Essays on the impact of education and family policies on the formation of human capital

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Contents

List of Tables

List of Figures

Rechtliche Erklärung

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Hiermit erkläre ich, dass ich mich noch keinem Promotionsverfahren unterzogen oder um Zulassung zu einem solchen beworben habe, und die Dissertation in der gleichen oder einer anderen Fassung bzw. Überarbeitung einer anderen Fakultät, einem Prüfungsausschuss oder einem Fachvertreter an einer anderen Hochschule nicht bereits zur Überprüfung vorgelegen hat.

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Huebener, M. & Marcus, J. (2015). G8 high school reform results in higher grade repetition rates and lower graduate age, but does not affect graduation rates. *DIW Economic Bulletin*, 18/2015, Deutsches Institut für Wirtschaftsforschung.

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Kapitel 5: Intergenerational effects of education on risky health behaviours and long-term health

- Kein Koautor
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Abstract

This dissertation analyses whether policy-makers can impact the formation of human capital and the emergence of differences in human capital. Four independent research articles empirically analyse the effects of education and family policies on human capital. The dissertation acknowledges that human capital is multidimensional and analyses different dimensions of it, including various skills in early childhood, different measures of student performance in school, as well as the health and health-related behaviours of individuals. It takes a lifecycle perspective on the formation of human capital and analyses reforms that affect individuals at various stages in life: in early childhood, in secondary school, and intergenerationally. The analyses employ modern micro-econometric techniques to various data sets in order to estimate the causal effects of policy interventions. Each of the four research articles makes an independent contribution to the literature on determinants of human capital formation.

Chapter 2 examines the effects of parental leave policies on child development. Parental leave policies support families around childbirth by offering job protection and benefits that compensate for the income losses of child-related work interruptions. Thereby, they affect several conditions in early childhood that may impact child development, such as parental labour supply before and after childbirth, time parents can spend with their children, and household income. A reform in Germany in 2007 both expanded eligibility for paid leave in the first year and removed eligibility for paid leave in the second year following childbirth. Higher-income households benefited relatively more from the reform than low-income households. The chapter analyses the reform effects on child development and substantial, pre-existing socio-economic gaps in child development. The analysis builds on rich administrative data from mandatory school entrance examinations conducted at age six. Eligibility for the new parental leave benefit system was based on children's birthdays, which is exploited by a regression discontinuity approach within a difference-in-differences framework to estimate causal reform effects. The precise and robust estimates reveal no effect of the reform on child development and on socio-economic development gaps therein. Consequently, the chapter concludes that such substantial changes in parental leave benefits are unlikely to considerably impact children's development.

Chapters 3 and 4 focus on two important school input factors, years of schooling and instruction time, and analyse their effects on student performance. Chapter 3 studies the impact of instruction time, an expensive school input factor that is a key lever across education systems. The identification of causal effects builds on an education reform in Germany that reduced the length of academic track schooling by one year, while increasing instruction hours in the remaining school years. The so-called G8-reform serves as a natural experiment for students in lower grade levels to estimate effects of increased instruction time on student performance. For students in grade 9, weekly instruction hours increased by about 6.5 percent over the course of five years. Additional instruction time covered new learning content - a reform feature that is highly relevant for policy-makers, as increases in instruction time are typically accompanied by new learning content. The empirical strategy exploits the fact that the reform was implemented in different years across states. Based on student competence measures in reading, mathematics, and science, as captured by PISA assessments, the empirical findings show that reform-induced increases in instruction time improve student performance on average. However, treatment effects are small and differ across the student performance distribution. Low-performing students benefit less than high-performing students. The content of additional instruction time is important to explain this pattern. Better-performing students cope better with additional content, while lower-performing students may need more time for remediation instead. The findings demonstrate that increases in instruction hours can widen the gap between low- and high-performing students.

Chapter 4 focuses on the substitutability of years of schooling and instruction time in driving student performance. The chapter is based on the G8-education reform that compressed secondary schooling into fewer school years. It reduced the length of academic track schooling by one year, while increasing instruction hours in the remaining school years, such that students are taught a very similar curriculum in a shorter period of time. To estimate the causal effects of the reform, the empirical strategy exploits the time variation in the implementation of the reform across federal states. Using aggregated administrative data on the full population of students, the chapter provides empirical evidence that the reform increases grade repetition rates and lowers final grade point averages, suggesting that affected students have a poorer command of the school material than peers who covered the same material over a longer period of time. These effects are not just transitory in nature, but are also apparent six years after the introduction of the reform. However, there is no evidence of reform effects on graduation rates. Overall, the results suggest that compressing instruction time into fewer years of schooling has adverse effects on student performance, but the economic significance of the effects appears moderate. Some students cope with the increased learning intensity by repeating a school year. The potential costs of the reform due to adverse effects on student performance must be weighed against the economic gains from earlier labour market entries that can raise individuals' lifetime earnings, while also benefiting society by mitigating skilled worker shortages and increasing social security contributions.

Chapter 5 investigates educational differences in risky health behaviours and health that strongly contribute to substantial educational gradients in morbidity, chronic conditions, and longevity of individuals - important dimensions of human capital. A large literature on the causal effects of education on health-related behaviours is still inconclusive and neglects the fact that education is also passed on to the next generation. This chapter estimates the causal effects of parental education on their children's health-related behaviours and health-status. It studies the intergenerational effects of a compulsory schooling reform in Germany after World War II that increased the minimum number of school years from eight to nine. Instrumental variable approaches and difference-in-differences methods exploit variation in the implementation of the reform across federal states. The analysis is based on two independent data sources, the German Micro Census and the German Socio-Economic Panel Study (SOEP). The findings reveal that increases in maternal schooling reduce their children's probability to smoke and to be overweight in adolescence. The effects persist into adulthood, reducing chronic conditions that often result from unhealthy lifestyles, and improving general health. No such effects are identified for paternal education. Increased investments in children's education and improvements in their peer environment early in life are important for explaining the effects. Changes in family income, family stability, fertility, and parental health-related behaviours are also examined, but appear less relevant empirically. The intergenerational effects of education on health and health-related behaviours exceed the direct effects, suggesting that there are substantial non-market benefits to education that also accrue as spill-over effects to the next generation.

Chapters 1 and 6 frame this dissertation. Chapter 1 places the dissertation in the economic literature, outlines the structure and summarises its general contributions. Chapter 6 draws general conclusions and outlines scope for future research.

Zusammenfassung

Diese Dissertation befasst sich mit der Frage, ob Politikmaßnahmen die Bildung von Humankapital beeinflussen können. Vier eigenständige Forschungsaufsätze untersuchen empirisch die Wirkung von bildungs- und familienpolitischen Maßnahmen in Deutschland auf unterschiedliche Dimensionen von Humankapital. Die Bildung von Humankapital wird als ein dynamischer Prozess betrachtet, der sich über den gesamten Lebenszyklus erstreckt. In diesem Sinne betrachtet die Dissertation Politikmaßnahmen, die Individuen zu unterschiedlichen Zeitpunkten im Lebenverlauf betreffen: direkt nach der Geburt, während der Sekundarschulzeit, sowie intergenerationell. In den Analysen kommen moderne mikroökonometrische Verfahren, sowie unterschiedliche Datensätze zum Einsatz, um die kausalen Effekte von Politikmaßnahmen empirisch zu schätzen. Jeder der vier Forschungsaufsätze leistet einen unabhängigen Beitrag zu spezifischen Aspekten der Literatur zur Bildung von Humankapital.

Kapitel 2 untersucht die Effekte von Elternzeitregelungen auf die Entwicklung von Kindern. Elternzeitregelungen unterstützen Familien nach der Geburt, indem sie Eltern geburtsbedingte Erwerbsunterbrechungen ermöglichen, nach denen sie in das Beschäftigungsverhältnis vor der Geburt des Kindes zurückkehren können. Teilweise werden auch Lohnersatzleistungen gewährt, die Einkommensverluste dieser Erwerbsunterbrechungen in unterschiedlichem Umfang ausgleichen. Elternzeitregelungen berühren damit aus Sicht der Kinder verschiedene Faktoren, die ihre frühe Entwicklung beeinflussen könnten, wie z.B. die elterliche Erwerbstätigkeit vor und nach der Geburt des Kindes, die Zeit, die Eltern ihren Kindern widmen können, sowie das Haushaltseinkommen. Eine umfangreiche Reform der Elternzeitregelung in Deutschland hat für Eltern von Kindern ab Januar 2007 den Anspruch auf Lohnersatzleistungen im ersten Jahr nach der Geburt des Kindes ausgeweitet, gleichzeitig aber den Anspruch auf Lohnersatzleistungen im zweiten Jahr nach der Geburt des Kindes reduziert. Von dieser Reform haben Haushalte mit höherem Einkommen relativ stärker profitiert als Haushalte mit geringen Einkommen. Dieses Kapitel untersucht, inwiefern sich die Elterngeldreform auf verschiedene Maße kindlicher Entwicklung ausgewirkt hat. Ein besonderer Fokus liegt dabei auf der Frage, ob sich sozio-ökonomische Ungleichheiten in der kindlichen Entwicklung verstärkt haben, da sozio-ökonomisch besser gestellte Haushalte stärker von der Reform profitierten. Die empirischen Untersuchungen basieren auf Daten von Schuleingangsuntersuchungen des Landes Schleswig-Holstein, in denen Expertinnen und Experten des Kinder- und

Jugendärztlichen Dienstes der Gesundheitsämter standardisierte Untersuchungen mit jedem schulpflichtigen Kind durchführen, die in unterschiedlichen Bereichen den Entwicklungsstand des Kindes und einen etwaigen Förderbedarf identifizieren. Die Schätzung kausaler Effekte basiert auf einem Regressions-Diskontinuitäten-Ansatz in Verbindung mit einem Differenz-von-Differenzen-Ansatz, der die Tatsache nutzt, dass Eltern abhängig vom Geburtsdatum des Kindes Anspruch auf das neue Elterngeld haben. Trotz sehr präziser Schätzergebnisse können keine statistisch signifikanten Effekte der Elterngeldreform auf die sprachliche und motorische Entwicklung, die sozio-emotionalen Stabilität, und die Schulfähigkeit, sowie auf sozio-ökonomische Unterschiede in diesen Entwicklungsmaßen festgestellt werden. Das Kapitel diskutiert vielfältige Erklärungsmöglichkeiten für diesen Befund und kommt zu dem Ergebnis, dass selbst substantielle Veränderungen in Elternzeitregelungen - wie die betrachteten - nur sehr eingeschränkt auf kindliche Entwicklung wirken.

Nach der frühen Kindheit richtet sich der Fokus in den Kapiteln 3 und 4 auf die Schulzeit, in denen zwei wichtige Faktoren formaler Bildungssysteme analysiert werden: die Anzahl an Schuljahren und die Anzahl an Unterrichtsstunden innerhalb der Schuljahre. Kapital 3 analysiert, inwiefern eine Erhöhung an Unterrichtsstunden Kompetenzen von Schülerinnen und Schülern steigert. Ein besonderer Fokus liegt auf der Frage, inwiefern das bestehende Leistungsniveau den Nutzen zusätzlicher Unterrichtseinheiten bestimmt. Um den kausalen Effekt von Unterrichtsstunden auf den Kompetenzerwerb zu schätzen, nutzt dieses Kapital die G8-Bildungsreform, die die Gymnasialschulzeit um ein Schuljahr reduziert, und die Unterrichtszeit in den verbleibenden Schuljahren zum Ausgleich erhöht hat. Für Schülerinnen und Schüler in früheren Klassenstufen stellt die Reform eine quasi-experimentelle Erhöhung der Unterrichtszeit dar. Anhand von PISA-Daten, die Kompetenzmessungen in Lesen, Mathematik und Naturwissenschaften in der 9. Klasse enthalten, wird der Effekt einer substantiellen Erhöhung der wöchentlichen Unterrichtszeit um 6.5 Prozent, bzw. zwei Wochenunterrichtsstunden, über einen Zeitraum von fünf Schuljahren untersucht. Die zusätzliche Unterrichtszeit hat dabei zusätzliche Lerninhalte vermittelt. Dieses Untersuchungsmerkmal ist von hoher politischer Relevanz, da bildungspolitische Maßnahmen, die eine Erhöhung der Unterrichtszeit für eine Verbesserung der Kompetenzen von Schülerinnen und Schülern anregen, typischerweise auch eine Ausweitung des Lehrplans vorsehen. Das empirische Vorgehen nutzt dabei die Tatsache, dass die Reform in den verschiedenen Bundesländern zu unterschiedlichen Zeitpunkten eingeführt wurde. Die Ergebnisse zeigen, dass sich Kompetenzen im Mittel verbessern. Allerdings gibt es große Effektunterschiede je nach Leistungsniveau der Schülerinnen und Schüler. Leistungsstärkere Schülerinnen und Schüler profitieren mehr von zusätzlicher Unterrichtszeit als leistungsschwächere, vermutlich weil sie neue Lerninhalte schneller verstehen. Damit demonstrieren die Ergebnisse, dass zusätzliche Unterrichtszeiten in der Schule die Leistungsunterschiede zwischen Leistungstärkeren und Lesitungsschwächeren verstärken können, insbesondere wenn in der zusätzlichen Unterrichtszeit zusätzliche Inhalte vermittelt werden.

Kapitel 4 widmet sich der Frage, inwiefern die Anzahl an Schuljahren und die Unterrichtszeit innerhalb dieser Schuljahre substituierende Faktoren beim Kompetenzaufbau von Schülerinnen und Schülern sind. Zur Untersuchung wird ebenfalls die G8-Reform herangezogen, die die Gymnasialschulzeit um ein Schuljahr reduziert hat, während die Unterrichtszeit in den verbleibenden Schuljahren entsprechend erhöht wurde. Die Reform hatte zum Ziel, den Lehrplan mit einer vergleichbaren Gesamtbeschulungszeit innerhalb einer kürzeren Schuldauer zu vermitteln, und damit Schülerinnen und Schüler mit vergleichbaren Kompetenzen früher an den Arbeitsmarkt zu führen. Dieses Kapitel untersucht den kausalen Einfluss der G8-Reform auf verschiedene Maße von Schulleistungen. Die zeitliche Variation der Reformeinführung über die verschiedenen Bundesländer hinweg wird mit Differenz-von-Differenzen-Schätzungen genutzt, um kausale Reformeffekte zu schätzen. Anhand von aggregierten, administrativen Daten, die die Gesamtheit der deutschen Schülerpopulation abdecken, wird untersucht, inwiefern sich die Reform auf den Übergang ans Gymnasium, Klassenwiederholungen, Abiturleistungen, und die Wahrscheinlichkeit zum Abiturabschluss ausgewirkt haben. Die Ergebnisse zeigen, dass sich der Anteil an Klassenwiederholungen erhöht, und die finale Abiturnote verschlechtert haben. Diese Effekte sind nicht nur von vorübergehender Natur, sondern auch sechs Jahre nach Einführung der Reform nachweisbar. Allerdings lässt sich nicht nachweisen, dass sich durch die Reform auch der Anteil an Abiturientinnen und Abiturienten verringert hat. Im Ergebnis hat die Reform einen negativen Einfluss auf Schulleistungen. Allerdings erscheint die ökonomische Signifikanz der Ergebnisse moderat, da Schülerinnen und Schüler schließlich erfolgreich ihre Schullaufbahn abschließen. Adverse Reformeffekte werden auch vor dem Hintergrund als moderat interpretiert, als dass die meisten früher in den Arbeitsmarkt übergehen können, und damit sowohl höhere Lebenszeiteinkommen erzielen können, als auch früher Sozialversicherungsbeiträge zahlen und den qualifizierten Arbeitskräftemangel in der Volkswirtschaft lindern.

Kapitel 5 widmet sich der Gesundheit von Individuen, einer weiteren wichtigen Dimension von Humankapital. Substantielle bildungsbezogene Unterschiede in Bezug auf die Gesundheit, dem Vorliegen chronischer Erkrankungen und die Lebenserwartung können auch auf starke Unterschiede in gesundheitsbezogenem Verhalten zurückgeführt werden. In Untersuchungen, inwiefern Bildung gesundheitsbezogenes Verhalten und die allgemeine Gesundheit kausal beeinflusst, wurde bislang weitgehend vernachlässigt, dass Bildung und Humankapital auch über Generationen hinweg übertragen werden. Dieses Kapitel untersucht daher den kausalen Effekt der Bildung der Eltern auf die Gesundheit und das Gesundheitsverhalten ihrer Kinder. Dazu wird eine Schulreform evaluiert, die zwischen 1949 und 1969 in den unterschiedlichen deutschen Bundesländern die Pflichtschuljahre an der Haupt-, bzw. Volksschule von acht auf neun Jahre erhöht hat. Diese Reform generierte zufällige Variation in elterlichen Bildungsjahren, die mit Instrumentvariablenschätzungen und Differenz-von-Differenzen-Schätzungen ausgenutzt wird, um den *Effekt* elterlicher Bildung auf das Rauchverhalten und Gewichtsprobleme von Kindern zu untersuchen. Die empirischen Analysen basieren auf dem Mikrozensus, sowie auf Daten des Sozio-oekonomischen Panels (SOEP). Im Ergebnis führt ein Anstieg in der mütterlichen Bildung zu Verringerungen im Rauchverhalten und von Gewichtsproblemen von Jugendlichen, die bis ins Erwachsenenalter fortbestehen und die Wahrscheinlichkeit chronischer Erkrankungen reduzieren, die durch ungesunde Lebensweisen herbeigeführt werden können. Für die väterliche Bildung können keine solchen Effekte ermittelt werden. Die Studie untersucht weiterhin potentielle Kanäle, über die eine Erhöhung der mütterlichen Bildung einen positiven Einfluss auf die Gesundheit und das Gesundheitsverhalten von Kindern haben kann. Dabei erscheinen höhere Investitionen in die Bildung, und eine Verbesserung des schulischen Umfelds als wichtige Erklärungsansätze für die Effekte. Direkte Effekte der Schulzeitverlängerung auf den Arbeitsmarkterfolg der Eltern, ihre Partnerwahl, Fertilität, Gesundheit, sowie ihr Gesundheitsverhalten spielen empirisch eine untergeordnete Rolle. Die Ergebnisse der Studie weisen darauf hin, dass die kausalen Effekt von Bildung auf Gesundheit und Gesundheitsverhalten deutlich größer als bisher angenommen sind, da intergenerationale Effekte ebenfalls berücksichtigt werden müssen.

Kapitel 1 und 6 bilden den Rahmen der Arbeit. Kapitel 1 ordnet die Arbeit in die ökonomische Literatur ein, erläutert den strukturellen Aufbau und stellt den allgemeinen Forschungsbeitrag heraus. Kapitel 6 stellt weiteren Forschungsbedarf heraus und zieht allgemeine Schlüsse.

Chapter 1

Introduction

1.1 Motivation

Individuals' opportunities in life differ greatly by the socio-economic status of their family.[1] It is an almost universal fact that children from families with a low socio-economic status (SES) obtain less education, earn less, are more likely to be unemployed, are less healthy, and have a lower life expectancy (see, e.g., Björklund & Salvanes, 2011; Black & Devereux, 2011, for overviews of this extensive literature). Consequently, children's own SES later in life correlates strongly with their parents' SES, thereby perpetuating inequality.

The relationship between children's outcomes and their family background already emerges early in life: Children from low and high SES families differ in their birth weight (e.g. Currie & Moretti, 2003). When entering school, they differ strongly in numerous dimensions of their development and skills (e.g. Feinstein, 2003; Cunha et al., 2006; Cunha & Heckman, 2007; Todd & Wolpin, 2007; Bradbury et al., 2015). These early gaps widen throughout schooling, such that children who are behind early in life will stay behind, if not fall further behind, later in life (e.g. Heckman & Mosso, 2014; Bradbury et al., 2015). Such differences by children's socio-economic background imply that some children cannot unfold their human capital potential.

Children's human capital comprises various dimensions, including their level of education and training, their health (e.g. Goldin, 2016), as well as their full stock of knowledge, character traits, attitudes and preferences (e.g. Becker, 1994; Carneiro & Heckman, 2003). If children cannot unfold their human capital potential, this has consequences for the individuals themselves, as well as for society and the economy

[1] The socio-economic status of families measures their social and economic position relative to other families, and is typically captured by household income and parental education.

as a whole. Increased human capital can lead to, for example, higher earnings, lower unemployment risks, more stable relationships, better health, and an increased life expectancy. At the societal level, investments in human capital can, for example, generate higher tax revenues, lower costs for social security systems, lower crime rates, increase civic engagement, and stimulate innovations (e.g. Becker, 1994; Cawley & Ruhm, 2011; Oreopoulos & Salvanes, 2011; Lochner, 2011). The stock of human capital of an economy is also central for economic growth. As populations are stagnating in most developed countries, it is mainly improvements in the quality of human capital, and its effective use, that can further promote economic growth and determine economies' international competitiveness (e.g. Lange & Topel, 2006).

However, human capital investments are costly for individuals and their parents. Investments in education, for example, may have direct costs of schooling, but also opportunity costs in the form of foregone earnings in the labour market, as well as potentially psychic costs of studying. Similarly, investments in health, for example, can be costly because of direct higher costs for healthier food or better health services, but also because of opportunity costs in the form of foregone earnings and stress when individuals exercise (Becker, 1962).

Ideally, individuals invest in their human capital until the total marginal returns equal the total marginal costs. However, several market failures can cause individuals to invest less in their human capital than what is optimal for themselves and for society. These market failures include incomplete credit markets, imperfect information, and market externalities. With incomplete credit markets, budget constrained parents or individuals may not be able to borrow against the future to make their desired human capital investments (e.g. Lochner & Monge-Naranjo, 2011). Equally, parents or individuals may have imperfect information about the future returns to human capital investments. Moreover, human capital returns that accrue at the societal level are typically not taken into account in individual investment decisions. All these factors can lead to underinvestments in children's human capital. They are a main justification of public policy interventions to support human capital investments, such that individuals and society can better benefit from the human capital potential. This dissertation asks whether policy-makers can impact the formation of human capital and the emergence of differences therein.

The economic concept of *human capital* goes back to Adam Smith (1776), who acknowledges the importance of an individual's skills for economic prosperity, as skills determine the capacity to produce economic value. However, until the work by Mincer (1958), Becker (1962), and Schultz (1963), economists typically took the labour force potential as given and not malleable (Goldin, 2016). Their work significantly contributed to the notion that investments in individuals' human capital can increase their productivity.[2] In contrast to physical or financial capital, an important difference of human capital is that it is intangible: Skills, health, and values cannot be separated from the person. Consequently, it cannot be traded or exchanged. This is an important property of human capital that requires understanding how human capital is formed at the individual level, such that policy-makers can alleviate underinvestments.

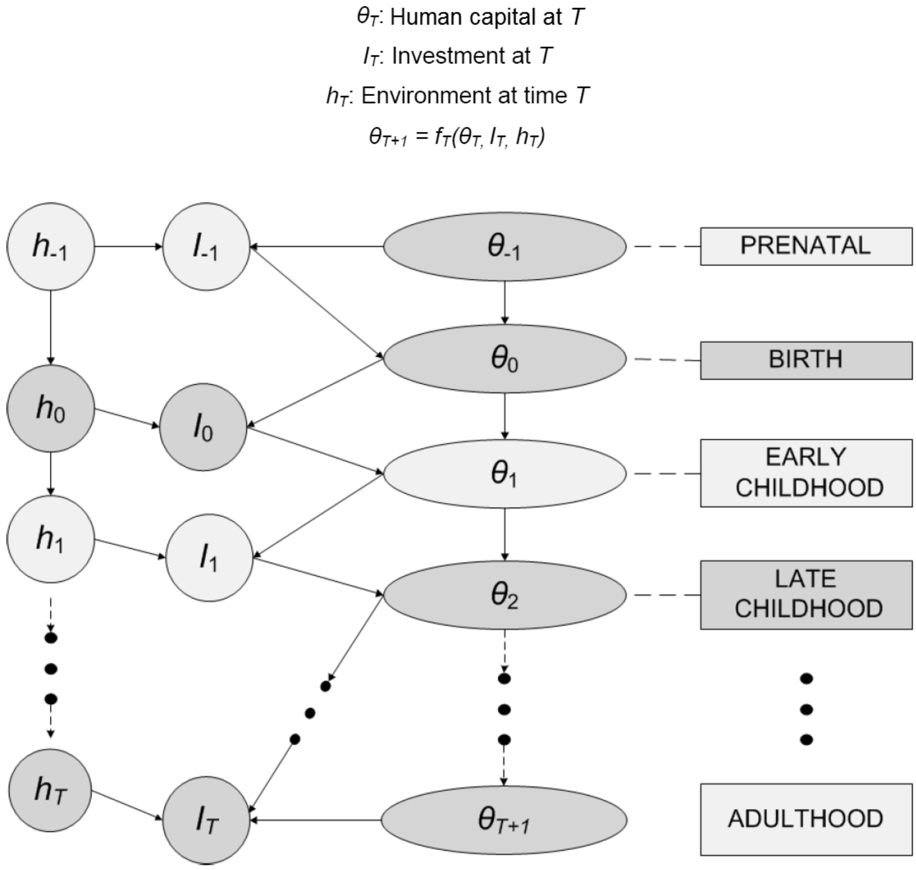
The early literature on the role of public policy on the formation of human capital focuses mainly on the provision of formal education as provided by schools or universities (Becker, 1962; Becker & Tomes, 1986). Advancements in the literature take a broader view and emphasise the multidimensionality of human capital. For simplicity, one can think of the multidimensionality of human capital as consisting of cognitive skills, non-cognitive skills, and health. Health, for example, is a very important dimension of human capital, as healthier individuals can attend school on more days, be more productive, and work more over the lifecycle (e.g. Goldin, 2016). Moreover, the literature stresses that the formation of human capital is a dynamic process spanning the entire lifecycle. The process starts with conception and lasts throughout life (e.g. Carneiro & Heckman, 2003; Cunha & Heckman, 2007; Conti & Heckman, 2014). The idea of the lifecycle framework of human capital formation is depicted in Figure 1.1.[3] It describes how the stock of human capital, together with contextual factors and investments, affect the development of human capital throughout life.

The model has two important features. First, due to the multidimensionality of human capital, individuals can achieve each outcome by various combinations of different dimensions of human capital. Second, the model takes a lifecycle perspective and separates the human capital formation process into multiple stages.

[2] The early literature focuses on the role of human capital, especially education, for productivity and earnings. It was only in the early 2000s that economists paid increased attention to non-market benefits of human capital.

[3] The framework is based on Conti & Heckman (2014), who focus on child well-being, but it can be generalised to human capital. Their theoretical framework also considers the preferences of parents, which influence investments in children's human capital. This is not a focus of this dissertation and, therefore, not described in greater detail.

Figure 1.1: A lifecycle framework for conceptualizing human capital formation



Source: Based on Conti & Heckman (2014), own modifications.

Initial endowments (θ_{-1}) are determined at conception. These initial endowments and environmental factors (h_{-1}), such as the family environment, determine prenatal investments (I_{-1}) and the stock of human capital at birth (θ_0). In subsequent periods, the stock of human capital (θ) at time $t + 1$ results from investments (I), environmental factors (h), and the stock of human capital at time t. Investments can be made by parents (for example parental time) or policy-makers (for example by providing formal schooling). Environmental factors include the institutional environment, such as parental leave legislation and the structure of the education system, but also the family and peer environment.

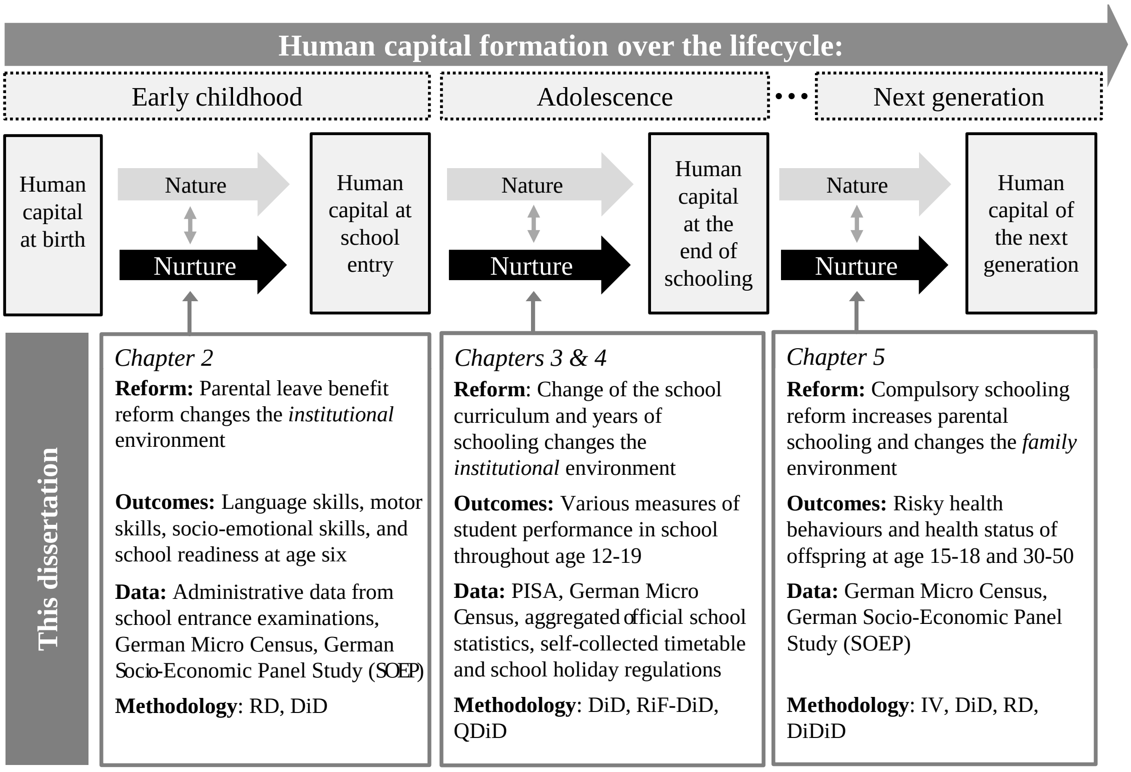
The human capital formation process is iterative throughout life. Consequently, earlier human capital fosters the development of later human capital (self-productivity). Additionally, investments are seen as complements in the production of human capital: They are more productive the higher the stock of human capital already is (*static complementarity*); moreover, early investments make later investments more productive (*dynamic complementarity*). What follows from this theory is that policy interventions can be more effective and efficient in promoting human capital if they target individuals early in life (e.g. Heckman, 2006).

The model helps distinguish between two main factors in the literature on the formation of human capital: Individuals' genetic endowments, which are determined with birth (the *nature* component), and contextual factors, such as the family environment, stimulation received by parents or the education system, that surround children (the *nurture* component). While genes are indeed important in the formation of human capital, their expression can be suppressed or enhanced by contextual factors (e.g. Heckman & Mosso, 2014). This interplay of nature and nurture is often referred to as *nature via nurture*. Policy-makers aiming at impacting the formation of human capital can only address the nurture component in human capital that can trigger or suppress the nature component.

These public policies may aim at changing investments into children directly, e.g. by providing a compulsory schooling system and the respective school input factors therein. Policies can also aim at impacting the formation of human capital more indirectly by changing contextual factors that may lead to changes in child investments. For example, by offering parental leave periods, policy-makers incentivise parents to spend more time with the child. However, it is still at parents' discretion to decide whether they take up parental leave, whether they use it to spend more time with their children, and how they spend the time with their children. The quality of the child-parent time then determines the effects on the children's development. As parents from higher SES backgrounds may differ in their parental leave take-up and time-use from lower SES parents, such policies may have implications for the emergence of early SES gaps in child development. For the allocation of public resources and the design of effective policies, one needs to understand which policy interventions work at all, when they work best, for whom do they work best, and what are unintended side-effects that come with these policies.

While answers to these questions are undoubtedly complex, collecting empirical evidence can help better understand determinants of effective policies. This dissertation collects empirical evidence and analyses different education and family policies. In the following, I provide an overview of the studies included in this dissertation (Section 1.2) and highlight their general contributions (Section 1.3).

Figure 1.2: Overview of the dissertation chapters



Source: Own illustration.

1.2 Overview and summary

The dissertation consists of four independent research articles that empirically investigate the effects of education and family policies in Germany on various dimensions of human capital. It takes a lifecycle perspective on the formation of human capital by analysing policy interventions that affect individuals at different stages in life: in early childhood, in secondary school, and across generations. The structure of the dissertation follows the lifecycle of individuals and is depicted in Figure 1.2.

Chapter 2 examines an intervention in early childhood and studies the effects of parental leave policies on child development. Parental leave policies provide an institutional environment that supports families around childbirth, with job protection during child-related work interruptions and benefits to compensate for income losses during these interruptions. Thereby, they affect several environmental conditions in early childhood that may impact child development, such as parental labour supply before and after childbirth, time parents can spend with their children, and household income. The chapter studies a substantial reform in Germany in 2007 that both expanded eligibility for paid leave in the first year and removed eligibility for paid leave in the second year following childbirth. Higher-income households benefited relatively more from the reform than lower-income households. The chapter analyses the effects of the reform on child development and substantial, pre-existing socio-economic gaps in child development. The analysis builds on rich administrative data from mandatory school entrance examinations conducted before school entry at age six. Eligibility for the new parental leave benefit system was based on children's birthday, which is exploited by a regression discontinuity approach within a difference-in-differences framework to estimate causal reform effects. The precise and robust estimates reveal no effect of the reform on child development and on socio-economic development gaps. The chapter concludes that such substantial changes in parental leave benefits are unlikely to considerably impact children's development.

After this family policy intervention affecting children in early childhood, Chapters 3 and 4 investigate education policy interventions in children's adolescence. The chapters focus on two important school input factors, namely years of schooling and instruction time, and analyse their effects on student performance. Chapter 3 studies the impact of instruction time, an expensive school input factor at the core of each education system. The identification of causal effects builds on an education reform in Germany that reduced the length of academic track schooling by one year, while increasing instruction hours in the remaining school years. For students in lower grade levels, the so-called G8-reform serves as a natural experiment to estimate the effects of instruction time on student performance. The chapter focuses on students in grade 9, for whom weekly instruction hours increased by about 6.5 percent over the course of five years. The additional instruction time covered new learning content - a reform feature that is highly relevant for policy-makers as increases in instruction time are typically accompanied by new learning content. The empirical strategy exploits the fact that the reform was implemented in different years across federal states. Based on student competence measures in reading, mathematics and science, as captured by PISA assessments, the empirical findings show that reform-induced increases in instruction time improve student performance on average. However, treatment effects are small and differ substantially across the student performance distribution. Low-performing students benefit less than high-performing students. The content of additional instruction time is important to explain this pattern.

Better-performing students can better cope with additional learning content, while lower-performing students may need more time for remediation instead. The findings demonstrate that increases in instruction hours foster skill development, but also widen the gap between low- and high-performing students.

Chapter 4 focuses on the substitutability of years of schooling and instruction time in driving student performance. The chapter is based on the G8-education reform in Germany, as analysed in Chapter 3, but now considers both components of the reform: The reduction in the length of academic track schooling by one year, and the simultaneous increase in instruction hours in the remaining school years, such that students are taught a very similar core curriculum over a shorter period of time. To estimate the causal effects of the reform, the empirical strategy exploits variation in when the reform was implemented across federal states. Using aggregated administrative data on the full population of students, the chapter provides empirical evidence that the reform increases grade repetition rates and lowers final grade point averages, implying that affected students have a poorer command of the school material than peers who covered the same material over a longer period of time. These effects are not only of a transitory nature, but are also apparent six years after the introduction of the reform. However, there is no evidence that the reform affects graduation rates. Overall, the results suggest that compressing instruction time into fewer years of schooling has adverse effects on student performance, but the economic significance of the effects appears moderate. Some students cope with the increased learning intensity by repeating a school year, especially students in the final years of academic track schooling. The potential costs of the reform due to adverse effects on student performance must be weighed against the economic gains from earlier labour market entries that can raise individuals' lifetime earnings, but also benefit society by mitigating skilled worker shortages and increasing social security contributions.

Chapter 5 moves further in the lifecycle and investigates the impact of parental education on their children's health and health-related behaviour. Unhealthy lifestyles and risky health behaviours strongly contribute to substantial educational gradients in health status, morbidity, chronic conditions, and longevity of individuals - important dimensions of human capital. A large literature on the causal effects of education on health and health-related behaviours mostly neglects the fact that education is also passed on to the next generation. This chapter studies the causal effects of parental education on their children's health and health-related behaviours. It studies the intergenerational effects of a compulsory schooling reform in West Germany after World War II that increased the minimum number of school years from eight to nine years. From the perspective of the next generation, the intervention constitutes a change in the education of their parents, thereby changing the family environment children are exposed to. Instrumental variable approaches and difference-in-differences methods use the fact that the reform was implemented across federal states at different points in time. The analysis is based on two independent data sources, the German Micro Census and the German Socio-Economic Panel Study (SOEP). The findings reveal that increases in maternal schooling reduce their children's probability to smoke and to be overweight in adolescence. The effects persist into adulthood, reducing chronic conditions that often result from unhealthy lifestyles. No such effects can be identified for paternal education. Increased investments in children's education and improvements in their peer environment early in life are important for explaining the effects. Changes in family income, family stability, fertility, parental health, and their health-related behaviours are also examined, but appear less relevant empirically. The intergenerational effects of education on health and health-related behaviours exceed the direct effects, suggesting that there are substantial non-market benefits to education that only accrue in the long-term by spill-over effects on the next generation. The results also suggest that changes in early childhood conditions can have substantial and lasting effects on children.

Chapter 6 generally discusses the findings, identifies scope for future research, and derives policy implications.

Chapters 2, 3, 4 and 5 complement each other in several dimensions, as depicted in Figure 1.2. First, they consider policies that affect individuals at different stages in life. In Chapter 2, I analyse a reform of parental leave benefits that originally affected parents in the first two years after childbirth. In Chapters 3 and 4, I analyse a secondary schooling reform affecting children between the ages 10 and 18. In Chapter 5, I analyse an education reform that originally affected children around age 15. However, from the perspective of the next generation, this reform constitutes an intervention in early childhood that changes the family environment by exposing children to better-educated parents.

Second, the dissertation considers different dimensions of human capital, including measures of cognitive skills and non-cognitive skills, but also health status and health-related behaviour that importantly interact with the extent to which individuals can make full use of their cognitive and non-cognitive potential (e.g. Conti & Heckman, 2014; Goldin, 2016). In particular, I analyse language skills, motor skills, socio-emotional stability, and school readiness in Chapter 2, mathematics, reading and science competencies in Chapter 3, the school track choice, grade repetitions, final grade-point averages and the completed school degree in Chapter 4, as well as smoking behaviour and being overweight in Chapter 5. It is important to acknowledge the complexity of multidimensional human capital as the different dimensions of human capital interact with each other and as interventions may only be effective in determining specific components of human capital.

Third, the analyses use various data sources, each chosen to best suit the purpose of the respective analyses. Within chapters, I complement analyses with different data sources to overcome some limitations that each data source typically entails. I employ micro level data and aggregated data, panel data and cross-sectional data, as well as administrative data and survey data. The data sources comprise administrative information from compulsory school entrance examinations (Chapter 2), data from the Programme for International Student Assessment (PISA, Chapter 3), aggregated data from official school statistics (Chapter 4), the German Micro Census (Chapters 2, 3, 5), the German Socio-Economic Panel Study (SOEP, Chapters 2, 5), as well as self-collected historical data of timetable regulations and school holidays of the federal states (Chapter 3).

Fourth, the chapters complement each other by using different empirical strategies for the identification of causal reform effects, including regression discontinuity designs (RD), difference-in-differences designs (DiD), difference-in-differences-in-differences designs (DiDiD), instrumental variable designs (IV), as well as different methods to identify treatment effects along the outcome distribution. The empirical strategies are chosen with the aim to best exploit the variation induced by the reform, to account for potentially confounding factors, to make best possible use of the available data, and also to account for data limitations such that policy effects can be estimated reliably.

Fifth, the analyses consider short-term, medium-term, and long-term effects of policy interventions. Limiting policy evaluations to only short-term effects may hide important overall effects: Short-term effects may suggest an effective programme, but these effects may fade-out over time. Alternatively, a policy programme may have small short-term effects, but unfold large effects later. Finally, short-term effects may temporarily fade-out and re-appear in the long-run. This dissertation evaluates short-term (Chapters 3 and 4), medium-term (Chapter 2), and long-term, intergenerational effects (Chapter 5) of policy interventions, which helps assess the overall impact of policy interventions.

1.3 General contributions

Each of the following chapters in this dissertation makes specific contributions to the economics literature. They are highlighted in the respective chapters. Moreover, this dissertation also makes general contributions that I emphasise here.

First, this dissertation makes a *methodological* contribution. All chapters use quasi-experiments (also called natural experiments) and state-of-the-art econometric methods to identify factors that causally impact the formation of human capital. If policy-makers want to support human capital formation or correct politically undesired developments, such as socio-economic gaps in children's human capital, they first need to know which policy programmes can effectively mitigate the problem. Understanding the effects of certain policies is not trivial: If policy-makers want to understand the effect of more financial support to families after childbirth, changes in the number of school years, or changes in the school curriculum, it is typically not enough to correlate child outcomes to the respective factor that policy-makers may want to change. For example, comparing children with eight years of schooling to children with nine years of schooling will not only reflect the effect of an additional year of schooling, but also include the effect of other differences between these children that may lead to differences in their schooling. Their cognitive ability, non-cognitive skills, and the family background may be unobserved factors that also determine individuals' schooling and may themselves have an impact on their school performance, labour market outcomes, and health. Children with eight years of schooling are unlikely to match children with nine years of schooling if they had gone to school for only eight years. Finding a suitable counterfactual is the fundamental problem of causal inference and the major challenge in the evaluation of policy programmes (Holland, 1986; Angrist et al., 2009).

While randomized controlled experiments are often considered the gold standard to overcome this fundamental problem, they also have some drawbacks. For many policy-relevant questions, they may be ethically, politically, or financially infeasible to implement (e.g. Athey & Imbens, 2017). Moreover, learning about long-term treatment effects requires a very large time gap between the experiment and its evaluation that is not always practical for policy decision-making. Alternatively, many policy questions can be approached by analysing natural experiments, in which exogenous shocks lead to random variation in the data. These quasi-experiments require an *empirical strategy* to identify causal treatment effects (Angrist & Krueger, 1999). The chapters in this dissertation use quasi-experiments in which treatment (assignment to certain policies) is based on individuals' birthday and state of residence. From the perspective of individuals, treatment assignment is random. I exploit this variation in treatment assignment with various modern empirical strategies (see Figure 1.2 for the methods used in this dissertation, and, e.g., Athey & Imbens, 2017, for an overview of advances in the programme evaluation literature), including regression discontinuity designs (RD), difference-in-differences designs (DiD), difference-in-differences-in-differences designs (DiDiD), and instrumental variable designs (IV). The analysis in Chapter 3 identifies treatment effects along the distribution of the outcome variables and additionally employs a difference-in-differences design within a quantile regression framework (QDiD) and within a recentered influence function approach (RIF-DiD). The main empirical approaches are chosen with the aim to best exploit the random variation induced by the reform, account for potentially confounding factors, make best possible use of the available data, and also account for data limitations to reliably estimate policy effects. However, each identification strategy relies on a specific set of identification assumptions. Therefore, all analyses pay particular attention to checking for the plausibility of the underlying identification assumptions. The analyses use multiple empirical strategies, where possible, to evaluate the sensitivity and robustness of the results to varying sets of assumptions. Similar findings across different empirical approaches reassure that the resulting estimates can be interpreted as causal effects of policy interventions on the formation of human capital.

The second major contribution is that all chapters analyse *actionable policies* to provide insights into factors of human capital formation. The variation in natural experiments could stem from various, arguably exogenous sources. In the literature on human capital formation, such sources of variation include, for example, institutional features (see, e.g., Dahl & Lochner, 2012, using discontinuities in the tax system to analyse the impact of family income on child development), weather conditions (see, e.g., Marcotte, 2007, using school closures due to adverse weather conditions to analyse the impact of instruction time on student performance), parental death (see, e.g., Dynarski, 2003, on the impact of financial constraints in the college attendance decision), or policy changes, as in this dissertation. A major advantage of the analysis of specific policy changes is that they represent feasible policy actions taken by policymakers that may also be actionable in other settings. Within a given institutional framework, it can be assessed whether policy programmes can effectively address societal or economic problems. The effectiveness of policy interventions depends on individuals' programme take-up, and eventually on the programme effect once individuals participate. For example, in Chapter 2 I analyse a change in parental benefit legislation. This change can only impact child development if parents take up parental leave, thereby experiencing a change in the amount and length of benefit payments. It is only then that one can expect effects on children. In Chapters 3, 4, and 5, I analyse education policies that are binding for affected children. However, even binding policies can be evaded by individuals. In the case of more instruction time, for example, students may not pay attention in the additional classroom time. Programme take-up is always part of the overall programme effectiveness, because individuals typically cannot be forced to participate in a specific policy programme. The evidence presented in this dissertation assesses the overall impact of education and family policies on the formation of human capital taking into account the potential problem of programme take-up.

The third major contribution is that all policy interventions are implemented *in Germany*. The economics literature on the formation of human capital is dominated by studies from the US. Germany - if compared to the US - provides an institutional environment that provides a generous social security systems, as well as education and health services at low private cost. Nevertheless, there is a strong statistical relationship between parental background and children's human capital. As institutional factors can be important determinants in the formation of human capital, gathering new evidence from an institutional environment, like that of Germany, is of high value for the economics literature. Understanding the policy impact on human capital formation requires collecting credible empirical evidence from policy interventions across different institutional settings, which then facilitates the identification of generalisable patterns that can universally guide policy-making.

The fourth general contribution of the dissertation is that it stresses the importance of considering *heterogeneous* treatment effects of education and family policies. Heterogeneities may not only reveal for whom policies are particularly effective. They may also reveal unintended side-effects. Knowledge about such side-effects is crucial for policy-makers to assess the overall success of a programme. Policies change the institutional environment of individuals, but these factors interact with, for example, individuals' existing human capital, their genetic endowments, or other contextual factors (such as the family environment). Interaction with these other factors determine the effectiveness of policies. Chapter 3 demonstrates that an increase in school instruction time benefits mainly the performance of higher-skilled children. Consequently, heterogeneity analysis reveals that increases in instruction time can widen the gap between high- and low-performing students. In Chapter 4, it is demonstrated that compressing instruction time into fewer years of schooling does not, on average, impact degree completion. However, lower-skilled children repeat grades more frequently in order to cope with the higher learning intensity before earning their degree. Thereby, Chapters 3 and 4 provide evidence that policy-makers face an equity-efficiency trade-off, thus contributing to increasing or reducing inequality in human capital.

However, heterogeneity analysis can also reveal that expected side-effects of policies do *not* occur: Chapter 2 analyses a German parental leave benefit reform for which policy-makers worried that child development gaps between children from low and high socio-economic status (SES) backgrounds increase. Such side-effects would harm the overall assessment of the policy that was successful in other dimensions. However, changes in parental leave benefits are ineffective in impacting child development for children across different SES backgrounds. Unintended consequences on SES gaps in child development cannot be confirmed. While average treatment effects are informative on the average programme effectiveness, they may hide important subgroup differences. This dissertation stresses that the consideration of heterogeneous treatment effects is important for the overall assessment of policies.

The fifth contribution is that the analyses *combine different data sources* to learn about causal reform effects and their potential mechanisms. The analyses use survey data and administrative data, panel data and cross-sectional data, as well as micro level data and aggregated data. Each data source has specific strengths for the analysis, but also limitations. A major advantage of administrative data, such as the data from school entrance examinations, is its large sample size that can provide precise and robust estimates of reform effects. As its content is often process-generated by the administrative body, the data is typically of high quality. This risk of misreporting and social desirability biases is minimized. However, administrative data are collected for specific purposes. Consequently, they are often short of information that go beyond this purpose. For example, they typically contain only limited information socio-economic characteristics. The restricted focus often limits the possibilities of heterogeneity analyses, or to learning about potential effect mechanisms. In contrast, panel survey data, such as the German Socio-Economic Panel Study (SOEP), have the advantage that they cover a vast amount of information on various important aspects of individuals' life over a longer period of time. As the collection of such rich data and panel maintenance is very costly, such data covers a smaller number of individuals. This may result in less precise or less robust estimates of reform effects, but it can still be very useful for validating findings in another data set, for expanding the set of outcomes, or for complementary analyses on potential mechanisms. To best exploit the strength of specific data, while compensating their limitations with other data, the analyses in this dissertation always use several data sources. Thereby, this dissertation demonstrates that secondary data sources, as collected for administrative or other purposes, can be very useful for research, but also generate important insights to guide policy-making.

The sixth contribution of this dissertation relates to the *lifecycle perspective* on the formation of human capital: The dissertation demonstrates that policy interventions can affect human capital at various stages in the life course, and that they can have a lasting impact. However, the specific policy action needs to be carefully considered. Taking the case of early childhood interventions, theory suggests that they can be very efficient because of dynamic complementarities. In this dissertation, changes in parental leave benefits turn out to be ineffective at changing children's human capital (Chapter 2). However, changes in parental education (as realised with a compulsory schooling reform, see Chapter 5) have a substantial and lasting effect on children's human capital. This result also demonstrates that human capital consists of many dimensions, including health, that are differently affected by education and family policies. Therefore, public policies targeting individuals' education or health should not be thought of in isolation by the respective policy-makers, as education and health are both important dimensions of human capital that importantly interact with each other.

Chapter 2

Parental leave policies and socio-economic gaps in child development: Evidence from a substantial benefit reform using administrative data*

Abstract

This paper examines the effect a substantial change in paid parental leave on child development and socio-economic development gaps. We exploit a German reform from 2007 that both expanded paid leave in the first year and removed paid leave in the second year following childbirth. Higher-income households benefited relatively more from the reform than low-income households. We use rich administrative data from mandatory school entrance examinations at age six within a regression discontinuity design augmented with a difference-in-differences approach. Our precise and robust estimates reveal no effect of the reform on child development and on socio-economic development gaps. The findings suggest that such substantial changes in parental leave benefits are unlikely to considerably impact children's development.

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2.1 Introduction

Over the past 15 years, a growing body of evidence has shown that early childhood conditions can have long-lasting effects on children's educational attainment, labour market outcomes, and adult health (e.g. Cunha et al., 2006; Almond & Currie, 2011; Heckman & Mosso, 2014). These early conditions differ considerably by children's socio-economic status (SES), contributing to the emergence of SES gaps in child development very early in life (e.g. Todd & Wolpin, 2007). Bradbury et al. (2015), for instance, show for the US, the UK, Australia, and Canada that SES gaps in child development are already pronounced at age 5 and increase further throughout the first years of schooling. Consequently, many children from low-SES backgrounds fall behind.[1]

To what extent then can public policies affect the link between a family's socio-economic status and child development? One of the most important policy tools across OECD countries to support families around childbirth are parental leave policies. The length of leave offered by these policies has substantially increased since the 1970s (OECD, 2016a). These policies affect several conditions in early childhood (e.g. Björklund & Salvanes, 2011) and, thus, may impact child development. In particular, expansions in parental leave policies reduce maternal labour supply after childbirth (e.g. Ondrich et al., 1996; Lalive & Zweimüller, 2009; Schönberg & Ludsteck, 2014), affecting the time parents can spend with their children. This parental time may be an important input for the development of children (e.g. Fiorini & Keane, 2014; Del Bono et al., 2016). Parental leave benefits also directly impact household income, which determines the resources and goods parents can invest into the development of their children (e.g. Dahl & Lochner, 2012; Løken et al., 2012).[2]

Despite the substantial impact that parental leave policies have on family resources, we know little about the effect of such policies on early child development and even less on SES development gaps: Previous studies almost exclusively focus on long-run effects of paid parental leave expansions occurring between the 1970s and 1990s. Since then, many factors related to child development have changed

[1] Other examples documenting considerable differences in children's skills at school entry include Feinstein (2003), Cunha et al. (2006) and Cunha & Heckman (2007).

[2] The various channels through which parental leave policies may impact child development are also carefully described in, e.g., Dustmann & Schönberg (2011) and Danzer & Lavy (2016).

substantially across countries, such as maternal labour force participation, day care availability, and social norms. Therefore, it remains unclear whether the findings from studies on earlier parental leave expansions are still valid today. Furthermore, previous contributions have paid little attention as to whether these policies impact high- or low-SES families differently, and whether they affect SES development gaps.

Our paper addresses these questions by examining the effects of a substantial paid parental leave reform on child development. The 2007 reform in Germany completely changed the eligibility criteria and benefit payments: For children born before January 1, 2007, parental leave benefits were means-tested and paid for up to two years after childbirth for eligible mothers. For children born thereafter, parental leave benefits were earnings-related and paid for up to 14 months in total per couple. The reform expanded the proportion of mothers eligible for up to 12 months of parental leave from 47% to almost 100%. The additional public benefit payments of the programme were fiscally substantial as they amounted to about 0.1% of GDP in the first year after implementation.[3] The reform increased the average net monthly income in the first year after childbirth by about 20% (Wrohlich et al., 2012). Reflecting the shift to an earnings-related benefit system, mothers with a university degree received about 40% more than mothers without a university degree. The reform caused the labour supply of mothers to decrease in the first year after birth (especially for highly educated mothers, by about 20%), and to increase in the second year after birth (especially for low-educated mothers, by about 23%; see, e.g. Kluve & Schmitz, 2017). Other studies document effects on fertility (Cygan-Rehm, 2016) and breastfeeding duration (Kottwitz et al., 2016).[4] Still, we do not know whether the reform affected child development even though critics worried at the time that the reform would widen substantial pre-existing SES gaps in child development (e.g. Henninger et al., 2008).

Our study makes the following contributions to the literature. *First*, we analyse a recent reform that both expanded eligibility for paid leave in the first year after childbirth, and removed paid leave in the second year after childbirth. Additionally, it encouraged fathers to take parental leave. Examining such a reform adds to the previous literature which exclusively studies introductions or expansions of parental leave periods. However, we still do not know whether reducing paid parental leave

[3] Own calculations based on Federal Ministry of Finance (2007), German Federal Statistical Office (2008), and German Federal Statistical Office (2016).

[4] Huebener et al. (2016) summarise the literature on the 2007 German paid parental leave reform.

entitlements impacts childrens' development. Our contribution is therefore particularly relevant for most OECD countries that already have paid parental leave policies in place and might consider reductions in paid parental leave entitlements.

Second, we shed new light on whether parental leave policies affect children from low- and high-SES families differently, thereby impacting SES development gaps. The reform we analyse changed means-tested benefits to earnings-related benefits causing high-SES households to gain more from the reform than low-SES households in terms of parental leave eligibility and benefit payments. Thereby, the German reform provides a useful setting to study the relationship between SES gaps in child development and parental leave policies.

Third, we provide novel evidence of parental leave policy effects on short-run outcomes of children using professionally assessed and exceptionally rich, administrative data. The data stems from compulsory school entrance examinations at age six covering the full population of children from one German state. The data includes very detailed information of child development assessed by licensed public health paediatricians. The unusual depth of information for children of this age group allows us to examine several important dimensions of child development which strongly predict later educational attainment, later health outcomes and labour market performance. In contrast, previous studies analysing short-run effects are either constrained by child outcomes conveying limited information on child development (e.g. birth weight, infant mortality, premature birth, and hospitalisations), by potentially biased parent-reported information about the children, or by small sample sizes requiring more restrictive assumption for the identification of causal effects (see Section 2.2). We also overcome a limitation to studies on children's long-run outcomes which mostly find small or no reform effects: These studies cannot answer the crucial policy question whether parental leave policies do not have any effects on children at all, or whether initial effects fade out over time. The policy implications differ substantially as in the latter case policy makers should try to prevent this fade-out.

We estimate the reform effects on child development with a regression discontinuity approach that compares children born before and after the 2007 reform cut-off date. To account for potential seasonal and age-at-examination effects, we augment the regression discontinuity approach with a difference-in-differences approach using children born in nearby years around the same cut-off date as our control group.

Our results show that this substantial change in parental leave benefits had no impact on children's language skills, motor skills, socio-emotional stability, and school readiness at age six. The point estimates from our large sample are close to zero and precisely estimated. When we stratify the sample by parents' education, an important predictor of earnings and paid parental leave eligibility in Germany, we estimate again very small and insignificant treatment effects on child development. The same picture emerges when we stratify the sample by parents who were likely previously eligible for paid leave, and parents who were likely previously ineligible. Consequently, we find no evidence for changes in the SES development gap, despite the strong and heterogeneous effects of the reform on mothers' labour supply and household income.

To explain our zero-reform effects, we draw on findings from recent economic studies on the determinants of child development. Households gaining parental leave benefits receive temporary, unrestricted transfers. Such transfers are unlikely to have a significant impact on parents' productive investments into their children (e.g. Carneiro & Ginja, 2016) and on child development (e.g. Heckman & Mosso, 2014; Del Boca et al., 2016). Moreover, maternal labour supply mostly reacted to the reform after the first six months following childbirth, and typically involved part-time employment. The literature suggests that maternal part-time employment beyond the first six months after childbirth has, at most, a small impact on child development, especially when alternative care arrangements are of comparable quality to maternal care (e.g. Brooks-Gunn et al., 2010; Bernal & Keane, 2010).

The remainder of the paper is organised as follows. Section 2.2 summarises the previous literature on parental leave policies and child development. Section 2.3 provides information about the institutional background and emphasises the heterogeneous effects of the 2007 parental leave reform. Section 2.4 introduces the data and provides descriptive statistics. Section 2.5 outlines our empirical strategy. We present the main results and a large set of robustness checks in Section 2.6. We discuss our findings in Section 2.7 and conclude in Section 2.8.

2.2 State of the literature

Only a small economic literature studies the effects of parental leave policies on child outcomes.[5] This small literature varies by country and year of reform, the timing and intensity of the change in paid or unpaid parental leave mandates, the age at which child outcomes are measured, and the main results (see Appendix Figure A2.1).[6] Two studies examine *introductions* of parental leave (i.e. changes at the extensive margin). For the US, Rossin (2011) analyses the introduction of 12 weeks of unpaid parental leave and finds evidence for small improvements in birth weight and reductions in infant mortality rates for children of highly educated mothers, but largely no effects for children of less-educated and single mothers. For Norway, Carneiro et al. (2015) examine the introduction of four months of paid parental leave in 1977 and find a decline in high school drop-out rates (for children from low-educated mothers) and an increase in wages.

Studies that examine paid parental leave *expansions* within the first year after childbirth (i.e. changes at the intensive margin) in four different countries cannot find effects on child outcomes (Dustmann & Schönberg, 2011; Würtz Rasmussen, 2010; Dahl et al., 2016; Beuchert et al., 2016; Baker & Milligan, 2008, 2010, 2015). Across these studies, the timing and intensity of the expansion, and the social and institutional contexts vary. But even within a similar institutional context, such as in Norway, Dahl et al. (2016) do not find any effects on child outcomes of further paid parental leave expansions from 18 to 35 weeks after childbirth, while Carneiro et al. (2015) detect effects of the introduction of parental leave. These findings suggest that the timing of parental leave policies matters, but an insufficient magnitude of the expansions may also explain the results. Furthermore, most child outcomes are measured between the age of 14 and 33, so it is not clear whether initial reform effects faded out over time. Furthermore, the reforms happened in the 1970s to 1990s, and it remains unclear whether their findings are still valid as female labour force participation rates, the availability of day care, and social norms changed significantly over this period.

[5]We focus on studies exploiting individual-level data and parental leave reforms for effect identification. Early studies on the topic examine variations in parental leave policies across countries in aggregated data (Ruhm, 2000; Tanaka, 2005).

[6]For previous detailed descriptions of the literature on parental leave policies and child development, see, e.g. Danzer & Lavy (2016) and Huebener (2016).

Studies that analyse paid and unpaid parental leave expansions in the second year after childbirth show some effects on child development measured at age 14 to 16. Liu & Skans (2010) examine the extension of paid parental leave from 12 to 15 months in Sweden. They find a modest improvement in grade point averages for daughters of highly educated mothers. Dustmann & Schönberg (2011) also evaluate one expansion in unpaid parental leave from 18 to 36 months in Germany in 1992, and find a very small, negative effect on children's school track choice. Danzer & Lavy (2016) analyse the effects of an Austrian expansion of paid parental leave from 12 to 24 months. The study finds positive reform effects on test scores at age 15 for sons of highly educated mothers, but negative effects for sons of low-educated mothers.

Few studies focus on the short-run effects of parental leave reforms on child outcomes. Beuchert et al. (2016) study a paid parental leave expansion from 6 to 11 months in Denmark and find no effects on children's hospital visits in the first three years after childbirth. Rossin (2011) also analyses child health outcomes (i.e. birth weight, infant mortality, premature birth). Yet, both studies are uninformative about the effects on several other dimensions of child development, including cognitive and non-cognitive outcomes. The only other studies that examine parental leave effects on richer early child development outcomes evaluate a Canadian parental leave expansion from 6 to 12 months. Baker & Milligan (2008, 2010, 2015) mostly find no effects of the reform on health and development outcomes up to age 3, or on measures of children's cognitive and non-cognitive development at ages 4 through 5. While the outcome measures are rich, they estimate causal reform effects through cohort comparisons in an eight-year window around the reform. This approach may be more sensitive to other confounding effects (such as cohort and age-at-test effects) than approaches that compare child outcomes in the close neighbourhood of reform eligibility cut-offs. Huber (2017) also analyses the effects of the 2007 German reform on child outcomes at ages 0 through 3. In contrast to our study, the analysis is based on parent-reported measures for child development and a comparably small sample from the German Socio-Economic Panel Study (SOEP). The point estimates are very large compared to previous findings in the literature, unstable across specifications and imprecisely estimated, preventing a clear conclusion.

Table 2.1: Parental leave benefits for parents of children born before and on or after January 1, 2007

		Mothers' education	
	All	Low & medium	High
Pre-birth household annual net income in EUR	31,712.29	27,267.56	37,530.56
Children born before January 1, 2007: Erziehungsgeld			
% recipients for 1-6 months	77.25	84.13	71.07
% recipients for 6-12 months	47.11	52.98	39.80
% recipients for > 12 months	39.91	45.34	33.02
N	311	173	138
Children born on or after January 1, 2007: Elterngeld			
% recipients	nearly 100%	nearly 100%	nearly 100%
Monthly benefits of the mothers in EUR	634.28	562.72	771.12
% fathers taking parental leave	12.81	9.32	20.85
Monthly benefits of the fathers in EUR	1,060.52	864.11	1,190.43
N	197	124	73

Notes: Descriptive statistics on parental leave benefits for parents of children born two years before and two years after the 2007 German paid parental leave reform (2005 through 2008). Statistics exclude civil servants and self-employed mothers, and consider household weights in the year of birth of the child. Survey information is cleaned based on plausibility checks on duration, amount and eligibility criteria under consideration of the provided net household income information.
Source: Own calculations based on SOEPv30 for children born in 2005 through 2008.

2.3 Background

2.3.1 The 2007 German paid parental leave reform

To set the stage for our analysis, we first provide some information on the institutional background in which the parental leave reform was implemented (based on OECD, 2016b,c,d). In Germany in 2006, the maternal labour force participation rate of women aged 25-54 with at least one child aged 0-14 was 63% (OECD average 66.1%), the fertility rate was 1.33 children per woman (OECD average 1.69), and the day care participation rate for 0-2 year olds, including centre-based and family day care services, was 13.6% (OECD average 30%). Mothers were generally not allowed to work during the six weeks before and, without exception, the eight weeks after childbirth. Mothers who were employed prior to giving birth received a full wage replacement during this mother protection period. Moreover, parents taking leave had the right to return to their job within 36 months after childbirth.

Parents of children born before January 1, 2007, were eligible for child-rearing benefits. These publicly funded benefits were means-tested and families were eligible if their yearly net income was below a certain threshold, which varied with the household structure (couples/singles), number of children, and time since giving birth (for details, see the law *Bundeserziehungsgeldgesetz*). Once the net income exceeded the threshold, benefit amounts were reduced. In Table 2.1, we provide descriptive statistics on parental leave eligibility based on representative household data from the German Socio-Economic Panel Study (SOEP, Wagner et al., 2007). Overall, column 1 shows that 77% of parents were eligible for 300 Euros[7] of monthly benefits (about 11% of pre-birth net household income) for up to six months after childbirth. Due to repeated means-testing after 6 and 12 months, and lower household income eligibility thresholds, the share of eligible parents falls to 47% for 7 to 12 months after childbirth, and to 40% for benefits 12 to 24 months after childbirth. Part-time work of up to 30 hours per week was permitted in the benefit payment period.[8]

In 2006, the German government substantially reformed the paid parental leave regulations (*Bundeselterngeld- und Elternzeitgesetz*) pursuing four main objectives (Bujard, 2013). First, the new benefit aimed to safeguard family income during the first year after childbirth. Second, the reform intended to increase parental care time during the first year after childbirth. Third, the reform aimed at increasing mothers' economic independence by increasing the financial incentives for an earlier return to work during the second year after childbirth. Fourth, the reform sought to increase paternal involvement in child rearing. Fifth, the reform intended to increase fertility. The reform did not explicitly target child development.

The paid parental leave reform was passed in September 2006 and affected parents of children born on or after January 1, 2007. Instead of being means-tested, the new benefit depends on the average net labour income earned in the 12 months prior to giving birth. Parents taking paid parental leave receive monthly benefits equalling 67% of their average monthly pre-birth net earnings; the benefit is capped at 1,800

[7]In 2007, 1 Euro corresponded to about 1.30 USD.

[8]Parents eligible for benefits for up to 24 months could also choose higher benefits (450 Euros) for a period of up to 12 months. For children born in 2005 and 2006, only 10% of all parents chose this option (own calculations based on SOEPv30).

Euros per month. As before, low-income parents, or those who did not work prior to giving birth, still receive 300 Euros per month. In addition to the changed eligibility criteria and benefit amounts, the transfer period was reduced from 24 to 12 months. Two additional months were granted for single parents and if both partners take parental leave for at least two months.[9] The reform did not change the 36-month job protection period, the maternal protection period around childbirth, or part-time employment regulations during the benefit payment period.[10]

The take-up rate of the new paid parental leave benefits is almost 100% for mothers (German Federal Statistical Office, 2008), with average benefits of 634 Euros per month, and about 13% for fathers, with average benefits of 1061 Euros.

Table 2.2: Changes in parental leave benefits in the first two years after childbirth after the 2007 German paid parental leave reform

		Changes in paid parental leave	
		1st year after birth	2nd year after birth
Before 2007 reform: After 2007 reform:	Ineligible or eligible for up to 6 months Eligible	PPL benefits ↑ PPL duration ↑	No change[a]
Before 2007 reform: After 2007 reform:	Eligible Minimum benefits	No change	PPL benefits ↓ PPL duration ↓
Before 2007 reform: After 2007 reform:	Eligible > minimum benefits	PPL benefits ↑ PPL duration ·	PPL benefits ↓ PPL duration ↓

Notes: This table describes the effects on paid parental leave (PPL) eligibility and benefit payments depending on the pre-reform eligibility for paid parental leave and the amount of benefit payments after the reform. [a] A small share of parents receives up to 14 months of paid parental leave if the partner also takes parental leave for at least two months.
Source: Own compilation.

[9]The maximum length of 14 months of paid parental leave could be split flexibly between both parents, with a minimum of two months per parent. Approximately 96% of parents assign the main benefit period (>7 months) to the mother. In our observation period, 13% of fathers take paid parental leave, mostly for 2 months. Alternatively, parents can also choose to receive only half of the monthly benefits for a doubled period of time, i.e. for up to two years. Only 8% of parents choose this option (German Federal Statistical Office, 2008).
[10]After the reform, parents who work part-time receive a benefit that amounts to 67% of the difference between pre- and post-birth earnings.

2.3.2 Heterogeneous effects of the paid parental leave reform

Overall, families were affected differently by the reform, depending on parents' pre-birth earnings and household income (see Table 2.2). First, consider high-earning mothers and high-income households that were previously ineligible for paid parental leave benefits (or for only 6 months); after the reform, these mothers gained eligibility for 12 months of paid parental leave. Thereafter, almost nothing changes for these households.[11] Second, consider mothers with low pre-birth earnings and low household income; these mothers were previously eligible for two years of paid parental leave. After the reform, they still receive the minimum benefits of 300 Euros per month in the first year, but they lose eligibility for benefits in the second year after childbirth. Finally, households between these two groups were previously eligible for benefits of 300 Euros per month for up to 24 months, but now receive higher benefit payments only during the first year after childbirth. The likelihood for households to receive higher overall benefits increases with their pre-birth earnings. Because of the strong correlation between earnings and education, and pronounced assortative mating, the reform benefited high-SES families more than low-SES families in terms of eligibility expansions and benefit payments.

As the school degree serves as a very reliable proxy for an individual's earnings potential in Germany, it serves as an important predictor for pre-reform eligibility for paid parental leave. Unlike the US, the German educational system tracks students into separate schools depending on their academic potential (e.g. Dustmann, 2004). In general, this tracking system only allows individuals who have graduated from high-ability school tracks to study at university. Consequently, graduates from low and medium school tracks have never attended university, while about 50% of graduates from high-ability school tracks have completed university (based on supplemental data from the German Microcensus 2008). Thus, classifying individuals' education level by their school degrees is typical in Germany and will distinguish between groups with very different earnings potentials.

To illustrate this, columns 2 and 3 of Table 2.1 summarise the benefit amounts and durations by mothers' education. On average, highly educated mothers (i.e. with upper-secondary school certificates) were less likely than low- and medium-

[11] If both parents take paid parental leave, the maximum paid leave period is 14 months. In our observation period, the share of fathers taking parental leave is relatively low, and we abstract from this detail to ease the discussion.

educated mothers (i.e. with lower- and middle-secondary school certificates) to receive parental leave benefits before the reform. While only 40% of the highly educated mothers received parental leave benefits for more than six months, 53% of low- and medium-educated mothers did. For the second year after childbirth, only 33% of highly educated mothers and 45% of low- and medium-educated mothers received benefits.

After the reform, almost all mothers were eligible for parental leave benefits. Highly educated mothers receive, on average, 771 Euros per months, while lower educated mothers receive, on average, 563 Euros per month. In addition, twice as many fathers take (higher-paid) parental leave among the group of highly educated mothers, which further increases the total benefit duration by up to two months. We will distinguish our analysis by mothers' education (as a typical measure of SES) to identify reform effect on SES gaps in child development. Additionally, we will also distinguish by families' predicted pre-reform eligibility for parental benefits to ensure that we are not missing heterogeneous effects.

2.3.3 Expected reform effects on child development

Given these heterogeneous effects, how then would we expect the reform to affect child development, and SES development gaps? First, with respect to income changes, highly educated mothers experience a stronger increase in benefit payments and household income in the first year after childbirth (Wrohlich et al., 2012), which may causally improve children's outcomes (Dahl & Lochner, 2012; Løken et al., 2012). At the same time, the recent literature suggests that parental child investments hardly react to transitory income shocks compared to changes in permanent income (Carneiro & Ginja, 2016) and that unrestricted transfers have little impact on child development (Heckman & Mosso, 2014; Del Boca et al., 2016). Given that the reformed parental leave benefit can be interpreted as a transitory income shock or as an unrestricted transfer, the income effect of the reform on child development is ambiguous.

Second, how do we expect changes in maternal labour supply to affect child development? The reform led less-educated mothers to increase their labour supply in the second year after childbirth. In contrast, highly educated mothers reduced their labour supply in the first year after childbirth (Wrohlich et al., 2012; Kluve & Tamm, 2013; Bergemann & Riphahn, 2015; Geyer et al., 2015; Kluve & Schmitz, 2017). Highly educated mothers may, therefore, spend more time with their children in the first year after childbirth after the reform. Increases in maternal care time may positively affect children's outcomes through a stronger mother-child attachment (e.g. Berger et al., 2005), longer breastfeeding durations (e.g. Borra et al., 2012), and more interactions between mother and child (Del Bono et al., 2016). However, the effect of maternal time on child development depends on the activities parents perform with their children (e.g. Leibowitz, 1977; Todd & Wolpin, 2007; Del Bono et al., 2016). For instance, educational activities benefit child development more than recreational activities and are more frequently provided by higher-SES parents (e.g. Kalil et al., 2012; Gimenez-Nadal & Molina, 2013). As the reform increased maternal time in the first year after childbirth more strongly for high-SES households, the reform may widen the already existing SES gaps in child development. Still, it remains unclear whether the parental leave reform led to actual changes in interactions between parents and children relevant for child development.

Furthermore, potential effects of maternal labour supply on child development depend on the timing of returning to work, on hours worked, and on the quality of alternative care (e.g. Brooks-Gunn et al., 2010). In our setting, the availability of publicly funded day care for children under the age of three is very low and the main alternative to parental care is usually informal care provided by grandparents or other relatives (e.g. Hank & Buber, 2009). The reform changed maternal (mostly part-time) labour supply mainly after the first six months after childbirth (Bergemann & Riphahn, 2015; Welteke & Wrohlich, 2016). These margins, i.e. part-time employment occurring beyond the first six months after childbirth, with alternative care arrangements of comparable quality, usually have small effects on child development (e.g. Bernal & Keane, 2010).[12]

Overall, the net reform effect on child development depends on whether parents effectively change their investments (time and goods) in their children, and whether marginal returns to additional child investments are meaningful for child development.

[12]Small reform effects on higher-order births of low-income mothers (Cygan-Rehm, 2016) could also suggest a child-quality/-quantity trade-off (Becker & Lewis, 1974). However, Angrist et al. (2010) find no causal effect for such a trade-off, so that we deem a slightly smaller number of siblings in our setting (which is not supported by our data) not to be an important explanation for our findings.

2.4 Data

2.4.1 School entrance examinations

To answer this empirical question, we use administrative data from school entrance examinations covering the full population of one German federal state, Schleswig-Holstein.[13] Before entering primary school at the age of six, every child is medically screened by a public health paediatrician. The paediatrician examines children's development in numerous dimensions. Taking into account the results from several tests, the paediatrician ultimately provides an assessment of the child's school readiness.

The administrative records we use cover all children from three cohorts entering school between 2012-2014. A school entrance cohort includes children born between July of the previous year, and June of the year of school entry. The school entrance examinations are conducted in the six months before school entry. Typically, older children are called in first to the examination. The data includes detailed information about children's health and development, and some information about family characteristics, such as parental schooling, migration background, and living arrangements. This information is reported voluntarily by the accompanying parent (typically the mother). The data does not contain information about parental employment or income.

In our analysis, we focus on four dimensions of child development: children's language skills, motor skills, socio-emotional stability, and an overall assessment of their school readiness. These outcomes are important predictors of later educational attainment (e.g. Duncan et al., 2007; Grissmer et al., 2010), later health outcomes and labour market performance (e.g. Cunha et al., 2006; Blanden et al., 2007; Carneiro et al., 2007).

Paediatricians examine children's language development with respect to their ability to use prepositions, build plural words, and repeat pseudo-words. Children receive a score that determines whether or not their language development lags behind.

[13]Schleswig-Holstein covers 3.6% of the German population. We examine Schleswig-Holstein due to restricted data access in the other federal states. To assess the external validity of our analysis, Table A2.1 compares the demographic and socio-economic characteristics of the population of Schleswig-Holstein to the population in other federal states in West Germany. Schleswig-Holstein is very close to other West German averages, apart from migration background and the degree of urbanisation.

To assess motor skill development, children need to jump on one leg, stand on one leg, and jump over a line as many times as possible within 10 seconds. If they do not manage to meet specific thresholds, they are classified as having motor skill deficiencies. Socio-emotional development is clinically assessed by the paediatrician: children are classified as having socio-emotional problems if they receive medical or psychological treatment, or if the paediatrician diagnoses that further treatment is necessary.[14] In the data, we observe the paediatrician's assessment of children's developmental deficiencies in their language skills, motor skills and socio-emotional stability as binary indicators. We reverse the scales such that higher outcomes are associated with better skills. Some counties also report the specific test results of children on which the paediatricians base their binary assessments. Our conclusions are not sensitive to the type of variables used in the analyses (see Section 2.6.3).

Children's overall school readiness is assessed by the paediatrician taking into account the examination results and other (to the researcher) unobserved factors related to children's development. It is also recorded in the data as a binary variable. A negative school readiness assessment does not defer children's school entry, but indicates a child's need for additional supportive development measures. All children turning six before June 30 need to enter school in the same year. Delayed school entries are granted only exceptionally based on adverse health conditions of the child.[15]

2.4.2 Descriptive statistics

Our sample consists of 44,997 children.[16] Descriptive statistics of the full sample and the subsamples stratified by mothers' level of education are provided in Table

[14]In some counties, paediatricians base their assessment additionally on information from the Strength and Difficulties Questionnaire (Goodman et al., 1998, SDQ,). Our econometric framework accounts for differences between counties regarding the additional usage of the SDQ through county-examination-year fixed effects.

[15]For the school entry cohort 2013, about 1% of children were delayed. We tested whether the reform affected children's age at examination, an indicator for early or delayed school entry, and found a very small (0.012 months, sample mean 72.6 months) and statistically insignificant effect (see Appendix Table A2.2).

[16]We restrict the sample to children for whom we observe the outcomes in all of the four domains of child development. Missing information is unrelated to the 2007 German parental leave reform. We account for different sample compositions of counties across school entry cohorts with examination-year-county fixed effects. Children belonging to Danish minorities living in Schleswig-Holstein are marked in the data, and have been removed from the sample. Our main results remain robust when we include these children in our sample (results are available upon request).

Table 2.3: Descriptive statistics

	(1)	(2)	(3)	(4)	(5)
	All	Sample stratified by mothers' education			
	children	Low & medium	High	Difference	s.e.
Panel A: Child development outcomes measured at age 6					
Language skills (0/1)	0.715	0.671	0.777	-0.106	(0.005)
Socio-emotional stability (0/1)	0.810	0.776	0.851	-0.075	(0.004)
Motor skills (0/1)	0.825	0.803	0.860	-0.057	(0.004)
School readiness (0/1)	0.840	0.809	0.911	-0.102	(0.004)
Panel B: Child characteristics					
Age at examination in months	72.604	72.718	72.479	0.239	(0.053)
Girl (0/1)	0.488	0.491	0.491	-0.000	(0.005)
Birth weight in grams	3381.661	3359.819	3436.341	-76.522	(6.431)
Birth weight missing (0/1)	0.038	0.026	0.019	0.006	(0.002)
Years in day care (at age 6)	3.417	3.389	3.555	-0.165	(0.011)
Years in kindergarten missing (0/1)	0.242	0.239	0.240	-0.001	(0.005)
Migration background (0/1)	0.212	0.214	0.183	0.030	(0.004)
Migration background missing (0/1)	0.088	0.073	0.059	0.013	(0.003)
Panel C: Family background characteristics					
Mother's years of schooling	10.949	9.649	13.000	-3.351	(0.004)
Mother's education missing (0/1)	0.183	–	–	–	–
Father's years of schooling	11.672	11.039	12.300	-1.262	(0.017)
Father's education missing (0/1)	0.238	0.104	0.034	0.070	(0.003)
Child lives with both parents (0/1)	0.792	0.745	0.882	-0.138	(0.004)
Child lives with one parent (0/1)	0.141	0.175	0.086	0.089	(0.004)
Child with other living arrangements (0/1)	0.066	0.081	0.032	0.048	(0.003)
Child's living arrangement missing (0/1)	0.086	0.006	0.006	0.000	(0.001)
Home language is German (0/1)	0.850	0.854	0.877	-0.023	(0.004)
German is main language (0/1)	0.107	0.102	0.093	0.009	(0.003)
Home language foreign (0/1)	0.043	0.044	0.029	0.014	(0.002)
Home language missing (0/1)	0.039	0.014	0.014	0.000	(0.001)
Number of children of the family	2.195	2.193	2.150	0.043	(0.011)
N	44,997	22,492	14,256	36,748	

Notes: This table reports descriptive statistics for our main samples. "Low & medium" education refers to lower and medium-secondary school certificates. "High" education refers to upper-secondary school certificates (*Abitur*). The means have been calculated based on non-missing information. The column "Difference" reports the difference in characteristics between children from high and low/medium educated mothers.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

2.3. Panel A describes children's developmental outcomes in the school entry examinations. 71.5% of children reach a sufficient level of language competencies, 81% are considered stable in their socio-emotional development, 82.5% show a sufficient level of motor skills development, and 84% of children are considered ready for school. Stratifying the sample by mothers' education reveals considerable SES development gaps between children (columns 2 and 3). For example, children of less-educated mothers are 10 percentage points less likely to be ready for school. These SES differences in children's development are highly statistically significant (see columns 4 and 5).

Panel B of Table 2.3 provides information on child characteristics. Children are on average 72.6 months (6 years) old when they are examined. 48.8% of children in the sample are girls. The mean birth weight is 3382 grams. At the time of the examination, children have spent on average 3.4 years in day care. 21.2% of children have a migration background (i.e. one or both parents are born abroad). Panel C of Table 2.3 provides information on children's family background. Mothers have an average of 10.9 years of schooling, fathers have an average of 11.7 years of schooling.[17] About 79.2% of children live with both parents and 14.1% live with one parent. For 85% of the children in our sample, German is the only language spoken at home, in another 10.7% it is the main language. On average, 2.2 children live in the household. There again exist significant differences in most of these characteristics when we stratify the sample by mothers' education.

We also report the share of missing information in the covariates. Mothers' education information is missing for 18% of children. This should not be a problem for our analysis as missing information is not related to the introduction of the reform (see Section 2.5). We also conducted the analysis for the group of children with missing information on parental education, reaching the same conclusions.

In Table 2.4, we present OLS estimates from multivariate regressions showing that these child and family characteristics strongly correlate with the child development outcomes. The age of the child, birth weight, time spent in day care, and parental years of schooling all correlate positively with children's skill development. Across all measures, girls show higher skill development levels. Children with more siblings and those who are not living with both parents show lower levels of skill

[17]In the data, we observe the school certificate that parents hold and assign the typical length of schooling.

Table 2.4: Relations between child outcomes and child and family characteristics

	(1)	(2)	(3)	(4)
	Dependent variable:			
	Language skills	Soc. emot. stability	Motor skills	School ready
Age at examination in months	0.0052**	0.0039**	0.0066**	0.0083**
	(0.0008)	(0.0006)	(0.0008)	(0.0009)
Girl	0.0717**	0.0807**	0.1223**	0.0829**
	(0.0040)	(0.0035)	(0.0035)	(0.0033)
Birth weight in grams $*10^{-4}$	2.2406**	2.8822**	3.4318**	5.1665**
	(0.3523)	(0.3184)	(0.3266)	(0.3130)
Years child spent in day care	0.0135**	0.0123**	0.0124**	0.0175**
	(0.0029)	(0.0026)	(0.0026)	(0.0023)
Mother's years of schooling	0.0318**	0.0215**	0.0177**	0.0293**
	(0.0015)	(0.0013)	(0.0013)	(0.0012)
Father's years of schooling	0.0064**	0.0062**	0.0030	0.0007
	(0.0019)	(0.0017)	(0.0016)	(0.0014)
Number of children of the family	-0.0281**	-0.0073**	-0.0048**	-0.0240**
	(0.0020)	(0.0018)	(0.0018)	(0.0018)
Living arrangements (reference: child lives with both parents)				
Child lives with one parent	-0.0452**	-0.0771**	-0.0272**	-0.0535**
	(0.0068)	(0.0063)	(0.0059)	(0.0058)
Child lives in other living arrangements	-0.0438**	-0.1154**	-0.0574**	-0.0802**
	(0.0092)	(0.0090)	(0.0084)	(0.0084)
Migration background (reference: no migration background)				
One parent born abroad	-0.0265**	0.0151*	0.0095	0.0018
	(0.0081)	(0.0071)	(0.0070)	(0.0066)
Both parents born abroad	-0.1361**	0.0393**	0.0112	-0.0383**
	(0.0103)	(0.0084)	(0.0084)	(0.0088)
Language spoken at home (reference: Home language is German)				
German is main language	-0.0742**	0.0178*	0.0131	-0.0356**
	(0.0098)	(0.0080)	(0.0079)	(0.0083)
Home language foreign	-0.1351**	-0.0079	0.0081	-0.1219**
	(0.0135)	(0.0114)	(0.0114)	(0.0126)
Missing information				
Mother's education missing	-0.0147	-0.0046	-0.0163	-0.0310**
	(0.0101)	(0.0094)	(0.0089)	(0.0092)
Father's education missing	-0.0215*	-0.0203*	-0.0044	-0.0200*
	(0.0090)	(0.0085)	(0.0080)	(0.0081)
Birth weight missing	-0.0141	0.0219*	0.0044	-0.0408**
	(0.0107)	(0.0089)	(0.0090)	(0.0104)
Years in kindergarten missing	-0.0419**	-0.0275**	-0.0246**	-0.0654**
	(0.0097)	(0.0081)	(0.0079)	(0.0092)
Living arrangement missing	0.0035	-0.0120	-0.0076	0.0127
	(0.0152)	(0.0132)	(0.0131)	(0.0139)
Migration background missing	-0.0088	-0.0154	0.0045	-0.0420**
	(0.0116)	(0.0106)	(0.0101)	(0.0110)
Home language missing	0.0145	0.0274*	0.0022	-0.0066
	(0.0150)	(0.0125)	(0.0128)	(0.0137)
Number of children missing	0.0395	0.0548**	0.0274	0.0616*
	(0.0251)	(0.0208)	(0.0220)	(0.0248)
Sample mean	0.715	0.810	0.825	0.840
N	44,997	44,997	44,997	44,997

Notes: This table reports multivariate OLS results of the child outcome on the variables listed in the rows. All regressions include examination year-by-county fixed effects, birth months fixed effects and birth cohort fixed effects and dummies for missing variables. Missing values are imputed (zero-category for dummy variables and sample means for continuous variables). Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

development. Children's migration background and the language spoken at home correlate negatively with children's language skills and their school readiness. We also report on the relationship between missing information and child outcomes. Information on parental education is missing for children with slightly lower levels of development. The relationships between child and family characteristics and child development are common in the literature (for reviews, see, e.g. Bradley & Corwyn, 2002; Maggi et al., 2010) and validate the relevance of the analysed dimensions of child development.

2.5 Empirical strategy

Our aim is to estimate the intention-to-treat effect (ITT) of the 2007 German paid parental leave (PPL) reform on children's development. The reform applies a sharp eligibility criterion based on children's birth dates, so that we could compare the developmental outcomes of children born shortly before and after the cut-off date. Such a comparison of means yields unbiased estimates of the reform effects if the birth date is as good as randomly assigned around the policy cut-off. However, as one broadens the window of comparison around the cut-off, this approach risks confounding the estimates of the reform effect with seasonal and age-at-examination effects. To eliminate these potential biases, we use children born in the same months but in years not affected by policy changes (both pre- and post-reform) as our control group. Similar to, for example, Dustmann & Schönberg (2011) and Danzer & Lavy (2016), this methodology combines a regression discontinuity design with a difference-in-differences approach to account for seasonal or age-at-examination effects.

This empirical framework relies on two main assumptions to produce unbiased estimates of the reform effect. The first assumption requires common trends in seasonal effects and age-at-examination effects of reform cohorts and control cohorts in the absence of the reform. We run several checks that support the plausibility of this assumption (see Section 2.6.3).[18]

[18]The availability of publicly funded day care in Schleswig-Holstein for children aged below the age of three experienced a continuous expansion from 7.5% in 2006 to 21.6% in 2011 (German Federal Statistical Office, 2012). Our identification strategy is not affected by this expansion as it relies on the birthday eligibility cut-off of the reform. The day care expansion affects children in reform cohorts and control cohorts born before and after the cut-off similarly.

The second assumption is that the reform does not impact the composition of child and family characteristics of children born in specific months. Three potential concerns may violate this assumption. First, strategic manipulations of birth dates could change the sample composition. The new parental leave law passed parliament in September 2006 and applied to all children born on or after January 1, 2007. Mothers giving birth in the neighbourhood of the cut-off date had already conceived when the law was passed. Still, parents may manipulate the actual birth dates around the reform cut-off through planned cesarean sections and labour inductions. Indeed, Neugart & Ohlsson (2013) and Tamm (2013) find that about 8% of births were shifted from the last week of December to the first week of January in a manner consistent with the economic incentives of the reform. Such strategic birth shifting introduces an endogenous sample selection bias around the reform cut-off. We address this concern by excluding children born in December and January from our main samples.[19]

Another concern regarding the sample composition would be reform effects on fertility patterns. If so, then children born in the year after the treatment cohort may show different child and family background characteristics. In Section 2.6.3, we show that our conclusions are the same if we exclude children born in the year after the treatment cohort from the sample. Still, we include the preceding and the subsequent cohorts in our control group to increase the sample size for more precise estimates.

The third concern is that children may select into (or out of) the sample because of the parental leave reform, e.g. by not attending the examination. However, participation in the school entrance examination is mandatory. We explicitly test for the balance of observable characteristics of the children and their family background in our samples, i.e. one sample covering all children, and two subsamples stratified by mothers' level of education (see Appendix Table A2.2). As we would expect, the covariates are balanced across all samples.

The choice concerning the width of the comparison windows around the reform cut-off generates a trade-off between the precision of estimates and potential biases through differences in observable and unobservable characteristics. To maximise precision, we compare children born up to six months before and after the reform

[19]Ideally, we would only drop individuals within a two-week window around January 1, but for data protection reasons the data lacks information on the exact date of birth. Our main conclusions are robust to including children born in December and January (see Section 2.6.3).

in reform cohort and control cohorts in our main specification. Empirically, we estimate the following regression model:

$$\begin{aligned} Y_i = &\beta_1\, treat_i + \beta_2\, after_i + \beta_{PPL}\,(treat_i \cdot after_i) + birth\ month_i'\delta \\ &+ birth\ cohort_i'\phi + (county_i \cdot examination\ year_i)'\theta + X_i'\gamma + \epsilon_i \end{aligned} \tag{2.1}$$

where Y_i describes the developmental outcome for child i. The variable $after_i$ is an indicator variable taking the value of 1 if child i is born between January and June, and 0 if born between July and December. The variable $treat_i$ identifies children who belong to the treatment cohort that was affected by the 2007 parental leave reform. It takes the value of 1 if child i is born between July 2006 and June 2007, and 0 if born between July 2005 and June 2006, and July 2007 and June 2008. The coefficient β_{PPL} identifies the intention-to-treat effect of the 2007 parental leave reform. Eligibility and take-up of paid parental leave under the new legislation was almost 100% (German Federal Statistical Office, 2008). In this case, the intention-to-treat effect almost corresponds to the average treatment effect on the treated (Angrist & Imbens, 1996).

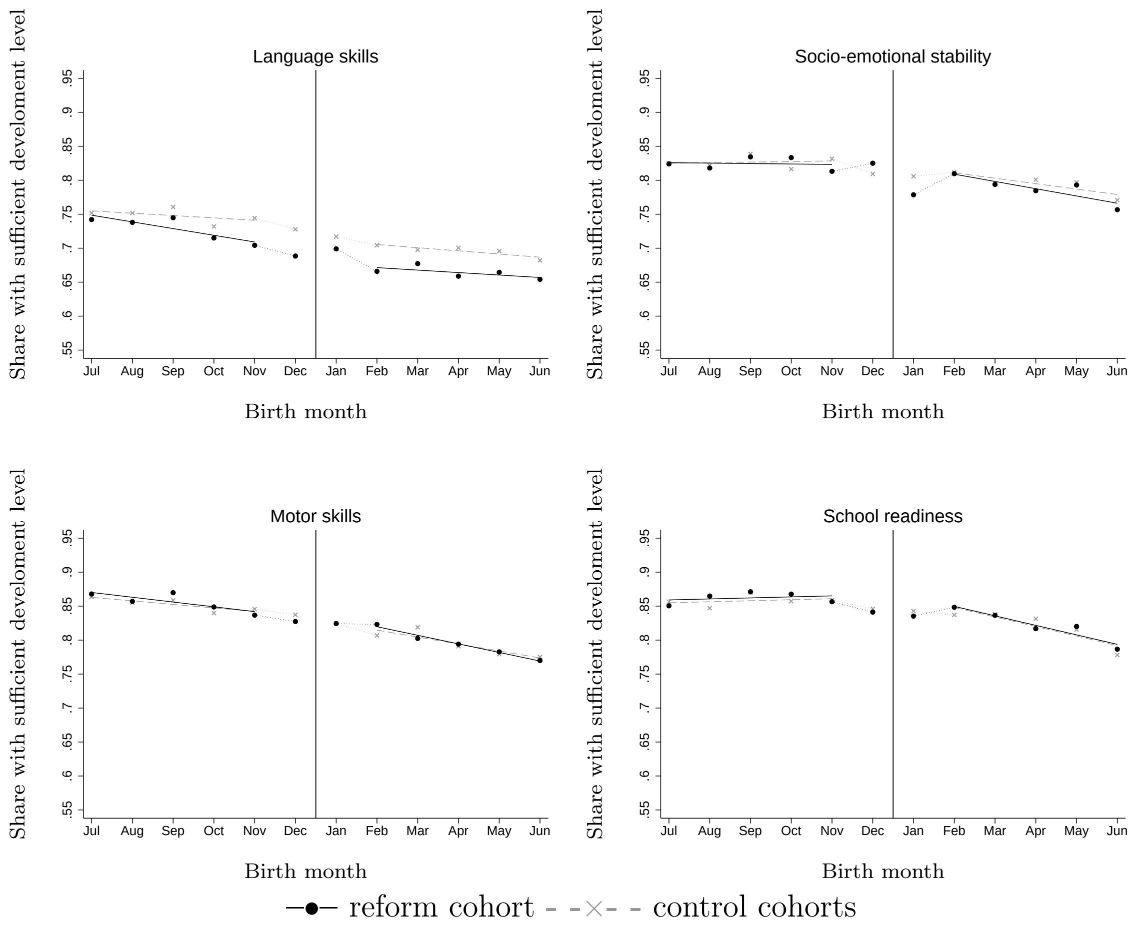
We further include birth month fixed effects ($birth\ month_i$), cohort fixed effects ($birth\ cohort_i$), and county-year-of-examination fixed effects ($county_i \cdot examination\ year_i$). To increase the precision of the estimates, we sequentially include additional control variables for child and family characteristics in our regressions (X_i, containing a quartic in child's age, gender, birth weight, indicators for father's and mother's education, and indicators for whether one or both parents have a migration background).

2.6 Results

2.6.1 Effects on child development and SES development gaps

We now document how the 2007 German parental leave reform affected children's development and first illustrate our results with a series of graphs. Figure 2.1 plots the average child outcomes by month of birth separately for children of the reform and control cohorts. We fit linear trends separately for children born on either side of the reform cut-off on January 1 and for children of the reform and control cohorts. We also plot average outcomes for children born in December and January, which we

Figure 2.1: Effects of the 2007 German parental leave reform on child development



Notes: The figure plots the share of children diagnosed with a sufficient level of the respective skill for children born 6 months before and 6 months after the new parental leave legislation in Germany (reform cohort), and for children born in the same months in the year before and the year after (control cohorts). The vertical bar between December and January indicates the introduction of the reform on January 1, 2007. The solid and dashed lines represents linear fits for children in our main sample. The dotted lines refer to children in months that are likely to be affected by birth date manipulations. They are exempted from our main analyses.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

drop from our main estimations (see Section 2.5). Figure 2.1 shows that the trends in outcomes are fairly smooth around the cut-off for both the treated and control cohorts. Furthermore, we do not observe level shifts in child outcomes after the cut-off compared to the control group. This provides some first visual evidence that the paid parental leave reform did not have substantial effects on child outcomes. The graphs also support the common trend assumption as the trends over birth months are almost parallel between the reform and control cohorts.

To corroborate the visual evidence, Table 2.5 reports the estimates based on equation 2.1. The rows in Table 2.5 denote the four different dependent variables. In column 1, we only include fixed effects for birth months, birth cohorts, and county-year-of-examination. Across the four different dimensions of child development, the point estimates are very small and statistically insignificant. In columns 2 and 3, we gradually add control variables for child and family characteristics (including dummies for missing variables). While the explanatory power of the model increases substantially with the inclusion of further control variables, the reform estimates remain very similar across the different specifications. The estimation results from column 3 show that the reform affected the probability of being diagnosed with a sufficient level of language skills by -0.0074 (sample mean of 0.715), of socio-emotional stability by -0.0035 (sample mean of 0.810), of motor skills by -0.0067 (sample mean of 0.825), and of being ready for school by 0.0048 (sample mean of 0.840). Given the fiscal size of the reform, these effects are tiny: Using a two-sided t-test with 95% confidence intervals, we can rule out positive effects that are on average larger than 1.5% for language skills and socio-emotional stability, 1% for motor skills, and 2.2% for school readiness. Similarly, we can rule out negative effects greater than 2.5% for language skills and 1.9% for all other dimensions. The stability of coefficients across columns 1-3 implies that the treatment indicator is unrelated to other determinants of children's development. As these additional controls increase the precision of our estimates, we proceed with our preferred specification from column 3.

In columns 4 and 5, we estimate the model separately by mothers' education. Again, we find that the effects of the paid parental leave reform are very small across the four domains of child development, independent of maternal education (for graphical evidence, see Appendix Figure A2.2).[20] In column 6, we statistically

[20]We also estimated the reform effects on children with missing information about mothers' level of education. The effects are also small and insignificant with coefficients (standard errors) of

Table 2.5: Main results: Estimated effects of the parental leave reform on child development and SES development gaps

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		All children			Mothers' education			Predicted pre-reform eligibility for parental leave benefits	
	Mean (SD)				Low & medium	High	Δ SES gap =col. (5)-(4)	Previously eligible	Previously ineligible
Language skills	0.715	-0.0079	-0.0065	-0.0074	-0.0181	-0.0092	0.0089	-0.0034	-0.0019
	(0.451)	(0.0094)	(0.0092)	(0.0091)	(0.0134)	(0.0157)	(0.0206)	(0.0169)	(0.0166)
		[0.0734]	[0.1110]	[0.1327]	[0.1286]	[0.1007]		[0.1565]	[0.0868]
Socio-emot. stability	0.810	-0.0035	-0.0029	-0.0035	-0.0127	0.0112	0.0238	-0.0211	0.0076
	(0.392)	(0.0082)	(0.0082)	(0.0081)	(0.0120)	(0.0135)	(0.0181)	(0.0154)	(0.0144)
		[0.0594]	[0.0777]	[0.0951]	[0.1042]	[0.0777]		[0.1017]	[0.0846]
Motor skills	0.825	-0.0067	-0.0062	-0.0067	-0.0051	-0.0127	-0.0076	-0.0139	-0.0056
	(0.380)	(0.0079)	(0.0078)	(0.0077)	(0.0115)	(0.0127)	(0.0172)	(0.0148)	(0.0139)
		[0.0375]	[0.0714]	[0.0802]	[0.0842]	[0.0765]		[0.0764]	[0.0823]
School readiness	0.840	0.0041	0.0054	0.0048	0.0032	0.0079	0.0047	0.0072	0.0074
	(0.366)	(0.0076)	(0.0074)	(0.0073)	(0.0110)	(0.0105)	(0.0152)	(0.0155)	(0.0110)
		[0.0384]	[0.0802]	[0.1110]	[0.1139]	[0.0660]		[0.1144]	[0.0618]
N	44,997	44,997	44,997	44,997	22,492	14,256	36,748	12,836	12,481
Control variables									
Child characteristics		No	Yes	Yes	Yes	Yes		Yes	Yes
Family characteristics		No	No	Yes	Yes	Yes		Yes	Yes

Notes: This table reports the coefficient estimates of the parental leave reform effect (β_{PPL}) on child outcomes and on development gaps between children from low/medium and high educated mothers. All regressions are based on equation 2.1 and include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects and dummies for missing variables. The stratification in columns 7 and 8 are based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 12-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. R^2 are reported in brackets. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

test whether the parental leave reform consequently affects socio-economic gaps in child development at age six. The effect estimates on the SES gaps are all very small compared to the SES gaps in Table 2.3 and not statistically significant. The point estimate on socio-emotional stability of children suggests an increase of the gap (0.0238), but it is not statistically different from zero.

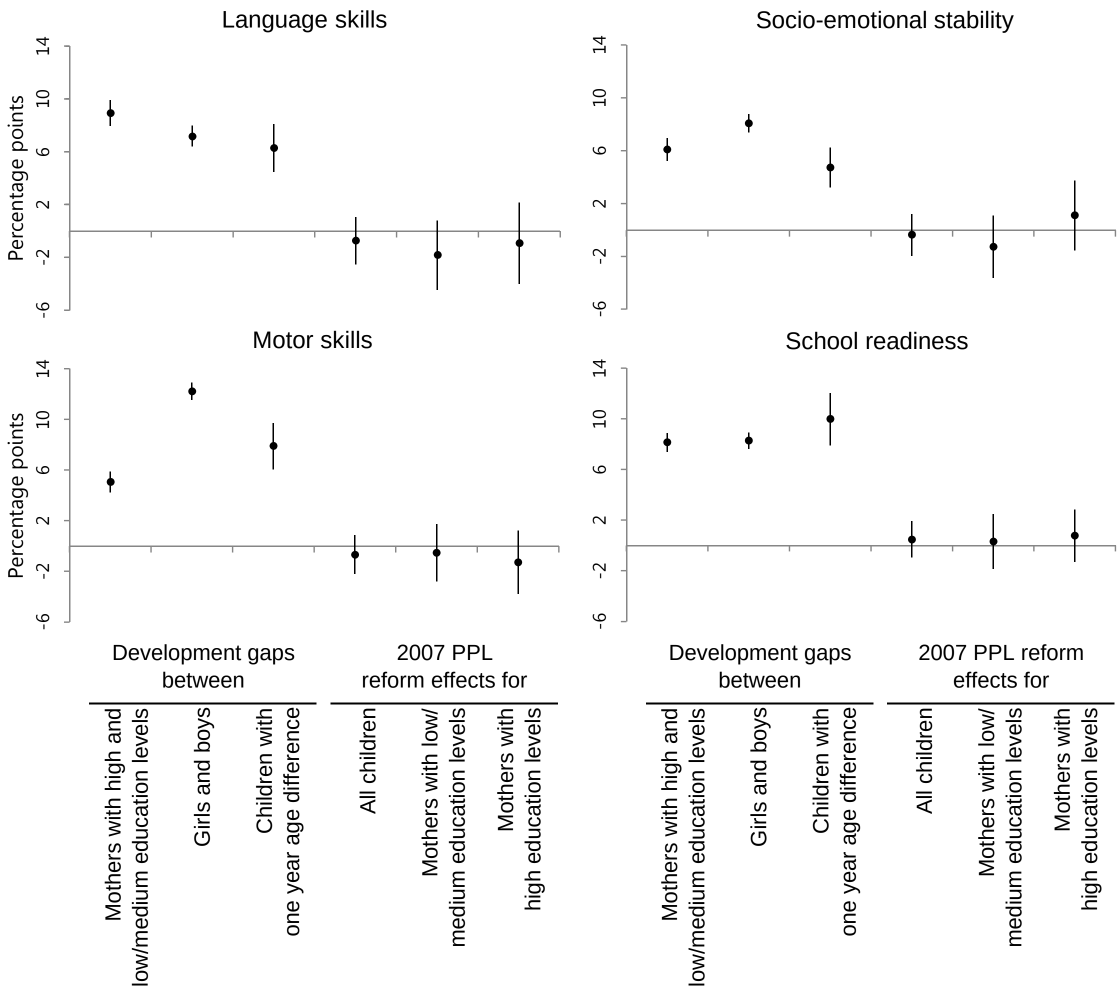
Figure 2.2 summarises our main findings graphically. It relates the estimated reform effects to the estimated coefficients on the child development gaps in terms of mothers' education, the gender and the child's age (based on Table 2.4). Across all four outcomes, Figure 2.2 shows that our estimates are small in magnitude compared to the development gaps by mothers' education, gender, and children's age, and they are precisely estimated.

Even though education is a reliable and widely used proxy for socio-economic status in Germany, the distinction by parental education may not entirely capture the different changes the reform had on lower and higher income families: Recall that certain families – especially lower-income households – were *previously eligible* for parental leave benefits for up to two years and lost eligibility for paid parental leave beyond the first year due to the reform. Other families (with higher household income) were *previously ineligible* and gained access to paid parental leave in the first year after childbirth due to the 2007 reform. While we cannot observe pre-reform benefit eligibility directly in our main data set, it still contains useful information to predict pre-reform eligibility: the household structure, the number of children, migration background and parents' education (which is highly correlated with their earnings potential). To make best use of this information, we supplement our analysis with information from the SOEP data to predict whether individuals in our main data were likely previously eligible for parental leave benefits.

Based on a sample of children born in 2005 and 2006, we first use the SOEP and generate variables on family characteristics as we observe them in the administrative data set. Based on these characteristics, we use a logit model to first predict the pre-reform eligibility for benefits for 12-24 months. This helps identify the group of *previously eligible* parents who lost eligibility for paid parental leave in the second year after birth due to the 2007 reform. Second, we predict the pre-reform eligibility for benefits for 6-12 months to identify the group of *previously ineligible* parents

0.0095 (0.0200) for language skills, -0.0026 (0.0181) for socio-emotional stability, -0.0067 (0.0178) for motor skills and 0.0010 (0.0183) for school readiness. The respective graphs are provided in Appendix Figure A2.2.

Figure 2.2: Comparing child development gaps to parental leave reform effects



Notes: The figure plots child development gaps at school entrance (coefficient estimates retrieved from Table 2.4, coefficient on age is scaled), and estimated treatment effects of the 2007 German paid parental leave (PPL) reform for all children, for children from low/medium educated mothers and from highly educated mothers. Bars indicate the 95% confidence interval of the estimated coefficients.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

who gained access to paid parental leave due to the 2007 reform. The predicted probabilities are plotted in Panel A of Appendix Figure A2.3.[21] We then take the estimated coefficients and predict pre-reform eligibilities in our main administrative data set. Reassuringly, the predicted probabilities in the original SOEP sample and the administrative data set match closely, suggesting that the characteristics are similarly distributed (see Panel B of Appendix Figure A2.3). In our administrative data, we classify parents as *previously eligible* if their predicted probability for pre-reform benefits for up to 24 months lies above 0.75. Furthermore, we classify parents as *previously ineligible* if their predicted probability for pre-reform benefits for 6-12 months lies below 0.25. We focus on predicted probabilities above 0.75 and below 0.25 as the model predicts about 80% correctly in the SOEP for these groups.

Columns 7 and 8 of Table 2.5 report the estimated reform effects separately for parents who were likely *previously eligible* (i.e. they lost paid leave due to the reform), and parents who were *previously ineligible* (i.e. they gained access to paid parental leave due to the reform). The estimates again do not reveal any statistically significant effect on children of these two very different groups. Moreover, none of the differences between both groups are significant (for graphical evidence, see Appendix Figure A2.4). While the more complex prediction may approximate household income and pre-reform eligibility more closely than parental education alone, it ensures that the parental benefit reform indeed had a very limited impact on child development and child development gaps.

2.6.2 Further treatment effect heterogeneities

Ample evidence suggests that boys typically react more sensitively to changes in early childhood conditions (e.g. Waldfogel, 2006), and even more so in low-SES families (e.g. Autor et al., 2016) Therefore, we now split the samples by children's gender to consider further heterogeneities in treatment effects of the parental leave reform (see Table 2.6). The results in column 1 show that the treatment effects are qualitatively very small and not statistically different from zero for both girls

[21] The regressors in the prediction include dummies for mothers' education and fathers' education, their interaction, a dummy for single parents, the number of children of the family, a dummy for migration background, and an interaction term of mothers' education and the number of children. The sign of the coefficient estimates are consistent with the institutional rules: for instance, the probability for eligibility increases with the number of children and single motherhood, and it decreases with the education level of the parents.

Table 2.6: Heterogeneity analysis by gender, parental education and pre-reform eligibility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Mothers' education		Fathers' education		Predicted eligibility for parental leave benefits	
	All	Low/med.	High	Low/med.	High	Previously eligible	Previously ineligible
Girls: Language skills	0.0069	-0.0093	0.0071	0.0064	-0.0152	0.0139	0.0065
	(0.0128)	(0.0189)	(0.0216)	(0.0199)	(0.0217)	(0.0238)	(0.0228)
Boys: Language skills	-0.0202	-0.0253	-0.0255	-0.0272	-0.0120	-0.0211	-0.0095
	(0.0130)	(0.0190)	(0.0226)	(0.0201)	(0.0226)	(0.0242)	(0.0239)
Girls: Socio-emo. stability	-0.0142	-0.0306	0.0069	-0.0223	-0.0174	-0.0176	-0.0177
	(0.0109)	(0.0163)	(0.0179)	(0.0170)	(0.0176)	(0.0210)	(0.0185)
Boys: Socio-emo. stability	0.0053	0.0028	0.0132	0.0012	0.0254	-0.0225	0.0271
	(0.0119)	(0.0175)	(0.0200)	(0.0186)	(0.0196)	(0.0224)	(0.0216)
Girls: Motor skills	-0.0038	0.0007	0.0007	0.0029	-0.0068	-0.0174	-0.0130
	(0.0095)	(0.0144)	(0.0146)	(0.0146)	(0.0155)	(0.0190)	(0.0162)
Boys: Motor skills	-0.0103	-0.0115	-0.0271	-0.0195	-0.0144	-0.0089	-0.0014
	(0.0120)	(0.0176)	(0.0202)	(0.0187)	(0.0202)	(0.0226)	(0.0221)
Girls: School readiness	0.0009	-0.0079	0.0145	-0.0038	0.0034	-0.0027	0.0153
	(0.0094)	(0.0143)	(0.0124)	(0.0145)	(0.0130)	(0.0207)	(0.0124)
Boys: School readiness	0.0076	0.0114	0.0013	-0.0003	0.0160	0.0154	0.0008
	(0.0110)	(0.0166)	(0.0166)	(0.0172)	(0.0170)	(0.0228)	(0.0175)
Number of girls	21,981	11,033	6,994	9,875	6,929	6,275	6,156
Number of boys	23,016	11,459	7,262	10,188	7,278	6,561	6,325

Notes: This table reports the estimation results of the parental leave reform on child outcomes on samples stratified by gender, different definitions of parental education, and pre-reform eligibility for parental leave benefits. Each coefficient comes from a separate regression. All regressions include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. The stratification in columns 6 and 7 are based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 12-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

and boys. In addition, no statistically significant differences of the treatment effects exist between the groups.

When we stratify the samples of girls and boys further by mothers' education (columns 2 and 3), the main picture remains the same. Unlike Danzer & Lavy (2016), we cannot detect significant treatment effects at this subgroup level.[22] Treatment effects are neither statistically different from zero, nor are there statistically significant differences of the treatment effects between girls and boys. In columns 4 and 5, we stratify the sample by fathers' education. The same picture emerges, but the effects are less precisely estimated as the data lacks more information on fathers' than on mothers' education.

In columns 6 and 7, we stratify the sample by the predicted pre-reform eligibility for parental leave benefits as described in Section 2.6.1. Again, we cannot find any evidence of treatment effects of the 2007 reform on children's development.[23]

Finally, we analyse whether the reform affected child development in other parts of the child development distribution that the development indicators employed in the main analysis miss. As some counties provide information on the specific test results the binary assessments are based on, we repeat the main analysis on the continuous measures for children's language skills, socio-emotional stability, and motor skills, and additionally on dummies representing different positions of children in the specific test distribution (see Appendix Table A2.3). The table reports the mean score in column 1, the average reform effect on the continuous outcomes variable in column 2, and reform effects on positions in the distribution in columns 3 to 7. The reported estimates show that we neither find any effect on average nor do we find evidence for any systematic patterns across the distribution of child development in language skills, motor skills and socio-emotional behaviour. This detailed distributional analysis ensures that our main analysis does not hide any effect heterogeneities across the skill distribution.

[22] Danzer & Lavy (2016) find that the 1990 paid parental leave expansions in Austria had positive (negative) effects on sons of highly (low-) educated mothers. The differences in findings are likely due to differences in the child development phases, as well as the usage and quality of alternative care arrangements. In our setting, the availability of publicly funded day care is very low, and the common alternative child care is provided by grandparents and relatives. The quality differences to maternal care are presumably small. Alternative care provided by the universal day care system in Germany is of relatively high quality (e.g. Spiess, 2008). We checked for different effects across counties with day care availability below and above the median availability rate for children aged 0-2; we again do not find substantial or significant differences between these groups.

[23] We cannot run the analysis by children's birth order due to data limitations.

Table 2.7: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Baseline	Window size without January & December				Including children born		Without children	Nonlinear models (marginal effects)	
	(Jul-Jun)	Aug-May	Sep-Apr	Oct-Mar	Nov-Feb	7/2004-6/2005	in Jan & Dec	born after 6/2007	Probit	Logit
Language skills	-0.0074	-0.0052	-0.0051	0.0064	-0.0035	-0.0121	-0.0037	-0.0022	-0.0055	-0.0050
	(0.0091)	(0.0101)	(0.0117)	(0.0144)	(0.0207)	(0.0086)	(0.0083)	(0.0104)	(0.0087)	(0.0087)
Socio-emot. stability	-0.0035	-0.0005	-0.0038	0.0013	0.0174	-0.0072	-0.0113	-0.0020	-0.0037	-0.0030
	(0.0081)	(0.0089)	(0.0102)	(0.0125)	(0.0179)	(0.0076)	(0.0073)	(0.0094)	(0.0078)	(0.0078)
Motor skills	-0.0067	-0.0033	-0.0058	-0.0011	0.0207	-0.0089	-0.0042	-0.0070	-0.0078	-0.0081
	(0.0077)	(0.0086)	(0.0099)	(0.0122)	(0.0175)	(0.0073)	(0.0070)	(0.0090)	(0.0076)	(0.0077)
School readiness	0.0048	0.0029	-0.0001	0.0026	0.0135	-0.0000	0.0033	0.0120	0.0045	0.0042
	(0.0073)	(0.0080)	(0.0092)	(0.0112)	(0.0161)	(0.0069)	(0.0067)	(0.0084)	(0.0073)	(0.0073)
N	44,997	35,552	26,166	17,157	8,315	60,590	53,627	29,016	44,997	44,997

Notes: This table reports robustness checks of the estimated reform effect of the parental leave reform on child outcomes. The window size around the reform-cut off and definitions of the control group are varied. Further, robustness to non-linear model specifications is tested. All regressions include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

2.6.3 Sensitivity checks

Table 2.7 presents various robustness checks to assess the sensitivity of our results to varying sample definitions and model specifications. The empirical strategy rests on the assumption that age-at-examination effects and seasonal birth effects are the same for the treated and the control cohorts. While a larger comparison window increases the sample size and statistical power, the potential impact of such age and seasonal effects may increase as one moves away from the reform cut-off. To check this potential issue, we gradually narrow the window of comparison from six to two months on both sides of the cut-off across columns 2 to 5.[24] Our results show that the estimated coefficients are still small, and not statistically different from our main specification. As we would expect, the standard errors of the estimated treatment effects increase as we narrow the window of comparison. We conclude that our main results are not biased by using a six-month window.

Alternatively, we could include further control cohorts from earlier years. While additional control cohorts may increase the precision of the estimates, these cohorts may also confound the estimated effects, for example, because of different unobserved treatments to the control cohorts. Column 6 shows that including children born between July 2004 and June 2005 in the control group increases the sample size by about one third, but the coefficients do not change much. The gain in the precision of our estimates is small.

To further increase precision, we pool all outcomes and estimate the reform effect with our main specification additionally allowing for level differences in the outcomes. While we need to assume that the reform had the same impact on all outcomes, we increase the sample size to obtain even more precise estimates and reach the same conclusions (see Appendix Table A2.5).

Given the evidence of birth shifting that is related to potential reform benefits, we dropped children born in December or January from our main specifications. In column 7, we include children born in December and January in our main sample and draw the same conclusions.

Next, we assess whether any endogenous fertility effects may bias our estimates. Fertility responses might affect children from the control group born one year after

[24] Predetermined variables are balanced across all window sizes, see Appendix Table A2.4.

the treatment cohort. Column 8 shows that excluding children born after June 2007 generates estimates that are very similar to our main specification.

Since our outcome variables are measured as dummy variables, columns 9 and 10 report the marginal effects on the interaction term of equation 2.1 from probit and logit models (Puhani, 2012). The estimated effects are very similar to our main results.

We assess the plausibility of the common trend assumption with a set of robustness checks reported in Table 2.8. First, we substitute the birth months fixed effects from our main model with linear (column 2) and quadratic (column 3) cohort-specific time trends. The treatment effect is now identified by differential jumps in the trends on January 1 between reform and control cohorts; reassuringly, we reach the same conclusions.

We additionally run two placebo policy reforms at points in time in which no treatment occurred. In the first placebo test, we pretend that the reform was implemented one year earlier. The second test assumes that the parental leave reform was implemented on April 1, 2007. For the second placebo test, we restrict the sample to children born three months before and after the placebo cut-off to avoid overlaps with the real cut-off, and specify the regression model analogously to equation 2.1. The estimates of both placebo tests are reported in columns 4 and 5. The small and insignificant placebo estimates support our underlying common trend assumption.

Finally, we use a basic regression discontinuity design to identify the treatment effects. Given only five birth months on each side of the cut-off, we assume a linear trend in the outcome variables across birth months for children born between July 2006 and June 2007 and identify the treatment effect with an indicator for children born on or after January 1, 2007. The results are reported in column 6 of Table 2.8 and support our main conclusion.[25]

We also run all robustness checks separately by maternal education (Appendix Tables A2.6 and A2.7) and predicted pre-reform eligibility (available upon request). Our conclusions are robust for the subsamples.

[25]As in the main specification, the underlying sample excludes children born in December and January. The results are similar if we include them in the sample.

Table 2.8: Common trend checks

	(1)	(2)	(3)	(4)	(5)	(6)
		Cohort-specific time trends		Placebo reforms		
	Baseline	Linear	Quadratic	One year earlier	Mar/Apr 2007	Regression discontinuity
Language skills	-0.0074	0.0092	0.0090	-0.0095	-0.0069	-0.0145
	(0.0091)	(0.0239)	(0.0239)	(0.0099)	(0.0131)	(0.0202)
Socio-emot. stability	-0.0035	0.0107	0.0102	-0.0010	-0.0138	0.0005
	(0.0081)	(0.0210)	(0.0210)	(0.0089)	(0.0117)	(0.0176)
Motor skills	-0.0067	0.0135	0.0132	0.0027	-0.0030	0.0101
	(0.0077)	(0.0204)	(0.0204)	(0.0087)	(0.0116)	(0.0170)
School readiness	0.0048	-0.0033	-0.0038	-0.0087	-0.0054	0.0070
	(0.0073)	(0.0191)	(0.0191)	(0.0082)	(0.0107)	(0.0159)
N	44,997	44,997	44,997	44,997	21,540	13,998

Notes: This table reports the results of sensitivity checks to alternative model specifications for the common trend assumption. It also reports the results from placebo regressions. All regressions include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

2.7 Discussion

Despite the substantial changes resulting from the 2007 paid parental leave reform in Germany, we find no evidence that the reform impacted any of the four important dimensions of child development and SES development gaps. One explanation could be that we miss out heterogeneous treatment effects. The reform affected families differently depending on their household income and mothers' pre-birth earnings. Although we lack this information in our data, we run our analyses in subsamples stratified by parental education and by predicted pre-reform eligibility and cannot detect any effects on child development. We further stratify the subsamples by gender and still do not detect any effects of the reform. As we benefit from a large sample size and professional medical screenings, we should be able to detect even small effects for these groups. Therefore, we would expect that a further refinement of groups would also not lead to considerable treatment effects.

More likely explanations for the zero-reform effects are provided by the recent economic literature on determinants of child development.[26] We discuss the po-

[26]Given that the production of children's skills is a very complex process in which the timing of investments, dynamic complementarities in inputs, self-productivity of skills, and endogenous re-

tential channels for the two groups experiencing the most extreme reform changes: *previously ineligible* parents who gain eligibility for up to one year and *previously eligible* parents who lost entitlement for the second year. Within these groups, we discuss the impact of changes along three different phases of child development: 0 to 6 months, 7 to 12 months, and 12 to 24 months after childbirth. For mothers in between both groups, the main arguments apply similarly.

Previously ineligible mothers (who gained parental leave benefits) stayed at home during the first eight weeks after childbirth because of the unchanged universal mother protection period with fully compensated pre-birth earnings (see Section 2.3). After these eight weeks, a substantial share of previously ineligible mothers took unpaid leave within the first six months after childbirth before the reform (Bergemann & Riphahn, 2015; Welteke & Wrohlich, 2016). During this early phase after childbirth, the reform largely substituted unpaid leave with paid leave. Effectively, previously ineligible families receive unrestricted income transfers which Heckman & Mosso (2014) and Del Boca et al. (2016) suggest to be ineffective at affecting child development. Moreover, changes in benefits can be interpreted as exogenous transitory household income shocks. Carneiro & Ginja (2016) show that parents do not adjust their child investments in terms of time and goods to transitory income shocks. Hence, investments in the first six months likely remained constant.[27]

Between 7 to 12 months after childbirth, previously ineligible mothers responded to the reform with a reduction in their employment, which was mostly part-time (Kluve & Schmitz, 2017). Reviewing the findings on the effects of maternal employment in the first year after childbirth on child development, Brooks-Gunn et al. (2010) and Bernal & Keane (2010), for instance, conclude that effects are rather small, especially for part-time employment beyond the initial six months following childbirth. Therefore, the substantial reform-related changes in mothers' employ-

actions of parental investments to children's development are important interrelated determinants (e.g. Todd & Wolpin, 2007; Cunha & Heckman, 2007; Fiorini & Keane, 2014), it is particularly hard to identify single channels through which changes in early conditions affect later child outcomes. While our data has a unique advantage in the measurements for child development, it lacks information on parental investments into children.

[27] Dahl & Lochner (2012) and Løken et al. (2012) provide evidence for effects of family income on child outcomes, but they do not decompose family income in its permanent and transitory components. Carneiro & Ginja (2016) find effects on parental child investments only for changes in permanent income. Further evidence that permanent income rather than transitory income fluctuations matter for child development is provided by Cameron & Heckman (1998) and Bernal & Keane (2010).

ment behaviour are also unlikely to have a considerable impact on child development. This is also consistent with Dahl et al. (2016) who analyse an expansion of paid parental leave in Norway within the first year after childbirth. While the reform induced women to almost completely substitute work with the new paid parental leave, they find no impact on children's long-term outcomes.

After the first 12 months following childbirth, some evidence suggests that previously ineligible mothers were now also more likely to work (Kluve & Schmitz, 2017; Bergemann & Riphahn, 2015). The effects of maternal employment beyond the first year after childbirth on children depend on the quality of alternative care arrangements. As the availability of publicly funded day care for children below the age of three was low, informal child care by grandparents or other relatives was the main alternative mode of care when mothers were working (e.g. Hank & Buber, 2009). However, mothers may only be willing to return to work if they can ensure alternative care arrangements of good quality. With a good quality of alternative child care, increases in maternal employment are also unlikely to have a large impact on children.

We now turn to children of parents who were *previously eligible*, but lost eligibility for paid parental leave in the second year after childbirth. In the first 12 months after childbirth, mothers still receive at least the same amount of benefits, such that parents could direct the same investments (time and goods) toward their children. In the second year after childbirth, these families experience a negative transitory income shock that they partly compensate for with increased maternal employment (Kluve & Schmitz, 2017; Bergemann & Riphahn, 2015) allowing them to maintain their material investments in children. The additional time spent working reduces the time mothers can spend with their children. Whether this affects child development depends on the activities both the mother and the alternative caregiver perform with the children: First, Del Bono et al. (2016) note that the educational activities mothers perform with their children correlate only weakly with maternal employment. Second, Hsin & Felfe (2014) suggest that maternal employment has no impact on maternal activities that positively affect children, while it reduces the time they spend on activities that are unproductive or even detrimental for child development. Third, the quality of alternative care is likely similar to maternal care as it is mostly provided by informal caregivers. Taken together, reform-induced transitory income shocks are unlikely to change parents' productive investments (material and time) in their children, as suggested by Carneiro & Ginja (2016).

2.8 Conclusion

In this paper, we examine the effects of a recent and substantial paid parental leave reform on child development. The 2007 German reform replaced a means-tested system with an earnings-dependent benefit system causing high-SES households to benefit more from the reform than low-SES households in terms of parental leave eligibility and benefit payments. To estimate causal reform effects, we make use of the eligibility criterion for the new benefit system based on children's birth date and use a regression-discontinuity design combined with a difference-in-differences approach. Our study extends the previous literature along three lines: First, we study not only an expansion of paid leave in the first year after childbirth, but a particular feature of the reform is that it also removed paid leave in the second year after childbirth. Second, these heterogeneous changes in parental leave eligibility and benefit payments for different socio-economic groups provide a unique setting to study the impact of parental leave policies on SES development gaps in children's outcomes. Third, we overcome limitations in the previous literature with exceptionally rich data on child development: We employ administrative, mandatory, and full population child development assessments by licensed public health paediatricians from school entrance examinations from one German federal state.

Our results provide new evidence that the drastic change in the parental leave benefit system had no impact on various measures for children's development at age six. Most point estimates are very close to zero and precisely estimated due to the large sample. We do not find effects on children from high-SES families, on children from low-SES families, or on SES gaps in child development. Our results are robust to numerous sensitivity checks accounting for predicted eligibility status, endogenous sample selection, variations to the estimation window and control cohorts, different sets of control variables, redefinitions of outcome measures, and alternative estimation methods and assumptions on time trends.

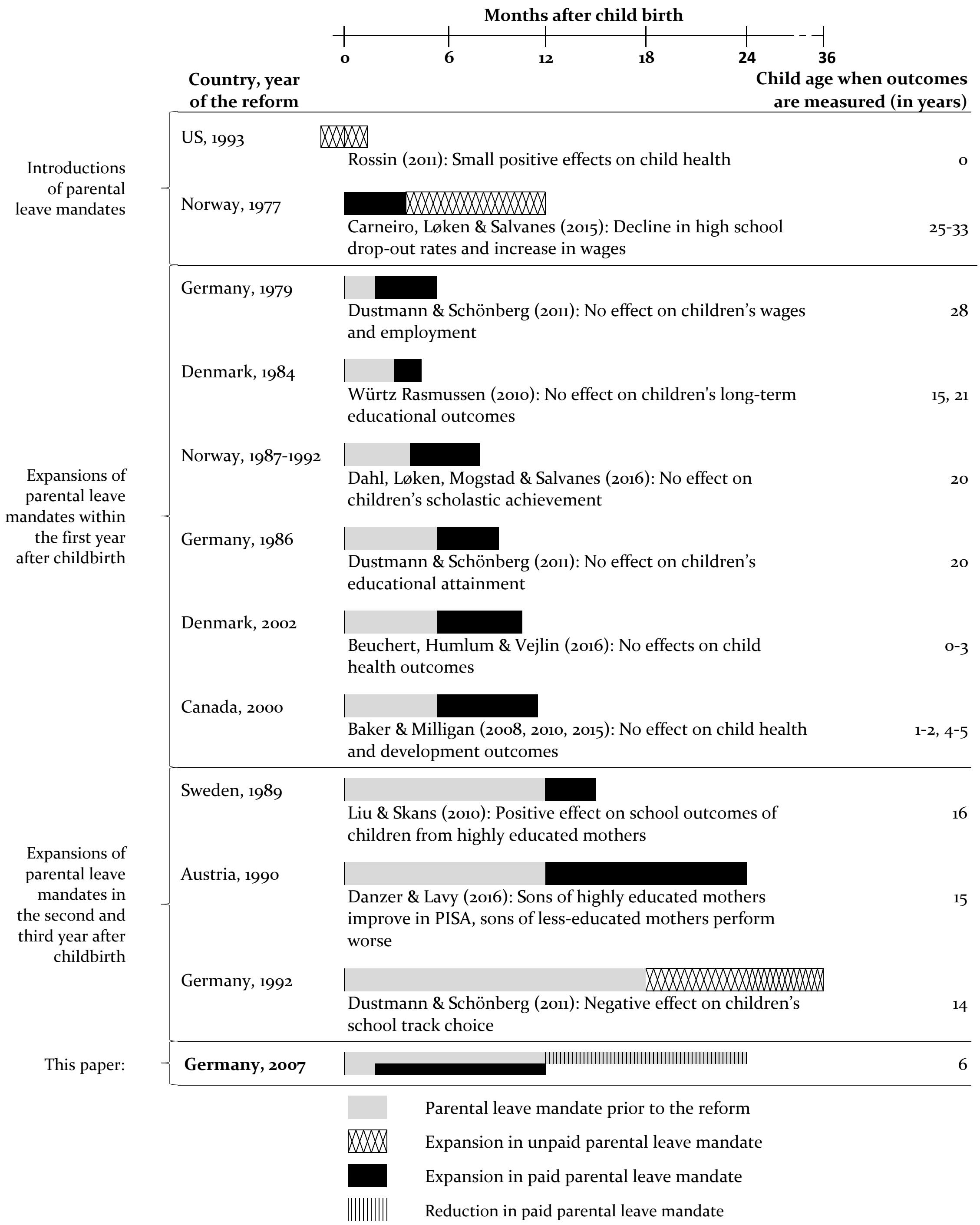
Our zero-reform effects are consistent with recent economic studies on the determinants of child development. In particular, Carneiro & Ginja (2016) show that temporary household income shocks do not change parents' productive investments and Heckman & Mosso (2014, p. 3) conclude that unrestricted income transfers "are a weak reed" to affect child outcomes. Moreover, the reform largely affected maternal labour supply at the part-time margin that has, at most, a small impact on child development (e.g. Bernal & Keane, 2010; Brooks-Gunn et al., 2010).

As with any other study, our analysis also has some limitations. While we are able to reliably estimate the reform effects for parents immediately affected by the reform, our empirical strategy cannot capture reform effects that unfold gradually over time, such as reform-related changes in social norms about maternal labour supply and paternal leave taking (Kluve & Schmitz, 2017; Welteke & Wrohlich, 2016). For example, mothers may decide to give birth at a higher age when they are more strongly attached to the labour market, which may itself have consequences for children's development. Furthermore, the reform may have impacted other child outcomes that are not reflected in the rich set of child development measures that we examine.

What do our results mean for public policy? Since most OECD countries now have paid parental leave policies in place, governments are mainly interested in re-designing these regulations to better incentivise female labour supply, fertility, or paternal involvement in the child rearing process. The German reform effectively changed maternal labour supply, family income, and paternal leave taking. In light of these findings, our study suggests that such policy objectives can be achieved without adverse effects on children's development or the SES development gap.

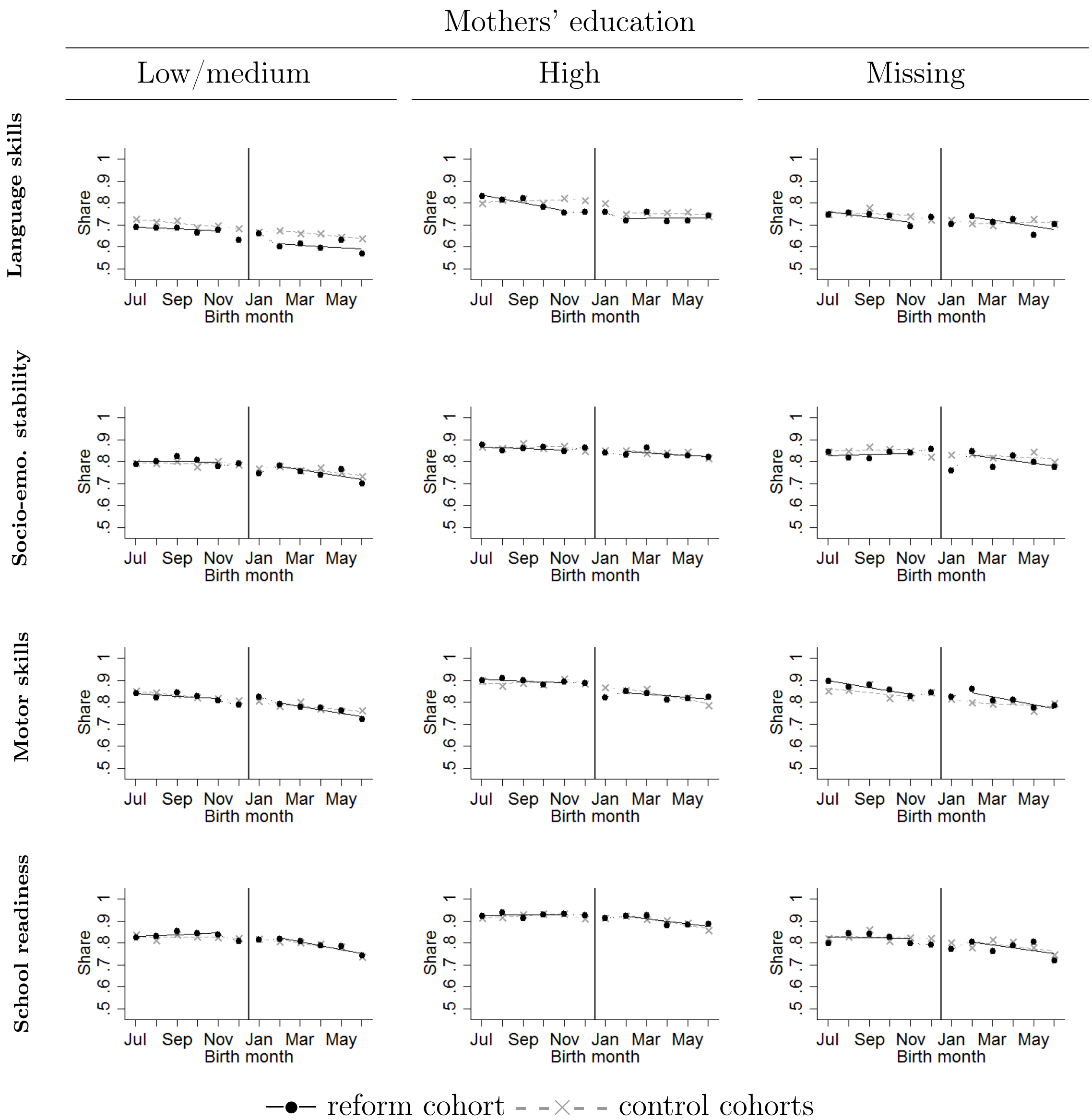
Appendix

Figure A2.1: Evaluated parental leave reforms and their impact on child outcomes



Notes: This figure provides an overview of peer-reviewed economic studies evaluating parental leave reforms and their impact on child outcomes in individual level data.
Source: Illustration based on Huebener (2016).

Figure A2.2: The impact of the 2007 German parental leave reform on child development for subgroups



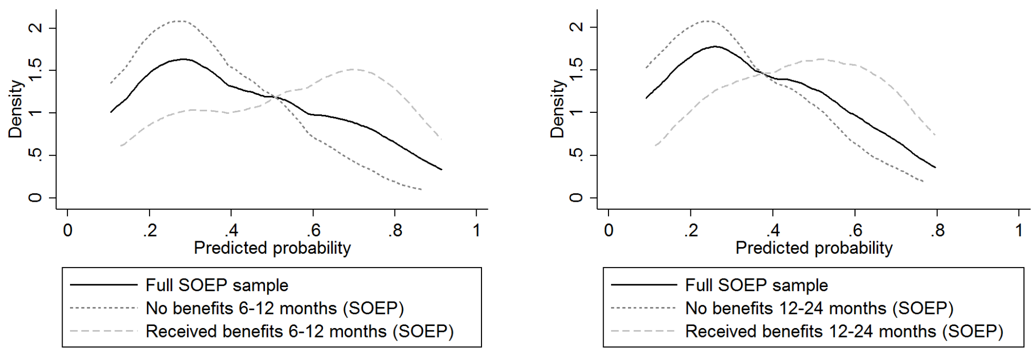
Notes: The figure plots the share of children diagnosed with a sufficient level of the respective skill for children born 6 months before and 6 months after the new parental leave legislation in Germany (reform cohort), and for children born in the same months in the year before and the year after (control cohorts) separately by mothers' education. The vertical bar between December and January indicates the introduction of the reform on January 1, 2007. The solid and dashed lines represents linear fits for children in our main sample. The dotted lines refer to children in months that are likely to be affected by birth date manipulations. They are exempted from our main analyses.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

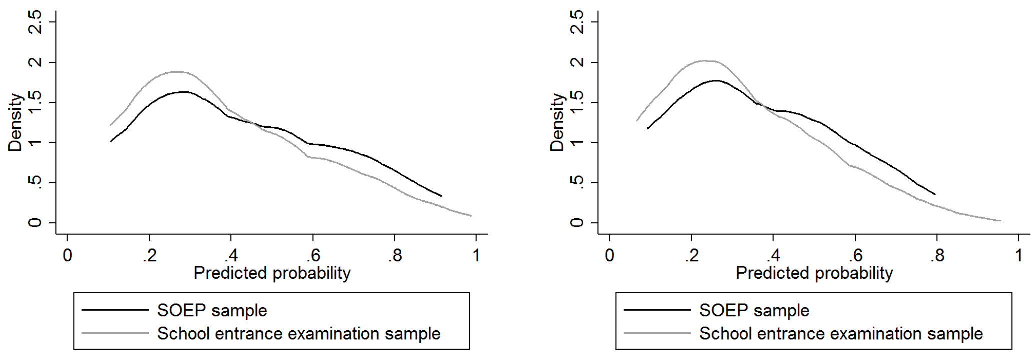
Figure A2.3: Predicted eligibility for parental leave benefits under the policy rules applying before the 2007 reform

Eligible for parental leave benefits:	
6-12 months after childbirth	12-24 months after childbirth

Panel A

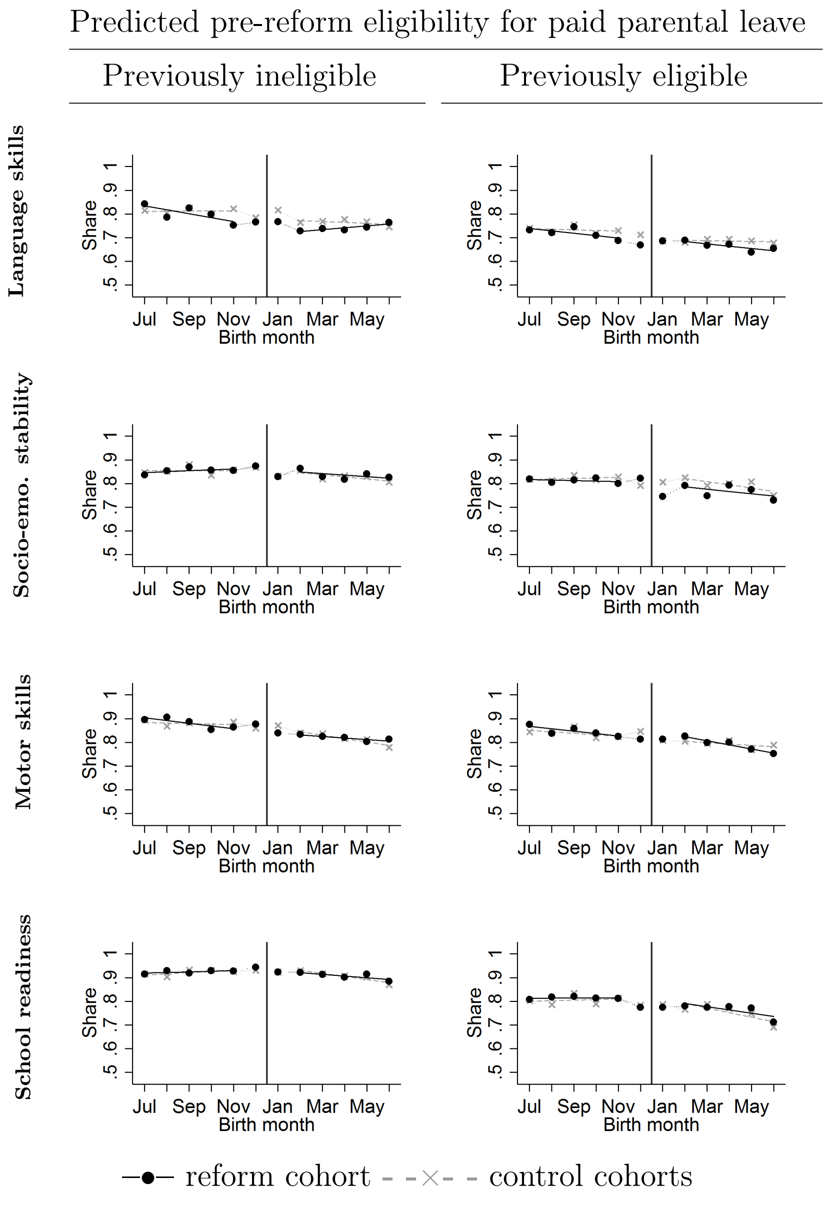


Panel B



Notes: The figures in Panel A show kernel density plots (Epanechnikov) of the predicted probabilities of families to receive paid parental leave for 6-12 months and for 12-24 months after childbirth under the policy rules applying before the 2007 parental leave reform. The prediction uses SOEP data on real take-up and is based on a logit model including dummies for mothers' education and fathers' education, their interaction, a dummy for single parents, the number of children of the family, a dummy for migration background, and an interaction terms of mothers' education and the number of children. All variables are measured at age 6 if available (earlier otherwise). The figures in Panel B show the predictions based on the original SOEP information, and the out-of-sample predictions in the school entrance examinations data.
Source: Own calculations based on SOEPv32 for children born in 2005 through 2006, and school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

Figure A2.4: The impact of the 2007 German parental leave reform on child development for subgroups



Notes: The figure plots the share of children diagnosed with a sufficient level of the respective skill for children born 6 months before and 6 months after the new parental leave legislation in Germany (reform cohort), and for children born in the same months in the year before and the year after (control cohorts) separately by parents predicted eligibility for paid parental leave before the 2007 reform. The vertical bar between December and January indicates the introduction of the reform on January 1, 2007. The solid and dashed lines represents linear fits for children in our main sample. The dotted lines refer to children in months that are likely to be affected by birth date manipulations. They are exempted from our main analyses.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

Table A2.1: Comparison of socio-economic characteristics of Schleswig-Holstein to the rest of West Germany

	Schleswig-Holstein	West	West*
Age	44.07	43.21	43.22
Female	0.52	0.52	0.52
Unmarried	0.38	0.39	0.39
Married	0.47	0.47	0.48
Divorced	0.07	0.06	0.06
Household size	2.67	2.75	2.76
Children in household	0.92	0.97	0.98
Born in Germany	0.89	0.85	0.85
Working	0.45	0.47	0.47
Unemployed	0.03	0.03	0.03
Out of the labour force	0.51	0.5	0.5
Female labour force participation rate	0.80	0.80	0.80
Share of children below age 3 in day care[a]	0.12	0.12	0.12
Share of children aged 3-6 in day care[a]	0.84	0.91	0.91
Share of fathers taking parental leave[b]	0.18	0.20	0.20
Highest level of education			
$\leq$ ISCED3	0.25	0.28	0.28
ISCED4	0.04	0.05	0.05
ISCED5	0.47	0.42	0.42
ISCED6	0.06	0.06	0.06
$\leq$ ISCED7	0.18	0.20	0.20
Personal monthly net income			
0 - 1,100	0.52	0.53	0.53
1,100-2,300	0.29	0.3	0.3
2,300-3,600	0.07	0.06	0.06
3,600-5,000	0.03	0.03	0.03
5,000-18,000	0.01	0.01	0.01
Household monthly net income			
0 - 1,100	0.1	0.1	0.1
1,100-2,300	0.29	0.31	0.31
2,300-3,600	0.19	0.2	0.2
3,600-5,000	0.14	0.15	0.15
5,000-18,000	0.09	0.09	0.09
Municipality size			
<2,000	0.19	0.05	0.05
2,000-5,000	0.11	0.09	0.09
5,000-10,000	0.12	0.12	0.12
10,000-50,000	0.33	0.35	0.36
50,000-100,000	0.08	0.1	0.1
>100,000	0.16	0.31	0.27
N	25,249	533,229	513,241

Notes: This table reports socio-economic and socio-demographic characteristics of the population in Schleswig-Holstein and West Germany. "West" includes only West German federal states, without Schleswig-Holstein. "West*" further excludes the city-states of Hamburg and Bremen. [a] Based on information in 2008. [b] Based on German Federal Statistical Office (2010). *Source:* Own calculations based on German Micro Census 2009.

Table A2.2: Balancing of covariates

	All		Sample stratified by mothers' education: Low & medium		Sample stratified by mothers' education: High	
	β_{PPL}	s.e.	β_{PPL}	s.e.	β_{PPL}	s.e.
Age at examination in months	0.0120	(0.0611)	-0.0217	(0.0801)	0.0335	(0.1314)
Girl	-0.0022	(0.0106)	-0.0030	(0.0149)	-0.0071	(0.0192)
Birth weight in grams	-7.9751	(12.6692)	-16.9056	(17.9797)	4.0378	(22.9523)
Birth weight missing	-0.0027	(0.0041)	-0.0047	(0.0047)	0.0014	(0.0052)
Years child spent in day care	-0.0145	(0.0152)	-0.0398	(0.0208)	-0.0087	(0.0276)
Years in day care missing	0.0006	(0.0046)	0.0031	(0.0060)	0.0074	(0.0070)
Mother's years of schooling	0.0411	(0.0313)	-0.0037	(0.0142)	—	—
Mother's education missing	0.0052	(0.0070)	—	—	—	—
Father's years of schooling	0.0327	(0.0234)	0.0403	(0.0330)	0.0199	(0.0470)
Father's education missing	0.0052	(0.0081)	0.0031	(0.0092)	0.0060	(0.0070)
Child lives with one parent	-0.0029	(0.0071)	-0.0172	(0.0113)	0.0125	(0.0109)
Child lives in other living arrangements	-0.0008	(0.0050)	0.0069	(0.0082)	-0.0091	(0.0069)
Living arrangement missing	0.0037	(0.0036)	0.0045	(0.0023)	0.0026	(0.0032)
One parent born abroad	0.0026	(0.0060)	0.0016	(0.0084)	0.0018	(0.0116)
Both parents born abroad	0.0033	(0.0067)	0.0139	(0.0098)	-0.0079	(0.0104)
Migration background missing	0.0067	(0.0045)	0.0008	(0.0049)	-0.0009	(0.0043)
German is main language	0.0090	(0.0065)	0.0080	(0.0091)	0.0032	(0.0111)
Home language foreign	-0.0022	(0.0042)	0.0029	(0.0061)	-0.0106	(0.0067)
Home language missing	0.0026	(0.0034)	0.0069	(0.0039)	0.0012	(0.0051)
Number of children of the family	0.0333	(0.0221)	0.0478	(0.0312)	-0.0052	(0.0336)
Number of children missing	-0.0011	(0.0015)	-0.0004	(0.0018)	0.0003	(0.0024)

Notes: This table reports coefficient estimates of β_{PPL} of regression models outlined in equation 2.1 (without X) to check the balance of child and family characteristics. The dependent variables are listed in the rows. The results are reported for the sample including all children, and subsamples stratified by mothers' education. "Low & medium" education refers to lower and medium-secondary school certificates. "High" education refers to upper-secondary school certificates (*Abitur*). The regressions include the following control variables: county-by-examination year fixed effects, birth months fixed effects and birth cohort fixed effects . Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

Table A2.3: Parental leave reform effects on alternative definitions of child development

Panel A: Language skills	Language score, mean (SD)	Treatment effect on: Language score	language score in plural words, pseudo words & prepositions: ≤ 12 (5%)	13 – 16 (13%)	17 – 19 (33%)	20 (21%)	21 (28%)
All children ($N = 28,001$)	18.5264 (3.0599)	-0.0980 (0.0714)	0.0065 (0.0056)	0.0129 (0.0086)	-0.0032 (0.0122)	-0.0039 (0.0106)	-0.0124 (0.0110)
Mothers with low/medium education ($N = 14,890$)	18.2052 (3.1551)	-0.1133 (0.1014)	0.0115 (0.0082)	0.0181 (0.0126)	-0.0238 (0.0168)	-0.0086 (0.0141)	0.0029 (0.0140)
Mothers with high education ($N = 8,936$)	19.4417 (2.3083)	-0.0453 (0.0974)	-0.0069 (0.0057)	0.0080 (0.0115)	0.0301 (0.0211)	0.0013 (0.0202)	-0.0324 (0.0220)

Panel B: Socio-emotional stability	SDQ score, mean (SD)	Treatment effect on: SDQ score	specific parts of the SDQ score distribution: Score = 0 (9%)	1 – 4 (31%)	5 – 8 (32%)	9 – 12 (17%)	≥ 13 (11%)
All children ($N = 20,603$)	6.4232 (4.8933)	0.1959 (0.1362)	-0.0024 (0.0077)	-0.0014 (0.0131)	-0.0249 (0.0139)	0.0189 (0.0110)	0.0098 (0.0091)
Mothers with low/medium education ($N = 11,371$)	7.2702 (4.9947)	0.0895 (0.1870)	0.0006 (0.0088)	-0.0126 (0.0168)	-0.0039 (0.0187)	0.0200 (0.0159)	-0.0041 (0.0135)
Mothers with high education ($N = 7,042$)	5.0849 (4.1355)	0.3692 (0.2083)	-0.0078 (0.0149)	-0.0033 (0.0249)	-0.0352 (0.0241)	0.0233 (0.0165)	0.0230 (0.0120)

Panel C: Motor skills	Jumps, mean (SD)	Treatment effect on: No. of side-jumps	side-jumps within 10 seconds: ≤ 7 (13%)	8 – 9 (28%)	10 (31%, mode)	11 – 13 (18%)	≥ 14 (10%)
All children ($N = 20,321$)	10.0705 (3.0472)	-0.1211 (0.0858)	0.0119 (0.0099)	0.0092 (0.0134)	-0.0184 (0.0134)	0.0100 (0.0116)	-0.0127 (0.0088)
Mothers with low/medium education ($N = 11,126$)	9.9229 (3.0253)	-0.0516 (0.1138)	0.0003 (0.0139)	0.0245 (0.0183)	-0.0203 (0.0179)	0.0003 (0.0154)	-0.0048 (0.0114)
Mothers with high education ($N = 6,947$)	10.4814 (3.0984)	-0.2016 (0.1585)	0.0060 (0.0150)	0.0114 (0.0221)	-0.0248 (0.0237)	0.0402 (0.0210)	-0.0328 (0.0172)

Notes: This table reports estimated reform effects on subdimensions tested in school entrance examinations, i.e. on SOPESS language test scores (plurals, pseudo words and prepositions, see Panel A), on the sum of SDQ subscales, ranging from 0 to 40 (Panel B), and on side-jumps (Panel C). The treatment effects are reported for the pooled sample, and for subsamples stratified by mothers' education. All regressions are based on equation 2.1, and include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. The sample is restricted to counties that delivered the raw scores to the data compiling Ministry of Social Affairs, Health, Family and Equal Opportunities in Schleswig-Holstein. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

Table A2.4: Covariates balancing for varying window sizes around the reform cutoff

	(1)	(2)	(3)	(4)
	Window size without January & December			
	Nov-Feb	Oct-Mar	Sep-Apr	Aug-May
Age at examination in months	0.0062	-0.0140	0.0105	-0.0013
	(0.1280)	(0.1062)	(0.0817)	(0.0693)
Girl	0.0062	-0.0042	0.0105	0.0030
	(0.0240)	(0.0166)	(0.0136)	(0.0117)
Birth weight in grams	-35.0317	0.8321	-8.8259	-12.8237
	(28.3773)	(19.8000)	(16.1827)	(14.1342)
Years child spent in day care	0.0145	-0.0076	-0.0149	-0.0179
	(0.0344)	(0.0240)	(0.0194)	(0.0167)
Mother's years of schooling	0.0752	0.0379	0.0077	0.0216
	(0.0704)	(0.0493)	(0.0403)	(0.0348)
Father's years of schooling	-0.0076	0.0181	0.0307	0.0174
	(0.0529)	(0.0369)	(0.0301)	(0.0261)
Child lives with both parents	0.0127	-0.0127	-0.0013	-0.0059
	(0.0195)	(0.0136)	(0.0111)	(0.0096)
Child lives with one parent	-0.0004	0.0120	-0.0001	0.0046
	(0.0160)	(0.0110)	(0.0091)	(0.0079)
Child lives in other living arrangements	-0.0086	-0.0015	-0.0009	-0.0014
	(0.0114)	(0.0080)	(0.0064)	(0.0056)
At least one parent with mig. back.	0.0199	0.0076	0.0110	0.0125
	(0.0199)	(0.0140)	(0.0115)	(0.0099)
German is main language	-0.0219	-0.0123	-0.0178	-0.0141
	(0.0176)	(0.0124)	(0.0101)	(0.0088)
Home language foreign	0.0085	0.0035	0.0004	-0.0029
	(0.0095)	(0.0067)	(0.0054)	(0.0047)
Number of children of the family	0.1110*	0.0724*	0.0515	0.0328
	(0.0508)	(0.0348)	(0.0282)	(0.0245)
N	8,315	17,157	26,166	35,552

Notes: This table reports results of difference-in-differences regressions as outlined in equation 2.1 on the covariates listed in the rows with varying window sizes around the reform cut-off. The regressions include the following control variables: examination year-by-county fixed effects, birth months fixed effects and birth cohort fixed effects. The regressions exclude the X-vector. Each coefficient estimates stems from a separate regression. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

Table A2.5: Results for pooled child outcomes: Difference-in-differences estimates of the parental leave reform effects on child development

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All children			Mothers' education		Predicted eligibility for parental leave benefits	
				Low/ medium	High	Previously eligible	Previously ineligible
Pooling language skills, motor skills, socio-emotional stability, and school readiness							
Child development	-0.0035	-0.0026	-0.0032	-0.0082	-0.0007	-0.0078	0.0019
	(0.0055)	(0.0053)	(0.0052)	(0.0078)	(0.0082)	(0.0101)	(0.0088)
	[0.0468]	[0.0708]	[0.0891]	[0.0922]	[0.0715]	[0.0896]	[0.0702]
N	179,988	179,988	179,988	89,968	57,024	51,344	49,924
Pooling language skills, motor skills, and socio-emotional stability							
Child development	-0.0060	-0.0052	-0.0058	-0.0119	-0.0036	-0.0128	0.0000
	(0.0058)	(0.0056)	(0.0055)	(0.0082)	(0.0091)	(0.0104)	(0.0098)
	[0.0583]	[0.0789]	[0.0943]	[0.0986]	[0.0752]	[0.0992]	[0.0716]
N	134,991	134,991	134,991	67,476	42,768	38,508	37,443
Control variables							
Child characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes
Family characteristics	No	No	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimation results of the parental leave reform on child development under the assumption of an equal impact of the reform on all pooled development outcomes. All regressions are based on equation 2.1. They include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables, and a dummy indicating the different child outcomes. The stratification in columns 6 and 7 are based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 12-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Standard errors are clustered at the child level (44,997 clusters in columns 1-3, 22,492 clusters in column 4, 14,256 clusters in column 5, 12,836 clusters in column 6, 12,481 clusters in column 7) and reported in parentheses. R^2 are reported in brackets. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

Table A2.6: Robustness checks separately by mothers' education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Baseline	Window size without January & December				Including children born		Without children born	Nonlinear models (marginal effects)	
	(Jul-Jun)	Aug-May	Sep-Apr	Oct-Mar	Nov-Feb	7/2004-6/2005	in Jan & Dec	after 6/2007	Probit	Logit
Mothers' education: Low/medium										
Language skills	-0.0181	-0.0136	-0.0270	-0.0248	-0.0530	-0.0239	-0.0074	-0.0219	-0.0161	-0.0150
	(0.0134)	(0.0149)	(0.0172)	(0.0213)	(0.0305)	(0.0127)	(0.0122)	(0.0152)	(0.0129)	(0.0128)
Socio-emot. stability	-0.0127	-0.0070	-0.0150	0.0050	0.0348	-0.0154	-0.0166	-0.0065	-0.0130	-0.0121
	(0.0120)	(0.0133)	(0.0153)	(0.0188)	(0.0270)	(0.0114)	(0.0109)	(0.0139)	(0.0116)	(0.0117)
Motor skills	-0.0051	0.0040	-0.0037	-0.0031	0.0156	-0.0098	0.0031	-0.0103	-0.0039	-0.0043
	(0.0115)	(0.0128)	(0.0147)	(0.0182)	(0.0267)	(0.0109)	(0.0104)	(0.0132)	(0.0112)	(0.0113)
School readiness	0.0032	0.0014	-0.0024	0.0048	0.0099	-0.0041	0.0039	0.0097	0.0000	0.0004
	(0.0110)	(0.0121)	(0.0139)	(0.0170)	(0.0247)	(0.0104)	(0.0100)	(0.0126)	(0.0110)	(0.0111)
N	22,492	17,716	12,998	8,533	4,115	30,195	26,876	14,629	22,492	22,492
Mothers' education: High										
Language skills	-0.0092	-0.0102	-0.0056	0.0249	0.0272	-0.0150	-0.0068	0.0051	-0.0069	-0.0057
	(0.0157)	(0.0174)	(0.0200)	(0.0247)	(0.0360)	(0.0147)	(0.0142)	(0.0179)	(0.0149)	(0.0149)
Socio-emot. stability	0.0112	0.0086	0.0076	0.0070	0.0080	0.0094	0.0030	0.0069	0.0113	0.0113
	(0.0135)	(0.0149)	(0.0169)	(0.0205)	(0.0302)	(0.0128)	(0.0122)	(0.0157)	(0.0130)	(0.0131)
Motor skills	-0.0127	-0.0246	-0.0235	-0.0130	0.0038	-0.0165	-0.0186	-0.0032	-0.0149	-0.0165
	(0.0127)	(0.0141)	(0.0162)	(0.0197)	(0.0278)	(0.0120)	(0.0116)	(0.0151)	(0.0130)	(0.0133)
School readiness	0.0079	0.0002	0.0015	0.0046	-0.0057	0.0044	0.0028	0.0129	0.0081	0.0079
	(0.0105)	(0.0115)	(0.0130)	(0.0154)	(0.0218)	(0.0099)	(0.0095)	(0.0120)	(0.0107)	(0.0110)
N	14,256	11,274	8,340	5,426	2,591	19,169	16,826	9,081	14,161	14,161

Notes: This table reports the results of sensitivity checks to alternative model specifications for the common trend assumption for subsamples stratified by mothers' education. It also reports the results from placebo regressions. All regressions include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

Table A2.7: Common trend checks separately by mothers' education

	(1)	(2)	(3)	(4)	(5)	(6)
		Cohort-specific time trends		Placebo reforms		Regression
	Baseline	Linear	Quadratic	1 year earlier	Mar/Apr 2007	discontinuity
Mothers' education: Low/medium						
Language skills	-0.0181	-0.0431	-0.0436	0.0093	0.0168	-0.0379
	(0.0134)	(0.0355)	(0.0355)	(0.0145)	(0.0195)	(0.0301)
Socio-emot. stability	-0.0127	0.0122	0.0126	-0.0105	-0.0165	0.0046
	(0.0120)	(0.0316)	(0.0316)	(0.0132)	(0.0176)	(0.0265)
Motor skills	-0.0051	0.0194	0.0199	0.0114	-0.0044	0.0150
	(0.0115)	(0.0307)	(0.0308)	(0.0126)	(0.0173)	(0.0259)
School readiness	0.0032	-0.0076	-0.0070	-0.0088	-0.0100	0.0016
	(0.0110)	(0.0291)	(0.0291)	(0.0123)	(0.0163)	(0.0241)
N	22,492	22,492	22,492	22,492	10,706	6,877
Mothers' education: High						
Language skills	-0.0092	0.0325	0.0338	-0.0254	-0.0122	-0.0245
	(0.0157)	(0.0406)	(0.0405)	(0.0168)	(0.0225)	(0.0345)
Socio-emot. stability	0.0112	0.0031	0.0017	0.0108	-0.0134	-0.0030
	(0.0135)	(0.0346)	(0.0345)	(0.0147)	(0.0190)	(0.0292)
Motor skills	-0.0127	-0.0201	-0.0200	-0.0129	0.0216	-0.0202
	(0.0127)	(0.0329)	(0.0328)	(0.0144)	(0.0191)	(0.0276)
School readiness	0.0079	-0.0048	-0.0064	-0.0048	-0.0055	0.0150
	(0.0105)	(0.0267)	(0.0266)	(0.0116)	(0.0153)	(0.0223)
N	14,256	14,256	14,256	14,256	6,910	4,426

Notes: This table reports the results of sensitivity checks to alternative model specifications for the common trend assumption for subsamples stratified by mothers' education. It also reports the results from placebo regressions. All regressions include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

Chapter 3

Increased instruction hours and the widening gap in student performance*

Abstract

Do increased instruction hours improve the performance of all students? Using PISA scores of students in ninth grade, we analyse the effect of a German education reform that increased weekly instruction hours by two hours (6.5 percent) over almost five years. In the additional time, students are taught new learning content. On average, the reform improves student performance. However, treatment effects are small and differ across the student performance distribution. Low-performing students benefit less than high-performing students. We argue that the content of additional instruction time is an important determinant explaining this pattern. The findings demonstrate that increases in instruction hours can widen the gap between low- and high-performing students.

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Chapter 4

Compressing instruction time into fewer years of schooling and the impact on student performance*

Abstract

Is it possible to compress instruction time into fewer school years without lowering education levels? A fundamental reform in Germany reduced the length of academic track schooling by one year, while increasing instruction hours in the remaining school years to provide students with a very similar core curriculum and the same overall instruction time. Using aggregated administrative data on the full population of students, we find that the reform increases grade repetition rates and lowers final grade point averages, without affecting graduation rates. The results suggest adverse reform effects on student performance, but the economic significance of the effects appears moderate.

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Chapter 5

Intergenerational effects of education on risky health behaviours and long-term health*

Abstract

This paper estimates the causal effects of parental education on their children's risky health behaviours and health status. I study the intergenerational effects of a compulsory schooling reform in Germany after World War II. Implemented across federal states at different points in time, the reform increased the minimum number of school years from eight to nine. Instrumental variable estimates and difference-in-differences estimates reveal that increases in maternal schooling reduce children's probability to smoke and to be overweight in adolescence. The effects persist into adulthood, reducing chronic conditions that often result from unhealthy lifestyles. No such effects can be identified for paternal education. Increased investments in children's education and improvements in their peer environment early in life are important for explaining the effects. Changes in family income, family stability, fertility and parental health-related behaviours are less relevant. The intergenerational effects of education on health and health-related behaviours exceed the direct effects. Studies neglecting the intergenerational perspective substantially understate the full causal effects.

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5.1 Introduction

Smoking, high blood pressure, being overweight, physical inactivity, and high blood glucose are the leading health risk factors in high-income countries. The World Health Organization (WHO, 2009) estimates that these factors account for more than fifty percent of pre-mature deaths. They are related to unhealthy lifestyles that are highly correlated with individuals' level of education. Educational differences in health behaviours are undoubtedly an important reason for the strong educational gradient in health status, chronic conditions, and longevity (e.g. Cutler & Lleras-Muney, 2010; Cawley & Ruhm, 2011; Mazumder, 2012).[1]

The extent to which the link between education, health-related behaviours and health is causal is subject to ongoing debate. Studies using exogenous variation in education produce mixed findings and are far from being conclusive (Cawley & Ruhm, 2011; Lochner, 2011; Clark & Royer, 2013; Grossman, 2015). What is more, previous studies on causal effects almost exclusively focus on the immediate effects of education on health and health behaviours. However, education is also transmitted to children (Black et al., 2005; Oreopoulos et al., 2006; Holmlund et al., 2011; Piopiunik, 2014; Dickson et al., 2016). As many health-related behaviours, such as smoking or dietary habits, may already be determined early in life before education reforms set in, we may miss a substantial share of the causal link between education, health and health-related behaviours if intergenerational effects are neglected.

This paper aims at closing the gap and estimates the intergenerational effects of education on health behaviours and long-term health. It studies the impact of parental schooling on children's smoking behaviour and being overweight, and traces the effects on chronic conditions and long-term health into children's adulthood. Both smoking and being overweight are important causes of future health problems and chronic conditions (such as cardiovascular diseases, type 2 diabetes, and cancer) as well as premature death (e.g. Must, 1999). While smoking is itself a risky health behaviour, being overweight is closely related to other common risky health behaviours, such as physical inactivity or a poor diet (see, e.g., Hill, 1998, 2003; Janssen et al., 2005). Despite the large number of ways through which parental education may affect children's health behaviours and health status, there may still be other determinants that correlate with parental education and falsely propose a

[1]Differences in health behaviours do not fully explain the educational gradient in health. This issue is not the focus of this paper.

causal link to children's health-related outcomes, such as parents' genetic endowments, general health, or character traits. Identification of causal effects requires exogenous variation in parental education, which I draw from a compulsory schooling reform in Germany. The reform increased the minimum number of school years from eight to nine, stepwise across federal states between 1949 and 1969. The analysis builds on data from the German Micro Census, an annual representative survey of one percent of all households in Germany. I focus on children aged 15 to 18 living with their parents. I also use supplementary data from the German Socio-Economic Panel Study (SOEP), tracking the effects into children's adulthood after they moved out from home. Carrying out the analysis for Germany has an advantage for the identification of causal effects compared to, for example, the US: The German education system tracks students into different schools mainly based on their ability. The compulsory schooling reform only affected students in the basic track. As the reform did not impact the completed school track of affected parents, the additional school track variation enables me to also estimate difference-in-differences models with children of basic track parents. I can also perform placebo tests and estimate triple-differences models using children with parents from higher tracks. The results pass a large set of robustness checks.

The main findings of the paper are as follows: One additional year of maternal education reduces children's probability to smoke (3.8 percentage points, or about 17 percent) and to be overweight (4.5 percentage points, or about 26 percent) in adolescence. The effects persist into children's adulthood, eventually lowering the risk of chronic conditions and improving the general health status. For smoking, the effects mainly arise because children never initiate smoking. Although fathers' years of schooling also correlate highly with children's health-related outcomes, I find no evidence for causal effects of increases in paternal education.

The effects are best explained by increased investments in children's human capital and its dynamic formation throughout life (Cunha & Heckman, 2007; Heckman, 2007). I present evidence that children of treated mothers obtain higher levels of education, they attend better school tracks and are exposed to a better peer environment in school during adolescence - a critical period for the initiation of smoking and other risky behaviours (such as drinking and substance abuse). This may already be a result of improved human capital at birth and throughout childhood (e.g. Currie & Moretti, 2003; Chou et al., 2010; Lindeboom et al., 2009) which can promote further human capital formation (e.g. Behrman & Rosenzweig, 2004; Black et al., 2007; Case et al., 2005). These increased levels of education also improve children's earnings prospects, and probably their discount rates and risk aversion, which may all decrease the utility drawn from engaging in unhealthy behaviours (Fuchs, 1982; Becker & Mulligan, 1997; Cutler & Lleras-Muney, 2010). I test other theoretical channels through which increases in maternal schooling could improve children's health behaviours and health status, including changes in family income and mating (which allows living in better neighbourhoods, or to purchase sports club memberships, better health services, or better food, see, e.g., Currie & Stabile, 2003; Carneiro et al., 2013), family stability and fertility (which may change the time and material investments parents can invest in their children, see, e.g., Hanushek, 1992; Francesconi et al., 2010), and parental health behaviours (as parents may serve as a role model, see, e.g. Powell & Chaloupka, 2005). The empirical relevance of these channels appears small overall, although even small changes in these dimensions may accumulate and contribute to substantial improvements in unhealthy behaviours and long-term human capital.

The study makes several important contributions: First, I extend the scarce literature on the *long-term* effects of educational interventions that focuses mainly on children's schooling outcomes (e.g. Black et al., 2005; Oreopoulos et al., 2006; Holmlund et al., 2011; Piopiunik, 2014; Dickson et al., 2016). I add novel evidence on intergenerational effects on health behaviours and health status that are likely moderated by increases in children's education. The fact that interventions can unfold substantial long-term effects is encouraging for programmes that initially have small direct effects on treated individuals, as in the "zero-returns to compulsory schooling" reform analysed in Pischke & von Wachter (2008), which is also the basis of this analysis. The long-term effects further justify public investments in education as individuals would typically not consider these spill-over effects in their educational investment decisions.

Second, I provide the first evidence for causal effects of parental schooling on children's health *behaviours*. The large literature on the causal effects of education on health behaviours exclusively focuses on the direct effects within the same generation (Lochner, 2011; Clark & Royer, 2013; Grossman, 2015).[2] My intergenerational

[2]Some studies document correlations between parental education and children's health behaviour (Waldron & Lye, 1990; Lowry et al., 1996) or try to estimate the causal effect using parental background information as instruments for parental schooling (e.g. Kemptner & Marcus, 2013).

perspective contributes to the literature on the strong associations between socio-economic status (SES) and health status, chronic conditions and longevity, for which SES gaps in health behaviours are an important explanation (e.g. Baum & Ruhm, 2009; Cawley & Ruhm, 2011). The results suggest that the intergenerational SES transmission also operates through the impact of mothers' education on children's health behaviours, and eventually on their health.

Third, I go beyond child outcomes in adolescence and demonstrate effects of parental schooling on children's health behaviours and health status when they are themselves *adults*. The small literature on intergenerational effects of education on health mainly focuses on health outcomes around childbirth. It mostly points towards improvements in child health at birth, and a more health-oriented behaviour of mothers during pregnancy (e.g. Currie & Moretti, 2003; Chou et al., 2010; McCrary & Royer, 2011). Few studies examine the effects of parental schooling on health-related outcomes in later childhood and adolescence (Lindeboom et al., 2009; Carneiro et al., 2013; Lundborg et al., 2014). They do not consider health behaviours and find no robust evidence on children's overweight.[3] I provide the first robust evidence for maternal schooling effects on children's smoking behaviour and being overweight as adolescents, and show that these effects persist into adulthood. I also show that children are less likely to suffer from chronic conditions and report better general health as adults.

However, these instruments do not credibly overcome the endogeneity problem (Kenkel et al., 2006).

[3] These studies consider being overweight or obese as an outcome variable among others, but data limitations or limitations in the identification strategy may cause some sizeable point estimates to be imprecisely estimated and insignificant. Lindeboom et al. (2009), exploiting the UK's 1947 increase in the minimum school leaving age, find little evidence for effects on several child health outcomes at ages 7, 11, and 16, including weight problems. Identification is based on a regression discontinuity (RD) design exploiting cohort variation in parents' minimum schooling requirements. This approach needs to rely on assumptions on general cohort trends, with RD-estimates likely underestimating the effects of the minimum schooling policy (Lochner, 2011). Carneiro et al. (2013) estimate effects of maternal education on a large number of child outcomes at ages 7-8 and 12-14. They instrument maternal schooling with regional and family characteristics that alter individuals' costs of schooling. The rather weak instrument results in imprecise estimates, such that some sizeable effects on being overweight (e.g. white children aged 7-8) only turn significant in robustness checks. Lundborg et al. (2014) study intergenerational effects of a set of education reforms on measures of cognitive and non-cognitive skills as well as a rich set of child health indicators, including obesity at age 18 in a sample of military draftees limited to males. IV-estimates on obesity appear small, but given the low incidence of only two percent of obese individuals in the sample, the estimates suggest substantial obesity reductions of 33 to 50 percent, which are only imprecisely estimated.

Fourth, the study shows that education has substantial *non-market* benefits, even if the income channel is closed. This is an important contribution to the growing literature on non-market benefits of education, which typically cannot disentangle the effects of education from effects of higher incomes. Increased schooling usually increases earnings, and it has many further benefits for individuals and society, such as reducing the risk of welfare dependence, teenage fertility, and health problems, engaging less in crime (see, e.g., Oreopoulos & Salvanes, 2011, for an overview) - or improving health status and health-related behaviours of children, as I demonstrate. However, it used to be unclear whether effects of education on non-market outcomes are a result of higher incomes or of more schooling *per se* (Oreopoulos & Salvanes, 2011). A major challenge in the literature is to disentangle the two because the policy implications vary widely. My analysis builds on a reform for which Pischke & von Wachter (2008) report zero-returns to compulsory schooling. Consequently, this leads to a natural experiment in which the income channel is mostly closed. I also find no evidence for reform effects on household income. My findings complement Lundborg et al. (2014), for whom it appears that income effects can explain most of the effects of maternal schooling on children's cognitive skills and health. I suggest that a multiplicity of factors is at work.

The remainder of the paper is organised as follows. Section 5.2 provides information about the German education system and compulsory schooling law changes. Section 5.3 introduces the data and outlines the empirical strategy. I present the main results in Section 5.4 and a large set of sensitivity checks in Section 5.5. Section 5.6 concludes.

5.2 The compulsory schooling reform in West Germany

At the time of the compulsory schooling reform, children typically entered primary school at age six and attended school jointly for four years. Thereafter, students were ability-tracked into three different secondary school tracks: The basic school track (*Volksschule* or *Hauptschule*) with school completion after eight or nine years of schooling, the middle track (*Realschule*) with completion after ten years of schooling, and the high-ability track (*Gymnasium*) with completion after 13 years of schooling.[4] Students completing the high track earned the general university entrance

[4]The mechanism for selecting students into the different school tracks varies across cohorts and federal states. Generally, it depends on grades in primary school, teacher recommendations, parental

qualification and could study at university. Students from the basic and middle tracks typically proceeded with vocational trainings. The basic track was attended by about eighty percent of students around 1940. As the availability of places at higher tracks has expanded rapidly since the 1950s, the share gradually declined to below fifty percent by 1970.[5]

After World War II, all West German federal states increased the minimum number of school years in the *basic school track* from eight to nine. The reform was implemented at different points in time across federal states (see Appendix Table A5.1). It is exploited in other studies analysing the impact of increases in schooling on labour market outcomes (Pischke & von Wachter, 2008; Kamhöfer & Schmitz, 2016), health behaviours and health status of the affected generation (Kemptner et al., 2011), civic engagement (Siedler, 2010), and fertility (Cygan-Rehm & Maeder, 2013). Piopiunik (2014) uses the reform to study the effects on children's school track. The studies by Cygan-Rehm & Maeder (2013) and Piopiunik (2014) report slightly different years of the reform in four small states. I adhere to Pischke & von Wachter (2008), but the main findings reported in this paper are also robust to using the alternative reform dates. Cohorts affected by the reform had to stay in school one year longer. The first cohorts with a ninth grade in the basic track are born between 1934 (in Hamburg) and 1955 (in Bavaria).

The motives for introducing a ninth grade vary across states, probably because the goals of basic schooling shifted over time in post-war Germany. The first reforms in the early postwar period, such as in Hamburg, were motivated by high youth unemployment rates and limited vocational training places (Schneider, 1952). In the Hamburg Accord (*Hamburger Abkommen*) of 1964, all federal states agreed that the minimum number of school years should be nine, emphasising educational and developmental goals of an additional school year. The economy needed better-educated individuals, and transitioning 14-year old children into the labour market may be harmful for their development because they are in a vulnerable stage of their psychological development (Petzold, 1981).[6]

choice, or, for the high-ability track, formal admission exams. Mobility between tracks after initial assignment is generally very low (Dustmann, 2004).

[5]In the empirical analysis, the general increase in education levels in the population is accounted for by cohort-fixed effects and state-specific time trends. A large set of robustness checks is dedicated to ruling out that the empirical model still captures some general trends not related to increases in compulsory schooling.

[6]In some states, the introduction of the ninth grade was preceded by local and temporary introductions of a ninth grade. For example, before the ninth grade became compulsory in the states

In the additional grade, continued general education was a focus. Students would typically be continuously taught in the main subjects (e.g. mathematics, language arts, sciences, and vocational preparation). The exact curricula differed partly between states. For example, the federal state of Bremen focused on general knowledge, while Niedersachsen focused more on consolidating basic skills and on teaching political responsibility (Petzold, 1981; Pischke & von Wachter, 2008).

5.3 Data and empirical strategy

5.3.1 Data

The analysis is based on the German Micro Census, an annual representative survey of one percent of all households in Germany (RDC, 2017). Participation in the Micro Census is required by law. The scientific use file of the rich data contains a 70 percent random subsample. Although the data is used in studies on the causal effects of education on health behaviours and health status (see, e.g., Kemptner et al., 2011), and on intergenerational associations in education outcomes (see, e.g., Riphahn & Trübswetter, 2013), this is the first study using the data to estimate *intergenerational effects* of education.

The large data set contains rich socio-economic information, including the highest school degree, labour market outcomes, the state of residence, the birth year, and information on children in the household. I focus on children aged 15 to 18, as the share living with their parents is very high at 97.6 percent in 2009, and stable over time (96.8 percent in 1989, see Appendix Table A5.2). Additionally, the age distribution of children aged 18 and below living with their parents is stable over time (see Appendix Figure A5.1). This is because most children live with their parents until they complete vocational training or until they graduate from the academic track school and because they need parental approval to move out before age 18.[7]

of Bavaria (1969) and Niedersachsen (1962), these states allowed counties and towns to mandate a temporary ninth grade in the early 1950s to reduce youth labour market tensions. The findings are robust to controlling for potentially affected cohorts of the temporary introduction of a ninth grade (see Section 5.5).

[7]To rule out that my findings are confounded by changes in children's moving-out behaviour, I also restrict the sample to children aged 15-16, when they are almost exclusively still in school and living at home. Furthermore, I estimate the effects in data from the German Socio-Economic Panel Study (SOEP), in which information on children is also available after moving out from home. The Micro Census data contain direct pointers from children to mothers and fathers in the household

I determine parents' years of schooling based on their highest school degree and the typical number of years required to obtain this degree. I assume that parents went to school in the state they currently live in. This assumption seems reasonable, as cross-state mobility is low in Germany: 85 percent of individuals aged 40 to 50 still live in the state they went to school in (based on data from the German Socio-Economic Panel Study, SOEP, see Wagner et al., 2007). I focus the analysis mainly on parents with basic track schooling, of which 91 percent still live in their schooling state.[8]

Health-related questions are asked in several waves of the Micro Census to a 45 percent random subsample of households. Information on smoking behaviour is available in the surveys from 1989, 1995, 1999, 2003, 2005, and 2009; information on self-reported body height and weight for the years 1999, 2003, 2005, and 2009. To make best use of the available information, I pool all waves constituting two random samples.[9]

I focus the analysis on two main outcomes: Smoking and being overweight. While smoking is an important risky health behaviour, the overweight measure serves as an indicator for individual's general health status that is also related to other common risky health behaviours, such as low levels of physical activity or a poor diet (see, e.g., Hill, 1998, 2003; Janssen et al., 2005). Both outcomes are strong predictors of future health problems and chronic conditions (such as heart diseases, diabetes and cancer), and they are key determinants of major health risks in high-income

from 2005 onward. In earlier waves, identifying parents is possible based on children's relationship to the household head, information on household heads' partners, their marital status, and an age-range plausibility check on the potential parents (especially in multi-generational households). Using data from the 2005 Micro Census including the parent-pointers for cross-validation, I can identify about 98 percent of parents correctly. Any differences between survey waves that may arise from the improvement in reporting quality after 2005 should be unrelated to the compulsory schooling reforms, and are accounted for by survey year fixed-effects. In the German Micro Census, information on smoking behaviour is only available from age 15 onward.

[8] I look at parental place of residence in their 40s, because parents are about 30 years old when their child is born, and I observe their children at age 15-18. As in other international data, direct measures on years of schooling are rare in German data. Assigning the usual length of schooling based on the highest educational degree is a typical procedure in the literature (Stephens & Yang, 2014). For a subsample, I calculate the years of schooling based on the year of the final educational degree and find similar, though noisier first stage coefficients.

[9] The Micro Census follows a subsample of individuals for four consecutive waves, but the health related questions appear only once during this period. As health information only became available after 1989 and as I consider children up to age 18, I over-represent younger parental cohorts. The results are robust to weighting the regressions with inverse probability weights to represent the original distribution of birth cohorts in the population (see Section 5.5.2).

countries (WHO, 2009). In my analysis, the smoking variable takes the value one if adolescents smoke regularly or occasionally. The overweight assessment is based on adolescents' body mass index (BMI, calculated from body height and weight) and standard overweight thresholds for Germany (Kromeyer-Hauschild et al., 2001). Children are classified as overweight if their BMI is above the age-dependent 90th percentile-threshold. This definition is employed in other analyses on child obesity in Germany (see, e.g., Cawley & Spiess, 2008; Reinhold & Jürges, 2012). I also use the international thresholds based on Cole et al. (2000) in a robustness check.

Responding to the health-related questions in the Micro Census is voluntary, and self-reporting is potentially prone to social desirability bias or misreporting. Unless potential misreporting or missing information are systematically related to parents' being affected by the compulsory schooling reform, the causal effect analysis is not biased. Whether misreporting is an issue cannot be tested directly. Still, I can compare the reported body height and the body weight information to a large representative health survey of children in Germany that is based on external assessments rather than self-reporting (KiGGS study, conducted between 2003 and 2006 by the Robert Koch-Institute, see Kurth, 2007). The average body height in the Micro Census is slightly larger, and the body weight slightly lower.[10] With respect to missing information, I run a multivariate regression of missings in children's health-related information on their age, gender, parents' years of schooling, and household income (Panel A of Appendix Table A5.3). The probability for missing information in smoking behaviour decreases with age. Parental education correlates positively with missing information. I test for a causal relationship between missing information and parental schooling within the estimation framework outlined in Section 5.3.2 and cannot find evidence (Panel B of Table A5.3). Therefore, missing information are also not biasing effect estimates.

In the empirical analysis, I focus on parents in West Germany (excluding Berlin) who were born between 1930 and 1960. Descriptive statistics on the main samples are reported in Appendix Table A5.4.

[10]For example, 15-year old girls' average height (weight) was 165 centimetres (59.9 kilograms) in the KiGGS study, and 166 centimetres (57.4 kilograms) in the 2005 Micro Census. 15-year old boys' average height (weight) was 175.1 centimetres (66.4 kilograms) in the KiGGS study, and 174.4 centimetres (65.1 kilograms) in the 2005 Micro Census (these values are within the 95 percent confidence interval). For details, see Stolzenberg et al. (2007). Smoking is also self-reported in other surveys and cannot be validated by external assessments.

5.3.2 Empirical strategy

I estimate the causal effects of mothers' and fathers' schooling S_i^p on child i's health-related outcome H_i separately with the following model:

$$\begin{aligned}\mathrm{H}_i =& \alpha_1 \mathrm{S}_i^p + \alpha_2(\text{birth year FE})^p + \alpha_3(\text{state FE})^p \\ &+ \alpha_4(\text{state-time trend})^p + X_i' \alpha_5 + u_i\end{aligned} \tag{5.1}$$

To overcome potential endogeneity in parental years of schooling, I use the introduction of a ninth grade in the basic school track as an instrumental variable (IV) Z_i^p for parental schooling S_i^p:

$$\begin{aligned}\mathrm{S}_i^p =& \beta_1 \mathrm{Z}_i^p + \beta_2(\text{birth year FE})^p + \beta_3(\text{state FE})^p \\ &+ \beta_4(\text{state-time trend})^p + X_i' \beta_5 + \epsilon_i\end{aligned} \tag{5.2}$$

The introduction of a ninth grade in the basic school track varied across cohorts and federal states. Conditional on parents' birth cohort ("birth year FE") and federal state ("state FE"), parental exposure to the compulsory schooling reform is exogenous. Stephens & Yang (2014) stress the importance of accounting for region-specific trends when using regional variation in compulsory schooling laws for identification, as differential, region-specific improvements in, e.g., economic conditions may falsely be assigned to changes in compulsory schooling and overestimate the true benefits. To rule out that differences in regional trends are driving the estimated effects, I include interaction terms of parental state of residence dummies with a linear trend in their year of birth ("state-time trend"). The vector X_i includes dummies for children's age, gender, and the survey year. The model accounts for policy changes at the federal level (such as cigarette tax increases) through birth year dummies, survey wave dummies and children's age dummies. Their combination controls flexibly for children's year of birth and parents' age. The IV-strategy is similar to previous studies using education reforms to estimate the intergenerational effects of education, such as Holmlund et al. (2011) and Chou et al. (2010).

The resulting IV-estimator is the Wald estimator that rescales the reduced form effects by the first stage effects. For the identification of causal effects of increases in compulsory schooling, the German tracking systems provides an advantage over other school systems without tracking, such as the US: The reform was only binding for students in the basic track. Other school tracks already had more years of schooling. For treated basic track students, the required number of years in school increases by one, and the first stage coefficient from eq. 5.2 within this subsample equals one.

This feature is useful for two reasons: First, if the compulsory schooling reform had no effect on the highest school degree parents obtain, I can estimate the reduced form for children with parents from the basic track. Children with parents from higher tracks constitute a natural placebo group to test whether the reform coefficients capture effects related to parents' compulsory schooling increase or general trends unaccounted for by the model. Second, estimating the reduced form model in the complier-sample, rather than the IV-model, can result in more precise estimates. I estimate the following difference-in-differences model in subgroups based on parents' school track:

$$\begin{aligned} \mathrm{H}_i =& \gamma_1 \mathrm{Z}_i^p + \gamma_2(\text{birth year FE})^p + \gamma_3(\text{state FE})^p \\ &+ \gamma_4(\text{state-time trend})^p + X_i'\gamma_5 + \xi_i \end{aligned} \tag{5.3}$$

The notation follows analogously, and the coefficient of main interest is γ_1.

The identification strategy rests crucially on the common trend assumption. In the robustness section, I perform several checks (beyond the checks in the placebo sample) on the plausibility of this assumption. I check whether the results are sensitive to the inclusion of state-time trends or control variables, I perform a series of placebo-reforms assuming that the ninth grade was introduced earlier and I estimate the effects on a health-related, but mostly genetically determined, placebo outcome (adult body height, as it is less malleable than body weight). I then use parents from higher school tracks as an additional control group in a difference-in-differences-in-differences model to account for potential nonlinear state-specific trends. I also forgo the comparison to other states and estimate effects based on the idea of a regression discontinuity model in which state-trends in children's health behaviours are interrupted by the introduction of the ninth grade for some parents. Finally, I provide graphical evidence reassuring that unaccounted trends are not driving the results.

All models are estimated with robust standard errors clustered at the state by parental year of birth level.[11]

5.4 Results

5.4.1 The compulsory schooling reform and mothers' schooling

I first analyse the effect of the changes in compulsory schooling on mothers' educational attainment, the first stage for the IV-estimations. In Panel A of Table 5.1, I estimate the reform effect on mothers' years of schooling with the model from eq. 5.2. In both samples, with information on children's smoking behaviour and overweight, mothers' years of schooling increase by 0.48 and 0.65 years, respectively, with the introduction of a compulsory ninth grade in the basic school track. The effect is highly statistically significant with F-statistics above 20. The estimated effects on mothers' years of schooling differ between the samples (although not statistically) because children's overweight is observed in later waves of the German Micro Census than children's smoking behaviour. The data then contains fewer mothers born early in the observation period when the share of students in the basic track was higher. Due to educational expansions during this period, the share of women attending higher school tracks increased continuously. Such general, nationwide, trends in educational attainments are independent of the compulsory schooling reform and accounted for by the empirical model.

Increases in the years of schooling may result from the basic track requirement to stay in school for one more year, or from upgrades of students to higher school tracks that require more years of schooling. I estimate the reform effect on the probability of completing school with the basic track degree instead of a higher track degree (Panel B of Table 5.1). The point estimates are very small and insignificant, suggesting that the introduction of a ninth grade increases schooling only through increases in basic track schooling. Consequently, the reform effect on mothers' schooling reflects the mean share of mothers enrolled in the basic track when the reform was implemented in the federal states.[12]

[11] I draw the same conclusions if I cluster standard errors at the federal state level. In this case, I account for the small number of ten clusters with Wild Cluster Bootstrap procedures (for details, see Cameron et al., 2008). Table A5.10 reports the results.

[12] As I do not observe years of schooling directly in the data, I assign the typical number of school years for different school degrees based on parents' year of birth and the federal state they live

Table 5.1: Reform effects on mothers' schooling

	Sample	
Independent variable	Currently smoking (1)	Overweight (2)
	Dep. variable: Mother's years of schooling (imp.)	
Cohort with 9th grade in basic track	0.6454***	0.4833***
	(0.0424)	(0.1051)
F-test: instrument=0	231.21	21.16
	Dep. variable: Mother with middle/high track schooling instead of basic track schooling	
Cohort with 9th grade in basic track	0.0064	0.0007
	(0.0117)	(0.0235)
Sample mean	0.54	0.43
Number of observations	27,339	12,794

Notes: All OLS regressions also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, dummies for the survey year and a quartic in mothers' age. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state × mothers' birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

5.4.2 Parental schooling and children's outcomes in adolescence

I now turn to the question of whether parental schooling impacts children's health behaviours and health status. In Panel A of Table 5.2, I first estimate the relationship between children's smoking behaviour and being overweight at age 15 to 18, and mothers' years of schooling using ordinary least squares regressions, as outlined in eq. 5.1, and find a strong correlation.

To circumvent the potential endogeneity problem in mothers' educational attainment, I instrument mother's years of schooling using a dummy for the introduction of a ninth grade in the basic track. The IV-estimates suggest that one additional year

in. From 2005 onward, the Micro Census contains information on the year in which individuals completed their latest professional degree. I use this information in the much smaller sample to estimate the effect of the compulsory schooling change on mothers' years of schooling (see Appendix Table A5.5). The point estimates on the observed years of schooling are similar to estimates on the imputed number of years of education, but due to the much smaller sample size and measurement error in the reported information, it is less precisely estimated.

Table 5.2: Effects of mothers' schooling on children's health-related outcomes

	Dependent variable (age 15-18)	
Independent variable	Child smokes (1)	Child is overweight (2)
	Panel A: Full sample	
	OLS estimates	
Mothers' years of schooling (OLS)	-0.0216***	-0.0188***
	(0.0018)	(0.0016)
Number of observations	27,339	12,794
	Instrumental variable estimates (IV)	
Mothers' years of schooling (IV)	-0.0384***	-0.0448*
	(0.0127)	(0.0247)
Number of observations	27,339	12,794
	Panel B: Reduced form estimates	
	Subsample: Mothers from basic track	
Mother's cohort with 9th grade in basic track	-0.0412***	-0.0474**
	(0.0137)	(0.0193)
Sample mean	0.18	0.13
Number of observations	14,799	5,468
	Subsample: Mothers from middle/high tracks	
Mother's cohort with 9th grade in basic track	0.0016	-0.0065
	(0.0136)	(0.0126)
Sample mean	0.12	0.07
Number of observations	12,540	7,326

Notes: All OLS regressions also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, and dummies for the survey year. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state $\times$ mothers' birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

of mothers' schooling significantly reduces the probability that children smoke (3.8 percentage points) and are overweight (4.5 percentage points). The IV-estimates are larger than the OLS estimates. One reason for this may be that more education has particularly large effects at low levels of parental education (e.g. Imbens & Angrist, 1994).[13]

[13] Alternatively, the estimates may be upward biased because of measurement error in mothers' years of schooling. Bingley & Martinello (2017) estimate a bias in returns to education of 38 percent

We may be worried that the IV-estimates capture general trends in children's outcomes rather than the causal effects of changes in compulsory schooling. First note that the general trends in children's outcome variables go in opposite directions: While adolescence smoking is downward-trending after 2000, overweight is upward-trending. Cohort-specific trends are taken into account by a set of dummies for mothers' birth year, children's age and the survey year. Further, the empirical model allows for state-specific trends in mothers' birth year.

To test whether unaccounted trends may still drive the results, I make use of the German tracking system. I directly estimate the reduced form effects of changes in compulsory schooling in subsamples stratified by the maternal school track completed (Panel B of Table 5.2). In the sample with mothers from the basic track, where the compulsory schooling law was binding (and where the first stage equals one), the reduced form estimates are very similar to the IV-estimates, and more precisely estimated for overweight. Mothers from higher school tracks – born in the same birth years and residing in the same states – were not affected by the reform. I use children of these mothers as a placebo group and find that the estimated effects are small and insignificant.[14] To get an idea of relative effect sizes, it is important to determine an appropriate baseline level. Relating the point estimates to the sample mean would be misleading, because of trending outcome variables and substantial differences in outcomes by parental education, i.e. between children from complying mothers from low tracks and always-taking mothers from higher school tracks. Therefore, I calculate the baseline level from counterfactual outcomes of children with mothers from the low track. Consequently, one more year of maternal education reduces children's smoking rates by about 17 percent, and the incidence of overweight by about 26 percent.[15]

in Danish data. The reason is that years of schooling is a bounded variable in which measurement error is non-classical, typically resulting in a negative correlation with the true value. Accounting for this potential source of bias leads to point estimates that are still larger than OLS estimates (smoking 2.4 percentage points, being overweight 2.8 percentage points), suggesting that the local average treatment effects of an additional year of maternal schooling are indeed substantial.

[14] I also calculate IV-weights based on the formulas provided in Løken et al. (2012) to identify the part of the maternal education distribution that is contributing to the linear IV-estimates. The IV-estimators assign 96-99 percent of the marginal effects of mothers' education to mothers with 8 to 9 years of schooling, i.e. to mothers with basic track schooling (see Appendix Table A5.6). Values below 100 percent may arise through measurement error in mothers' years of schooling.

[15] Calculations of counterfactual means for the treatment group are obtained from the following equation: $E(H_i|Z_i = 1, \text{mother from low track}) - \hat{\gamma}_1$. For smoking, $E(H_i|Z_i = 1, \text{mother from low track}) = .204$; for overweight, $E(H_i|Z_i = 1, \text{mother from low track}) = .134$.

Table 5.3: Further reform effect estimates on children's health-related outcomes

	Dependent variable related to									
	Smoking behaviour						Weight problems			
Independent variable	Quitted Smoking (1)	Never smoked (2)	Smokes regularly (3)	Smokes occasionally (4)	Smoking starting age (5)	No. of cigarettes (6)	Overweight (international thresholds) (7)	Child is obese (8)	BMI (9)	Under-weight (10)
	Instrumental variable estimates(IV)									
Mothers' years of schooling (IV)	0.0057	0.0384***	-0.0236*	-0.0148*	0.0847	0.2761	-0.0406	-0.0189	-0.0551	-0.0441
	(0.0037)	(0.0127)	(0.0132)	(0.0085)	(0.1266)	(0.5694)	(0.0322)	(0.0156)	(0.2540)	(0.0310)
Number of observations	27,339	27,339	27,339	27,339	4,223	4,293	12,794	12,794	12,794	12,794
	Reform effect estimates (reduced form)									
	Subsample: Mothers from basic track									
Mothers' cohort with 9th grade	0.0086***	0.0412***	-0.0283**	-0.0129	0.0006	0.4219	-0.0370	-0.0263**	-0.1545	-0.0115
	(0.0032)	(0.0137)	(0.0119)	(0.0080)	(0.1301)	(0.5560)	(0.0236)	(0.0127)	(0.1533)	(0.0188)
Sample mean	0.01	0.82	0.14	0.04	15.58	10.69	0.17	0.05	21.83	0.10
Number of observations	14,799	14,799	14,799	14,799	2,673	2,727	5,468	5,468	5,468	5,468
	Subsample: Mothers from middle/high tracks									
Mother's cohort with 9th grade	-0.0034	-0.0016	0.0058	-0.0042	0.1910	0.1857	-0.0097	0.0053	0.0826	-0.0322
	(0.0049)	(0.0136)	(0.0129)	(0.0061)	(0.1390)	(0.9154)	(0.0172)	(0.0064)	(0.1894)	(0.0253)
Sample mean	0.01	0.88	0.09	0.04	15.56	9.18	0.10	0.02	21.07	0.11
Number of observations	12,540	12,540	12,540	12,540	1,550	1,566	7,326	7,326	7,326	7,326

Notes: All regressions also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, and dummies for the survey year. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state × mothers' birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

In Table 5.3, I investigate the effects on children's health behaviours and health status in more detail. I provide the IV-estimates (Panel A) followed by the reduced form effects for mothers from basic tracks (Panel B). To immediately check whether the common trend assumption is plausible for this set of outcomes, I report the estimates for children of unaffected mothers below. The reductions in smoking are mainly caused by children who never start smoking, but there are also small effects on quitting rates (only significant in reduced form, see columns 1-2). Moreover, increased maternal schooling reduces regular smoking, but there are also small reductions in occasional smoking (only significant in IV-estimates, see columns 3-4). Conditional on smoking, I find no effects on the age at smoking initiation or on the number of cigarettes smoked (columns 5-6).

With respect to children's weight problems, the effect on being overweight is similar if the BMI-thresholds determining overweight are based on international reference values reported in Cole et al. (2000), rather than the reference values for Germany as in Kromeyer-Hauschild et al. (2001, see column 7). However, the effect is less precisely estimated and just turns insignificant (the reduced form estimate has a p-value of 0.115). I also estimate the effect on children's obesity (BMI above the 97th percentile, based on Kromeyer-Hauschild et al., 2001). IV-estimates are imprecise, but the reduced form estimates suggest a significant reduction in adolescents' obesity of 2.6 percentage points (column 8). With respect to offspring's BMI, the reform estimates are negative, but insignificant. This may be related to increases in the BMI at the bottom of the BMI distribution, and reductions in the probability that children are underweight. The IV-estimate on being underweight indeed points to such a reduction, but it is imprecisely estimated (column 9-10).

The effects on smoking and overweight may vary by children's gender. Boys typically react more strongly to changes in early childhood conditions (e.g. Waldfogel, 2006), which may also relate to changes in maternal schooling. This is particularly true for children from lower socio-economic backgrounds (e.g. Autor et al., 2016). In Table 5.4, I report estimates of gender-specific treatment effects, interacting the treatment dummy with gender dummies. The increase in maternal compulsory schooling reduces girls' and boys' smoking probability, but the reduction is stronger for boys (column 1). With respect to being overweight, the effect on girls is slightly larger but not statistically different from the effect on boys (column 2). In Table 5.4, I report further effect estimates for children from families with household income

Table 5.4: Heterogeneity analysis of mothers' schooling effects

	Dependent variable:			
	Currently smoking		Overweight	
Independent variable	Coefficient	s.e.	Coefficient	s.e.
	By gender			
Cohort with 9th grade · female	-0.0259*	(0.0148)	-0.0531**	(0.0215)
Cohort with 9th grade · male	-0.0554***	(0.0156)	-0.0419*	(0.0215)
P-value for group difference	0.03		0.55	
	By household income			
Cohort with 9th grade · below median	-0.0473***	(0.0155)	-0.0412*	(0.0227)
Cohort with 9th grade · above median	-0.0316**	(0.0142)	-0.0575**	(0.0234)
P-value for group difference	0.18		0.51	
	By single mother status			
Cohort with 9th grade · single mother	-0.0493**	(0.0236)	-0.0831**	(0.0369)
Cohort with 9th grade · both parents	-0.0386***	(0.0139)	-0.0423**	(0.0199)
P-value for group difference	0.63		0.26	
	By mother's smoking behaviour			
Cohort with 9th grade · smoking	-0.0343*	(0.0186)	-0.0327	(0.0271)
Cohort with 9th grade · non-smoking	-0.0406***	(0.0138)	-0.0483**	(0.0206)
P-value for group difference	0.70		0.53	
	By mother's overweight			
Cohort with 9th grade · overweight	-0.0395***	(0.0143)	-0.0590**	(0.0234)
Cohort with 9th grade · not overweight	-0.0496**	(0.0217)	-0.0453**	(0.0214)
P-value for group difference	0.62		0.60	
Number of observations	14,799		5,468	

Notes: All OLS regressions are based on the sample of children with mothers from low tracks. The regressions also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, dummies for the survey year, a quartic in mothers' age, and the interaction variable. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state × mothers' birth year level and reported in parentheses. Mothers' smoking behaviour is missing for 29 observations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table 5.5: Effects of fathers' schooling on children's health-related outcomes

	Dependent variable:					
	Father's years of schooling	Father with middle/high track schooling	Children's health behaviour (sample-dependent)			
	Subsample by fathers' education:					
Independent variable	All fathers (1)	All fathers (2)	All fathers (3)	All fathers (4)	Only basic track (5)	Only high tracks (6)
	Sample: Child is smoking					
Cohort with 9th grade	0.5762***	0.0015			0.0154	0.0007
	(0.0538)	(0.0143)			(0.0159)	(0.0116)
Fathers' years of schooling (OLS)			-0.0163***			
			(0.0015)			
Fathers' years of schooling (IV)				0.0101		
				(0.0171)		
F-test: instrument=0	114.51					
Number of observations	22,252	22,252	22,252	22,252	12,393	9,859
	Sample: Child is overweight					
Cohort with 9th grade	0.5396***	0.0118			0.0128	0.0105
	(0.0891)	(0.0207)			(0.0183)	(0.0126)
Fathers' years of schooling (OLS)			-0.0175***			
			(0.0016)			
Fathers' years of schooling (IV)				0.0230		
				(0.0229)		
F-test: instrument=0	36.71					
Number of observations	10,189	10,189	10,189	10,189	4,741	5,448

Notes: All OLS regressions also include the full set of fathers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in fathers' year of birth, a dummy for female, dummies for children's age, and dummies for the survey year. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state × fathers' birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

below and above the median, from single mothers and both parents, from smoking and non-smoking mothers, as well as from overweight and non-overweight mothers, but there are no significant differences between these groups. If at all, the effects on smoking appear larger for children from households with incomes below the median (p-value= 0.18).[16]

Does paternal schooling also improve offspring's health behaviours and health status? I turn to this question in Table 5.5 and analyse the same sample of children, now basing the analysis on their fathers born between 1930 and 1960. In column 1, I report the reform effect of the introduction of a ninth grade on fathers' years of schooling. In both samples on children's outcomes, the estimated coefficients suggest an increase of 0.53-0.56 years. As for mothers, there is no evidence for reform effects on the highest completed school track of fathers (column 2). While OLS-estimates suggest that paternal schooling reduces children's smoking and being overweight (column 3), IV-estimates swap the sign of the relationship and turn insignificant (column 4). The reduced form effect of the introduction of a ninth grade in the subsample of children with fathers from the (treated) basic school track produces similar result (column 5). Effects in the subsample of children with untreated fathers from higher tracks are expectedly close to zero and insignificant (column 6).[17]

Within compulsory schooling changes, the limited impact of fathers' schooling on children's outcomes has been documented in other contexts as well. For example, Holmlund et al. (2011) and Lundborg et al. (2014) do not find evidence of effects on children's schooling and general health if fathers are affected by increases in compulsory schooling.

5.4.3 Parental schooling and children's outcomes in adulthood

Both smoking and being overweight in adolescence are highly predictive of related unhealthy behaviours and chronic conditions later in life (e.g. Guo et al., 2002; Jürges & Meyer, 2017). Therefore, improvements in adolescent health behaviours and health status should persist into adulthood, eventually resulting in better long-term health. In order to examine the long-term effects of intergenerational education

[16] The estimations also include the respective group dummy. The effect estimates are only unbiased if family income, single motherhood, mothers' smoking behaviour and overweight are not affected by the compulsory schooling reform. I provide evidence for this in Section 5.4.4.

[17] I also check for heterogeneous effects of paternal schooling on girls and boys, but cannot find any evidence.

Table 5.6: Effects of parental schooling on children's health-related outcomes at age 30-50

	Dependent variable measured at age 30-50:						
Independent variable	Currently smoking (1)	Quitted smoking (2)	Never smoked (3)	BMI (4)	Overweight (BMI>25) (5)	Chronic condition (6)	General health (z-score) (7)
	Panel A: Effects of mothers' schooling						
	Sample: Mothers from basic track						
Mother's cohort with 9th grade	-0.0905**	0.0436	0.0802*	-0.8893**	-0.0606*	-0.0677***	0.0960*
	(0.0383)	(0.0347)	(0.0442)	(0.3803)	(0.0335)	(0.0231)	(0.0528)
Sample mean	0.35	0.19	0.39	25.92	0.52	0.31	-0.05
Number of person-year obs.	27,901	10,991	10,991	21,320	21,320	21,180	65,845
Number of individuals	8,035	5,238	5,238	7,421	7,421	6,826	9,572
	Sample: Mothers from middle/high tracks						
Mother's cohort with 9th grade	0.0279	-0.0285	0.0046	0.4826	0.0199	0.0214	0.0197
	(0.0361)	(0.0525)	(0.0662)	(0.4462)	(0.0435)	(0.0291)	(0.0573)
Sample mean	0.27	0.20	0.46	24.92	0.42	0.29	0.11
Number of person-year obs.	9,014	3,024	3,024	7,553	7,553	8,624	21,992
Number of individuals	3,136	1,675	1,675	2,997	2,997	2,841	3,766
	Panel B: Effects of fathers' schooling						
	Sample: Fathers from basic track						
Father's cohort with 9th grade	-0.0047	0.0001	0.0321	0.0629	0.0266	0.0139	-0.0273
	(0.0410)	(0.0504)	(0.0564)	(0.4265)	(0.0381)	(0.0280)	(0.0591)
Sample mean	0.36	0.18	0.40	26.00	0.52	0.31	-0.06
Number of person-year obs.	22,709	8,704	8,704	17,603	17,603	17,592	52,757
Number of individuals	6,650	4,211	4,211	6,175	6,175	5,733	7,777
	Sample: Fathers from middle/high tracks						
Father's cohort with 9th grade	0.0265	0.0273	-0.0163	0.1218	-0.0519	0.0183	0.0573
	(0.0475)	(0.0525)	(0.0822)	(0.4705)	(0.0457)	(0.0361)	(0.0617)
Sample mean	0.28	0.21	0.44	24.95	0.42	0.29	0.13
Number of person-year obs.	8,824	2,942	2,942	7,448	7,448	8,495	20,925
Number of individuals	3,059	1,643	1,643	2,933	2,933	2,794	3,575

Notes: All OLS regressions also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, and dummies for the survey year. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state × mothers' birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP v32long, own calculations.

transmission, I draw a sample of individuals aged 30 to 50 from the German SOEP data.[18] I employ the same empirical strategy as outlined in eq. 5.3 and restrict the sample again to children of parents born between 1930 and 1960. Individuals providing the necessary information at different ages are included repeatedly to increase the sample size and the precision of the estimates. Inference is based on robust standard errors clustered at the state by parental year of birth level.

The results are reported in Table 5.6. Panel A shows that children of affected mothers are significantly less likely to smoke (column 1), mostly because they never initiated smoking (columns 2-3, based on a smaller sample as information on quitting and never-smoking is only available in fewer waves). In addition, children also have a lower BMI and a lower probability of being overweight (columns 4-5). As smoking and being overweight can cause severe chronic conditions, I test whether children of treated mothers are eventually less likely to suffer from chronic conditions later in life. I find evidence of a significant reduction (column 6). Children also report an improved general health status (column 7).[19] There are no such effects in the placebo sample of children with mothers from higher school tracks. As with adolescents, there are also no effects of paternal increases in compulsory schooling on children's health-related outcomes (see Panel B of Table 5.6). The results show that health-related behaviours are already determined early in life by maternal schooling. It also shows that increased maternal schooling reduces the socio-economic gap in health conditions.

5.4.4 Potential channels

What explains the substantial effects of increases in maternal schooling on children's health behaviours and health status? I focus the analysis on changes in children's human capital, their peer environment and family characteristics (family income,

[18] To maximise the sample size, I use children-reported information on parental birth year and highest level of education if it is not reported by parents themselves. I restrict the sample to individuals below age 50 in order to reduce the risk of endogenous sample selection that may result from maternal schooling effects on children's longevity.

[19] The outcome "chronic condition" is based on the survey question "Have you been suffering from any conditions or illnesses for at least one year or chronically?"; the outcome "general health" is based on "How would you describe your current health?" (measured in five categories ranging from very good to bad, rescaled such that higher values indicate better outcomes). "General health" is standardised to have a mean of zero and a standard deviation of one.

mating, family stability, fertility, as well as parental health status and health behaviours).

Children's human capital. I build on the theoretical framework by Cunha & Heckman (2007) and Heckman (2007) on the dynamic formation of human capital. In this framework, human capital is multidimensional and results from a multi-stage production technology. Improvements in earlier human capital, e.g. in the form of better health at birth, foster the development of later human capital (self-productivity). Moreover, increased human capital in early stages of life make investments in later periods more productive (dynamic complementarities). Currie & Moretti (2003) show that increases in maternal education in the US (caused by opening new colleges) improve prenatal care, lower smoking during pregnancy, increase gestational age, and reduce the risk of low birth weights. Further, Chou et al. (2010) show that increases in maternal education lower the incidence of low birth weights and infant mortality in Taiwan.[20] This may immediately reduce the inherent disadvantage that children are born with. Behrman & Rosenzweig (2004) and Black et al. (2007) show that low birth weights affect outcomes in adulthood, resulting in lower educational attainments and lower earnings. Case et al. (2005) show that poor health during childhood is associated with lower educational attainment, poorer health, and lower social status in adulthood.

Consequently, improvements in early health may improve other human capital dimensions. With dynamic complementarities, healthier children may benefit more from schooling. Indeed, numerous studies provide evidence that increases in maternal education increase the educational attainments of their children (e.g. Oreopoulos et al., 2006; Maurin & McNally, 2008; de Haan, 2011; Holmlund et al., 2011; Chevalier et al., 2013; Piopiunik, 2014). Increases in children's educational attainments, in turn, improve cognition (helping them process health-related information) and earnings prospects. These factors increase the costs of engaging in unhealthy behaviours, whose reductions ultimately improve long-term health (e.g. Grossman, 2006; Cutler & Lleras-Muney, 2010).[21]

[20]McCrary & Royer (2011) cannot find such effects using school entry policies as an instrument for schooling. However, the authors cannot rule out that maternal schooling effects on infant health are heterogeneous and not captured by their instrument.

[21]Increased education may also impact time preferences and risk aversion which may be related to unhealthy behaviours (Fuchs, 1982; Becker & Mulligan, 1997). However, the empirical relevance of this channel appears small (e.g. Cutler & Lleras-Muney, 2010).

Table 5.7: Effects of mothers' schooling on children's human capital

	Dependent variable: Child's school track			
Independent variable	Middle/high (1)	Middle (2)	High (3)	Middle/high (4)
Mother's cohort with 9th grade	0.0376***	0.0312*	0.0064	
	(0.0144)	(0.0163)	(0.0163)	
Mother's cohort with 9th grade · female				0.0259
				(0.0166)
Mother's cohort with 9th grade · male				0.0483***
				(0.0164)
Sample mean	0.56	0.29	0.27	0.56
Number of observations	16,081	16,081	16,081	16,081

Notes: All analyses are based on children aged between 17 and 18 with mothers from the basic track. The OLS regressions include the treatment dummy, the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, and dummies for the survey year. Standard errors are clustered at the federal state × mothers' birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

To test whether maternal schooling affects children's human capital in my data, I analyse children's educational attainment as one important dimension of it. I observe whether adolescents attend the basic, middle or high school track.[22] The school track is highly correlated with children's cognitive capabilities and earnings prospects: Dustmann & Schönberg (2011) report that the PISA scores in reading and mathematics of middle (high) track students are about 0.6 (1.5) standard deviations higher than those of children in the low track. They earn 24 (49) percent higher wages.

Indeed, the probability that children attend a higher track increases significantly if mothers are affected by the increase in compulsory schooling (column 1 of Table 5.7), mainly because children are more likely to attend the middle track rather than the low track (columns 2-3). Estimating the reform effect separately by gender suggests that the effect is stronger for boys (column 4), corroborating the evidence that boys from lower socio-economic backgrounds react more strongly to changes in early childhood conditions (Autor et al., 2016). These results are in line with

[22]Due to data limitations in early waves of the German Micro Census, I only reliably observe the (completed) school track at age 17 and 18, when children either completed schooling or are still enrolled to the final grades of academic track schools. I restrict the sample accordingly.

Piopiunik (2014), who evaluates the effects of the same reform on children's school track choice in data from the German SOEP.

Children's peer environment. Improvements in children's school track also improve the peer environment. Children in Germany are ability-tracked into physically separated schools as early as age ten. The differences in health behaviours and human capital of the peer environment are substantial: Compared to children in the basic track, children from higher tracks are 46 percent less likely to smoke, 50 percent less likely to be overweight, and 2.6 times more likely to have parents with a university entrance qualification.[23] These stark differences in the adolescent peer environment may play an important role in developing risky health behaviours. With respect to smoking, the large majority of individuals who smoke initiate smoking while they are still in school. Most of the educational gradient in smoking already exists before compulsory education is completed (Jürges & Meyer, 2017). Powell et al. (2005) show that school-level smoking rates play an important role for smoking initiation of adolescents. Moving students from a non-smoking school to a school where 25 percent of students smoke increases their smoking probability by about 14.5 percentage points, *ceteris paribus*. Lundborg (2006) also provides evidence for substantial school-peer effects in smoking, binge drinking, and drug use. Peer effects are also identified for weight problems (Trogdon et al., 2008; Carrell et al., 2011) and related behaviours such as sports, exercise, and unhealthy diets (Ali et al., 2011). Consequently, the tracking system may amplify improvements in early human capital and schooling resulting from increases in maternal schooling.

Family income and mating.[24] Numerous studies document a strong family income gradient in child health-related behaviours (e.g. Soteriades & DiFranza, 2003) and health (e.g. Case et al., 2002; Currie & Stabile, 2003). Higher family income allows parents to purchase better health services for their children as well as to invest in healthier lifestyles or safer and healthier environments. However, increased income may be less important in countries with almost universal health care that is of low private cost. Reinhold & Jürges (2012) find only weak evidence for a causal effect of parental income on child health in Germany, Kuehnle (2014) finds only small effects for the UK.

[23]Statistics are based on data from the German Micro Census and children in the analysed sample.
[24]For the following analyses, I use the same Micro Cencus waves as in the main analysis and include mothers with basic track schooling of all children aged 18 or younger to increase the sample size.

Table 5.8: Effects of mothers' schooling on family characteristics and parents' health-related outcomes

	Sample: Mothers from basic track			
Dependent variable	Sample mean (1)	Reduced form effect (2)	s.e. (3)	Number of observations (4)
	Panel A: Mothers' labour market outcomes			
Mother works	0.48	0.0158	(0.0107)	70,477
Mother's log hourly wage	1.49	0.0058	(0.0124)	70,477
	Panel B: Assortative mating			
Father's age in years	43.96	0.1555*	(0.0828)	62,811
Father's years of schooling	8.99	0.2324***	(0.0346)	62,811
Father's from middle/high tracks	0.22	0.0081	(0.0095)	62,811
Father works	0.91	-0.0039	(0.0075)	62,811
Father's log hourly wage	2.36	0.0112	(0.0156)	62,811
	Panel C: Family characteristics			
Single mother	0.13	-0.0065	(0.0063)	70,477
Mother is married	0.88	0.0040	(0.0063)	70,477
Number of children in HH	2.14	-0.0467*	(0.0273)	70,477
Mother's age at child birth	29.93	0.0639	(0.0670)	70,477
	Panel D: Parents' smoking behaviour			
Mother smokes	0.35	0.0061	(0.0139)	36,462
Mother has never smoked	0.49	-0.0080	(0.0150)	36,462
Father smokes	0.40	0.0080	(0.0160)	32,597
Father has never smoked	0.35	0.0172	(0.0174)	32,597
At least one parent smokes	0.51	0.0084	(0.0172)	38,117
Parents have never smoked	0.28	0.0064	(0.0151)	36,462
	Panel E: Parents' overweight			
Mother's BMI	25.08	0.1178	(0.3227)	10,723
Mother is overweight	0.43	0.0162	(0.0386)	10,723
Father's BMI	27.16	-0.2069	(0.2362)	9,280
Father is overweight	0.70	-0.0773***	(0.0251)	9,280

Notes: The table reports the coefficient estimates of the treatment dummy "mothers' cohort with 9th grade in basic track" from OLS regressions further including the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, and dummies for the survey year. All analyses are based on parents of children aged 18 and younger. Fathers' outcomes refer to reform effect estimates of maternal education on fathers' health behaviour (assortative mating and mothers' spill over effects). Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state $\times$ mothers' birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

To investigate the role of the family income channel in my setting, I first estimate reduced form effects on maternal labour market outcomes (reported in Panel A of Table 5.8). Affected mothers have a 1.5 percentage point higher probability to work and wages increase by 0.5 percent.[25] These small effects are in line with the zero-returns to the compulsory schooling reform in Pischke & von Wachter (2008) and Kamhöfer & Schmitz (2016). The authors argue that basic track students already learned labour market-relevant skills earlier.

In Panel B, I examine the reform effects on maternal mating behaviour. Treated mothers mate slightly older men with 0.23 more years of schooling. The increase in partners' years of schooling is largely related to partners also being affected by changes in compulsory schooling. The reform has no impact on the highest school degree of the partner, on the employment probability, or on earnings. The absence of labour market returns is not surprising given that the increase in partners' schooling stems from increases in compulsory schooling that did not generate significant labour market returns. Note, however, that the estimates are only informative on changes in family income, but not on the allocation of income. Affected mothers could still allocate more family income towards health-related inputs (Grossman, 1972). For example, mothers may provide healthier food for their children, invest in physical activity, or move to better neighbourhoods.

Family stability and fertility. Family disruptions can cause stress and make children more likely to initiate smoking (Francesconi et al., 2010) or to become overweight (Schmeer, 2012). I find no evidence for a reform effect on the probability of living with a single mother or of being married. Another potential explanation relates to fertility effects of education. If more schooling reduces the number of children or increases the age at birth, more resources may be allocated to a child (e.g., Hanushek, 1992). I find some evidence for effects on the number of children living in the household, pointing to fertility effects of the compulsory schooling reform. The effect estimate on maternal age when giving birth is slightly positive, but insignificant. The fertility effects are consistent with McCrary & Royer (2011), Cygan-Rehm & Maeder (2013), and Lundborg et al. (2014). The effects are also con-

[25] For the calculation of maternal log hourly wage I divide the net monthly income by the weekly working hours times 4.3, as in Pischke & von Wachter (2008). The log wage of non-working mothers is set to zero.

sistent with the quantity-quality trade-off theory. According to this theory, better educated parents invest more resources in the human capital of fewer children.[26]

Parental health status and health behaviours. An important explanation for children's health is seen in the health status and health-related behaviours of parents who may serve as role models (e.g. Powell & Chaloupka, 2005; Loureiro et al., 2010; Göhlmann et al., 2010). In Panel D, I analyse effects on the mother's own smoking behaviour and that of her partner, and cannot find any evidence. Potentially , this is because the introduction of a ninth school year affected children at a time when they had already initiated smoking. Moreover, most cohorts may have started smoking before the dangers of smoking became publicly recognized following the 1964 reports of the US Surgeon General on the harms of smoking (Lochner, 2011). The findings correspond to Kemptner et al. (2011), who evaluate the direct health effects of the reform. With respect to parental weight problems, I do not find evidence for effects on mothers' BMI and being overweight, but there is a reduction in the probability that the partner is overweight (Panel E).

In sum, the effects of increased maternal schooling on children's smoking and overweight are best explained by taking a dynamic perspective on human capital formation, including self-productivity of human capital and dynamic complementarity. Parents invest more in children's human capital (likely already improving early health outcomes) which improves children's schooling attainments. Resulting better cognition and earnings prospects may increase the costs of unhealthy behaviours. The effects are amplified by the tracking system, which exhibits a strong gradient in peer's health behaviours in adolescence – a sensitive developmental period. Improvements in the family environment (i.e. in terms of family income, mating, family stability, household size, and parental health behaviours) appear small overall. One explanation may be that the employed measures are not differentiated enough. For example, despite only small effects on parental health behaviours, parents may still be more aware of the negative consequences of unhealthy behaviours, thus encouraging children's physical activity, improving children's diet, or imposing smoking rules at home (Powell & Chaloupka, 2005). Additionally, the parent-child relationship may have changed, which can play a role in the prevention of risky health

[26]Substantial effects of education on fertility may induce an endogeneity problem, as selection into motherhood may confound the sample. This feature is shared by nearly all studies on intergenerational effects of schooling that also document effects on fertility as in, e.g., Carneiro et al. (2013) and Lundborg et al. (2014).

behaviours (Powell & Chaloupka, 2005). With a dynamic human capital production function in mind, even small changes in these dimensions may contribute to substantial improvements in health behaviours and long-term human capital.[27]

5.5 Robustness checks

5.5.1 Identification assumptions

At the heart of the identification strategies is the common trend assumption: States that introduced the compulsory schooling reform would have developed similarly over time with respect to children's outcomes as states that did not (yet) increase compulsory schooling. I perform several tests to check whether this assumption is plausible.

Throughout the analysis, I demonstrate that children's smoking behaviour and overweight only improve if mothers indeed attended the affected basic school track, assuring that the model is not just capturing general trends. In further checks on the common trend assumption, I simulate placebo reforms (Panel A of Table 5.9). I drop children with treated mothers from the sample and assume that the compulsory schooling reform was implemented two to five years before the actual reform.[28] The coefficient estimates vary around zero with changes in their sign and are statistically insignificant. The smaller sample size, however, increases the noise in the estimates.

Alternatively, I check whether the econometric model captures effects on health-related outcomes that should not be affected by changes in compulsory schooling (Panel B of Table 5.9). One such placebo outcome could be body height in adulthood, which is largely determined by genetic factors in high-income countries (Silventoinen, 2003). I estimate the effect on adult children in the SOEP data (as in Section 5.4.3) for children with mothers from the basic track.[29] The coefficient estimate is very small and insignificant, suggesting that effects related to children's BMI

[27] When I add the school track as a control variable to the main estimations, the coefficient on an additional year of schooling of the mother does not change significantly. I interpret this as evidence that the effect of maternal education on children's health behaviours works through a multiplicity of factors, is likely dynamic, and can only insufficiently be captured by the school track serving as a proxy for individual's human capital.

[28] A placebo reform in the year preceding the actual reform may be confounded by grade repeaters and late school entry.

[29] I use the sample of adult children rather than adolescents because smoking can stunt growth in adolescence (e.g. Stice & Martinez, 2005).

Table 5.9: Placebo reforms and placebo outcome

	Panel A: Placebo reforms				
	Actual	Placebo reforms in			
	reform	t-2	t-3	t-4	t-5
Independent variable	(1)	(2)	(3)	(4)	(5)
	Dependent variable: Currently smoking				
Mother's cohort with 9th grade	-0.0412***	0.0308	-0.0181	-0.0031	0.0196
	(0.014)	(0.021)	(0.022)	(0.023)	(0.020)
Number of observations	14,799	7,087	7,087	7,087	7,087
	Dependent variable: Overweight				
Mother's cohort with 9th grade	-0.0474**	0.0306	-0.0555	-0.0029	0.0315
	(0.019)	(0.058)	(0.039)	(0.064)	(0.058)
Number of observations	5,468	1,166	1,166	1,166	1,166
	Panel B: Placebo outcome				
	Dependent variable: Body height in cm (age 30-50) SOEP sample, children with mothers from basic track				
Mother's cohort with 9th grade	-0.0629				
	(0.5393)				
Sample mean	172.88				
Number of person-year observations	21,320				

Notes: All OLS regressions also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, dummies for the survey year and a quartic in mothers' age. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state × mothers' birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009 and SOEP v32long, own calculations.

result from changes in more malleable body weight rather than mostly genetically determined body height.

As the common trend assumption is conditional on covariates, I assess the sensitivity of the estimates to varying sets of covariates (columns 2-8 of Table 5.10): I first drop the X-variables (gender, children's age, survey wave); I add controls for states' GDP and unemployment rate when children are aged 18 to account for differential state trends; I drop state controls and state-specific time trends (X-variables put back in); I include two dummies in the main specification to indicate cohorts that were exposed to short/long school years when the national school calendar was har-

Table 5.10: Robustness checks: Control variables, time trends and models with alternative identification assumptions

		Control variables				State-specific time trends			Regression Discontinuity	Triple-Difference
	Main (1)	Without *X*-vector (2)	With state controls (3)	Short school years (4)	Early intro-duction (5)	Without state-trends (6)	Trends in child birth year (7)	Trends in mother & child birth years (8)	In mothers' birth year (9)	Between mothers' school tracks (10)
Panel A: Dependent variable: Currently smoking										
IV-Sample: All mothers										
Mother's years of schooling (IV)	-0.0384***	-0.0324**	-0.0391***	-0.0287*	-0.0368***	-0.0334***	-0.0431***	-0.0404***	-0.0346***	-0.0388***
	(0.013)	(0.013)	(0.013)	(0.015)	(0.013)	(0.012)	(0.013)	(0.013)	(0.013)	(0.014)
Number of obs.	27,339	27,339	27,339	27,339	27,339	27,339	27,339	27,339	25,591	27,339
Reduced form sample: Mothers with basic track schooling										
Mother's cohort with 9th grade	-0.0412***	-0.0388***	-0.0416***	-0.0338**	-0.0408***	-0.0403***	-0.0434***	-0.0403***	-0.0237*	
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)	(0.012)	
Number of obs.	14,799	14,799	14,799	14,799	14,799	14,799	14,799	14,799	13,739	
Panel B: Dependent variable: Overweight										
IV-Sample: All mothers										
Mother's years of schooling (IV)	-0.0448*	-0.0386**	-0.0446*	-0.0557*	-0.0409*	-0.0384**	-0.0349**	-0.0443*	-0.0345	-0.0346**
	(0.025)	(0.017)	(0.025)	(0.031)	(0.024)	(0.017)	(0.017)	(0.025)	(0.022)	(0.016)
Number of obs.	12,794	12,794	12,794	12,794	12,794	12,794	12,794	12,794	12,212	12,794
Reduced form sample: Mothers with basic track schooling										
Mother's cohort with 9th grade	-0.0474**	-0.0346**	-0.0480**	-0.0475**	-0.0462**	-0.0365**	-0.0348**	-0.0483**	-0.0289	
	(0.019)	(0.016)	(0.019)	(0.022)	(0.019)	(0.016)	(0.016)	(0.019)	(0.018)	
Number of obs.	5,468	5,468	5,468	5,468	5,468	5,468	5,468	5,468	5,272	

Notes: All regressions include the treatment dummy, the full set of year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in year of birth. Standard errors are clustered at the federal state × birth year level and reported in parentheses. State-controls are unemployment rates and GDP per capita at age 18. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

monized around 1966 (for reform details, see Pischke, 2007); I include two dummies for cohorts potentially affected by region-specific or temporary school year increases before the general increase in compulsory schooling was mandated (see Section 5.2 for details); I substitute the linear trend in mothers' year of birth with a linear trend in children's year of birth; and I include both linear trends in children's and mothers' year of birth. Across the specifications, the IV-estimates and reduced form estimates are very stable, and none of the estimated coefficients are statistically different from the main estimates.

In another set of tests on the common trend assumption, I employ models that rest on alternative identification assumptions. First, I use the idea of a regression discontinuity design (column 9): I centre the sample 15 years before and after the respective reforms and allow for a linear trend in the maternal year of birth for children's outcomes in each federal state. This trend is allowed to jump with the increase in mothers' compulsory schooling. The resulting IV-estimates are similar to the main effects. Reduced form estimates are smaller, but in the confidence band of the main estimates. Perhaps due to the parsimonious model, the effect on children being overweight turns just insignificant ($p = 0.11$). Note, however, that regression discontinuity designs likely underestimate the long-term benefits of education if outcomes are impacted by spill-overs from other cohorts (which is likely the case for health-related outcomes, see Lochner, 2011).

Alternatively, state-specific smoking regulations, education policies, overweight campaigns, or macro-economic conditions may create non-linear regional trends in children's outcomes. These factors are shared by all children in the same federal state, no matter whether their mother attended the basic track or higher tracks. I use children with mothers from higher tracks as an additional control group in a difference-in-differences-in-differences model. The estimates are very similar to the main results (column 10).[30]

Finally, I check on omitted trends graphically. I calculate residuals from the difference-in-differences models with the treatment dummy added back in. Appendix Figure A5.2 plots average residuals by the distance of mothers' cohorts to the compulsory schooling reform in their federal state. Systematic time trends that were not captured by the difference-in-differences model would be revealed in the

[30] IV-estimates are identical to the reduced form estimates because maternal years of schooling are generated based on their school degree, such that the first stage in the sample of basic-track mothers is one, and zero in the sample of mothers from higher tracks.

residuals. This is not the case: Before the introduction of the reform, the residuals vary around zero. After the reform, they are constantly below zero for children of mothers from the basic track. In the placebo sample of children with mothers from higher tracks, the residuals continuously vary around zero.

5.5.2 Sample choices and weighting

The analysis requires decisions on the selected main sample. I perform checks on the sensitivity of the main results to changes in the sample choice (see Appendix Table A5.8).

While I restrict the main sample to children of mothers born between 1930 and 1960, I could also centre the sample around the reforms, as employed by, e.g., Brunello et al. (2009). In columns 2-7 of Table A5.8, I include children of mothers born 10 to 15 years before and after the increase in compulsory schooling. I obtain similar estimates.

Next, I remove certain states and cohorts. In the analysis, I assume that parents went to school in the state they are currently living in. Cross-state mobility is generally low in Germany: 85 percent of adults in their 40s are still living in the same state where they went to school. This number is smaller in city-states (in the present sample Hamburg, 63 percent, and Bremen, 70 percent). Removing observations from these states yields similar results (column 8). Furthermore, a lagged roll-out of the programme within certain regions of the federal states, as well as early school entry of parents may introduce some fuzziness in the treatment assignment around the reform introduction. Dropping maternal cohorts that should have been treated first results in the same conclusions (column 9).

The data on children's smoking behaviour (overweight) was only collected in the German Micro Census from 1989 (1999) onward. Therefore, the probability of observing younger cohorts of mothers is higher. In Panel A of Appendix Figure A5.3, I plot the frequency with which mothers' birth cohorts appear in the main sample, and the original frequency of female births in the population (based on the German Micro Census 1989). Similarly, the overweight-sample over-represents mothers giving birth at older ages (Panel B of Appendix Figure A5.3, based on the full sample from the German Micro Census of children aged 0-18 living with their parents). I run the main analysis using inverse probability weights to match the population frequencies on mothers' year of birth and age at birth as plotted in Figure A5.3

(see Table A5.9). Assigning higher weights to cohorts with few observations and less informational content, and lower weights to cohorts with more observations, increases the noise in some estimates. In the sample containing children's weight information, I remove the small number of mothers born before 1940 to reduce this noise. Overall, the results are equivalent to the main findings.[31]

5.6 Conclusion

This paper traces the effects of an increase in compulsory schooling in Germany on children's health behaviours and long-term health. Mothers' increase in schooling substantially reduces children's probability to smoke and to be overweight in adolescence and adulthood. The findings pass a large set of robustness and placebo checks. For fathers, I do not find such effects. The effects on children's health-related outcomes are likely a product of a multiplicity of factors, including improvements in early childhood health, better schooling attainment, improved cognition, and a better peer environment in adolescence. Improvements in the family environment, including family income, mating, family stability, household size, and parental health behaviours, appear small overall. Still, in a dynamic framework of human capital formation, even small changes in these dimensions may contribute to substantial improvements in health behaviours and long-term human capital.

Since 2000, there has been a substantial reduction in teenage smoking rates in high income countries (e.g. WHO, 2015). In the same time period, women significantly increased their education levels (e.g. OECD, 2015). My findings suggest that the increase in female education is causally related to the reduction in teenage smoking rates. If we assume a homogeneous treatment effect of mothers' education on children's health behaviours, back-of-the-envelope calculations suggest that increases in female schooling account for approximately thirty percent of the reduction in teenage smoking rates.[32] With respect to increases in overweight and obesity that

[31] To reassure that selective moving-out of adolescent children is not confounding the results, I also estimate effects separately for children aged 15-16, who are almost exclusively in education and still living with their parents, and for children aged 17-18 (Table A5.7). Effects on smoking behaviour are slightly smaller for children aged 15-16 (although not significantly), which may relate to lower smoking rates at younger ages. Effects on being overweight are very similar to the main results, but smaller subsamples increase the sensitivity to including state-specific time trends and reduce the precision of the estimates.

[32] Between 1999 and 2009, the total years of schooling of mothers in my sample increased by approximately 1.5 years because the share of women in the basic track declined as capacities in higher

can be observed globally, increases in maternal schooling still have a protective effect on children's weight problems.

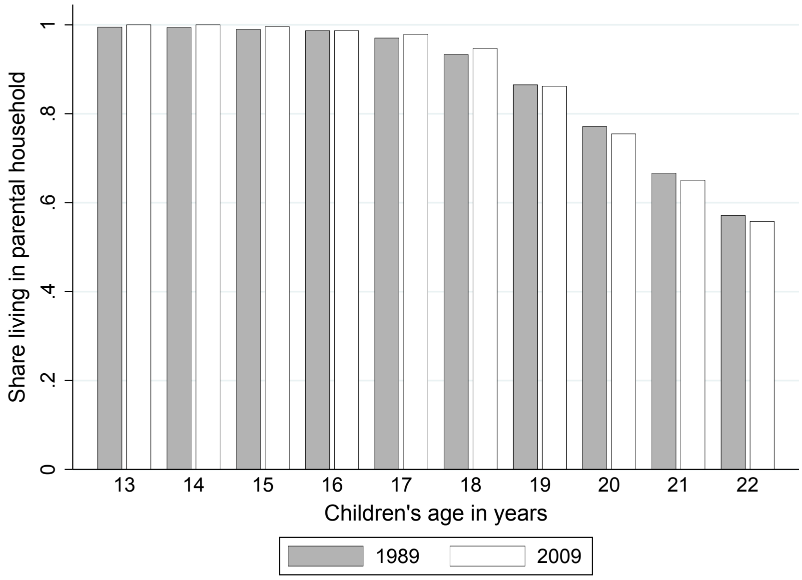
While this study establishes a causal link for long-term effects of parental education on children's health, the analysis also has some limitations. One should bear in mind that the results apply to children of parents with rather low levels of education (although more than half of the parents attended this track). If we expect the benefits of schooling to be larger for lower levels of education (Imbens & Angrist, 1994), the estimates should be interpreted as an upper-bound. Future research should try to identify effects in higher parts of the parental education distribution. Additionally, the proposed mechanisms deserve further investigation. An exciting, though challenging, avenue for future research is to better understand *how* parental education impacts children's health. A major challenge will be to acquire detailed information on, e.g., parental inputs around birth and throughout childhood, on peers of children together with exogenous variation in parental education or on child-parent relationships.

The paper contributes to our understanding of the link between education and health, suggesting that a substantial portion of the causal relationship is overlooked if intergenerational effects are not considered. The results show that the impact of maternal education on health behaviours and health is an important mechanism through which economic status is transmitted. The paper significantly contributes to our understanding of non-market benefits of education. While the literature typically cannot disentangle the effects of education from effects of higher incomes, I provide evidence from a setting in which the income channel is mostly closed. The paper also improves our understanding of long-term effects of educational interventions that strengthen the case for public investments in general education.

school tracks increased (see, e.g., Jürges et al., 2011). The share of women with college education also increased. Over the same time period, the teen smoking rate declined by approximately 13 percentage points (Federal Center for Health Education, 2011). The back-of-the-envelope estimation considers the conservative point estimate as described in Section 5.4, i.e. 2.6 percentage points $\times$ 1.5 years / 13 percentage points = 0.3.

Appendix

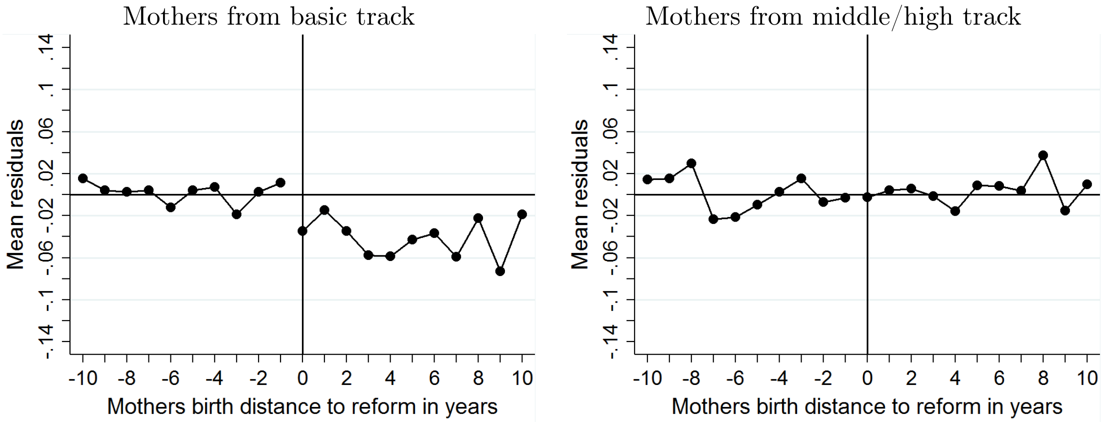
Figure A5.1: Share of children living with at least one parent by children's age



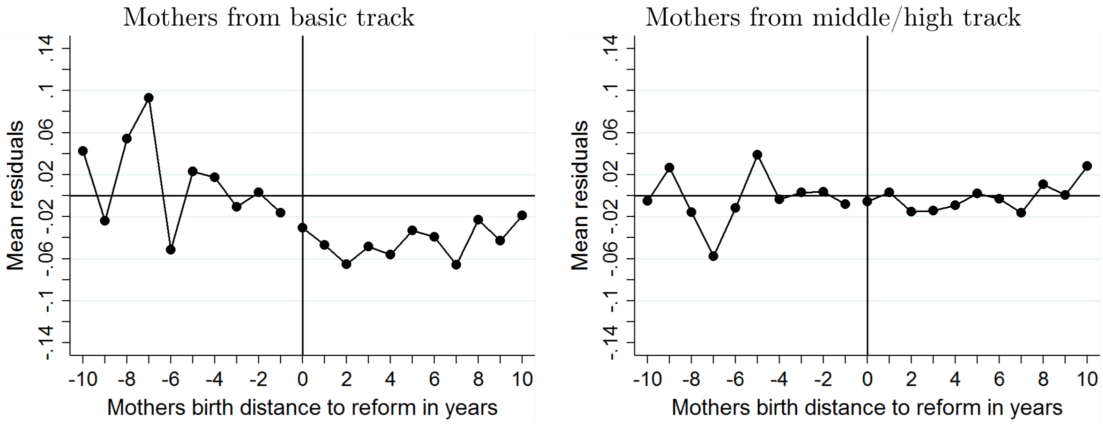
Notes: The figure plots the share of children in private households living with at least one parent. Children in the main samples are between 15 and 18 years old.
Source: RDC (2017), German Micro Census 1989, 2009, own illustration.

Figure A5.2: Residuals from the difference-in-differences regression models of children's health-related outcomes on mothers' compulsory schooling exposure

Panel A: Outcome: Child is smoking (age 15-18)



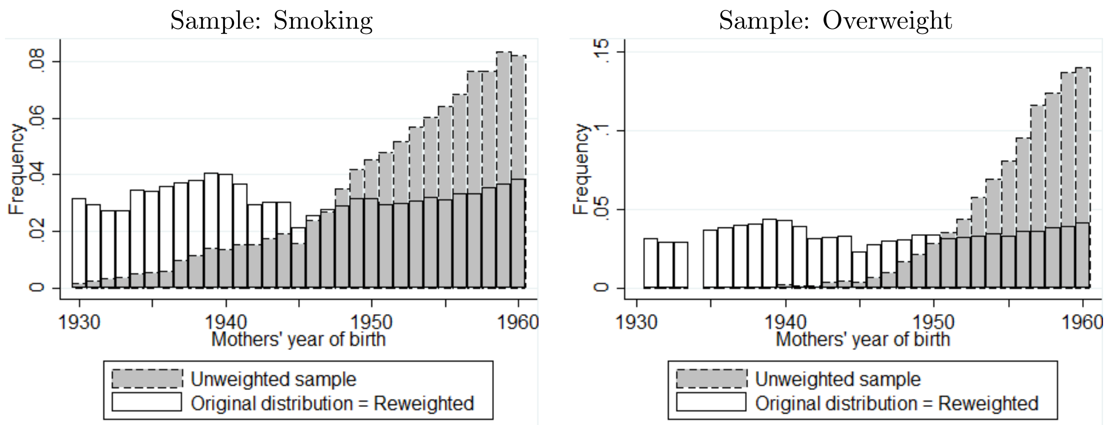
Panel B: Outcome: Child is overweight (age 15-18)



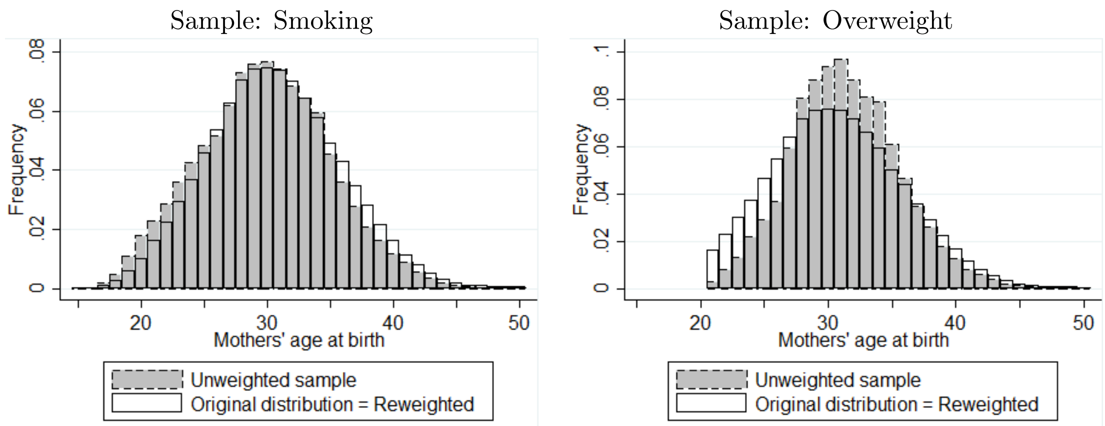
Notes: The graphs plot residuals from the main difference-in-differences regression models employed to estimate the impact of mothers' exposure to the compulsory schooling reform on children's smoking behaviour and overweight (treatment dummy added back in) for children of mothers from basic track schools (affected group) and middle/high track schools (unaffected group).
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own illustration.

Figure A5.3: Distributions of mothers' year of birth and mothers' age at birth

Panel A: Distribution of mothers' year of birth



Panel B: Distribution of mothers' age at birth



Notes: The histograms plot the distributions of mothers' year of birth and mothers' age at childbirth in the population and in the two main samples. The population distributions are used to construct inverse probability weights that are employed in weighted regressions reported in Table A5.9.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own illustration.

Table A5.1: Introduction of 9th grade in basic track of secondary school.

State (Bundesland)	First year when all students are supposed to graduate after 9 years	Birth cohorts with 9 years of school
Hamburg	1949	1934
Schleswig-Holstein	1956	1941
Bremen	1958	1943
Niedersachsen	1962	1947
Saarland	1964	1949
Nordrhein-Westfalen	1967	1953
Hessen	1967	1953
Rheinland-Pfalz	1967	1953
Baden-Württemberg	1967	1953
Bayern	1969	1955

Source: Pischke and von Wachter (2005).

Table A5.2: Share of children aged 15-18 living with at least one parent

	Year					
All years	1989	1995	1999	2003	2005	2009
96.50	96.75	96.21	95.78	95.89	96.72	97.63
(18.39)	(17.74)	(19.09)	(20.11)	(19.86)	(17.81)	(15.20)

Notes: The table reports descriptive statistics on the share of individuals in private households aged 15-18 living with at least one parent. Standard deviations are reported in parentheses.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table A5.3: Statistical relationship for missing information on child outcomes

Independent variable	Dependent variable (child outcome): Missing information for: Currently smoking (1)	Overweight (2)
	Panel A: Multivariate regressions	
Child age	-0.0056***	0.0000
	(0.0019)	(0.0030)
Female	-0.0045	0.0117
	(0.0044)	(0.0072)
Mothers' years of schooling	0.0043**	0.0077***
	(0.0020)	(0.0029)
Fathers' years of schooling	0.0058***	0.0057**
	(0.0016)	(0.0024)
Household net income (in 1000 EUR)	-0.0021	-0.0058**
	(0.0017)	(0.0026)
Number of observations	31,353	18,268
	Panel B: Instrumental variable estimations	
	Sample: Mothers	
Mother's years of schooling (IV)	0.0022	-0.0191
	(0.0157)	(0.0344)
Sample mean	0.16	0.30
Number of observations	31,353	18,268
	Sample: Fathers	
Father's years of schooling (IV)	0.0215	0.0253
	(0.0231)	(0.0371)
Sample mean	0.16	0.30
Number of observations	25,417	14,525

Notes: OLS regressions in Panel A also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth and dummies for the survey year. The regressions include dummy variables for missing information for socioeconomic characteristics. In Panel B, IV-estimations are based on the main estimation model outlined in Section 5.3.2. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state × mothers' birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table A5.4: Descriptive statistics for the main samples

	Sample by child outcome (age 15-18)			
	Smoking[1]		Overweight[2]	
	Sample mean	s.d.	Sample mean	s.d.
	Child characteristics			
Currently smoking (D)	0.16	(0.36)		
Overweight (D)			0.10	(0.30)
Female (D)	0.49	(0.50)	0.48	(0.50)
Age in years	16.58	(1.11)	16.62	(1.11)
	Mother characteristics			
Years of schooling	9.69	(1.62)	10.15	(1.61)
Married (D)	0.87	(0.33)	0.86	(0.34)
Working (D)	0.66	(0.47)	0.73	(0.44)
Work hours/week (if working)	26.94	(14.15)	25.14	(13.16)
Log hourly wage in EUR	2.23	(0.58)	2.30	(0.59)
Age at birth in years	29.82	(5.24)	31.44	(4.31)
Number of children in household	2.07	(0.96)	2.01	(0.91)
Currently smoking (D)	0.27	(0.44)	0.27	(0.44)
BMI	24.33	(4.20)	24.36	(4.20)
BMI>25 (D)	0.35	(0.48)	0.35	(0.48)
	Partner characteristics			
Years of schooling	9.81	(1.87)	10.29	(1.89)
Working (D)	0.91	(0.28)	0.91	(0.29)
Log hourly wage in EUR	2.65	(0.47)	2.73	(0.49)
Currently smoking (D)	0.31	(0.46)	0.28	(0.45)
BMI	26.57	(3.66)	26.57	(3.63)
BMI>25 (D)	0.64	(0.48)	0.64	(0.48)
	Household characteristics			
Household size	4.00	(1.09)	3.91	(1.05)
Household net income in EUR	3039.29	(1849.74)	3499.39	(2099.31)
Both parents in household	0.86	(0.35)	0.84	(0.36)
Number of observations	27,339		12,794	

Notes: The table provides descriptive statistics for the different samples depending on the child outcomes. Standard deviations are reported in parentheses.
Source: RDC (2017), [1] based on German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, [2] based on German Micro Census 1999, 2003, 2005, 2009.

Table A5.5: Comparing first-stage coefficients on mothers' schooling of imputed and observed information

	Sample					
	Smoking		Overweight		All	
	Dependent variable: Mother's years of education:					
Independent variable	Imputed (1)	Observed (2)	Imputed (3)	Observed (4)	Imputed (5)	Observed (6)
Mother's cohort with 9th grade	0.6454***	0.8044*	0.4833***	0.4304	0.6485***	0.7114*
	(0.0424)	(0.4208)	(0.1051)	(0.4444)	(0.0297)	(0.4004)
Number of observations	27,339	7,736	12,794	6,700	55,217	9,074

Notes: The table reports reduced form estimates of mothers' years of education on an indicator of a 9th grade in the basic track. "Imputed" years of education are assigned based on mothers' school degree and the typical length of schooling in the federal state. "Observed" years of education are calculated from information on the year in which the highest vocational degree was completed. This information is only available for a subsample of 90% of individuals from 2005 onward. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table A5.6: IV-weights

	Sample	
Years of schooling margins	Child is smoking (1)	Child is overweight (2)
8 to 9 years	0.961	0.988
9 to 10 years	0.004	0.001
10 to 12 years	0.018	0.010
12 to 13 years	0.017	0.000

Notes: The table reports weights that the IV estimator assigns to the marginal effects of maternal education across the years of schooling distribution. The weights are reported for the two main samples employed in the analyses. The weights were obtained based on the formulas provided in Løken et al. (2012).
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table A5.7: Only for certain age groups

	Dependent variable (child outcome):			
	Smoking behaviour		Overweight	
Independent variable	Without state time trends (1)	With state time trends (2)	Without state time trends (3)	With state time trends (4)
	Reduced form estimates for children with mothers from the basic track			
	Sample: Only children aged 15-16 years			
Mother's cohort with 9th grade	-0.0378**	-0.0388***	-0.0484*	-0.0384
	(0.0146)	(0.0149)	(0.0261)	(0.0283)
Number of observations	6,256	6,256	2,416	2,416
	Sample: Only children aged 17-18 years			
Mother's cohort with 9th grade	-0.0468**	-0.0509**	-0.0314	-0.0522**
	(0.0202)	(0.0204)	(0.0222)	(0.0262)
Number of observations	7,774	7,774	3,052	3,052

Notes: All OLS regressions also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, dummies for the survey year and a quartic in mothers' age. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state × mothers' birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table A5.8: Robustness checks on sample restrictions

	Symmetric time window around reform implementation (in years)							Sample restrictions	
Independent variable	Main (1)	+/- 15 (2)	+/- 14 (3)	+/- 13 (4)	+/- 12 (5)	+/- 11 (6)	+/- 10 (7)	w/o Hamburg & Bremen (8)	w/o first treated cohort (9)
Panel A: Dependent variable: Currently smoking									
IV-sample: All mothers									
Mother's years of schooling (IV)	-0.0384***	-0.0300**	-0.0345***	-0.0313***	-0.0332***	-0.0308**	-0.0237*	-0.0365***	-0.0417***
	(0.013)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.014)	(0.013)	(0.013)
Number of observations	27,339	25,591	25,228	24,575	23,905	23,197	22,431	26,641	25,898
Reduced form sample: Mothers from basic track									
Mother's cohort with 9th grade	-0.0412***	-0.0353**	-0.0400***	-0.0391***	-0.0405***	-0.0380**	-0.0298*	-0.0400***	-0.0460***
	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.016)	(0.014)	(0.016)
Number of observations	14,799	13,739	13,503	13,157	12,797	12,399	11,982	14,500	14,008
Panel B: Dependent variable: Overweight									
IV-sample: All mothers									
Mother's cohort with 9th grade	-0.0448*	-0.0495**	-0.0465*	-0.0499**	-0.0465*	-0.0439*	-0.0369	-0.0474*	-0.0476*
	(0.025)	(0.025)	(0.025)	(0.025)	(0.024)	(0.026)	(0.027)	(0.027)	(0.026)
Number of observations	12,794	12,212	12,153	11,909	11,673	11,432	11,168	12,482	12,107
Reduced form sample: Mothers from basic track									
Mother's cohort with 9th grade	-0.0474**	-0.0492**	-0.0476**	-0.0478***	-0.0486***	-0.0434**	-0.0369**	-0.0501**	-0.0497**
	(0.019)	(0.019)	(0.018)	(0.018)	(0.018)	(0.019)	(0.018)	(0.020)	(0.021)
Number of observations	5,468	5,272	5,255	5,168	5,079	4,983	4,879	5,370	5,151

Notes: All regressions include the treatment dummy, the full set of year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in year of birth. Standard errors are clustered at the federal state × birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table A5.9: Weighted regression models

Independent variable	Unweighted (main) (1)	Regressions reweighted to match distribution of: Female births 1930-1960 (2)	Regressions reweighted to match distribution of: Mothers' age at birth (3)
	Panel A: Dependent variable: Currently smoking		
	IV sample: All mothers		
Mother's years of schooling (IV)	-0.0384***	-0.0352*	-0.0414***
	(0.013)	(0.019)	(0.013)
Number of observations	27,339	27,339	27,339
	Reduced form sample: Mothers from basic track		
Mother's cohort with 9th grade	-0.0412***	-0.0324*	-0.0447***
	(0.014)	(0.018)	(0.014)
Number of observations	14,799	14,799	14,799
	Reduced form sample: Mothers from middle/high tracks		
Mother's cohort with 9th grade	0.0016	-0.0005	0.0005
	(0.014)	(0.014)	(0.014)
Number of observations	12,540	12,540	12,540
	Dep. variable: Overweight		
	IV sample: All mothers		
Mother's cohort with 9th grade	-0.0448*	-0.0554	-0.0503*
	(0.025)	(0.047)	(0.026)
Number of observations	12,794	12,752	12,794
	Reduced form sample: Mothers from basic track		
Mother's cohort with 9th grade	-0.0474**	-0.0462	-0.0598***
	(0.019)	(0.029)	(0.021)
Number of observations	5,468	5,442	5,468
	Reduced form sample: Mothers from middle/high tracks		
Mother's cohort with 9th grade	-0.0065	-0.0071	-0.0051
	(0.013)	(0.017)	(0.012)
Number of observations	7,326	7,310	7,326

Notes: The table reports IV and reduced form estimates from unweighted and weighted regressions. Weighted regressions reweight the observations to match the birth frequencies based on mothers' birth year (information based on Micro Census 1989 for all females born between 1930 & 1960) and based on mothers' age at birth (based on the full sample of children aged 0-18 living with their parents, irrespective of provided child outcome information). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table A5.10: Robustness check: Clustering of standard errors

Independent variable	Coefficient estimate (1)	p-value for clustering at: Federal state - mothers' birth year level (2)	Federal state level (3)
	Dependent variable: Child is smoking		
Mother's cohort with 9th grade	-0.0412	[0.0029]***	[0.0120]**
	Dependent variable: Child is overweight		
Mother's cohort with 9th grade	-0.0474	[0.0150]**	[0.0841]*

Notes: Column (1) reports OLS coefficient estimates of the main specification in the sample of children with mothers from basic track schools. Column (2) reports p-values based on robust standard errors clustered at the federal state $\times$ mothers' birth year level (303 clusters for smoking, 202 clusters for overweight). Column (3) reports p-values based on clustering at the federal state level using wild cluster bootstrap procedures to account for the small number of clusters (10 clusters, 999 replications, Mammen weights, testing under H0, for details see Cameron et al., 2008). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Chapter 6

Conclusion

This dissertation examines the effects of education and family policies on the formation of human capital. It analyses different dimensions of human capital, including various skills in early childhood, measures of student performance in school, as well as indicators of individuals' health status and health-related behaviours. It takes a lifecycle perspective on the formation of human capital, analysing a family policy in early childhood, changes in school inputs during secondary schooling, and intergenerational effects of an education reform that effectively changes children's family environment. The findings presented in this dissertation demonstrate that public policies can impact human capital formation at various stages in the lifecyle. Public policies can also contribute to widening or reducing differences in human capital. In the following, specific limitations of the analyses and scope for future research are discussed. The dissertation closes by deriving policy implications and by acknowledging its general contributions.

6.1 Limitations and scope for future research

Chapter 2 examines the effects of a substantial change in parental leave benefits on child development and socio-economic gaps in child development. The chapter studies a reform in Germany that changed eligibility for paid parental leave such that higher-income households benefited relatively more from the reform than lower-income households. The precise and robust estimates reveal no effect of the reform on child development and on socio-economic development gaps. The chapter concludes that such substantial changes in parental leave benefits are unlikely to considerably impact children's development.

The methodological approach, the large sample size, and the high quality of the data used for the analysis allow for reliably estimating precise zero-effects of the reform. Still, the analysis focuses on a specific set of child outcomes at age six. It may still be conceivable that the parental leave reform unfolds effects that are not yet captured at age six or that effects occur on different dimensions of child development that are not captured by the rich set of outcomes. Small changes could accumulate to larger changes over time, such that long-term effects of the reform may reveal effects on children. Such sleeper-effects are observed for various other policy interventions. For example, Chapter 5 demonstrates that increases in compulsory schooling have only minor direct effects on individuals' health status and health-related behaviours, but if one looks at the next generation, effects are substantial. Future research on parental leave policies may investigate the impact of parental leave policies on further dimensions of human capital and on longer-term outcomes observed later in life. As the empirical estimation strategy is based on children's birthday, other administrative data sources including information on children's birthday, such as records from health care providers, could provide precise and robust estimates of reform effects.

The absence of treatment effects in the analysis of Chapter 2 raises the question *why* there are no reform effects. This may either occur because effects are heterogeneous in dimensions not considered by the analysis, because effects on different potential channels cancel each other out, or because the reform has no effect on factors relevant for child development. The analysis pays particular attention to heterogeneity analysis, considers other studies on the same reform analysing parental reactions to learn about potential channels, and scans the economics literature to provide plausible arguments for the absence of treatment effects. Still, future research should try to better understand whether parental leave policies impact parental investments at all (in terms of time and material investments) at margins that are relevant for child development. Detailed analyses of potential effect channels would require detailed information on parental investments at different stages in early childhood, on disposable household income, child-related expenditures, or alternative modes of child care. Future research should also pay further attention to heterogeneity analyses in dimensions that are not possible to consider in the analysis in Chapter 2 due to data limitations. For example, effects may differ between children of first-time mothers and children whose mother already has a child. Furthermore, effects may differ by the actual length of parental leave taking, as well as paternal involvement in parental leave. While such analyses could help better understand why the parental benefit reform has no impact on child development, a major challenge will be to identify data sets with the relevant information *and* a sufficiently large number of observations that would allow for a reliable estimation of the reform effects.

A caveat of the empirical, quasi-experimental approach of Chapter 2 is that it can only capture the immediate effects of a policy shock. The methodology cannot capture effects on children of mothers that give birth several months or years after the policy change. The immediate effects may differ from later effects because mothers may consider parental leave regulations in their timing of birth (which may change mothers labour market attachment and financial resources at the time of birth), social norms about the length of maternity leave may change, or gradual increases in the availability of public child care may interact with effects of parental leave regulations. These factors may change early childhood conditions and eventually children's development in ways not captured by estimates of the policy shock. Learning about the impact of parental leave on child development that includes longer-term adjustments would require a more structural estimation approach. In contrast to quasi-experimental methods, such approaches sometimes need to impose strong assumptions on the formation of human capital, but they may contribute interesting insights into aspects of parental leave policies and the impact on children that quasi-experimental methods cannot capture.

Chapter 3 studies the role of school instruction time in skill development. The estimation of causal effects is based on the G8-education reform in Germany that increased instruction hours to reduce the length of academic track schooling. For students in grade 9, this reform is used to identify causal effects of increased instruction time. The results show that instruction time improves students' PISA performance on average. Estimating treatment effects along the distribution of student performance reveals important heterogeneities: Higher-performing students benefit more from additional instruction time than low-performing students. The content of additional instruction time is an important determinant of the skill returns. The findings demonstrate that increased instruction time can widen the gap between low- and high-performing students.

The policy experiment provides new insights into one of the most expensive and most important school input factors across countries. Despite its importance, the economic literature knows little about the determinants of returns to school instruction time. This chapter hypothesises that the content of instruction time is an important determinant leading to substantial heterogeneities that has received little attention. However, it cannot be tested statistically whether the pattern across the student performance distribution is indeed related to the content of instruction time, as the reform lacks sufficient variation in this dimension. Although other economic studies corroborate this hypothesis, it still remains to be tested empirically whether the pattern across the student performance distribution is less pronounced or even reversed if additional classroom time is dedicated to remediation instead of additional learning content.

Furthermore, it would be interesting and highly relevant for policy-makers to learn about interaction effects of instruction time with other school input factors. How do the returns to instruction time vary with teacher characteristics, the class size, as well as the school and peer environment? Answers to these questions are very important, but addressing these questions with reliable empirical strategies is very challenging, because they require exogenous variation in instruction time *and* the respective heterogeneity dimension. Future research on instruction time should take the curricular content of additional classroom time into account, provide more evidence on differential patterns of the effects, and identify other natural experiments or conduct field experiments that provide the relevant exogenous variation in instruction time, curricular content, *and* other school input factors to further inform the literature on determinants of effective instruction time in the formation of human capital.

While the analysis focuses on student competencies in key domains of PISA, future research should also investigate how instruction time affects children's health and non-cognitive skills. The importance of non-cognitive skills for future labour markets is increasing and their formation may be impacted by the time spent both in school and outside school. With a limited time budget set, increases in instruction time imply that students have less time for activities outside the classroom. For example, students may engage less in sports clubs, volunteering, or political activities, which may have consequences for their health, non-cognitive skills, or social responsibility. Thiel et al. (2014) and Dahmann & Anger (2014) provide first evidence on the effects of school instruction time on non-cognitive skills; Quis & Reif (2017) analyse the effects on students' health and well-being; Meyer & Thomsen (2015) and Krekel (2017) analyse which activities outside school are substituted by increased instruction time. The findings are mixed, and more remains to be learned about the impact of instruction time on further dimensions of human capital.

A further limitation of the analysis is that it cannot consider medium- and long-term effects of increased instruction time on, e.g., students' earnings because the reform also entails a one-year reduction in the total years of schooling. Consequently, the reform only serves as a suitable experiment to investigate short-term effects of increased instruction time. With respect to longer-term effects, the reform can still shed light on the question whether increased instruction time in earlier years can compensate reductions in the years of schooling. This question is approached in Chapter 4. Learning about the long-term impact of increased instruction time alone requires other (policy) experiments.

Chapter 4 examines the effects of compressing instruction time into fewer years of schooling on various measures of student performance using the G8-education reform. The reform is a unique natural experiment that was implemented at different points in time across federal states, which the empirical approach exploits for estimating causal effects. The analysis uses measures of student performance from aggregated administrative data on the full population of students. The results show that the reform increases grade repetition rates and lowers final grade point averages, without affecting final graduation rates. Overall, the results suggest that compressing instruction time into fewer years of schooling has adverse effects on student performance, but the economic significance of these negative effects appears moderate.

The analyses use administrative data with the major advantage of covering the universe of school children in Germany. A caveat of the administrative data is that students cannot be tracked individually over time or linked across outcomes, and that it lacks detailed information on the socio-economic characteristics of students. Such information would allow further heterogeneity analysis with respect to students' ability, their family background or their gender (gender information is not available for all outcomes). It could improve our understanding of who suffers most from compressing instruction time, and it may help reveal potential adverse reform effects on SES gaps in student performance. However, other data sets that contain such information, such as the National Educational Panel Study (NEPS) or the Socio-Economic Panel Study (SOEP), contain much fewer observations per cohort and state, the level of variation that identifies causal reform effects. The smaller sample size would probably lead to less robust estimation results, while also complicating heterogeneity analyses.

While the analyses in this chapter focus on measures of student performance during secondary schooling, future research should use this unique policy experiment to evaluate the G8-reform effects on other dimensions of human capital, on post-secondary education paths, and eventually on labour market outcomes. Some of the above-mentioned studies also analyse the effects of compressing instruction time (compared to the effects of increased instruction time captured when children are still in school) on personality traits (Thiel et al., 2014; Dahmann & Anger, 2014), cognitive skills (Dahmann, 2017), and health-related outcomes (e.g. Quis & Reif, 2017). Meyer & Thomsen (2016) and Zambre & Marcus (2017) evaluate transitions to post-secondary education. As time passes, there is much more to be learned, thus improving our understanding of the role of school instruction time and years of schooling. The G8-reform may be of growing interest for policy-makers in other countries experiencing demographic ageing as they also face a trade-off between maintaining and increasing levels of education and early labour market entries. The G8-reform tries to address this challenge by compressing instruction time into fewer years.

Chapter 5 sheds new light on the long-term impact of education, in particular on the effects of parental schooling on children's health status and health-related behaviours. The findings reveal that increases in maternal schooling reduce their children's probability to smoke and to be overweight. It also lowers the risk that children suffer from chronic conditions in adulthood that often result from unhealthy lifestyles. No such effects can be identified for paternal schooling. The effects can be best explained within a dynamic lifecycle model of human capital formation, in which increased maternal education increases investments in children's education and improves their peer environment early in life. These changes accumulate, leading to substantial improvements in children's health-related behaviours and health status across their lifecycle.

A major challenge in the analysis of such intergenerational causal effects is to identify suitable data sets that capture the relevant time period, contain a sufficiently large number of observations, and relevant outcome variables. The analysis in Chapter 5 employs two data sources that complement each other with respect to some of their limitations. Analyses in the German Micro Census provide a sufficiently large number of relevant observations and robust identification; however, the data are restricted to certain health-related and education outcomes in children's adolescence. Moreover, the analysis needs to impose assumptions on parents' state of schooling (which are plausible based on checks in complementary data). Analyses in SOEP data, in turn, provide a very rich set of information, even when children are adults. However, the analyses need to rely on a smaller number of individual level observations and less precise estimates.

The data limitations restrict possibilities to conduct further analyses on the channels through which parental education impacts children's health and health-related behaviours. The chapter hypothesises that increased investments in children's education and improvements in their peer environment early in life are important for explaining the effects. Suggestive empirical evidence supports this hypothesis. The analysis also investigates the role of other potential channels, including changes in family income, family stability, fertility, and parental health-related behaviours, but these channels appear less relevant empirically. Note, however, that the channels are restricted to the time of their measurement. The data lacks information on potential channels in childhood, for example. Moreover, there are many other potential channels that the analysis could not consider because of lacking data, such as improvements in children's diet, increases in their physical activity, the neighbourhoods they live in during childhood, or information on the parent-child relationship. A major challenge for future research on this topic, however, will be to identify data sources with detailed information on potential channels that satisfy the high data requirements that analyses of intergenerational causal effects of education impose.

Finally, as the analysis focuses mainly on health-related outcomes, it also appears promising to estimate further intergenerational returns to education, including effects on children's labour market outcomes, welfare dependencies, delinquency rates, fertility and life expectancy, contributing to the growing research on non-market benefits of education. Long and ageing panel data sets, such as the Socio-Economic Panel Study (SOEP), may become increasingly suitable to approach such questions and may even allow for estimating causal effects of education for the third generation, i.e. effects of grandparents' schooling.

Some limitations shared by all chapters should also be borne in mind when interpreting the findings of this dissertation. All chapters present empirical findings that emerge from specific policy interventions conducted at a certain point in time within a certain institutional setting, applying to a certain group of individuals, and relating to specific sets of outcomes. The empirical approaches and robustness checks suggest a high internal validity of the findings. However, the specific estimates of causal effects may not be immediately transferable to similar policies implemented at a different point in time, in other institutional settings, or targeted at different groups. One may, therefore, wonder about the usefulness of studying specific, sometimes "old" reforms, such as compulsory schooling increases from the 1950s and 1960s to inform research and policy-making. Studying these reforms is very useful for at least two reasons: First, analysing specific policy changes can provide *reliable* estimates of causal relationships that theory suggests. This in turn can support or weaken certain theories. Second, they can help *identify* causal relationships that have not yet been considered. In this sense, studying policy reforms can be an explorative research approach that can help develop economic theory. If one wants to learn about causal long-term effects with quasi-experimental approaches, relying on "old" policies is inevitable.[1] Gathering evidence from different quasi-experiments can provide a first idea of potential effects in the short-run to inform policy-making. In the long-run, they are important to identify generalisable patterns that can guide economic theory-building and policy-making, always having external validity limitations in mind.[2]

It is also important to note that all studies presented in this dissertation assess the *effectiveness* of education and family policies in determining certain dimensions of human capital. However, for the allocation of scarce public resources, it is also important to assess the *efficiency* of policies. This perspective additionally contrasts policy benefits with the related costs. Detailed knowledge on the costs and benefits of policies can help policy-makers allocate public resources towards measures that reach similar benefits at lower costs, or higher benefits at similar costs. However, solid efficiency assessments are complex, require strong assumptions and can be highly dependent on parameters chosen for such analyses.[3] The results of efficiency

[1] Alternatively, one could approach such questions with a structural modelling approach and policy simulations, but this approach needs in turn to rely on assumptions of structural relationships.

[2] To increase the external validity of the findings presented in this dissertation, reforms at different points in time at different margins could analyse similar outcomes. For example, the effect of parental schooling on children's health status and health-related behaviours could be analysed with subsequent increases in compulsory schooling from nine to ten years, or using the expansion of places at academic track schools. To identify generalisable patterns across a large number of reliable empirical studies, meta analyses are one possible analytical approach.

[3] The most common approaches used to assess the effectiveness of education and family policies are cost-effectiveness analyses and cost-benefit analyses (e.g. Spiess, 2013). Cost-effectiveness analyses compare cost-effectiveness ratios of a specific outcome between different programmes. Cost-benefit analyses monetise programme effects and related costs to calculate cost-benefit ratios or internal

analyses depend on, *inter alia*, the outcome dimensions considered, when outcomes are measured, how the effects are monetised, at what rate they are discounted, as well as on the consideration of externalities and spill-over effects that may be difficult to assess and monetise. For example, a programme that results in reduced smoking rates may not only lower the future costs of health care services, but it may also lower the burden of others experienced by passive smoking, and increase utility of family members if individuals live healthier, longer lives. Next to the assessment of benefits, the assessment of related costs can be complex. For example, programme costs may not be documented (e.g. because they are integrated in broader budgets), they may be borne at different levels (at the federal level, regional level, and by private contributors), and programmes may create subsequent costs. This may happen, for example, if some early childhood intervention increases children's probability to study at a publicly funded university. Despite the complexity of efficiency analyses, they are very important for the allocation of public resources. This dissertation contributes robust estimates of causal policy effects that build a crucial basis for subsequent efficiency assessments.

6.2 Policy implications and general conclusion

Despite the limitations and outlined scope for future research, the findings presented in this dissertation carry important implications for policy-makers.

Chapter 2 shows that even drastic changes in parental benefits are unlikely to have a large impact on children if a country already has a generous parental leave system in place that allows parents to take longer birth-related work interruptions. Two major implications follow from this finding. First, if the support of children's development is an important policy goal, other early childhood interventions appear more effective, such as increasing the supply of high-quality public child care or targeted parenting programmes (e.g. Francesconi & Heckman, 2016; Cornelissen et al., 2017).

Second, if parental leave reforms target policy goals other than child development, such as increasing fertility, raising paternal involvement in child rearing, and supporting maternal labour supply after childbirth, they can be achieved without

rates of return of single programmes. Monetising costs and benefits allows considering multiple outcome dimensions and aggregating short-, medium- and long-term effects.

adverse effects on children and socio-economic development gaps. The 2007 German parental benefit reform, for example, did not target child development. In contrast, policy-makers worried that the reform may widen SES gaps in child development. Major goals were safeguarding family income and increasing parental care time after childbirth, increasing mothers' economic independence, and increasing paternal involvement in child rearing. To achieve these goals, public resources were redistributed from low- to high-income households without impacting children's development.

Chapter 3 demonstrates that increases in instruction time can increase student performance on average, but also widen the performance gap between high- and low-performing students, contributing to inequality in human capital. Consequently, policy-makers can face an equity-efficiency trade-off if they increase instruction time. Students need different amounts of time to learn, which should be taken into account in the design of education systems. Varying the learning speed according to students' needs could be realised within a system that tracks students according to their ability. However, policy-makers should bear in mind that there are also disadvantages to tracking. For example, early tracking can increase the intergenerational link in education and socio-economic status (e.g. Pekkarinen, 2014). As an alternative policy option, children of different ability levels may not be separated into different schools, but they could rather be supported within the same school environment through, e.g., subject-specific tracking that considers students' learning needs and ability levels without socially segregating children early in life.

Chapter 4 demonstrates that a younger school-leaving age with a similar skill level of students is achievable only if reductions in school years are compensated by redistributing curriculum and instruction time to earlier years. The compensation is important: A policy-experiment in Canada that essentially removed one year of schooling without instruction time increases in earlier years had detrimental effects on student performance (e.g. Morin, 2013; Krashinsky, 2014). The reform in Germany, however, had only moderate negative effects on student performance: a slight increase in grade repetition rates and a small decline in GPAs. At the same time, final graduation rates were stable and the vast majority of students graduated from school at a younger age with similar skill levels. This provides novel insights into the substitutability of instruction time and years of schooling, and questions the economic concept of *years of schooling* altogether. The economics literature often abstracts from institutional details and summarises individuals' schooling in the single measure *years of schooling*. This chapter, however, reveals that this concept does not capture the many dimensions of schooling appropriately, and that a more nuanced view is required.

The chapter also contributes significantly to the German policy debate around the G8-reform and the understanding of effects of this major education reform that is still highly controversial and discussed intensively in the public domain. Some states have already returned to the system with more years of schooling, while other states are still discussing their length of schooling. The public discussion around this topic is often based on subjective perceptions and emotions of teachers, parents, children and policy-makers. Adding empirical evidence to the debate provides a more research-based ground to guide policy-making. Considering that a major objective of the reform was to lower students' age at labour market entry with a similar level of skills, the finding that graduation rates are not affected by the reform, together with the results that most students graduate earlier, shows that adverse reform effects are moderate.

Chapter 5 shows that changes in maternal schooling can have a lasting and substantial impact on the next generation. Improvements in children's long-term health status and health-related behaviours reveal substantial spill-over effects of education that are typically not taken into account by individuals in their educational investment decisions. However, such spill-over effects have a high value for society: Improved health of the next generation will likely result in reduced costs for social security and health care systems, freeing up resources that can be used for other public policy objectives.

The results also highlight the value of general education. A major benefit of education is that it typically raises individual earnings. This makes it hard to disentangle whether benefits of increased education on other nonpecuniary dimensions, such as health, result from higher earnings or from increased education *per se* (Oreopoulos & Salvanes, 2011). However, the reform studied in Chapter 5 has no effects on earnings (Pischke, 2007; Kamhöfer & Schmitz, 2016), creating the unique opportunity to isolate non-pecuniary effects of education when the income channel is largely closed. The findings reveal that despite the absence of labour market returns, increases in schooling can generate other substantial benefits, such as long-term improvements in health status and health-related behaviours, that justify public investments in general education.

Overall, the dissertation contributes four quasi-experimental analyses demonstrating that public policies can impact human capital formation, but also the emergence of differences in human capital and closing gaps therein.

The chapters analyse ethically, politically, and financially actionable policies. They could generally be transferred to other institutional contexts, although the effects may differ in other institutional settings. The policies analysed in this dissertation are implemented in Germany, an environment providing education and health services at low private cost and a generous social security system, especially when compared to the US. Evidence from different institutional settings will help identify generalisable patterns that can serve theory-building and guide policy-making.

The dissertation stresses the importance of considering heterogeneous treatment effects of education and family policies. The effectiveness of policy interventions can depend on the exact design of policies (such as whether additional instruction time carries additional learning content), on individual characteristics (such as ability or gender), on institutional characteristics (such as pre-intervention support schemes), and on the timing of the intervention. Heterogeneity analysis is not only important for revealing for whom policies are particularly effective, but it also reveals unintended side-effects that are important for the overall assessment of policy programmes. They are particularly important for the debate on rising inequality.

Furthermore, the dissertation illustrates the value of secondary data sources for conducting research and guiding policy-making. None of the data sources used for the empirical analysis in this dissertation were generated for the purpose of the analyses. Still, they provide valuable insights into effects of significant policy interventions and their impact on human capital formation. Related to this, the dissertation emphasises the usefulness of combining data from different sources, as each data source has limitations that can be compensated for by complementary data. There is still significant potential for the use of secondary, especially administrative, data sources in Germany for research and guiding policy that could be lifted if research-related access to data was granted more systematically and if data from different sources could be linked more easily.

Finally, the results of this dissertation demonstrate that there is no easy answer to *when* policy interventions are most effective. Theory suggests that early childhood interventions can be very effective because of dynamic complementarities. In this dissertation, changes in parental leave benefits turn out to be ineffective at changing children's human capital (Chapter 2), while changes in parental education (Chapter 5) have a substantial and lasting effect on children's human capital. These findings demonstrate that the effectiveness of measures do not just depend on the timing of the intervention, but also on the specific policy action taken.

In sum, the findings in this dissertation support the idea that human capital consists of many dimensions that are formed over the lifecycle. There are many aspects in the formation of human capital that deserve further research attention to better guide public policies. Still, the dissertation demonstrates that education and family policies can be effective tools that impact the formation of human capital throughout the lifecycle.

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