Birthright Citizenship and Youth Crime

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More than a quarter of people aged 15 to 34 living in OECD countries are foreign-born or have foreign-born parents (OECD & European Commission, 2023). Migration can have positive effects such as strengthening the pool of skilled labor and boosting innovation (see Glennon, 2024, for a recent review). However, at the same time, migration also creates the challenge of integrating people into the educational system, the labor market, and society as a whole. Failures in integration might not only result in increasing inequality, but can also exacerbate social tension and political backlash (Alesina and Tabellini, 2024). An inclusive educational system, access to employment opportunities, or policies fostering language learning are common elements of integration policy. Another important, but controversially debated, policy lever is access to host-country citizenship.

Granting citizenship to immigrants has been shown to have positive effects on social integration (e.g., Hainmueller, Hangartner and Pietrantuono, 2015, 2017), employment, and earnings (e.g., Bratsberg, Ragan and Nasir, 2002; Gathmann and Keller, 2018), which can be explained by increased incentives to invest in host-country language skills, removing employment barriers and reducing informality, or mitigating discrimination. Related research also finds that legalizing immigrants can lead to reductions in crime (Baker, 2015; Mastrobuoni and Pinotti, 2015; Pinotti, 2017) by increasing their labor market opportunities and thus opportunity costs of committing a crime (Becker, 1968).

While this literature has primarily examined adult immigrants, a smaller but growing set of studies investigates the consequences of granting citizenship at birth for children of immigrants. Germany provides a particularly informative setting, as it is the only major immigrant-receiving country to have introduced birthright citizenship in recent decades. Using this institutional context, Felfe, Rainer and Saurer (2020) and Felfe et al. (2021) show that granting birthright citizenship improves the educational outcomes of immigrant children and promotes their social integration. However, little is known about whether these effects extend beyond the educational context.

In this paper, we revisit Germany's birthright citizenship reform to estimate its effects on youth crime. Combining newly collected administrative data on police-reported criminal cases from three German states with a novel empirical approach, we find that immigrant youth who acquired citizenship at birth are substantially less likely to engage in criminal activity, with estimates indicating a 70% reduction in crime.

I. Background, Data, and Empirical Design

Prior to 2000, children born in Germany acquired German citizenship at birth only if at least one parent was a German citizen (*ius sanguinis*). A major amendment to the German Citizenship and Nationality Law, passed on July 15, 1999, introduced a conditional form of birthright citizenship. Under the new regime, children born in Germany on or after January 1, 2000 automatically acquired German citizenship (*ius soli*) if at least one parent had legally resided in Germany for a minimum of eight years at the time of birth. The reform was strictly binding, as parents could not opt out.[1] Using

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[1] A transitional provision allowed parents of children born between 1991 and 1999 to apply for German citizenship in 2000; take-up was limited, with only about one-sixth of eligible families applying (Felfe, Rainer and Saurer, 2020).

auxiliary data from the Microcensus for our three German states, citizenship at birth for second and third generation migrants increased from 22% for those born before the reform (July to December 1999) to 89% for those born after (January to June 2000). [2]

To study the effect of this reform on youth crime, we collected administrative data covering the universe of police-reported criminal cases in the German states of Baden-Württemberg, Hesse, and Berlin. Together, these states comprise approximately 20.5 million residents, or about 25 percent of the German population, and rank among the states with the highest shares of immigrants. For our project, we coordinated with the State Offices of Criminal Investigation to match operational data of police departments with official crime records. The resulting dataset provides detailed information on the date, location, and type of each offense, as well as the month of birth, gender, and nationality of suspects.[3] One limitation is the absence of direct information on their migration background. Hence, it is not possible to identify whether a German suspect is a native German or a naturalized German with a migration background.

We propose a novel empirical approach which sidesteps this limitation using crime data aggregated to the county $\times$ birth month $\times$ year of birth $\times$ citizenship status level. The intuition is as follows. In a first step, we compare the number of crimes committed by German and non-German suspects born just before versus just after January 1, 2000. Because the reform "switched" many children of immigrants from being non-German to German at birth, we expect to observe a mechanical decline in the number of crimes attributed to non-German individuals born after the cutoff. In a second step, we examine whether this post-reform decline among non-Germans is accompanied by a corresponding increase in crimes attributed to Germans. Such a pattern would arise if individuals with a migration background who acquired citizenship at birth did not change their criminal behavior. By contrast, no commensurate increase in crimes among Germans would indicate that birthright citizenship reduced criminal involvement among the treated immigrant population.

To implement this design, we restrict the sample to crime incidents committed by two adjacent birth cohorts of youth around the turn of the millennium. The reform cohort consists of German and non-German suspects born between July 1999 and June 2000, while the control cohort comprises German and non-German suspects born between 1998 and 1999. Individuals in these cohorts typically attend the same school grade, but those in the reform cohort are differentially exposed to birthright citizenship depending on whether they were born before or after January 1, 2000. We consider all crimes committed by youth between ages 14 and 19 years and 8 months, corresponding to offenses recorded between July 2012 and February 2020. We impose this upper age limit to avoid confounding effects associated with the Covid–related lockdowns.

II. Results

Figure 1 illustrates our design. The graph plots the number of crimes committed by German (gray) and non-German (black) youth against their month of birth. The vertical line represents the birth date cut-off of the birthright citizenship reform. The counts are deseasonalized separately for Germans and non-Germans (i.e., netting out month-of-birth fixed effects) using data from the previous birth cohort (July 1998 to June 1999).

The figure reveals a sharp drop in offenses committed by non-Germans born after the cut-off. This is the mechanical drop mentioned above; it is due to fewer children with a migration background being born as non-Germans. Under the null of no treatment effect, this decline should be offset by a commensurate increase in offenses committed by Germans, since the reform "switches" immigrant children from being non-German to German. The dashed gray line represents this counterfactual. The actual increase in offenses committed by Germans (solid gray line) is smaller, providing initial

[2] It is above zero for those born before the reform because some immigrant children are citizens by right of blood, and it is below one for those born after because some parents do not meet the 8-year eligibility criteria.

[3] Unless an incident is classified as severe or the suspect is involved in multiple incidents, police departments delete some suspect-level information—such as month of birth—two years after the last recorded incident. As a result, the data underrepresent infrequent offending and minor crimes.

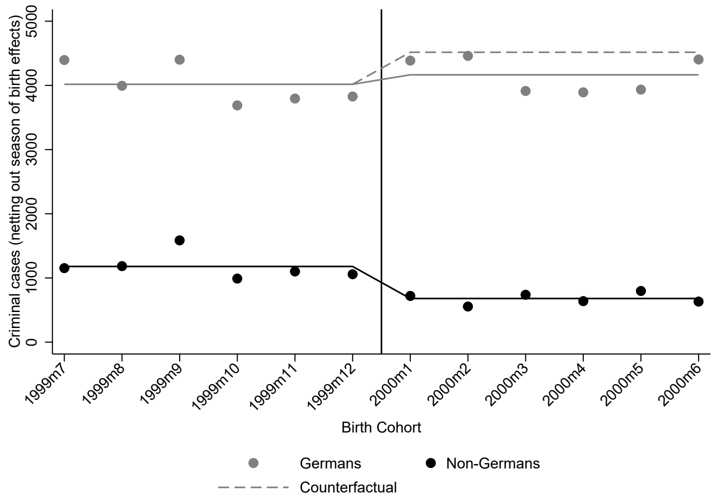

FIGURE 1. THE IMPACT OF BIRTHRIGHT CITIZENSHIP ON YOUTH CRIME

Note: The figure shows the aggregate number of crimes by month of birth for Germans and non-Germans (after accounting for seasonal effects using data from the previous birth cohort born from July 1998 to June 1999). The solid lines depict the 6-month averages before and after the reform. The vertical line represents the cut off for the birthright citizenship reform.

evidence that birthright citizenship reduces crime among treated immigrant youth.

Our design can be implemented by estimating two difference-in-differences models and summing up the two estimated effects. The first difference-in-differences model uses non-Germans and accounts for seasonal effects; it takes the difference in the number of crimes committed by non-Germans born from January to June 2000 to those born from July to December 1999, and subtracts from this the analogous difference from the preceding cohort. The second difference-in-differences model does the same for Germans. By adding up the two difference-in-differences, we can identify the deviation in crimes for Germans born after the reform cut-off compared to the counterfactual where the mechanically missing crimes of non-Germans are simply added to the number of crimes attributed to Germans. The design allows seasonal effects, such as age-for-grade effects, to differ for Germans and non-Germans.

We estimate this design using a single regression:

$$\begin{aligned} Crimes_{c,m,y,n} &= \beta_0 + \beta_1 NG + \beta_2 T + \beta_3 (NG \times T) + \beta_4 (T \times P) \\ &+ \beta_5 (NG \times P) + \beta_6 (NG \times T \times P) + \gamma_m + \lambda_c + \varepsilon_{c,m,y,n} \end{aligned} \tag{1}$$

where $Crimes_{c,y,m,n}$ is the number of crimes committed in county c by suspects born in month m of year y with nationality n. NG is one for Non-German suspects, and zero for German suspects. T is one for the treated cohort of suspects born from July 1999 to June 2000, and zero for those born from July 1998 to June 1999. P is one for Non-German suspects born from January to June and zero for Non-German suspects born from July to December, and reversed for German suspects.[4] γ_m are month of birth fixed effects and λ_c are county fixed effects. The coefficient of interest is β_6, capturing the aforementioned sum of two difference-in-differences. The error term is $\varepsilon_{c,m,y,n}$; our regressions cluster standard errors at the birth month × year of birth level.

The first column of Table 1 documents our headline finding of a statistically significant crime

[4]Instead of reversing this indicator for German suspects, we could alternatively run the regression equation as a conventional triple differences equation and compute the targeted sum of the two difference-in-differences estimates as $2 \times \beta_4 + \beta_6$.

TABLE 1—ESTIMATION RESULTS

	Criminal cases			Types of crime			
	All	Boys	Girls	Violent	Property	Drug	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(VII)
Treatment (β_6)	-4.941	-6.225	1.284	-0.127	-3.077	-0.901	-0.836
	(1.943)	(1.974)	(0.527)	(0.479)	(1.424)	(0.333)	(0.554)
Observations	3,408	3,408	3,408	3,408	3,408	3,408	3,408
Counterfactual (est.)	7.013	6.652	0.362	1.073	4.438	0.620	0.885
TOT in %	-70.45	-93.58	355.19	-11.84	-69.33	-145.45	-94.43
ITT in %	-47.20	-62.70	237.98	-7.93	-46.45	-97.45	-63.27
Criminal cases	120,441	97,960	22,481	29,634	50,041	21,196	19,570

Note: OLS estimates of equation (1). Our estimation sample of 3,408 observations covers crimes committed in 71 counties by 2 types of offenders (German and non-German) born over the 24 months from July 1998-June 2000. Standard errors are reported in parentheses and are clustered at the birth month $\times$ year of birth level.

reducing effect of birthright citizenship. The estimated treatment effect is a reduction of 4.941 crimes at the county $\times$ month $\times$ year level. To interpret the size of this effect, we compare it to the number of crimes that would have been committed by those gaining citizenship in the absence of the reform. This corresponds to the decrease in the number of crimes committed by non-Germans after the cutoff. We estimate this counterfactual using the first difference-in-differences model for non-Germans described above. Dividing the estimated treatment effect of 4.941 by the estimated counterfactual of 7.013 yields a treatment effect on the treated (TOT) of -70.45%.

Typical evaluations of policy reforms estimate the intention-to-treat (ITT) effect and recover the TOT by scaling the ITT with the first-stage take-up rate. In contrast, our design directly estimates the TOT (i.e., the effect of receiving birthright citizenship among treated youth), and allows us to use the first stage to recover the ITT (i.e., the effect of the reform among all children with a migrant background). Multiplying the TOT effect by the first-stage increase in citizenship at birth of 67 percentage points discussed in Section I, the ITT is 47%.

In the remaining columns of the table, we explore heterogeneity. The reduction in crime due to birthright citizenship is exclusively driven by boys (column 1), with the effect for girls (column 2) being positive.[5] Since boys commit crime at a much higher rate, they dominate the overall effect. The table also breaks down effects by type of crime. We find negative estimates for violent (column 4), property (column 5), drug (column 6), and other crimes (column 7), although only the property and drug crime effects are statistically significant.

Robustness checks show that narrowing the estimation window or implementing a donut hole around January 1 do not materially change the estimates. Our results are also robust to leaving out the state of Berlin, which had a concurrent education reform which could have biased our estimates (see Appendix Table 1).

Since our dependent variable is crime counts, differential changes in German and non-German cohort sizes around the cutoff are not accounted for. This could create a bias since there is a mechanical link between the size of a cohort and the crimes committed by it. To address this concern, we alternatively estimate a Poisson difference-in-differences model including youth population as an exposure variable. This model differs in that it pools together the crimes committed by both German and non-German youth, and then implements a difference-in-difference Poisson model which takes the difference in the number of crimes committed by youth born from January to June 2000 to those born from July to December 1999, and subtracts from this the analogous difference from the preceding cohort. This Poisson model yields similar findings. Moreover, we find the reform had a stronger impact in counties with a higher share of migrant youth (see Appendix Table 2).

[5]This result is in line with the finding by Dahl et al. (2022) that the reform decreased life-satisfaction and lowered integration and educational achievement of girls (but not boys), arguably because treated girls were pushed by parents to conform to traditional culture.

Another concern might be spillover effects from treated youth on the criminal behavior of untreated peers. However, to affect our estimates, any spillover effects would need to be discontinuous at the reform cut-off. This is unlikely since those born six months before and after the cutoff date are typically assigned to the same school cohort and therefore face similar exposure to any behavioral changes among treated peers.

III. Conclusion

Revisiting Germany's birthright citizenship reform with newly collected administrative crime data, we show that receiving citizenship at birth leads to a large reduction in crime among immigrant youth. These results are particularly relevant in light of ongoing debates in the U.S. about abolishing birthright citizenship. Our findings suggest that inclusive citizenship policies can reduce crime and its associated costs, which in turn could strengthen social cohesion.

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Online Appendix for "Birthright Citizenship and Youth Crime"

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This appendix presents robustness tests for the main results presented in the paper "Birthright Citizenship and Youth Crime".

Appendix Table 1 re-estimates equation (1) using alternative estimation samples that vary along four dimensions. Column 1 narrows the estimation window by excluding births in June and July. Specifically, instead of retaining observations within ±6 birth months around the January 1 cutoff, the specification includes only those within ±5 months. Column 2 further restricts the window to ±4 birth months around January 1. Column 3 implements a donut-hole design by excluding births in December and January, that is, those closest to the cutoff.

The purpose of the specifications in columns 1–3 is to assess whether the baseline estimates are sensitive to potential endogenous fertility responses to the reform. The progressively narrower windows in columns 1 and 2 restrict the sample to children who were necessarily conceived before the German birthright citizenship reform was ratified on July 15, 1999, thereby ruling out selective conception in anticipation of the policy change. The donut-hole specification in Column 3 addresses the possibility that parents may have attempted to manipulate the timing of births around the cutoff date—for example, by delaying delivery—to secure birthright citizenship. Across all specifications, the estimated effects remain stable, suggesting that such forms of parental manipulation are unlikely to drive the results.

Column 4 excludes crimes committed in the state of Berlin from the estimation sample. In 2004, Berlin implemented a schooling reform that reduced the average school starting age from 6.7 to 6.2 years. The first cohort affected by this reform consisted of children born between July 1999 and June 2000. To address the potential concern that individuals born before January 1, 2000, may have been differentially exposed to the reduction in school starting age relative to those born after the cutoff, we exclude all observations from Berlin. The estimated effects remain virtually unchanged, indicating that the results are not driven by this concurrent policy change in Berlin.

APPENDIX TABLE 1—ROBUSTNESS: ESTIMATION WINDOW, DONUT HOLE, DROPPING BERLIN

	Smaller windows		Donut hole	Dropping
	$\pm$ 5 months	$\pm$ 4 months	w/o Dec. & Jan.	Berlin
	(1)	(2)	(3)	(4)
Treatment (β_6)	-5.572	-5.394	-4.868	-4.421
	(2.214)	(2.792)	(2.276)	(2.019)
Observations	2,840	2,272	2,840	3,360
Counterfactual (est.)	7.657	8.472	7.423	6.98
TOT in %	-72.77	-63.67	65.58	-63.31
ITT in %	-48.76	-42.66	43.94	-42.42
Criminal cases	99,712	79,760	100,110	85,827

Note: OLS estimates of equation (1) in the main paper. Our estimation sample of 3,408 observations covers crimes committed in 71 counties by 2 types of offenders (German and non-German) born over the 24 months from July 1998 to June 2000. Standard errors are reported in parentheses and are clustered at the birth month $\times$ year of birth level.

Our main empirical strategy utilizes crime counts as the dependent variable to assess whether youth with migrant backgrounds, whose legal status was altered by the reform (from non-German to German at birth), exhibited changes in criminal behavior. A potential concern is the presence of discontinuous changes in cohort sizes around the reform cutoff that may differ between Germans and non-Germans. For example, if economic shocks reduced fertility immediately after the cutoff among

Germans but not non-Germans, cohort sizes would decline disproportionately. Because crime counts scale mechanically with cohort size, such group-specific fluctuations could bias our estimates.

To address this concern, we alternatively estimate a Poisson difference-in-differences model that includes the youth population as an exposure variable, effectively converting the analysis to one of crime rates. This model differs in that it pools crimes committed by both German and non-German youth and implements a Poisson difference-in-differences specification that compares crime rates among youth born between January and June 2000 with those born between July and December 1999, and then subtracts the analogous difference for the preceding cohort. We estimate the following regression:

$$(1)\qquad E\left[Crimes_{c,y,m} \mid T, T\times P, \gamma_m, \lambda_c, Pop_{c,y,m}\right] = \exp\left(\beta_0 + \beta_1 T + \beta_2 (T\times P) + \gamma_m + \lambda_c\right) Pop_{c,y,m},$$

where $Crimes_{c,y,m}$ is the number of crimes committed by suspects residing in county c and born in year y and month m.[1] Consistent with the baseline specification, T is one for the treated cohort of suspects born from July 1999 to June 2000, and zero for suspects born from July 1998 to June 1999. P is one for suspects born from January to June and zero for suspects born from July to December. The interaction of T and P switches on for suspects born after the reform cut-off date, i.e., between January and June 2000. The corresponding coefficient, β_2, is the coefficient of interest, capturing the reform effect. γ_m, are month of birth fixed effects and λ_c are county fixed effects. Standard errors are clustered at the birth month $\times$ year of birth level. The exposure variable $Pop_{c,y,m}$ is the average population size (across individuals aged 14 to 19 years and eight months) at the time of the crime incidents (between July 2012 and February 2020) at the county of residence $\times$ birth year $\times$ birth month level.

Column 1 of Appendix Table 2 reports results using this alternative specification. The estimated coefficients capture intent-to-treat (ITT) effects for the full population of youth—both with and without a migrant background—measured at the county $\times$ birth-month $\times$ birth-year level. Converting the estimated coefficient of -0.083 using the transformation $(e^{-0.083}-1)\times 100$, the ITT estimate implies a reduction in youth crime of approximately 8 percent in the full population. To recover the treatment-on-the-treated (TOT) effect, we scale the estimated coefficient of -0.083 by the first-stage impact of the reform on the full population, which increased citizenship at birth by 7.1 percentage points,[2] and then convert the rescaled coefficient using the same transformation formula as above.

APPENDIX TABLE 2—POISSON DIFFERENCE-IN-DIFFERENCES MODEL

		Migrant share	
	All	Above median	Below median
	(1)	(2)	(3)
Treatment (β_2)	-0.083	-0.098	-0.050
	(0.031)	(0.032)	(0.069)
Observations	1,704	840	864
ITT in %	-7.95	-9.34	-4.84
TOT in %	-68.92	-60.50	-73.86
Criminal cases	116,538	82,804	33,734

Note: Estimates of equation (1). For columns 2 and 3, we split the sample by the median of the migrant share across all counties in the sample (6.4 percent). Migrant shares are computed as the ratio between the number of *ius soli* births and the total number of births between January and June 2000 in Baden-Württemberg, Berlin, and Hesse.

[1]Because our exposure variable measures the number of residents by county, we aggregate criminal cases at the level of suspects' counties of residence rather than at the level of crime-scene counties. This yields a somewhat smaller number of criminal cases than in the baseline specification, which also includes cases committed in any of the three states by suspects residing in other German states (i.e., outside Berlin, Baden-Württemberg, and Hesse).

[2]The first stage on the full population is measured with birth register data from Baden-Württemberg, Berlin, and Hesse. It represents the average share of *jus soli* births over the total number of births across all counties between January and June 2000.

The implied TOT effect is -69 percent, which is almost identical to our main estimate reported in the paper. Columns 2 and 3 split the sample into counties with above versus below median migrant shares. Because the reform should have had a stronger impact in counties with higher migrant shares, we expect larger crime-reducing effects of birthright citizenship in these areas. This pattern is indeed reflected in columns 2 and 3, with the ITT effects for the full population being almost twice as large in above median counties.[3]

[3]To obtain TOT effects in columns 2 and 3, we scale the estimated coefficients by split-sample specific first stage estimates (10.6 and 3.7 percentage points, respectively, for the above and below median migrant share samples), and then apply the same transformation formula as above.