.278)	(0.272)	(0.264)
Agreeableness	-0.136	-0.130	-0.171*	0.057	0.044	0.054
	(0.093)	(0.090)	(0.094)	(0.300)	(0.299)	(0.326)
Neuroticism	-0.216**	-0.211**	-0.186*	-0.493	-0.455	-0.623*
	(0.087)	(0.088)	(0.095)	(0.335)	(0.331)	(0.323)
Full controls	Yes	Yes	Yes	Yes	Yes	Yes
Parents' personality controls	No	Yes	Yes	No	Yes	Yes
Regional controls	No	No	Yes	No	No	Yes

Notes: The table shows OLS and 2SLS estimates from the main SOEP sample (N=631) where the county-level slot-child-ratio from the year 1994 (for birth cohorts 1990-1994) and from 1998 (for birth cohorts 1995-1998) is used as instrument for one additional year of childcare on the respective personality trait. Standard errors are clustered at the county level and are given in parentheses. The sample for models in columns (1) through (6) consists of all children who enter childcare in the year that they turn three (treatment group) or four (control group) in western Germany. Full controls include the child's gender, migration background as well as dummies for the children's birth year and month, the mother's age at birth, a dummy for being a single parent, dummies for her education background and employment status, dummies for household income and size. Parents' personality controls comprise Big Five personality traits of mothers and (social) fathers. Regional controls include the county's population density, unemployment rate, female employment rate, share of foreigners, per capita GDP, and conservative vote share. Own calculations. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.
Source: SOEP v32 (2007-2015, birth cohorts 1990-1998), RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1998 and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018).

Table 2.6 shows OLS and IV estimates for the SOEP data controlling for an even larger set of characteristics which are likely to be correlated with both early childcare entry and adolescents' personality traits. Since the personality traits of parents and their children are modestly correlated,[27] we would expect OLS coefficients to change once we additionally control for parents' personality traits, if there is selection into early childcare based on children's personality. However, comparing OLS results with and without controlling for parent's personality traits in columns 1 and 2 of Table 2.6 yield very similar results for all five personality traits. Importantly, the results for extroversion can be replicated with the SOEP data: The OLS results for extroversion in columns 1-3 are significantly positive and somewhat larger than the NEPS results. The estimate for the specification with the full set of control variables suggests that entering childcare one year earlier is associated with a 0.21 SD increase in extroversion. Although the associated standard errors and confidence intervals are quite large, the IV estimates also clearly indicate a significant positive effect of early childcare on extroversion of about 0.59 SD.[28] For the OLS specifications, we further find significant positive effects for openness and negative effects for neuroticism. The magnitude of these effects are similar in the IV specifications, but because of the increase in standard errors, they lose statistical significance. Importantly, controlling for parents' personality traits in the IV specification also does not affect the estimates. This alleviates concerns that any potential relationship between parental personality traits and the supply of childcare biases our IV results.

2.7 Sensitivity analysis

In this section, we further test the robustness of our results. One concern is that our instrument, the slot-child-ratio, partly captures unmeasured parental prefer-

[27] Anger (2011) reports correlations in Big Five measures between parents and their adolescent children ranging from 0.12 to 0.24 for the SOEP data.

[28] The first stage in the SOEP sample is less strong compared to the NEPS, but the F-statistics is still larger than 10 (see Table 2.12 in the Appendix).

ences for early childcare even after conditioning on an extensive set of background variables. This is a threat to our identification strategy if these preferences are also related to child outcomes. Therefore, as a robustness check we use the slot-child-ratio at the county-level in 1994 instead of 1998 as an instrument for early childcare entry. This is a measure of supply from the year prior to the birth of the students in the NEPS sample who were mostly born in 1995 and 1996. If parental preferences are not perfectly correlated over time, the earlier supply should capture less of the parental preferences for childcare for the children in our sample, but already reflect regional differences in the supply of childcare. Table 2.7 shows that results using the 1994 ratio as an instrument are very similar to the main results in Table 2.3.[29] The estimated effect for extroversion is 0.33 SD in our preferred specification and the estimates for conscientiousness are positive, albeit statistically insignificant at conventional significance levels.[30] There is also a large and significant effect on agreeableness, which, however, vanishes once regional controls are included.

[29]The corresponding first stage estimates are reported in Table 2.11 in the Appendix.
[30]The p-value of the coefficient in Column 3 of Table 2.7 equals 0.18.

Table 2.7: Robustness of 2SLS estimation of one additional year of childcare on adolescents' personality traits (NEPS)

	2SLS			
	(1)	(2)	(3)	(4)
Openness	0.057	-0.016	0.004	0.136
	(0.182)	(0.179)	(0.174)	(0.171)
Conscientiousness	0.279	0.291	0.295	0.187
	(0.199)	(0.191)	(0.189)	(0.183)
Extraversion	0.394**	0.382**	0.359**	0.327**
	(0.162)	(0.160)	(0.157)	(0.160)
Agreeableness	0.579***	0.532**	0.550***	0.168
	(0.214)	(0.209)	(0.208)	(0.188)
Neuroticism	-0.054	-0.075	-0.061	-0.110
	(0.186)	(0.183)	(0.184)	(0.161)
Birth month FE	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	Yes
Full controls	No	No	Yes	Yes
Regional controls	No	No	No	Yes

Notes: The table shows 2SLS estimates where the slot-child-ratio for 1994 is used as an instrument (N=4,505) on one more year of childcare on the respective personality trait at age 15. Standard errors are clustered at the county level are given in parentheses. The sample for the models in columns (1) through (3) consists of all children who enter childcare within two months of the start of the school year in the year that they turn three (treatment group) or four (control group) in former-West Germany. Individual controls include migration background, mother's age at birth, sex of the child, a dummy for being a single parent and dummies for mother's years of education. Full controls include mother's employment status, dummies for household income and size. Regional controls include quintile dummies for the county population density, unemployment rate, share of foreigners, per capita GDP, as well as county level conservative vote share, and female employment share. Own calculations. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Source: NEPS Data, Data Version SC4: 7.0.0 remote access, RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1998 and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018).

We further test the validity of the instrument by falsification tests estimating the effect of an additional year of childcare on arguably pre-determined outcomes of children in the NEPS. Table 2.8 reports the results for these placebo regressions for height at birth and adolescence, an indicator for low birth weight, and premature birth, using the same specifications as in Table 2.3. To facilitate interpretation, we standardize the height variables to have mean zero and variance one. If our previous results are capturing a causal impact of earlier entry into childcare, then we should not observe any significant effects in these regressions. This is indeed what we find. None of these outcomes are statistically significantly affected by early childcare entry. Unfortunately, the birth characteristics are only available for less than half the sample. Thus, standard errors are large and the power to detect imbalances is limited. However, if anything, the effect of early childcare attendance on height at adolescence indicates a negative effect. Since height is generally positively related to extroversion this speaks against pre-determined characteristics driving the strong effects on extroversion.

The SOEP results with additional controls for parental Big Five traits in Section 2.6 alleviate concerns that our findings can be explained by regional differences in personality traits that correlate with the local supply of childcare places. Yet, to provide further evidence that our effects for extroversion are not driven by this, we also investigate regional differences in extroversion. To this end, we aggregate information on adults' personality traits in 2005 at the county-level from the SOEP. We use information for all adults in the SOEP rather than the sample restricted to parents used for our results in Table 2.6. This increases the sample to roughly 20,000 adult observations. 2005 is the first year that the Big Five personality traits were surveyed. The measures from 2005 should be good proxies for the regional distribution of extroversion among adults in 1998, because mobility is low in Germany and the Big Five have been shown to be relatively stable in adulthood (e.g. McCrae and Costa, 1996; Cobb-Clark and Schurer, 2012). Figure 2-4 (b) shows a map of these aggregated extroversion levels. There is no clear discernible pattern and regional differences in extroversion appear to be unrelated to the supply of childcare places shown in Panel (a). We also use these aggregated personality

Table 2.8: Estimation of one additional year of childcare on pre-determined outcomes (NEPS)

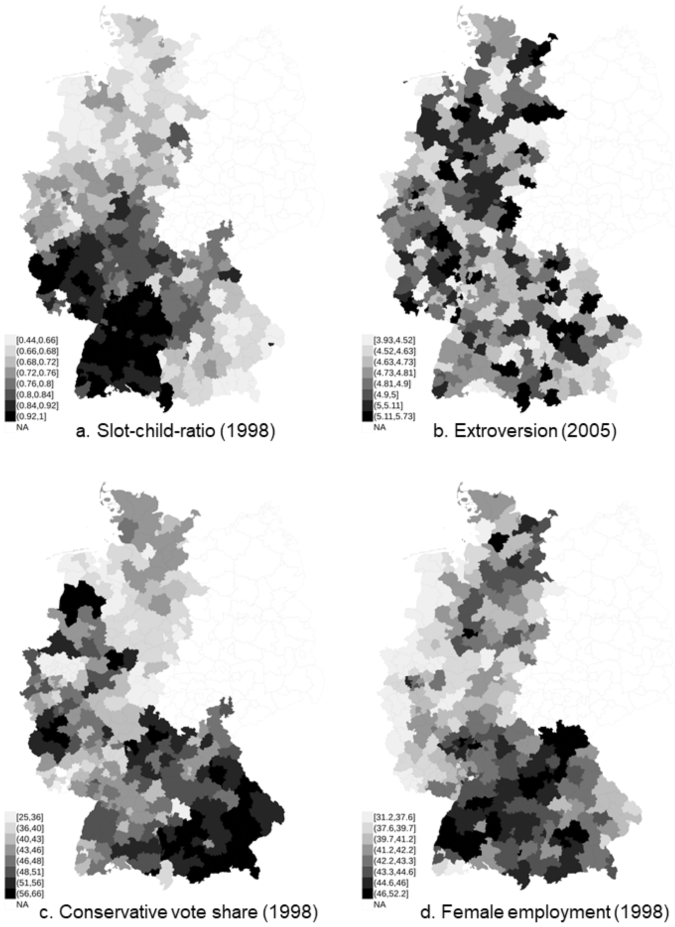
	Height (in SD)		**Height at birth (in SD)**		**Low birthweight (< 2,500 g)**		**Premature birth**	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Early childcare entry	-0.046	-0.160	0.006	-0.204	-0.012	0.013	-0.009	0.007
	(0.030)	(0.148)	(0.057)	(0.193)	(0.013)	(0.040)	(0.014)	(0.049)
N	4,464	4,464	1,806	1,806	1,855	1,855	2,069	2,069

Notes: The table shows OLS and 2SLS estimates where the county-level slot-child-ratio for 1998 is used as an instrument on one additional year of childcare. Standard errors are clustered at the county level and are given in parentheses. The displayed regression coefficient includes all controls from our preferred specification from Table 2.3, including migration background, mother's age at birth, sex of the child, a dummy for being a single parent, dummies for mother's years of education as individual controls, as well as mother's employment status, dummies for household income and size, and regional controls such as quintile dummies for the county population density, unemployment rate, share of foreigners, per capita GDP, conservative vote share, and female employment share. Own calculations. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Source: NEPS Data, Data Version SC4: 7.0.0 remote access, RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1998 and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018).

measures as additional regional controls in our baseline IV specifications. This produces very similar results to those in Table 2.6 reported above. As an additional robustness check, we drop counties from the federal state of Baden-Wuerttemberg from the analyses, since they have the highest supply of slots (see Figure 2-2 (a)). This gives very similar results, but standard errors increase substantially.

Figure 2-4: County level maps of different indicators related to demand and supply of childcare



Notes: This figure shows four maps. On the upper left-hand side (a) the slot-child-ratio of each county in western Germany measured in the year 1998 is depicted from Figure 2-2, showing a very heterogeneous level of childcare provision in 1998. The upper right-hand side (b) shows the aggregated personality trait extroversion of adults participating in the 2005 SOEP wave using county level data of 20,000 observations. In the lower left-hand side (c), the conservative vote share in 1998 is depicted, showing also heterogeneous levels among counties. The lower right-hand side (d) provides the female employment rate in 1998 in western Germany giving another indication for differences in among counties. Both figures in the lower part give some indication of factors related to the demand side of childcare in the late 1990s. Own graphical display.
Source: SOEP v32 (wave 2005), RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1998, and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018).

To further check whether differences in parental preferences across counties could drive our results, we plot in Figure 2-4 (c) conservative vote shares in the 1998 election of the German Federal Parliament as a proxy for regional political attitudes. The map shows clear patterns for regional differences in political attitudes. For example, the south-east of Germany is clearly more conservative with conservative vote shares between 46% and 66% compared to the north-west of Germany, where the conservative vote share ranges from 25% to 46%. Looking at Figure 2-4 (c), it shows that the conservative vote share and the availability of childcare places are negatively correlated, which is not surprising given that more liberal governments are more likely to invest in childcare places to increase maternal employment.[31] In the same vein, Figure 2-4 (d) shows that the higher supply of childcare in the south-west of Germany also corresponds to higher female employment. It is not clear how these regional differences in political attitudes and female employment affect our estimates. Therefore, all specification with regional controls include the conservative vote share and female employment rates as proxies for the demand for childcare.

Overall, the sensitivity analyses confirm the main findings presented in Section 2.5. For students who attend childcare starting from age three onward we find a large increase in extroversion at age fifteen compared to students who started approximately one year later.

2.8 Conclusion

This paper analyzes the long-run effect of early entry in universal childcare at age three compared to age four on personality traits. Specifically, we look at the effect of one additional year of childcare on the Big Five personality traits in adolescence when children are fifteen years old. We use German data from the NEPS and exploit geographical disparities in the local supply of childcare places in an instrumental variable approach. Both OLS and IV estimates suggest

[31] These results are available upon request.

that starting childcare earlier at age three versus age four significantly increases students' levels of extroversion in adolescence and to some extent conscientiousness. We can replicate these results in a smaller data set based on another nationwide representative sample from the SOEP.

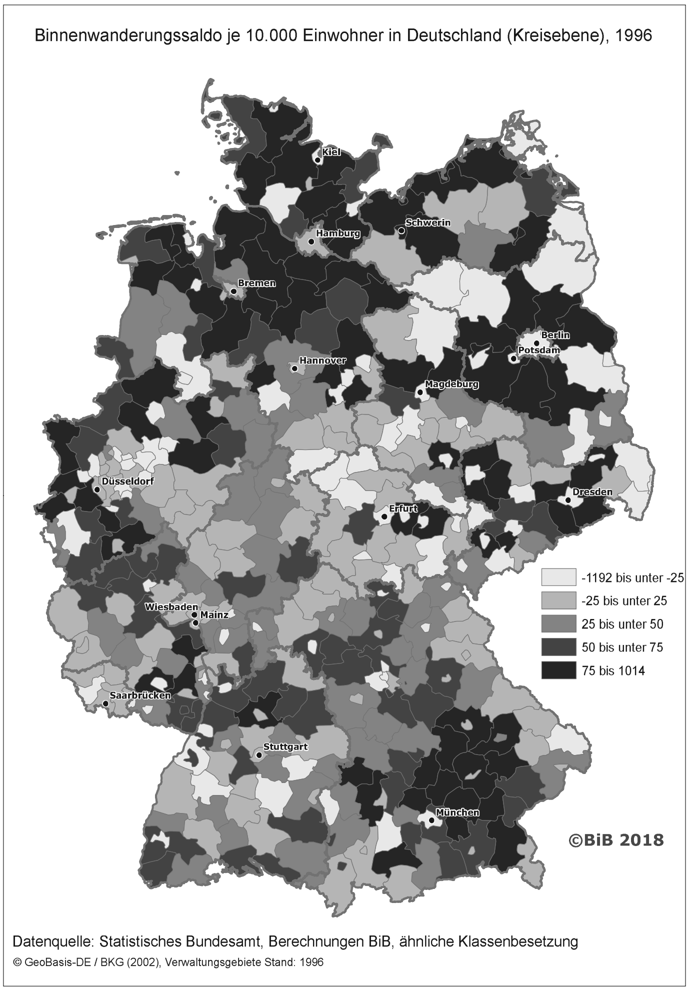
Our findings show that students' personality traits are affected by an additional year of universal childcare. This indicates that childcare impacts on personality traits beyond short-run outcomes. Our instrument, the slot-child-ratio in 1998 at the county level as an indicator for sufficient supply of childcare places, is a strong and relevant predictor of early childcare entry in the western federal states in Germany. We find a large and robust positive effect for the personality trait extroversion, i.e. for students' level of 'sociableness' and 'tendency to enjoy to interact in their surroundings'. This finding supplements previous findings on medium- or long-run effects of universal childcare attendance also accounting for unobserved heterogeneity in childcare participation (Apps et al., 2013; Baker et al., 2019; Kuehnle and Oberfichtner, 2020).

Our findings are also related to the economic literature using personality traits as input factors in estimations of even longer run outcomes, such as educational attainment and wages (for an overview, see Almlund et al., 2011). In particular, we show that personality traits in adolescence can be affected by early childhood experiences and environments. Studies in the psychological literature suggest that having a high score on extroversion helps individuals to react to negative life events, as they might be more able to rely on personal resources (for example, see findings of Sarubin et al., 2015). Fletcher (2013) provides evidence using sibling differences of young adults from the U.S. on the importance of personality measures for labor market success. In particular, he finds that extroversion leads to favorable labor market outcomes. A one standard deviation increase in extroversion increases employment by two pp and earnings by five to six pp which is robust over all specifications. When making the strong assumption of similar labor market norms in the U.S. and Germany, a rough back of the envelope calculation suggests that our results could translate into an increase of employment by 0.74 pp and an 1.85 percent increase in earnings (approximately 740 $ a year) for the compliers who attend one additional year of universal childcare. Future research might help to nourish this empirical question and to identify direct effects of childcare entry on labor market performance also within the European context.

Evaluating the implications of our study, we argue that our results are of particular interest to policy makers, as we find positive effects for children, who were shifted into early childcare through less restricted availability. For these complier children we observe a significant positive non-cognitive skill development, which supports to continue the roll-out of the childcare system and to remove remaining entrance barriers. Today, the public focus has shifted to children starting childcare even earlier below the age of three. Even though conclusions from our study cannot directly be transferred to younger age groups, we hypothesize that the underlying mechanisms assigning children into early or late entry persist until today. Parents and their younger children still face the same rationing as well as geographical variation in the provision of childcare discussed in this paper. Whether or not children's non-cognitive skill development is also affected by an early entry at age one remains an important question for future research. This paper, however, provides a strong case for including non-cognitive skill development as a core determinant when evaluating the long-term success of universal childcare programs.

2.9 Appendix

Figure 2-5: Internal migration in Germany per 10,000 people (1996)



Notes: This figure shows the net internal migration balance measuring the in- and outflow on the local level. Overall, internal migration numbers are very low. Counties shaded in black represent counties with a positive migration balance, lighter areas stand for counties with more people emigrating than migrating.
Source: German Federal Institute for Population Research (2018)

Table 2.9: Definition of personality traits

Personality Trait	**Definition**
Openness (to Experience)	Tendency to be open to new cultural or intellectual experiences
Conscientiousness	Tendency to be organized, responsible, and hardworking
Extroversion	Refers to sociableness, activeness, assertiveness, tendency to orientate one's energies to the outer world of people
Agreeableness	Tendency to act in cooperation, an unselfish manner, and flexibility
Neuroticism (Emotional stability)	Different facets of anxiety, insecurity, impulsiveness, and vulnerability

Notes: Information taken from Almlund et al. (2011).

Table 2.10: Big Five personality traits inventory (NEPS and SOEP)

Panel A: NEPS items	
	I am a person who...:
Openness	has few artistic interests (-), has an active imagination,
Conscientiousness	tends to be lazy (-), does a thorough job
Extroversion	is outgoing, sociable, is reserved (-)
Agreeableness	tends to find fault with others (-), is generally trusting
Neuroticism	gets nervous easily, is relaxed/handles stress well (-)
Panel B: SOEP items	
	I am a person who...:
Openness	has new ideas, values cultural experience, has an active imagination, is inquisitive
Conscientiousness	works thoroughly, is lazy (-), handles her tasks efficiently
Extroversion	is communicative, mixes well, is reserved/guarded (-)
Agreeableness	sometimes manhandles others (-), forgives, is considerate
Neuroticism	worries a lot, is easily flustered, is relaxed/deals well with stress (-)

Notes: For Panel A information is taken from Rammstedt and John (2007) and for Panel B from the SOEP youth questionnaire. The (-) sign indicates items that are reversely coded.

Table 2.11: First stage estimation of slot-child-ratio instrument in 1994 on one additional year of childcare (NEPS)

	First stage IV estimation			
	(1)	(2)	(3)	(4)
Slot-child-ratio 1994	0.763***	0.777***	0.969***	0.931***
	(0.077)	(0.075)	(0.075)	(0.085)
Birth month FE	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	Yes
Full controls	No	No	Yes	Yes
Regional controls	No	No	No	Yes
N	4,700	4,700	4,700	4,700
R^2	0.036	0.073	0.080	0.102
First-stage F-test	99.47	106.32	107.66	129.30

Notes: The table shows first stages estimates where the county level slot-child-ratio for 1994 is used as an instrument on one additional year of childcare. Standard errors are clustered at the county level and are given in parentheses. The sample for the models in columns (1) through (4) consists of all children who enter childcare within two months of the start of the school year in which they turn three (treatment group) or four (control group). Individual controls include migration background, mother's age at birth, sex of the child, a dummy for being a single parent and dummies for mother's years of education. Full controls further include mother's employment status, dummies for household income and size. Regional controls include quintile dummies for the county population density, unemployment rate, share of foreigners, per capita GDP, as well as county level conservative vote share, and female employment share. Own calculations. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.
Source: NEPS Data, Data Version SC4: 7.0.0 remote access, RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1994 and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018).

Table 2.12: First stage estimation slot-child-ratio instrument on one additional year of childcare (SOEP)

	First stage estimation		
	(1)	(2)	(3)
Slot-child-ratio	1.164***	1.166***	1.382***
	(0.167)	(0.164)	(0.186)
Full controls	Yes	Yes	Yes
Parents' personality controls	No	Yes	Yes
Regional controls	No	No	Yes
R^2	0.236	0.229	0.277
First-stage F-test	17.495	30.284	14.952

Notes: The table shows first stage estimates of the instrument slot-child-ratio on one more year of childcare for the SOEP main sample (N=631). Birth cohorts 1990-1994 are matched to the regional slot-child-ratio for the year 1994, and birth cohorts 1995-1998 to the slot-child-ratio for 1998. Standard errors are clustered at the county level and are given in parentheses. The sample for models in columns (1) through (3) consists of all children who enter childcare in the year that they turn three (treatment group) or four (control group) in western Germany. Full controls include the child's gender, migration background as well as dummies for the children's birth year and month, the mother's age at birth, a dummy for being a single parent, dummies for her education background and employment status, dummies for household income and size. Parents' personality controls comprise Big Five personality traits of mothers and (social) fathers. Regional controls include the county's population density, unemployment rate, female employment rate, share of foreigners, per capita GDP, and conservative vote share. Own calculations. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Source: SOEP v32 (2007-2015, birth cohorts 1990-1998), RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1994 and 1998, and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018),

Table 2.13: Summary statistics of additional child characteristics and regional characteristics by treatment status (SOEP)

	Control	Treatment	Diff
Child characteristics:			
Male	0.52	0.45	0.08
Migration background	0.38	0.29	0.08*
Birthorder:			
First-Born Child	0.40	0.33	0.07
Second-Born Child	0.32	0.39	-0.07
Third-Born Child	0.13	0.12	0.01
Fourth-Born Child	0.05	0.04	0.01
Fifth- or Higher-Born Child	0.03	0.01	0.02*
Missing	0.06	0.10	-0.04
Maternal characteristics:			
Maternal Employment:			
Full Time	0.01	0.06	-0.05***
Part Time	0.23	0.32	-0.10**
Not Working	0.69	0.51	0.18***
Missing	0.07	0.11	-0.04
Maternal Education:			
Less than High School	0.22	0.17	0.04
High School	0.54	0.54	0.00
More than High School	0.16	0.16	-0.01
Missing	0.09	0.13	-0.04
Single parent	0.05	0.06	-0.01
Mother's age at birth	28.70	29.43	-0.73
Household characteristics:			
Household Income:			
First Quartile	0.27	0.14	0.12***
Second Quartile	0.28	0.26	0.02
Third Quartile	0.21	0.21	0.00
Fourth Quartile	0.17	0.26	-0.09**
Missing	0.08	0.13	-0.05*
Number of Children in Household:			
One Child	0.28	0.26	0.03
Two Children	0.40	0.48	-0.08*
Three Children	0.16	0.11	0.06*
Four or More Children	0.08	0.05	0.03
Missing	0.07	0.11	-0.04
Regional characteristics:			
Slot-child-ratio	0.64	0.72	-0.08***
Population density (in people/km^2)	783.02	811.97	-28.95
Unemployment rate	8.89	8.35	0.53*
Share of foreigners	9.73	10.31	-0.57
Per capita GDP (in 1000 Euro)	25.84	26.53	-0.69
Conservative vote share	44.34	43.57	0.78
Female employment share	41.76	42.73	-0.97**
Adult extroversion (SOEP 2005)	4.78	4.82	-0.04*
N	263	368	631

Notes: This table depicts additional summary statistics for the SOEP main sample, separately for treatment and control group and their difference. Own calculations. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.
Source: SOEP v32 (2007-2015, birth cohorts 1990-1998) and SOEP v32 (wave 2005), RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1994 and 1998, and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018)

Table 2.14: Estimation of one additional year of childcare on adolescents' personality traits (NEPS)

	O		**C**		**E**		**A**		**N**	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early childcare entry	0.059*	0.026	-0.088***	0.251	0.107***	0.367**	-0.040	-0.014	0.011	-0.026
	(0.031)	(0.173)	(0.033)	(0.170)	(0.032)	(0.171)	(0.034)	(0.192)	(0.035)	(0.172)
Migration backgr.	0.141***	0.141***	-0.025	-0.028	0.012	0.010	0.076**	0.076**	0.022	0.022
	(0.037)	(0.037)	(0.036)	(0.036)	(0.037)	(0.037)	(0.033)	(0.033)	(0.039)	(0.038)
Mother's years of edu.										
10 years	0.155	0.155	-0.228**	-0.223**	0.098	0.101	-0.178*	-0.178*	0.045	0.045
	(0.101)	(0.100)	(0.096)	(0.094)	(0.097)	(0.095)	(0.101)	(0.101)	(0.111)	(0.110)
12 years	0.154**	0.154**	-0.089	-0.092	0.061	0.059	-0.108	-0.108	0.002	0.002
	(0.074)	(0.073)	(0.071)	(0.073)	(0.074)	(0.075)	(0.073)	(0.072)	(0.069)	(0.068)
13 years	0.195***	0.196***	-0.138**	-0.142**	0.083	0.079	-0.058	-0.058	-0.024	-0.024
	(0.070)	(0.069)	(0.066)	(0.068)	(0.072)	(0.072)	(0.074)	(0.073)	(0.064)	(0.064)
15 years	0.182**	0.182**	-0.219***	-0.221***	0.035	0.033	-0.167**	-0.167**	-0.052	-0.052
	(0.075)	(0.074)	(0.071)	(0.072)	(0.077)	(0.077)	(0.076)	(0.075)	(0.068)	(0.067)
16 years	0.311***	0.311***	-0.247***	-0.249***	-0.044	-0.046	-0.182*	-0.183*	0.111	0.111
	(0.101)	(0.100)	(0.092)	(0.093)	(0.105)	(0.104)	(0.101)	(0.100)	(0.092)	(0.091)
18 years	0.343***	0.344***	-0.146*	-0.150*	0.069	0.065	-0.008	-0.008	0.005	0.006
	(0.084)	(0.084)	(0.080)	(0.081)	(0.084)	(0.084)	(0.092)	(0.091)	(0.081)	(0.080)
Missing	0.028	0.033	0.243	0.195	0.035	-0.007	-0.101	-0.105	0.247	0.252
	(0.142)	(0.143)	(0.169)	(0.170)	(0.145)	(0.148)	(0.160)	(0.161)	(0.157)	(0.157)
Father's years of edu.										
10 years	-0.132	-0.131	-0.094	-0.113	-0.133	-0.143	0.113	0.111	0.032	0.034
	(0.131)	(0.130)	(0.148)	(0.146)	(0.129)	(0.130)	(0.161)	(0.160)	(0.143)	(0.143)
12 years	-0.057	-0.057	-0.154	-0.147	-0.120	-0.114	0.057	0.057	0.093	0.092
	(0.090)	(0.089)	(0.104)	(0.102)	(0.113)	(0.111)	(0.133)	(0.132)	(0.117)	(0.116)
13 years	-0.078	-0.079	-0.161	-0.158	-0.123	-0.121	0.040	0.040	0.092	0.092
	(0.093)	(0.092)	(0.104)	(0.102)	(0.117)	(0.114)	(0.134)	(0.133)	(0.117)	(0.115)
15 years	-0.001	-0.002	-0.246**	-0.236**	-0.043	-0.035	0.036	0.037	-0.010	-0.012
	(0.106)	(0.106)	(0.115)	(0.114)	(0.125)	(0.121)	(0.138)	(0.138)	(0.123)	(0.122)
16 years	0.035	0.034	-0.158	-0.151	-0.075	-0.069	0.005	0.006	0.072	0.071
	(0.107)	(0.106)	(0.114)	(0.113)	(0.128)	(0.125)	(0.143)	(0.142)	(0.126)	(0.125)
18 years	-0.010	-0.010	-0.198*	-0.199*	-0.108	-0.109	-0.040	-0.040	0.039	0.039
	(0.102)	(0.101)	(0.116)	(0.112)	(0.122)	(0.122)	(0.134)	(0.132)	(0.126)	(0.125)
Missing	-0.051	-0.050	-0.163	-0.166	-0.014	-0.017	0.066	0.066	0.119	0.119
	(0.094)	(0.094)	(0.110)	(0.106)	(0.113)	(0.111)	(0.135)	(0.133)	(0.112)	(0.111)
Single parent	-0.035	-0.034	-0.019	-0.030	-0.048	-0.056	-0.122*	-0.123*	0.020	0.021
	(0.068)	(0.068)	(0.070)	(0.069)	(0.075)	(0.075)	(0.068)	(0.068)	(0.072)	(0.072)
Mother's age at birth	-0.000	-0.000	0.004	0.004	-0.010**	-0.010**	-0.001	-0.001	0.008**	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Male	0.020	0.020	0.023	0.030	-0.017	-0.011	-0.002	-0.001	0.024	0.023
	(0.030)	(0.030)	(0.028)	(0.028)	(0.030)	(0.030)	(0.030)	(0.031)	(0.028)	(0.028)
Mother's working status										
Working	-0.108**	-0.107**	-0.047	-0.054	0.085**	0.078*	-0.041	-0.041	-0.050	-0.050
	(0.047)	(0.048)	(0.046)	(0.046)	(0.041)	(0.040)	(0.044)	(0.044)	(0.044)	(0.044)
Missing	-0.039	-0.042	-0.225	-0.193	0.245	0.272	-0.117	-0.115	-0.323*	-0.327*
	(0.166)	(0.165)	(0.189)	(0.187)	(0.175)	(0.176)	(0.170)	(0.169)	(0.190)	(0.189)
Household income										

Continued on next page

Table 2.14: Estimation of one additional year of childcare on adolescents' personality traits (NEPS) (continued)

	O		C		E		A		N	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Second quartile	0.035	0.035	-0.014	-0.013	0.039	0.039	0.034	0.034	-0.045	-0.045
	(0.051)	(0.051)	(0.051)	(0.051)	(0.048)	(0.048)	(0.057)	(0.057)	(0.049)	(0.049)
Third quartile	0.050	0.051	-0.006	-0.019	0.102*	0.092	0.016	0.015	-0.064	-0.062
	(0.057)	(0.056)	(0.054)	(0.055)	(0.055)	(0.057)	(0.059)	(0.059)	(0.057)	(0.058)
Fourth quartile	0.046	0.048	-0.015	-0.038	0.233***	0.215***	-0.051	-0.053	-0.111**	-0.109*
	(0.059)	(0.058)	(0.057)	(0.059)	(0.058)	(0.061)	(0.055)	(0.055)	(0.055)	(0.057)
Missing	-0.015	-0.014	0.017	0.013	0.185***	0.182***	-0.025	-0.026	-0.078	-0.078
	(0.057)	(0.057)	(0.052)	(0.053)	(0.057)	(0.059)	(0.053)	(0.053)	(0.052)	(0.052)
Household size										
Two members	-0.282	-0.288	0.117	0.177	0.553*	0.600**	-0.040	-0.036	0.271	0.264
	(0.280)	(0.278)	(0.329)	(0.332)	(0.282)	(0.283)	(0.245)	(0.244)	(0.284)	(0.281)
Three members	-0.415	-0.420	0.109	0.156	0.430	0.467	-0.180	-0.177	0.297	0.291
	(0.278)	(0.276)	(0.328)	(0.332)	(0.290)	(0.290)	(0.234)	(0.232)	(0.267)	(0.263)
Four members	-0.525*	-0.531*	0.177	0.238	0.382	0.431	-0.200	-0.195	0.383	0.376
	(0.280)	(0.278)	(0.340)	(0.343)	(0.288)	(0.289)	(0.227)	(0.227)	(0.271)	(0.266)
Five members	-0.554*	-0.562**	0.189	0.259	0.364	0.420	-0.139	-0.134	0.458*	0.450*
	(0.284)	(0.283)	(0.337)	(0.342)	(0.299)	(0.300)	(0.226)	(0.227)	(0.272)	(0.268)
Six members+	-0.565**	-0.572**	0.213	0.280	0.264	0.318	-0.155	-0.150	0.484*	0.476*
	(0.286)	(0.285)	(0.338)	(0.342)	(0.288)	(0.289)	(0.237)	(0.238)	(0.266)	(0.261)
Missing	0.748**	0.718**	-0.876***	-0.564	2.331***	2.572***	-1.859***	-1.835***	2.175***	2.141***
	(0.304)	(0.336)	(0.335)	(0.377)	(0.299)	(0.332)	(0.323)	(0.362)	(0.294)	(0.318)
Population density										
Second quintile	0.071	0.070	0.150**	0.158**	-0.043	-0.037	0.053	0.053	0.077	0.076
	(0.060)	(0.060)	(0.064)	(0.064)	(0.060)	(0.058)	(0.074)	(0.073)	(0.064)	(0.063)
Third quintile	0.078	0.078	0.179***	0.173***	-0.037	-0.042	0.100	0.099	-0.022	-0.022
	(0.054)	(0.053)	(0.065)	(0.067)	(0.062)	(0.060)	(0.079)	(0.079)	(0.066)	(0.065)
Fourth quintile	0.050	0.051	0.140**	0.128*	-0.002	-0.012	0.112	0.111	-0.003	-0.001
	(0.058)	(0.058)	(0.070)	(0.073)	(0.069)	(0.067)	(0.080)	(0.081)	(0.074)	(0.073)
Fifth quintile	0.117	0.118	0.196*	0.184*	0.120	0.110	0.125	0.124	-0.028	-0.027
	(0.075)	(0.075)	(0.105)	(0.102)	(0.085)	(0.082)	(0.105)	(0.104)	(0.094)	(0.092)
Unemployment rate										
Second quintile	-0.008	-0.008	-0.122**	-0.119**	-0.016	-0.011	-0.127***	-0.127***	-0.037	-0.037
	(0.048)	(0.048)	(0.051)	(0.053)	(0.044)	(0.045)	(0.049)	(0.048)	(0.041)	(0.041)
Third quintile	0.068	0.066	-0.086	-0.062	-0.005	0.015	-0.136**	-0.134**	-0.009	-0.012
	(0.060)	(0.063)	(0.060)	(0.062)	(0.056)	(0.057)	(0.064)	(0.066)	(0.054)	(0.054)
Fourth quintile	0.028	0.026	-0.136	-0.114	0.009	0.026	-0.199**	-0.197**	-0.044	-0.047
	(0.073)	(0.074)	(0.091)	(0.089)	(0.076)	(0.075)	(0.088)	(0.088)	(0.076)	(0.077)
Fifth quintile	0.244*	0.242*	-0.329**	-0.315**	-0.340***	-0.327***	-0.412*	-0.411*	-0.099	-0.101
	(0.128)	(0.127)	(0.141)	(0.142)	(0.096)	(0.095)	(0.219)	(0.219)	(0.125)	(0.124)
Fraction of Foreigners										
Third quintile	0.044	0.044	-0.147***	-0.140***	-0.008	-0.002	0.052	0.053	-0.011	-0.011
	(0.053)	(0.053)	(0.054)	(0.054)	(0.049)	(0.048)	(0.068)	(0.068)	(0.052)	(0.051)
Fourth quintile	0.074	0.075	-0.150**	-0.162***	0.022	0.015	0.046	0.046	-0.040	-0.039
	(0.063)	(0.062)	(0.059)	(0.059)	(0.058)	(0.057)	(0.073)	(0.072)	(0.059)	(0.058)
Fifth quintile	0.045	0.046	-0.160**	-0.166**	-0.020	-0.024	0.069	0.068	0.046	0.047

Continued on next page

Table 2.14: Estimation of one additional year of childcare on adolescents' personality traits (NEPS) (continued)

	O		C		E		A		N	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	(0.069)	(0.068)	(0.070)	(0.070)	(0.067)	(0.066)	(0.077)	(0.076)	(0.071)	(0.069)
Per capita GDP										
Second quintile	-0.061	-0.060	0.114	0.107	-0.015	-0.020	0.022	0.021	-0.115	-0.114
	(0.085)	(0.085)	(0.092)	(0.091)	(0.057)	(0.061)	(0.084)	(0.083)	(0.095)	(0.094)
Third quintile	-0.009	-0.009	0.065	0.060	-0.046	-0.050	0.076	0.076	-0.022	-0.022
	(0.089)	(0.088)	(0.088)	(0.086)	(0.059)	(0.062)	(0.082)	(0.081)	(0.097)	(0.096)
Fourth quintile	-0.079	-0.077	0.098	0.074	-0.052	-0.070	0.011	0.009	-0.106	-0.103
	(0.086)	(0.086)	(0.088)	(0.088)	(0.053)	(0.060)	(0.082)	(0.082)	(0.090)	(0.090)
Fifth quintile	-0.158*	-0.156*	0.089	0.074	-0.084	-0.095	0.027	0.026	-0.119	-0.118
	(0.094)	(0.093)	(0.100)	(0.098)	(0.066)	(0.069)	(0.095)	(0.095)	(0.100)	(0.099)
Conservative vote share	-0.002	-0.002	0.003	0.004	0.003	0.004	0.004	0.005	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)
Female employment share	0.008	0.008	-0.008	-0.011	0.011	0.009	0.004	0.004	-0.015**	-0.015**
Birth month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	4,579	4,579	4,579	4,579	4,579	4,579	4,579	4,579	4,579	4,579

Notes: O=Openness, C=Conscientiousness, E=Extroversion, A=Agreeableness, N=Neuroticism. The table shows OLS and 2SLS estimates where the county-level slot-child-ratio for 1998 is used as an instrument on one additional year of childcare. Standard errors are clustered at the county level and are given in parentheses. The sample for the models in columns (1) through (10) consists of all children who enter childcare within two months of the start of the school year in which they turn three (treatment group) or four (control group) in western Germany. For regional variable "fraction of foreigners" the second quintile is the reference category, since there are no counties from the first quintile in our sample. Own calculations. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.
Source: NEPS Data, Data Version SC4: 7.0.0 remote access, RDC of the Federal Statistical Office and the Statistical Offices of the Länder on the county level, statistics of children and youth welfare (*Kinder- und Jugendhilfestatistik*) for 1998 and INKAR data for 1998 (German Federal Office for Building and Regional Planning, 2018).

Chapter 3

The Length of Schooling and the Timing of Family Formation[1]

Abstract

Individuals typically traverse several life phases before forming a family. We analyse whether changing the duration of one of these phases, the education phase, affects the timing of marriage and childbearing. For this purpose, we exploit the introduction of short school years in Germany in 1966-67, which compressed the education phase without affecting the curriculum. Based on difference-in-differences regressions and German Micro Census data, we find that short school year exposure affects the timing of marriage for individuals in all secondary school tracks and shifts forward the birth of the first child mainly for academic-track graduates. This highlights that education policies might not only affect family formation through human capital accumulation, but also through changing the duration of earlier life phases. This is important as not only age at marriage and first birth increases in many countries, but also the duration of the education phase.

[1]This chapter is joint work with Jan Marcus. Published in *CESifo Economic Studies*, Volume 68(1), pages 1-45.

3.1 Introduction

Almost all industrialized countries see a secular trend toward postponing family formation to later ages (OECD, 2019) – with important economic consequences. Postponed childbearing decreases – all else equal – the number of children born per year, putting the funding of social security systems under pressure (e.g. Billari et al., 2006). The timing of marriages and first births also have large impacts on other economic decisions, including on savings and the spacing of subsequent births (Díaz-Giménez and Giolito, 2013; Hodsdon and Marini, 2019). Additionally, medical costs increase with later child births due to adverse health effects (e.g. Gustafsson, 2001; Myrskylä and Fenelon, 2012) and because fecundity declines with age (Larsen and Vaupel, 1993) making fertility treatments and involuntary childlessness more likely.

This paper contributes to our understanding of whether and how policies that alter the duration of specific life phases can act as drivers of marriage and fertility timing. In most developed countries, individuals traverse several life phases in a rather strict order before they form a family (Blossfeld and Rose, 1992; Billari et al., 2000; Lutz and Skirbekk, 2005; Huinink and Kohli, 2014). Primary and secondary schooling precede tertiary education, education phases typically precede labor market entry and labor market entry typically precedes family formation. Given the sequencing of life phases, there is surprisingly little research on the consequences of extending or reducing one of these phases. However, analyzing the duration of earlier life phases is not only relevant in explaining later childbearing and decreasing fertility rates, but it may also offer a potential lever for public policies.

We study a policy change in Germany that reduced the length of the education phase. This policy allowed entire cohorts to graduate from secondary school about eight months earlier – with the same degree and the same curriculum. Before the reform, the school year started in spring in some German states and in fall in other states. After a policy change to harmonize the education system across states, school started in fall in all states. States achieved the shift of the school year start from spring to fall by the introduction of so-called short school years (*Kurzschuljahre*), in which two school years were put in about 16 calendar months in 1966/67. Pischke (2007) analyses this reform as well. He finds that the short school years did not have any negative impacts on human capital acquisition as it reduced neither labor income nor employment prospects; a finding that we replicate in this paper. Further, Braakmann (2010) shows that there are no effects of the reform on health outcomes. Similar to Pischke (2007) and Braakmann (2010), we exploit this reduction of the length of schooling in a difference-in-differences framework, where we compare cohorts before and after the reform in affected states with the same cohorts in states that did not introduce short school years. We pool several cross-sections of the German Micro Census, a one percent sample of the German population, to study the effect of this reform on the timing of marriage and fertility.

We find that short school year exposure affects the timing of marriage and child birth. More specifically, we show that the short school years increase the probability to be married eight years after the normal graduation age by about 3.7 percentage points (pp) for individuals in the middle track and by about 4.1 pp for individuals in the academic track (compared to sample means of 43 and 37 percent, respectively). Moreover, individuals in the academic track are also 3.2 pp more likely to have a first child eight years after graduation (compared to a sample mean of 22 percent). These effects do not only hold eight years after graduation but also five years after graduation and up to ten years after graduation. However, the effects fade over time, indicating that the short school years affect the timing of marriage and parenthood, but not the probability to ever marry or become a parent. We further show that the obtained effects are driven by both males and females. Additionally, we find suggestive evidence that the reform also affects subsequent births and completed fertility for individuals in the academic track. Our findings provide evidence that policies that change the duration of specific life phases can affect family formation.

Our study contributes to the literature on the relationship between education and family formation. There is ample evidence that higher levels of education are associated with later childbearing (see, e.g., Skirbekk, 2008) and marriage (see, e.g., Jejeebhoy, 1995; Oppenheimer, 1997) in various countries and time periods. The literature discusses mainly lock-in and human capital effects as mechanisms why education can causally affect family formation. The lock-in effect means that individuals are less likely to marry and give birth while in school (Black et al., 2008), e.g. due to a high degree of economic dependence on the parents or the incompatibility of child rearing and acquiring education (Blossfeld and Rose, 1992). The human capital effect relates to the idea that education increases labor market opportunities and, thereby, the opportunity costs of children (Becker, 1981).[2] While the former mechanism relates only to family formation during education, the latter mechanism looks at family formation after the education phase.

[2]Becker's theoretical approach targets marriage and fertility behavior alike, as his approach regards child production and rearing as the main purpose of marriage. He formulates the argument of sex-specific division of labor as an incentive to enter into marriage and also with respect to the decision to have children.

In order to empirically test whether the negative education-fertility relationship is causal, many empirical studies capitalize on unintended fertility consequences induced by educational reforms. Most studies use exogenous variation from laws changing age at school entry or compulsory schooling reforms. The effect of education using school entry rules is found to be more profound with respect to teenage pregnancies (Black et al., 2011; Tan, 2017), while McCrary and Royer (2011) find little evidence for school entry policies affecting age at first birth. Almost all studies on compulsory schooling reforms provide evidence that longer educational attainment leads to postponement of first births (see, e.g., Black et al., 2008; Monstad et al., 2008; Silles, 2011; Cygan-Rehm and Maeder, 2013; Grönqvist and Hall, 2013). The empirical evidence regarding the effect on completed fertility is more mixed. While some studies find that education decreases completed fertility (see Cygan-Rehm and Maeder (2013); Fort et al. (2016) for England), other studies show that education has no effect on completed fertility (Monstad et al. (2008); Fort et al. (2016) for Continental Europe). Furthermore, Devereux and Tripathi (2009) find that increasing the length of compulsory schooling also leads to higher ages at first marriage.

There are also some studies that exploit institutional changes at higher levels of the educational system. Currie and Moretti (2003) and Kamhöfer and Westphal (2019) use college expansions in the U.S. and Germany, respectively, as an instrument for education. Currie and Moretti (2003) find that higher educational attainment reduces completed fertility and Kamhöfer and Westphal (2019) find that increasing education affects the timing of childbirth and reduces the probability of becoming a mother.

We contribute to the literature on the relationship between education and family formation by proposing a third causal mechanism, a duration effect. Education might affect family formation since it affects the timing of subsequent life phases (in particular, labor market entry), which individuals typically traverse before forming families. It is very difficult to separate this duration effect from the human capital effect and, actually, all studies that rely on post-education effects of compulsory schooling reforms and college expansions look at the combined human capital and duration effect. The short school years are, therefore, a particular policy reform as (i) this reform allowed for earlier graduation from school without affecting the curriculum; and (ii) previous empirical studies find no evidence that this reform had adverse effects for human capital acquisition (Pischke, 2007; Braakmann, 2010). The idea of the duration effect of education is also in line with the finding of Humlum et al. (2017) that delayed college enrollment leads to the postponement of marriages and childbearing.

Our study also contributes to the literature on policies that affect family formation by highlighting the importance of unintended consequences of policies that reduce or extend specific life phases. While pro-natalist and pro-marriage policies are highly controversial (see, e.g., Cherlin, 2003), it is important to know whether and how existing policies affect family formation, irrespective of the normative standpoint. Many empirical fertility studies focus on the impact of specific family policies including direct financial transfers like child allowances and fiscal incentives (e.g., Björklund, 2006) as well as work-related family policies like parental leave benefits (e.g., Lalive and Zweimüller, 2009; Cygan-Rehm, 2016; Kluve and Schmitz, 2018; Raute, 2019) and child care availability (e.g., Rindfuss et al., 2010; Mörk et al., 2013; Bauernschuster et al., 2016). In her literature review, Gauthier (2007) concludes that several family policies are found to increase fertility but that the magnitude of these effects is small. There are also several studies that deal with the effect of specific policies on the marital status. These policies almost exclusively focus on financial incentives; for instance, tax penalties (Alm and Whittington, 1997; Baker et al., 2004) and benefits (Fink, 2020), welfare expansion (Halla et al., 2016), the elimination of survivors insurance (Persson, 2020), and cash-on-hand marriage subsidies (Frimmel et al., 2014). While the majority of these studies provide evidence that financial incentives affect the timing of marriage, only some find effects on the probability to ever marry.

Our study emphasizes that family formation is not only affected by pro-natalist or pro-marriage policies, but also as a side effect of other policies and institutional features. Related to the idea that couples prefer to achieve a certain level of financial security before childbirth, Auer and Danzer (2016) show that for women in Germany, starting a career with a fixed-term contract is associated with postponement of first births and a lower number of children, even after ten years. Similarly, policies that alter the duration of the education phase might affect family formation. This is not only important for policymakers to keep in mind when discussing education reforms, but it also might offer a tool for those who would like to change the timing of family formation. Further, our study points out that the consequences for family formation should be considered when discussing policies that affect the duration of specific life phases. This is important, for instance, in current debates about the European Bologna reform shortening the time to a fist university degree (Hahm and Kluve, 2019), the suspension (and re-introduction) of compulsory military service in several countries (Imbens and van der Klaauw, 1995; Bauer et al., 2012), the German G8 reform shortening the schooling phase (Huebener and Marcus, 2017; Marcus and Zambre, 2019), and the general education expansion occurring in many developed and developing countries.

Therefore, our study contributes to both the literature on the relationship between education and family formation as well as the literature on policies that affect family formation. An additional contribution that this study has to offer is the detailed compilation of dates (and primary sources) for several relevant education reforms in West Germany, where there is some ambiguity in the previous literature. These reforms do not just include the short school years but also regulations regarding school entry ages, the beginning of the school year, and compulsory schooling. The collection of reform dates and law sources allows for isolating the short school year reform from previous changes of the school year start and assigning the short school years more precisely compared to previous studies. This framework that we propose in Appendix 3.10 and the accompanying discussion paper Koebe and Marcus (2020) can also be used by other researchers.

The remainder of the paper is structured as follows: Section 3.2 describes the institutional setting and Section 3.3 the data, while Section 3.4 outlines the empirical strategy. This is followed by the main results in Section 3.5, sensitivity analyses in Section 3.6 and additional results on human capital, gender differences, longer time horizons, and subsequent births in Section 3.7. Section 3.8 concludes.

3.2 Institutional background

We study a policy change in West Germany in 1966/67 that reduced the length of the education phase by introducing short school years (SSY). These short school years compressed two school years into 16 calendar months. The SSY were introduced in an effort to harmonize the start of the school year across states.

In October 1964, the Ministers of Education of the West German federal states decided, in what is known as the Hamburg Accord *(Hamburger Abkommen)*, that the school year would begin in the fall in all eleven states (Froese, 1969, pp.327-323). Before this decision, Easter marked the begin of the school year in most states, while in Bavaria the school year began in fall.[3]

[3] This was actually not the first change of the start of the school year: During the Nazi regime, in 1941, the start of the school year was shifted to a common start in fall (see *Reichsgesetzgebung* in 1941 in 3.29 in Appendix 3.10), after the Second World War most federal states successively switched back to a starting date at Easter (KMK, 1962). In February 1955, the states' Ministers of Education proclaimed Easter as a uniform start of the school year across all federal states in the so-called Düsseldorf Accord *(Düsseldorfer Abkommen)* (Froese, 1969, pp.307-311). However, the Bavarian parliament voted against the implementation of this resolution. As a result, children in Bavaria have started their school year in the fall ever since 1941. For a comprehensive collection of schooling laws related to the shift of the start of the school year in the German federal states, see Appendix 3.10.

Seven states (Baden-Württemberg, Bremen, Hesse, North Rhine-Westphalia, Rhineland-Palatinate, Saarland, Schleswig-Holstein) achieved the shift of the school year start by introducing so-called short school years *(Kurzschuljahre)*, in which two school years were put in about 16 calendar months (see Helbig and Nikolai, 2015, p.70-73): The first SSY started on April 1, 1966, and ended on November 30, 1966, while the second SSY year started on December 1, 1966, and ended on July 31, 1967. Due to these short school years, affected individuals graduated about two-thirds of a school year earlier (or about eight months of calendar time), but with the same degree and curriculum taught.[4]

Three states did not introduce SSY: Bavaria (where schools already started in fall before the Hamburg Accord) as well as Hamburg and West-Berlin. The two latter states opted for a long school year to transition to the uniform start in fall. In both states, students who were in their graduating year and had once begun their school career with a school start at Easter also graduated in March. Hence, students from Berlin and Hamburg attended the regular amount of time required without any school year reductions despite the school year transition to a start in the fall.

In West Germany, students in all states opt for one of three secondary school tracks after four years of joint primary schooling at around age ten (see Dustmann et al. (2017) for a detailed synopsis on the German tracking system): basic (*Hauptschule*, grades 5-8/9), middle (*Realschule*, grades 5-10), or academic track (*Gymnasium*, grades 5-13). Generally, the shifting of the school year start in 1966/67 affected students in primary school as well as in all secondary school tracks. Several states changed compulsory schooling regulations during our observation period from eight to nine years (Backhaus, 1963; Leschinsky and Roeder, 1980; Petzold, 1981). These changes mainly affect students in basic track and we discuss compulsory schooling regulations in more detail in Appendix 3.10 and potential consequences for our estimates in the robustness section (see Section 3.6).

[4]Pischke (2007) provides evidence that the curriculum was not affected by SSY, meaning that human capital acquisition is likely to be unaffected.

Lower Saxony was the only state that differentiated between tracks in terms of SSY exposure. Basic track students were unaffected by SSY, as for graduating classes their short school year losses were added in their final year. Depending on their school starting cohort, middle track students were ambiguously affected by SSY due to changing regulations with respect to the graduating classes in the years after the short school years,[5] while academic track students were fully affected by the policy reform without any school year extensions for graduating classes. Pischke (2007) assigns the seven states that introduced short school years to the treatment group, while he assigns the three states that did not introduce short school years to the control group. Lower Saxony is partly assigned to the treatment group and partly due to the control group, depending on the institutional details described above.

3.3 Data

Our analysis uses mainly data from the German Micro Census (RDC, 2019), a one percent sample of all German households. Once drawn for the survey, participation is mandatory and, hence, selective non-response and attrition is not a concern. We use the scientific use file, a 70 percent random sample of the data, and the 18 waves from 1976 to 2003.[6] The data set is well-suited for our analysis as it contains rich information on family structure, marriage, and education. Importantly, each of the 18 waves includes about 300,000 to 400,000 individual observations in the West German states, providing a large number of observations in target cohorts and allowing for a precise estimation of reform effects.

[5] As described in more detail by Pischke (2007), students entering their final year with the first SSY were exposed to one SSY, the next three cohorts starting their final year in December 1966 through August 1968 were exposed to two SSY. The next six cohorts, however, who were in their last school year from August 1969 to August 1974 were subject to school year extensions such that they graduated from March 1971 until March 1976 after the regular amount of schooling of 10 years. See Table 3.24 in Appendix 3.10 for an overview.

[6] The Micro Census was conducted in 1976, 1978, 1980, 1982, 1985, 1987, 1989, 1991, 1993, and in all years from 1995 to 2003. 2003 is the last Micro Census wave in our main sample as we focus on respondents up to age 39 and on birth cohorts up to 1964.

For auxiliary analyses, we make use of two additional data sets that allow us to overcome shortcomings of the Micro Census with respect to potential measurement error due to remarriage, regional mobility, family relationships, and residence of children. First, we make use of the Socio-Economic Panel (SOEP), a representative, multi-cohort survey asking all individuals in selected households since 1984 about a variety of topics (Goebel et al., 2019). Because the same people are surveyed every year, it is possible to track individual marriage biographies as well as changes in an individual's state of residence.[7] Second, we draw on the DJI Family Survey of 1988 (Bertram, 1991), which is likewise a representative survey asking 18-55-year old individuals from West Germany about their family relationships.[8]

3.3.1 Outcome measures

Our main outcome variables are based on the Micro Census and relate to the timing of marriage and parenthood. More specifically, we look at the probabilities to be married and to have a child in or before period $p \in \{5, 8, 10\}$, i.e., five, eight, and ten years after the regular graduation age. We focus on specific years after graduation as patterns of family formation are more similar across tracks when looking at years after graduation and not at specific ages (see Section 3.3.5).[9]

More formally, the outcome variables $Y^{e,p}$ are defined as

$$Y^{e,p} = \begin{cases} 1 & \text{if } e \text{ in } t \leq p \\ 0 & \text{otherwise,} \end{cases} \tag{3.1}$$

[7]We apply the sample restrictions as in the Micro Census (see below). This results, however, in a much smaller sample size compared to the Micro Census of 3,620 individuals for whom we have information on remarriage before age 40 and 3,964 individuals for whom we observe residential behaviour.

[8]Again, we apply the sample restrictions as in the Micro Census (see below), which leads to a sample size of 1,904 individuals (with 3,211 children) for whom we know the nature of their family relationship and 1,737 individuals (with 2,950 children) for whom we identify their current residence.

[9]The results are very similar, when we look at specific ages (see Table 3.8 in the Appendix). While focusing on specific years after graduation means that individuals in different tracks are evaluated at different ages, it allows for a more compact layout of the main regression tables.

where $t \in [1, 10]$ indicates the time (measured in years after the regular graduation age) the event $e \in \{marriage, parenthood\}$ took place. The regular graduation age refers to the age at which individuals usually graduate from a specific track, i.e. in the absence of short school years: age 15 when in basic track, at age 16 when in middle track, and at age 19 when in academic track. In further analyses, we also look at other time intervals.

We construct the marriage outcomes based on the wedding year of the current marriage as provided in the Micro Census. For the construction of the fertility outcomes we make use of the birth information of all children in the household. Based on this information, we calculate the parents' age at the birth of their first child as the difference between the year of birth of the oldest child in the household and that of the parents.

3.3.2 Treatment assignment

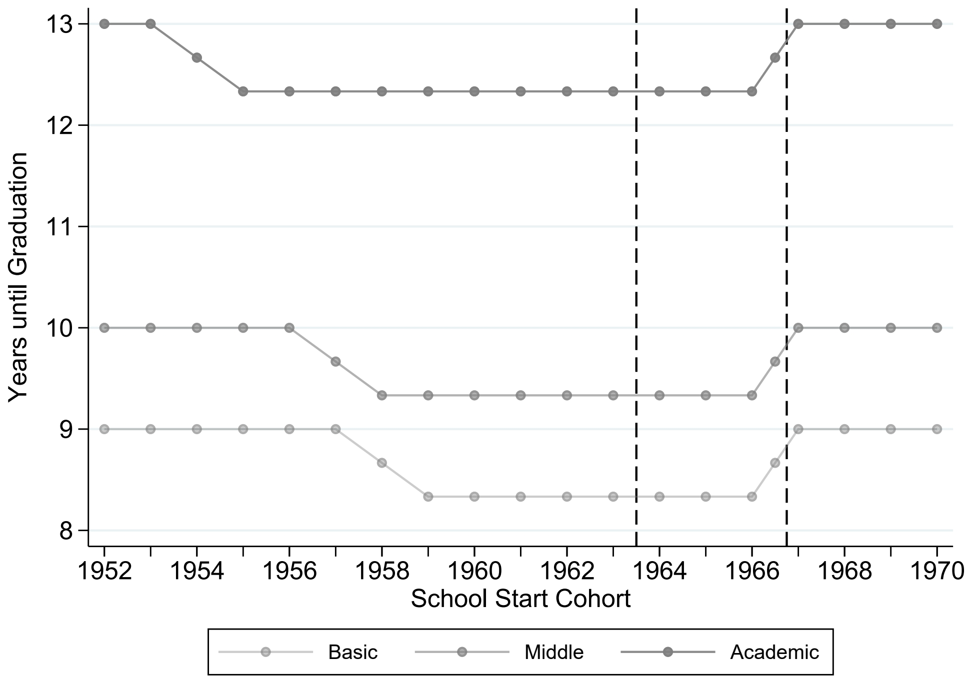
We basically follow Pischke (2007) in assigning the treatment variable and restricting the sample, but make some small changes to allow for a more precise assignment of the length of schooling.[10] SSY exposure depends on three characteristics: the federal state of students' school location, students' school starting cohort, and the secondary school track (see Section 3.2). In principle, SSY affected all students in treatment states who were enrolled in primary or secondary school in 1966/67. However, at a given point in time, additional cohorts are enrolled in academic and middle track compared to the basic track, as the basic track caters grades 5-8 or 5-9,[11] the middle track grades 5-10, and the academic track grades 5-13.

[10] Pischke (2007) finds that SSY exposure had no adverse long-term effect on human capital acquisition and we can replicate this finding using his sample restrictions (see Table 3.9 in the Appendix).

[11] Several states increased the number of years of compulsory schooling from eight to nine (see the discussion in Section 3.6, Appendix 3.10 and in Koebe and Marcus (2020)).

Figure 3-1 is a stylized graph that visualizes SSY exposure in treatment states and the resulting years spent in school until graduation, depending on individuals' secondary school track and school starting cohort.

Figure 3-1: Exposure to Short School Years by Secondary School Track and Cohort



Notes: This stylized figure visualizes treatment identification in a given treatment state, depending on school start cohort and secondary school track. School starting cohorts within the dashed lines are cohorts that were affected by the short school years during primary school and that are excluded in our main analyses. Deviations from the regular school years required in each track (9 in basic, 10 in middle, and 13 in academic) imply exposure to short school years. Own calculations and graphical display.

A student entering primary school in a treatment state in 1952 had already left secondary school by the start of the first SSY in 1966, irrespective of the attended track. The same holds for the school starting cohorts 1953 and 1967-1970. For the school starting cohorts 1954-1958, however, the choice of secondary school track matters for SSY exposure. For instance, students from the 1957 school starting cohort were exposed to two SSY in academic track, while they were exposed to only one SSY in middle track and no SSY in basic track. For school starting cohorts 1959-1966, students in all school forms were exposed to two SSY and, hence, graduated two-thirds of a school year earlier, while the four school starting cohorts 1964, 1965, 1966 (first SSY), and 1966 (second SSY) were all exposed to the reform during primary school. In sum, SSY exposure depends on federal state, school starting cohort, and secondary school track; consequently, we assign the treatment variable based on these three characteristics.

Our treatment variable takes on the value 1 if an individual was exposed to two SSY and the value 0 if an individual was not exposed to SSY. Individuals who were in the last year of secondary school when SSY were introduced as well as individuals who started primary school with the second SSY were only exposed to one SSY. For these individuals, the treatment variable is set to 0.5.

3.3.3 Measurement error in key variables

This section discusses the extent of measurement error in key variables and potential consequences for the estimation results. We begin by discussing measurement error in the outcome variables and continue with potential measurement error in the assignment of the treatment indicator.

Measurement error in outcome variables

We construct the marriage outcomes based on the wedding year of the current marriage. While divorced and widowed individuals are also asked about the year of their last marriage, there is no information on whether the current marriage is also respondents' first marriage. Hence, remarriages can induce measurement error in our marriage outcomes, as we do not assign the correct year of first marriage. We limit the extent of measurement error by considering only the information of respondents up to age 40. To provide some information on the extent of remarriages in our sample, we resort to the SOEP data. Figure 3-2a shows that about 7 percent of individuals remarried below age 40. This share is slightly lower in the academic track (4.4 percent) and slightly higher in the basic track (8.3 percent). Hence, for about 7 percent of individuals, we do not assign the correct year of first marriage.[12]

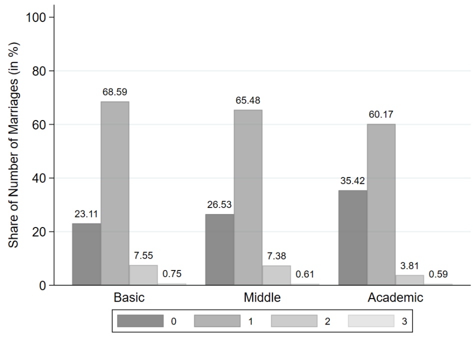
[12] Note that not all of these 7 percent actually constitute measurement error in our marriage outcomes. For instance, if an individual's first marriage is three years after graduation and their second marriage is seven years after graduation, we still correctly code the outcome relating to being married eight and ten years after graduation.

There is also measurement error in the fertility outcomes. In our data, individuals are not directly asked about their biological children. However, birth information is available for all children in the household. Hence, there is measurement error in households that adopted the oldest child or households in which the oldest child died or has left the household. This measurement error is of particular concern as older children are more likely to move out of the household. Therefore, we restrict the analyses to individuals who are up to 39 years at the time of the interview, as it is less likely they have children who have already left the household.[13] In order to provide information on family structure for cohorts of our main sample, we make use of the DJI Family Survey 1988. This survey asks each respondent for every child whether or not child is biological, child of partner, adopted, or foster child as well as the current residence of each child. Figure 3-2b shows that nearly all children (i.e., about 95 percent for male respondents and 98 percent for female respondents) from a parent belonging to our main sample are biological children. Figure 3-2c further reveals that when restricting age to below 40 at the time of the interview, nearly all children live in the same household with their parents (about 97 percent). In the robustness section, we show that our results are insensitive to alternative cut-off ages.

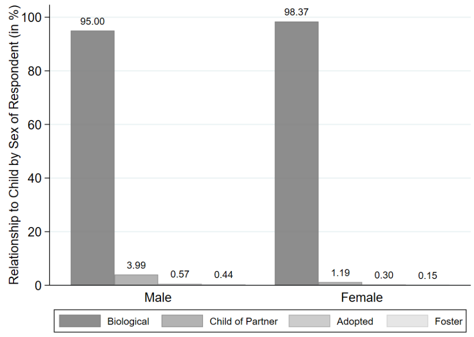
[13]This cut-off below 40 is recommended by Krapf and Kreyenfeld (2015), who compare the number of children based on this procedure with the number of biological children.

What are the consequences of these types of measurement error in the dependent variable? The answer to this question depends on whether the measurement error is related to our key explanatory variable, the introduction of short school years, or not. Assuming that remarriages are unrelated to short school years, the measurement error is random and, hence, does not bias our estimators of SSY exposure (Wooldridge, 2015). However, if SSY exposure not only increases the chances to be married at a given age but also the chances to be remarried at a given age, we will underestimate the effect of SSY exposure on first marriages. Similarly, if SSY exposure not only decreases the age at child birth but also the age at which children move out of the household, the SSY effect will be biased toward zero: Some respondents are assigned to have no child at a given age (because the child already moved out) and this incorrect assignment happens (slightly) more often for treated individuals.

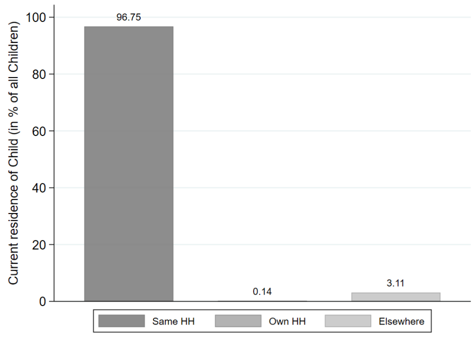
Figure 3-2: Assessing Measurement Error



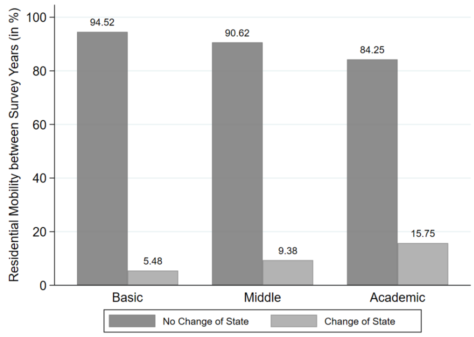
(a) Share of Number of Marriages below Age 40



(b) Relationship to Child by Sex of Respondent



(c) Current Residence of Child



(d) Residential Mobility between Survey Years

Notes: All four graphs provide descriptive statistics to assess the size of potential measurement errors. Figure 3-2a displays the share of second marriages before age 40, while Figures 3-2b and 3-2c deal with the concern of biased fertility outcomes and display the relationship to the child by sex of respondent and the current residence of the child. Figure 3-2d provides information on regional mobility.
Source: Figures 3-2a and 3-2d: SOEP v35. Figures 3-2b and 3-2c: DJI Family Survey 1988.

Generally, we face a trade-off between looking at longer time horizons and systematic measurement error in our outcome variables due to children moving out of the household and remarriages. Therefore, our main specification focuses on outcomes up to five, eight, and ten years after graduation. For these outcomes, measurement error will be smaller compared to outcomes like higher order parities or completed fertility, which we examine in Section 3.7.3.

Measurement error in right-hand side variables

When assigning the treatment variable, we must deal with several challenges due to the nature of the Micro Census data. First, we only have information on the current state of residence, but not on the state where an individual went to school. This is a common issue when working with the Micro Census, but residential mobility across states is very low in Germany and the current state of residence is found to be a good proxy for the state in which an individual went to school (see, e.g., Pischke, 2007; Pischke and von Wachter, 2008; Jürges et al., 2011).

To provide information on regional mobility, we again rely on the SOEP and examine whether individuals in our target group move between federal states. We see that a share of 9.7 percent changed their state of residence at least once in the SOEP. Figure 3-2d plots this share by secondary school track. As expected, residential mobility is higher among individuals with an academic track degree. Residential mobility does not necessarily lead to an incorrect assignment of the SSY treatment indicator. While individuals might move to a different state, they might later move back to the previous state. Further, we also correctly assign the treatment indicator for individuals who move from one treatment state to another treatment state and for individuals who move from one control state to another control state. However, for individuals who move from a treatment state to a control state (or vice versa), we incorrectly assign the treatment indicator. Again, the extent of this measurement error is limited by considering only respondents below the age of 40.

Second, retrospective information on the attended school track in a given year is not provided in the Micro Census. However, switching tracks and degree upgrading as adults is not very common in the analyzed cohorts in Germany (Dustmann et al., 2017). Generally, students in secondary schooling may change school tracks at any grade. However, based on a School Census for two states, Dustmann et al. (2017) find that only few students (about 2 %) make use of this opportunity. Switching tracks is another common issue when working with this data set and we follow the procedure of previous studies (see, e.g., Pischke, 2007; Pischke and von Wachter, 2008; Jürges et al., 2011; Cygan-Rehm and Maeder, 2013), assigning the school track based on information on the highest secondary school degree obtained. Again, the age limit of 40 reduces the extent of measurement error.

Third, information on grade repetition is not available. Therefore, we mistakenly assign grade repeaters from the last cohort before the SSY introduction to the control group, even though they actually experienced SSY due to their grade repetition. This misassignment of the treatment variable results in a (small) downward bias. Our results are robust to excluding the last pre-treatment cohort (see Section 3.6).[14]

[14] Further, Pischke (2007) shows that SSY exposure increases grade repetition among cohorts who were exposed in primary school. We exclude these cohorts from our main analysis (see below).

Fourth, the data lacks information on the exact month of birth, which would allow a more precise assignment of students' school starting cohorts based on the respective legal school-starting age cut-off in each federal state at the time (see Appendix 3.10 orKoebe and Marcus (2020) for a comprehensive collection of educational laws on school-starting cut-off rules for all West German states.). The Micro Census data provide only information on the season of birth, that is, whether individuals were born between January and April or between May and December. In most federal states, the age cut-offs coincide to a large extent with this season of birth information. While Pischke (2007) and Braakmann (2010) assign the treatment indicator only based on the year of birth and assume a school start in the year the child turns seven, we exploit the season of birth information to reduce misassignment of actual treatment status.[15]

Wrongly assigning the treatment indicator, our estimators are biased toward zero: We assign some individuals to the treatment group who are not treated and some individuals to the control group who are actually treated.

Summing up the discussion on measurement error, we see that measurement error in both the outcome variables and the treatment assignment is likely to bias our estimators toward zero. However, we expect that this downward bias is not very large due to the aforementioned reasons and because we limit the sample to include respondents only up to age 40.

[15]Our results are robust to assigning the treatment based only on the year of birth (see Section 3.6).

3.3.4 Sample restrictions

We restrict our analysis to German respondents in private households in West Germany and exclude individuals who obtained their school degree in East Germany. We impose several additional sample restrictions and, in Section 3.6, we show that our conclusions also hold if we apply different constraints. In our main analysis, we consider cohorts starting school between 1952 and 1970. This translates to using birth years ranging from 1945-1964. However, we exclude school starting cohorts 1964, 1965, and both school starting cohorts in 1966 in our main specification. These cohorts were exposed to SSY during primary school and Pischke (2007) shows that these cohorts have a lower probability of enrolling in either the middle or academic tracks. Hence, for these cohorts, the secondary school track is endogenous. The remaining birth cohorts in our sample made their track choice before the SSY introduction. While Pischke (2007) and Braakmann (2010) use birth cohorts from 1943-1964, we focus on a slightly smaller window of cohorts (1945-1964). The main reason for this is that, in several states, the birth cohorts 1943 and 1944, thus, school starting cohorts 1950 and 1951, were affected by previous changes of the start of the school year from fall to Easter in line with the Düsseldorf Accord. However, for the federal state of Saarland, narrowing the sample window does not allow for isolating the impact of the SSY, as more than the first two cohorts were affected by altered length of schooling (see also footnote 3, Figure 3.27 and Tables 3.18-3.27 in Appendix 3.10 or in Koebe and Marcus (2020)).[16] Therefore, we exclude Saarland from our main analysis.

[16]Saarland was last to implement the common start of the school year at Easter in 1957 and, hence, had to switch start of the school year again within a shorter time period. Our results are robust to including Saarland (see Section 3.6).

We also impose a restriction on the respondents' age. We only consider the information of respondents ten to twenty years after regular graduation.[17] We impose the lower age limit of ten years after graduation in order to have the same sample for all outcome variables - irrespective of the considered time window after graduation. We apply the upper age limit of twenty years after graduation to reduce measurement error in our outcome variables due to second marriages and children who have already moved out (see the discussion above). Twenty years after graduation also means that for individuals in the academic track, the maximum age of respondents is 39, which is also below 40, the age cut-off recommended by Krapf and Kreyenfeld (2015). Consequently, basic track students' reporting age is between 25 and 35 years, middle track students' between 26 and 36 years and academic track students between 29 and 39 years of age. Our sample restrictions lead to the final estimation samples of 203,501 observations in basic track, 98,448 in middle, and 109,199 in academic track, a grand total of 411,148 observations.

[17]We drop 879 observations with births before age 15 (accounting for 0.2 percent of the final sample). We assume that these young ages rather relate to classification errors as we only observe children currently living in the household – and not necessarily only biological children.

3.3.5 Descriptive statistics

Table 3.1 reports sample summary statistics by track. Several points are worth noting. First, the share of females is substantially lower in the academic track in the considered cohorts. Second, while the samples in all three tracks comprise the same school starting cohorts 1952-1970, the average birth year differs between the tracks. This is due to the general trend of increasing educational attainment over time: Whereas 59 percent of students from the 1952 school starting cohort graduated from basic track, by the end of our sample period in 1970, this share had decreased to 37 percent. Third, the age difference between the tracks is a result of the sample restriction to only consider the answers of respondents ten to twenty years after graduation. Fourth, at the time of the interview, 22% of individuals in the basic track had not married, while this share is 25% for the middle, and 32% for the academic tracks.[18] Among individuals who are married (or who have been married), the average age at marriage is clearly lower for individuals in the basic track (22.7 years) than in the middle (24 years) or academic (26.7 years) tracks. Similar differences across tracks can be obtained for the age at birth of the first child. Fifth, at the time of the interview (i.e. ten to twenty years after graduation), the share of individuals without children is ten percentage points higher in the academic track than in the basic track.

[18] Note that about 4-5% of individuals in our sample are divorced. However, we can correctly assign the year of marriage for them as the Micro Census also asks both divorced and widowed individuals about the year of their marriage.

Next, we examine the patterns of family formation between the tracks in more detail. Figures 3-3a and 3-3b show that the higher the school track, the lower the probability to be married or to have a first child at any given age.[19] This observation is in line with both the human capital effect of education and the duration effect of education.[20] It is striking that the differences between individuals in basic and middle tracks are smaller compared to the differences between middle and academic tracks. This reflects that the length of the education phase is more similar between individuals in basic (eight to nine years) and middle tracks (ten years) than individuals in the academic track (thirteen years). Moreover, the graphs are also in line with the lock-in effect of schooling. Individuals in the academic track are clearly less likely to be married or to have a first child before the age of 19, the age when they finish secondary education. When accounting for the different lengths of secondary schooling (as in Figure 3-3c and Figure 3-3d), the differences between the tracks are still existing, which is in line with the human capital effect of education. However, the differences get much smaller, providing suggestive evidence for the duration effect of education.

[19]These figures are based on our main sample, which includes respondents ten to twenty years after graduation. Therefore, the lines for the basic track ends at an earlier age compared to the other tracks. Figure 3-6 in the Appendix displays the patterns of family formation separately for males and females. It is evident that women are more likely than males to marry and to have a first child at younger ages. We examine effect differences between males and females in Section 3.7.

[20]The differences are also in line with a pure selection effect in the sense that individuals with preferences for earlier marriage and earlier parenthood select themselves into lower tracks.

Table 3.1: Summary Statistics by Secondary School Track

	Basic			**Middle**			**Academic**		
	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Female	0.49	0	1	0.58	0	1	0.42	0	1
Birthyear	1954.38	1945	1964	1956.54	1945	1964	1957.15	1945	1964
Birthmonth									
Jan-Apr	0.36	0	1	0.37	0	1	0.37	0	1
May-Dec	0.64	0	1	0.63	0	1	0.63	0	1
Age at Interview	30.75	25	35.5	31.93	26	36.5	34.89	29	39.5
School Start Year	1961.04	1952	1970	1963.19	1952	1970	1963.80	1952	1970
Years in School	8.52	8	9	9.85	9.33	10	12.79	12.3	13
Marital Status									
Single	0.22	0	1	0.25	0	1	0.32	0	1
Married	0.73	0	1	0.70	0	1	0.64	0	1
Widowed	0.00	0	1	0.00	0	1	0.00	0	1
Divorced	0.05	0	1	0.05	0	1	0.04	0	1
Age at Marriage	22.65	15.5	35.5	23.99	15.5	36.5	26.74	15.5	39.5
Share Married									
5 Years after Graduation	0.25	0	1	0.22	0	1	0.21	0	1
8 Years after Graduation	0.49	0	1	0.43	0	1	0.37	0	1
10 Years after Graduation	0.59	0	1	0.53	0	1	0.45	0	1
Age at First Birth	23.73	15	35.5	25.58	15	36.5	28.44	15	39.5
Share First Birth									
5 Years after Graduation	0.15	0	1	0.10	0	1	0.10	0	1
8 Years after Graduation	0.33	0	1	0.24	0	1	0.22	0	1
10 Years after Graduation	0.44	0	1	0.35	0	1	0.32	0	1
Childless at Interview	0.38	0	1	0.43	0	1	0.48	0	1

Notes: The table displays for relevant variables sample mean, minimum and maximum by secondary school track.

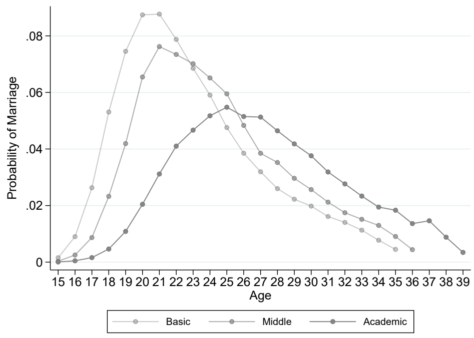
3.4 Empirical strategy

To identify the effect on family formation of shortening the education phase through the introduction of short school years, we estimate the following difference-in-differences (DiD) equation separately for each track J, with $J \in \{basic, middle, academic\}$:

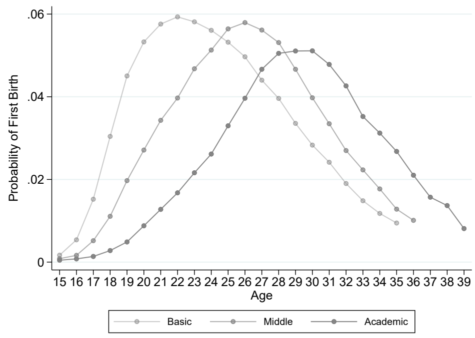
$$Y_{isc}^{J} = \beta^{J} SSY_{sc}^{J} + \gamma_{s}^{J} + \lambda_{c}^{J} + X_{isc}' \delta^{J} + \varepsilon_{isc}^{J}, \tag{3.2}$$

where Y_{isc} is the outcome for individual i in state s and school starting cohort c. SSY_{sc} denotes the variable of interest. Based on an individuals' school starting cohort and state, it takes on the value 1 if an individual is exposed to two SSY and the value 0 if an individual is not exposed to SSY (see Section 3.3.2 for a detailed description of the exposure of different cohorts in the different states to short school years). Hence, β denotes the effect of being exposed to two SSY, which is equivalent to graduating about two-thirds of a school year earlier. γ_s and λ_c are fixed effects for state and school starting cohorts, respectively, thus taking into account general differences in the outcomes between states and across school starting cohorts. X_{isc} is a vector of pre-determined individual characteristics. In the baseline specification, it includes a gender dummy and fixed effects for the wave of the Micro Census and, in our main specification, it includes interactions between the gender dummy and the fixed effects for cohort, state, and wave. Finally, ε_{isc}^{J} denotes the error term that is allowed to be clustered at the cohort-state level, i.e. the level the treatment variable is assigned.

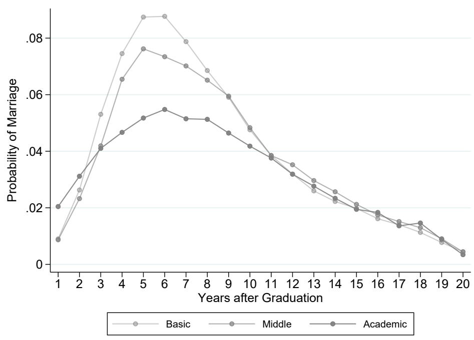
Figure 3-3: Probability of Marriage and First Birth by Age and Years after Graduation



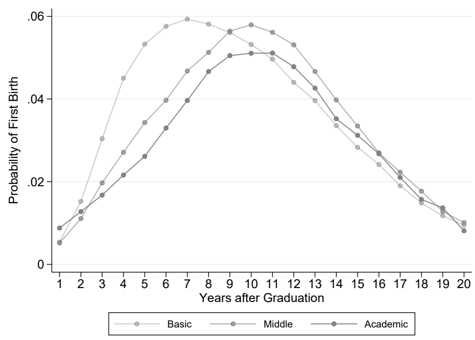
(a) Marriage by Age



(b) First Birth by Age



(c) Marriage by Years after Graduation



(d) First Birth by Years after Graduation

Notes: All four graphs show probabilities of being married and becoming a parent by age (Figures 3-3a and 3-3b) and by years after graduation (Figures 3-3c and 3-3d), using graduating ages for basic track at 15, for middle track at 16, and academic track at 19 as year 0. Own calculations and graphical display.
Source: Micro Census, main estimation sample.

We estimate Equation (3.2) separately by track for several reasons. First, the pattern of family formation differs between individuals across the three tracks in the sense that individuals in the basic track get married earlier and also give birth to children earlier (see Figure 3-3). Second, other reforms (in particular, the compulsory schooling reforms) implemented in a similar time period affected only specific tracks. Estimating the regressions separately for each track, allows for considering these other reforms more easily (see Section 3.6). Third, different cohorts are affected by SSY in the different tracks due to the differences in the number of years needed for graduation.

Our DiD identification strategy assumes that the track-specific family-formation outcomes would have evolved in parallel in the treated and control states, if SSY were not introduced. This common trend assumption could be violated if SSY exposure provoked parents to move to control states. Since the decisions on the SSY introduction were made at the beginning of 1966 and were communicated shortly before its implementation, it is unlikely that parents would have had enough time to move to avoid SSY exposure for their children. Further, moving across states is rather costly for families.

3.5 Results

We begin our discussion of the results by looking at the effect of the short school years on the probability to be married several years after graduation. We focus on the estimates for β from Equation (3.2), the SSY effects. Taking the first coefficient in column (1) of Table 3.2 at face value suggests that for individuals in the basic track being exposed to two short school years (i.e. graduating about two-thirds of a school year earlier) increases the probability to be married five years after graduation by 0.4 percentage points (pp). This effect is not statistically significant at conventional levels and it does not change substantially with the inclusion of gender-specific fixed effects for cohort, state, and wave (column 2). The marriage effect for individuals in the basic track remains statistically insignificant eight and ten years after graduation (columns 3-6). However, for individuals in the middle and academic tracks, the probability to be married five years after graduation increases substantially and significantly. While we see that the effects wash out over time, they are still statistically significant eight and ten years after graduation. The effect for the academic track decreases from 4.6 pp (five years after) to 4.1 pp (eight years after) and to 2.5 pp (ten years after), while in the middle track it remains 3.7 pp five years and eight years after, then decreases to 2.6 pp.

Table 3.2: Effect of Short School Years on Marriage and First Birth

	5 Years		8 Years		10 Years	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Marriage						
Basic	0.004	0.005	0.002	0.003	0.003	0.003
	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)
N	203,501	203,501	203,501	203,501	203,501	203,501
Middle	0.038***	0.037***	0.038***	0.037***	0.026**	0.026**
	(0.008)	(0.008)	(0.011)	(0.011)	(0.011)	(0.011)
N	98,448	98,448	98,448	98,448	98,448	98,448
Academic	0.046***	0.046***	0.041***	0.041***	0.025**	0.025**
	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)	(0.011)
N	109,199	109,199	109,199	109,199	109,199	109,199
Panel B: First Birth						
Basic	-0.000	0.000	-0.005	-0.005	-0.011*	-0.011*
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
N	203,501	203,501	203,501	203,501	203,501	203,501
Middle	0.008	0.007	0.013	0.012	0.019*	0.019*
	(0.005)	(0.005)	(0.009)	(0.009)	(0.011)	(0.011)
N	98,448	98,448	98,448	98,448	98,448	98,448
Academic	0.016**	0.016**	0.032***	0.032***	0.036***	0.036***
	(0.007)	(0.007)	(0.010)	(0.010)	(0.010)	(0.010)
N	109,199	109,199	109,199	109,199	109,199	109,199
School Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Sex FE	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
School Cohort-Sex FE	No	Yes	No	Yes	No	Yes
State-Sex FE	No	Yes	No	Yes	No	Yes
Wave-Sex FE	No	Yes	No	Yes	No	Yes

Notes: The table shows the effect of the short school years on marriage and first birth five, eight, and ten years after graduation from secondary school for individuals in different secondary school tracks based on Equation (3.2). Standard errors clustered at the cohort-state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

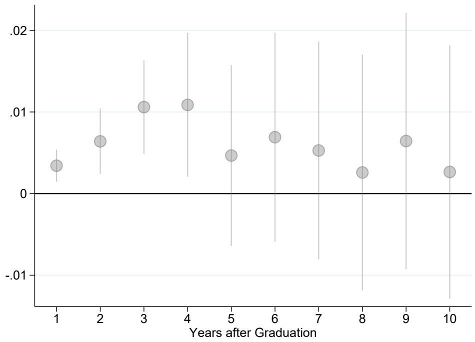
Next, we focus on SSY effects on the probability to have a first child five, eight, and ten years after graduation (Panel B). For individuals in the basic track, the earlier graduation due to SSY does not appear to result in increased probabilities to have a first child five, eight, or ten years after graduation. This is similar for individuals in the middle track, although the effect ten years after graduation is borderline significant. For individuals in the academic track affected by SSY, the probability to have a first child increases by about 3 pp eight and ten years after graduation.

While Table 3.2 focuses on three specific years after graduation, Figure 3-4 graphically shows the effects separately for one to ten years after graduation. In all three tracks, we observe a hump-shaped pattern for the marriage outcome: SSY exposure increases the probability to be married for the first years after graduation, but the effect fades over time. There is also evidence that SSY exposure significantly affects individuals in the basic track but only up to four years after graduation. For the fertility outcome, the point estimates are generally positive for the basic and middle tracks but statistically insignificant. However, for individuals in the academic track, SSY exposure does not only affect the timing of marriage, but also the timing of fertility: In Panel (f) of Figure 3-4 the point estimate is statistically significant for four to ten years after graduation.[21]

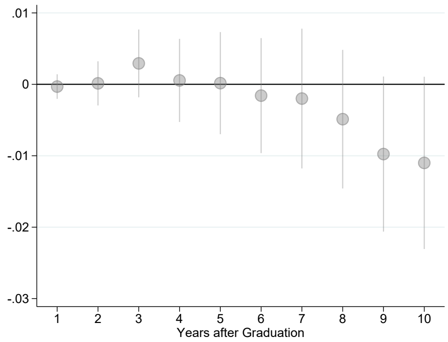
[21] Similar to Humlum et al. (2017), our main specification focuses on years after graduation and not on specific ages. When estimating the effects by age instead of years after graduation (see Table 3.8 in the Appendix), we obtain similar results. The marriage effect first kicks in for individuals in the basic track with significant coefficients at ages 16-19, while for the middle track the largest point estimates are obtained at ages 20-25 and for the academic track at ages 23-27. The higher the education, the later the strongest effects. At age 29, the marriage effects remain significant only for the academic track. Panel B of this table shows significant fertility effects in the academic track for ages 21-29.

Figure 3-4: Effect of Short School Years on Marriage and First Birth

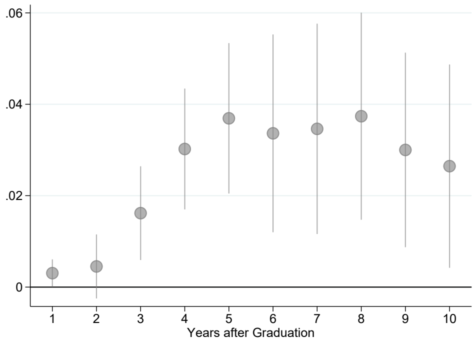
(a) Marriage - Basic



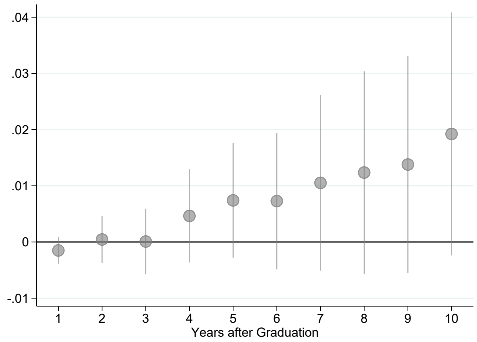
(b) First Birth - Basic



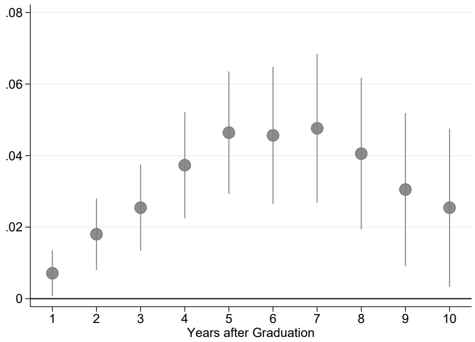
(c) Marriage - Middle



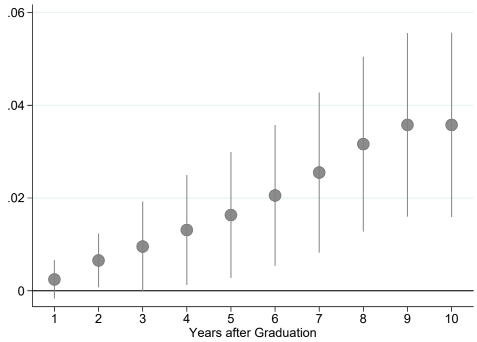
(d) First Birth - Middle



(e) Marriage - Academic



(f) First Birth - Academic



Notes: The graphs show the estimated short school year effects (and their 95% confidence intervals) *up* to different years after graduation. The underlying samples are identical to the samples in Table 3.2. All coefficients are based on Equation (3.2).

Taken together, Table 3.2 and Figure 3-4 provide evidence that earlier graduation affects the timing of marriage for individuals across tracks, but that it shifts forward the birth of the first child mainly for individuals in the academic track. In the following, we discuss some explanations for these patterns. Once an individual enters the labor market, the German tax system induces strong incentives for getting married. For example, Germany's combination of progressive taxes and joint taxation (*Ehegattensplitting*) provides large financial benefits for married couples – in particular, for spouses with strongly differing income – and the social health insurance in Germany allows for insuring (non-working) spouses without cost. These incentives affect individuals in all three school tracks and, therefore, might explain why we find that individuals prepone marriage across tracks. Regarding the differential fertility response, Figure 3-3 provides evidence that the age at first birth is much higher for individuals in the academic track compared to individuals in the other tracks. Therefore, the fertility response to an earlier labor market entry might be stronger for individuals in this group, e.g., because they are closer to the biological age limit for childbirth or because they prefer that the age does not differ too much between parents and children.

3.6 Sensitivity analysis

Table 3.3 examines the sensitivity of our findings to alternative age, wave, and state restrictions.[22] In our main specification, we construct the outcome variables based on information provided ten to twenty years after graduation. We set the lower bound to ten years in order to have a constant sample for five, eight, and ten years after graduation. Further, we do not include information from more than twenty years after graduation, as the more years after graduation we include, the more likely it is that marriages have been dissolved and/or children have moved out of the household. At the same time, a larger number of observations would increase the precision of our estimates. Hence, there is a bias-variance trade-off.

[22]This Table presents the results for marriage and first birth eight years after graduation, Tables 3.10 and 3.11 in the Appendix show the results for five and ten years after graduation, respectively.

Table 3.3: Robustness Checks - Alternative Age, Wave, State, and SE Specifications Eight Years after Graduation

	Other Age Restrictions			**Wave Restrictions**		**State Restrictions**		**Birthyear**	**SE**
						-LS	+SL		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Marriage									
Basic	0.004	0.008	-0.003	0.003	0.006	-0.006	0.003	0.005	0.003
	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.013)
N	99,250	228,149	237,942	203,501	166,094	180,009	207,963	192,430	203,501
Middle	0.037**	0.036***	0.032***	0.037***	0.040***	0.043***	0.038***	0.029***	0.037**
	(0.014)	(0.010)	(0.009)	(0.011)	(0.011)	(0.014)	(0.011)	(0.010)	(0.014)
N	44,503	111,614	118,417	98,448	89,000	86,245	99,807	89,226	98,448
Academic	0.045***	0.044***	0.035***	0.039***	0.039***	0.045***	0.040***	0.036***	0.041*
	(0.017)	(0.012)	(0.010)	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)	(0.021)
N	51,566	122,504	113,732	103,698	106,139	98,348	110,756	97,768	109,199
Panel B: First Birth									
Basic	0.001	-0.003	-0.016***	-0.005	-0.007	-0.008	-0.005	-0.001	-0.005
	(0.007)	(0.005)	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.004)
N	99,250	228,149	237,942	203,501	166,094	180,009	207,963	192,430	203,501
Middle	0.007	0.013	-0.001	0.012	0.015*	0.012	0.014	0.010	0.012
	(0.013)	(0.008)	(0.007)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.008)
N	44,503	111,614	118,417	98,448	89,000	86,245	99,807	89,226	98,448
Academic	0.052***	0.033***	0.021**	0.033***	0.030***	0.030***	0.033***	0.030***	0.032***
	(0.012)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.010)	(0.010)	(0.008)
N	51,566	122,504	113,732	103,698	106,139	98,348	110,756	97,768	109,199
Years after Grad.	10-15	8-20	Age 30-40	10-20	10-20	10-20	10-20	10-20	10-20
Age Basic	25-30	23-35	30-40	25-35	25-35	25-35	25-35	25-35	25-35
Age Middle	26-31	24-36	30-40	26-36	26-36	26-36	26-36	26-36	26-36
Age Academic	29-34	27-39	30-40	29-39	29-39	29-39	29-39	29-39	29-39
Waves	1976-2003	1976-2003	1976-2003	1976-2001	1980-2003	1976-2003	1976-2003	1976-2003	1976-2003

Notes: The table shows the effect of the short school years eight years after graduation for various alternative specifications. Columns (1)-(3) rely on information provided in differing numbers of years after graduation, as indicated in the bottom part of the table. Column (4) disregards the last two waves of the Micro Census, and column (5) drops the first two waves. Columns (6)-(7) drop or include certain states from the analysis (Lower Saxony (LS), Saarland (SL), respectively). Column (8) relies on treatment assignment based on individual's birth year and column (9) on standard errors clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

In the following, we analyze whether our results are robust to alternative ways of dealing with this bias-variance trade-off. Column (1) considers information by individuals ten to fifteen years after graduation, while column (2) considers information eight to twenty years after graduation. Column (3) relies on information provided between age 30 and 40. The next two sensitivity analysis restrict the sample to fewer waves. Column (4) drops the last two waves (2002 and 2003) and column (5) disregards the first two waves (1976 and 1978). Columns (6) and (7) relate to the number of included states. Column (6) drops Lower Saxony from the analysis, where SSY exposure was track-specific (see Section 3.2), while column (7) additionally includes Saarland, which was excluded in the main analyses due to earlier shifts of the start of the school year that affected many school starting cohorts in this particular state. Column (8) uses individuals' birth years (1945-1963) to identify treatment status by assuming a start of primary school in the year individuals turn seven, similar to the approach of Pischke (2007), but leaving all other sample restrictions unchanged. Column (9) clusters the standard errors at the level of the federal state and not at the state-cohort level. Our conclusions are insensitive to all the alternative specifications in Table 3.3.

In Appendix Table 3.12, we deal with alternative cohort restrictions. First, we also include cohorts that were affected by SSY exposure in primary school. Second, we exclusively consider cohorts with two SSY and disregard cohorts with only one SSY. Third, we exclude the last pre-treatment cohort as the treatment status in this cohort would be wrongly assigned for individuals who repeated a grade. Again, our results are robust to these alternative specifications. Co-treatments in the form of other policies are one threat to our identification strategy. For this reason, we excluded the school starting cohorts 1950 and 1951 (birth cohorts 1943 and 1944) from our main analysis. While these cohorts were included in previous SSY studies (Pischke, 2007; Braakmann, 2010), they were affected by reductions in the length of specific school years in four federal states (Baden-Württemberg, Bremen, Saarland, and West Berlin) due to moving the start of the school year from fall to Easter in line with the Düsseldorf Accord (DA) in 1955 (see also Footnote 3 and Appendix 3.10). We aim to only identify variations in the length of the schooling phase induced by the 1966/67 short school years, for which previous studies show that they did not hamper human capital accumulation. That is why we excluded Saarland completely from the main analysis: in this state, later cohorts were also affected by other changes in the length of the schooling phase.

Further, during our observation period, several states increased compulsory schooling from eight to nine years. This change mainly affected the basic track, as the regular school length is ten years in the middle track and thirteen years in the academic track. However, previous studies use two slightly different sets of compulsory schooling reform dates (we discuss this in more detail in Appendix 3.10). Therefore, we searched for the original law texts and propose refined reform dates, which we display together with the primary sources. We use these reform dates to assign to each cohort in each state the compulsory number of school years. Table 3.13 shows the results for the basic track when we control for compulsory schooling reforms using our refined dates or reform dates used in Pischke (2007). Controlling for compulsory schooling does not change our results meaningfully – irrespective of the used set of reform dates.

Our difference-in-differences identification strategy builds on the assumption that–in the absence of the short school years–the outcome variables would follow the same trend in treatment and control states. While it is generally not possible to prove this common trend assumption, we conduct falsification exercises with placebo outcomes and placebo treatments to assess the plausibility of this assumption.

For the placebo-outcome analysis, we examine the "effect" of short school years on track choice. As our main specification exclusively includes cohorts that were already in secondary school at the time of SSY implementation, SSY should have no effect on track choice. A significant SSY coefficient would indicate that the selection into tracks evolves differently in treatment and control states. In this analysis, we have to restrict the sample to cohorts and states for which the assignment of the treatment variable does not depend on the track. Hence, we have to drop Lower Saxony and the cohorts 1954 – 1958 (see Figure 3-1). Otherwise, there would be mechanic effects. Table 3.14 shows that there is no "effect" of short school years on the probability to attend the academic track. However, there is indication that the share of students in basic and middle tracks develops slightly, but statistically significantly, differently in treatment and control states. This significant coefficient remains if we consider two additional pre-treatment cohorts (see column 2).[23] Table 3.15 analyzes whether the slight differential selection into basic and middle track between treatment and control states affects our overall conclusions. In this table, we re-estimate our main specification but pool all observations from basic and middle track. This specification confirms our conclusion that short school years have no effect on fertility outcomes for individuals in basic and middle track. Further, Table 3.15 confirms that SSY affects the timing of marriage. Hence, we conclude that while there is some evidence that the selection into basic and middle track develops slightly differently in treatment and control states, this differential development does not change our overall conclusions. Moreover, selection into the academic track, for which we obtain the largest effects for both marriage and fertility outcomes, does not evolve differently in treatment and control states.

[23] In column (1) of Table 3.14, we can only work with two pre-treatment cohorts due to the necessary additional sample restrictions for this specification. The results in column (2) are very similar if we exclude the four states that were treated in 1950 or 1951 in line with the Düsseldorf Accord (see Appendix 3.10 or our working paper (Koebe and Marcus, 2020)).

For the placebo-treatment analysis, we restrict the sample to the pre-treatment cohorts only. Further, we include two additional cohorts (1950 and 1951) to increase statistical power.[24] Hence, the sample includes the cohorts 1950 - 1957 for the basic track, 1950 - 1956 for the middle track, and 1950 - 1953 for the academic track. We then pretend that SSY were introduced 1, 2, and 3 years before the actual SSY introduction, respectively, and estimate whether there is a significant "effect" of this placebo SSY introduction. Table 3.16 in the Appendix shows that the 18 estimated placebo effects are generally small and statistically insignificant.[25] While this is not a proof of the common-trend assumption, Table 3.16 suggests that our outcomes developed similarly in treatment and control states before the short school years, making it more plausible that trends would be similar in the absence of the short school years as well.

3.7 Further results

This section presents different sets of additional results on (i) effect differences by gender; (ii) the exact timing of marriage and parenthood; (iii) longer time horizons and subsequent births; and (iv) human capital-related outcomes.

[24] The results of the falsification exercise are very similar if we drop the four states (Berlin, Baden-Württemberg, Bremen and Saarland) that were treated in 1950 or 1951 in line with the Düsseldorf Accord (see Appendix 3.10).

[25] There is one statistically significant coefficient (placebo treatment three years earlier for the marriage outcome in the middle track). Given the number of tests (18) that we perform, this is roughly what one would expect.

3.7.1 Gender-specific results

Thus far, we pool the effects for males and females. Table 3.4 splits the sample according to an individual's gender. It shows that the obtained marriage and fertility effects are driven by both males and females. Generally, the effects for five years after graduation are slightly larger for females than for males (with only the marriage-effect difference for middle track individuals being statistically significant). One reason for this difference might be that females are, on average, younger than males when they marry and have children (see also Figure 3-6). For instance, about 30% of middle track women in our sample are married five years after graduation, while the corresponding share is only 12% for men. The effect differences between women and men become smaller eight and ten years after graduation, i.e. when men are also more likely to be married and have a first child. The effect differences eight and ten years after graduation are statistically insignificant throughout.

Table 3.4: Effect Heterogeneity by Gender

	5 Years		8 Years		10 Years	
	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Marriage						
Basic	0.007	0.002	-0.004	0.009	0.010	-0.005
	(0.009)	(0.004)	(0.008)	(0.010)	(0.011)	(0.008)
N	99,685	103,816	99,685	103,816	103,816	99,685
p-value of the difference	0.557		0.243		0.136	
Middle	0.054***	0.013	0.042***	0.031**	0.023	0.029***
	(0.010)	(0.010)	(0.013)	(0.014)	(0.016)	(0.011)
p-value of the difference	0.001		0.459		0.660	
N	57,381	41,067	57,381	41,067	41,067	57,381
Academic	0.035**	0.055***	0.029**	0.049***	0.033***	0.015
	(0.014)	(0.010)	(0.014)	(0.012)	(0.013)	(0.014)
p-value of the difference	0.215		0.150		0.193	
N	46,289	62,910	46,289	62,910	62,910	46,289
Panel B: First Birth						
Basic	-0.001	0.002	-0.004	-0.005	-0.007	-0.015*
	(0.006)	(0.003)	(0.008)	(0.005)	(0.008)	(0.008)
N	99,685	103,816	99,685	103,816	103,816	99,685
p-value of the difference	0.586		0.925		0.455	
Middle	0.010	0.004	0.018	0.005	0.014	0.023*
	(0.007)	(0.006)	(0.011)	(0.010)	(0.013)	(0.013)
N	57,381	41,067	57,381	41,067	41,067	57,381
p-value of the difference	0.477		0.227		0.532	
Academic	0.022*	0.012**	0.033**	0.031***	0.033***	0.040***
	(0.011)	(0.006)	(0.014)	(0.009)	(0.010)	(0.014)
N	46,289	62,910	46,289	62,910	62,910	46,289
p-value of the difference	0.335		0.891		0.607	

Notes: The table shows the effect of the short school years separately for males and females and presents p-values of a t-test for the difference in the effects between males and females. Standard errors clustered at the cohort-state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

3.7.2 Exact timing of marriage and parenthood

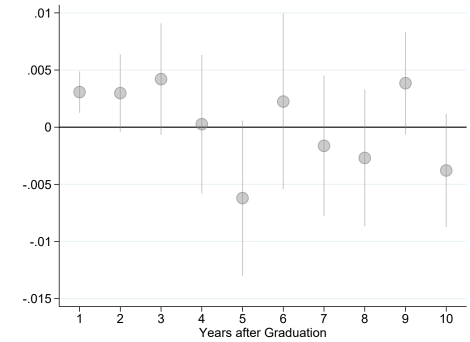
While the previous analyses focus on the probability of family formation *at or before* a specific point in time, the next analysis looks at the probability of family formation *at* a specific point in time.[26] This analysis helps to better understand the effects of SSY exposure on the exact timing of marriage and parenthood. Figure 3-5 graphically presents the associated results. Generally, the largest positive point estimates for marriage are found in periods when marriage is most likely (compare Figure 3-3c). Interestingly, significant and positive point estimates for marriage precede negative coefficients in all three tracks. For instance, individuals in the academic track are significantly *more* likely to marry in the second, third, fourth, and fifth years after graduation due to SSY exposure, while they are significantly *less* likely to marry in the eighth and ninth years after graduation. This suggests that the control cohorts are catching-up over time and that the head start of the treated cohorts diminishes over time, suggesting that SSY exposure affects the timing of marriage, but not the overall probability to ever marry.

[26] For this analysis, we redefine our outcome variables from Equation (3.2) as

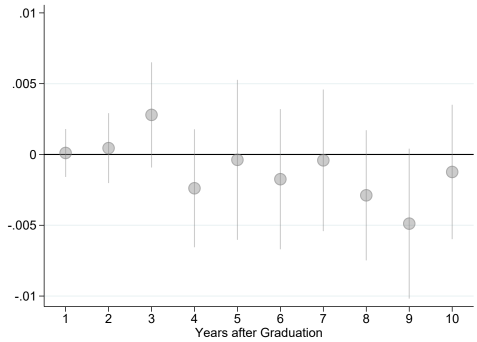
$$Y^{e,p} = \begin{cases} 1 & \text{if event } e \text{ in } t = p \\ 0 & \text{otherwise.} \end{cases}$$

Figure 3-5: Effect of Short School Years *at* different Years after Graduation

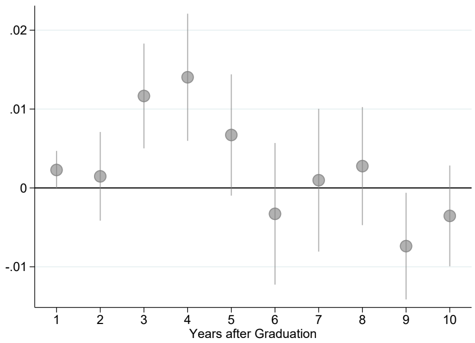
(a) Marriage - Basic



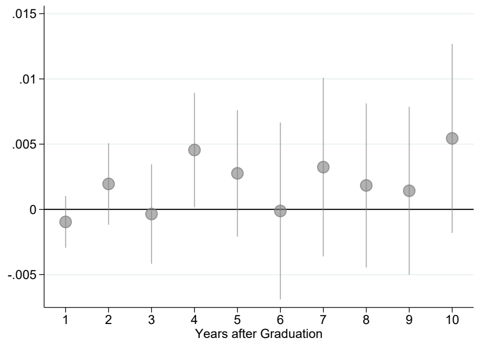
(b) First Birth - Basic



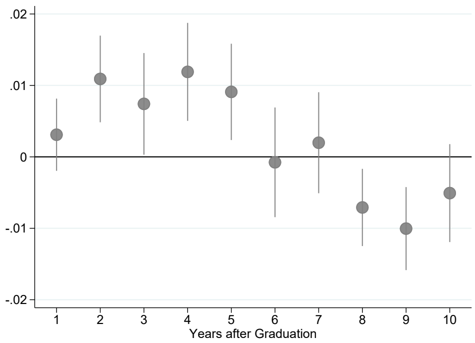
(c) Marriage - Middle



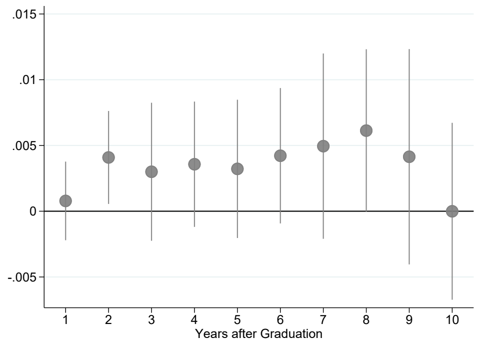
(d) First Birth - Middle



(e) Marriage - Academic



(f) First Birth - Academic



Notes: The graphs show the estimated short school year effects (and their 95% confidence intervals) *at* different years after graduation. The underlying samples are identical to the samples in Table 3.2 and all coefficients are based on Equation (3.2). The figure is related to Figure 3-4, which shows short school year effects *up* to different years after graduation.

Moreover, this pattern also shows why point estimates in our main specification (with cumulative marriage rates) are higher five years after graduation than ten years after graduation. When looking at the timing effects separately for females and males (Figures 3-7 and 3-8 in the Appendix), it can be seen that the developments of the effects for males lag slightly behind those for females. This is in line with the observation that we obtain the largest marriage effects in periods when marriage is most likely (compare Appendix Figure 3-6). For parenthood, in the academic track all coefficients for $t \in [2, 9]$ years after graduation are clearly positive and some coefficients are significant at the 10% level. Further, the magnitude of the coefficients declines for $t \in [8, 10]$ years after graduation, suggesting a similar pattern as for the marriage outcomes.

3.7.3 Longer-run effects and subsequent births

To analyze whether SSY affect only the timing of family formation or also the probability to ever marry/have children, this subsection focuses on longer time horizons. More specifically, Table 3.5 looks at family formation 10 to 15 years after graduation.[27] The table shows a striking pattern for our two main outcomes (Panels A and B): The longer the considered time horizon, the smaller the previously significant effects. For instance, while individuals in the middle track are 2.6 percentage points more likely to marry in the first ten years after graduation due to SSY exposure, this effect decreases to 1.8 percentage points twelve years after graduation and to 0.1 percentage points fifteen years after graduation. All-in-all, the first two panels of Table 3.5 suggest that the SSY introduction affects the timing of marriage and parenthood, but not the probability to ever marry or become a parent.

[27] While our main analyses focuses on the reports of individuals 10 to 20 years after graduation, these analyses consider reports of individuals p to 20 years after graduation, where $p \in [10, 15]$ is the number of years until which the event could have taken place in the respective analysis. Therefore, the number of observations differs across the specifications in Table 3.5. However, Appendix Table 3.17 shows that we obtain a similar pattern when we work with the same sample size in all specifications by considering only reports of individuals 15 to 20 years after graduation (as in the last column of Table 3.5). Hence, the observed pattern is unlikely to be driven by different sample compositions (or potentially different sample attrition patterns).

Table 3.5: Short School Year Effects: Longer Time Horizons

	Years after Graduation					
	10	11	12	13	14	15
Panel A: Marriage						
Basic	0.003	-0.002	-0.004	-0.008	-0.006	-0.008
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
N	203,501	188,330	173,232	157,588	140,755	122,803
Middle	0.026**	0.022**	0.018*	0.013	0.011	0.001
	(0.011)	(0.009)	(0.010)	(0.009)	(0.010)	(0.009)
N	98,448	91,869	84,934	77,690	69,823	61,960
Academic	0.025**	0.014	0.008	0.007	-0.001	-0.008
	(0.011)	(0.010)	(0.009)	(0.009)	(0.009)	(0.010)
N	109,199	101,972	94,829	87,503	78,097	68,785
Panel B: First Birth						
Basic	-0.011*	-0.015**	-0.009	-0.008	-0.005	-0.003
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)
N	203,501	188,330	173,232	157,588	140,755	122,803
Middle	0.019*	0.015	0.020*	0.024**	0.020*	0.012
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)
N	98,448	91,869	84,934	77,690	69,823	61,960
Academic	0.036***	0.031***	0.027**	0.025**	0.020	0.017
	(0.010)	(0.011)	(0.011)	(0.011)	(0.013)	(0.013)
N	109,199	101,972	94,829	87,503	78,097	68,785
Panel C: Second Birth						
Basic	-0.004	-0.011**	-0.008	-0.010*	-0.010*	-0.007
	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)	(0.007)
N	203,501	188,330	173,232	157,588	140,755	122,803
Middle	0.013***	0.012**	0.012*	0.012	0.014	0.014
	(0.005)	(0.006)	(0.007)	(0.008)	(0.009)	(0.009)
N	98,448	91,869	84,934	77,690	69,823	61,960
Academic	0.016**	0.010	0.006	0.002	0.005	0.001
	(0.007)	(0.007)	(0.008)	(0.010)	(0.012)	(0.013)
N	109,199	101,972	94,829	87,503	78,097	68,785
Panel D: Third Birth						
Basic	0.004**	0.005**	0.006**	0.005*	0.007*	0.007*
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)
N	203,501	188,330	173,232	157,588	140,755	122,803
Middle	-0.000	0.001	0.001	0.001	0.003	0.004
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)
N	98,448	91,869	84,934	77,690	69,823	61,960
Academic	0.002	0.002	0.006**	0.007**	0.006	0.010**
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)
N	109,199	101,972	94,829	87,503	78,097	68,785

Notes: The table shows the effect of the short school years on various outcomes as indicated in the panel header for individuals in different secondary school tracks based on Equation (3.2). Standard errors clustered at the cohort-state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

The next two panels of Table 3.5 examine whether short school years also affect the timing of the birth of the second and third children. For these analyses, we redefine the outcome variable in such a way that it only takes on the value of 1 if *two* children (Panel C) and *three* children (Panel D), respectively, were born at or before a specific point in time (instead of just one child). Panel C provides evidence that SSY exposure increases the probability for individuals in the academic track to have two children ten years after graduation by about 1.5 percentage points. As expected, the point estimates are smaller than the effects for the first child. There is also some evidence for positive effects in the middle track ten to twelve years after graduation. While SSY exposure does not affect third births in the middle track (Panel D), there is some evidence that individuals in the academic track are slightly more likely to have a third child twelve to fifteen years after graduation.

Table 3.6: Completed Fertility

	N of children before age 45	
	(1)	(2)
Basic	0.003	0.003
	(0.013)	(0.013)
N	206,241	206,241
Middle	0.007	0.007
	(0.023)	(0.023)
N	76,418	76,418
Academic	0.086***	0.089***
	(0.032)	(0.034)
N	74,847	74,847
School Cohort FE	Yes	Yes
State FE	Yes	Yes
Sex FE	Yes	Yes
Wave FE	Yes	Yes
School Cohort-Sex FE	No	Yes
State-Sex FE	No	Yes
Wave-Sex FE	No	Yes

Notes: The table shows the effect of the short school years on completed fertility - the number of children up to age 45 - for individuals in different secondary school tracks based on Equation (3.2). The sample is restricted to individuals who are at least 45 years old. Standard errors clustered at the cohort-state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

The results in Table 3.5 have to be taken with a grain of salt as we only observe children living in the household. The older the respondents are, the higher the chance that their children have already moved out of the house. Hence, measurement error is much larger for the outcomes ten to fifteen years after graduation. Measurement error is even larger when looking at completed fertility. This is what we do in Table 3.6. Here, we use the number of children up to age 45 as an outcome variable and consider the answers of individuals from our cohorts who are at least 45 years old. The table shows that while there is no effect on completed fertility for individuals with basic or middle track degrees, there is a statistically significant effect of SSY exposure on the number of children for individuals in the academic track, suggesting that, for these individuals, SSY exposure does not just affect the timing of family formation but also completed fertility. However, due to the aforementioned measurement issues, we do not want to over-interpret this finding.[28]

3.7.4 Human capital-related outcomes

The interpretation of our results as evidence for the duration effect hinges on the assumption that SSY exposure has no effect on human capital acquisition. While previous studies show that this reform had no adverse effects on health outcomes (Braakmann, 2010) or on wages and employment (Pischke, 2007), in this section we provide further evidence that the reform has no impact on human capital-related outcomes.

[28]If SSY exposure decreases not only the age at child birth but also the age at which children move out of the household, we will underestimate the effect of SSY exposure on completed fertility. The SSY effect will be biased toward zero as we assume that some individuals have fewer children at a given age (because we do not observe an older child who already moved out) and this incorrect measure of the number of children happens (slightly) more often for treated individuals.

Table 3.7: Effect of Short School Years on Human Capital Outcomes

	Main	
	(1)	(2)
Panel A: Wage		
SSY (All)	0.005	0.008
	(0.014)	(0.014)
N	411,148	411,148
Panel B: Employment		
SSY (All)	0.003	0.003
	(0.003)	(0.003)
N	411,148	411,148
Panel C: Apprenticeship		
SSY (Basic and Middle)	0.004	0.003
	(0.007)	(0.007)
N	140,588	140,588
Panel D: University		
SSY (Academic)	0.013	0.014
	(0.015)	(0.015)
N	58,136	58,136
School Cohort FE	Yes	Yes
State FE	Yes	Yes
Sex FE	Yes	Yes
Wave FE	Yes	Yes
Track FE	Yes	Yes
School Cohort-Track FE	Yes	Yes
State-Track FE	Yes	Yes
Sex-Track FE	Yes	Yes
Wave-Track FE	Yes	Yes
School Cohort-Track-Sex FE	No	Yes
State-Track-Sex FE	No	Yes
Wave-Track-Sex FE	No	Yes

Notes: This table shows SSY effects on wages, employment and tertiary education decisions using our main estimation sample. In Panel A, the dependent variable is the log hourly wage and estimation is performed with a Tobit model accounting for the left censoring of the outcome variable (results are very similar with a Poisson model). In Panel B, the dependent variable is a dummy for being employed in the survey week. The outcome in Panel C is the probability of obtaining an apprenticeship as highest vocational degree, while the outcome in Panel D is the probability of holding a university degree. The latter two outcomes are only available in the Micro Census starting from wave 1995, hence the reduced sample size. Standard errors in parentheses are clustered at the track-cohort-state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

First, we replicate the finding of Pischke (2007) that SSY exposure neither reduces wages nor the probability to be employed - we show that these results hold both for a replication sample based on Pischke (2007) as well as our sample (see the first two panels of Table 3.7 and Table 3.9 in the Appendix). Second, we show that the reform has no impact on tertiary education degrees.[29] More specifically, we find that the reform does not affect the probability to obtain a college degree for individuals in the academic track. Similarly, for individuals in basic and middle tracks, we show that short school years do not affect the probability to obtain a vocational education degree (see the last two panels of Table 3.7). Taken together, these findings are in line with our interpretation of a duration effect as there is no evidence for human capital effects of SSY exposure.

3.8 Discussion and conclusion

This study examines the effects on the timing of fertility and marriage of a policy that allowed students to finish secondary school about eight months earlier – with the same degree and the same curriculum taught. We find that earlier graduation leads to earlier marriages. There is also evidence that the earlier graduation affects the timing of childbirth. We find that the effects fade over time, indicating that the short school years affect the timing of marriage and parenthood but not the probability to ever marry or to become a parent. We further show that the obtained effects are driven by both males and females. Additionally, we find suggestive evidence that the reform also affects subsequent births and completed fertility.

[29] For these outcomes, we can only rely on the Micro Census waves from 1996 onward as in the previous waves individuals are asked about their last vocational education level, but not their highest obtained degree.

Our findings highlight that policies altering the duration of specific life phases can affect the timing of marriage and childbirth. This is relevant for both academic and political discussions. Our study contributes to the literature on policies that affect family formation by highlighting the importance of unintended consequences of policies that reduce or extend specific life phases. Our study also contributes to the literature on the relationship between education and family formation by proposing the *duration effect* as a third mechanism, how education might causally affect family formation. While the previous literature focuses on human capital effects and lock-in effects, the duration effect is so far neglected. We provide evidence that education influences family formation through its effect on the timing of subsequent life phases that individuals typically traverse before forming families. Another contribution of this study is the compilation of relevant education reforms in West Germany and the respective law sources, which can be also used by other researchers.

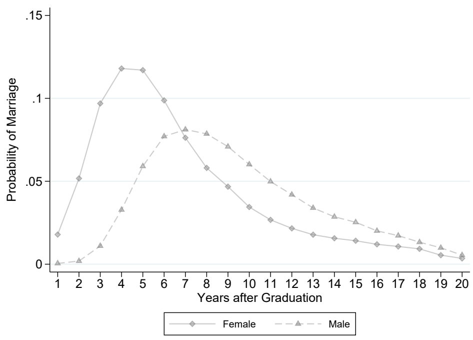
Our findings carry also important messages for policymakers. On the one hand, policymakers should be aware of the consequences for family formation when discussing policies that affect the duration of specific life phases (e.g., changing the duration of compulsory military service, secondary schooling, or university education). On the other hand, our study highlights that these policies might offer a tool for policymakers who would like to change the timing of family formation. Both aspects are particularly relevant, given that not only is the age at marriage and age of first birth increasing in many countries, but the duration of the education phase is also increasing.

While there are some worries that the lockdown policies enacted in many countries (including school closures) to fight the Covid-19 Pandemic will prolong the education phase, its effects on marriage and fertility are ambiguous as these lockdown policies might not only affect the duration of the education phase but also human capital acquisition.

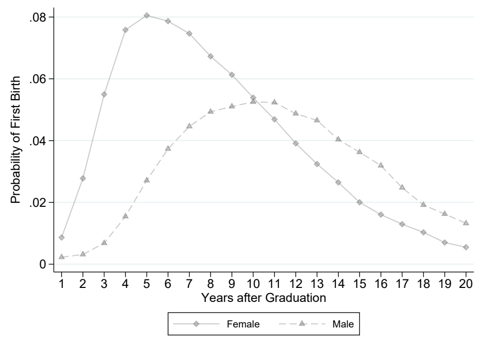
3.9 Appendix A

Figure 3-6: Probability of Marriage and First Birth by Years after Graduation and Gender

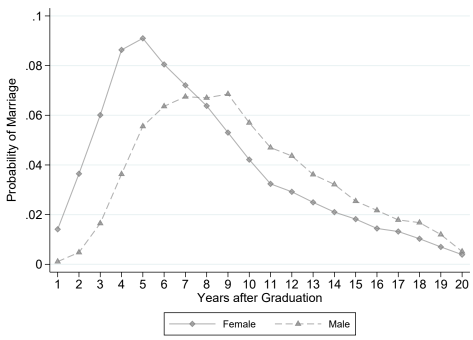
(a) Marriage - Basic



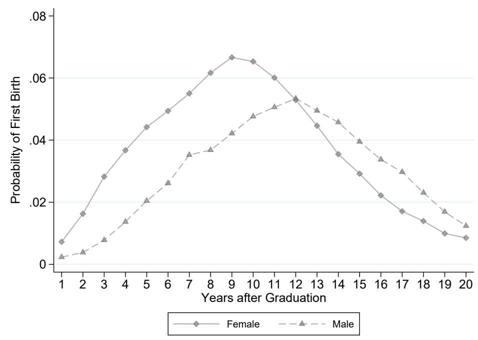
(b) First Birth - Basic



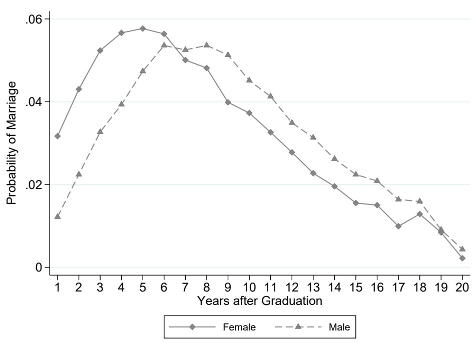
(c) Marriage - Middle



(d) First Birth - Middle



(e) Marriage - Academic

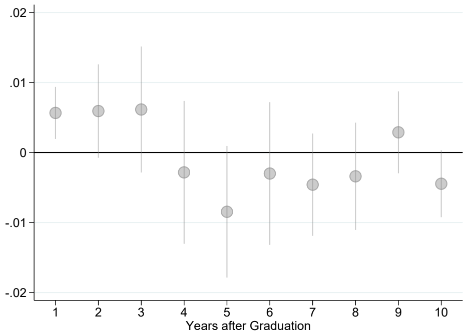


(f) First Birth - Academic

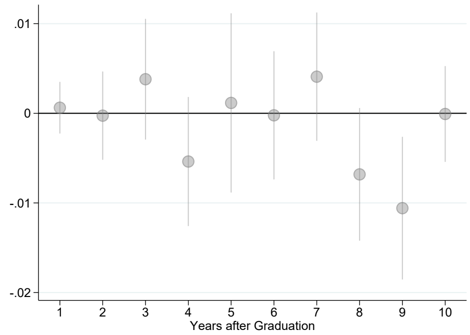
Notes: The table presents graphs from Figure 3-3 separately for men and women.

Figure 3-7: Effect of Short School Years *at* different Years after Graduation (Females)

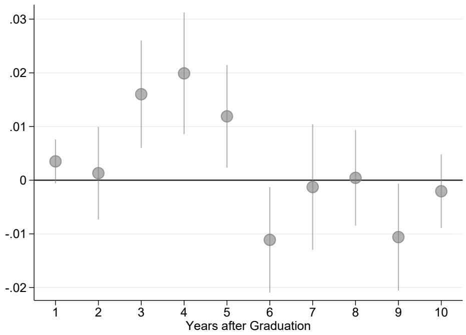
(a) Marriage - Basic



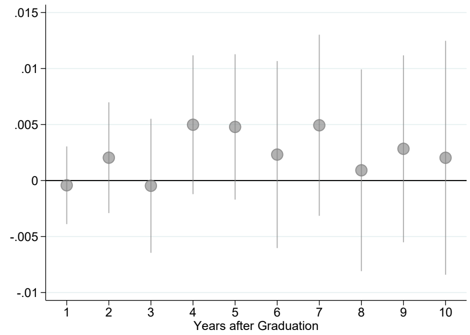
(b) First Birth - Basic



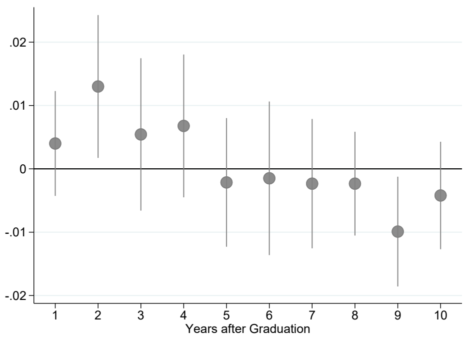
(c) Marriage - Middle



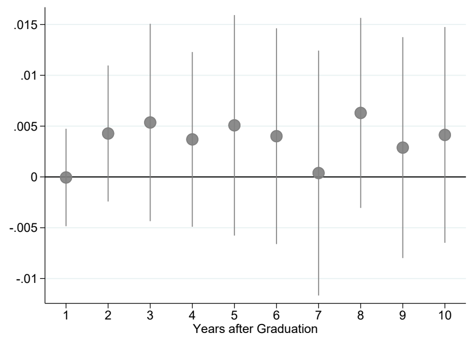
(d) First Birth - Middle



(e) Marriage - Academic



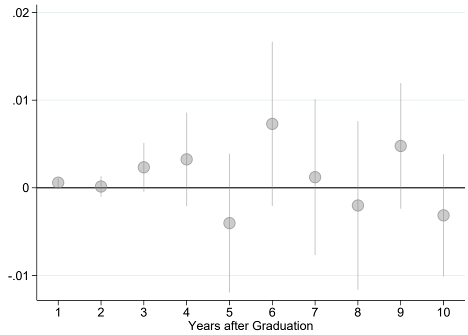
(f) First Birth - Academic



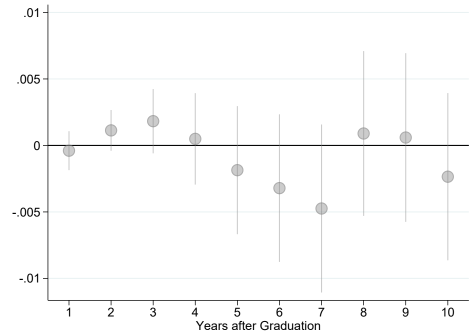
Notes: The graphs show the estimated short school year effects (and their 95% confidence intervals) *at* different years after graduation for females. The underlying samples are identical to the samples in Table 3.2 and all coefficients are based on Equation (3.2). The figure is related to Figure 3-4, which shows short school year effects *up* to different years after graduation.

Figure 3-8: Effect of Short School Years *at* different Years after Graduation (Males)

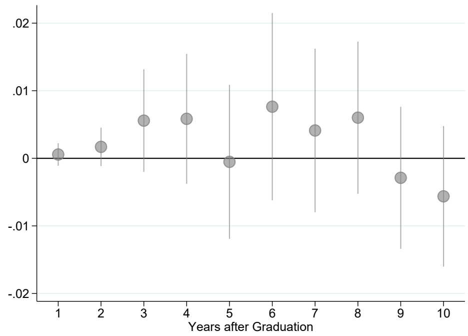
(a) Marriage - Basic



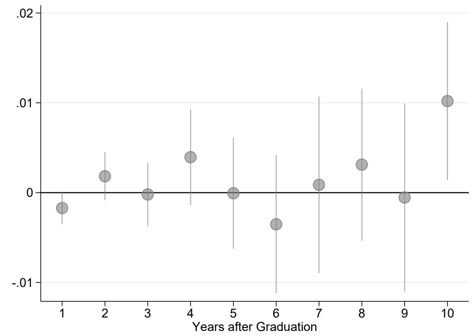
(b) First Birth - Basic



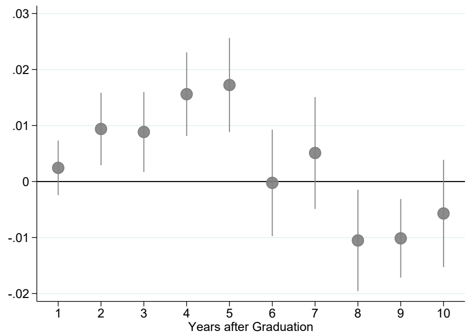
(c) Marriage - Middle



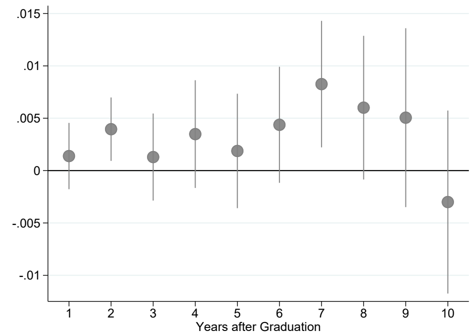
(d) First Birth - Middle



(e) Marriage - Academic



(f) First Birth - Academic



Notes: The graphs show the estimated short school year effects (and their 95% confidence intervals) *at* different years after graduation for males. The underlying samples are identical to the samples in Table 3.2 and all coefficients are based on Equation (3.2). The figure is related to Figure 3-4, which shows short school year effects *up* to different years after graduation.

Table 3.8: Effect of Short School Years on Marriage and First Birth after Graduation from Secondary Schooling by Age

	Age													
	16	17	18	19	20	21	22	23	24	25	26	27	28	29
Panel A: Marriage														
Basic	0.003***	0.006***	0.011***	0.011**	0.005	0.007	0.005	0.003	0.006	0.003	-0.002	-0.004	-0.008	-0.006
	(0.001)	(0.002)	(0.003)	(0.004)	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)
N	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	188,330	173,232	157,588	140,755
Middle	0.001	0.003**	0.005	0.016***	0.030***	0.037***	0.034***	0.035***	0.037***	0.030***	0.026**	0.022**	0.018*	0.013
	(0.001)	(0.002)	(0.004)	(0.005)	(0.007)	(0.008)	(0.011)	(0.012)	(0.011)	(0.011)	(0.011)	(0.009)	(0.010)	(0.009)
N	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	91,869	84,934	77,690
Academic	0.000	0.001*	0.002	0.004**	0.007**	0.018***	0.025***	0.037***	0.046***	0.046***	0.048***	0.041***	0.031***	0.025**
	(0.000)	(0.001)	(0.001)	(0.002)	(0.003)	(0.005)	(0.006)	(0.008)	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)
N	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199
Panel B: First Birth														
Basic	-0.000	0.000	0.003	0.001	0.000	-0.002	-0.002	-0.005	-0.010*	-0.011*	-0.015**	-0.009	-0.008	-0.005
	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
N	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	188,330	173,232	157,588	140,755
Middle	-0.001	-0.002	0.000	0.000	0.005	0.007	0.007	0.011	0.012	0.014	0.019*	0.015	0.020*	0.024**
	(0.001)	(0.001)	(0.002)	(0.003)	(0.004)	(0.005)	(0.006)	(0.008)	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)
N	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	91,869	84,934	77,690
Academic	-0.000	-0.000	0.000	0.002	0.002	0.007**	0.010*	0.013**	0.016**	0.021***	0.025***	0.032***	0.036***	0.036***
	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.005)	(0.006)	(0.007)	(0.008)	(0.009)	(0.010)	(0.010)	(0.010)
N	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199

Notes: The table shows the effect of the short school years by age instead of by years after graduation. All regressions include the same set of fixed effects as the main regression specification. Standard errors clustered at the cohort-state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.9: Effect of Short School Years on Wage and Employment

	Main	**Male**
	(1)	(2)
Panel A: Wage		
Pischke (2007) Results	0.017	0.001
	(0.011)	(0.011)
N	723,470	430,859
Pischke (2007) Replication	0.014	-0.007
	(0.014)	(0.015)
N	755,093	454,518
Panel B: Employment		
Pischke (2007) Results	0.016***	0.013*
	(0.006)	(0.007)
N	1,032,744	509,770
Pischke (2007) Replication	0.020**	0.015*
	(0.008)	(0.009)
N	1,071,256	536,235

Notes: This table replicates wage and employment regressions using the same sample and wave restrictions as specified in Pischke (2007). Exposure to short school years is assigned according to secondary school track, birth cohort (1943-1964), and state of residence. The first row of each Panel displays estimation results taken from Table 5 and Table 8 in Pischke (2007), respectively. In Panel A, the dependent variable is the log hourly wage, in Panel B a dummy for being employed in the survey week. Standard errors in parentheses are clustered at the track-cohort-state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.10: Robustness Checks - Alternative Age, Wave, State and SE Specifications *Five* Years after Graduation

	Other Age Restrictions			**Wave Restrictions**		**State Restrictions**		**Birthyear**	**SE**
						-LS	+SL		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Marriage									
Basic	0.005	0.010*	-0.005	0.005	0.006	0.006	0.005	0.006	0.005
	(0.008)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)
N	99,250	250,131	237,942	203,501	166,094	180,009	207,963	192,430	203,501
Middle	0.039***	0.033***	0.041***	0.037***	0.038***	0.041***	0.037***	0.030***	0.037***
	(0.011)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.007)
N	44,503	127,077	118,417	98,448	89,000	86,245	99,807	89,226	98,448
Academic	0.055***	0.042***	0.039***	0.046***	0.044***	0.048***	0.047***	0.046***	0.046**
	(0.013)	(0.009)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.014)
N	51,566	140,849	113,732	103,698	106,139	98,348	110,756	97,768	109,199
Panel B: First Birth									
Basic	0.005	0.001	-0.007*	0.000	-0.002	0.000	0.000	0.003	0.000
	(0.006)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
N	99,250	250,131	237,942	203,501	166,094	180,009	207,963	192,430	203,501
Middle	0.008	0.008*	0.004	0.007	0.009*	0.007	0.008	0.008	0.007
	(0.007)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)	(0.005)
N	44,503	127,077	118,417	98,448	89,000	86,245	99,807	89,226	98,448
Academic	0.030***	0.010	0.011*	0.017**	0.015**	0.015**	0.017**	0.018**	0.016**
	(0.009)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
N	51,566	140,849	113,732	103,698	106,139	98,348	110,756	97,768	109,199
Years after Grad.	10-15	5-20	Age 30-40	10-20	10-20	10-20	10-20	10-20	10-20
Age Basic	25-30	20-35	30-40	25-35	25-35	25-35	25-35	25-35	25-35
Age Middle	26-31	21-36	30-40	26-36	26-36	26-36	26-36	26-36	26-36
Age Academic	29-34	24-39	30-40	29-39	29-39	29-39	29-39	29-39	29-39
Waves	1976-2003	1976-2003	1976-2003	1976-2001	1980-2003	1976-2003	1976-2003	1976-2003	1976-2003

Notes: The table shows the effect of the short school years five years after graduation for various alternative specifications. Columns (1)-(3) rely on information provided in different periods after graduation, as indicated in bottom part of the table. Column (4) disregards the last two waves of the Micro Census and column (5) drops the first two waves. Columns (6)-(7) drop or include certain states from the analysis (Lower Saxony (LS), Saarland (SL), respectively). Column (8) relies on treatment identification based on individual's birthyear and column (9) on standard errors clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.11: Robustness Checks - Alternative Age, Wave, State and SE Specifications *Ten* Years after Graduation

	Other Age Restrictions		**Wave Restrictions**		**State Restrictions**		**Birthyear**	**SE**
					-LS	+SL		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Marriage								
Basic	0.004	-0.001	0.003	0.005	-0.006	0.004	0.007	0.003
	(0.009)	(0.008)	(0.008)	(0.007)	(0.009)	(0.008)	(0.008)	(0.014)
N	99,250	237,942	203,501	166,094	180,009	207,963	192,430	203,501
Middle	0.028*	0.023***	0.026**	0.028**	0.031**	0.027**	0.019**	0.026*
	(0.014)	(0.009)	(0.011)	(0.011)	(0.013)	(0.011)	(0.009)	(0.013)
N	44,503	118,417	98,448	89,000	86,245	99,807	89,226	98,448
Academic	0.039**	0.022**	0.027**	0.024**	0.032***	0.025**	0.022*	0.025
	(0.018)	(0.010)	(0.012)	(0.011)	(0.011)	(0.011)	(0.012)	(0.025)
N	51,566	113,732	103,698	106,139	98,348	110,756	97,768	109,199
Panel B: First Birth								
Basic	-0.005	-0.024***	-0.011*	-0.013**	-0.018***	-0.011*	-0.006	-0.011
	(0.008)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)
N	99,250	237,942	203,501	166,094	180,009	207,963	192,430	203,501
Middle	0.009	0.012	0.019*	0.023**	0.019	0.020*	0.016	0.019
	(0.015)	(0.008)	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)
N	44,503	118,417	98,448	89,000	86,245	99,807	89,226	98,448
Academic	0.059***	0.026***	0.038***	0.035***	0.037***	0.037***	0.036***	0.036***
	(0.012)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)
N	51,566	113,732	103,698	106,139	98,348	110,756	97,768	109,199
Years after Grad.	10-15	Age 30-40	10-20	10-20	10-20	10-20	10-20	10-20
Age Basic	25-30	30-40	25-35	25-35	25-35	25-35	25-35	25-35
Age Middle	26-31	30-40	26-36	26-36	26-36	26-36	26-36	26-36
Age Academic	29-34	30-40	29-39	29-39	29-39	29-39	29-39	29-39
Waves	1976-2003	1976-2003	1976-2001	1980-2003	1976-2003	1976-2003	1976-2003	1976-2003

Notes: The table shows the effect of the short school years ten years after graduation for various alternative specifications. Columns (1)-(2) rely on information provided in differing numbers of years after graduation, as indicated in the bottom part of the table. Column (3) disregards the last two waves of the Micro Census, and column (4) drops the first two waves. Columns (5)-(6) drop or include certain states from the analysis (Lower Saxony (LS), Saarland (SL), respectively). Column (7) relies on treatment identification based on individual's birth year and column (8) on standard errors clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.12: Robustness Checks: Alternative Cohort Restrictions

	5 Years				8 Years				10 Years			
	Main	+Prim	-0.5 SSY	-last pre-treat	Main	+Prim	-0.5 SSY	-last pre-treat	Main	+Prim	-0.5 SSY	-last pre-treat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Marriage												
Basic	0.005	0.001	0.006	0.006	0.003	-0.000	0.006	0.006	0.003	-0.000	0.006	0.005
	(0.006)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.008)
N	203,501	247,074	190,370	189,221	203,501	247,074	190,370	189,221	203,501	247,074	190,370	189,221
Middle	0.037***	0.027***	0.036***	0.037***	0.037***	0.026***	0.035***	0.038***	0.026**	0.014	0.025**	0.026**
	(0.008)	(0.006)	(0.008)	(0.008)	(0.011)	(0.009)	(0.011)	(0.012)	(0.011)	(0.009)	(0.011)	(0.011)
N	98,448	125,936	93,999	94,046	98,448	125,936	93,999	94,046	98,448	125,936	93,999	94,046
Academic	0.046***	0.031***	0.046***	0.049***	0.041***	0.023**	0.040***	0.045***	0.025**	0.014	0.025**	0.029**
	(0.009)	(0.008)	(0.009)	(0.008)	(0.011)	(0.010)	(0.011)	(0.010)	(0.011)	(0.010)	(0.011)	(0.011)
N	109,199	143,134	106,052	106,285	109,199	143,134	106,052	106,285	109,199	143,134	106,052	106,285
Panel B: First Birth												
Basic	0.000	0.000	0.001	-0.001	-0.005	-0.003	-0.003	-0.005	-0.011*	-0.006	-0.008	-0.010
	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
N	203,501	247,074	190,370	189,221	203,501	247,074	190,370	189,221	203,501	247,074	190,370	189,221
Middle	0.007	0.005	0.006	0.007	0.012	0.008	0.009	0.012	0.019*	0.012	0.016	0.019*
	(0.005)	(0.004)	(0.005)	(0.005)	(0.009)	(0.006)	(0.009)	(0.009)	(0.011)	(0.008)	(0.011)	(0.011)
N	98,448	125,936	93,999	94,046	98,448	125,936	93,999	94,046	98,448	125,936	93,999	94,046
Academic	0.016**	0.008	0.016**	0.018***	0.032***	0.019**	0.031***	0.035***	0.036***	0.026***	0.035***	0.040***
	(0.007)	(0.006)	(0.007)	(0.007)	(0.010)	(0.008)	(0.009)	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)
N	109,199	143,134	106,052	106,285	109,199	143,134	106,052	106,285	109,199	143,134	106,052	106,285

Notes: The table shows the effect of the short school years based on Equation (3.2) using different cohort restrictions. Columns (1), (5), and (9) repeat for comparison the main results for five, eight, and ten years after graduation, respectively. Columns (2), (6), and (10) include also cohorts that were affected by short school years in primary school, while columns (3), (7), and (11) disregard cohorts with only one short school year. Finally, columns (4), (8), and (12) exclude the last pre-treatment cohort. Standard errors clustered at the cohort-state level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.13: Controlling for Compulsory Schooling (CS) Reforms

	Main	**CS Control**	
	(1)	(2)	(3)
Panel A: Marriage - Basic Track			
5 Years after Graduation	0.005	-0.001	0.005
	(0.006)	(0.006)	(0.007)
8 Years after Graduation	0.003	-0.006	-0.006
	(0.007)	(0.008)	(0.007)
10 Years after Graduation	0.003	-0.007	-0.008
	(0.008)	(0.009)	(0.008)
N	203,501	203,501	203,501
Panel B: First Birth - Basic Track			
5 Years after Graduation	0.000	-0.000	-0.001
	(0.004)	(0.004)	(0.004)
8 Years after Graduation	-0.005	-0.004	-0.007
	(0.005)	(0.006)	(0.005)
10 Years after Graduation	-0.011*	-0.009	-0.014**
	(0.006)	(0.007)	(0.007)
N	203,501	203,501	203,501
No CS Control	Yes	No	No
CS Control Refined	No	Yes	No
CS Control Pischke (2007)	No	No	Yes

Notes: The table shows the effect of the short school years on marriage and first birth five, eight, and ten years after graduation for individuals in the basic track, without controlling for compulsory schooling reforms (column 1), controlling for compulsory schooling reforms according to our refined dates (column 2), and the reform dates used by Pischke (2007) in column (3). The latter reform dates coincide with reform dates used in Pischke and von Wachter (2005, 2008). Standard errors clustered at the cohort-state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.14: Placebo Outcome: Track Choice

	Sample 1952	**Sample 1950**
	(1)	(2)
Basic	0.030***	0.027***
	(0.008)	(0.009)
N	271,634	290,336
Middle	-0.024***	-0.020***
	(0.004)	(0.005)
N	271,634	290,336
Academic	-0.006	-0.006
	(0.008)	(0.007)
N	271,634	290,336

Notes: The table shows the effect of the short school years on secondary school track choice to help identify potential differential trends in treatment and control states. We therefore exclude cohorts 1954-1958 and Lower Saxony in both models to assign the treatment indicator independent from track. Sample 1952 comprises the other cohorts from our main sample, while Sample 1950 additionally includes cohorts 1950 and 1951. In both samples, we control for cohort, state, sex, wave FE and all interactions with sex. Standard errors clustered at the cohort-state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.15: Results for Basic and Middle Track Pooled

	5 Years	8 Years	10 Years
	(1)	(2)	(3)
Panel A: Marriage			
Basic and Middle	0.013***	0.013*	0.011
	(0.004)	(0.007)	(0.007)
N	301,949	301,949	301,949
Panel B: First Birth			
Basic and Middle	0.001	-0.000	-0.003
	(0.003)	(0.005)	(0.006)
N	301,949	301,949	301,949

Notes: The table shows the effect of the short school years on marriage and first birth five, eight, and ten years after graduation from secondary school for individuals pooled together in basic and middle secondary school track based on Equation (3.2). Standard errors clustered at the cohort-state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.16: Placebo Treatments

	Placebo Treatment in		
	-3 years	-2 years	-1 years
	(1)	(2)	(3)
Panel A: Marriage			
Basic	0.009	0.011	0.009
	(0.008)	(0.008)	(0.007)
N	66,333	66,333	66,333
Middle	0.033**	0.012	-0.014
	(0.016)	(0.015)	(0.011)
N	17,565	17,565	17,565
Academic	0.021	0.008	-0.023
	(0.016)	(0.020)	(0.018)
N	7,043	7,043	7,043
Panel B: First Birth			
Basic	-0.004	-0.003	-0.009
	(0.006)	(0.007)	(0.006)
N	66,333	66,333	66,333
Middle	0.007	-0.014	-0.017
	(0.010)	(0.009)	(0.011)
N	17,565	17,565	17,565
Academic	0.017	0.006	0.008
	(0.010)	(0.011)	(0.011)
N	7,043	7,043	7,043

Notes: The table presents the effects of placebo treatments for our outcomes measured fife years after graduation. Placebo treatments assume that the treatment took place 3, 2, and 1 years earlier, respectively. The sample includes just pre-treatment cohorts, i.e. cohorts 1950-1957 for basic, 1950-1956 for middle, and 1950-1953 for academic track. Results look very similar when the four states that have been treated in 1950 or 1951 in line with the Düsseldorf Accord (see Appendix 3.10 and 3-9) are excluded from this analysis. Standard errors clustered at the cohort-state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.17: Longer Time Horizons (constant sample)

	Years after Graduation					
	10	11	12	13	14	15
Panel A: Marriage						
Basic	0.001	-0.002	-0.006	-0.007	-0.006	-0.008
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)
N	122,803	122,803	122,803	122,803	122,803	122,803
Middle	0.026**	0.026**	0.018*	0.012	0.007	0.001
	(0.011)	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)
N	61,960	61,960	61,960	61,960	61,960	61,960
Academic	0.015	0.008	0.006	0.004	-0.003	-0.008
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.010)
N	68,785	68,785	68,785	68,785	68,785	68,785
Panel B: First Birth						
Basic	-0.014*	-0.013*	-0.009	-0.006	-0.005	-0.003
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
N	122,803	122,803	122,803	122,803	122,803	122,803
Middle	0.027**	0.020*	0.027***	0.023**	0.018*	0.012
	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
N	61,960	61,960	61,960	61,960	61,960	61,960
Academic	0.019	0.019	0.018	0.019	0.016	0.017
	(0.013)	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)
N	68,785	68,785	68,785	68,785	68,785	68,785
Panel C: Second Birth						
Basic	-0.003	-0.009*	-0.006	-0.007	-0.010	-0.007
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
N	122,803	122,803	122,803	122,803	122,803	122,803
Middle	0.013**	0.017***	0.014*	0.012	0.013	0.014
	(0.005)	(0.006)	(0.007)	(0.009)	(0.009)	(0.009)
N	61,960	61,960	61,960	61,960	61,960	61,960
Academic	0.008	0.002	-0.001	-0.001	0.005	0.001
	(0.009)	(0.010)	(0.010)	(0.012)	(0.013)	(0.013)
N	68,785	68,785	68,785	68,785	68,785	68,785
Panel D: Third Birth						
Basic	0.007***	0.008***	0.008**	0.007*	0.007*	0.007*
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
N	122,803	122,803	122,803	122,803	122,803	122,803
Middle	-0.002	0.000	0.001	0.000	0.001	0.004
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
N	61,960	61,960	61,960	61,960	61,960	61,960
Academic	0.002	0.002	0.005*	0.005*	0.005	0.010**
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
N	68,785	68,785	68,785	68,785	68,785	68,785

Notes: The table shows the effect of the short school years on various outcomes as indicated in the panel header for individuals in different secondary school tracks based on Equation (3.2). In contrast to Table 3.5, the sample in each track is constant across specifications. Standard errors clustered at the cohort-state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

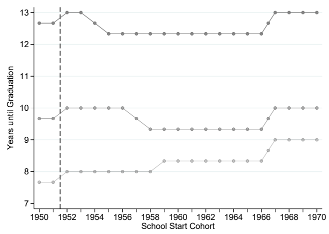
3.10 Appendix B

This paper exploits changes in the length of the schooling phase induced by short school years. Therefore, Tables 3.18 - 3.28 list for students from each West German state and the birth years 1935 through 1965 the regular duration of the schooling phase depending on their secondary school track (columns (8) for basic, (11) for middle, (14) for academic). Based on these numbers, Figure 3-9 graphically depicts the development of the regular length of schooling across cohorts for all West German states and tracks.

Figure 3-9: Refined Assignment to Short School Years

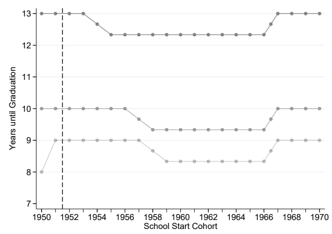
(a) Berlin

(b) Baden-Württemberg



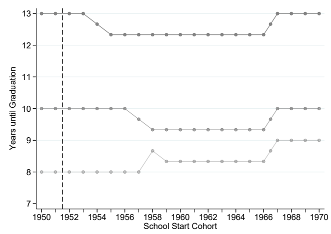
(c) Bavaria

(d) Bremen

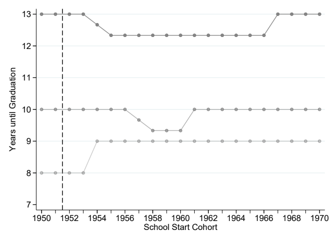


(e) Hamburg

(f) Hesse



(g) Lower Saxony



(h) North Rhine-Westph.

(i) Rhineland-Palatine

(j) Saarland

(k) Schleswig-Holstein

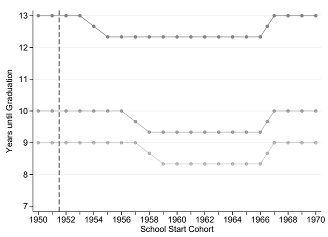


Table Notes Figure 3-9: This figure visualizes the proposed refined assignment to short school years for each West German federal state and school track (dark gray academic, gray middle and light gray basic) based on individuals' regular duration of the schooling phase (see also columns (8) for basic, (11) for middle, and (14) for academic in Tables 3.18-3.28). Narrowing the sample window by two years compared to the setting applied in Pischke (2007) and Braakmann (2010) (as indicated by the dashed lines) and excluding Saarland from our analysis allows us to abstain from using school cohorts that have experienced shorter school years due to earlier shifts of the school starting year from fall to Easter as control units (as in Baden-Württemberg, Bremen, Saarland and West Berlin). Hence, within our sample period using school starting years from 1952 until 1970, deviations from the regular school years required in each track (8/9 years in basic, 10 in middle and 13 in academic) imply exposure to short school years.

Tables 3.18 - 3.28 exhibit three other noteworthy features. First, they provide the necessary information for the computation of the regular duration of the schooling phase (see below). Second, the tables list the relevant legal sources, which we display in detail in Table 3.29.[30] Third, the tables mark the SSY treatment status and the cohorts included in the main analyses (columns (6), (9), and (12)).

Regarding the first point: For an individual in a given state and track, the regular duration of the schooling phase depends on the individual's birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), and the subsequent date of school entry (column (4)). Besides the short school years in 1966/67 (columns (6), (9), and (12)), there are two other sources of institutional variation affecting the regular length of the schooling phase. First, several states moved the start of the school year from fall to Easter in line with the Düsseldorf Accord (DA) in 1955. These changes are considered in the computation of the regular school duration as well and the respective law sources are listed in law column (17) of Tables 3.18 - 3.28. Cohorts affected by these changes are generally excluded from our analyses (see also Section 3.3.4 and the dashed lines in Figure 3-9, which indicate deviations from the time windows used in Pischke (2007) and Braakmann (2010)).

[30]In all tables, law sources are stated in columns (15)-(18) and compiled in Table 3.29 in case of changes concerning school-entry cut-off rules, compulsory schooling (CPS), or start of the school year reforms - either in line with the Düsseldorf Accord (DA) or with the Hamburg Accord (HA).

Second, the basic track was extended from eight to nine years between World War II and the SSY introduction in different West German states at different points in time (columns 5). In the states of Baden-Württemberg, North-Rhine-Palatine, Rhineland-Palatine, and Hesse, the introduction of a mandatory ninth grade coincided with the implementation of short school years. In these states but Hesse, instead of adding an extra year of schooling to graduating classes in order to fulfill the new minimum of nine years of schooling, students had to attend two short school years. As a result, these students who would have graduated regularly after eight years at Easter 1967 stayed in school until fall 1967. Hence, the net effect for these affected cohorts was an increase in time spent in school compared to previous cohorts (four months longer), but less of an increase relative to states for which the change in compulsory schooling implied an increase of a full year of schooling. In the state of Hesse, implementation of an additional school year took place with the first short school year. As a result, students who entered school in April 1958 would have graduated after eight years in March 1966 but had to attend one additional school year and thus, the first short school year before graduating in November 1966 after eight years and eight months time of instruction.

Several economic studies exploit these increases in compulsory years of schooling in Germany as an instrument for education. The literature mainly uses two sets of compulsory schooling reform dates; reform years initially proposed in the study by Pischke and von Wachter (2005, 2008)[31] and a second set referring to a book chapter by Leschinsky and Roeder (1980) with slightly different reform dates used in studies by e.g. Piopiunik (2011, 2014). For the former set of reform years, the authors do not specify any primary sources for the applied reform dates, which is also pointed out in Helbig and Nikolai (2015, p.62).[32] We propose a refined set of reform years based on a comprehensive collection of educational laws as primary sources for regulating compulsory schooling in each federal state.[33] We thereby corroborate the second set of reform dates and also take age cut-offs for primary school enrolment into account.[34] In Saarland, the school year was prolonged to eight years and eight months due to the change of school year start in 1958 (SL58 in Table 3.29), and as the statistical year book (issue 1967) does not show a double graduating class in 1965 German Federal Statistical Office (1973), we interpret the year of 1958 as reform year that introduced a ninth school year.

[31]Studies applying the respective reform set include e.g. Siedler (2010); Fort et al. (2011); Kemptner et al. (2011); Huebener (2019); Margaryan et al. (2021).

[32]Furthermore, studies also vary with respect to assigning birth cohorts that were first to experience longer compulsory schooling. Most studies use a school start in the year children turn six or seven. None of these studies take into consideration that the legal age cut-offs determining binding school starts vary on the federal level.

[33]Our legal sources largely coincide with sources collected in Helbig and Nikolai (2015, p.61 ff), but in addition include reform dates for the states of Bremen, Hamburg, and West Berlin; for all three of which Helbig and Nikolai (2015) assign a mandatory ninth school year before the start of their analysis in 1949. However, for the city-state of Bremen, we find that even though HB49 (in Table 3.29) establishes compulsory schooling of nine years of schooling, implementation depended on a separate resolution of the Senate, which never appeared. Consequently, we assign the compulsory schooling reform according to HB57 to the school starting cohort 1951 as the first cohort to attend nine years of basic track schooling and graduate at Easter 1960. For the state of Baden-Württemberg, we also deviate from Helbig and Nikolai (2015), as BW66 (in Table 3.29) clearly states the joint implementation of a ninth grade with the second short school year.

[34]We compared and discussed the results of our background research on the reform dates with Kamila Cygan-Rehm, who simultaneously and independently conducted institutional research on this issue for continuing research projects building on Cygan-Rehm (2018). Hampf (2019) and Bömmel and Heineck (2020) also use this set of reform dates.

Reading help: The first line of Table 3.18 (West Berlin) shows that for individuals of the 1935 birth cohort (1) the relevant cut-off for school entry was December 31st (3). Children started school in August 1941 (4), for basic track students compulsory schooling was nine years (5) and they did not have any short school years (6). Consequently, their regular graduation point was July 1950 (7) after nine years of school (8). The "-" in column (6) indicates that these individuals are neither part of the treatment group nor of the control group in our main analysis. Similarly, individuals in the middle track are not part of our main sample (9). Their regular graduation time was July 1951 (10) after 10 years of schooling (11). Individuals in the academic track regularly finished school in March 1954 (13), after 12.67 school years (14). They did not spend a full 13 years in school due to the moving the start of the school year from fall to Easter in line with the Düsseldorf Accord. Columns (15)-(18) display abbreviations of the relevant law sources that can be looked up in Table 3.29. Columns (15) and (16) indicate the first school starting cohort that was affected by changes in the cut-off for school entry and compulsory schooling, respectively. Columns (17) and (18) indicate all cohorts that are affected by shorter school years due to Düsseldorf Accord and Hamburg Accord, respectively.

Table 3.18: West Berlin

Birth		School Start		Basic: Graduation				Middle: Graduation			Academic: Graduation			Law Sources			
Year (1)	Mths (2)	Cut-Off (3)	Year/Mth (4)	CPS (5)	SSY (6)	Year/Mth (7)	SchYrs (8)	SSY (9)	Year/Mth (10)	SchYrs (11)	SSY (12)	Year/Mth (13)	SchYrs (14)	Cut-Off (15)	CPS (16)	DA (17)	HA (18)
1935	1-12	12/31	1941/08	9	-	1950/07	9	-	1951/07	10	-	1954/03	12.67		BE48	BE51	
1936	1-12	12/31	1942/08	9	-	1951/07	9	-	1952/03	9.67	-	1955/03	12.67			BE51	
1937	1-12	12/31	1943/08	9	-	1952/03	8.67	-	1953/03	9.67	-	1956/03	12.67			BE51	
1938	1-12	12/31	1944/08	9	-	1953/03	8.67	-	1954/03	9.67	-	1957/03	12.67			BE51	
1939	1-12	12/31	1945/08	9	-	1954/03	8.67	-	1955/03	9.67	-	1958/03	12.67			BE51	
1940	1-12	12/31	1946/08	9	-	1955/03	8.67	-	1956/03	9.67	-	1959/03	12.67			BE51	
1941	1-12	12/31	1947/08	9	-	1956/03	8.67	-	1957/03	9.67	-	1960/03	12.67			BE51	
1942	1-12	12/31	1948/08	9	-	1957/03	8.67	-	1958/03	9.67	-	1961/03	12.67			BE51	
1943	1-12	12/31	1949/08	9	-	1958/03	8.67	-	1959/03	9.67	-	1962/03	12.67			BE51	
1944	1-12	12/31	1950/08	9	-	1959/03	8.67	-	1960/03	9.67	-	1963/03	12.67			BE51	
1945	1-12	12/31	1951/08	9	-	1960/03	8.67	-	1961/03	9.67	-	1964/03	12.67			BE51	
1946	1-6	06/30	1952/04	9	0	1961/03	9	0	1962/03	10	0	1965/03	13	BE51			
1946	7-12	06/30	1953/04	9	0	1962/03	9	0	1963/03	10	0	1966/03	13				
1947	1-6	06/30	1953/04	9	0	1962/03	9	0	1963/03	10	0	1966/03	13				
1947	7-12	06/30	1954/04	9	0	1963/03	9	0	1964/03	10	0	1967/03	13				
1948	1-6	06/30	1954/04	9	0	1963/03	9	0	1964/03	10	0	1967/03	13				
1948	7-12	06/30	1955/04	9	0	1964/03	9	0	1965/03	10	0	1968/03	13				
1949	1-6	06/30	1955/04	9	0	1964/03	9	0	1965/03	10	0	1968/03	13				
1949	7-12	03/31	1956/04	9	0	1965/03	9	0	1966/03	10	0	1969/03	13	BE55			
1950	1-3	03/31	1956/04	9	0	1965/03	9	0	1966/03	10	0	1969/03	13	BE55			
1950	4-12	03/31	1957/04	9	0	1966/03	9	0	1967/03	10	0	1970/03	13				
1951	1-3	03/31	1957/04	9	0	1966/03	9	0	1967/03	10	0	1970/03	13				
1951	4-12	03/31	1958/04	9	0	1967/03	9	0	1968/03	10	0	1971/03	13				
1952	1-3	03/31	1958/04	9	0	1967/03	9	0	1968/03	10	0	1971/03	13				
1952	4-12	03/31	1959/04	9	0	1968/03	9	0	1969/03	10	0	1972/03	13				
1953	1-3	03/31	1959/04	9	0	1968/03	9	0	1969/03	10	0	1972/03	13				
1953	4-12	03/31	1960/04	9	0	1969/03	9	0	1970/03	10	0	1973/03	13				
1954	1-3	03/31	1960/04	9	0	1969/03	9	0	1970/03	10	0	1973/03	13				
1954	4-12	03/31	1961/04	9	0	1970/03	9	0	1971/03	10	0	1974/03	13				
1955	1-3	03/31	1961/04	9	0	1970/03	9	0	1971/03	10	0	1974/03	13				
1955	4-12	03/31	1962/04	9	0	1971/03	9	0	1972/03	10	0	1975/03	13				
1956	1-3	03/31	1962/04	9	0	1971/03	9	0	1972/03	10	0	1975/03	13				
1956	4-12	03/31	1963/04	9	0	1972/03	9	0	1973/03	10	0	1976/03	13				
1957	1-3	03/31	1963/04	9	0	1972/03	9	0	1973/03	10	0	1976/03	13				
1957	4-12	03/31	1964/04	9	0	1973/03	9	0	1974/03	10	0	1977/03	13				
1958	1-3	03/31	1964/04	9	0	1973/03	9	0	1974/03	10	0	1977/03	13				
1958	4-12	03/31	1965/04	9	0	1974/03	9	0	1975/03	10	0	1978/03	13				
1959	1-3	03/31	1965/04	9	0	1974/03	9	0	1975/03	10	0	1978/03	13				
1959	4-12	03/31	1966/04	9	0	1975/03	9	0	1976/03	10	0	1979/03	13				
1960	1-3	03/31	1966/04	9	0	1975/03	9	0	1976/03	10	0	1979/03	13				
1960	4-12	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	BE66			
1961	1-6	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	BE66			
1961	7-12	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	1-6	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	7-12	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	1-6	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	7-12	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	1-6	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	7-12	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				
1965	1-6	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				

Notes: This table lists for students of birth years 1935 through 1965 and residence in West Berlin for each school track, the regular duration of the schooling phase (column (8) for basic, (11) for middle, and (14) for academic) based on a comprehensive collection of state education laws (see columns (15)-(18) and compilation in Table 3.29). In addition to track choice, the regular duration of the schooling phase depends on individuals birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), the subsequent date of school entry (column (4)), changes concerning compulsory schooling (CPS) (for basic track students column (5)), as well as the start of the school year reforms - either in line with the Düsseldorf Accord (DA) (moving start of the school year from fall to Easter) or with the Hamburg Accord (HA) (changing it from Easter back to fall). For West Berlin, the number of short school years that stem from the latter policy reform (columns (6), (9), and (12)) is zero for all school starting years and tracks.

Table 3.19: Baden-Württemberg

Birth		School Start		Basic: Graduation				Middle: Graduation			Academic: Graduation			Law Sources			
Year (1)	Mths (2)	Cut-Off (3)	Year/Mth (4)	CPS (5)	SSY (6)	Year/Mth (7)	SchYrs (8)	SSY (9)	Year/Mth (10)	SchYrs (11)	SSY (12)	Year/Mth (13)	SchYrs (14)	Cut-Off (15)	CPS (16)	DA (17)	HA (18)
1935	1-12	12/31	1941/08	8	-	1949/07	8	-	1951/07	10	-	1954/03	12.67			BW52	
1936	1-12	12/31	1942/08	8	-	1950/07	8	-	1952/03	9.67	-	1955/03	12.67			BW52	
1937	1-12	12/31	1943/08	8	-	1951/07	8	-	1953/03	9.67	-	1956/03	12.67			BW52	
1938	1-12	12/31	1944/08	8	-	1952/03	7.67	-	1954/03	9.67	-	1957/03	12.67			BW52	
1939	1-12	12/31	1945/08	8	-	1953/03	7.67	-	1955/03	9.67	-	1958/03	12.67			BW52	
1940	1-12	12/31	1946/08	8	-	1954/03	7.67	-	1956/03	9.67	-	1959/03	12.67			BW52	
1941	1-12	12/31	1947/08	8	-	1955/03	7.67	-	1957/03	9.67	-	1960/03	12.67			BW52	
1942	1-12	12/31	1948/08	8	-	1956/03	7.67	-	1958/03	9.67	-	1961/03	12.67			BW52	
1943	1-12	12/31	1949/08	8	-	1957/03	7.67	-	1959/03	9.67	-	1962/03	12.67			BW52	
1944	1-12	12/31	1950/08	8	-	1958/03	7.67	-	1960/03	9.67	-	1963/03	12.67			BW52	
1945	1-9	09/30	1951/08	8	-	1959/03	7.67	-	1961/03	9.67	-	1964/03	12.67	BW52		BW52	
1945	10-12	03/31	1952/04	8	0	1960/03	8	0	1962/03	10	0	1965/03	13	BW52			
1946	1-3	03/31	1952/04	8	0	1960/03	8	0	1962/03	10	0	1965/03	13				
1946	4-12	04/15	1953/04	8	0	1961/03	8	0	1963/03	10	0	1966/03	13	BW53			
1947	1-4,5	04/15	1953/04	8	0	1961/03	8	0	1963/03	10	0	1966/03	13	BW53			
1947	4.5-12	04/15	1954/04	8	0	1962/03	8	0	1964/03	10	1	1966/11	12.67				BW66a
1948	1-4.5	04/15	1954/04	8	0	1962/03	8	0	1964/03	10	1	1966/11	12.67				BW66a
1948	4.5-12	04/15	1955/04	8	0	1963/03	8	0	1965/03	10	2	1967/07	12.33				BW66a
1949	1-4.5	04/15	1955/04	8	0	1963/03	8	0	1965/03	10	2	1967/07	12.33				BW66a
1949	4.5-12	04/15	1956/04	8	0	1964/03	8	0	1966/03	10	2	1968/07	12.33				BW66a
1950	1-4.5	04/15	1956/04	8	0	1964/03	8	0	1966/03	10	2	1968/07	12.33				BW66a
1950	4.5-12	04/15	1957/04	8	0	1965/03	8	1	1966/11	9.67	2	1969/07	12.33				BW66a
1951	1-4.5	04/15	1957/04	8	0	1965/03	8	1	1966/11	9.67	2	1969/07	12.33				BW66a
1951	4.5-12	12/31	1958/04	8	0	1966/03	8	2	1967/07	9.33	2	1970/07	12.33	BW57			BW66a
1952	1-12	12/31	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33		BW66b		BW66a
1953	1-12	12/31	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				BW66a
1954	1-12	12/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33				BW66a
1955	1-12	12/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33				BW66a
1956	1-12	12/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				BW66a
1957	1-12	12/31	1964/04	9	2	1972/07	8.33	2	1973/07	9.33	2	1976/07	12.33				BW66a
1958	1-12	12/31	1965/04	9	2	1973/07	8.33	2	1974/07	9.33	2	1977/07	12.33				BW66a
1959	1-12	12/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33				BW66a
1960	1-6	06/30	1966/12	9	1	1975/07	8.67	1	1976/07	9.67	1	1979/07	12.67	BW66a			BW66a
1960	7-12	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13				
1961	1-6	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13				
1961	7-12	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	1-6	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	7-12	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	1-6	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	7-12	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	1-6	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	7-12	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				
1965	1-6	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				

Notes: This table lists for students of birth years 1935 through 1965 and residence in Baden-Württemberg for each school track, the regular duration of the schooling phase (column (8) for basic, (11) for middle, and (14) for academic) based on a comprehensive collection of state education laws (see columns (15)-(18) and compilation in Table 3.29). In addition to track choice, the regular duration of the schooling phase depends on individuals birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), the subsequent date of school entry (column (4)), changes concerning compulsory schooling (CPS) (for basic track students column (5)) and start of the school year reforms - either in line with the Düsseldorf Accord (DA) (moving start of the school year from fall to Easter) or with the Hamburg Accord (HA) (changing it from Easter back to fall). For Baden-Württemberg, the number of short school years that stem from the latter policy reform are indicated in columns (6), (9), and (12). Before 1953, Baden-Württemberg was divided into Württemberg-Hohenzollern, Baden and Württemberg-Baden. BW52 implies a law source from Baden. However, variations between the three substates only concerned age cut-off rules for school-starting cohort 1951 (30.09. in Baden and Württemberg-Hohenzollern; 31.05. in Württemberg-Baden).

Table 3.20: Bavaria

Birth		School Start		Basic: Graduation				Middle: Graduation			Academic: Graduation			Law Sources			
Year (1)	Mths (2)	Cut-Off (3)	Year/Mth (4)	CPS (5)	SSY (6)	Year/Mth (7)	SchYrs (8)	SSY (9)	Year/Mth (10)	SchYrs (11)	SSY (12)	Year/Mth (13)	SchYrs (14)	Cut-Off (15)	CPS (16)	DA (17)	HA (18)
1935	1-12	12/31	1941/08	8	-	1949/07	8	-	1951/07	10	-	1954/07	13				
1936	1-12	12/31	1942/08	8	-	1950/07	8	-	1952/07	10	-	1955/07	13				
1937	1-12	12/31	1943/08	8	-	1951/07	8	-	1953/07	10	-	1956/07	13				
1938	1-12	12/31	1944/08	8	-	1952/07	8	-	1954/07	10	-	1957/07	13				
1939	1-12	12/31	1945/08	8	-	1953/07	8	-	1955/07	10	-	1958/07	13				
1940	1-12	12/31	1946/08	8	-	1954/07	8	-	1956/07	10	-	1959/07	13				
1941	1-12	12/31	1947/08	8	-	1955/07	8	-	1957/07	10	-	1960/07	13				
1942	1-12	12/31	1948/08	8	-	1956/07	8	-	1958/07	10	-	1961/07	13				
1943	1-12	12/31	1949/08	8	-	1957/07	8	-	1959/07	10	-	1962/07	13				
1944	1-12	12/31	1950/08	8	-	1958/07	8	-	1960/07	10	-	1963/07	13				
1945	1-12	12/31	1951/08	8	-	1959/07	8	-	1961/07	10	-	1964/07	13				
1946	1-9	09/30	1952/08	8	0	1960/07	8	0	1962/07	10	0	1965/07	13	BY52			
1946	10-12	09/30	1953/08	8	0	1961/07	8	0	1963/07	10	0	1966/07	13				
1947	1-9	09/30	1953/08	8	0	1961/07	8	0	1963/07	10	0	1966/07	13				
1947	10-12	09/30	1954/08	8	0	1962/07	8	0	1964/07	10	0	1967/07	13				
1948	1-9	09/30	1954/08	8	0	1962/07	8	0	1964/07	10	0	1967/07	13				
1948	10-12	09/30	1955/08	8	0	1963/07	8	0	1965/07	10	0	1968/07	13				
1949	1-9	09/30	1955/08	8	0	1963/07	8	0	1965/07	10	0	1968/07	13				
1949	10-12	09/30	1956/08	8	0	1964/07	8	0	1966/07	10	0	1969/07	13				
1950	1-9	09/30	1956/08	8	0	1964/07	8	0	1966/07	10	0	1969/07	13				
1950	10-12	09/30	1957/08	8	0	1965/07	8	0	1967/07	10	0	1970/07	13				
1951	1-9	09/30	1957/08	8	0	1965/07	8	0	1967/07	10	0	1970/07	13				
1951	10-12	09/30	1958/08	8	0	1966/07	8	0	1968/07	10	0	1971/07	13				
1952	1-9	09/30	1958/08	8	0	1966/07	8	0	1968/07	10	0	1971/07	13				
1952	10-12	09/30	1959/08	8	0	1967/07	8	0	1969/07	10	0	1972/07	13				
1953	1-9	09/30	1959/08	8	0	1967/07	8	0	1969/07	10	0	1972/07	13				
1953	10-12	09/30	1960/08	8	0	1968/07	8	0	1970/07	10	0	1973/07	13				
1954	1-9	09/30	1960/08	8	0	1968/07	8	0	1970/07	10	0	1973/07	13				
1954	10-12	09/30	1961/08	9	0	1970/07	9	0	1971/07	10	0	1974/07	13		BY69		
1955	1-9	09/30	1961/08	9	0	1970/07	9	0	1971/07	10	0	1974/07	13		BY69		
1955	10-12	09/30	1962/08	9	0	1971/07	9	0	1972/07	10	0	1975/07	13				
1956	1-9	09/30	1962/08	9	0	1971/07	9	0	1972/07	10	0	1975/07	13				
1956	10-12	09/30	1963/08	9	0	1972/07	9	0	1973/07	10	0	1976/07	13				
1957	1-9	09/30	1963/08	9	0	1972/07	9	0	1973/07	10	0	1976/07	13				
1957	10-12	09/30	1964/08	9	0	1973/07	9	0	1974/07	10	0	1977/07	13				
1958	1-9	09/30	1964/08	9	0	1973/07	9	0	1974/07	10	0	1977/07	13				
1958	10-12	09/30	1965/08	9	0	1974/07	9	0	1975/07	10	0	1978/07	13				
1959	1-9	09/30	1965/08	9	0	1974/07	9	0	1975/07	10	0	1978/07	13				
1959	10-12	09/30	1966/08	9	0	1975/07	9	0	1976/07	10	0	1979/07	13				
1960	1-9	09/30	1966/08	9	0	1975/07	9	0	1976/07	10	0	1979/07	13				
1960	10-12	09/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13				
1961	1-9	09/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13				
1961	10-12	09/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	1-9	09/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	10-12	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13	BY69			
1963	1-6	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13	BY69			
1963	7-12	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	1-6	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	7-12	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				
1965	1-6	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				

Notes: This table lists for students of birth years 1935 through 1965 and residence in Bavaria for each school track, the regular duration of the schooling phase (column (8) for basic, (11) for middle, and (14) for academic) based on a comprehensive collection of state education laws (see columns (15)-(18) and compilation in Table 3.29). In addition to track choice, the regular duration of the schooling phase depends on individuals birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), the subsequent date of school entry (column (4)), changes concerning compulsory schooling (CPS) (for basic track students column (5)) and start of the school year reforms - either in line with the Düsseldorf Accord (DA) (moving start of the school year from fall to Easter) or with the Hamburg Accord (HA) (changing it from Easter back to fall). For Bavaria, the number of short school years that stem from the latter policy reform (columns (6), (9), and (12)) is zero for all school starting years and tracks.

Table 3.21: Bremen

Birth		School Start		Basic: Graduation				Middle: Graduation			Academic: Graduation			Law Sources			
Year (1)	Mths (2)	Cut-Off (3)	Year/Mth (4)	CPS (5)	SSY (6)	Year/Mth (7)	SchYrs (8)	SSY (9)	Year/Mth (10)	SchYrs (11)	SSY (12)	Year/Mth (13)	SchYrs (14)	Cut-Off (15)	CPS (16)	DA (17)	HA (18)
1935	1-12	12/31	1941/08	8	-	1949/03	7.67	-	1951/03	9.67	-	1954/03	12.67			HB48	
1936	1-12	12/31	1942/08	8	-	1950/03	7.67	-	1952/03	9.67	-	1955/03	12.67			HB48	
1937	1-12	12/31	1943/08	8	-	1951/03	7.67	-	1953/03	9.67	-	1956/03	12.67			HB48	
1938	1-12	12/31	1944/08	8	-	1952/03	7.67	-	1954/03	9.67	-	1957/03	12.67			HB48	
1939	1-12	12/31	1945/08	8	-	1953/03	7.67	-	1955/03	9.67	-	1958/03	12.67			HB48	
1940	1-3	03/31	1946/04	8	-	1954/03	8	-	1956/03	10	-	1959/03	13	HB46			
1940	4-12	03/31	1947/04	8	-	1955/03	8	-	1957/03	10	-	1960/03	13				
1941	1-3	03/31	1947/04	8	-	1955/03	8	-	1957/03	10	-	1960/03	13				
1941	4-12	03/31	1948/04	8	-	1956/03	8	-	1958/03	10	-	1961/03	13				
1942	1-3	03/31	1948/04	8	-	1956/03	8	-	1958/03	10	-	1961/03	13				
1942	4-12	03/31	1949/04	8	-	1957/03	8	-	1959/03	10	-	1962/03	13				
1943	1-3	03/31	1949/04	8	-	1957/03	8	-	1959/03	10	-	1962/03	13				
1943	4-12	03/31	1950/04	8	-	1958/03	8	-	1960/03	10	-	1963/03	13				
1944	1-3	03/31	1950/04	8	-	1958/03	8	-	1960/03	10	-	1963/03	13				
1944	4-12	03/31	1951/04	9	-	1960/03	9	-	1961/03	10	-	1964/03	13		HB57		
1945	1-3	03/31	1951/04	9	-	1960/03	9	-	1961/03	10	-	1964/03	13		HB57		
1945	4-12	03/31	1952/04	9	0	1961/03	9	0	1962/03	10	0	1965/03	13				
1946	1-3	03/31	1952/04	9	0	1961/03	9	0	1962/03	10	0	1965/03	13				
1946	4-12	03/31	1953/04	9	0	1962/03	9	0	1963/03	10	0	1966/03	13				
1947	1-3	03/31	1953/04	9	0	1962/03	9	0	1963/03	10	0	1966/03	13				
1947	4-12	03/31	1954/04	9	0	1963/03	9	0	1964/03	10	1	1966/11	12.67				HB67
1948	1-3	03/31	1954/04	9	0	1963/03	9	0	1964/03	10	1	1966/11	12.67				HB67
1948	4-12	03/31	1955/04	9	0	1964/03	9	0	1965/03	10	2	1967/07	12.33				HB67
1949	1-3	03/31	1955/04	9	0	1964/03	9	0	1965/03	10	2	1967/07	12.33				HB67
1949	4-12	03/31	1956/04	9	0	1965/03	9	0	1966/03	10	2	1968/07	12.33				HB67
1950	1-3	03/31	1956/04	9	0	1965/03	9	0	1966/03	10	2	1968/07	12.33				HB67
1950	4-12	03/31	1957/04	9	0	1966/03	9	1	1966/11	9.67	2	1969/07	12.33				HB67
1951	1-3	03/31	1957/04	9	0	1966/03	9	1	1966/11	9.67	2	1969/07	12.33				HB67
1951	4-12	03/31	1958/04	9	1	1966/11	8.67	2	1967/07	9.33	2	1970/07	12.33				HB67
1952	1-3	03/31	1958/04	9	1	1966/11	8.67	2	1967/07	9.33	2	1970/07	12.33				HB67
1952	4-12	03/31	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33				HB67
1953	1-3	03/31	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33				HB67
1953	4-12	03/31	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				HB67
1954	1-3	03/31	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				HB67
1954	4-12	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33				HB67
1955	1-3	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33				HB67
1955	4-12	03/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33				HB67
1956	1-3	03/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33				HB67
1956	4-12	03/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				HB67
1957	1-3	03/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				HB67
1957	4-12	03/31	1964/04	9	2	1972/07	8.33	2	1973/07	9.33	2	1976/07	12.33				HB67
1958	1-3	03/31	1964/04	9	2	1972/07	8.33	2	1973/07	9.33	2	1976/07	12.33				HB67
1958	4-12	03/31	1965/04	9	2	1973/07	8.33	2	1974/07	9.33	2	1977/07	12.33				HB67
1959	1-3	03/31	1965/04	9	2	1973/07	8.33	2	1974/07	9.33	2	1977/07	12.33				HB67
1959	4-12	05/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33	HB65			HB67
1960	1-5	05/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33	HB65			HB67
1960	6-11	11/29	1966/12	9	1	1975/07	8.67	1	1976/07	9.67	1	1979/07	12.67	HB67			HB67
1960	12	06/29	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	HB67			
1961	1-6	06/29	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	HB67			
1961	7-12	06/29	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	1-6	06/29	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	7-12	06/29	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	1-6	06/29	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	7-12	06/29	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	1-6	06/29	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	7-12	06/29	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				
1965	1-6	06/29	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				

Notes: This table lists for students of birth years 1935 through 1965 and residence in Bremen for each school track, the regular duration of the schooling phase (column (8) for basic, (11) for middle, and (14) for academic) based on a comprehensive collection of state education laws (see columns (15)-(18) and compilation in Table 3.29). In addition to track choice, the regular duration of the schooling phase depends on individuals birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), the subsequent date of school entry (column (4)), changes concerning compulsory schooling (CPS) (for basic track students column (5)) and start of the school year reforms - either in line with the Düsseldorf Accord (DA) (moving start of the school year from fall to Easter) or with the Hamburg Accord (HA) (changing it from Easter back to fall). For Bremen, the number of short school years that stem from the latter policy reform are indicated in columns (6), (9), and (12).

Table 3.22: Hamburg

Birth		School Start		Basic: Graduation				Middle: Graduation			Academic: Graduation			Law Sources			
Year (1)	Mths (2)	Cut-Off (3)	Year/Mth (4)	CPS (5)	SSY (6)	Year/Mth (7)	SchYrs (8)	SSY (9)	Year/Mth (10)	SchYrs (11)	SSY (12)	Year/Mth (13)	SchYrs (14)	Cut-Off (15)	CPS (16)	DA (17)	HA (18)
1935	1-8	08/31	1941/08	9	-	1950/03	8.67	-	1951/03	9.67	-	1954/03	12.67	HH41	HH49b	HH49a	
1935	9-12	10/31	1942/08	9	-	1951/03	8.67	-	1952/03	9.67	-	1955/03	12.67	HH41		HH49b	
1936	1-10	10/31	1942/08	9	-	1951/03	8.67	-	1952/03	9.67	-	1955/03	12.67	HH41		HH49b	
1936	11-12	12/31	1943/08	9	-	1952/03	8.67	-	1953/03	9.67	-	1956/03	12.67	R41		HH49b	
1937	1-12	12/31	1943/08	9	-	1952/03	8.67	-	1953/03	9.67	-	1956/03	12.67			HH49b	
1938	1-12	12/31	1944/08	9	-	1953/03	8.67	-	1954/03	9.67	-	1957/03	12.67			HH49b	
1939	1-12	12/31	1945/08	9	-	1954/03	8.67	-	1955/03	9.67	-	1958/03	12.67			HH49b	
1940	1-3	03/31	1946/04	9	-	1955/03	9	-	1956/03	10	-	1959/03	13	HH45			
1940	4-12	03/31	1947/04	9	-	1956/03	9	-	1957/03	10	-	1960/03	13				
1941	1-3	03/31	1947/04	9	-	1956/03	9	-	1957/03	10	-	1960/03	13				
1941	4-12	03/31	1948/04	9	-	1957/03	9	-	1958/03	10	-	1961/03	13				
1942	1-3	03/31	1948/04	9	-	1957/03	9	-	1958/03	10	-	1961/03	13				
1942	4-12	03/31	1949/04	9	-	1958/03	9	-	1959/03	10	-	1962/03	13				
1943	1-3	03/31	1949/04	9	-	1958/03	9	-	1959/03	10	-	1962/03	13				
1943	4-12	03/31	1950/04	9	-	1959/03	9	-	1960/03	10	-	1963/03	13				
1944	1-3	03/31	1950/04	9	-	1959/03	9	-	1960/03	10	-	1963/03	13				
1944	4-12	03/31	1951/04	9	-	1960/03	9	-	1961/03	10	-	1964/03	13				
1945	1-3	03/31	1951/04	9	-	1960/03	9	-	1961/03	10	-	1964/03	13				
1945	4-12	03/31	1952/04	9	0	1961/03	9	0	1962/03	10	0	1965/03	13				
1946	1-3	03/31	1952/04	9	0	1961/03	9	0	1962/03	10	0	1965/03	13				
1946	4-12	03/31	1953/04	9	0	1962/03	9	0	1963/03	10	0	1966/03	13				
1947	1-3	03/31	1953/04	9	0	1962/03	9	0	1963/03	10	0	1966/03	13				
1947	4-12	03/31	1954/04	9	0	1963/03	9	0	1964/03	10	0	1967/03	13				
1948	1-3	03/31	1954/04	9	0	1963/03	9	0	1964/03	10	0	1967/03	13				
1948	4-12	03/31	1955/04	9	0	1964/03	9	0	1965/03	10	0	1968/03	13				
1949	1-3	03/31	1955/04	9	0	1964/03	9	0	1965/03	10	0	1968/03	13				
1949	4-12	03/31	1956/04	9	0	1965/03	9	0	1966/03	10	0	1969/03	13				
1950	1-3	03/31	1956/04	9	0	1965/03	9	0	1966/03	10	0	1969/03	13				
1950	4-12	03/31	1957/04	9	0	1966/03	9	0	1967/03	10	0	1970/03	13				
1951	1-3	03/31	1957/04	9	0	1966/03	9	0	1967/03	10	0	1970/03	13				
1951	4-12	03/31	1958/04	9	0	1967/03	9	0	1968/03	10	0	1971/03	13				
1952	1-3	03/31	1958/04	9	0	1967/03	9	0	1968/03	10	0	1971/03	13				
1952	4-12	03/31	1959/04	9	0	1968/03	9	0	1969/03	10	0	1972/03	13				
1953	1-3	03/31	1959/04	9	0	1968/03	9	0	1969/03	10	0	1972/03	13				
1953	4-12	03/31	1960/04	9	0	1969/03	9	0	1970/03	10	0	1973/03	13				
1954	1-3	03/31	1960/04	9	0	1969/03	9	0	1970/03	10	0	1973/03	13				
1954	4-12	03/31	1961/04	9	0	1970/03	9	0	1971/03	10	0	1974/03	13				
1955	1-3	03/31	1961/04	9	0	1970/03	9	0	1971/03	10	0	1974/03	13				
1955	4-12	12/31	1962/04	9	0	1971/03	9	0	1972/03	10	0	1975/03	13	HH61			
1956	1-12	12/31	1963/04	9	0	1972/03	9	0	1973/03	10	0	1976/03	13				
1957	1-12	12/31	1964/04	9	0	1973/03	9	0	1974/03	10	0	1977/03	13				
1958	1-12	12/31	1965/04	9	0	1974/03	9	0	1975/03	10	0	1978/03	13				
1959	1-12	12/31	1966/04	9	0	1975/03	9	0	1976/03	10	0	1979/03	13				
1960	1-12	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	HH66			
1961	1-6	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	HH66			
1961	7-12	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	1-6	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	7-12	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	1-6	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	7-12	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	1-6	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	7-12	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				
1965	1-6	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				

Notes: This table lists for students of birth years 1935 through 1965 and residence in Hamburg for each school track, the regular duration of the schooling phase (column (8) for basic, (11) for middle, and (14) for academic) based on a comprehensive collection of state education laws (see columns (15)-(18) and compilation in Table 3.29). In addition to track choice, the regular duration of the schooling phase depends on individuals birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), the subsequent date of school entry (column (4)), changes concerning compulsory schooling (CPS) (for basic track students column (5)) and start of the school year reforms - either in line with the Düsseldorf Accord (DA) (moving start of the school year from fall to Easter) or with the Hamburg Accord (HA) (changing it from Easter back to fall). For Hamburg, the number of short school years that stem from the latter policy reform (columns (6), (9), and (12)) is zero for all school starting years and tracks.

Table 3.23: Hesse

Birth		School Start		Basic: Graduation				Middle: Graduation			Academic: Graduation			Law Sources			
Year (1)	Mths (2)	Cut-Off (3)	Year/Mth (4)	CPS (5)	SSY (6)	Year/Mth (7)	SchYrs (8)	SSY (9)	Year/Mth (10)	SchYrs (11)	SSY (12)	Year/Mth (13)	SchYrs (14)	Cut-Off (15)	CPS (16)	DA (17)	HA (18)
1935	1-12	12/31	1941/08	8	-	1950/03	8.67	-	1951/03	9.67	-	1954/03	12.67			HE50	
1936	1-12	12/31	1942/08	8	-	1951/03	8.67	-	1952/03	9.67	-	1955/03	12.67			HE50	
1937	1-12	12/31	1943/08	8	-	1952/03	8.67	-	1953/03	9.67	-	1956/03	12.67			HE50	
1938	1-12	12/31	1944/08	8	-	1953/03	8.67	-	1954/03	9.67	-	1957/03	12.67			HE50	
1939	1-12	12/31	1945/08	8	-	1954/03	8.67	-	1955/03	9.67	-	1958/03	12.67			HE50	
1940	1-12	12/31	1946/08	8	-	1955/03	8.67	-	1956/03	9.67	-	1959/03	12.67			HE50	
1941	1-12	12/31	1947/08	8	-	1956/03	8.67	-	1957/03	9.67	-	1960/03	12.67			HE50	
1942	1-12	06/30	1949/04	8	-	1957/03	8	-	1959/03	10	-	1962/03	13	HE48			
1943	1-6	06/30	1949/04	8	-	1957/03	8	-	1959/03	10	-	1962/03	13				
1943	7-12	06/30	1950/04	8	-	1958/03	8	-	1960/03	10	-	1963/03	13				
1944	1-6	06/30	1950/04	8	-	1958/03	8	-	1960/03	10	-	1963/03	13				
1944	7-12	06/30	1951/04	8	-	1959/03	8	-	1961/03	10	-	1964/03	13				
1945	1-6	06/30	1951/04	8	-	1959/03	8	-	1961/03	10	-	1964/03	13				
1945	7-12	06/30	1952/04	8	0	1960/03	8	0	1962/03	10	0	1965/03	13				
1946	1-6	06/30	1952/04	8	0	1960/03	8	0	1962/03	10	0	1965/03	13				
1946	7-12	06/30	1953/04	8	0	1961/03	8	0	1963/03	10	0	1966/03	13				
1947	1-6	06/30	1953/04	8	0	1961/03	8	0	1963/03	10	0	1966/03	13				
1947	7-12	06/30	1954/04	8	0	1962/03	8	0	1964/03	10	1	1966/11	12.67				HE65a
1948	1-6	06/30	1954/04	8	0	1962/03	8	0	1964/03	10	1	1966/11	12.67				HE65a
1948	7-12	06/30	1955/04	8	0	1963/03	8	0	1965/03	10	2	1967/07	12.33				HE65a
1949	1-6	06/30	1955/04	8	0	1963/03	8	0	1965/03	10	2	1967/07	12.33				HE65a
1949	7-12	06/30	1956/04	8	0	1964/03	8	0	1966/03	10	2	1968/07	12.33				HE65a
1950	1-6	06/30	1956/04	8	0	1964/03	8	0	1966/03	10	2	1968/07	12.33				HE65a
1950	7-12	03/31	1957/04	8	0	1965/03	8	1	1966/11	9.67	2	1969/07	12.33	HE56			HE65a
1951	1-3	03/31	1957/04	8	0	1965/03	8	1	1966/11	9.67	2	1969/07	12.33	HE56			HE65a
1951	4-12	03/31	1958/04	9	1	1966/11	8.67	2	1967/07	9.33	2	1970/07	12.33		HE65a		HE65a
1952	1-3	03/31	1958/04	9	1	1966/11	8.67	2	1967/07	9.33	2	1970/07	12.33		HE65a		HE65a
1952	4-12	03/31	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33				HE65a
1953	1-3	03/31	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33				HE65a
1953	4-12	03/31	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				HE65a
1954	1-3	03/31	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				HE65a
1954	4-12	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33				HE65a
1955	1-3	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33				HE65a
1955	4-12	12/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33	HE61			HE65a
1956	1-12	12/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				HE65a
1957	1-12	12/31	1964/04	9	2	1972/07	8.33	2	1973/07	9.33	2	1976/07	12.33				HE65a
1958	1-12	12/31	1965/04	9	2	1973/07	8.33	2	1974/07	9.33	2	1977/07	12.33				HE65a
1959	1-12	12/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33				HE65a
1960	1-3	03/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33	HE65b			HE65a
1960	4-11	11/30	1966/12	9	1	1975/07	8.67	1	1976/07	9.67	1	1979/07	12.67	HE65b			HE65a
1960	12	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	HE65a			
1961	1-6	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	HE65a			
1961	7-12	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	1-6	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	7-12	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	1-6	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	7-12	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	1-6	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	7-12	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				
1965	1-6	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				

Notes: This table lists for students of birth years 1935 through 1965 and residence in Hesse for each school track, the regular duration of the schooling phase (column (8) for basic, (11) for middle, and (14) for academic) based on a comprehensive collection of state education laws (see columns (15)-(18) and compilation in Table 3.29). In addition to track choice, the regular duration of the schooling phase depends on individuals birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), the subsequent date of school entry (column (4)), changes concerning compulsory schooling (CPS) (for basic track students column (5)) and start of the school year reforms - either in line with the Düsseldorf Accord (DA) (moving start of the school year from fall to Easter) or with the Hamburg Accord (HA) (changing it from Easter back to fall). For Hesse, the number of short school years that stem from the latter policy reform are indicated in columns (6), (9), and (12).

Table 3.24: Lower Saxony

Birth		School Start		Basic: Graduation				Middle: Graduation			Academic: Graduation			Law Sources			
Year (1)	Mths (2)	Cut-Off (3)	Year/Mth (4)	CPS (5)	SSY (6)	Year/Mth (7)	SchYrs (8)	SSY (9)	Year/Mth (10)	SchYrs (11)	SSY (12)	Year/Mth (13)	SchYrs (14)	Cut-Off (15)	CPS (16)	DA (17)	HA (18)
1935	1-12	12/31	1941/08	8	-	1950/03	8.67	-	1951/03	9.67	-	1954/03	12.67			NI48	
1936	1-12	12/31	1942/08	8	-	1951/03	8.67	-	1952/03	9.67	-	1955/03	12.67			NI48	
1937	1-12	12/31	1943/08	8	-	1952/03	8.67	-	1953/03	9.67	-	1956/03	12.67			NI48	
1938	1-12	12/31	1944/08	8	-	1953/03	8.67	-	1954/03	9.67	-	1957/03	12.67			NI48	
1939	1-12	12/31	1945/08	8	-	1954/03	8.67	-	1955/03	9.67	-	1958/03	12.67			NI53	
1940	1-3	03/31	1946/04	8	-	1954/03	8	-	1956/03	10	-	1959/03	13	NI45			
1940	4-12	03/31	1947/04	8	-	1955/03	8	-	1957/03	10	-	1960/03	13				
1941	1-3	03/31	1947/04	8	-	1955/03	8	-	1957/03	10	-	1960/03	13				
1941	4-12	06/30	1948/04	8	-	1956/03	8	-	1958/03	10	-	1961/03	13	NI48			
1942	1-6	06/30	1948/04	8	-	1956/03	8	-	1958/03	10	-	1961/03	13	NI48			
1942	7-12	06/30	1949/04	8	-	1957/03	8	-	1959/03	10	-	1962/03	13				
1943	1-6	06/30	1949/04	8	-	1957/03	8	-	1959/03	10	-	1962/03	13				
1943	7-12	06/30	1950/04	8	-	1958/03	8	-	1960/03	10	-	1963/03	13				
1944	1-6	06/30	1950/04	8	-	1958/03	8	-	1960/03	10	-	1963/03	13				
1944	7-12	06/30	1951/04	8	-	1959/03	8	-	1961/03	10	-	1964/03	13				
1945	1-6	06/30	1951/04	8	-	1959/03	8	-	1961/03	10	-	1964/03	13				
1945	7-12	06/30	1952/04	8	0	1960/03	8	0	1962/03	10	0	1965/03	13				
1946	1-6	06/30	1952/04	8	0	1960/03	8	0	1962/03	10	0	1965/03	13				
1946	7-12	06/30	1953/04	8	0	1961/03	8	0	1963/03	10	0	1966/03	13				
1947	1-6	06/30	1953/04	8	0	1961/03	8	0	1963/03	10	0	1966/03	13				
1947	7-12	06/30	1954/04	9	0	1963/03	9	0	1964/03	10	1	1966/11	12.67		NI54		NI66
1948	1-6	06/30	1954/04	9	0	1963/03	9	0	1964/03	10	1	1966/11	12.67		NI54		NI66
1948	7-12	03/31	1955/04	9	0	1964/03	9	0	1965/03	10	2	1967/07	12.33	NI54			NI66
1949	1-3	03/31	1955/04	9	0	1964/03	9	0	1965/03	10	2	1967/07	12.33	NI54			NI66
1949	4-12	03/31	1956/04	9	0	1965/03	9	0	1966/03	10	2	1968/07	12.33				NI66
1950	1-3	03/31	1956/04	9	0	1965/03	9	0	1966/03	10	2	1968/07	12.33				NI66
1950	4-12	03/31	1957/04	9	0	1966/03	9	1	1966/11	9.67	2	1969/07	12.33				NI66
1951	1-3	03/31	1957/04	9	0	1966/03	9	1	1966/11	9.67	2	1969/07	12.33				NI66
1951	4-12	03/31	1958/04	9	0	1967/03	9	2	1967/07	9.33	2	1970/07	12.33				NI66
1952	1-3	03/31	1958/04	9	0	1967/03	9	2	1967/07	9.33	2	1970/07	12.33				NI66
1952	4-12	03/31	1959/04	9	0	1968/03	9	2	1968/07	9.33	2	1971/07	12.33				NI66
1953	1-3	03/31	1959/04	9	0	1968/03	9	2	1968/07	9.33	2	1971/07	12.33				NI66
1953	4-12	03/31	1960/04	9	0	1969/03	9	2	1969/07	9.33	2	1972/07	12.33				NI66
1954	1-3	03/31	1960/04	9	0	1969/03	9	2	1969/07	9.33	2	1972/07	12.33				NI66
1954	4-12	03/31	1961/04	9	0	1970/03	9	0	1971/03	10	2	1973/07	12.33				NI66
1955	1-3	03/31	1961/04	9	0	1970/03	9	0	1971/03	10	2	1973/07	12.33				NI66
1955	4-12	03/31	1962/04	9	0	1971/03	9	0	1972/03	10	2	1974/07	12.33				NI66
1956	1-3	03/31	1962/04	9	0	1971/03	9	0	1972/03	10	2	1974/07	12.33				NI66
1956	4-12	03/31	1963/04	9	0	1972/03	9	0	1973/03	10	2	1975/07	12.33				NI66
1957	1-3	03/31	1963/04	9	0	1972/03	9	0	1973/03	10	2	1975/07	12.33				NI66
1957	4-12	03/31	1964/04	9	0	1973/03	9	0	1974/03	10	2	1976/07	12.33				NI66
1958	1-3	03/31	1964/04	9	0	1973/03	9	0	1974/03	10	2	1976/07	12.33				NI66
1958	4-12	03/31	1965/04	9	0	1974/03	9	0	1975/03	10	2	1977/07	12.33				NI66
1959	1-3	03/31	1965/04	9	0	1974/03	9	0	1975/03	10	2	1977/07	12.33				NI66
1959	4-12	03/31	1966/04	9	0	1975/03	9	0	1976/03	10	2	1978/07	12.33				NI66
1960	1-3	03/31	1966/04	9	0	1975/03	9	0	1976/03	10	2	1978/07	12.33				NI66
1960	4-12	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	NI66			
1961	1-6	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	NI66			
1961	7-12	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	1-6	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	7-12	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	1-6	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	7-12	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	1-6	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	7-12	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				
1965	1-6	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				

Notes: This table lists for students of birth years 1935 through 1965 and residence in Lower Saxony for each school track, the regular duration of the schooling phase (column (8) for basic, (11) for middle, and (14) for academic) based on a comprehensive collection of state education laws (see columns (15)-(18) and compilation in Table 3.29). In addition to track choice, the regular duration of the schooling phase depends on individuals birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), the subsequent date of school entry (column (4)), changes concerning compulsory schooling (CPS) (for basic track students column (5)) and start of the school year reforms - either in line with the Düsseldorf Accord (DA) (moving start of the school year from fall to Easter) or with the Hamburg Accord (HA) (changing it from Easter back to fall). For Lower Saxony, the number of short school years that stem from the latter policy reform are indicated in columns (6), (9), and (12) (see Section 3.2 for more details on the divergent pattern of short school years compared to other treatment states).

Table 3.25: North Rhine-Westphalia

Birth		School Start		Basic: Graduation				Middle: Graduation			Academic: Graduation			Law Sources			
Year (1)	Mths (2)	Cut-Off (3)	Year/Mth (4)	CPS (5)	SSY (6)	Year/Mth (7)	SchYrs (8)	SSY (9)	Year/Mth (10)	SchYrs (11)	SSY (12)	Year/Mth (13)	SchYrs (14)	Cut-Off (15)	CPS (16)	DA (17)	HA (18)
1935	1-12	12/31	1941/08	8	-	1950/03	8.67	-	1951/03	9.67	-	1954/03	12.67			NW49	
1936	1-12	12/31	1942/08	8	-	1951/03	8.67	-	1952/03	9.67	-	1955/03	12.67			NW49	
1937	1-12	12/31	1943/08	8	-	1952/03	8.67	-	1953/03	9.67	-	1956/03	12.67			NW49	
1938	1-12	12/31	1944/08	8	-	1953/03	8.67	-	1954/03	9.67	-	1957/03	12.67			NW49	
1939	1-12	12/31	1945/08	8	-	1954/03	8.67	-	1955/03	9.67	-	1958/03	12.67			NW53	
1940	1-12	12/31	1946/04	8	-	1954/03	8	-	1956/03	10	-	1959/03	13				
1941	1-12	12/31	1947/04	8	-	1955/03	8	-	1957/03	10	-	1960/03	13				
1942	1-12	12/31	1948/04	8	-	1956/03	8	-	1958/03	10	-	1961/03	13				
1943	1-12	12/31	1949/04	8	-	1957/03	8	-	1959/03	10	-	1962/03	13				
1944	1-6	06/30	1950/04	8	-	1958/03	8	-	1960/03	10	-	1963/03	13	NW49			
1944	7-12	06/30	1951/04	8	-	1959/03	8	-	1961/03	10	-	1964/03	13				
1945	1-6	06/30	1951/04	8	-	1959/03	8	-	1961/03	10	-	1964/03	13				
1945	7-12	06/30	1952/04	8	0	1960/03	8	0	1962/03	10	0	1965/03	13				
1946	1-6	06/30	1952/04	8	0	1960/03	8	0	1962/03	10	0	1965/03	13				
1946	7-12	06/30	1953/04	8	0	1961/03	8	0	1963/03	10	0	1966/03	13				
1947	1-6	06/30	1953/04	8	0	1961/03	8	0	1963/03	10	0	1966/03	13				
1947	7-12	06/30	1954/04	8	0	1962/03	8	0	1964/03	10	1	1966/11	12.67				NW66
1948	1-6	06/30	1954/04	8	0	1962/03	8	0	1964/03	10	1	1966/11	12.67				NW66
1948	7-12	06/30	1955/04	8	0	1963/03	8	0	1965/03	10	2	1967/07	12.33				NW66
1949	1-6	06/30	1955/04	8	0	1963/03	8	0	1965/03	10	2	1967/07	12.33				NW66
1949	7-12	06/30	1956/04	8	0	1964/03	8	0	1966/03	10	2	1968/07	12.33				NW66
1950	1-6	06/30	1956/04	8	0	1964/03	8	0	1966/03	10	2	1968/07	12.33				NW66
1950	7-12	06/30	1957/04	8	0	1965/03	8	1	1966/11	9.67	2	1969/07	12.33				NW66
1951	1-6	06/30	1957/04	8	0	1965/03	8	1	1966/11	9.67	2	1969/07	12.33				NW66
1951	7-12	06/30	1958/04	8	0	1966/03	8	2	1967/07	9.33	2	1970/07	12.33				NW66
1952	1-6	06/30	1958/04	8	0	1966/03	8	2	1967/07	9.33	2	1970/07	12.33				NW66
1952	7-12	06/30	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33		NW66		NW66
1953	1-6	06/30	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33		NW66		NW66
1953	7-12	06/30	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				NW66
1954	1-6	06/30	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				NW66
1954	7-12	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33	NW60			NW66
1955	1-3	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33	NW60			NW66
1955	4-12	03/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33				NW66
1956	1-3	03/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33				NW66
1956	4-12	03/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				NW66
1957	1-3	03/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				NW66
1957	4-12	03/31	1964/04	9	2	1972/07	8.33	2	1973/07	9.33	2	1976/07	12.33				NW66
1958	1-3	03/31	1964/04	9	2	1972/07	8.33	2	1973/07	9.33	2	1976/07	12.33				NW66
1958	4-12	03/31	1965/04	9	2	1973/07	8.33	2	1974/07	9.33	2	1977/07	12.33				NW66
1959	1-3	03/31	1965/04	9	2	1973/07	8.33	2	1974/07	9.33	2	1977/07	12.33				NW66
1959	4-12	03/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33				NW66
1960	1-3	03/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33				NW66
1960	4-11	11/30	1966/12	9	1	1975/07	8.67	1	1976/07	9.67	1	1979/07	12.67	NW66			NW66
1960	12	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	NW66			
1961	1-6	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	NW66			
1961	7-12	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	1-6	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	7-12	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	1-6	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	7-12	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	1-6	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	7-12	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				
1965	1-6	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				

Notes: This table lists for students of birth years 1935 through 1965 and residence in North Rhine-Westphalia for each school track, the regular duration of the schooling phase (column (8) for basic, (11) for middle, and (14) for academic) based on a comprehensive collection of state education laws (see columns (15)-(18) and compilation in Table 3.29). In addition to track choice, the regular duration of the schooling phase depends on individuals birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), the subsequent date of school entry (column (4)), changes concerning compulsory schooling (CPS) (for basic track students column (5)) and start of the school year reforms - either in line with the Düsseldorf Accord (DA) (moving start of the school year from fall to Easter) or with the Hamburg Accord (HA) (changing it from Easter back to fall). For North Rhine-Westphalia, the number of short school years that stem from the latter policy reform are indicated in columns (6), (9), and (12).

Table 3.26: Rhineland-Palatine

Birth		School Start		Basic: Graduation				Middle: Graduation			Academic: Graduation			Law Sources			
Year (1)	Mths (2)	Cut-Off (3)	Year/Mth (4)	CPS (5)	SSY (6)	Year/Mth (7)	SchYrs (8)	SSY (9)	Year/Mth (10)	SchYrs (11)	SSY (12)	Year/Mth (13)	SchYrs (14)	Cut-Off (15)	CPS (16)	DA (17)	HA (18)
1935	1-12	12/31	1941/08	8	-	1949/07	8	-	1951/03	9.67	-	1954/03	12.67			RP52	
1936	1-12	12/31	1942/08	8	-	1950/07	8	-	1952/03	9.67	-	1955/03	12.67			RP52	
1937	1-12	12/31	1943/08	8	-	1951/07	8	-	1953/03	9.67	-	1956/03	12.67			RP52	
1938	1-12	12/31	1944/08	8	-	1952/07	8	-	1954/03	9.67	-	1957/03	12.67			RP52	
1939	1-12	12/31	1945/08	8	-	1953/07	8	-	1955/03	9.67	-	1958/03	12.67			RP52	
1940	1-12	12/31	1946/08	8	-	1954/07	8	-	1956/03	9.67	-	1959/03	12.67			RP52	
1941	1-12	12/31	1947/08	8	-	1955/07	8	-	1957/03	9.67	-	1960/03	12.67			RP52	
1942	1-12	12/31	1948/08	8	-	1956/07	8	-	1958/03	9.67	-	1961/03	12.67				
1943	1-12	03/31	1950/04	8	-	1958/03	8	-	1960/03	10	-	1963/03	13	RP49			
1944	1-3	03/31	1950/04	8	-	1958/03	8	-	1960/03	10	-	1963/03	13	RP49			
1944	4-12	03/31	1951/04	8	-	1959/03	8	-	1961/03	10	-	1964/03	13				
1945	1-3	03/31	1951/04	8	-	1959/03	8	-	1961/03	10	-	1964/03	13				
1945	4-12	03/31	1952/04	8	0	1960/03	8	0	1962/03	10	0	1965/03	13				
1946	1-3	03/31	1952/04	8	0	1960/03	8	0	1962/03	10	0	1965/03	13				
1946	4-12	03/31	1953/04	8	0	1961/03	8	0	1963/03	10	0	1966/03	13				
1947	1-3	03/31	1953/04	8	0	1961/03	8	0	1963/03	10	0	1966/03	13				
1947	4-12	03/31	1954/04	8	0	1962/03	8	0	1964/03	10	1	1966/11	12.67				RP66
1948	1-3	03/31	1954/04	8	0	1962/03	8	0	1964/03	10	1	1966/11	12.67				RP66
1948	4-12	03/31	1955/04	8	0	1963/03	8	0	1965/03	10	2	1967/07	12.33				RP66
1949	1-3	03/31	1955/04	8	0	1963/03	8	0	1965/03	10	2	1967/07	12.33				RP66
1949	4-12	03/31	1956/04	8	0	1964/03	8	0	1966/03	10	2	1968/07	12.33				RP66
1950	1-3	03/31	1956/04	8	0	1964/03	8	0	1966/03	10	2	1968/07	12.33				RP66
1950	4-12	03/31	1957/04	8	0	1965/03	8	1	1966/11	9.67	2	1969/07	12.33				RP66
1951	1-3	03/31	1957/04	8	0	1965/03	8	1	1966/11	9.67	2	1969/07	12.33				RP66
1951	4-12	03/31	1958/04	8	0	1966/03	8	2	1967/07	9.33	2	1970/07	12.33				RP66
1952	1-3	03/31	1958/04	8	0	1966/03	8	2	1967/07	9.33	2	1970/07	12.33				RP66
1952	4-12	03/31	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33		RP66		RP66
1953	1-3	03/31	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33		RP66		RP66
1953	4-12	03/31	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				RP66
1954	1-3	03/31	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				RP66
1954	4-12	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33				RP66
1955	1-3	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33				RP66
1955	4-12	03/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33				RP66
1956	1-3	03/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33				RP66
1956	4-12	03/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				RP66
1957	1-3	03/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				RP66
1957	4-12	03/31	1964/04	9	2	1972/07	8.33	2	1973/07	9.33	2	1976/07	12.33				RP66
1958	1-3	03/31	1964/04	9	2	1972/07	8.33	2	1973/07	9.33	2	1976/07	12.33				RP66
1958	4-12	03/31	1965/04	9	2	1973/07	8.33	2	1974/07	9.33	2	1977/07	12.33				RP66
1959	1-3	03/31	1965/04	9	2	1973/07	8.33	2	1974/07	9.33	2	1977/07	12.33				RP66
1959	4-12	03/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33				RP66
1960	1-3	03/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33				RP66
1960	4-11	11/30	1966/12	9	1	1975/07	8.67	1	1976/07	9.67	1	1979/07	12.67	RP66			RP66
1960	12	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	RP66			
1961	1-6	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	RP66			
1961	7-12	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	1-6	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	7-12	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	1-6	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	7-12	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	1-6	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	7-12	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				
1965	1-6	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				

Notes: This table lists for students of birth years 1935 through 1965 and residence in Rhineland-Palatine for each school track, the regular duration of the schooling phase (column (8) for basic, (11) for middle, and (14) for academic) based on a comprehensive collection of state education laws (see columns (15)-(18) and compilation in Table 3.29). In addition to track choice, the regular duration of the schooling phase depends on individuals birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), the subsequent date of school entry (column (4)), changes concerning compulsory schooling (CPS) (for basic track students column (5)) and start of the school year reforms - either in line with the Düsseldorf Accord (DA) (moving start of the school year from fall to Easter) or with the Hamburg Accord (HA) (changing it from Easter back to fall). For Rhineland-Palatine, the number of short school years that stem from the latter policy reform are indicated in columns (6), (9), and (12).

Table 3.27: Saarland

Birth		School Start		Basic: Graduation				Middle: Graduation			Academic: Graduation			Law Sources			
Year (1)	Mths (2)	Cut-Off (3)	Year/Mth (4)	CPS (5)	SSY (6)	Year/Mth (7)	SchYrs (8)	SSY (9)	Year/Mth (10)	SchYrs (11)	SSY (12)	Year/Mth (13)	SchYrs (14)	Cut-Off (15)	CPS (16)	DA (17)	HA (18)
1935	1-12	12/31	1941/08	8	-	1949/07	8	-	1951/07	10	-	1954/07	13				
1936	1-12	12/31	1942/08	8	-	1950/07	8	-	1952/07	10	-	1955/07	13				
1937	1-12	12/31	1943/08	8	-	1951/07	8	-	1953/07	10	-	1956/07	13				
1938	1-12	12/31	1944/08	8	-	1952/07	8	-	1954/07	10	-	1957/03	12.67			SL56	
1939	1-12	12/31	1945/08	8	-	1953/07	8	-	1955/07	10	-	1958/03	12.67			SL56	
1940	1-12	12/31	1946/08	8	-	1954/07	8	-	1956/07	10	-	1959/03	12.67			SL56	
1941	1-12	12/31	1947/08	8	-	1955/07	8	-	1957/03	9.67	-	1960/03	12.67			SL56	
1942	1-12	12/31	1948/08	8	-	1956/07	8	-	1958/03	9.67	-	1961/03	12.67			SL56	
1943	1-12	12/31	1949/08	8	-	1957/03	7.67	-	1959/03	9.67	-	1962/03	12.67			SL56	
1944	1-12	12/31	1950/08	9	-	1959/03	8.67	-	1960/03	9.67	-	1963/03	12.67		SL58	SL58	
1945	1-12	12/31	1951/08	9	-	1960/03	8.67	-	1961/03	9.67	-	1964/03	12.67			SL58	
1946	1-12	12/31	1952/08	9	-	1961/03	8.67	-	1962/03	9.67	-	1965/03	12.67			SL58	
1947	1-12	12/31	1953/08	9	x	1962/03	8.67	x	1963/03	9.67	x	1966/03	12.67			SL58	
1948	1-9	09/30	1954/08	9	x	1963/03	8.67	x	1964/03	9.67	x	1966/11	12.33	SL54		SL58	
1948	10-12	09/30	1955/08	9	x	1964/03	8.67	x	1965/03	9.67	x	1967/07	12			SL58	
1949	1-9	09/30	1955/08	9	x	1964/03	8.67	x	1965/03	9.67	x	1967/07	12			SL58	
1949	10-12	09/30	1956/08	9	x	1965/03	8.67	x	1966/03	9.67	x	1968/07	12			SL58	
1950	1-9	09/30	1956/08	9	x	1965/03	8.67	x	1966/03	9.67	x	1968/07	12			SL58	
1950	10-12	09/30	1957/04	9	0	1966/03	9	1	1966/11	9.67	2	1969/07	12.33				SL66
1951	1-9	09/30	1957/04	9	0	1966/03	9	1	1966/11	9.67	2	1969/07	12.33				SL66
1951	10-12	12/31	1959/04	9	2	1967/07	8.33	2	1967/07	9.33	2	1970/07	12.33	SL58			SL66
1952	1-12	12/31	1959/04	9	2	1967/07	8.33	2	1967/07	9.33	2	1970/07	12.33	SL58			SL66
1953	1-3	03/31	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33	SL59			SL66
1953	4-12	03/31	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				SL66
1954	1-3	03/31	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				SL66
1954	4-12	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33				SL66
1955	1-3	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33				SL66
1955	4-12	03/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33				SL66
1956	1-3	03/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33				SL66
1956	4-12	03/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				SL66
1957	1-3	03/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				SL66
1957	4-12	03/31	1964/04	9	2	1972/07	8.33	2	1973/07	9.33	2	1976/07	12.33				SL66
1958	1-3	03/31	1964/04	9	2	1972/07	8.33	2	1973/07	9.33	2	1976/07	12.33				SL66
1958	4-12	03/31	1965/04	9	2	1973/07	8.33	2	1974/07	9.33	2	1977/07	12.33				SL66
1959	1-3	03/31	1965/04	9	2	1973/07	8.33	2	1974/07	9.33	2	1977/07	12.33				SL66
1959	4-12	03/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33				SL66
1960	1-3	03/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33				SL66
1960	4-12	12/31	1966/12	9	1	1975/07	8.67	1	1976/07	9.67	1	1979/07	12.67	SL66			SL66
1961	1-9	09/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	SL66			
1961	10-12	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	1-6	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	7-12	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	1-6	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	7-12	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	1-6	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	7-12	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				
1965	1-6	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				

Notes: This table lists for students of birth years 1935 through 1965 and residence in Saarland for each school track, the regular duration of the schooling phase (column (8) for basic, (11) for middle, and (14) for academic) based on a comprehensive collection of state education laws (see columns (15)-(18) and compilation in Table 3.29). In addition to track choice, the regular duration of the schooling phase depends on individuals birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), the subsequent date of school entry (column (4)), changes concerning compulsory schooling (CPS) (for basic track students column (5)) and start of the school year reforms - either in line with the Düsseldorf Accord (DA) (moving start of the school year from fall to Easter) or with the Hamburg Accord (HA) (changing it from Easter back to fall). For Saarland, the number of short school years that stem from the latter policy reform are indicated in columns (6), (9), and (12). Saarland joined the Federal Republic of Germany in 1957 and was the last West-German state to implement the move of the school year to Easter in line with the DA. Consequently, there is an overlap between the DA and the subsequent HA reform, resulting in divergent duration of the schooling phase for school starting years up until 1957. Affected years are marked with an x in columns (6), (9), and (12).

Table 3.28: Schleswig-Holstein

Birth		School Start		Basic: Graduation				Middle: Graduation			Academic: Graduation			Law Sources			
Year (1)	Mths (2)	Cut-Off (3)	Year/Mth (4)	CPS (5)	SSY (6)	Year/Mth (7)	SchYrs (8)	SSY (9)	Year/Mth (10)	SchYrs (11)	SSY (12)	Year/Mth (13)	SchYrs (14)	Cut-Off (15)	CPS (16)	DA (17)	HA (18)
1935	1-12	12/31	1941/08	9	-	1950/03	8.67	-	1951/03	9.67	-	1954/03	12.67		SH47	SH46	
1936	1-12	12/31	1942/08	9	-	1951/03	8.67	-	1952/03	9.67	-	1955/03	12.67			SH46	
1937	1-12	12/31	1943/08	9	-	1952/03	8.67	-	1953/03	9.67	-	1956/03	12.67			SH46	
1938	1-12	12/31	1944/08	9	-	1953/03	8.67	-	1954/03	9.67	-	1957/03	12.67			SH46	
1939	1-12	12/31	1945/08	9	-	1954/03	8.67	-	1955/03	9.67	-	1958/03	12.67			SH46	
1940	1-3	03/31	1946/04	9	-	1955/03	9	-	1956/03	10	-	1959/03	13	SH55			
1940	4-12	03/31	1947/04	9	-	1956/03	9	-	1957/03	10	-	1960/03	13				
1941	1-3	03/31	1947/04	9	-	1956/03	9	-	1957/03	10	-	1960/03	13				
1941	4-12	03/31	1948/04	9	-	1957/03	9	-	1958/03	10	-	1961/03	13				
1942	1-3	03/31	1948/04	9	-	1957/03	9	-	1958/03	10	-	1961/03	13				
1942	4-12	03/31	1949/04	9	-	1958/03	9	-	1959/03	10	-	1962/03	13				
1943	1-3	03/31	1949/04	9	-	1958/03	9	-	1959/03	10	-	1962/03	13				
1943	4-12	03/31	1950/04	9	-	1959/03	9	-	1960/03	10	-	1963/03	13				
1944	1-3	03/31	1950/04	9	-	1959/03	9	-	1960/03	10	-	1963/03	13				
1944	4-12	03/31	1951/04	9	-	1960/03	9	-	1961/03	10	-	1964/03	13				
1945	1-3	03/31	1951/04	9	-	1960/03	9	-	1961/03	10	-	1964/03	13				
1945	4-12	03/31	1952/04	9	0	1961/03	9	0	1962/03	10	0	1965/03	13				
1946	1-3	03/31	1952/04	9	0	1961/03	9	0	1962/03	10	0	1965/03	13				
1946	4-12	03/31	1953/04	9	0	1962/03	9	0	1963/03	10	0	1966/03	13				
1947	1-3	03/31	1953/04	9	0	1962/03	9	0	1963/03	10	0	1966/03	13				
1947	4-12	03/31	1954/04	9	0	1963/03	9	0	1964/03	10	1	1966/11	12.67				SH66
1948	1-3	03/31	1954/04	9	0	1963/03	9	0	1964/03	10	1	1966/11	12.67				SH66
1948	4-12	03/31	1955/04	9	0	1964/03	9	0	1965/03	10	2	1967/07	12.33				SH66
1949	1-3	03/31	1955/04	9	0	1964/03	9	0	1965/03	10	2	1967/07	12.33				SH66
1949	4-12	03/31	1956/04	9	0	1965/03	9	0	1966/03	10	2	1968/07	12.33				SH66
1950	1-3	03/31	1956/04	9	0	1965/03	9	0	1966/03	10	2	1968/07	12.33				SH66
1950	4-12	03/31	1957/04	9	0	1966/03	9	1	1966/11	9.67	2	1969/07	12.33				SH66
1951	1-3	03/31	1957/04	9	0	1966/03	9	1	1966/11	9.67	2	1969/07	12.33				SH66
1951	4-12	03/31	1958/04	9	1	1966/11	8.67	2	1967/07	9.33	2	1970/07	12.33				SH66
1952	1-3	03/31	1958/04	9	1	1966/11	8.67	2	1967/07	9.33	2	1970/07	12.33				SH66
1952	4-12	03/31	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33				SH66
1953	1-3	03/31	1959/04	9	2	1967/07	8.33	2	1968/07	9.33	2	1971/07	12.33				SH66
1953	4-12	03/31	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				SH66
1954	1-3	03/31	1960/04	9	2	1968/07	8.33	2	1969/07	9.33	2	1972/07	12.33				SH66
1954	4-12	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33				SH66
1955	1-3	03/31	1961/04	9	2	1969/07	8.33	2	1970/07	9.33	2	1973/07	12.33				SH66
1955	4-12	03/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33				SH66
1956	1-3	03/31	1962/04	9	2	1970/07	8.33	2	1971/07	9.33	2	1974/07	12.33				SH66
1956	4-12	03/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				SH66
1957	1-3	03/31	1963/04	9	2	1971/07	8.33	2	1972/07	9.33	2	1975/07	12.33				SH66
1957	4-12	12/31	1964/04	9	2	1972/07	8.33	2	1973/07	9.33	2	1976/07	12.33	SH63			SH66
1958	1-12	12/31	1965/04	9	2	1973/07	8.33	2	1974/07	9.33	2	1977/07	12.33				SH66
1959	1-12	12/31	1966/04	9	2	1974/07	8.33	2	1975/07	9.33	2	1978/07	12.33				SH66
1960	1-11	11/30	1966/12	9	1	1975/07	8.67	1	1976/07	9.67	1	1979/07	12.67	SH66			SH66
1960	12	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	SH66			
1961	1-6	06/30	1967/08	9	0	1976/07	9	0	1977/07	10	0	1980/07	13	SH66			
1961	7-12	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	1-6	06/30	1968/08	9	0	1977/07	9	0	1978/07	10	0	1981/07	13				
1962	7-12	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	1-6	06/30	1969/08	9	0	1978/07	9	0	1979/07	10	0	1982/07	13				
1963	7-12	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	1-6	06/30	1970/08	9	0	1979/07	9	0	1980/07	10	0	1983/07	13				
1964	7-12	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				
1965	1-6	06/30	1971/08	9	-	1980/07	9	-	1981/07	10	-	1984/07	13				

Notes: This table lists for students of birth years 1935 through 1965 and residence in Schleswig-Holstein for each school track, the regular duration of the schooling phase (column (8) for basic, (11) for middle, and (14) for academic) based on a comprehensive collection of state education laws (see columns (15)-(18) and compilation in Table 3.29). In addition to track choice, the regular duration of the schooling phase depends on individuals birth year and month (columns (1) and (2)), the legal age cut-off date for school entry (column (3)), the subsequent date of school entry (column (4)), changes concerning compulsory schooling (CPS) (for basic track students column (5)) and start of the school year reforms - either in line with the Düsseldorf Accord (DA) (moving start of the school year from fall to Easter) or with the Hamburg Accord (HA) (changing it from Easter back to fall). For Schleswig-Holstein, the number of short school years that stem from the latter policy reform are indicated in columns (6), (9), and (12).

Table 3.29: Law Sources Overview

Abbr.	Type	Title	Date	Source	Link
Berlin					
BE48	Erstfassung	Schulgesetz für Groß-Berlin	26.06.1948	VOBl. I 1948, 358	Link
BE51	Erstfassung	Dritte Durchführungsverordnung zum Schulgesetz für Berlin	13.12.1951	GVBl. 1951, 1147	Link
BE55	Änderung	Drittes Gesetz zur Änderung des Schulgesetzes für Berlin	09.08.1955	GVBl. 1955, 723	Link
BE66	Neufassung	Bekanntmachung der Neufassung des Schulgesetzes für Berlin	13.09.1966	GVBl. 1966, 1485	Link
Baden-Württemberg					
BW52	Erstfassung	Das badische Landesgesetz über Schuljahranfang und Beginn der Schulpflicht vom 12.2.1952	12.02.1952	GVBl. 1952, 25	
BW53	Neufassung	Gesetz über Schuljahr und Schulpflicht	09.03.1953	GBl. 1953, 17	Link
BW57	Änderung	Gesetz zur Änderung des Gesetzes über Schuljahr und Schulpflicht	09.12.1957	GBl. 1957, 147	Link
BW64	Erstfassung	Gesetz zur Vereinheitlichung und Ordnung des Schulwesens (SchVOG)	05.05.1964	GBl. 1964, 235	
BW66a	Änderung	Gesetz zur Änderung des Gesetzes zur Vereinheitlichung und Ordnung des Schulwesens	29.03.1964	GBl. 1966, 47	Link
BW66b	Bekanntmachung	Brief des Kultusministers Prof. Dr. Wilhelm Hahn an die Eltern	07.02.1966	Landesarchiv BW	
Bavaria					
BY52	Erstfassung	Schulpflichtgesetz	15.01.1952	GVBl. 1952, 11	
BY69	Änderung	Änderung des Gesetzes über die Schulpflicht (Schulpflichtgesetz)	15.04.1969	GVBl. 1969, 97	
Bremen					
HB46	Bekanntmachung	Anmeldung für die Grundschule für das Schuljahr 1946/47	16.02.1946	Weser Kurier	
HB48	Bekanntmachung	Versorgung der Ostern 1948 in das 1. Schuljahr der Volksschule eintretenden Kinder	04.03.1948	Weser Kurier	
HB57	Neufassung	Gesetz über das Schulwesen der Freien Hansestadt Bremen	25.05.1957	GBl. 1957, 57	Link
HB65	Änderung	Gesetz über die Erweiterung der Schulpflicht für das Schuljahr 1966	09.11.1965	GBl. 1965, 137	Link
HB67	Neufassung	Bekanntmachung der Neufassung des Gesetzes über das Schulwesen der Freien Hansestadt Bremen	01.06.1967	GBl. 1967, 65	Link
Hamburg					
HH41	Bekanntmachung	Schreiben des Schulrats an die Leitungen der Mittel-, Volks-, Sonder- und Hilfsschulen: Beginn des Schuljahres und der Schulpflicht	26.03.1941	Staatsarchiv HH, 361-2 VI, 356-10	
HH45	Bekanntmachung	Schreiben des Schulrats an die Leitungen der Volks- und Mittelschulen: Verlängerung des Schuljahres 1945	02.11.1945	Staatsarchiv HH, 361-2 VI, 356-22	
HH49a	Bekanntmachung	Schreiben des Senators an die Leitungen sämtlicher Schulen: Schulentlassung 1950	27.06.1949	Staatsarchiv HH, 361-2 VI, 356-27	
HH49b	Erstfassung	Gesetz über das Schulwesen der Hansestadt Hamburg	25.10.1949	GVBl. 1949, 257	Link

Continued on next page

Table 3.29: Law Sources Overview (continued)

Abbr.	Type	Title	Date	Source	Link
HH61	Änderung	Zweites Gesetz zur Änderung des Gesetzes über das Schulwesen der Freien und Hansestadt Hamburg	03.10.1961	GVBl. 1961, 316	Link
HH66	Erstfassung	Schulgesetz der Freien und Hansestadt Hamburg	09.12.1966	GVBl. 1966, 257	Link
Hesse					
HE48	Amtsblatt	Erlass vom 18.08.1948	01.09.1948	ABl. 5 1948, 121	
HE50	Erstfassung	Gesetz über die Schulpflicht im Lande Hessen (Schulpflichtgesetz)	27.05.1950	GVBl. 1950, 68	Link
HE56	Änderung	Drittes Gesetz zur Änderung des Schulpflichtgesetzes	07.12.1956	GVBl. 1956, 163	Link
HE61	Neufassung	Hessisches Schulpflichtgesetz	17.05.1961	GVBl. 1961, 69	Link
HE65a	Neufassung	Neufassung des Hessischen Schulpflichtgesetzes	01.12.1965	GVBl. 1965, 323	Link
HE65b	Verordnung	Zweite Verordnung zur Überleitung des Schuljahresbeginns	20.12.1965	GVBl. 1965, 356	
Lower Saxony					
NI45	Anordnung	Schreiben des Oberpräsident der Provinz Hannover	27.12.1945	Nds 120 Lüneburg Acc. 165, 86 Nr. 217	
NI48	Änderung	Gesetz zur Änderung der Schulpflicht in Niedersachsen	21.12.1948	GVBl. 1948, 184	
NI53	Erstfassung	Gesetz zur Bestimmung der Schulpflicht für die Geburtenjahrgänge 1939 bis 1943	16.01.1953	GVBl. 1953, 5	Link
NI54	Erstfassung	Gesetz über das öffentliche Schulwesen in Niedersachsen	14.09.1954	GVBl. 1954, 89	Link
NI66	Neufassung	Bekanntmachung der Neufassung des Gesetzes über das öffentliche Schulwesen in Niedersachsen	27.06.1966	GVBl. 1966, 127	Link
North-Rhine Westphalia					
NW49	Neufassung	Gesetz zur Änderung des Gesetzes über die Schulpflicht im Deutschen Reich (Reichsschulpflichtgesetz)	27.07.1949	GVBl. 1949, 244	
NW53	Änderung	Gesetz zur Änderung des Gesetzes über die Schulpflicht im Deutschen Reich (Reichsschulpflichtgesetz) vom 6. Juli 1938 in der Fassung vom 27. Juli 1949	10.02.1953	GVBl. 1953, 166	Link
NW60	Änderung	Gesetz zur Änderung des Gesetzes über die Schulpflicht im Deutschen Reich (Reichsschulpflichtgesetz)	29.06.1960	GVBl. 1960, 198	Link
NW66	Neuerlass	Gesetz über die Schulpflicht im Lande Nordrhein-Westfalen (Schulpflichtgesetz - SchpflG)	14.06.1966	GVBl. 1966, 365	Link
Reichsgesetzgebung					
R38	Erstfassung	Gesetz über die Schulpflicht im Deutschen Reich	06.07.1938	RGBl. I 1938, 799	Link
R41	Verordnung	Zweite Verordnung zur Durchführung des Reichsschulpflichtgesetzes	16.05.1941	RGBl. I 1941, 283	Link
Rhineland-Palatine					

Continued on next page

Table 3.29: Law Sources Overview (continued)

Abbr.	Type	Title	Date	Source	Link
RP49	Erstfassung	Nr. 204 Aufnahme von Schulneulingen für die Volksschulen	01.07.1949	Runderlass Ministerium für Unterricht und Kultus II, E 2 Tgb., Nr. 851-49	
RP52	Änderung	Landesgesetz zur Änderung des Reichschulpflichtgesetzes	28.03.1952	GVBl. 1952, 67	Link
RP66	Neufassung	Bekanntmachung der Neufassung des Schulpflichtgesetzes	23.08.1966	GVBl. 1966, 243	Link
Saarland					
SL54	Änderung	Gesetz Nr. 422 zur Änderung des Gesetzes über die Schulpflicht im Deutschen Reich (Reichsschulpflichtgesetz) vom 6. Juli 1938 in der Fassung vom 16. Mai 1941	07.07.1954	ABl. 1954, 831	
SL56	Erlass	Erlass des Saarländischen Ministeriums für Kultus, Unterricht und Volksbildung vom 12. Juni 1956 - Beginn und Ende des Schuljahres; Verlegung auf den Ostertermin	20.06.1956	Amtliches Schulblatt 1956, Nr.8, S.21	
SL58	Änderung	Gesetz Nr. 621 zur Änderung des Gesetzes über die Schulpflicht im Deutschen Reich (Reichsschulpflichtgesetz) vom 6. Juli 1938 in der Fassung vom 07. Juli 1954	14.02.1958	ABl. 1958, 297	Link
SL59	Änderung	Gesetz Nr. 663 zur Änderung des Gesetzes über die Schulpflicht im Deutschen Reich (Reichsschulpflichtgesetz) vom 6. Juli 1938 in der Fassung vom 14.02.1958	06.02.1959	ABl. 1959, 598	Link
SL66	Erstfassung	Gesetz Nr. 826 über die Schulpflicht im Saarland (Schulpflichtgesetz)	11.03.1966	ABl. 1966, 205	Link
Schleswig-Holstein					
SH46	Amtsblatt	Erlass des Amts für Volksbildung vom 22.7. 1946	22.07.1946	ABl. 1946, 10	
SH47	Erstfassung	Gesetz betreffend die Wiedereinführung des 9. Schuljahres	11.02.1947	GVOBl. 1947, 10	Link
SH55	Erstfassung	Gesetz über die Schulpflicht	05.12.1955	GVOBl. 1955, 169	Link
SH63	Änderung	Gesetz zur Änderung des Gesetzes über die Schulpflicht	25.09.1963	GVOBl. 1963, 115	Link
SH66	Änderung	Zweites Gesetz zur Änderung des Gesetzes über die Schulpflicht	06.04.1966	GVOBl. 1966, 88	Link

Notes: The table collects all law sources used to disentangle school-entry cut-off rules, compulsory schooling (CPS), and start of the school year reforms, either in line with the Düsseldorf Accord (DA) moving start of the school year from fall to Easter or in line with the Hamburg Accord (HA) changing the start of the school year from Easter back to fall for the birth cohorts 1935-1965 in all federal states from West Germany displayed in table 3.18-table 3.28.

Chapter 4

Green Cities, Healthier Children: The Effect of Urban Green Space on the Body Weight of Primary School Starters

Abstract

The discussion on tackling childhood obesity as one of the greatest public health concerns is often centered around fostering physical activity. One potential causal relationship yet overlooked in the literature could run from providing public urban green space to reduced weight problems by providing the proper environment for physical activity and healthy lifestyles. A unique quasi-experimental setting of the transformation of former airport grounds to a large urban green space allows me to test this hypothesis by applying a difference-in-differences approach and comparing several weight outcomes based on the body mass index (BMI) of treated children living within a 1500m radius around park entrance to children living further away before and after the park was opened to the public. I use new administrative data on the Berlin district level from mandatory school entrance examinations that entail objectively measured body height and weight information. I provide robust evidence of a lower probability by 4.3 percentage points for children to be overweight (BMI $>$ 90 P.) as a result of park opening, driven entirely by girls, mainly by children from foreign cultural backgrounds and children with less childcare exposure. For the probability of being obese (BMI $>$ 97 P.), I find small park effects, more pronounced for children with lower maternal education background. My results are robust to alternative weight specifications and corrective methods of inference, including synthetic controls, and may open a new perspective for obesity policy action and prevention.

4.1 Introduction

Since 1975 global obesity has nearly tripled to a number of 650 million obese adults (18 years and older) out of 1.9 billion overweight people in 2016 (World Health Organization, 2021).[1] In Germany, results from the KiGGS Wave 2 (2014-2017) show that 15.4 percent of children aged 3 until 17 are overweight out of which 5.9 percent are obese (Schienkiewitz et al., 2018). For the individual, abnormal or excessive fat accumulation may impair health as studies have shown significant associations with higher risks of chronic diseases such as cardiovascular disease (for a meta-analysis see Friedemann et al., 2012), hypertension (e.g. Jiang et al., 2016), certain cancers and type-2 diabetes (Kahn et al., 2006) as well as a wide range of mental health problems (for a systematic review see Rajan and Menon, 2017). At the societal level, higher obesity rates translate in substantial direct and indirect costs with repercussions on public healthcare and social resources (e.g. Wang et al., 2011; Hammond and Levine, 2010; Cawley and Meyerhoefer, 2012; OECD, 2019).

[1]The WHO statistic further reveals that over 340 million children and adolescents aged 5-19 were overweight or obese [see https://www.who.int/news-room/fact-sheets/detail/obesity-and-overweight (last accessed 09/12/2022) for the whole fact sheet.].

As being overweight is largely preventable, it is of great public interest to identify potential levers that might affect individual behavior. As studies have shown evidence on substantial long-term consequences of child and adolescent obesity for adult-health (see e.g. Reilly and Kelly, 2011; Simmonds et al., 2016), children have become a target group for intervention policies (Verjans-Janssen et al., 2020; Marcus et al., 2022; Cawley et al., 2007, 2013). One major lever for policy and determinant of obesity is the lack of physical exercise on the individual level (Romieu et al., 2017; Prentice-Dunn and Prentice-Dunn, 2012). In addition to targeted programs, children's physical activities might also be affected by unintended policy interventions. In this study, I ask whether an increased access to green space (GS) in an urban environment causally affects the likelihood of being overweight in children. This research question is of particular policy interest, as not only obesity trends are increasing but urbanization is also ongoing.[2]

[2]The United Nations Department of Economic and Social Affairs Population Division (2019) shows that an estimated 55.3 percent of the world's population lived in urban settlements in 2018 and a projected 60 percent by 2030. Europe belongs to the most urbanized regions of the world with a share of 74 percent of its population living in urban areas.

A unique quasi-experimental setting allows me to study the effects of an exogenous increase in accessible green space due to the transformation of the Tempelhof airport to one of the world's largest urban open spaces, the so-called Tempelhofer Feld.[3] I apply a difference-in-differences approach, comparing several weight outcomes based on the Body Mass Index (BMI)[4] of treated school-starting children (aged 5-6) living within a 1500m radius around park entrance (treatment group) to children living further away (control group) before and after the park was opened to the public. I use new administrative data on the district level of Germany's capital city (Berlin Tempelhof-Schöneberg) from mandatory school entrance examinations that entail precise information on body height and weight measured by the medical examiner. I provide robust evidence that expanding urban green space decreases the probability to be overweight (BMI $>$ 90 percentile) by 4.3 percentage points (pp) as a result of park opening, driven by girls, children from foreign cultural backgrounds and children with less childcare exposure. My results are robust to alternative weight specifications and corrective methods of inference, including synthetic controls.

[3]Figure 4-8 in the Appendix depicts a park map.

[4]The Body Mass Index (BMI) is a commonly used value calculated from the mass (weight) and height of a person. The BMI is defined as the body mass divided by the square of the body height, and is expressed in units of kg/m^2, resulting from mass in kilograms and height in metres. I give further details on this index in Section 4.3.4.

There is a growing environmental health literature that examines the relationship between parks and children's physical exercise (Sallis et al., 2000; Sanders et al., 2015; Padial-Ruz et al., 2021; Huang et al., 2020; Ma et al., 2022). Some also link access to parks[5] directly to weight indices (Norman et al., 2006) or other child health and development outcomes (e.g. Christian et al., 2015). In their systematic review on the latter, McCormick (2017) concludes that there is a positive association between access to green space and mental well-being, overall health and cognitive development of children. One exception to research linking GS to weight outcomes based on mostly cross-sectional data is Wolch et al. (2011). Their longitudinal study from 12 communities in Southern California, following children for eight years, finds inverse associations with attained BMI at age 18 for children with better access to park space at age 9-10 (measured as 500 meter buffer from children's residence). From a policy perspective, however, none of these studies aim at isolating a "pure" GS effect on children's weight outcomes, which invites more causal research in this domain.[6]

[5]Measurements of park access varies from subjective measures, such as perceived proximity (Aggio et al., 2015) to green space, to different built-environment and distance measures (Liu et al., 2007; Feng and Astell-Burt, 2017). Giles-Corti et al. (2005) take into consideration factors of attractiveness and size in addition and proximity and find that parks are tighter linked to physical activity in the case of pleasing aesthetics.

[6]Wolch et al. (2011) refer to a set of potential confounder variables in the built environment that may be associated with BMI and access to parks, such as the availability of food resources (e.g. supermarkets and fast-food outlets (Leal and Chaix, 2011; Burdette and Whitaker, 2004)) as well as characteristics related to pollution exposure or traffic density (Jerrett et al., 2014). Furthermore, social conditions, such as crime (Burdette and Whitaker, 2004), might be correlated with park use and weight problems. However, in addition, there might be also unobserved factors, such as individual preferences to live within close access to parks, that cannot be controlled for and that lead to biased (in this case overestimated) green space effect estimates.

Economists have mainly studied adult obesity and its major determinants[7], with Cawley et al. (2007), Cawley et al. (2013) and Marcus et al. (2022) exceptionally focusing on children and how to nudge them into exercising more actively.[8] Cawley et al. (2007) and Cawley et al. (2013) exploit variation in U.S. state laws as quasi-natural experiments in order to estimate the causal impact of physical education (PE) class time on overall student physical activity and weight. While they find raising PE time for high-school students induces girls to more physical activity, they do not find evidence that PE lowers BMI or the probability that a student is overweight (Cawley et al., 2007). For students in fifth-grade, however, they find lower BMI z-scores for boys (Cawley et al., 2013). Marcus et al. (2022) evaluate a large-scale policy program in Germany that distributes sports club membership vouchers for all third graders in one German state. Their difference-in-differences analysis does not detect any statistically significant short- or long-term effects on either physical activity or being overweight, contesting the effectiveness of this policy tool.

[7]For a comprehensive compilation of economics studies on obesity and its drivers see Marcus et al. (2022).

[8]There is also a U.S. literature on targeted early education programs designed for children from lower socio-economic backgrounds, such as Head Start (e.g. Carneiro and Ginja, 2008; Frisvold and Lumeng, 2011) that finds obesity-reducing effects.

In addition to evaluating anti-obesity initiatives, economists have studied the relationship between childcare attendance and weight outcomes. The economics literature on the childcare-obesity nexus[9] depends on the institutional background of the early educational system under study. Studies from the U.S. mostly find positive effect estimates of childcare attendance inducing higher weight outcomes. Herbst and Tekin (2011, 2012) report sizable overweight and BMI effects because of childcare. In Germany, the picture is rather mixed. Cornelissen et al. (2018) apply a marginal treatment effect framework and exploit a universal childcare program to study childcare selection patterns for various child development outcomes, including BMI. For being overweight, they find reverse selection on unobserved gains (less likely overweight) with the most beneficial childcare effects for untreated children significant at the 10 percent level. Point estimates suggest a positive effect on BMI and the risk of being overweight for the currently treated child, however not statistically significantly different from zero. Lauber and Lampert (2014) exploit regional differences in childcare supply in Germany to deal with selection into childcare, and find that childcare significantly reduces weight problems (hence, a negative relationship between childcare attendance and BMI values). Their findings fit in the range of studies from Felfe and Lalive (2018) and Felfe and Zierow (2018) that find positive development effects from using early childcare in Germany. They argue that contradicting results to Herbst and Tekin (2011) could stem from the different population under study, as Herbst and Tekin (2011) analyze the effect to childcare subsidy for disadvantaged single mothers strongly increasing labor supply, which might have led to the use of low quality childcare. This branch of research is related to my research question because the roll-out of the childcare system in Germany coincides with the transformation of the park.[10] However, in Section 4.7, I discuss this threat to identification in more detail and rule out a statistically significant change of childcare use coinciding with the opening of the park.

[9]Related, but not congruent, is the large and mostly U.S. literature on maternal employment and weight problems (e.g. Cawley and Liu, 2012; Courtemanche, 2009; Greve, 2011; Morrissey et al., 2011; Liu et al., 2009). For example, Anderson et al. (2003) find positive causal effects of a higher probability to be overweight if the mother works more hours per week. For the UK, Fitzsimons and Pongiglione (2019) confirm this relationship by estimating household fixed effects models and find higher effect sizes for single mothers. Gwozdz et al. (2013), however, find little evidence of a relationship between maternal employment and obesity studying eight countries in Europe.

[10]In 2008, the law on support for children ("Kinderförderungsgesetz") was introduced that commits German federal states to a gradual expansion of childcare supply for children below

The remainder of the paper is structured as follows: Section 4.2 gives historical background on the Tempelhofer Feld relevant for identification and Section 4.3 describes the data. Section 4.4 outlines the empirical strategy followed by the main DiD-results along with some event study analysis in Section 4.5. I present sensitivity analyses in Section 4.6 and show additional results on heterogeneous effects including high risk groups in Section 4.7. Section 4.8 concludes.

4.2 Institutional background

Situated on former airport grounds[11], the Tempelhofer Feld is one of the world's largest inner-city parks today, offering over 3 million square meters of mostly undeveloped space for sports, recreation, and nature experience.

In 2008, airport operations stopped after a sequence of judicial conflicts following the joint agreement from 1996 by Berlin, Brandenburg, and the federal Ministry of Transport to concentrate air traffic to one airport in Berlin-Schönefeld. Ultimately, the agreement was backed by the Federal Constitutional Court in 2006 (Referendum, 2008). A referendum in 2008 against closing the airport failed, paving the way for conversion of the Feld. While the Senate argued in favor of major development including commercial use and new housing (Referendum, 2008), each attempt since park opening in May 2010 to develop and privatize the space has led to push-backs by citizens. All plans were finally stopped by a successful referendum to completely preserve the public space in 2014 (Referendum, 2014).

three years. The law also includes a legal claim to every parent with a child aged one to three years to a subsidized childcare slot by August 2013.

[11] For a comprehensive study on the complex history of the Tempelhofer Feld describing its multiple narratives, see Copley (2017).

The advanced announcement of the transition from airport to park could threaten my identification by allowing time for people with certain characteristics, e.g. higher preference for recreational environment, to move to the treatment area in anticipation, biasing my estimates. However, the lengthy political dispute up until the referendum in 2008 speaks against this concern, as the continuous use of the airport could have been a plausible turnout. In addition, even after the failed referendum, uncertainty remained whether the Feld would be opened to the public up until the opening in 2010[12]. One could further argue that the successful referendum in 2014 might have attracted another inflow of people with high preference for recreational space moving to the treatment area. I, therefore, show descriptively in Section 4.4 that the residential composition of the treatment area remained stable during the whole period, supporting my identification strategy.

[12] There are many articles covering this topic, see e.g. https://www.welt.de/regionales/berlin/article2653050/Der-Flughafen-Tempelhof-ist-endgueltig-Geschichte.html and also the media archive by the citizen initiative 100 % Tempelhofer Feld https://www.thf100.de/pressespiegel.html (last accessed: 09/16/2022).

Since the park's opening, a comprehensive monitoring system has been installed to provide empirical evidence on the use of the Feld via different quantitative methods, e.g. total visitor counts during representative periods at all entries, structured observations, and short interviews after random sampling. The data from the monitoring results[13] support the park as a mechanism to exogenously increase access to green space for local children (see Figure 4-1). Despite its international popularity[14], Figure 4-1a shows the Tempelhofer Feld is mainly used as recreational GS by people living in Berlin (Berlin Users) and by the largest subset, i.e. people living in bordering residential area, or at most within a 1500 meter radius around the park (Berlin Resident Users) on a regular basis.[15] Additionally, the share of children below the age of 15 are in line with the share of children of this age group in Berlin, suggesting that the park's particular characteristics are inclusive to children (see Figure 4-1b).[16]

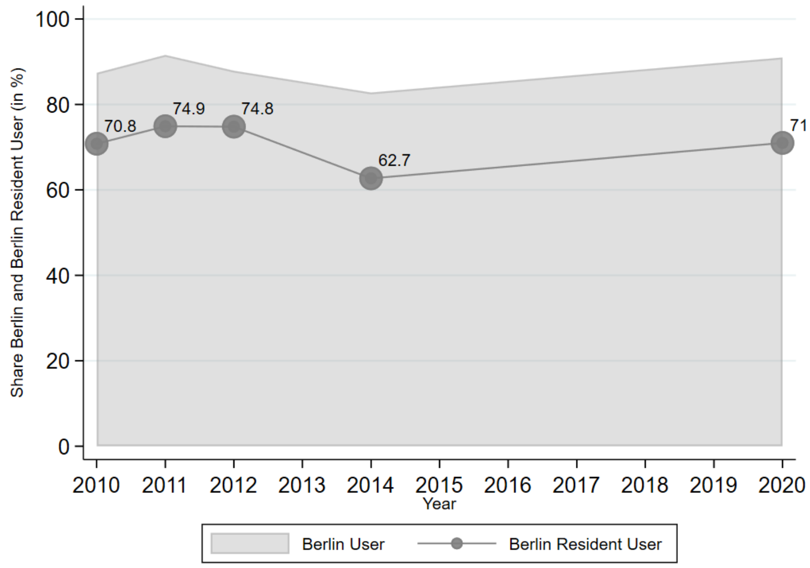
[13]All reports can be downloaded here: `https://gruen-berlin.de/en/projects/parks/tempelhofer-feld/service-info/visitor-monitoring-on-the-tempelhofer-feld` (last accessed 06/30/2022)

[14]Brenck et al. (2021) has recently published a comprehensive study on the societal values of the Tempelhofer Feld.

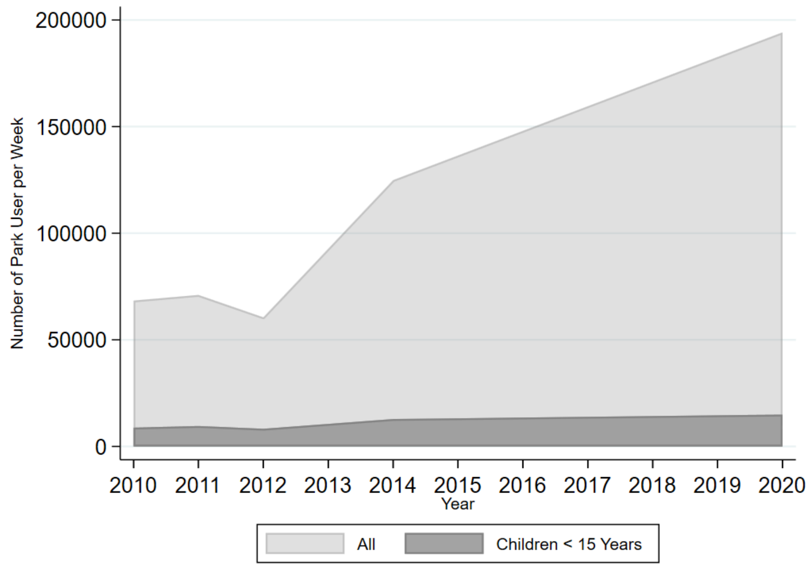
[15]While in 2010 the share of first-time visitors was still at 24.9 percent, it went down to 3 percent in 2020, which speaks in favor of regular park usage.

[16]In addition, the Tempelhof side proves to be a relevant gateway to the park, as about a third of all people enter the park from Tempelhofer Damm (see Figure 4-8 in the Appendix for a map of the entrances).

Figure 4-1: Utilization of the Park



(a) Share Berlin and Berlin Resident Users



(b) Number of Park Users

Notes: These figures plot (a) the share of Berlin and Berlin Resident users for all monitoring years; and (b) the total number of park users per week as well as children below 15 years of age. Berlin Resident Users are defined as people living in bordering residential area and within a 1500 meter radius. Own data compilation, calculation and graphical display.
Source: "Grün Berlin", Visitor Monitoring Reports (2010, 2011, 2012, 2014, 2020) https://gruen-berlin.de/projekte/parks/tempelhofer-feld/service-infos/besuchermonitoring-auf-dem-tempelhofer-feld (last accessed: 09/01/2022).

4.3 Data

Studying the causal impact of a sudden increase in nearby urban green space and its effect on child outcomes requires an ambitious data set-up in terms of geographic and socio-demographic information. My main data source is a set of administrative records on the results of school readiness examination covering the years 2007-2009 and 2014-2018 for one large district of Berlin, Tempelhof-Schöneberg, that also comprises the Tempelhofer Feld. I combine these data with data on small area socio-demographic characteristics provided by the Senate Department for Urban Development, Building and Housing (SenStadtUm, 2016) (see Table 4.8 for more information). I also use additional data provided by the monitoring system of the Tempelhofer Feld (see footnote 13) as well as by the Statistical Office Berlin-Brandenburg (see Figure 4-5 for data sources).

Assembling these different data sources creates a unique, high quality data set with unprecedented potential to study child outcomes in an urban environment with three main strengths. First, the richness of the data set allows me to condition on a large set of individual as well as family background covariates. Second, it provides geographical information on individual's residence allowing me to merge time-varying controls on a small area level. Third, most of the data are from objective examination by the trained health professional rather than being self-reported.[17] Therefore, social desirability bias and self-reporting errors are not a concern. And fourth, the compulsory nature of examination provides repeated cross-sections of the full universe of school entry-aged children each year. Therefore, there is no systematic missing reporting.

[17] This holds for all outcome variables and some of the covariates. For the voluntary parent-reported information, I include dummy variables for missing information.

The data does have a few shortcomings. First, I do not observe school readiness examinations between 2010 and 2014. Hence, I cannot study the intensive margin of the effect.[18] Second, the smallest geographical unit ("Teilverkehrszelle") in the data cannot be harmonized over time as there have been changes in spatial planning structure (see Figure 4-10 for a display of both systems).[19] Therefore, I must base my analysis on the somewhat larger geographical unit which comes along with some methodological concerns of statistical inference that I address in Section 4.6. And third, Kleiser et al. (2009) stress the data's shortcoming of a small age range. However, the available age range has been shown to have high predictive power for adult health (e.g. Simmonds et al., 2016).

[18]The first post-transformation cohort is from 2014 and consists of children born in 2009 and 2010 who have already been exposed for at least four years to the park.

[19]Nevertheless, I use an approximated set-up using "Teilverkehrszellen" as robustness check in Section 4.6.1.

4.3.1 School entrance examinations

All children in Germany must attend compulsory school entry examinations (ESU)[20] in the year before entering elementary school[21]. During examination, licensed public health service pediatricians assess whether children face any health- or development-related restrictions that might require additional support before attending school. In addition, the results from the ESU are further used in health reporting,[22] providing empirical evidence on the full population of school entry aged children on different aggregate levels[23]. The assessment includes an interview with the child as well as a set of standardized tests on various skill dimensions. Parents are asked to fill out a questionnaire covering further socio-economic background information such as parents' education level, working status, smoking habits, living arrangements, and number of children in the household among others.

[20]A few influential education economics studies have used scattered ESU records from various German states and regions to study different research questions on child development (e.g. Cornelissen et al., 2018; Felfe and Zierow, 2018; Felfe and Lalive, 2018). Since this type of administrative data is held by the local health authorities, data availability hinges on personal contacts of researchers which has been questioned in Huebener et al. (2019) given the high potential for a more comprehensive use of this data pool.

[21]For Berlin, the school readiness examination is prescribed in the Berlin Education Act (§ 55a, Para. 5).

[22]The objectives are defined in the Health Service Reform Act (§5, Para. 3).

[23]All basic reports from the Senate for Health, Care and Equal Opportunities for all years can be downloaded here: https://gsi-berlin.info/ (last accessed: 09/13/2022).

4.3.2 Outcome measures

My main outcome variable of interest is a child being overweight. The most common definition of overweight status is based on the body mass index (BMI). The BMI is calculated from the quotient of body weight and height squared (kg/m^2). Burkhauser and Cawley (2008) argue that this index - despite its widespread use in social science research - is a noisy measure of body fat and overall health. By using only body weight and height as ingredients, it may not distinguish fat from muscle or bone. As a consequence, the BMI may overestimate fatness among those children who are muscular. In my analysis, this measurement error in the dependent variable potentially reduces the power of my statistical tests. However, as the BMI remains the most commonly used measure and the analysis is more standardized, it remains a valuable measure to determine children's weight status. Additionally, my results are similar when employing different BMI reference distributions (details below).

Table 4.1: Descriptive Statistics

	All	Control	Treatment	Diff
Panel A: Outcomes				
Reference Percentiles Robert Koch-Institute (2013):				
Overweight (BMI > 90P.)	0.10	0.11	0.09	0.01
Obese (BMI > 97P.)	0.04	0.04	0.04	0.01
BMI*	-0.08	-0.08	-0.14	0.06*
Reference Percentiles Kromeyer-Hauschild et al. (2001):				
Overweight (BMI > 90P.)	0.10	0.10	0.09	0.01
Obese (BMI > 97P.)	0.04	0.04	0.03	0.01
BMI*	0.00	0.01	-0.05	0.06*
BMI (in kg/m^2)	15.66	15.67	15.55	0.12**
BMI (log.)	2.75	2.75	2.74	0.01*
Body Weight (in kg)	21.21	21.21	21.15	0.06
Panel B: Child and Family Controls				
Age (in months)	68.73	68.73	68.75	-0.02
Female	0.48	0.48	0.47	0.00
Body Height (in cm)	116.08	116.06	116.40	-0.34*
Cultural Background:				
Missing	0.04	0.05	0.03	0.02***
Foreign	0.43	0.43	0.45	-0.01
Low Birthweight:				
Missing	0.09	0.09	0.04	0.05***
Low Birth Weight	0.06	0.06	0.06	0.00
Childcare Attendance:				
Missing	0.01	0.01	0.00	0.01***
No Childcare	0.02	0.02	0.01	0.01***
Childcare < 1/2 Year	0.01	0.01	0.00	0.01***
Childcare 1/2 Year - 2 Years	0.08	0.08	0.06	0.02**
Childcare > 2 Years	0.88	0.87	0.92	-0.05***
Family Status:				
Missing	0.02	0.02	0.01	0.01
Single Parent	0.22	0.23	0.20	0.02*
Tertiary Education Mother:				
Missing	0.06	0.06	0.05	0.01
Tertiary Education	0.46	0.45	0.49	-0.04*
Working Status Mother:				
Missing	0.08	0.08	0.06	0.02*
Working Full- or Part-Time	0.63	0.62	0.69	-0.07***
Number of Children in Household:				
Missing	0.03	0.03	0.02	0.01**
1 Child	0.26	0.27	0.24	0.02
2 Children	0.48	0.48	0.49	-0.01
3 Children	0.16	0.16	0.19	-0.03**
4 or more Children	0.07	0.07	0.05	0.02**
Number of Smokers in Household:				
Missing	0.04	0.04	0.04	0.00
0 Smoker	0.63	0.63	0.66	-0.02
1 Smoker	0.24	0.24	0.23	0.01
2 or more Smoker	0.08	0.08	0.07	0.01
N	19,993	18,641	1,352	19,993

Notes: The table reports mean outcomes and individual and family controls, separately for control and treatment group and their difference. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

Both ingredients for the body mass index (BMI) used to determine being overweight - body weight as well as body height - are objectively assessed by the pediatrician. I use the age- and gender-specific BMI distribution proposed by the Robert Koch-Institute based on the German KiGGS study and a representative data pool from the years 2003-2006 (Robert Koch-Institute, 2013). Results do not change considerably when I apply the cut-offs proposed by Kromeyer-Hauschild et al. (2001) whose reference data stem from a much older data pool from the years 1985-1999, but arguably a time frame before the steep rise of the overweight epidemic (see Section 4.6.2). The literature differs with respect to critical values to determine corresponding BMI-for-age percentiles depending on the population under study. In the U.S., Herbst and Tekin (2011, 2012), for example, children at or above the 85th percentile are coded as overweight and children at or above the 95th percentile of the BMI distribution as obese. For both the Robert Koch-Institute (2013) and Kromeyer-Hauschild et al. (2001) distributions based on German population, the critical value for being overweight is above the 90th percentile and above the 97th percentile for obesity. For my analyses, I use these thresholds and construct binary indicators for being overweight and obese as well as the BMI variable itself standardized to mean zero within the (half-year) age-gender distribution.[24] Panel A of Table 4.1 displays basic summary statistics of these outcomes for the main estimation sample, also separately for control and treatment group, and Panel A of Table 4.2 for pre-intervention cohorts.

[24] I mark the BMI variable with a * to indicate standardizing according to Cole (1990)'s LMS-method and percentile tables from Robert Koch-Institute (2013) (and Kromeyer-Hauschild et al. (2001) in Table 4.9)

$$BMI^* = \frac{[\frac{BMI}{M_t}]^{L_t} - 1}{L_t S_t} \tag{4.1}$$

where L_t stands for the Box-cox-power transformation, M_t for the median and S_t for the coefficient of variation dependent on t as combination of half-year age and sex of the child.

Table 4.2: Pre-Treatment LOR Differences

	All	Control	Treatment	Diff
Panel A: Main Outcomes				
Reference Percentiles Robert Koch-Institute (2013):				
Overweight (BMI > 90P.)	0.13	0.13	0.14	-0.02
Obese (BMI > 97P.)	0.05	0.05	0.06	-0.01
BMI*	0.11	0.11	0.16	-0.05
Panel B: LOR Controls				
Population Density (in people/km^2)	8631.50	9028.51	2882.41	6146.10***
Share Children	3.99	3.98	4.11	-0.14***
Residential Mobility Children	0.52	0.40	2.30	-1.90***
Residential Duration	44.77	44.62	47.08	-2.46***
Youth Unemployment	4.38	4.35	4.93	-0.59***
Child Poverty	31.53	31.77	28.14	3.63***
N	6,471	6,053	418	6,471

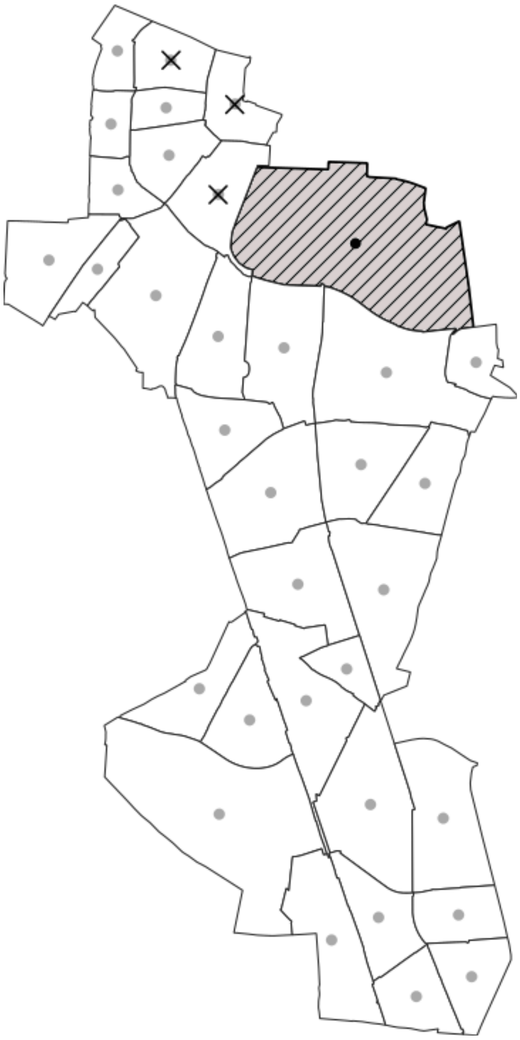
Notes: The table displays mean outcomes and LOR controls prior to transformation from airport to park (examination cohorts 2007-2009) from the main estimation sample, separately for control and treatment group and their difference. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

4.3.3 Geographical units

Figure 4-2 shows for Berlin Tempelhof-Schöneberg the "Lebensweltlich orientierte Räume" (LOR) planning unit system that Berlin developed in 2006 in order to provide a spatial base for planning, prediction and surveillance for demographic and social development. LOR district cutting was based on both homogeneity in terms of structural similarities (e.g. same architecture) but also similar socio-economic structures.[25]

[25] See https://www.stadtentwicklung.berlin.de/planen/basisdaten$_s$tadtentwicklung/lor/ for further details (last accessed: 2022/07/30).

Figure 4-2: Treatment Assignment



Notes: This figure shows the district Tempelhof-Schöneberg, its 34 LOR units and each LOR's centroid. Treatment is defined in terms of a maximum Euclidean distance of 1500 meters from each LOR's centroid to the centroid (black dot) of the LOR including the Tempelhofer Feld (ruled area). This results in only one treatment LOR (shaded area). The three LOR's marked with an "x" are excluded from the analysis due to the proximity (< 1500 meter) to another large green space in the neighbouring district ("Gleisdreieckpark") which has been opened in the same time window as the Tempelhofer Feld. Own graphical display.
Source: LOR Vector Data, Statistical Office Berlin-Brandenburg,
https://www.stadtentwicklung.berlin.de/planen/basisdaten$_s$tadtentwicklung/lor/de/download.shtml (last accessed: 09/13/2022).

In this paper, I mainly work with the smallest planning unit of the LOR system available in my dataset, the so-called "Planungsräume" (PLR). For the district Tempelhof-Schöneberg, there are 34 PLR, each covering, on average, an area of 1.56 square kilometer.

In Section 4.6.2, I also make use of a second geographical unit available in my data based on small traffic units, so-called "Teilverkehrszellen" (TVZ). The advantage is that it provides information on the residence of the children on an even smaller scale. There are, however, three caveats: First, it lacks two post-treatment years 2017-2018. Second, there has been an adjustment of the cutting of the TVZ system after 2009 (see Figure 4-10 in the Appendix for a visualisation). Hence, harmonisation brings about some measurement error for treatment assignment, potentially downwards-biasing my estimates. And third, there are no comparable time-varying TVZ-specific information to include in my regression model. These regression results should, therefore, be viewed as additional robustness checks only.

4.3.4 Treatment assignment

Identifying the treatment group in my setting is not straight-forward, as treatment is not clear-cut and based on some change in regulation on e.g. a county or state unit. It is rather the geographic distance to the park after transformation that I assume to have an effect on regular physical activity of children. Therefore, I make use of a Euclidean distance measure and define children to belong to the treatment group if the centroid of their PLR is 1500 meters away from the centroid of the LOR containing the Tempelhofer Feld (see Figure 4-2).

This identification set-up is based on three reasonings. First, I use representative monitoring data from the park's operating company to show that in 2010, 70.8 percent of the park users are from adjacent residential area or a 1500 meter radius (see Fig 4-1a). Second, given the size of the park, the Tempelhofer Feld qualifies to a district park ("Bezirkspark") for which the area of influence is determined at 1500 meters by the Senate Department for Urban Development, Building and Housing (SenStadtW, 2017). And third, this approach is backed by a literature on walkability and access to green spaces that considers 1500 meters a reasonable distance in the urban context (e.g. Schindler et al., 2022).

For my main analysis, this leads to a single LOR unit being treated. The rest of my data serves as control group except the excluded observations delineated in the following subsection. In Section 4.6.1, I present various control group samples that show that results are robust against various geographic specifications.

4.3.5 Sample restrictions

The original data set contains 23,002 children from the examination cohorts 2007-2009 and 2014-2018. Each cohort is a complete count of all children one year prior to their elementary school start who reside in Berlin-Tempelhof-Schöneberg. For treatment assignment, I need to identify children's residence and thereby lose 106 observations (0.5 percent of the original sample) with missing LOR information. I further exclude individuals with missing information on either sex (1 obs.), body height (79 obs., 0.3 percent) or body weight (137 obs., 0.6 percent).[26] In a last step, I exclude children from three LORs that lie in the catchment area ($<=$ 1500 meter) from the "Gleisdreieckpark", another park in the neighbouring district of Mitte that was opened three years after the Tempelhofer Feld, but still falls in the same treatment years (marked with an "X" in Figure 4-2). This leads to a remaining final sample size of 19,993 children.

[26]I control for missing information on further individual and family characteristics due to the voluntary nature of the parent questionnaire with dummy variables.

4.3.6 Descriptive statistics

Table 4.1 provides sample means for the main estimation sample, and separately for control and treatment group. 10 percent of all examined children are considered overweight, which is comparable to the national average of 10.8 percent for girls and 7.3 percent for boys Schienkiewitz et al. (2018) age 3-6 in comparable years. Treated children score statistically significantly lower on the BMI distribution than control children (15.55 vs. 15.67 points) and are also slightly less likely to be overweight (9 vs. 11 percent). These raw mean comparisons of the outcomes suggest that treatment reduced problems with weight, but Panel B of Table 4.1 reveals several differences between treated and control children which justifies the need for an identification strategy to causally estimate the effect.

Table 4.3: OLS Estimates Weight Outcomes on Child and Family Characteristics

	Overweight (1)	**Obesity** (2)	**BMI*** (3)
Age (in Months)	-0.007***	-0.003***	-0.036***
	(0.001)	(0.000)	(0.002)
Female	0.007*	0.007**	0.043***
	(0.004)	(0.003)	(0.015)
Body Height (in cm)	0.013***	0.007***	0.055***
	(0.001)	(0.001)	(0.002)
Cultural Background: Reference German			
Missing	0.046***	0.020***	0.168***
	(0.015)	(0.007)	(0.033)
Foreign	0.048***	0.022***	0.199***
	(0.005)	(0.003)	(0.017)
Low Birthweight (< 2500g): Reference No Low Birthweight			
Missing	-0.021**	-0.007	-0.081***
	(0.008)	(0.006)	(0.029)
Low Birth Weight	-0.002	0.009*	-0.356***
	(0.007)	(0.005)	(0.038)
Childcare Attendance: Reference No Childcare Attendance			
Missing	-0.070***	-0.044	-0.098
	(0.025)	(0.027)	(0.110)
Childcare < 1/2 Year	-0.002	-0.015	-0.030
	(0.020)	(0.017)	(0.101)
Childcare 1/2 Year - 2 Years	-0.013	-0.023*	-0.001
	(0.013)	(0.012)	(0.059)
Childcare > 2 Years	-0.023	-0.035**	0.025
	(0.014)	(0.013)	(0.061)
Family Status: Reference: No Single Parent			
Missing	-0.044*	-0.008	-0.113
	(0.022)	(0.014)	(0.078)
Single Parent	0.007	0.002	0.082***
	(0.005)	(0.003)	(0.022)
Tertiary Education Mother: No Tertiary Education			
Missing	-0.013	-0.011	-0.077**
	(0.008)	(0.006)	(0.032)
Tertiary Education	-0.043***	-0.023***	-0.154***
	(0.006)	(0.004)	(0.019)
Working Status (Mother): Reference Not Working			
Missing	-0.008	-0.005	0.008
	(0.009)	(0.007)	(0.031)
Working Full- or Part-Time	-0.021***	-0.015***	-0.053***
	(0.005)	(0.005)	(0.014)
Number of Children in Household: Reference 1 Child			
Missing	0.040	0.001	0.025
	(0.026)	(0.014)	(0.075)
2 Children	-0.006	-0.004	0.011
	(0.005)	(0.002)	(0.022)
3 Children	-0.008	-0.003	0.038
	(0.007)	(0.005)	(0.037)
4 or more Children	-0.011	-0.011	0.034
	(0.012)	(0.007)	(0.052)
Number of Smoker in Household: Reference No Smoker			
Missing	0.006	0.009	0.089
	(0.019)	(0.008)	(0.057)
1 Smoker	0.046***	0.028***	0.198***
	(0.005)	(0.004)	(0.023)
2 or more Smoker	0.072***	0.039***	0.317***
	(0.011)	(0.006)	(0.025)
N	19,993	19,993	19,993

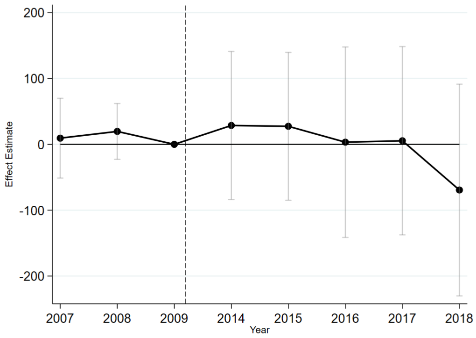
Notes: This table reports OLS regressions for each outcome on the full set of individual and family controls. In each regression, the time-varying LOR controls are also included (but not displayed) as well as LOR district and year fixed effects. Standard errors clustered at the LOR district level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

Most notable is the higher share of more than 2 years exposure to childcare for treated children (92 vs. 87 percent)[27] along with the significantly higher share of full- or part-time working mothers (69 versus 62 percent). The share of single families is slightly lower among treated children (20 vs. 23 percent), and mothers of treated children are also more likely to have a tertiary education (49 vs. 45 percent), which speaks in favor of a more positive socio-economic composition of the treatment group compared to the rest of Tempelhof-Schöneberg. The share of children with foreign descent is, however, somewhat higher in the treatment group compared to the control group (45 vs. 43 percent). In Table 4.3, I present OLS estimates showing the relationship of these individual child and family characteristics with my weight outcomes. Being of foreign descent increases the probability to be overweight by 4.8 percentage points (pp) and to be obese by 2.2 pp in line with studies finding ethnic disparities in early childhood obesity (e.g. Guerrero et al., 2015). Childcare attendance for more than 2 years is associated with a 2.3 pp lower probability to be overweight and statistically significant 3.5 pp lower probability to be obese, corroborating the direction of results found by Lauber and Lampert (2014). The established socio-economic positive relationship of single parenting (e.g. Duriancik and Goff, 2019) and number of smokers in the household (e.g. Chen and Morris, 2007) with weight problems hold for this sample as well, while higher education and working status both are negatively associated with weight indicators (for the highly influential study on the SES-health gradient, see Case et al., 2002). This supplementary analysis shows that the set of individual and family background controls matters for the outcomes of interest and should be included in further analysis.

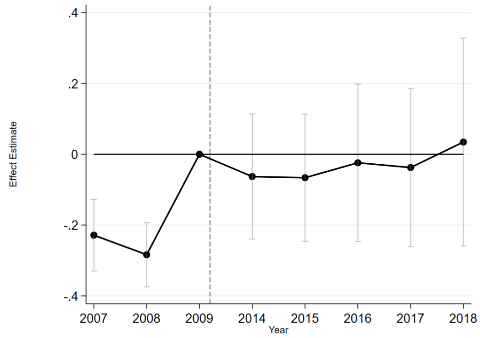
[27] I show in Section 4.4 with an event study graph that there is no statistically significant change in duration of childcare attendance coinciding with opening of the park.

Figure 4-3: Event Study of LOR Characteristics

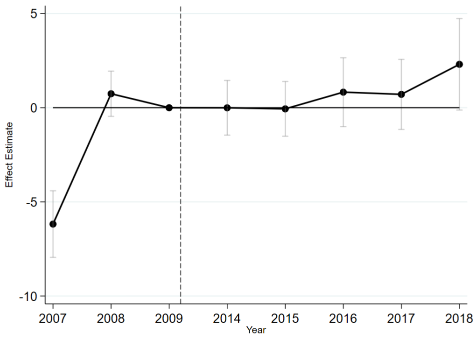
(a) Population Density



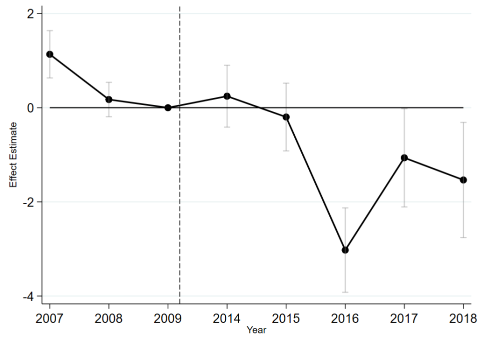
(b) Share of Children



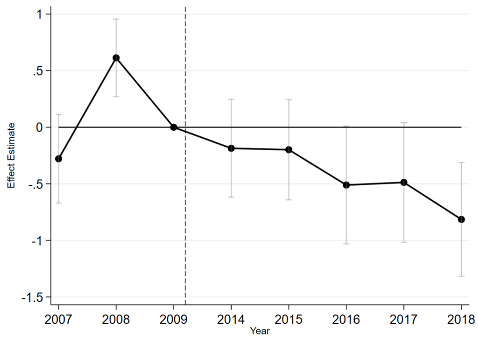
(c) Residential Mobility Children



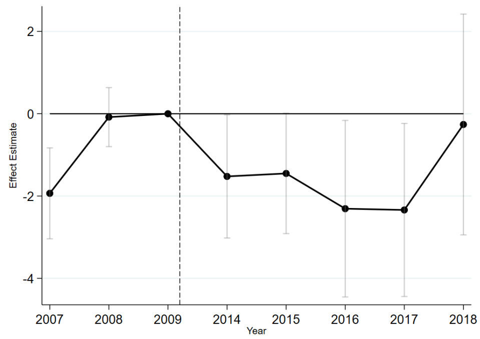
(d) Residential Duration



(e) Youth Unemployment



(f) Child Poverty



Notes: All six event study graphs assess the threat of simultaneous change in the LOR characteristic between treatment and control group at the time of the treatment. Figure 4-3a displays the population density (measured in people/m^2), while Figure 4-3b displays the share of children below 5 years of age. Figures 4-3c and 4-3d display event study graphs for the influx share of children below 6 years (children moving in - children moving out of LOR) and the share of residents living at least 10 years at the same address. Figures 4-3e shows the event study graph for youth unemployment, measured as share of unemployed 15-25 year old adolescents, and an indicator for child poverty in Figure 4-3f, measured as the share of children below age 15 who live in households that receive transfer payments relative to the population below age 15. See also Figure 4-9 in the Appendix for graphs showing the means over time of each characteristic. *Source:* See Table 4.8 in the Appendix for more details on each indicator and data source.

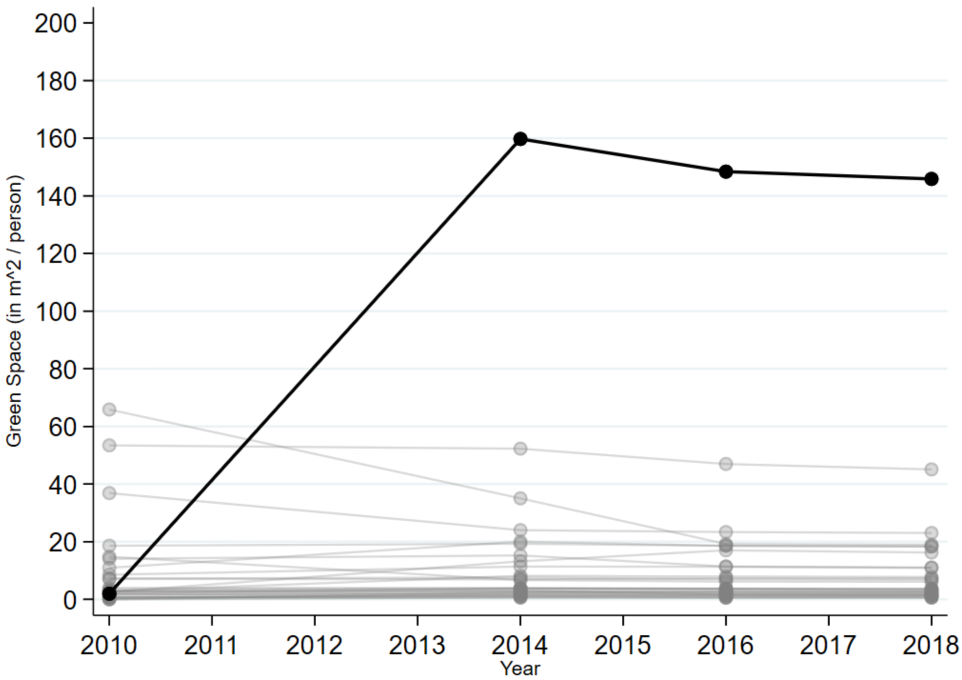
Panel B of Table 4.2 displays LOR characteristics prior to the increase in green space for treatment and control group and shows statistically significant level differences. Whether these differences also come along differential trends between treatment and control group is assessed in Figures 4-3a-4-3f (Figures 4-9a-4-9f in the Appendix additionally plot the means over time by treatment status). A sudden increase in population density post transformation of the Tempelhofer Feld would threaten the assumption of a stable population composition. Figure 4-3a shows that this is not the case. Furthermore, the opening of the park in 2010 has not led to an increase in the share of children in the treatment LOR, as can be seen in Figure 4-9b. The potential concern of an upwards biasing gentrification trend that healthier families with unobserved characteristics, such as preferences for green spaces, move to the treatment area is alleviated by Figures 4-3c and 4-3d who do not show statistically significant changes around park entry. Residential Mobility measures the share of the net sum of children below the age of 6 having moved into a LOR district minus those who moved away in percent of all children below age 6 in that district in a certain time period.[28] There is a somewhat higher influx in the treated LOR district with a share of 4.9 percent in 2008 compared to 0.55 percent in the control LOR districts which can be seen in Figure 4-9c. However, as this inflow happened before park opening (see Figure 4-3c), I am not worried about potential bias concerning mobility trends. Figure 4-3d also provides a stable picture in terms of residential duration. The share of people who have lived at least 10 years at the same address in the treated LOR district did not decrease after the opening of the park, which backs the assumption of a stable resident composition and leaves little room for reverse causality, selection and sorting issues. Also with respect to socio-economic disparities, measured in terms of youth unemployment share and child poverty as the share of transfer payments granted to children below 15 years of age, can be stated that trends move parallel (Figures 4-9e and 4-9f) without any coinciding change with park opening (see Figures 4-3e and 4-3f).

[28] Up until 2010, the time period is yearly, while from 2012 all four indicators are based on a two years span.

4.4 Empirical strategy

Figure 4-4 plots the amount of green space by LOR district measured in m^2/person. The black thick line visualizes the sharp increase in available green space for children living in the treatment area, while the light gray lines show the available green space in each control LOR, providing identifying variation for this analysis.[29]

Figure 4-4: Green Space by LOR district



Notes: This graph shows the average green space measured in m^2 per person in each LOR from the main estimation sample. The black line shows the treatment LOR, while the light gray lines are the control LORs. Own graphical display.
Source: Statistical Office Berlin-Brandenburg. Data collected by SenStadtBW, PRISMA, https://www.stadtentwicklung.berlin.de/soziale$_s$tadt/sozialraumorientierung/de/prisma.shtml (last accessed: 09/02/2022)

[29] I run a robustness check in Section 4.6.2 excluding the downward-trending LOR starting with more than 60 m^2/person in 2010, which is LOR [7050503] Eisenacher Straße. It does not impact my findings.

To identify the effect on weight of an exogenous increase in green space through the nearby transformation of a former airport into a vast recreational park, I estimate the following difference-in-differences (DiD) equation:

$$Y_{ilc} = \beta GS_{lc} + \gamma_l + \lambda_c + X'_{ilc}\delta + \varepsilon_{ilc}, \tag{4.2}$$

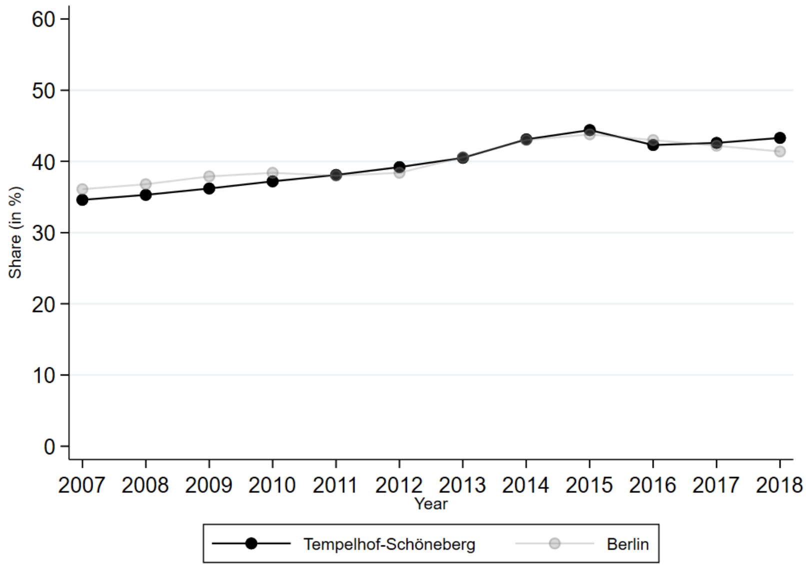
where Y_{ilc} is the outcome (e.g. being overweight, being obese, or BMI*) for individual i from LOR district l and examination cohort c. GS_{lc} denotes the variable of interest. It is the interaction term between the post variable (examination cohorts 2014-2018) and the binary treatment indicator variable equal to one if the centroid of an individual's LOR residence is less than 1500m away from the centroid of the LOR of Tempelhofer Feld. Hence, β denotes the effect of the transformation on children from the nearby residential area. γ_l and λ_c are fixed effects for LOR districts and examination cohorts, respectively, thus taking into account general differences in the outcomes between LOR districts and across examination cohorts. X_{ilc} is a vector of a rich set of pre-determined individual, parental and household characteristics (see Table 4.1). In addition, X_{ilc} also contains a set of time-varying control variables at the LOR district level (see Table 4.2 for pre-transformation means). Finally, ε^J_{ilc} denotes the error term which might be correlated within LOR districts. Hence, I cluster standard errors on the LOR level[30], which coincides with the level the treatment variable is assigned.

[30]See Section 4.6.2 for a detailed discussion on the single cluster concern.

This difference-in-differences (DiD) framework hinges on the key assumption of parallel trends between treatment and control group, which requires that the latter mimics the counterfactual outcome for the treatment group in the absence of treatment. Otherwise estimates of the causal effect will be biased. In addition to event study analyses in Section 4.5, I provide a battery of sensitivity checks in Section 4.6 to validate this assumption. DiD further implies that the composition of population in treatment and control groups before and after intervention should be stable, which I discussed in more detail in Section 4.3.6. A related threat to identification is that I do not know whether children have lived all their lives at the indicated residence at the time of the examination. Thus, there is a source of potential downwards bias for my effect estimates through falsely categorizing children who have been assessed in the treatment LOR, but have spent their early childhood in a control LOR. Therefore, I run a specification in Section 4.6.2 excluding children who are not born in Germany which does not alter neither size nor statistical significance level of the estimates. I further provide descriptive evidence in Figures 4-9c and 4-9d that residential mobility in this relevant age group of below 6 year old children is low, while general residential duration at the same address for 10 years is fairly high.[31]

[31] Residential duration for at least 5 years (provided as indicator K10 in the MSS data) is even higher ranging from 62.65 (66.3) percent in 2007 to 65.81 (67.08) percent in 2018 for the control (treatment) group.

Figure 4-5: Roll-Out of Childcare System



(a) Slot-Child-Ratio for Children < 3 Years over Time

(b) Event Study Graph on Childcare Attendance in Months

Notes: Figure 4-5a displays the attendance rates of children below age 3 for the district Tempelhof-Schöneberg (in black) and for all of Berlin (in light gray) for the whole analysis period. Figure 4-5b plots an event study graph on childcare attendance duration in months.
Source:
Figure 4-5a: Statistical Office Berlin-Brandenburg. Statistical Report K V 7 - years 2007-2018. https://www.statistik-berlin-brandenburg.de/k-v-7-j (last accessed: 09/01/2022)
Figure 4-5b: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

A second potential threat to identification could be that another reform coincides with the opening of the park and affects weight outcomes, especially since the time window for the treatment is large in the available data set. A simultaneous policy change is the roll-out of the German childcare system for children below age three. Reassuringly however, Barschkett (2022) does not find any reform effects from this particular childcare expansion on obesity using administrative health records for the whole of Germany. My effect estimates picking up potential effects of this alternative reform is also not a concern in my data as I can show in Figure 4-5a that the roll-out happened very smoothly over time in the district of Tempelhof-Schöneberg.[32] Zooming into the district to check for differential roll-out trends between treatment and control LORs, I can show with an event study graph in Figure 4-5b that there is no statistically significant jump in childcare attendance duration coinciding with the treatment window.

4.5 Results

I start with the main regression results and the simplest DiD specification in Panel A of Table 4.4 in Column (1). This regression only includes a binary treatment group indicator (which is 1 for children who reside in the treatment area), a binary post-reform indicator (which is 1 for examination cohorts 2014-2018), and its interaction term denoted as "GS Effect".

[32] In Berlin, the counterparts to municipalities who are responsible for the early educational system in Germany are the districts. In the presence of severe supply shortages of childcare spots, attendance rates are a reasonable proxy for the availability of places (see chaper 2 or Bach et al. (2019) for a detailed discussion on slot-child-ratios as instrument for childcare attendance).

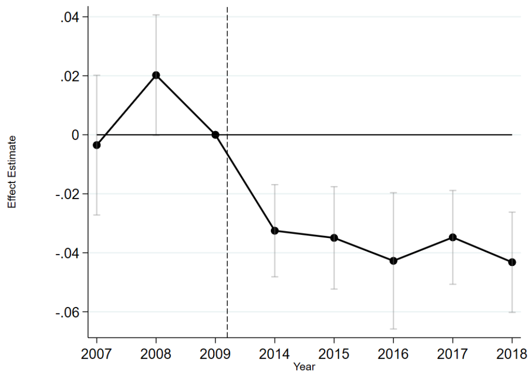
Table 4.4: Effect of Green Space on Overweight, Obesity and BMI*

	(1)	(2)	(3)	(4)
Panel A: Overweight				
GS Effect	-0.042***	-0.040***	-0.039***	-0.043***
	(0.008)	(0.008)	(0.007)	(0.007)
Panel B: Obesity				
GS Effect	-0.016***	-0.015***	-0.014***	-0.015***
	(0.004)	(0.003)	(0.004)	(0.003)
Panel C: BMI*				
GS Effect	-0.155***	-0.152***	-0.155***	-0.171***
	(0.029)	(0.029)	(0.033)	(0.027)
LOR FE	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes
Child and Family Controls	No	No	Yes	Yes
Time-Varying LOR Controls	No	No	No	Yes

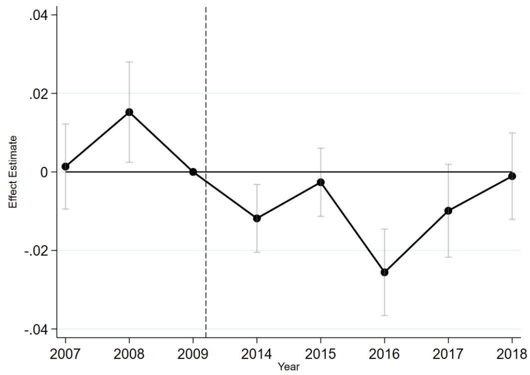
Notes: The table shows the green space effect of the Tempelhofer Feld transformation for the main outcomes of interest, being overweight (BMI > 90 P.), being obese (BMI > 97 P.) and the standardized BMI score according to the (half-year) age-gender distribution proposed by Robert Koch-Institute (2013) for individuals based on Equation (4.2). Standard errors clustered at the LOR district level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

I estimate that treated children who live in the neighbouring area of the park after its opening have a 4.2 percentage points (pp) lower probability of being overweight and a 1.6 pp lower probability of being obese. The direction of the effect also holds for the whole BMI distribution with a decrease in 0.16 standard deviations (SD). All three coefficients are highly significant at the one percent level. Moreover, they are robust to the inclusion of adding LOR and year fixed effects in column (2) which alleviates concerns of confounding time trends or geographical factors. Column (3) adds a rich vector of individual and family background controls, while Column (4) includes time-varying controls on the LOR level, which is my preferred specification and serves as reference for sensitivity analysis presented in Section 4.6. Compared to a sample mean of 10 percent of all children being overweight and 4 percent being obese, the estimated coefficients are fairly large. The negative sign of the estimates are in line with the results compiled by Wolch et al. (2011) from their systematic review who also find negative (more beneficial) relationships between park access and BMI outcomes.

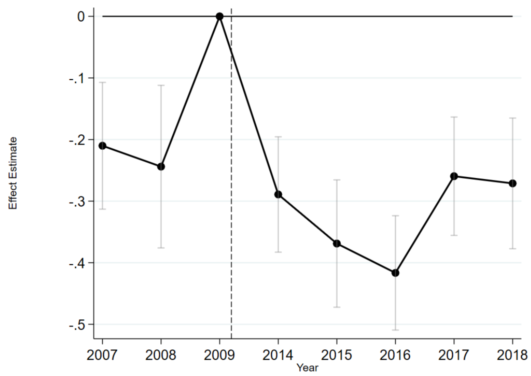
Figure 4-6: Event Study Approach



(a) Overweight



(b) Obesity



(c) BMI*

Notes: All three subfigures show event study graphs for the main outcomes of interest, being overweight (BMI > 90 P.), being obese (BMI > 97 P.) and the standardized BMI score according to the (half-year) age-gender distribution proposed by Robert Koch-Institute (2013).
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

The validity of the common trend assumption between treatment and control group cannot be tested directly. However, event study graphs are a useful tool to assess whether pre-trends are present that threaten causality. The idea is that examination cohorts prior to the park opening should have small and insignificant interaction coefficients of year and treatment group, taking 2009 as the year prior to park opening as reference year. When looking at Figure 4-6, this is the case for my main outcome of interest, children being overweight. For being obese, Figure 4-6b shows tiny significant deviations from zero. Figure 4-6c reveals fairly large and statistical significant pre-trends for the overall standardized BMI distribution, suggesting that both outcomes from the upper tails of the distribution are more suitable for causal analysis.[33] Another noteworthy result is that in Figure 4-6a the green space effect persists over time, while for being obese it disappears by the end of the observation period.

4.6 Sensitivity analysis

4.6.1 Different control groups

[33]Nevertheless, I present sensitivity analysis for all three outcomes.

Table 4.5: Robustness Checks - Different Control Groups

	Main	Geographic Sensitivity				Synthetic Control			TVZ
		- Deter. Green LOR	Only T	Only T + Buffer	Dist <=2500m	EB 1	EB 2	Synth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Overweight									
GS Effect	-0.043***	-0.043***	-0.044***	-0.035***	-0.018	-0.022*	-0.042***	-0.041***	-0.031
	(0.007)	(0.007)	(0.007)	(0.006)	(0.027)	(0.012)	(0.007)	(0.007)	(0.028)
N	19,993	19,505	13,321	10,384	19,993	19,993	19,993	19,993	14,561
Panel B: Obesity									
GS Effect	-0.015***	-0.016***	-0.013***	-0.012*	-0.000	-0.011**	-0.015***	-0.014***	-0.017*
	(0.003)	(0.003)	(0.005)	(0.006)	(0.010)	(0.004)	(0.003)	(0.003)	(0.010)
N	19,993	19,505	13,321	10,384	19,993	19,993	19,993	19,993	14,561
Panel C: BMI*									
GS Effect	-0.171***	-0.172***	-0.126***	-0.089	-0.100	-0.180***	-0.170***	-0.173***	-0.183**
	(0.027)	(0.028)	(0.026)	(0.052)	(0.067)	(0.026)	(0.029)	(0.029)	(0.076)
N	19,993	19,505	13,321	10,384	19,993	19,993	19,993	19,993	14,561

Notes: The table shows the green space effect for various alternative specifications with respect to the choice of control groups. Columns (1) repeats the main results and preferred specification from Table 4.4. In Columns (2)-(5) I test different treatment and control group assignments according to Figure 4-11. Column (2) excludes children residing in the LOR with deteriorating green space in the post-treatment years, column (3) only includes children from Tempelhof and column (4) adds a buffer zone between treatment LOR and the rest of Tempelhof. Column (5) increases the treatment area to children residing 2500 meter away from the park entrance. Columns (6)-(8) show results from synthetic control group analysis. For columns (6) and (7), I follow Hainmueller (2012) and apply entropy balancing as pre-processing before estimation. I reweight the LOR units in my control group in order for the two pretreatment cohorts to have the same mean as the treatment group in terms of all three outcomes (EB 1) and in a second specification also on all individual and family controls with statistically significant differences (EB 2). In column (8), I construct the control group by searching for a weighted combination of control units with the synth stata package from the pool of the time-varying LOR covariates and the two pre-treatment means of the respective outcome (see Figure 4-12 for the respective synth plot exemplary for the outcome of being overweight). In column (9), I apply the smaller geographical unit of "Teilverkehrszellen" (TVZ) in my data (see graphical visualisation in Figure 4-10). * p<0.1, ** p<0.05, *** p<0.01.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

To underline the reasoning for my choice of treatment group presented in Section 4.3.4, I provide several robustness checks in terms of geographic sensitivity[34]. In columns (2) - (5) of Table 4.5, I present for all three outcomes different treatment and control group assignments according to Figure 4-11 in the Appendix. Column (2) excludes children residing in the LOR with deteriorating green space in the post-treatment years, column (3) only includes children from Tempelhof and column (4) adds a buffer zone between treatment LOR and the rest of Tempelhof. Column (5) increases the treatment area to children residing 2500 meter away from the park entrance. For being overweight, neither sign nor significance level of the effect is altered. All three effect estimates are smaller when only including Tempelhof and the buffer zone (column (4)) in the analysis, and the effect for BMI* is no longer statistically significant. This is also the case when increasing the treatment zone to 2500 meter, which suggests that this distance is too large to actually offer children a nearby opportunity for recreation and physical activity and supports my definition of 1500 meters. In columns (6) and (7), I conduct a synthetic control group exercise based on entropy balancing (Hainmueller, 2012; Abadie et al., 2010). Hence, I reweight the LOR units in my control group in order for the two pretreatment cohorts to have the same mean as the treatment group in terms of all three outcomes (EB 1) and in a second specification also on all individual and family controls with statistically significant differences (EB 2). In column (8), I construct the control group by searching for a weighted combination of control units with the synth stata package from the pool of the time-varying LOR covariates and the two pre-treatment means of the respective outcome (see Figure 4-12 in the Appendix displaying treated and synthetic time series together for being overweight). These regression results confirm the main findings of significantly reducing the probability of being overweight, obese, and the child's BMI*.

[34] Defining treatment status based on geography is common in literature using spatial difference-in-differences models to assess impacts of certain points of interests or infrastructure, on economics outcomes such as housing values (e.g. Diao et al., 2017; Baum-Snow and Kahn, 2000). As the elasticity of housing prices is large, it is much easier to empirically detect a suitable buffer zone for treatment assignment by looking at housing price gradients of network distances before and after the specific infrastructure was built. Unfortunately, this cannot be applied to my research question. However, by using synthetic control methods, I also use a data-driven way to select comparison groups in addition to manually choosing certain control-treatment group combinations

In a last specification testing several control groups, I apply the smaller geographical unit of "Teilverkehrszellen" (TVZ) in my data (see graphical visualisation in Figure 4-10). The sign of the effects remain the same, but standard errors increase and for being overweight, there is no longer a statistically significant effect. However, as with harmonisation comes along substantial measurement error in the treatment variable, this result should not be heavily weighted.

4.6.2 Other sample restrictions and estimation issues

I now turn to sample restrictions in terms of temporal or other group sample sensitivities displayed in Table 4.6.

Table 4.6: Robustness Checks - Other Sample Restrictions and Estimation

	Main	Temporal Sensitivity		Sample Sensitivity				Estimation Issues	
		-Pre Year	-Last Two Post Years	Only Comp. School Age	Only 60-72 Months	Only Born in GE	Excl. Treatment Outliers	+ LOR-Linear Time Trends	Placebo Year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Overweight									
GS Effect	-0.043***	-0.045***	-0.041***	-0.035***	-0.034***	-0.044***	-0.019***	-0.043***	0.027
	(0.007)	(0.009)	(0.009)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.019)
N	19,993	17,806	14,702	17,661	15,872	18,399	17,992	19,993	6,471
Panel B: Obesity									
GS Effect	-0.015***	-0.016***	-0.018***	-0.010***	-0.006	-0.013***	-0.003***	-0.015***	0.006
	(0.003)	(0.005)	(0.004)	(0.003)	(0.004)	(0.003)	(0.000)	(0.003)	(0.010)
N	19,993	17,806	14,702	17,661	15,872	18,399	17,992	19,993	6,471
Panel C: BMI*									
GS Effect	-0.171***	-0.096***	-0.217***	-0.182***	-0.181***	-0.162***	-0.102***	-0.176***	0.067
	(0.027)	(0.034)	(0.030)	(0.028)	(0.025)	(0.026)	(0.023)	(0.026)	(0.073)
N	19,993	17,806	14,702	17,661	15,872	18,399	17,992	19,993	6,471

Notes: The table shows the green space effect for various other sample restrictions and estimation. Column (1) again repeats the main estimation results from Table 4.4. Column (2) drops the last pre-examination cohort, i.e. the year prior to the transformation of the park, from the sample, while column (3) leaves out the last two post years (2017 and 2018). In column (4) I restrict the sample to children who are of compulsory school age to exclude those children with deferred school enrolment from the analysis and to only 5-year-olds in column (5). Column (6) only includes children who are born in Germany, and column (7) excludes treatment outliers (5 percent highest and lowest BMI values). Column (8) accounts for LOR-linear time trends, and column (9) executes a placebo test, assuming that the park opening was in 2008 and drops all post years after 2009 from the analysis (following Lechner (2010)). * $p<0.1$, ** $p<0.05$, *** $p<0.01$.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

In column (2), I drop the last pre-examination cohort, i.e. the year prior to the transformation of the park, from the sample. In column (3), I leave out the last two post years (2017 and 2018), which does not alter estimation results for Panels A and B. The BMI* coefficients, however, react more sensitively, but estimates do not change in terms of sign or significance level. In column (4) I restrict the sample to children who are of compulsory school age to exclude those children with deferred school enrolment from the analysis.[35] Restricting the sample hardly changes the coefficients. The same holds for restricting the sample to only 5 year old children in column (5) in order to base the analysis on an even more homogeneous group. Column (6) only includes children who are born in Germany to reduce the potential measurement error in the treatment variable when categorizing children incorrectly into treatment and control group by assuming they have lived at the same residence at the time of the examination for all of their lives. As coefficients do not change much, this suggests that this does not play a big role for my analysis. Excluding treatment outliers (5 percent highest and lowest BMI values) causes the effects for the probability to be overweight and obese at the upper tail of the BMI distribution become smaller, but still remain statistically significant and negative. In column (7), I show that my estimates are robust against accounting for LOR-linear time trends, and column (8) executes a placebo test, assuming that the park opening was in 2008 and drops all post years after 2009 from the analysis (following Lechner (2010)). I also choose 2008, thus the year of airport closure, as the placebo treatment year to test whether the area around the former airport was worse-off prior to conversion in terms of children's health due to negative effects of traffic and pollution by the airport. However, if airport closure was the mechanism driving my findings, I would expect statistically significant effects differently from zero, which I do not find. In addition to this reassuring placebo year exercise, I implement three placebo regressions on outcomes that should be unaffected by park opening (body height, healthy teeth and whether or not children bring the preventive medical care booklet to the examination). Figure 4-13 in the Appendix shows that I cannot detect a statistically significant effect different from zero for any of these outcomes.

[35] As school readiness advice is not binding, parents can decide to file an application for deferred school enrollment. For those children, there has been a change in examination proceedings. Before 2013, children would have been examined twice in case of a deferred school start. Starting in 2013, children without any developmental issues are no longer examined twice. For the year 2013, the basic reports show for all variables, including weight factors, statistics for both proceedings and there are only negligible differences.

As a last sensitivity check, I show several alternative BMI specifications in Table 4.9 in the Appendix (BMI Thresholds based on (Kromeyer-Hauschild et al., 2001), the raw BMI score, logarithmic BMI and weight measured in kilogram), all confirming the beneficial park-induced impact on weight factors.

4.6.3 Statistical inference

In this paper, I use standard errors clustered at the level of treatment assignment to conduct inferences, and thus, the geographical LOR unit level to correct for likely serial correlation as common in empirical work and proposed by Bertrand et al. (2004), Colin Cameron and Miller (2015) or Abadie et al. (2017). However, clustering at treatment level relies on the assumption that the number of children within a LOR and/or the number of LORs grow large. This assumption does not hold in my setting since treatment is linked to geographic proximity around the park which only entails one treated cluster. Conley and Taber (2011) show that clustered standard error methods fail with only one treated unit in DiD estimation, and the cluster–robust variance matrix estimator can be severely biased. As outlined by Roodman et al. (2019), Mackinnon and Webb (2017) show that also the wild cluster bootstrap fails and propose in MacKinnon and Webb (2018) a sub-cluster bootstrap.

Table 4.7: Exploring p-values.

	Overweight (1)	**Obesity** (2)	**BMI*** (3)
Main			
Main Estimate	-.043	-.015	-.171
Cluster-robust Standard Error	0.007	0.003	0.0270
t-statistic	-6.27	-4.63	-6.36
p-value	0.000	0.000	0.000
Bootstrap Exercise as in Roodman et al. (2019):			
Bootstrap by LOR, Rademacher Weights, Restricted	0.387	0.386	0.328
Bootstrap by LOR, Rademacher Weights, Unrestricted	0.000	0.001	0.000
Bootstrap by LOR, Mammen Weights, Restricted	0.344	0.323	0.317
Bootstrap by LOR, Mammen Weights, Unrestricted	0.000	0.000	0.000
Bootstrap by LOR, Webb Weights, Restricted	0.435	0.415	0.391
Bootstrap by LOR, Webb Weights, Unrestricted	0.000	0.000	0.000
Bootstrap by LOR-year, Rademacher Weights, Restricted	0.068	0.093	0.093
Bootstrap by LOR-year, Rademacher Weights, Unrestricted	0.000	0.025	0.028
Bootstrap by Individual, Rademacher Weights, Restricted	0.086	0.191	0.071
Bootstrap by Individual, Rademacher Weights, Unrestricted	0.087	0.198	0.071

Notes: This table reports for each main outcome, after repeating the main results from the preferred specification in Table 4.4, p-values after conducting several bootstrap variants varying the bootstrap clustering and whether the null hypothesis is imposed (restricted vs. unrestricted) as indicated in Table and proposed by Roodman et al. (2019) using 10000 replications for each test.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

I follow the empirical example conducted by Roodman et al. (2019) and apply six variants of the wild cluster bootstrap to my data. In addition, I add four more variants to also look at Mammen (1993) (Mammen) and Mackinnon and Webb (2017) (Webb) weights in order to test the hypothesis that the park opening does not impact nearby living children's weight status. For each bootstrap and weight type, I show p-values from imposing this null hypothesis and from not imposing it in Table 4.7. First, I bootstrap by LOR and weights by Rademacher, Mammen and Webb, restricted and unrestricted. Second, I use only Rademacher weights[36] and change bootstrap errors to LOR-year combination, and third, refrain from clustering, thus, cluster by individual. This exercise confirms Mackinnon and Webb (2017) and MacKinnon and Webb (2018)'s theory that the wild cluster unrestricted bootstrap test strongly rejects irrespective of the weight type applied and for all three outcomes, but the wild cluster restricted bootstrap test does not reject when clustering by LOR. Clustering the bootstrap errors by LOR-year brings both tests closer together, while individual (no) clustering reveals almost the same p-values. Figure 4-14 in the Appendix plots the simulated distributions for the six variants based on Rademacher weights with the vertical line marking the actual t-statistic from my main estimate. The upper graphs show the distributions when the null is imposed (bimodal in case of LOR bootstraps) and the lower ones for when it is not imposed (unimodal and narrow for LOR bootstraps). It further shows that bootstrapping with finer clustering results in distributions that are of similar shape irrespective of the restriction imposed or not.

Following this reasoning, I estimate my main effects with individual clustering in Table 4.10 and find that the effects for being overweight as well as for the overall BMI* are still statistically significant in my preferred specification at the five percent level (Panel A) and at the one percent level (Panel C). The probability of being obese, however, is no longer significant when not clustering standard errors at the LOR level.

[36]The argumentation stays the same when using either Mammen or Webb weights for the other bootstrap types.

4.7 Further results

This section presents different sets of additional results on effect differences by (i) gender, (ii) cultural background, (iii) childcare usage and (iv) mothers' education level (see Figure 4-7 and Table 4.11 in the Appendix).

4.7.1 Gender-specific results

From a policy perspective, gender-specific effects are interesting, as studies have shown that heavy women tend to be more prone to adverse labor market outcomes than overweight men (e.g. Mocan and Tekin, 2013).[37] Furthermore, I want to know whether providing green space in an urban environment affects girls and boys differently, since early child health might affect skill formation as studies have shown. For example, Cawley and Spiess (2008) find with data from the German Socio-Economic Panel that among girls aged 2-4, obesity is associated with reduced verbal skills. Figure 4-7a reveals that the effects for all three outcomes are entirely driven by girls. Treated girls have a 8.9 pp lower probability to be overweight after park opening (statistically significant at the one percent level), while for boys the effect is 0.1 pp and statistically insignificant. This is in line with Cawley et al. (2007) who find from evaluating an intervention program that raising PE time induces (high-school) girls to more physical activity.

[37] For Germany, Caliendo and Gehrsitz (2016) show that wage penalties for overweight and obese women are observable in white-collar occupations.

4.7.2 Cultural background-specific results

By stratifying for cultural descent in Figure 4-7b, I find that children with foreign background benefit more from the GS increase compared to children with German background. Their probability to be overweight decreases by 6.1 pp (compared to 2.2 pp for German children) and by 1.6 pp to be obese (compared to 0.3 pp). The BMI* decreases by 0.181 SD for children with foreign descent and by 0.160 SD for German children. This finding is of particular policy interest as studies have shown, in Germany, children are at a higher risk of being overweight or obese if they have a migration background (Kurth and Schaffrath Rosario, 2007). Hence, if these children react more strongly to increases in neighbouring GS, this insight is highly informative for urban planning.

4.7.3 Childcare-specific results

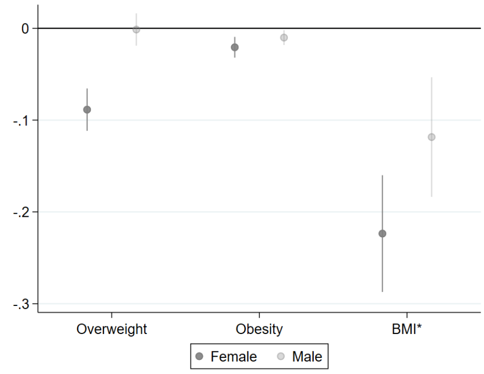
For the subgroups stratified by duration of childcare use (see Figure 4-7c), effect sizes differ whether children have stayed more or less than two years in childcare. Treated children with more than two years of childcare exposure have a 4.2 pp lower probability to be overweight induced by park opening and for treated children with less than or equal to two years of childcare this effect size is larger at 7.5 pp, both statistically significant at the one percent level and also statistically different from each other. This finding suggests that recreational space may be even more important for children with lower childcare duration intensity.

4.7.4 Maternal education-specific results

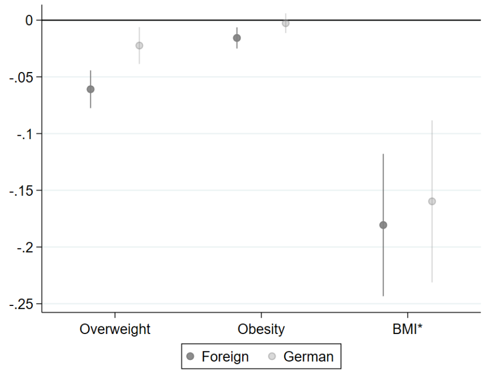
Stratifying by mothers' education background[38], i.e. whether or not they have undergone tertiary education, is of particular interest, as Schienkiewitz et al. (2018) among others have shown strong social gradients for children with low socioeconomic status. In this sample, I find that treated children with a higher educated mother benefit slightly more from expanding GS (-4.5 pp for being obese, and -0.20 SD BMI*) than treated children with a lower educated mother (-2.7pp and -0.14 SD, respectively). However, the probability to be obese is more affected by children with low education background. This suggests that for the very upper tail of the distribution (BMI $>$ 97 P.), children from a lower educational background are slightly more affected by GS exposure.

[38] I do not have information on parents' income, but maternal education status serves as well-established proxy for children's socio-economic status.

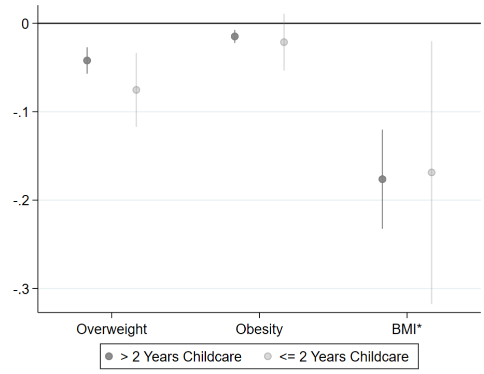
Figure 4-7: Effect Heterogeneity Analysis by



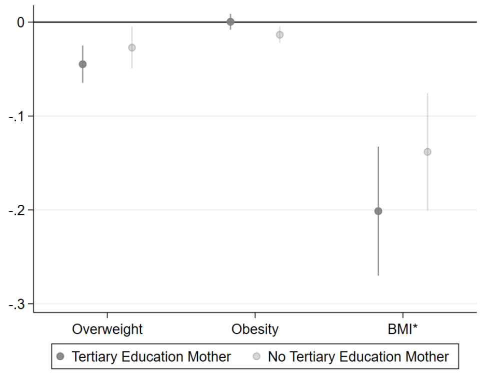
(a) Gender



(b) Cultural Background



(c) Childcare Use



(d) Maternal Education

Notes: The figure shows effect heterogeneity estimates for four binary stratification variables (gender in Figure 4-7a, cultural background in Figure 4-7b, childcare use for more than 2 years in Figure 4-7c and for children of mothers with or without tertiary education in Figure 4-7d) for each main outcome together with 95 percent confidence intervals.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

4.8 Discussion and conclusion

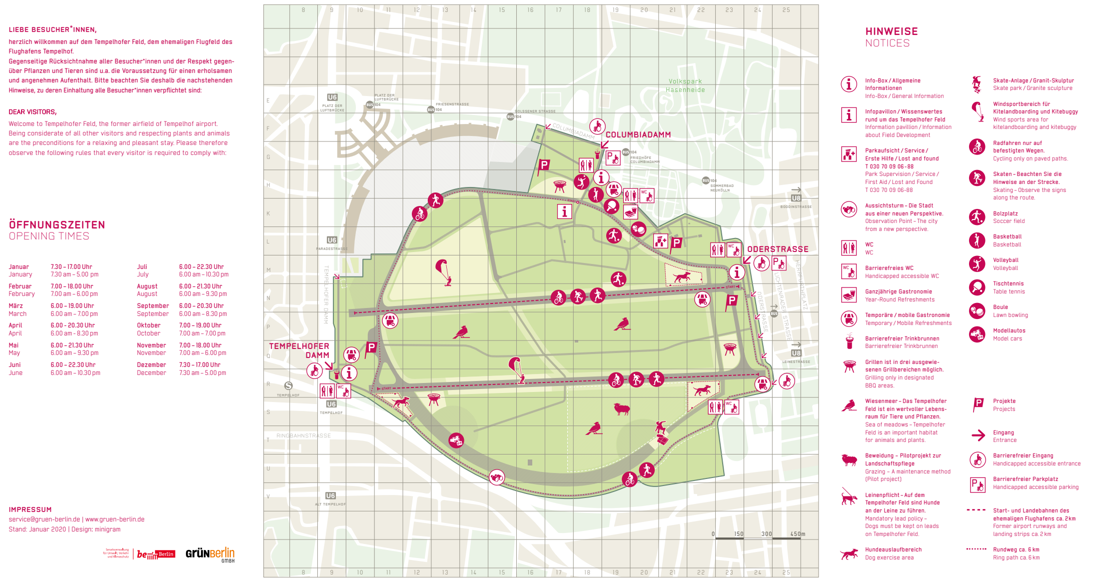
In its recent study "The heavy burden of obesity" the OECD (2019) reports that Germany has one of the largest rate of health expenditures spent on overweight and associated conditions accounting for 10.7 percent of overall health expenditure. They further estimate that due to obesity-induced lower labor market outputs, Germany's GDP is reduced by 3 percent, which translates to an additional 431 Euro for each German taxpayer per year. Thus, the research question of how opening vast green space in an urban environment affects children's weight outcomes raised in this paper is highly relevant for policy-makers trying to tackle the obesity epidemic and reducing health care costs.

In this paper, I present novel administrative data on the full universe of school-starting-aged children from Germany's capital's third largest district, including objective assessment of the outcome variables. Furthermore, the data allows me to control for a rich set of covariates on the individual, family, as well as on the small area geographic level around children's residence. I detect a plausibly causal and robust pathway of opening vast green space in an urban environment, finding reductions in being overweight for treated children. These effects are entirely driven by girls and mainly by children from other cultural backgrounds, the latter group having been recognized as a high-risk group for weight-related health problems. I also find a small social gradient effect for a lower probability to be obese for children of mothers with no tertiary education. In sum, these results add up to supportive evidence that greening cities may contribute to effective obesity policy prevention.

In the face of current global challenges, such as climate change and pandemics, the necessity for urban greening policies in order to establish more resilience are frequently debated (Lambert et al., 2020; UNESCO, 2020). Helbig and Salomo (2021) add a social justice perspective to this discussion by showing for seven big cities in Germany that opportunities for participation and public infrastructure distribute differently dependent on socio-economic dynamics. With respect to green space, they find that in almost all analysed cities, including Berlin, districts that are socially more privileged also have more recreational green space per child. In addition to this perspective, Brenck et al. (2021) hold urban planning responsible to prepare for the future not only in terms of countering segregation, but also fostering health prevention and protection. This paper corroborates that the provision of public green space in densely populated urban areas can serve both goals simultaneously.

4.9 Appendix

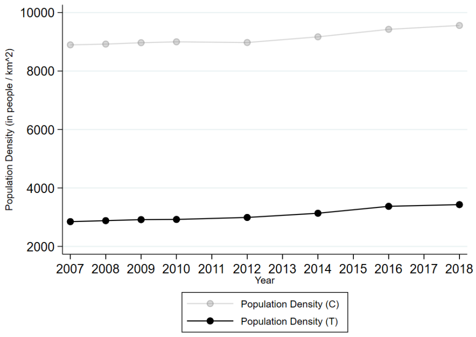
Figure 4-8: The Tempelhofer Feld Park Map



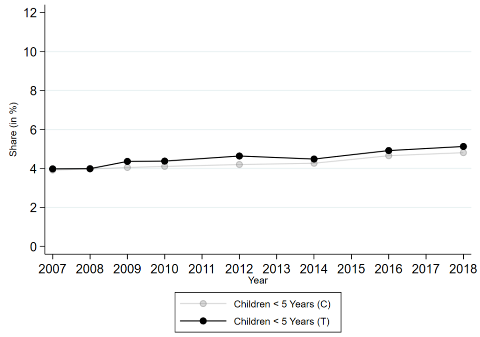
Notes: This visitor map gives an overview of the park, its entrances and provides some further information for users.
Source: The map is provided online by the park's operating company "Grün Berlin" and can be downloaded at https://gruen-berlin.de/fileadmin/user$_{u}$pload/Downloads/tempelhofer-feld/gruenberlin$_{p}$rojekte$_{p}$arks$_{t}$empelhoferfeld$_{p}$arkplan.pdf (last accessed: 07/01/2022)

Figure 4-9: LOR Controls over Time

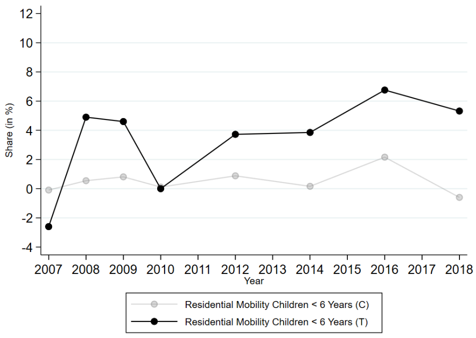
(a) Population Density



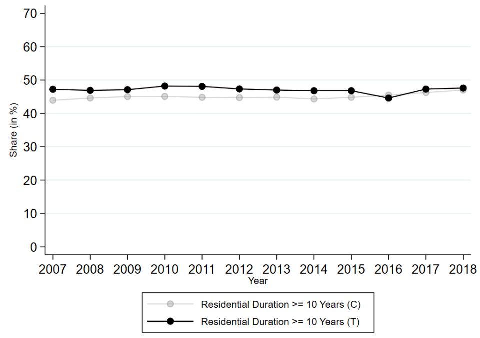
(b) Share of Children



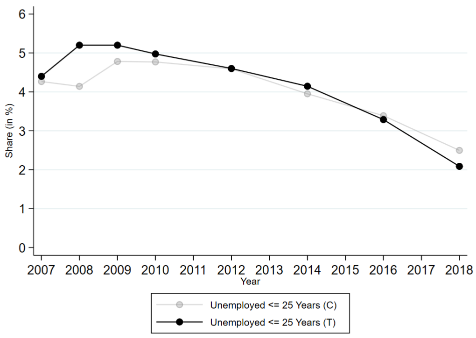
(c) Residential Mobility Children



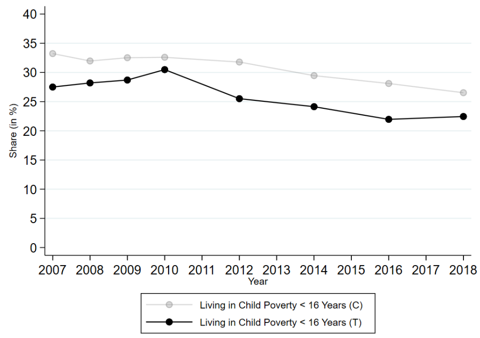
(d) Residential Duration



(e) Youth Unemployment

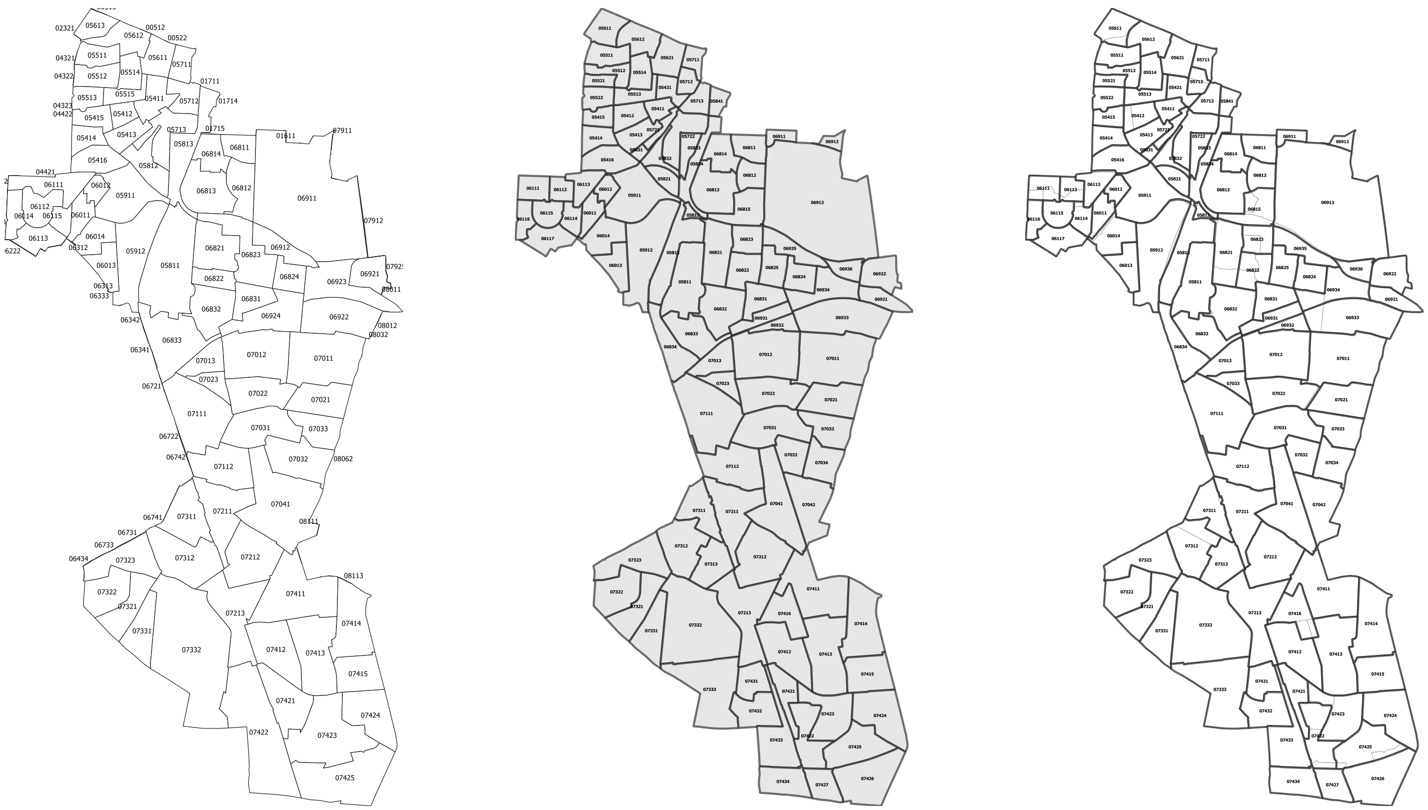


(f) Child Poverty



Notes: All six graphs plot LOR control means over time. Figure 4-9a displays the population density (measured in people/m^2), while Figure 4-9b shows the share of children below 5 years of age. Figures 4-9c and 4-9d display the influx share of children below 6 years (children moving in - children moving out of LOR) and the share of residents living at least 10 years at the same address. Figures 4-9e plots youth unemployment means, measured as share of unemployed 15-25 year old adolescents, and child poverty in Figure 4-9f, measured as the share of children below age 15 who live in households that receive transfer payments relative to the population below age 15.
Source: See Table 4.8 for more details on each indicator and respective data source.

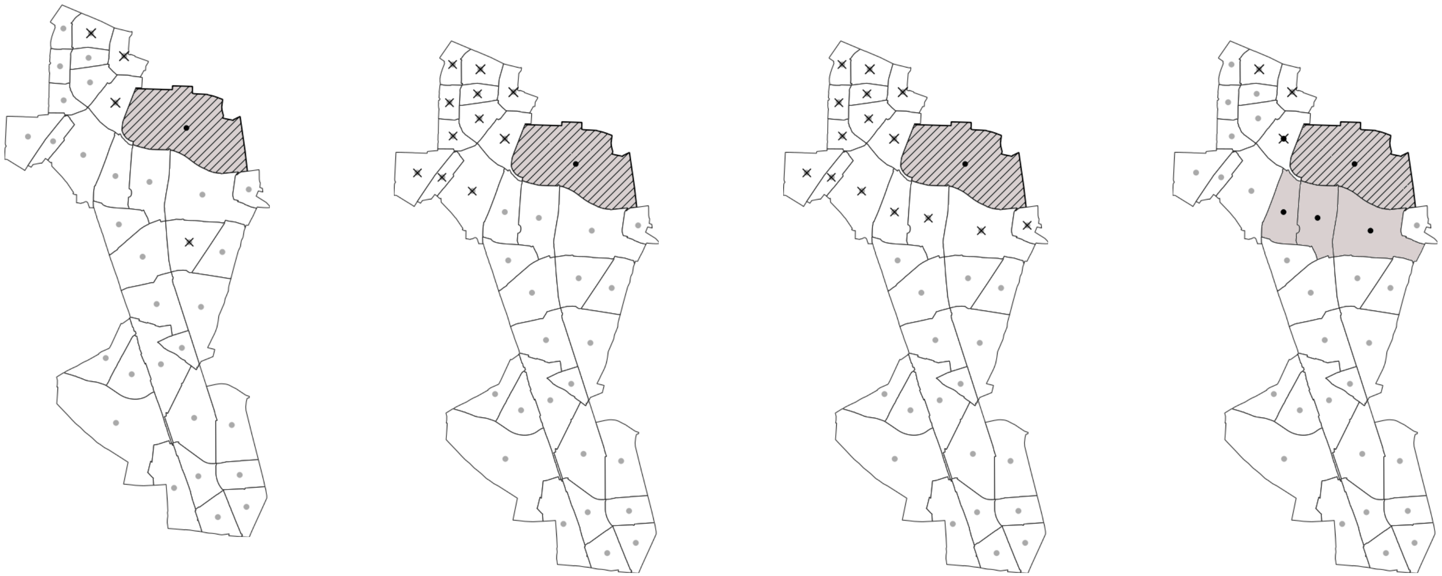
Figure 4-10: Different Control Groups: Visualization of TVZ Analysis



(a) TVZ System (881) <= 2009 (b) TVZ System (1223) > 2009 (c) TVZ Overlay

Notes: These figures plot the smaller geographical unit of children's residence in "Teilverkehrszellen" (TVZ) used for a robustness check in Table 4.5 in column (9). Figure 4-10a shows the TVZ system in place until 2009, Figure 4-10b the system after 2009 and Figure 4-10c plots the overlay of both systems. Own graphical display.
Source: SenUMVK, Statistical Office Berlin-Brandenburg
https://www.berlin.de/sen/uvk/verkehr/verkehrsdaten/teilverkehrszellen-und-verkehrszellen/ (last accessed: 09/13/2022)

Figure 4-11: Different Control Groups - Visualization of Geographic Sensitivity Analysis



(a) - Deter. Green LOR (1)

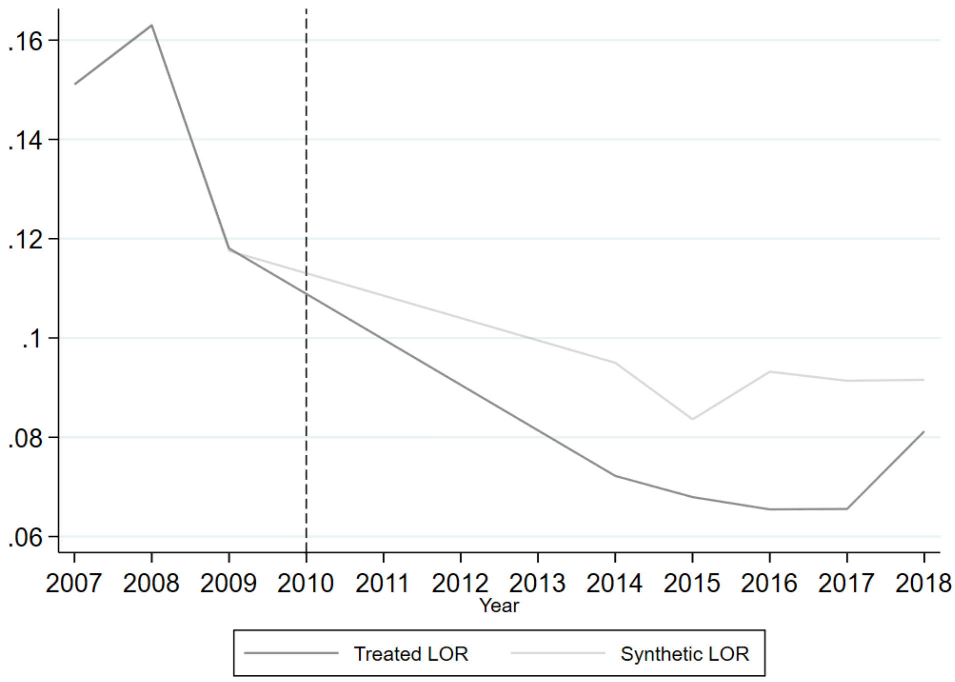
(b) Only T (2)

(c) Only T + Buffer (3)

(d) Dist <=2500m (4)

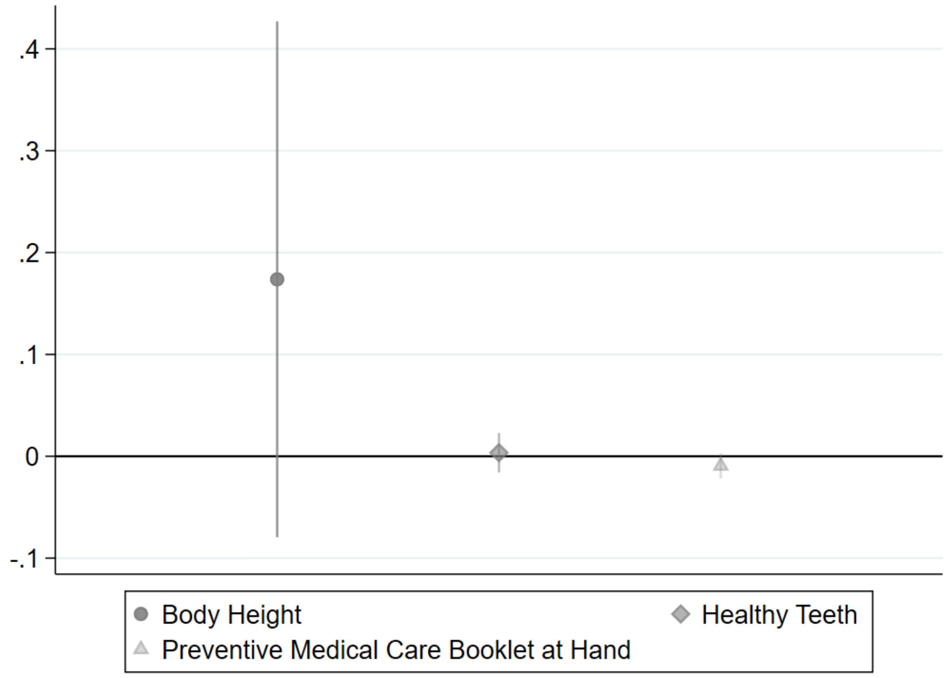
Notes: These figures visualize alternative control and treatment group choices tested in Table 4.5 in columns (2)-(5). The "X" marks LOR districts that are excluded from the analysis. The ruled area marks the LOR that includes the Tempelhofer Feld park, while the shaded area along with the black dot shows the applied treatment area. Own graphical display.
Source: LOR Vector Data, Statistical Office Berlin-Brandenburg,
https://www.stadtentwicklung.berlin.de/planen/basisdaten$_s$tadtentwicklung/lor/de/download.shtml (last accessed: 09/13/2022).

Figure 4-12: Synthetic Control: Overweight



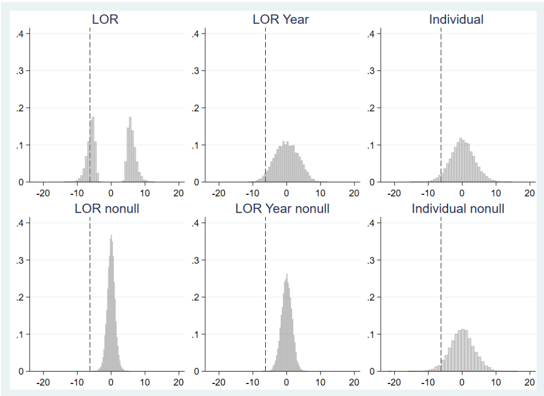
Notes: This figure visualizes the analysis conducted in Table 4.5 by searching for a weighted combination of control units from the donor pool of the time-varying LOR covariates and the two pre-treatment means of the respective outcome (in this graph: being overweight). The dotted line marks the time of the intervention, which lies in the time frame between 2009 and 2014 for which I do not have data. The difference between the two graphs marks the estimated treatment effect.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

Figure 4-13: Placebo Outcomes

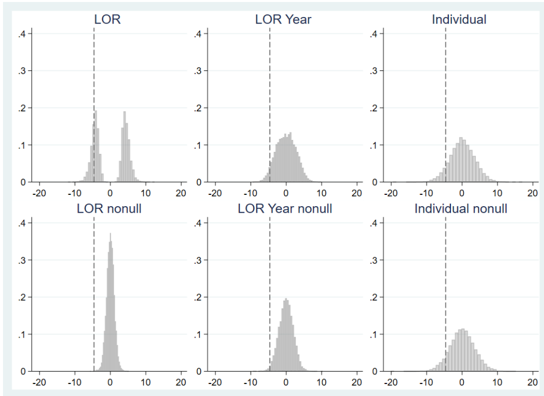


Notes: This figure visualizes estimation results from Placebo regressions using pre-determined outcomes, i.e. body height (circle), the probability of having healthy teeth (diamond) and whether or not the medical care booklet was at hand at the time of the examination (triangle). Each coefficient is from a separate regression and shown with 95 percent confidence intervals.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

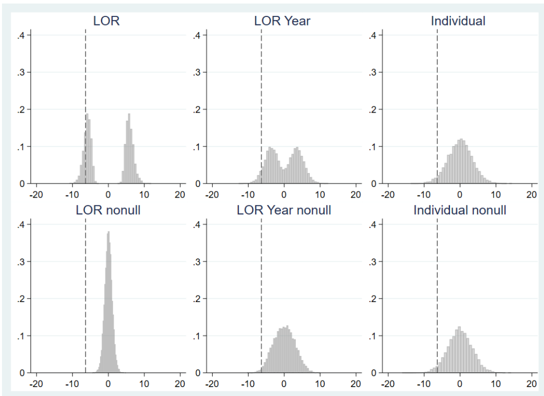
Figure 4-14: Wild Bootstrap Distributions of t-statistic



(a) Overweight



(b) Obesity



(c) BMI*

Notes: Figure 4-14 plots for each main outcome the simulated wild bootstrap distributions of t-statistic for six variants based on Rademacher weights, using three different bootstrap types (clustering by LOR, LOR-year or individual or no clustering) with the vertical line marking the actual t-statistic from my main estimate. The upper graphs of each subfigure show the distributions when the null hypothesis is imposed and the lower ones for when it is not imposed. *Source:* ESU Berlin Tempelhof-Schöneberg, main estimation sample.

Table 4.8: LOR Characteristics

LOR Indicator	**Year (Indicator)**	**Data Source**
Population Density		
Population / Area in m^2	2007, 2008, 2009, 2010, 2012, 2014, 2016, 2018	MSS Berlin
Share of Children		
(Population < 5 Years) / (Population) * 100	2007 (EINW5), 2008 (EINW5), 2009 (EINW5), 2010 (EINW5), 2012 (EINW5), 2014 (EINW5), 2016 (EINW5), 2018 (EINW5)	Berlin Open Data
Residential Mobility Children		
(Children < 6 Years moving in LOR) - (Children < 6 Years moving out LOR) / (Population < 6 Years) * 100	2007 (D3), 2008 (D3), 2009 (D3), 2010 (K13), 2012 (K13), 2014 (K13), 2016 (K13), 2018 (K13)	Berlin Open Data
Residential Duration		
Population Living >= 10 Years at Same Address / Population >= 5 Years * 100	2007 (PDAU10), 2008 (PDAU10), 2009 (PDAU10), 2010 (PDAU10), 2012 (PDAU10), 2014 (PDAU10), 2016 (PDAU10), 2018 (PDAU10)	Berlin Open Data
Youth Unemployment		
Unemployed (15-25 Years) / Population (15-65 Years) * 100	2007 (S2), 2008 (S2), 2009 (S2), 2010 (S2), 2012 (K01), 2014 (K01), 2016 (K01), 2018 (K01)	MSS Berlin
Child Poverty		
Transfer-Recipients < 15 Years / Population < 15 Years * 100	2007 (S5), 2008 (S5), 2009 (S5), 2010 (S5), 2012 (S4), 2014 (S4), 2016 (S4), 2018 (S4)	MSS Berlin

Notes: Own compilation and display. For further details on indicators, see SenStadtUm (2016).
Source: Statistical Office Berlin-Brandenburg. Data provided by
MSS Berlin
https://www.stadtentwicklung.berlin.de/planen/basisdaten$_s$tadtentwicklung/monitoring/index.shtml (last accessed: 09/13/2022)
Berlin Open Data
https://daten.berlin.de/datensaetze (last accessed: 09/13/2022)

Table 4.9: Alternative BMI-Thresholds and Additional Weight Outcomes

	(1)	(2)	(3)	(4)
Panel A: Overweight				
Reference Percentiles: Kromeyer-Hauschild et al. (2001)				
GS Effect	-0.035***	-0.033***	-0.032***	-0.034***
	(0.007)	(0.007)	(0.007)	(0.007)
Panel B: Obesity				
Reference Percentiles: Kromeyer-Hauschild et al. (2001)				
GS Effect	-0.015***	-0.015***	-0.014***	-0.014***
	(0.003)	(0.003)	(0.003)	(0.003)
Panel C: BMI*				
Reference Percentiles: Kromeyer-Hauschild et al. (2001)				
GS Effect	-0.139***	-0.136***	-0.139***	-0.154***
	(0.026)	(0.026)	(0.029)	(0.024)
Panel D: BMI				
GS Effect	-0.263***	-0.256***	-0.259***	-0.288***
	(0.046)	(0.046)	(0.051)	(0.042)
Panel E: log. BMI				
GS Effect	-0.016***	-0.015***	-0.016***	-0.017***
	(0.003)	(0.003)	(0.003)	(0.003)
Panel F: Weight (in kg)				
GS Effect	-0.211***	-0.196**	-0.354***	-0.396***
	(0.075)	(0.074)	(0.070)	(0.059)
LOR FE	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes
Child and Family Controls	No	No	Yes	Yes
Time-Varying LOR Controls	No	No	No	Yes

Notes: The table shows the green space effect of the Tempelhofer Feld transformation for several alternative outcome specifications; BMI Thresholds based on Kromeyer-Hauschild et al. (2001) in Panel A, B and C; the raw BMI score in Panel D; logarithmic BMI in Panel E, and weight measured in kilogram in Panel F for individuals based on Equation (4.2). Standard errors clustered at the LOR district level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

Table 4.10: Effect of Green Space on Overweight, Obesity and BMI* using no (Individual) Clustering

	(1)	(2)	(3)	(4)
Panel A: Overweight				
No Clustering	-0.042**	-0.040**	-0.039**	-0.043**
	(0.019)	(0.019)	(0.018)	(0.018)
Panel B: Obesity				
No Clustering	-0.016	-0.015	-0.014	-0.015
	(0.012)	(0.012)	(0.012)	(0.012)
Panel C: BMI*				
No Clustering	-0.155**	-0.152**	-0.155**	-0.171***
	(0.067)	(0.066)	(0.063)	(0.064)
LOR FE	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes
Child and Family Controls	No	No	Yes	Yes
Time-Varying LOR Controls	No	No	No	Yes

Notes: The table shows the green space effect of the Tempelhofer Feld transformation for individuals based on Equation (4.2) with standard errors clustered at the individual level (thus, no clustering) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

Table 4.11: Effect Heterogeneity Analysis

	Gender		**Cultural Background**		**Childcare Use**		**Maternal Education**	
	Female	Male	Foreign	German	>2 Years	<=2 Years	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Overweight								
GS Effect	-0.089***	-0.001	-0.061***	-0.022***	-0.042***	-0.075***	-0.045***	-0.027**
	(0.011)	(0.009)	(0.008)	(0.008)	(0.007)	(0.020)	(0.010)	(0.011)
N	9,500	10,493	8,673	10,434	17,537	2,311	9,109	9,667
p-value		0.014		0.000		0.000		0.000
Panel B: Obesity								
GS Effect	-0.021***	-0.010**	-0.016***	-0.003	-0.015***	-0.021	0.000	-0.013***
	(0.006)	(0.004)	(0.005)	(0.004)	(0.004)	(0.016)	(0.004)	(0.004)
N	9,500	10,493	8,673	10,434	17,537	2,311	9,109	9,667
p-value		0.162		0.000		0.032		0.190
Panel C: BMI*								
GS Effect	-0.224***	-0.118***	-0.181***	-0.160***	-0.176***	-0.169**	-0.201***	-0.138***
	(0.031)	(0.032)	(0.031)	(0.035)	(0.027)	(0.073)	(0.034)	(0.031)
N	9,500	10,493	8,673	10,434	17,537	2,311	9,109	9,667
p-value		0.015		0.001		0.000		0.000

Notes: The table shows effect heterogeneity estimates for four binary stratification variables; gender in columns (1)-(2), cultural background in columns (3)-(4), childcare use (more or less/equal than 2 years) in columns (5)-(6) and maternal education (high means that mother has a tertiary education, low means she does not) in columns (7)-(8). It further presents p-values of a t-test for the difference in the effects between the respective groups in columns (2), (4), (6), (8). Standard errors clustered at the LOR district level in parentheses. See also Figure 4-7 for graphical display of the group estimates.
Source: ESU Berlin Tempelhof-Schöneberg, main estimation sample.

Summary

This dissertation compounds three papers with independent contributions to the literature on early childhood education and care, education and environmental economics. All three papers aim at identifying arguably causal relationships by tying certain policy reforms to various unintended outcomes with the help of a wide range of microeconometric evaluation tools and different data sets (e.g., survey data from the National Educational Panel Study (NEPS), the German Socio-Economic Panel (SOEP), Micro Census data and novel administrative data from school entrance examinations). The first paper exploits the policy framework of high geographic variation in childcare supply and resulting cut-off rules for childcare entry at the time of introducing a legal claim of a childcare spot for 3-6 year-olds in Germany in 1996. It finds that an earlier entry in universal childcare increases extroversion in adolescence as part of children's non-cognitive skills set in the long-run. The second paper uses exposure to the policy reform of harmonizing the German school year by introducing short school years in 1966-67. Being exposed to short school years affects the timing of marriage for individuals in all secondary school tracks and shifts forward the birth of the first child mainly for academic-track graduates. This shows that education policies might not only affect family formation through human capital accumulation, but also through changing the duration of earlier life phases. The third paper exploits the policy of transforming former airport grounds into a large urban green space in Germany's capital city in 2010. It provides evidence that expanding urban green space decreases the probability for school-starting children to be overweight as a result of park opening, driven entirely by girls, mainly by children from foreign cultural backgrounds and children with less childcare exposure. For the probability of being obese, the paper finds small effects for children with lower maternal education background. The findings of this dissertation of unintended, yet effective consequences from policy reforms help to inform current and future political decision-making.

Keywords: early childcare, non-cognitive skills, personality traits, family formation, instruction time, fertility, marriage, urban planning, children, health, overweight, inequality

JEL-Classification: I12, I14, I18, I21, I26, J12, J13, J18, J24, R58

Zusammenfassung (Summary in German)

Diese Dissertation beinhaltet drei Aufsätze, die jeweils unabhängige Beiträge zur ökonomischen Literatur in den Bereichen der frühkindlichen Bildung, Sekundarbildung und Umwelt leisten. Alle drei Aufsätze zielen darauf ab, mit Hilfe einer Vielzahl von mikroökonometrischen Methoden und Datensätzen (z.B. Umfragedaten des Nationalen Bildungspanels (NEPS), des Sozio-oekonomischen Panels (SOEP), Mikrozensusdaten und neuen administrativen Daten aus Schuleingangsuntersuchungen) kausale Zusammenhänge zu identifizieren und dabei unvorhergesehene Auswirkungen von Politikreformen zu quantifizieren. Der erste Aufsatz nutzt die regional stark variierende Verfügbarkeit von Kinderbetreuungsplätzen zum Zeitpunkt der Einführung des Rechtsanspruchs auf einen Betreuungsplatz für 3-6-Jährige in Deutschland im Jahr 1996 aus. Er stellt heraus, dass Kinder, die abhängig des regionalen Platzangebots früher eine Kindertageseinrichtung (Kita) besuchen können als andere, noch im Jugendalter Unterschiede in den Persönlichkeitseigenschaften, insb. Extravertierheit, aufweisen. Der zweite Aufsatz befasst sich mit der politischen Reform zur Harmonisierung des deutschen Schuljahres durch die Einführung von Kurz-Schuljahren 1966-67 in Deutschland. Er legt dar, dass die von der kürzeren Schulzeit Betroffenen aller Schulzweige den Zeitpunkt der Eheschließung nach vorne verschieben, während die Geburt des ersten Kindes auf Grund der kürzeren Schulzeit hauptsächlich für Schülerinnen und Schüler aus dem Gymnasialzweig früher erfolgt. Dies zeigt, dass die Bildungspolitik die Familiengründung möglicherweise nicht nur durch die Akkumulation von Humankapital beeinflusst, sondern auch durch die Dauer früherer Lebensphasen. Der dritte Aufsatz untersucht bislang unentdeckte Folgen der Umwandlung eines ehemaligen Flughafengelände zu einer großen urbanen Grünfläche, dem "Tempelhofer Feld", in Berlin im Jahr 2010. Es weist nach, dass die Ausweitung städtischer Grünflächen die Wahrscheinlichkeit von Übergewicht bei Schulanfänger*innen verringert. Die Effekte gehen beinahe ausschließlich auf Mädchen zurück und vor allem auf Kinder mit nicht-deutschem kulturellem Hintergrund sowie Kinder mit geringerem Kinderbetreuungsbedarf. Für die Wahrscheinlichkeit, fettleibig zu sein, zeigen sich kleine Effekte, die von Kindern mit einem niedrigeren mütterlichen Bildungshintergrund getrieben werden. Die Ergebnisse dieser Dissertation zu unbeabsichtigten, aber effektiven Konsequenzen politischer Reformen tragen dazu bei, bestehende Wissenslücken zu Auswirkungen von Politikreformen zu schließen und dabei die Grundlage zukünftiger politischer Entscheidungen zu erweitern.

Keywords: Frühe Bildung, Nicht-kognitive Fähigkeiten, Persönlichkeitsmerkmale, Familienplanung, Schuldauer, Fertilität, Eheschließung, Urbane Planung, Kinder, Gesundheit, Übergewicht, Ungleichheit

JEL-Classification: I12, I14, I18, I21, I26, J12, J13, J18, J24, R58

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Declarations

Erklärung

Hiermit erkläre ich, Josefine Koebe, dass ich keine kommerzielle Promotionsberatung in Anspruch genommen habe. Die Arbeit wurde nicht schon einmal in einem früheren Promotionsverfahren angenommen oder als ungenügend beurteilt.

Bensheim, den 30. September, 2022

J. Koebe

Josefine Koebe

Eidesstattliche Versicherung

Ich, Josefine Koebe, versichere an Eides statt, dass ich die Dissertation mit dem Titel: „Unintended Yet Effective: Evidence from Policy Reforms in Early Childhood Education and Care, Education and Environmental Economics“ selbst und bei einer Zusammenarbeit mit anderen Wissenschaftlerinnen oder Wissenschaftlern gemäß den beigefügten Darlegungen nach §6 Abs. 3 der Promotionsordnung der Fakultät für Wirtschafts- und Sozialwissenschaften vom 18. Januar 2017 verfasst habe. Andere als die angegebenen Hilfsmittel (Microsoft Office, Latex, Stata, QGIS, R, Mendeley und die im Quellenverzeichnis angegebene Literatur) habe ich nicht benutzt.

Bensheim, den 30. September, 2022

J. Koebe

Josefine Koebe

Selbstdeklaration

Kapitel 1

Die Eigenleistung von Josefine Koebe für die in Kapitel 1 präsentierte Einleitung liegt bei 100%.

Kapitel 2

Kapitel 2 ist in Koautorenschaft mit Maximilian Bach und Frauke Peter verfasst. Die folgende Einschätzung in Prozent über die von Josefine Koebe erbrachte Eigenleistung wurde mit den am Artikel beteiligten KoautorInnen einvernehmlich abgestimmt.

- Konzeption: 30%
- Durchführung: 40%
- Manuskripterstellung: 50%

Kapitel 3

Kapitel 3 ist in Koautorenschaft mit Jan Marcus verfasst. Die folgende Einschätzung in Prozent über die von Josefine Koebe erbrachte Eigenleistung wurde mit Jan Marcus einvernehmlich abgestimmt.

- Konzeption: 40%
- Durchführung: 90%
- Manuskripterstellung: 60%

Kapitel 4

Die Eigenleistung von Josefine Koebe für die in Kapitel 4 präsentierte Arbeit liegt bei 100%.

Bensheim, den 30. September, 2022

Josefine Koebe

Publikationsliste

Kapitel 2

Bach, M., Koebe, J. and F. Peter (2019). Long Run Effects of Universal Childcare on Personality Traits. *DIW Discussion Paper 1815*.

Kapitel 3

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