

# Caught between Cultures: Unintended Consequences of Improving Opportunity for Immigrant Girls

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April 14, 2021

**Abstract:** What happens when immigrant girls are given increased opportunities to integrate into the workplace and society, but their parents value more traditional cultural outcomes? We answer this question in the context of a reform which granted automatic birthright citizenship to eligible immigrant children born in Germany after January 1, 2000. Using survey data we collected from students in 57 schools and comparing those born in the months before versus after the reform, we find the introduction of birthright citizenship lowers measures of life satisfaction and self-esteem for immigrant girls by .32 and .25 standard deviations, respectively. This is especially true for Muslims, where parents are likely to prefer more traditional cultural outcomes than their daughters. Moreover, we find that Muslim girls granted birthright citizenship are less integrated into German society: they are both more socially isolated and less likely to self-identify as German. Exploring mechanisms for these unintended drops in well-being and assimilation, we find that immigrant Muslim parents invest less in their daughters' schooling and that these daughters receive worse grades in school if they are born after the reform. Parents are also less likely to speak German with these daughters. Consistent with a rise in intrafamily conflict, birthright citizenship results in disillusionment where immigrant Muslim girls believe their chances of achieving their educational goals are lower and the perceived odds of having to forgo a career for a family rise. In contrast, immigrant boys experience, if anything, an improvement in well-being, integration, and schooling outcomes. Taken together, the findings point towards immigrant girls being pushed by parents to conform to a role within traditional culture, whereas boys are allowed to take advantage of the opportunities that come with citizenship. To explain these findings, we construct a simple game-theoretic model which builds on Akerlof and Kranton (2000), where identity-concerned parents constrain their daughter's choices, and hence lower their daughter's well-being, when faced with the threat of integration. Alternative models can explain some of the findings in isolation.

**Keywords:** Immigrant assimilation, cultural identity, intergenerational conflict

**JEL codes:** J15, J16

**Acknowledgments:** We thank colleagues and seminar participants at several universities and conferences for valuable feedback and suggestions. We are grateful to the almost 20 research assistants and interns for their invaluable assistance with data collection. The project received generous financial support from the ifo Institute, University of Munich, University of St. Gallen, and University of Hamburg.

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# 1 Introduction

A frequent occurrence in immigrant families is a clash between parents and children over cultural values. This can have far-reaching consequences for the welfare of immigrant youth. For example, in Germany, five times as many adolescent girls with Turkish roots attempt to commit suicide compared to native girls, and many experts attribute this to conflicts that arise from immigrant parents adhering to traditional cultural beliefs while their children want to endorse mainstream Western values (Heredia-Montesinos et al. 2019). In a similar vein in Canada, differences in parent and child expectations regarding religious and cultural practices is cited as the most common reason for immigrant youth ending up homeless (McKenzie et al. 2014).

Although this evidence is alarming in itself, it also raises the possibility that well-intentioned, opportunity-enhancing interventions for immigrant youth could backfire by increasing the cultural tension between generations. Take, as an example, the case study of 17-year old Havva (El-Mafaalani and Toprak 2011, p. 89). Based on her affinity for mathematics and accounting, a supportive teacher suggests she pursue commercial training. One challenge is that Havva wears a headscarf because she is Muslim, but her teacher finds companies which will accommodate this. Havva receives two offers and is initially excited, but soon tells her teacher she has changed her mind. When pressed, Havva admits she still wants the training, but her family does not support the decision. The teacher meets with her parents to try and persuade them, but Havva’s father responds that such training is not for their daughter. Havva reluctantly agrees not to pursue the opportunity.

In this example, the teacher’s well-meant intervention leads to a collision of two worlds of thought, and eventual disappointment for Havva. More generally, the case study speaks to two well-documented behavioral patterns that characterize many immigrant families. First, girls are frequently brought up to be “keepers of the culture” and often face strict parental restrictions on choices and activities that boys do not face (Suárez-Orozco and Qin 2006). Second, sociologists have argued this gender-specific parenting style is likely to intensify when immigrant parents perceive the host society as posing a threat to the values of their own culture (Dion and Dion 2001).

Against this backdrop, we explore the consequences for immigrant girls of increased opportunities to integrate into the workplace and society. Could it result in a dilemma of being “caught between cultures,” where immigrant girls aspire to use the new opportunities to assimilate into mainstream society, but their parents expect them to conform to traditional gender roles such as raising a large family and not working outside the home? Crucially, if that is the case, are there unwanted negative effects for immigrant girls in the form of reduced well-being and lower social integration? Could it even be, as our motivating example suggests, that as economic and political opportunities increase, immigrant parents sabotage the assimilation efforts of their daughters by restricting their choices? In this paper, we provide some of the first causal tests of these questions, and rationalize the findings through the lens of economic theory.

Testing how increased integration opportunities matter for child well-being and assimilation is a challenging task. The first reason is that immigrant children with more opportunities likely

also differ in other unobservable ways. In particular, immigrant youth with more opportunities to integrate may also have parents for whom traditional cultural norms are less important. To break this link, the ideal research design would take advantage of an exogenous shift in the opportunity set for some immigrant youth, but not others, while at the same time not directly affecting parents. A second challenge is that measures of well-being, career aspirations, cultural norms, parental investments, national identity, and assimilation are usually not available in most datasets. An ideal dataset would include these types of variables in a context where second-generation immigrant youth can be identified.

We overcome these challenges using a legal reform which granted automatic citizenship to eligible immigrant children born in Germany after January 1, 2000. The probability of being a citizen at birth jumps 52 percentage points for second-generation immigrant children born post-reform. A compelling feature of the reform from an identification perspective is that it occurs in between school year cutoffs. This means that immigrant youth born six months before and after the cutoff will typically be in the same grade in school, while having different probabilities of being a German citizen at birth. To learn more about the effects of this reform, we conducted in-class surveys of immigrant and native students in their final year of compulsory schooling (normally 15-16 years old) in 57 German schools.

We use this birthdate cutoff as an exogenous shock to youth immigrant opportunity. Citizenship provides new rights and possibilities, such as the ability to vote in general elections, be employed in the public sector, and work in other EU countries (but does not change access to social assistance benefits). Prior research suggests that citizenship improves economic outcomes: naturalized adult immigrants earn more compared to their non-naturalized peers (Chiswick 1978; Steinhardt 2012), have higher job-finding rates (Fougère and Safi 2009; Gathmann and Keller 2018), and experience steeper wage-tenure profiles (Bratsberg et al. 2002).<sup>1</sup> Using the large jump in citizenship around the birthdate cutoff, we analyze how the reform affected youth’s subjective well-being and a host of other outcomes. Focusing on a narrow one-year window around the cutoff, we compare second-generation immigrant children born in the months before versus after the reform. Native German children are used to difference out any common age effects within a school year.

In a first step, we examine whether birthright citizenship had unintended consequences on the well-being and assimilation of immigrant girls. Self-reported life satisfaction falls by almost a third of a standard deviation for those born after the reform. The implied effect is similar in magnitude to the effect of a medium-level depression on life satisfaction (Frijters et al. 2020). The estimate is robust to a narrowing of the sample window ( $\pm 6$ ,  $\pm 5$ ,  $\pm 4$ ,  $\pm 3$ , or  $\pm 2$  months around the cutoff) or the use of a regression discontinuity design, but with larger standard errors. The effects are concentrated among immigrant daughters in Muslim families, where cultural differences relative to German mainstream culture are starkest. In contrast, immigrant boys experience, if anything, an improvement in well-being. We find the same pattern using self-esteem measures, consistent with the loss in well-being being driven by identity conflicts as hypothesized by Kranton (2016) and

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<sup>1</sup>Related work shows that legalized immigrants commit less crime (Mastrobuoni and Pinotti 2015; Pinotti 2017).

Akerlof (2017).

Strikingly, we find that Muslim immigrant girls affected by the reform are less integrated into German society, which is opposite the intent of the reform. Immigrant Muslim girls granted birthright citizenship are less likely to participate in after-school social activities with natives and are less likely to have a friendship network they can turn to for support when they experience challenges. Moreover, immigrant Muslim girls are 14 percentage points less likely to self-identify as German if they have been granted birthright citizenship. There are no effects for immigrant girls from non-Muslim backgrounds and immigrant boys are, if anything, more socially integrated.

In a second step, we explore whether intergenerational conflict can explain these results. Guided by our motivating example, we focus in particular on the possibility of parental sabotage, whereby parents from a gender-conservative background try to counteract their daughters' assimilation aspirations when they are at a higher risk of integration. Our analysis yields two key sets of results. First, parental schooling investments and grades in school jointly fall for Muslim immigrant girls affected by the reform. In particular, Muslim immigrant girls who have access to birthright citizenship are 15 percentage points less likely to receive parental support with their homework and learning compared to their non-naturalized peers, and these girls simultaneously experience a significant drop in their academic achievement. In contrast, opposite-signed effects are found for immigrant boys and for non-Muslim immigrant girls, with the former being statistically significant. Muslim immigrant parents are also less likely to speak German with their daughters if they are born after the reform.

Building on these findings, birthright citizenship results in disillusionment for Muslim immigrant girls, where they believe the chances of achieving their educational and career goals are lower. Muslim immigrant girls exposed to the citizenship reform are more likely to aspire to get tertiary schooling, but the odds they place on reaching their educational goals fall by 21 percentage points. This drop is driven by the same girls who, due to birthright citizenship, receive less schooling support from their parents. In contrast, there is no disillusionment for immigrant boys or non-Muslim immigrant girls. We further find that for Muslim immigrant girls, the perceived odds of having to forgo a career for a family rise by 8 percentage points. The reverse is true for non-Muslim immigrant girls, and there is no significant effect for boys.

While we focus on intergenerational conflict as a mechanism for our findings, we recognize that other factors could simultaneously be in play. Three leading alternatives we discuss for the drop in well-being and/or assimilation among Muslim immigrant girls are unmet expectations associated with citizenship, resource shifting towards sons, and convergence to native well-being. While these alternatives are interesting in their own right and could play concurrent roles, in isolation they are not consistent with the entirety of our findings and supplementary analyses. In line with parent-child conflict as the first-order mechanism, we find that Muslim immigrant girls believe their lives will get better in the future, once they are likely to have left their parents' households.

In a third step, we formally rationalize our empirical findings through the lens of economic theory. There are, in principal, three classes of economic models that could be adapted to explain

the results: theories of authoritarian parenting whereby parents monitor their children and force them to obey their wishes (Doepke and Zilibotti 2017; Doepke et al. 2019); models that portray cultural transmission as an interaction between direct vertical socialization and oblique horizontal socialization (Bisin and Verdier 2000, 2001); and theoretical work that considers how identity affects economic outcomes (Akerlof and Kranton 2000). We propose an intergenerational version of the seminal identity model of Akerlof and Kranton (2000), which builds on the idea that (i) “choice of identity may be the most important ‘economic’ decision people make” and (ii) “[l]imits on this choice may be the most important determinant of an individual’s economic well-being” (p. 717). Importantly, in this class of models, policy-makers face a dilemma as it is not possible to incentivize individuals to engage in certain activities and, at the same time, protect them from the reactions of others for whom these activities cause discomfort and anxiety. Translated into our intergenerational context, as economic and political opportunities increase, immigrant youth aspire to assimilate into mainstream society, but identity-concerned parents sabotage their assimilation by restricting their choices and investments. This mechanism gives rise to model predictions that are consistent with all of the empirical facts we document.

In terms of questions addressed, our study has an important antecedent in the work of Fouka (2019), who shows how a specific assimilation policy in the US—the prohibition of German language in elementary schools after World War I—instigated a backlash whereby affected minority individuals were more likely to marry within their own ethnic group, more often chose distinctly German names for their offspring, and were less likely to volunteer in World War II. While that paper thinks about how limits placed by society on individuals’ identity affect assimilation outcomes, we consider whether parents’ proscriptions on their children’s choices can tighten as opportunities to assimilate into society increase. Another paper close in spirit to ours is Carlana et al. (2017), which studies a program called “Equality of Opportunity for Immigrant Students” that provided tutoring and career counseling. They find effects of the intervention on immigrant boys’ educational outcomes, driven by changes in academic motivation and teachers’ recommendations. In contrast, they find no effect for immigrant girls. Our paper also reveals a stark gender difference in response to increased opportunities, and provides both an explanation for why such differences might exist as well as estimates of the consequences for immigrant girls’ well-being and assimilation.

There is also important research studying the introduction of birthright citizenship in Germany for a different set of questions. This prior research has focused on how the policy affected either immigrant parents or their children, whereas we leverage the reform to study the clash of cultures between generations and the unintended effects this can have on welfare and assimilation. The three papers studying parental responses find that immigrant parents reduced their fertility and became more culturally assimilated (Avitabile et al. 2013, 2014), while labor market participation decreased for immigrant mothers (Sajons 2019). Felfe et al. (2020b) examine the policy’s impact on immigrant children, showing that it improved schooling outcomes around the time of preschool and primary school. While that study did not distinguish between male and female immigrants, in subsequent work, Felfe et al. (2020a) show that the positive reform effects, not only in terms of education but

also in terms of cooperation with natives, are an entirely male phenomenon. This prior work raises the puzzle of why boys seem to benefit but girls do not, a fact readily explained by our empirical analysis and identity model.

More broadly, our study is related to the literature on culture’s impact on economic outcomes. Several papers have used an epidemiological approach to study how cultural factors affect female work and fertility (Fernández 2007; Fernández and Fogli 2009), family living arrangements (Giuliano 2007), education (Figlio et al. 2019), and household saving behavior (Fuchs-Schündeln et al. 2020). Others have exploited natural experiments of history or within-country language borders to study the cultural determinants of female labor supply (Boelmann et al. 2020), gender identity (Steinhauer 2018), and domestic violence (Tur-Prats 2019). In addition, several papers have studied the economic implications of gender and racial identity (Bertrand et al. 2015, 2020; Bursztyn et al. 2017, 2018; Dahis et al. 2019; Fortin 2005, 2015; Ichino et al. 2019).

The main contribution of our paper is to document the unintended consequences of increasing opportunity for Muslim immigrant girls and highlight the role of intrafamily conflict. Our findings are a sobering illustration that increased opportunities are not offered to people in isolation of competing claims on the loyalty of a person. From a policy perspective, the fact that immigrant girls are made unintentionally worse off and less integrated after receiving birthright citizenship suggests other measures are needed to promote second-generation assimilation of females.

The remainder of the paper proceeds as follows. We begin by describing the birthright citizenship reform, our empirical strategy, and the data in Sections 2 and 3. In Section 4 we present results on the unintended consequences of the reform on well-being. In Section 5 we provide evidence on possible mechanisms. Section 6 develops a simple model with intergenerational identity concerns which rationalizes our findings. The final section concludes.

## 2 Policy Reform and Identification

### 2.1 Birthright Citizenship Reform

At the turn of the millennium, Germany undertook a major reform of its citizenship law. The most prominent aspect of this reform related to the acquisition of citizenship at birth. We discuss the background and details of this reform in some of our previous work (Felfe et al. 2020a,b), and so only briefly repeat the most relevant details here.

Prior to January 1, 2000, citizenship at birth was granted according to *jus sanguinis* (right of blood), i.e., children only gained German citizenship if at least one parent was naturalized. All other children were granted either temporary or permanent residency. In some ways, the rights attached to citizenship and permanent residency are similar, as both allow indefinite stays in Germany and access to the same welfare benefits. But the political and economic rights of citizens are more expansive: citizens are allowed to vote in general elections, apply for civil servant positions, and move and work freely across the EU. Permanent residents do not enjoy the same privileges. They can lose residency status if they leave the country for more than a year and can be deported

for committing a crime. Prior research also documents that naturalized immigrants have better employment opportunities, higher earnings, and steeper wage-tenure profiles (Bratsberg et al. 2002; Chiswick 1978; Fougère and Safi 2009; Gathmann and Keller 2018; Steinhardt 2012).

Starting January 1, 2000, the prevailing regime changed to a restricted version of *jus soli* (right of soil). Children qualified for German citizenship as long as one parent was a legal resident in Germany for eight years or more at the time of birth. Parents did not need to apply for naturalization; instead, German citizenship was automatically recorded on the child’s birth certificate. Importantly, parents could not refuse their child’s German citizenship. A transition rule applied for the year 2000, where parents residing in Germany for at least 8 years could apply for their existing children to become citizens. However, only a small fraction of parents took advantage of this (approximately one-sixth), possibly due to poor publicity or a low demand by parents for their children to become German citizens.<sup>2</sup>

## 2.2 Empirical Model

The reform of Germany’s citizenship law specifies a birthdate eligibility cutoff, which creates a discrete and plausibly exogenous shock to immigrant opportunity. We exploit the quasi-random assignment of birthright citizenship around the cutoff using a local difference-in-differences design. We model outcome  $Y_i$  for child  $i$  as:

$$Y_i = \beta_0 + \beta_1 \text{Immig}_i + \beta_2 \text{Post}_i + \beta_3 (\text{Immig}_i \times \text{Post}_i) + \gamma_m + \epsilon_i \quad (1)$$

where  $\text{Immig}_i$  is an indicator for whether a child is a second-generation immigrant versus a native and  $\text{Post}_i$  is an indicator for whether a child was born in the months after January 1, 2000. The coefficient of interest is  $\beta_3$ , which identifies the effect of the reform for immigrant children born after the policy cutoff. We include a set of birth month dummies ( $\gamma_m$ ) to capture any effects which are common to both natives and immigrants within the same birth month during the year. In extended specifications, we also include a vector of controls which contain a limited set of family characteristics (maternal and paternal age, maternal and paternal education) and city characteristics (city size and group-specific local unemployment rates).

An advantage of our estimation approach is that we are comparing students in the same grade as each other. Since the citizenship cutoff differentially impacts students within the same grade, it eliminates the confounding factor of how well-being depends on which grade a student is in. To make the estimate even more local, we also consider robustness checks which narrow the window around the reform. For our main welfare outcome (life satisfaction), we report what happens when the window narrows from  $\pm 6$  months around the cutoff to  $\pm 5$ ,  $\pm 4$ ,  $\pm 3$ , or  $\pm 2$  months. In a related robustness check, we also estimate a regression discontinuity model, using date of birth as the

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<sup>2</sup>The law originally allowed children with German citizenship to temporarily hold two passports until age 23. At that point, they would be required to choose German citizenship or their other citizenship. In 2014, this “option model” was relaxed even further, so that individuals now have the ability to hold dual citizenship even past the age of 23 (as long as they have lived in Germany for 8 years, attended German school for 6 years, or acquired formal education in Germany). Many origin countries for immigrants, such as Turkey, also allow for dual citizenship.

running variable. The RD estimates have standard errors which are roughly twice as large compared to our baseline specification, and similar to the standard errors with a window of  $\pm 2$  months. There is enough precision for our main welfare outcome that these robustness checks are informative. However, there is not enough power to use such a small window or an RD for the majority of our secondary outcomes.

Equation 1 captures the reduced form effect of the introduction of birthright citizenship, or alternatively, the intention-to-treat (ITT) effect. This ITT effect is a lower-bound estimate of the impact of citizenship at birth, since our sample includes pre-policy children who may have qualified for citizenship at birth through *jus sanguinis* or the transition rule. Moreover, our sample includes post-policy children whose parents did not meet the 8 year residency requirement and hence these children were ineligible for birthright citizenship.

### 2.3 Effect of the Reform on Citizenship at Birth

To gauge the magnitude of the reform’s effect on citizenship at birth, we use the German Microcensus from 2001. This dataset has information on both parent and child citizenship around the time of the child’s birth, country of birth and length of residence. We define second-generation immigrant children as those born in Germany to parents who are both foreign born. Using second-generation immigrant children in the 2001 survey wave who were recently born, we find the reform substantially increased the fraction who acquired German nationality at birth. As illustrated in panel A of Figure 1, 28% of second-generation children born pre-reform qualified for citizenship either from *jus sanguinis* (right of blood) or the transition rule, while 80% of children born post-policy qualified for German citizenship from *jus soli* (right of soil).<sup>3</sup> In order to obtain back-of-the-envelope estimates of the local average treatment effect of endowing immigrant children with citizenship at birth, this gap of 52 percentage points means we need to scale our reduced form coefficients by a factor of 1.9.

An alternative estimate for the effect of the reform can be constructed using questions we asked in our own survey. We asked students whether they were born in Germany, whether they are citizens, and if so, when they acquired citizenship (at birth or later on), as well as the year each of their parents arrived in Germany. This series of questions can be used to construct a measure of citizenship at birth. Imputing citizenship status at birth based on combinations of the questions, we find a similarly large jump in birthright citizenship.<sup>4</sup> Panel B of Figure 1 shows a jump of 43 percentage points (78%-35%).

We view the German Microcensus estimate as more reliable since it is based on parental reports near the time of the birth, and will use it when we talk about how to scale our main reduced form estimates. But an advantage of our survey measure is that it can be calculated for Muslim

<sup>3</sup>These estimates are based on families where both the mother and the father reside in the household.

<sup>4</sup>We correct two obvious inconsistencies when imputing status. First, if children say they have held citizenship since birth, but are born prior to the reform and neither of their parents fulfilled the residency requirement, we reclassify them as not having birthright citizenship. This accounts for 3% of the data. Second, if children say they are not citizens, but report being born in Germany post-reform and have a parent who fulfilled the residency requirement, we assign them citizenship-at-birth status. This accounts for 4% of the data.



versus non-Muslim immigrants. For second-generation children with a Muslim background, the gap using our survey measure is 54 percentage points (83%-29%), while for non-Muslims the gap is smaller at 27 percentage points (73%-46%). Taking the estimate from the German Microcensus as accurate, and our survey measure as biased downwards, we can inflate the Muslim survey measure by multiplying 54 percentage points by 52 divided by 43. This yields an inflated Muslim-specific jump of 65 percentage points, implying a reduced form scaling factor of 1.5 for this group.<sup>5</sup> When reporting results in tables, we focus on the reduced form, but keep these scaling factors in mind when interpreting the magnitudes of the estimates.

## 2.4 Threats to Identification

A useful feature of the reform is that the birthdate cutoff occurs in between school year cutoffs, so we are able to compare individuals born earlier versus later within the same school grade. Even so, one possible threat to identification is bias due to age or season of birth effects within a school year. Age and season of birth could matter for two reasons. First, older children might do better academically (Cascio and Lewis 2006; Black et al. 2011; Cornelissen and Dustmann 2019) or differ on other margins such as self confidence or competitiveness (Page et al. 2019) and these factors could influence the outcomes we study. Second, socioeconomic characteristics of parents have been shown to change over the year (Buckles and Hungerman 2013; Carlsson et al. 2015), so children born at different times may not have comparable backgrounds. We address issues related to age and season of birth in a variety of ways, including the use of natives as controls, fixed effects for birth month, and robustness to narrower sample windows.

A conceptually distinct, but additional age-related concern, is manipulation. Since our identification strategy relies on a birthdate cutoff, the worry is strategic fertility choices. We explore this by narrowing the sample window so that children were all conceived in advance of the reform’s ratification date. We also implement a “donut” strategy that drops children born in the 2-week window around January 1, 2000. This avoids potential selection into treatment through birth-date manipulation by parents via the postponement of inductions or elective c-sections.<sup>6</sup>

## 3 Survey Data

Our analysis is based on data we collected to assess the effects of the introduction of birthright citizenship in Germany. This data collection (i) took place between June and November 2015, (ii) covered 15 to 16 year old students in 219 classes in 57 German schools (all in their final year of compulsory schooling), and (iii) included both a traditional survey and a lab-in-the-field experiment

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<sup>5</sup>To provide insight into the precision of the scaling factor, which is a non-linear combination of survey-based estimators, we calculated its standard error using the bootstrap and found it to be .13.

<sup>6</sup>Another way to assess self-selection into treatment is by looking at whether immigrants time their births differently compared to natives, who were not affected by the reform. We regress a dummy variable for immigrant status on birth month dummies. The resulting p-value for the joint test without any control variables is .25. Including the baseline set of controls we will use in our regressions, the p-value rises further to .57.

on cooperation. In this paper, we only draw upon our survey data. Felfe et al. (2020a) reports results for the lab-in-the-field experiment; much of the following data description is taken from that work, which provides more detail.

### 3.1 Survey Design

Our data collection took place in two German federal states: Schleswig-Holstein (SH) and North Rhine-Westphalia (NRW). In a first step, we asked the respective ministries of education for permission to collect data at all secondary schools in selected cities.<sup>7</sup> In a second step, the two ministries strongly encouraged the principals of the targeted schools to participate. In a third step, we contacted the principals of all targeted school directly and asked for formal permission to conduct the experiment and survey at their school.

Out of the 92 contacted schools, 57 agreed to participate (62%). This is a relatively high response rate compared to other school-based surveys (the CILS4EU and the NEPS Starting Cohort 3, respectively, have 53% and 30% participation rates). We were able to work with such a large fraction of schools due to the support of and collaboration with the ministries of education in SH and NRW. Our final sample covers all types of secondary schools: 10 secondary general schools (“Hauptschule“), 8 intermediate (“Realschule“), 29 comprehensive without the final years of grammar education (“Gesamtschule ohne gymnasialer Oberstufe“), 8 comprehensive with the final years of grammar education (“Gesamtschule mit gymnasialer Oberstufe“), and 2 grammar schools or high schools (“Gymnasium“).

We collected the data in two waves. The school year runs from August/September to June/July. Prior to the summer break (June 2 to July 16), we surveyed all 9th graders from 31 schools (spread over 122 classes) in SH. After the summer break (October 19 to November 16), we surveyed all 10th graders of 26 schools (spread over 97 classes) in NRW.<sup>8</sup> We conducted the study at the school class-level in regular classrooms during two regular consecutive class periods (45 minutes each). One class period was used for the survey, while the other for the lab experiment discussed in Felfe et al. (2020a). The order of the survey versus the lab experiments was randomly assigned each day. The survey was done using pen and paper, and to increase privacy, mobile screens were set up between students.

Our target populations give us a single school cohort of children primarily born in 1999 and 2000. This allows us to exploit the introduction of birthright citizenship in Germany on January 1, 2000. An advantage of our design is that the introduction of birthright citizenship falls in the middle of the school year, whereas the school starting age cutoffs occur in the summer.

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<sup>7</sup>The cities in SH are Flensburg, Kiel, Lübeck, Neumünster, Elmshorn, and Pinneberg, with populations ranging from 42,266 to 246,306. The cities in NRW are Duisburg and Wuppertal, with populations of 491,231 and 350,046.

<sup>8</sup>Compulsory education lasts nine years in SH and ten years in NRW.

### 3.2 Analysis Sample

Two weeks prior to the study, parents received a letter announcing our study, but not containing any information about its specific objectives. Parents were given an opt-out option, i.e., they could proscribe their children’s participation. In addition, students present in class had the option to drop out of the study at any time. On the days we conducted the study, a total of 4,634 students were present in the 219 classes. Parents made use of an opt-out option for 44 students (less than 1%), while 154 students (3.5%) chose to opt out themselves. The high final response rate of roughly 95.5% in part reflects that it was conducted with the school’s cooperation during regular school hours.<sup>9</sup> Thus, 4,436 students participated in the study. Of those, 270 did not provide the survey information necessary for our analysis (i.e., own gender, birthdate, country of birth or parental migration background). This leaves us with a baseline sample of 4,166 students.

The survey provides information, *inter alia*, about participants’ date of birth, country of birth, gender, religion, well-being, aspirations, preferences and interests, personality traits, school achievements, and family background. Two key family background variables are the birthplaces of both parents, which we use to categorize participants into three groups: (i) native children, whose parents are both German-born; (ii) immigrant children, whose parents are both foreign-born; and (iii) mixed-background children, who have one German-born and one foreign-born parent. Overall, according to our definitions, the sample comprises 2,250 native children (54%), 1,260 immigrant children (30%) and 672 mixed-background children (16%). Roughly 77% of all immigrant children in our sample are German-born (i.e., second-generation immigrants), while 23% are foreign-born (i.e., first-generation immigrants).

For the current paper, we restrict the baseline sample along several dimensions. We drop first-generation immigrants, since the introduction of birthright citizenship only affected second-generation immigrant children. Mixed-background children are not used in the analysis, as we cannot determine whether they were affected by the reform. We draw upon native German children (i.e., those whose parents are both German) as a control group. Since our identification strategy centers around the birthdate cutoff of January 1, 2000, we only retain second-generation immigrants and native German children born in a  $\pm 6$ -month window of this cutoff. This leaves us with a sample of 598 second-generation immigrant children and 1,535 native German children.

### 3.3 Summary Statistics

Appendix Table A1 reports summary statistics for our estimation sample of native and immigrant children, broken down by gender. Natives and immigrants differ along several dimensions. Second-generation immigrant boys and girls have less educated and younger parents compared to their native counterparts. Immigrant children are also concentrated in bigger cities ( $>100,000$  residents).

The third and fourth columns of Appendix Table A1 further break down immigrants into two groups: those born before the cutoff date of January 1 (and hence ineligible for birthright

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<sup>9</sup>We have no information on non-participating students and so cannot comment on differential non-response.

citizenship) and those born after. There are no statistically significant differences in any of the background characteristics for girls in the top panel. For boys, there is a small difference in regional unemployment and mother’s education being at the middle level. The finding of two estimates significant at the 10% level and one at the 5% level is roughly what one would expect by chance for these 30 variables. We conclude that immigrants born pre and post reform appear to be similar to each other on average, although we cannot rule out selection on unobservables.

Additional summary statistics for religion and mother’s country of origin are found in Appendix Table A2. For natives, the dominant religion is Protestant, with some Catholics and some claiming no religious affiliation. In sharp contrast, 60% of immigrants are Muslim, and only 3% claim no religious affiliation. Turning to the mother’s country of origin, the largest group of immigrant mothers come from Turkey (42%). Other common origins include Balkan, European, and Post-Soviet bloc countries. We will use both of these variables later to define “traditional” families as those in which the children report being Muslim or as having parents who migrated from predominantly Muslim countries.

For reference, descriptive statistics for the dependent variables can be found in Table A3. We explain the construction of these outcome variables as we discuss our empirical findings.

## 4 Unintended Consequences

Policymakers promoted birthright citizenship as a reform that would help immigrant youth integrate into society and improve their well-being. In this section, we document that the opposite occurred for immigrant girls, with unwanted effects concentrated especially among those with a Muslim background. We use self-reported life satisfaction as our main measure of well-being, supplemented with related questions on self-esteem. For assimilation outcomes we use measures of social integration and German self-identification.

### 4.1 Birthright Citizenship and Well-Being

**4.1.1 Life Satisfaction.** A standard approach to measuring well-being in a survey is to ask respondents directly how they think things are going in their life. Our primary measure of well-being was prefaced with the statement: *“Finally, we would like to ask you about your satisfaction with your life as a whole!”* We then asked the question: *“Overall, how satisfied are you with your life?”* Respondents were given a visual scale with 11 boxes ordered on a line to choose from. The left side of the scale, which started at 0, was labeled *completely dissatisfied* and the right side of the scale, which ended at 10, was labeled *completely satisfied*. Respondents checked the appropriate box to indicate their answer. We also asked two follow-up questions about life satisfaction in the future: *“And what do you think, what will it be in a year?”* and *“And what do you think, what will it be in five years?”* These questions were answered on the same scale. We delay talking about future life satisfaction until the end of the paper.

This type of life satisfaction question has the advantage of being asked as a simple, single

question. It is meant to capture a global measure of subjective well-being across all areas of one’s life and has been asked in a variety of ways.<sup>10</sup> Life satisfaction measures have been shown to correlate with measures of mental health, and predict future behavior such as suicide attempts.<sup>11</sup> It is advocated by the Sen-Stiglitz-Fitoussi report, which in general argues for the use of many indicators in a policy dashboard, but considers life satisfaction as the best all-encompassing single measure (see Stiglitz et al. 2017). To ease interpretation, we transform our life satisfaction variable so that it is mean 0 and has a standard deviation of 1 for the sample of natives of both genders. Average life satisfaction for natives in our survey is 7.46 on the 0 to 10 scale, with a standard deviation of 2.27.

We begin with a graphical presentation of how the policy reform affected children’s life satisfaction. Figure 2 plots average life satisfaction for those born before versus after the reform, separately by both immigrant status and gender. The blue dots are for those born before the policy reform, while the red dots are for those born afterwards. The graphs plot pre-post comparisons for various window widths around the reform date. The first thing which stands out in Figure 2 is that immigrant girls born post-reform report substantially lower life satisfaction compared to those born before (panel A). This is true regardless of the sample window. The second thing to notice is that the reform seems to have had little effects on immigrant boys’ life satisfaction (panel B). Third, native girls born before versus after the cutoff have similar life satisfaction, and the same is true for native boys (panels C and D).<sup>12</sup> This is not surprising since the reform did not apply to natives.

Table 1 presents regression results analogous to the figures with an age window of  $\pm 6$ , using the model described in equation 1. The first regression specification includes no additional controls and confirms that immigrant girls born post-reform suffer a sizable reduction in well-being. Immigrant girls born after the birthright citizenship reform are .31 standard deviations less satisfied with their life, a drop which is statistically significant. As a reminder, these are reduced form estimates, and as discussed in Section 2.2, to get the effect of birthright citizenship, they should be scaled up by roughly 1.9, for a scaled effect of .59 standard deviations.

One way to think about the size of the estimate is to calculate the change not in standard deviation terms, but rather as the percent decrease in the raw index, which has a mean of 7.46 on a 0-10 point scale. The scaled drop is 1.34, which represents a 16 percent decrease on the raw scale ( $1.34/7.46$ ). To put this in perspective, this effect size is similar in magnitude to the effect of a medium-level depression on life satisfaction (Frijters et al. 2020) and larger than the effect of moving from rich to poor (Clark et al. 2018).

Column 1 does not account for more granular age or season of birth effects which are common

<sup>10</sup>See Linton et al. (2016). Variants compared to our question introduce slight wording changes, differences in the labeling and range of the Likert response scale, and difference in the reference time period. For example, the World Happiness Report, which is conducted annually by the United Nations (Helliwell et al. 2019), uses a Cantril ladder survey, where individuals are asked to think of the rungs of a ladder, with the best possible life being a 10 and the worst possible life being a 0. Related life satisfaction measures, such as the Satisfaction with Life Scale (Diener et al. 1985), ask a series of questions and create an index based on the responses.

<sup>11</sup>See Frijters et al. 2020 for key estimates from the literature on the determinants of life satisfaction.

<sup>12</sup>Another interesting pattern is that native girls report lower life satisfaction compared to the other three groups, a pattern also observed by other researchers in recent years (Zorlu and Frijters 2019).

to both immigrants and natives. Adding in birth month fixed effects does little to alter the estimates appearing in column 2. In columns 3 and 4, we further include a basic set of controls for family characteristics (parental education and age) and regional characteristics (group specific local unemployment and city size).<sup>13</sup> These additions do not change the estimates appreciably.

Turning to panel B for boys, we find that the introduction of birthright citizenship increased life satisfaction by a little over one-tenth of a standard deviation. But this result is not statistically significant. As with girls, the introduction of controls does little to change the estimates. If we had not allowed the birthright citizenship effect to vary by gender, we would have estimated a statistically insignificant overall effect of  $-.086$  (s.e.=.095). This combined estimate would have given an incomplete picture of the effects of the reform.

**4.1.2 Robustness.** To explore the robustness of our main finding that birthright citizenship decreased immigrant girls' well-being, we estimate a variety of additional specifications. To start, in Table 2 we narrow the sample window as we did in Figure 2, and run regressions which include the full set of controls (Table 1, column 4 specification). By narrowing the sample window, we make the estimate more local to the cutoff, minimizing concerns that differential age or season of birth effects between immigrants and natives are driving our results.

In each column of Table 2 we shrink the window by 1 month on each side of the cutoff. In the first column of panel A, with a window of  $\pm 5$  months, there is little change in the estimate and a small increase in the standard error. As we move across the columns, the standard error continues to increase, and the estimates are larger compared to the baseline window. The estimates remain statistically significant, but by the time we have a  $\pm 2$  month window, the standard error has increased by 75 percent (consistent with the sample size falling by almost a third). In panel B for boys, the different windows do not materially affect the results either.

In Table 3 we report additional robustness checks. In the first specification, we use a regression discontinuity estimator. We limit the sample to immigrants, use birthdate in days as the running variable, and have January 1, 2000 as the cutoff. We estimate the RD using triangular weights, separate linear trends to the left and right of the cutoff, and the full sample window of  $\pm 6$  months. The RD estimate of the reform is  $-.488$ , which is larger than our baseline estimate appearing in Table 1, but similar to the more local estimates in Table 2 which restrict the window to be  $\pm 4$  months or less. In column 2 we estimate a difference in RDs, where we subtract the jump at the cutoff for natives from the jump at the cutoff for immigrants. This effect is even larger, and remains statistically significant.

A natural question is why we don't use an RD specification as our baseline specification throughout the paper. The answer is revealed by looking at the standard error of the RD estimate in column 1, which is substantially larger compared to our baseline approach (s.e. = .255 versus .139). To make another comparison, the standard error of the RD estimate is close to the standard

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<sup>13</sup>While we have other family characteristics from the survey, such as whether the parents are divorced, we do not include them in the regression as controls, as they could be endogenous to the cutoff. Including these potentially endogenous covariates has little effect on the estimates, however.

error which uses a  $\pm 2$  month window in Table 2 (s.e. = .255 versus .244). While the RD estimate for our main outcome variable of life satisfaction is precise enough to be informative in Table 3, once we start to split the sample or look at other outcomes, the RD standard errors generally become too large to permit a useful analysis.

Turning to column 3, we perform a different type of robustness check. We estimate the baseline difference-in-differences model, but use first generation immigrants as the control group since they were not treated by the policy reform. We find a drop in life satisfaction equal to half a standard deviation, a result significant at the 10% level. The estimate is imprecise because we have few first generation immigrants in our sample. As an alternative, we use both natives and first generation immigrants as the control group in column 4 and find a similar estimate to the baseline. In column 5 we add in school fixed effects (57 schools), and find similar results. In column 6, we estimate our baseline model, but instead of including birth month fixed effects, we control for age in days. The estimate is similar to the baseline estimate reported in Table 1. Column 7 allows for a donut hole, where observations within 1 week on either side of the cutoff are excluded. This is to deal with mothers who may have strategically delayed birth to obtain citizenship for their child. The results remain virtually unchanged. As a reminder, narrowing the sample window to be within  $\pm 3$  months of the reform, as we did in the prior table, deals with parents who may have strategically tried to time conception, as the reform was not announced until July 1999. In the last column, we cluster our standard errors at the school level. This has little effect on the standard errors.

As a different robustness check, in Appendix Table A4, we report results using an interaction term with gender (i.e., a three-way-interaction) instead of splitting the sample by gender. The estimated effect ( $-.418 + .115 = -.303$ ) is similar to baseline. Finally, we note that in our analysis, we treat life satisfaction as measured on a ratio scale, which makes means, variances, and OLS regression coefficients admissible statistics. As another robustness check, we use an ordered logit model for the 11 possible answers of the 0-10 life satisfaction scale. The estimated odds ratio for immigrants girls born postreform is .625 (s.e.=.151), which implies a similarly-sized negative effect as in the baseline model.

Throughout the paper, we estimate effects for a total of 21 unique outcomes for immigrant (Muslim) girls. Our statistical inference is robust to multiple inference adjustments for this set of outcomes using the false discovery rate control (Newson 2010). After adjusting the p-values (using the qqvalue package in Stata and the simes method), 12 out of the 14 estimates which were statistically significant at the 5% level remain so, with the other two dropping to the 10% significance level. Likewise, 1 out of the 2 estimates which were statistically significant at the 10% level remain so.<sup>14</sup>

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<sup>14</sup>The total of 21 different outcomes does not include the estimates using different sets of control variables in columns 1-3 of Table 1, the battery of robustness tests for the same outcome of life satisfaction in Tables 2 and 3, or the estimates in Table 10 (two of which are hypothesized to have no effect and two of which represent a sample split for life satisfaction). Note that including all 40 of the outcomes instead of the 21 unique outcomes does not change our conclusion.

**4.1.3 Alternative Measures of Well-Being.** We now explore an alternative set of well-being measures. In particular, we asked students a series of questions assessing their self-esteem, which are intended to capture a person’s overall sense of self-worth or personal value. Self-esteem is particularly relevant as a measure of well-being for teenagers. It differs conceptually from life satisfaction in that it is meant to capture confidence and satisfaction with oneself, rather than satisfaction with life more generally. The two measures are related, with self-esteem being found to be a strong predictor of life satisfaction (e.g., Diener and Diener 1995). Self-esteem has recently been argued to be a crucial component in identity theory, as it is the esteem placed on one’s own identity (Akerlof 2017; Kranton 2016).

The self-esteem portion of our survey began with the preface “*How well do the following statements apply to you?*” We listed five statements which are generally viewed as indicators of positive self-esteem: (i) *Overall, I am satisfied with myself*, (ii) *I have many positive character traits*, (iii) *I am as capable as other people*, (iv) *I am a person with value and self-worth*, and (v) *I have a positive attitude towards myself*. Respondents were given 6 boxes ordered on a line to chose from. The left side of the scale, which started at 1, was labeled “*not at all accurate*” and the right side of the scale, which ended at 6, was labeled “*completely accurate*”.

As we did for the life satisfaction question, for ease of interpretation we normalize the responses to be mean 0 and standard deviation 1 relative to the population of all natives. We further create an aggregate index which sums across these 5 normalized variables, and divides by 5. Our positive self-esteem index has a correlation of .54 with our life satisfaction measure.

Results using these positive self-esteem measures as outcomes can be found in Table 4. Start with panel A, which reports results for girls. As we found for life satisfaction, the self-esteem index is higher for immigrants compared to their native counterparts. And the index similarly falls for immigrant girls born post-reform: self-esteem falls by one-fourth of a standard deviation, a drop which is statistically significant. All five of the individual components entering the index have large and negative coefficients, two of which are significant at the 5% level.

Turning to panel B for boys, the effect of the reform flips signs. The self-esteem index rises by .17 standard deviation for immigrant boys born after the reform relative to their native counterparts. And each of the components of the index has a sizable and positive estimated coefficient, although only one is statistically significant at the 10% level.<sup>15</sup>

This alternative measure of well-being comports well with those reported using life satisfaction. Both sets of results document a drop in well-being for immigrant girls, but not immigrant boys.

**4.1.4 Heterogeneity by Religious Background.** One of the most salient cultural identities in many countries is religion. There are particularly stark differences in gender norms in Muslim

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<sup>15</sup>In the survey, we also listed five statements which are generally viewed as indicators of negative self-esteem. When we run similar regressions using a negative self-esteem index (a larger score here means worse self-esteem), the post-reform x immigrant interaction term is .091 (s.e.=.121) for girls and -.164 (s.e.=.103) for boys. So while insignificant, the results broadly line up with those for the positive self-esteem index. The fact that the positive self-esteem index is statistically significant, while negative self-esteem is not, is likely due to negative self-esteem being a less clear concept and thus less well predicted (see Schmitt and Allik 2005; Chyung et al. 2018).



and non-Muslim societies (Alba 2005; Bisin et al. 2008; Ersanilli 2012). To illustrate these stark differences in the context of Germany, we draw upon the Global Gender Gap Index constructed by the World Economic Forum. Appendix Table A5 displays the overall gender gap index as well as the various subindices based on the mother’s country of birth. For each child in our sample, we assign them the gender index associated with their mother’s birth country, and report the average of the indices weighted by the number of children.

For native children, whose mothers are all from Germany, the overall gender gap index is .75, which has the interpretation that there are 25 percent fewer resources and opportunities available to German women compared to German men. Looking at the subindices, the gender gap is larger for access to economic opportunities and participation and political leadership, while the gap has basically closed in terms of educational attainment and health.

We next split second-generation immigrant children into two groups: those who report their religion as Muslim versus non-Muslim. Non-Muslim immigrant children have mothers who come predominantly from Poland and Russia. Gender disparities are already more pronounced for this weighted set of countries compared to Germany: the overall gender gap index amounts to .67 and the gap is again most pronounced when it comes to access to economic opportunities and representation in political leadership positions. There are only small disparities in terms of education or health.

Mothers of Muslim immigrant children come predominantly from Turkey, but also commonly from Iraq, Lebanon, and Morocco. In these countries, women’s access to opportunities and resources is severely restricted. The overall global gender index is .59, with the biggest gap existing in the sphere of political empowerment (.06), followed by a gap in economic opportunities (.44). There is also a gender gap in terms of educational attainment (.89), a gap which does not exist for native Germans or non-Muslim immigrants.

This large discrepancy in gender norms motivates a heterogeneity analysis by religious affiliation. In Table 5, we repeat our baseline specification, but estimate separate regressions for second-generation Muslim and non-Muslim immigrant children. This stratification by religion does not reveal any clear heterogeneities for boys. In contrast, the subgroup results for girls are striking. The citizenship reform had at most a modest, but insignificant effect on the well-being of non-Muslim immigrant girls. Among Muslim immigrant girls, however, it caused a significant drop in life satisfaction of almost half of a standard deviation. In other words, the reduction in life satisfaction observed around the introduction of birthright citizenship is an entirely Muslim-girl phenomenon. Although not shown, a similar heterogeneity by religion shows up for the index of positive self-esteem.

In Appendix Table A6, we further probe the effects we found for Muslim immigrant boys and girls. While unlikely, a possible concern is that being born before versus after the reform changes one’s religion from Muslim to something else. To explore whether this affects our results, we use a predicted Muslim measure based on exogenous characteristics instead. Using the sample of second-generation immigrants, we first regress the self-reported Muslim variable on dummy variables for the countries of origin for both mothers and fathers. Using these estimated coefficients, for each child we predict the probability they are Muslim. We then estimate the effect of the reform for

the sample of immigrant children whose predicted probability of being Muslim is over 75%. These results are reported in column 1. The estimate for immigrant girls who are predicted to be Muslim with high probability is a statistically significant  $-.412$ , which is comparable to the estimate based on self-reported Muslim affiliation. As another approach, in column 2 we restrict the sample to immigrant children whose mother and father both come from either Turkey or the Middle East, regions which are predominantly Muslim. The estimated effect of the reform on girls from these countries is  $-.362$  and statistically significant.<sup>16</sup>

## 4.2 Birthright Citizenship and Integration

Having documented the unintended consequences of birthright citizenship on well-being, we now turn to measures of social integration and German identity.

**4.2.1 Social Integration.** Our first measure of integration is an index of social participation based on the number of extracurricular activities a child has ever participated in. The four activities include sports, band/orchestra, theater, and the school newspaper. Our index simply counts the number activity types a child has ever participated in, and therefore ranges from 0 to a maximum of 4 (natives participate in .90 activities on average). These voluntary activities create increased interactions with native children in settings not directly related to normal academic studies, and arguably increase a sense of social belonging.

Table 6 reveals that immigrant Muslim girls born after the reform participate in .30 fewer activities compared to those born before the reform, a result which is statistically significant. Scaled by the first stage, this is a drop of almost half of an activity. Muslim immigrant boys born after the reform, in contrast, increase their participation by .23 activities. We interpret these results as showing that when Muslim girls gain birthright citizenship, they participate in fewer extracurricular activities with natives, which is opposite of the goal of many policymakers.

To provide further insight, in columns 3 through 6 we probe how birthright citizenship affects feelings of loneliness and one’s friendship network. In the survey, we listed a series of statements prefaced by “*These statements describe your relationship with friends and other people. Please tell us, on the following scale, how much the statements apply to you.*” We then listed the following statements: (i) *It’s easy for me to make new friends*, (ii) *I often feel lonely*, (iii) *I want to have more contact with others*, (iv) *My circle of friends and I do a lot together*, and (v) *I receive support from my circle of friends when I have worries and problems*. The left side of the scale, which started at 1, was labeled “*not at all accurate*” and the right side of the scale, which ended at 6, was labeled “*completely accurate*”. As we did for the life satisfaction question, for ease of interpretation we normalize the responses to be mean 0 and standard deviation 1 relative to the population of all natives. We use the sum of the first of these three normalized variables (i-iii) divided by three to create a “loneliness index” and the last two normalized variables summed together and divided by

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<sup>16</sup>If we instead use whether the mother is from Turkey or the Middle East the estimate is  $-.385$  (s.e.=.188) or whether the father is from Turkey or the Middle East the estimate is  $-.315$  (s.e.=.185).

two to create a “friendship support” index (iv-v).

Starting with the friendship index, we find that immigrant Muslim girls and boys have stronger friendship networks, with Muslim immigrant girls born before the reform scoring almost a quarter of a standard deviation higher on average compared to natives. But this friendship support index falls by 27 percent of a standard deviation for immigrant Muslim girls exposed to birthright citizenship; scaled by the first stage, this is a decrease of .40 standard deviations, making them even worse off than native girls. Turning to the loneliness index, birthright citizenship increases loneliness for Muslim immigrant girls born after the reform, but the estimate is not statistically significant.

The drop in friendship support could be related to a variety of factors. First, it could relate directly to the drop in life satisfaction: as individuals become unhappier and depression rises, they often withdraw from their friends. It could also be related to the reduced number of extracurricular activities we document in the first column of Table 6. Finally, a drop in assimilation could limit the number of potential native friends.

**German Identity.** The remaining columns in Table 6 examine how birthright citizenship affects German identity. Columns 7 and 8 are based on a survey question which asks: “*Generally speaking, how much do you identify as a German?*” Respondents could answer “*fully*”, “*mostly*”, “*in some ways*”, “*barely*”, or “*not at all*”. We create a dummy variable for German identity which equals one if the respondent answered fully or mostly.

Not surprisingly, a large fraction of natives self-identify as German (87%). In contrast, only 36% of Muslim immigrant girls born pre-reform think of themselves as German. This self-identification as German drops even further for the Muslim immigrant girls born after birthright citizenship became law. Fourteen percent fewer of these girls think of themselves as German, so that now only 22% identify as German.

This result is notable given that 83% of second-generation Muslims born after the reform were German citizens at birth, compared to only 29% born before the reform. Apparently, being granted German citizenship does not increase feelings of being German for this group of girls. There is no corresponding shift in German self-identification for the other groups in the table.

## 5 Evidence on Mechanisms

In this section, we explore what factors might explain the unintended consequences we document. There are many potential mechanisms, but to provide a parsimonious picture, we first focus on the one suggested by our motivating example: intergenerational conflict arising when immigrant children’s aspirations to assimilate into mainstream culture clash with parents perceiving this as a threat to the values of their own culture. This form of intergenerational conflict, often referred to as dissonant acculturation, has been argued to have a strong gender component: daughters are expected to embody traditional ideals more than sons, and parents try to enforce these gendered expectations when their daughters face a risk of integration (e.g., Dion and Dion 2001; Suárez-Orozco

and Qin 2006). Of course, other mechanisms could also be in play and are not necessarily mutually exclusive; we briefly examine three other possibilities at the end of this section. Moreover, we note that since the survey took place only once, we cannot chronologically order the different effects we document below.

## 5.1 Intergenerational Conflict

The introduction of birthright citizenship can be seen as a positive shock to the return to education for immigrant children. Thus, in the absence of dissonant acculturation, immigrant children granted citizenship should receive increased parental schooling support, do better in school, have higher educational and career aspirations, and have confidence in their ability to achieve these goals. However, daughters whose parents see career success as a threat to traditional culture will have a harder time taking advantage of the opportunities presented to them, as their parents will not support—or may even sabotage—their educational and labor market goals. As a consequence, school performance could fall and disillusionment about professional success and family-career tradeoffs could set in. We now test this mechanism, finding evidence consistent with the observed drops in child well-being and integration for immigrant Muslim girls.

**5.1.1 Parental Schooling Investments, Academic Achievement, and Language Spoken at Home.** If traditional parents do not want their daughter to take advantage of the increased opportunities provided by birthright citizenship, one way to limit integration is by undermining educational investments. We investigate this using a question which asks *“Do your parents support you in your homework and learning?”* Students could answer that either their mother, their father, both parents or neither parent supports them. We create an indicator for not receiving schooling support from either parent. We view this measure as capturing a low level of parental investment in their child’s education.

The results are striking. Column 1 in the top panel of Table 7 reveals that for Muslim immigrant daughters affected by the reform, parents’ schooling investments fall by a statistically significant 16 percentage points. This is a sizable drop. When scaled by the first stage effect of the reform on birthright citizenship, it implies a lowering of parental support of 23 percentage points (a 38% drop relative to their non-naturalized peers). This drop is limited to immigrant Muslim girls, with the other immigrant groups experiencing, if anything, a rise in parental investments (with statistically significant increases for non-Muslim immigrant boys).

Does this withdrawal of parental schooling support for immigrant Muslim girls translate into lower academic achievement? We answer this question by looking at self-reported grades for math and German, which are measured on a discrete scale from 1 (very good) to 6 (insufficient). Grades roughly follow a discretized normal distribution centered around 3. We classify low academic achievement as having average grades above 3. Our outcome variable in columns 3 and 4 is defined as the interaction of receiving no schooling support from parents with low academic achievement. Starting with Muslim immigrant girls, we find those born after the reform are 14 percentage points

more likely to receive no schooling support from their parents and have low academic achievement. In contrast, the estimated effect of the citizenship reform for the other groups goes in the other direction, and is statistically significant for non-Muslim boys (the same group which experienced a statistically significant increase in parental support).

To aid in interpretation, consider the link between birthright citizenship and academic success conditional on receiving parental support, which is not reported in the table. Muslim immigrant girls whose parents do support them in school have a 15 percentage point reduction in receiving low grades if they are born after the reform, consistent with these girls wanting to work harder and being able to do so with parental support. Of course, conditioning on parental support is endogenous, and indeed these investments fell on average in response to the reform, so this is not a causal estimate. But it points towards immigrant girls wanting to take advantage of citizenship, but only being able to do so with parental support.

Another way parents can push their child to be more traditional, and maintain the culture of their home country, is to speak their native tongue with their child instead of German. On our survey we asked “*What language do you speak with your mother?*” and “*What language do you speak with your father?*” We create a dummy variable which equals 1 if the child only speaks a language other than German with either one or both parents interacted with whether the child lives with them. We view this measure as capturing an investment in traditional culture. A parent’s refusal to speak German with their child may transmit cultural priorities, and could further isolate a child from natives (for example, if they have a German-only speaking friend over to their house).

As expected, natives rarely say they never speak German with their parents (less than .3 percent, regardless of gender). But for Muslim immigrant girls born pre-reform, 24% never speak German with one or both parents. This rises by 8 percentage points for Muslim immigrant girls exposed to the birthright citizenship reform.<sup>17</sup> Scaled by the first stage, this estimate indicates a 50% increase in the fraction where at least one parent does not speak German with their daughter. In contrast, there is no effect for non-Muslim immigrant girls born after the reform. Completing the picture, there is no significant effect for Muslim immigrant sons, while for non-Muslim immigrant sons we observe a significant increase. The latter finding might reflect an investment in sons’ bilinguality, which would be consistent with the increased parental schooling support for non-Muslim immigrant boys.

These results, combined with those in the next two subsections, align well with our intergenerational identity model for traditional parents. They suggest that parents of immigrant Muslim girls sabotage their daughter’s educational and professional ambitions by withdrawing support and instead pushing for the more traditional role of being a mother and homemaker. The language results likewise point towards a greater emphasis on traditional versus mainstream culture.<sup>18</sup>

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<sup>17</sup>If we alternatively create two separate dummy variables for never speaking German with the mother or never speaking German with the father, the effect is roughly twice as large for mothers (estimate=.067, s.e.=.029) as for fathers (estimate=.034, s.e.=.027).

<sup>18</sup>Another possible source of family conflict is that parents try to influence the choice of which citizenship their child would adopt at age 23. By the time of our study, dual citizenship was allowed as the option model was abolished in 2014 (see footnote 2). It could still have triggered conflict in the years leading up to our study, however, which would

**5.1.2 Disillusionment.** Intergenerational conflict arises when daughters’ aspirations differ from their parents. We start by looking at educational aspirations, which are analyzed in the first two columns of Table 8. We define an indicator variable for high educational aspirations which equals one if the child states they would like to pursue a university education. Fifty-six percent of all native girls and 62 percent of Muslim immigrant girls born pre-reform have high educational aspirations. The fact that immigrants have higher aspirations compared to natives is known as the “immigrant aspiration paradox” (e.g., Salikutluk 2016). Being born after the reform raises this probability by 11 percentage points for Muslim immigrant girls. While this is a large effect, it is not statistically significant (p-value=.16).

Even more interesting are the results in columns 3 and 4. We asked respondents two questions: “*How likely is it that you will be able to complete the training or education required for your desired profession?*” and “*How likely is it that you will be able to find a job in your desired profession?*” Respondents answered both questions on a scale which went from 0 to 100% in increments of 10%. We create an indicator variable which equals one if the answers to both questions are greater than 50%. We think of this variable as capturing a high versus low chance of achieving one’s professional aspirations. Immigrant Muslim girls born before the reform are more optimistic compared to natives, with 80% believing they have a high chance of reaching their professional goals compared to 67% of natives. But this optimism falls dramatically for Muslim immigrant girls born after the reform: their estimated coefficient falls by 21 percentage points and is statistically significant. No such drop is found for immigrant Muslim boys, non-Muslim boys, or non-Muslim girls, where the estimates go in the other direction and are not statistically significant.

In columns 5 and 6, we interact the two dummy variables used in the first two columns. This interaction captures disillusionment, which is defined as having high educational aspirations but a low perceived probability of achieving one’s professional aspirations. The baseline value of this disillusionment variable is low: 17% for natives and 10% for Muslim immigrant girls born pre-reform. Disillusionment spikes upwards for Muslim immigrant girls born after the reform by 19 percentage points, a result which is statistically significant. This finding indicates that it is the same immigrant Muslim girls born post reform whose aspirations increase who also believe they won’t be able to reach them.

In columns 7 and 8, we go one step further and interact disillusionment with the dummy variable for not receiving schooling support from parents. The estimate reveals that the rise in disillusion is driven by the same girls who, due to birthright citizenship, receive less schooling support from their parents. This last estimate completes a consistent story for Muslim immigrant girls affected by the reform: these girls may have higher educational goals, but their parents hold them back by withdrawing schooling support, which lowers their grades, and which ultimately results in disillusionment about their professional aspirations.

Despite the fact that immigrant Muslim girls granted citizenship report being less satisfied

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then be a part of the intergenerational conflict mechanism our paper centers on. The option model could in theory also have created an internal conflict for children. However, survey evidence suggests that few immigrant children were conflicted about this issue (Worbs 2014).

and more disillusioned, it is possible that they are objectively more successful. The results in Table 7 speak against this possibility, showing that the reform caused a withdrawal of parental schooling support combined with a drop in academic achievement. In Appendix Table A7 we further examine how the reform affected grades overall. We find that immigrant Muslim girls grades did not improve after the reform, whereas immigrant Muslim boys grades did. Immigrant Muslim boys born post-reform have a GPA which is 33.4 percent of a standard deviation higher, whereas immigrant Muslim girls have a statistically insignificant 0.4 percent change. These results mirror those found in Felfe et al. (2020a).

**5.1.3 Family versus Career.** In the last two columns of Table 8, we directly examine the tradeoff between pursuing one’s professional aspirations versus family responsibilities. We asked respondents “*How likely is it that you will have to forgo your career for family reasons, such as parenting?*” Respondents answered on a scale which went from 0 to 100% in increments of 10%. This question is tightly linked to gender norms. As we discussed in Section 4.1.4, second-generation immigrants from Muslim countries are much more likely to have mothers who come from countries where women receive less education, participate less often in politics, and work substantially less often outside the home (see Appendix Table A5).

Starting with girls in panel A, 48% of natives and 37% of pre-reform Muslims believe they will have to forgo a career for family. But for Muslim immigrant girls born post-reform, the odds of having to forgo a career to take care of a family rise by 8 percentage points, a result which is significant at the 10% level. When scaled by the first stage, the effect is a 12 percentage point increase. This suggests that even as birthright citizenship increases opportunities for Muslim immigrant girls, they feel less able to take advantage of those economic opportunities relative to family responsibilities. It points towards parental efforts to strengthen their daughters’ role as “keeper of the culture” to be at least partially successful. Interestingly, the results go in the opposite direction for non-Muslim immigrant girls, and significantly so.

A related channel is that citizenship could raise the probability a parent will be able to arrange a traditional marriage for their daughter, in part because a citizen can more easily bring a foreign spouse into Germany. In this sense, citizenship could raise a daughter’s value in the marriage market, but in a way which lowers their current well-being. Likewise, citizenship could cause parents to send their daughters back to their country of origin to imbue them with traditional culture.

There is qualitative evidence on each of these channels from other researchers. Keim et al. (2012) argue that, for Muslim immigrant families in Germany, “Parents want to correct their child’s deviant behavior through marriage and prevent their child from getting off track; [parents] further want to facilitate immigration of relatives through arranged marriages.” A survey of 59 institutions serving immigrants in Hamburg finds similar explanations for forced marriages.<sup>19</sup> Regarding being sent back,

<sup>19</sup>The survey asked counselors and other employees in the institutions about motives for forced marriages. Forty-four percent of answers were categorized as “Maintain the honor of the family and preserve traditional culture” with 13% explicitly mentioning “To control the daughters so that they don’t live too Western and modern.” In 13% of responses, the reason was “immigration of the groom or securing the residence status of husbands” (Mirbach et al. 2006).

El-Mafaalani and Toprak (2011) write that “children are sent back to Turkey for a limited time span in order to be taught discipline and order... [Non-naturalized] children usually return to Germany so as not to risk their residence permit.” If a daughter does not want to marry someone from her parents’ home country and does not want to be sent back for a lengthy period of time, this could create an intergenerational conflict. In other words, we view these possibilities as complementary to our clash of cultures hypothesis.

**5.1.4 Future Life Satisfaction.** Taken together, the parental investments, schooling, disillusionment, and family-career results align well with what we found in Section 4, where the drop in child well-being and assimilation was exclusively an immigrant Muslim girl phenomenon. As a final piece of evidence on the intergenerational conflict channel, we look at the expected change in life satisfaction in the future, after a child is likely to have left their parents’ household. We are able to do this because in addition to asking about current life satisfaction, our survey asked about future life satisfaction. In Table 9, we report results for the expected change in life satisfaction 5 years out relative to current life satisfaction. While Muslim immigrant girls born pre-reform are more pessimistic about their future well-being compared to natives, this reverses for Muslim immigrant girls born post-reform. Indeed, their expected change in satisfaction 5 years in the future relative to today is almost two thirds of a standard deviation higher than those born just before the cutoff.<sup>20</sup>

This result on improved future life satisfaction accords well with work which studies reforms of German naturalization law that relate to citizenship granted later in life, rather than at birth. Gathmann and Keller (2018) and Gathmann et al. (2020) study two reforms which allowed adult immigrants to naturalize if they had lived in Germany for 15 and 8 years, respectively. They find that easier and faster access to citizenship helped immigrants, and women in particular—resulting in better labor market performance and the postponement of early marriages and childbearing. Hence, gaining German citizenship later in life not only aided integration, but also the social position of women.<sup>21</sup>

Comparing this work to ours, the reforms being studied and the age groups under consideration are different. A possible reconciliation is that our paper uncovers the effect of birthright citizenship while daughters are still living at home with their parents, but that the future looks much brighter consistent with the results of Gathmann and Keller (2018) and Gathmann et al. (2020). In other words, immigrant Muslim girls granted birthright citizenship currently have large conflicts with their parents about traditional culture, but perhaps feel hopeful that they can integrate in the future once they grow older and can leave their parents’ house.

## 5.2 Alternative Explanations

We now consider three alternative mechanisms that could potentially explain why birthright citizenship causes a drop in well-being and assimilation for immigrant Muslim girls. While we find

<sup>20</sup>Results for the expected change 1 year out go in the same direction, but are smaller and insignificant.

<sup>21</sup>Hainmueller et al. (2017) study a reform affecting adult immigrants who apply for citizenship in Switzerland, and also find evidence of increased integration.



evidence that each of these explanations cannot explain all of the results jointly, it is still possible for them to play a more limited role. We also note that any alternative mechanism has to be gender specific given the pattern of our findings.

**5.2.1 Unmet Expectations.** The first alternative is what we label the “unmet expectations” hypothesis, where Muslim girls granted citizenship expect to be able to accomplish more and fit into society, but are disappointed when they realize society continues to discriminate against them. While this would readily explain the current drop in well-being we observe in our data, it is inconsistent with treated immigrant Muslim girls believing their life satisfaction will improve in the future, after they are likely to have left home and entered the labor market.

To probe the plausibility of the unmet expectations hypothesis further, we turn to Table 10. We asked two survey questions related to perceptions of what it takes to be successful in Germany. The first presented students with the statement “*In Germany, foreigners have to do a great deal to gain recognition and acceptance*”. Responses were recorded on a scale from 1 (*not at all accurate*) to 6 (*completely accurate*), and normalized to be mean 0 and standard deviation 1 relative to the sample of all natives. For Muslim immigrant girls, exposure to the reform results in a small and insignificant reduction in the belief that foreigners have to do more to be accepted in Germany. This is not what would be predicted according to the unmet expectations hypothesis, which predicts an increase. For the other immigrant groups, a similar pattern emerges with negative, but insignificant, estimates.

Another survey question asked whether “*You need to be German*” in order to succeed in Germany. The question is not perfect, as it could be interpreted as being either about German culture or German citizenship. It is also not as clean as the first one, since self-identification as German was affected by the reform (see Table 6). With these caveats in mind, we note that it reveals a similar pattern. Answers were measured on a scale from 1 (*completely disagree*) to 4 (*completely agree*), and normalized to be mean 0 and standard deviation 1 relative to natives. The estimate for immigrant Muslim girls born post-reform is close to zero, whereas the unmet expectations hypothesis would have predicted a positive coefficient. The estimate is likewise near zero for immigrant Muslim and non-Muslim boys, and a statistically significant negative for non-Muslim immigrant girls.

Another possible source of unmet expectations is that teachers or classmates treat immigrant Muslim boys more positively if they are born after the reform, but treat immigrant Muslim girls in the same way as before the reform. This would allow the boys the opportunity to become better integrated and improve in school, while girls feel resentment. In related work (Felfe et al., 2020a), we show that native children do not treat immigrant boys and girls differentially based on whether they are naturalized. That paper used an experimental game to measure cooperation among immigrant and native youth. So the evidence from that paper argues against effects stemming from the behavior of classmates. Teachers do not have official information on children’s citizenship status, although in theory it is possible that teachers acquire this information and act upon it.

**5.2.2 Resource Shifting.** A second alternative we consider is what we label the “resource shifting” hypothesis, which says that after birthright citizenship becomes law, Muslim immigrant

parents shift resources away from their daughters and towards their sons. This would occur because traditional parents favor their sons succeeding above their daughters, and with citizenship the returns to parental investments have gone up. This explanation is broadly consistent with Muslim immigrant daughters experiencing a drop in well-being after the reform, while immigrant sons report, if anything, increased well-being. But this explanation has a more targeted prediction: Muslim immigrant girls born after the reform should only be negatively affected if there is a younger brother in the household (who has been granted birthright citizenship) to whom resources could be shifted. If there are only older brothers in the household there is no reason for parents to shift resources.<sup>22</sup> However, comparing columns 5 and 7, we see that the estimates for these two groups of Muslim immigrant girls are quite similar. This is evidence against the resource shifting hypothesis.

**5.2.3 Convergence to Natives via Integration.** A third alternative for the drop in well-being for immigrant Muslim girls is convergence to the native German level of well-being due to assimilation. To provide some context on levels, teenage girls appear to have lower well-being than teenage boys, a pattern we also observe for natives in our data (Pandya 2017).<sup>23</sup> Regarding the influence of religion on youth, Kosher and Ben-Arieh (2017) find that Muslim (as well as Jewish and Eastern Christian) children tend to be more satisfied with life as a whole.

Less is known about life satisfaction for teenage immigrant boys and girls in particular. One notable exception is Hole and Ratcliffe (2020) which looks at adolescent well-being (ages 14-15) in the UK intersected with the 2005 London Bombings (a series of coordinated Islamist suicide attacks). They find a significant decrease in the well-being of Muslim teenage girls, but not in Muslim teenage boys, suggesting that the two genders react differently also to other exogenous events. Also relevant to our study is that at baseline, Muslim girls were markedly happier than non-Muslim girls, but the bombing shock brought them to the same level. At the same time, in the baseline, Muslim teenage girls and boys were roughly equally happy. These patterns broadly line up with those found in our data.

Since native German girls report a lower level of well-being in our study, the third alternative mechanism says that citizenship causes immigrant Muslim girls to feel like natives as their integration increases. While increased integration can explain the convergence to native female well-being in isolation, it does not line up with the decline in self-identification as German. Nor does it match up with the drop in parental schooling investments and educational achievement.

Turning to the other outcomes besides well-being, other models can explain some of the results in isolation, but likewise have a hard time explaining the entirety of our findings. For example, the decrease in parental schooling investments for Muslim immigrant girls could be due to parents thinking they do not need to invest as much now that their daughter's opportunities have

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<sup>22</sup>We note that it is possible that older brothers may have been granted citizenship via the transition rule. However, the take-up rate was low, with only 1 out of 6 families taking advantage of this rule (see Section 2.1). So while this could impact the estimates, most of the effect for the older brother sample is identified off of those not granted birthright citizenship.

<sup>23</sup>Interestingly, this pattern is not found for adults, with male well-being dropping around the age of 18-20.

increased (substitution), but this does not readily explain the drop in well-being nor does it explain disillusionment on the part of the child. Moreover, there is no obvious reason why this explanation would only apply to daughters and not sons.

In sum, there are several interesting possibilities which can explain a portion of our findings in isolation. However, the broader set of results and supplementary analyses are not generally consistent with these alternatives. We conclude our results for Muslim immigrant girls are best explained by intergenerational conflict, a mechanism which can explain all of our findings within a single framework, while still recognizing that multiple factors could be operating simultaneously.

## 6 Theoretical Framework

To more formally rationalize our empirical results, we develop a simple model for how connecting immigrant youth to opportunity affects their well-being. As mentioned earlier, three classes of economic models could be adapted to rationalize our results: theories of authoritarian parenting (Doepke and Zilibotti 2017); theories of cultural transmission arising from vertical and horizontal socialization (Bisin and Verdier 2000, 2001); and theories of cultural identity (Akerlof and Kranton 2000). In this section, we develop an intergenerational version of the seminal identity model of Akerlof and Kranton, adding new insights into the causes and consequences of intrafamily conflict when assimilation opportunities increase.

The model we present demonstrates how cultural identity, a prominent strand of the literature, can speak to intergenerational conflict and its downstream consequences. Since the consequences of naturalization are unexpectedly negative for immigrant Muslim girls, this framework provides a context to understand our empirical results.

Since our empirical setting analyzes students who are in their final year of compulsory schooling, we frame this discussion in terms of an early career effort choice, such as investment in education. But the ideas are more general than this, and can easily be adapted to other investments a child can make to integrate into a host country’s economic and social life. The child chooses between two levels of effort:  $e_H$  (“high”) and  $e_L$  (“low”). The outcome of a child’s effort can be either a high-success or low-success career:  $j_H$  or  $j_L$ . The probability of the high outcome is  $p \in (0, 1)$  if the child chooses  $e_H$ , but  $p = 0$  if the child chooses  $e_L$ . We think of  $p$  as a parameter that captures the economic opportunities immigrants face in the host country. The cost of effort  $e_H$  (respectively,  $e_L$ ) is given by  $c$  (respectively, 0).

Within this framework, we analyze the implications of two models. First, we consider a model in which young immigrants exhibit neoclassical preferences and decide on career effort independently of their parents. In this model, we briefly discuss changes in welfare in response to better economic opportunities for immigrant youth (i.e., an increase in  $p$ ). Second, in the spirit of Akerlof and Kranton (2000), we consider the same comparative static in an economic model of identity in which the career choices of young immigrants are determined through a sequential bargaining game between them and their parents. Crucially, in this model, we assume that children impose an

“identity externality” on parents if their career outcome deviates from the exogenous identity-based reference point of a low-success career (i.e., a traditional career which has low returns in the labor market, such as remaining at home to take care of children).

## 6.1 Individual Decision-Making with Neoclassical Preferences

Consider first a single agent model, which we label as neoclassical, where the child makes decisions without any interference by their parents. Suppose the preferences of a young immigrant are given by a utility function that is additively separable into  $u_k$  ( $k \in \{H, L\}$ ), which converts career outcomes  $j_k$  into utility, and the cost of effort supplied. The individual then chooses  $e \in \{e_L, e_H\}$  to maximize her expected payoff

$$V(e) = \begin{cases} u_L & \text{if } e = e_L; \\ pu_H + (1 - p)u_L - c & \text{if } e = e_H. \end{cases} \quad (2)$$

The resulting effort choice is then:

$$e^* = \begin{cases} e_L & \text{if } c > \hat{c} \\ e_H & \text{if } c \leq \hat{c} \end{cases} \quad \text{where } \hat{c} \equiv p(u_H - u_L). \quad (3)$$

The comparative static of interest is the effect of an increase in economic opportunities for immigrants, captured by  $p$ , on their well-being. Suppose that  $p$  increases, from  $p^-$  to  $p^+$ . This has two effects. First, conditional on high effort under  $p^-$  (i.e., for children with  $c \leq \hat{c}^-$ ), better economic opportunities for young immigrants increases their expected payoffs by  $(p^+ - p^-)(u_H - u_L)$ . Second, the increase in  $p$  raises the cutoff from  $\hat{c}^-$  to  $\hat{c}^+$ , i.e., children with  $c \in (\hat{c}^-, \hat{c}^+)$  will now choose  $e_H$  instead of  $e_L$ , and increase their utility by  $p^+(u_H - u_L) - c$ . Finally, for children with  $c > \hat{c}^+$ , the increase in  $p$  has no effect.

## 6.2 Family Bargaining with Traditional Identity-Based Preferences

We now extend the model along four dimensions (see Figure 3). First, we assume a sequential bargaining game between a child and her parents. In particular, since our data captures immigrant children at an age where parents arguably still hold considerable decision-making power over them, we assume that parents are dictators who have the last say on the child’s career effort.<sup>24</sup> The sequence of events is as follows. In the first stage, the child proposes a career effort  $e \in \{e_L, e_H\}$ . In the second stage, the parents can either accept the child’s effort proposal or reject it and enforce the opposite effort level. In the last stage of the game, which we think of as the child’s “adult stage”, career outcomes are realized.

Second, we include frictions between immigrant identities and children’s career choices. In order to keep the model as parsimonious as possible, we only impose identity-based preferences on the parents. In particular, we follow the prototype identity model outlined by Akerlof and Kranton

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<sup>24</sup>Allowing parents to be “imperfect” dictators, where parents can punish their children when they do not comply with their parents’ wishes, yields similar insights.

(2000), where immigrant parents have a preference for their children to pursue a low-success career ( $j_L$ ), and experience a loss of identity ( $\lambda I$ ) whenever their child's realized career deviates from this outcome. The loss of identity comes from the child assimilating into "mainstream" culture, versus "traditional" immigrant culture. A low success career refers to success in terms of formal labor market returns, and could include forgoing paid work to remain at home to take care of children and the household. More generally, immigrant parents could experience an identity loss whenever their children assimilate in other ways into mainstream society.

Third, to stay as close as possible to Akerlof and Kranton's identity model, and to make the implications of parents' identity concerns as salient as possible, we abstain from making the (realistic) assumption that parents are altruistic and care about their children's career outcomes.<sup>25</sup> We do, however, assume that both parents and the child incur a cost  $\kappa$  whenever parents enforce an effort level  $e \in \{e_L, e_H\}$  which differs from the child's proposed effort level. We think of this cost as capturing payoff losses due to intrafamily conflict.

Fourth, we assume that the child feels regret  $R$  if she has to forgo pursuing her individually rational strategy as in the neoclassical model. For example, for a child with  $c \leq \hat{c}$ , it is individually rational to choose high effort  $e_H$ . If, in a subgame perfect outcome, the child either proposes  $e_L$  (and the parents accept it) or the parents enforce  $e_L$  (after the child proposes  $e_H$ ), the child feels regret because the foregone effort level is individually preferable to the chosen one. A natural assumption we make is that  $R$  is an increasing function of the career opportunities a child has to forgo by not being able to pursue her individually rational strategy. Thus, if in equilibrium a child with  $c \leq \hat{c}$  realizes  $e_L$  instead of her preferred  $e_H$ , her foregone career opportunities are given by  $p(u_H - u_l) - c$ , and she experiences regret in the amount  $R = R(p(u_H - u_l) - c)$ . By a similar logic, if a child with  $c > \hat{c}$  realizes  $e_H$  instead of  $e_L$ , she experiences regret of  $R = R(c - p(u_H - u_l))$ .

Expected payoffs are given at the end nodes of the game in Figure 3. The top payoff is for the child, and the bottom payoff for parents. Consider, for example, a child with  $c \leq \hat{c}$ . If the child proposes  $e_H$  (which is individually rational), and the parents accept this, then the expected payoff of the child is  $pu_H + (1 - p)u_L - c$ , i.e., she has a positive probability of a high-success career, but has to incur the cost of effort  $c$ . For parents, the expected payoff is given by  $y - p\lambda I$ . It increases with parents' exogenous income  $y$ ; however, with probability  $p$ , the child realizes a high-success career, which causes traditional immigrant parents to suffer identity losses in amount  $\lambda I$ . If the parent had instead rejected the child's proposal of  $e_H$  and enforced  $e_L$  instead, the child would have no chance at a high-success career, face intrafamily conflict at cost  $\kappa$ , and feel regret  $R$  because the foregone effort level is individually preferable to the one chosen by her parents. The parent would now face intrafamily conflict  $\kappa$ , but no longer suffer an identity loss.

To solve the game, it is useful to define the following critical value for the intensity of identity,  $\lambda$ :

$$\hat{\lambda} = \frac{\kappa}{pI}$$

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<sup>25</sup>This could easily be relaxed by adding a term  $\alpha u_k$  to parental payoffs at the end nodes of the game in Figure 3 (bottom payoff). It would not change the main gist of our results, but would come at the cost of having to make some additional assumptions.

Given this critical value, it is straightforward to verify the game has three possible subgame perfect outcomes:

- (a) If  $\lambda < \hat{\lambda}$  and  $c > \hat{c}$ , then the child proposes low effort  $e_L$ , and the parents accept it.
- (b) If  $\lambda < \hat{\lambda}$  and  $c \leq \hat{c}$ , then the child proposes high effort  $e_H$ , and the parents accept it.
- (c) If  $\lambda \geq \hat{\lambda}$ , the child proposes low effort  $e_L$  for any  $c$ , and the parents accept it.

The first two subgame perfect outcomes correspond to the neoclassical outcome described above; they arise if parents' identity concerns are sufficiently low, relative to the cost of intrafamily conflict. The more interesting case is where identity concerns are more salient,  $\lambda \geq \hat{\lambda}$ .

In the third subgame perfect outcome, traditional parents suffer a large identity loss whenever their child realizes a high-success career ( $j_H$ ). When  $\lambda \geq \hat{\lambda}$ , parents' identity concerns are so high that they enforce  $e_L$  if the child proposes  $e_H$ . Anticipating this, and to avoid intrafamily conflict (i.e., costs  $\kappa$ ), the child proposes low effort  $e_L$  irrespective of whether  $c > \hat{c}$  or  $c \leq \hat{c}$ , which the parents accept. From the point of view of a child with  $c < \hat{c}$ , the third outcome features parents who are “under-ambitious” due to identity concerns: by enforcing low effort, parents hold their children back to a low success career. This outcome features children who also feel regret  $R$ .

An increase in economic opportunities for young immigrants (i.e., an increase in  $p$ ) has an ambiguous effect on their well-being, depending on how important identity concerns are for their parents. This is illustrated in Figure 4, which considers the simple thought experiment of increasing  $p$  from  $p^-$  to  $p^+$ . In terms of the model basics, this has two effects. First, it increases the critical value from  $\hat{c}^-$  to  $\hat{c}^+$ : more children – i.e., those with  $c \in (\hat{c}^-, \hat{c}^+)$  – will now find it individually rational to supply high effort ( $e_H$ ) instead of low effort ( $e_L$ ). Second, it decreases the critical value  $\hat{\lambda}$  from  $\hat{\lambda}^+$  to  $\hat{\lambda}^-$ : due to identity concerns, more parents – i.e., those with  $\lambda \in (\hat{\lambda}^+, \hat{\lambda}^-)$  – will now enforce  $e_L$  regardless of their child's proposed effort. We now provide some interpretation of the various regions in Figure 4.

In regions (i)-(iii) (“low identity intensity”), the identity parameter is low enough that both initially and after the increase in  $p$ , parents do not make use of their “veto” threat, and subgame perfect outcomes are neoclassical in nature. Children with  $c > \hat{c}^+$  choose low effort both initially and after the increase in  $p$ ; children with  $c \in (\hat{c}^-, \hat{c}^+)$  choose  $e_H$  instead of  $e_L$  after the change, and realize an (expected) utility gain in amount  $p^+(u_H - u_L) - c$ ; and children with  $c \leq \hat{c}^-$  choose high effort both before and after the increase in  $p$ , and see their expected payoffs increase by  $(p^+ - p^-)(u_H - u_L)$ .

In regions (iv)-(vi) (“moderate identity intensity”), the increase in  $p$  causes some subgame perfect outcomes to change from being neoclassical in nature to being shaped by parents' identity concerns. In region (iv), choosing low effort remains individually optimal from the child's perspective, and this does not conflict with parents' identity, so there is no change in outcomes or utility. But in region (v), the increase in  $p$  makes choosing high effort individually rational now, but due to parents' identity concerns, the child is “held back” to low effort. Thus, her expected career outcomes are

unaffected by the increase in  $p$ , but she feels regret  $R$  due to not being able to pursue what has become her individually optimal strategy. In region (vi), if a child proposes  $e_H$  as they did prior to the increase in  $p$ , the parents no longer accept it, but threaten to enforce  $e_L$ ; anticipating this, the child immediately proposes  $e_L$  (which the parents accept), and her expected payoff decreases by  $c - p^-(u_H - u_L) - R$ .

In regions (vii)-(ix) (“high identity intensity”), the parameters are such that both initially and after the increase in  $p$ , subgame perfect outcomes are shaped by parents’ identity concerns. In region (vii), the increase in  $p$  leaves children’s well-being unaffected. However, this is not the case in region (viii), where the child initially preferred  $e_L$ ; with the increase in  $p$ , the child now prefers  $e_H$  over  $e_L$ , but parents’ veto threat forces the child to accept  $e_L$ , i.e., she ends up in a regret equilibrium. In region (ix), the child already had regret for not being able to pursue a high success career; after the increase in opportunity  $p$ , this regret increases and hence the child is even worse off than before.

Taken together, the model predicts the welfare effects of connecting immigrant youth to opportunity depend on the underlying strength of parents’ identity concerns. Children whose parents have little or no traditional identity concerns will be (weakly) positively affected by an increase in  $p$  (positive effects in regions (ii), (iii) and no effect in (i)). In contrast, the welfare of children whose parents have moderate to strong identity concerns will be (weakly) negatively affected (negative effects in regions (v), (vi), (viii), (ix) and no effect in (iv), (vii)). We found support for this model, and against the neoclassical model, for immigrant girls in our empirical work.

### 6.3 Mainstream Identity-Based Preferences

It is straightforward to extend the model described in subsection 6.2 to the case where immigrant parents have mainstream identity instead of traditional identity. In this case, parents have a preference for their children to achieve a high-success career ( $j_H$ ), and experience a loss of identity ( $\lambda I$ ) whenever their child’s (realized) career outcome deviates from this reference point. As before, both parents and the child incur a cost  $\kappa$  whenever parents enforce an effort level which differs from the child’s proposed effort level. This happens when the child would like to choose low effort ( $e_L$ ) but the parent enforces high effort ( $e_H$ ) due to identity concerns.

The expected payoffs for the case with mainstream identity are given in Appendix Figure A1. There are three possible subgame perfect outcomes. The first two are the same as (a) and (b) listed for the traditional case in subsection 6.2, and correspond to the neoclassical outcome. But the third differs as follows:

(c’) If  $\lambda \geq \hat{\lambda}$ , the child proposes high effort  $e_H$  for any  $c$ , and the parents accept it.

In this third subgame perfect outcome, parents’ identity concerns are so high that they enforce  $e_H$  if the child proposes  $e_L$ . Anticipating this, and to avoid intrafamily conflict with costs  $\kappa$ , the child proposes high effort for any effort cost  $c$ .

In Appendix Figure 2, we again consider the thought experiment of increasing  $p$  from  $p^-$  to  $p^+$ , as we did in Figure A2. If parents have mainstream identity concerns, an increase in youth

opportunity will, for most combination of parameter values, increase children’s welfare (regions ii, iii, v, vi, vii, viii, ix). In region (i) it remains unchanged. Only in region (iv) will child well-being decrease. This exception is the region where parents force children to “overachieve” by making them exert high effort  $e_H$  when they would prefer to exert  $e_L$ .

In sum, the model of traditional identity-based preferences presented in subsection 6.2 best explains our results for Muslim immigrant girls. The model of mainstream identity-based preferences presented in this subsection is consistent with the results for immigrant boys. But so is a neoclassical model, since we do not have direct evidence for whether parents force some boys to overachieve, which is the only distinguishing prediction between the two models.

## 7 Conclusion

This paper studies a law change where those born in Germany after January 1, 2000 became over 50 percentage points more likely to be given automatic citizenship. The reform unintentionally reduces life satisfaction of immigrant girls when they are 15-16 years old by 31% of a standard deviation, which translates to an effect of citizenship of 1.34 on a 0-10 scale. These effects are larger than the usual effects of divorce or unemployment on life satisfaction and are similar in magnitude to the effect of a medium-level depression (Frijters et al. 2020). There are correspondingly large drops in self-esteem, which suggests the effect on well-being came largely via pressure on self-esteem.

The results turn out to be driven by the effect on Muslim immigrant girls. Summarizing the scaled estimates, those granted birthright citizenship experience a 39% drop in reporting a high chance of obtaining the training, education, and job they aspire to, and a 33% increase in the perception they will have to forgo a career for family. These dramatic changes are consistent with a cultural clash between parents and children: there is a 16 percentage point drop in parental help with homework and learning and a corresponding drop in grades. This is indicative of a conscious attempt by parents to counteract the pull of German culture which increases with citizenship. The effect is that Muslim immigrant girls with birthright citizenship engage in 40% fewer extracurricular activities and score 40% of a standard deviation lower in a measure of friendship support. Moreover, Muslim immigrant girls granted German birthright citizenship are paradoxically 21 percentage points less likely to self-identify as German.

None of these effects hold for immigrant boys, who experience no significant change or even slightly better outcomes. Of the 40 pairs of estimates for second generation immigrant (Muslim) girls and boys appearing in Tables 1 to 10, 31 of these are statistically different from each other at the 10% significance level.<sup>26</sup> Taken together, our findings provide evidence that Muslim parents reacted strongly against the citizenship reform in order to keep their daughters within a traditional culture, whereas boys were allowed to take advantage of the opportunities that come with citizenship.

In the paper, our main outcome variable was current life satisfaction, i.e., well-being at age 15 or 16. A valid question is what happens to the rest of life, something we address using our survey

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<sup>26</sup>Only the 9 pairs of estimates appearing in Table 3 column 3, Table 4 column 3, Table 6 column 5, Table 7 column 5, Table 8 column 1, and Table 10 columns 1, 3, and 7 are not statistically different.



questions on future life satisfaction. The expected change in satisfaction five years in the future relative to today is two thirds of a standard deviation higher for treated immigrant Muslim girls. This is consistent with the idea that immigrant Muslim girls granted birthright citizenship currently have large conflicts with their parents about traditional culture, but feel hopeful that these parental constraints will lessen in the future as they grow older and leave their parents' house. The jury is thus still out on whether the citizenship reform is good for Muslim immigrant girls over their entire lifetime. While the indications are strong that their current well-being and human capital investments are reduced due to the reform, their higher hopes for changes in the future indicate it is possible there are longer-term gains yet to come.

The German birthright citizenship reform was enacted to lower the bars to cultural and economic assimilation. While a neoclassical model would predict that increased opportunities should increase well-being and assimilation, for immigrant girls it paradoxically made them worse off and less integrated. A simple model with intergenerational identity concerns can explain these otherwise unexpected and puzzling findings. Our results are a sobering illustration that increased opportunities are not offered to people in isolation of competing claims on the loyalty of a person. Family members (or other social groups) will consciously take countermeasures if they consider assimilation to be a threat to their cultural identity. From a policy perspective, the fact that immigrant girls are made worse off and feel less integrated after receiving birthright citizenship suggests other actions are needed to promote second-generation assimilation of females.

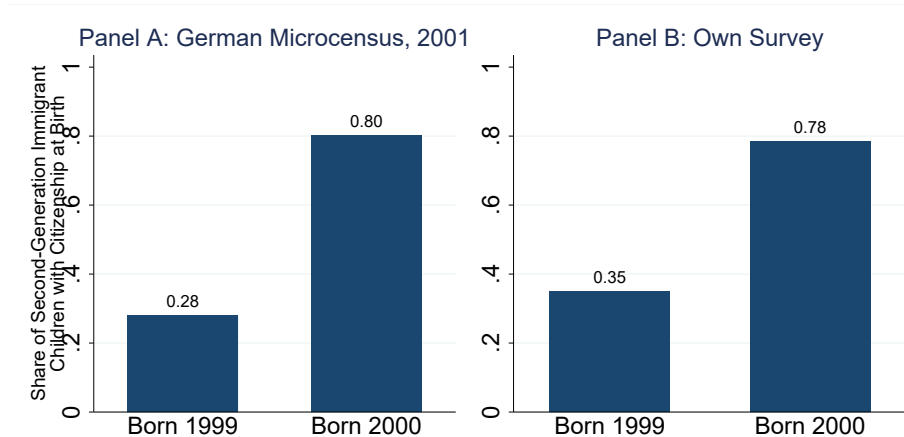
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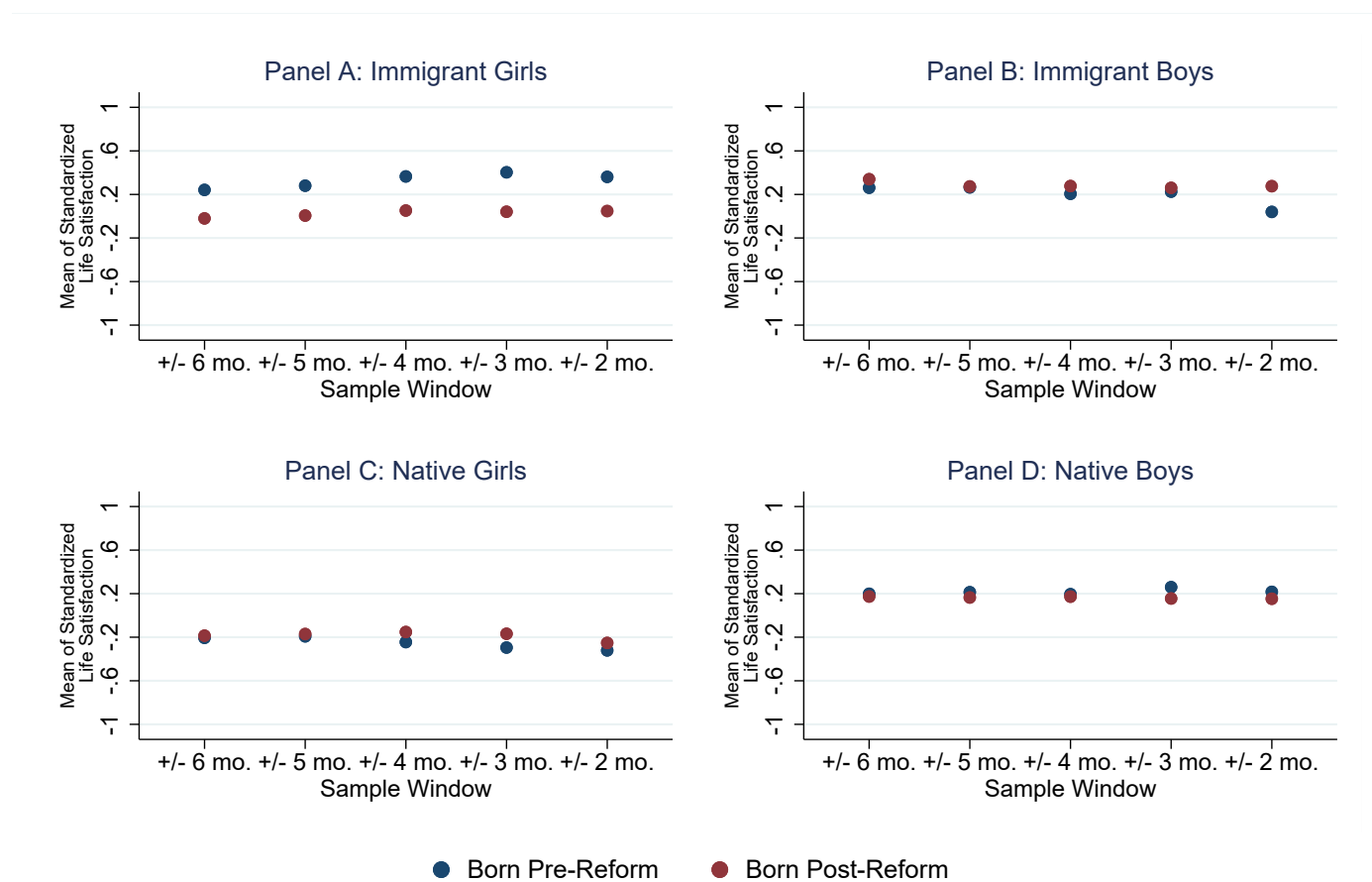
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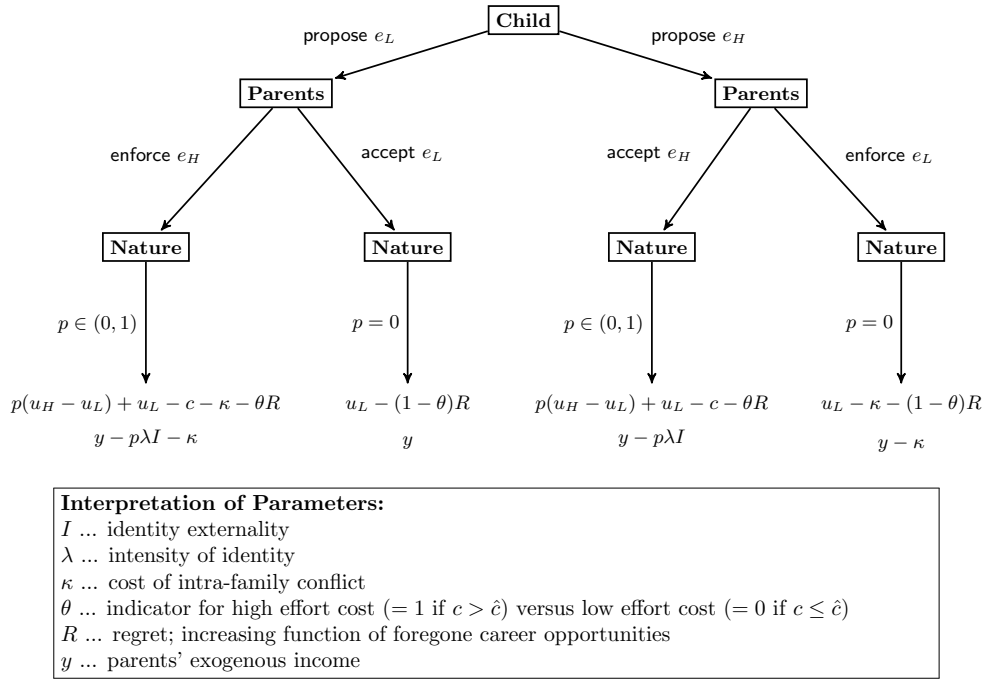
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**Figure 1.** Citizenship at Birth: Second-Generation Immigrant Children Born Before versus After the Birthright Citizenship Reform (January 1, 2000)

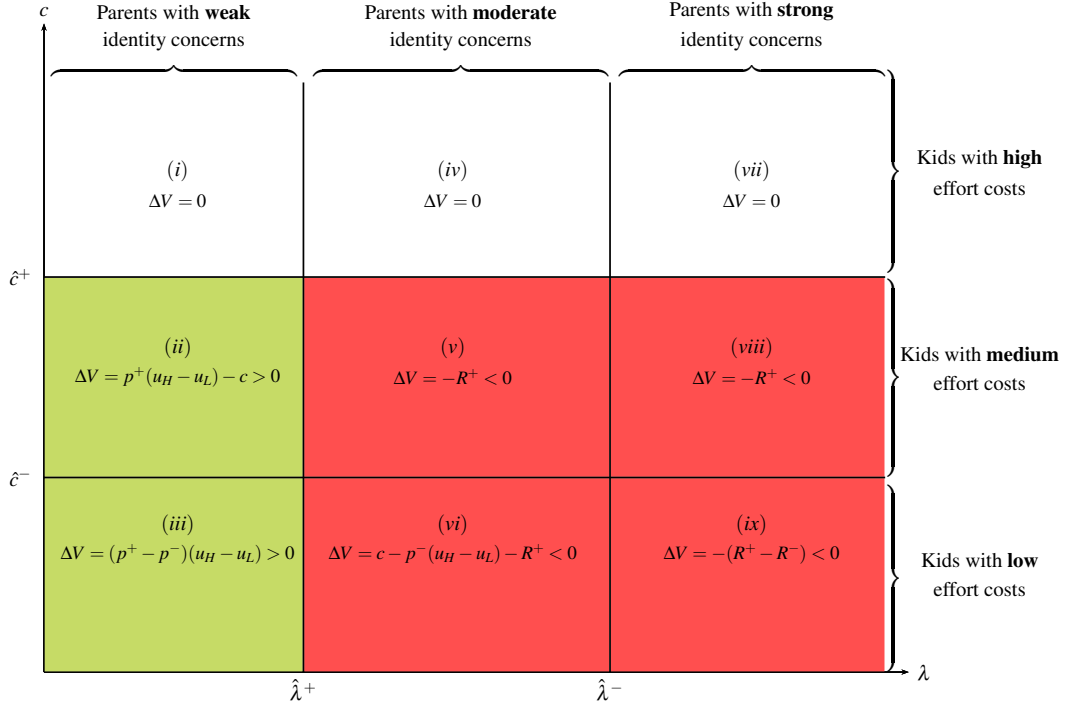


**Figure 2.** Life Satisfaction Among Immigrant and Native Youth Born Before versus After the Birthright Citizenship Reform (January 1, 2000)



Note: For simplicity, the figure suppresses the arguments of  $R$  (i.e.,  $p(u_H - u_L) - c$  if  $\theta = 0$ ;  $c - p(u_H - u_L)$  if  $\theta = 1$ ).

**Figure 3.** Family Bargaining with Traditional Identity-Based Preferences



Notes: For simplicity, the figure uses the notation  $R^+ = R(p^+(u_H - u_L) - c)$  and  $R^- = R(p^-(u_H - u_L) - c)$ . Red shaded areas represent parameter values where an increase in  $p$  results in a reduction in children's utility. Green shaded areas represent parameter values where an increase in  $p$  results in an increase in children's utility.

**Figure 4.** Changes in Well-Being when Economic Opportunities Increase: The Case of Traditional Identity-Based Preferences

**Table 1.** Impact of Birthright Citizenship on Subjective Well-Being

Dependent Variable:	<i>Standardized Life Satisfaction</i>			
	(1)	(2)	(3)	(4)
<b>Panel A: Girls</b>				
Immigrant	0.458** (0.098)	0.453** (0.099)	0.582** (0.102)	0.559** (0.103)
Post-reform*Immigrant	-0.309** (0.139)	-0.297** (0.140)	-0.336** (0.139)	-0.320** (0.139)
Observations	1,082	1,082	1,082	1,082
<b>Panel B: Boys</b>				
Immigrant	0.054 (0.091)	0.057 (0.091)	0.069 (0.095)	0.058 (0.095)
Post-reform*Immigrant	0.109 (0.123)	0.107 (0.123)	0.119 (0.124)	0.123 (0.124)
Observations	1,051	1,051	1,051	1,051
Birth Month FE	-	✓	✓	✓
Family Characteristics	-	-	✓	✓
City-Level Controls	-	-	-	✓

*Notes:* Estimates of equation (1) for an age window of  $\pm 6$  months around January 1, 2000. The dependent variable is normalized to be mean 0 and standard deviation 1 relative to the population of all natives. Family characteristics include mother's and father's age, dummy variables for mother's and father's education (four groups each). City-level controls include male unemployment, female unemployment, youth unemployment, immigrant unemployment, and a dummy variable for city size larger than 100,000. Standard errors reported in parentheses. \*\*, \* indicate significance at the 5% and 10% level, respectively.



**Table 2.** Narrowing the Sample Window

Dependent Variable:	<i>Standardized Life Satisfaction</i>			
	(1)	(2)	(3)	(4)
	$\pm 5$ months	$\pm 4$ months	$\pm 3$ months	$\pm 2$ months
<b>Panel A: Girls</b>				
Immigrant	0.585** (0.110)	0.766** (0.122)	0.883** (0.147)	0.827** (0.195)
Post-reform*Immigrant	-0.326** (0.149)	-0.447** (0.162)	-0.570** (0.186)	-0.458* (0.244)
Observations	924	759	597	388
<b>Panel B: Boys</b>				
Immigrant	0.047 (0.105)	0.027 (0.119)	-0.018 (0.130)	-0.126 (0.168)
Post-reform*Immigrant	0.084 (0.137)	0.088 (0.155)	0.112 (0.171)	0.242 (0.218)
Observations	881	703	544	370
Birth Month FE	✓	✓	✓	✓
Family Characteristics	✓	✓	✓	✓
City-Level Controls	✓	✓	✓	✓

*Notes:* Estimates similar to those in Table 1, but with different sample windows. Standard errors reported in parentheses. \*\*, \* indicate significance at the 5% and 10% level, respectively.

**Table 3.** Additional Robustness Checks

Dependent Variable:	<i>Standardized Life Satisfaction</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	RD	Difference in RD's	1st generation immigrants as control group	Natives and 1st generation immigrants as control group	School fixed effects	Control for age in days	Donut hole	Clustered s.e.'s
<b>Panel A: Girls</b>								
Post-reform	-0.488* (0.255)							
Immigrant		0.862*** (0.205)	0.295 (0.224)	0.536*** (0.102)	0.511*** (0.108)	0.562*** (0.102)	0.552*** (0.104)	0.559*** (0.114)
Post-reform*Immigrant		-0.630** (0.303)	-0.502* (0.297)	-0.340** (0.139)	-0.287** (0.142)	-0.333** (0.138)	-0.317** (0.141)	-0.320** (0.145)
Observations	326	1,081	388	1,144	1,082	1,081	1,048	1,082
<b>Panel B: Boys</b>								
Post-reform	0.135 (0.212)							
Immigrant		-0.150 (0.173)	-0.068 (0.158)	0.049 (0.093)	0.075 (0.099)	0.054 (0.095)	0.055 (0.099)	0.058 (0.101)
Post-reform*Immigrant		0.077 (0.257)	-0.145 (0.234)	0.108 (0.122)	0.124 (0.127)	0.130 (0.124)	0.138 (0.127)	0.123 (0.122)
Observations	272	1,051	330	1,109	1,051	1,051	1,002	1,051
Birth Month FE	-	-	✓	✓	✓	-	✓	✓
Family Characteristics	✓	✓	✓	✓	✓	✓	✓	✓
City-Level Controls	✓	✓	✓	✓	✓	✓	✓	✓

*Notes:* See notes to Table 1. Column 1 is a regression discontinuity using immigrants only, an age window of  $\pm 6$  months around January 1, 2000, separate linear trends in child age (in days) to the left and right of the cutoff, and triangular weights. Column 2 is a difference in RDs for immigrants versus natives. Column 3 presents estimates of equation (1) using first-generation immigrants as the control group. Column 4 includes both natives and immigrants as the control group. Column 5 includes school fixed effects. Columns 6-8 mirror those in found in column 4 of Table 1, except that column 6 controls for child age in days, column 7 excludes children born  $\pm 1$  week around January 1, 2000, and column 8 clusters standard errors at the school level. Columns 2 and 6 drop one observation where day of birth (but not month of birth) is missing. Standard errors reported in parentheses. \*\*, \* indicate significance at the 5% and 10% level, respectively.

**Table 4. Self-Esteem as an Alternative Measure of Subjective Well-Being**

Dependent Variable:	Index of self-esteem	Standardized components of the index					
	(1)	<i>I am satisfied with myself</i> (2)	<i>I have many positive character traits</i> (3)	<i>I am as capable as other people</i> (4)	<i>I am a person with value and self-worth</i> (5)	<i>I have a positive attitude towards myself</i> (6)	
Panel A: Girls							
Immigrant	0.445** (0.080)	0.650** (0.107)	0.374** (0.103)	0.529** (0.102)	0.223** (0.107)	0.449** (0.105)	
Post-reform*Immigrant	-0.252** (0.108)	-0.420** (0.145)	-0.098 (0.140)	-0.370** (0.137)	-0.223 (0.145)	-0.147 (0.142)	
Observations	1,038	1,038	1,038	1,038	1,038	1,038	
Panel B: Boys							
Immigrant	-0.073 (0.075)	0.0091 (0.096)	0.001 (0.099)	-0.103 (0.104)	-0.257** (0.110)	-0.016 (0.099)	
Post-reform*Immigrant	0.167* (0.098)	0.149 (0.125)	0.125 (0.129)	0.194 (0.135)	0.125 (0.142)	0.241* (0.129)	
Observations	1,011	1,011	1,011	1,011	1,011	1,011	
Birth Month FE	✓	✓	✓	✓	✓	✓	
Family Characteristics	✓	✓	✓	✓	✓	✓	
City-Level Controls	✓	✓	✓	✓	✓	✓	

*Notes:* See notes to Table 1. Each component of the index is normalized to be mean 0 and standard deviation 1 relative to the population of all natives. The aggregate index of self-esteem sums across the 5 normalized variables, and divides by 5. Standard errors reported in parentheses. \*\* \* indicate significance at the 5% and 10% level, respectively.

**Table 5.** Muslim vs. Non-Muslim Immigrants

Dependent Variable:	<i>Standardized Life Satisfaction</i>	
	(1)	(2)
	Muslim	Non-Muslim
<b>Panel A: Girls</b>		
Immigrant	0.822** (0.126)	0.222 (0.146)
Post-reform*Immigrant	-0.474** (0.167)	-0.087 (0.206)
Observations	953	881
<b>Panel B: Boys</b>		
Immigrant	0.165 (0.120)	-0.065 (0.137)
Post-reform*Immigrant	0.052 (0.153)	0.191 (0.183)
Observations	942	888
Birth Month FE	✓	✓
Family Characteristics	✓	✓
City-Level Controls	✓	✓

*Notes:* See notes to Table 1. Standard errors reported in parentheses. \*\*, \* indicate significance at the 5% and 10% level, respectively.

**Table 6.** Social Integration and German Identity

Dependent Variable:	<i>Index of social participation</i>		<i>Index of friendship support</i>		<i>Index of loneliness</i>		<i>Self-identity as German</i>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Muslim	Non-Mus.	Muslim	Non-Mus.	Muslim	Non-Mus.	Muslim	Non-Mus.
<b>Panel A: Girls</b>								
Immigrant	0.183 (0.119)	0.004 (0.136)	0.227** (0.099)	0.028 (0.117)	-0.118 (0.075)	0.091 (0.086)	-0.507*** (0.044)	-0.361*** (0.050)
Post-reform*Immigrant	-0.298* (0.158)	-0.074 (0.192)	-0.268** (0.133)	-0.224 (0.164)	0.122 (0.100)	0.012 (0.121)	-0.137** (0.058)	-0.009 (0.071)
Observations	953	881	934	867	923	858	953	881
<b>Panel B: Boys</b>								
Immigrant	0.020 (0.104)	-0.215* (0.115)	0.166 (0.120)	0.031 (0.139)	0.103 (0.077)	0.027 (0.085)	-0.619*** (0.049)	-0.391*** (0.056)
Post-reform*Immigrant	0.233* (0.133)	0.112 (0.153)	0.170 (0.153)	0.023 (0.185)	-0.093 (0.099)	-0.010 (0.113)	0.057 (0.063)	-0.039 (0.075)
Observations	942	888	917	865	907	859	942	888
Birth Month FE	✓	✓	✓	✓	✓	✓	✓	✓
Family Characteristics	✓	✓	✓	✓	✓	✓	✓	✓
City-Level Controls	✓	✓	✓	✓	✓	✓	✓	✓

*Notes:* See notes to Table 1. The social participation index ranges from 0 to 4, counting the number of extracurricular activities (sports, band/orchestra, theater, school newspaper) a child has ever participated in. The loneliness index is the equally weighted sum of three normalized variables (mean 0 and standard deviation 1 relative to the population of natives). The friendship support index is the equally weighted sum of two similarly normalized variables. Columns 7-10 are binary outcomes. Standard errors reported in parentheses. \*\*\*, \*\*\*, \* indicate significance at the 5% and 10% level, respectively.

**Table 7.** Parental Schooling Investments, Academic Achievement, and Language at Home

Dependent Variable:	<i>No schooling support from parents</i>		<i>No schooling support from parents and low academic achievement</i>		<i>Never speaks German with one or both parents</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
	Muslim	Non-Mus.	Muslim	Non-Mus.	Muslim	Non-Mus.
<b>Panel A: Girls</b>						
Immigrant	0.065 (0.055)	0.164*** (0.062)	0.025 (0.045)	0.055 (0.050)	0.241*** (0.026)	0.227*** (0.023)
Post-reform*Immigrant	0.157** (0.073)	-0.043 (0.087)	0.136** (0.061)	-0.024 (0.070)	0.076** (0.034)	-0.022 (0.033)
Observations	935	868	921	857	923	856
<b>Panel B: Boys</b>						
Immigrant	0.230*** (0.063)	0.371*** (0.071)	0.211*** (0.051)	0.318*** (0.057)	0.443*** (0.030)	0.132*** (0.024)
Post-reform*Immigrant	-0.061 (0.081)	-0.246** (0.096)	-0.103 (0.066)	-0.258*** (0.077)	0.036 (0.038)	0.142*** (0.031)
Observations	931	876	911	857	921	868
Month of Birth Fixed Effects	✓	✓	✓	✓	✓	✓
Family Characteristics	✓	✓	✓	✓	✓	✓
City-Level Controls	✓	✓	✓	✓	✓	✓

*Notes:* See notes to Table 1. Schooling support is defined as whether one or both parents support their child in school interacted with whether their child lives with them. Low academic achievement is defined as receiving below average grades in school. Language at home is defined as whether one or both parents never speak German with their child interacted with whether their child lives with them. Standard errors reported in parentheses. \*\*, \* indicate significance at the 5% and 10% level, respectively.

**Table 8.** Disillusionment and Forgoing a Career for Family

Dependent Variable:	Aspires to pursue tertiary education		High vs. low odds of achieving educational aspirations		Disillusionment (high aspirations and low success odds)		No schooling support from parents and disillusionment		Odds of having to forgo career for family	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Muslim	Non-Mus.	Muslim	Non-Mus.	Muslim	Non-Mus.	Muslim	Non-Mus.	Muslim	Non-Mus.
<b>Panel A: Girls</b>										
Immigrant	0.063 (0.059)	0.159** (0.067)	0.134** (0.057)	0.027 (0.066)	-0.066 (0.045)	0.078 (0.053)	-0.062** (0.029)	0.040 (0.034)	-10.73*** (3.561)	0.971 (3.992)
Post-reform*Immigrant	0.110 (0.078)	0.051 (0.094)	-0.210*** (0.076)	0.012 (0.093)	0.194*** (0.060)	-0.001 (0.074)	0.146*** (0.039)	-0.014 (0.048)	8.198* (4.759)	-16.77*** (5.671)
Observations	952	880	934	865	933	864	916	852	931	867
<b>Panel B: Boys</b>										
Immigrant	0.171*** (0.065)	0.190** (0.074)	-0.033 (0.059)	-0.126* (0.068)	0.059 (0.040)	0.118** (0.047)	0.074*** (0.024)	0.108*** (0.028)	-4.912 (3.713)	-9.467** (4.198)
Post-reform*Immigrant	0.009 (0.084)	-0.050 (0.099)	0.073 (0.076)	0.034 (0.091)	-0.034 (0.051)	-0.040 (0.062)	-0.048 (0.031)	-0.075** (0.037)	4.822 (4.744)	6.902 (5.639)
Observations	942	888	920	866	920	866	909	854	930	876
Month of Birth Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Family Characteristics	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
City-Level Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: See notes to Table 1. Columns 1-8 report results for binary outcomes, while columns 9-10 report results for an outcome measured as a percent. Standard errors reported in parentheses. \*\*, \* indicate significance at the 5% and 10% level, respectively.

**Table 9.** Expected Future Change in Life Satisfaction

Dependent Variable:	<i>Expected change in life satisfaction 5 years in the future</i>	
	(1)	(2)
	Muslim	Non-Muslim
<b>Panel A: Girls</b>		
Immigrant	-0.679*** (0.131)	-0.115 (0.148)
Post-reform*Immigrant	0.646*** (0.174)	0.150 (0.208)
Observations	938	867
<b>Panel B: Boys</b>		
Immigrant	0.088 (0.130)	0.075 (0.145)
Post-reform*Immigrant	-0.118 (0.167)	-0.344* (0.193)
Observations	932	881
Birth Month FE	✓	✓
Family Characteristics	✓	✓
City-Level Controls	✓	✓

*Notes:* See notes to Table 1. The dependent variable is the standardized difference (mean 0 and standard deviation 1 relative to the population of natives) between life satisfaction five years in the future and life satisfaction now. Standard errors reported in parentheses. \*\*, \* indicate significance at the 5% and 10% level, respectively.



**Table 10.** Evidence on Alternative Mechanisms

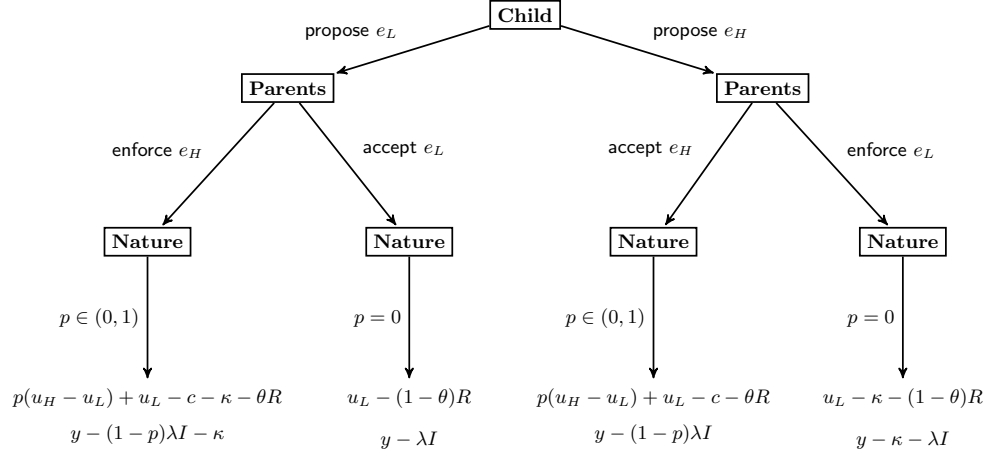
Dependent Variable:	<i>Foreigners have to do a great deal to be recognized</i>		<i>Need to be German to succeed</i>		<i>Life satisfaction with younger sibling of opposite sex</i>		<i>Life satisfaction with older sibling of opposite sex</i>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Muslim	Non-Mus.	Muslim	Non-Mus.	Muslim	Non-Mus.	Muslim	Non-Mus.
<b>Panel A: Girls</b>								
Immigrant	0.138 (0.135)	0.070 (0.141)	0.000 (0.024)	0.116** (0.041)	0.771** (0.142)	0.250 (0.210)	0.954** (0.215)	0.295* (0.163)
Post-reform*Immigrant	-0.045 (0.158)	-0.143 (0.240)	-0.006 (0.033)	-0.127** (0.055)	-0.509** (0.182)	-0.138 (0.268)	-0.573* (0.294)	-0.138 (0.296)
Observations	913	845	942	869	517	464	436	417
<b>Panel B: Boys</b>								
Immigrant	0.443** (0.161)	0.116 (0.163)	0.052 (0.045)	0.051 (0.057)	0.029 (0.189)	-0.057 (0.161)	0.327** (0.149)	-0.019 (0.190)
Post-reform*Immigrant	-0.198 (0.214)	-0.334 (0.230)	0.053 (0.063)	-0.023 (0.070)	0.195 (0.207)	0.235 (0.220)	-0.111 (0.201)	0.008 (0.246)
Observations	899	846	928	874	488	457	454	431
Birth Month FE	✓	✓	✓	✓	✓	✓	✓	✓
Family Characteristics	✓	✓	✓	✓	✓	✓	✓	✓
City-Level Controls	✓	✓	✓	✓	✓	✓	✓	✓

*Notes:* See notes to Table 1. Columns 1-4 are binary outcomes, and columns 5-8 is standardized life satisfaction. Standard errors reported in parentheses. \*\*, \* indicate significance at the 5% and 10% level, respectively.

## For Online Publication: Appendix Figures and Tables

### “Caught between Cultures: Unintended Consequences of Improving Opportunity for Immigrant Girls”

by Gordon B. Dahl, Christina Felfe, Paul Frijters, and Helmut Rainer

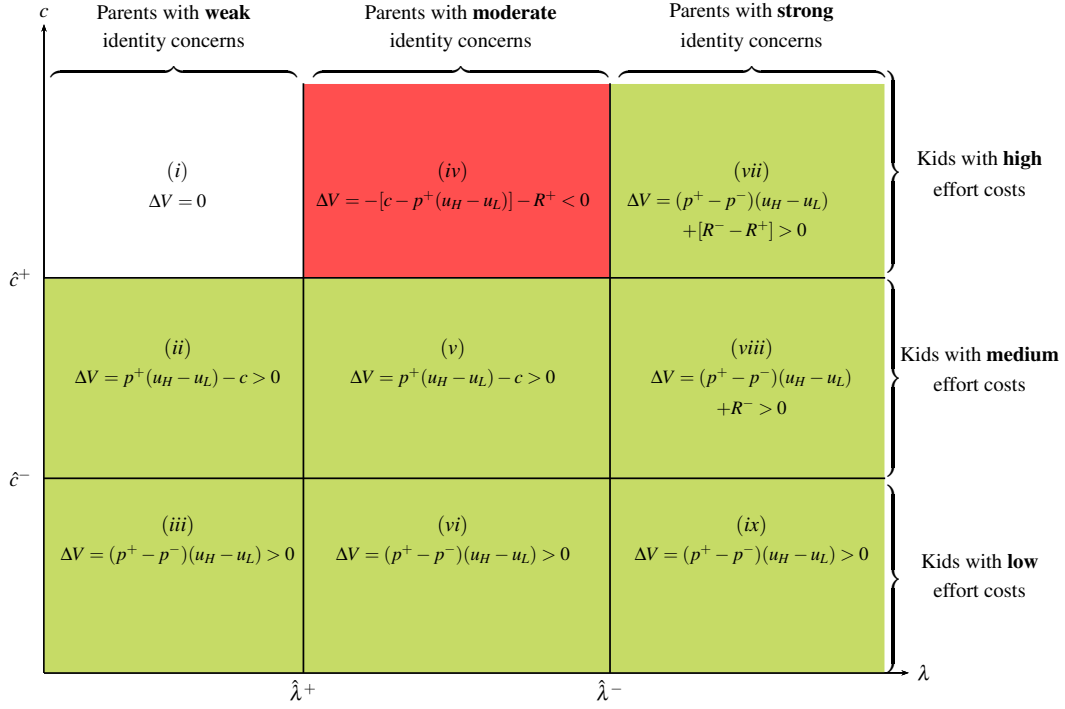


#### Interpretation of Parameters:

$I$  ... identity externality  
 $\lambda$  ... intensity of identity  
 $\kappa$  ... cost of intra-family conflict  
 $\theta$  ... indicator for high effort cost ( $= 1$  if  $c > \hat{c}$ ) versus low effort cost ( $= 0$  if  $c \leq \hat{c}$ )  
 $R$  ... regret; increasing function of foregone career opportunities  
 $y$  ... parents' exogenous income

*Note:* For simplicity, the figure suppresses the arguments of  $R$  (i.e.,  $p(u_H - u_L) - c$  if  $\theta = 0$ ;  $c - p(u_H - u_L)$  if  $\theta = 1$ ).

**Figure A1.** Family Bargaining with Mainstream Identity-Based Preferences



Notes: For simplicity, the figure uses the notation  $R^+ = R(c - p^+(u_H - u_L))$  and  $R^- = R(c - p^-(u_H - u_L))$ . Red shaded areas represent parameter values where an increase in  $p$  results in a reduction in children's utility. Green shaded areas represent parameter values where an increase in  $p$  results in an increase in children's utility.

**Figure A2.** Changes in Well-Being when Economic Opportunities Increase: The Case of Mainstream Identity-Based Preferences

**Table A1.** Descriptive Statistics

	Natives (1)	Immigrants (2)	Immigrants born pre-policy (3)	Immigrants born post-policy (4)	p-value (3)-(4)
<b>Panel A: Girls</b>					
<i>Family Characteristics</i>					
Education Mother: Low	.23	.37	.38	.36	.66
Education Mother: Medium	.44	.18	.18	.19	.87
Education Mother: High	.20	.15	.13	.18	.29
Education Mother: Unknown	.13	.29	.31	.28	.61
Mother's Age	45.24	43.51	44.03	42.96	.17
Education Father: Low	.26	.29	.27	.32	.29
Education Father: Medium	.33	.23	.24	.21	.54
Education Father: High	.24	.15	.15	.16	.65
Education Father: Unknown	.18	.33	.35	.31	.41
Father's Age	49.23	48.02	48.77	47.24	.16
<i>City-Level Controls</i>					
City Size>100,000	.25	.10	.08	.11	.39
Male Unemployment	11.12	11.24	11.31	11.17	.34
Female Unemployment	10.09	10.73	10.89	10.57	.23
Immigrant Unemployment	24.92	24.94	25.10	24.78	.29
Youth Unemployment	8.75	8.78	8.85	8.70	.29
Observations	756	326	166	160	
<b>Panel B: Boys</b>					
<i>Family Characteristics</i>					
Education Mother: Low	.22	.28	.31	.25	.31
Education Mother: Medium	.42	.22	.16	.27	.02
Education Mother: High	.24	.15	.16	.14	.72
Education Mother: Unknown	.13	.35	.37	.33	.46
Mother's Age	46.15	44.38	44.50	44.28	.85
Education Father: Low	.25	.25	.26	.24	.63
Education Father: Medium	.31	.23	.21	.25	.46
Education Father: High	.27	.17	.14	.19	.31
Education Father: Unknown	.17	.35	.39	.33	.31
Father's Age	50.04	48.71	48.84	48.61	.86
<i>City-Level Controls</i>					
City Size>100,000	.27	.12	.12	.12	1.00
Male Unemployment	10.98	11.07	11.26	10.93	.10
Female Unemployment	9.93	10.60	10.86	10.40	.14
Immigrant Unemployment	24.49	24.6	24.74	24.5	.54
Youth Unemployment	8.64	8.66	8.84	8.51	.06
Observations	779	272	119	153	

*Notes:* Sample restricted to a  $\pm 6$ -month window centered around the reform's cut-off date. Natives are children whose parents are both German-born. Immigrants are children who are German-born but whose parents are both foreign-born (second generation immigrants). Pre-policy and post-policy refer to immigrants who are either born before (in 1999) or after (in 2000) the reform's cut-off date. Parents with low, medium, and high education have completed, respectively, at most lower secondary school (Hauptschule), at most intermediate secondary school (Realschule), and upper secondary school (Gymnasium) or university. All unemployment measures are based on unemployment rates at the city level measured in 2015, the year of our survey.

**Table A2.** Descriptive Statistics for Religious Affiliation and Country of Origin

	Natives		Immigrants	
	Girls	Boys	Girls	Boys
	(1)	(2)	(3)	(4)
<i>Religion</i>				
Catholic	0.14	0.15	0.19	0.15
Protestant	0.61	0.49	0.08	0.09
Muslim	0.03	0.01	0.60	0.60
None	0.19	0.30	0.03	0.07
Other, Missing	0.04	0.06	0.10	0.09
<i>Mother's Country of Origin</i>				
Germany	1.00	1.00	-	-
Turkey	-	-	0.42	0.43
Balkan	-	-	0.11	0.11
Eastern Europe	-	-	0.12	0.12
Post-Soviet	-	-	0.11	0.12
Southern Europe	-	-	0.05	0.02
Central and Northern Europe	-	-	0.01	0.01
Middle East	-	-	0.05	0.07
Asia	-	-	0.03	0.05
Africa	-	-	0.08	0.06
Rest of World	-	-	0.01	0.01
Unidentified	-	-	0.02	0.01
Observations	756	779	326	272

*Notes:* Sample restricted to a  $\pm 6$ -month window centered around the reform's cutoff date. Natives are children whose parents are both German born. Immigrants are children who are German born but whose parents are both foreign born (second generation immigrants).

**Table A3.** Descriptive Statistics for Dependent Variables

	<i>Girls and Boys</i>		<i>Girls</i>		<i>Boys</i>	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Standardized Variables</i>						
Life satisfaction	7.46	2.27				
I am satisfied with myself	4.41	1.29				
I have positive character traits	4.46	1.12				
I am as capable as other people	4.35	1.14				
I am person with value and self-worth	4.47	1.43				
I have a positive attitude towards myself	4.41	1.45				
It's easy for me to make new friends	4.16	1.44				
I often feel lonely	2.07	1.38				
I want to have more contact with others	3.08	1.49				
My circle of friends and I do a lot together	4.59	1.34				
I receive support from my friends when I have worries and problems	5.03	1.31				
Foreigners have to do a great deal to be recognized	3.23	1.47				
Change in future life satisfaction	0.71	1.89				
<i>Non-Standardized Variables</i>						
Social Participation (count of no. of extracurricular activities, 0-4)			0.90	1.00	0.59	0.76
Strong vs. weak identification with Germany (1/0)			0.87	0.34	0.87	0.34
No schooling support from parents (1/0)			0.26	0.44	0.29	0.45
No schooling support from parents and low academic achievement (1/0)			0.14	0.35	0.15	0.35
Aspires to pursue tertiary education (1/0)			0.56	0.50	0.44	0.50
High vs. low odds of achieving educational aspirations (1/0)			0.67	0.47	0.76	0.43
Disillusionment (high aspirations and low success odds) (1/0)			0.15	0.36	0.084	0.28
No schooling support from parents and disillusionment (1/0)			0.06	0.23	0.02	0.15
Odds of having to forgo career for family (0-100%, 10% increments)			47.73	29.21	35.85	26.90
Beliefs that one needs to German to succeed (1/0)			0.07	0.26	0.08	0.27

*Notes:* For standardized variables, we report the mean and standard deviation for all native children (born pre- and post-policy). For non-standardized variables, we report the mean and standard deviation for native children born pre-policy.

**Table A4.** Impact of Birthright Citizenship on Subjective Well-Being, Triple Interactions

Dependent Variable:	<i>Standardized Life Satisfaction</i>			
	(1)	(2)	(3)	(4)
Female	-0.423*** (0.070)	-0.425*** (0.070)	-0.433*** (0.070)	-0.428*** (0.070)
Post-reform	-0.030 (0.069)	0.120 (0.120)	0.112 (0.120)	0.108 (0.120)
Post-reform*Female	0.078 (0.099)	0.080 (0.099)	0.095 (0.098)	0.085 (0.099)
Immigrant	0.054 (0.101)	0.047 (0.101)	0.117 (0.103)	0.102 (0.103)
Immigrant*Female	0.404*** (0.135)	0.408*** (0.136)	0.405*** (0.135)	0.402*** (0.135)
Post-reform*Immigrant	0.109 (0.137)	0.120 (0.137)	0.109 (0.137)	0.115 (0.137)
Post-reform*Immigrant*Female	-0.418** (0.187)	-0.420** (0.188)	-0.426** (0.187)	-0.418** (0.187)
Observations	2,133	2,133	2,133	2,133
Birth Month FE	-	✓	✓	✓
Family Characteristics	-	-	✓	✓
City-Level Controls	-	-	-	✓

*Notes:* See notes to Table 1. Standard errors reported in parentheses. \*\*, \* indicate significance at the 5% and 10% level, respectively.

**Table A5.** Gender Norms

	WEF Gender Gap Index	Economic Participation	Political Empowerment	Educational Attainment	Health & Survival
<b>Natives</b>	0.752	0.669	0.366	0.995	0.979
<b>Immigrants:</b>					
Non-Muslim	0.674	0.635	0.104	0.979	0.976
Muslim	0.589	0.442	0.056	0.887	0.969

*Notes:* Source data come from the 2018 Global Gender Gap report provided by the World Economic Forum. Each subindex measures the gap between men and women, where the highest possible score is 1 (full equality) and the lowest is 0 (full inequality). The WEF Gender Gap Index is the average of the four subindices. We assign each child in our sample the gender indices associated with his or her mother's birth country. The numbers reported above correspond to the respective means for the subgroup of native, Muslim immigrant, and non-Muslim immigrant children.

**Table A6.** Alternatives Measures of Traditionality

Dependent Variable:	<i>Standardized Life Satisfaction</i>	
	(1)	(2)
	<b>Prob. Muslim<math>\geq</math>.75</b>	<b>Turkey &amp; Middle East</b>
<b>Panel A: Girls</b>		
Immigrant	0.777*** (0.125)	0.738*** (0.143)
Post-reform*Immigrant	-0.412** (0.168)	-0.362* (0.190)
Observations	952	903
<b>Panel B: Boys</b>		
Immigrant	0.143 (0.119)	0.179 (0.131)
Post-reform*Immigrant	0.046 (0.153)	0.001 (0.168)
Observations	944	911
Birth Month FE	✓	✓
Family Characteristics	✓	✓
City-Level Controls	✓	✓

*Notes:* See notes to Table 1. Column 1 shows results for the sample of children whose predicted probability of being Muslim is over 75%. To predict the probability children are Muslim, we regress the self-reported Muslim variable on dummy variables for the countries of origin for both mothers and fathers. Column 2 shows results for the sample of children whose mother and father both come from either Turkey or the Middle East, regions which are predominantly Muslim. Standard errors reported in parentheses. \*\*, \* indicate significance at the 5% and 10% level, respectively.



**Table A7.** Impact of Birthright Citizenship on Academic Achievement

Dependent Variable:	<i>Standardized GPA</i> (lower is better)	
	(1)	(2)
	<b>Muslim</b>	<b>Non-Muslim</b>
<b>Panel A: Girls</b>		
Immigrant	0.069 (0.112)	-0.124 (0.129)
Post-reform*Immigrant	-0.004 (0.150)	0.001 (0.183)
Observations	939	870
<b>Panel B: Boys</b>		
Immigrant	0.215 (0.142)	0.051 (0.162)
Post-reform*Immigrant	-0.334* (0.182)	0.003 (0.217)
Observations	923	870
Birth Month FE	✓	✓
Family Characteristics	✓	✓
City-Level Controls	✓	✓

*Notes:* See notes to Table 1. Standardized GPA is average grades in German and mathematics, standardized by subtracting the mean and dividing by the standard deviation of natives. Standard errors reported in parentheses. \*\*, \* indicate significance at the 5% and 10% level, respectively.