

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

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December 11, 2014

Abstract

Does birthright citizenship boost immigrant children's participation and success in the host country's educational system? We address this question using a reform of the German naturalization law in 1999 that entitled children born after January 1, 2000 to birthright citizenship. We use a difference-in-difference design that compares children born shortly before and shortly after the cutoff in years of policy change and years in which no policy change took place. Our analysis draws on administrative data from school entrance examinations and on the German Micro Census. Results indicate an increased participation of immigrant children in the early education system. Despite medium-run benefits in terms of socio-behavioral development, there is no significant evidence that immigrant children are better prepared for primary school and there is just some evidence suggesting positive effects on enrolment in the academic track of secondary school.

JEL Classification: I21, K37

Keywords: immigration law, human capital formation, difference-in-difference

^{*}We thank Natalia Danzer, Eva Deuchert, Christian Dustmann, Beatrix Eugster, Albrecht Glitz, Martin Huber, Helmut Rainer, 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, EALE 2014, Verein für Socialpolitik 2014, SMYE 2014, and RES 2014. 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. Corresponding author: Christina Felfe, University of St. Gallen and CESifo, Varnbühlstr. 14, CH-9000 St. Gallen, Email: christina.felfe@unisg.ch

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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.¹ Moreover, the disadvantages inherent in immigration are not only suffered by first-generation immigrants; their descendants, for example, tend to have relatively lower educational achievement.² 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, further hampering their integration.

One highly debated policy, albeit contentious, intended to foster the integration of immigrants is the granting of citizenship. Expected benefits are chiefly related to immigrants' labor market participation (Chiswick, 1978; Brantsberg, Ragan, & Nasir, 2002; Fougère & Safi, 2009; Steinhardt, 2012; Gathmann & Keller, 2014). In addition to the usual channels for obtaining citizenship, many countries are considering the introduction of birthright citizenship to immigrant children (e.g. Germany in 2000, Portugal in 2004, Greece in 2010, Austria and Italy in 2013). In contrast, the United States is contemplating the abolishment of birthright citizenship in light of rising birth rates among illegal immigrants. 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. Specifically, these studies find reduced remittances and a lower probability of return migration, as well as increased interaction with the local community and more use of the local language (Avitable, Clots-Figueras, & Masella, 2013, 2014; Piracha & Zhu, 2012; Sajons, 2010, 2012). Far less understood are the direct consequences of birthright citizenship for children.³

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

¹For a description of the economic situation of first- and second-generation immigrants in Europe and the United States see Algan, Dustmann, Glitz, and Manning (2010); Borjas (1985); Chiswick (1980)

²A comparison of the educational attainment of second-generation immigrants with that of children born to native parents is provided by Dustmann, Frattini, and Lanzara (2012); Riphahn (2003).

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

to adolescence.⁴ Our particular focus lies on the participation and success of immigrant children in the host country’s educational system.

Analyzing the impact of birthright citizenship on children’s educational outcomes is challenging, and the potential for selection bias is a major concern. 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*, meaning that 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.⁵ 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 (second semester of 1999 and first semester of 2000, respectively), but draw upon immigrant children from adjacent school cohorts as a control group (second semester of 1998 or 2000 and first semester of 1999 or 2001, respectively). 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 enabling us to derive relatively 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 use administrative records from school entrance examina-

⁴As we have recently learned, there is simultaneous work by Clots-Figueras and Sajons. 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. Clots-Figueras and Sajons 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 born before and after the cutoff date in years of policy change and years in which no policy change took place. Clots-Figueras and Sajons, on the contrary, rely on a comparison of immigrant and native children.

⁵The reform changed other features of the naturalization law. For details on the reform, see Section 2.1.

tions for all children born in one German federal state (Schleswig-Holstein). These records contain physicians' assessments of children's school readiness at age six based on a set of cognitive and non-cognitive skills (e.g. language proficiency and socio-behavioral development). An accompanying questionnaire, filled out by the parents, provides information on children's preschool attendance and important background characteristics. We use the records of 6,752 immigrant children born between July 1998 and June 2001 and examined for school entrance in 2005, 2006 and 2007. Our second source of data is the German Micro Census, Europe's largest household survey. This dataset - available for the whole of Germany - provides us with educational outcomes at a later stage of childhood, namely tracking into different types of secondary school. The Micro Census gives us the opportunity to look only at children who in fact were eligible for birthright citizenship. We use the survey years 2009-2012 and obtain a sample of 1,011 immigrant children born between July 1998 and June 2001 at the age of 10, the targeted age when tracking takes place.

Our results suggest a positive impact of birthright citizenship on immigrant children's participation in the German educational system. Introducing birthright citizenship increases non-mandatory preschool attendance by 3.1 percentage points (ppt), which corresponds to a 3.4 % increase. The introduction of birthright citizenship also enhances immigrant children's socio-behavioral development: specifically, at the onset of primary school, we find positive effects on children's behavior (1.8 ppt) and emotional stability (2.1 ppt) – a 1.9% and 2.2% increase, respectively. Results also point towards gains in German language proficiency (3.4 ppt or 5.6 %), but this estimate lacks precision since very few children participated in the language tests. Despite these benefits we find no evidence that immigrant children are better prepared for primary school. Evidence for long-run effects on immigrant children's access to the academic track is at best weak: children eligible for birthright citizenship are on average 6.5 ppt more likely to attend the academic track in secondary school (31.9 %), but again this estimate lacks precision such that we cannot exclude a nil effect. Our results are robust to a variety of sensitivity checks, such as narrowing the window around the cut-off date, allowing for a trend in immigrant children's educational outcomes, and introducing another control group – native children.

Finding positive effects of birthright citizenship on the educational participation and success of immigrant children in the host country's educational system immediately raises the question of "Why?". Why does birthright citizenship enhance the integration of immigrant children? First, citizenship is a basis for political and professional equality and thus for suc-

cessful integration in the host country in the long-run. As a result, it is likely to enhance children’s future labor market opportunities and thus increase their family’s likelihood of staying in the host country. Immigrant parents therefore 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). In addition, children themselves may put forth more effort in school and form closer ties to their native peers. Second, citizenship may reduce discrimination 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 provides a brief overview of the institutional background and discusses the main mechanisms through which birthright citizenship may exert its effects on children’s educational outcomes. Section 3 introduces the empirical strategy. Section 4 describes the different data-sets used for our analysis. Section 5 presents our main results and provides a sensitivity analysis. Section 6 concludes.

2 Background

2.1 Reform of the Nationality Law in 1999

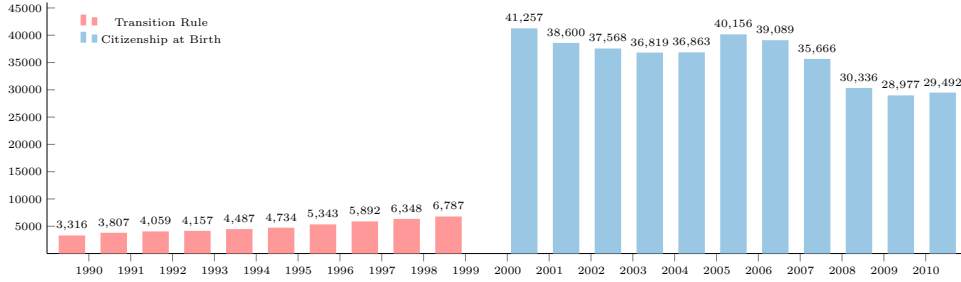
After a long and heated debate over how to deal with the rising number of immigrants, in May 1999 the German parliament undertook a major revision of the 1913 ”German Citizenship and Nationality Law”. On July 15, 1999, the German parliament ratified the new version of the law.⁶ The reform made three 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

⁶The legal text can be found in StAG 4 Abs.3 (Bundesministerium der Justiz und für Verbraucherschutz, 1999).

no need for the parents to apply for it. 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 (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 and who were thus granted citizenship automatically.

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



Source: 9. Bericht der Beauftragten der Bundesregierung für Migration, Flüchtlinge und Integration über die Lage der Ausländerinnen und Ausländer in Deutschland

The reform also changed the general eligibility criteria for citizenship.⁷ 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 have increased substantially.

In regard to obtaining/applying for German citizenship at birth, as mentioned, 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

⁷Notice that these criteria apply for all immigrants independently of the birth of their children, and thus apply for parents whose children are born before and after the cutoff date.

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, application for citizenship is free; for children born before the cutoff date, application for citizenship carries administrative costs and requires renunciation of the 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, 92 % of all three- to five-year old children attended preschool (Federal Statistical Office, 2012). Among three- to five-year old immigrant children, this share was 85 % (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 (born around 2000) 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 (SEE), 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. In the context of the SEE, local health services have recently begun testing nonnative children for German-language proficiency. In addition, pediatricians assess children's socio-behavioral development.

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:⁸ 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 Grades 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. Depending on the federal state, these recommendations may be binding or not (Ständigen Konferenz der Kultusminister der Länder in der Bundesrepublik Deutschland, 2010). In five states, the decision on further education is completely 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 immigrant-native gap amounts to 11.2 ppt. This difference in attendance rates could be the result of worse performance during elementary school or, in case of non-binding recommendations, of lower educational aspirations (either of the immigrant child or the parents), but could also be due to discrimination by teachers. In fact, Lüdemann and Schwerdt (2013) detect a immigrant-native gap of 6.7 ppt when accounting for children's performance during primary school.

⁸There 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 of schools.

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.⁹ Figure 2 illustrates this framework.

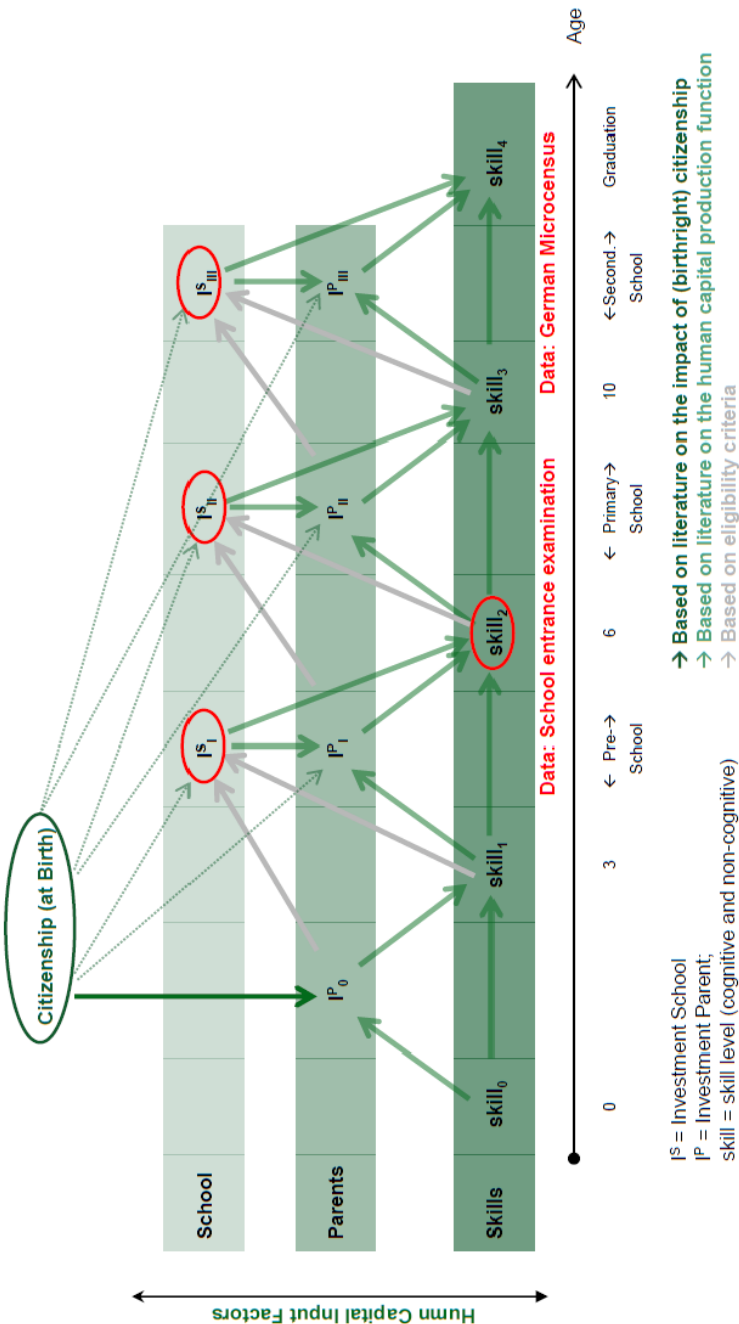
As discussed above, there are three levels in the German educational system, denoted by $T = (I; II; III)$ and represented by the horizontal axis in Figure 2: preschool (age 3-5, level I), primary school (age 6-9, level II) and secondary school (from age 10 onward, level III). The transition from one level to the next requires a certain skill level $\overline{skill}_t$, but parents can influence the decision whether to enroll their child. Thus, a child’s progress from one educational level to the next depends on his or her skills and the parents’ decision, which is part of the parents’ overall investment. In other words, 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 $skills_{t+1}$ are the outcome of parental investment I_T^P and school investment I_T^S .¹⁰ 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: $skills_{t+1} = skills(I_T^P; I_T^S; skills_t)$.

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?

⁹For an overview of the human capital production theory, see Cunha, Heckman, Lochner, and Masterov (2006)

¹⁰For 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., teachers may invest more in citizen children or peers may bully citizen children less. However, the extent to which teachers and peers are aware of the citizenship status of immigrant children is not clear.

Figure 2: Birthright Citizenship and Human Capital Production Function



Source: Authors own elaboration.

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 & Safi, 2009; Steinhardt, 2012; Gathmann & Keller, 2014). In particular, it opens the door to any job requiring civil servant status.¹¹ 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.¹² 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. Indeed, Sajons (2010) finds a reduction in return migration in the aftermath 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 two 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 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

¹¹Access to civil servant posts is not restricted to EU citizens. Thus, 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. As such, 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.

¹²This is not a German specialty, but in the United States, for instance, civil servant posts make up to 7 % of all jobs.

of immigrant children (B. Becker, 2006; R. Becker & Tremel, 2011; Dustmann, Raute, & Schönberg, 2013). These additionally acquired skills may then influence the recommendation for primary school and later qualification for gymnasium. 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 for primary school or gymnasium. If, as suggested in Figure 2, there is a feedback mechanism between children’s skills and parental investments, parents may then reduce their integration efforts.

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 in preschool, children’s skills observed at the beginning of primary school, recommendation for primary school, and participation in gymnasium.

3 Empirical Approach

Analyzing the impact of citizenship on children’s development 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 children born shortly before the cutoff with 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 one of our data sources (described in more detail in Section 4) does 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 includes a group of children who are unaffected by the reform and the resulting estimates should

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 other data source, which contains information on parents’ duration of residence.¹³

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 (the *ReformCohort*) and adjacent years in which no policy change took place (the *ControlCohorts*). A similar strategy is used by Lalive and Zweimüller (2009), Dustmann and Schönberg (2012), Danzer and Lavy (2013), and Schönberg and Ludsteck (in press). The equation to be estimated is:

$$\begin{aligned} Y_i^{s,t} = & \alpha + \beta ReformCohort_i + \gamma After_i + \delta ReformCohort_i * After_i + \\ & + \theta_m D_{i,m} + \theta_t A_{i,t} + \epsilon_{i,m,t} \end{aligned} \quad (1)$$

where $Y_i^{s,t}$ represents the educational outcome in dimension s of child i who is t years old. *ReformCohort_i* is a binary variable indicating whether child i belongs to the reform cohort (i.e., whether child i belongs to the cohort 1999/2000 when the law changed). *After_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 *ReformCohort_i * After_i* is thus 1 for all children born between January and June 2000. The effect of interest is captured by the coefficient δ preceding the interaction term *ReformCohort_i * After_i* and identifies the causal effect of a switch from a *ius sanguini* to a *ius soli* regime on immigrant children’s educational outcomes.

¹³It is unclear whether children whose parents had resided for less than eight years in Germany are an ideal control group because, depending on duration of residence, families might belong to different immigration waves and thus to different ethnic groups.

To make sure that we capture all systematic differences between children born in different months of the year we additionally control for a set of birth month dummies $D_{i,m}$ as well as a set of age dummies $A_{i,a}$. Controlling for children’s age will account for the fact that children born in the first semester of the school year are on average half a year older and thus more mature when assessed for school readiness or when referred to secondary school than children born in the second semester of the school year. Just a one-month difference in age can make quite a difference in children’s educational development, especially during the preschool years. Thus, unconditional DiD estimates are likely to represent a lower bound of the potentially positive effect of introducing birthright citizenship. Controlling for children’s birth month accounts for the 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 & Hungerman, 2013). Thus, in this case, unconditional DiD estimates would likely be upward biased.¹⁴

One remaining threat to our identification strategy is that parents may have reacted to the policy change. For example, they could have delayed the birth of their child such that the child was born under the new policy regime. Second, parents might have adjusted their desired number of children downward. The argument for a reduction in fertility is based on the quantity-quality tradeoff put forward by G. S. Becker and Tomes (1976): as discussed in section 2.3, birthright citizenship is likely to increase the returns to country-specific skills and thus the “quality” of the children. Indeed, Avitable et al. (2014) show a reduction in fertility after introduction of birthright citizenship in Germany, but only from 2001 onward. Hence, the children included in our sample, who were conceived before September 1999, are unlikely to be affected by this concern. Nevertheless, to address these two concerns, 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). In addition, introduction of birthright citizenship made return

¹⁴The assignment variable $After_{i,m}$ 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.

migration less attractive (Sajons, 2010). As a result, the pool of families remaining in the country may change after the introduction of birthright citizenship. The resulting selection bias, however, is likely to lead 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, additional specifications control for proxies of families’ socioeconomic status, such as single parenthood, parental education and parents’ country of origin.

Finally, we address the concern that there could be unobserved changes in the conditions to which children born before and after the cutoff were exposed. For this purpose, we allow for trends specific to immigrant children and introduce a dummy variable for each cohort and interact it with the dummy *After_i*. We also introduce another control group - native children - and employ a triple DiD design. The exact specification is explained in Section 5.3.

4 Data

Answering the question under study empirically requires very comprehensive data. First, we need data that provide information on children’s educational outcomes over the first 10 years after children’s birth. Second, our analysis relies on a very small subgroup of the German population: a few cohorts (1998/1999, 1999/2000, and 2000/2001) of immigrant children. Nevertheless, two unique data sources allow us to answer the question under study empirically: first, school entrance examinations (SEE) from one German state, Schleswig-Holstein;¹⁵ and second, the German Micro Census (GMC). The following subsections describe both datasets. Descriptive statistics can be found in the Appendix, in Table A.1 to A.3.

4.1 School Entrance Examination Data

The school entrance examinations (SEEs) of Schleswig-Holstein are the basis of our analysis on children’s educational outcomes up to primary school. 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

¹⁵Unfortunately, administrative data on children’s early educational outcomes are not available for the whole German territory, but for selected states only. In fact, access to the SEE is highly restricted and only occurs by special permission from the respective office in charge.

SEEs 2005, 2006, and 2007.¹⁶ 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 6,752 observations.

The recommendation as to school readiness is a summary measure of the entire set of diagnoses that the pediatrician in charge of the SEE performs. 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.

We additionally draw on the assessment of immigrant children's language proficiency and socio-behavioral development, both dimensions for which birthright citizenship is likely to make a difference. The assessment of children's socio-behavioral development was only gradually introduced from 2005 onward and thus sample sizes regarding the different dimensions of children's socio-behavioral development are slightly smaller (between 5,177 and 6,473 children). Rating of children's socio-behavioral development is based on the pediatrician's observations as well as on a brief behavioral screening questionnaire designed to identify emotional problems, behavioral problems, pro-social behavior, and peer relations (Goodman, 1997). The questionnaire is usually filled out by the accompanying caregiver.¹⁷ The diagnoses for the different dimensions of children's socio-behavioral development are "no problems", "problems, but no treatment is necessary", "problems, already in treatment", "problems, treatment necessary", and "problems which probably reduce child's performance in school". We recode all diagnoses to take the value 1 if there is no diagnosis of a developmental problem, that is, if the child is developing normally, and 0 otherwise.

German-language proficiency is only tested with immigrant children. These tests were introduced in 2005 and at first only in the city of Lübeck. As a result, the sample used for analyzing children's language proficiency contains only 446 observations. The ranking of language proficiency goes

¹⁶Parents 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.

¹⁷Thus, the assessment of children's socio-behavioral development might be affected by the caregiver's subjective perceptions. Moreover, survey responses might be affected by non-response. Yet, given that the diagnosis is based on a pediatric assessment of those responses, the diagnosis is probably not much affected by response bias. Also, in 93 % of all cases, a medical diagnoses regarding socio-behavioral development is available. This indicates that non-response bias is not a major concern.

from 1 to 9, indicating "fluent" to "no competence". We recode this measure into a binary variable taking the value 1 if a child is fluent or makes at most small mistakes.

The questionnaire filled out by the accompanying caregiver also contains a series of questions on the child and family background. As a result, we have information on attendance at preschool as well as on the child's age and gender, the household composition, and parents' education. Unfortunately, the questionnaire only asks about the parents' country of origin, and does not query their length of residence in Germany. As a result, we cannot restrict our sample only to those children whose parents have lived in Germany for a minimum of eight years.¹⁸ Our sample thus also includes children who are unaffected by the reform and the resulting estimates are attenuated (provide a lower bound of the impact of the reform on educational outcomes up to primary school). As discussed in Section 3, we use our second dataset – the German Micro Census, described in detail in the next subsection – to provide evidence of attenuation bias.

A raw comparison of the educational outcomes between foreign children born in the second semester of 1999 and the first semester of 2000 reveals already several differences (see Table A.1).¹⁹ Children born after the cutoff date are more likely to attend preschool than are children born before the cutoff date (94.2 % vs. 91.7 %). Yet, there is no significant difference in terms of school readiness (80.6 % vs. 82.4%). In terms of language proficiency, children born after the cutoff date are slightly, but not significantly more fluent in German than children born before the cutoff date (61.9 % vs. 60.6 %). In terms of children's socio-behavioral development, children born after the cutoff behave better (97.5 % vs. 95.7 %), are more stable emotionally (97.4 % vs. 93.8 %), and have better peer relations and social skills (98.0 % vs. 96.7 % and 96.6 % vs. 95.8 %, respectively). The latter two differences, however, are not significant at any conventional level.

Importantly, aside from the age difference, there are no remarkable differences between children born before and after the cutoff date either in terms of their demographic and socio-economic background characteristics or in terms of parents' country of origin (see Table A.2).

¹⁸In fact, all we know is whether the family was living in Germany at the time of the survey. Thus, we do not even know whether the child is born in Germany.

¹⁹All variables measuring children's educational performance are age adjusted, which allows us to ignore age differences between children born before and after the cutoff, which amounts to 5.7 months in the sample based on the SEEs.

4.2 German Micro Census

The German Micro Census (GMC), which includes 1% of the German population and is thus the largest household survey in Europe, serves as our database for analyzing children’s referral to secondary school. Transition from primary school to secondary school takes place after Grade 4. To capture children in their fifth grade, we draw on the GMC in 2009, 2010, 2011, and 2012. Specifically, the cohort 1998/1999 is based on surveys taking place between July 2009 and June 2010, the cohort 1999/2000 is based on surveys between July 2010 and June 2011, and the cohort 2000/2001 is based on surveys between July 2011 and June 2012.

To guarantee the highest degree of comparability between sample based on the SEE and the sample based on the GMC, we initially restrict our sample to children whose parents were migrants (i.e., parents who were both born abroad) and did arrive to Germany before starting primary school.²⁰ We further exclude children residing in Berlin and Brandenburg, as in these two states tracking into secondary school takes place after Grade 6 instead of after Grade 4.²¹ The resulting sample - which we refer to as the *baseline sample* - contains 2985 children. In addition, we construct the so-called *eligible sample*: for this purpose, we use only children who are born in Germany and for whom at the time of birth at least one parent had a minimum duration of residence of eight years. In addition, we exclude all children whose parents received German citizenship before childbirth. The resulting sample contains 1011 observations.

The GMC contains information about all household members – including children. Using the information on the school type attended by the children, we create our binary outcome variable *gymnasium* that is equal to 1 if the child attends gymnasium and 0 otherwise. In our baseline sample, on average, 26.6 % of the immigrant children attend gymnasium. Immigrant children belonging to the cohort 1999/2000 and born after the cutoff date are 6.7 ppt less likely to attend gymnasium than immigrant children born before the cutoff date (20.7 % vs. 27.4 %). When restricting the sample to eligible children only, this difference changes in favour of the immigrant children born after the cut-off and amounts to 0.6 ppt (20.7 % vs. 20.1 %).

²⁰As mentioned in Section 4.1, the SEEs contain only information on whether the child was living in Germany at the date of the SEE and thus in 2005 for the cohort 1998/1999, in 2006 for the cohort 1999/2000, and in 2007 for the cohort 2000/2001.

²¹Unfortunately, the sample size prevents us from restricting the sample to children residing in Schleswig-Holstein only (there are only 76 residing in Schleswig-Holstein.)

Like the SEE, the GMC contains information on children’s age and gender, household composition, parents’ education, and parents’ country of origin (see Table A.2). Again, there is a remarkable age difference between children born before and after the cutoff date (43.1 quarter vs. 41.0 quarter). Yet, there are also differences with respect to parental education. Specifically, mothers whose children are born after the cutoff are on average less educated than mothers whose children are born before the cutoff: only 10.4 % of the first group attended gymnasium vs. 16.1 % of the second group. This difference also exists between fathers, yet is slightly smaller, but still remarkable (13.2 % vs. 10.2 %). What can explain this differences in parental education? As discussed in Sections 1 and 3, the introduction of birthright citizenship led to a reduction in return migration, in particular among low educated families. As a result, the pool of remaining immigrants is likely to be less educated. This might even aggravate over time – return migration is likely to increase over time – which might also explain why we do not observe any difference in terms of parental education in the SEE data. Our empirical specifications consider these differences in parental education.

Finally, the GMC contains information on children’s citizenship. This information enables us to analyze whether the introduction of the *ius soli* did indeed lead to an increase in citizenship among immigrant children born in Germany. Section 5.1 provides an estimate of the actual share of children obtaining citizenship due to the reform or, technically speaking, on the actual share of compliers.

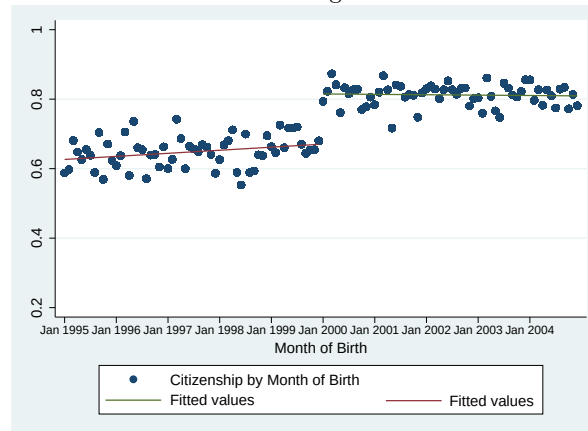
5 Results

We now present empirical evidence on how granting birthright citizenship to second-generation immigrant children 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.

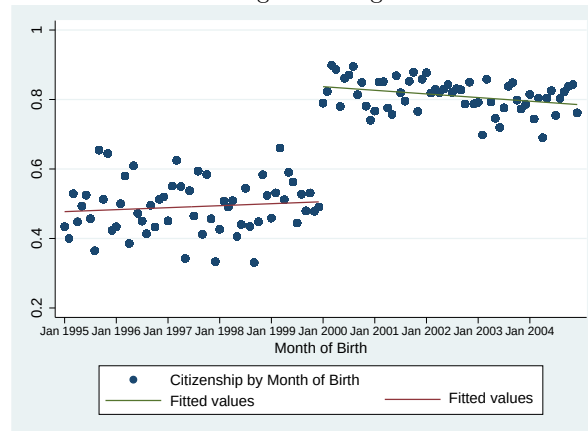
5.1 The Increase in Citizenship at Birth

How many children did actually benefit from the introduction of birthright citizenship? Figure 3 displays the increase in citizenship among all immigrant children, eligible immigrant children, and, for the purpose of robustness, non-eligible immigrant children.

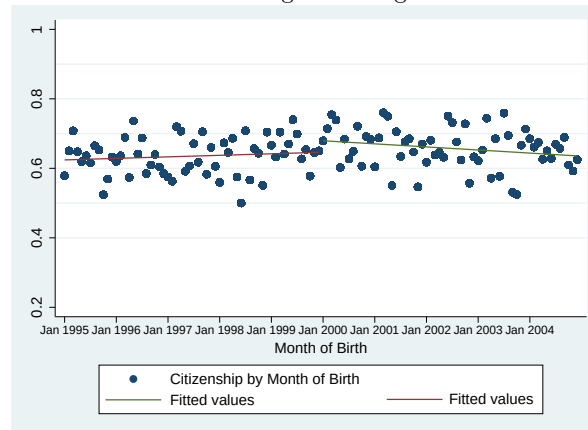
Figure 3: Citizenship by Month of Birth
Panel A: All immigrant children



Panel B: All eligible immigrant children



Panel C: All non-eligible immigrant children



Source: German Micro Census 2009-2012

Let us first assess the share of compliers (i.e., the share of children acquiring citizenship due to 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, 68.2 % 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 13.8 %.

The reason the share of children with German citizenship after the reform of the naturalization law does not equal 100% is twofold: First, not all children were eligible for birthright citizenship; second, children are permitted more than one nationality up to their 23rd birthday.. When asked to report the nationality of their child, however, parents may not report both nationalities, but only their own nationality. 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 for the share of citizens being 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 %.

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. Clearly, there is no jump in citizenship around the cutoff date specified in 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 introduction of birthright citizenship.

5.2 Birthright Citizenship and Children's Education

Do immigrant children gain access to and progress better in the educational system due to introduction of *ius soli*? Table 1 displays the DiD estimates based on Equation 1 using the sample of all immigrant children. The estimates displayed in Column (1) stem from a DiD equation controlling for children's age and month of birth only and thus correspond to Equation 1

discussed in Section 3. In further specifications, shown in Columns (2) and (3), we additionally control for children’s characteristics, such as gender and number of siblings, and family characteristics, such as single parenthood, parental education and parents’ country of origin. Results are robust across the three specifications. In fact, coefficients are basically unaffected by the inclusion of further control variables. This result provides evidence in favor of our identifying assumption that the timing of birth is random and that there is no selection due to differential access to citizenship. In turn, we discuss the results when including the full set of control variables.

Introduction of birthright citizenship stimulates immigrant children’s preschool attendance: after the introduction of birthright citizenship, immigrant children are 3.1 ppt more likely to attend preschool than they were before. Given the attendance rate of 91.7 % at baseline, this implies an increase of 3.4 %.

Positive effects of the introduction of birthright citizenship are also observed when children are assessed whether they are ready for primary school: children falling under the *ius soli* regime display a more appropriate behavior and are more emotionally stable. Specifically, the introduction of birthright citizenship leads to an improvement in immigrant children’s behavior of 1.8 ppt and emotional stability of 2.1 ppt. Given that 95.7 % and 93.8 % of all immigrant children do not exhibit any behavior or emotional problems prior to the introduction of birthright citizenship, this increase corresponds to a 1.9 % and 2.2 % increase, respectively. There are no significant improvements of children’s peer relations or social skills.

There is some evidence for gains in children’s language competency, but the estimate is not significant at any conventional level. This lack of precision is likely due to a power problem – throughout the years 2005 to 2007 only one county tested immigrant children’s language skills and thus we have only 472 observations for language competency. In fact, when scaling the standard error by the number of observations included in our baseline sample, the coefficient turns out to be marginally significant.

Nevertheless, despite these gains in several developmental dimensions, the introduction of birthright citizenship does not seem to cause a substantial improvement of immigrant children’s readiness for primary school: the share of immigrant children assessed being ready to follow the curriculum taught in primary school is basically unchanged by the naturalization law reform.

Table 1: Estimation Results

	(1)	(2)	(3)
Panel A: Preschool - Attendance (6752 Observations)			
Born after the reform	0.032** (0.013)	0.032*** (0.013)	0.031** (0.013)
Panel B: Primary School			
a. Behavior (6268 Observations)			
Born after the reform	0.019** (0.009)	0.019** (0.009)	0.018** (0.009)
b. Emotional Stability (6473 observations)			
Born after the reform	0.022** (0.010)	0.022** (0.010)	0.021** (0.010)
c. Peer relations (5177 Observations)			
Born after Reform	-0.003 (0.009)	-0.003 (0.009)	-0.003 (0.009)
d. Social skills (5177 Observations)			
Born after the reform	0.001 (0.008)	0.001 (0.008)	0.000 (0.008)
e. Language Proficiency (446 observations)			
Born after the reform	0.012 (0.098)	0.012 (0.098)	0.034 (0.099)
f. School Readiness (6717 observations)			
Born after the reform	-0.006 (0.019)	-0.006 (0.019)	-0.013 (0.019)
Panel C: Secondary School (Gymnasium)			
a. Baseline Sample (2985 observations)			
Born after the reform	0.001 (0.034)	-0.006 (0.033)	0.014 (0.032)
b. Eligible Sample (1011 observations)			
Born after the reform	0.053 (0.055)	0.044 (0.055)	0.065 (0.053)
Age & Birth month Dummies	yes	yes	yes
Child Controls	no	yes	yes
Family Controls	no	no	yes

Source: German Micro Census 2009-2012 (for secondary school outcomes) and administrative data/school entrance examination 2005-2007; own calculations

Note: Robust standard errors in parentheses; Child controls consist of children's gender and number of siblings; Family controls include a dummy for single parenthood, a set of dummies indicating parent education and a set of dummies indicating parents' country of origin; *p < 0.1; **p<0.05; ***p<0.01

One explanation for the lack of any effect on immigrant children's school

readiness is that gains in children’s development due to the introduction of birthright citizenship and the subsequent increase in preschool attendance – for instance, observed in children’s behavior and emotional stability – are either not sufficient to make a difference in children’s readiness for primary school or occur in dimensions that are not or only partially considered in the overall assessment of school readiness.

There is also only limited evidence for a longer-lasting effect of the switch to the *ius soli* regime: Using the baseline sample the point estimate resulting from the regression of immigrant children’s access to gymnasium amounts to 1.4 ppt.. This coefficient, however, is not significant at any conventional level. When restricting the sample to eligible children only the effect increases remarkably: specifically, it rises from 1.4 ppt to 6.5 ppt. Note that this increase is evidence in favor of the baseline estimates providing a lower bound for the causal effect of introducing birthright citizenship. Nevertheless, while the point estimate indicates some favorable effect of birthright citizenship on children’s long-run opportunities in the educational system of the host-country, the lack of precision does not allow us to exclude a nil effect.

In sum, our results point towards birthright citizenship being beneficial for immigrant children’s early integration into the host-country educational system, but are ambiguous whether birthright citizenship is successful in reaching long-lasting improvements.

5.3 Robustness

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 reacted to the introduction of birthright citizenship by either reducing fertility or delaying birth. An additional threat to our identification strategy is the existence of other changes to any determinant of children’s educational development. To the best of our knowledge, however, there was no other reform around our cutoff date that could have stimulated the educational development of children born in the first half of 2000, while not having affected the educational development of children born in the second half of 1999.

To address these potential threats to identification, we pursue the following robustness checks: First, we allow for a flexible time trend which may capture any changes over time. Specifically, we include an additional dummy for the birth cohort 2000/2001. Second, we restrict our sample exclusively 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 introduce another control group – native children born between July 1998 and June 2001 – and estimate a triple DiD.²² The equation to be estimated is:

$$\begin{aligned}
Y_{i,m}^s = & \alpha + \beta ReformCohort_i + \gamma After_{i,m} + \delta ReformCohort_i * After_{i,m} \\
& + \zeta Migrant_i + \eta Migrant_i * ReformCohort_i \\
& + \theta Migrant_i * After_{i,m} \\
& + \iota Migrant_i * ReformCohort_i * After_{i,m} \\
& + \sum_{m=1}^{12} \kappa_m D_{i,m} + \eta X_i + \epsilon_{i,m}
\end{aligned} \tag{2}$$

Compared to the baseline DiD, the triple DiD approach additionally controls for the variable $Migrant_i$, which indicates whether or not a child has a migratory background. In addition, we interact this variable with $Cohort99/00_i$, $After_{i,m}$, and $ReformCohort_i * After_{i,m}$. The coefficient of interest is thus ι , which measures the change in educational outcomes between immigrant children born shortly before and shortly after the introduction of birthright citizenship, net of not only any general differences between children born in different semesters of a school year, but also net of any differences between children born in different semesters of the school year 1999/2000 unrelated to ethnicity.

Results of our sensitivity checks are shown in Table 2. Overall, the coefficients capturing the impact of the reform on immigrant children’s educational outcomes are basically unchanged. Specifically we observe the following. Introducing a time trend, makes results, if anything, stronger: children born after the cutoff date are 3.4 ppt more likely to attend preschool, display 1.1 ppt better behavior and 2.1 ppt stronger emotional skills. The likelihood that eligible immigrant children attend gymnasium increases by 8 ppt, even though this result is still not significant. Restricting the sample to a 4-month window gives similar results: children under the *ius soli* regime are 3.8 ppt more likely to attend preschool, display 1.9 ppt better behavior and 3.0 ppt stronger emotional skills. The estimates for all remaining educational outcomes lack precision.

²²We use only those children whose parents were both born in Germany so as to have a homogeneous control group.

Table 2: Sensitivity Analysis

	Trend	4-month window	DIDID
Panel A: Preschool - Attendance			
Born after the reform	0.034** (0.013)	0.038** (0.015)	0.038*** (0.011)
Panel B: Primary School			
a. Behavior			
Born after the reform	0.011* (0.007)	0.019* (0.011)	0.021* (0.011)
b. Emotional Stability			
Born after the reform	0.021** (0.010)	0.030** (0.012)	0.023** (0.011)
c. Peer relations			
Born after Reform	0.003 (0.009)	-0.002 (0.011)	0.000 (0.008)
d. Social skills			
Born after Reform	0.002 (0.008)	0.003 (0.009)	0.001 (0.010)
e. Language Proficiency			
Born after the reform	0.048 (0.104)	0.000 (0.123)	- -
f. School Readiness			
Born after the reform	0.003 (0.020)	-0.000 (0.022)	-0.010 (0.016)
Panel C: Secondary School (Gymnasium)			
a. Baseline Sample			
Born after the reform	0.014 (0.032)	.020 (0.039)	0.038 (0.051)
b. Eligible Sample			
Born after the reform	0.081 (0.096)	0.033 (0.067)	0.065 (0.053)
Age & Birth month Dummies	yes	yes	yes
Child Controls	yes	yes	yes
Family Controls	yes	yes	yes

Sources: Micro Census Data 2009-2012 (for secondary school outcomes) and Administrative data/School entrance examination 2005-2007; own calculations

Note: Robust standard errors in parentheses; Child controls consist of children's gender and the presence of siblings. Family controls include single parenthood, parental education and a set of dummies indicating parents' country of origin; †p<0.15 *p<0.1; **p<0.05; ***p<0.01

Finally, the DiDiD produces the following results: The introduction of birthright citizenship leads to an increase in immigrant children's preschool

attendance by 3.8 ppt. Thus, given the initial level of preschool attendance among immigrant children and native children – 91.7 % and 95.0 %, respectively –, immigrant children are catching up with native children in terms of preschool attendance. The estimated effects on children’s emotional stability and conduct are also robust and amount to 2.1 ppt and 2.3 ppt, respectively. The robustness of language proficiency cannot be assessed in the DiDiD framework, as data on this issue are only available for immigrant children. And again, all remaining estimates are insignificant.

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. Identification is based on the introduction of birthright citizenship in Germany in 1999. Drawing on two large and comprehensive datasets – school entrance examinations for one German state and the German Micro Census – 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 early participation in the German educational system. In particular, gains are visible in terms of attending preschool (by 3.2 ppt). In addition, granting birthright citizenship fosters appropriate behavior in children (by 1.8 ppt) as well as contributes to their socio-emotional stability (by 2.1 ppt). Even though we do not observe that immigrant children born under the *ius solis* regime are better prepared for primary school, we find some positive evidence for the attendance of the academic track in secondary school at age 10.

We find that granting birthright citizenship is a policy that is successful in integrating immigrants into the host country’s society. The *ius solis* seems to encourage parental integration efforts (Sajons, 2012; Avitable et al., 2013) and allows second-generation immigrant children to gain earlier access to the host country’s educational system and improve their socio-emotional development, which could benefit the children in later years (Cunha et al., 2006). Despite these initial gains in children’s development, there is no evidence that immigrant children are better prepared to follow the curriculum taught in primary school. Finally, evidence in favor of some longer-lasting effect of switching to a *ius soli* regime on children’s educational outcomes – access

to the academic track in secondary school – is at best weak.

Taken together, our results point towards birthright citizenship being beneficial for immigrant children’s early integration into the host-country educational system, but are ambiguous whether birthright citizenship is a successful policy in reaching long-lasting improvements and thus, indeed being a door opening for immigrant children’s educational and thus, ultimately, professional success.

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A Appendix

Table A.1: Descriptives Statistics of Outcome Variables

	All	99/00	II/99	I/00	Diff	p-Value
Panel A: Preschool						
Preschool Attendance	0.935	0.930	0.917	0.942	0.025	(0.019)
Panel B: Primary School						
Behavior*	0.970	0.967	0.957	0.975	0.018	(0.086)
Emotional Stability*	0.961	0.957	0.938	0.974	0.036	(0.002)
Peer relations*	0.976	0.959	0.950	0.966	0.016	(0.209)
Social Skills*	0.982	0.974	0.967	0.980	0.013	(0.203)
German Proficiency*	0.600	0.614	0.606	0.619	0.013	(0.859)
School Readiness*	0.827	0.814	0.824	0.806	-0.018	(0.413)
Panel C: Secondary School						
Gymnasium Attendance - Baseline	0.266	.241	.274	.207	-.067	(0.016)
Gymnasium Attendance - Eligible	0.222	0.204	0.201	0.207	0.006	(0.91)

Sources: Administrative data/School entrance examination 2005-2007; German Microcensus 2009-2012 (for secondary school); own calculations

Note: The baseline sample (for the GMC) contains all children of migrant parents who were in Germany at the onset of primary school. The eligible sample contains only immigrant children who were born in Germany and whose parents had a minimum duration of residence of eight year and possessed of no German citizenship at the time of child birth. *These variable are net of children's age.

Table A.2: Descriptives Statistics of Background Characteristics

	All	99/00	II/99	I/00	Diff	p-Value
Panel A: SEE						
Age (in months)	73.815	73.335	76.396	70.724	-5.672	(0.000)
Female (d)	0.480	0.476	0.454	0.499	0.045	(0.197)
Siblings (excl. child)	1.233	1.271	1.287	1.257	-0.030	(0.561)
Mom: Single (d)	0.088	0.089	0.088	0.087	-0.001	(0.910)
Mom: Hauptschule (d)	0.222	0.228	0.220	0.235	0.014	(0.830)
Mom: Realschule (d)	0.250	0.240	0.240	0.240	0.000	(0.984)
Mom: Gymnasium (d)	0.162	0.162	0.154	0.169	0.015	(0.307)
Mom: Turkish (d)	0.300	0.311	0.306	0.315	0.009	(0.648)
Mom: East Europe(d)	0.398	0.400	0.405	0.395	-0.011	(0.600)
Mom: EU 15 (d)	0.051	0.048	0.040	0.055	0.015	(0.186)
Mom: Rest of the World (d)	0.248	0.247	0.244	0.231	-0.013	(0.446)
Dad: Hauptschule (d)	0.225	0.229	0.236	0.222	-0.014	(0.429)
Dad: Realschule (d)	0.234	0.235	0.224	0.245	0.021	(0.235)
Dad: Gymnasium (d)	0.172	0.169	0.164	0.171	0.007	(0.178)
Dad: Turkish (d)	0.304	0.314	0.311	0.316	0.005	(0.803)
Dad: East Europe(d)	0.385	0.384	0.393	0.376	-0.016	(0.410)
Dad: EU 15 (d)	0.050	0.047	0.042	0.051	0.009	(0.320)
Dad: Rest of the World(d)	0.260	0.247	0.247	0.248	0.001	(0.953)
Panel B: GMC - Baseline						
Female	0.49	0.483	0.469	0.498	0.029	(0.191)
Age (in Quarter)	43.043	42.042	43.064	40.996	-2.068	(0.00)
Siblings	1.395	1.423	1.454	1.391	-0.063	(0.23)
Single Parents	0.18	0.166	0.168	0.165	-0.003	(0.458)
Mom: Age	38.464	38.217	38.359	38.072	-0.287	(0.225)
Mom: Hauptschule	0.158	0.328	0.327	0.328	0.001	(0.48)
Mom: Realschule	0.327	0.269	0.257	0.28	0.024	(0.209)
Mom: Gymnasium	0.18	0.133	0.161	0.104	-0.057	(0.005)
Mom: Turkish	0.247	0.241	0.234	0.248	0.014	(0.305)
Mom: Russian	0.111	0.113	0.115	0.111	-0.004	(0.427)
Mom: EU12	0.115	0.102	0.11	0.093	-0.017	(0.197)
Mom: EU15	0.056	0.064	0.062	0.067	0.006	(0.359)
Dad: Age	41.998	41.694	41.878	41.506	-0.372	(0.208)
Dad: Hauptschule	0.326	0.345	0.348	0.341	-0.007	(0.412)
Dad: Realschule	0.182	0.197	0.176	0.217	0.041	(0.056)
Dad: Gymnasium	0.128	0.117	0.132	0.102	-0.029	(0.08)
Dad: Turkish	0.221	0.216	0.204	0.228	0.024	(0.182)
Dad: Russian	0.094	0.095	0.085	0.104	0.019	(0.155)
Dad: EU12	0.078	0.063	0.07	0.057	-0.014	(0.198)
Dad: EU15	0.047	0.059	0.062	0.057	-0.005	(0.372)

Sources: Administrative data/School entrance examination 2005-2007; German Microcensus 2009-2012; own calculations

Note: The baseline sample (for the GMC) contains all children of migrant parents who were in Germany at the onset of primary school.

Table A.3: Descriptives Statistics - Eligible Sample

	All	99/00	II/99	I/00	Diff	p-Value
Female	0.459	0.471	0.453	0.487	0.033	(0.571)
Age (in Quarter)	42.996	42.076	43.194	41.04	-2.154	(0.000)
Siblings	1.431	1.412	1.475	1.353	-0.121	(0.389)
Single Parents	0.14	0.149	0.137	0.16	0.023	(0.579)
Mom: Age	39.472	39.207	39.438	38.993	-0.445	(0.493)
Mom: Hauptschule	0.359	0.349	0.353	0.347	-0.006	(0.917)
Mom: Realschule	0.18	0.197	0.245	0.153	-0.091	(0.051)
Mom: Gymnasium	0.11	0.107	0.108	0.107	-0.001	(0.973)
Mom: Turkish	0.267	0.225	0.223	0.227	0.004	(0.941)
Mom: Russian	0.014	0.003	0.007	0	-0.007	(0.299)
Mom: EU12	0.08	0.073	0.072	0.073	0.001	(0.964)
Mom: EU15	0.092	0.104	0.101	0.107	0.006	(0.869)
Dad: Age	43.076	42.668	43.434	41.938	-1.497	(0.06)
Dad: Hauptschule	0.403	0.394	0.388	0.4	0.012	(0.842)
Dad: Realschule	0.124	0.131	0.137	0.127	-0.01	(0.802)
Dad: Gymnasium	0.092	0.107	0.122	0.093	-0.029	(0.428)
Dad: Turkish	0.248	0.215	0.216	0.213	-0.002	(0.959)
Dad: Russian	0.014	0.003	0.007	0	-0.007	(0.30)
Dad: EU12	0.056	0.038	0.043	0.033	-0.01	(0.664)
Dad: EU15	0.082	0.111	0.115	0.107	-0.008	(0.82)

Sources: Administrative data/School entrance examination 2005-2007; German Microcensus 2009-2012 (for secondary school); own calculations

Note: The eligible sample contains only immigrant children who were born in Germany and whose parents had a minimum duration of residence of eight year and possessed of no German citizenship at the time of child birth.