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
	± 5 months	± 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 × 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. We use official administrative population counts for Germany which are calculated by updating census benchmarks using registered births, deaths, and migration flows (Federal Statistical Office of Germany, 2012–2020).

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. 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. We use birth records to classify high versus low ius soli birth counties (Federal Statistical Office of Germany, 1998–2000). 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]

[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.

[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.

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.

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