

Granting Birthright Citizenship –  
A Door Opener for Immigrant Children's Educational  
Participation and Success?

Christina Felfe  
Judith Saurer

Ifo Working Paper No. 208

December 2015

An electronic version of the paper may be downloaded from the Ifo website  
[www.cesifo-group.de](http://www.cesifo-group.de).

## Granting Birthright Citizenship – A Door Opener for Immigrant Children's Educational Participation and Success?\*

### Abstract

Does birthright citizenship boost immigrant children's educational participation and success? We address this question relying on a reform of the German naturalization law in 1999 that entitled children born after January 1, 2000 to birthright citizenship. We isolate the impact of birthright citizenship by comparing children born shortly before and shortly after the cutoff in years of policy change and years in which no policy change took place. Results based on administrative data indicate positive effects across all educational levels – preschool, primary school and secondary school – which, however, fade out over time. The underlying mechanism seems to be parental decisions and not improved (evaluations of) children's skills.

JEL Code: I21, K37.

Keywords: Immigration law, education, difference-in-difference.

Christiane Felfe\*\*  
University of St. Gallen  
and CESifo  
Varnbuelstr. 14  
9000 St. Gallen, Switzerland  
Phone: +41(0)71/224-2329  
christina.felfe@unisg.ch

Judith Saurer  
Ifo Institute – Leibniz Institute for  
Economic Research  
at the University of Munich  
Poschingerstr. 5  
81679 Munich, Germany  
Phone: +49(0)89/9224-1691  
saurer@ifo.de

\* We thank Prashant Bharadwaj, Natalia Danzer, Eva Deuchert, Christian Dustmann, Beatrix Eugster, Albrecht Glitz, Martin Huber, Helmut Rainer, Heather Royer, Andreas Steinmayr and Steven Stillmann for valuable comments on earlier versions of this paper. We presented the paper to seminar audiences at Ifo Institute Munich, University of St. Gallen, NIW Hannover, University of Mannheim, UC San Diego, UC Santa Barbara, University of Southern California, UC Davis, EALE 2014, Verein für Socialpolitik 2014, SMYE 2014, RES 2014, and SOLE 2015. We are indebted to the MSGWG (Ministry of Social Affairs, Health, Science and Equality) of Schleswig Holstein/Germany, which granted us access to and further information about their annual reports of the school entrance examinations. We especially thank our cooperation partners – Heidrun Thaiss, Ute Thyen and Sabine Brehm – for their support and thoughtful input and Leibniz Society for funding through the CEMIR project.

\*\* Corresponding author.

# 1 Introduction

The integration of immigrants is a challenge for many developed countries. Economic disadvantages, such as worse job prospects or lower earnings, are only some of the problems immigrants face.<sup>1</sup> Moreover, the disadvantages inherent in immigration are not only suffered by first-generation immigrants, but also by their descendants. One example for this is their relatively lower educational achievement.<sup>2</sup> Closing such gaps between the immigrant and native population matters for several reasons. Immigrants' lack of educational and economic success may lead to social and economic exclusion, which in turn may lead to social unrest. In addition, poor performance in the educational system or in the labor market may foment native population prejudice against the immigrant population and thus may further hamper integration.

One highly debated, albeit contentious policy intended to foster the integration of immigrants is the granting of citizenship. Expected benefits are chiefly related to immigrants' labor market outcomes (Chiswick, 1978; Brantsberg et al., 2002; Fougère and Safi, 2009; Steinhardt, 2012; Gathmann and Keller, 2014). In addition to the usual channels for obtaining citizenship, many countries are considering the introduction of birthright citizenship for second-generation immigrants (e.g. Germany in 2000, Portugal in 2004, Greece in 2010, Austria and Italy in 2013). In a similar vein, the United States is contemplating the legalization of undocumented immigrants, also known as the DREAM act. What are the consequences of granting immigrant children citizenship at birth? Several recent studies document positive effects of birthright citizenship on the length of stay and integration efforts of immigrant children's parents (Avitabile et al., 2013, 2014; Piracha and Zhu, 2012; Sajons, 2010, 2012). Far less understood are the direct consequences for immigrant children themselves.<sup>3</sup>

To the best of our knowledge, we are the first to examine the effect of granting birthright citizenship on children's educational outcomes from birth to adolescence.<sup>4</sup> Our particular focus lies on the participation and success of immigrant children in the host country's educational system.

---

<sup>1</sup>For a description of the economic situation of first- and second-generation immigrants in Europe and the United States see Algan et al. (2010), Borjas (1985) and Chiswick (1980).

<sup>2</sup>A comparison of the educational attainment of second-generation immigrants with that of children born to native parents is provided by Dustmann et al. (2012) and Riphahn (2003).

<sup>3</sup>The exception is Avitabile et al. (2014) who analyze whether birthright citizenship affects fertility and children's body mass index.

<sup>4</sup>There is simultaneous work by Clots-Figueras and Sajons (CS). The differences to our study are as follows: First, we use administrative data which provide information on children's educational outcomes from birth up to adolescence. CS rely on survey data which is much smaller in size and contains only one of our outcome variables, namely attendance to the academic track of secondary school. Second, our data is available for several school cohorts and thus allows for a comparison of immigrant children over time. CS, on the contrary, rely on a comparison of immigrant and native children.

Analyzing the impact of birthright citizenship on children’s educational outcomes is challenging. In particular, naturalization applications are more likely to be filed from families that feel part of the host country’s society or see themselves living in the host country in the long-run. A mere comparison between immigrant children with and without citizenship is thus prone to yield biased results. Therefore, our identification strategy relies on a natural experiment, specifically a reform of the naturalization law in Germany that implied a change from *ius sanguini* – only descendants of home country nationals receive citizenship – to *ius soli* – everyone born on the national territory is eligible for citizenship. To be more precise, the reform under study established that all immigrant children born in Germany after January 1, 2000, with at least one parent having a minimum duration of legal residence of eight years, were automatically granted German citizenship.<sup>5</sup> This setup provides us with a sharp cutoff regarding the entitlement to birthright citizenship. To avoid possible seasonal effects or age of school entrance effects on children’s educational outcomes, we not only compare immigrant children born shortly before and shortly after the cutoff date, but draw upon immigrant children from adjacent cohorts as a control group. In other words, we employ a difference-in-difference design (DiD) to isolate the causal effect of introducing birthright citizenship on the educational attainment of children.

We draw upon two large and unique datasets from one German federal state (Schleswig-Holstein) enabling us to derive precise results despite the fact that our empirical analysis focuses on a very small subset of the German population – a few cohorts of immigrant children. First, we rely on administrative records from school entrance examinations. These records contain physicians’ assessments of children’s school readiness at age six. An accompanying questionnaire, filled out by the parents, provides information on children’s previous preschool enrollment and family background. We use the records of 6,740 immigrant children born between July 1998 and June 2001 and examined for school entrance between 2005 and 2007. Second, we rely on administrative school registers. These registers contain information on children’s enrollment and progress in formal schooling. We use the school registers 2009-2012 as by these years the cohorts under study are targeted to be enrolled in secondary school. Depending on the school grade, our sample consists of 2,530 (5<sup>th</sup> grade) and 2,695 (6<sup>th</sup> grade) immigrant children born between July 1998 and June 2001. We furthermore draw upon one additional dataset, the German Micro Census. Doing so allows us to address the main caveat of the administrative data, the scarcity of background characteristics and thus the lack of information needed to construct eligibility status regarding birthright citizenship and to stratify with respect to background characteristics.

---

<sup>5</sup>The reform changed also other features of the naturalization law. For details, please refer to Section 2.1.

Our results suggest a positive impact of birthright citizenship on immigrant children’s participation across all educational levels. First, introducing birthright citizenship increases non-mandatory preschool attendance by 3.2 percentage points (ppt), which corresponds to a 3.5 % increase from the baseline level of 92.5 %. Second, it leads to an anticipation of primary school start by 5.0 ppt, which corresponds to a 43% from the baseline level of 11.6 %; i.e. immigrant children start primary school at the age of five and not six as scheduled. Third, the introduction of birthright citizenship increases immigrant children’s attendance to the academic track of secondary school by initially 5.1 ppt, which corresponds to a 23.2 % increase from the baseline level of 22.0 %. This effect, however, is only marginally significant and fades out over time: when analyzing the same outcome one year later and thus, when children are supposed to be in 6<sup>th</sup> grade, the effect goes down to 1.6 ppt and renders significance at any conventional level. Our results are robust to a variety of sensitivity checks, such as narrowing the window around the cutoff date, dropping the months around the cutoff date and allowing for flexible time trends. In addition, a placebo experiment using the sample of native children does not reveal any impact. Further analyses drawing upon official evaluations of children’s skills, such as pediatricians’ assessments of children’s school readiness, grade retentions and teachers’ recommendations regarding transition to secondary school – do not point to an improvement of children’s skills as the underlying mechanism. Increased parental integration efforts and aspirations are thus the more likely mechanism at work, in particular in light of the discretion parents have regarding schooling decisions in the state under study.

Finding positive effects of birthright citizenship on immigrant children’s educational outcomes immediately raises the question of "Why?". What is the underlying mechanism for the effects of birthright citizenship on the integration of immigrant children? First, citizenship is a basis for political and professional equality and thus for successful integration in the host country in the long-run.<sup>6</sup> It is thus likely to enhance children’s future labor market opportunities and increase their family’s likelihood of staying in the host country (Sajons, 2010). As a result, immigrant parents may decide to invest in their children’s host-country-specific human capital by, for instance, increasing their use of the local language, developing a network of native friends, or adopting cultural habits (Avitable et al., 2013; Sajons, 2012). Sending their children to preschool, enrolling them earlier in primary school or in the academic track in secondary school are further alternatives to invest in children’s human capital. In addition, children themselves may put forth more effort in school and form closer ties to their native peers once they are aware of the benefits of citizenship. Second, citizenship may reduce dis-

---

<sup>6</sup>In contrast to the US, welfare programs do not hinge upon citizenship, but on permanent residence. This is particularly true for Hartz IV, the largest benefit program available nowadays in Germany.

crimination by peers or local decision makers (e.g., teachers or school principals). We discuss these channels in detail in Section 2.3.

The remainder of the paper is structured as follows. The next section describes the institutional background and sketches a conceptual framework of the underlying mechanisms. Section 3 introduces the empirical strategy. Section 4 describes the data used for our analysis. Section 5 presents our main results and provides a series of sensitivity checks. Section 6 concludes.

## 2 Background

### 2.1 Reform of the Nationality Law in 1999

After a long and heated debate over how to deal with the rising number of immigrants, in May 1999 the German parliament undertook a major revision of the 1913 "German Citizenship and Nationality Law". On July 15, 1999, the German parliament ratified the new version of the law.<sup>7</sup> The reform made three major changes to the law: (1) it introduced birthright citizenship; (2) it changed the eligibility criteria for naturalization; and (3) it forbade dual citizenship (beyond the age of 23 years).

This paper focuses on the first change. Until 1999, citizenship was granted according to *ius sanguinis*, that is, children were granted German citizenship only if at least one parent was a German citizen. As of January 1, 2000, the prevailing regime changed to *ius soli*, which grants each child born on German territory a conditional right to German citizenship at birth, the condition being that at least one parent has had a minimum duration of legal residence in Germany of eight years. If this condition is fulfilled, German citizenship is automatically recorded in the register of birth with no need for the parents to apply for it. Given the right of dual citizenship up to the age of 23 years, there were no known cases of refusal of German citizenship at birth.

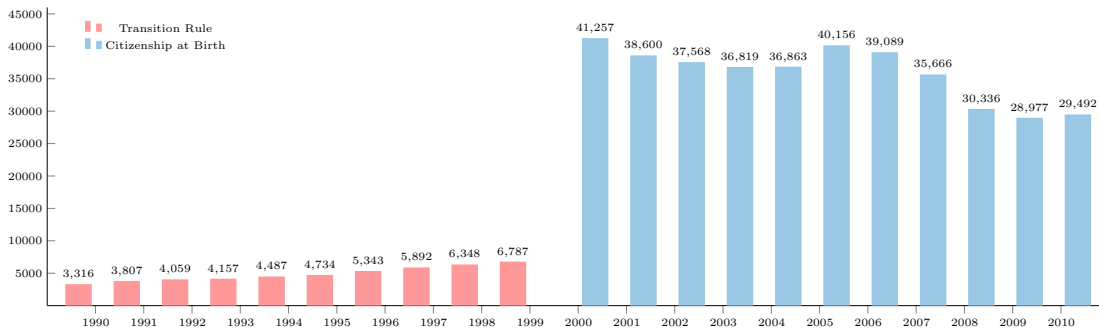
Parents of children born between 1991 and 1999 could take advantage of a transition rule: conditional on having legally resided for eight or more years in Germany, they could retrospectively apply for their children's citizenship within a transition period of one year (January 1 – December 31, 2000). However, only a small fraction of eligible families made use of this transition rule (see Figure 1): the number of eligible children possessing citizenship rises constantly over the cohorts born in the 1990s; the number of eligible children born in 1999 and making use of the transition rule, however, is only about a sixth of the number of eligible children born in 2000 who were granted citizenship automatically. Likely explanations for this low number of families making

---

<sup>7</sup>The legal text can be found in StAG 4 Abs.3 Bundesministerium der Justiz und für Verbraucherschutz (1999).

use of the transition rule are as follows: first, families were not directly informed about the transition rule; second, the public discussion focused more on the other two aspects of the reform than the introduction of birthright citizenship and thus even less on the transition rule. As a result, it is likely that the children benefitting from the transition rule are elderly siblings of children actually benefitting from the *ius soli*. In this case parents were indirectly informed about the possibility to file a naturalization application for the remaining siblings.

Figure 1: Citizenship Granted on the Basis of *Ius Soli*



Source: Beauftragte der Bundesregierung für Migration, Flüchtlinge und Integration (2012)

The reform also changed the general eligibility criteria for citizenship.<sup>8</sup> On the one hand, it relaxed the length of residence requirement from 15 years down to eight. On the other hand, it tightened certain requirements having to do with loyalty to the German constitution, financial independence (i.e., applicants may not be receiving social security or unemployment benefits), criminal record, and German-language proficiency, even going so far as to demand the renunciation of former citizenship. Thus, while immigrants now have to wait fewer years to apply for German citizenship, the required effort and costs involved in doing so are substantially higher.

In regard to obtaining/applying for German citizenship at birth, the reform of the German naturalization law thus implied the following change: children born after the cutoff date are automatically granted citizenship at birth with no action necessary on the part of their parents; children born before the cutoff date can be granted citizenship only in the event their parents apply for German citizenship (with the exception of the one-year transition period during which parents could apply for their children’s citizenship independently of their own). In other words, the reform of the German naturalization law dramatically reduced the costs of endowing children with German citizenship: for children born after the cutoff date, there is no need to file an application

<sup>8</sup>Notice that these criteria apply to all immigrants independently of the birthdate of their children, and thus for parents whose children are born before and after the cutoff date.

and thus citizenship comes at no cost; for children born before the cutoff date, application for citizenship carries administrative costs and requires renunciation of parents' original citizenship.

## 2.2 The Education System in Germany

The German education system is comprised of three parts: (1) first years between birth and primary school, which are divided into early care available for children age zero to two and preschool available for children age three to five; (2) primary school, which starts usually at age six and continues for four years; and (3) secondary schooling, which is targeted to start at age 10 and continues for between five (mandatory) and nine years.

Preschool attendance is non-mandatory in Germany. Nevertheless, since 1996, every child turning three years old is legally entitled to a place in preschool. As a result, in the late 1990s, the supply of preschool slots rose dramatically, essentially to meet demand in the early 2000s. Slots are heavily subsidized and in the case of severe financial constraints, fees can even be reimbursed by the local youth welfare service (Bundesministerium für Familie, Senioren, Frauen und Jugend, 2013). In 2012, 96 % of all three- to five-year old native children attended preschool (Federal Statistical Office, 2012). Among three- to five-year old immigrant children this share amounted to 87 % (Bildungsberichterstattung, 2012). Supply of slots in early care only started to increase in 2005 when the expansion of early care was legally mandated. The cohorts under study were unaffected by this expansion. It is thus safe to say that they were unlikely to participate in the formal education system before age three.

Between birth and primary school, children undergo several mandatory medical screenings. These medical screenings are intended to document children's health, diagnose medical anomalies, and provide necessary treatment as early as possible. An important medical screening is the school entrance examination, which is offered by the local health service and takes place in the year prior to entering primary school (thus when children are around six years old). In addition to documenting a child's health, a determination is made as to whether the child is "ready" to follow the school curriculum. The school readiness diagnosis is an important factor in school enrollment: a negative assessment can lead to school entrance being deferred by one year. Yet, the ultimate decision when to enroll their children lies in the hand of parents.

After passing all four grades of primary school, students are referred to secondary school (around age 10). Secondary school is divided into the following three tracks:<sup>9</sup>

---

<sup>9</sup>There are also a number of alternative school types, such as the Waldorfschule and the comprehensive school (Gesamtschule). Overall, around 10 % of all children of secondary school age attend alternative types



the lowest track – the so-called Hauptschule; the intermediate track – the so-called Realschule; and the highest track – the so-called Gymnasium. Hauptschule continues up to Grade 9 or 10, gives students a general education, and prepares them for an apprenticeship; Realschule goes up to Grade 10 and can either lead to an apprenticeship or to a higher-level vocational school; Gymnasium, which is considered to be the academic track, goes up to Grade 12 or 13 and prepares students for university.

Primary school teachers make tracking recommendations. Recommendations are meant to be based on a child’s personality and performance, as revealed during primary school, and not on a child’s socioeconomic or demographic background. In five states, including Schleswig-Holstein, the ultimate track choice is at the discretion of the parents. In all other states, teacher recommendations are binding and children either have to pass a special exam or undergo a probationary period in the event they are denied attendance at Gymnasium (but want to go). In 2009, 24.1 % of all 15-year-old immigrant children attended Hauptschule, in comparison to 13.3 % of their native peers. In contrast, 25.9 % of all 15-year-old immigrant students attended Gymnasium, whereas 37.1 % of their native peers did so (Bildungsberichterstattung, 2012). In other words, the unconditional immigrant-native gap amounts to 11.2 ppt. This difference in attendance rates could be the result of worse performance during elementary school, of discrimination by teachers or of lower educational aspirations (either of the immigrant child or the parents). In fact, Lüdemann and Schwerdt (2013) detect a immigrant-native gap of 6.7 ppt when accounting for children’s performance during primary school.

## 2.3 Conceptual Framework

Why should citizenship at birth have any effect on children’s educational participation and success? In what follows we describe a simple conceptual framework of children’s human capital production that highlights the main mechanisms underlying any effect of birthright citizenship on children’s educational outcomes.<sup>10</sup> Figure 2 illustrates this framework.<sup>11</sup>

As discussed above, there are three levels in the German educational system, denoted by  $T = (I; II; III)$  and represented by the horizontal axis in Figure 2: preschool (age 3-5, level I), primary school (age 6-9, level II) and secondary school (from age 10

---

of schools.

<sup>10</sup>For an overview of the human capital production theory, see Cunha et al. (2006)

<sup>11</sup>For simplicity, we ignore environmental influences, such as peers or neighbors, on children’s development. It is certainly plausible that the environment also changes for children possessing citizenship, e.g., peers may bully citizen children less. However, the extent to which peers are aware of immigrant children’s citizenship status is not clear.

onward, level III). The transition from one educational level to the next depends on teachers' recommendations which in turn depend on children fulfilling a certain skill levels  $\overline{skill}$  and parents' decisions which are part of parents' overall investment into their children's development. Thus, participation at a certain educational level, which we refer to as school investment  $I_T^S$ , can be expressed as:  $I_T^S = I^S(I_{T-1}^P; skill_t > \overline{skill}_t)$ , where  $t$  denotes the moment of transition to educational level  $T$  (where  $t = 1, 2, 3$ ). Parental investment  $I_T^P$  responds to a child's skill level – parents may support (promote) a troubled (talented) child – and to feedback from the school:  $I_T^P = I^P(I_T^S; skill_t)$ . A child's skills,  $skills_t$ , are the outcome of parental investment  $I_{T-1}^P$  and school investment  $I_{T-1}^S$ . In addition, earlier acquired skills beget later skills (the concept of self-productivity in the human capital production theory). Hence, a child's skills can be expressed as:  $skills_t = skills(I_{T-1}^P; I_{T-1}^S; skill_{t-1})$ .

Where does birthright citizenship come into play? As shown by Avitable et al. (2013) and Sajons (2012), birthright citizenship is likely to stimulate parents' initial investment in their children  $I_0^P$ : specifically, the introduction of birthright citizenship increases parents' integration efforts, including increased interaction with the local community and more use of the German language. But, *why* should parents be more willing to invest in children who have been granted citizenship at birth? Being granted citizenship at birth is a positive shock to children's initial endowment. Citizenship improves a child's legal position by allowing for political participation and thus improving a child's later standing in society. Citizenship also improves professional opportunities, and thus increases employment and wages (Chiswick, 1978; Brantsberg et al., 2002; Fougère and Safi, 2009; Steinhardt, 2012; Gathmann and Keller, 2014). In particular, it opens the door to any job requiring civil servant status.<sup>12</sup> Civil servant posts are typically prestigious, for example, teachers or judges, but also include jobs that might be popular among children, such as firefighters or police officers. There are 1.4 mio. civil servant posts in Germany, which corresponds to 4 % of all jobs in Germany and likely to a much higher share of jobs aspired to by children and their parents.<sup>13</sup> In addition, citizenship may act as a signal of long-term commitment and thus reduce existing barriers to career mobility. In light of these future professional opportunities for their children, immigrant families are likely to prolong their stay in the host country. In fact, Sajons (2010) finds a reduction in return migration in the aftermath

---

<sup>12</sup>Access to civil servant posts is not restricted to EU citizens. As a result, gains are likely to be smaller for children of EU citizens. Ideally, we would like to distinguish between EU and non-EU citizens in our analysis. However, such a subgroup analysis would lack precision as only a small fraction (around 5 %) of immigrant children have parents from an EU 12 member state – states belonging to the EU in 2000. In light of the small sample size children from EU 12 member states do not qualify as a potential control group, even aside from any concerns about the comparability of families from EU and non-EU countries.

<sup>13</sup>This is not a German specialty, but in the United States, for instance, federal civil servant posts make up to 7 % of all jobs.

of the naturalization law reform in Germany in 2000 (especially among low-educated families). More evidence that birthright citizenship leads to a prolonged stay in the host country is the reduction in remittances found by Piracha and Zhu (2012).

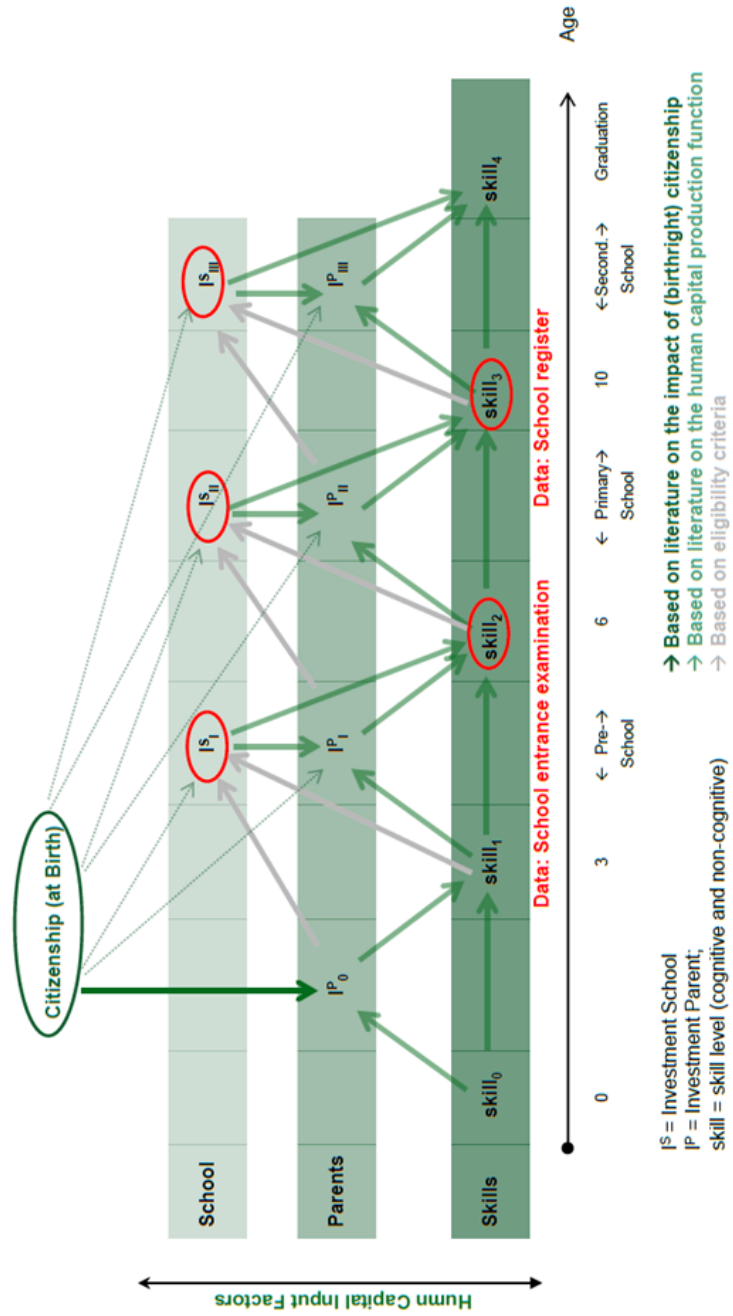
*Why* should birthright citizenship have a long-lasting impact on immigrant children’s educational participation and success? There are at least three reasons: First, birthright citizenship may not only stimulate parents’ initial investments into their children’s host-country-specific skill development, but also later investments. In particular, parents’ efforts to send their children to the academic track of secondary school may be substantial as this gives them ultimately access to prestigious civil servant jobs. In addition, children may be more likely to exert effort in school or establish closer ties with their native peers once they are aware of better opportunities in the host country.

Second, as explained above, children’s human capital production is a dynamic process: initial investments influence the development of children’s skills and initially acquired skills beget later skills. For example, the increased integration efforts of parents may encompass the decision to enroll their children in preschool. Preschool, in turn, may boost children’s skill development, particularly that of immigrant children (Becker, 2006; Becker and Tremel, 2011; Dustmann et al., 2013). These additionally acquired skills may then influence the recommendation for primary and secondary school. As a result, birthright citizenship may boost immigrant children’s educational success even in the long-run. Yet, it may also be the case that initial investments are not sufficient to alter recommendations. If, as suggested in Figure 2, there is a feedback mechanism between children’s skills and parental investments, the lack of any positive impact on recommendations may discourage parental integration efforts.

Finally, there might be a direct effect of children’s citizenship on teachers’ recommendation for school progression by reducing discrimination against immigrant children. Yet, to the best of our knowledge there is no study investigating discrimination against immigrant children on grounds of citizenship, but only on grounds of ethnicity (Lüdemann and Schwerdt, 2013; Sprietsma, 2013).

In sum, it is both possible: the impact of birthright citizenship on children’s participation and success in the host country’s educational system may increase or fade out over time. Our data, which are described in Section 4, allow us to investigate this question empirically and estimate the direct impact of birthright citizenship on children’s participation across all three educational levels and children’s skills evaluated by pediatricians at primary school entrance and school teachers during primary and secondary school.

Figure 2: Birthright Citizenship and Human Capital Production



Source: Authors own elaboration.

### 3 Empirical Approach

Analyzing the impact of citizenship on children’s education is challenging as parents applying for citizenship may differ from parents not applying for citizenship in their willingness to integrate and stay in the host country. To overcome this selection bias, we take advantage of the 1999 reform of the German naturalization law. As described in Section 2.1, immigrant children born after January 1, 2000 are granted German citizenship at birth conditional on at least one of their parents having lived in Germany for a minimum of eight years. This cutoff for citizenship eligibility at birth is the core of our identification strategy.

More precisely, we compare immigrant children born shortly before the cutoff with immigrant children born shortly after the cutoff. To avoid differences across school cohorts, we restrict our sample to one school cohort and thus to children born six months before and after the cutoff date (second semester of 1999 and first semester of 2000, respectively). Ideally, we would like to further restrict our sample and consider only eligible children, i.e., children whose parents have lived in Germany for a minimum of eight years. Since our main data sources (described in more detail in Section 4) do not contain information on residence duration in Germany, we do not impose this sample restriction in our baseline analysis. As a result, our baseline sample may include a group of children who are unaffected by the reform and the resulting estimates are likely to provide a lower bound of the reform’s causal impact. We provide evidence in support of the baseline estimate being indeed a lower bound using the German Microcensus, which contains information on parents’ duration of residence.<sup>14</sup>

A simple comparison between children born before and after the cutoff date will lead to a biased estimate of the reform’s causal impact if there are systematic differences between children born in different seasons of the year. Being of different ages is an obvious difference, but family background could vary, too (see, e.g., Buckles and Hungerman (2013) for a recent analysis of the different socio-economic characteristics of mothers giving birth in different months of the year). To isolate the causal impact of the policy reform from such seasonal effects, we not only compare children born shortly before and shortly after the cutoff date, but use children from earlier and later school cohorts as control groups (specifically, from the second semester of 1998 and 2000, and the first semester of 1999 and 2001). In other words, we employ a difference-in-difference design (DiD) that compares outcomes of children born shortly before and shortly after the cutoff date in the year of policy change and adjacent years in which

---

<sup>14</sup>It is unclear whether a comparison of children whose parents had and had not resided for more than eight years in Germany would be ideal because, depending on duration of residence, families might belong to different immigration waves and thus to different ethnic groups.

no policy change took place. A similar strategy is used by Lalive and Zweimüller (2009), Dustmann and Schönberg (2012), Danzer and Lavy (2013), and Schönberg and Ludsteck (2015). The equation to be estimated is:

$$\begin{aligned}
Y_i^{s,t} &= \alpha + \beta Cohort99/00_i + \gamma Birthmonth01/06_i + \\
&+ \delta Cohort99/00_i * Birthmonth01/06_i + \theta_m D_{i,m} + \epsilon_{i,m,t} \quad (1)
\end{aligned}$$

where  $Y_i^{s,t}$  represents the educational outcome in dimension  $s$  of child  $i$  who is  $t$  years old.  $Cohort99/00_i$  is a binary variable indicating whether child  $i$  belongs to the school cohort 1999/2000 and thus whether the child is born in the year around the reform under study.  $Birthmonth01/06_i$  is a binary variable indicating whether child  $i$  was born in the months after the cutoff date (i.e., whether the child was born between January and June). The interaction term  $Cohort99/00_i * Birthmonth01/06_i$  is thus 1 for all children born between January and June 2000. The effect of interest is captured by the coefficient  $\delta$  preceding the interaction term and captures the deviation from the general differences between children born in the first and the second semester of the year; i.e. the coefficient  $\delta$  identifies the causal effect of a switch from a *ius sanguini* to a *ius soli* regime on immigrant children’s educational outcomes.

We additionally control for a set of birth month dummies  $D_{i,m}$  which shall capture the following systematic differences between children born in different months of the year:<sup>15</sup> first, children belonging to one school cohort differ by up to 12 months in age. For the age group under study just a one-month difference in age can make quite a difference in terms of educational development. Children born in earlier months are thus likely to be more mature at any point in time. As a result, unconditional DiD estimates are likely to represent a lower bound of the potentially positive effect of introducing birthright citizenship. Second, as mentioned previously there is a well-documented seasonal pattern in children’s educational development: children born in the spring generally come from more advantaged socioeconomic backgrounds than children born in winter (Buckles and Hungerman, 2013). In this case, unconditional DiD estimates would likely be upward biased.

What are possible threats to our identification strategy? First of all, parents may

---

<sup>15</sup>The assignment variable  $Birthmonth01/06_i$  correlates perfectly with the birth months January to June. We therefore omit not just one, but two birth month dummies. Specifically, we omit January and December as they are immediately around the cutoff date. Note that we can control for the set of birth months dummies because we rely on a comparison between children born in the year of policy change and children born in adjacent years in which there was no policy change. A simple regression discontinuity design would not allow us to do so, as the assignment variable would be a perfect linear combination of the included set of birth month dummies.

adjust their family planning behavior in light of the reform of the naturalization law. Specifically, parents may delay conception such that their child was born under the new policy regime. Following the argument of the quantity-quality tradeoff put forward by Becker and Tomes (1976) parents may not only delay, but also adjust their desired number of children downward: the introduction of birthright citizenship leads to an increase of the returns to country-specific skills and thus to an increase in the "quality" of the children. Indeed, Avitable et al. (2014) show a reduction in fertility after the introduction of birthright citizenship in Germany, but only from 2001 onward. Hence, the children included in our sample, who were conceived before September 2000, are unlikely to be affected by this concern. In fact, a histogram of the number of immigrant children born around the cut-off date does not reveal any discontinuity (see Figure A.1 in the Appendix). Nevertheless, to address the concern of adjusted family planning behavior, we perform a robustness check where we restrict our sample to children who were conceived prior to ratification of the new naturalization law (hence, children conceived before July 1999 and thus born by April 2000); i.e. we restrict the window to 4 month around the cutoff date.

Second, mothers scheduled to give birth closely before the cutoff date may try to postpone the birth date to fall under the *ius soli* regime. Even if postponing is rather difficult, we perform a robustness check where we exclude children born in the month right before and after the cutoff date (December and January).

A third concern is that the introduction of birthright citizenship made return migration less attractive (Sajons, 2010). As a result, the pool of families remaining in the country may change after the introduction of birthright citizenship. Thus, if anything, the resulting selection bias leads to a lower bound of the effect as the reform might have induced less integrated families and thus more disadvantaged children to stay in Germany. Nevertheless, to test the robustness of our results to potential sample selection bias, we include a series of family background characteristics, such as single parenthood, parental education and parents' country of origin. Notice that this is only possible in two out of three datasets used in this study (see Section 4 for more details).

Forth, we address the concern of a general time trend and control for cohort dummies and their interactions with birth semester in a flexible manner. We additionally address the concern of general fluctuations in children's educational outcome over time, e.g. due to variation in the business cycle, and estimate a placebo regression using the sample of native children. Note, however, that there was no major recession in the years our cohorts were born.

For the purpose of robustness, we finally provide estimates based on a regression-discontinuity design assuming a specific functional form of the seasonal and age-of-school-entrance effects.

## 4 Data

Answering the question under study empirically requires very comprehensive data. First, we need data that provide information on children’s educational outcomes for more than 10 years after children’s birth. Second, our analysis relies on a very small subgroup of the German population: a few cohorts of immigrant children. We therefore rely on two large and unique data sources from one federal state in Germany (Schleswig-Holstein): school entrance examinations and school registers.<sup>16</sup> Both data sources allow us to proxy immigrant status, but lack the information necessary to construct eligibility status. We therefore draw upon the German Micro Census as an additional data source. In what follows we describe all three datasets. Descriptive statistics can be found in the Appendix, in Table A.1 and A.2.

### School Entrance Examinations

School entrance examinations (SEEs) serve as the basis for our analysis of children’s educational outcomes up to primary school (please refer to figure 2 for the respective outcome variables provided by the SEEs, circled in red). As described in Section 2, SEEs are mandatory for all children who turn six between July of the previous year and June of the year of school entrance. Children born between July 1998 and June 2001 are thus included in the SEEs 2005, 2006, and 2007.<sup>17</sup> Our baseline sample of second-generation immigrant children, that is, children whose parents are both migrants (which means they are not born in Germany), consists of 6,740 observations.

The main focus of the SEE lies on children’s school readiness which is a summary measure of the entire set of diagnoses that the pediatrician in charge of the SEE performs. Diagnoses relate to children’s intellectual, socio-emotional and motor skills. Recommendations are either ”a child is ready for school”, ”the child needs special education (either during school hours by the teacher or in addition to official school hours by a specialist)” or ”the child is better off starting school one year later”. We generate a binary outcome measure of school readiness that is 1 when the child is ready for school and 0 otherwise.

An additional questionnaire filled out by the accompanying caregiver contains a series of questions on the child’s background. We have information on preschool enrollment as well as on the child’s age and gender, the household composition, and parents’ education. The questionnaire also asks about the parents’ country of origin,

---

<sup>16</sup>Unfortunately, administrative data on children’s educational outcomes are not available for the whole German territory, but for selected states only.

<sup>17</sup>Parents can request that their child is examined a year earlier than the official SEE. We exclude these children since they are a non-random subsample of younger age cohorts. Moreover, each child is included only once in the SEE as children who are assessed as not being ready for school in one year undertake a special examination one year later.



but does not query their length of residence in Germany and their citizenship. As a result, we cannot restrict our sample to eligible children – children whose parents have lived in Germany for a minimum of eight years and did not possess of German citizenship. In fact, all we know is whether the family was living in Germany at the time of the survey, but not whether the child was born in Germany. Our sample thus also includes children who are unaffected by the reform – we refer to them as ineligible children – and the resulting estimates are likely to be attenuated.

In the SEE sample, 92.5 % of the immigrant children born prior to the reform were enrolled in preschool and 84.9 % were assessed to be ready for primary school (see Table A.1, Panel A). This stands in contrast to the preschool enrollment (95.4 %) and school readiness (90.9 %) among native children of the respective cohorts – children whose parents are both born in Germany. A raw comparison of the educational outcomes between foreign children born shortly before and after the reform reveals the following differences. Children born shortly after the cutoff date are more likely to be enrolled in preschool than children born shortly before the cutoff date (94.2 % vs. 91.7 %). The difference in terms of school readiness (77.2 % vs. 86.3%) reflects the age difference between children born shortly before the reform and children born shortly after the reform (5.7 months on average). Importantly, aside from the age difference, there are no significant differences in terms of children’s demographic or socio-economic background (see Table A.1, Panel B).

### **School Registers**

School registers (SRs) serve as the basis for our analyses of children’s educational outcomes related to primary and secondary school (please refer to figure 2 for the respective outcome variables provided by the SEEs, circled in red). All primary and secondary schools are legally obligated to provide their records to the federal ministry of education by the beginning of each school year and thus by September of each year. The birth cohorts under study are supposed to start secondary school by 2009, 2010 and 2011, respectively. We therefore draw on SRs from these three years. Since by then only 73.5% of the children in our sample are actually attending secondary school, we additionally draw on the SR in the subsequent year (2010, 2011, and 2012) and thus on the years when the birth cohorts under study are supposed to be in 6<sup>th</sup> grade. Doing so allows does not only ensure that the majority of the children have gone through primary school and are actually attending secondary school (98.5 %), but also allows us to address the persistence of any potential findings.

SRs contain some basic demographic characteristics such as gender, birthdate and several proxies for children’s migrant status. Specifically, they contain information on children’s ethnicity, country of birth and main language spoken at home.<sup>18</sup> We restrict

---

<sup>18</sup>Information on the main language is only surveyed from 2008 onwards. Schools requested this informa-

our sample to children who are born in Germany, but are not ethnic German and use a different language than German as their main language.<sup>19</sup> The resulting sample based on the years 2009-2011 contains 2,530 observations, the resulting sample based on the years 2009-2011 contains 2,695 observations. Notice that the samples based on the SRs are substantially smaller than the sample based on the SEE data. This difference is due to the restriction to children being born in Germany – an information not available in the SEE data – and the restriction related to the main language.

As in the case of the SEEs, the SRs do not contain any information on parents' duration of stay in Germany at the time of child birth and citizenship. Thus, also the sample based on the SRs is likely to contain ineligible immigrant children and thus is likely to produce attenuated results. To which degree the sample of immigrant children not speaking German as their main language may be a selective sample and thus may not lead to representative results is a priori unclear as there may be various reasons for not using German as the main language (e.g. language proficiency, cultural roots). We discuss this issue further in Section 5.3.

Information on children's enrollment and progress in school allows us to construct the following educational outcomes (see Panel A, Table A.1). First, SRs contain the year when a child was first enrolled in primary school. Based on this information and on children's birthdate, we can construct the variable school starting age. School starting age may not only vary due to variation in birth dates, but also due to variation in recommendations regarding school readiness and parents' discretion in deciding when their child starts school. Among children born prior to the reform, the average school starting age amounts to 78.5 months or 6.5 years old, but this varies between 5.3 and 7.8 years. In fact, 11.6 % of the children in our sample start school one year before they are supposed to. Unfortunately, we cannot provide reliable statistics for the group of native children as the remaining sample – the group of children speaking German as a main language – is likely to contain also migrant children.

Second, SRs contain information on the type of school a child is enrolled in. As a result, we can construct a binary variable indicating which secondary school track a child is enrolled in. Among children born prior to the reform, 22.0 % are enrolled in the academic track when supposed to be in 5<sup>th</sup> grade. One year later, when children are supposed to be in 6<sup>th</sup> grade, this share is slightly lower (17.4 %). This may be

---

tion during registration or, if the child was already enrolled in the school (which was the case for children not yet in secondary school), surveyed this information retrospectively. As a result, there are more missing values regarding this information in the years 2009, 2010 and 2011, than in the years 2010, 2011 and 2012 (when 98.5 % actually attended secondary school). As such, the sample of 6<sup>th</sup> graders is slightly bigger than the sample of 5<sup>th</sup> graders.

<sup>19</sup>The assumption is that there is no native child speaking a different language than German as her main language.

due to two reasons: among children who are running late in the school system – either because they started primary school later (4.4 %) or because they had to repeat a grade (21.9 %) – there are disproportionately more children enrolled in the two lower tracks of secondary school or because there is a substantial share of immigrant children who leave the academic track during 5<sup>th</sup> grade.

Besides information on enrollment, school registers contain certain information on children’s skills, or better said on teachers’ evaluations of children’s skills. Based on the year when first attended primary school and the current grade of a child, we can construct a binary variable indicating retention during primary school; i.e. whether a child had to repeat a grade. Among children born prior to the reform, 21.9 % had to repeat a grade during primary school. Once being in fifth grade, school registers also contain information on teachers’ recommendations regarding the secondary school track. Recommendations stand in stark contrast to actual attendance rates: at baseline 22.0 % of all immigrant children attend the academic track in secondary school, while only 13.1 % were recommended to do so. Inspecting teachers’ recommendations and parents’ decisions jointly confirms that 11.7 % of the parents dismiss the teacher’s recommendation.

Differences in the educational outcomes of children born shortly before the reform and shortly after the reform are strongly influenced by the age difference between the two groups (almost half a year): 8.6 % of the children born before the cutoff date start primary school one year earlier than scheduled, while only 1.9 % of the children born after the cutoff date do so. This is not surprising, as children born in the first half of the school year are born much closer to the cutoff date regarding school start (June 30). Among children born before the cutoff, 23.6 % have to repeat a grade during primary school, among children born after the cutoff this share amounts to 30.5 %. This worse performance during primary school is also reflected in the recommendation regarding secondary school track choice: while 15.4 % of all children born before the cutoff receive a recommendation for the academic track of secondary school, only 10.5 % of all children born after the cutoff do so. Comparing the share of children actually enrolled reveals a similar difference (19.4 % vs. 13.2 %). Nevertheless, these disadvantages seem to fade over time: at the beginning of 6<sup>th</sup> the difference in attendance to the academic track of secondary school is reduced by half (19.3 % vs. 15.5 %) and not significant anymore. These differences stress once more the need to employ a DiD framework and to control flexibly for seasonal and age-at-school-entrance effects.

Unfortunately, the SRs do not provide us with information on children’s background characteristics. As such, we can not test whether children born shortly before and after the cutoff date balance in their background characteristics. Notice, however, that the SR data and the SEE data refer to the same children – children born between 07/1998

and 06/2001 and residing in Schleswig- Holstein. As such the sample based on the SR data is to a large extent a subsample of the sample based on the SEE data.

### **German Micro Census**

We additionally draw upon the German Micro Census (GMC), which is the largest European household survey and interviews 1 % of all German households. In particular, we use the survey years 2009-2012 which correspond to the time when children born between 07/1998-06/2001 are targeted to be in 5<sup>th</sup> grade.<sup>20</sup> In contrast to the administrative data used in this study, the GMC contains a series of background characteristics and thus is useful for the purposes explained in turn.

First of all, the GMC contains information on children’s citizenship. This information allows us to get an estimate of the actual share of children obtaining citizenship due to the reform or, technically speaking, on the actual share of compliers (see Section 5.1). In addition, the GMC allows us to construct children’s eligibility status. Specifically, it contains information on the year of parents’ arrival to Germany and of reception of German citizenship. We use this information, to discuss to which extent eligible children may be a selected group of all immigrant children. For this purpose, we first restrict the sample to children who are born in Germany, but their parents are immigrants: the resulting sample, to which we refer as the sample containing *all* immigrant children, contains 2,532 observations. We then restrict the sample to children with at least one parent with a minimum duration of residence of eight years, but without German citizenship at the time of birth: the resulting sample, to which we refer as the sample containing *eligible* immigrant children, contains 1,011 observations. Notice that the samples based on the GMC rely on children from the whole federal territory – with the exception of Berlin and Brandenburg, as in these two states tracking into secondary school takes place in Grade 7 instead of Grade 5. Unfortunately, the sample size prevents us from restricting the sample to children residing in Schleswig-Holstein only.

To which extent are immigrant children eligible to birthright citizenship a special group among all immigrant children? Overall there are no striking differences between the two samples (see Table A.2 in the Appendix), with the exception of the composition of countries of origin. This was to be expected as the main difference between eligible and non-eligible immigrant children is parents’ arrival year to Germany. Among the eligible children there are slightly less Turkish descendants – likely to be the result of an earlier naturalization wave among Turkish families –, much less descendants from families coming from Eastern Europe – likely to be the results of many migrants coming

---

<sup>20</sup>Surveys are done all year long. To capture the whole academic year, we draw upon surveys done between the forth quarter in 2009 and the second quarter in 2012. Unfortunately, the survey year 2013 is not yet available, and thus we cannot analyze children’s educational outcomes when targeted to be in 6<sup>th</sup> grade.

from Eastern Europe being actually ethnic Germans –, and slightly more descendants from families coming from the Balkan or the EU. Yet, there is a sizeable amount of eligible children for which the information on mothers’ country of origin is missing.

## 5 Results

We now present empirical evidence on how birthright citizenship changes the actual share of citizens among second-generation immigrant children (Section 5.1) and how it influences second-generation children’s educational outcomes (Section 5.2). Section 5.3 discusses the robustness of our results.

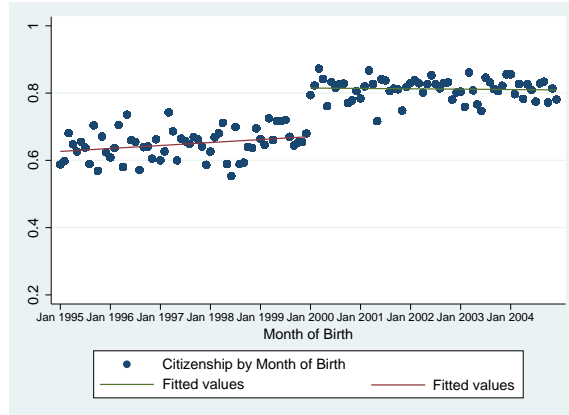
### 5.1 The Impact on Immigrant Children’s Citizenship

How many children did actually benefit from the introduction of birthright citizenship? We address this question using data from the GMC. Yet, recall that the GMC is a survey and thus misreporting is likely to be an issue. Figure 3 displays the increase in citizenship among all immigrant children, eligible immigrant children, and, for the purpose of robustness, non-eligible immigrant children.

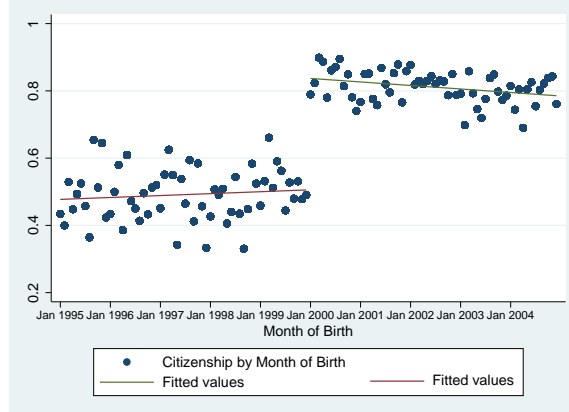
Let us first assess the share of compliers (i.e., the share of children acquiring citizenship due to the reform of the naturalization law) in our baseline sample and thus in the sample comprising all immigrant children regardless of their parents’ length of residence in Germany (see Figure 3, Panel A). Among immigrant children born prior to the cutoff date, 68.3 % were granted German citizenship at birth. Note that, with the exception of children who benefited from the transition rule, this implies that at least one of these children’s parents had applied for citizenship and thus gave up their original citizenship. Among children born after the cutoff date, the share of children who are German citizens jumps up to 82 %. The share of compliers in our baseline sample thus corresponds to 13.6 %.

Why does the share of children with German citizenship born after the reform of the naturalization does not equal 100%? First, not all children in our baseline sample were eligible for birthright citizenship (not all parents exhibited a minimum residence of eight years at child birth). Second, the GMC does not allow us to identify the biological parents. Instead, we have to rely on information provided for the head of the household to infer children’s eligibility status. Doing so may incur some misclassification error. Third, parents may report their own nationality only when asked to report the nationality of their child despite the fact that their child has double citizenship. In addition, some parents may not be aware of their child having double citizenship.

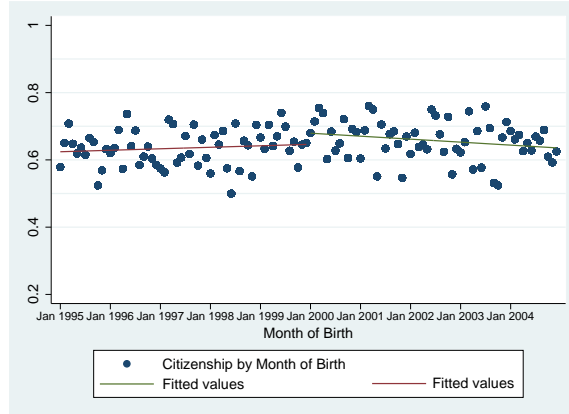
Figure 3: Citizenship by Month of Birth  
Panel A: All immigrant children



Panel B: All eligible immigrant children



Panel C: All non-eligible immigrant children



Source: German Micro Census 2009-2012

When using the sample of eligible immigrant children only, the share of German citizens among children born before the reform amounts to 46.8 % only (see Figure 3, Panel B). The reason why the share of citizens is substantially lower in the eligible

sample than in the baseline sample is that the eligible sample excludes all children whose parents were German citizens at the time of their birth and thus who were entitled to citizenship by *ius sanguini*. After the reform the share of German citizens jumps up to 80.7 %. Thus, in the eligible sample the share of compliers amounts to 33.9 %. The fact that even among children who are actually entitled to German citizenship a substantial share does not possess German citizenship underlines the problem of misreporting or misclassification in the GMC.

Panel C in Figure 3 shows the share of children with German citizenship among children whose parents had not yet resided in Germany for eight years at the time of their birth; i.e. it displays citizenship among children who are not eligible to birthright citizenship. Clearly, there is no jump in citizenship around the cutoff date specified by the reform of the German naturalization law, but a smooth upward trend in the share of children with German citizenship. Hence, we feel confident in concluding that the jump in citizenship discussed above is related to the reform of the naturalization law.

Who are the complier children, i.e., children who only receive German citizenship when being born under the *ius soli*? Is there any evidence that complier children differ in observable characteristics? Or put it differently, is there any evidence of selective use of the transition rule? A priori it is unclear in which direction selection might go: first and foremost, complier children might be from less informed families, i.e. families who are unaware of the transition rule. Second, complier children might be from families who shy away from the costs of filing the application for naturalization. Under both scenarios, complier children might stem from rather disadvantaged backgrounds. Yet, complier children might also be children from families who have less to gain from German citizenship (i.e. EU citizens) or families who are more patriotic. Descriptive statistics for the complier population calculated using the method by Abadie (2003) do not reveal any important differences between children who are eligible to birthright citizenship and children who actually comply with it (see Column 2 and 3, Table A.2 in the Appendix). As such, there is no evidence for selective use of the transition rule.

## 5.2 Birthright Citizenship and Children’s Education

Do immigrant children gain access to and progress better in the educational system due to the introduction of *ius soli*? Table 1 displays our baseline estimates. The estimates displayed in Column (1) stem from a DiD equation controlling for children’s month of birth only and thus correspond to Equation 1 discussed in Section 3. In a further specification, shown in Column (2) we additionally control for children’s gender

and in the case of the SEE data for children’s age.<sup>21</sup> The SEE allows us to further control for a set of family characteristics, such as number of siblings, single parenthood, parents’ education and country of origin (Column 3). Results are robust across the three specifications. In fact, coefficients are basically unaffected by the inclusion of the control variables. This result provides evidence in favor of our identifying assumption that the timing of birth is random and that selective migration does not bias our results. In turn, we discuss the results using our baseline specification shown in equation 1.

We observe a positive impact of the introduction of birthright citizenship on immigrant children’s participation across all educational levels. First of all, immigrant children born after the reform of the German naturalization law are 3.2 ppt more likely to attend preschool than immigrant children born before the reform. This increase is significant at the 1 % significance level. Given the attendance rate of 92.5 % at baseline, this implies an increase of 3.5 %. Second, the introduction of birthright citizenship leads to an increase in the share of children starting primary school ahead of time. Precisely, the reform led to a 5.0 ppt increase in the share of children starting primary school at age 5 instead of age 6. Given that prior to the reform only 11.6 % of all immigrant children started primary school at age 5, this amounts to an increase of 43.1 %. Third, we observe a substantial initial increase in immigrant children’s participation in the academic track of secondary school. Specifically, when analyzing school track choice among children who are targeted to be in 5<sup>th</sup> grade, we observe an increase by 5.1 ppt in the share of children enrolled in the academic track. This increase is only marginally significant (at the 15 % significance level), but in light of the baseline level of 22.0 %, it corresponds to an increase of 23.2 %. Nevertheless, when analyzing the track choice one year later – when children are supposed to be in 6<sup>th</sup> grade – this advantage is basically gone: the difference amounts to only 1.6 ppt and is not significant at any conventional level. The underlying reason explaining the fading out may be twofold: on the one hand, among the children who start secondary school one year later – either because they started primary school one year later or because they had to repeat a grade – the share of children enrolling in the academic track may be disproportionately low. On the other hand, children initially enrolled in the academic track may not be able to follow the curriculum and need to adjust the track during or after the first year in secondary school. The latter explanation is supported by the absence of any increase in teachers’ recommendation for the academic track discussed further below (see Table 1, Panel B).

---

<sup>21</sup>Examinations are done all year long. As such the age and the birthmonth are not perfectly collinear in the case of the SEE.



Table 1: Estimation Results

	(1)	(2)	(3)
<b>Panel A: Participation</b>			
<b>Preschool:</b>			
Attendance (SEE data; N=6740)	0.032** (0.013)	0.032** (0.013)	0.031** (0.013)
<b>Primary school:</b>			
Early start (SR data; N=2498)	0.050** (0.023)	0.051** (0.023)	- -
<b>Secondary school:</b>			
5 <sup>th</sup> grade: Academic track (SR data; N=2530)	0.051 (0.032)	0.052 (0.033)	- -
6 <sup>th</sup> grade: Academic track (SR data; N=2695)	0.016 (0.030)	0.016 (0.030)	- -
<b>Panel B: Evaluation</b>			
<b>Primary school:</b>			
School Readiness (SEE data; N=6740)	0.009 (0.019)	0.009 (0.019)	0.013 (0.019)
Retention (SR data; N=2482)	-0.049 (0.037)	-0.051 (0.037)	- -
<b>Secondary school:</b>			
Recommendation (SR data; N=1959)	0.001 (0.032)	0.001 (0.032)	- -
Birth months	yes	yes	yes
Child Characteristics	no	yes	yes
Family Characteristics	no	no	yes

Source: School entrance examinations (SEE) 2005-2007 and school registers (SR) 2009-2012; own calculations

Note: Estimates are based on OLS regressions using equation (1). Child characteristics contain children's gender and in the case of the SEE data children's age at examination; family characteristics contain information about number of siblings, single-parent household, parents' educational degree and parents' country of origin.

Robust standard errors in parentheses. \*p < 0.1; \*\*p < 0.05; \*\*\*p < 0.01

Do we observe an improvement in children's skills? Interestingly, we do not observe any significant increase in the share of immigrant children being assessed to be ready for primary school by pediatricians in the course of the school entrance examinations, any significant improvement in immigrant children's progress through school proxied by the share of immigrant children staying down one year, or any significant increase in the share of immigrant children being recommended to attend the academic track in secondary school. The lack of the latter is particularly surprising given the increase in initial enrollment in the academic track in secondary school, but highlights the discretion parents have regarding children's schooling decisions. In fact, analyzing teachers' recommendations and parents' decision regarding secondary school track choice jointly reveals that the introduction of birthright citizenship leads to a significant increase in the share of parents dismissing the teacher's recommendation (3.9 ppt, which corresponds to an increase by 33.3%; see Table A.3, Panel A in the Appendix). Moreover, using the GMC and stratifying the sample according to the state regulations regarding secondary school track choice reveals that the initial increase in participation in the academic track of secondary school is only present in states where teachers' recommendation regarding secondary school track choice is not binding. In states where teachers' recommendations are binding, the coefficient is basically zero (see Table A.3, Panel A in the Appendix).

It is unclear whether the absence of any effect of birthright citizenship on the evaluation of children's skills indeed reflects the absence of any improvement of children's underlying skills, or whether it reflects a continuous discrimination of pediatricians/teachers against children with immigrant background, independently of their nationality. Moreover, the available skill measures are binary measures and it might be the case that a potential improvement in children's skills occurs at a different margin than the one which is reflected by the binary evaluation measure.

To sum up, the differential results for children's participation in the educational system and pediatricians'/teachers' evaluation of children's skills indicate that parental integration efforts and educational aspirations for their children are the driving force behind the effect of birthright citizenship on immigrant children's participation in the host country's educational system. Preschool attendance is voluntary and despite the recommendations regarding primary and secondary school enrollment, the ultimate decision when and where to enroll a child lies in the hands of parents. As such the discrepancy between the results on children's educational participation and pediatrician's/teachers' evaluations of children's skills tell us that parents are more likely to overthrow official recommendations and push for their children's participation in the host country's educational system when their children enjoy birthright citizenship.

### 5.3 Sensitivity

Is the introduction of birthright citizenship solely responsible for the discovered effects on immigrant children’s educational outcomes? As discussed in Section 3, one threat to our identification strategy is that parents might have reacted to the introduction of birthright citizenship by either reducing fertility or delaying birth. An additional threat to our identification strategy are potential simultaneous changes in any determinant of children’s educational development.

To address these potential threats to identification, we pursue the following robustness checks: First, we drop the months just around the cutoff date (December and January) – the so-called donut strategy – to exclude any potential delays of births from late December to early January. Second, we restrict our sample to children conceived before ratification of the reform and thus narrow our sample to birth months closer to the cutoff date. Specifically, we restrict the sample to children born four months around the cutoff date (September to April). Third, we control flexibly for children’s birth cohort and allow for an interaction between being born between January and June and each birth cohort separately. Forth, we test the robustness of our baseline specification and rely on a regression discontinuity design assuming a linear form of the underlying running variable (birth month). Finally, we use native children – children whose parents are both German – and perform a placebo regression. Notice that the placebo regressions are only done based on the SEE data, as the SR do not allow us to define a group of native children only (the group of children speaking German as their main language is likely to contain immigrant children as well). In addition, we probe the robustness of the results regarding attendance to the academic track in secondary school using the GMC data. Results of our sensitivity checks are shown in Table 2.

Overall, results are robust across the different estimations. Specifically we observe the following. Excluding the months December and January, makes results, if anything, stronger: children born after the cutoff date are 4.1 ppt more likely to attend preschool, 7.2 ppt more likely to start primary school one year earlier than targeted, 5.6 ppt more likely to be enrolled in the academic track of secondary school when supposed to be in 5<sup>th</sup> grade, but not effect when supposed to be in 6<sup>th</sup> grade.

Table 2: Sensitivity Analysis

	Donut	4-months window	Trend	RDD	Placebo
<b>Panel A: Participation</b>					
<b>Preschool:</b>					
Attendance (SEE data)	0.041*** (0.014)	0.037** (0.015)	0.036*** (0.013)	0.034*** (0.012)	-0.007 (0.005)
<b>Primary school:</b>					
Early start (SR data)	0.072*** (0.026)	0.027 (0.025)	0.100*** (0.027)	0.045** (0.022)	- -
<b>Secondary school:</b>					
5 <sup>th</sup> grade: Academic track (SR data & GMC data)	0.056* (0.035)	0.032 (0.040)	0.092** (0.038)	0.028 (0.031)	0.012 (0.018)
6 <sup>th</sup> grade: Academic track (SR data)	0.012 (0.036)	0.000 (0.038)	0.012 (0.036)	0.006 (0.029)	- -
<b>Panel B: Evaluation</b>					
<b>Primary School:</b>					
School Readiness (SEE data)	0.020 (0.021)	0.008 (0.023)	0.001 (0.020)	0.017 (0.018)	0.007 (0.013)
Retention (SR data)	-0.022 (0.041)	-0.067 (0.046)	-0.055 (0.044)	-0.038 (0.035)	- -
<b>Secondary School:</b>					
Gymnasium recommendation (SR data)	0.031 (0.035)	0.021 (0.040)	0.042 (0.037)	0.008 (0.031)	- -

Sources: School entrance examination 2005-2007, School register 2009-2012, German Microcensus (Placebo regressions) 2009-2012; own calculations

Note: All regressions control for a set of birth month dummies. Placebo regression regarding attendance of the academic track in secondary school are done using data from the German Microcensus. Robust standard errors in parentheses; \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Restricting the sample to a four-months window gives the following results: children born under the *ius soli* regime are 3.7 ppt more likely to be enrolled in preschool and are 2.7 ppt more likely to start primary school one year earlier than targeted. The coefficient of primary school start does not only lose magnitude, but also precision. This is likely to come from the fact that this specification drops children born around the cutoff date regarding school start (June 30). Yet it is exactly these children – children born in July and August – who are the most likely to deviate from the recommended school starting age (mostly because they are closest in age to the earlier

school cohort and thus are more likely to exhibit the necessary skills to start school one year earlier). In a similar vein, we also observe a reduction in the coefficient resulting from the regression using enrollment in the academic track of secondary school as the dependent variable (if children are less likely to start school earlier, they are also less likely to attend already secondary school). Controlling flexibly for potential underlying trend leads, if anything, to stronger results: the reform of the naturalization law leads to an increase in preschool enrollment by 3.6 ppt, an increase in the probability to start primary school one year earlier than targeted by 10.0 ppt and an increase in the probability to be enrolled in the academic track when supposed to be in  $5^{th}$  grade by 9.2 ppt and no effect when supposed to be in  $6^{th}$  grade. The results from the RDD specification are similar in magnitude to the results from our baseline specification: participation in preschool increases by 3.4 ppt, probability to start primary school one year earlier than targeted increases by 4.5 ppt, but no effect on the attendance to the academic track of secondary school when being targeted to be in  $5^{th}$  and  $6^{th}$  grade. Finally, the coefficients resulting from the placebo regressions using the sample of native children are all negligible in size and not significant.

Last, we would like to comment on the empirical test whether the results based on the sample including all immigrant children born in Germany provide indeed a lower bound of the results when using the sample of eligible children only. This can be done using the GMC and analyzing children’s track choice when targeted to be in  $5^{th}$  grade. Results are shown in Table A.3, Panel B. Albeit not significant, we can see that the coefficients increase substantially when restricting the sample to eligible children only (from 3.0 to 5.3 ppt). The baseline coefficient resulting from the regression using the GMC is substantially smaller than the coefficient resulting from the regression using the SR data. Where does this difference come from? There are basically two explanations, institutional differences – as already pointed out above, in Schleswig-Holstein teacher recommendations regarding secondary school are not binding, while in most other federal states they are – and the selection arising due to the fact that we use the main language as a proxy for migrant status. Subgroup analysis provides supportive evidence for both explanations: results are only present in states where secondary school track choice is fully at the discretion of parents (see Table A.3, Panel B) and results are driven by decisions of better educated families which points towards expected returns to education being the driving mechanism (see Table A.3, Panel C).

## 6 Conclusion

This study is the first to shed light on how granting birthright citizenship influences immigrant children’s educational development from birth until adolescence. Identifica-

tion is based on the introduction of birthright citizenship in Germany in 2000. Drawing on two administrative datasets – school entrance examinations and school registers for one German state – allows us to employ a difference-in-difference approach that compares immigrant children born shortly before and shortly after the cutoff date in the year of policy change and in adjacent years when there was no policy change. The DiD estimates provide evidence of positive effects from introducing birthright citizenship on second-generation immigrant children’s participation across all educational levels. In particular, gains are visible in terms of attending preschool (by 3.2 ppt), earlier primary school start (5.0 ppt) and increased attendance of the academic track at the onset of secondary school (5.1 ppt). Yet, the initial increase in attendance to the academic track quickly fades out: one year later – when the birth cohorts under study are supposed to be in 6<sup>th</sup> grade - the effect is basically gone. In addition, there is no evidence for an improvement of children’s evaluated skills: there is no effect on children’s readiness for primary school, retention or recommendation for attending the academic track of secondary school. In light of the discretion parents have regarding school decisions – pediatricians’ and teachers’ recommendations are not binding in the state under study – it is thus likely that the observed effects on children’s participation in the German educational system are driven by increased parental integration efforts and educational aspirations.

Taken together, our results point towards birthright citizenship being beneficial for immigrant children’s early integration into the host-country educational system. Yet, initial beneficial effects fade out over time leaving it an open issue whether the ”ius soli” is a successful policy to reach long-lasting improvements in immigrant children’s integration and thus acting as a door opener for immigrant children’s into the host country’s society.

## References

- Abadie, A. (2003). Semiparametric instrumental variable estimation of treatment response models. *Journal of Econometrics*.
- Algan, Y., Dustmann, C., Glitz, A., and Manning, A. (2010). The Economic Situation of First- and Second-Generation Immigrants in France, Germany, and the UK. *Economic Journal*, 120(542):F4–F30.
- Avitable, C., Clots-Figueras, I., and Masella, P. (2013). The Effect of Birthright Citizenship on Parental Integration Outcomes. *Journal of Law and Economics*, (56(3)).
- Avitable, C., Clots-Figueras, I., and Masella, P. (2014). Citizenship, Fertility and Parental Investment. *American Economic Journal: Applied Economics*, (6(4)).
- Beauftragte der Bundesregierung für Migration, Flüchtlinge und Integration (2012). 9. Bericht der Beauftragten der Bundesregierung für Migration, Flüchtlinge und Integration über die Lage der Ausländerinnen und Ausländer in Deutschland.
- Becker, B. (2006). Der Einfluss des Kindergartens als Kontext zum Erwerb der deutschen Sprache bei Migrantenkinder. *Zeitschrift für Soziologie*, 35(6).
- Becker, G. S. and Tomes, N. (1976). Child Endowments and the Quantity and Quality of Children. *The Journal of Political Economy*, 84(4).
- Becker, R. and Tremel, P. (2011). Bildungseffekte vorschulischer Betreuung, Erziehung und Bildung für Migranten im deutschen Schulsystem. *Integration durch Bildung*.
- Bildungsberichterstattung, A., editor (2012). *Bildung in Deutschland; Ein indikatorengestützter Bericht mit einer Analyse zur kulturellen Bildung im Lebenslauf*. W. Bertelsmann Verlag, Bielefeld.
- Borjas, G. (1985). Assimilation, changes in cohort quality, and the earnings of immigrants. *Journal of Labour Economics Vol.3 Nr. 4*.
- Brantsberg, B., Ragan, J. F., and Nasir, Z. M. (2002). The Effect of Naturalization on Wage Growth: A Panel Study of Young Male Immigrants. *Journal of Labour Economics*, 20(3).
- Buckles, K. and Hungerman, D. (2013). Season of Birth and Later Outcomes : Old Question, new Answers. *Review of Economics and Statistics*, 95(3):711–724.

- Bundesministerium der Justiz und für Verbraucherschutz (1999). Staatsbürgerschaftsgesetz .
- Bundesministerium für Familie, Senioren, Frauen und Jugend (2013). Familienwegweiser-Kinderbetreuung: Betreuung für Kinder im Alter von drei Jahren bis Schuleintritt.
- Chiswick, B. (1978). The Effect of Americanization on the Earnings of Foreign-born Men. *Journal of Political Economy*, 86(51).
- Chiswick, B. (1980). The earnings of white and coloured male immigrants in Britain. *Economica*, Vol.47, Nr.185.
- Cunha, F., Heckman, J., Lochner, L., and Masterov, D. (2006). Interpreting the evidence on life cycle skills formation. In Hanushek, E., editor, *Handbook of the Economics of Education*, volume I, pages 310–451, Amsterdam. Elsevier.
- Danzer, N. and Lavy, V. (2013). Parental Leave and Children’s Schooling Outcomes: Quasi-Experimental Evidence from a Large Parental Leave Reform. *NBER Working Paper No. 19452*.
- Dustmann, C., Frattini, T., and Lanzara, G. (2012). Educational Achievement of Second Generation Immigrants; an international comparison. *Economic Policy*, 69(27):143–185.
- Dustmann, C., Raute, A., and Schönberg, U. (2013). Does Universal Child Care Matter? Evidence from a Large Expansion in Pre-School Education. Draft.
- Dustmann, C. and Schönberg, U. (2012). Expansions in maternity leave coverage and children’s long-term outcomes. *American Economic Journal: Applied Economics*.
- Federal Statistical Office (2012). Press release 314/12.
- Fougère, D. and Safi, M. (2009). The Effects of Naturalization on Immigrants’ Employment Probability (France, 1968-1999). *International Journal of Manpower*, 30(1-2):83–96.
- Gathmann, C. and Keller, N. (2014). Returns to Citizenship? Evidence from Germany’s Recent Immigration Reforms. *CESifo Working Paper Series No. 4738*.
- Lalive, R. and Zweimüller, J. (2009). How does parental leave affect fertility and return to work? evidence from two natural experiments. *The Quarterly Journal of Economics*.



- Lüdemann, E. and Schwerdt, G. (2013). Migration Background and Educational Tracking: Is there a Double Disadvantage for Second-Generation Immigrants? *Journal of Population Economics*, 26(2).
- Piracha, M. and Zhu, Y. (2012). Precautionary savings by natives and immigrants in Germany. *Applied Economics*, 44(21).
- Riphahn, R. T. (2003). Cohort Effects in the Educational Attainment of Second Generation Immigrants in Germany: An Analysis of Census Data. *Journal of Population Economics*, 16(4):711–738.
- Sajons, C. (2010). Does Granting Citizenship to Immigrant Children Affect Family Return Migration? Draft.
- Sajons, C. (2012). Does immigrant’s integration behavior change when their children are born with the host-country citizenship? Draft.
- Schönberg, U. and Ludsteck, J. (2015). Expansions in maternity leave coverage and mothers’ labour market outcomes after childbirth. *Journal of Labor Economics*.
- Sprietsma, M. (2013). Discrimination in grading? experimental evidence from primary school. *Empirical Economics*, 113:231–263.
- Steinhardt, M. F. (2012). Does citizenship matter? The economic impact of naturalizations in Germany. *Labour Economics*, 19(6):813–823.

# A Appendix

Table A.1: Descriptives Statistics

	Baseline		II/99	I/00	Diff
	Native*	Migrant	Migrant	Migrant	Migrant
<b>Panel A: Outcome variables</b>					
<b>i. Participation</b>					
Preschool: attendance (SEE data)	0.954	0.925	0.917	0.942	0.025**
Primary school: early start (SR data)	-	0.116	0.086	0.019	-0.067***
Secondary school: academic track 5 <sup>th</sup> grade (SR data)	-	0.220	0.194	0.132	-0.062**
Secondary school: academic track 6 <sup>th</sup> grade (SR data)	-	0.174	0.193	0.155	-0.038
Secondary school: academic track (GMC data- born in D)	0.371	0.233	0.253	0.213	-0.040
Secondary school: academic track (GMC data-eligible)	-	0.207	0.201	0.207	0.005
<b>ii. Evaluations</b>					
Primary school – readiness (SEE data)	0.909	0.849	0.863	0.772	-0.091***
Primary school – retention (SR data)	-	0.219	0.236	0.305	0.069**
Secondary school – recommendation (SR data)	-	0.131	0.154	0.105	-0.048*
<b>Panel B: Background Characteristics (SEE data)</b>					
Age in month	74.762	73.805	76.396	70.724	-5.672***
Female	0.473	0.481	0.478	0.498	0.020
Siblings	0.869	1.233	1.287	1.257	-0.030
Single parent	0.131	0.089	0.088	0.087	-0.001
Mom’s education: low	0.200	0.222	0.220	0.235	0.014
Mom’s education: intermediate	0.332	0.251	0.240	0.240	0.000
Mom’s education: high	0.234	0.162	0.154	0.169	0.015
Mom’s education: missing	0.234	0.365	0.386	0.356	-0.030
Dad’s education: low	0.236	0.226	0.236	0.222	-0.014
Dad’s education: intermediate	0.229	0.235	0.229	0.240	0.011
Dad’s education: high	0.266	0.172	0.154	0.181	0.027
Dad’s education: missing	0.269	0.367	0.381	0.357	-0.024
Mom’s origin: Turkey	-	0.300	0.306	0.315	0.009
Mom’s origin: East Europe	-	0.399	0.405	0.395	-0.011
Mom’s origin: Balkan	-	0.078	0.082	0.074	-0.008
Mom’s origin: EU 12	-	0.051	0.045	0.050	0.005
Mom’s origin: missing	-	0.172	0.162	0.166	0.004
Dad’s origin: Turkey	-	0.304	0.311	0.316	0.005
Dad’s origin: East Europe	-	0.385	0.393	0.376	-0.016
Dad’s origin: Balkan	-	0.079	0.081	0.077	-0.004
Dad’s origin: EU 12	-	0.050	0.042	0.051	0.009
Dad’s origin: missing	-	0.182	0.173	0.180	0.007

Sources: School entrance examination 2005-2007; School registers 2009-2012; German Microcensus 2009-2012; own calculations

\* Note: Migrant status is differently defined depending on the data source: in the SEE data a migrant is defined as having two parents born outside of Germany; in the SR data migrant status is defined as being born in Germany, but not speaking German as the main language. Since the group of children using German as their main language may also include immigrant children, we can not provide descriptive statistic information for native children in the sample based on SR data.

Table A.2: Immigrant children, eligible children and compliers

	All	Eligible	Complier
<b>Panel A: Outcome Variable</b>			
Secondary school: academic track (5 <sup>th</sup> grade)	0.233	0.204	0.196
<b>Panel B: Background Characteristic</b>			
Age	42.017	42.076	43.007
Female	0.476	0.471	0.489
Siblings	1.415	1.412	1.441
Single parent	0.161	0.149	0.152
Mom's education: low	0.342	0.349	0.366
Mom's education: intermediate	0.279	0.197	0.197
Mom's education: high	0.112	0.107	0.101
Mom's education: other or missing	0.267	0.347	0.336
Dad's education: low	0.362	0.394	0.408
Dad's education: intermediate	0.201	0.131	0.129
Dad's education: high	0.11	0.107	0.101
Dad's education: other or missing	0.327	0.378	0.368
Mom's origin: Turkey	0.263	0.225	0.227
Mom's origin: East Europe	0.236	0.076	0.090
Mom's origin: Balkan	0.125	0.159	0.122
Mom's origin: EU 12	0.055	0.104	0.094
Mom's origin: other country	0.278	0.374	0.408
Mom's origin: missing	0.043	0.152	0.059
Dad's origin: Turkey	0.234	0.215	0.205
Dad's origin: East Europe	0.172	0.042	0.057
Dad's origin: Balkan	0.103	0.135	0.104
Dad's origin: EU 12	0.052	0.111	0.094
Dad's origin: other country	0.257	0.332	0.372
Dad's origin: missing	0.183	0.165	0.168

Sources: German Microcensus 2009-2012; own calculations

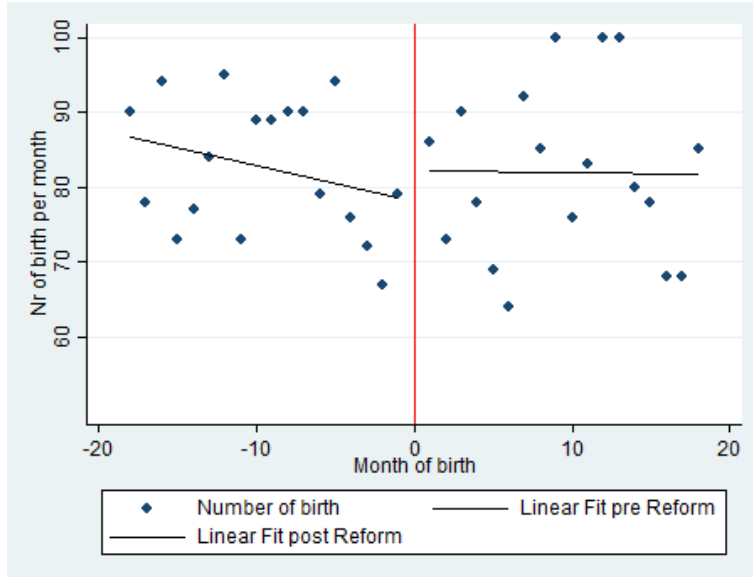
Table A.3: Additional Estimation Results

	(1)	(2)	(3)
<b>Panel A: Parental discretion regarding secondary school choices</b>			
Parents' dismissal of teachers' recommendation (SR data, N=2,530)	0.039 (0.024)	0.040* (0.024)	- -
Non-binding states: academic track (GMC data, N=1,578)	0.047 (0.047)	0.048 (0.047)	0.042 (0.044)
Binding states: academic track (GMC data, N=953)	-0.004 (0.057)	-0.007 (0.057)	0.010 (0.056)
<b>Panel B: Eligibility Status</b>			
All children: academic track (GMC data, N= 2,532)	0.030 (0.036)	0.030 (0.036)	0.038 (0.035)
Eligible children: academic track (GMC data, eligible; N=1,011)	0.053 (0.056)	0.053 (0.056)	0.060 (0.053)
<b>Panel C: Parental background</b>			
Low educated parents: academic track (GMC data, N=162)	0.034 (0.034)	0.035 (0.060)	0.038 (0.069)
High educated parents: academic track (GMC data, N=117)	0.238 (0.245)	0.225 (0.246)	0.255 (0.260)
Birth months	yes	yes	yes
Child Characteristics	no	yes	yes
Family Characteristics	no	no	yes

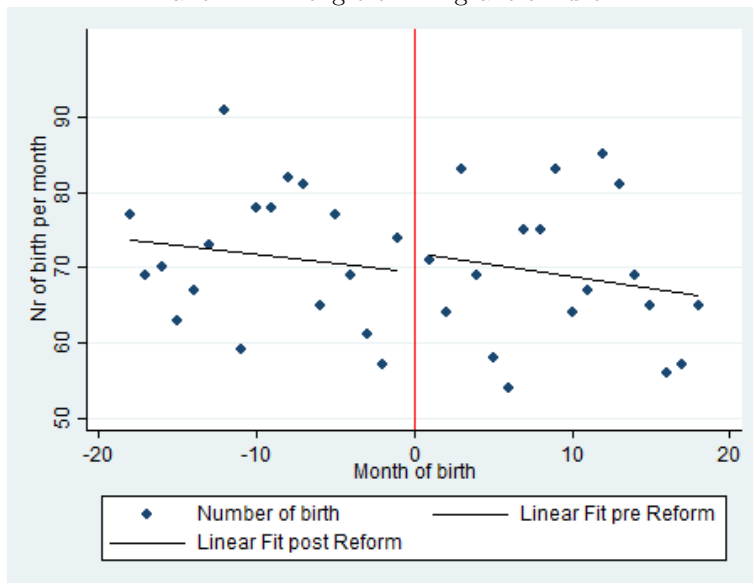
Source: School registers (SR) 2009-2012 and German Microcensus (GMC) 2009-2012; own calculations  
 Note: Estimates are based on OLS regressions using equation (1). Child characteristics contain children's gender and in the case of the SEE data children's age at examination; family characteristics contain information about number of siblings, single-parent household, parents' educational degree and parents' country of origin.

Robust standard errors in parentheses. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Figure A.1: Number of Births by Month of Birth  
Panel A: All immigrant children



Panel B: All eligible immigrant children



Source: German Micro Census 2009-2012

## Ifo Working Papers

- No. 207 Angerer, S., P. Lernerporer, D. Glätzle-Rützler and M. Sutter, How to measure time preferences in children – A comparison of two methods, October 2015.
- No. 206 Kluge, J., Sectoral Diversification as Insurance against Economic Instability, September 2015.
- No. 205 Kluge, J. and M. Weber, Decomposing the German East-West wage gap, September 2015.
- No. 204 Marz, W. and J. Pfeiffer, Carbon Taxes, Oil Monopoly and Petrodollar Recycling, September 2015.
- No. 203 Berg, T.O., Forecast Accuracy of a BVAR under Alternative Specifications of the Zero Lower Bound, August 2015.
- No. 202 Henderson, M.B., P. Lernerporer, P.E. Peterson, K. Werner, M.R. West and L. Woessmann, Is Seeing Believing? How Americans and Germans Think about their Schools, August 2015.
- No. 201 Reischmann, M., Creative Accounting and Electoral Motives: Evidence from OECD Countries, July 2015.
- No. 200 Angerer, S., D. Glätzle-Rützler, P. Lernerporer and M. Sutter, Cooperation and discrimination within and across language borders: Evidence from children in a bilingual city, May 2015.
- No. 199 Schulz, B., Wage Rigidity and Labor Market Dynamics with Sorting, May 2015.
- No. 198 Jochimsen, B. and R. Lehmann, On the political economy of national tax revenue forecasts – Evidence from OECD countries, March 2015.
- No. 197 Marz, W. and J. Pfeiffer, Resource Market Power and Levels of Knowledge in General Equilibrium, March 2015.
- No. 196 Lehmann, R., Survey-based indicators vs. hard data: What improves export forecasts in Europe?, March 2015.

- No. 195 Fabritz, N., ICT as an Enabler of Innovation: Evidence from German Microdata, January 2015.
- No. 194 Kauder, B. and N. Potrafke, Just hire your spouse! Evidence from a political scandal in Bavaria, December 2014.
- No. 193 Seiler, C., Mode Preferences in Business Surveys: Evidence from Germany, November 2014.
- No. 192 Kleemann, M. and M. Wiegand, Are Real Effects of Credit Supply Overestimated? Bias from Firms' Current Situation and Future Expectations, November 2014.
- No. 191 Kauder, B, Spatial Administrative Structure and Intra-Metropolitan Tax Competition, October 2014.
- No. 190 Auer, W. and N. Danzer, Fixed-Term Employment and Fertility: Evidence from German Micro Data, October 2014.
- No. 189 Rösel, F., Co-Partisan Buddies or Partisan Bullies? Why State Supervision of Local Government Borrowing Fails, October 2014.
- No. 188 Kauder, B., Incorporation of Municipalities and Population Growth – A Propensity Score Matching Approach, October 2014.
- No. 187 Méango, R., Financing Student Migration: Evidence for a Commitment Problem, September 2014.
- No. 186 Nagl, W. and M. Weber, Unemployment compensation and unemployment duration before and after the German Hartz IV reform, September 2014.
- No. 185 Potrafke, N. and M. Reischmann, Explosive Target balances of the German Bundesbank, July 2014.
- No. 184 Eppinger, P.S. and G.J. Felbermayr, Bilateral Trade and Similarity of Income Distributions: The Role of Second Moments, July 2014.
- No. 183 Wohlrabe, K., Das FAZ-Ökonomenranking 2013: Eine kritische Betrachtung, Juli 2014.
- No. 182 Lehmann, R. and A. Weyh, Forecasting employment in Europe: Are survey result helpful?, June 2014.