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**Working Paper**

## Birthright citizenship and parental labor market integration

Freiburger Diskussionspapiere zur Ordnungsökonomik, No. 16/07

**Provided in Cooperation with:**

Institute for Economic Research, University of Freiburg

*Suggested Citation:* Sajons, Christoph (2016) : Birthright citizenship and parental labor market integration, Freiburger Diskussionspapiere zur Ordnungsökonomik, No. 16/07, Albert-Ludwigs-Universität Freiburg, Institut für Allgemeine Wirtschaftsforschung, Abteilung für Wirtschaftspolitik und Ordnungsökonomik, Freiburg i. Br.

This Version is available at:

<https://hdl.handle.net/10419/148070>

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## Birthright Citizenship and Parental Labor Market Integration

Christoph Sajons

16/07

Freiburger **Diskussionspapiere**  
zur Ordnungsökonomik

Freiburg **Discussionpapers**  
on Constitutional Economics

Instituts für allgemeine Wirtschaftsforschung  
**Abteilung Wirtschaftspolitik und  
Ordnungsökonomik**

Albert-Ludwigs-Universität Freiburg



# Birthright citizenship and parental labor market integration \*

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*Abstract:* Do migrant parents change their labor market behavior when their children are born with the citizenship of the host country? In this study, I implement a difference-in-discontinuities approach to examine possible adjustments in employment and working hours following the introduction of birthright citizenship for immigrant children in Germany in 2000. In particular, I compare the changes in labor market outcomes between the parents of migrant children born before and after the enactment date with those of children of mixed couples (migrants and Germans) who were unaffected by the law change. The analysis of data from the Microcensus from 2001 to 2008 suggests that mothers and fathers react differently to having a German-citizen child: While fathers' labor force participation is unaffected, I find mothers to be more likely to stay at home. By contrast, there seems to be no effect on the number of hours in the job.

(147 words)

JEL classification: J15, J22, J61

Keywords: Birthright citizenship, Integration, Naturalization, Labor force participation

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\* This research was supported by the Catalan Agència de Gestió d'Ajuts Universitaris i de Recerca (AGAUR). I would further like to thank the Institute for the Study of Labor (IZA) for the data services of the IDSC, and seminar participants at the Universitat Pompeu Fabra, the Berlin Network of Labor Market Economists (BeNA), the University of Lüneburg, AEDE 2011 in Barcelona, EALE 2012 in Bonn, ESPE 2016 in Berlin, Antonio Ciccone, Thijs van Rens, Irma Clots-Figueras, Francesco Fasani, Ronny Freier, Max Steinhardt, and especially Libertad Gonzalez for many helpful comments and suggestions.

## 1. Introduction

Most developed countries experience increasing shares of immigrants and their descendants in their population, often accompanied by the typical problems of integration like lower education and higher unemployment (e.g., Lazear, 1999; Algan et al., 2010). These problems frequently spark heated debates about the size and value of immigration and the best way to integrate foreigners, especially in election times or when a foreign crisis leads to large migration flows as can be experienced in Europe right now. In this controversy, the argument about the importance of facilitating the access to citizenship plays a prominent role. And while there is a large amount of studies suggesting that citizenship improves the economic and social integration of migrants (among others Chiswick, 1978; Bratsberg et al., 2002; Scott, 2008; Bratsberg and Raaum, 2011; Steinhardt, 2012; Gathmann and Keller, 2014; Keller et al., 2015), proposals to reduce requirements or to allow for double citizenships often meet stiff political opposition.

A policy option that seems more acceptable to many people is to grant automatic citizenship to the children of legal immigrants who are born and grow up in the host country.<sup>1</sup> Recent research has shown that this is not only beneficial for the children themselves (Sajons and Clots-Figueras, 2014; Felfe et al., 2016), but also for the integration of their parents. In particular, having a child with the host-country citizenship leads to better social integration (Avitabile et al., 2013) and a lower propensity to leave the guest country again (Sajons, 2016). It is unclear, however, whether these positive effects also spill-over to the parents' labor market integration and could thus produce an economic "double dividend" of benefitting both the children and their parents. This could happen if better language proficiency and more frequent contact with natives (Avitabile et al., 2013) improve the migrants' labor market position. On the other hand, the effect could also go the other way if parents invest more time in the upbringing of their children (Avitabile et al., 2014) or change their expectations about the duration of stay and reduce their work effort (Dustmann and Görlach, 2016).

The present paper aims at examining the potential effect of birthright citizenship for a child on parental labor market integration. This analysis is complicated because children typically cannot decide to naturalize on their own. To the contrary, they usually obtain the host-country citizenship only when their parents decide to do so and apply for the whole family simultaneously. Thus, the more parents are willing to integrate into the host country society and the more they gain from that step, the more likely we will find their children as citizens of that country. Technically speaking, the treatment variable "citizenship of the child" is therefore not exogenous and its coefficient likely upward biased.

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<sup>1</sup> This is true even for the US (see Condon, 2010), where many lawmakers and even presidential candidates want to restrict birthright citizenship for the children of illegal migrants, fearing that it may attract the illegal immigration of soon-to-be parents, as parents of under-age citizens are usually exempted from deportation (see, for instance, Kahn, 2010; and Trump, 2015).

In this paper, I circumvent this problem by focusing on the effect of *automatically* obtaining the host-country citizenship for one's child at birth on the parents. To do so, I exploit the introduction of birthright citizenship for the children of immigrants in Germany at the beginning of 2000 as exogenous source of variation in the citizenship status of immigrant children. This effectively removes the influence of parental self-selection, as the treatment does not depend on the parents' intentions to integrate in the host country anymore.

For the evaluation, I examine pooled data from the German Microcensus covering the years 2001 to 2008. Since the automatic receipt of citizenship is typically not asked for and citizenship itself may be reported with some measurement error (e.g., only stating the main nationality but not a potential second), I use the well-defined conditions under which newborn children automatically obtain the German nationality to estimate consistent intent-to-treat (*ITT*) effects of birthright citizenship for children on their parents' integration behavior. To avoid interference from time trends or macroeconomic shocks, I focus on migrant parents with children born within three years around the enactment of the reform, i.e., between 1997 and 2003, and estimate the difference in outcomes at the threshold. Furthermore, to control for possible age or composition effects, I use "mixed" families as an additional control group, i.e., families with children born in the same time period, but with a German and a foreign-citizen as parents. This group is similar in terms of migration background, but the introduction of birthright citizenship in 2000 did not change anything in the legal status of their children, as they got the German citizenship at birth already before the reform due to their German ancestry. Thus, I can apply a difference-in-discontinuities design by estimating a "treatment" effect at the cutoff for them as well and subtracting it from the estimated impact of the reform on the treatment group.

In the analysis, I distinguish between the reactions of fathers and mothers, as they tend to adjust their labor market behavior differently after the birth of a child and depending on its characteristics (e.g., Sanchez and Thomson, 1997; Angrist and Evans, 1998). The focus lies on current employment and the number of hours worked in the week before the interview, in order to provide insights on changes along both the extensive and intensive margin of labor supply. Given the attention these measures typically attract, it is surprising that they have not been studied in the context of birthright citizenship for immigrant children so far. Previous studies by Avitabile et al. (2013, 2014) and Sajons (2016) on the consequences of birthright citizenship for children on their parents examine other outcomes like the social integration of the parents, their fertility, and family outmigration, respectively, but abstain from looking at the important issue of labor market integration. To the best of my knowledge, the present paper therefore provides the first insights into this topic.

The results point towards three main findings: (1) Getting a child with the citizenship of the host country does not seem to affect the employment rate of the fathers, but reduces the labor market attachment of the mothers. This is in line with existing research showing that the female partner in a

couple reacts stronger to the birth of a child and its characteristics than the male one (e.g., Salkever, 1982; Corman et al, 2005). It also suggests that birthright citizenship may affect the behavior of parents differently across integration dimension, as previous studies found only positive effects of this measure on other important outcomes (in particular, reduced outmigration, better social integration, and more investment into the children; see Sajons, 2016, and Avitabile et al., 2013 and 2014, respectively). (2) There is no impact on the number of hours worked in a week. (3) The result for the employment of mothers does not seem to be driven by self-selection into outmigration, nor differences in the probability to naturalize oneself. This supports the interpretation that the reported effects in this paper reflect behavioral adjustments and not mere compositional changes.

The rest of the paper proceeds as follows: Section 2 provides the details of the reform of the German Citizenship Law in 1999. Section 3 relates the topic to existing research, followed by theoretical considerations on why and how citizenship for the child may affect parental labor market integration behavior in section 4. A closer description of the identification strategy is given in section 5. Section 6 introduces the data used in the empirical analysis and section 7 presents the results, as well as some robustness checks and considerations about possible channels of influence. Finally, section 8 summarizes the findings, discusses their limitations and political implications, and points out directions for further research.

## **2. The introduction of birthright citizenship in Germany**

At the end of the 1990s, about 7.3 million individuals in Germany or about 9% of the country's total population were foreign citizens with residence permission (Statistisches Bundesamt, 2001). About 40% of them had already stayed in Germany for at least 15 years and over 30% for more than 25 years. One of the main reasons for the existence of such a large long-term foreign-citizen population was the very restrictive citizenship law. For a long time, a person could only become German citizen if one of her ancestors had been German (the principle of *jus sanguini*), independent of how long she had lived in Germany already and how well integrated she was (Brubaker, 1992). Only in 1990, a legal entitlement to naturalization was introduced for foreign citizens who had legally lived in Germany for at least 15 years and renounced their former nationality. Although this represented an important change, the stated conditions were still among the strictest in Western countries and the number of naturalizations remained low for most of the 1990s.

Reforming the German citizenship law was therefore one of the first major initiatives of the newly-elected government of Gerhard Schröder in 1998/99. It aimed at improving the integration of foreign citizens who had been living in Germany for a long time (Coalition Treaty, 1998). The final version of the reform was passed in July, 1999, and came into effect on January, 1<sup>st</sup>, 2000. It contained two main elements: First, a *reduction in the minimum residency requirement* for adults from 15 to 8 years,

and second, the *introduction of birthright citizenship* for newborn children, if at least one of the parents has legally lived in Germany for more than eight years and possesses a permanent residence permission.<sup>2</sup> Under these conditions, children of foreign citizens automatically obtain the German nationality at birth together with the one of their parents. This state of dual citizenship was only permitted up until the age of 23, when the child was supposed to choose between the two nationalities at the latest.<sup>3</sup>

A third, but less prominent component of the reform was a *transition regulation for children* born in the 10 years before the enactment of the law, i.e., between 1990 and 1999. If their parents met the same two conditions at the time of birth as described above, they could apply to get the same treatment for their children as if they were born after the law change. The application period was limited to the calendar year 2000, however.

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Insert figure 1 here  
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The effect of the new law on the number of foreign citizens acquiring the German nationality in the years around its enactment is depicted in figure 1, broken down by component. While the number of naturalizations in the 1990s grew steadily from around 43,000 in 1994 to 114,000 in 1999, it experienced an immediate and large increase to about 166,000 new citizens directly after the reform in 2000. After this initial boost, however, the numbers decreased again and returned to pre-reform levels by 2007. This pattern is the same for the transition regulation, where we can see that the effect is almost exclusively concentrated on the years 2000 and 2001. In total, about 50,000 children obtained the German citizenship through this channel.

Contrary to these short-lived effects, the introduction of automatic birthright citizenship led to about 30-40,000 immigrant children becoming German citizens *every* year, which is about half of all newborn children with foreign-citizen parents. This means that this element of the reform will have a much larger effect on the long-run demographics in Germany than both the adult component and the transition regulation. Therefore, it is important that we examine the outcomes of this particular measure more closely, first on the parents of these children and later on the treated children themselves.

### 3. Literature

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<sup>2</sup> The law also introduced new requirements for naturalization, for instance, sufficient knowledge of the German language, an oath to the German constitution, and the non-receipt of welfare benefits. For more details of the reform, see Avitabile et al. (2013, 2014) and Sajons (2016).

<sup>3</sup> In the meantime, this provision has been changed again already to end an ongoing discussion about its constitutionality. According to the new German citizenship law of 2014, children born with the German citizenship do not have to opt for one nationality if they lived most of their childhood and adolescence in Germany.

This paper closely relates to three branches of research. First and most important, it contributes to a number of recent studies evaluating the different elements and effects of the citizenship reform of 1999 in Germany. Second, it is thus also part of the larger literature discussing the role of naturalization in general for the integration of migrants in their respective host countries. And third, it adds a new facet to the research on how different characteristics or treatments of children affect their parents' behavior, especially with respect to the labor market. In this section, I will primarily focus on the main findings from the first strand and how this paper contributes to it, and only briefly point out the connection to the other two.

### *3.1 The effects of the 1999 citizenship reform in Germany*

As Germany is one of the leading destination countries for migrants in the world and the citizenship reform was hotly contested at the time, it sparked a great deal of interest among researchers. Thus, several studies exist which examine the consequences of the reform as a whole or its individual components. So far, all of them point towards improvements in the integration of the target group in different areas.

With respect to adult migrants, Piracha and Zhu (2012) evaluate the consequences of the new law as a whole for precautionary savings and remittance payments. They use a difference-in-difference approach comparing the migrants with their corresponding native counterparts and show that the reform led to a significant reduction in both outcomes, suggesting that it reduced the uncertainty about future income and legal status. Likewise, Avitabile et al. (2014) provide evidence that the reform reduced the probability to get a child for the affected foreign-citizen couples compared to their German counterparts and “mixed” households, i.e., couples with a foreign and a German citizen. At the same time, they find improvements in several health and non-cognitive outcomes for those children in the target group who were born after the law came into effect. In turn, Gathmann and Keller (2014) examine whether the expansions in the eligibility to naturalize both in 1991 and in 2000 improved the labor market attachment of adult immigrants in Germany. Their results indicate that this seems to be the case for female and more recent migrants, but not for males in general. Finally, Keller et al. (2015) complement this analysis by studying whether eligibility also changes marriage and fertility patterns. They show that simply being entitled for naturalization leads to postponing the first birth and being more likely to choose partners outside someone’s own ethnicity.

Looking more specifically at the introduction of birthright citizenship for newborn children of immigrants, papers by Sajons and Clots-Figueras (2014) and Felfe et al. (2016) show that growing up with the host country nationality improves the educational integration of the affected children. Sajons and Clots-Figueras (2014) use data from the National Education Panel Study (NEPS) to look at the transition from primary to secondary school. The results of a difference-in-difference approach with native German children as comparison group suggest that the eligibility to birthright citizenship increases the probability of migrant children to enter one of the two higher school tracks in the German education system. Felfe et



al. (2016) take a more comprehensive approach at this issue, evaluating the impact of birthright citizenship on the parents' educational decisions for their children from early childhood to adolescence. They show with administrative data that treated migrant parents consistently attempt to achieve better outcomes for their children, starting with being more likely to send their children to pre-school, to enrolling them earlier in school, and finally selecting a higher school track for secondary school if possible.

The two papers most closely related to this study are Avitabile et al. (2013) and Sajons (2016). The former analyzes the impact of the transition regulation for the children born in the 1990s on their parents' social integration. Using data from the German Socio-Economic Panel (GSOEP), the authors compare eligible families with a last child born between 1990 and 1999 and control families whose last child was born in the 1980s. They find positive effects of the option to apply on outcomes such as German proficiency, reading German newspapers, and visiting or hosting German friends. Likewise, Sajons (2016) evaluates the introduction of automatic citizenship for the child on family outmigration decisions by applying a regression discontinuity design on cohort-level data of migrant families. The results suggest that families with children born in the year after the enactment of the reform are more likely to stay in the host country than similar families with children born in the year before.

The current paper contributes to this literature by examining whether *automatic* citizenship for children affects their parents' *labor market integration*. Arguably, this counts to the most important measures for successful integration, but to the best of my knowledge, this is the first study to look at it in this specific context. While Gathmann and Keller (2014) also evaluate labor market outcomes, their focus lies on the adult component of the reform instead of the introduction of birthright citizenship. Avitabile et al. (2013), on the other hand, examine the effect of citizenship for migrant children on their parents, but concentrate on social integration measures. This paper therefore closes an important gap in the literature. Furthermore, by showing negative effects on the mothers' labor force participation, it is one of the first papers to point out that providing citizenship may not have a uniformly positive impact on all indicators of integration at the same time, but could involve certain tradeoffs.

### 3.2 Further related research

With these contributions, the paper also relates to the other two strands of literature mentioned above. First, the research dedicated to evaluating the effect of citizenship on individual integration in general. Most of the focus here lies on labor market integration as well, either in terms of wages (e.g., Chiswick, 1978; Bratsberg et al., 2002; Bratsberg and Raaum, 2011; Steinhardt, 2012) or employment probabilities (e.g., Scott, 2008; Fougère and Safi, 2009). It is very difficult to capture true causal effects in this area, however, since the problem of self-selection into naturalization of those who will profit the most from this action is hard to solve even with panel data. A rare exception is Duguet et al., 2010, who examine the

recruiting process of companies in France with an experimental correspondence test and find that possessing the French citizenship increases the chances of applicants with Moroccan-sounding names.

Additionally, this paper also adds to the literature examining whether and how children's characteristics and circumstances influence their parents' labor market behavior. This direction of a causal effect is less frequently examined than the other way round, but studies exist in different areas. Examples include the effect of becoming parent or having a third child on parental labor supply (Sanchez and Thomson, 1997, and Angrist and Evans, 1998, respectively), but also how different characteristics of the children affect their parents. The latter contains, for instance, the consequences of poor child health on mothers' employment (see the review by Powers, 2003; or Corman et al., 2005) and the sex of the child on fathers' work activities and family return migration decisions (Lundberg and Rose, 2002, and Dustmann, 2003, respectively). They show that it is often not only the fact of having children that matters, but also how these children are, what they need, and what kind of future parents would like to see for them.

#### **4. Theoretical Considerations**

Given these findings in existing research, how do we expect citizenship for the child to influence its parents' integration into the labor market? In order to consider this properly, it is important to know first how birthright citizenship changes the economic perspectives of the affected children. In this section, I therefore start by addressing this issue and then discuss several possibilities for how it may translate into changes in the observed labor market behavior of the parents.

##### *4.1 Economic advantages of the German citizenship*

Obtaining the German citizenship leads to a number of economic advantages for an individual (see also Avitabile et al., 2014). The possibly most important one is gaining *access to all professions*. In the case of Germany, this concerns in particular the possibility to become a public servant, for which either the German or an EU citizenship is required. Many stable and relatively well-paid positions in the public sector are the exclusive or almost exclusive domain of public servants, like judges, attorneys, soldiers, policemen, university teachers, school teachers, etc.<sup>4</sup> Furthermore, there are also benefits of possessing the German citizenship in the private sector, most importantly because it entails the right to *work and live in all other EU countries* and easier *international traveling*. This extends the potential labor market and is of great value for positions in travel-intense sectors like tourism, logistics, controlling, or consulting. Additionally, being a citizen of the host-country may serve as a *positive signal for potential employers* and thus improve the chances to obtain better-paid jobs. This may come both from indicating longer residence in Germany, raising the profitability of investing into the person's human capital (see LaLonde and Topel,

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<sup>4</sup> In recent years, some of these positions have been opened to regular employees as well (e.g., teachers) for which a German passport is not required.

1997), and from lower administrative costs in terms of paperwork when employing a German citizen (see Duguet et al., 2010, for the case of France). And finally, being German may also *reduce possible statistical or taste-based discrimination*, both during the years of education and in the labor market.

Together, the above points may lead to sizable economic advantages for an individual, in particular, if she is comparatively young and can adjust to them (see Steinhardt, 2012, for an analysis for Germany).<sup>5</sup>

#### *4.2 Possible effects on the parents*

Since multiple different factors influence the labor market attachment of migrants, there are many ways in which obtaining a child with the nationality of the host-country could affect its parents. On the one hand, they may feel more attached to that country and start to identify with the native population, which also includes their own child now after all. Additionally, they may perceive the receipt of citizenship for their child as a sign of goodwill and want to reciprocate. Either way, we would expect the parents to undertake greater efforts to assimilate, which should be seen, for instance, in improvements in social integration measures like German language proficiency and the frequency of contacts with natives. This is precisely what Avitabile et al. (2013) find. Eventually, such a behavior should have a positive impact on the parents' labor market position, as language proficiency and having natives in your network are important determinants for job and earnings perspectives (Bertrand et al., 2000; Dustmann and Fabri, 2003; and Gonzalez, 2005).

On the other hand, several mechanisms could also lead to a lower supply of labor by the parents. First, the improved employment and earnings prospects of the child in the future may be considered as a positive shock to the expected lifetime family income. If a share of the parents' motivation to work hard originates in the desire to provide a better future for their child, this may reduce the need to earn more today. Second, parents may want to provide their children with the best means to take full advantage of their better earnings perspectives by investing more in their education early on. This could take different forms like being more willing to send them to daycare centers and kindergartens (Avitabile et al., 2014; Felfe et al., 2016), but also spending more time at home supporting the child in learning and getting in contact with German children. Third, parents could take the greater opportunities of their child in the host country into account when they decide on how long they plan to stay (Djajic, 2008; Sajons, 2016). As shown in Dustmann and Görlach (2016), a large share of migrants usually intends to leave the destination country again to return home or move on to another country. If some of them decide to remain longer or even permanently in Germany for the sake of their offspring, this could significantly change their behavior

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<sup>5</sup> Note that the eligibility to receive public assistance does not depend on the German citizenship, as the system of public assistance in Germany focuses on the place of living rather than nationality, at least for all individuals with permanent residence permission.

on the labor market. In particular, as Dustmann (1997) demonstrates, temporary migrants (especially women) tend to be more attached to the host country's labor market than permanent ones, as they often face worse economic conditions at home and want to profit as much from their limited stay abroad as possible. Thus, if the introduction of birthright citizenship causes parents to change their intentions from temporary to permanent residence in Germany, this could result in weaker labor market integration on average.

To sum up, the theoretical effect of granting automatic citizenship on the labor market behavior of the migrant parents is ambiguous. Moreover, there are additional complications as this step could possibly also influence other dimensions that may interact with observed labor market attachment. The potentially most important ones are selective outmigration and naturalization, as well as changing marriage and fertility patterns. I will discuss these issues and their relevance for the present study in greater detail below.

## 5. Identification Strategy

### 5.1 Main idea

For the empirical analysis, I apply a difference-in-discontinuities design to identify the effect of birthright citizenship for immigrant children on their parents' labor market integration. That is, I first look at the differences in current employment and hours of work between migrant families with children born in the three years directly before and after the enactment of the reform.<sup>6</sup> Then, I compare this with the same difference among the parents of children born around the enactment date within mixed families, i.e., families in which one of the parents is German and the other a foreign national. For both groups, I additionally control for linear trends in the birth year of the children (centered on the year 2000), which are allowed to differ in the pre- and post-reform period.<sup>7</sup> Thus, this strategy effectively identifies the impact of automatic citizenship at the enactment date. To be as comparable as possible, I restrict the sample of mixed families to the outcomes of the migrant parent. Econometrically, this setup can be formalized for children-year observations  $it$  in the following way:

$$\begin{aligned}
Y_{it} = & \beta_0 + \beta_1 \text{After}_{it} + \beta_2 \text{Trend}_{it} + \beta_3 (\text{After} * \text{Trend})_{it} + \\
& + \gamma_1 \text{Elig}_{it} + \gamma_2 (\text{Elig} * \text{After})_{it} + \gamma_3 (\text{Elig} * \text{Trend})_{it} + \\
& + \gamma_4 (\text{Elig} * \text{After} * \text{Trend})_{it} + \delta \mathbf{X}_{it} + \tau_t + v_{it}
\end{aligned} \tag{2}$$

Here,  $Y$  represents the two labor market related outcomes of the parents, current employment and hours of

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<sup>6</sup> Families can be included in both groups if they have children born before and after the enactment of the new law. This means that I allow for potential spillover effects of the treatment towards the comparison group, which could bias the results towards zero.

<sup>7</sup> This follows Lee and Lemieux (2010), as it avoids estimating the trend on one side of the cutoff using values from the other.

work. *After* is an indicator equal to 1 if the birth of the child happened in the years 2000 to 2002 and 0 if it was born between 1997 and 1999. *Trend* contains the year of birth of the child, centered on the year 2000, and *Elig* (eligible) is a dummy variable indicating that a child belongs to the group of migrant families. Additionally, I condition on a vector of personal, regional, and household characteristics  $X$  and include indicators for the different years of the Microcensus,  $\tau$ . The coefficient of interest in this model is  $\gamma_2$ , describing the average change in the outcome at the threshold for parents in all-migrant couples relative to migrant parents in mixed couples.

With this approach, I aim at disentangling the real effect from a number of potentially confounding factors: (1) The *adult component of the law*, i.e., the reduction in the minimum residence requirement from 15 to 8 years for adult immigrants. This provision in the reform means the same main eligibility condition is set for both the individual naturalization of the parents and birthright citizenship for the children. Hence, any attempt to identify the effect of granting citizenship to immigrant children by comparing eligible migrant families with ineligible ones or native families would fail, as the results would reflect the joint impact of both components. In the present paper, I solve this problem by restricting the sample to foreign-citizen parents who are themselves entitled for naturalization at the time of birth of the child, i.e., who came to Germany more than eight years before the birth of the child or were already born there. That way, they are equally affected by the adult component, but differ with respect to the introduction of automatic birthright citizenship for their children.

(2) *Differences in the integration behavior* of migrants who get their children at different points in time. This could be due to changing labor market conditions in Germany over time, shifting attitudes toward foreigners among the native population, or an evolving propensity towards integration among the migrant parents. If this is the case, any difference in outcome variables we may find between the families of children born before and after the reform could simply reflect the underlying trend rather than the treatment effect of introducing birthright citizenship. I account for this possibility in two ways: On the one hand, by including a linear trend in birth-year cohort (*Trend*), which should pick up any continuous development in the above mentioned aspects as well as the systematic differences in age of the affected children. On the other hand, I additionally control for possible non-linear influences that may have affected migrants in that period by comparing the behavior of parents in pure migrant families with that of migrant parents in mixed partnerships.

(3) The *potential endogeneity* of the reported German citizenship status of the child. As the parents' underlying willingness to integration is not observed, it is possible that both their own integration outcomes and whether they report the German citizenship for their children are positively correlated. This could be due to a larger willingness to naturalize the whole family or to apply for the German citizenship only for the child under the transition regulation of the law. Furthermore, it could also reflect a greater accuracy in reporting a German citizenship of the child. Hence, any estimate of the effect that simply uses

the reported “treatment” (i.e., the German citizenship status of the child) will obtain biased results in favor of finding a more positive impact of host-country citizenship for the children. In the analysis, I address this likely endogeneity by using the combination of being eligible for birthright citizenship and being born after the enactment date (the interaction *Elig\*After*) instead of reported German citizenship as the main explanatory variable of interest. The estimated coefficient can therefore be considered as a reduced form “intent to treat” effect.

## 5.2 Validity

This identification strategy needs to fulfill the following four conditions in order to produce valid results: First, there has to exist a noticeable difference in the treatment intensity around the cutoff, i.e., there has to be a significant effect of the reform on the possession of German citizenship among newborn children in migrant families after 1999. Figure 2 shows that this is what we find in the official data from the German Statistical Office. It depicts the total number of births to foreign-citizen parents in Germany (the black line) from 1990 to 2007 and how many of them eventually obtained the German citizenship through the transition rule of the law (the dotted grey line) or automatic birthright citizenship (the solid grey line). Here, we can see the large and discontinuous change in treatment intensity around the enactment date (the vertical line). While only 7.1% of newborn children of foreign-citizen parents in 1999 obtained the German nationality retrospectively on application, this share rose to 45.6% of them automatically becoming Germans at birth in the cohort of 2000 (Statistisches Bundesamt, 2010).<sup>8</sup>

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Insert figure 2 here

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Second, parents do not manipulate the birth date of their children around the enactment date or systematically change their fertility behavior afterwards. Otherwise, those families with the highest expected returns from having a German citizen child would all get their children in 2000, whereas any family who, for some reason, does not want the German citizenship for their child would deliver in 1999. In such a situation, the difference in integration outcomes between the two groups would not reflect the causal effect of introducing birthright citizenship, but simply the difference in the underlying motivation to integrate. In our case, however, there are several reasons why such a sorting closely around the enactment date seems highly unlikely: (a) Conception itself cannot be controlled with high precision. (b) The first draft of the new law was presented in January, 1999, but met stiff resistance and was subsequently changed several times. The final passage of the law ultimately took place in July, 1999, such

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<sup>8</sup> In my sample, this "first stage" is a bit higher with a difference of about 41 percentage points around the enactment date.

that all births up to March/April, 2000, were already predetermined. Risking a premature birth to advance the date does not seem very likely in this case either, as there were other possibilities available to circumvent the treatment, for instance, by giving birth outside of Germany. And (c), the incentive to postpone a birth to 2000 was low, since the reform contained the transition rule for immigrant children born between 1990 and 1999, which means that their parents could get them the same treatment easily if they really wanted it.

Looking again at figure 2 provides descriptive support for this argumentation. It shows that the total number of births to foreign-citizen parents in Germany (the black line) increased in the 1990s to a maximum of around 107,000 newborns in 1996 and 1997, but then turned continuously downwards for the following years. Looking at the relevant years around the enactment date of the new law in particular, i.e., at 1999 and 2000, we notice a drop from 95,200 to 91,000 newborn children. This is contrary to what we would expect if foreign-citizen parents had indeed adjusted their fertility behavior to take advantage of the new law. Avitabile et al. (2014) also show that the probability of getting a child did not increase in the wake of the reform. To the contrary, they even find a small reduction starting in 2001.

Third, potentially affected families do not systematically sort themselves into either of the comparison groups. If the reform made those foreign citizens who are better suited to perform well in the labor market more likely to enter relationships with Germans and get children with them, for instance, this could lead to downward biased estimates of the effect of birthright citizenship. This is of concern here, as Keller et al. (2015) show that becoming eligible for naturalization has an impact on marriage patterns. In particular, higher educated migrants get slightly more likely to marry Germans and migrants from another origin than lower educated migrants. For the present analysis, I take this issue into account by limiting the sample to families with children born within the first three years of the reform. As it usually takes some time to find the right partner and get a child, this should largely avoid the risk of including individuals who have already responded to the new law in their choice of spouse. As additional robustness checks, I restrict the sample to two and one year windows around the enactment date to reduce this probability even further.

Last but not least, in order to attribute a difference in integration behavior of families with children born around the enactment date to the introduction of birthright citizenship, there should not be any other institutional change happening at the same time that could affect the parents under consideration differentially. If this was the case, we would not know exactly which part of the estimated effect is due to which component. With respect to the 1<sup>st</sup> of January, 2000, I already discussed in the previous section how I deal with the simultaneous reduction in the minimum residency requirement for the naturalization of adult migrants. Apart from that, the only other change taking place at the same time was a provision of the larger tax reform act of 1999 which lowered the tax rates for all tax payers in Germany in three steps in

1999, 2000, and 2001. As the reduction in tax liabilities was moderate in this step<sup>9</sup> and affected both comparison groups equally, however, it should not influence our estimation of the effect of birthright citizenship.

## 6. Data

The data for the empirical analysis come from the German Microcensus, an annual cross-section of 1% of German households which provides the government with the official statistics of the country's demographic development and labor market situation.<sup>10</sup> There are important advantages of using the Microcensus: To start, its large size of about 270,000 households in the 70% scientific-use files is uniquely suited to get a large enough sample for the narrowly defined target group of foreign-citizen families who lived in Germany for a longer time already and received children around the enactment date. Furthermore, in contrast to other widely used data sets like the German Socio-Economic Panel, parents report the citizenship status of their children in the Microcensus. This allows me to determine whether there is a discontinuous change in the treatment intensity around the enactment date in the sample. And finally, answering most Microcensus questions is mandatory by law, so that the loss of observations due to non-responses to crucial questions is limited.

The period under investigation is the first years after the reform, i.e., from 2001 to 2008. I choose 2001 as the first year of the analysis instead of 2000, because interviews for the Microcensus were always conducted in spring up until 2004, such that there are only very few observations of migrant families with children born after enactment in the Microcensus of 2000. To identify the group of relevant migrant families in each of these years, I impose the following restrictions: (1) The child was born between 1997 and 2002, (2) the country of birth of the child is Germany, (3) both parents were foreign citizens in 2000, i.e., any possible naturalization happened afterwards, (4) at least one parent arrived in Germany prior to 1991 and no parent arrived after 2000.<sup>11</sup> For the mixed families, condition 3 only applies for the migrant parent and condition 4 is not necessary at all, since a child becomes German citizen because of the German parent even if the migrant parent only arrived recently.

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Insert table 1 here  
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Table 1 presents the descriptive statistics for the resulting sample of 21,179 children-year observations. On the left hand side, we see the average characteristics of the group of *eligible* migrant

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<sup>9</sup> The tax rate was reduced from 53% to 51% in the top bracket and from 23.9% to 22.9% in the lowest.

<sup>10</sup> For the empirical analysis, I used the remote processing tool JoSuA developed by the IDSC of IZA (see Askitas, 2008, for details).

<sup>11</sup> Appendix A.1 provides more details about the identification of observations in the Microcensus.



families, separated by whether their child was born before or after the 1<sup>st</sup> of January, 2000. In the middle, the same is reported for the comparison group of mixed families. The two final columns on the right hand side display the coefficient of *Elig\*After* in a simple difference-in-discontinuities regression without any covariates, together with the corresponding p-value. In short, it shows the change in the respective variable at the enactment date in the group of eligible families net of the same change for the group of mixed parents.

From the averages, we can see that there are some important differences between migrants living together with another migrant and those who are in a relationship with a German citizen. In particular, migrant families are less likely to live in East Germany, have on average about 0.3 more children, and are a bit younger and less educated. Additionally, more of them were born in Germany, but their families tend to have come originally from non-EU member countries. Finally, they have also stayed longer in Germany already on average. These differences between the two groups are relatively stable across families with children born pre- and post-enactment, however. As such, the summary statistics confirm the assumption that the two groups are very similar in their descriptive characteristics around the cutoff on a range of observable attributes, including the regional distribution over Germany, the parents' marital status, the age of the children in years, the fraction of male children, and the age of the parents in years. Some statistically significant differences between the respective changes at the enactment date exist, however, especially among the fathers.<sup>12</sup> Here, we see that the increase in the rate of completed secondary education is larger for migrant fathers than for those in mixed families, while it is smaller in the share of fathers being born in Germany. Also, the reduction in years since arrival at the cutoff is larger for migrant fathers, although the averages over the different cohorts seem to suggest the opposite. Additionally, the change in the fraction of children with a reported German citizenship differs by construction by almost 41 percentage points between the two groups around the enactment date, demonstrating the large and discontinuous change in the treatment intensity in the group of eligible migrant families.<sup>13</sup>

Figure 2 also contains raw information on the development of the outcome variables in the two groups, the share of parents who reported to work in the week of the interview and how many hours they had worked in the previous week. These numbers reveal some interesting tentative insights. First, the employment rate is on average lower for migrants with a foreign-citizen partner than for those with a

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<sup>12</sup> Note that the statistical significance here is determined by the wild cluster bootstrap procedure presented in Cameron et al. (2008). This accounts for potential clustering at the cohort-eligibility status level, while at the same time preventing the small number of clusters to cause an over-rejection of the Null hypothesis. See also section 7.1 for more this.

<sup>13</sup> Two reasons can possibly explain why only 64.2% of the children in the assignment group reportedly possess the German citizenship: First, as there is no information on the legal status of the parents in the Microcensus, the restrictions imposed to identify the target group families cannot filter out all the ineligible families. And second, it usually takes several months for the relevant agencies to confirm the citizenship status of the newborn child. As the data for the Microcensus were collected in early April up until 2004, it is likely that many parents had not received the final confirmation yet at the time of the interview in the earlier waves.

German spouse. Among mothers, the difference is roughly 28% to 32.5%, for fathers, it is a bit smaller with 77.5% to 79.3%. Second, mothers with younger children (i.e., those born after the enactment) participate less in the labor force than those with an older offspring by around 7.3 percentage points. At the cutoff, this decrease in the average employment of mothers is significantly larger in migrant families than in mixed ones (by 3.8 percentage points). This indicates that the reform could have lowered the labor force participation of mothers in the target group. Third, migrants in mixed couples report to have worked roughly one hour more during the previous week, conditional on being employed. And fourth, female migrants work mostly half time on average, independent of the citizenship of their partner. This is not surprising, however, as we are looking at a population of young mothers here.

## 7. Results

This section presents the results of the empirical analysis in the following order: At first, we look at the effect of birthright citizenship on the current employment of the parents, i.e., at the extensive margin of labor supply. This is arguably the most important dimension of labor market integration, as the simple fact of "having a job" is a crucial in being able to care for oneself and one's family, and at the same time a key contributor for life satisfaction in general (see, for instance, Grün et al., 2010). After that, we move on to the interior margin and consider the reported hours of work in the week before the interview. Finally, I discuss whether channels other than behavioral adjustments may be driving the results.

### 7.1 Main results

Table 2 reports the results of estimating equation (2) with current employment as outcome and varying covariates for fathers and mothers, respectively. For the sake of brevity, it only includes the estimated coefficient of interest,  $\gamma_2$ , for each specification.<sup>14</sup> Additionally, it states the respective standard errors which are clustered by eligibility status and birth-year cohort of the child in order to allow for correlations between the different individuals of each cohort and migrant group. With only six cohorts and two comparison groups in the sample, however, the number of clusters is very low and thus conventional clustered standard errors will have a tendency to produce statistically significant results. In order to draw correct inference, I therefore also report the respective p-value from a wild cluster bootstrap with 1000 replications, which has been shown to lead to more accurate conclusions in the case of few clusters (see Cameron et al., 2008, for an extensive discussion and simulation results).

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Insert table 2 here  
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<sup>14</sup> The complete tables of estimated coefficients for each outcome are available from the author upon request.

In column 1, the estimates are obtained from a simple difference-in-discontinuities specification without any personal characteristics or fixed effects. Note that the construction of the DiDisc approach should already capture a large part of the individual influence of the year of birth of the child on the parents' integration both by imposing a linear trend and by using mixed families with children of the same cohorts as control group. This is important as the age of a child strongly affects the parents' (in particular, the mothers') possibilities to work, as younger children need more care and attention than older ones. Column 2 adds the vector of controls  $X$ , containing further variables that could influence the employment of parents, including their age (linear and squared), educational attainment, years since arrival, marital status, and regional origin (EU, Turkey, and the rest of the world), as well as whether they are born in Germany themselves, the age of the youngest child in the family (linear and squared), the total number of children in the family, and the quarter of interview. All of these variables are listed and described in table A.1 in the appendix. In order to avoid the loss of observations due to missing values for certain control variables, these are set to 0 or the sample mean in the case of discrete and continuous variables, respectively. Additionally, I include dummy variables indicating each computation. Finally, column 3 further contains indicators for the regional state in Germany and column 4 year fixed-effects.

Starting with the results for the fathers in panel A, we can see that the estimates are consistently positive, but insignificant across the different specifications. Thus, birthright citizenship for migrant children seems to have no effect on the employment of their fathers. The results in panel B, however, show a significant and robust negative effect of citizenship for the child on the current employment of the mothers. As we have seen in the previous section, this impact is estimated to reduce the probability that mothers work by 3.8 percentage points if we do not condition on any covariates. The size of the effect gets smaller with the inclusion of more and more control variables (especially with the age of the youngest child and its square term), but remains at -1.9 percentage points in the most conservative specification (column 4). Relative to the average of 27.7% among mothers in migrant families over the whole time period, this represents a decrease of almost 7%. This finding is in line with the notion in standard textbooks in Labor Economics (e.g., Borjas, 2010) that women's labor supply is more elastic on the participation margin than men's.

To test whether these baseline findings are sensitive to changes in their derivation, the sample, or the identification strategy, I conduct a number of robustness checks. The results are displayed in table 3, which is again divided into two panels for the outcomes of fathers and mothers, respectively. For ease of comparison, I state the coefficients of the benchmark specification in column 1. To start, I vary the size of the sample by restricting it in turn to families with children born within two and one years around the enactment date, on the one hand, and increasing it to a four-year window, on the other (columns 2 to 4). This procedure tests whether the chosen window of three years around the threshold drives the empirical results. In principle, the estimates should capture the causal effect better the closer we get to the enactment

date, as this reduces the potential influences of changes in marriage and fertility patterns. On the other hand, the smaller number of birth-year cohorts limits the utility of including trends, so it is not clear a priori which window to use. In any case, the results indicate two patterns: One is that the estimated effect on the fathers is always positive, but mostly insignificant (except in the 2-year window), whereas the coefficients for the mothers remain negative and mostly statistically significant throughout (only in the 4-year window, this is not the case). The point estimates vary a bit across the different setups, but remain in the same order of magnitude. In particular, the coefficients of interest in the case of mothers range from -0.8 percentage points to -2.9 percentage points, with the benchmark estimate of -1.9 percentage points right in the middle. This supports the results from the main specification. The other pattern is that the contrary effects for mothers and fathers appear to be of roughly similar size in most circumstances. Although the estimates for the fathers are not significant, this could be an indication of an ongoing process of intra-household specialization between the parents.

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 Insert table 3 here  
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Next, I test whether the results rely on the linear trends I impose in the main specification. To this end, I estimate a simple difference-in-differences model of the following form:

$$Y_{it} = \alpha_0 + \alpha_1 \text{After}_{it} + \alpha_2 \text{Eligible}_{it} + \alpha_3 (\text{After} * \text{Eligible})_{it} + \theta \mathbf{X}_{it} + \tau_t + \varepsilon_{it} \quad (3)$$

That is, I use the same control variables and fixed effects as in equation (2), but focus now on the difference between the *average* changes in the employment rate across the two groups instead of the difference in changes *at the enactment date*. From the results in column 5, we can see that this setup produces the same signs for the estimates as before, and also the point estimates are comparatively similar. For fathers, the coefficient of interest increases from 2.3 to 3.1 percentage points, but remains without statistical significance. For mothers, the DiD estimate of -0.8 percentage points is still in the range of results of columns 1-4, but turns insignificant. Thus, while the overall results are rather similar for DiDisc and DiD, controlling for the linear trend seems important in this case. This is understandable, as mothers in the treatment group may be more affected by the younger age of their children than migrant mothers in mixed families, since the latter are more likely to have their husbands' family around to help them.

Finally, I conduct a falsification test to examine whether the results could be caused by cohort effects in general instead of the reform. To this end, I use the same DiDisc design, but replace the eligible migrant families by those in which the parents did not fulfill the requirement for birthright citizenship at the time of birth of their children and were therefore not affected by the new legislation. Thus, I compare families in which the children do *not* get the German citizenship automatically with the mixed families in which they do. For both groups, the year of birth of the children is therefore irrelevant for their legal

status. If we find similar results in this placebo experiment as in the main analysis nevertheless, this would indicate that the latter does not properly measure the impact of introducing birthright citizenship, but may have picked up something else or produce random outcomes. This seems not the case here, however, as can be seen in column 6. For both fathers and mothers, the placebo experiment produces very small coefficients which are far from statistical significance (0.7 and -0.1 percentage points, respectively). This suggests that the results in the main analysis are not simply coincidence.

In the next step, I switch the focus from the binary indicator for employment to the number of hours worked in the week before the interview, as the adjustment to the treatment may be more along the intensive rather than the extensive margin of labor supply. The results are reported in table 4 in the same way as in table 2. That is, we see the estimated coefficients for the main variable of interest (the interaction between *Eligible* and *After*) together with the conventional clustered standard errors (on the cohort and eligibility level) and the p-values from a wild cluster bootstrap with 1,000 replications. The dependent variable is the absolute number of reported hours of work in the previous week. Individuals who are not working are coded as 0, in order to take the potentially large effects of beginning or quitting a job into account as well. As before, we start off in column 1 without any further covariates and then add different sets of controls.

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Insert table 4 here

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The results for the fathers and mothers are displayed in panels A and B, respectively. For both parents, the estimates are close to zero and statistically insignificant in all specifications. Thus, parents do not seem able or willing to change their work schedule because of the legal status of the child. This is noteworthy in particular for the mothers, as we saw in table 1 that employed mothers in the eligible group tended to have a higher number of hours of work if their children were born directly after the reform in comparison to the change among migrant mothers in mixed families (by almost 1.6 hours on average around the enactment). Possibly, this may indicate that there is a small, but statistically insignificant heterogeneous effect, in which some mothers drop out of the work force completely, while others even increase their hours.

Summing up, the results suggest that mothers of eligible children reduce their current employment marginally, but do not react with respect to the average hours of work in the last week. At the same time, fathers do not seem to adjust their labor market behavior to the new legal status of their children.

## 7.2 Channels of influence

So far, I have interpreted the estimated reduced form effects as changes in the labor market integration behavior of the affected parents. Alternatively, the introduction of birthright citizenship for migrant

children could have affected the composition of the groups that I examine. Thus, it could be the case, that what I find here is a mere consequence of changes in the selection of individuals into the different comparison categories. This would require a different interpretation of the results and their political implications. In this subsection, I want to discuss four possible mechanisms for this and whether they are likely in this context.

The first and most prominent in migration-related issues is *selective outmigration*. If the reform caused families with mothers who were relatively more successful in the labor market to leave or others with relatively lower attachment to stay, this could have also produced the pattern of results we observe. In Germany, this is theoretically possible, as Sajons (2016) shows that the citizenship of the child affects migrant parents in their decision to leave again. It is unclear, however, in which direction this should influence observable integration measures, as different schools of thought exist on the reasons why migrants typically move on or return to their home countries. In the neoclassical theory of migration, return migrants are those who “failed” in the host country and therefore go back home. For the new economics of labor migration, on the other hand, migration is a dominantly temporary affair and return migrants are those who have successfully achieved their goals of saving money or acquiring human capital (e.g., Constant and Massey, 2002). Thus, the effect of this channel on the average integration of the remaining families could go either way, depending on the relative relevance of these two theories.

In the absence of appropriate panel data, I cannot examine this issue directly. Nevertheless, it is possible to indirectly test the degree of potential selection into outmigration by looking at how the averages of essentially fixed (country of origin or sex) or only mechanically evolving characteristics (age, years since arrival) of a well-defined migrant group change over time (see Cohen and Haberland, 2001). To this end, I first take eligible migrant families with children born in 2000 and consider the evolution of their mean age, sex, birth in Germany, region of origin, and years of arrival over the period of analysis, i.e., between 2001 and 2008.<sup>15</sup> Second, I compare these changes with the same differences among eligible migrant families with children born directly before the introduction of birthright citizenship in 1999 to net out common influences for long-term migrants in general over this period. This amounts to testing the significance of difference-in-difference estimators like:

$$\lambda_{DiD} = (Age_{2008} - Age_{2001})_{BY2000} - (Age_{2008} - Age_{2001})_{BY1999} \quad (4)$$

In this example, if all members of the two groups remained in Germany, we would expect them to age by exactly the same amount over these years. In this case,  $\lambda_{DiD}$  would be close to zero and insignificant. The same would be true if both groups selected into outmigration in the same way or randomly, i.e., not

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<sup>15</sup> I concentrate on the cohort of 2000, because the later cohorts could not be identified in the Microcensus of 2001 yet. Thus, conducting this analysis for the cohort of 2001 would require moving the start of the analysis to 2002, going further away from the enactment date and reducing the evaluation period to 2002 to 2008.

correlated with these observable characteristics. On the other hand, if automatic citizenship for the child changed the propensity of the affected families into outmigration in a systematic way, we would see significant differences in the changes between the two groups.

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Insert table 5 here  
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Table 5 reports the results of this exercise. The average characteristics for families with children born in 1999 are displayed on the left hand side, those for families with children born in 2000 on the right hand side. For each group and variable, the mean for 2001 is stated first, followed by the value for 2008 and the simple difference between the two. The last column shows the size of the respective DiD estimator calculated by equation 4. All the numbers reported there are small and insignificant, suggesting that granting birthright citizenship to immigrant children did not affect their parents' selection into outmigration in a systematic way.

A second way how citizenship for a child could influence the labor market performance of its parents would be if it affected their decision to *naturalize themselves*. In the literature, the acquisition of the host-country citizenship has been consistently found to positively correlate with higher earnings and employment wages (e.g., Chiswick, 1978; Bratsberg et al., 2002; Scott, 2008; Fougère and Safi, 2009; Bratsberg and Raaum, 2011; Steinhardt, 2012). Hence, if the enactment of automatic citizenship at birth reduced the propensity of mothers to naturalize themselves and thus their employment chances on the labor market, this could alternatively explain the results. This is possible, as parents had to apply for the German citizenship themselves if they wanted to get it for their children before the reform. With birthright citizenship, this is no longer necessary, thereby reducing the benefits of naturalization for parents. To see whether this effect plays a role here, I use the same specification as in the main part of the analysis (equation 2) with naturalization as outcome variable. The data set is limited to the years 2005 to 2008 in this case, as the Microcensus did not ask about naturalization in its earlier waves. Note that all parents in the sample are eligible for naturalization under the new law by construction, as they have to have lived in Germany for more than eight years before the birth of their child already. Thus, there is no difference in their own legal status between the parents in the different groups.

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Insert table 6 here  
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Table 6 shows the coefficients of interest for both fathers and mothers separately. For the fathers, we see the effect turning out as discussed above, with a significant reduction of 5.4 percentage points in the probability to naturalize within the first years after the birth of the child, which is robust to the inclusion of

the different control variables. By contrast, there is no statistically significant impact on the mothers if we take the clustering of standard errors on the cohort and eligibility level into account. This indicates that selective naturalization does not explain the labor market results obtained above, as the pattern would have to be exactly the opposite.

Finally, the observed findings could also be an artifact of changes in the composition of the comparison groups due to evolving marriage and fertility behavior among the target group of long-term migrants. Thus, it could be that economically more successful female migrants start to marry German spouses after the reform, leaving a larger number of relatively lower skilled females in the treatment group. Alternatively, if the more able females in the target group decide to delay their family plans after the reform, we would observe relatively more of the less talented ones in the group of mothers in those years. Keller et al. (2015) and Avitabile et al. (2014) show results that point towards these two possibilities, respectively. However, two arguments stand against this reasoning. First, the children in the sample are on average the second child in their families, suggesting that most of them are born to parents who are already together for a while. And second, it usually takes some time to find the right partner to marry and get children. Thus, the first one or two years after the enactment should not be affected that much by these phenomena, in particular, since the final passage of the reform only happened in July 1999, leaving hardly any time for a reaction to appear in the sample of newborn children in 2000. If we restrict the analysis to children who are born closer to the enactment date, these potential channels of influence should therefore lose their importance. Table 3 contained the estimated coefficients for exactly these variations in the design and showed that the results do not change sign or significance for both fathers and mothers. This gives me confidence that the main findings of this paper are not driven by changes in fertility and marriage decisions.

## **8. Conclusions**

In this paper, I study the effect of granting citizenship to immigrant children at birth in Germany on the labor market integration of their parents. The empirical analysis shows that migrant fathers do not change their labor market behavior in response to the legal status of their children in any way, while mothers get somewhat more likely to stay at home with their children. This finding fits into the long-standing notion in Labor Economics that labor supply is more elastic for the secondary earner in a family than for the main one. Supplementary analysis supports the interpretation of the result as behavioral change, as it does not seem to be caused by selective outmigration or different naturalization patterns after the reform. And while the adult component of the law may have led to changes in fertility and marriage patterns among migrants, these effects likely need some time to emerge and thus should not influence my analysis of families with children born around the enactment date.



What exactly motivates these migrant mothers to stay at home remains unclear. On the one hand, it could be that automatic citizenship for the child changes their expectations about the duration of stay in the host-country towards a more permanent migration. This would reduce the necessity to work and earn as much as possible in short time before facing the worse labor market conditions in the home country again (see Dustmann, 1997, and Dustmann and Görlach, 2016). On the other hand, it could also reflect an intentional investment in the upbringing of their children, so that they are able to reap the full benefits of the better perspectives in the host-country. This line of reasoning would be supported by findings in Avitabile et al. (2014) and Felfe et al. (2016).

There are certain limitations to the broader application of the findings of this study, as they are derived from the particular case examined here. To start, it could be that the estimates are influenced by the institutional situation of Germany with its rather restrictive citizenship legislation previous to the reform. Introducing birthright citizenship in countries with easier access to naturalization may therefore produce smaller effects on the parents, as the children there are more likely to end up with the host-country nationality anyway. By contrast, the impact may be more pronounced if a country enacts birthright citizenship without a transition regulation, as it would not be mitigated by the possibility for families with older children to apply for the same treatment. Likewise, the requirement that parents need to have lived in Germany for at least eight years before the birth of the child may imply that most of the integration process has already taken place before the treatment occurs. Thus, the results only apply to migrant parents who have already lived in the host-country for a long time. Reforms that are less strict in this respect could have a stronger effect on the parents, as they may not be as integrated already.

Despite these limits, learning more about the consequences of automatic citizenship for immigrant children on their parents is highly interesting from a political economy point of view. The results imply that there is no “free-lunch” in granting citizenship to the children of immigrants and hoping that this measure would simultaneously solve all existing integration problems of their parents. Improving the situation of first-generation migrants will therefore require other measures like ongoing efforts to raise their qualification level and language proficiency as early as possible, but also fighting discrimination in the labor market, facilitating the recognition of degrees obtained abroad, or changing immigration laws to boost the entry of high-skilled migrants.

Several questions about the consequences of birthright citizenship still remain. For one, there is the possibility of effects on the composition of future immigration, i.e., the prospect of obtaining citizenship for one’s offspring may influence the destination choice of new migrants. Given that most developed countries face demographic problems and labor shortages in the near future, evaluating which tools help to attract motivated and skilled migrants is necessary to devise efficient policies. Moreover, it could be that there are some long-run intra-family consequences for migrant families in which the children possess the host-country nationality, but the parents do not. And most importantly, we also need to know

more about how birthright citizenship affects those who are the actual target, i.e., the children that grow up as citizens of the host country. Further research on these different areas is therefore necessary to evaluate the full effect of this policy measure.

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## Figures and tables

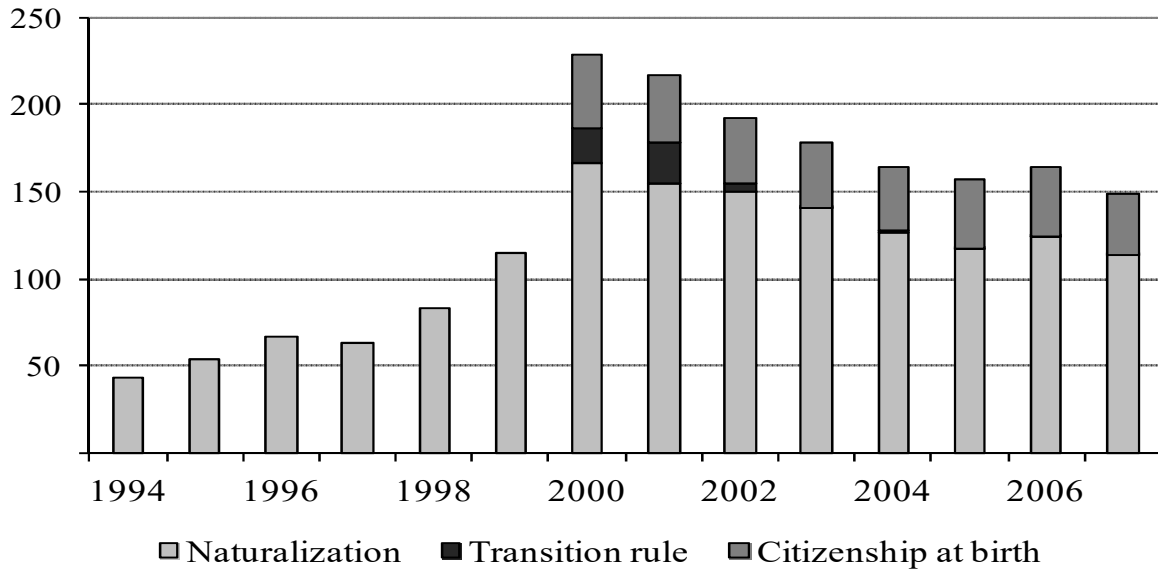


Fig. 1 Acquisition of German citizenship by type [in 1000], 1994-2007

Source: German Statistical Office

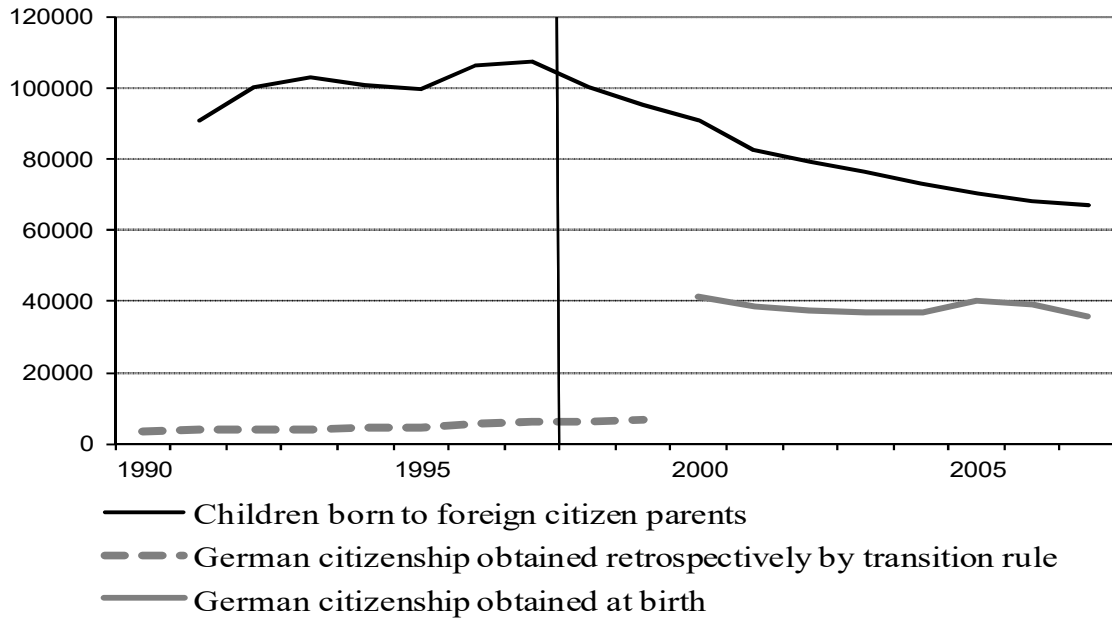


Fig. 2 *Foreign-citizen children and receipt of German citizenship by year of birth*

Source: German Statistical Office



Table 1  
*Descriptive statistics by family type and year of birth*

	<b>Migrant families (Eligible)</b>				<b>Mixed families (Control)</b>				<b>Difference-in-discontinuities</b>	
	Born before 2000		Born in or after 2000		Born before 2000		Born in or after 2000			
	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.	Coefficient	p-value
Sample size	4641		4431		5909		6198			
<b>Households:</b>										
North	0.114	(0.318)	0.120	(0.325)	0.135	(0.342)	0.136	(0.343)	0.011	0.370
East	0.057	(0.231)	0.065	(0.247)	0.095	(0.294)	0.095	(0.294)	0.031	0.132
South	0.353	(0.478)	0.338	(0.473)	0.366	(0.482)	0.351	(0.477)	-0.006	0.834
West	0.476	(0.499)	0.476	(0.499)	0.403	(0.491)	0.418	(0.493)	-0.036	0.362
Number of children	2.453	(1.032)	2.386	(1.101)	2.153	(0.947)	2.054	(0.945)	0.149***	0.002
Parents married	0.987	(0.114)	0.976	(0.153)	0.943	(0.232)	0.928	(0.259)	0.002	0.708
<b>Children:</b>										
Age	6.047	(2.610)	3.659	(2.184)	5.655	(2.542)	3.366	(2.183)	0.019	0.900
Male	0.512	(0.500)	0.515	(0.500)	0.512	(0.500)	0.492	(0.500)	-0.011	0.460
German citizenship	0.190	(0.392)	0.642	(0.480)	0.936	(0.245)	0.976	(0.154)	0.409***	0.000
<b>Mothers:</b>										
Age	33.211	(5.519)	31.590	(5.372)	34.504	(5.966)	32.789	(5.705)	0.032	0.928
Secondary education	0.387	(0.487)	0.436	(0.496)	0.702	(0.458)	0.715	(0.451)	0.005	0.856
Born in Germany	0.195	(0.396)	0.216	(0.411)	0.117	(0.321)	0.114	(0.318)	0.022	0.366
Years since arrival	18.084	(8.332)	15.391	(8.509)	13.214	(7.667)	10.555	(6.947)	-0.131	0.788
Non EU origin	0.841	(0.366)	0.858	(0.349)	0.750	(0.433)	0.778	(0.416)	0.047**	0.040

**Fathers:**

Age	36.845	(6.582)	35.612	(6.575)	37.108	(7.095)	35.094	(6.768)	-0.536	0.492
Secondary education	0.549	(0.498)	0.589	(0.492)	0.749	(0.433)	0.757	(0.429)	0.056**	0.018
Born in Germany	0.167	(0.373)	0.179	(0.383)	0.139	(0.346)	0.167	(0.373)	-0.023*	0.062
Years since arrival	21.562	(8.223)	19.587	(8.317)	16.819	(9.567)	14.177	(9.249)	-1.681**	0.016
Non EU origin	0.835	(0.371)	0.850	(0.357)	0.640	(0.480)	0.672	(0.470)	0.054	0.776

**Labor market integration:**

Employment mothers	0.315	(0.465)	0.238	(0.426)	0.363	(0.481)	0.291	(0.454)	-0.038***	0.002
Employment fathers	0.789	(0.408)	0.760	(0.427)	0.824	(0.381)	0.763	(0.425)	0.004	0.894
Hours worked mothers	20.536	(13.927)	20.205	(14.083)	21.032	(14.601)	19.516	(14.267)	1.597	0.286
Hours worked fathers	39.544	(11.689)	39.149	(12.484)	40.769	(13.616)	40.022	(13.852)	-1.380	0.382

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\*, \*\*, \*\*\* = significant difference on the 10%, 5%, and 1% level.

*Notes:* (1) The group of "migrant families" consists of families with children born in Germany between 1997 and 2002, in which both parents were foreign citizens in 2000 or the year of birth (if this is later), at least one of them arrived in Germany more than 8 years prior to birth, and none arrived later than the year of birth. (2) "Mixed families" are restricted in the same way, with the exception that one of the parents has to be a German citizen in the year of birth of the child. The means reported in the table correspond only to the migrant parent. (3) "Hours of work" only reports the average of those individuals who report to have a job. (4) The numbers in the "Difference-in-discontinuities" column represent the estimated difference in the jump of the respective variable at the cutoff between eligible and control families conditional on separate linear trends in the year of birth. (5) Clustered p-values on the cohort-eligibility status level are obtained from a wild cluster bootstrap with 1000 replications.

Table 2  
*Current employment of the parents*

	(1)	(2)	(3)	(4)
<b>A. Fathers</b>				
Coefficient	0.004	0.019	0.023	0.023
Standard error	(0.025)	(0.024)	(0.021)	(0.021)
p-value wild bootstrap	0.894	0.562	0.452	0.446
No. of clusters (Cohort - Eligibility)	12	12	12	12
Sample size	14853	14853	14853	14853
Adj. R <sup>2</sup>	0.004	0.066	0.087	0.088
BIC	15886	14944	14583	14563
<b>B. Mothers</b>				
Coefficient	-0.038***	-0.020**	-0.021***	-0.019**
Standard error	(0.006)	(0.007)	(0.006)	(0.006)
p-value wild bootstrap	0.002***	0.050**	0.004***	0.006***
No. of clusters (Cohort - Eligibility)	12	12	12	12
Sample size	15398	15398	15398	15398
Adj. R <sup>2</sup>	0.010	0.150	0.157	0.158
BIC	19506	17158	17030	17003
Individual controls	No	Yes	Yes	Yes
Region FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

\*, \*\*, \*\*\* = Statistical significance on the 10%, 5%, and 1% level

*Notes:* (1) The coefficients report the estimated effect for the interaction *eligibility\*after* for separate linear regressions. The dependent variable is binary in each case. (2) The statistical significance of each coefficient is determined using clustered standard errors on the cohort-eligibility level, obtained by the STATA command *cluster*. (3) The wild bootstrap method is implemented following Cameron et al. (2008) with 1,000 replications in order to control for the small number of clusters. (4) Controls include: Age, educational attainment (secondary, tertiary), born in Germany, current school attendance, regional origin (EU or Turkish), and years since arrival for the respective parent, as well as whether the parents are married, the age of the youngest child (normal and squared), the number of children in the family, and the quarter of the interview. (5) To prevent the loss of observations due to missing values for individual controls, they are set to 0 in the case of binary variables and to the mean for continuous ones. A separate indicator variable for each imputation is included.

Table 3  
*Robustness - Parental employment*

	Benchmark	Different window widths			Diff-in-diff	Mixed vs. ineligible
	1997-2002	1999-2000	1998-2001	1996-2003		
	(1)	(2)	(3)	(4)	(5)	(6)
<b>A. Fathers</b>						
Coefficient	0.023	0.009***	0.011**	0.012	0.031*	0.007
Standard error	0.021	0.001	0.003	0.018	0.016	0.020
p-value wild bootstrap	0.446	0.110	0.008***	0.616	0.206	0.800
No. of clusters (Cohort - Eligibility)	12	4	8	16	12	12
Sample size	14853	5338	10236	18951	14853	9810
<b>B. Mothers</b>						
Coefficient	-0.019**	-0.008**	-0.029***	-0.010	-0.008	-0.001
Standard error	0.006	0.002	0.003	0.013	0.010	0.011
p-value wild bootstrap	0.006***	0.002***	0.002***	0.630	0.436	0.988
No. of clusters (Cohort - Eligibility)	12	4	8	16	12	12
Sample size	15398	5454	10522	19661	15398	10355

\* = 10%, \*\* = 5%, \*\*\* = 1% significance levels

*Notes:* (1) The coefficients report the estimated effect for the interaction *eligibility\*after* for separate linear regressions. The dependent variable is binary in each case. (2) The statistical significance of each coefficient is determined using clustered standard errors on the cohort-eligibility level, obtained by the STATA command *cluster*. (3) The wild bootstrap method is implemented following Cameron et al. (2008) with 1,000 replications in order to control for the small number of clusters. (4) The included controls and the way to impute missing values in individual characteristics are the same as in the benchmark specification.

Table 4  
*Number of hours worked the previous week*

	(1)	(2)	(3)	(4)
<b>A. Fathers</b>				
Coefficient	-0.901	-0.050	0.179	0.218
Standard error	(1.280)	(1.081)	(0.980)	(0.970)
p-value wild bootstrap	0.642	0.944	0.904	0.868
No. of clusters (cohorts)	12	12	12	12
Sample size	14853	14853	14853	14853
Adj. R <sup>2</sup>	0.005	0.071	0.09	0.091
BIC	130989.7	129958.3	129633.1	129619.7
<b>B. Mothers</b>				
Coefficient	-0.315	0.163	0.082	0.090
Standard error	(0.250)	(0.323)	(0.273)	(0.260)
p-value wild bootstrap	0.360	0.746	0.828	0.796
No. of clusters (cohorts)	12	12	12	12
Sample size	15398	15398	15398	15398
Adj. R <sup>2</sup>	0.007	0.120	0.125	0.125
BIC	120416.8	118546.2	118451.8	118438.3
Individual controls	No	Yes	Yes	Yes
Region FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

\*, \*\*, \*\*\* = Statistical significance on the 10%, 5%, and 1% level

*Notes:* (1) The coefficients report the estimated effect for the interaction eligibility\*after for separate linear regressions. The dependent variable is the number of hours worked in the week before the interview. Missing values or non-working individuals are coded as working 0 hours. (2) The statistical significance of each coefficient is determined using clustered standard errors on the cohort level, obtained by the STATA command cluster. (3) The wild bootstrap method is implemented following Cameron et al. (2008) with 1,000 replications in order to control for the small number of clusters. (4) The included controls and the way to impute missing values in individual characteristics are the same as in the benchmark specification.

Table 5  
*Selective outmigration among migrant families with children born around the enactment*

	Child born in 1999			Child born in 2000			Difference-in-Differences
	2001	2008	Change	2001	2008	Change	
Number of children	226	200		183	207		
<b>Children:</b>							
Age	1.274	8.515	7.241	0.311	7.536	7.225	-0.016
	0.447	0.501		0.464	0.500		
Male	0.531	0.520	-0.011	0.503	0.512	0.009	0.020
	0.500	0.501		0.501	0.501		
<b>Mothers:</b>							
Age	29.257	35.760	6.503	28.169	35.169	7.000	0.496
	5.283	5.563		4.649	4.944		
Born in Germany	0.235	0.175	-0.060	0.202	0.184	-0.019	0.041
	0.425	0.381		0.403	0.388		
Years since arrival	14.105	19.976	5.871	12.710	18.065	5.355	-0.515
	8.174	7.830		8.472	7.858		
Non EU origin	0.819	0.870	0.051	0.814	0.884	0.070	0.018
	0.386	0.337		0.390	0.321		
<b>Fathers:</b>							
Age	32.832	39.555	6.723	32.410	39.010	6.600	-0.123
	5.963	6.573		6.665	6.464		
Born in Germany	0.190	0.155	-0.035	0.131	0.130	-0.001	0.035
	0.393	0.363		0.338	0.338		
Years since arrival	17.388	24.420	7.032	16.108	23.626	7.518	0.486
	8.113	8.070		8.314	7.661		
Non EU origin	0.810	0.875	0.065	0.814	0.894	0.080	0.014
	0.393	0.332		0.390	0.309		

\*, \*\*, \*\*\* = significant difference on the 10%, 5%, and 1% level.

*Notes:* (1) The analysis is restricted to the group of "migrant families" with children born directly around the enactment date for automatic birthright citizenship. (2) The means reported in the table correspond to those observations with valid information on the respective characteristic. (3) The numbers in the "Difference-in-differences" column represent simple estimated differences in the changes of the respective variable in the Microcensus from 2001 to 2008 between families with children born after and before the enactment of the reform.

Table 6  
*Naturalization of the parents*

	(1)	(2)	(3)	(4)
<b>A. Fathers</b>				
Coefficient	-0.052***	-0.048**	-0.056***	-0.054**
Standard error	(0.016)	(0.017)	(0.018)	(0.018)
p-value wild bootstrap	0.036**	0.082*	0.022**	0.028**
No. of clusters (cohort*eligible)	12	12	12	12
Sample size	8141	8141	8141	8141
Adj. R <sup>2</sup>	0.009	0.066	0.073	0.074
<b>B. Mothers</b>				
Coefficient	0.033**	0.023*	0.026*	0.026*
Standard error	(0.013)	(0.013)	(0.013)	(0.013)
p-value wild bootstrap	0.070*	0.202	0.156	0.172
No. of clusters (cohort*eligible)	12	12	12	12
Sample size	8436	8436	8436	8436
Adj. R <sup>2</sup>	0.001	0.037	0.041	0.041
Individual controls	No	Yes	Yes	Yes
Region FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

\*, \*\*, \*\*\* = Statistical significance on the 10%, 5%, and 1% level

*Notes:* (1) The coefficients report the estimated effect for the interaction eligibility\*after for separate linear regressions. The dependent variable Naturalization is binary in each case. (2) The statistical significance of each coefficient is determined using clustered standard errors on the cohort-eligibility level, obtained by the STATA command cluster. (3) The wild bootstrap method is implemented following Cameron et al. (2008) with 1,000 replications in order to control for the small number of clusters. (4) Controls include: Age, educational attainment (secondary, tertiary), born in Germany, current school attendance, regional origin (EU or Turkish), and years since arrival for the respective parent, as well as whether the parents are married, the age of the youngest child (normal and squared), the number of children in the family, and the quarter of the interview. (5) To prevent the loss of observations due to missing values for individual controls, they are set to 0 in the case of binary variables and to the mean for continuous ones. A separate indicator variable for each imputation is included.

## Appendix:

### Appendix A. 1: Identification of the sample

To identify the sample of migrant and mixed families with children between 1996 and 2003, I undertake the following steps:

1. In each wave of the Microcensus from 2001 to 2008, I drop all households which do not have a child born in Germany in the relevant years. If a family got more than one child in that period, it appears more than once in the data, as the structure of the data centers on the children and their birthdates. The only exception to that are twins or siblings born in the same year, for which I only use one of the two since the “treatment” is the same.
2. In order to make sure that I identify the target families properly, I disregard all households in which only non-relatives live together. (*ef541*>4 in the Microcensus waves up until 2004, *ef721*>4 afterwards) and focus exclusively on children living in a two-generation family with parents and children (*ef542*=2 and *ef722*=2, respectively).
3. To concentrate on “normal” age ranges of the parents, I drop families with mothers older than 49 and fathers older than 69 at the year of birth of the child.
4. I abstain from analyzing single-parent families, as there exists no information about the other parent in the data, which would be necessary to sort them into the different categories of eligible migrants, mixed parents, and ineligible migrants.
5. Then, I sort the family-child observations into 3 different samples:
  - a) The group of eligible migrants, whose children should get automatic citizenship at birth. In this group, both parents were still foreign citizens in 2001 or in the year after the birth of the respective child, if that is later. Furthermore, at least one of the two has to have arrived in Germany at least 9 years before the birth of the child or was born there.
  - b) The group of mixed parents, i.e., those with one parent being a German citizen at the time of birth, while the other possesses a foreign nationality. In this group, the children obtain the German citizenship automatically due to their German parent.
  - c) The group of ineligible migrants. Here, the same restrictions with respect to the nationality of the parents apply as in group A, but both parents arrived in Germany more recently than 8 years before the birth. Thus, they do not fulfill the requirements for birthright citizenship for the respective child.

Note that these criteria allow that couples may enter the data set in different categories if they have several children. For instance, they could be ineligible for automatic citizenship at the time of birth of the first child (group C), become eligible before the birth of the second (group



A), and later even enter in group B with the third if one of the parents naturalized in the meantime.

6. Finally, I exclude the German parents in the sample of mixed families from the analysis in order to restrict the comparison to individuals with migration background, but different legal situations for their children.

Table A.1  
*Definition of variables used in the analysis*

<i>Control variables</i>	<i>Description</i>	<i>Original variable in the Microcensus</i>	
		<i>2001 - 2004</i>	<i>2005 - 2008</i>
<b>Household:</b>			
Regional fixed effects	Dummies for 15 federal states, with the state of Schleswig-Holstein as benchmark.	ef1	ef1
Year fixed effects	Dummies for each wave of the Microcensus, the baseline is 2001.	Generated	Generated
Quarter fixed effects	Dummies for the quarter of the year in which the interview was conducted. The first quarter serves as reference category. In years 2001 to 2004, all participants were interviewed in the second quarter.	Generated	Generated
Number of children under 18	Generated from the observations in the respective family and year.	Generated	Generated
Parents married	Binary indicator for whether the parents are legally married.	ef35	ef49
Age youngest child	Age of the youngest child in the family in years. Before 2005, this information is derived from the observations in the respective family.	Generated	ef803
<b>Parents</b>			
Age and age <sup>2</sup>	Age in years.	ef30	ef44
Educational attainment	Indicators for whether the highest educational or professional attainment falls into the category <i>secondary</i> or <i>tertiary</i> education. Having only absolved primary school successfully serves as the benchmark.	ef287 (before 2003) / ef259 (Degree from school) ef289 (before 2003) / ef261 (Professional degree)	ef310 (Degree from school) ef312 (Professional degree)
Current school attendance	Binary indicator for whether the respective parent attended any kind of school at the time of the interview.	ef209	ef233
Born in Germany	Binary indicator for whether the respective parent was born in Germany.	Generated from ef53 (Year of arrival)	ef366

Years since arrival	Number of years since the self-reported year of arrival in Germany and the year of interview.	ef53	ef367
EU origin	Binary indicator for whether the respective parent possesses the nationality of another EU country.	ef44	ef369 (Citizenship) ef374 (Citizenship before naturalization)
Turkish origin	Binary indicator for whether the respective parent possesses the Turkish nationality.	ef44	ef369 (Citizenship) ef374 (Citizenship before naturalization)
<b>Outcomes</b>			
Employment	Binary indicator for whether the respective parent was currently in employment in the week of the interview.	ef95	ef77
Hours worked	Self-reported number of hours worked in the week before the interview.	ef143	ef134
Naturalization	Binary indicator for whether the respective parent obtained the German citizenship after 2000 or the year of birth of the child under consideration, if that happened later. (Only available after 2004.)	-	ef372

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*Data:* Microcensus 2001-2008

*Note:* The sorting and numbering of the variables changes from 2004 to 2005 due to change in the sampling design and questionnaire of the Microcensus.

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