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# The Effect of Birthright Citizenship on Parental Integration Outcomes\*

Ciro Avitabile<sup>†</sup>, Irma Clots-Figueras<sup>‡</sup>, Paolo Masella<sup>§</sup>

## Abstract

The integration of immigrants is the subject of ongoing public debate, and devising measures to enable the assimilation of newcomers is high on the political agendas of many countries. This paper focuses on the legal institution of citizenship and analyzes the consequences of birthright citizenship introduced in Germany. Based on the exogenous variation provided by the 1999 reform of the German nationality law, we study the effect of child legal status on the integration of immigrant parents. We find that foreign born parents are more likely to interact with the local community and use the German language if their children are entitled to German citizenship at birth.

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# 1 Introduction

Since the 1960s, Western societies have been experiencing large increases in migration inflows, and the immigrant populations in the OECD countries have more than tripled in the intervening decades. The ethnic riots that occurred in the 2000s<sup>1</sup> prompted lively discussion about how governments could and should deal with the increasing diversity, and the best frameworks to apply to regulate the social status of newcomers and their descendants and to promote their integration into local communities.

The legal institution of citizenship has frequently been a key issue in political and cultural debates on immigration, welfare programs, multiculturalism, and nationalism. However, there is no evidence of whether the attribution of formal citizenship has an effect on how immigrants and ethnic groups identify themselves within host societies. It is also not clear more generally whether migration policies help to foster the integration process of immigrants. Migration inflows are still seen often as threatening the stability of the host country's customs, norms, and culture (see, for example, Huntington, 2004; Sarrazin, 2010). Debates over more restrictive migration policies and naturalization criteria have been taking place in Europe and the United States. The 14th Amendment to the US Constitution<sup>2</sup> has been attacked by politicians and congressmen, and a recent proposal for a law in Arizona, part of its measures to tackle illegal immigration, would deny citizenship at birth to the children born in the United States to undocumented immigrants.

Citizenship laws vary across countries and over time,<sup>3</sup> but they generally fall into two main groups based on the principles underlying the possibility to be granted citizenship at birth: *jus sanguinis* ("right of blood"), and *jus soli* ("right of soil") or *birthright citizenship*. According to the *jus sanguinis* principle, citizenship of a country is based on having an ancestor who is a citizen. Under the *jus soli* principle, citizenship is granted to any individual born in the country's territory. While the *jus soli* was and continues to be encoded in the US Constitution, in Europe the rules on citizenship are characterized by a mix of *jus soli* and *jus sanguinis*, which has provoked much debate and revision. In 1984, the British Nationality Act restricted *jus soli* in the UK; in 1999 a new German citizenship law injected some elements of *jus soli* into the obtaining *jus sanguinis* system.

This paper studies how the introduction of birthright citizenship for children born in

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<sup>1</sup>Outbreaks of social unrest occurred in towns in the north of England in 2001 and in the Paris suburbs in 2005 and 2006.

<sup>2</sup>"All persons born or naturalized in the United States, and subject to the jurisdiction thereof, are citizens of the United States and of the State wherein they reside."

<sup>3</sup>Bertocchi and Strozzi (2010) provide empirical evidence on how economic, legal, and cultural variables affected the evolution of citizenship laws in the post-world war II period.

Germany, to non-German citizen parents, affects the degree of parental integration with the local community, measured by the probability of interacting with German people, and the propensity to speak in German and read German newspapers. In May 1999, the German parliament amended the 1913 Citizenship and Nationality Law, which had granted German citizenship to a child if at least one parent had German citizenship at the time of the child's birth. The reform included elements of the birthright citizenship system: a child born to foreign parents on 1<sup>st</sup> January 2000 or after, was eligible for citizenship at birth if a) at least one parent had been ordinarily resident in Germany for at least eight years and b) had been granted permanent right of residence. The reform also included a transitional provision for the children of foreign residents, aged 10 or under at 1<sup>st</sup> January 2000. These children would be naturalized upon application (to be completed before 31<sup>st</sup> December 2000) if at least one parent had been ordinarily resident in Germany for at least eight years at the time of their birth.

In order to avoid potential problems of endogeneity related to the child bearing decisions of immigrants, and variations over time in the composition of immigrant inflows, we identify the effect of the regulatory framework of child citizenship by exploiting the retrospective component of the 1999 German reform. Among households composed of foreign parents whose youngest child was born in Germany between 1990 and 1999, only those for whom at least one of the parents had resided legally in Germany for more than 8 years at the time of its birth were affected by the reform. In the terminology of the evaluation literature this is the *treatment* group. The *control* group includes households where the youngest child was born between 1980 and 1989, and those where parents had been living in Germany for less than eight years when their youngest child was born between 1990 and 1999. We investigate how the legal framework of child citizenship can affect parental integration comparing the integration outcomes of parents in the treatment and control groups before and after the reform. Our analysis is based on data from the German Socio-Economic Panel (GSOEP), a household-based panel survey that over samples immigrants.

Our results show that the introduction of jus soli citizenship produces a significant and not negligible increase in the probability of socializing with Germans, measured as visiting or being visited by Germans, and in the level of acquaintance with the German culture, proxied by the probability of reading German newspapers. We investigated also whether the effect of the birthright citizenship system on parental integration varies with the language distance between Germany and the country of origin of the parents, and with their level of human capital, measured by number of years of education. The reform increased the propensity to use the German language only among respondents whose language of origin, like the German

language, belongs to the Indo European family. The parental level of education produced different patterns of integration after the reform: among poorly educated respondents we observe a higher propensity to interact with Germans, but the increased use of the German language was confined mainly to the better educated respondents.

This paper contributes to the large economic literature on migration developed since the 1990s. Borjas and Hilton (1996) explain that the historical debate on immigration policy, especially in the US, focuses primarily on two issues: 1) how well immigrants integrate into the native community; and 2) whether or not the presence of immigrants affects the labor market outcomes of native residents. The economic literature has mainly focused on the second issue (see Card (1990) and, more recently, Ottaviano and Peri (2012) for the US, Dustmann, Frattini, and Preston (2008) and Manacorda, Manning, and Wadsworth (2012) for the UK, ? for Germany). There has been less attention on the economic and cultural assimilation of immigrants.<sup>4</sup>

Recent works use plausibly exogenous variations in migration policies to study the economic consequences of a change in legal status.<sup>5</sup> The present study is the first to provide a quantitative assessment of the effects of a citizenship law on measures of cultural integration. Also, in showing that the regulatory framework of child legal status can significantly affect parental behavior, we provide evidence that migration rules can have indirect effects on individuals other than those directly targeted by the policy.

There is extensive evidence suggesting that higher levels of cultural integration can eventually lead to better economic circumstances. Studies for different countries (see, among others, Chiswick (1991) and Bleakley and Chin (2004) for the US, Dustmann and Van Soest (2002) for Germany) show that acquaintance with the host country language is positively correlated with immigrants' earnings. Frijters, Shields, and Price (2005), when analyzing the job search methods of immigrants and natives in the UK, find that adverts in newspapers are one of the most common sources of information, thereby penalizing immigrants who do not read local newspapers. There is also evidence showing that immigrants benefit

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<sup>4</sup>? proposes a theoretical model that studies the cultural assimilation and migration decisions jointly, and uses US data to test its implications. Akresh and Akresh (2011) show a rapid language assimilation of immigrant children in the US. Casey and Dustmann (2008) and Bleakley and Chin (2008) analyze the relationship between first and second generation immigrants language proficiency in Germany and the US, respectively. ? provides an empirical analysis of the determinants of immigrants' social assimilation in Germany. Algan, Dustmann, Glitz, and Manning (2010) compare the labor market outcomes of first and second generation immigrants in France, Germany and the UK.

<sup>5</sup>Mastrobuoni and Pinotti (2010) exploit the last round of EU enlargement to study the propensity to recommit crime among Romanians living in Italy. Mazzolari (2009) takes advantage of the introduction of dual citizenship in five sending countries to analyze the effect of dual citizenship on the labor market performance of immigrants in the US.

from their social interactions with natives. In particular, Kanas, van Tubergen, and van der Lippe (2009) find that immigrants' contacts with Germans led to better occupational status. Drever and Hoffmeister (2008) show that nearly half of the immigrant-origin job changers in Germany got their jobs through social contacts. Borjas and Hilton (1996) and Bertrand, Luttmer, and Mullainathan (2000) point at ethnic segregation and lack of social and cultural integration as possible explanations for the high participation of immigrants in welfare state programs in the US.

We contribute to two other strands of the literature. First, we add to the literature on the determinants of ethnic assimilation and identification. Bisin et al. (2010) find that in the UK, ethnic identity is more intense in mixed than in segregated neighborhoods, Clots-Figueras and Masella (2010) argue that the introduction in Catalonia in 1983 of bilingual education had an effect on the identity of Catalan residents and their political preferences. Recent economic literature investigated also the relationship between the degree of ethnic identification of minorities and their performance in the labor market and education.<sup>6</sup> In our case, the level of usage of the German language and the extent of interaction with German citizens could be interpreted as a measure of self-identification with the host country. We focus on citizenship laws and isolate the legal status of the children as another possible determinant of ethnic identification.

Second, focusing on the effect of child status on parental integration, we contribute to the stream of literature that studies how the gender and the human capital of the children affect parental behavior.<sup>7</sup> This paper provides evidence that legal status is another way that children affect parental behavior.

The paper is organized as follows. Section 2 describes the German nationality law and discusses the potential channels for children's citizenship rights to affect parental integration. Section 3 presents the data, the empirical strategy and the baseline results. Section 4 provides some robustness checks. Section 5 discusses the heterogeneous effects of the German nationality law; Section 6 concludes.

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<sup>6</sup>See, among others, Bisin et al. 2011; Battu and Zenou 2010; Zimmermann, Zimmermann, and Constant 2007.

<sup>7</sup>Washington (2008) finds that the number of daughters increases the propensity of US male congressmen to vote liberally, particularly on rights issues. Oswald and Powdthavee (2010) describe how political preferences in the UK are driven by the proportion of female offspring. Kuziemko (2011) documents that immigrant adults in California are less likely to learn English if there are English-speaking children in the household on whose language skills they can depend.

## 2 Background to Legal Status

### 2.1 The German Nationality Reform

Citizenship is the legal institution that assigns full membership of a state and determines the associated rights and duties. Citizenship can be acquired through birth, naturalization, adoption, or marriage. Citizenship at birth determines the legal position of second generation immigrants. In most cases, citizenship provides the right to vote, the right to run for public office, the possibilities to travel without restrictions and to obtain visas for relatives, and legal protection in the case of criminal charges. There may be other advantages such as a wider range of public benefits and better employment opportunities.

In Germany, a German passport is required for a number of public sector jobs (e.g., jobs in the justice, national defense, and administrative departments) and for some private sector professions such as dentistry, medicine, pharmacy, and law. In addition, German citizenship is likely to favor access to occupations that require unrestricted mobility and no bureaucratic hurdles (transport sector or cross-border services that are associated with very frequent travel). Non-entitlement to a German passport often implies higher hiring costs for employers, because non-citizen employees might require visas and other special permits. Steinhardt (2008) documents the existence of a strong correlation between legal status and labor market outcomes in Germany. Naturalization is associated with an immediate wage increase and subsequently faster wage growth. These differences are only partially explained by sociodemographic characteristics.

Citizenship status often implies some costs, such as the military draft and renunciation of original citizenship, and naturalization to obtain citizenship as an adult carries a broader set of monetary and non-monetary costs. For example, adults applying for naturalization have to pay administrative charges, and in many cases are required to take language and cultural tests, spend time queuing at registration offices, and avoid certain activities that might result in disqualification.

Citizenship laws should be seen as a part of broader migration policies. However, as stressed by Bertocchi and Strozzi (2010), while other measures (i.e. quotas and visa requirements) tend to change in response to short term contingencies (especially economic conditions and current government orientation), citizenship laws are the outcome of long term and complex processes that often require constitutional changes. The rules that determine the award of citizenship, in most cases reflect the relationship between a country's

legal origins and the historical process.<sup>8</sup>

In May 1999, the German Parliament amended the 1913 Citizenship and Nationality Law. The reform had three main elements:

- Introduction of jus soli citizenship;
- Changes to naturalization criteria;
- Denial of dual citizenship.

Before the reform, a child born in Germany to at least one parent with German citizenship was also granted German citizenship.<sup>9</sup> The new regime introduced elements of the jus soli system. A child born in Germany to foreign parents on or after 1<sup>st</sup> January 2000 would be granted citizenship at birth if: a) at least one parent had been ordinarily resident in Germany for eight years, and b) had been granted permanent right of residence. The law also introduced a transitional provision for those foreign residents in Germany under the age of 10 at 1<sup>st</sup> January 2000. They would be granted naturalization upon application (to be completed before 31<sup>st</sup> December 2000) if at least one parent had been legally resident in Germany for eight years when the child was born. For these children, dual citizenship up to the age of 23 was allowed, after which time they would have to renounce their former citizenship in order to retain German citizenship (this is known as the *Optionsmodell*).

Unlike the citizenship at birth provision, the policy for naturalization of adults had been revised several times in the years before the reform. Laws affecting naturalization applications were passed in 1990 and 1993. The changes introduced limited discretion of officials to deny naturalization, and provided foreigners with the legal right to claim naturalization; in particular, foreigners aged between 16 and 23 years with 8 or more years of residency, and foreigners over the age of 23 with a minimum of 15 years of residency, had legal claims to naturalization. The law approved in 1999 introduced further changes to the naturalization criteria: it lowered the minimum residency requirement to 8 years (without age restriction) and refined the legal entitlement to naturalization with additional requirements such as swearing loyalty to the German Constitution, being able to support oneself and one's family without social security or unemployment benefits, a clean criminal record, adequate

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<sup>8</sup>See Bertocchi and Strozzi (2010) for detailed historical information on the evolution of citizenship systems.

<sup>9</sup>In the case that it was the father who had citizenship, the child's citizenship was dependent on recognition or determination of paternity under the German law.



command of the German language. Moreover, applicants have to renounce their former citizenship, to which they are legally entitled only if they are aged 18 or older.<sup>10</sup>

The law passed by the German Parliament in 1999 includes an explicit denial of dual citizenship. Pre-reform, dual citizenship was not legally recognized and Anil (2006) reports anecdotal evidence suggesting that German officialdom generally was unwilling to entertain the idea of dual citizenship.

According to Thomsen, Gernandt, and Aldashev (2008), in 2005, 20% of the population had an immigrant background - i.e. they had migrated to Germany or were the descendants of earlier immigrants - and 47% of them had foreign citizenship. Our empirical analysis considers immigrants who were not citizens when the citizenship reform was approved and tests how the transitional provision of the reform has changed their incentives to integrate with native Germans. Those targeted by the transitional provision include the offspring of individuals who had not exercised their right based on the residency requirements described above, to become naturalized German citizens. We next discuss: 1) the possible reasons why eligible parents did not apply for German citizenship; 2) why the incentives to apply for citizenship might be more important for their children.

The costs of naturalization might be higher for parents than for their children, for different reasons. Guest workers who arrived in Germany during the 1960s and the 1970s were encouraged by their home governments to maintain ties with their home countries in order to guarantee flows of remittances.<sup>11</sup> As a result of German denial of dual citizenship, immigrants had to relinquish their home citizenship in order to become German citizens and, for many years, migrants who had done this faced various restrictions in their home countries.<sup>12</sup>

For first generation immigrants, relinquishing birth country citizenship might also imply some psychological costs; these costs would likely be much lower for their children born and educated in Germany. Thus, children born to immigrants in Germany are likely to have weaker ties with their parents' home country. As mentioned above, an important provision in the new citizenship system allows immigrants' children to have dual citizenship (and its benefits) up to the age of 23, when they are required to make a choice. This measure allows the children of foreign born parents to have an extended period to choose which country's citizenship rights they prefer.

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<sup>10</sup>Parents are allowed to relinquish their children's citizenship at an earlier age only after they have relinquished their own one and become German citizens.

<sup>11</sup>Sayary (1986) reports that Turkish migrants were encouraged to remit their savings by the offer of special interest rates for foreign currency savings accounts in Turkey, and certain privileges related to the import of goods.

<sup>12</sup>Mueller (2006) reports that pre-1996, Turkish regulation deprived individuals of their property rights in Turkey if they abandoned Turkish citizenship.

The economic benefits of citizenship are supposed to be higher for children than for parents. In fact, citizenship rights should matter more when agents enter the labor market, than later when the relevant skills and experience have been acquired.<sup>13</sup> Finally, parents eligible to apply for citizenship for their children under the transitional regime might do so in anticipation of the higher naturalization costs their children might face in the future as a result of the new naturalization provisions.

There are no official statistics on the number of individuals who became citizens under the transitional scheme or on those who were eligible to register for it. The GSOEP provides information on parents, but not children's citizenship, and to the best of our knowledge, this information is not obtainable from other survey data. However, using data provided by the German Statistical Office on number of naturalizations by age, and number of individuals born to foreign born married couples (see Table A1), we can show the extent to which non-citizen immigrants exploit the transitional citizenship reform clause. Since the number of naturalizations by birth cohort is only available from 1998 onwards, Figure 1 plots for each year in the interval 1998 to 2005 the number of naturalizations granted to individuals born between 1990 and 1999 (including those that potentially could benefit from the transitional component of the reform), compared to the number of naturalizations granted to those born between 1980 and 1989, none of whom qualified for the transitional scheme. In 1999, 41,329 individuals born between 1990 and 1999 were naturalized, as opposed to 46,188 individuals born between 1980 and 1989.<sup>14</sup> Among individuals born between 1990 and 1999 there is a sharp increase in the number of naturalizations in 2000 and 2001, respectively 47,937 and 48,232.<sup>15</sup> In contrast, there is a reduction in the number of naturalizations among those born between 1980 and 1989,<sup>16</sup> respectively 28,877 and 28,053 units in 2000 and 2001. In Section 3.3 we provide an estimate of the take-up rate of the retrospective component of the reform within the sample used in our empirical exercise.

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<sup>13</sup>As discussed in Section 2.1, several jobs and professions are precluded to residents without German passports.

<sup>14</sup>Under the old regime, individuals aged 16 or older were eligible for naturalization if they had been legally resident in Germany for 8 years.

<sup>15</sup>The time lag between application and registration varies between 1 and 3 years.

<sup>16</sup>This reduction is in line with the drop in the total number of naturalizations. Steinhardt (2008) argues that this decline could be explained by the fact that data pre-2000 includes ethnic Germans, i.e. individuals of German origin who returned after living in other countries, who could be naturalized immediately.

## 2.2 Effect on Parental Integration

There are different mechanisms through which the legal status of children might affect parental integration.<sup>17</sup> A priori it is not clear whether the acquisition of German citizenship at birth by children will have a positive or negative effect on the assimilation of foreign parents.

Integration with the local culture may enter directly into parents' preferences. All else being constant, the shock in their children's legal status might be perceived by the parents as a change in the attitude of native nationals towards immigrants and induce them to increase their efforts to assimilate the local culture. In principle, returns on cultural assimilation may also be higher because the change in child legal status can affect parents' return migration decisions, inducing them to stay longer in Germany.

Parental assimilation might also affect the child's future earnings. That would be the case where parents are altruistic and care about the future earning power of their children (or expect to receive transfers from their children later on in life) and the future earnings of a child depend on his/her skills and the investment made by the parents in terms of the quality of the social networks and the language spoken at home. In this setting, a change in citizenship status can be interpreted as a technology shock that increases the return on investment decisions of both parents and children. As result, parents will decide to expend more effort on improving the quality of their social networks and the grasp of the host country language.

The effect of child legal status on parental integration can also be analyzed using Bisin and Verdier (2001) and Bisin, Topa, and Verdier (2004) as reference frameworks. In such environments, parents evaluate their children's actions based on their own preferences and therefore believe that the welfare of their children will be higher if they are socialized with the parental type.<sup>18</sup> The larger the difference between the parental evaluation of the children's welfare when socialized with their own type and when socialized with the "German" type, the larger will be the effort invested by parents to socialize their children with their own type.

In this setting there are two alternative mechanisms through which child legal status can affect parental integration. First, the legal status of their offspring may reduce the efficacy of the effort exerted by parents to socialize their children with their own type. In our empirical

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<sup>17</sup>Alesina and Schuendeln (2007), Aghion, Algan, and Cahuc (2011) document instances of institutions and policies that have a significant impact on individual attitudes and beliefs.

<sup>18</sup>Bisin, Topa, and Verdier (2004) provide evidence that high intermarriage rates between people from the same religious groups in the US can be explained by parental efforts to socialize their children with their own religious traits.

analysis a decrease in the effort to socialize children with parental type implies a higher usage of German language and more social interaction with natives. This would explain a positive effect of birthright citizenship on parental assimilation. Second, the legal status of the children might well have an impact on the difference between the parents' evaluations of their children's welfare when socialized with their own type and when socialized with the "German" type. However, it is not obvious in which direction the difference will change, with the result that the sign of the effect of the reform on parental integration is unclear. On the one hand parents may believe that their children will take full advantage of the economic opportunities connected to German citizenship only if they acquire a German type (the distance between the two welfare evaluations in the Bisin and Verdier (2001) framework would then shrink); on the other hand, parents may anticipate that if their children have citizenship their actions will become even more "distant" from the parental culture and from the parents' perspective will be conducive to lower welfare.

Finally, we propose a further mechanism that entails a slight variation to the framework mentioned above. Suppose that for parents being culturally distant from their children involves an additional utility cost and that, once the children's type is revealed, they may decide to exercise the effort to socialize themselves within the local culture. Then, because the nationality reform might increase the probability that children will acquire the "German" type, parents might accelerate their assimilation with the German culture in order to reduce the cultural distance from their children. In other words, immigrants might decide to integrate more with the culture of the host country because their children will be growing up as German citizens, speaking German, and adopting German habits.

### 3 Empirical Analysis

#### 3.1 Identification Strategy and Econometric Method

The objective of our empirical analysis is to identify whether introduction of the birthright system has an effect on the level of integration of foreign born parents. We exploit the retrospective provision in the 1999 citizenship reform which allows foreign born parents without German citizenship to naturalize their children born in Germany between 1990 and 1999 subject to the requirement of the parents being resident in Germany for at least 8 years before the child was born.

By exploiting the retrospective component of the reform we rule out two potential sources of *endogeneity*. First, in deciding whether or not to have a child, foreign born individuals

might potentially be affected by the regulatory changes that became effective with the reform. Second, the composition of the migrant population might change after the reform since potential incomers might be attracted by the fact that, under the new regime, any children born to them would have full German citizenship.

In our context, foreign citizen parents, resident in Germany for at least 8 years when their youngest child was born between 1990 and 1999, represent the *treatment* group. The *control* group includes both foreign citizen parents whose youngest child was born in Germany between 1980 and 1989 and foreign citizen parents who had been resident in Germany for less than 8 years when their youngest child was born in Germany in the period 1990 to 1999. By comparing the integration outcomes of the treatment and control groups, before and after the reform, we are able to capture the effect of the provision that introduces birthright citizenship on parental integration. Ideally we would want to perform our analysis separately for the two groups of individuals that make up the control group: those whose youngest child was born between 1980 and 1989 and those resident in Germany for less than 8 years when their youngest child was born. However, the sample size of the control group becomes too small (37 households) if we restrict it only to parents resident in Germany for less than 8 years.

There are at least three dimensions along which children born between 1990-1999 to immigrant parents, who had been in Germany for at least 8 years at the time of birth, benefited from the changes introduced by the naturalization reform, compared to children of parents in the control group: a) age at which they can acquire German citizenship; b) uncertainty about the citizenship acquisition and c) possibility to hold double citizenship.

Because of the transitional component, all children born between 1990 and 1999 to parents who had been in Germany for at least 8 years can be naturalized and become German citizens at the latest by age 10. For these children, the citizenship acquisition is automatic upon application registration. Children of parents in the control group, instead, can apply for German citizenship not earlier than age 18 when they have the legal right to relinquish their former citizenship. The success of the application is conditional on 1) providing evidence of 8 years of almost uninterrupted ordinary residence in Germany;<sup>19</sup> 2) fulfilment of the criteria discussed in Section 2.1. When earned at an early stage, citizenship status is more likely to represent an important cultural shock that fosters both children's and parents' assimilation with German culture. More importantly, children of parents in the treatment group will

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<sup>19</sup>According to the German Nationality Act, in the presence of interruptions longer than 6 months, a previous period of residence in Germany might not be counted as contributing towards the duration of residence requirement.

have a higher probability of entering the labor market with a German passport than those in the control group.

Both points 1) and 2) mentioned above make the citizenship acquisition potentially uncertain for children of parents in the control group. For these children the likelihood of becoming German citizens crucially depends on the return migration decisions of their parents. Using information on return intentions elicited in the GSOEP, we find that on average 45% of the immigrants are willing to return their home country within 8 years. Parents who do not plan to stay in Germany long enough to allow their children to meet the residence requirements might be less willing to invest in their' s and their children's country-specific human capital.<sup>20</sup>

Finally, children of parents in the control group are not allowed to hold double citizenship, as opposed to those in the treatment group who can maintain double citizenship till the age of 23. In Section 2.1 we already discussed the psychological and economic costs that may be associated with the decision to relinquish the citizenship of origin.

Since the treatment group includes all those individuals who were offered the possibility to apply for their children's citizenship, irrespective of whether or not they did so, our strategy identifies the effect of eligibility to apply, the so called intention-to-treat (ITT) effect. The main advantage of this strategy relies on the possibility to control for potential selection issues inherent in the decision to apply.

In order to test formally how the introduction of jus soli citizenship affects parental integration, we estimate the following differences-in-differences (DD) model:

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 D_{it} + \beta_3 T_i * D_{it} + \gamma' \mathbf{X}_{it} + \mu_t + u_{it} \quad (1)$$

where  $Y_{it}$  is the integration outcome of parent  $i$  at time  $t$ .  $T_i$  is the treatment dummy which is equal to 1 if in 1999 either parent  $i$  or his/her partner had been living in Germany for 8 or more years when the youngest child was born between the 1<sup>st</sup> of January 1990 and the 31<sup>st</sup> of December 1999, and is equal to 0 if the youngest child of parent  $i$  was born between 1980 and 1989, or she/he and her/his partner had been resident in Germany for less than 8 years when the youngest child was born between 1990 and 1999. For individual  $i$ , the dummy  $D_{it}$  takes the value 1 for survey interviews conducted after the reform was passed in Parliament (May 1999), and 0 for those conducted before that date.<sup>21</sup>

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<sup>20</sup>Previous work for Germany shows that the uncertainty related to the return migration decisions can significantly affect immigrants' human capital investment (Dustmann (1999)) and saving and remittance decisions (Dustmann and Mestres (2010), and Dustmann and Mestres (2011)).

<sup>21</sup>Since the interviews were not conducted at the same time, in the 1999 survey some individuals were interviewed before the reform was approved and others after it was passed.

$\mathbf{X}_{it}$  includes the full set of individual and household characteristics. In the baseline specification we include dummies for single year age (of respondent), gender, household head status, years of education, number of years living in Germany, number of children, and marital status. Since the speed of the integration process might vary according to immigrants' origins, we include country of origin dummies. A full set of year dummies,  $\mu_t$ , controls for time specific shocks affecting all individuals in the time interval covered by our analysis.

The main parameter of interest is  $\beta_3$  which identifies the effect of the introduction of jus soli citizenship on parental integration. Since treatment status is determined at household level, we account for within household correlated shocks, clustering standard errors at the household level.

The key identifying assumption relies on the fact that integration trends would be the same for both the treatment and control groups in the absence of treatment. In Section 4.1 we test whether there are differential time trends that potentially could explain our results.

At each period in time our sample includes only foreign born parents who are not German citizens and are either married or cohabiting. We exclude single parents because we do not know the citizenship status of the other parent. We exclude foreign born individuals with German citizenship since the survey questions on language use and social interactions are addressed only to non-German respondents. Ethnic Germans are of no interest for our analysis, because this population is entitled to immediate German citizenship, enjoys full political rights, and also is allowed to retain former citizenship (see Anil (2006)). For this reason, we identify and drop them.

## 3.2 Data and Descriptives

The main data source for our analysis is the GSOEP, which started in 1984 and is the longest-running longitudinal survey of private households and persons in the Federal Republic of Germany. This survey provides representative micro-data on individuals and households. Most importantly, it oversamples migrants.<sup>22</sup> The data therefore are unique in providing continuous information on a large sample of immigrants over a long period of time. In the relevant household, every individual aged over 15 is interviewed. The household head provides information on children under 15. Individuals who have left the household to set up on their own are tracked and included in the panel. The GSOEP data provides retrospective

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<sup>22</sup>Questionnaires for migrant households are available in native languages. This rules out potential sample selection problems due to differential response rates.

information on all children born to mothers in the survey, including year of birth, irrespective of whether they are living with their parents or have left the parental home.

In our sample, the units of observation are household heads and their partners. Our sample includes 374 households in total. In 69% of cases both partners initially responded to questions about their socioeconomic status and integration behavior;<sup>23</sup> in the remaining 31% of households only one of the partners provides information.

The dataset contains detailed information on country of origin and arrival date of immigrants, and family composition. Crucial for our analysis, in each survey foreign born individuals are asked about their citizenship status and, if naturalized, the year of their naturalization. This allows us to construct a data set of foreign born, non-citizen parents, with at least one child born in Germany in the time period 1980-1999, excluding all parents of children born after the reform. In order to make our treatment and control groups more homogeneous in terms of observable characteristics, in the main analysis we restrict our sample to those households where both parents were born after 1950. Our main specification considers only surveys after 1993 in order to avoid possible confounding effects due to changes to the naturalization policies enacted in 1990 and 1993. Thus we use surveys between 1994 and 2006.

As expected, individuals living in treated households on average are younger than those in control households (34 vs. 41 years): see Table 1 which reports the socio-demographic characteristics of the two groups elicited in the 1999 survey, the year the reform was approved. Consistent with the age difference, individuals in the control group are less well educated and have higher annual earnings. Differences are statistically not significant and, as shown in Table 1 column (4), become negligible when we include single year age (of respondent) dummies. Individuals in the control group have been in Germany slightly longer than individuals in the treatment group, but the difference is not statistically significant. These results provide support for our assumption that the control group represents a valid counterfactual of the treatment group, after accounting for age differences.

Table 2 reports the average levels of integration for the treatment and control groups, before and after the reform. Respondents were asked whether, in the previous year, they had visited Germans in their homes and had received a visit from any German people. In the period covered by our analysis these questions applied to every second year, starting from 1995. We use the responses to these questions in order to construct the *Interactions with Germans* index that measures the intensity of contact with German individuals. The index is equal to 0 if the individual has not received any German people at home and has not visited

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<sup>23</sup>In about 10% of these households, over time, the number of respondents changes from 2 to 1.



any German individuals, and 0.5 if the individual has either visited German individuals or has had a visit from a German person. The index takes the value of 1 if the answer is positive to both types of contacts. Before the reform, the average interaction index for the control group is almost 0.80, as opposed to 0.71 for those in the treatment group. After the reform, the average for the control group is virtually the same, but increases to 0.77 for those in the treatment group.

The survey includes a question on use of the German language. Immigrants were asked what language they mainly spoke in Germany: i) mostly German; ii) their mother tongue; iii) both. This question was included in the surveys for 1996, 1997, 1998, 1999, 2000, 2001, 2003 and 2005. The variable *German spoken* is defined as a dummy, where 0 denotes that the individual mostly uses his/her mother tongue, and 1 if the individual speaks either both languages or mostly German. On average, around 70% of the individuals in the control group said they spoke German before the reform, as opposed to 65% of those in the treatment group. There is no variation in the use of the German language for those in the control group after implementation of the new citizenship law, while the proportion of those who regularly speak German increases to 69% among those in the treatment group.

In the period covered by our analysis, non-citizen respondents were asked every second year from 1994 whether they read: i) newspapers only from their country of origin; ii) newspapers mainly from their country of origin; iii) about half and half - German and country of origin; iv) mostly German newspapers; v) only German newspapers; vi) not applicable, do not read a newspaper regularly. The variable *German newspapers* is defined over the range 1-5 and takes the value 1 if the individual reports reading only home country newspapers or no newspapers, and 5 if she/he reads only German newspapers. While there is an increase in the propensity to read German newspapers for both groups after the reform, the increase is noticeably larger only for those in the treatment group.<sup>24</sup>

In summary, the results suggest an increase in the level of assimilation of foreign born individuals affected by the transitional scheme in the reform, as measured by both level of social interactions with native Germans and level of acquaintance with the German culture. At the opposite extreme, those immigrants unaffected by the reform display no change in their level of integration.

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<sup>24</sup>Given that each question is asked in different years we have different observations for each of the dependent variables.

### 3.3 Baseline Results

Table 3 reports the results of the OLS estimate of equation (1). In columns (1), (4) and (7) we report the results for the specification that controls for age linearly. In columns (2), (5) and (8) we report the results of our baseline specification, where dummies for single year age (of respondent) are included.

The reform has a positive and statistically significant effect on the probability of interacting with Germans, irrespective of whether age is included linearly or not (columns (1) and (2)). The size of the effect is relatively large since it is equal to approximately one-third of the standard deviation of the dependent variable at the baseline. Estimates for outcomes related to German language use are reported in Table 3 columns (4)-(5) and (7)-(8). When age is included linearly there is a positive and significant effect of the reform on the propensity to speak the German language. However, the coefficient becomes smaller and not significant in the baseline specification. Note, however, that the variable *German Spoken* as defined above, proxies only for whether the individual ever speaks German but does not capture the quality of the spoken German or the situations in which German is used. Finally, we study the effect of the reform on the propensity to read German newspapers. The effect is positive and significantly different from zero. The size of the coefficient is approximately one-fifth of the standard deviation of the variable. If we exclude those who do not read a newspaper, the results are in line with those presented.<sup>25</sup>

In order to account for the possibility that our results are capturing time-invariant household specific characteristics, we estimate a model that adds household fixed effects to the baseline specification. Results are presented in Table 3 columns (3), (6) and (9). In this case, although the coefficients are slightly higher, the results are in line with those of the baseline specification: a sizeable and statistically significant effect of the reform on *Interactions with Germans* and *German newspapers*, positive but not significant on *German Spoken*.<sup>26</sup>

When we estimate the baseline DD on a more homogeneous sample, that only includes parents whose youngest child was born between 1984 and 1995, the results obtained using German Newspapers as dependent variable, although less precise, are qualitatively similar to those presented in Table 3 (0.214 with a s.e. of 0.168); the coefficient of  $T_i * D_{it}$  is slightly smaller (0.7 with a s.e. of 0.069) when the dependent variable is *Interactions with Germans* and is smaller and has a very large standard error when the dependent variable is *German*

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<sup>25</sup>Results not reported here but available from the authors upon request. In alternative specifications, we use ordered probit models for the *German newspapers* regressions and the results for the marginal effects are in line with the OLS estimates.

<sup>26</sup>All the exercises and checks performed in the following sections of the paper are also robust to the inclusion of household fixed effects.

spoken.<sup>27</sup> Overall, the evidence presented so far suggests that the transitional component of the German nationality reform led to an increase of (proxies for) both social and cultural integration.

In order to provide *prima facie* evidence on the possibility of pre-trend differences between the treatment and control groups, and to evaluate the time profile of the effect of the reform on the two measures that seem to be affected by the reform (*Interactions with Germans* and *German newspapers*), we estimate the baseline model including interactions between treatment status and unrestricted time effects. Since the two outcome variables are not available for the same survey years, we choose as the reference the survey year prior to approval of the reform and set the treatment effect equal to zero for that year. Figure 2 plots the year specific treatment effects on our dependent variables of interest, *Interactions with Germans* and *German newspapers* respectively. Before the reform, parents in the treatment group display a significantly lower level of interactions with Germans compared to those in the control group, although the trends are similar. After the reform, the level of interactions for those in the control group does not vary, while it increases for the treatment group, thus leading to a convergence. This translates into the pattern of year specific treatment effects plotted in the top panel of Figure 2. Although the reform was approved only in May 1999, it had been widely discussed in the media in the months before Parliament’s approval. This probably explains why, from 1999 onwards, the difference in the propensity to interact with Germans between individuals in the treatment and control group increases peaking in 2001 and flattening out afterwards. We do not have information on the likelihood of reading German newspapers in the year the reform was approved, but while up to 1998 there is no statistically significant difference between the two groups, from 2000 onwards those in the treatment group are more likely to read German newspapers than those in the control group. The difference tends to remain constant after 2002. One of the main objectives of the reform was to promote integration of young immigrant cohorts and this argument was used frequently by policy makers to bolster public support. This might have added salience to the cultural integration especially among parents of younger children, and might partly explain the immediate increase in our measures of assimilation for parents of children eligible for the transitional component. Differences between the treatment and the control group tend to

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<sup>27</sup>Using the restricted sample, in an alternative specification we adopt a regression discontinuity approach that exploits the discrete jump in the possibility to apply for those born in 1990 or afterwards. The estimates of the coefficient of interest are 0.216 (with s.e. 0.299) and 0.08 (with s.e. 0.097) when the dependent variables are German Newspapers and Interactions with Germans, respectively. When the dependent variable is German spoken the estimates are very imprecise. These results, that are available from the authors upon request, are broadly consistent with the main ones presented in the paper.

persist for a few years after the reform, thus supporting the hypothesis that reform changed immigrants' incentives to invest in cultural integration.

The results presented in this section measure the effect of the transitional component of the German nationality reform on the sample of parents in the GSOEP whose youngest children were eligible for naturalization. The ITT estimates have to be interpreted as the effect of the increased possibility to apply for children's German citizenship on parental integration.<sup>28</sup> Parents in the treatment and control groups have been in Germany for a long period of time (on average more than 20 years) and the possibility to apply for their children's naturalization might not necessarily have the same effect on the propensity to integrate of immigrants who have spent less time in Germany, a group for which the effect of legal status might be stronger.

While the ITT estimates offer the advantage that they do not suffer from the possible self-selection effect that is inherent in the decision to apply for child naturalization, in what follows we discuss how the ITT estimates translate into the actual effect of child legal status on parental integration under alternative hypotheses on the returns from child legal status. Under the assumption that the effect of child legal status on parental integration is homogeneous across the whole population of immigrants resident in Germany, the ratio between the ITT and the fraction of eligible parents who applied for their children's naturalization identifies the Average Treatment Effect (ATE) of child legal status on parental integration.

The take-up of the transitional component is characterized by *partial compliance*: only a fraction of the parents of eligible children applied for their children's citizenship before December 31<sup>st</sup> 2000. Partial compliance might suggest that the returns from child legal status differ across immigrants and parents who expect higher returns from their children's citizenship are more likely to apply. Both observed - e.g. years spent in Germany, age and education - and unobserved characteristics can potentially lead to heterogeneity in the effect of child legal status on parental integration. If the returns from child legal status of eligible parents who applied (*compliers*) are different from the returns of eligible parents who did not apply (*not-compliers*), the ratio between the ITT and the fraction of eligible parents who applied would capture the Local Average Treatment Effect (LATE), that is the effect of child legal status on those who complied with the provisional component. If the treatment effects for compliers are not different from the treatment effects for not-compliers, but they are different from those in the control group, the LATE would be equivalent to the Average Treatment on the Treated (ATT) effect. If parents who applied for their children's

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<sup>28</sup>Among individuals born to immigrant parents between 1980 and 1989, those who are 18 or above can potentially apply for naturalization according to the rules described in Section 2.1.

naturalization are those with the highest returns from child legal status, the LATE would be an upward bound of both ATE and ATT.

The Appendix provides estimates of the take-up rate of the provisional component within our sample. In our estimates of take-up, we account for two potential reasons for which children born between 1990 and 1999 to foreign born couples might not take advantage of the transitional scheme. First, their parents might return to the home country. Second, parents awarded citizenship before 2000, through naturalization, could apply immediately for their child’s naturalization. Therefore, the results are presented under alternative scenarios, for return migration and naturalization.

The estimated take-up rate (within our sample) varies in the range 34% to 46%. This implies that, for instance, in the presence of homogeneous (or heterogeneous) treatment effects of child legal status the ATE (or LATE) for *German Newspapers* varies between 0.65 and 0.48, the latter corresponding to about half of the standard deviation and a quarter of the average of the dependent variable at the baseline.<sup>29</sup>

Taken together, the above results show that the possibility to naturalize children under the transitional component of the German nationality reform had a significant and fairly large effect on immigrant parents’ propensity to interact with Germans and to use the German language.

## 4 Robustness

### 4.1 Differential Trends

In this section we provide support for our identification assumption that integration trends would be the same for both the treatment and control groups, in the absence of the reform, and, therefore, rule out the possibility that our estimates are an artifact of exogenous trends in the propensity to interact with German born citizens, to speak German, and to read German newspapers.

In order to check the robustness of our identifying assumption we perform several tests. For reasons of space, we report only the results for the baseline specification. First, we test whether differences in time trends driven by unobservable characteristics could potentially

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<sup>29</sup>In our setting there might be spillovers from compliers to not-compliers, mainly driven by the possibility that social interactions between eligible parents who applied and those who did not - e.g., school and sport related activities - might determine changes in the behavior of the latter. Note that in this case the size of the ATE would be smaller than the one above estimated (it would instead be larger in the less likely case that there are spillovers from compliers to parents whose children are not eligible)

bias our results. This seems unlikely given the patterns of integration of the control and treatment group depicted in Figure 2; however, in order to provide further evidence against this possibility, we perform the following falsification exercise. After restricting the sample to the pre-1999 survey results, we assume that the reform was implemented in a year prior to 1999 and estimate the same specification as in eq. (1). In other words, we compare the integration outcomes of the treatment and control groups before and after this year. If our results are artificially generated by non-parallel trends in the integration outcomes of the treatment and control groups, we would expect the difference between the level of integration of the treatment and the control groups to be significantly different after the "placebo" reform. The results reported in the top panel of Table 4 (Placebo I) were obtained on the assumption that the reform was implemented in 1997. Reassuringly, the coefficients are much smaller than our baseline coefficients and not significantly different from zero. Our results are robust to the choice of alternative years for the placebo reform.

Parental incentives to socialize with the host country society might be affected by the age composition of their children, since immigrant parents might put more effort into socialization with the host country when their children are young. This might be because more occasions arise for socialization with native Germans (i.e. visits to kindergartens and schools) or because the time horizon to exploit the investment in human capital is longer. As a first test of the possibility that our results are driven by differences in the age structure of offspring, we augment the model in equation (1) to include the age of the oldest child in single year dummies.<sup>30</sup> Results are presented in Panel B of Table 4. We find that for each of our integration outcomes, the coefficients are in line with those discussed in Table 3.

The results displayed in Panel B do not support the hypothesis that differences in the ages of the oldest child determine differential integration patterns and, therefore, it seems unlikely that our results are an artifact of a non-linear relationship between degree of parental integration and children's age composition - i.e. when the children have grown up, parental integration and the age of offspring might be unrelated. In order to provide further evidence on this confounding mechanism, however, we perform the following falsification test. Foreign citizen parents resident in Germany for at least 8 years when the youngest child was born, between 1988 and 1997, are now the *treatment* group. The *control* group includes all foreign citizen parents whose youngest child was born between 1978 and 1987 or whose residence in Germany was less than 8 years when the youngest child was born between 1988 and 1997. We then consider only the surveys prior to 1999, assume the reform was passed in

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<sup>30</sup>Note, however, that this test does not control for the possibility that different birth spacing of children across families might also be correlated with different levels of integration.

1997, and estimate the specification in eq. (1). The age profile of the youngest children of individuals belonging to the fictitious treatment and control groups is the same as in the original treatment and control groups, respectively. If the results in our main specification are capturing a bias due to a relationship between the level of parental integration and the age of the oldest children, we would expect  $\beta_3$  to be significantly different from zero. The results in the bottom panel of Table 4 (Placebo II) show that the coefficients of interest are negative and not significantly different from zero. Also for this falsification exercise the results are robust to choosing alternative reference years.

## 4.2 Attrition

In this section we show that the validity of our results does not seem to be affected by the possibility that individuals exit the sample non-randomly and, in particular, by the possibility that the reform we study have affected differentially the probability of leaving the sample of individuals in the treatment and control groups, in which case our estimates would be capturing the effect of the reform on the composition of the sample rather than on the level of integration of the respondents.<sup>31</sup> In our sample, on average 16.2% of the individuals exit; this is a similar number to the one found in other studies of Germany using the GSOEP (see, for instance, Bellemare, 2007)).

We define a dummy variable that takes the value 1 if in the following survey year the individual is not surveyed, and 0 otherwise, and we use the model in eq. (1) to assess whether attrition probability changes differentially for the treatment and the control group after the introduction of the nationality reform, but we do not find evidence to support this hypothesis.<sup>32</sup>

## 4.3 Other Provisions of the Reform

The results of the analysis in this paper show that children’s citizenship rights have a positive impact on parental integration into the adopted home country, in this case Germany, as measured by the propensity to read its national newspapers, and interact with Germans. However, the other provisions of the reform related to the naturalization of immigrants might also have an effect on their integration. In this section we provide evidence that our main results are not capturing changes in integration outcomes driven by these provisions.

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<sup>31</sup>Results in ? show that return migration is unlikely to generate significant bias in the cross-sectional estimates of earnings assimilation of immigrants in Germany.

<sup>32</sup>The coefficient of interest is equal to 0.012, with a standard error of 0.036.

Before the reform, foreigners aged between 16 and 23 years with 8 or more years of residency, and foreigners over the age of 23 with a minimum of 15 years residency, had a legal claim to naturalization. The citizenship law approved in 1999, which came into force at the beginning of 2000, establishes a minimum residency requirement of 8 years. However, it imposes certain requirements for naturalization: allegiance to the German Constitution, ability to support oneself and one's family without social security or unemployment benefits, a clean criminal record, renunciation of previous citizenship, and adequate command of the German language (tested through an exam).

On the one hand, the seemingly more lenient residency requirements might encourage higher levels of integration among immigrants; on the other hand, the response of immigrants to the additional requirements introduced by the law might have determined increased frequency of interactions with German individuals, and increased interest in learning more about German culture and language, in order especially to meet the new language requirements for naturalization. This might bias our results if the number of respondents in the treated group that plan to apply for naturalization is larger than the number in the control group. Using information available in the GSOEP, in results not reported for lack of space, we find no evidence that in the pre-reform period the treatment and control groups differ in the willingness to become German citizens.

As a further check of whether the results of our analysis capture the effects of other of the reform's provisions, we restrict our sample to respondents who in 2000 had been resident in Germany for 15 years or more. Individuals in this restricted sample are not affected by the changes in the residency requirements, while how they are influenced by the additional conditions does not depend on whether they are in the treatment or the control group. The results for this restricted sample are presented in Table 5: the coefficients of our variables of interest remain positive, of similar size, and significantly different from zero. These estimates also suggest that the difference between the treatment and control group in the number of years of permanence in Germany is unlikely to drive our main results.

Taken together, these results suggest that increased use of the German language and interactions with Germans, among individuals in the treatment group, are not due to changes in the naturalization criteria.

## 5 Heterogeneous Effects

In this section we analyze whether the reform has heterogeneous effects, by testing whether individuals with different language backgrounds and different levels of education respond



differentially to the reform.

We explore first whether the effect of the reform depends on the similarities between the language spoken in the country of origin of the respondent and the German language. Since German belongs to the family of Indo European languages we split the original sample into two subsamples depending on whether or not the language of origin of the respondent is an Indo European language. Pre-reform, immigrants from countries with Indo European background were more likely to use German: for those with an Indo European background the averages of the *German Spoken* and *German Newspapers* variables are 0.702 and 2.901 respectively, as opposed to 0.633 and 2.42 for those without. Results in the top panel of Table 6 obtained by estimating the baseline specification on the two subsamples, show that the use of German language increases as a result of the reform only among respondents whose language of origin belongs to the Indo European family. The effect of the reform on the propensity to have contacts with Germans instead does not vary with the subsample used.

It is worth noticing that Turkish immigrants, that represent the largest ethnic minority in Germany, belong to the not Indo European subsample. There is large evidence that shows a persistent lack of integration of the Turkish community with the native population. According to Mueller (2006), the rate of exogamy among Turks living in Germany is impressively low. In 1995, 98% of married Turkish women and 95% of all married Turkish men had partners of the same nationality.<sup>33</sup> Marginalization seems to affect younger generations as well. *Der Spiegel* (24<sup>th</sup> February 2002) reports that four out of five first graders have no knowledge at all of the German language. Our results can be interpreted as evidence that language and cultural distance can contribute to explain the limited effect of integration policies on the cultural assimilation of the Turkish community.

As a second step, we study whether the levels of education shape the influence of the reform on the level and the nature of integration of immigrants. We split the sample into two subsamples according to number of years of education: 9 or fewer years of education, and more than 9 years of education.<sup>34</sup> When we estimate eq. (1) using each of the two subsamples, we find that different levels of human capital promote different patterns of integration as a result of the reform: less well educated respondents show an increased level

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<sup>33</sup>The Berlin Institute for Population and Development classifies migrants with a Turkish background as the least integrated ethnic group when using the IMI (“Integration Measurement Index”), a complex index based on 20 different indicators that are supposed to capture the performance of each group in terms assimilation, social security, education and labor outcomes.

<sup>34</sup>For most of the immigrants in the sample 9 years represent the minimum requirement to complete the lower level of secondary education in their country of origin.

of interaction with the local community while the better educated respondents show a greater level of integration with the local culture (see bottom panel in Table 6). This finding is in line with previous evidence. Chiswick (2008) suggests that education might increase the efficiency of acquisition of the second country language, and other things being equal, those with more schooling are more proficient at the second language. This might be because those with higher levels of schooling are more efficient learners, either inherently (due to their higher abilities) or because they acquire learning skills in school.

## 6 Conclusions

This paper contributes to the debate on the effects of migration policy, focusing on the levels of integration of immigrants. There is surprisingly no evidence on whether policies, and in particular citizenship policies, affect immigrant integration. We study how the introduction of elements of the birthright system in Germany had an effect on the acquisition by immigrants of the German language and on their social networks. Our results show that the introduction of the *jus soli* system determines a significant increase in the integration of the adults. Parents of children affected by the reform are more likely to read German newspapers and have social interactions with native Germans. The information available on the respondents in our samples, however, does not allow us to identify the exact mechanisms behind our findings.

Although it is not obvious that our results can be extended to countries with different levels and composition of migration flows, we believe that our findings might help explain why some countries are more successful than others in assimilating immigrants into their cultures and habits. By discussing the causal link between citizenship and integration of immigrants, our study tries to provide some guidance on the instruments and frameworks that should be adopted in order to deal with the increased levels of diversity in Western societies.

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## Appendix: Take-Up of the Transitional Component

In this section we calculate the take up rate of the provisional component of the reform, as defined as the fraction of eligible children who became citizens. In order to measure take up we need information on the number of naturalizations due to the transitional provision in the law, and the number of individuals eligible for it. There are no official statistics on either the number of applications or the number of potential applicants. For this reason we estimate the take up rate using (i) data from the German Statistical Office on the number of individuals born to foreign married couples in the interval 1990 to 1999 and the number of naturalizations by age, (ii) existing estimates on the number of illegal immigrants in Germany from various sources, (iii) comparisons between adult naturalization rates in the sample we use and in the overall immigrant population in Germany (OECD source), and (iv) yearly estimates of the proportion of children born to couples resident in Germany for at least eight years at the time of their birth, from the GSOEP.

When computing the number of the parents potentially able to exploit the one- year time window for application, we need to take into account that some parents may have been naturalized (thus securing naturalization of their children) or decided to return to their home countries.

First, we construct a measure for the number of naturalizations attributable to the transitional component in the German nationality law. The German Statistical Office provides data on the number of naturalizations by year and age group, enabling us to recover the total number of naturalizations of individuals born between 1990 and 1999 (the treated group) and of individuals born between 1980 and 1989 (the control group). We use  $N_{T,x}$  and  $N_{C,x}$  to denote the total number of naturalizations in a given year  $x$  for the treatment and control groups, respectively. Once an application for citizenship has been filed, registration can take one to three years. We therefore assume that the number of naturalizations between 2000 and 2002 can be affected by the transitional component and we calculate the number of naturalizations due to the transitional component,  $N_{trans.}$  using the following simple equation:

$$N_{trans.} = (N_{T,2002} - N_{C,2002}) + (N_{T,2001} - N_{C,2001}) + (N_{T,2000} - N_{C,2000}) - 3(N_{T,1999} - N_{C,1999}). \quad (2)$$

We assume that in the absence of the transitional component, for each year between 2000 and 2002 the difference in the number of naturalizations for the treatment and control

groups would be the same as in 1999. Based on the formula in eq. (2), we find that the number of naturalizations due to the provisional component is slightly over 52,000.

Next, we estimate the number of individuals eligible for the provisional component. Estimating the number who can apply is complicated and requires additional assumptions. The German Statistical Office provides data on the number of total births to foreign national parents from 1980 onwards, and starting from 1991 births are classified according to whether parents are married or not. When registering their children, only married couples are obliged to declare citizenship. We impute the total number of births to foreign married couples in 1990 assuming that they represent the same proportion of total births to foreign national individuals as in 1991. See Table A1 column (1) for the number of total births to foreign national married couples for the period covered by the transitional component. We use data from the GSOEP to construct the proportion of births to foreign national individuals resident in Germany for at least 8 years. The estimated proportions are reported in Table A1 column (2). Multiplying the total number of births by the estimated proportions provides estimates of the number of births in the treatment group.

Children born to foreign couples between 1990 and 1999 might not take advantage of the provisional component for two reasons. First, their parents might have decided to leave Germany and return to their home country. Second, their parents might have obtained German citizenship before or during 2000 and applied for citizenship for their children (in this case application is a formality). We compute the number of potential applicants under two alternative scenarios for the attrition due to return migration and parents' naturalization. The OECD (2008) provides estimates of return migration with five years, for a set of countries that does not include Germany, as ranging between 28% (Netherlands) and 60% (Ireland). In the first scenario we set the sum of return migration and naturalization equal to 10%, in the second scenario we set the sum to be the same as the total yearly attrition rate discussed in 4.2, that is 16%. Table A1 shows that for first scenario estimated take-up is around 23%, and for the second one it is 30%.

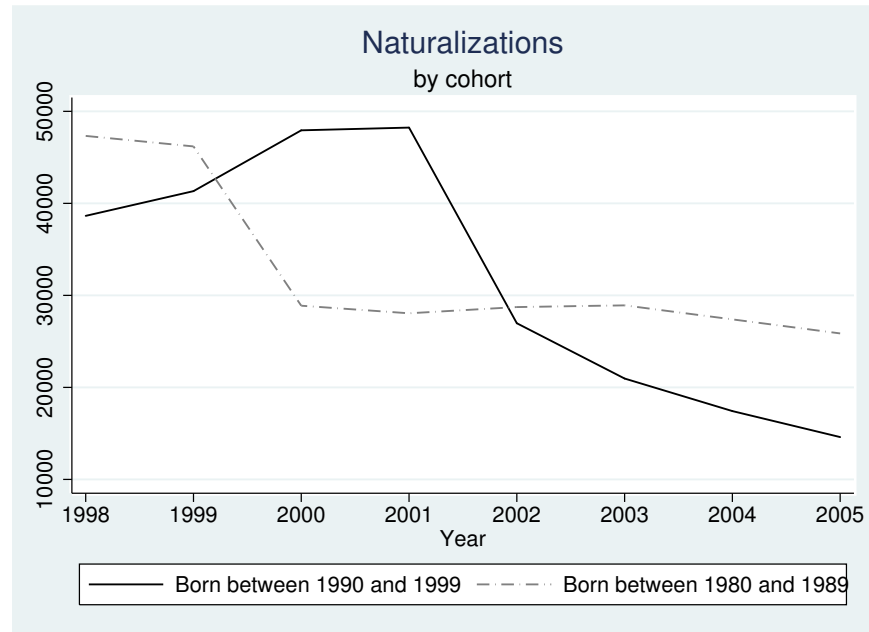
The GSOEP does not survey illegal immigrants, who also are excluded from our estimation sample. The number of births in column (1) Table A1 includes children born to both legal and illegal immigrants. However, illegal immigrants are not entitled to take advantage of the provisional component of the reform, because it applies only to legal residents. Schneider (2003) estimates that there were 1.2 million illegal immigrants in Germany in 2001. Since the total number of foreign nationals in Germany according to OECD estimates is 7.3 million, adjusting for the fraction of illegal immigrants, the estimated take-up rates under the two alternative scenarios is approximately 27% and 36%.



A final reason why the take up rate for our sample might be substantially higher than for the overall population is that respondents to the GSOEP, with a foreign background are more likely to be German citizens than individuals resident in Germany but born elsewhere. According to OECD estimates, in 2007, when we consider only individuals aged 15 to 64 who are not in education and who have been legally resident in the host country for ten years or more, the fraction of immigrants who are Germans through naturalization is around 37%. Exploiting data from the 2006 GSOEP survey and assuming a naturalization rate of 2% in 2007, we estimate that, in a sample with the same characteristics, the fraction of naturalized foreigners in the GSOEP is around 49%, which is substantially higher.

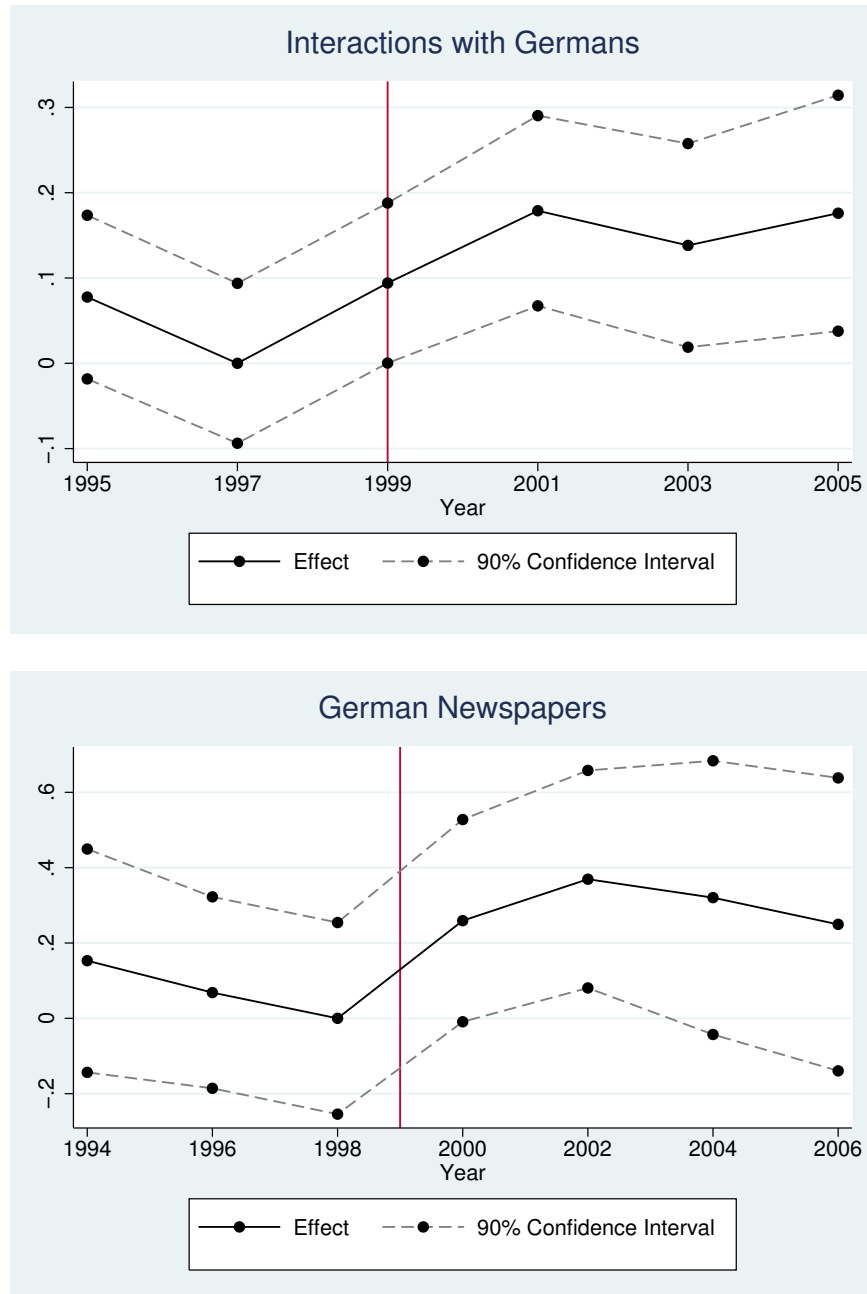
Under the assumption that the fractions of naturalized foreigners in Germany and in the GSOEP have experienced the same rate of growth between 1999 and 2007, the ratio of the fraction of non-naturalized immigrants in the overall population as surveyed in 2007, to the fraction of non-naturalized immigrants in the GSOEP in the same year, provides a measure of what extent the number of potential applicants for the provisional component in the overall population outreaches the one in the GSOEP. Based on the numbers discussed above, we find that the ratio is equal to 1.23. This implies that the take-up rate in the sample used for our empirical analysis varies in the range 34% to 46%. The underlying assumption in the last part of this accounting exercise is that in the GSOEP sample the increase in the proportion of naturalizations of children born between 1990 and 1999 due to the provisional component is the same as in the overall foreign population. Lack of information on the legal status of children in the GSOEP does not allow us to remove this assumption.

Figure 1: The effect of the transitional component on the number of naturalizations



**Note:** The graph plots the number of naturalizations for each year in the time period between 1998 and 2005. Data from the German Statistical Office.

Figure 2: Time Varying Effects



**Note:** The graphs plot the year specific treatment effects, obtained interacting the treatment dummy with unconstrained time effects in the specification presented in eq. 1. For each outcome, the survey year prior to approval of the reform is chosen as reference year and the treatment effect is set to be equal to zero for that year.

Table 1: Individual Characteristics: Descriptives

|                    | Treatment<br>Group<br>(1) | Control<br>Group<br>(2) | Diff<br>(3)             | Diff<br>with Age F.E<br>(4) |
|--------------------|---------------------------|-------------------------|-------------------------|-----------------------------|
| Age                | 34.669<br>(5.089)         | 41.547<br>(4.932)       | -6.878***<br>(0.625)    |                             |
| Male               | 0.452<br>(0.499)          | 0.358<br>(0.482)        | 0.093<br>(0.061)        | 0.098<br>(0.078)            |
| Married            | 0.958<br>(0.202)          | 0.896<br>(0.306)        | 0.062*<br>(0.035)       | 0.074<br>(0.045)            |
| Head Household     | 0.512<br>(0.501)          | 0.434<br>(0.498)        | 0.078<br>(0.062)        | 0.027<br>(0.077)            |
| Years of Education | 9.681<br>(1.633)          | 9.292<br>(1.986)        | 0.388*<br>(0.221)       | -0.027<br>(0.274)           |
| Number of Children | 2.464<br>(1.110)          | 2.604<br>(1.057)        | -0.140<br>(0.170)       | 0.365<br>(0.236)            |
| HH Monthly Income  | 2212.381<br>(801.644)     | 2480.400<br>(905.477)   | -268.020**<br>(132.392) | -7.529<br>(181.624)         |
| Years in Germany   | 20.747<br>(7.602)         | 21.943<br>(7.599)       | -1.196<br>(0.945)       | 1.216<br>(1.123)            |
| Observations       | 166                       | 106                     |                         |                             |

**Note:** Sample characteristics as reported in the 1999 wave. The Treatment group includes all foreign born individuals who had resided in Germany for at least 8 years when the youngest child was born between the 1<sup>st</sup> of January 1990 and the 31<sup>st</sup> of December 1999. The Control group includes those who had resided for less than 8 years when the youngest child was born between 1990 and 1999 and those whose youngest child was born between 1980 and 1989. Household monthly income is expressed in Euros.

Table 2: Integration Outcomes: Descriptives

|                           | Treatment Group  |                  |                  | Control Group    |                  |                  |
|---------------------------|------------------|------------------|------------------|------------------|------------------|------------------|
|                           | Before<br>(1)    | After<br>(2)     | Total<br>(3)     | Before<br>(4)    | After<br>(5)     | Total<br>(6)     |
| Interactions with Germans | 0.71<br>(0.426)  | 0.768<br>(0.386) | 0.739<br>(0.407) | 0.797<br>(0.375) | 0.792<br>(0.380) | 0.794<br>(0.377) |
| German Spoken             | 0.645<br>(0.479) | 0.693<br>(0.462) | 0.67<br>(0.471)  | 0.699<br>(0.459) | 0.706<br>(0.456) | 0.703<br>(0.457) |
| German Newspapers         | 2.63<br>(1.211)  | 2.912<br>(1.310) | 2.786<br>(1.274) | 2.724<br>(1.241) | 2.863<br>(1.246) | 2.798<br>(1.245) |

**Note:** The variable *Interactions with Germans* takes value 0 for no interactions, 0.5 if the respondent has either visited Germans at home or received the visit of Germans at home in the last 12 months, and value 1 if the respondent has both visited and been visited. The variable *German spoken* takes the value 1 if the individual speaks either both or mostly German, 0 if the individual mostly uses his/her mother tongue language. The variable *German newspapers* varies over the range 1-5 and takes the value 1 if the individual only reads newspapers from the country of origin or does not read any newspapers, and takes the value 5 for reading only German newspapers.

Table 3: Baseline Results

|                       | Interactions with |                   |                   |                   | German            |                   | German            |                   |                   |
|-----------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|
|                       | (1)               | (2)               | (3)               | (4)               | (5)               | (6)               | (7)               | (8)               | (9)               |
|                       | Germans           |                   |                   |                   | Spoken            | Newspapers        |                   |                   |                   |
| Treatment Group       | -0.065<br>(0.049) | -0.056<br>(0.052) | 0.007<br>(0.077)  | -0.066<br>(0.069) | -0.047<br>(0.047) | -0.013<br>(0.072) | -0.022<br>(0.113) | 0.017<br>(0.120)  |                   |
| After                 | 0.007<br>(0.063)  | 0.003<br>(0.067)  | 0.116*<br>(0.055) | 0.068+<br>(0.041) | -0.113<br>(0.073) | 0.053<br>(0.044)  | -0.180<br>(0.163) | 0.049<br>(0.157)  | 0.378<br>(0.303)  |
| Treatment Group*After | 0.120*<br>(0.048) | 0.142*<br>(0.066) |                   |                   | 0.029<br>(0.045)  |                   | 0.285*<br>(0.116) | 0.229+<br>(0.127) | 0.301*<br>(0.150) |
| Household FE          |                   |                   | x                 |                   |                   | x                 |                   |                   | x                 |
| Age Dummies           |                   | x                 | x                 |                   | x                 | x                 |                   | x                 | x                 |
| Observations          | 1774              | 1774              | 1774              | 2475              | 2475              | 2475              | 2099              | 2099              | 2099              |
| Numb. of cluster      | 337               | 337               | 337               | 332               | 332               | 332               | 362               | 362               | 362               |
| R <sup>2</sup>        | .103              | .117              | .563              | .141              | .155              | .448              | .255              | .267              | .561              |

**Note:** In columns (1), (4) and (7) the respondent's age is introduced linearly; in the other specifications it is included using single year dummies. All the specifications control for gender, years of education, household head status, number of children, the number of years spent in Germany, marital status, survey year dummies and country of origin dummies. Robust standard errors are clustered at the household level.  
+ P < .10; \* P < .05; \*\* P < .01.

Table 4: Falsification Tests

|   | Interactions<br>with Germans<br>(1) | German<br>Spoken<br>(2) | German<br>Newspapers<br>(3)   |
|---|-------------------------------------|-------------------------|-------------------------------|
| <i>Panel A: PLACEBO I</i>                   |                                     |                         |                               |
| Treatment Group*After                       | -0.066<br>(0.059)                   | 0.009<br>(0.058)        | -0.08<br>(0.156)              |
| Observations                                | 659                                 | 985                     | 974                           |
| <i>Panel B: AGE OF OLDEST CHILD DUMMIES</i> |                                     |                         |                               |
| Treatment Group*After                       | 0.101 <sup>+</sup><br>(0.058)       | 0.027<br>(0.047)        | 0.229 <sup>+</sup><br>(0.133) |
| Observations                                | 1744                                | 2475                    | 2099                          |
| <i>Panel C: PLACEBO II</i>                  |                                     |                         |                               |
| Treatment Group*After                       | -0.094<br>(0.062)                   | 0.046<br>(0.060)        | 0.015<br>(0.177)              |
| Observations                                | 626                                 | 918                     | 912                           |

**Note:** In Panels A and C the placebo reform is set in 1997. Additional regressors include single year (respondent's) age dummies, gender, years of education, household head status, number of children, the number of years spent in Germany, marital status, survey year dummies and country of origin dummies. Robust standard errors are clustered at the household level.

<sup>+</sup> P < .10; \* P < .05; \*\* P < .01.

Table 5: Other Components of the Reform

|                       | Interactions<br>with Germans<br>(1) | German<br>Spoken<br>(2) | German<br>Newspapers<br>(3) |
|-----------------------|-------------------------------------|-------------------------|-----------------------------|
| Treatment Group       | -0.044<br>(0.060)                   | -0.095*<br>(0.054)      | -0.080<br>(0.131)           |
| After                 | 0.002<br>(0.074)                    | -0.176**<br>(0.084)     | -0.107<br>(0.198)           |
| Treatment Group*After | 0.109 <sup>+</sup><br>(0.062)       | 0.061<br>(0.052)        | 0.386*<br>(0.150)           |
| Observations          | 1356                                | 1811                    | 1578                        |
| Numb. of clusters     | 276                                 | 265                     | 301                         |
| R <sup>2</sup>        | .111                                | .121                    | .268                        |

**Note:** The sample is restricted to foreign born individuals who have been in Germany for more than 15 years. Additional regressors include single year (respondent's) age dummies, gender, years of education, household head status, number of children, the number of years spent in Germany, marital status, survey year dummies and country of origin dummies. Robust standard errors are clustered at the household level.

<sup>+</sup> P < .10; \* P < .05; \*\* P < .01.



Table 6: Treatment Heterogeneity

|                           | Interactions with Germans |                  | German Spoken     |                  | German Newspapers |                   |
|---------------------------|---------------------------|------------------|-------------------|------------------|-------------------|-------------------|
|                           | (1)                       | (2)              | (3)               | (4)              | (5)               | (6)               |
| <i>PANEL A: LANGUAGE</i>  |                           |                  |                   |                  |                   |                   |
|                           | Non-Indo EU               | Indo EU          | Non-Indo EU       | Indo EU          | Non-Indo EU       | Indo EU           |
| Treatment Group*After     | 0.111<br>(0.085)          | 0.103<br>(0.077) | -0.065<br>(0.061) | 0.088<br>(0.062) | -0.034<br>(0.168) | 0.439*<br>(0.193) |
| Observations              | 818                       | 956              | 1133              | 1342             | 977               | 1122              |
| Numb. of clusters         | 141                       | 198              | 138               | 196              | 150               | 215               |
| R <sup>2</sup>            | .107                      | .208             | .175              | .201             | .212              | .295              |
| <i>PANEL B: EDUCATION</i> |                           |                  |                   |                  |                   |                   |
|                           | Low                       | High             | Low               | High             | Low               | High              |
| Treatment Group*After     | 0.166*<br>(0.073)         | 0.022<br>(0.068) | 0.006<br>(0.063)  | 0.082<br>(0.066) | 0.090<br>(0.165)  | 0.388*<br>(0.176) |
| Observations              | 1035                      | 739              | 1415              | 1060             | 1214              | 885               |
| Numb. of clusters         | 247                       | 205              | 242               | 209              | 269               | 223               |
| R <sup>2</sup>            | .134                      | .172             | .138              | .212             | .252              | .301              |

**Note:** Individual are classified as Low Educated if they have completed 9 or less years of formal schooling. They are classified as High Educated if they have completed more than 9 years of formal schooling. Additional regressors include single year (respondent's) age dummies, gender, years of education, household head status, number of children, the number of years spent in Germany, marital status, survey year dummies and country of origin dummies. Robust standard errors are clustered at the household level.

+ P < .10; \* P < .05; \*\* P < .01.

Table A1: Estimates of the Take-up Rate

| Year  | Births to Foreign<br>Married Couples | Percentage<br>of Births to<br>Imm. with 8 or More Years | Potential<br>Applicants |           |
|---|--------------------------------------|---|-------------------------|-----------|
|   |                                      |   | S1<br>(3)               | S2<br>(4) |
| 1990  | 75962.00                             | 0.3796  | 28835.18                | 28835.175 |
| 1991  | 82189.00                             | 0.5157  | 68336.52                | 66606.414 |
| 1992  | 89979.00                             | 0.3794  | 95640.91                | 90087.421 |
| 1993  | 91397.00                             | 0.348   | 117883                  | 107479.59 |
| 1994  | 89312.00                             | 0.4174  | 143373.5                | 127561.68 |
| 1995  | 88052.00                             | 0.3931  | 163649.4                | 141765.06 |
| 1996  | 93292.00                             | 0.4685  | 190991.8                | 162789.95 |
| 1997  | 93792.00                             | 0.4629  | 215308.9                | 180159.87 |
| 1998  | 86270.00                             | 0.5234  | 238931.7                | 196488.01 |
| 1999  | 79999.00                             | 0.439   | 250158.1                | 200169.49 |
| 2000  |                                      |   | 225142.3                | 168142.37 |
| Estimate Naturalizations transitional comp. |                                      |   | 52060                   |           |
| Take-Up Overall Pop.                        |                                      |   | 0.23                    | 0.31      |
| Take-Up accounting Illegal Imm.             |                                      |   | 0.28                    | 0.37      |
| Take-Up GSOEP Sample                        |                                      |   | 0.34                    | 0.46      |

**Note:** Data on the number of births to foreign married couples and the number of naturalizations for people born in 1990 or after are provided by the German Statistical Office. The percentage of births in the treatment group is estimated using the GSOEP. The number of births to married couples in 1990 is imputed assuming it represents the same proportion as in 1991. In the Scenario S1 r+n is equal to 0.10, in S2 is equal to 0.16.