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Does a Passport Get You a Degree? Citizenship Reform and Educational Achievement

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Does a Passport Get You a Degree? Citizenship Reform and Educational Achievement

Abstract

This paper examines the impact of introducing birthright citizenship in Germany on the educational trajectories of second-generation immigrants. Our identification strategy exploits a legal change in 2000 that granted children of foreigners with longtime residency automatic citizenship at birth. Using high-quality census data, we show that the reform contributes to closing pre-existing educational gaps in secondary school track choice and completion. These findings also hold when relying exclusively on within-household variation across siblings. We provide evidence for the underlying mechanisms, highlighting the roles of higher expected returns to education and of an increased sense of belonging to Germany.

JEL-Codes: J150, J240, K370, O150.

Keywords: birthright citizenship, education, human capital, integration, immigration.

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1 Introduction

Obtaining citizenship is often considered a crucial step in the lives of second-generation immigrants, confirming a long-term residence perspective as well as integration into and identification with the host society. Naturalization entails important citizenship rights, such as being eligible to vote and run for public office. It can also open up new possibilities in the labor market, such as access to occupations in the civil service or jobs with extensive travel requirements. However, naturalization usually comes with a host of administrative and personal hurdles. In Germany, the focal country of this paper, these hurdles range from a lengthy application process to the need to give up one's original nationality. Confronted with these high costs, even eligible individuals often refrain from acquiring the citizenship of their country of residence, even if this means that they forgo crucial rights and remain in a more marginalized position in society (Gathmann and Garbers, 2023).

When Germany first adopted a limited birthright citizenship system in 2000, these hurdles were suddenly removed for children of longtime immigrants. Since then, each year, around 40,000 additional children automatically acquire German nationality at birth. We study how this legal change affects the lives of these individuals throughout childhood and early adulthood, focusing on their educational achievements, which condition their subsequent transition into the labor market. Methodologically, we rely on a difference-in-differences strategy to compare the outcomes of eligible second-generation immigrants born shortly before and after the reform with those of children born to German nationals.

Drawing on high-quality survey data from the German Microcensus, the German Socioeconomic Panel (SOEP), and the National Educational Panel Study (NEPS), we find persistent improvements in several dimensions. We find that children who attain citizenship at birth feel more integrated in Germany, improve their German skills, and complete more academically-oriented school tracks. We stress this last point, as it is decisive for children's future careers and their access to the tertiary education system: While children born to foreigners are initially 16.1 percentage points less likely than their native peers to complete the *Gymnasium*, i.e., the most academic school type in Germany, we find that the reform reduces the size of this gap by 40 percent. This key finding is robust to introducing a wide range of control variables as well as narrowing down the control group to children with one German and one foreign parent. Additionally, by estimating the within-family treatment effect for pairs of siblings born before

and after the reform, we show that our effects is not driven by (unobservable) characteristics at the household level or changes in the composition of the treatment group over time.

Overall, our findings align with the notion that removing obstacles toward unconditional permanent residency and full labor market integration increases the returns to country-specific human capital and, therefore, the incentive to invest in it. We examine survey questions on attitudes and behaviors to investigate the mechanisms underlying our results. We show that, besides the canonical returns-to-human-capital channel, increased identification with and integration into German society play a crucial role in increasing educational success, for example, by improving language skills through increased interaction with natives. Finally, we also document suggestive evidence that the effect is particularly pronounced for children whose parents are non-academics and have resided in Germany for a long time period.

The economic effects of attaining citizenship have been studied broadly in the literature. The dimensions that are studied include wages and wage growth (Bratsberg et al., 2002; Chiswick, 1978; Hainmueller et al., 2019; Steinhardt, 2012) as well as labor force attachment (Gathmann and Keller, 2018; Govind, 2021). Questions of social integration and cultural norms, for example, regarding marriage decisions and fertility choice, have also been examined (Avitabile et al., 2014; Gathmann et al., 2019). In the sociological and political science literature, the role of naturalization as a catalyst versus an endpoint of integration has been discussed extensively. Reichel (2011) views citizenship as a milestone and catalyst for integration in Austria. In Germany, Fick (2016) confirms a positive effect of receiving citizenship on national identification and a tentative improvement in reconciling several national identities. This said, Worbs (2014) argues that the effect of naturalization in Germany is only small, given that it typically occurs rather late in the integration process. In line with this rationale, Hainmueller et al. (2017) find an improvement in long-term integration spurred by earlier naturalization in Switzerland.

Most of the existing literature focuses on individuals who actively choose to attain citizenship in adulthood, which introduces concerns about selection bias and reverse causality. We circumvent this problem by focusing on the quasi-experimental variation in birthright citizenship rights of second-generation immigrants generated by a reform in Germany enacted in 1999 and put into force in 2000 ("Staatsangehörigkeitsgesetz", Deutscher Bundestag, 1999). Previous research has made use of this setting, focusing primarily on early-life outcomes and parental behavior.

Avitabile et al. (2014) show that in response to the reform, immigrant families reduce their fertility but increase their early-life investment in children, consistent with Becker's (1973) "quality-quantity" theory of fertility. They show that children's health and socio-emotional outcomes improve as a consequence of these adjustments. Using school records from the state of Schleswig-Holstein, Felfe et al. (2020) find that participation in preschool, which is non-compulsory in Germany, increases for children that are potentially affected by the citizenship reform. They also enter school at a younger age and are more likely to enroll in the academic track in high school. Dahl et al. (2022) conduct a survey with school children in two federal states and find that (particularly Muslim) girls report lower scores for life satisfaction and measures of integration, whereas boys tend to benefit from the reform. The parents of children who gain access to citizenship are also affected: they are more likely to interact with the local community and use the German language (Avitabile et al., 2013) and less likely to out-migrate (Sajons, 2016). Mothers are less likely to be employed (with fathers being unaffected), possibly because they take on a more long-term perspective and prefer to invest more time into the human capital development of their children instead of trying to maximize earnings before a possible forced return to their country of origin (Sajons, 2019).

In summary, the existing literature is concerned primarily with early-life outcomes of children affected by the German reform and the general effects of naturalization later in life (see Gathmann and Garbers (2023) for a comprehensive summary). As new data becomes available, the reform of naturalization law in Germany opens up the opportunity to study how citizenship matters for the crucial transition from childhood to adulthood in a setting that allows for establishing clear causal links.

We add to the literature in three ways. First, we re-evaluate the existing findings on how the German birthright citizenship reform affected school-related outcomes by relying on three nationally representative datasets and a more precise identification of eligible individuals. While previous research on schooling outcomes was based on data from one state only and had to rely on proxies for eligibility, such as first language, we link parent and child surveys to precisely assign individuals to the treatment and control group based on their parents' nationality and migration history. Our main results are consistent across all three datasets, confirming that they are not driven by the particularities of a specific source of information.

Second, we extend the time horizon of the analysis and determine whether the gains from early childhood translate into the completion of school-leaving degrees. The segmented

nature of the German school system facilitates studying this question, given its step-wise achievement structure and different levels of requirements for the different school-leaving degrees. Considering outcomes that go beyond medium-term measures of educational success is crucial to determine whether the impacts of birthright citizenship carry over into the labor market and have the potential to lead to higher employment and wages, affecting measures of individual welfare and intergenerational mobility.

Lastly, we exploit the detailed survey modules from the SOEP and the NEPS to shed light on the mechanisms through which citizenship affects educational success. We show that measures of performance and intentions to pursue tertiary education align with the hypotheses and evidence put forward in the literature regarding an increased return to (country-specific) human capital investments. Additionally, we document increased German language skills and more frequent use of the language with peers, which are an important mediator for educational success. We then discuss how the automatic and, thus, barrier-free acquisition of citizenship may have increased the sense of belonging to the host society, creating another channel through which the reform increased educational achievement.

Understanding the effects to citizenship reforms is crucial for a sensible design of citizenship law and for increasing acceptance of immigration policies. In Germany, where 11.8 out of its 84 million inhabitants do not have citizenship, and 22 million have a migratory background, there is still much scope to implement further reforms to foster integration. In January 2024, a set of new citizenship laws was passed, with consequences also for access to birthright citizenship (Bundesministerium des Innern und für Heimat, 2023; Deutsche Bundesregierung, 2024). Given the salience of both migration and integration in the political debate, further reforms are to be expected not only in Germany but worldwide. In Colombia, for example, birthright citizenship has been granted to children born to Venezuelan refugee parents since 2019, primarily to avoid issues relating to statelessness for the affected children. Similar to our findings, Rude (2023) shows that the reform increased educational outcomes for descendants of Venezuelans. In Europe, Portugal has adopted and extended provisions for birthright citizenship in recent years (Erdilmen and Honohan, 2020), and Italy is currently debating whether to do so (Testore, 2023). In all these contexts, policymakers can benefit from gaining insights into the effects of the German reform to gauge whether moving forward with a more liberal citizenship policy is an effective tool for promoting the longer-term integration of immigrants.

The remainder of the paper is structured as follows. In Section 2, we provide details on

the German reform and how it affected eligible children. We also outline the basic structure of the German educational system, which represents the context in which the educational outcomes that we analyze are realized. Sections 3 and 4 describe the empirical strategy and the data that we use for our analysis. Our findings are then presented in Section 5. Our main results stem from analyses based on Microcensus data, which corresponds to the largest available sample and is nationally representative. We then add to these findings with evidence obtained by analyzing SOEP and NEPS data. Both of these surveys contain substantially more detailed information on the socio-emotional outcomes and educational performances of respondents, allowing us to gain insights into the mechanisms at play. We test the robustness of our results in Section 6 before concluding in Section 7.

2 Institutional Background

2.1 Immigration Policies in Germany

While Germany has a long history of in and outflows of people, the country has only recently begun to publicly acknowledge its status as a society shaped by immigration ("Einwanderungsgesellschaft"). The first big wave of immigration occurred during the 1950s and 1960s when the surging post-war economy gained steam and began facing a shortage of workers. At that time, the German government decided to systematically attract so-called "guest workers," primarily via labor recruitment agreements with Mediterranean countries, such as Italy or Turkey. In East Germany, similar agreements were formed in the 1960s and 1970s with other socialist countries such as Poland or Vietnam. While these guest workers were initially only meant to temporarily fill the labor supply gap, it soon became apparent that many would become long-term residents. The number of these immigrants was substantial: West Germany alone recruited over 14 million guest workers during the post-war period, of whom 3 million stayed permanently. Despite their contribution to the German economy, the immigrants of this period saw themselves confronted with highly limited access to citizenship and other forms of civil integration.

An entirely different set of rules applied to immigrants of German origin who relocated to Germany after the collapse of the Soviet block. These ethnic Germans (so-called "Spätaussiedler") and their descendants enjoyed privileged access to residence permits and naturalization. A total of 4.5 million immigrants entered Germany using these privileges

between 1950 and 2020. This said, a particularly large wave of naturalization, comprising almost 400,000 "Spätaussiedler" in just two years, took place right after the fall of the Berlin wall (Bundeszentrale für politische Bildung, 2022). Perhaps unsurprisingly, the first major reform of the German citizenship law took place in 1991, shortly after this decisive event. The law provided that foreigners who had legally resided in Germany for at least 15 years (8 years for adolescents) could attain citizenship if they fulfilled certain requirements, such as economic self-sufficiency and a clean criminal record. Upon naturalization, they were required to give up their original citizenship. However, only few immigrants made use of this rule: between 1990 and 2000, an average of merely 231,616 migrants naturalized each year. The fact that most of these naturalizations were filed with references to special rules for spouses or ethnic Germans points toward the requirement of giving up one's original citizenship being perceived as rather high.

Until this point, Germany made no distinction between first and second-generation immigrants. Instead, the country remained firmly tied to the principle of *ius sanguinis*, the "law of the blood" – a legal principle that postulates that newborn children receive the nationality of their parents. In 1999, the governing coalition of Social Democrats and Green Party passed the second major citizenship reform, which challenged this longstanding principle: the reform introduced elements of *ius soli* – the "law of the land," as it is common in the Anglosaxon countries – into the German citizenship legislation. In its purest form, this principle postulates that newborn children automatically receive the nationality of their birth country.

While debates about citizenship rules had been ongoing for some time, the new reform itself was enacted swiftly after a sudden change in government. After being discussed in parliament for the first time in May 1999, it was officially passed into law on July 15, 1999, and took effect on January 1, 2000. Since only seven months passed between the official debate and the onset of the reform, any anticipatory behavioral responses, such as postponing births until after the cutoff, were hardly possible.

For all children born in Germany from January 1, 2000, onwards, the new law stated that if at least one of their parents fulfilled a minimum legal residency requirement of eight years at the time of birth, the child would automatically receive German citizenship. This reform was large in scale: since 2000, it has enabled around 40,000 additional newborn children per year to receive German citizenship (Deutscher Bundestag, 2011).

An important element of the reform was that children could initially keep any additional

nationalities inherited from their parents. The so-called “option requirement” stated that they had time until their 23rd birthday to choose between their multiple passports.³ The vast majority chose to keep their German nationality and relinquish the citizenship of their parents (Deutscher Bundestag, 2013; Worbs, 2014). This requirement was eventually abolished in 2014, allowing children to permanently hold two passports provided that they demonstrate strong ties to Germany. Therefore, most children affected by the reform ultimately did not have to choose between their nationalities. Until 2014, however, their behavior (and that of their parents) may still have been influenced by the anticipation of this choice.

It should be noted that for children born from 1990 to 1999, a transitional provision applied: parents who had fulfilled the residency requirement at the time of birth could apply for immediate citizenship for their children until December 31, 2000. However, this provision was sparsely used and affected only around 49,000 children in total, i.e., between 7 and 18 percent of all eligible children per birth cohort (Deutscher Bundestag, 2011; Statistisches Bundesamt, 2023).

Another crucial element of the 2000 reform package concerned not children but their parents: for adult immigrants, the minimum residency requirement for naturalization was decreased from 15 to eight years, while additional conditions (such as a formal language test and an explicit commitment to the German constitution) were introduced. This element of the reform did not lead to an immediate change in status and implied mainly a change in eligibility, which explains the modest findings for affected adults in the literature.⁴ That said, families may be affected by both elements of the reform package at the same time when the parents have resided in Germany for at least eight but less than 15 years at the time of birth of their child. However, compared to the element of birthright citizenship, the shortened waiting times for naturalization are likely less influential for our outcomes of interest due to the indirect nature of the effect only via the parents and given the very low rates of adult naturalization in Germany.⁵

The next major overhaul of citizenship law was passed in February 2024 and took effect in June 2024. The comprehensive reform package intends to modernize the German

³*Ius soli* children that, in addition to German citizenship, held the citizenship of another EU country or Switzerland were not subject to the option requirement and could apply to keep both passports. Children from countries where it is impossible to relinquish citizenship could also apply for an exemption.

⁴This reform has, for example, been used by Gathmann and Keller (2018) to study employment and earnings, by Gathmann et al. (2019) to study family formation, and by Zillesen (2022) to study the savings behavior of migrants.

⁵We verify this intuition empirically in Section 5.2.

citizenship law to reflect the country's nature as an immigration society (Bundesministerium des Innern und für Heimat, 2023). The reform reduced the waiting period before naturalization from eight to five years, with a reduction to three years possible for exceptionally well-integrated individuals. More importantly for this paper, the parental residency requirement for birthright citizenship was also lowered to five years, and all remaining restrictions on the option requirement were abolished. Based on the distribution of residency times around 2000, this would allow around 700 additional children per year to obtain German nationality automatically at birth – an increase of roughly 1.7 percent. Given the composition of immigrants in recent years, the reform's impact is, however, expected to be much larger: while the birth cohorts around 2000 were dominated by children of guest workers with over eight years of residency, more recent generations of immigrants that arrived during and after the large inflow of migrants around 2015, as well as those fleeing from the war in Ukraine, will fall into the shortened residency window during the coming years. Given the increasing number of individuals eligible for birthright citizenship, it becomes ever more important to understand the effects of this policy on the children of migrants.

2.2 Remarks on the 2000 Reform of Birthright Citizenship

We treat the German citizenship reform in 2000 as a quasi-experiment that allows us to establish causality regarding the effects of attaining citizenship at birth on education and integration without having to be concerned about issues of selection into citizenship. However, to interpret the results correctly, it is essential to understand the concrete changes that the reform implied for second-generation immigrants. Importantly, even before the reform, there were several avenues to citizenship for the children of non-German parents, which were commonly known and relatively accessible. In particular, for all those born in Germany and attending the local school system, there existed a possibility of acquiring citizenship through a facilitated process starting at age 16, i.e., shortly before reaching voting age and entering the labor market. Later in life, these individuals could still apply for citizenship through the regular procedures, with high chances of being granted citizenship. This said, there are three key differences that make obtaining citizenship automatically upon birth substantially different from acquiring it through an application later in life.

Firstly, the fact that citizenship is attained upon birth increases its cumulative benefits due to the longer time horizon in which benefits may accrue. Importantly, it allows for adjustments

in parental behavior and institutional acceptance during early childhood, which is crucial for children's educational success and developmental outcomes (Cunha and Heckman, 2007; Rude, 2023).⁶ Building on the view of education as a cumulative and self-reinforcing process, this implies that children who attained citizenship earlier will have an advantage in their educational career (and later in the labor market) over peers who were naturalized later in life.

Secondly, the reform removed the uncertainty about whether one would ever become a citizen and attain all the rights that came with it. In particular, citizenship entails a full and permanent residency right. Perceived uncertainty over one's residency options is an important factor in the life planning of immigrants, even in a setting where the probability of expulsion is objectively low, as in Germany.⁷ Because parents and children adjust their behavior based on the perceived and not necessarily the actual likelihood of staying, resolving this uncertainty can matter more for behavior than the actual numbers on loss of residency rights would suggest.

For the second-generation immigrants themselves, obtaining citizenship also allows them to make long-term plans independent of their parents' decision to return to their home country. Avitabile et al. (2014) report that over 60 percent of immigrants in Germany plan on returning to their home country, and 45 percent plan on doing so within the next eight years. Before 2000, if a family left Germany prior to a child's sixteenth birthday, this would make a future application for German citizenship considerably more complicated, even if the child or the entire family decided to return later on. From a child's perspective, the birthright citizenship reform thus makes future plans to live or work in Germany independent of the location choices of their parents.

Lastly, the reform abolished the cumbersome and costly naturalization process that applicants typically need to go through to obtain citizenship. The 2011 Integration Study by the Federal Office for Migration and Refugees reports an average waiting time of 14 months and costs of 500 Euro, which many potential candidates for naturalization cite as a reason not to apply for citizenship (Weinmann et al., 2012).⁸ In addition, the reform enabled

⁶In the US, Currie and Thomas (1999) show that participating in the Head Start preschool program can help substantially to close later-life gaps in test scores and grade transitions between Hispanic and non-Hispanic white children.

⁷In qualitative interviews, Zillesen (2022) finds that immigrants in Germany tend to hold off on long-term investments when they are not certain where they will live in the future. About half of the interviewees mentioned not having a permanent right to stay or access to citizenship as a key reason for this uncertainty.

⁸Evidence from the Netherlands (Peters et al., 2023) and the United States (Yasenov et al., 2019) also suggests that application fees are an important deterrent to naturalization. By 2023, the waiting times for naturalization in Germany seem to have rather increased (Harder et al., 2023).

children to keep both their parents' and the German nationalities at least until turning 23. Having to give up one's previous citizenship is a major deterrent for many candidates for naturalization (Mazzolari, 2009; Peters and Vink, 2024; Weinmann et al., 2012) and can lead to feelings of conflict for individuals who are forced to make a decision (Reichel, 2011; Worbs, 2014). Being granted citizenship unconditionally resolves this conflict at least temporarily and thus allows for an easier reconciliation of national identities that exist in parallel.

In summary, while the 2000 birthright citizenship reform did not strongly alter the *de facto* legal chances of second-generation immigrants to obtain citizenship, the three aspects discussed above may be decisive for the observed behavioral responses to the reform. Throughout our analysis, we hypothesize that two channels are particularly important to children's behavioral responses: the *economic returns channel* and the *identity channel*. While we cannot precisely disentangle these, we find evidence that both play an important role in explaining the effects of the reform.

The *economic returns channel* is based on the idea that birthright citizenship resolves the uncertainty about having full access to German labor and capital markets in the long run. Regarding the labor market, citizenship opens up avenues to jobs that were previously inaccessible or difficult to obtain, such as jobs as civil servants or those that require extensive international business travel.⁹ Experiences of discrimination that hinder being hired or promoted – for example, as a response to the uncertainty over the prospective employee's residence perspective or in expectation of higher administrative efforts – are also likely to be reduced. Removing all these barriers can increase the match quality of employer-employee pairs and lead to higher levels of job satisfaction and income. More importantly, however, compared to most countries of origin outside the EU, the local labor market itself is characterized by substantially higher shares of academically oriented jobs. Given the prospect of joining the German labor market, citizenship can thus incentivize investments into education in general and country-specific knowledge, such as German language skills or studies in the legal field, in particular.

What we term the *identity channel* refers to how access to citizenship can affect the identification of second-generation immigrants with their host country. Granting citizenship at birth provides a clear signal of acceptance from the host country and gives the descendants of migrants the same rights and obligations as their peers with entirely German ancestry.

⁹The existing literature on immigrants' labor market outcomes highlights restricted access to certain occupations as a key driver of the citizenship wage premium (see e.g. Bratsberg et al. (2002) for the United States and Steinhardt (2012) for Germany).

This signal can lead to an increased identification with the host country, facilitate the societal integration process, and delay or resolve any dissonance that arises from having to choose one national identity over another. As children feel more accepted by society, they are also more likely to interact with natives, feel comfortable with the native language, and remain in the country in the long run. Even without the explicit intention to improve future earnings, the increased feeling of belonging and interactions with ethnic Germans may favor acquiring country-specific skills, such as language and cultural awareness. It may also improve children's performance in school, interest in civil matters (such as politics and voting decisions), and financial decisions favoring an indefinite stay in Germany.

Finally, it should be mentioned that parents also play an important role in determining the outcomes of their children. Previous research has documented increased parental time investments in children affected by the reform and linked this to Becker's (1973) "quality-quantity" theory of fertility (Avitabile et al., 2014). The authors interpret the reform as "a positive shock to the returns to investment in child human capital," which triggers a forward-looking reaction of parents to promote the latter. The knowledge that their child can stay in Germany forever with certainty and that they themselves are allowed to stay there at least until the child's eighteenth birthday also increases parents' incentive to learn German, connect with the local society (Avitabile et al., 2013), make long-term financial investments (Zillessen, 2022) and acquire skills specific to the German labor market. All these factors may affect children indirectly, as their parents can help them to navigate life in Germany better and support them in their educational path.

2.3 The German Educational System

In what follows we briefly outline the key characteristics of the German educational system, which is crucial to contextualize many of our outcome variables. We note that this system is rather dynamic and – given that educational policy is the responsibility of the federal states – there are not only significant differences over time but also across regions.

In Germany, children generally enter primary school in the fall after their sixth birthday. However, school enrollment depends not only on children's age but also on the assessments of their parents and preschool teachers, who judge their preparedness for entering school and can postpone or accelerate the initial enrollment. After completing primary school, which typically takes four years, children continue their education in a specific secondary

school track, which can be more or less academically oriented.¹⁰ The choice of school track is based on the recommendation of a child's primary school teachers but can, depending on the state, be influenced or even overruled by the parents' assessment.¹¹ The track choice reflects the cognitive abilities of each child (as demonstrated by their success in primary school), the perception of the teacher (incorporated in the recommendation), and the preferences and ambitions of parents and children regarding the type of education that the child should ultimately obtain. It should be noted here that the perceptions of the teacher may directly be influenced by the reform if the teacher is informed about a child's citizenship and consciously or subconsciously treats children with different nationalities differently. Consequently, any shifts in track enrolment that we observe may be driven not only by different ambitions or abilities of children at the time of the choice but also by changes in teachers' perceptions.

The various school tracks differ starkly concerning their objectives and duration. The *Hauptschule* is a school type that runs until grade 9 and is typically followed by vocational education.¹² It represents the basic track of the school system and opens pathways for jobs that require no specific professional education or only basic vocational training. The *Realschule* runs up to grade 10 and allows for entry into vocational training or an additional 3-year school that offers a pathway toward university education. Lastly, the *Gymnasium* is the most academically oriented track and leads to a degree qualifying individuals for university entrance, which is called *Abitur* (German A-levels). This school track runs to grade 12 or 13, depending on the specific school and state. Switching across these three secondary school tracks is generally possible when supported by the school administration.

Besides these three traditional tracks, the (*Integrierte*) *Gesamtschule*, resembling comprehensive schools in the UK, is an increasingly popular school type.¹³ In these schools, the differentiation between tracks occurs *within the school* itself, allowing children of various academic abilities or ambitions to interact and switch across tracks more flexibly. In practice, most children in these schools complete the *Realschule* track and then decide whether or not to move

¹⁰In Berlin and Brandenburg, primary school lasts six years, which implies that the secondary school track choice is only made after the 6th grade.

¹¹The recommendation is binding in Bayern, Brandenburg, Sachsen, Sachsen-Anhalt, and Thüringen and non-binding in all other states. In Baden-Württemberg and Saarland, the recommendation was binding until 2012 and 2010, respectively.

¹²Statistics by Federal Statistical Office (Statistisches Bundesamt, 2018) show that several federal governments abolished this school type since 2009, while other states abolished or reformed this school type significantly.

¹³The documentation provided by the Federal Statistical Office shows that many schools which were originally operated as a *Hauptschule* or *Realschule* are being reformed to a comprehensive school, or *Gesamtschule*. The share of students in lower secondary school attending the *Gesamtschule* increased from 8.7 to 18.3 percent between 2006 and 2016 Statistisches Bundesamt (2018).

on to completing the *Gymnasium* degree. Depending on the school, they can stay in the same institution or move to a different one for the final two years. In this paper, whenever we are interested in children's *current school track*, we pool *Gesamtschule* and *Realschule* given the school-leaving degree typically associated with these schools and to avoid working with too many categories. That said, whenever we consider *school leaving degrees* as our outcome variable, it is possible to map graduates from a *Gesamtschule* more precisely to the actual degree they obtained, which is associated either with the *Hauptschule*, *Realschule*, or *Gymnasium* school track.

Finally, it should be noted that certain schools in Germany do not neatly fit into any of the four main school types. Among these are the so-called *Förderschulen* ("support schools"), which are meant to facilitate students' pathways towards obtaining a *Hauptschule*-degree or vocational training in the presence of special student needs, such as a physical or learning disability. Given their low representation in the data (less than 3 percent) and highly individualized character, we are not considering less represented school types in the analysis.

3 Methodology

To measure the effects of the 2000 reform, we make use of its unique timing and eligibility structure. In a simple difference-in-differences setup, we compare the outcomes of affected versus unaffected children, distinguishing between cohorts born before versus after 2000. We assign children to a treatment and a control group based on their parents' nationality and compliance with residency requirements. The treatment group consists of children whose parents were not German citizens at the time of giving birth but who fulfilled the residence requirements necessary for the new citizenship rule to apply to their child. More specifically, at least one of the foreign parents must have lived in Germany for at least eight years. The control group consists of children who have at least one German parent and thus automatically attained German citizenship at birth. From a policy perspective, this group of German nationals represents the relevant control group: assuming that one would want to create equitable educational opportunities that are as independent as possible from children's parental background, one should want to achieve a full closing of the pre-existing educational achievement gap between the descendants of immigrants and the children of German nationals.¹⁴ Our main differences-in-differences estimator compares the outcomes

¹⁴Theoretically, children of foreign parents who did not yet fulfill eight-year residency requirement at the time of birth would also represent a suitable control group. However, parents' length of residency is only available with sufficient precision for a small subset of individuals in the data. Therefore, we do not rely

for the treatment and control group across the different birth cohorts, with the individuals born before 2000 constituting the pre-reform cohorts and those born in or after 2000 the post-reform cohorts. The empirical strategy is summarized in equation (1), with subscript i indexing individuals, c indexing birth cohorts, and t indexing survey years.

$$Y_{itc} = \alpha \cdot Treated_i + \beta \cdot Post_c \times Treated_i + \mu_c + \gamma \cdot X'_{itc} + \epsilon_{itc} \quad (1)$$

Y_{itc} denotes the outcome of interest, such as whether an individual is enrolled in a certain school track or holds a certain degree. $Treated_i$ is an indicator for the treatment group, such that α captures the pre-reform conditional outcome gap between children with German versus non-German parents. $Post_c$ is a dummy variable that takes the value of one when a child was born in or after 2000, i.e., after the enactment of the reform. Accordingly, β captures how children who became eligible for citizenship through the reform evolve differently compared to the group that has always been eligible for citizenship. This coefficient can be interpreted as a measure of how much the outcome gap between natives and second-generation immigrants is closed due to conferring citizenship at birth. The coefficient μ_c represents birth cohort fixed effects, which replace the $Post$ dummy commonly used in a difference-in-differences setting. This allows us to account more comprehensively for variation across birth cohorts, e.g., changes induced by general educational policies or societal circumstances affecting all children equally.¹⁵ The vector X_{itc} contains individual-specific control variables, such as gender and age at the time of the survey. While these controls are not necessary for the identification strategy to work, they help in increasing the precision of the estimates and in addressing concerns related to changes in the sample composition across birth cohorts.¹⁶ Finally, ϵ_{itc} represents the idiosyncratic error term. We estimate the above model by ordinary least squares using robust standard errors. This said, whenever individuals appear more than once in our data, we account for these multiple and potentially correlated observations by clustering standard errors at the individual level.

on these individuals as a control group and rather exclude them from our analysis. In a robustness check, we also narrow the control group to children with only one German parent, who are more similar to children in the treatment group regarding their characteristics in the data.

¹⁵We assign children to birth cohorts based on their birth years for analyses using SOEP or NEPS data and based on their birth semesters when using the substantially larger Microcensus data.

¹⁶In a robustness check, we also control for parental education, household income and place of residence. For the NEPS results, we also use school-level fixed effects to account for unobserved variation at the school level and reduce the sampling procedure's impact on our outcomes. The respective table footnotes list the specific controls used in each regression.

4 Data and Sample Definition

Our empirical analysis is based on three data sources. The first and main source is the Microcensus of the German Federal Statistical Office. We describe the preparation of this dataset in detail below. We also use two additional datasets. The German Socioeconomic Panel (SOEP) is a representative longitudinal study that annually surveys around 12,000 German households and that allows us to identify second-generation immigrants with an even higher level of precision than in the Microcensus.¹⁷

The National Educational Panel Study (NEPS) offers additional information on education-related outcomes conditional on attending a specific school or school type in Germany.¹⁸

Although the sample sizes of the latter two data sets are considerably smaller than that of the Microcensus, they allow us to gain more nuanced insights into the reform's effects. The data preparation for the SOEP and NEPS closely follows that of the Microcensus. A detailed description as well as summary statistics can be found in Online Appendices A.2 and A.3.

The central dataset used in our analysis is the German Microcensus, which covers a one percent random sample of the German population each year.¹⁹ For each household member aged 15 and older, key characteristics are collected, including education outcomes, labor market status, and earnings. The dataset also contains information on the migration history, nationality and naturalization events of respondents.²⁰ We rely on the survey waves 2014–2021, meaning we have overlapping self-reported information for children born both before and after the 2000 cutoff for outcomes collected between the ages of 15 and 21. For our main analyses, we focus attention on observations for children of these age groups born in Germany during a five-year window around the reform, i.e., the 1995–2004 cohorts.

We go through several steps to construct our final sample and obtain well-defined treat-

¹⁷The SOEP data is provided by the German Institute for Economic Research (DIW). We use version 38 covering the years 1984–2021, available under the DOI 10.5684/soep.core.v38eu. For details on the dataset see Goebel et al. (2019).

¹⁸The NEPS is a longitudinal panel provided by the Leibniz Institute for Educational Trajectories (LifBi). (Blossfeld et al., 2011; NEPS-Netzwerk, 2022). We rely on Starting Cohort 3, which consists of children who were enrolled in the fifth grade of the German school system in school year 2010/11, and covers the years 2010/11–2020/21.

¹⁹Data is available under the DOIs 10.21242/12211.2014.00.00.1.1.1 to 10.21242/12211.2021.00.00.1.1.1 . The research data center of the Federal Statistical Office and Statistical Offices of the Federal States of Germany provided us access to the data through guest research visits.

²⁰For younger individuals, information is more limited and is sometimes reported by another household member.

ment and control groups. First, we limit the sample to individuals born in Germany for which we have complete information for key variables, such as gender, age, education, and disability status.²¹ Secondly, we consider the eligibility requirements of the birthright citizenship reform to classify children into a control and a treatment group. Most fundamentally, we distinguish between children born to non-German nationals (treatment group) and children for which at least one parent had German citizenship at the time of birth (control group). In addition, the citizenship reform only applied to children of non-German parents if at least one of these parents had legally resided in Germany for at least eight years at the time of birth. Unfortunately, this information is only available for individuals who are living in the same household as their parents. To avoid working exclusively with this subsample, which is prone to selection issues as children age, we make the general assumption that children of foreigners are eligible for the treatment. This is true for around 80 percent of the individuals for which we do have full information.²² Whenever we clearly observe that a child is *not eligible*, we remove it from the sample.

Since 2005, the Microcensus contains a highly granular variable on individuals' migration background. It classifies individuals into 39 categories based on their own nationality, the nationalities of their parents, their place of birth, and the naturalization options applicable to them. Importantly, this variable also contains a specific identifier for individuals who obtained citizenship through the *ius soli* rule, which we use to cross-check and fill in missing information in our group assignment variable. Furthermore, we use it to calculate how many children born to non-German parents before 2000 obtained citizenship under the transitional ruling. In our sample, this affects 14.7 percent of eligible second-generation immigrants born in the ten years before 2000, which is in line with the number reported by the official statistics (Deutscher Bundestag, 2011; Statistisches Bundesamt, 2023). We exclude these individuals from our sample.

We continue by providing some descriptive statistics for our final Microcensus sample. Table 1 presents the total number of observations for the treatment and control groups across cohorts. The share of observations by birth cohort behaves similarly for the treatment and the control group. The relatively higher shares for the cohorts around 2000 are due to the combination of Microcensus waves (2014–2021) and age groups (15–21) that we work with in this paper.

²¹To improve the data quality, we leverage the limited rotating panel component of the Microcensus for the years 2016–2019 and fill in and cross-check information on the same individuals from different waves.

²²In a robustness check (Table 8), we restrict our sample to children still living at home to show that this assumption regarding eligibility is not driving the results.

TABLE 1: SAMPLE COMPOSITION (MICROCENSUS)

Birth Year	Control (\geq one German parent)	Treatment (no German parent)	Total
1995	17,442	972	18,414
1996	21,389	1,059	22,448
1997	23,657	1,239	24,896
1998	23,182	1,177	24,359
1999	23,578	1,127	24,705
2000	20,743	1,535	22,278
2001	13,870	1,195	15,065
2002	8,609	857	9,466
2003	4,509	491	5,000
2004	2,006	210	2,216
Total	158,985	9,862	168,847
Percent	94.2	5.8	100

Table 2 presents key summary statistics. There are important differences regarding key household-level and educational outcomes that are aligned with patterns documented in the descriptive literature on families with migration backgrounds. Individuals in the treatment group live in larger households with lower average incomes and less university degrees. Regarding key outcomes of interest – such as current school attendance, highest school degree attained, and university enrolment – we observe significant unconditional gaps between the treatment and the control group. In line with previous findings, the educational performance of the treatment group is significantly worse (see e.g. Gathmann and Keller, 2018).

In Figure 1, we depict how key outcomes of interest evolve across cohorts for the treatment and control groups, respectively. The dots in the graphs represent average outcomes for children born in the ten semesters²³ around the 2000 cutoff. In panel A, we illustrate the average share of children who report having obtained German citizenship at birth. While the probability remains significantly lower for the treatment groups across all cohorts, the jump following the 2000 citizenship reform is clearly visible. The probability that a child reports having citizenship at birth increases from essentially zero to over 70 percent after the reform.

Why do we not observe a full jump from zero to 100 percent in this figure, given that all individuals in the treatment group should become eligible if they are born in or after January 2000? The first and most important reason is likely that we do not observe the length of

²³While graphs displaying the monthly averages would be desirable and paint a similar visual picture, the underlying observations are sometimes very low. To protect the confidentiality of survey respondents, we can thus not publicly share the monthly graphs and show semester levels instead.

TABLE 2: SUMMARY STATISTICS BY GROUP (MICROCENSUS)

	Control (\geq one German parent)	Treatment (no German parent)	t-statistic of differences
<i>Basic information</i>			
Female	0.49 (0.50)	0.50 (0.50)	-1.89
Birth year	1,997.13 (1.38)	1,997.08 (1.38)	2.75
Birth month	6.54 (3.40)	6.40 (3.42)	3.09
Age at interview	19.46 (1.33)	19.37 (1.36)	5.27
German nationality at birth	1.00 (0.07)	0.00 (0.04)	1115.07
German nationality at interview	1.00 (0.06)	0.29 (0.45)	453.64
<i>Family background</i>			
Household size	3.16 (1.39)	3.99 (1.46)	-43.39
Adjusted HH income	2,198.06 (12,140.25)	1,693.28 (10,270.76)	2.50
Large city	0.22 (0.42)	0.37 (0.48)	-25.60
At least one academic parent	0.29 (0.45)	0.06 (0.24)	34.29
<i>Education outcomes</i>			
In education (school or uni)	0.81 (0.39)	0.76 (0.43)	8.71
In university (18+)	0.34 (0.47)	0.25 (0.43)	12.12
Completed Hauptschule	0.14 (0.35)	0.28 (0.45)	-26.72
Completed Realschule	0.46 (0.50)	0.49 (0.50)	-4.44
Completed Gymnasium	0.39 (0.49)	0.23 (0.42)	24.20

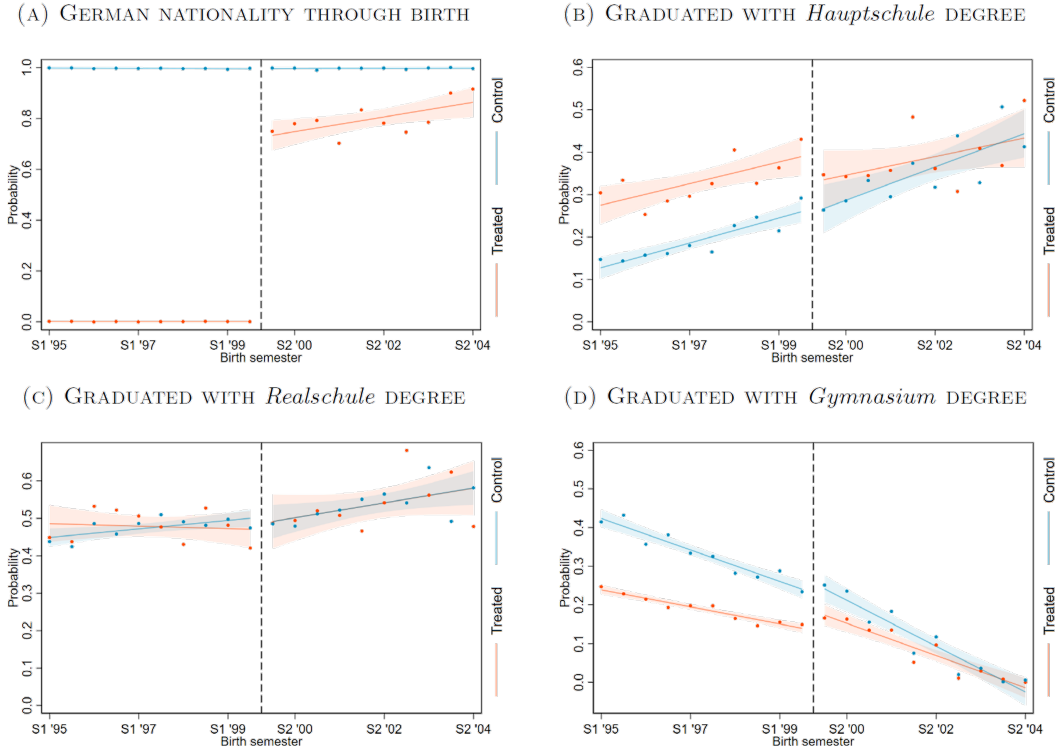
Notes: The table shows summary statistics for the 114,822 individuals in our main sample (i.e. those with complete information on school degrees) born *before* the enactment of the reform, i.e. between 1995 and 1999, and aged 15-21 at the time of the survey. Group means are pooled over all pre-reform birth cohorts and computed as unweighted averages. The third column reports the t-statistic for testing whether the two means are significantly different from each other. Detailed descriptions of the underlying variables are available in Section A.1 of the Appendix.

residency at birth for all parents and attribute those for whom we have no information to the treatment group. If neither of the parents resided in Germany legally for eight years at the time of birth, their child is not eligible for birthright citizenship.²⁴ Secondly, as the Microcensus is based on survey data, misreporting may be present. While we do not see a reason for conscious misreporting, it is possible that the responding second-generation immigrants do not know how they acquired German citizenship, e.g., if through birth or because their parents naturalized them during early childhood. Lastly, administrative procedures may play a role. While the eligibility for birthright citizenship should be checked automatically by the local authorities when a child is born, some offices might have updated their procedures with a delay

²⁴In the sub-sample for which we have information on the length of parental residency at birth, 20 percent of children are indeed not eligible to benefit from the reform (see discussion above). These clearly ineligible second-generation immigrants are already excluded in the final sample underlying Figure 1. In addition, we cannot observe the type of residency parents held, which may further reduce the number of eligible individuals that we attribute to the treatment group. It was required for parents to hold an unlimited residence permit at the time of birth ("unbefristetes Aufenthaltsrecht").

or did not have access to the necessary information to examine eligibility. In this vein, the increase over time in the share of eligible children who are conferred citizenship could be caused by a gradual improvement in the administrative implementation of the reform over time.

FIGURE 1: TRENDS IN KEY OUTCOME VARIABLES AROUND THE CITIZENSHIP REFORM



Notes: The figures averaged data at the birth semester level for the main estimation sample of 168,847 individuals. A linear fit line is estimated separately for the treatment and control group in the pre- and the post-reform period, respectively. Confidence intervals are computed at the 90% significance level. The overall data is age-weighted to ensure the equal representation of all underlying age groups. Note, however, that the age composition varies across birth cohorts due to the structure of the 2014–2021 Microcensus data: the first illustrated birth semester relies on observations at ages 19–21, whereas the last data point relies on ages 15–17. Importantly, for the 1999–2000 birth cohorts, the entire age spectrum of 15–21 years is covered.

Next, we portray our key outcome of interest – the type of completed school degree – in the remaining panels of Figure 1. As outlined in Section 2.3, it is possible to distinguish between three basic school leaving degrees in Germany: a degree from *Hauptschule*, *Realschule*, or *Gymnasium*, ordered from the school type with the shortest to the longest duration. For each of these school types we code a dummy that takes the value of one if a child states to have completed the associated school degree and zero if another type of degree has been achieved. These dummies are mutually exclusive and only the longest (and most academically oriented) school type completed at the time of the survey is considered.

Overall, it is visible that before the 2000 reform, children of non-German parents completed significantly less academic degrees than their German peers. The general trends

over birth semester cohorts appear to be broadly parallel across the treatment and control groups up until January 2000.²⁵ For the cohorts born after the enactment of the reform, we see an insignificant jump in the share of treated children completing the least academic degree (*Hauptschule*) and a significantly positive jump in completion of the most academic degree (*Gymnasium*).

5 Results

5.1 Main Results

Before examining how the citizenship reform affected the educational success of second-generation immigrants, we first estimate how the probability of obtaining German citizenship through birth changed for the second-generation immigrants in our sample. The first column of Table 3 presents our result and indicates that this probability increased by 78.5 percentage points, in line with the graphical evidence provided in Figure 1. This shows that, on average, over three-quarters of our designated treatment group born after 1999 were (knowingly) affected by the reform. As discussed in Section 4, the reason why we do not observe a full closure in the differences of reported citizenship at birth between our treatment and control groups is likely related to misreporting and minor imprecisions in how we identify the treatment group in the Microcensus. This said, the fact that the reform had a less-than-full bite in our treatment group means that our education-related results should mark the lower bound of the direct effect of obtaining citizenship at birth on education outcomes.²⁶

The other columns of Table 3 show how the distribution of completed degrees from less or more academically oriented school tracks change following the reform. More concretely, we estimate the difference-in-differences model described in Equation 1, where the dependent variable is a dummy that takes the value of one for the track corresponding to the most academic school degree obtained by an individual. This means that if a student first finished *Hauptschule* (shortest track) but then proceeded to finish the degree corresponding to *Realschule* (middle track), only their dummy for *Realschule* takes the value of one while the

²⁵Note that the age composition varies across birth semesters, and the average age becomes successively lower. Accordingly, as schooling is not completed for all individuals yet, overall trends and levels differ from official statistics on completed degrees.

²⁶In fact, they may be understood as intention-to-treat effects, reflecting the effect of introducing a new policy into a complex environment instead of measuring the *isolated effect* of obtaining citizenship at birth. To obtain a back-of-the-envelope estimate for the direct effect of obtaining citizenship, one can scale up the coefficients on education outcomes by $1/0.785 = 1.27$, as for example Felfe et al. (2020) do.

other dummies are coded as zero. Given that the age distribution varies across birth cohorts, in our regressions, we weight observations by the inverse frequency of the age occurrence. This allows us to obtain an average treatment effect for our sample of second-generation immigrants aged 15–21. Additionally, introducing age-fixed effects ensures that we account for the fact that the probability of having completed a specific school degree varies with age.

The first column shows the probability of only having completed the shortest and least academically oriented school track, the *Hauptschule*. The probability of obtaining such a degree decreases by 6.3 percentage points for the treated group following the reform, substantially closing the ex-ante conditional (unconditional) gap of 13.3 (13.1) percentage points. This change is statistically significant at the 1% confidence level. The probability of graduating at most from *Realschule* (shown in column 2) remains stable, while treated individuals become 6.5 percentage points more likely to graduate from the most academic track (*Gymnasium*, column 3) and hence obtain direct access to tertiary education. The increase in the likelihood of finishing *Gymnasium* closes the conditional (unconditional) achievement gap by 54 (40) percent and is highly statistically significant.

TABLE 3: MAIN RESULTS: HIGHEST COMPLETED DEGREE

	German nationality through birth	Hauptschule	Realschule	Gymnasium
Post × Treated	0.785*** (0.015)	-0.063*** (0.020)	-0.002 (0.021)	0.065*** (0.007)
Treated	-0.995*** (0.000)	0.133*** (0.008)	-0.013 (0.010)	-0.121*** (0.006)
Pre-treatment gap	-0.994	0.131	0.030	-0.161
N	168,847	168,847	168,847	168,847
N (Treated)	9,862	9,862	9,862	9,862

Notes: Sample based on the Microcensus and restricted to individuals born between 1995–2004, aged 15–21 at the time of the survey, and who reportedly obtained a secondary school degree. The independent variables for the last three columns are coded in a way that they are mutually exclusive: for each individual, we record only the most academically oriented track that they completed. *Hauptschule* also includes a very limited amount of individuals with a school degree of eight years or less. In all columns, we additionally control for gender and include birth semester and age-fixed effects. We apply age-specific weights to obtain balanced estimates for individuals aged 15–21. Standard errors clustered at the individual level are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Section 6, we verify the robustness of our main result in a variety of ways. We include a wide array of control variables to our regressions, use alternative sample restrictions, and rely on the group of children with only one German parent as the control group. Secondly, we replicate our main results using the SOEP as an alternative data source. Thirdly, we test

for the possibility of selection into treatment and unobservable changes between the pre-and post-reform cohorts by exploiting within-household variation between siblings born before and after the reform. Finally, we conduct a placebo test, in which we define the children with only one German parent as the treatment group and compare their outcomes to those of children with two German parents.

In Online Appendix B.1, we additionally evaluate how the citizenship reform altered educational trajectories at different points in life. In line with previous research on the German state of Schleswig-Holstein (Felfe et al., 2020), we find that the likelihood of being initially enrolled in more academic secondary school tracks increases significantly for second-generation immigrants born after 1999. The nationally representative Microcensus data thus allows us to show that the reform did not only affect second-generation immigrants in particular status states. We also verify that individuals are generally also able to complete their newly assigned school tracks successfully by restricting attention to the older subgroup of 18 to 21-year-olds.

Overall, our results hint at the existence of pre-reform frictions in the assignment of second-generation immigrants to the various school tracks. It also highlights the great potential to create more equitable (educational) opportunities by granting second-generation immigrants citizenship early in life. Especially in an early school tracking system like the one prevalent in Germany, removing any potential sources of inequality as early as possible is crucial to not perpetuate or even multiply differences in educational opportunities as children age.

5.2 Heterogeneity Analyses

Next, we conduct heterogeneity analyses to gain first insights into the dynamics and mechanisms underlying the reform. For simplicity and given its relevance for educational justice, we only report the effects of completing the *Gymnasium*, i.e., the most academically oriented school track that renders direct access to tertiary education. We investigate heterogeneities in the dimensions of gender, parental education, length of residency, and region of origin in Table 4.

In column 1, analyze whether obtaining citizenship at birth affects girls differently than boys. Girls are, on average, 7 percentage points more likely than boys to complete the *Gymnasium*, with no noticeable difference for second-generation immigrants. This difference does not change as a result of the reform, in the line with the intuition that the reform was

largely gender-neutral.²⁷

TABLE 4: HETEROGENEITY – COMPLETED MOST ACADEMIC SCHOOL TRACK (*GYMNASIUM*)

Heterogeneity Dimension:	Female	Academic parent	Residency ≥ 15 y.	EU citizenship
Post × Treated × Dummy	-0.003 (0.014)	-0.064 (0.042)	0.040** (0.018)	-0.020 (0.018)
Post × Treated	0.067*** (0.010)	0.028*** (0.007)	0.042*** (0.016)	0.084*** (0.011)
Treated	-0.117*** (0.008)	-0.049*** (0.006)	-0.088*** (0.006)	-0.143*** (0.009)
Pre-treatment gap	-0.161	-0.129	-0.182	-0.209
N	168,847	119,603	165,337	134,032
N (Treated)	9,862	7,280	6,352	4,641
Share of Treated in split	0.495	0.059	0.767	0.348

Notes: Sample based on the Microcensus and restricted to individuals born between 1995–2004, and aged 15–21 at the time of the survey. The independent variable is a dummy that takes the value of one if an individual completed the most academically oriented track of the German school system (i.e., the *Gymnasium*) at the time of the survey and zero otherwise. The dummy variable refers to the category denoted in the header, e.g., it takes on the value one for females in column 1. Column 1 refers to the child’s own gender, while the other columns describe parental characteristics. The last row of the bottom panel indicates which percentage of the individuals in the treated group belong to the category denoted in the header. In all columns, we add the appropriate set of interactions with the dummy variable (Treated, Dummy (only columns 1 and 2), Treated × Dummy, Post × Dummy, Post × Treated (only columns 1 and 2), Treated × Dummy × Post). We additionally control for gender and include birth semester and age-fixed effects. We apply age-specific weights to obtain balanced estimates for individuals aged 15–21. Standard errors clustered at the individual level are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Next, we dig deeper into how the characteristics of children’s parents mediate the effect of the reform. Among treated individuals for whom we observe at least one parent’s educational background, only 5.9 percent report having at least one parent who completed tertiary education. Importantly, this small subgroup does not suffer from any pre-reform educational achievement gaps compared to children with academic parents in the control group. It is thus not surprising that in column 2, we document larger (though insignificant) educational benefits of the reform for second-generation immigrants with *non*-academic parents.²⁸

In column 3 we add an interaction term with a dummy that takes on the value of one when (at least) one parent resided in Germany for more than 15 years at the time of childbirth. We find slightly more positive effects of the reform for the children of these long-term residents. One reason for this could be that children from these families are more affected by conflicting feelings regarding their identity (as their parents chose to maintain strong ties to their

²⁷For the subset of Muslim immigrants in the sample, Dahl et al. (2022) document heterogeneities across genders. They find a negative educational effect of the reform on Muslim girls but not boys, which they rationalize with parents’ ambition to conform their daughters to a role within their traditional culture.

²⁸In non-reported regressions we also differentiate between parents with and without at least 12 years of schooling (i.e., the minimum schooling time needed to obtain a *Gymnasium*-degree in Germany).

home country and did not naturalize despite being eligible) and that the barrier-free recognition as part of the German society allows them to more easily reconcile their national identities.

Testing whether there are differential effects for long-term residents is important also for another reason. As described in Section 2, besides introducing birthright citizenship, the 2000 citizenship reform also lowered the residency requirement to obtain citizenship for adult immigrants from 15 to 8 years. Therefore, at the same time that their children were granted birthright citizenship, some parents also newly obtained the chance to naturalize, potentially confounding the birthright citizenship effect that we are trying to measure. In our sample, around 33 percent of treated individuals have parents with more than eight but less than 15 years of residence. If parents' own avenue to citizenship was the decisive factor for how well their children integrated into the German educational system, we should observe differentially *larger* effects for these children. Our regression from column 3, however, suggests that this is not the case, strengthening the argument that birthright citizenship for second-generation immigrants was the main element of the reform affecting their educational performance.

Lastly, we test the role of individual migration background and legal situation. In column 4, we add an interaction term with a dummy that takes on the value of one when at least one non-German parent is a EU citizen.²⁹ Given that children of EU citizens typically have full access to the European (and German) labor market already, the German citizenship reform opens up fewer new possibilities for them. Hence, one may expect that the reform would act less strongly on second-generation immigrants with EU ties relative to those without EU ties. However, our regressions reveal that they did not experience a significantly different effect of the reform. In Online Appendix Section B.2, we further examine differences in effect size by parental region of birth. There, we show that for children of whom at least one parent was born in Turkey or the Middle East, the effects are significantly larger than for other children in the treatment group.

²⁹We only have information on *EU citizenship* and *Residency in Germany* for the non-German parents of the children in our treatment group. This is why we do only control for *Treated* \times *EU citizenship* and *Treated* \times *Residency* ≥ 15 years in our regressions, and not for *EU citizenship* or *Residency* individually. For the same reason, we also omit the controls for *Post* \times *EU citizenship* and *Post* \times *Residency* ≥ 15 years, and focus only on estimating the coefficients on the triple interactions through our regression.

5.3 Mechanisms

We continue by collecting evidence for the mechanisms underlying the observed effects of the reform. For these analyses, we rely exclusively on SOEP and NEPS survey data, which contains more detailed information about educational, societal, and socioemotional outcomes. The caveat of this detailed and high-quality data is, however, their much smaller sample sizes.³⁰ The limited sample sizes make it more challenging to precisely identify small quantitative effects, and the estimates presented in this section should therefore mostly be seen as suggestive evidence.

As outlined in Section 2, we hypothesize that two channels are central to understanding how the introduction of birthright citizenship facilitated the educational success of second-generation immigrants. These are, firstly, the increased returns to (country-specific) education and, secondly, an improved sense of identification with Germany. While we cannot disentangle the two channels or quantify their relative importance, we provide further references to findings from the literature and add empirical evidence on outcomes related to each of these two channels. More specifically, we show how the reform affected crucial mediating factors by analyzing changes in key measures of education and societal integration.

Economic returns to human capital channel. We begin by presenting further evidence on how the reform affected children’s educational performance in Table 5. We begin by examining grades, which in German schools run from 1 (very good) to 6 (insufficient). Although the coefficient estimates are not statistically significant, we consistently find that the reform improved the school grades of second-generation immigrants in German language and literature classes, roughly closing the pre-treatment gaps in the NEPS and the SOEP samples.³¹ In the NEPS data, we use the fact that we have school identifiers and control for the latter by adding school-fixed effects to our regressions.³² It is reassuring that the results from the different datasets, with or without this control, are highly consistent.

Next, we look at the effects on tertiary education. First, using information from the

³⁰The share of the treatment group relative to the overall sample size in the SOEP (3.7 percent) is somewhat lower than the Microcensus (5.8 percent). This is likely because we are able to more accurately filter for the non-eligible individuals in the treatment group when using the SOEP. Their relative share is significantly higher in the NEPS (17.0 percent), given the intentional oversampling of people with a migration history from survey wave 3 onward.

³¹We find similarly stark improvements in math grades. Results are available upon request.

³²Note that the NEPS data is sampled at the school level, and results should therefore always be understood as *conditional* on the effect of the reform on initial track enrolment. We make this even more explicit by examining *within-school* effects.

SOEP youth questionnaire, we record an increase in children's ambitions to enroll in tertiary education of about 6.5 percentage points, again statistically insignificant. We also document an increase in the actual probability of enrolling in university, conditional on the specific school that children attended, which roughly closes the unconditional pre-treatment gap.

TABLE 5: PERFORMANCE INDICATORS

	German grade (SOEP)	German grade (NEPS)	Targets tertiary educ. (SOEP)	Ever in tertiary educ. (NEPS)
Post \times Treated	-0.221 (0.158)	-0.210 (0.164)	0.065 (0.095)	0.144 (0.134)
Treated	0.176* (0.091)	0.205* (0.119)	0.035 (0.055)	-0.025 (0.102)
<i>Fixed effects</i>				
School		✓		✓
Pre-treatment gap	0.194	0.263	-0.039	-0.122
N	3,374	2,889	4,405	1,793
N (Treated)	108	193	125	96

Notes: Data stem from both the SOEP and the NEPS. The SOEP outcomes are taken from the youth questionnaire, which is administered exclusively to individuals aged 16 or 17. The *German grade* is recorded on the German grading scale from 1 (highest) to 6 (lowest) grade. Information on grades is only asked of those adolescents who are still enrolled in school at the moment of the interview. The variable *Targets tertiary education* is a dummy variable that takes the value of one when a child affirms that he/ she plans to attend tertiary education and zero otherwise. The NEPS *German grade* outcome stems from the wave 6 questionnaire (approx. 10th grade) and is recorded on the German grading scale from 1 (highest) to 6 (lowest) grade. Information on grades is only asked of those adolescents who are still enrolled in school at the moment of the interview. "Ever in tertiary education" is a dummy variable that takes the value of one when a child is ever recorded in a university or university of applied sciences. For this outcome, we restrict the sample to children who participated in the NEPS studies at least until wave 11 (average age > 19.3), counting each child only once. Both NEPS regressions are weighted with the wave-specific cross-sectional survey weights. All SOEP and NEPS regressions additionally include controls for gender, whether a child had at least one parent with a university degree, and birth year fixed effects. Robust standard errors are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

While we cannot directly examine the motivation behind these changes, we take this as evidence that the children affected by the reform improve their educational performance (even conditional of the assigned school track) and increase their ambitions for their educational career overall. This is in line with the hypothesis that, as children gain full and permanent access to the German labor market, their expected returns to education increase.

This theoretical prediction is also supported by empirical findings in the literature, albeit so far only for the case of adult naturalizations. Facilitated entry to restricted and prestigious occupations is a commonly proposed channel for citizenship wage premia and for how

naturalizations increase earnings (Bratsberg et al., 2002; Steinhardt, 2012)³³ Concretely, in Germany, the occupations restricted to German (and sometimes EU) nationals include judges, military personnel, and high political positions such as chancellor or president. Depending on the state, restrictions exist also for lower-level public service positions, such as the police. Lastly, non-citizens may face access restrictions in the private sector as well, for example, for positions that require extensive business travel and for which visa restrictions may play a role. Generally, if (perceived or actual) discrimination based on citizenship is expected to reduce success or match quality in the labor market, granting citizenship may strengthen the motivation to invest in education and skills to realize more profitable future job options.

Another important reason for an increase in the expected returns to education is that the demand for skilled labor is large in Germany in comparison to most immigrants' countries of origin. If second-generation immigrants anticipate that they may need to leave Germany and move to their parents' country of origin, they may be counting on their education to yield a smaller return than their German peers. Additionally, a higher staying probability may encourage children to invest in country-specific skills that are difficult to transfer but are crucial to succeeding in the local work environment.³⁴

Lastly, the behavior of potential employers also contributes to a change in the returns to education. Gathmann and Garbers (2023) argue that firms perceive citizenship as a signal for a long-term staying perspective and are more willing to hire and invest in these individuals, which in turn leads to higher earnings. Knowing that the likelihood of obtaining a rewarding job later in life increases, individuals may try to maximize their chances to go to top employers by signaling their ability through education. In summary, both theory and literature support the notion that knowing with certainty that one can stay in Germany forever, independent of the return decision of one's parents, increases the incentive to invest in education in general and country-specific skills in particular.

Identity channel. With regards to the identity channel, we begin by analyzing whether the social and linguistic integration of second-generation immigrants ameliorated after the

³³For France, on the contrary, Govind (2021) does not find evidence that access to public service jobs drives the citizenship wage premium in France. However, she rationalizes this with the fact that entry into these jobs is only possible via special education and competitive exams, and individuals who are naturalized later in life cannot participate anymore.

³⁴Friedberg (2000) shows for Israel that the return to education acquired abroad is lower than for education acquired domestically, even after controlling for possible differences in quality. This suggests that the value of an education is highest in the place where it is acquired, both due to the country-specific skills and knowledge one acquires and due to the limited recognition and understanding of foreign degrees and educational structures.

reform. Table 6 provides evidence that this is, in fact, the case. The probability of speaking mostly German at home and speaking only German with one's friends increases by 8.4 and 13.6 percentage points, respectively. Both of these effects are statistically insignificant. In column 3, we test whether children feel that they belong to Germany.³⁵ The share of respondents answering this question rises by 17.6 percentage points, which is equivalent to a 66 (65) percent reduction in the conditional (unconditional) gap. In column 4, we test whether a child wants to stay in Germany, which is expressed as a dummy variable that takes the value of one if a child affirms this, and zero if not. The share of children in the treatment group who report wanting to stay in Germany rises by 10.3 percentage points as a result of the reform, though the corresponding coefficient is statistically insignificant.

TABLE 6: INTEGRATION OUTCOMES

	Speaks mostly German at home	Speaks only German with friends	Feels belonging to Germany	Wants to stay in Germany
Post × Treated	0.084 (0.120)	0.136 (0.117)	0.176** (0.074)	0.103 (0.085)
Treated	-0.537*** (0.080)	-0.327*** (0.083)	-0.265*** (0.061)	0.344*** (0.060)
Pre-treatment gap	-0.539	-0.324	-0.271	0.357
N	3,429	3,429	3,842	4,104
N (Treated)	231	231	253	289

Notes: Data stem from the NEPS. The first two outcomes are part of the wave 6 questionnaire, i.e., when children are in grade 10. The third outcome stems from the wave 4 questionnaire (grade 8), and the fourth outcome from wave 3 (grade 7). All regressions are weighted with the respective wave-specific cross-sectional survey weights. They include controls for gender, whether a child had at least one parent with a university degree, and birth year fixed effects. Robust standard errors are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7 provides evidence on how children's self-reported mastery of the German language changed for the cohorts affected by the reform. The sample here is limited to those children who report speaking a language other than German in their households. We find surprisingly large and significant effects in the dimensions of "understanding," "speaking," and "reading," and also large but insignificant effects in the dimension of "writing". This completely closes the conditional and unconditional treatment gaps in all specifications. While the improvements in language mastery could in principle also come from an increased learning effort as a response to increased returns to education, we conjecture that it is mainly driven by the increased usage in the private room, as documented in the previous table.

³⁵This variable is based on the variable t428060, which reports how strongly people agree with the following statement on a scale from 1 (not at all) to 5 (very much): "I feel like a part of German society." Our dummy variable takes the value of one when a person agrees strongly (4) or very strongly (5) and zero otherwise.

Increased integration due to receiving citizenship therefore has the potential to improve educational outcomes indirectly via increasing the number of interactions with native German individuals and with German society in general.

TABLE 7: SUBJECTIVE GERMAN SKILLS (1 BEST, 5 WORST)

	Understanding	Speaking	Reading	Writing
Post \times Treated	-0.210* (0.117)	-0.247** (0.119)	-0.236* (0.123)	-0.124 (0.146)
Treated	0.207** (0.082)	0.245*** (0.084)	0.138 (0.086)	0.104 (0.093)
Pre-treatment gap	0.120	0.247	0.079	0.033
N	934	935	935	936
N (Treated)	244	245	245	245

Notes: Data stem from the NEPS. All outcomes are part of the wave 3 questionnaire, i.e., when children are in grade 7. All regressions are weighted with the cross-sectional survey weights corresponding to wave 3. They also include controls for gender, whether a child had at least one parent with a university degree, school, and birth year fixed effects. Robust standard errors are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The literature also supports this notion, albeit again with a focus on older individuals. There is well-established evidence that (adult) naturalization increases national identification, reduces dissonance between competing identities, and provides additional opportunities for interaction with the host society (Fick, 2016; Reichel, 2011). In the context of Switzerland, Hainmueller et al. (2017) confirm that naturalization increases various dimensions of social integration, such as reading Swiss newspapers, participating in local clubs, or planning to remain in the country. A few studies have also looked at the context of birthright citizenship, usually using the reform in Germany also used in this paper. Felfe et al. (2021) show experimentally that cooperation with natives increases for boys as a consequence of granting birthright citizenship. Further, Avitabile et al. (2013) demonstrate at the parental level that it increases the likelihood of German being spoken at home and of interaction with the local community. In general, both our findings and the findings from the existing literature support the hypothesis that a greater identification with the host country comes along with an improved feeling of acceptance both generally and within the school context, which in turn ameliorates children's learning processes.

6 Robustness

6.1 Alternative Model Specifications

In this Section, we carry out a set of regressions to test the robustness of our main results regarding the model specification, control group definition, and sample selection. As in Section 5.2, we focus our tests on the *Gymnasium* outcome, i.e., whether someone receives direct access to the tertiary education system in Germany.

TABLE 8: ROBUSTNESS – COMPLETED MOST ACADEMIC SCHOOL TRACK (*GYMNASIUM*)

	Additional Controls	Parent match	≥ One non-German parent
Post × Treated	0.084*** (0.011)	0.067*** (0.009)	0.065*** (0.009)
Treated	-0.091*** (0.008)	-0.121*** (0.007)	-0.107*** (0.008)
Pre-treatment gap	-0.145	-0.142	-0.145
N	73,149	85,459	21,471
N (Treated)	4,299	5,509	9,862

Notes: Sample based on the Microcensus and restricted to individuals born between 1995–2004 aged 15–21 at the time of the survey. In all columns, we control for gender and include birth semester and age-fixed effects. In column 1, we augment our regression with additional controls: a dummy for living in an urban area (city with more than 500,000 inhabitants), log household income adjusted to the individual level using the OECD equivalence scale, parental schooling, and state-fixed effects. Parental schooling is a dummy variable that takes the value of one if at least one parent had 12 years of schooling, and zero otherwise. In column 2, we restrict the sample to children who are still living at home and whom we can thus match to both of their parents. In column 3, we restrict the control group to children who had one German and one foreign parent at birth. We apply age-specific weights to obtain balanced estimates for individuals aged 15–21. Standard errors clustered at the individual level are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In column 1 of Table 8, we add a large variety of controls to our main regression specification to make our treatment and control groups more comparable. While we would ideally like to do so in all instances, these additional control variables are not always available, and doing so would thus significantly reduce our sample size. It may also introduce bias because many of these control variables are only collected for children still living at home. Adding controls for parental school education, household income, living in an urban area, and state-fixed effects increases the estimated effect size from 6.5 to 8.4 percentage points without altering its significance.

Next, we limit the sample to individuals we can match with both of their parents via their household identifiers. This restriction ensures that we can assign children to the treatment

and control groups with high precision and do not have to make any assumptions about their eligibility status. However, it requires us to exclude all individuals who we only observe after they have moved out or who have only one parent in the household, thus inducing concerns about selection. Despite this somewhat different and smaller sample, our coefficient estimate remains very close to that in our main specification. The marginally increased effect size is in line with increasing precision in the estimation and reducing attenuation biases.

The third column restricts the control group to all children with one (but not two) German parents to increase the comparability between our treatment and control groups. This subgroup of German children is likely more similar to our treatment group, as foreign cultural or linguistic influences are also part of these children's environments. Factors such as appearance-based discrimination may also affect individuals in both our treatment and alternative groups more similarly. We again find a significant increase in the probability of completing the most academic school track (*Gymnasium*), which is very similar in magnitude to the main result and which closes around half of the pre-reform outcome gap between children with no versus only one German parent. This reassures us that we are measuring the actual effect of the reform and are not simply picking up broader trends of increased cultural openness over time, which would (to some extent) have also benefited children with one foreign parent.

6.2 Alternative Data Set (SOEP)

Next, we test the robustness of our main result. In this analysis based on data from the Microcensus, we rely on some assumptions about the length of parents' residency in Germany when attributing children of non-German parents to the treatment group. Whenever there is no available information on parents' length of residency, we assume that children are eligible and belong to the treatment group. To test the robustness of our main results to this assumption, we rely on data from the SOEP. While the SOEP covers a much smaller share of the German population, it allows us to identify children of the treatment group in a much more precise way, as it is possible to link information on parents and children even when they do not reside in the same household. Additionally, using another data set adds external validity to the results based on the Microcensus, showing that they are not only driven by the peculiarities of a particular dataset.

Relying on the SOEP, we estimate a regression similar to the one in Table 3. To avoid working with a very small sample, we keep working with the 1995–2004 cohorts but widen

the range of ages considered to all individuals that we observe at least once at age 11 or higher (i.e., at least once when the individual is already enrolled in secondary school). We then proxy our completed degree variable from the Microcensus by the most academically oriented track an individual ever attended in the SOEP. Therefore, we capture *both completed and ongoing* schooling.

TABLE 9: ROBUSTNESS - MOST ACADEMIC TRACK EVER ATTENDED (SOEP)

	Hauptschule	Real-/Gesamtschule	Gymnasium
Post \times Treated	-0.056 (0.039)	0.049 (0.053)	0.088* (0.049)
Treated	0.080*** (0.030)	0.014 (0.035)	-0.151*** (0.031)
Pre-treatment gap	0.081	-0.003	-0.172
N	10,158	10,158	10,158
N (Treated)	321	321	321

Notes: Sample based on the SOEP and restricted to individuals born between 1995–2004 aged 11 or older at the time of the last survey participation. In all columns, we control for gender and include birth cohort and age-fixed effects. For each individual, we use the last survey observation only. Robust standard errors are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The results presented in Table 9 again show substantial shifts towards more academic school tracks, although most coefficients are statistically insignificant. For the result that is of the greatest interest, the probability of attending the most academic track (*Gymnasium*), however, we find an increase of 8.8 percentage points, significant at the 10 percent level. This amounts to closing the pre-reform conditional (unconditional) achievement gap by over 58 (51) percent, which is in line with our findings based on the Microcensus.

6.3 Sibling Analysis

Next, we investigate whether our main results could be driven by unobservable changes between the pre- and post-reform cohorts of the treatment group that do not move in parallel with the control group. For example, if foreigners who gave birth to children before and after 2000 differ regarding their countries of origin or attitudes towards education, we could (mistakenly) be attributing changes in children’s school performance to the reform rather than to changes in parental or household characteristics.

To overcome this issue, we focus on the subsample of Microcensus households that

have at least one child born before *and* one child born after the 2000-cutoff. This restriction allows us to obtain estimates of the effect of the reform *within* households, which are not contaminated by any compositional differences across the pre- and post-reform cohorts. By limiting the sample to families with children born before and after the reform, we can estimate the reform's effect without being worried about the confounding role of simultaneous changes in (unobservable) household characteristics. Since we need both children to still live in the same household, in order to avoid working with a selected sample, we focus on children aged 12–15 when all children can still be expected to live with their parents. Accordingly, we have to restrict our main sample to children born between 1998 and 2004, as we only observe these younger cohorts at the chosen age.

The empirical strategy that we employ closely resembles the differences-in-differences strategy used for our main analysis and presented in equation (1). There are two minor differences. Firstly, we define treatment and control groups at the household rather than at the individual level. We assign a household to the treatment group if neither parent held the German nationality at the time of birth of the oldest sibling in the sample.³⁶ The second difference is that in some of our regressions, we can now replace the simple group-fixed effects – accounting for differences between the treatment and control group – with more granular household-fixed effects. This allows us to accurately control for the substantial heterogeneity within the treatment and control groups at the level of individual households. The coefficients thus capture the effect of obtaining citizenship at birth for individuals within the *same household*.

Table 10 presents the results of running our differences-in-differences regressions to investigate, first, the reported take-up of the reform and, second, the educational trajectories. In both cases, we present the result of our regressions for the full sample of 12 to 15-year-olds next to our sibling analyses to ensure the comparability of our coefficient estimates.

We first show how the probability of having German nationality at birth changes for the cohorts born after the reform. Amongst the full sample of 12 to 15-year-olds, the citizenship reform induces a substantial 71.7 percentage point increase in the probability of obtaining German nationality through birth. This estimate is in line with that of our main analysis in Section 5.1 (78.5 percentage points). Using only the sample of siblings yields similar

³⁶Parents in a treated household may acquire German citizenship themselves between the birth of the first (pre-reform) sibling and subsequent (post-reform) siblings. In these cases, both the citizenship reform and the fact that a parent actually held German citizenship at birth would allow post-2000 children to obtain German nationality at birth. This applies only in a small number of cases - 67 out of 4,997 individuals in our sample - and excluding these individuals from the sample does not substantially alter the results.

results. Importantly, the inclusion of household-fixed effects in column 3 does not lead to significant changes in the estimated coefficient.³⁷ This exercise is reassuring for the validity of our model specification.

TABLE 10: SIBLING ANALYSIS – ENROLLED IN MOST ACADEMIC SCHOOL TRACK/ GYMNASIUM

	German nationality through birth			Enrolled in most academic track/ <i>Gymnasium</i>		
	Full sample	Siblings	Siblings	Full sample	Siblings	Siblings
Post × Treated	0.717*** (0.006)	0.724*** (0.043)	0.700*** (-0.045)	0.035* (0.018)	0.070* (0.037)	0.067* (.037)
Treated	-0.997*** (0.000)	-0.990*** (0.007)		-0.188*** (0.017)	-0.256*** (0.037)	
Household fixed effects			✓			✓
Pre-treatment gap	-0.997	-0.997	-0.997	-0.189	-0.256	-0.256
N	116,241	4,977	4,977	116,241	4,977	4,977
N (Treated)	8,289	237	237	8,289	237	237

Notes: The sample consists of siblings aged 12–15. We restrict attention to households where at least one child was born between 1998–1999 and at least one child was born between 2000–2004. A household is classified as part of the treatment group if the neither parent held the German nationality at the birth of the first child. In some rare cases, they acquire the nationality until the birth of their children born after the 2000-citizenship reform. Standard errors clustered at the household level are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We next turn to investigate how the reform affected the educational outcomes of second-generation immigrants. Given the younger sample, we pay attention to children’s current secondary school track enrollment instead of focusing on whether they completed the *Gymnasium* degree. In the overall sample for 12 to 15-year-olds, we document a 3.5 percentage point increase in the likelihood of being enrolled in the *Gymnasium*. While smaller than our main results, this effect remains statistically and economically significant, closing the pre-treatment gap by 3.5 percentage points (18.5 percent). When using only the sample of siblings, the effect is larger again, with a 6.7–7.0 percentage points increase in the probability of attending this school track. Importantly, the inclusion of household fixed effects does again not significantly change the size of the estimated coefficient.

6.4 Placebo Test

To further solidify the validity of our results, we conduct a placebo test in which we define individuals with one German and one foreign parent as an artificial treatment group. We

³⁷In concurrent research, Ziege (2024) finds positive spillover effects of naturalized children on the educational success of their older, not-naturalized siblings. Such spillover effects would entail a downward bias in the coefficients from our sibling analysis relative to our main results.

then test whether the reform had any effect on this group when comparing it to the control group of individuals with two German parents. Because of their one German parent, the children in our placebo treatment group were eligible for German citizenship even before the 2000 reform. At the same time, the children in our placebo treatment group also have a (partial) migration background – just like those in our main treatment group. Thus, if our main results were driven by developments other than the 2000 citizenship reform that affected children with at least one non-German parent, this should be reflected by non-zero results in our placebo difference-in-differences analysis.

TABLE 11: PLACEBO TEST: COMPARING ONLY CHILDREN WITH GERMAN PARENTS

	German nationality through birth	Hauptschule	Realschule	Gymnasium
Post × Placebo-Treated	0.023*** (0.008)	-0.018 (0.022)	0.009 (0.022)	0.009 (0.007)
Placebo-Treated	-0.049*** (0.003)	0.034*** (0.011)	-0.009 (0.012)	-0.026*** (0.006)
Pre-treatment gap	-0.056	0.027	-0.009	-0.019
N	158,381	158,381	158,381	158,381
N (Treated)	11,609	11,609	11,609	11,609

Notes: Sample based on the Microcensus and restricted to individuals born between 1995–2004 to at least one German parent, and aged 15–21 at the time of the survey. The control group consists of individuals with two German parents, and the treatment group of those with one German parent at the time of birth. All other specifications as in Table 3. Standard errors clustered at the individual level are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The results of the placebo analysis are displayed in Table 11. Column 1 focuses on whether individuals report having obtained German citizenship through birth. Not all individuals in the pseudo-treatment group hold citizenship at birth before the reform, and we observe a very small but significant increase of 2.3 percentage points in citizenship after the reform. This reflects the slight imprecision in our group assignment, consistent with the explanations in Sections 4 and 5.1. In columns 2–4, we test whether the reform led to any significant changes in the educational performance of our placebo treatment group. In line with our expectations, all education-related outcomes do not change significantly after the reform.

7 Conclusion

Children of immigrants face many disadvantages compared to their fully native peers: besides frequent economic disadvantages, their parents are often not as familiar with the

institutional structure, culture and language of the local society, preventing them from providing their children with the same support as other parents. The difference in family backgrounds is one of the leading causes of significant achievement gaps in the educational performance of fully local children versus the descendants of immigrants. These gaps later translate into reduced access to the university system, and they eventually become one of the major factors underlying wage and employment gaps observed in the labor market.

This paper documents how a reform of birthright citizenship laws in Germany affected the educational trajectories of second-generation immigrants. For cohorts born before the reform, we find large achievement gaps between second-generation immigrants without German passports and their German peers. We then document how the reform, which introduced conditional birthright citizenship to children of non-German parents, led to a closure of these gaps over time.

We find large and significant effects on the enrolment in and completion of more academically-oriented secondary school tracks, which are particularly pronounced for children of long-term immigrants. These results are robust to including additional controls and to alternative sample specifications, and they persist when only examining within-household effects. We also document positive effects on German language skills, which are an important precondition to excel in the national context. In addition, we provide suggestive evidence that the feeling of belonging to the country and the integration of second-generation immigrants improves. We posit two mechanisms through which obtaining citizenship at birth can improve educational outcomes: first, an increase in expected returns to education and an ensuing greater incentive to invest in it, and second, an increase in identification with and integration into the receiving society, which indirectly facilitates learning and educational success.

As outcomes relating to post-secondary education and labor market trajectories become available, evaluating the effect of citizenship in these dimensions will become an interesting object of study. Specifically, it will be insightful to see whether the gains in secondary education will indeed translate into higher enrolment rates in tertiary education or if the impact of having been granted citizenship at birth will taper out as children age. Based on the limited data already available, we document insignificant increases in intention to attend university and initial enrolment, but it is still too early to fully document changes in the post-school educational trajectories of *ius soli* children.

In terms of labor market outcomes, besides the canonical objects of study such as wages

and employment, two aspects are of particular interest to further research. The first concerns occupational choice, in the sense of whether second-generation immigrants who benefited from birthright citizenship will more frequently choose occupations that actually require citizenship or country-specific skills. Such occupations include those in the legal field, the police, and, more generally, jobs performed as civil servants. The second aspect of interest relates to the increased mobility that a German passport (instead of a permanent residency) entails: will the affected individuals use more opportunities to study abroad, particularly within the European Union, where they can now move free of any visa requirements? And will they be more active in careers that require international mobility, or decide to relocate for career purposes within the European Union?

Overall, the positive effects of the birthright citizenship reform that we document in this paper are crucial for academics and policymakers interested in designing legal systems that facilitate the successful integration of immigrants. By boosting the educational success of second-generation immigrants, the reform helped close pre-existing educational achievement and linguistic gaps in an inexpensive way. Of course, citizenship laws are highly complex and take into account many aspects beyond those relating to education. Nevertheless, in broad terms, our research supports the view that birthright citizenship represents a powerful accelerator of integration.

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Online Appendix

A Additional Data Descriptions and Summary Statistics

In this section, we provide additional information on the construction of key variables from the German Microcensus as well as a detailed description of the data preparation process for SOEP and NEPS.

A.1 Construction of Microcensus Variables

- **Adjusted HH income:** This variable is computed by first converting the 24 income brackets (for total net income reported by all household members) to exact numbers (using the help values provided by the Microcensus based on a uniform random number generation within the income bracket, or taking the mean of the bracket if this measure is unavailable), deflating the resulting income with the series provided by the Federal Statistical Office, and converting it to per capita income by applying the OECD modified equivalence scale (i.e. dividing by the number of household members, counting the first adult in the household with weight one, any further adults with 0.5, and children with 0.3).
- **Large city:** Large city is a dummy that takes the values one when the city of residence has more than 500,000 inhabitants. This is similar to the category “Kreisfreie Städte” (independent cities).
- **Academic parent:** This dummy variable takes on the value one if at least one of the parents holds a university degree.
- **In university (18+):** This dummy variable is computed only for individuals aged 18+ and takes on the value one if an individual currently reports being enrolled in university.
- **Completed Hauptschule/Realschule/Gymnasium:** This dummy variable takes on the value one if an individual’s highest completed school degree falls into this category. The variables are mutually exclusive, i.e. an individual who completed first the “Realschule” and then the “Gymnasium” will have a zero for *Completed Realschule* and a one for *Completed Gymnasium*. Note that the shares do not confirm with official statistics on school degrees as we record the highest degree at the time of the interview and some individuals may have not concluded their schooling career.

A.2 SOEP Data Description

The SOEP is a representative longitudinal study that annually surveys around 12,000 German households. It covers topics such as household composition, migration history, employment status, earnings, health, and life satisfaction. Participants are selected at the household level, and children born into surveyed households are included as new adult participants for the study once they turn 18. Information on minors is primarily collected through their parents' questionnaires. At the age of 17, adolescents are, for the first time, surveyed directly through so-called youth questionnaires. As in the Microcensus, we restrict our sample to children born in Germany in a 5-year bandwidth around the 2000-reform cutoff for which key demographics and a clear treatment classification are available.

The SOEP makes it possible to connect information on children and their biological parents through matched identifiers even when they do not reside in the same household. Whenever parents are surveyed in the SOEP, we rely on their individual questionnaires to determine their nationality in the precise year of childbirth. When a parent is not surveyed, we complement this information with child-reported data from the youth questionnaire. We restrict our sample to children for whom we have reliable information on their parents' nationalities at birth.

To determine whether parents fulfilled the residence requirement for *ius soli*-citizenship at childbirth, we reconstruct their immigration history. In 92 percent of cases, this information is available in sufficient detail to precisely identify second-generation immigrants eligible for the reform. Whenever it is unclear when the parents immigrated, we assume that the children are eligible for citizenship, as only around 15 percent of parents for whom we know the length of residence lived in Germany for less than eight years at the time of childbirth. In addition, we assume that the information is less likely to be missing for parents who immigrated more recently, given that their immigration history is still more salient. Only a small number of second-generation immigrants are not eligible for the reform, and we exclude them from the sample.

Finally, we harmonize key migration and educational variables by combining information from various questionnaires, consistently prioritizing primary data sources. Therefore, we rely on household questionnaires, individual questionnaires filled in by parents and adult children, and youth questionnaires. Given that the latest published survey year is 2021, some variables are only available for a restricted set of birth cohorts, namely the cohorts born

before 2004 for the youth questionnaire and before 2003 for the standard adult questionnaire. Additionally, since participation in the survey is not mandatory, and the questionnaire is rather long, (partial) non-response is rather frequent, lowering the number of observations for these outcomes. In Tables 1 and 2, we present the number of children by treatment and control group in each birth cohort as well summary statistics for our main sample.

TABLE 1: OBSERVATIONS BY GROUP AND BIRTH YEAR (SOEP)

Birth Year	Control (\geq one German parent)	Treatment (no German parent)	Total
1995	957	43	1,000
1996	1,150	59	1,209
1997	1,168	51	1,219
1998	1,229	49	1,278
1999	1,225	43	1,268
2000	1,230	58	1,288
2001	1,172	56	1,228
2002	1,284	32	1,316
2003	1,283	36	1,319
2004	1,287	28	1,315
Total	11,985	455	12,440
Percent	96.3	3.7	100

TABLE 2: SUMMARY STATISTICS BY GROUPS (SOEP)

	Control (≥ One German parent)	Treatment (No German parent)	t-statistic of differences
<i>Basic information</i>			
Female	0.49 (0.50)	0.47 (0.50)	0.84
Birth year	1,999.68 (2.83)	1,999.11 (2.70)	4.22
Age at last observation	16.38 (4.99)	14.53 (5.63)	7.75
Was ever directly interviewed	0.41 (0.49)	0.31 (0.46)	4.20
<i>Family background</i>			
Number of siblings	1.97 (1.41)	2.25 (1.60)	-2.18
Highest school edu father	2.32 (0.66)	2.02 (0.56)	8.36
Highest school edu mother	2.32 (0.52)	2.02 (0.50)	11.48
HH net income (age 10)	3,295.81 (1,722.22)	2,687.39 (892.33)	5.88
<i>Schooling outcomes</i>			
Age (months) at first enrollment	78.43 (5.49)	78.98 (6.38)	-1.30
Hauptschule at age 12	0.12 (0.32)	0.20 (0.40)	-3.74
Real-/Gesamtschule at age 12	0.39 (0.49)	0.38 (0.49)	0.15
Gymnasium at age 12	0.37 (0.48)	0.22 (0.42)	4.38
Ever in Hauptschule	0.16 (0.37)	0.29 (0.46)	-6.71
Ever in Real-/Gesamtschule	0.51 (0.50)	0.46 (0.50)	1.64
Ever in Gymnasium	0.37 (0.48)	0.22 (0.42)	5.65
Ever in alternative school types	0.11 (0.31)	0.12 (0.33)	-0.88
<i>Performance indicators</i>			
Repeated grade	0.22 (0.41)	0.28 (0.45)	-1.69
Targets Abitur (at age 17)	0.74 (0.44)	0.55 (0.50)	4.53
German test score (at age 17)	2.85 (0.88)	2.97 (0.81)	-1.41
Ever completed Abitur	0.25 (0.43)	0.12 (0.33)	3.48
<i>Post-school outcomes</i>			
Ever in vocational training	0.30 (0.46)	0.24 (0.43)	2.70
Ever in uni. of applied sciences	0.12 (0.32)	0.12 (0.33)	-0.11
Ever in university	0.18 (0.39)	0.04 (0.20)	3.85

Notes: The table shows summary statistics for our main sample born between 1995 and 2004. Group means are pooled over all birth cohorts and computed as unweighted averages. Standard deviations are reported in parentheses. Dummies in the table are coded with (1) for "yes" and (0) for "no." Parental school education is defined as an increasing categorical variable with (1) indicating "Hauptschule"/9 years or less, (2) "Realschule"/10 years, and (3) "Gymnasium"/12–13 years. Average household net income is reported monthly and expressed in 2016 Euros. Values above the 99th percentile are top-coded to that value to limit the impact of outliers. The German test score ranges from 1 (best) to 6 (worst). "Gymnasium" and "Targets Abitur" are dummy variables, i.e., the mean indicates the share of children that attend a Gymnasium (highest track) or target the Abitur (high school degree), respectively.

A.3 NEPS Data Description

The NEPS is a longitudinal panel that follows various cohorts through their (educational) careers and covers the years 2010/11–2020/21 (Blossfeld et al., 2011; NEPS-Netzwerk, 2022). We rely on Starting Cohort 3, which consists of children who were enrolled in the fifth grade

of the German school system in school year 2010/11. Over 90 percent of this sample was born in 1999 and 2000. Because the study oversamples children with Turkish and Soviet migration backgrounds, the share of treated children is relatively large (17 percent), allowing us to carry out meaningful analyses despite the overall small sample size of the NEPS. We primarily use the NEPS to analyze reform-induced changes in school performance, language skills and integration-related outcomes conditional on the school type or specific institution that a child attends.

To classify children into treatment and control groups, we combine information reported by the children with information that parents contributed in five rounds of (voluntary) parental surveys. The respondent of the latter is most frequently the target child's mother. However, in principle, any parent or legal guardian is eligible to complete it, and the responding person may change across waves. In either case, information on the respondent's partner is also elicited, providing us with sufficient information to assign children to treatment and control groups. For each parent, we combine all available information on their current nationality, migration status, immigration year, naturalization year, and their own parents' nationalities to approximate whether they held the German nationality when their child was born.

Unfortunately, due to the voluntary nature of the parental surveys, there are many missing data points in the NEPS concerning the year in which parents immigrated to Germany or naturalized, preventing us from precisely observing which children meet the eligibility criteria. Given the overall low share of ineligible second-generation immigrants across all datasets, we again remove those individuals for which we know that their parents lived in Germany for less than eight years at the time of birth and make the assumption that the rest were eligible for citizenship.³⁸

In a next step, we restrict attention to those individuals for which we have information on both parents' nationalities and can thus approximate the children's treatment status with relatively high confidence. We check the consistency of our grouping approach by relying on a variable explicitly stating how children acquired citizenship (e.g., at birth vs. through naturalization). There are only a few misclassifications (less than 1 percent), which

³⁸In the NEPS, we know the length of parents' residency in Germany at birth for 125 out of the 1,009 children born to foreign parents. Of these 125 cases, less than 5 percent of parents (six cases) had lived in Germany for less than eight years at the time of childbirth. This low share is in line with the fact children with a Turkish or Soviet background were oversampled, which are both likely to have longer-dated migration histories. In the Microcensus and the SOEP, which have a different sampling approach, the ineligible second-generation immigrants constituted 16 and 15 percent of second-generation immigrants, respectively, which is still a clear minority.

we attribute to reporting errors. We drop all misclassified individuals. Finally, we apply two sample restrictions to increase the comparability of educational and career outcomes across the NEPS target children. Firstly, we restrict attention to children born in Germany between 1997 and 2002.³⁹ Secondly, we exclude children sampled from special needs schools, given their relatively low number and the peculiarities of this educational track. We also ensure that information on children's gender and their parents' education is available for all observations. This leaves us with a final sample of 6,066 individuals who fulfill the criteria and participate in the survey at least once. The number of children by treatment and control group in each birth cohort together with key summary statistics are displayed in Tables 3 and 4.

TABLE 3: OBSERVATIONS BY GROUP AND BIRTH YEAR (NEPS)

Birth Year	Control (\geq one German parent)	Treatment (no German parent)	Total
1997	5	6	11
1998	193	94	287
1999	2,008	495	2,503
2000	2,760	433	3,193
2001	60	4	64
2002	8	0	8
Total	5,034	1,032	6,066
Percent	83.0	17.0	100

It is important to note that the original NEPS sample is drawn at the level of schools and is stratified by track. The individuals in our sample are thus children who have already entered the fifth grade of the German school system when we first observe them and are already assigned to a track.⁴⁰ This implies that we cannot cleanly identify the reform's effect on outcomes such as the initial track choice since we would confound potential treatment effects with the sampling procedure. Thus, we mainly use the NEPS data to investigate the progress given an initial track assignment and analyze non-academic outcomes such as cultural integration. Generally, all NEPS-based results presented in this paper should be interpreted as *conditional* on having been in fifth grade and a particular track in 2010. Whenever using the NEPS data, we weigh our analysis to account for the stratified multistage sampling of schools (by type and region) and the oversampling of migrants. For time-constant outcomes, such as whether someone ever obtained German nationality, we use

³⁹Over 90 percent of our sample are born in 1999 or 2000. However, some individuals report being born as early as 1989 and as late as 2005 or report no sensible information at all.

⁴⁰The federal states Berlin and Brandenburg feature a later track choice after grade 6.

TABLE 4: SUMMARY STATISTICS BY GROUPS (NEPS)

	Control (≥ One German parent)	Treatment (No German parent)	t-statistic of differences
<i>Basic information</i>			
Ever had German nationality	0.99 (0.08)	0.66 (0.47)	-42.56
Female	0.51 (0.50)	0.52 (0.50)	0.90
Birth year	1,999.56 (0.50)	1,999.42 (0.49)	-7.80
Orig. sample: Hauptschule	0.17 (0.38)	0.33 (0.47)	10.85
Orig. sample: Real-/ Gesamtschule	0.33 (0.47)	0.35 (0.48)	1.10
Orig. sample: Gymnasium	0.50 (0.50)	0.33 (0.47)	-9.70
<i>Family background</i>			
Mother: has German nationality	0.93 (0.26)	0.00 (0.00)	-107.05
Father: has German nationality	0.93 (0.26)	0.00 (0.00)	-107.90
Mother's year of birth	1,968.67 (5.00)	1,972.11 (5.68)	13.01
Father's year of birth	1,965.94 (5.43)	1,968.62 (6.56)	9.41
Academic parent (dummy)	0.34 (0.47)	0.14 (0.34)	-8.85
Max. observed HH size	4.51 (1.46)	4.87 (1.83)	4.82
Max. observed monthly HH income	3,792.82 (3,051.56)	2,580.43 (1,261.98)	-5.49
<i>Schooling outcomes</i>			
Age in months at kindergarten start	38.20 (9.14)	42.02 (10.70)	6.91
Age in months at primary school start	77.34 (5.72)	77.76 (7.41)	1.49
Ever attended Hauptschule	0.12 (0.32)	0.24 (0.43)	7.91
Ever attended Realschule	0.52 (0.50)	0.59 (0.49)	2.99
Ever attended Gymnasium	0.62 (0.48)	0.45 (0.50)	-7.75
Ever attended Waldorfschule	0.04 (0.20)	0.05 (0.22)	1.10
Ever attended Foerderschule	0.00 (0.05)	0.01 (0.09)	2.20
Attended G9 school (13 grades)	0.51 (0.50)	0.50 (0.50)	-0.13
<i>Performance indicators</i>			
Repeated grade	0.33 (0.48)	0.22 (0.51)	-0.37
Max. school educ. Haupt/Real/ Abi	2.38 (0.73)	2.05 (0.81)	-9.40
<i>Post-school outcomes</i>			
Ever in military/ FSJ	0.13 (0.34)	0.06 (0.25)	-4.57
Ever in voc. preparation	0.09 (0.29)	0.16 (0.36)	4.84
Ever in Fachhochschule	0.17 (0.37)	0.17 (0.38)	0.12
Ever in University	0.38 (0.49)	0.24 (0.43)	-4.81
Ever in employment	0.78 (0.42)	0.67 (0.47)	-5.40

Notes: The table shows summary statistics for the starting cohort 3 (SC3) of the NEPS data. Group means are pooled over all birth cohorts and computed as weighted averages using the wave-3 cross-sectional weights. Standard deviations are reported in parentheses. Dummies in the table are coded with (1) for "yes" and (0) for "no."

wave-3 cross-sectional weights, which is the first wave that includes the migrant sample. For all other outcomes, we use the weights provided for the survey round from which they stem.

B Additional Results

B.1 Educational Trajectories

Besides the final degrees obtained by second-generation immigrants, we are also interested in gaining insights into their educational trajectories *before* completing school. To understand changes in the trajectories of children born between 1995 and 2004 (i.e., the cohorts corresponding to the ones considered in our main regression specification), we rely on the highly detailed survey data from the SOEP. While the sample size of the SOEP is much lower than that of the Microcensus, it allows insights into outcomes at younger ages. In addition, the long panel dimension and identifiers of biological parents allow us to assign our treatment indicator even more precisely in this subset of children.

Table 1 shows reform-induced changes in early schooling outcomes, which condition the school completion degrees observed in the Microcensus. In column 1, we find a decrease of 1.4 months for the school enrolment age of affected children. Although this effect is statistically insignificant, it aligns with the magnitude and direction of previous work, which shows that the reform increased children's kindergarten enrolment rates and lowered the age at which they entered primary school, in line with previous work (Felfe et al., 2020).

TABLE 1: ENROLMENT AND FIRST TRACK CHOICE (SOEP)

	Enrolment age	Haupt- schule	Real-/ Gesamtschule	Gymnasium
Post \times Treated	-1.378 (0.925)	-0.113** (0.052)	0.086 (0.066)	0.123** (0.058)
Treated	1.039 (0.661)	0.128*** (0.041)	-0.040 (0.044)	-0.201*** (0.036)
Pre-treatment gap	1.062	0.131	-0.048	-0.196
N	2,891	7,210	7,210	7,210
N (Treated)	182	223	223	223

Notes: The age of first enrolment is indicated in months. The choice of the school track is shown as the school track observed at age 12 when children are typically in grade 6. We additionally control for gender and include birth cohort and age fixed effects. Robust standard errors are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Additionally, we observe clear and significant effects regarding children's initial secondary school types, which are decided upon by children's primary school teachers and

parents. As in our main results, we see a significant shift towards more academically oriented tracks. The probability of being in a Gymnasium at age 12 (approx. 6th grade) increases by 12.3 percentage points, which is equivalent to a closure of the conditional (unconditional) pre-treatment gap of 61 (63) percent and double the size of the effect on completed degrees that we obtain using the Microcensus.⁴¹

Acknowledging that the small sample sizes in the SOEP may be problematic, we conduct a similar exercise on the school trajectories of second-generation immigrants using Microcensus data. Focusing on respondents aged 15 and 16, we estimate how the citizenship affected the school type in which they are currently enrolled in. The sample we use here differs from the one used for our main specification in that the cohorts considered are narrowed down to 199–2001. These correspond to the cohorts for which we have overlaps in observations at ages 15 and 16 for pre- and post-reform cohorts in our 2014–2021 Microcensus data. Furthermore, we here focus on individuals who are still enrolled in school as opposed to those who report having obtained a school-leaving degree. These make up 90 percent of all 15- and 16-year-olds in our cleaned dataset.

TABLE 2: SCHOOL TRACK, AGE 15–16 (MICROCENSUS)

	Hauptschule	Real-/Gesamtschule	Gymnasium
Post × Treated	-0.044*** (0.016)	-0.012 (0.022)	0.056*** (0.019)
Treated	0.134***	0.088***	-0.221***
Pre-treatment gap	0.134	0.088	-0.223
N	41,568	41,568	41,568
N (Treated)	2,671	2,671	2,671

Notes: Sample based on the Microcensus and restricted to individuals born between 1998–2001, and aged 15 or 16 at the time of the survey. The independent variable are mutually exclusive and indicate the secondary school track in which each adolescent is currently enrolled. In all columns, we additionally control for gender and include birth semester and age-fixed effects. We apply age-specific weights to obtain balanced estimates for individuals aged 15 and 16. Standard errors clustered at the individual level are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 shows the results of this exercise. As in our main results on final school-leaving degrees, we document a clear shift towards more academically-oriented school tracks in the post-reform cohorts of second-generation immigrants. Although the estimate is smaller than the one we find for degree completion in our main results and for initial track enrolment in

⁴¹The probabilities here do not add up to one as in some federal states, children are still in primary school at this age and thus have a dummy equal to zero in all three school categories.

the SOEP, the share of children attending the *Gymnasium* exhibits a significant increase of 5.6 percentage points. This corresponds to a 25 percent closure of the unconditional pre-reform achievement gap.

Lastly, we examine the effects of the reform later, instead of earlier, in life. In Table 3, we show results for re-estimating regression 1 using only individuals aged 18–21. While schooling may still be in progress for some individuals, there is a higher chance that the final school-leaving degree is obtained at this point and individuals are not continuing to study at a more academic track. We find the same patterns as for ages 15–21, albeit with a somewhat smaller magnitude: the increase in completion of the most academic track as a result of the reform is now 3.4 instead of 6.9 percentage points.

TABLE 3: HIGHEST COMPLETED DEGREE, AGE 18–21 (MICROCENSUS)

	Hauptschule	Realschule	Gymnasium
Post × Treated	-0.049*** (0.010)	0.017 (0.013)	0.033*** (0.011)
Treated	0.127*** (0.007)	0.039*** (0.008)	-0.165*** (0.007)
Pre-treatment gap	0.129	0.045	-0.174
N	142,928	142,928	142,928
N (Treated)	8,066	8,066	8,066

Notes: Sample based on the Microcensus and restricted to individuals born between 1995–2004, and aged 18–21 at the time of the survey. The sample is restricted to those individuals who reportedly obtained a secondary school degree. The independent variables are coded in a way that they are mutually exclusive: for each individual, we record only the most academically oriented track that they completed. *Hauptschule* also includes a very limited amount of individuals with a school degree of eight years or less. In all columns, we additionally control for gender and include birth semester and age-fixed effects. We apply age-specific weights to obtain balanced estimates for individuals aged 18–21. Standard errors clustered at the individual level are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.2 Additional Heterogeneity Analyses

In Section 5.2, we explore differences in the effect of the reform by region of origin of the parents, focusing on the European Union. Here, we look at various regions of the world in more detail. Column 1 of Table 4 shows that almost 50 percent of individuals in the treatment group have family origins in Turkey. For this group, the reform has a particularly large effect on educational outcomes, with the share of individuals finishing with the highest school degree increasing by 8 percentage points as a result of the reform. Southern Europeans and individuals from Sub-Saharan Africa follow the average of the treatment group, those from the Middle East benefit very strongly and those from Eastern Europe and Asia benefit (much) less than the average. While these results have to be interpreted with caution as the number of individuals in each group is small, the results suggest that groups which are initially lagging behind less or not at all - such as individuals whose parents come from Asia, a group known to place a high value on educational achievement - also benefit less from the boost that the reform offers.

TABLE 4: HETEROGENEITY – COMPLETED MOST ACADEMIC SCHOOL TRACK (*GYMNASIUM*)

	Turkey	Southern Europe	Eastern Europe	Middle East	Asia	Sub-Sah. Africa
Post \times Treated \times Parents' birthplace	0.082*** (0.018)	-0.012 (0.028)	-0.078* (0.040)	0.104* (0.053)	-0.199*** (0.069)	0.023 (0.105)
Treated \times Parents' birthplace	-0.104*** (0.016)	-0.006 (0.024)	0.085** (0.038)	-0.071 (0.048)	0.186*** (0.068)	0.021 (0.079)
Post \times Treated	0.044*** (0.014)	0.093*** (0.010)	0.096*** (0.009)	0.089*** (0.009)	0.096*** (0.009)	0.092*** (0.009)
Treated	-0.093*** (0.013)	-0.150*** (0.009)	-0.155*** (0.008)	-0.149*** (0.008)	-0.154*** (0.008)	-0.151*** (0.008)
Pre-treatment gap	-0.117	-0.117	-0.117	-0.117	-0.117	-0.117
N	166,811	166,811	166,811	166,811	166,811	166,811
N (Treated)	7,826	7,826	7,826	7,826	7,826	7,826
Share of Treated in split	0.484	0.089	0.100	0.033	0.065	0.008

Notes: Sample based on the Microcensus and restricted to individuals born between 1995–2004 and aged 15–21 at the time of the survey. Since the 2014 and 2015 waves of the Microcensus do not contain information on parental country of birth, only waves 2016–2021 are used. Within the treatment group, we distinguish between groups of children whose parents were born in different foreign countries or regions. This information is often missing for individuals not living at home, which is why the treatment group here is reduced to 7,826 individuals and slightly younger than the main sample. *Parental birthplace* is a dummy variable if at least one parent of a child was born in a specific country or region and zero otherwise. The independent variable is a dummy that takes the value of one if an individual completed the most academically oriented track of the German school system (i.e., the *Gymnasium*) at the time of the survey and zero otherwise. The last row of the bottom panel indicates which percentage of the individuals in the treated group belong to the category denoted in the header. We additionally control for gender and include birth semester and age-fixed effects. We apply age-specific weights to obtain balanced estimates for individuals aged 15–21. Standard errors clustered at the individual level are reported in parentheses. Significance levels are denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.