More Opportunity, More Cooperation? The Behavioral Effects of Birthright Citizenship on Immigrant Youth*

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Abstract: Inequality of opportunity, particularly when overlaid with cultural differences, increases the social distance between individuals, which limits the scope of cooperation. A central question, then, is how to overcome such obstacles to cooperation. We study this question in the context of Germany, by asking whether the propensity of immigrant youth to cooperate with native peers was affected by a major opportunity-enhancing reform: the introduction of birthright citizenship. Our unique setup exploits data from a large-scale lab-in-the-field experiment in a quasi-experimental evaluation framework. We find that the policy caused male, but not female, immigrants to significantly increase their cooperativeness toward natives. *JEL Codes:* C93, D90, J15, K37.

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I. Introduction

Immigration has shaped, and continues to shape, many nations. This brings with it the challenge of integrating immigrants and their children into society. Among economists, one important take on integration—and on policies that promote it—is to emphasize convergence in the outcomes of immigrants and those of the host population in economic dimensions such as educational attainment and labor market participation (Card and Krueger [1992]; Algan *et al.* [2010]; Sweetman and van Ours [2014]).

Another fundamental, but much less scrutinized, aspect of integration pertains to social interactions between immigrants and natives. Many such interactions, from everyday private exchanges to the provision of neighborhood amenities to working in teams, are not governed by enforceable contracts. Therefore, they almost always involve conflicts of interest and hold-up problems, and socially efficient outcomes will only be achieved if people are willing to cooperate.

However, socioeconomic differences between immigrants and natives can act as a barrier to cooperation. For example, inequality of opportunity—particularly when overlaid with racial, ethnic, or cultural differences—increases the social distance between individuals, which is believed to limit the scope of cooperation (Hoffman *et al.* [1996]). It is also conceivable that individuals from disadvantaged groups adopt oppositional identities, which is said to involve "anti-social" behavior (Akerlof and Kranton [2000]). Thus, to think clearly about integration interventions, it is not enough to know about their impact in terms of educational or occupational outcomes; we should also be concerned about their potential in fostering cooperation between individuals of diverse backgrounds and perspectives.

To examine this issue, this paper zooms in on one fundamental mechanism for immigrant inclusion. Specifically, we study the effects of a major citizenship reform in Germany—the introduction of *birthright citizenship* on January 1, 2000—in terms of inter-group cooperation between immigrant and native youth. Our unique setup combines the advantages of experimental economics in studying in-group/out-group phenomena (Fershtman and Gneezy [2001]; Chen and Li [2009]) with the way in which labor economists have come to frame causal questions. In particular, we (i) conducted an incentivized lab-in-the-field experiment based on the investment (or "trust") game with a sample of more than 4,000 adolescents;[1] (ii) allowed participants to condition their strategies on the identity of their opponents; (iii) linked the experimental data with information from an extensive socioeconomic survey; and (iv) chose a sample design that allows us to connect the experiment with the citizenship reform using quasi-experimental identification strategies.

Birthright citizenship—the rule that all children born on a nation's soil obtain citizenship at birth—is subject to much controversy. For example, when Donald Trump became the first major U.S. presidential contender to endorse ending birthright citizenship, some saw it as an effective way of containing illegal immigration and birth tourism.[2] Yet others rallied to point out that birthright citizenship is one of the most powerful mechanisms of social inclusion (National Academies of Sciences, Engineering, and Medicine [2015]). This controversy, and similar debates in Europe, is surprisingly uninformed by reliable evidence

[1] As will be explained in more detail below, we have avoided selection in and attrition from the experiment by running it in 219 classes of 57 German schools during regular school hours in the final year of compulsory schooling. Throughout, we will use the terms children, youth and adolescents interchangeably to refer to the participants in our study.

[2] See, for example, a CNN article dated 18 August 2015, "Birthright citizenship: Can Donald Trump change the constitution?" (`http://edition.cnn.com/2015/08/18/politics/birthright-citizenship-trump-constitution/index.html`, accessed Oct 10, 2017)).

from countries that have changed their regulations regarding birthright citizenship.

We exploit such a change and take a first step towards tracing its impact on the lives of young immigrants. The main mechanism we have in mind is about opportunity. When Germany introduced birthright citizenship, a large portion of immigrant children were automatically endowed with the host country's nationality at birth and, thus, with the same legal rights, and the attendant political and economic opportunities, as their native counterparts. This had a number of important consequences. First, it has been a catalyst for integration efforts and investments in child "quality" in immigrant families (Avitabile *et al.* [2013, 2014]). Second and relatedly, the policy led to a near-closure of the immigrant-native gap in a series of educational outcomes measured over children's early life cycle (Felfe *et al.* [forthcoming]). These effects have contributed to levelling the playing field between immigrant and native youth and have reduced the social distance between them. Therefore, in this paper, we hypothesize that the introduction of birthright citizenship has spilled over into the sphere of inter-group cooperation. We test this hypothesis among the first cohort of immigrant children affected by the reform in their final year of compulsory schooling, more than 15 years after the policy took effect.

The context of our study is the OECD's second-largest country of immigration after the United States (OECD [2014]). Currently, immigrant youth account for one-third of Germany's children under the age of 15, and the largest minority group of youths, by far, are immigrant children of Turkish origin. Our own survey, as well as other household surveys, suggests that there are several major dividing lines between them and their native peers. The first is religion: the majority of native German children have a Christian religious affiliation, while Islam is the dominant religion among immigrant children. Second, immigrant children are more likely than non-immigrant children to have parents with low educational attainment and to grow up in low-income conditions. Third, a good deal of evidence also indicates that immigrant children are outperformed by their German peers along multiple indicators of academic achievement (Dustmann *et al.* [2012]). Thus, we are dealing with native and immigrant children who are strongly segmented in terms of cultural and social characteristics.

Our lab-in-the-field experiment builds on the pioneering work of Fershtman and Gneezy [2001], who used the investment game (Berg *et al.* [1995]) to study non-market interactions between real social groups. The advantage of using an incentivized experiment rather than a questionnaire is, as succinctly put by Fershtman and Gneezy [2001], that it captures people's behavior and not what people claim or believe to be their own behavior. We take the investment game as a vehicle to measure the extent to which individuals belonging to different groups are willing to cooperate, and the version we implement is based on the following idea. In a segmented migration society such as Germany, "being a native" or "being an immigrant" are amongst the core attributes of individuals (next to gender) that determine their social identities. Moreover, these attributes are ubiquitous and easy to perceive, and therefore, they are likely to feed into social interactions. Thus, using the strategy-vector method (Selten [1967]; Falk and Zehnder [2013]), we allowed participants to condition their decisions on the gender and migration background of their opponents.[3] Our main measure for intra- versus inter-group cooperation is the in-group/out-group investment gap that senders reveal in the first stage of the investment game. For natives it is the amount they send to other natives relative to the quantity they send to immigrants, while for immigrants it is the amount they send to other immigrants relative to the quantity they send to natives.

[3]For example, in the first stage of the investment game, participants had to decide how much of their initial endowment to send to a boy/girl with German-born/foreign-born parents.

The main contribution of this paper is to examine the experimental data by exploiting natural variation induced by the following policy change. Until December 31, 1999, Germany granted birthright citizenship based on *jus sanguinis* (right of blood), i.e., only children born to German nationals received citizenship at birth. After January 1, 2000, the regime changed to a restricted version of *jus soli* (right of soil), i.e., every child born on German territory gained a conditional right to German citizenship. The conditionality attached to *jus soli* was that at least one parent had been a legal resident in Germany for eight years or more at the time of birth of the child. This setting provides us with a birth date eligibility cut-off, which serves as our source of identification. In particular, exploiting the quasi-random assignment of birthright citizenship around the cut-off, we analyze whether the policy caused discontinuities in immigrant children's propensity to cooperate with in-group and out-group members. To that end, we employ a regression discontinuity difference-in-differences (RD-DID)-type model that compares immigrant children born shortly before and shortly after the cut-off date and draws upon native children as a control group.

We obtain three main sets of results. First, the investment behavior of immigrant children born *pre-policy*, both boys and girls, reveals a marked gap between intra- and inter-group cooperation: on average, they transfer roughly 60% of their endowment to children with whom they share an immigrant identity, while their investments to native German children are 15% lower. Second, the introduction of birthright citizenship significantly affected immigrant children's in-group/out-group behavior, but in a gender-specific way. In particular, we observe an in-group/out-group investment gap of 15% for immigrant boys born pre-policy, while for those born under *jus soli*, it is 11 percentage points, or almost 70% lower. This effect is entirely driven by an increase in out-group cooperation. For immigrant girls, the birth date cut-off does not matter at all: among those born pre-policy, investments to immigrants exceed investments to natives by 17%, and this difference persists for those born post-policy. Third, we restrict our analysis to the sample of children with Turkish origin—not only by far the largest minority group in our sample, but also the group with the longest immigration history in Germany. Given the residency criterion attached to birthright citizenship, for Turkish immigrants the reform had strong bite and thus represents a particularly effective source of exogenous variation. We find, indeed, that the reform effects are particularly pronounced for this immigrant subgroup: for Turkish boys born pre-policy, the in-group/out-group investment gap amounts to 20%, which completely vanishes for those born post-policy. Several robustness checks corroborate these results.

In a next step, we explore the main hypothesized channel—a reduction in social distance—for the reform effect. To that end, we return to the main theme of our own previous research (Felfe *et al.* [forthcoming]) and extend our findings on the educational effects of *jus soli* in two directions: we (i) exploit information on school grades from our own survey, and (ii) conduct an analysis by gender. For immigrant boys, we find that the policy not only caused more cooperation with natives; it also led to a near-closure of a substantial pre-existing educational achievement gap between them and their native peers. Thus, education relative to natives appears to be an important factor in immigrant boys' in-group/out-group cooperation and is our main mechanism for the reform effect on out-group cooperation.[4] This explanation, however, does not hold for girls: those born pre-policy were not lagging much behind their native female peers educationally, but nevertheless were unwilling to cooperate with them in the experiment; moreover, the

[4] We also provide evidence that other plausible channels—e.g., a stronger host-country identification or a differential treatment of naturalized and non-naturalized immigrants by natives—are unlikely explanations for the reform effect.

policy did not change this picture.

Thus, in a final step, we investigate what might explain immigrant girls' low out-group cooperation and the lack of an effect of increased opportunities. Although our study was not set up to address this (unanticipated) result in a causal way, we take some first steps to explore it. Across many immigrant groups, girls are socialized to be "keepers of the culture" (Suárez-Orozco and Qin [2006]). The existing literature suggests that this may have two relevant consequences: immigrant girls are more likely than immigrant boys to self-identify with their parents' immigrant origins; and they are more likely to face high levels of parent-child conflict and, consequently, to have lower self-esteem. To the extent that high ethnic self-identification and low self-esteem have been linked to out-group discrimination (Hogg and Abrams [1990]; Rubin and Hewstone [1998]), we consider these as plausible explanations for the gender-specific patterns we have uncovered. Based on our survey, we provide descriptive evidence that is consistent with these explanations.

Our study relies on the combination of a lab-in-the-field experiment with a natural experiment and provides novel insights into the interface between immigration, citizenship and inter-group cooperation. It builds upon and connects a number of papers that span the fields of experimental economics, labor economics, and political science. Our experimental design is an outgrowth of ideas developed by Fershtman and Gneezy [2001]. In their experiments with Ashkenazi (Western) and Sephardi (Eastern) Jews in Israel, they find systematic discrimination towards men of Eastern origin. Inspired by this work, there has developed a small but active stream of literature in experimental economics on cooperation, trust, and discrimination between immigrants and natives. For instance, Guillen and Ji [2011] focus on domestic university students and their Asian international peers in Australia; Cox and Orman [2015] study first-generation immigrants and native-born Americans in the United States; and Cettolin and Suetens [forthcoming] assess non-Western immigrants and native Dutch in the Netherlands. A study that is close in spirit to ours is Albrecht and Smerdon [2016]. They exploit a refugee resettlement to a small rural town in Australia to study the effects of a migration shock on social capital. Combining trust data from a lab-in-the-field experiment with survey data from both treatment and similar control towns, they find that citizens in the treated town (i.e., who experienced the refugee resettlement) trust refugees relatively more than those in untreated towns.

Our study connects the literature described above with a number of studies in labor economics and political science that explore the effects of citizenship on immigrants. Chiswick [1978b] was among the first to analyze the effect of Americanization on the earnings of foreign-born men. More recent contributions have focused on the effects of citizenship on wage growth (Bratsberg *et al.* [2002]; Steinhardt [2012]), employment prospects (Fougère and Safi [2009]; Gathmann and Keller [2018]) and remittances (Piracha and Zhu [2012]). Hainmueller *et al.* [2017] exploit the quasi-random assignment of citizenship in Swiss municipalities that used referendums to decide on the naturalization applications of immigrants. Their main finding is that receiving the host country's nationality strongly improves immigrants' long-term social integration.

The remainder of the paper is structured as follows. In Section II, we focus on the lab-in-the-field experiment: we describe our setting, sample and experimental design, and provide basic results on intra- and inter-group cooperation among Germany's youth. Section III explains how we connect the lab-in-the-field experiment with the natural experiment of Germany's introduction of birthright citizenship. Section

IV contains the main results on the reform effect and its possible explanations. Section V concludes by offering some thoughts on policy implications and directions for future research. Four Appendices collect additional material.

II. The Lab-in-the-Field Experiment

II.A. Motivation and Key Implementation Challenge

The cetnral idea of this study is to use experimental data on cooperation in a quasi-experimental evaluation framework. The specific question we ask is whether the propensity of immigrant youth to cooperate with native peers was affected by the introduction of birthright citizenship in Germany. A necessary condition for the implementation of our empirical approach is a large sample of children born in a narrow window around the reform's enactment date (January 1, 2000). To get at this group of children, and to ensure a large enough number of observations for immigrant youth in particular, we opted to run the experiment in schools. A close collaboration with two state ministries of education and school principals allowed us to collect data in 219 classes of 57 German schools during regular school hours. Given our empirical strategy, we restricted attention to a single school cohort, namely that composed of children mainly born in 1999 and 2000. When the experiment took place, all participants were in their final year of compulsory schooling, and thus 15 to 16 years of age.

II.B. Setting and Subject Pool

We first sought approval of and support for our study from educational authorities in Germany. The ministries of education of two German federal states—Schleswig-Holstein (SH) and North Rhine-Westphalia (NRW)—approved our design and offered to support the project's implementation. Critically, this support included encouraging secondary schools in eight cities to participate in our project, which resulted in 57 school principals providing their agreement. The two federal states in which the study was run have independent education systems, which differ along one important dimension: in SH, the duration of compulsory schooling is nine years, while in NRW it lasts for ten years. In both federal states, a school year starts in August/September and ends in June/July. There were two waves of data collection. In the first wave, lasting from June 2 to July 16, our target population were all 9th graders from 31 schools (spread over 122 classes) in six cities of SH.[5] In the second wave, lasting from October 19 to November 16, we targeted all 10th graders of 26 schools (spread over 97 classes) in two cities of NRW.[6]

The 57 participating schools belong to five school types: ten schools are secondary general schools ("Hauptschule"); eight are intermediate schools ("Realschule"); 29 are comprehensive schools without the final years of grammar school-type education ("Gesamtschule ohne gymnasialer Oberstufe"); eight are comprehensive schools with the final years of grammar school-type education ("Gesamtschule mit gymnasialer Oberstufe"); and two are grammar schools or high schools ("Gymnasium"). Two weeks prior to the study, school principals informed parents about the study and gave them an opt-out option,

[5] The cities are Flensburg, Kiel, Lübeck, Neumünster, Elmshorn, and Pinneberg, with population sizes ranging from 42,266 in Pinneberg to 246,306 in Kiel.

[6] The cities are Duisburg and Wuppertal, with population sizes of 491,231 and 350,046, respectively.

i.e., parents could proscribe their children's participation.[7] Moreover, immediately before the experiment started, all students present in class were informed by us that participation was voluntary. The experiment was run at the school class-level during two regular consecutive school hours.

On the days we conducted the study, a total of 4,634 students were present in the 219 classes. Parents made use of the opt-out option for 44 of them (less than 1%), while 154 students (3.5%) chose to opt out themselves. Thus, 4,436 students participated in the study. Of those, 133 participants did not fully complete the experimental task, while 226 did not provide the survey information necessary for our basic analysis (i.e., own gender and/or parental migration background). This leaves us with a baseline sample of 4,077 students.

The study consisted of two parts, the investment game (described in detail below) and an extensive socioeconomic survey. Each part lasted approximately one school hour (45 minutes), and the order of the two parts was randomized on a daily basis in order to avoid any potential bias stemming from that sequence.[8] The study was conducted in regular classrooms and was done by paper and pen. To guarantee privacy, we installed mobile privacy screens between students.[9] We ensured anonymity by assigning a unique identity code to each participant.

II.C. Sample Description

Our survey provides information, *inter alia*, about participants' date of birth, country of birth, gender, school achievements, aspirations, preferences and interests, personality traits, and family background. Two key family background variables are the birth places 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 2,201 native children (54%), 1,218 immigrant children (30%) and 658 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).

The empirical analysis contained in this paper mainly draws upon the experimental choices of immigrant and native children, while using that of mixed-background children only in passing. Thus, the following sample description is confined to the former two groups (for details, see Appendix Table A.1). In Germany, the largest minority group of youth, by far, are immigrant children of Turkish origin. This is also evident in our sample. Specifically, 38% of immigrant children have parents who are Turkish-born, 14% have Middle-Eastern or African backgrounds, 12% have parents born in a post-Soviet country, 11% have parents from a Balkan country, 11% have Eastern European backgrounds, and 14% come from other countries. In terms of family characteristics, roughly one-third of immigrant children have parents with low educational attainment, three-fourths live in two-parent households, and more than one-half report a Muslim religious affiliation. When contrasted with same school-cohort data from the nationally representative German Microcensus, our sample of immigrant youth is comparable in terms of parental education, but it contains more children with a Turkish background (38% versus 31%; see Felfe *et al.* [forthcoming], Table A2).

[7]Parents were, however, not informed about the objectives of the study.

[8]Because none of our experimental results depend qualitatively on the order of events in the session (i.e., whether the survey or the experiment was conducted first), we pool the data of the two types of sessions in the analysis.

[9]See Figure B.1 in Appendix B for a photo of a classroom setup.

A comparison of native and immigrant children suggests several marked differences, of which we mention four. First, roughly one-third of immigrant children have parents with low educational attainment, while the corresponding share for native children is just under one-fourth.[10] Second, immigrant children are more likely than non-immigrant children to live in two-parent households (74% vs. 55%). Third, the majority of native children report a Christian (i.e., Catholic or Protestant) religious affiliation (67%), while the group of immigrants is predominantly made up of Muslim children (59%). Finally, 69% of immigrant children report that they speak a language other than German with their parents at home. This evidence reflects the pronounced cultural, social and economic gaps between native and immigrant children that are also observed in representative surveys.

II.D. The Investment Game: Design and Implementation

Our experiment is based on the standard investment game (Berg *et al.* [1995]), which consists of two players, called the first-mover (sender) and the second-mover (receiver). Each player is endowed with five euros at the beginning of the game. The first-mover decides on the amount to be sent to the second-mover ($x \in [0, 5]$) in steps of 50 €-cents. The transferred amount is then tripled by the experimenter. The second-mover can decide whether to send back any amount $y \in [0, 5 + 3x]$ to the first-mover. The final payoff for the first-mover is $5 - x + y$ and for the second-mover is $5 + 3x - y$. Under the assumption of selfish preferences, the only subgame-perfect equilibrium prescribes no investment and zero returns. By contrast, the social optimum involves "full" cooperation, i.e., the first-mover invests his entire endowment.

In our experiment, we employed the strategy method, i.e., each participant had to decide as first-mover and as second-mover. Moreover, and most importantly for the purpose of this paper, we allowed first-movers to condition their investment decisions on the gender and migration background of possible interaction partners. We implemented this by allowing first-movers to decide whether, and if so, how much, to transfer to each of six possible receiver types (indexed by k): a boy with German parents (S_1), a girl with German parents (S_2), a boy with foreign parents (S_3), a girl with foreign parents (S_4), a boy with foreign parents who possesses German citizenship (S_5), and a girl with foreign parents who possesses German citizenship (S_6).[11] In principle, this setup allows us to understand the extent to which cooperation is dependent on migration background as well as gender. However, the main task this paper sets itself is to examine intra- and inter-group cooperation between native and immigrant youth and how it is influenced by public policy. We will therefore largely abstract away from cooperation conditional on gender, apart from remarks, when deemed necessary. Thus, we collapse the six choices $\{S_1, ..., S_6\}$ into two variables: a participant's average investment to natives (S_N) and his or her average investment to immigrants (S_I), defined as

$$S_N = \frac{1}{2}\sum_{k=1}^{2} S_k \quad \text{and} \quad S_I = \frac{1}{4}\sum_{k=3}^{6} S_k.^{12}$$

[10] As Appendix Table A.1 also shows, a relatively large proportion of immigrant children report that they do not know their parents' educational attainment.

[11] See Appendix B for the translated decision sheets.

[12] Receiver types $k \in \{3, 4\}$ capture all immigrants (i.e., boys and girls with foreign parents), while receiver types $k \in \{5, 6\}$ capture only the subset of naturalized immigrants (i.e., those who possess German citizenship). The reason we have allowed for this distinction will become clear in Section III, where we examine the effects of the German citizenship reform. For our main results, we have chosen not to drop any data, and hence, we compute S_I by averaging over their investments to receiver types $k \in \{3, 4, 5, 6\}$. That said, our results do not hinge on this specification, i.e., they remain qualitatively

Throughout the paper, we refer to S_N as native children's in-group investments and as immigrant children's out-group investments, respectively. Likewise, we refer to S_I as native children's out-group investments and immigrant children's in-group investments, respectively. Our main measure for intra- versus inter-group cooperation is the in-group/out-group investment gap (IG) of senders with and without migration backgrounds. Formally, it is defined as

$$IG = \begin{cases} S_N - S_I & \text{for native children;} \\ S_I - S_N & \text{for immigrant children.} \end{cases}$$

Figure 1 provides summary statistics on the investment choices of native and immigrant children by migration background of second-movers, both for the entire sample and separately by gender. Several interesting patterns emerge. First, for native adolescents, the evidence speaks against a strong pattern of unequal treatment of natives and immigrants. In the full sample [Panel (a)], natives' in-group investments exceed their out-group investments by a statistically significant 2.1% ($S_N = 2.90, S_I = 2.84$; paired t-test with $p < .01$). Looking at this result separately by gender [Panels (b) and (c)], we observe that there is no in-group/out-group variation in the investment choices of native girls ($S_N = 2.70, S_I = 2.72$). Native boys, by contrast, reveal moderate in-group favoritism: their in-group investments exceed their out-group investments by a statistically significant 4.8% ($S_N = 3.08, S_I = 2.94$; $p < .01$). Second, among immigrant children [Panel (a)], we detect a strong bias against natives that manifests itself in a statistically significant in-group/out-group investment gap of 13.4 percent ($S_I = 2.97, S_N = 2.62$, $p < .01$). The subgroup results by gender [Panels (b) and (c)] suggest that this gap is more pronounced for immigrant girls (16.1%; $p < .01$) than for immigrant boys (10.1%; $p < .01$). The main question of this paper, to which turn in the next section, will be whether the introduction of birthright citizenship in Germany has caused discontinuities in immigrants' in-group/out-group behavior.

After participants had completed the first stage of the investment game, they were asked to indicate on their decision sheet the expected back transfer $E_k \in [0, 20]$ from the six possible interaction partners in steps of ten €-cents.[13]

At the final stage of the investment game, participants were asked to play the role of second-movers, and we employed the contingent response method to elicit their back transfers (returns). For example, on a first decision sheet, participants were asked to decide on their back transfers to a boy with German-born parents, contingent on the eleven possible investments of the boy as the first mover. Using the same strategy vector variant, we elicited back payments to the other five potential interaction partners. Amounts between and including 0 and $5 + 3x$ in steps of ten €-cents were allowed.

Before the experiment started, the instructions were distributed to all students in class and read out by an experimenter.[14] Students were informed that they would first play the investment game as the first-mover and thereafter as the second-mover. They were told that they could earn real money and that their payoffs would depend on their own choices and those of another, randomly assigned experiment

unchanged when we compute S_I by averaging over participants' investments to receiver types $k \in \{3, 4\}$, i.e., by letting $S_I = \frac{1}{2}(S_3 + S_4)$.

[13]We chose not to incentivize the elicitation of expectations for reasons of practicality. We thus have to interpret the results based on expectations cautiously.

[14]The translated instructions can be found in Appendix B. All sessions were conducted by one leading experimenter—in most cases, one of the authors—and one or two students assistants, previously trained by us. The experiment followed a strict protocol that was obeyed in every session.

participant from a different school.[15] The average payoff in the experiment was €7.26.[16] Participants received their payoffs no later than two weeks after the experiment took place (in envelopes with their unique identity codes, distributed by school secretaries or head teachers), which was known to them at the beginning of the experiment. All participants faced exactly the same decision tasks, instructions, and payoffs, and all procedures described here were common knowledge.

II.E. Discussion of Experimental Design

Several aspects of the experiment should be discussed. In light of our data requirements, the strategy method—i.e., asking participants to submit contingent decisions for native and immigrant opponents—was the only feasible way for collecting the experimental data. Specifically, through the strategy method, one obtains for each participant a full set of strategies for all possible types of interaction partners, which keeps the required sample size within reasonable bounds. By contrast, through the direct-response method—i.e., randomly assigning to each participant either the role of sender or receiver and randomly matching him/her to one interaction partner of fixed type—one obtains only one decision per participant, which would have required us to draw a sample at least twice as large. Furthermore, the strategy method was the only feasible option in respect of data protection requirements, since it allowed for matching and payoff-calculation procedures entirely based on anonymous IDs. The direct-response method, instead, would have necessitated prior access to class lists containing personal data, which was incompatible with the data protection regulations set out by the cooperating school authorities.

Of course, having to submit strategies for both immigrant and native opponents may lead participants to think about decisions in a different way than had it been feasible to choose the direct-response method. For example, the potential role for experimenter demand biases (i.e., participants confirming or contradicting the experimenter's inferred hypothesis) may be larger. However, our aim is not to assess quantitative responses within our experiment, but to uncover qualitative results. That is, our interest does not lie in the *level* of in-group versus out-group investments *per se*, but in the behavioral effect of birthright citizenship on immigrant youth. To get at this effect empirically, we exploit natural, extra-experimental variation provided by a reform that led to a quasi-random assignment of birthright citizenship around a birthdate cut-off. Importantly, since our causing variable of interest—children's birthdate—is orthogonal to the experimental design, participants could not form conjectures about our objectives. Thus, we are confident that experimenter demand does not generate or reverse a reform effect.

Finally, although our paper is not about a quantitative assessment of experimental responses, we make a brief note on the levels of elicited investments. A survey of the literature shows that the strategy method does not yield experimental results that differ systematically from those gathered through the direct-response method, especially if the stakes involved are high (Brandts and Charness [2011]). In our

[15]To be precise, participants were told that we would calculate final payoffs as follows: (i) we randomly match two participants from two different schools; (ii) we randomly assign the roles of first-mover and second-mover; (iii) we determine the true type k of both the first-mover and the second-mover based on survey information on own/other gender and whether parents are German-born or foreign-born; (iv) we implement the first-mover's decision for the true type of the second-mover; (v) we implement the second-mover's back transfer for the true type of the first-mover and his or her choice implemented in step (iv); and (vi) based on the pair of choices implemented in steps (iv) and (v), we calculate the participants' final payoffs. When we implemented this procedure to calculate participants' payoffs, we treated mixed-background children as children with foreign-born parents. Questions regarding the treatment of mixed-background children in the matching procedure were not raised by participants.

[16]The maximum payoff was €20. For participants whose payoffs were lower than €2, we paid out an unannounced consolation prize of €2.

experiment, the monetary incentives were indeed substantial and should have made it costly to deviate from "true" preferred choices: data from the representative German Socio-Economic Panel suggest that the average payout in the experiment corresponds to more than 70% of the average amount of weekly pocket money given to adolescents with roughly the same characteristics as our participants.

Our estimation strategy in the next section will require us to narrow our full baseline sample, which contains the experimental choices of 4,077 participants. Thus, two self-contained Appendices have been included for readers interested in general (i.e., not reform-related) experimental results based on the full baseline sample. Appendix C demonstrates that general investment and back-transfer patterns in our experiment are comparable to what has been observed in previous, comparable experiments. Moreover, it provides detailed evidence on in-group/out-group investment patterns among Germany's immigrant and native youth, including a heterogeneity analysis for different immigrant groups. Appendix D provides an analysis of the motives behind observed in-group/out-group investments gaps. Specifically, it asks whether such gaps are consistent with distrust (i.e., asymmetric beliefs about opponents' tendency to exploit them) or asymmetric social preferences towards in- and out-group members.

III. The Natural Experiment

III.A. Institutional Background: Jus Soli *vs.* Jus Sanguinis

The path to citizenship for immigrant children varies considerably across immigrant-receiving countries. In the United States, any person born on the nation's territory automatically gains U.S. citizenship, regardless of the nationality or immigration status of the person's parents. This rule, based on *jus soli* ("right of soil"), has been in place since the 19th century and is commonly referred to as birthright citizenship. By contrast, many countries in Europe have granted citizenship at birth based upon the principle of *jus sanguinis* ("right of blood"), meaning that citizenship is inherited through parents rather than determined by the place of birth. For children born to foreign nationals, this rule implies that citizenship can only be acquired through naturalization (i.e., upon application) later in life. Not surprisingly, in countries that have *jus soli*, virtually all native-born children of immigrants have the host-country nationality, while the lowest percentages of immigrant children with host-country nationality are found in countries that adhere to *jus sanguinis* (OECD [2011]).

The context of our study is Germany, a country that has recently witnessed a switch from *jus sanguinis* to *jus soli*. Throughout the 20th century, German citizenship could only be acquired by descent from a German mother and/or a German father.[17] With the turn of the millennium, this principle of *jus sanguinis* was replaced by a restricted version of *jus soli*. In particular, every child born on Germany territory after December 31, 1999 gained a conditional right to German citizenship. The conditionality attached to *jus soli* was that at least one parent had been a legal resident in Germany for eight years or

[17] The legal status of immigrant children born to non-German citizens was either that of a temporary or a permanent resident. Although citizenship and permanent residency allow individuals to live in Germany indefinitely, they are very different statuses. Rights granted to permanent residents include the right to work in Germany and access to welfare benefits. However, permanent residents do not have the right to vote in general elections, are unable to apply for civil servant positions, cannot work in other EU countries, may lose their residency status if out of Germany for more than a year, and face the risk of deportation if they commit a crime. The two legal statuses, citizenship and residency, also have different implications for immigrants' labor market outcomes: compared with their non-naturalized peers, naturalized immigrants earn more [Chiswick, 1978a; 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].

more at the time of birth of the child. For children born to parents who satisfied this residency criterion, German citizenship was *automatically* registered in the birth record without parents (i) having to apply for it and (ii) being able to disclaim it. Between January 1 and December 31, 2000, immigrant parents of children born between 1991 and 1999 were able to use a transition rule allowing them to *retrospectively* apply for their children's citizenship conditional on having legally resided in Germany for at least eight years. However, due to a lack of public information about this transition rule, only a small fraction of eligible families (roughly one-sixth) made use of it.[18]

To use the reform effectively as a source of exogenous variation, it is important to demonstrate that it had bite. The evidence on this can be seen in Figure 2, which draws upon two questions from our survey asking respondents about (i) whether they hold the German citizenship and (ii) since when they have held it. Panel (a) shows that, for all second-generation immigrant children in our sample, 41% percent of those born pre-policy have held the German citizenship since birth, while the corresponding share for those born post-policy amounts to 85%. Put differently, the reform increased the share of second-generation immigrant children who have held German citizenship since birth by 44 percentage points. That said, due the residency criterion attached to birthright citizenship, the intensity of this increase varied largely across immigrant groups. Specifically, when the reform was implemented, a large portion of Turkish immigrants—an immigrant group which started to arrive in Germany via guest worker arrangements in the 1970s—fulfilled the residency criterion and became eligible for *jus soli*. This is confirmed in panel (b) of Figure 2, which shows that 26% of all Turkish children born pre-policy have held the German citizenship since birth, while among those born post-policy this share increased by 68 percentage points to 94%. By contrast, among non-Turkish second-generation immigrants, the reform increased the share of children endowed with citizenship rights at birth by only 24 percentage points (from 54% to 78%, see panel (c) of Figure 2). In reflection of these differential treatment intensities, we will not only provide estimates for a full sample of second-generation immigrant children, but also stratify the sample into Turkish and non-Turkish immigrant children, respectively.

III.B. Empirical Strategy: Exploiting the Natural Experiment

We consider the introduction of *jus soli* in Germany on January 1, 2000, as an exogenous event that led to a quasi-random assignment of birthright citizenship among immigrant children. Since first-generation immigrant children (i.e., those born outside Germany) were unaffected by the reform, we exclude them from the analysis and only retain second-generation immigrant children (i.e., those born in Germany) for the estimation.

To isolate the effect the reform had on immigrant children's behavior, in a first step, we compare the experimental decisions of second-generation immigrants born before and after January 1, 2000. In so doing, it is important to ensure that immigrant parents could not self-select into treatment. Since our source of identification is a birth date cut-off, the main concern is strategic fertility behavior. We address this issue in two ways. First, we restrict our sample to children born in the $\pm$4-month window around January 1, 2000. This ensures that our sample only comprises children who were conceived before July

[18] Children who acquired German citizenship via *jus soli* or the transition rule can hold two passports until the age of 23, when they have to opt for either German citizenship or that of their parents. When the cohort of immigrant children born in 1991 (who acquired German citizenship through the transition rule) had to choose between the two options in 2014, a large fraction opted in favor of their German citizenship (Worbs [2014]).

1999, the month in which the German citizenship reform was ratified. In robustness checks, we further narrow the window around the reform cut-off date. Second, we 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. Having imposed these sample restrictions, we first descriptively examine the behavior of second-generation immigrant children born around the reform's cut-off date by estimating the following simple regression model:

$$\text{IG} = \beta_0 + \beta_1 \text{Born Post-Reform} + \varepsilon, \tag{1}$$

where *IG* refers to the in-group/out-group investment gap. The explanatory variable *Born Post-Reform* is a binary variable indicating whether an immigrant child was born in the months before (=0) or after (=1) January 1, 2000. Estimates of the parameter β_0 thus capture the in-group/out-group investment gap among immigrant children born pre-reform, while estimates of β_1 show how the behavior of immigrant children born post-reform differs from the behavior of those born pre-reform.

A simple before-after comparison such as Equation (1) may be partly driven by the fact that immigrant children born after the policy change are always younger than those born before it. Moreover, it is possible that the characteristics of parents changes over the year (Buckles and Hungerman [2013]). If age or season of birth has an impact on immigrant children's behavior, Equation (1) provides us with a biased estimate of the reform effect. To net out these potential biases, we construct a second difference between pre-policy and post-policy native German children (who were unaffected by the reform cut-off date) and estimate the following difference-in-differences regression discontinuity (RD-DID)-type model:[19]

$$\begin{aligned} \text{IG} =& \gamma_0 + \gamma_1 \text{Immigrant} + \gamma_2 \text{Born Post-Reform} + \gamma_3 (\text{Immigrant} \times \text{Born Post-Reform}) \\ & + \theta \text{Birth Month} + \xi \text{Family} + \vartheta \text{Classroom} + \varepsilon, \end{aligned} \tag{2}$$

In this specification, *Immigrant* is a binary variable indicating whether a child is a second-generation immigrant (=1) or a native (=0). The parameter γ_1 captures differences between immigrant and native children born prior to the policy change. *Born Post-Reform* is a binary assignment variable indicating whether a child was born in the months just after January 1, 2000 (i.e., it is equal to one for children born between January and April 2000 and zero for children born between September and December 1999). The coefficient γ_2 measures general differences between children born before and after the citizenship reform that could cause changes in behavior even in the absence of a policy change. The coefficient of interest is γ_3, which multiplies the interaction *Immigrant*×*Born post-reform* and thus identifies all immigrant children born after the policy change. We include a set of *Birth Month* dummies in all regressions. In extended specifications, we also include *Family* characteristics (i.e., maternal age, maternal education, family structure) and *Classroom* characteristics (i.e., class size, proportion of students with migration background, the gender ratio and five victimization measures[20]).

The coefficient of interest, γ_3, represents the reform's reduced form effect and can be interpreted as the intention-to-treat (ITT) effect of granting immigrant children citizenship at birth. This ITT effect is a conservative estimate of the impact of citizenship at birth, since our sample includes pre-policy children

[19] Similar approaches have been used by Lalive and Zweimüller [2009], Dustmann and Schönberg [2012], Schönberg and Ludsteck [2014], and Danzer and Lavy [2018] within the context of parental leave reforms.

[20] These victimization measures capture the proportions of students who report having been victims in the past year of physical abuse, verbal abuse, lies, theft or exclusion.

who may have qualified for citizenship at birth through *jus sanguinis*. Moreover, our sample includes not only immigrant children who were eligible for birthright citizenship when they were born but also those who were ineligible for it and were thus unaffected by the reform.[21]

Our full estimation sample comprises 920 native German children and 360 second-generation immigrant children. To verify whether treatment was balanced on observables, we present the mean values of key family and classroom characteristics for immigrant children born before and born after the reform in Appendix Table A.2. The evidence shows that there are no systematic differences between children born before and after January 1, 2000. Among the 24 mean difference tests in both samples (see p-values in the last column of Table A.2), only two mean differences are statistically significantly different from zero at the 5 percent level. One of these refer to mother's age, a difference that is to be expected given the reform's cut-off date. This supports the notion that the German citizenship reform was likely an "as-good-as-random" event with no systematic self-selection of particular types of immigrant families across the cut-off date.

IV. Results

IV.A. Behavioral Effects of Birthright Citizenship

Descriptive Evidence

We start by comparing the in-group/out-group behavior of second-generation immigrant children born pre- and post-policy. Figure 3 presents estimates of Equation 1.

Let us first consider the results of the regression run for boys and girls together (labeled "All" on the x-axis). Among second-generation immigrant children born pre-policy, there is a marked gap between in-group and out-group cooperation: investments to immigrants (€3.00) exceed investments to natives (€2.59) by €0.41 or 16%. By contrast, among second-generation immigrant children born post-policy, this gap is €0.12 or roughly one-third lower, although this estimate is not statistically different from zero at 5% significance level. Behind this result is an interesting gender-specific pattern. Let us first consider the behavior of immigrant boys: Among pre-reform immigrant boys, investments to immigrants (€3.15) exceed investments to natives (€2.75) by €0.40 or 15%. By contrast, among post-reform immigrant boys, the in-group/out-group investment gap is a statistically significant 68%, or €0.27 lower, i.e., immigrant boys born after the introduction of birthright citizenship are almost equally inclined to invest toward immigrants and natives. Turning to the behavior of immigrant girls, it is interesting to observe that the birth date cut-off appears not to matter at all: among pre-reform immigrant girls, investments to immigrants exceed investments to natives by €0.42, or 17%, and this investment gap persists for post-reform immigrant females.

[21] Ideally, we would like to further restrict our sample to eligible second-generation immigrant children, i.e., those whose parents fulfilled the residency criterion of eight years when they were born. However, data limitations prevent us from doing so. In particular, although our survey contains a question on parents' length of residence in Germany, a sizeable number of immigrant children report that they "don't know" their parents' residency duration. Consequently, restricting the sample on the available information on parents' length of residence in Germany would lead to a smaller and likely non-random subsample of immigrant children and would thus provide us with biased and imprecise estimates.

RD-DID Estimates

The main concern with the results presented so far is that they may be confounded by age or season of birth effects. Thus, we now turn to our RD-DID specification in Equation 2. Before delving into regression results, it is important to consider the plausibility of our key identifying assumption. In particular, in analogue to the standard common trends assumption, our strategy requires that age-for-grade effects on children's behavior do not play out differently for immigrants and natives. Thus, in Figure 4, we bin our main outcome variable by months of birth before and after the birthdate cut-off, and plot mean values for immigrant and native youth, respectively. We observe that, among children born pre-policy, the in-group/out-group investment gap is more pronounced among immigrants than among natives. In addition, and importantly for our identification strategy, the figure provides visual evidence of immigrant and native youth underlying common age-for-grade effects, and a treatment effect for immigrant boys (but not girls) that induces a sharp drop in the in-group/out-group investment gap at the birthdate cut-off.

In Table 1, we present estimates for Equation 2, with and without the augmented set of control variables. In each Panel (A-C), the estimated coefficients in the first row ($\hat{\gamma}_1$) capture differences in in-group/out-group behavior between second-generation immigrant children and native children born prior to the policy change. The estimated coefficient of interest is $\hat{\gamma}_3$, which identifies the ITT effect of citizenship at birth on immigrant children's in-group versus out-group investments.

Let us first discuss the results of regressions run for boys and girls together (see Panel A). In Column (1), we only condition on gender and a full set of birth month fixed effects. For children born pre-policy, the in-group/out-group investment gap of immigrants exceeds that of natives by €0.36. The estimate of -0.103 suggests that the introduction of birthright citizenship reduced this gap by approximately 29%, although the estimate is not statistically significant at conventional levels. Columns (2) and (3) show the result to be robust to including controls for family background and classroom characteristics, respectively.

The remaining two panels of Table 1 break down the estimates by gender. Panel B presents the results for boys. Throughout all specifications, the reform effect turns out negative, is large in magnitude and is statistically significant at the 5% level. In our preferred specification [Column (3)], the in-group/out-group investment gap among immigrant boys born pre-policy exceeds that of native boys by €0.28, but the introduction of *jus soli* reduced this difference by €0.26, or 93%. In Panel C, where we repeat the analysis for immigrant girls, we find confirmation for the descriptive evidence presented above: the reduction in immigrants' in-group favoritism due to *jus soli* is an entirely male phenomenon. Irrespective of the specification, the estimated coefficients for immigrant girls are small in magnitude—both in absolute terms and relative to estimates of $\hat{\gamma}_1$—and are statistically indistinguishable from zero.

Overall, we conclude that the introduction of birthright citizenship caused immigrant boys to almost completely drop their in-group favoritism. However, it had no measurable impact on the behavior of immigrant girls.

Heterogeneity

The introduction of *jus soli* did not affect all second-generation immigrant children in the same way. The stand-out group were children with a Turkish background, for whom the reform had strong bite and therefore represents a particularly effective source of exogenous variation. Thus, we expect an above-average reform effect among the children of this immigrant group. Hence, we now restrict the treatment group to all second-generation immigrant children with a Turkish background and re-estimate our RD-DID specification. For completeness, we also provide estimates for the complement case where the treatment group is made up of all second-generation immigrant children with a non-Turkish background. Table 2 presents the results, with and without the augmented set of controls.

Columns (1) to (3) confirm the intuition that the reform's effect should be particularly pronounced among children with a Turkish background. In regressions run for boys and girls together [Panel A], the estimate of -0.254 from our preferred specification in Column (3) suggests that the policy reduced the pre-reform difference between immigrants' and natives' in-group/out-group investment gap (=€ 0.497) by 51%. Moreover, in all three specifications of Panel A, the reform effect is precisely estimated and differs from zero at the 5% significance level. The estimates by gender [Panel B and C] confirm that the reform effect is an entirely male phenomenon. For boys, the RD-DID estimates from our preferred specification [Column (3) of Panel B] show a pre-reform difference between immigrants' and natives' investment bias of € 0.46 and a policy-induced reduction thereof of € 0.57. This suggests that the reform induced Turkish boys to treat in- and out-groupers less unequally than their native peers. By contrast, the estimated coefficients for immigrant girls are small in magnitude and are statistically indistinguishable from zero.

In Columns (4) to (6), we restrict the treatment group to all second-generation immigrant children with a non-Turkish background.[22] Compared to their counterparts with a Turkish background, this group of children were, on average, much less affected by the introduction of *jus soli*. Specifically, for this group, the variation in birthright citizenship across the birthdate cut-off was only one-third of that for children with a Turkish background (see Figure 2). Our findings show (both for regressions run for boys and girls together, as well as those run separately by gender) that the reform effect is small and statistically indistinguishable from zero.

IV.B. Robustness of the Results

We now test the robustness of our main findings, both in the full sample (FS) and the Turkish subsample (TS). All sensitivity checks, which are reported in Table 3, are conducted for our preferred specification [see Column (3) in Tables 1 and 2, respectively].

For our first robustness check, we recalculate our main dependent variable (IG) by letting $S_N = \frac{1}{2}(S_1 + S_2)$ and $S_I = \frac{1}{2}(S_3 + S_4)$ (see our discussion in footnote 11). Columns (FS.1) and (TS.1) in Table 3 show that the estimates based on this alternative outcome measure remain qualitatively unchanged compared to the benchmark results in Table 1.

In the next robustness check, we use a dichotomous outcome measure as dependent variable [see Columns (FS.2) and (TS.2)]. In particular, we classify participants as *strong discriminators* if their in-group investment exceeded their out-group investment by 25% or more. The results suggest that the citizenship reform substantially reduced the share of strong discriminators among immigrant boys, especially in the Turkish subsample. For example, in Column (TS.2) of Panel B, we observe that Turkish boys born pre-policy are 20.4 percentage points more likely to be strong discriminators than their native

[22] Small sample sizes prevent us from splitting the subsample of non-Turkish immigrants into further subgroups.

counterparts, but the statistically significant reform effect of -20.8 percentage points eliminates this difference completely. By contrast, both in the full sample and in the Turkish subsample, immigrant girls' propensity to (strongly) discriminate between in- and out-groupers was unaffected by the introduction of *jus soli*.

In our analysis, standard errors are clustered by school type and school location, and there are a total of 18 clusters. Since reliable inference is a concern when there are few clusters (Donald and Lang [2007]; Cameron *et al.* [2008]), our third robustness check tests whether the results also hold using wilder cluster bootstrap t-procedures.[23] The estimates in Columns (FS.3) and (TS.3) show that the p-values obtained from this bootstrap procedure confirm the levels of statistical significance reported in Table 1.

The citizenship reform we study was ratified and announced in July 1999 but was already openly discussed in the German parliament during the previous month. Thus, our fourth robustness check provides estimations with a narrower $\pm$3-month-window around January 1, 2000. This additional restriction reduces the sample size by approximately 25%. Notwithstanding this, the results in Columns (FS.4) and (TS.4) show that the coefficients remain largely unchanged compared to the benchmark estimates in Table 1, although they are less precisely estimated.

Our fifth and final robustness check involves placebo reform regressions. In particular, we shift the introduction of *jus soli* backward in time, assuming that it took effect on November 1, 1999. Moreover, we exclude all children born on or after January 1, 2000, from our placebo sample. The results in Columns (FS.5) and (TS.5) show that the coefficients on the DiD interaction term are close to zero (or even positive) and statistically indistinguishable from zero. We conclude that the immigrant-native difference in intra- versus inter-group cooperation did not converge already among children born pre-policy.

IV.C. Possible Explanations for the Reform Effect

In this section, we seek to provide possible explanations for the effect of birthright citizenship and question why it is gender-specific. In so doing, we present results for both the full estimation sample and the Turkish subsample.

Educational Integration

The main mechanism we have in mind is about opportunity: citizenship rights improve immigrants' long-term economic perspective in the host country and may therefore be a catalyst for human capital investments in immigrant families. In settings where immigrant children are lagging behind their native peers educationally, this conceivably reduces the social distance between immigrants and natives, with possible spill-overs into the domain of inter-group cooperation.

In previous research based on administrative data, a subset of us (Felfe *et al.* [forthcoming]) has shown that the introduction of *jus soli* in Germany has led to a near-closure of the immigrant-native gap in a series of educational outcomes measured over children's early life cycle (e.g., preschool enrolment, school readiness, grade retention in primary school, tracking into secondary school). We now extend these findings in two directions: we (i) exploit information on school grades from our own survey, and (ii)

[23] We estimated the wild cluster bootstrap standard errors using 1000 replications under H_1, as discussed in Cameron *et al.* [2008].

conduct an analysis by gender.

Our main outcome measure captures children's school performance. In the German school system, the grades vary discretely from 1 (excellent) to 6 (insufficient), with grades below 3 being considered achievements that exceed average requirements. We calculate a grade point average (GPA) based on participants' self-reported grades in the subjects "German" and "Mathematics". Thus, we create the indicator *Above-Average GPA*, which equals one if a participant's average test score is better than 3, and zero otherwise.

In Table 4, we re-run Equation (2) but with the indicator *Above-Average GPA* as dependent variable. Let us consider the estimates from our preferred specifications [Column (3) for the full sample, Column (6) for the Turkish subsample]. The key message one may extract from Panel A, in which all regressions are run for boys and girls together, is that the citizenship reform substantially reduced the immigrant-native gap in school performance. For example, in the full sample [Column (3)], immigrant children born pre-policy are 9.9 percentage points less likely than their native peers (at an average of 41%) to achieve above-average grades. The statistically significant estimate of 8.9 percentage points suggests that the policy reduced this achievement gap by nearly 90%. Once we restrict our attention to the high-eligibility treatment group of immigrants with a Turkish background [Column (6)], this effect becomes more pronounced, though somewhat less precisely estimated. In Panels B and C, in which the analysis is broken down by gender, it is evident that the reform effect is almost entirely driven by male immigrants. For example, in Column (6) of Panel B, we see that Turkish boys born pre-policy are 27.3 percentage points less likely than native boys (at an average of 40%) to achieve above-average grades, but the statistically significant (at the 1% level) estimate of 24 percentage points implies a reduction of this gap by 88%. For immigrant girls (see Panel C), we obtain a different picture: the pre-reform immigrant-native achievement gap ($\hat{\gamma}_1$) among girls is much lower and insignificant, both in the full sample (3.6 percentage points less than the average of 42% among native girls) and in the Turkish subsample (-4.4 percentage points). Loosely put, this implies that immigrant girls had much less to catch up on (educationally) than immigrant boys. Moreover, the coefficients on the RD-DID interaction are, though positive, statistically indistinguishable from zero.

We have also investigated an outcome variable intended to capture parents' involvement in their children's learning. Specifically, we have created the indicator *Parental Involvement*, which equals one if a participant reports receiving parental support in homework and is zero for those who do not obtain such support. Appendix Table A.3 presents the results. Overall, we find that the introduction of *jus soli* led immigrant boys, but not immigrant girls, to receive substantially more learning support from their parents. For example, based on the full sample, we find that immigrant boys born pre-policy are 33.8 percentage points less likely than their native peers (at an average of 77%) to have parents who support their learning, but for those born post-policy, the gap is a statistically significant 21.3 percentage points, or 63% smaller. As before, this effect is more pronounced in the Turkish subsample. Thus, the reform appears to have induced parents of immigrant boys to provide them with similar support as boys in native families.

Other potential channels

Host Country Identification. The introduction of birthright citizenship could have changed immigrant children's sense of identification with Germany, which may also affect their behavior towards natives. To explore this possibility, we exploit the following question in our survey: How much do you feel like a German (Very much, rather much, in some sense, not much, not at all)? Our outcome measure *Identification with Germany* equals one for participants who choose the answer categories "very much" or "rather much", and is zero otherwise. We re-run Equation (2) but with the indicator *Identification with Germany* as the dependent variable. Appendix Table A.4 reports the results, which can be summarized in brief: second-generation immigrant children born post-policy are *not* more likely to self-identify with Germany than those born pre-policy. This result holds both in the full sample and in the Turkish subsample, and it applies equally to male and female immigrants. We therefore consider a direct social identity change as an unlikely channel for the reform effect on in-group versus out-group behavior. We also observe that immigrant girls show a weaker sense of host country identification than immigrant boys, a point we shall return to at the end of the section.

Differential Treatment by Natives. The increase in out-group cooperation due to *jus soli* could be interpreted as a rational response by immigrant children if their native peers treat them differentially based on their citizenship status. Recall that in our design, opponent types $k \in \{3, 4\}$ refer to immigrants as a whole (i.e., boys and girls with foreign parents), while opponent types $k \in \{5, 6\}$ refer to the subset of naturalized immigrants (i.e., naturalized boys and girls with foreign parents). In Appendix Figure A.1, we analyze whether this distinction matters for the behavior of native children. In brief, the evidence suggests that native children do not systematically treat immigrants differently based on their citizenship status. For example, in Panel (a), in which we illustrate the investment choices of native children as first-movers, we observe a small bias in favor of naturalized immigrants, but the investment gap to immigrants as a whole hardly exceeds 1%. In Panel (b), where we look at the back transfers of native children as second-movers, the citizenship status of immigrant children appears not to matter at all. Finally, no gender-specific patterns can be observed. Thus, based on these findings, we conclude that differential treatment by natives is unlikely to be a main channel for the reform effect and its gender-specific nature.

Immigrant Girls: What Explains Their Low Out-Group Cooperation and the Lack of a Reform Effect?

Let us interpret the findings to this point. For immigrant boys, there is a strong relationship between the educational gap separating them from their native peers and in-group/out-group cooperation: Before the citizenship reform took effect, they were lagging behind their native peers educationally and strongly disfavored them, compared to other immigrants, in the investment game. The introduction of *jus soli*, in turn, saw a substantial reduction in in-group favoritism among immigrant boys together with a near-closure of the achievement gap between them and their native peers. Thus, one interpretation is that the immigrant-native gap in education is an important factor in immigrant boys' in-group/out-group behavior and a likely channel for changes thereof in the aftermath of the citizenship reform.

This explanation, however, leaves us with a puzzle regarding immigrant girls' decisions to cooperate. In particular, in stark contrast to immigrant boys, immigrant girls born pre-policy were not lagging much behind their native peers educationally. Despite this, we observe a strong degree of in-group favoritism among them. Moreover, the introduction of *jus soli* had no discernible effect on immigrant girls' in-group/out-group behavior, nor did it foster their educational integration. Overall, this suggests that education and/or educational integration does not exert the same influence on immigrant girls as on immigrant boys. This raises an important question: What explains immigrant girls' low out-group cooperation and the lack of an effect of increased opportunities?

Although our study was not set up to address this (unanticipated by us) result in a causal way, we take some first steps to explore it. Our hypothesis is that cultural factors, ones that were unaffected by the reform, play an important role in immigrant girls' in-group/out-group behavior. A consistent finding in many psychological and sociological studies of immigrant families is that parents adopt different socialization strategies for their daughters and their sons (for an insightful review, see Suárez-Orozco and Qin [2006]). In particular, across many immigrant groups, girls are socialized to be "keepers of the culture" and often face strict parental restrictions on extra-household activities that boys are free to choose (e.g., spending time with friends, attending parties, participating in after-school programs). This double standard in parental control has been found to be particularly strong when immigrant parents perceive the host society as posing a threat to the values of their native culture (Dion and Dion [2001]).

The existing literature suggests two implications of this gender-specific socialization pattern that may be relevant in our context. First, it shapes the process of ethnic self-identification. For example, in the United States, immigrant girls are more likely than immigrant boys to ethnically self-identify with their parents' immigrant origins. Immigrant boys, by contrast, are more likely to choose a national identity (Qin-Hilliard [2003]; Yip and Fuligni [2002]), potentially independently from their parents. This, in turn, may influence the extent to which immigrant boys and girls differentiate between in-group and out-group children. Second, due to the double standard in parental control, immigrant girls are more likely than immigrant boys to face a high level of parent-child conflict and, consequently, to have lower self-esteem (Rumbaut [1994]). According to the "self-esteem hypothesis" in the psychological theory of social identity (Hogg and Abrams [1990]; Rubin and Hewstone [1998]), immigrant girls might restore a more positive self-concept through out-group discrimination.

Our survey allows us to descriptively examine whether there are gender differences in immigrants' sense of identification with the host country and their self-esteem. Appendix Figure A.2 presents the results. In Panel (a), we plot the proportions of second-generation immigrants reporting that they identify with Germany, separately for girls and boys.[24] In the full sample, we detect no noticeable gender differences: roughly one-third of all immigrant boys and girls self-identify with their host nation. However, once we restrict our attention to Turkish immigrants, we observe that roughly 30 percent of all Turkish boys self-identify with Germany, while the corresponding share among immigrant girls is one-third lower (statistically significant at the 5% level). This finding ties in well with the above discussion: Turkish immigrants are predominantly Muslim, and evidence suggests that their identification with the Islamic culture is strong—not just in Germany but also in other major European destinations of Turkish migration, such as France and the Netherlands (Ersanilli and Koopmans [2009]). As argued above, such an environment typically reinforces differential socialization pressures on immigrant girls and boys.

In Panel (b), we plot gender-specific proportions of second-generation immigrants reporting a low level

[24] Here, we once more exploit the variable *Identification with Germany*, which we have described above.

of self-esteem.[25] In both the full sample and the Turkish subsample, roughly one-tenth of all immigrant boys report a low level of self-esteem, while the corresponding share among immigrant girls is twice as a large. Unconditional OLS regressions show the gender differences to be statistically significant at the 1% level. Moreover, the results hold for several alternative measures of self-esteem.[26] Taken as whole, we view this as suggestive evidence that immigrant boys show better psychological adaption than immigrant girls, a finding also reached in a recent cross-country study of offspring of Turkish and Vietnamese immigrants (Berry *et al.* [2006]). To the extent that low self-esteem can reinforce negativity towards out-groups, we also view it as a possible explanation for the strong and persistent in-group favoritism shown by immigrant girls.

We consider this evidence as suggestive but in no way conclusive. There are other plausible explanations for immigrant girls' low out-group cooperation. Perhaps most importantly, we cannot rule out the possibility that perceived discrimination by native peers is a salient factor in immigrant girls' out-group behavior. This seems important, not least because immigrant girls and immigrant boys often differ in external markers (e.g., wearing of headscarves) that may give rise to subtle and difficult-to-measure forms of discrimination. We plan to address these issues in future research.

V. Conclusions

Immigration has put many developed countries on a new demographic path. Immigrant children, in particular, make up a large and growing proportion of youth populations around the Western world. As a result, many scholars and policy makers argue that success in integrating immigrant children will be a crucial nation-building tool for years to come. Our starting point was the recognition that integration has several relevant dimensions. In particular, if we are to get a glimpse of the future face of Western societies, it is not just important to understand how today's immigrant children fare in the education system—we also need to know how children with and without migration backgrounds interact and whether integration policies can help overcome in-group/out-group phenomena and bring about cooperation between individuals with diverse backgrounds and perspectives. We have addressed this issue by combining a natural experiment—the introduction of birthright citizenship in Germany—with a lab-in-the-field experiment based on the investment game with more than 4,000 adolescents in their final year of compulsory schooling.

We find evidence for an asymmetric pattern of cooperation among Germany's youth: native children are in almost equal measure willing to cooperate with native and immigrant interaction partners. Immigrant children, by contrast, have a high propensity to cooperate with other immigrants but a low willingness to cooperate with their native peers. This suggests a need to reconsider some of the conventional wisdom about migrant integration. Discrimination *against* immigrants is an issue that figures prominently in many debates. Although we are not questioning the importance of this issue, it largely ignores the cleavages we have identified, i.e., that immigrants and natives may not be able to resolve social dilemmas because immigrants' cooperative decision-making is in-group bounded.

[25] In our survey, we asked participants: To what extent does the following statement apply to you [on a discrete scale from 1 (does not apply at all) to 6 (applies completely)]: "I have a positive attitude towards myself". Our outcome measure *Low Self-Esteem* equals one for participants who place themselves in the bottom half of the six-point scale, and zero otherwise.

[26] The findings for these alternative self-esteem measures are available upon request.

In connecting the experiment to the introduction of birthright citizenship, we have shown that these cleavages are not carved in stone. In particular, immigrant boys who, through the reform, received the same legal status as their native counterparts and as a result have caught up with them in terms of educational achievement appear to have extended their willingness to cooperate to their native peers. Thus, an important conclusion of our results is that governments can modify and nurture prosocial behavior, in our case resulting in more cooperation between immigrant males and native youth and, consequently, higher levels of efficiency in the interaction between social groups.

That said, the results also point to an important open challenge for policy makers: we have found that immigrant girls strongly discriminate in favor of immigrants and against natives in the investment game; yet, the positive reform effects we have uncovered—both in terms of out-group cooperation and education—are an entirely male phenomenon, i.e., the reform appears to have done little for the social integration of immigrant girls. This suggests that integration interventions are unlikely to offer "one fits all" solutions; those targeted at immigrant children may need to be gender-specific and take into account cultural factors and different socialization pressures that immigrant girls and boys experience in the process of integration.

One issue we have not touched upon so far is whether our findings are generalizable to non-market interactions between immigrants and natives outside our laboratory setting. One interesting testing ground in this respect could be occupational settings. The cohorts born around January 1, 2000, will soon enter the labor market, where they will very likely encounter multicultural work environments. In such environments, immigrants and natives will need to cooperate, *inter alia*, as employees. With a suitably designed study, it would be feasible to analyze the scope for workplace cooperation between immigrants and natives and the long-run impact of birthright citizenship along this dimension. Thus, an important agenda for future research remains. Finally, our evidence comes from a single country of immigration, albeit the world's second-largest, and we prefer to avoid conjectures about external validity in this respect. Nonetheless, we believe that the results in this paper are useful for thinking about how widening the opportunities for disadvantaged groups may crowd-in social behavior that benefits society as a whole.

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Figures and Tables

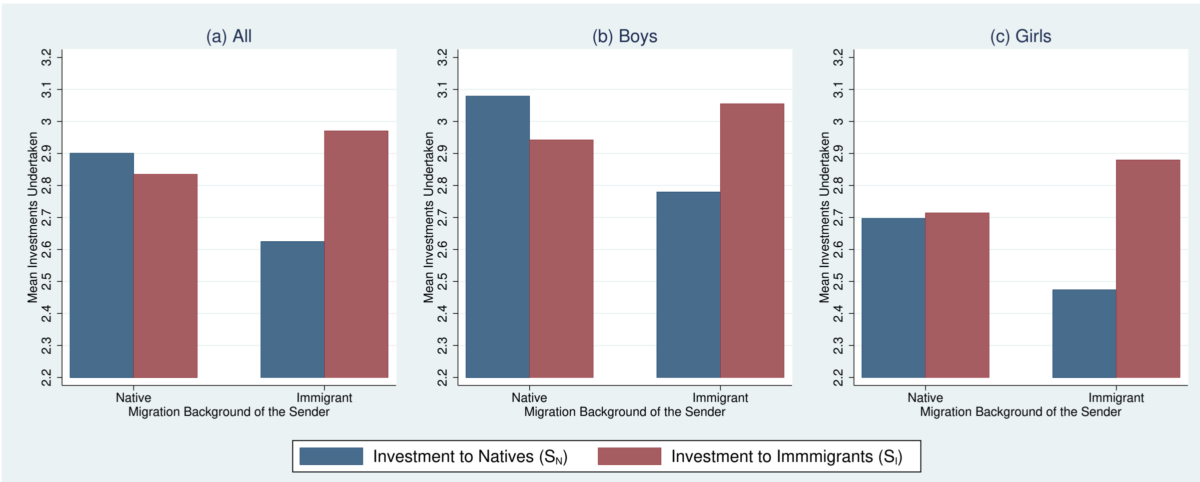

Figure 1: *First-Mover Investment Decisions of Native and Immigrant Children by Migration Background of Second-Movers*

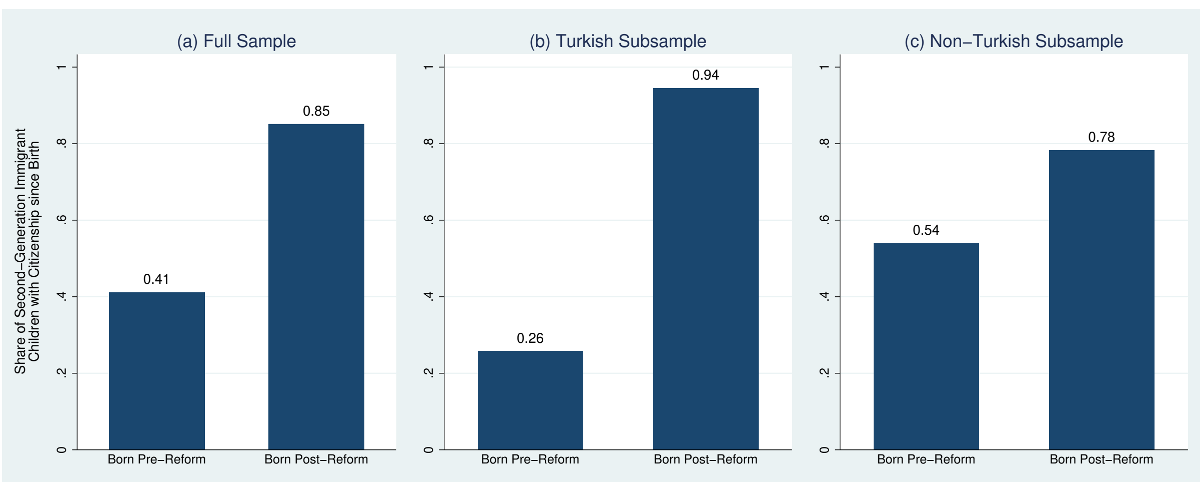

Figure 2: *Share of Second-Generation Immigrants with German Citizenship since Birth: A Comparison of Children Born Pre- and Post-Policy.*

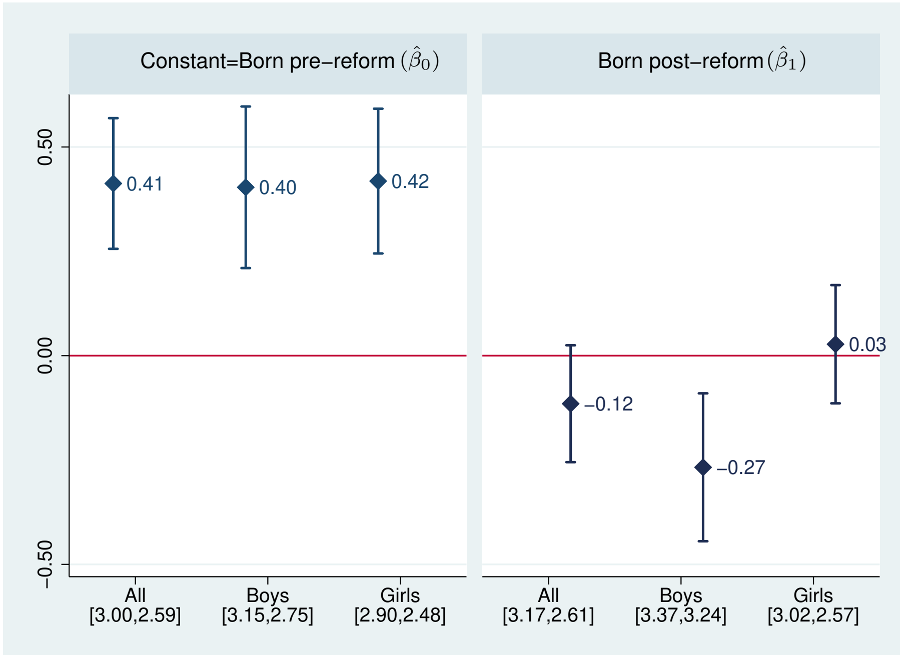

FIGURE 3: *In-Group/Out-Group Investment Gap Among Immigrants Born Around January 1, 2000*

Notes: OLS estimates of Equation (1). Sample comprises all immigrant children born between September 1999 and April 2000. ±2-week donut around the cut-off. Standard errors clustered by school type and school location. In square brackets, we report mean investments to immigrants (first entry) and mean investments to natives (second entry). Whiskers indicate the 95% confidence interval.

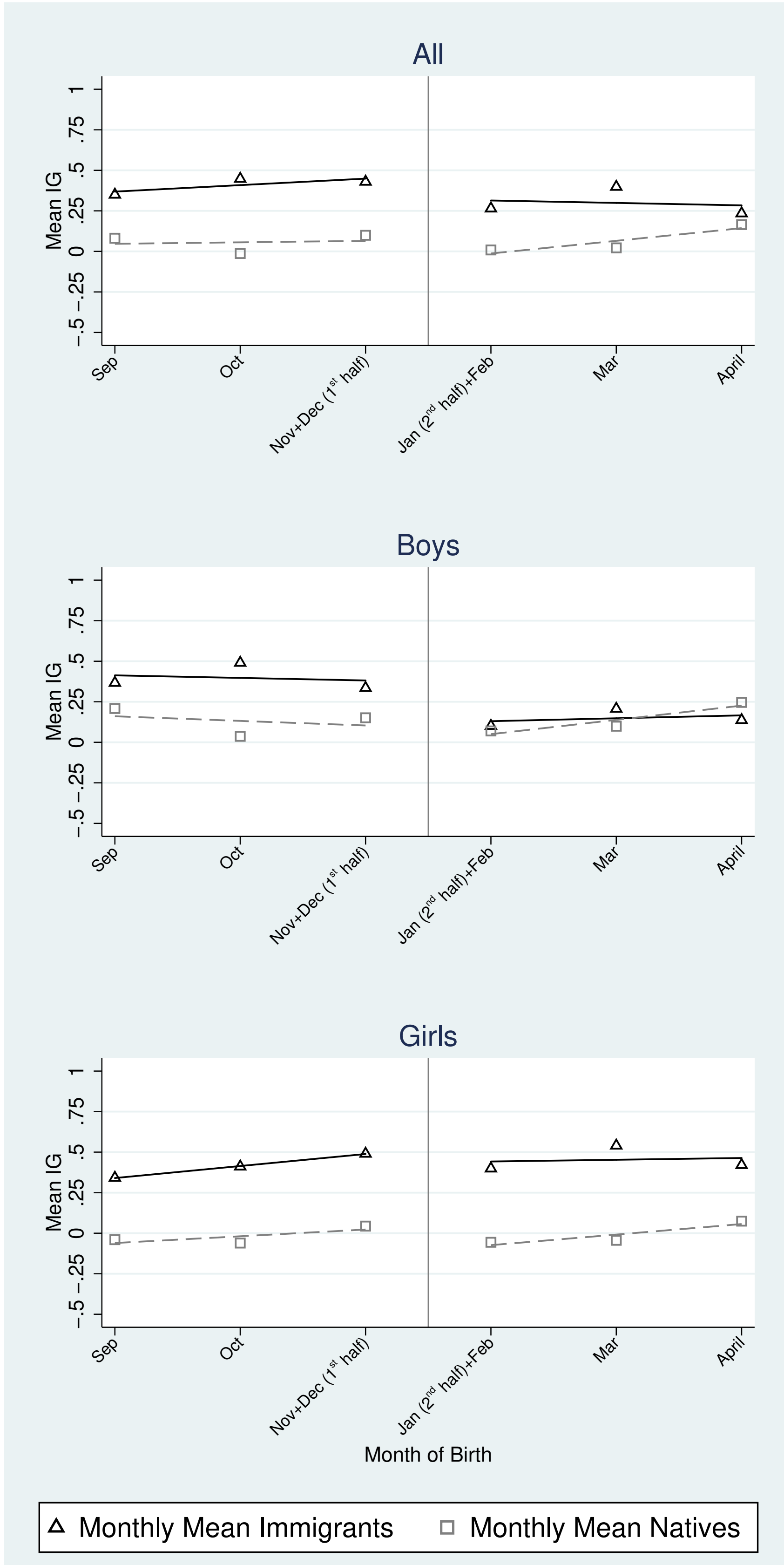

Figure 4: *In-Group/Out-Group Investment Gap (IG) Among Immigrant and Native Youth, Binned by Month of Birth Before and After January 1, 2000*

Table 1

Behavioral Effects of Birthright Citizenship, RD-DID Analysis

Dependent Variable: In-Group/Out-Group Investment Gap (IG)			
	(1)	(2)	(3)
Panel A: All			
Immigrant ($\hat{\gamma}_1$)	0.360***	0.343***	0.337***
	(0.084)	(0.067)	(0.054)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	-0.103	-0.099	-0.106
	(0.078)	(0.073)	(0.071)
Observations	1,280	1,280	1,280
R-squared	0.038	0.048	0.056
Panel B: Boys			
Immigrant ($\hat{\gamma}_1$)	0.278**	0.314***	0.281**
	(0.109)	(0.100)	(0.112)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	-0.261**	-0.284**	-0.260**
	(0.106)	(0.106)	(0.094)
Observations	618	618	618
R-squared	0.017	0.049	0.062
Panel C: Girls			
Immigrant ($\hat{\gamma}_1$)	0.434***	0.427***	0.418***
	(0.083)	(0.085)	(0.066)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	0.045	0.037	0.034
	(0.085)	(0.095)	(0.090)
Observations	662	662	662
R-squared	0.086	0.107	0.118
Month of Birth FE	Yes	Yes	Yes
Family Characteristics		Yes	Yes
Class Characteristics			Yes

Notes: OLS estimates of Equation (2). Standard errors clustered by school type and school location and reported in parentheses; p-values reported in square brackets. All specifications in Panel A control for gender. *Family characteristics* include mother's age, dummy variables for mother's education (eight groups) and dummy variables for family structure (five groups). *Class characteristics* include class size, proportion of students with migration background, the gender ratio and five victimization measures (i.e., the proportion of students who report having been victims in the past year of physical abuse, verbal abuse, lies, theft or exclusion). *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table 2

Behavioral Effects of Birthright Citizenship, RD-DID Analysis for the Turkish and Non-Turkish Subsample

Dependent Variable: In-Group/Out-Group Investment Gap (IG)	Turkish Subsample			Non-Turkish Subsample		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All						
Immigrant ($\hat{\gamma}_1$)	0.505***	0.494***	0.497***	0.242***	0.252***	0.250***
	(0.154)	(0.138)	(0.126)	(0.076)	(0.069)	(0.064)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	-0.245**	-0.244**	-0.254**	0.013	0.010	0.007
	(0.085)	(0.086)	(0.089)	(0.111)	(0.107)	(0.105)
Observations	1,078	1,078	1,078	1,122	1,122	1,122
R-squared	0.045	0.054	0.060	0.029	0.045	0.056
Panel B: Boys						
Immigrant ($\hat{\gamma}_1$)	0.402**	0.460***	0.459***	0.162*	0.207**	0.188
	(0.157)	(0.153)	(0.157)	(0.091)	(0.091)	(0.112)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	-0.558***	-0.571***	-0.565***	-0.049	-0.116	-0.083
	(0.157)	(0.146)	(0.150)	(0.138)	(0.138)	(0.140)
Observations	529	529	529	550	550	550
R-squared	0.026	0.056	0.070	0.020	0.065	0.083
Panel C: Girls						
Immigrant ($\hat{\gamma}_1$)	0.589***	0.610***	0.586***	0.307**	0.328***	0.323**
	(0.176)	(0.176)	(0.156)	(0.117)	(0.111)	(0.115)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	0.013	-0.060	-0.060	0.069	0.094	0.080
	(0.143)	(0.170)	(0.172)	(0.104)	(0.099)	(0.094)
Observations	549	549	549	572	572	572
R-squared	0.104	0.134	0.142	0.050	0.066	0.089
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Family Characteristics		Yes	Yes		Yes	Yes
Class Characteristics			Yes			Yes

Notes: OLS estimates of Equation (2). Standard errors clustered by school type and school location and reported in parentheses; p-values reported in square brackets. All specifications in Panel A control for gender. *Family characteristics* include mother's age, dummy variables for mother's education (eight groups) and dummy variables for family structure (five groups). *Class characteristics* include class size, proportion of students with migration background, the gender ratio and five victimization measures (i.e., the proportion of students who report having been victims in the past year of physical abuse, verbal abuse, lies, theft or exclusion). *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table 3

Behavioral Effects of Birthright Citizenship, RD-DID Robustness Checks for the Full Sample and the Turkish Subsample

Dependent Variable: In-Group/Out-Group Investment Gap (IG)	Alternative IG		Strong discriminator		t-wild cluster		3-month window		Placebo reform	
	(FS.1)	(TS.1)	(FS.2)	(TS.2)	(FS.3)	(TS.3)	(FS.4)	(TS.4)	(FS.5)	(TS.5)
Panel A: All										
Immigrant ($\hat{\gamma}_1$)	0.300***	0.459***	0.135***	0.173**	0.337***	0.497***	0.330***	0.490***	0.335**	0.430**
	(0.059)	(0.130)	(0.043)	(0.079)	[0.000]	[0.000]	(0.085)	(0.154)	(0.121)	(0.189)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	-0.112	-0.216**	-0.035	-0.058	-0.106	-0.254**	-0.069	-0.210*	0.048	0.014
	(0.082)	(0.077)	(0.062)	(0.085)	[0.263]	[0.020]	(0.095)	(0.113)	(0.158)	(0.149)
Observations	1,280	1,078	1,280	1,078	1,280	1,078	961	800	557	473
R-squared	0.05	0.05	0.052	0.05	0.06	0.06	0.07	0.07	0.112	0.13
Panel B: Boys										
Immigrant ($\hat{\gamma}_1$)	0.262*	0.381**	0.136*	0.204**	0.281**	0.459**	0.226	0.505**	0.356**	0.420
	(0.130)	(0.172)	(0.069)	(0.089)	[0.040]	[0.020]	(0.146)	(0.238)	(0.161)	(0.257)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	-0.282**	-0.508**	-0.086	-0.208**	-0.260**	-0.565**	-0.214	-0.612**	-0.035	-0.002
	(0.125)	(0.211)	(0.074)	(0.082)	[0.040]	[0.020]	(0.146)	(0.222)	(0.201)	(0.270)
Observations	618	529	618	529	618	529	461	390	265	232
R-squared	0.07	0.07	0.05	0.06	0.06	0.07	0.08	0.10	0.15	0.15
Panel C: Girls										
Immigrant ($\hat{\gamma}_1$)	0.370***	0.575***	0.139***	0.170*	0.418***	0.586***	0.454***	0.547***	0.352**	0.476**
	(0.057)	(0.176)	(0.036)	(0.080)	[0.000]	[0.000]	(0.085)	(0.154)	(0.149)	(0.196)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	0.029	-0.020	0.010	0.038	0.034	-0.056	0.027	0.007	0.114	0.010
	(0.108)	(0.186)	(0.064)	(0.081)	[0.667]	[0.889]	(0.105)	(0.161)	(0.171)	(0.126)
Observations	662	549	662	549	662	549	500	410	292	241
R-squared	0.10	0.13	0.10	0.10	0.12	0.14	0.15	0.16	0.14	0.18
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Class Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS estimates of Equation (2). Columns labeled “FS” (respectively, “TS”) report results based on the full sample (respectively, Turkish subsample). Standard errors clustered by school type and school location are reported in parentheses. For the specifications based on t-wild cluster bootstrap procedures, we report p-values in square brackets. All specifications in Panel A control for gender. *Family characteristics* include mother’s age, dummy variables for mother’s education (eight groups) and dummy variables for family structure (five groups). *Class characteristics* include class size, proportion of students with migration background, the gender ratio and five victimization measures (i.e., the proportion of students who report having been victims in the past year of physical abuse, verbal abuse, lies, theft or exclusion). *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table 4

Educational Effects of Birthright Citizenship, RD-DID Analysis for the Full Sample and the Turkish Subsample

Dependent Variable: Above-Average GPA						
	Full Sample			Turkish Subsample		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All						
Immigrant ($\hat{\gamma}_1$)	-0.057*	-0.058**	-0.099**	-0.101***	-0.079	-0.123**
	(0.029)	(0.024)	(0.040)	(0.032)	(0.049)	(0.054)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	0.074**	0.076**	0.089**	0.115**	0.111*	0.128*
	(0.031)	(0.035)	(0.035)	(0.054)	(0.054)	(0.062)
Observations	1,256	1,256	1,256	1,057	1,057	1,057
R-squared	0.009	0.025	0.045	0.010	0.026	0.044
Panel B: Boys						
Immigrant ($\hat{\gamma}_1$)	-0.136***	-0.148***	-0.199***	-0.188***	-0.208***	-0.273***
	(0.044)	(0.038)	(0.056)	(0.058)	(0.053)	(0.063)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	0.183***	0.184***	0.184***	0.248***	0.244***	0.240***
	(0.050)	(0.058)	(0.060)	(0.046)	(0.045)	(0.052)
Observations	604	604	604	517	517	517
R-squared	0.014	0.035	0.052	0.017	0.036	0.054
Panel C: Girls						
Immigrant ($\hat{\gamma}_1$)	0.002	0.008	-0.036	-0.037	0.003	-0.044
	(0.046)	(0.050)	(0.067)	(0.055)	(0.076)	(0.079)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	-0.016	-0.014	0.014	0.008	0.019	0.056
	(0.052)	(0.054)	(0.048)	(0.108)	(0.106)	(0.106)
Observations	652	652	652	540	540	540
R-squared	0.005	0.029	0.067	0.007	0.035	0.065
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Family Characteristics		Yes	Yes		Yes	Yes
Class Characteristics			Yes			Yes

Notes: OLS estimates of Equation (3). Standard errors clustered by school type and school location and reported in parentheses. All specifications in Panel A control for gender. *Family characteristics* include mother's age, dummy variables for mother's education (eight groups) and dummy variables for family structure (five groups). *Class characteristics* include class size, proportion of students with migration background, the gender ratio and five victimization measures (i.e., the proportion of students who report having been victims in the past year of physical abuse, verbal abuse, lies, theft or exclusion). *** (**) (*) indicates significance at the 1% (5%) (10%) level.

For Online Publication

Appendix for "More Opportunity, More Cooperation? The Behavioral Effects of Birthright Citizenship on Immigrant Youth"

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A. Additional Figures and Tables

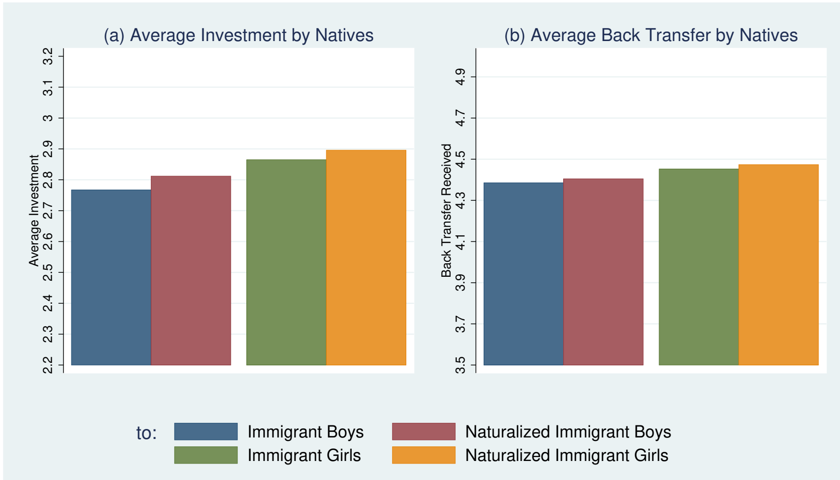

Figure A.1: *First-Mover Investment Decisions and Second-Mover Back Transfers of Native Children by Gender and Citizenship Status of Immigrant Opponents*

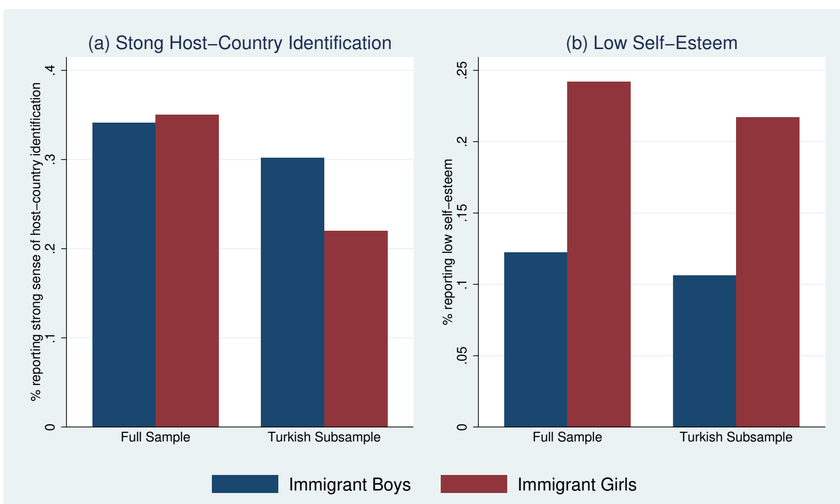

Figure A.2: *Gender Differences in Immigrants' Sense of Host-Country Identification and Self-Esteem*

Table A.1

Descriptive Statistics by Migration Background, Baseline Sample

	Natives	Immigrants
Gender and Family Structure		
Female	0.536	0.500
Lives with both parents	0.547	0.738
Lives with one parent	0.381	0.172
Lives with: other	0.027	0.024
Lives with: missing	0.044	0.066
Mother's Age	46.124	44.102
Mother's Education		
None or Low ("Hauptschule")	0.248	0.324
Intermediate ("Realschule")	0.412	0.173
High ("Abitur") or University	0.206	0.169
Other, Unknown or Missing	0.134	0.334
Religious Affiliation		
Catholic	0.141	0.163
Protestant	0.525	0.085
Islamic	0.018	0.590
None, Other, Missing	0.316	0.162
Language Spoken at Home		
Mostly German	0.976	0.289
Mostly Foreign Language	0.011	0.690
Missing	0.013	0.021
Mother's Country/Region of Birth		
Turkey	/	0.376
Middle East & Africa	/	0.139
Post-Soviet Country	/	0.123
Balkan Country	/	0.112
Eastern Europe	/	0.106
Other Country	/	0.144
Observations	2,201	1,218

Notes: "Natives" are children whose parents are both German-born. "Immigrants" are children whose parents are both foreign-born.

Table A.2

Descriptive Statistics for the Full Estimation Sample

	Natives	Immigrants	Immigrants born pre-reform	Immigrants born post-reform	p-value
			Gender and Family Structure		
Female	.499	.564	.612	.521	.004
Lives with both Parents	.570	.808	.824	.795	.419
Lives with Mother	.321	.103	.100	.105	.796
Lives with Father	.048	.022	.012	.032	.229
Lives with: Other	.014	.022	.029	.016	.324
Lives with: Missing	.048	.044	.035	.053	.368
Mother's Age	46.057	44.542	45.424	43.753	.009
			Mothers' Education		
None	.010	.172	.165	.179	.566
Low	.198	.144	.159	.132	.456
Intermediate	.432	.197	.165	.226	.146
High	.149	.092	.088	.095	.828
University	.077	.050	.059	.042	.426
Other	.005	.025	.041	.011	.148
Unknown	.100	.292	.288	.295	.891
Missing	.029	.028	.035	.021	.359
			Class Characteristics		
Class Size	19.186	17.903	17.924	17.884	.936
Share Immigrants	.337	.569	.562	.574	.695
Share Males	.489	.500	.503	.498	.643
Share Insulted	.724	.702	.696	.708	.272
Share Ignored	.503	.473	.460	.485	.223
Share Hurt	.098	.116	.119	.114	.470
Share Lied	.827	.738	.716	.759	.052
Share Stolen	.217	.242	.239	.245	.632
Observations	920	360	170	190	

Notes: Sample restricted to an 8-month window centered around the reform's cut-off date and excluding a ±2-week window 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-reform" and "Post-reform" refer to "Immigrants" who are either born before (in 1999) or after (in 2000) the reform's cut-off date. P-value refers to the respective differences between the groups.

Table A.3

Effects on Parental Educational Involvement, RD-DID Analysis for the Full Sample and the Turkish Subsample

Dependent Variable: Parental Educational Involvment						
	Full Sample			Turkish Subsample		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All						
Immigrant ($\hat{\gamma}_1$)	-0.228***	-0.202***	-0.194***	-0.198**	-0.144*	-0.147
	(0.043)	(0.050)	(0.053)	(0.070)	(0.080)	(0.086)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	0.029	0.025	0.033	-0.013	0.003	0.007
	(0.070)	(0.069)	(0.069)	(0.120)	(0.121)	(0.122)
Observations	1,262	1,262	1,262	1,060	1,060	1,060
R-squared	0.055	0.101	0.107	0.034	0.077	0.084
Panel B: Boys						
Immigrant ($\hat{\gamma}_1$)	-0.386***	-0.352***	-0.338***	-0.346***	-0.321***	-0.305***
	(0.075)	(0.072)	(0.075)	(0.059)	(0.054)	(0.074)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	0.211**	0.197**	0.213**	0.240**	0.254***	0.245***
	(0.094)	(0.083)	(0.081)	(0.089)	(0.070)	(0.075)
Observations	610	610	610	521	521	521
R-squared	0.081	0.124	0.150	0.041	0.074	0.095
Panel C: Girls						
Immigrant ($\hat{\gamma}_1$)	-0.118**	-0.088	-0.096*	-0.092	-0.016	-0.029
	(0.049)	(0.057)	(0.050)	(0.072)	(0.085)	(0.085)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	-0.106	-0.114	-0.109	-0.224	-0.217	-0.219
	(0.078)	(0.076)	(0.073)	(0.146)	(0.155)	(0.153)
Observations	652	652	652	539	539	539
R-squared	0.055	0.115	0.125	0.057	0.131	0.140
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Family Characteristics		Yes	Yes		Yes	Yes
Class Characteristics			Yes			Yes

Notes: OLS estimates of Equation (2). Standard errors clustered by school type and school location and reported in parentheses; p-values reported in square brackets. All specifications in Panel A control for gender. *Family characteristics* include mothers' age, dummy variables for mothers' education (eight groups) and dummy variables for family structure (five groups) . *Class characteristics* include class size, proportion of students with migration background, the gender ratio and five victimization measures (i.e., the proportion of students who report having been victims in the past year of physical abuse, verbal abuse, lies, theft or exclusion). *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table A.4

Effects on Identification with Germany, RD-DID Analysis for the Full Sample and the Turkish Subsample

Dependent Variable: Identification with Germany						
	Full Sample			Turkish Subsample		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All						
Immigrant ($\hat{\gamma}_1$)	-0.492***	-0.472***	-0.451***	-0.570***	-0.544***	-0.538***
	(0.042)	(0.044)	(0.040)	(0.063)	(0.057)	(0.063)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	-0.0309	-0.0288	-0.0256	-0.0287	-0.0169	-0.0199
	(0.0578)	(0.0597)	(0.0562)	(0.0713)	(0.0738)	(0.0707)
Observations	1,269	1,269	1,269	1,067	1,067	1,067
R-squared	0.271	0.283	0.292	0.267	0.278	0.285
Panel B: Boys						
Immigrant ($\hat{\gamma}_1$)	-0.461***	-0.444***	-0.422***	-0.543***	-0.518***	-0.506***
	(0.070)	(0.070)	(0.062)	(0.078)	(0.085)	(0.086)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	-0.074	-0.069	-0.048	0.015	0.035	0.042
	(0.095)	(0.096)	(0.096)	(0.102)	(0.108)	(0.102)
Observations	612	612	612	523	523	523
R-squared	0.275	0.298	0.318	0.225	0.252	0.275
Panel C: Girls						
Immigrant ($\hat{\gamma}_1$)	-0.508***	-0.472***	-0.452***	-0.593***	-0.550***	-0.550***
	(0.050)	(0.052)	(0.053)	(0.067)	(0.068)	(0.066)
Born post-reform*Immigrant ($\hat{\gamma}_3$)	-0.010	-0.011	-0.024	-0.069	-0.078	-0.084
	(0.082)	(0.085)	(0.081)	(0.072)	(0.082)	(0.075)
Observations	657	657	657	544	544	544
R-squared	0.290	0.313	0.320	0.325	0.346	0.352
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Family Characteristics		Yes	Yes		Yes	Yes
Class Characteristics			Yes			Yes

Notes: OLS estimates of Equation (2). Standard errors clustered by school type and school location and reported in parentheses; p-values reported in square brackets. All specifications in Panel A control for gender. *Family characteristics* include mother's age, dummy variables for mother's education (eight groups) and dummy variables for family structure (five groups). *Class characteristics* include class size, proportion of students with migration background, the gender ratio and five victimization measures (i.e., the proportion of students who report having been victims in the past year of physical abuse, verbal abuse, lies, theft or exclusion). *** (**) (*) indicates significance at the 1% (5%) (10%) level.

B. Experimental Setup and Design

B.I. Classroom Setup

Figure B.1: *An Example of a Classroom Setup*

B.II. Translation of Instructions

Participation in this game is voluntary!

Thank you very much for participating. From now on, please do not speak with anyone else apart from us about the game. Unfortunately, if you break this rule, we will have to exclude you from the game.

The objective of this game is to examine how people make decisions. There are no "right" or "wrong" decisions in the game and our aim is not to test your knowledge. Make your decisions exactly as you wish. During this game, you will be earning real money. We guarantee that you will receive a cash payout within two weeks. You will receive your money in an envelope marked with your ID number, so please make sure you keep your ID number in a safe place! These envelopes will be passed out by one of your teachers or can be collected from the secretary's office.

The amount of money you earn depends on your decisions and the decisions of the other participants. We will now describe the rules in detail. It is therefore especially important that you listen very carefully.

There are no "right" or "wrong" decisions in this game. You should make your decisions based on your own personal deliberations. Your decisions will remain anonymous, which means that no one else will know what you decide.

If you have any questions after reading these instructions, please raise your hand. Someone will then come over to you and answer your questions in private (i.e., quietly).

Process:

There are two roles in this game: **sender** and **responder**.

The game starts as follows: Each sender and each responder receives 5 euros. The sender must decide how much of the 5 euros he/she wishes to give to the responder.

The amount the sender gives to "his/her" responder will then be tripled. In other words, the responder receives precisely three times the amount the sender has given him/her.

Next, it is the responder's turn. He/she now has three times the amount the sender has given him/her plus his/her own 5 euros. The responder must now decide how much of this money he/she would like to return to "his/her" sender. Please note: The sum the responder returns to the sender is not tripled.

Payment:

At the end of the game, the sender receives the sum that he/she kept plus the sum that the responder returned to him/her.

Payment to sender = 5 euros - sum sent + sum returned (by responder)

The responder receives the sum he/she was given by the sender (times 3), minus the sum he/she returned to the sender.

Payment to responder = 5 euros + 3 x sum sent (by sender) - sum returned

Decisions:

You will be required to make one decision in the role of sender and one in the role of responder. You can also choose between different "categories" of senders and responders; you obviously do not have to treat these groups differently, however. These categories are described on the decision sheet. You can, for instance, choose whether you send or return money to a boy or a girl. It is your decision, there is no "right" or "wrong".

Calculating your payment:

Some of the following points will be easier to understand once you have seen the decision sheets. We will now go through the points and then look at the decision sheets together. If you still have questions after that, we will be happy come back to these points.

Once the game has been carried out in several schools, the following will happen:

1. Two students from different schools will be randomly paired; you will therefore not know "your" sender or "your" responder personally; however, he or she will be around the same age as you and will also go to school in North Rhine-Westphalia.

2. Who is to play the role of the sender and who the role of the responder will also be randomly decided.

3. Next, we identify the category (see decision sheet) that the sender and responder are each from. This information is extracted from the questionnaire you completed. The sender can be a girl and the responder a boy, for instance.

4. Next, the sender's decision is implemented based on the actual category of the responder.

5. Finally, the responder's decision is implemented based on the actual category of the sender and the actual amount received from "their" sender.

6. We now know how much the sender has sent and how much the responder has returned. Based on this, we can calculate the payment to both the sender and the responder. This money is then placed in the appropriate envelopes marked with the corresponding sender and responder ID numbers and taken to the schools.

7. At the end, you will be able to collect the envelope containing your payment at your school.

Now look at the decision sheets. This will help you to better understand some of the points described above. Think carefully about the decisions you wish to make. You have plenty of time! If you have any questions, please raise your hand. Someone will then come over to you and answer your questions in private (i.e., quietly).

ID: □□□□□□

✂ - ✂

Please KEEP your ID!!!!!

ID: □□□□□□

Sender

You are the sender and you have 5 EURO. Which amount would you like to send to the receiver (max. 5 EURO)? Please check one box in each column 1-6.

The receiver is...					
COLUMN 1	**COLUMN 2**	**COLUMN 3**	**COLUMN 4**	**COLUMN 5**	**COLUMN 6**
... a boy with German parents	*... a girl with German parents*	*... a boy with foreign parents*	*... a girl with foreign parents*	*... a boy with foreign parents who possesses German citizenship*	*... a girl with foreign parents who possesses German citizenship*
☐ 0 EURO ☐ 0.5 EURO ☐ 1 EURO ☐ 1.5 EURO ☐ 2 EURO ☐ 2.5 EURO ☐ 3 EURO ☐ 3.5 EURO ☐ 4 EURO ☐ 4.5 EURO ☐ 5 EURO	☐ 0 EURO ☐ 0.5 EURO ☐ 1 EURO ☐ 1.5 EURO ☐ 2 EURO ☐ 2.5 EURO ☐ 3 EURO ☐ 3.5 EURO ☐ 4 EURO ☐ 4.5 EURO ☐ 5 EURO	☐ 0 EURO ☐ 0.5 EURO ☐ 1 EURO ☐ 1.5 EURO ☐ 2 EURO ☐ 2.5 EURO ☐ 3 EURO ☐ 3.5 EURO ☐ 4 EURO ☐ 4.5 EURO ☐ 5 EURO	☐ 0 EURO ☐ 0.5 EURO ☐ 1 EURO ☐ 1.5 EURO ☐ 2 EURO ☐ 2.5 EURO ☐ 3 EURO ☐ 3.5 EURO ☐ 4 EURO ☐ 4.5 EURO ☐ 5 EURO	☐ 0 EURO ☐ 0.5 EURO ☐ 1 EURO ☐ 1.5 EURO ☐ 2 EURO ☐ 2.5 EURO ☐ 3 EURO ☐ 3.5 EURO ☐ 4 EURO ☐ 4.5 EURO ☐ 5 EURO	☐ 0 EURO ☐ 0.5 EURO ☐ 1 EURO ☐ 1.5 EURO ☐ 2 EURO ☐ 2.5 EURO ☐ 3 EURO ☐ 3.5 EURO ☐ 4 EURO ☐ 4.5 EURO ☐ 5 EURO

How much do you think "your" receiver will send back to you? Consider that he or she now has 5 EURO plus three times the amount that you sent (max. 20 EURO). Recall: We tripled the amount you sent. Please fill in columns 1-6 and use one decimal place at most (10-cent steps).

The receiver is...					
COLUMN 1	**COLUMN 2**	**COLUMN 3**	**COLUMN 4**	**COLUMN 5**	**COLUMN 6**
... a boy with German parents	*... a girl with German parents*	*... a boy with foreign parents*	*... a girl with foreign parents*	*... a boy with foreign parents who possesses German citizenship*	*... a girl with foreign parents who possesses German citizenship*
______ EURO	______ EURO	______ EURO	______ EURO	______ EURO	______ EURO

Receiver 1

You are the receiver. The sender is a <u>boy with German parents.</u> How much do you want to send back to him? Please fill in an amount for each possible case (at most one decimal place = 10-cent steps.)

Assume the sender has sent you the following amount:	*The sender still has:*	*You have:*	*Which amount do you want to send back:*	*Potential amount to send back:*
0 EURO	5 EURO	5 EURO	______ EURO	(0 to 5 EURO)
0.5 EURO	4.5 EURO	6.5 EURO	______ EURO	(0 to 6.5 EURO)
1 EURO	4 EURO	8 EURO	______ EURO	(0 to 8 EURO)
1.5 EURO	3.5 EURO	9.5 EURO	______ EURO	(0 to 9.5 EURO)
2 EURO	3 EURO	11 EURO	______ EURO	(0 to 11 EURO)
2.5 EURO	2.5 EURO	12.5 EURO	______ EURO	(0 to 12.5 EURO)
3 EURO	2 EURO	14 EURO	______ EURO	(0 to 14 EURO)
3.5 EURO	1.5 EURO	15.5 EURO	______ EURO	(0 to 15.5 EURO)
4 EURO	1 EURO	17 EURO	______ EURO	(0 to 17 EURO)
4.5 EURO	0.5 EURO	18.5 EURO	______ EURO	(0 to 18.5 EURO)
5 EURO	0 EURO	20 EURO	______ EURO	(0 to 20 EURO)

Receiver 2

You are the receiver. The sender is a girl with German parents. How much do you want to send back to her? Please fill in an amount for each possible case (at most one decimal place = 10-cent steps.)

Assume the sender has sent you the following amount:	*The sender still has:*	*You have:*	*Which amount do you want to send back:*	*Potential amount to send back:*
0 EURO	5 EURO	5 EURO	______ EURO	(0 to 5 EURO)
0.5 EURO	4.5 EURO	6.5 EURO	______ EURO	(0 to 6.5 EURO)
1 EURO	4 EURO	8 EURO	______ EURO	(0 to 8 EURO)
1.5 EURO	3.5 EURO	9.5 EURO	______ EURO	(0 to 9.5 EURO)
2 EURO	3 EURO	11 EURO	______ EURO	(0 to 11 EURO)
2.5 EURO	2.5 EURO	12.5 EURO	______ EURO	(0 to 12.5 EURO)
3 EURO	2 EURO	14 EURO	______ EURO	(0 to 14 EURO)
3.5 EURO	1.5 EURO	15.5 EURO	______ EURO	(0 to 15.5 EURO)
4 EURO	1 EURO	17 EURO	______ EURO	(0 to 17 EURO)
4.5 EURO	0.5 EURO	18.5 EURO	______ EURO	(0 to 18.5 EURO)
5 EURO	0 EURO	20 EURO	______ EURO	(0 to 20 EURO)

Receiver 3

You are the receiver. The sender is a <u>boy with foreign parents.</u> How much do you want to send back to him? Please fill in an amount for each possible case (at most one decimal place = 10-cent steps.)

Assume the sender has sent you the following amount:	*The sender still has:*	*You have:*	*Which amount do you want to send back:*	*Potential amount to send back:*
0 EURO	5 EURO	5 EURO	______ EURO	(0 to 5 EURO)
0.5 EURO	4.5 EURO	6.5 EURO	______ EURO	(0 to 6.5 EURO)
1 EURO	4 EURO	8 EURO	______ EURO	(0 to 8 EURO)
1.5 EURO	3.5 EURO	9.5 EURO	______ EURO	(0 to 9.5 EURO)
2 EURO	3 EURO	11 EURO	______ EURO	(0 to 11 EURO)
2.5 EURO	2.5 EURO	12.5 EURO	______ EURO	(0 to 12.5 EURO)
3 EURO	2 EURO	14 EURO	______ EURO	(0 to 14 EURO)
3.5 EURO	1.5 EURO	15.5 EURO	______ EURO	(0 to 15.5 EURO)
4 EURO	1 EURO	17 EURO	______ EURO	(0 to 17 EURO)
4.5 EURO	0.5 EURO	18.5 EURO	______ EURO	(0 to 18.5 EURO)
5 EURO	0 EURO	20 EURO	______ EURO	(0 to 20 EURO)

Receiver 4

You are the receiver. The sender is a girl with foreign parents. How much do you want to send back to her? Please fill in an amount for each possible case (at most one decimal place = 10-cent steps.)

Assume the sender has sent you the following amount:	*The sender still has:*	*You have:*	*Which amount do you want to send back:*	*Potential amount to send back:*
0 EURO	5 EURO	5 EURO	______ EURO	(0 to 5 EURO)
0.5 EURO	4.5 EURO	6.5 EURO	______ EURO	(0 to 6.5 EURO)
1 EURO	4 EURO	8 EURO	______ EURO	(0 to 8 EURO)
1.5 EURO	3.5 EURO	9.5 EURO	______ EURO	(0 to 9.5 EURO)
2 EURO	3 EURO	11 EURO	______ EURO	(0 to 11 EURO)
2.5 EURO	2.5 EURO	12.5 EURO	______ EURO	(0 to 12.5 EURO)
3 EURO	2 EURO	14 EURO	______ EURO	(0 to 14 EURO)
3.5 EURO	1.5 EURO	15.5 EURO	______ EURO	(0 to 15.5 EURO)
4 EURO	1 EURO	17 EURO	______ EURO	(0 to 17 EURO)
4.5 EURO	0.5 EURO	18.5 EURO	______ EURO	(0 to 18.5 EURO)
5 EURO	0 EURO	20 EURO	______ EURO	(0 to 20 EURO)

Receiver 5

You are the receiver. The sender is a boy with foreign parents who possesses German citizenship. How much do you want to send back to him? Please fill in an amount for each possible case (at most one decimal place = 10-cent steps.)

Assume the sender has sent you the following amount:	*The sender still has:*	*You have:*	*Which amount do you want to send back:*	*Potential amount to send back:*
0 EURO	5 EURO	5 EURO	______ EURO	(0 to 5 EURO)
0.5 EURO	4.5 EURO	6.5 EURO	______ EURO	(0 to 6.5 EURO)
1 EURO	4 EURO	8 EURO	______ EURO	(0 to 8 EURO)
1.5 EURO	3.5 EURO	9.5 EURO	______ EURO	(0 to 9.5 EURO)
2 EURO	3 EURO	11 EURO	______ EURO	(0 to 11 EURO)
2.5 EURO	2.5 EURO	12.5 EURO	______ EURO	(0 to 12.5 EURO)
3 EURO	2 EURO	14 EURO	______ EURO	(0 to 14 EURO)
3.5 EURO	1.5 EURO	15.5 EURO	______ EURO	(0 to 15.5 EURO)
4 EURO	1 EURO	17 EURO	______ EURO	(0 to 17 EURO)
4.5 EURO	0.5 EURO	18.5 EURO	______ EURO	(0 to 18.5 EURO)
5 EURO	0 EURO	20 EURO	______ EURO	(0 to 20 EURO)

Receiver 6

You are the receiver. The sender is a girl with foreign parents who possesses German citizenship. How much do you want to send back to her? Please fill in an amount for each possible case (at most one decimal place = 10-cent steps.)

Assume the sender has sent you the following amount:	*The sender still has:*	*You have:*	*Which amount do you want to send back:*	*Potential amount to send back:*
0 EURO	5 EURO	5 EURO	_____ EURO	(0 to 5 EURO)
0.5 EURO	4.5 EURO	6.5 EURO	_____ EURO	(0 to 6.5 EURO)
1 EURO	4 EURO	8 EURO	_____ EURO	(0 to 8 EURO)
1.5 EURO	3.5 EURO	9.5 EURO	_____ EURO	(0 to 9.5 EURO)
2 EURO	3 EURO	11 EURO	_____ EURO	(0 to 11 EURO)
2.5 EURO	2.5 EURO	12.5 EURO	_____ EURO	(0 to 12.5 EURO)
3 EURO	2 EURO	14 EURO	_____ EURO	(0 to 14 EURO)
3.5 EURO	1.5 EURO	15.5 EURO	_____ EURO	(0 to 15.5 EURO)
4 EURO	1 EURO	17 EURO	_____ EURO	(0 to 17 EURO)
4.5 EURO	0.5 EURO	18.5 EURO	_____ EURO	(0 to 18.5 EURO)
5 EURO	0 EURO	20 EURO	_____ EURO	(0 to 20 EURO)

C. Detailed Results of the Lab-in-the-Field Experiment

In this Appendix, we present detailed results from our lab-in-the-field experiment. To that end, we exploit our full baseline sample, which contains the experimental choices of 4,077 participants.

C.I. General Investment and Back-Transfer Patterns

We begin with a brief description of general investment and back-transfer patterns. Panel (a) in Figure C.1 shows a histogram of all investment decisions in the experiment. On average, first-movers invest €2.85, or 57% of their initial endowment. The two most frequent investment choices are transfers of respectively 50 and 100% of the initial endowment. These investment patterns are comparable to what has been observed in previous experiments based on the investment game.[1]

Panel (b) in Figure C.1 shows (i) averages of second-mover back transfers for each possible first-mover investment and (ii) average expected back transfers, conditional on the first-mover's investment. It is apparent that second-movers show reciprocal behavior, on average: the higher the first-mover investments, the higher the back transfers. The degree of reciprocity appears to be quite high. For example, second-movers are willing to send back approximately €4.50 if they receive €2.50 from first-movers, which almost equalizes final payments. Over the whole range of investments, the ratio of paybacks to investments estimated from an OLS regression is 1.42. Finally, Panel (b) also reveals that, on average, the level of actual back transfers matches first-movers' expectations about back transfers almost one-to-one, especially for intermediate investments.

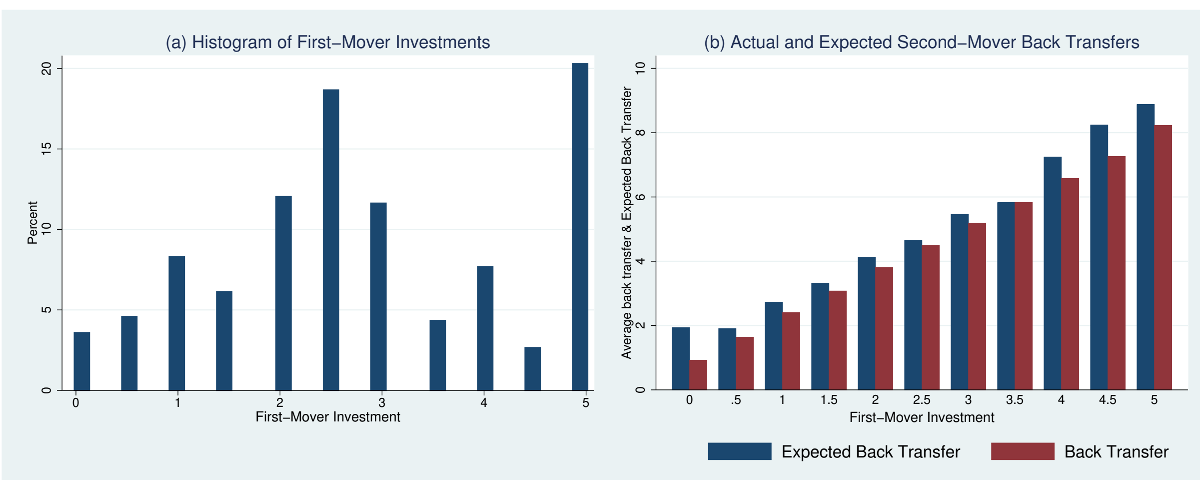

Figure C.1: *First-Mover Investment Decisions, Expected Back Transfers, and Actual Second-Mover Back Transfers*

Notes: Panel (a) shows a histogram of *all* investment decisions in the experiment. Since we used the strategy method to collect the decisions, each subject made six investment decisions, one for each of the possible groups of second-movers. All these decisions are included in the data underlying this figure. Panel (b) shows actual and expected second-mover back transfers. Red bars show averages of second-mover transfers for each possible first-mover investment. Note that each second-mover indicated a transfer decision for each possible first-mover investment. The data underlying this figure thus contain six×eleven decisions per second-mover. Blue bars show means of expected back transfers conditional on own first-mover investments.

[1] For example, the distribution of first-mover investments is quite comparable to that in Falk and Zehnder's [2013] experiment in the city of Zurich, which was run with adults.

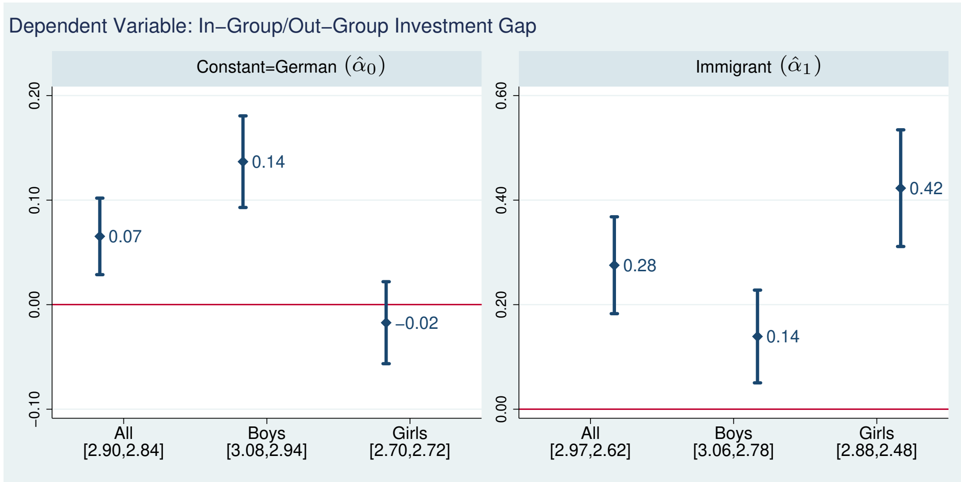

FIGURE C.2: *In-Group/Out-Group Investment Gaps*

Notes: OLS estimates of Equation (1). Standard errors clustered by school type and school location. Whiskers indicate the 95% confidence interval. In square brackets, we report means of average in-group investments (first entry) and average out-group investments (second entry). Sample Sizes: 3,419 (All); 1,789 (Boys); 1,630 (Girls).

C.II. In-Group/Out-Group Investment Gaps

We now analyze whether and how the migration background of interaction partners affects the investment decisions of first-movers. For the main part of the analysis, we restrict our sample to native German children (i.e., both parents German-born) and their immigrant peers (i.e., both parents foreign-born). At the end of this Appendix, we briefly discuss the experimental choices of mixed-background children (i.e., one native and one foreign-born parent).

In a first step, we provide a regression-analogue to Figure 1 in the main text. We do this by running the following simple specification:

$$\text{IG} = \alpha_0 + \alpha_1 \text{Immigrant} + \varepsilon. \tag{C.1}$$

The dependent variable is defined for each first-mover as the difference between average in-group investment and average out-group investment. *Immigrant* is a binary variable indicating whether a child has parents who are both foreign-born; the omitted category is native children, i.e., those whose parents are both German-born. Standard errors are clustered by school type and school location.

Figure C.2 presents OLS estimates of Equation (C.1). In the full sample (labeled "All" on the x-axis), the mean of the in-group/out-group investment gap is €0.07 for native children, while for immigrants, it is a statistically significant €0.28 higher. Among native boys, the in-group/out-group investment gap amounts to €0.14, while for immigrant boys, it is exactly twice as high, with the difference being significant at the 1% level. For native girls, the in-group/out-group investment gap is both quantitatively and statistically indistinguishable from zero (€-0.02), while for immigrant girls it is a statistically significant €0.42 higher.

In Figure C.3, we look at investment decisions in terms of propensities to strongly discriminate between in-group and out-group opponents. To do so, we classify participants as strong discriminators if their average in-group investments exceed their average out-group investments by 25% or more. The results

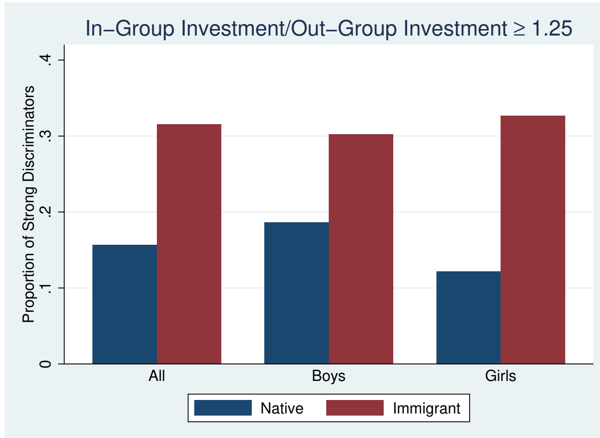

FIGURE C.3: *Proportions of (Strong) Discriminators*

are quite striking. It can been seen that roughly one-in-three immigrant children (31.4%) are strong discriminators, while the corresponding share among native children is only half that (15.6%). This pattern is more pronounced among girls (32.7 v/s 12.1%) than among boys (30.2 v/s 18.6%).

The main message so far is that the scope for cooperation between immigrants and natives is limited because immigrants, although showing a high willingness to send money to other immigrants, have a low inclination to invest towards natives. Why is this the case? In the investment game, there are typically two underlying motives for cooperation: the sender's beliefs about whether her choice to cooperate will be exploited by opponents and individual preferences such as other-regarding concerns and risk aversion (see, e.g., **???**). As described in Section II.A, we have elicited the expectations of senders regarding the back-transfer behavior of receivers. Moreover, given that we have employed the strategy method, we can use individuals' behavior as receivers as an indication of their other-regarding preferences. Finally, our survey contains a question on risk attitudes. Thus, we are able to examine the extent to which the investment behavior of senders is driven by these three factors.

For brevity, we relegate the details of this analysis to Appendix D and summarize the two main findings here. First, we run a simple regression of participants' in-group investments on measures of their risk attitudes, their social preferences towards in-group members, and their beliefs about in-group members' tendency to exploit them. This exercise reveals that senders' in-group behavior is driven both by beliefs and social preferences, which is in line with the findings of **?**. Second, we regress the in-group/out-group investment gap on measures that capture in-group/out-group differences in participants' beliefs and preferences. The main finding is that immigrants' in-group favoritism is to a large extent explained by differences in other-regarding preferences toward in-group and out-group members and only to a small extent by asymmetric beliefs. In Appendix D, we also show that, in our experiment, being matched with out-group opponents involves a loss of money, compared to being matched with in-group opponents. For immigrants, these losses are almost exclusively explained by their own in-group favoritism as first-movers. For natives, the payoff losses are largely due to the fact they receive lower back transfers from immigrant than from native opponents. Taken together, since immigrants' low out-group cooperation has a preference-based explanation and involves a willingness to sacrifice money, one might describe their social exchange behavior as being shaped by parochial altruism (**?**).

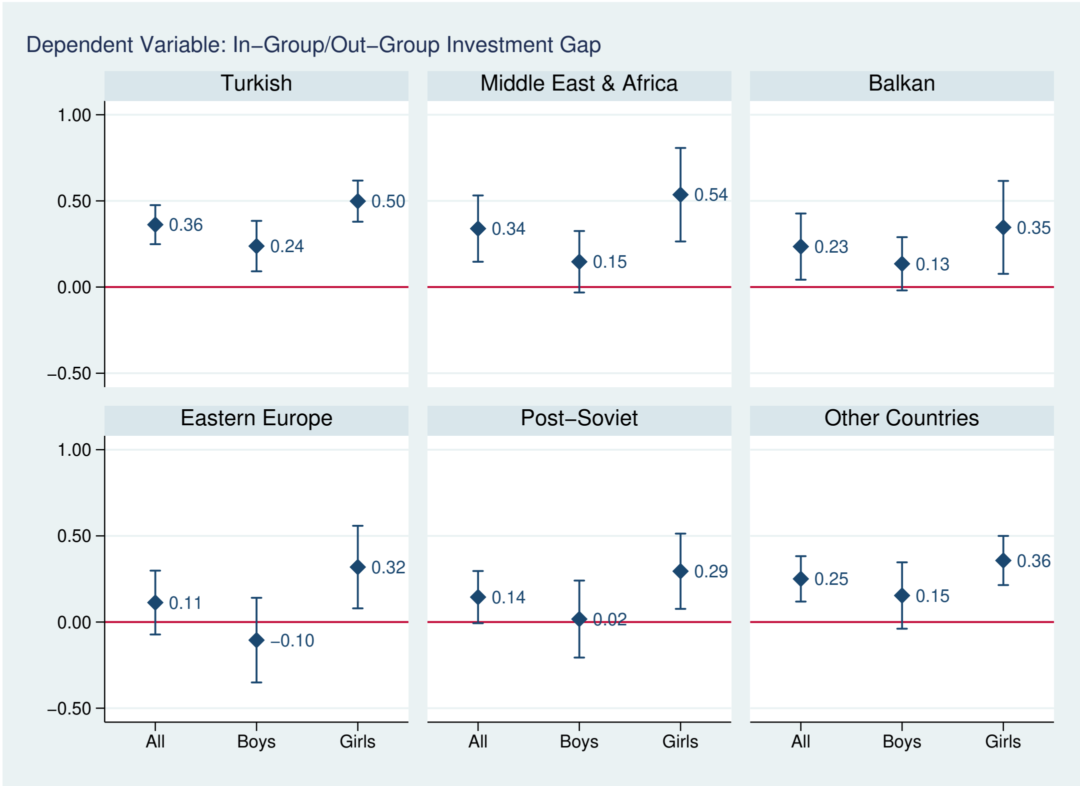

FIGURE C.4: *Heterogeneity Across Immigrant Groups*
Notes: OLS estimates of Equation (1), with *Immigrant* replaced by six dummy variables indicating whether a child's mother was born in Turkey, an Eastern European or post-Soviet country, a Balkan country, a country in the Middle East, an African country, or a country other than these. Standard errors clustered by school type and school location. The omitted category is native German children. Estimates of the constant (=trust discrimination among native children; non-reported) correspond to those reported in Figure C.2 (All: 0.07, Boys: 0.14, Girls: -0.02). Whiskers indicate the 95% confidence interval. Sample Sizes: 3,419 (All); 1,789 (Boys); 1,630 (Girls).

C.III. Heterogeneity

We now examine heterogeneity in in-group favoritism across immigrant groups. To that end, we rerun Equation (1) with the variable *Immigrant* replaced by dummy variables for six mutually exclusive groups of immigrant children. The first group (*Turkey*; 461 observations) comprises immigrant children of Turkish origin—by far the largest minority group of youths in Germany. In the second group (*Middle East & Africa*; 280 observations), we pool together immigrant children with Middle-Eastern and African backgrounds. The third group (*Balkan*, 137 observations) is made up of immigrant children whose parents come from a Balkan country. The fourth group contains immigrant children with an Eastern European background (*Eastern Europe*; 130 observations), while the fifth group is made up of immigrant children whose parents come from a post-Soviet country (*Post-Soviet*; 130 observations). The final, miscellaneous group (*Other Countries*, 176 observations) contains all other immigrant children.

Figure C.4 shows that the most pronounced above-average in-group favoritism occurs among immigrant children of Turkish origin.[2] In particular, while the in-group/out-group investment gap for German native children amounts to €0.07 (Boys: €0.14; Girls: €-0.02; see Figure C.2), it is €0.36 (Boys: €0.24;

[2]Note that we do not report estimates of the constant here, which remain the same as in Figure C.2.

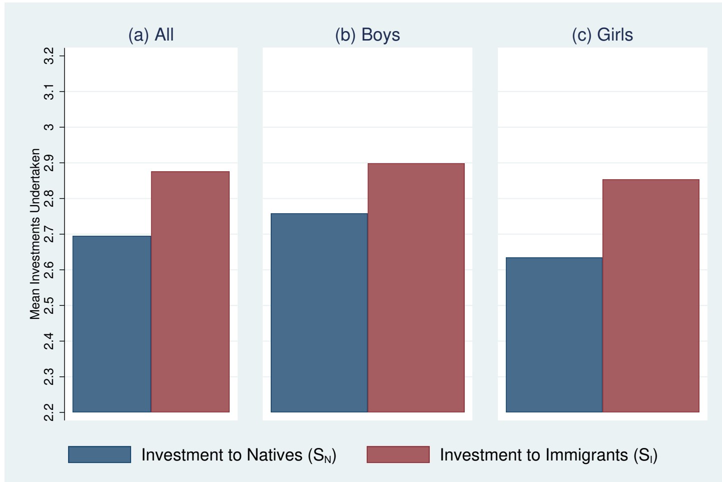

FIGURE C.5: *First-Mover Investment Decisions of Mixed-Background Children by Migration Background of Second-Movers*

Girls: €0.50) higher for children with Turkish-born parents. The second group of immigrants that displays above-average in-group favoritism are children with Middle Eastern and African backgrounds, followed by those whose parents originate from Balkan countries. By contrast, immigrant children whose parents come from Eastern European and Post-Soviet countries reveal a below-average in-group/out-group investment bias.[3] F-tests on the equality of the estimated coefficients on the dummies *Turkey* and *Eastern European* reject equality at p-values of 0.01 (full sample) and 0.02 (boys), but not for girls with a p-value of 0.11. A comparison of the estimated coefficients on the dummies *Turkey* and *Post-Soviet* yields a qualitatively similar conclusion. What is noticeable about these results is that the three strongly in-group biased immigrant groups share the characteristic that they are predominantly made up of children with a Muslim background (Turkey: 92%, Middle East & Africa: 75%, Balkan: 68%). By contrast, the religious background of immigrant children with an Eastern European or post-Soviet background—with shares of Christians of 84% and 61%, respectively—is quite comparable to that of native German children (67%).

Two final points should be made about the analysis in this Appendix. First, we have focused on comparing the experimental choices of immigrant and native children, but we have been silent on the behavior of children with mixed-backgrounds. Figure C.5 reveals that mixed-background children also favor immigrants over natives when choosing to cooperate ($S_I = 2.88$; $S_N = 2.69$; gap 7.1%; $p < .01$), but to a lesser extent than immigrant children and with less-pronounced gender differences. Second, in discussing the choices of children with migration backgrounds, we have made no distinction between first-generation (i.e., foreign-born) and second-generation (i.e., German-born) immigrant children. Our empirical strategy in Section III involves narrowing the baseline sample to second-generation immigrant children. Figure C.6 shows that the the degree of in-group favoritism that we identified in the investment game does not differ markedly between first- and second-generation immigrant children.

[3]The in-group/out-group investment gap of boys from these two immigrant groups is identical to or even lower than that of German native boys.

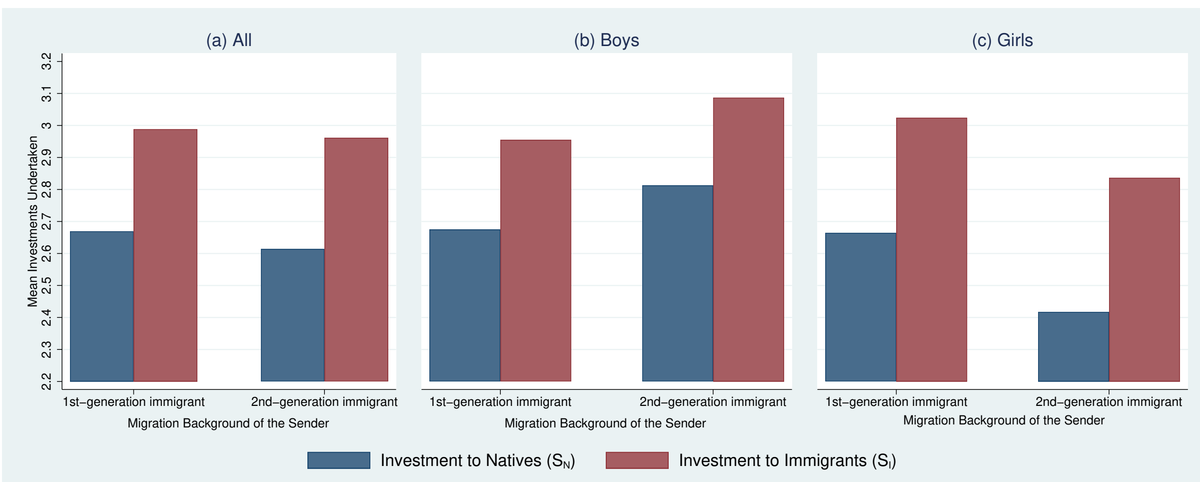

FIGURE C.6: *First-Mover Investment Decisions of First- and Second-Generation Immigrant Children by Migration Background of Second-Movers*

D. Are Decisions to Cooperate Explained by Preferences or Beliefs?

As discussed in Appendix C, in the investment game, there are typically two underlying motives for cooperating: the sender's beliefs about whether her choice to cooperate will be exploited by opponents and individual preferences such as other-regarding concerns and risk aversion (see, e.g., **???**).

As part of our design, we have elicited the expectations of senders regarding the back-transfer behavior of receivers. We have done this in a simple manner: after the investment stage, we asked participants to indicate how much they expect to receive back from the six possible receiver types (henceforth denoted E_k, $k = \{1, ..., 6\}$). Based on this, we first construct a measure of a participant's belief about whether receiver type k's will exploit her decision to give money; it is the share of the total resources available to the receiver that she expects to receive back: $EX_k = E_k/(5 + 3S_k)$. If EX_k is high, the sender expects that receiver type k will not to exploit the vulnerability that she has created for herself by sending S_k. If EX_k is low, the opposite is the case. For a measure of a participant's beliefs about being exploited by in-group versus out-group opponents, we collapse the six expectations $\{EX_1, ..., EX_6\}$ into the two variables, $EX_N = \frac{1}{2}(EX_1 + EX_2)$ and $EX_I = \frac{1}{4}(EX_3 + EX_4 + EX_5 + EX_6)$, and then define the in-group/group gap in beliefs as follows:

$$\Delta EX = \begin{cases} EX_N - EX_I & \text{for native children;} \\ EX_I - EX_N & \text{for immigrant children.} \end{cases}$$

Second, given that we have employed the strategy method, we can use individuals' behavior as receivers as an indication of their other-regarding preferences, as have many researchers before us. Recall that we used the contingent response method to elicit back transfers: each participant was asked to decide on the their back transfers to the six possible sender types k, contingent on the eleven possible investments (henceforth indexed by m) of sender types k. Let B_{km} denote an individual's back transfer to sender type $k = \{1, ..., 6\}$ who has send an amount $m = \{0, 0.5, 1...4, 4.5, 5\}$, and let $SP_{km} = B_{km}/(5 + S_{km})$ be the share that the receiver transfers back to sender type k of the total amount she has after sender type k has send the amount m. Our main proxy for a person's other-regarding preference towards sender type k is $SP_k = \frac{1}{11}\sum SP_{km}$, i.e., the receiver's "back transfer share" to k averaged over the 11 possible investments from sender type k. For a measure of a participant's asymmetric other-regarding preferences to in-group versus out-group opponents, we collapse the six "back transfer shares" $\{SP_1, ..., SP_6\}$ into the two variables, $SP_N = \frac{1}{2}(SP_1 + SP_2)$ and $SP_I = \frac{1}{4}(SP3 + SP_4 + SP_5 + SP_6)$, and then define in-group/group gap in other-regarding preferences as follows:

$$\Delta SP = \begin{cases} SP_N - SP_I & \text{for native children;} \\ SP_I - SP_N & \text{for immigrant children.} \end{cases}$$

Finally, our survey contains a question on risk attitudes. In particular, we asked participants: "Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?" Participants were asked to tick a box on the scale 0 to 10, where the value 0 means "not at all willing to take risks" and the value 10 means "very willing to take risks".

TABLE D.1

Explaining In-Group Investment Behavior

Dependent Variable: In-group Investments	Immigrant Boys	Immigrant Girls	Native Boys	Native Girls
	(1)	(2)	(3)	(4)
Social preferences toward in-group	0.335***	0.424***	0.348***	0.323***
	(0.063)	(0.053)	(0.043)	(0.043)
Beliefs about being exploited by in-group	0.282***	0.115**	0.377***	0.292***
	(0.063)	(0.055)	(0.044)	(0.044)
Risk attitudes	0.042	0.025	0.001	0.072
	(0.060)	(0.050)	(0.043)	(0.042)
Age	0.006	-0.006	0.001	0.010
	(0.004)	(0.005)	(0.001)	(0.005)
Observations	509	508	1,051	919
R-squared	0.113	0.145	0.145	0.136

NOTES: Results from four OLS regressions. Standard errors in parentheses. Proxies for social preferences, beliefs, and risk attitudes have been transformed into standardized variables with mean of zero and a standard deviation of one. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

With these three measure, we are able to empirically examine the extent to which the investment behavior of senders is driven by beliefs, other-regarding preferences and risk attitudes. In a first step, we run a simple regression of participants' *in-group investments* on the above-defined measures of their risk attitudes, their social preferences towards in-group members, and their beliefs about in-group members' tendency to exploit them (while controlling for age in months). The results, which are reported in Table D.1, shows that senders' in-group behavior is driven both by beliefs and social preferences; this is in line with the findings of **?**.

TABLE D.2

Explaining In-Group/Out-Group Investment Gaps

Dependent Variable: In-group/Out-group Investment Gap (IC)	Immigrant Boys	Immigrant Girls	Native Boys	Native Girls
	(1)	(2)	(3)	(4)
Asymmetry in social preferences (ΔSP)	0.136***	0.188***	0.264***	0.167***
	(0.035)	(0.033)	(0.023)	(0.021)
Asymmetry in exploitation beliefs (ΔEX)	0.056	0.081**	0.065***	0.107***
	(0.036)	(0.038)	(0.019)	(0.026)
Risk attitudes	-0.004	-0.018	0.045**	0.014
	(0.040)	(0.034)	(0.021)	(0.022)
Age	-0.002	-0.005	0.001	0.004
	(0.003)	(0.003)	(0.001)	(0.003)
Observations	509	508	1,049	917
R-squared	0.035	0.076	0.120	0.083

NOTES: Results from four OLS regressions. Standard errors in parentheses. Proxies for social preferences, beliefs, and risk attitudes have been transformed into standardized variables with mean of zero and a standard deviation of one. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

In a second step, we regress the *in-group/out-group investment gap* on measures that capture in-group/out-group differences in participants' beliefs and preferences. The results are reported in Table D.2. Here, the main finding is that immigrants' in-group favoritism is to a large extent explained by

Table D.3

Calculation of Payoff Losses Due to In-Group Favoritism

	Natives			Immigrants		
	All	Boys	Girls	All	Boys	Girls
Expected in-group payoffs	7.27	7.38	7.13	7.22	7.23	7.21
Expected out-group payoffs	6.99	7.06	6.90	7.04	7.09	6.99
In-group/out-group payoff gap	**0.28**	**0.32**	**0.23**	**0.18**	**0.14**	**0.22**
Marginal out-group payoffs	0.35	0.36	0.34	0.47	0.48	0.46
In-group/out-group investment gap	0.07	0.14	-0.02	0.35	0.28	0.40
% of the in-group/out-group payoff gap due to trust discrimination	**8.8%**	**15.8%**	**0%**	**91.4%**	**96%**	**83.6%**

Notes: *Expected in-group payoffs* (respectively, *expected out-group payoffs*) are the payoffs a participant can expect when randomly matched to a second-mover of his or her own migration background (respectively, a second-mover who is not of his or her own migration background). *Marginal out-group payoffs* are the slopes between expected out-group payoffs and out-group investments estimated from OLS regressions. The *share of the in-group/out-group payoff gap due to trust discrimination* is therefore the ratio of the product between marginal out-group payoffs and the in-group/out-group investment gap to the in-group/out-group payoff gap.

differences in other-regarding preferences toward in-group and out-group members and only to a small extent by asymmetric beliefs. For example, the estimates for immigrant boys in Column (1) show that a one standard deviation increase in the in-group/out-group gap in social preferences is associated with an increase in the in-group/out-group investment gap of €0.14, while the correlation between the in-group/out-group gap in beliefs and in-group/out-group investment gap is only one-third that. A similar conclusion applies to immigrant girls. This suggest that immigrants' in-group favoritism has a preference-based explanation and, as such, might also be thought of as discrimination.

Finally, we analyze how in-group/out-group behavior affects payoffs (in expected terms). The results of this exercise are reported in Table D.3. The first three rows of the table show that first-movers do better in terms of expected payoffs when matched with an in-group rather than an out-group opponent, a result that holds for both natives and immigrants. The table then goes on to provide back-of-the-envelope calculations of the shares of the in-group/out-group payoff gaps that are due to own in-group favoritism in giving money (as opposed to the shares of the payoff gaps that are due to differences in back transfers by in-group and out-group second-movers, respectively). For immigrants, we observe that the in-group/out-group payoff gap is almost entirely explained by their own in-group favoritism when sending money (96% of the gap for boys, 84% of the gap for girls). For natives, by contrast, only a small share of the in-group/out-group payoff gap stems from their in-group favoritism as first movers (16% of the gap for boys, 0% of the gap for girls). This, in turn, implies that their monetary losses when matched with out-group opponents can, to a large extent, be explained by the decisions made by in-group vs. out-group second-movers. To summarize, in our experiment, being matched with an out-group opponents involves a loss of money, compared to being matched with an in-group opponents. For immigrants, these losses are largely explained by their own in-group favoritism as first-movers. For natives, the payoff losses are largely explained by the fact they receive lower back transfers from immigrant than from native opponents.

Taken together, the findings presented in this appendix suggests that immigrants' social exchange behavior is shaped by parochial altruism (**?**). In general, parochial altruism involves, at the same time, strong prosocial behavior towards fellow group members and hostility toward individuals not of one's own ethnic, racial, or other group. Of course, our setup does not allow to make claims about "hostility", but there is close analogy to it here: immigrants' low out-group cooperation as first-movers and their back-transfer behavior as second-movers harms (monetarily) natives and involves at the same time a willingness to sacrifice money.