Essays in Immigration Economics and Political Economy

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To Gwendolin, Joan, Amaya, and Nuno

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Abstract

This thesis revolves around two themes. The first is whether granting citizenship to immigrant children at birth affects their parents' return migration decisions and integration into the host-country society. Evaluating the introduction of birthright citizenship in Germany in 2000, I show in chapter 1 that migrant families are less likely to return to their home countries if their children automatically obtain the German citizenship. Chapter 2 continues the analysis of the same reform and finds that it has different effects across integration dimensions. Finally, chapter 3 is dedicated to the second theme. It examines whether stating the profession of candidates in open-list elections influences voter satisfaction and voting behavior. The results of a field experiment conducted in Barcelona indicate that voters are more satisfied if they know the profession of the candidates and that candidates working in high-skill occupations enjoy an electoral advantage.

Resumen

Esta tesis estudia dos temas. El primero es si otorgar la nacionalidad a los hijos de inmigrantes afecta las decisiones de sus padres de retornar a su país y la integración en el país de destino. En el primer capítulo muestro que las familias migrantes tienen menos probabilidad de volver a sus países de origen si sus hijos automáticamente obtienen la nacionalidad alemana. El capítulo dos sigue analizando la misma reforma y encuentra que sus efectos varían entre diferentes dimensiones de integración. Finalmente, el capítulo tres se dedica al segundo tema, el efecto de incluir la profesión de los candidatos en elecciones con listas abiertas. Los resultados de un experimento de campo en Barcelona indican que los votantes están más satisfechos si conocen la profesión de los candidatos y que los candidatos que trabajan en ocupaciones cualificadas disfrutan de una ventaja electoral.

Preface

This thesis contributes to two separate issues which are publicly debated in many countries these days. The first is the question of what developed countries can do to improve the integration of growing numbers of immigrants and their descendants into the host-country society. Given the increasing need for motivated and skilled migrants to dampen the consequences of the demographic transition in most industrialized countries, knowing about the effectiveness of individual measures becomes more and more important. Chapters 1 and 2 of this thesis are therefore dedicated to assessing the effect of a particular policy aiming at fostering integration: the introduction of birthright citizenship for newborn children of immigrants.

In the first chapter, I examine whether having a child that becomes citizen of the host country at birth influences family return migration decisions. For the empirical analysis, I use the reform of the German citizenship law in 1999, which introduced birthright citizenship for immigrant children in Germany, and exploit the discrimination of different birth-year cohorts contained in its provisions to apply a regression discontinuity design. I compute cohort return migration rates for the first years after the enactment of the reform (2001-2006) from the German Microcensus and compare these between families with children born before and after the enactment. The results provide evidence that granting citizenship to immigrant children reduces the likelihood of return migration among the affected parents.

In chapter 2, I go one step further and analyze the effect of automatic citizenship for immigrant children on the integration behavior of those parents who stay in the country. Taking advantage again of the differences in the treatment of children born on either side of the enactment date, I compare the integration behavior of parents with children born in the year directly before and after the enactment. The results suggest that the effect of automatically granting citizenship to immigrant children at birth on their parents' behavior varies across integration dimensions. On the one hand, I confirm previous findings of positive effects on the social integration dimension, shown by improvements in the parents' German language proficiency and a higher frequency of reading German newspapers. On the other hand, the results suggest detrimental effects on the parents' willingness to apply for naturalization themselves (formal integration). With respect to the parents' labor market integration, the results are not clear and require further investigation.

Taken together, chapters 1 and 2 indicate that certain spill-over effects of policies targeting the children of immigrants on their parents exist. Policymakers should be aware of these effects, especially since they may not always go into the desired direction.

The second topic of this thesis is whether stating the profession of candidates on the ballot in open-list elections affects the satisfaction of voters with their elected representatives and the whole voting process, on the one hand, and their actual voting behavior, on the other. This issue relates to the debate about what can be done against the increasing alienation between the citizens and the political system and its parties in many countries. In this debate, the introduction or wider use of open-list elections is frequently demanded by think tanks and popular movements. The special feature of open-list elections is the possibility to vote not only for a party but for individual candidates of that party, thus granting the voters more influence on the final composition of the elected council or parliament. It is unclear, however, whether simply introducing open-list elections would really lead to more voter satisfaction and political participation, as choosing between a large number of candidates involves large information and decision costs that could even cause citizens to abstain from voting.

In chapter 3, I examine whether giving the voters more information about the candidates by stating their profession on the ballot could solve this problem. I present the results of a voting experiment conducted with real voters in Barcelona, Spain, in which the respondents were randomly assigned to varying amounts of information about hypothetical candidates. The findings show two main effects of including profession information on the ballot: (1) It raises voters' satisfaction with the selected candidates and the electoral system as a whole, leading to stronger support for open-list elections. This suggests that what matters for voters is not having a choice per-se, but having a meaningful choice. (2) It changes the composition of the elected body as voters use profession information to identify more "qualified" candidates, i.e., those working in high-skill professions. Additional results show that profession information also helps the voters to express their own political opinion more explicitly. Supporters of left-leaning parties are thus more inclined to vote for candidates with a "social" profession like physicians, teachers, and sociologists, whereas more "business oriented" voters of right-leaning parties prefer lawyers, engineers, and managers. Finally, stating profession information does not seem to reduce the electability of female candidates.

CONTENTS

1. Does Granting Citizenship To Immigrant Children Affect Family Return Migration?

1.1 Introduction

Does getting the citizenship of the host country increase an immigrant's willingness to remain in that country and integrate in its society, or is it already the final step in the integration process without further consequences? As the fraction of first and second-generation immigrants among the population has increased for many Western countries in recent years, the answer to this question would provide important information for legislators around the world who are concerned about the integration of foreigners and the long-run demographic development of their societies. Empirical evaluations of the effect of obtaining citizenship are, however, inhibited by the presence of strong self-selection. Those eager to remain in the host country and to integrate into its society are at the same time more likely to apply for naturalization, leading to biased estimates of the impact of getting citizenship on individual return migration and other integration outcomes.

In this paper, I attempt to circumvent this endogeneity problem by broadening the focus to family units and the citizenship status of newborn children. The aim is to estimate the causal effect of citizenship for children on family return migration (henceforth RM) behavior, as remaining in the host country is the necessary first step towards individual integration. To this end, I use the 1999 reform of the German citizenship law as quasi-natural experiment. The new law introduced birthright citizenship for newborn immigrant children as of January 1^{st,}, 2000, and thus provides a source of variation in the citizenship status of immigrant children that is independent of their parents' willingness to remain and integrate in the host country. For the empirical analysis, I look at immigrant families with children born between 1991 and 2002 and compare the RM behavior of families with children born before and after the enactment date, i.e., I apply a regression discontinuity (RD) approach. I show that the families of children born in the year before and after the enactment date are almost identical on a whole range of observable characteristics, suggesting that they are allocated around the cutoff "as if" randomly assigned. The only difference between these families is that those with a child born after the enactment of the new law are automatically "treated"

with the German citizenship for their child, whereas the families of children born before only had a limited time period to apply for it under a transitional provision of the law.

The results of the empirical evaluation provide evidence that introducing birthright citizenship for children has influenced more families to stay in Germany and thus decreased family RM. This qualitative finding is robust to a number of different specifications. The point estimates, on the other hand, are sensitive to the details of constructing the main dependant variable, cohort RM rates.

In principle, there could be two reasons why automatically granting citizenship to immigrant children at birth may affect their parents' RM behavior differently than only giving their parents the right to apply for them. First, the act itself may be perceived by the parents as a sign of goodwill of the host country, prompting them to be more willing to increase integration efforts on their part. Second, being "treated" with the host country citizenship for their child may induce parents to pay more attention to the benefits of this citizenship. Since potential benefits may include less discrimination in school and at job applications, as well as extended job possibilities, this may improve the perceived economic and social prospects of the child in the host country. If parents take their children's perspectives into account when considering whether to return to the home country, they could therefore be more inclined to stay if their children are citizens of the host country.

The paper continues as follows: Section 1 provides information on past research related to citizenship and RM. In section 2, I describe the change in the German citizenship law of 1999. Theoretical considerations of how this reform may influence immigrant families' RM behavior are presented in section 3. Section 4 discusses the identification strategy and section 5 the data source used and the way the dependent variable is computed. The results for the estimated effect of granting citizenship for children on family RM rates are presented in section 6, as well as several robustness checks. Finally, I conclude and point out some potentially fruitful directions for further research in section 7.

1.2 The Effects of Children and Citizenship on Return Migration

Existing research on the determining factors of return migration has mainly focused on the impact of socio-economic and country-of-origin specific characteristics on the

decision of *individuals* to return. In this literature, having acquired the citizenship of the host country and the presence of children are commonly used to control for the individual's attachment to the residence country (e.g., Sander, 2007; Gundel and Peters, 2008, for the case of Germany). In general, both of these variables seem to increase the duration of stay in the host country, or alternatively, decrease the probability of return migration. To the best of my knowledge, however, no author so far interprets the significant effect of having acquired the citizenship of the host country on return migration as a causal effect due to the endogeneity of citizenship as explanatory variable. Immigrants who are more inclined to integrate themselves are at the same time more likely to obtain the citizenship and less likely to return to their home countries.[1] Thus, the estimated effect of having acquired the citizenship includes the effect of the unobserved underlying willingness to integrate.

One possible way to solve this problem is to look at how exogenously granting citizenship to children affects *family* return migration behavior. Past research usually only included the presence of children in an immigrant's household as control, but did not take any particular characteristics or circumstances of these children into account. Recent work on the effect of children on the return migration of their parents is trying to fill this gap (e.g., Dustmann, 2003; and Djajic, 2008). The key innovation of these papers is that parents are assumed to possess paternalistic or altruistic preferences with respect to the expected well-being of their children. As consequence, differences in children's characteristics and expected future outcomes directly affect their parents' expected discounted life-time utility and hence their return migration decision today. Dustmann (2003) confirms this theoretical prediction by showing that there is a differential effect of children on parents' return migration decision depending on the sex of the child. Examining the first 14 waves of the German Socio-Economic Panel (GSOEP), i.e., the period between 1984 and 1997, he finds that foreign-born parents are significantly less likely to return if they have a son compared to a daughter. He attributes this result to a tendency of immigrant parents to care more about the better economic perspectives in the host country for their sons and the greater ability to maintain traditional ways of living in the home country for their daughters.

Djajic (2008) uses a slightly different theoretical model to examine under which conditions in the relative positions of children and parents in the decision making

[1] Constant and Zimmermann (2007) find that the possession of German citizenship increases the

process the whole family will move to the home country of the parents, stay in the host country, or split up with the child remaining in the host country and the parents returning. The key determinants for the single scenarios in this model are the expectations of future earnings in host and home country, the respective preference for remaining in the host country, and the strength of the desire to stay together as a family. Independent of the assumption about the relative bargaining position of the child, any improvement in the child's expected future income in the host relative to the home country is predicted to increase the probability that the whole family remains in the host country.

The paper most related to the present work is Avitabile et al. (2010), in which the authors use GSOEP data to examine the effect of a transition rule in the new German citizenship law of 1999 on some cultural integration outcomes of the parents. That is, they analyze the effect of granting the *option* to get the German citizenship retrospectively for children born between 1990 and 1999 on their parents' German language fluency, whether they read German newspapers, and how often they invite or visit German friends. Comparing the development of foreign-citizen families with the youngest child born between 1990 and 1999 with foreign-citizen couples without children and families with the youngest child born before 1990 in a difference-in-differences approach, they find significantly positive effects on all three variables.

1.3 The Reform of the German Citizenship Law

The reform of the German citizenship law in 1999 was the first major social reform of the newly-elected government under Chancellor Schröder and the center-piece of its integration policy (Coalition Treaty, 1998). Its publicly stated major goal was to facilitate the integration of foreign citizens who had been living in Germany for a long time into the German society. The size of this part of the population was substantial, with 40% of the 7.3 million foreign-citizens in Germany having lived there for at least 15 years, over 30% even for more than 25 years (Statistisches Bundesamt, 2001). Historically, Germany did not consider itself an immigration country and naturalization only happened on a discretionary basis if it was beneficial for the country (Brubaker, 1992). A personal entitlement to naturalization was introduced for the first time in 1990, but only after 15 years of legal residence in Germany and if the previous citizenship was given up.

By contrast, the citizenship law of 1999 was meant as an offer to integration driven by the assumption that excluding a substantial part of the population from equal rights harms a society. In its final version, the reform contained two major components coming into effect on January, 1st, 2000. The first relaxed the regulations for the naturalization of foreign adults and children, reducing the required minimum residence duration from 15 to 8 years, while at the same time enacting some new requirements, e.g., sufficient language proficiency, an oath to the German constitution, and the non-receipt of welfare benefits. A general acceptance of dual citizenship was also planned initially, but could not gather enough political support.

The second component introduced the birthplace principle (*jus soli*) for the acquisition of the German citizenship. Thus, for the first time in German history, the children of foreign citizens automatically receive the German citizenship at birth if their parents meet two conditions: At least one of them has to have lived in Germany legally for more than eight years prior to the birth of the child and to possess the permanent residence permission. Before these children turn 23, they have to declare before the German authorities whether they want to maintain their German citizenship or keep the citizenship of their parents (the so-called "option model"). If they fail to do so, the German citizenship will be withdrawn.[2] Additionally, the law contained a transition regulation for those children born between 1990 and 1999 whose parents had fulfilled the above requirements at the time of their birth. The parents of these children could file an application for their children throughout the year 2000 in order to receive the German citizenship under the new birthplace principle.

Figure 1 illustrates the impact of the new law on the number of foreigners obtaining German citizenship over the years. While the number of naturalizations by foreigners was already trending upwards throughout the 1990s (from about 42,000 in 1994 to 114,000 in 1999), it further jumped up in 2000 to more than 166,000 after the reform loosened the restrictions. This increase was much smaller than expected (Deutscher Bundestag, 1999) and only temporary, however, as the numbers gradually went down again to about 113,000 in 2007. The transition regulation for foreign children born between 1990 and 1999 further accentuated this short-lived spike by

[2] This feature is disputed, however, since the German Constitution forbids the removal of the German citizenship in most circumstances.

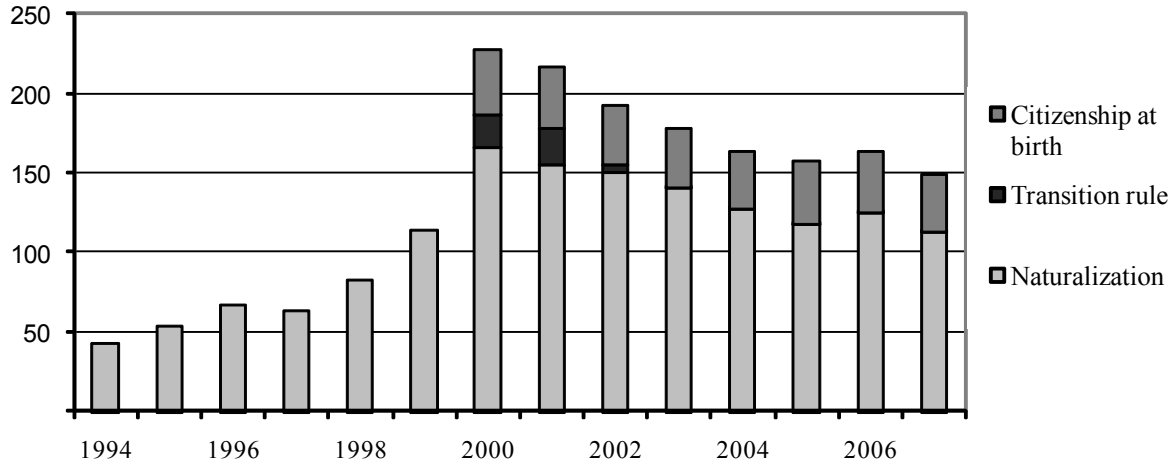

Fig. 1. *Acquisition of German Citizenship by Type [in 1000], 1994-2007*

Source: Statistisches Bundesamt

creating another 50,000 new German citizens, almost all of them in 2000 and 2001.[3] The introduction of the birthplace principle, on the other hand, had a more lasting impact, adding around 40,000 new citizens to the German population every year. Thus, roughly one half of all children who are born to foreign citizens in Germany receive the German citizenship at birth now, representing more than 24% of all new citizenship acquisitions in 2007. The introduction of the birthplace principle can therefore be seen as the most important component of the new citizenship law in terms of long-run effects on the composition of the German citizenry.

1.4 Theoretical Considerations

How may immigrant parents change their RM decision in response to the introduction of birthright citizenship? To answer this question, it is helpful to look at a stylized model of family return migration in which parents may take their children's future perspectives into account. This can be modeled by assuming that the utility of parents i derived from living in country s, $\boldsymbol{U_{is}}$, depends both on their own and their child's utility of living there. For simplicity, assume that $\boldsymbol{U_{is}}$ is separable by parents and child (subscripts P for parents and K for kid, respectively) and that each party's utility of living in s is derived from two sources, consumption of goods C in s and non-monetary utility NM of living there:

[3] Although applications under the transition regulation had to be filed in 2000, it usually took several months to process them, in some cases even years.

$$\boldsymbol{U_{is}} = U_{P,is}(C_{P,is}, NM_{P,is}) + \alpha U_{K,is}(C_{K,is}, NM_{K,is}) \quad (1)$$

Consumption depends on the expected future income streams and the price level of goods, while non-monetary utility includes the level of social interaction in country *s* and the attachment to it. The degree to which the parents consider their child's future wellbeing for the RM decision is reflected by parameter α. Standard assumptions (see Djajic, 2008) about the relative size of the respective utility component for parents and children in source country *S* and host country *H* can be summarized as follows:

$$\begin{aligned} C_{P,H} > C_{P,S} \quad , \quad NM_{P,H} < NM_{P,S} \\ C_{K,H} > C_{K,S} \quad , \quad NM_{K,H} > NM_{K,S} \end{aligned} \quad (2)$$

These inequalities state that we assume consumption to be greater in the host country for both parents and children, reflecting the larger earnings potential in the host country that led the parents to migrate in the first place. Non-monetary utility, on the other hand, is assumed to be larger in the source country for the parents, but smaller for the child who prefers to remain in her country of birth.[4] Without further assumptions about the form of the utility function, we would expect a family to return to the parents' source country if family utility is greater in the source than in the host country (net of moving costs), that is, if

$$\boldsymbol{U}_{iH} < \boldsymbol{U}_{iS} \quad (3)$$

Introducing birthright citizenship for immigrant children may influence a family's RM decision through two channels. First, it may be perceived by the parents as a "sign of goodwill" of the host country, which may prompt them to feel more attached to it in turn. This would increase $NM_{P,H}$ and thus reduce the likelihood of returning to the source country. Second, automatically obtaining the host country citizenship for their child may cause parents to start thinking about the economic and social advantages that citizenship entails. In Germany, these advantages include the free choice of profession (many professions in public service are open only to German citizens, e.g., teachers, judges, policemen, and the armed forces), work permission in all EU countries, the right to vote and get elected, potentially less discrimination by peers, teachers, and employers, free international traveling, etc. If parents consider these aspects, they may

[4] This assumption is, however, not crucial for the considerations developed in this paper.

raise their expectations of $C_{K,H}$ and $NM_{K,H}$, again reducing the probability of family RM.

Both effects should be much weaker for those families who only got the option to obtain the German citizenship for their children retrospectively under the transitional regulation of the law, that is, those with children born between 1990 and 1999 (the "control" group in the empirical part of this paper). First, although getting the option may also be perceived as a sign of goodwill, it can be assumed to be much weaker since it is not such an unconditional display of courtesy by the host country. Second, since the take-up rates among this group lay only between 15 and 20% of the eligible children per birth year, a much lower fraction of parents will increase the expectations for their child compared to those families with automatic receipt.[5]

1.5 Identification Strategy

In this study, I apply a regression discontinuity (RD) design to identify the effect of introducing birthright citizenship for immigrant children on the RM behavior of their parents. The main idea is to compare immigrant families with children born in the years after the enactment of the reform (treatment group) and before (control group), identifying the causal effect of birthright citizenship on family RM as the difference between the two groups' RM rates at the cutoff. This approach aims at disentangling the effect of granting birthright citizenship from three potentially confounding effects that could drive family RM behavior: First, the impact of other elements of the reform itself, in particular, the reduction in the minimum residence requirement from 15 to 8 years for adults. Second, possible general trends affecting all immigrants equally over the examination period, e.g., changes in labor market conditions in both Germany and/or

[5] Own estimation based on data from the German Statistical Office. Three reasons could possibly explain these low take-up rates: (1) Contrary to automatically getting the German citizenship, taking advantage of the option involved incurring some positive monetary, informational, and psychological costs in the form of paying fees, spending time and effort to gather information, get necessary documents, and go to the respective offices, as well as actively deciding on binding your child closer to the host country and thereby reducing the bonds to the parents' own country of origin. (2) The tangibility and certainty of the involved costs and benefits may be asymmetric. While the costs of the application are known and certain, it is hard to know how much exactly a child would benefit from possessing the German citizenship. (3) Some parents may be inclined to postpone the decision until the child has grown up and can choose for herself, knowing that she will have the right to naturalize later anyway (although at the cost of giving up the former citizenship).

the home country. And third, the potential endogeneity between family RM behavior and whether German citizenship is reported for a child, as parents who intend to stay in Germany may be both more likely to get the German citizenship for their children under the transition regulation of the new law and more accurate in reporting a possible German citizenship of their children.

The chosen RD design for the evaluation addresses all three issues. To prevent that the results are influenced by other components of the reform or differing time trends, I define the two comparison groups such that they are composed of families that are as similar as possible. In particular, both groups only include those immigrant families in which at least one parent is herself eligible for naturalization under the new law, i.e., was either born in Germany herself or arrived prior to 1991 to meet the new residence requirement of having lived in Germany for eight years already. This way, the parents in both groups should be equally affected by the other main component of the reform, so the only difference is whether their children were born after the enactment of the reform and therefore received a different treatment. Likewise, as the two groups are "drawn" from the same pool of individuals, developments over time like changes in economic conditions in Germany and the countries of origin or changing attitudes toward foreigners among the native population should have the same impact on both.

The problem of potential endogeneity between family RM behavior and reported German citizenship of the child can also be dealt with in the RD design. Reported actual receipt of German citizenship (the "treatment") among the children of foreign citizens in my sample does not jump from 0 to 100% at the enactment date and may be endogenous and prone to measurement error, so actual treatment cannot be used to separate the two groups in order to estimate the average treatment effect. [6] The law, however, states clear eligibility requirements for automatically obtaining German citizenship at birth and an exogenously determined starting date. This allows me to use a dummy variable *treatment assignment* to clearly distinguish between the two groups. In particular, given the restrictions stated above, I code assignment as equal to 1 for those families with

[6] Since the transition component of the reform allowed children born between 1990 and 1999 to obtain the German citizenship retrospectively, there is a small fraction of children in each cohort of the control group who have received the treatment. At the same time, no German citizenship is reported for some children born in 2000 and afterwards. This may be caused by a lack of information in the Microcensus on the legal status of foreigners in Germany, which makes it impossible to exactly identify the set of eligible families. Alternatively, some parents may be either not aware of the German citizenship of their children or not willing to report it.

children born in 2000 or afterwards, and as 0 for those with children born in 1999 and before.[7] Since initial treatment assignment happened independently from the parents' plans to remain in Germany but determines the actual receipt of treatment to a large extent, it is possible to use it to avoid endogeneity and identify an unbiased "intent-to-treat" (ITT) effect.

Technically speaking, the methodology can therefore be described as a "fuzzy" RD design with the year of birth of a child as discrete forcing variable. The validity of this approach crucially depends on three conditions: (1) the exogenous determination of a precise cutoff value, (2) the inability of families to precisely manipulate the variable determining their assignment status, and (3) a significant difference in the treatment intensity around the cutoff. [8] Taken together, the first two produce a distribution of families that is "as-if" randomly assigned in the close neighborhood of the cutoff, which creates a credible counterfactual for the assignment group as in a randomized experiment.

For this study, the first condition is clearly fulfilled as the enactment date of the new law, the 1st of January, 2000, is clear-cut and was decided independently by the German parliament. The second requires that affected families cannot or do not precisely manipulate the birth date of their children. This should be the case for several reasons: The most important is that conception itself cannot be controlled precisely. Furthermore, only a small fraction of parents even had the possibility to consider postponing or advancing a planned child for some months as the first draft was only presented in mid-January 1999, which means that all births up to October 1999 were already predetermined. Finally, as the law included the transition rule for immigrant children born between 1990 and 1999, there was also no motivation to intentionally adjust one's fertility behavior. If parents really wanted their child to get the German citizenship, the law gave them the possibility to simply apply for it throughout the year 2000. Thus, it can be expected that families did not adjust the timing of any planned birth in response to the law change. The third requirement for the validity of the RD

[7] This approach implies that families with more than one child are counted in different birth-year cohorts. Furthermore, it also blurs the sharp distinction into the two groups, since the same family may have children born before and after the enactment date. This is a problem in the case of non-significant results, as it biases the estimates towards zero. On the other hand, it should enhance the credibility of significant results.

[8] See Imbens and Lemieux (2008) and Lee and Lemieux (2010) for well-written and conclusive overviews on how to use and interpret RD designs in applied research.

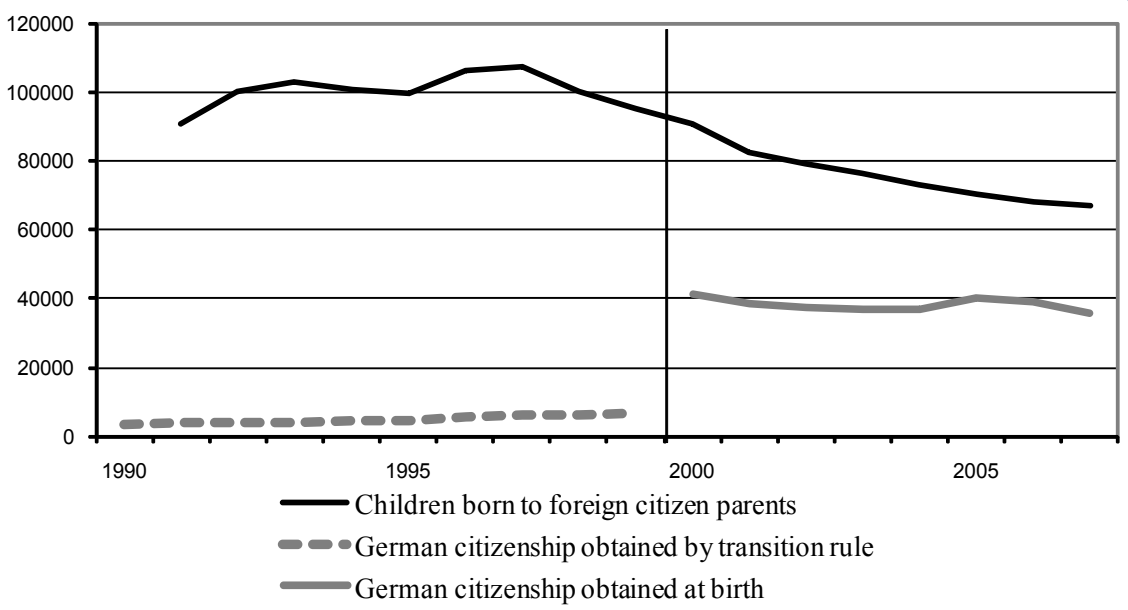

Fig. 2. *Foreign-citizen Children and Receipt of German Citizenship by Year of Birth*

Source : Statistisches Bundesamt (2010)

design, the discontinuous jump in the treatment intensity at the cutoff, holds as well, as demonstrated by official numbers from the German Statistical Office. They show that the fraction of newborn children of foreign-citizen parents who became German citizens at birth or due to the transition rule jumps from 7.1% to 45.6% between the birth cohorts of 1999 and 2000 (Statistisches Bundesamt, 2010).

As the main conditions for the correct use of an RD approach seem to be fulfilled, the reform can be treated as a locally randomized experiment around the enactment date. This conclusion is supported by figure 2 which presents the official numbers from the German Statistical Office on total births to foreign-citizen parents in Germany by year of birth for the period 1991 to 2007 (black line). It shows a peak in the years 1996 and 1997 with around 107,000 newborn children and a declining and continuous trend over the rest of the period. In particular, we can see a reduction of births from 95,200 in 1999 to 91,000 in 2000, whereas we would suspect to see an increase if parents had adjusted the timing of birth of their children. Figure 2 additionally depicts the number of children per year of birth who obtained the German citizenship because of the new law, either through the introduction of birthright citizenship for the cohorts born in 2000 or later (the solid grey line) or due to the transition rule for children born before 2000 (the dotted grey line). It nicely demonstrates the large and discontinuous change in treatment intensity between the birth cohorts of 1999 and 2000 that justifies the identification strategy outlined above.

1.6 Data and Dependent Variable

1.6.1 Data

For the empirical analysis, I use cross-sectional data for the years 2001 to 2006 from the German Microcensus.[9] The Microcensus is a yearly conducted representative survey of 1% of all households in Germany and is used by the German authorities to produce the official statistics of the demographic development and the labor market situation. The available scientific-use files contain information on a subsample of 70% of the included observations, which amounts to about 270,000 households (500,000 individuals). Working with the Microcensus has two main advantages: First, its large sample size allows observing enough observations of the very specific target group of this analysis. This is, in particular, not the case for the other dataset often used for migration-related issues in Germany, the German Socio-Economic Panel. Second, it includes information on the citizenship status of the respondents rather than the country of origin which is necessary to evaluate the effects of a change in the citizenship law. The main disadvantage is that the cross-sectional nature of the Microcensus makes it impossible to follow individual families over time, which means that the analysis has to be done on the level of birth-year cohorts.

A family is counted towards a particular birth-year cohort if at least one of the children was born in Germany in that year, both parents report to have been foreign-citizens at the beginning of 2001, [10] and at least one of the parents arrived in Germany before 1991 or was already born there. Given these restrictions, we can test the "as-if" random distribution of families to assignment and control group in the close neighborhood of the cutoff by comparing families with children born in 1999 and 2000 at the beginning of the evaluation period in 2001. "Randomization" can be seen as successful if these families are very similar on a number of important covariates.

Table 1 presents the descriptive statistics for these two groups, reporting standard errors in parentheses and marking significant differences obtained by a two-sided t-test with asterisks depending on the significance level. It shows that, on average, the two birth-year cohorts around the cutoff are very similar on a wide range of

[9] The data used in this paper were analyzed using the remote processing tool JoSuA developed by the IDSC of IZA, see Askitas (2008) for details.

[10] The choice of 2001 as the starting year for the evaluation is due to the fact that it is the first Microcensus in which the complete birth cohort of 2000 can be identified.

Table 1

Assignment and Control Group Characteristics around the Cutoff (Microcensus 2001

	Birthyear 2000 (Assignment)			**Birthyear 1999 (Control)**		
	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.
Families:						
North	0.115	(0.320)	200	0.103	(0.305)	262
East	0.090	(0.287)	200	0.069	(0.253)	262
South	0.295	(0.457)	200	0.359	(0.481)	262
West	0.500	(0.501)	200	0.469	(0.500)	262
Num. of kids	2.035	(1.063)	200	2.088	(1.074)	262
Parents married	0.940	(0.238)	200	0.916	(0.278)	262
HH net income	1826	(742)	195	1791	(698)	258
Children:						
Age child	**0.32*****	(0.466)	200	**1.27*****	(0.447)	262
Male child	0.495	(0.501)	200	0.534	(0.500)	262
German cit. of the child	**0.46*****	(0.500)	200	**0.115*****	(0.319)	262
Mothers:						
Age	**28.1****	(5.198)	194	**29.3****	(5.874)	259
Secondary education	0.572	(0.496)	159	0.522	(0.501)	203
Born in GER	0.242	(0.430)	194	0.239	(0.428)	259
Years since arrival	13.014	(8.571)	147	13.750	(8.053)	196
EU origin	0.191	(0.394)	194	0.193	(0.395)	259
Turkish origin	0.521	(0.501)	194	0.564	(0.497)	259
Fathers:						
Age	31.347	(5.904)	190	32.113	(5.868)	239
Secondary education	**0.629****	(0.484)	170	**0.521****	(0.501)	190
Born in GER	0.163	(0.370)	190	0.197	(0.398)	239
Years since arrival	15.639	(8.131)	158	16.599	(8.230)	192
EU origin	0.184	(0.389)	190	0.184	(0.388)	239
Turkish origin	0.537	(0.500)	190	0.586	(0.494)	239

*, **, *** = significant difference between the two groups on the 10%, 5%, and 1% level, respectively.

Note: The two groups are restricted to families of children born in Germany in 1999 or 2000 (directly before and after the enactment of the reform, respectively), with both parents being foreign-citizens in 2000, at least one parent arrived in Germany prior to 1991, and none arrived later than 2000.

Data: *Microcensus 2001*

observable characteristics, including the region of residence, number of children in the family, net monthly household income, and sex of the child, as well as age, educational attainment, and non-EU-citizenship of the parents (with the two exceptions that assignment group mothers are younger by one year than control group mothers, and assignment group fathers possess more secondary education than control group fathers).

By construction, children in the control group are on average one year older, whereas a much higher proportion of children in the assignment group possess the German citizenship (46% vs. 11%).[11]

Thus, the important condition of assignment status being "as-if" randomly assigned around the cutoff is fulfilled in the sample and any differential development between the two groups can be reasonably attributed to the assignment of families to obtain birthright citizenship for their children.[12]

1.6.2 Computation of the Dependent Variable: Cohort RM Rates

To be able to assess whether granting citizenship to the children of foreigners at birth affects their families' RM behavior, I compute the RM rates between 2001 and 2006 for each birth-year cohort of children born between 1991 and 2002.[13] In the absence of appropriate panel data, [14] I use the approach of Borjas and Bratsberg (1996) and Cohen and Haberfeld (2001) who calculated RM rates using representative cross-sections of the population and the following formula:

$$Estimated\ RM\ rate_{i,01-06} = \frac{adj.\ cohort\ size_{01} - adj.\ cohort\ size_{06}}{adj.\ cohort\ size_{01}} \qquad (4)$$

It is necessary to use adjusted cohort sizes, as there are three other reasons apart from RM that could explain a change in the size of a given birth-year cohort of immigrant

[11] Three reasons could potentially explain why German citizenship is only reported for 46% of the children in the assignment group. (1) Due to the missing information about the legal status of the parents, the identification of assignment families is not exact and includes therefore some proportion of ineligible children. (2) There is a time gap of up to several months between registering a newborn child and finally getting the information that it indeed fulfilled all requirements and thus became German citizen at birth. For some families with children born in late 2000, it could therefore be the case that the parents had not been informed about the German citizenship of their child yet. (3) As the information in the Microcensus is self-reported, it may be that some parents were not fully aware of the citizenship status of their child at the time and thus did not report it.

[12] Running a linear regression of assignment status on all the covariates that are not significantly different because of the setup of the analysis (i.e., excluding age and German citizenship of the child) yields an F-test value of only 1.06, demonstrating that all the above mentioned covariates do not jointly explain a family's assignment status either.

[13] For the cohorts of children born in 2001 and 2002, the Microcensus data of 2002 and 2003 are used to compute the initial cohort size, as these two cohorts could not be identified before. As consequence, their computed RM rates only represent their behavior over a period which is one and two years shorter, respectively, than for the other cohorts.

[14] The German Socio-Economic Panel, which is often used for migration related research, would produce a far too small sample size to conduct this kind of RD analysis.

children between two Microcensuses. First, some children may die in the meantime. Second, differences in the design of the Microcensus over the years may change the ability to identify the children in the target group. In particular, it became mandatory in 2005 to state the year of arrival in Germany, making it much easier to pinpoint the group of eligible children. Third, there may be changes in any possible over- or undersampling of the cohort. If these three sources of changes in cohort sizes can be accounted for, the residual change can be interpreted as return migration.

For the present paper, mortality does not play any role, since the probability to die within a five-year period is extremely low for young children in Germany and is the same across the examined cohorts. Official mortality rate tables from the German Statistical Office show that only 1 out of 1000 children with initial ages of up to 10 years dies within the following five years (in the 2001 Microcensus, the 1991 birth cohort consists of nine and ten-year olds, while the children of later cohorts are younger). Therefore, this aspect can be safely neglected.[15] Thus, I compute the adjusted size of cohort *i* in any year *t* as follows:

$$Adjusted\ cohort\ size_{i,t} = \#\ of\ \ foreign\ \ families_{i,t} + non-response\ adjustment_{i,t} \qquad (5)$$

This means that for each cohort *i* and year *t* of the Microcensus, I start with the number of families in which a child was born in Germany to foreign-citizen parents who arrived in Germany before 1991. Then, as stating the year of arrival was voluntary up until 2005 but compulsory afterwards, I adjust for the large decline in non-responses to this question and the resulting increase in cohort sizes over the evaluation period. For each cohort and Microcensus year, I assume that the fraction of children whose parents fulfill the minimum residence requirement is the same for the non-response families as for those who answered the year-of-arrival question.[16] To give an example: If the number of children born to foreign-citizens in a certain year was 550, divided into 300 eligible (i.e., children are born in Germany and parents fulfill the minimum residence

[15] The probability to die within five years is a bit higher for newborn children (5 out of 1000), since all the birth-related deaths are counted in for them. So even if the youngest cohorts in the sample (the children born in 2000, 2001, and 2002; see footnote 13) were completely composed of newborns, this difference in mortality rates would be too small to significantly influence estimated return migration rates.

[16] This assumption is supported by the finding that, for each cohort, the fraction of families fulfilling the minimum residence requirement is basically the same over the Microcensus years, independent of whether or not answering the year-of-arrival question is compulsory.

Table 2

Computation of birth-year cohort sizes and 2001-06 RM rates

Cohort	Initial CS 2001 *	Adjusted CS 2001 *	Initial CS 2006	Adjusted CS 2006	Unadjusted RM rate [%]	Adjusted RM rate [%]
1991	231	295	316	333	-36.8	-12.8
1992	253	321	304	313	-20.2	2.7
1993	245	301	317	327	-29.4	-8.5
1994	224	280	317	324	-41.5	-15.6
1995	258	308	264	272	-2.3	11.8
1996	254	309	281	292	-10.6	5.4
1997	263	315	309	317	-17.5	-0.7
1998	246	309	318	326	-29.3	-5.5
1999	275	323	256	260	6.9	19.5
2000	215	257	279	285	-29.8	-11.2
2001	203	239	225	235	-10.8	1.6
2002	193	221	205	207	-6.2	6.6
Mean	238.3	289.9	282.6	290.9	-19.0	-0.6
Std. dev.	25.25	33.65	38.49	40.62	14.8	10.6

* Initial and adjusted cohort sizes in 2001 are reported for the years 2002 and 2003 for the birth cohorts of 2001 and 2002, respectively, since they cannot be identified earlier.

requirement), 200 not eligible, and the remaining 50 did not answer to the year of arrival question, I would add (300/500) x 50 = 30 to the number of 300 eligible children for that year.

Table 2 presents the initial and adjusted cohort sizes in 2001 and 2006, as well as the effect of the non-response adjustment on the resulting estimated RM rates. The reported numbers suggest two observations: First, adjusting for changes in non-response behavior goes a long way towards more realistic RM rates. As we can see in the second-last column, the average estimated cohort RM rate would be minus 19% if this effect is not taken into account. This clearly would not make much sense, since real cohort RM rates cannot be negative.[17] Nevertheless, we still observe an average RM rate of minus 0.6% even with the adjustments, which may indicate that the real fraction of eligible children among the non-response families is even greater than assumed.[18] Second, there

[17] Children who are born in that year and migrate to Germany later cannot influence the estimated RM rate, because only children born in Germany without any later arrival are included in the computation of the cohorts.

[18] This could be if the non-responses were, for instance, completely composed of foreign-citizen parents who were born in Germany themselves and therefore skipped the year-of-arrival question. Counting all

seems to be a large variation in computed RM rates across cohorts ranging from -15.6% to 19.5%. Since there is no reasonable explanation for such large differences, this suggests the presence of measurement error, probably caused by changes in under- or oversampling of cohorts as mentioned above. This measurement error can, however, be assumed to be random and statistically independent of the explanatory variables that will be used in the estimation, assignment status and year of birth. Thus, the estimated coefficients should be unbiased and consistent and only their standard errors inflated (Wooldridge, 2002).

1.7 Results

1.7.1 The Effect on Family Return Migration Rates

Figure 3 displays the computed cohort RM rates for all birth-year cohorts from 1991 to 2002 over different periods of time. The RM rates for periods shorter than 2001-2006 are computed in the same way as described above.[19] The enactment date of the citizenship reform is indicated by the vertical line between birth years 1999 and 2000. The graph reveals two main observations. First, the computed RM rate for the cohort born directly after the enactment of the reform (in 2000) is consistently lower than the one for the cohort born directly before (in 1999) in all panels. This difference is already present in 2002 and 2003, but the gap really opens up in 2004 and remains large afterwards, indicating that there may be an effect of the reform on cohort RM rates which is evolving over time. Second, there seems to be a certain level of noise in the data, as the values of some observations jump up or down from one examined period to the other. This suggests that any result should be interpreted with caution and checked for robustness.

The empirical analysis is conducted by estimating five different RD regressions using the computed RM rates over the whole evaluation period from 2001 to 2006, i.e.,

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cases of non-response into the respective cohort size increases the average RM rate to +3.4%, but does not significantly change the results reported below (see section 6.2).

[19] The only difference is that the Microcensus years 2002 to 2005 do not provide information on whether one or both foreign-citizen parents naturalized themselves after 2000. Since this would lead to undercounting cohorts in which many parents became German citizens during this period, I use the Microcensus of 2006 to identify for each cohort the number of families in which one or both parents naturalized between 2000 and the respective year and add this number to that year's cohort size.

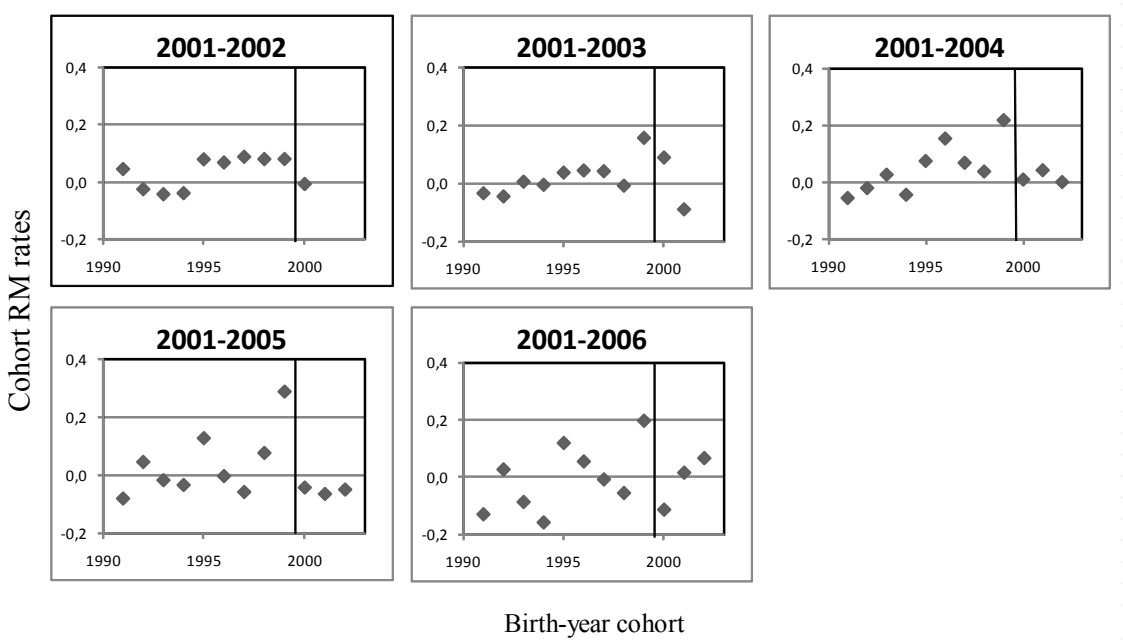

Fig. 3. *Computed cohort RM rates for different time intervals*

Note. The points represent computed cohort RM rates between 2001 and the respective year. The starting date for the calculation is different for the birth cohorts of 2001 and 2002, for which it is 2002 and 2003, respectively, since they can only be identified then. The vertical line indicates the enactment date of the citizenship reform.

those of the last panel in figure 3.[20] The first model only includes the indicator for treatment assignment. The second and most important model is the standard RD regression with a linear trend over birth cohorts and a change in this trend at the cutoff. This specification can be written as:

$$Est.\,RM\ rate_{i,01\text{–}06} = \beta_0 + \beta_1 assignment_i + \beta_2 cohort_i + \beta_3 (assignment_i \times cohort_i) + u_i \quad (6)$$

where *assignment* equals 0 for every birth cohort before 2000 and 1 for all following cohorts. Following Lee and Lemieux (2010), the trend variable *cohort* is centered around the year 2000 as the cutoff date. Thus, the value of the constant can be interpreted as the regression's prediction of RM rates when approaching the cutoff from the left and the coefficient of the assignment dummy, β_1, represents the predicted jump of the RM rate at the cutoff.

Specification three is an extension to the baseline RD specification, additionally allowing for separate quadratic trends over birth cohorts born before and after the

[20] The computed RM rates for the other time intervals are examined and discussed as robustness check in section 6.2.

Table 3

Regression Discontinuity regressions of 2001-06 cohort RM rates

Depvar: Cohort RM rate	(1)	(2)	(3)	(4)	(5)
Assignment	-0.006	-0.213**	-0.261	-0.16	-0.259*
	(0.062)	(0.086)	(0.171)	(0.098)	(0.124)
Cohort[1]		0.024	0.043	0.026*	0.06
		(0.014)	(0.076)	(0.012)	(0.034)
Ass*cohort		0.065***	0.123		
		(0.018)	(0.076)		
Cohort^2			0.002		0.004
			(0.007)		(0.003)
Ass*Cohort^2			-0.041***		
			(0.007)		
Constant	-0.004	0.114	0.149	0.124	0.183*
	(0.04)	(0.085)	(0.171)	(0.078)	(0.088)
R^2	0.001	0.398	0.416	0.331	0.391
Adj R^2	-0.099	0.172	-0.071	0.182	0.163
AIC	-1.405	-1.578	-1.274	-1.639	-1.567

Note . Robust standard errors reported in parentheses.
The sample consists of the cohorts of immigrant children born in Germany between 1991 and 2002, whose parents were foreign citizens at the time of birth and at least one of them arrived in Germany before 1991.
*, **, *** = statistical significance on 10%, 5%, and 1% level

[1] Variable "cohort" is the year of birth of the child centered around 2000.

enactment of the reform. Models four and five are basically the same as specifications two and three, but impose a common trend restriction for the pre- and post-enactment cohorts. Although this deviates from the usual RD practice, it makes sense as additional information in this case, as having only three observations in the post- enactment period renders the estimation of a separate assignment group trend more sensitive to outliers.

Table 3 reports the results of the five RD regression models. Note at this point that the magnitudes reported here should not be taken at face value, since they depend on the assumptions and adjustments used in the derivation of computed RM rates and thus do not perfectly reflect the size of the causal effect on the real RM rates. Nevertheless, since the RM rates are computed in the same way for all birth cohorts, finding significant differences between them should reveal the underlying real trends. In the discussion of the results, I therefore concentrate on *sign* and *significance* of the estimates and abstain from interpreting their sizes.

As we can see from column (1), assignment status by itself does not explain any variation in family RM rates. This result changes a lot in the main specification with a linear trend in RM rates over time. In column (2), we can see that although the trend is not significant in itself (p-value of 0.12), its introduction results in a negative and significant impact of treatment assignment and a large increase in the goodness-of-fit measures reported. Allowing for non-linearity, on the other hand, does not further improve the accuracy of the estimation. While the point estimate for assignment status in column (3) does not change much, it becomes insignificant with a p-value of 0.178. At the same time, including two more explanatory variables decreases the goodness-of-fit of the model a lot as can be seen by the large reduction in the adjusted R^2 measure and the higher Akaike Information Criterion (AIC) of model selection. Interestingly, this pattern is reversed when restricting the respective trends to be the same for the cohorts born before and after the enactment of the reform. In the case of a common linear trend (column 4), assignment status is not statistically significant anymore with a p-value of 0.136, but turns significant on the 10%-level with a common non-linear trend (column 5).

Overall, table 3 suggests that introducing birthright citizenship for immigrant children lowers their families' RM propensity. Among the three specifications with the best fit according to adjusted R^2 and the AIC (columns 2, 4, and 5), two, including the main RD regression, yield negative and significant results for the coefficient of assignment status, while it is also negative and close to significant in the third.

1.7.2 Robustness Checks

To see whether this finding is sensitive to changes in its derivation, I conduct a number of robustness checks. First, I limit the so-called window width, i.e., I restrict the observations used to only three birth cohorts before and after, respectively. That is, I only include the computed RM rates for the cohorts from 1997 to 2002 to check the importance of observations away from the cutoff, since they may only possess limited predictive power in case the trend is not linear over all birth cohorts. Table A.1 in the appendix presents the results for the same specifications as above. Due to the reduced number of observations, the estimation in general gets more imprecise and there are not enough degrees of freedom to estimate the coefficients for model 3. The results for the other specifications, however, show that the estimated effect for assignment status increases in magnitude compared to the broader window width, but it remains at the

margin of being significant. Out of three specifications, only model 4 yields a statistically significant estimate for assignment. This model also provides the best goodness of fit, again according to adjusted R^2 and AIC. The coefficients estimated in specifications 2 and 5 have the same magnitude, but larger standard errors, resulting in p-values of 0.135 and 0.113, respectively. By and large, the results in table A.1 indicate that the estimated effect in section 6.1 seems not to depend crucially on the particular window width chosen.

Second, I test whether different adjustments for the change in non-responses lead to different outcomes. To do this, I look at four alternative assumptions to the one used above, which was that the fraction of eligible families among the non-responses is the same as among the families answering the year-of-arrival question. The examined alternatives are: (A) No adjustment for changes in non-responses at all; (B) the absolute number of non-responses in 2001 is the same as in 2006, with the rest added to the eligible families; (C) the proportion of non-responses among all potentially eligible families (those fulfilling the residency requirement and those without information on year of arrival) is the same in 2001 as in 2006, with the rest again assumed to be eligible; and (D) all non-responses counted as eligible. As can be seen in table A.2 in the appendix, the results for alternatives B to D are mainly in line with the baseline results. While estimated coefficients are somewhat smaller than in the basic scenario, they are still negative and significant in model 2 as the main RD specification, and not far from significance in the two specifications with common trend restriction (p-values around 0.15). The major exception is the case of not adjusting for changes in non-responses at all. In this situation, the coefficient of assignment status is not significant in any of the presented specifications.

Third, I conduct a series of "placebo" experiments in which I use the same data for the computed cohort RM rates as in the baseline scenario, but move the enactment date of the citizenship reform to different points in time. If it is truly the introduction of birthright citizenship that affects immigrant families RM decision, the estimated coefficient for assignment status should be insignificant for all of these hypothetical enactment dates. The results of these placebo experiments are summarized in figure 4, presenting the respective estimates for the assignment indicator under specification 2 for each hypothetical enactment date as a bar. Significant results are presented in black, insignificant ones in grey. Thus, we can easily see that almost all hypothetical enactment dates yield much smaller and insignificant coefficients in comparison to the

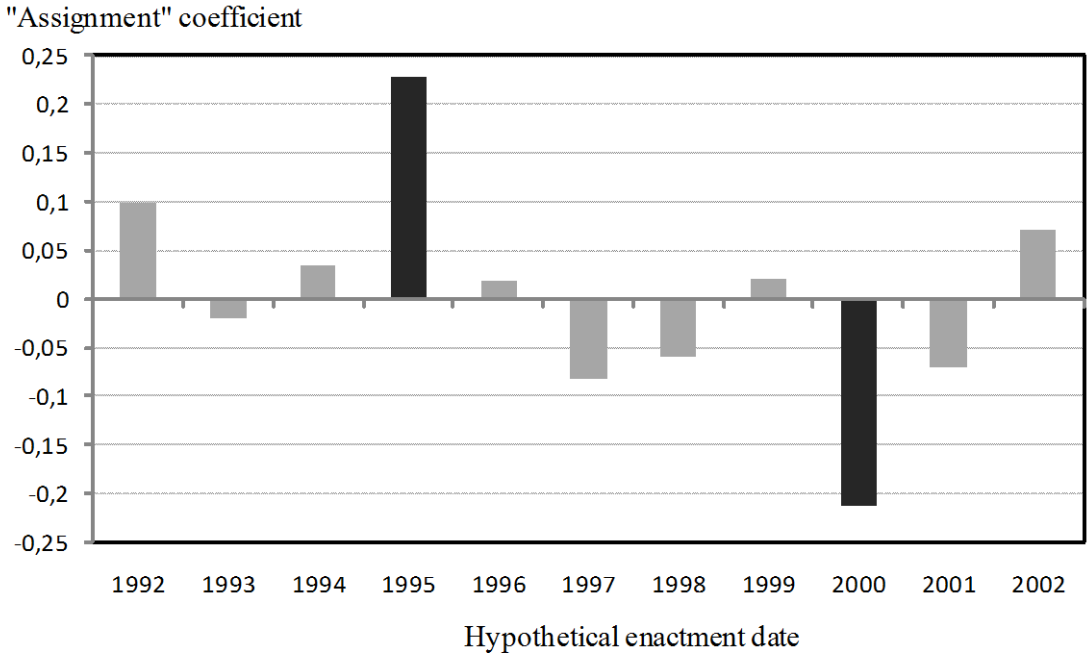

Fig. 4. *Results of Placebo Experiments*

Note. The bars indicate the estimated coefficient for assignment status in regressions in which the enactment of the reform is moved to a hypothetical date. Statistically significant coefficients (10% level) are marked in black.

real cutoff at the beginning of 2000. One hypothetical enactment date, however, the beginning of 1995, does result in a statistically significant coefficient of assignment status on cohort RM rates. This suggests that one cannot completely rule out the possibility that the measured impact of birthright citizenship on family RM is simply due to a large degree of noise in the data rather than conscious reactions of the children's parents.

The final robustness check also tries to provide a feeling for the amount of noise in the data. Since the Microcensus is not a long-running panel, a part of the sample is drawn anew every year.[21] Thus, it could be that the main results presented in this paper were simply driven by a particularly large sampling of assignment group families in the 2006 Microcensus relative to control group families. If this was the case, it would mean that we should observe large swings in the estimated coefficients of assignment status for the different time intervals. To check this, I run the two main models from section 1.7.1 (specifications two and four) using the computed RM rates for the shorter time

[21] The Microcensus follows a rotating sampling scheme in which every household remains in the sample for four years and every year one quarter is renewed. This means that after four years, none of the original households is remaining in the sample anymore.

intervals from 2001 to 2002, 2003, 2004, and 2005, respectively. Table A.3 in the appendix presents the results of specification two (allowing for a different linear time trend after the enactment) in panel A and those of specification four (imposing a common trend restriction) in panel B. It shows that the estimated coefficients for assignment status are consistently negative for all considered time periods and more or less increasing in magnitude over time. Although the result for the 2001-03 period goes against this trend, the overall pattern looks more like a gradual buildup of the effect than random fluctuations from year to year.

To sum up, the results of sections 1.7.1 and 1.7.2 indicate that the introduction of birthright citizenship for immigrant children significantly reduces family RM rates. Furthermore, although it cannot be ruled out completely that the effect is driven by noise in the data, it seems to be robust with respect to a number of changes in its derivation, but mostly at the margin of statistical significance.

1.8. Conclusion

The purpose of this paper is to evaluate the effect of introducing the birthplace principle in the German citizenship law in 2000 on the RM behavior of immigrant families. To gauge the size of a potential effect, I apply a regression discontinuity design to compare the computed RM rates of foreign-citizen families in Germany with children born around the enactment date, the 1st of January, 2000. The results provide evidence that granting citizenship to children reduces the likelihood that their families return to the parents' home country. This finding is quite stable with respect to a number of changes in the derivation of the dependent variable, cohort RM rates, and the chosen model specification. It thus supports the argumentation in Dustmann (2003) and Djajic (2008) that children's characteristics and expected future outcomes significantly influence their parents' return plans.

The results of this paper have two important implications: First, they suggest that acquiring citizenship has an impact on the decisions of immigrant families by itself and not only through self-selection of those who are more willing to remain and integrate in the host country in the first-place. This means that relaxing naturalization regulations could potentially help to increase integration efforts of immigrants. Second, it seems that introducing birthright citizenship for immigrant children may also be a small contribution to alleviate the demographic changes taking place in most advanced

countries. A short back-of-the-envelope calculation may give an idea of the importance of the results in terms of real numbers. Since the introduction of birthright citizenship, about 40,000 newborn children with foreign-citizen parents obtained the German citizenship at birth every year. Taking the most conservative significant estimate of all specifications presented (a reduction of about 15%), this would mean that this component of the new citizenship law alone may have caused up to 60,000 families to remain in Germany over the first decade after enactment.

The main drawback of the presented analysis lies in the use of repeated cross-sectional data, which means that family RM decisions cannot be directly observed but have to be estimated on the cohort level, making them vulnerable to sampling biases and measurement error.

Further research on the impact of granting birthright citizenship to the children of immigrants using the same identification strategy is already on the way, since the "as-if" random treatment assignment depicts an appropriate way to circumvent the potential endogeneity of getting naturalized for a whole array of other integration related outcomes. In a companion paper (Sajons, 2011), I examine the effect of birthright citizenship on parental integration behavior in terms of own naturalization of the parents, as well as their social and labor market integration. The most important effects of the new law, however, will most likely only surface over time, when the treated children grow up and enable researchers to evaluate the effect of citizenship on such crucial aspects as educational performance and labor market outcomes.

Appendix to Chapter 1:

Table A.1

RD Regressions of 2001-06 RM rates for cohorts 1997 to 2002

Depvar: Cohort RM rate	(1)	(2)	(3)[2]	(4)	(5)
Assignment	-0.054	-0.346		-0.34**	-0.34
	(0.093)	(0.142)		(0.102)	(0.125)
Cohort[1]		0.101		0.095**	0.095*
		(0.061)		(0.026)	(0.024)
Ass*cohort		-0.012			
		(0.063)			
Cohort^2					0.00
					(0.011)
Ass*Cohort^2					
Constant	0.044	0.247		0.235*	0.235
	(0.077)	(0.14)		(0.082)	(0.123)
R^2	0.079	0.722		0.719	0.719
Adj R^2	-0.152	0.305		0.532	0.298
AIC	-1.242	-1.773		-2.097	-1.764

Note. Robust standard errors reported in parentheses.
The sample consists of the cohorts of immigrant children born in Germany between 1997 and 2002, whose parents were foreign citizens at the time of birth and at least one of them arrived in Germany before 1991.
*, **, *** = statistical significance on 10%, 5%, and 1% level

[1] Variable "cohort" is centered around 2000.

[2] Not enough degrees of freedom to compute proper significance levels.

Table A.2

RD Regressions of 2001-06 RM rates with different adjustments for non-responses

	A. No adjustment for changes in NRs					B. Number of NRs in 2001 equal to 2006				
Explanatory Variables:	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Assignment	0.022	-0.185	-0.234	-0.132	-0.215	-0.047	-0.171*	-0.174	-0.131	-0.176
	(0.073)	(0.121)	(0.242)	(0.13)	(0.183)	(0.047)	(0.076)	(0.161)	(0.085)	(0.117)
Cohort[1]		0.024	0.033	0.026	0.054		0.012	0.007	0.014	0.03
		(0.017)	(0.107)	(0.016)	(0.048)		(0.012)	(0.071)	(0.011)	(0.032)
Ass*cohort		0.066*	0.246*				0.05**	0.135		
		(0.032)	(0.107)				(0.017)	(0.071)		
Cohort^2			0.001		0.003			-0.001		0.002
			(0.01)		(0.004)			(0.007)		(0.003)
Ass*Cohort^2			-0.096***					-0.039***		
			(0.01)					(0.007)		
Constant	-0.181***	-0.063	-0.046	-0.052	-0.003	0.074**	0.136	0.125	0.144*	0.17*
	(0.049)	(0.113)	(0.242)	(0.104)	(0.127)	(0.032)	(0.074)	(0.161)	(0.068)	(0.081)
R^2	0.006	0.271	0.304	0.225	0.253	0.061	0.267	0.281	0.209	0.228
Adj R^2	-0.093	-0.003	-0.276	0.053	-0.027	-0.033	-0.008	-0.318	0.034	-0.061
AIC	-0.995	-0.971	-0.684	-1.077	-0.947	-1.875	-1.79	-1.476	-1.88	-1.737

Note . Robust standard errors reported in parentheses.

The sample consists of the cohorts of immigrant children born in Germany between 1991 and 2002, whose parents were foreign citizens at the time of birth and at least one of them arrived in Germany before 1991. In panel A, no adjustment for non-responding families has taken place. In panel B, the absolute number of non-responses in 2001 is assumed to be the same as in 2006, with all excess non-responses counted towards the eligible families.

*, **, *** = statistical significance on 10%, 5%, and 1% level

[1] Variable "cohort" is centered around 2000.

Table A.2 continued

	C. Proportion of NRs in 2001 the same as in 2006					D. Every NR family counted as eligible				
Explanatory Variables:	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Assignment	-0.046	-0.165*	-0.172	-0.127	-0.173	-0.045	-0.167*	-0.172	-0.128	-0.174
	(0.045)	(0.074)	(0.156)	(0.082)	(0.113)	(0.045)	(0.074)	(0.157)	(0.082)	(0.113)
Cohort[1]		0.012	0.009	0.013	0.029		0.012	0.008	0.014	0.03
		(0.012)	(0.069)	(0.01)	(0.031)		(0.012)	(0.069)	(0.011)	(0.031)
Ass*cohort		0.048**	0.132				0.049**	0.124		
		(0.016)	(0.069)				(0.016)	(0.069)		
Cohort^2			0		0.002			0		0.002
			(0.006)		(0.003)			(0.006)		(0.003)
Ass*Cohort^2			-0.04***					-0.036***		
			(0.006)					(0.006)		
Constant	0.076**	0.135*	0.129	0.143*	0.17*	0.071**	0.132	0.125	0.14	0.167*
	(0.031)	(0.072)	(0.156)	(0.066)	(0.078)	(0.031)	(0.073)	(0.157)	(0.067)	(0.079)
R^2	0.062	0.265	0.28	0.208	0.229	0.06	0.27	0.282	0.211	0.233
Adj R^2	-0.032	-0.01	-0.32	0.032	-0.06	-0.034	-0.003	-0.316	0.036	-0.055
AIC	-1.947	-1.858	-1.545	-1.95	-1.81	-1.939	-1.859	-1.542	-1.947	-1.808

Note . Robust standard errors reported in parentheses.

The sample consists of the cohorts of immigrant children born in Germany between 1991 and 2002, whose parents were foreign citizens at the time of birth and at least one of them arrived in Germany before 1991. In panel C, the proportion of non-responding families among all potentially eligible families in 2001 is assumed to be the same as in 2006, with all the excess non-responses in 2001 counted as eligible families. In panel D, every non-responding family is assumed to be eligible.

*, **, *** = statistical significance on 10%, 5%, and 1% level

[1] Variable "cohort" is centered around 2000.

Table A.3

RD Regressions for different time intervals

	A. Different trend					B. Common trend				
Explanatory Variables:	2001-02	2001-03	2001-04	2001-05	2001-06	2001-02	2001-03	2001-04	2001-05	2001-06
Assignment	-0.112***	-0.016	-0.159**	-0.214*	-0.213**	-0.112***	-0.104	-0.183**	-0.237*	-0.16
	(0.021)	(0.053)	(0.06)	(0.1)	(0.086)	(0.021)	(0.097)	(0.063)	(0.108)	(0.098)
Cohort[1]	0.014*	0.017*	0.026**	0.025	0.024	0.014*	0.015*	0.025***	0.024	0.026*
	(0.006)	(0.008)	(0.008)	(0.015)	(0.014)	(0.006)	(0.008)	(0.008)	(0.014)	(0.012)
Ass*cohort	(dropped)	-0.195***	-0.030*	-0.029	0.065***					
		(0.008)	(0.014)	(0.016)	(0.018)					
Constant	0.106***	0.106*	0.180**	0.164	0.114	0.106***	0.098*	0.175***	0.16	0.124
	(0.021)	(0.053)	(0.058)	(0.1)	(0.085)	(0.021)	(0.051)	(0.054)	(0.093)	(0.078)
R^2	0.486	0.73	0.617	0.458	0.398	0.486	0.316	0.592	0.446	0.331
Adj R^2	0.34	0.615	0.474	0.255	0.172	0.34	0.144	0.501	0.322	0.182
AIC	-2.959	-3.234	-2.606	-1.680	-1.578	-3.159	-3.234	-2.708	-1.823	-1.639

Note . Robust standard errors reported in parentheses. The table reports in panel A the results of the baseline RD estimation for different time periods as indicated. In panel B, a common trend restriction is imposed on the regressions.

*, **, *** = statistical significance on 10%, 5%, and 1% level

[1] Variable "cohort" is centered around 2000.

2. Does Immigrants' Integration Behavior Change When Their Children Are Born With The Host-Country Citizenship?

2.1 Introduction

Most developed countries experience increasing shares of immigrants and their descendants in their population, often accompanied by the typical problems of integration like ghettoization, lower education, higher unemployment, and (perceived) higher crime rates. These problems frequently spark heated public debates about the size and value of immigration and the integration of foreigners. In this debate, many institutions, researchers, and lawmakers propose to facilitate access to the host country citizenship for adult migrants and automatic citizenship for their children as a means to foster integration efforts and outcomes among this part of the population (e.g., OECD, 2010). It is, however, very difficult to evaluate the true causal effect of obtaining citizenship of the host country on *individual* integration behavior for two reasons: First, the decision to naturalize is voluntary and therefore involves a high degree of self-selection of those who are willing to integrate into naturalization. Second, eligibility for naturalization often requires that potential applicants have already achieved a certain level of integration, leading to the problem of reverse causality. Both reasons result in biased estimates of the effect of obtaining citizenship on individual integration behavior, effectively preventing a clean estimation of whether becoming a citizen of the host country by itself increases integration efforts.

In this paper I evaluate the effect of a policy in Germany granting *automatic* host-country citizenship to immigrant children at birth on their *parents'* integration behavior. I thus extend a previous investigation by Avitabile et al. (2010) on the impact of giving immigrant parents the *option* to apply for citizenship for their children. Furthermore, I broaden the focus of the analysis from looking exclusively at indicators of social integration to measures of formal and labor market integration. This is highly interesting from a political economy point of view, as granting citizenship to the children of immigrants tends to be much more accepted in many countries than easing access to citizenship or tolerating dual citizenship for the adult immigrants themselves. Therefore, this measure may become increasingly more popular and important, and

likewise knowing about its effects, not only on the treated children themselves in the future, but also immediately on the parent generation.

In principle, the citizenship status of immigrant children is correlated with their parents' willingness to integrate into the host country in the same way as the one of their parents, as especially those children will obtain the host-country nationality whose parents are eager to integrate. The introduction of birthright citizenship for the children of immigrants in Germany, however, provides an exogenous source of variation in the citizenship status of immigrant children which can be used to obtain unbiased estimates of the causal effect of citizenship for children on their parents' integration behavior. Starting on January 1st, 2000, children who are born in Germany to two foreign-citizen parents become German citizens at birth if at least one of their parents has legally lived in Germany for more than eight years already. This "treatment" is automatic and independent of the parents' willingness to integrate into the host-country society, effectively removing self-selection and reverse causality into obtaining citizenship as potential driving factors behind any results.

To identify the causal effect of citizenship for the child on parental integration behavior, I compare the integration behavior of parents with children born in the year before and the year after the enactment of the reform. For the empirical analysis, I use pooled data from the German Microcensus covering the years 2001 to 2008, and the 2000-2005 waves of the German Socio-Economic Panel (GSOEP). The results suggest that the introduction of birthright citizenship has negative effects on the parents' willingness to apply for naturalization themselves (formal integration), but positive effects on the parents' German language proficiency (social integration), where the latter result is consistent with the findings of Avitabile et al. (2010). With respect to labor market integration, the effect is unclear. The main estimates point towards a negative impact on fathers' employment status and number of hours worked, but the results of a series of robustness checks suggest the presence of substantial noise in the data for these variables. All in all, these outcomes imply that while granting citizenship to immigrant children may foster the integration of the affected children themselves, it does not, at the same time, improve their parents' integration on all dimensions.

The rest of the paper is organized as follows: Section 2 relates the topic to existing research. Section 3 then provides the details of the reform of the German Citizenship Law in 1999, followed by theoretical considerations on why and how citizenship for the child may affect the parents' 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. Section 7 presents the main empirical results, as well as some extensions and robustness checks. Finally, section 8 discusses the conclusions and political implications of the results.

2.2 Related Research

This paper relates closely to several existing studies about the impact of the reform of the German citizenship law in 1999 and its different components on the affected foreign-citizen population in Germany. Recent research has shown that this reform substantially affected the behavior of targeted immigrant families in various outcomes. Avitabile et al. (2010) analyze the impact of a *transition regulation* in the new law that allowed parents of children born between 1990 and 1999 to apply for the German citizenship for their children throughout the year 2000 if the parents met the eligibility requirements for birthright citizenship at the time of birth. Comparing eligible families with last children born between 1990 and 1999 and control families whose last child was born in the 1980s, they find positive effects for the parents of these children on a number of outcomes related to *social* integration (German proficiency, reading German newspapers, and visiting or hosting German friends). Furthermore, Piracha and Zhu (2008) use a difference-in-difference approach comparing the affected immigrant population with the corresponding German natives to examine the *joint* effect of the different components of the reform on precautionary savings and remittance payments. Their results suggest that the law change led to a significant reduction in precautionary saving and remittance payments. Finally, in a companion paper to this (Sajons, 2010), I apply a regression discontinuity design to evaluate the effect of *automatic* birthright citizenship for the child on family return migration decisions. The results suggest that families with children born in the year after the enactment of the reform are more likely to remain in the host country than families with children born in the year before.

With the current paper, I contribute to this literature in three ways: First, by examining the effect of *automatic* citizenship for children on their parents' integration behavior compared to offering them the opportunity to *apply* for citizenship for their children as studied in Avitabile et al. (2010). This is the necessary second step to complete the analysis of the two child-related components of the reform.

Second, I broaden the focus of the analysis from looking exclusively at one dimension of integration outcomes (e.g., at social integration in Avitabile et al., 2010) to providing a more complete overview on the effects of birthright citizenship on different dimensions of integration. More specifically, I look at various measures of formal and social integration, as well as the migrants' labor market attachment.

And third, by using more similar comparison groups for the analysis than what has been done so far, I try to come closer to identifying the causal effect of the citizenship status of the child on the parents' integration. While previous papers relied on identification strategies comparing migrants with natives (e.g., Piracha and Zhu, 2008) or migrant families with children born in the 1990s with migrant families with children born in the 1980s (Avitabile et al., 2010), I compare the same type of migrant families with each other, where the major difference is whether their children are born immediately before or after the enactment of the new law, i.e., in 1999 or 2000.

With these contributions, the paper also relates to two more strands of literature: First, the research dedicated to evaluating the effect of citizenship in general on individual integration. Most of the focus here lies on labor market integration in terms of wages (e.g., Chiswick, 1978; Bratsberg et al., 2002; Steinhardt, 2008; Bratsberg and Raaum, 2011) and employment probabilities (e.g., Duguet et al., 2007; 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.

And even more broadly, this paper also adds to the literature examining whether and how children's characteristics and circumstances influence their parents' behavior. This direction of causal effects is much less frequently examined than the other way round, but there are several studies in different areas. Examples include Angrist and Evans' (1998) article on the effect of having a third child on parental labor supply, Dustmann (2003) evaluating the importance of the child's gender for the return decision of migrant couples, Duran's (2003) work on how children's homework from school improves the parents' knowledge of the host-country language, and Washington's (2008) study on how the presence of daughters influences the legislative behavior of US congressmen on women's issues.

2.3 The Reform of the German Citizenship Law in 1999

At the end of the 1990s, about 7.3 million foreign-citizens were legally living in Germany, which was about 9% of the total population. According to the German Statistical Office, about 40% of these individuals had already lived in Germany for at least 15 years and over 30% even for more than 25 years (Statistisches Bundesamt, 2001). One of the main reasons for the existence of such a large long-term foreign-citizen population was the very restrictive German citizenship law by which a person could only become a German citizen if one of the ancestors had been German (the principle of *jus sanguini*). Before 1990, no legal entitlement existed for naturalization, independent of how long somebody had lived in Germany already and how well integrated the person was (Brubaker, 1992). After a change in 1990, foreign-citizens became entitled to naturalization if they had legally lived in Germany for at least 15 years and renounced their former citizenship.

Reforming the German citizenship law was one of the first major initiatives of the newly-elected government under Chancellor Schröder in 1998/99. It aimed at improving the integration of foreign citizens who had been living in Germany for a long time into the German society (Coalition Treaty, 1998). The final version of the reform was passed in July 1999 and came into effect on January, 1st, 2000. The two main elements were:[22] (1) A *reduction in the minimum residency requirement* from 15 to 8 years,[23] and (2) the *introduction of birthright citizenship* for the children of immigrant parents, if at least one of the parents has legally lived in Germany for more than eight years and possesses permanent residence permission. Under these conditions, children of foreign citizens automatically obtain the German citizenship at birth together with the citizenship of their parents. This dual citizenship can go on until a child turns 23, when it has to choose between the two nationalities at the latest.

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 as described above at the time of giving birth, they could apply to get the same treatment for their children as if they were

[22] For more details of the reform see Avitabile et al. (2010) and Sajons (2010).

[23] The law also introduced new requirements for obtaining citizenship, for instance, a sufficient knowledge of the German language, an oath to the German constitution, and the non-receipt of welfare benefits.

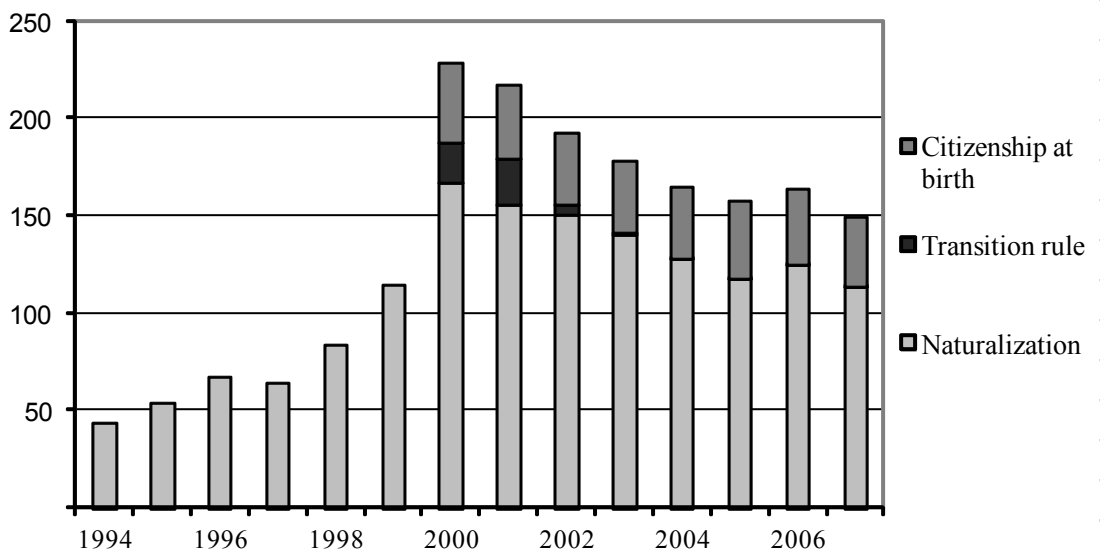

Fig. 1. *Acquisition of German Citizenship by Type [in 1000], 1994-2007*
Source: Sajons (2010)

born after the law change. The application period was limited, however, to the calendar year 2000.

Figure 1 (taken from Sajons, 2010) depicts the number of foreign citizens acquiring the German nationality between 1994 and 2007 and the effect of the new law broken down by component. In numbers, the reduction of the minimum residency requirement was followed by an increase in naturalizations, from about 114,000 in 1999 to 166,000 in 2000 directly after the reform. After this initial boost, however, the numbers went down 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 trough this channel.

Contrary to these short-lived effects, the introduction of automatic birthright citizenship led to about 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 to examine the other effects of this measure more closely, first on the parents of these children and later on the treated children themselves.

2.4 Theoretical Considerations

The impact of granting citizenship to immigrant children on the integration behavior of their parents is not clear-cut and may work through a variety of different channels with potentially different effects.

The first and most intuitive effect is that the parents may feel *more attached to the host country* and its population. This could happen because parents start to identify more with the natives or because they perceive the citizenship for their child as a sign of goodwill of the host country and want to reciprocate. Either way, the parents may be expected to undertake greater efforts to assimilate, i.e., to behave like the natives, which should be seen, for instance, in more parents becoming German citizens themselves and improvements in social integration measures like German language proficiency and the frequency of contacts with natives. In the long run, this should also have a positive impact on the parents' labor market position, as language proficiency and networks that include natives are important determinants for job and earnings perspectives (see, for instance, Dustmann and Fabbri, 2003, and Gonzalez, 2005, for language skills, and Bertrand et al., 2000, for social networks).

On the other hand, there are other mechanisms that could lead to a relative deterioration in integration outcomes with respect to "untreated" parents. If, for instance, foreign-citizen parents are searching for a way to signal their individual willingness to integration, having a child with the German citizenship may act as a *substitute for more costly measures* like naturalizing themselves, attending language courses, or even buying a house. Besides, the improved employment and earnings prospects of the child in the future may be considered by the parents as a *positive shock to the expected lifetime family income*, which could prompt them to smooth their income, leisure, and consumption paths over time by increasing present consumption and reducing current work and savings efforts. If parents behave like that, being "treated" would thus lead to worse labor market integration.

Apart from these direct effects on the behavior of parents who remain in Germany independent of whether their child becomes German citizen, there may also exist an *impact on the composition* of foreign-citizen families living in Germany. If parents take the greater job and earnings perspectives of their child in the host country into account when they consider returning to their home country, some may decide to remain in Germany for the sake of their child although they would have left otherwise

(Djajic, 2008). This seems to happen in Germany, as Sajons (2010) shows that foreign-citizen parents are less likely to return to their home countries if they have a child with German citizenship. From a theoretical point of view, however, it is unclear in which direction this should influence integration behavior, as different schools of thought exist on the reasons why migrants 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 savings goals (Constant and Massey, 2002). If it is mostly the unsuccessful who decide to remain in Germany, we would expect a negative effect on integration outcomes as labor market participation or home ownership. In contrast, if obtaining the German citizenship for the child convinces skilled and successful migrants to stay longer or permanently, the composition effect would be positive.

Finally, changes in return migration incentives could have an additional effect apart from its influence on the composition of foreign-citizen parents by *changing their expectations about the duration of their stay in the host country*. As Dustmann (1997) shows, temporary migrants tend to be more attached to the host country's labor market than permanent migrants, as they face worse economic conditions at home and want to get as much out of their limited stay in the host country as possible. If the introduction of birthright citizenship causes parents to change their intentions from temporary to permanent residence in Germany, this could result in a weaker labor market integration of the remaining target population.

Given these different channels of influence and their potentially contrary effects, it is thus hard to predict the overall impact of granting citizenship to immigrant children on their parents' integration outcomes with existing theoretical models. In order to evaluate the reform's success on this dimension, an empirical analysis is therefore required.

2.5 Identification Strategy

The strategy used in this paper to identify the causal effect of birthright citizenship for immigrant children on their parents' integration behavior is very similar to the one used in the companion paper (Sajons, 2010). Its main approach is to compare integration outcomes between long-term migrant families with children* born in the year directly before and after the enactment of the reform (forming control and treatment group, respectively). To get a broader

overview on the impact on different integration dimensions, I evaluate a number of integration-related outcomes such as naturalization of the parents (formal integration), employment and hours worked (labor market integration), and German language proficiency (social integration). The aim of this identification strategy is to disentangle the effect of automatic citizenship at birth from three other factors that could drive adjustments in the integration behavior of foreign-citizen parents.

First, the *adult component of the law*, i.e., the reduction in the minimum residence requirement from 15 to 8 years for adult immigrants. This means that the same main eligibility condition has to be fulfilled both for the individual naturalization of the parents and to obtain birthright citizenship for the children. Attempts to identify the effect of granting citizenship to immigrant children by comparing eligible migrant families with ineligible ones or native families would therefore lead to results reflecting the joint impact of both components. Examining two groups which are similarly affected by the adult component but differ with respect to the introduction of automatic birthright citizenship for their children solves this problem. That is, I restrict the sample to foreign-citizen families with children born in 1999 or 2000 in which at least one parent came to Germany prior to 1991 or was born there herself. Thus, the requirements for both the naturalization of the parents and automatic citizenship for the child are fulfilled for both treatment and comparison group and the difference in the treatment intensity only comes from the fact that about half of the children are born in 2000, when they automatically become German citizens, and the other half in 1999, when their parents only obtained the opportunity to apply for the German citizenship for their children.

This restriction of the sample additionally deals with the second potentially confounding factor, *differences in trends* in integration behavior between the comparison groups. As the two groups are "drawn" from the same pool of individuals and thus should be almost identical in terms of composition, they should experience the same developments over time like changing labor market conditions in Germany relative to the respective countries of origin or shifting attitudes toward foreigners among the native population.

Most importantly, the chosen approach also allows to disentangle the effect of automatic birthright citizenship from the *potential endogeneity* between observed integration outcomes of the parents and reported German citizenship status of the child. Since the parents' underlying willingness to integration is not observed but very likely positively correlated with both their own integration outcomes and whether they report the German citizenship for their children[24], any estimate of the effect that uses the reported "treatment" (i.e., the German

[24] This could be due to three reasons: a) a higher propensity to naturalize the whole family, b) a higher probability to apply for the German citizenship only for their children under the transition regulation of

citizenship status of the child) will obtain biased results in favor of finding a more positive impact of host-country citizenship for the children.

This likely endogeneity is addressed in this paper by using *treatment assignment* instead of reported "real treatment" as the main explanatory variable of interest. As the law provides clear eligibility requirements for automatically receiving the German citizenship at birth and an exogenously determined, sharp starting date, I can identify and distinguish the two groups of foreign-citizen families by whether they are assigned or not to the automatic treatment. Thus, the main variable of interest is a dummy variable "assignment" which is coded as 1 for those families with children born in 2000, and as 0 for those with children born in 1999.[25]

The estimated coefficient of "assignment" captures the unbiased "intent-to-treat" (ITT) effect of introducing automatic birthright citizenship under three conditions (Imbens and Lemieux, 2008; and Lee and Lemieux, 2010): First, the enactment date has to be exogenously determined and clearly defined. In the case of introducing automatic citizenship at birth, this is certainly fulfilled, as the German parliament independently set the starting date for the new law to the 1st of January, 2000.

Second, potentially affected families cannot precisely sort into either assignment status, i.e., they cannot perfectly manipulate the birth date of their children around the enactment date. In combination with condition 1, this leads to an "as-if" random distribution of families to either side of the enactment date, which creates a credible counterfactual for the assignment group as in a randomized experiment and thus allows to infer causal effects.

If families were able to precisely determine the birth year of their children, 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 by all means would get it in 1999. In such a situation, the difference in integration outcomes between the two groups would not reflect the clean causal effect of introducing birthright citizenship. In our case, however, there are several reasons why such a sorting 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, so all births up to October 1999 were already predetermined and there was hardly any scope for postponing or advancing a planned child for some months.

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the law, and c) a potentially greater accuracy in reporting a possible German citizenship status of the child.

[25] To distinguish clearly between the two groups, I exclude families with children born in both years from the analysis.

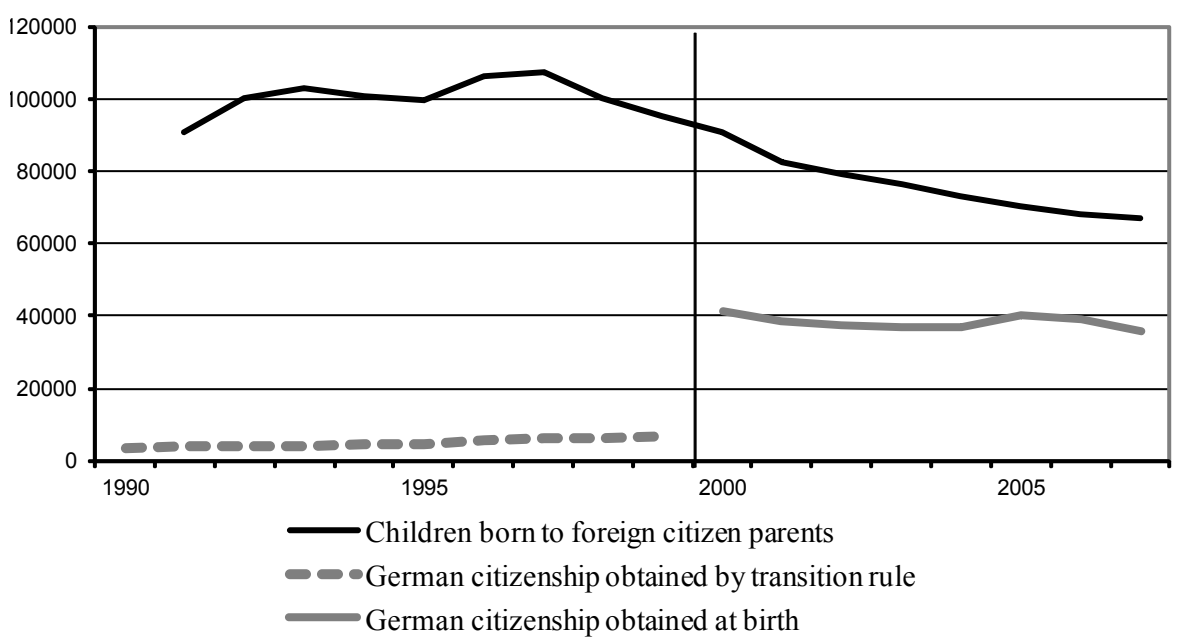

Fig. 2. *Foreign-citizen Children and Receipt of German Citizenship by Year of Birth*
Source : Sajons (2010)

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.

Last but not least, there has to exist a significant difference in the treatment intensity around the cutoff, i.e., there has to be a significant effect of "assignment" on the real "treatment". This can be shown using official data from the German Statistical Office. They report that only 7.1% of newborn children of foreign-citizen parents in 1999 obtained the German nationality on application following the transition regulation in the new law, whereas 45.6% of the newborn children in the birth cohort of 2000 fulfilled the requirements for automatic birthright citizenship and thus became Germans at birth (Statistisches Bundesamt, 2010).

The statistical evidence supporting points two and three is summed up in figure 2, taken again from Sajons (2010). 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 the trend 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.[26]

Figure 2 also illustrates the large and discontinuous change in treatment intensity around the enactment date (the vertical line). While only a small percentage of children born in the years between 1990 and 1999 eventually obtained the German citizenship through the transition rule of the law (the dotted grey line), we observe a large increase to close to 50% after the introduction of automatic birthright citizenship beginning in 2000 (the solid grey line).

Taken together, the descriptive numbers presented in this section provide support for the validity of the chosen empirical approach and the claim that it can be used to identify the causal effect of granting automatic citizenship at birth.

2.6 Data

To conduct the analysis, I use pooled data from the German Microcensus[27] for the years 2001-2008 as well as pooled observations from the 2000-2005 waves of the German Socio-Economic Panel (GSOEP)[28]. The Microcensus is a yearly conducted cross-section of 1% of German households which is used to compute the official statistics of the country's demographic development and labor market situation. Apart from its large sample size of about 270.000 households in the scientific-use files, there are three main advantages of using the Microcensus: First, it includes the current citizenship status of the respondents, which is necessary to identify the target groups of a change in the citizenship law. Second, parents also report the citizenship status of their children in the Microcensus. This allows me to get an impression of the size of the endogeneity involved by comparing the regression results when using the reported citizenship of the child as main explanatory variable with those including assignment to automatic birthright citizenship instead. And third, answering most Microcensus questions is mandatory, so that the loss of observations due to non-responses to crucial questions is limited.

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[26] Avitabile et al. (2011) show results suggesting that the introduction of birthright citizenship may have actually *decreased* fertility for the target group, as parents would now be willing to invest more in their children instead of having more of them. The constant to slightly increasing fraction of children born by eligible parents after 2000 in figure 2, however, does not seem to confirm this result.

[27] For the empirical analysis, I used the remote processing tool JoSuA developed by the IDSC of IZA (see Askitas, 2008, for details).

[28] Several variables of interest were dropped from the GSOEP questionnaire after 2005, so only the waves before are included.

Table 1

Descriptive statistics by birth year cohort (Microcensus 2001)

	Birthyear 1999 (Control)			**Birthyear 2000 (Assignment)**		
	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.
North	0.103	(0.305)	262	0.115	(0.320)	200
East	0.069	(0.253)	262	0.090	(0.287)	200
South	0.359	(0.481)	262	0.295	(0.457)	200
West	0.469	(0.500)	262	0.500	(0.501)	200
Num. of kids	2.088	(1.074)	262	2.035	(1.063)	200
Parents married	0.916	(0.278)	262	0.940	(0.238)	200
HH net income	1791	(698)	258	1826	(742)	195
Age child	**1.27*****	(0.447)	262	**0.32*****	(0.466)	200
Male child	0.534	(0.500)	262	0.495	(0.501)	200
German cit. of the child	**0.115*****	(0.319)	262	**0.46*****	(0.500)	200
Age mother	**29.3****	(5.874)	259	**28.1****	(5.198)	194
Secondary educ. mother	0.522	(0.501)	203	0.572	(0.496)	159
Mother born in GER	0.239	(0.428)	259	0.242	(0.430)	194
Years since arrival mother	13.750	(8.053)	196	13.014	(8.571)	147
EU origin mother	0.193	(0.395)	259	0.191	(0.394)	194
Turkish origin mother	0.564	(0.497)	259	0.521	(0.501)	194
Age father	32.113	(5.868)	239	31.347	(5.904)	190
Secondary educ. Father	**0.521****	(0.501)	190	**0.629****	(0.484)	170
Father born in GER	0.197	(0.398)	239	0.163	(0.370)	190
Years since arrival father	16.599	(8.230)	192	15.639	(8.131)	158
EU origin father	0.184	(0.388)	239	0.184	(0.389)	190
Turkish origin father	0.586	(0.494)	239	0.537	(0.500)	190

*, **, *** = significant difference between the two groups on the 10%, 5%, and 1% level, respectively.

Note: The sample consists of families with children born in Germany in 1999 or 2000, both parents were foreign citizens in 2000, at least one of them arrived in Germany prior to 1991, and none arrived later than 2000.

Data: *Microcensus 2001*

The GSOEP is a multi-year household panel starting in 1984 with up to 15,000 participating households in each year. By design, the GSOEP oversamples migrant households in order to enable research on migration and integration related issues. For the same reason, it also includes a series of questions with respect to the social integration of migrants which are not typically asked in other surveys like the Microcensus, e.g., proficiency in German, contacts with German natives, and political interest.

To identify the two comparison groups, I impose the following restrictions: (1) parents of a child born in either 1999 or 2000, (2) the child was born in Germany, (3) both parents (or the single parent) had to be foreign citizens in 2000, (4) at least one parent arrived in Germany prior to 1991 and no parent arrived after 2000. Additionally, I excluded families with children born in both years to have a clear separation of the two groups. Table 1 presents the descriptive statistics for the resulting samples in the Microcensus of 2001, the first in which both groups can be identified. It confirms the assumption made earlier that the two groups are very similar on a wide range of observable characteristics, including the region of living in Germany, the number of children, household income, and the fraction of male children, as well as the parents' marital status, number of years in the country, and non-EU origin. The only characteristics of the parents that differ significantly between the two groups are the age of the mothers and the fraction of fathers who report to have obtained secondary education. By construction, the average age of the children differs by exactly one year between the two groups and the fraction of children for which a German citizenship is reported by as much as 35%.[29]

2.7 Results

2.7.1 Main results

For the empirical analysis of the Microcensus data, I use two different sets of regressions for each integration outcome. The first can be stated as follows:

$$INT\_MEASURE_{it} = \alpha_0 + \alpha_1 GER\_CIT_{it} + \gamma CONTROLS_{it} + \varepsilon_{it} \qquad (1)$$

This is a simple estimation of the difference in a particular integration outcome between those immigrant parents *i* in time *t* whose children are German citizens (GER_CIT_{it}=1) and those whose children are not (GER_CIT_{it}=0). The vector of $CONTROLS_{it}$ is the same in every regression and includes the parents' age,

[29] Two reasons could explain why only 46% of the children in the assignment group reportedly possess the German citizenship in 2001: 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 interviews for the 2001 Microcensus were conducted in early April 2001, it is likely that many parents of children born in the second half of 2000 had not received the final confirmation yet.

educational attainment (primary, secondary, tertiary), years since arrival, marital status, and regional origin (EU, Turkey, rest of the world), as well as whether they are born in Germany themselves, the age of the youngest child in the family, and region of living (north, west, south, east), quarter of interview, and year fixed effects. This specification provides a measure of the observed effect of the "real treatment", i.e., whether those parents who report to have a child with the German citizenship act systematically different than those without. As described above, this measure is likely biased, as those parents who are more willing to integrate may self-select into the reported "treatment".[30]

The second set of regressions is almost identical to the first, but replaces the indicator for real treatment by the indicator for whether a child is born after enactment, $ASSIGNMENT_{it}$.

$$INT\_MEASURE_{it} = \alpha_0 + \alpha_1 ASSIGNMENT_{it} + \gamma CONTROLS_{it} + \varepsilon_{it} \qquad (2)$$

As discussed in section 5, using the main criterion for eligibility to automatic birthright citizenship enables us to uncover the unbiased "intent to treat" effect (ITT), represented by the estimates of α_1.

The results for these two sets of regressions both with and without controls are reported below in tables 2 (formal integration, further education efforts, and home ownership) and 3 (labor market integration). For the sake of brevity, they only include the estimated coefficients of interest, α_1, for each integration outcome and specification.[31] The results of specification 1 are listed in the columns headed with "ENDOG" and those of specification (2) in the columns titled "ITT".

The first thing to note is that the ENDOG estimates are consistently better in terms of integration than the ITT ones for almost all examined outcomes. This demonstrates the large amount of endogeneity involved and cautions against inferring any causal meaning from simple comparisons of families of children with the German citizenship and those without. The most drastic example is the difference in estimated

[30] Those more willing to integrate are more likely to apply for citizenship for their child under the transition rule of the law if the child is born in 1999 and more likely to correctly report a potential German citizenship of the child if the child is born in 2000.

[31] The complete tables of estimated coefficients for each outcome are available from the author upon request.

Table 2

The Effect of Citizenship of the Child on Naturalization of the Parents, Home Ownership, and Further Education Efforts

	Without controls		With controls	
	ENDOG	ITT	ENDOG	ITT
Integration outcome	(1)	(2)	(3)	(4)
Naturalization	**0.192*** **	**-0.053***	**0.164*** **	**-0.064****
of the father	(0.01)	(0.02)	(0.01)	(0.02)
[0 / 1]	{1750}	{1750}	{1730}	{1730}
Naturalization	**0.18*** **	**-0.014***	**0.162*** **	**-0.027****
of the mother	(0.02)	(0.01)	(0.02)	(0.01)
[0 / 1]	{1951}	{1951}	{1931}	{1931}
Home ownership	-0.011	-0.054	-0.043	**-0.069***
[0 / 1]	(0.03)	(0.01)	(0.02)	(0.01)
	{928}	{928}	{920}	{920}
Further education	**0.013****	0.00	-0.002	-0.004
efforts of the father	(0.01)	(0.01)	(0.01)	(0.01)
[0 / 1]	{3219}	{3219}	{3186}	{3186}
Further education	0.015	0.002	0.004	0.006
efforts of the mother	(0.01)	(0.01)	(0.01)	(0.01)
[0 / 1]	{3520}	{3520}	{3487}	{3487}
Controls	No	No	Yes	Yes

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

Notes: (1) Robust standard errors reported in parentheses, the number of observations included in the respective regression in curly brackets. (2) The numbers in this table report the estimated coefficient of interest in separate regressions on the respective integration outcome. The explanatory variable of interest is "real treatment" (whether German citizenship is actually reported for a child) in the ENDOG columns and "assignment" (whether child is born after enactment) in the ITT columns. (3) Controls include: age, educational attainment, born in Germany, and years since arrival for both parents, whether parents are citizens of another EU country or Turkey, whether the parents are married, the age of the youngest child, dummies for the region of living in Germany, as well as the quarter and the year of the interview.

Data: *Microcensus* 2001-2008

coefficients for the effect on each parent's propensity to naturalize herself, changing from a very large and significant impact of +16% in column 3 to negative and significant estimates of -6.4% and -2.7% for fathers and mothers, respectively, in column 4. This suggests that introducing birthright citizenship has a negative impact on formal integration of the parents. A possible explanation for this finding could be that parents after the reform do not need to naturalize themselves anymore in order to get the German citizenship for their children.

Another interesting result is the negative and significant estimated impact on home ownership, indicating that having a child born in 2000 leads to an almost 7% drop in the probability to own a house or apartment. This effect could be caused either by a reduction of savings and investments following the reform as suggested by Piracha and Zhu (2008) or by a compositional effect of the reform. If the new law retains relatively more unsuccessful migrants in the assignment group from returning to their home countries than in the control group, this would result in a relative decline in the ability of the average family in the assignment group to invest in property.

In contrast to these two often used proxies for integration, I do not find any effect on parents' efforts to get more education or training. While returns to investment in host country specific human capital may have increased for some migrant parents who decide to extent their duration in Germany because of the better prospects of the child, they may simply not have the opportunity to do so being over 30 already on average and having at least one young child at home. Furthermore, they could assume that it is more efficient or profitable to invest in their children's education as this may promise high returns in the future. This cannot be tested, however, since there is no measure available for that.

The results in table 3 provide an overview on the effect of birthright citizenship on different labor market integration measures, both for fathers in panel A and mothers in panel B. Focusing on the specifications that contain the whole set of controls (columns 3 and 4 in each panel), we can see again that simply comparing families with children who possess the German citizenship with others whose children are not German citizens is biased towards showing more positive effects. Apart from that, the findings indicate that the mothers' labor market integration is not affected by the citizenship status of their children. The analysis of the fathers' labor market behavior, on the other hand, does reveal some changes. Although there seems to be no impact on

Table 3

The Effect of Citizenship of the Child on the Parental Labor Market Integration

A. Fathers

	Without controls		With controls	
	ENDOG	ITT	ENDOG	ITT
Integration outcome	(1)	(2)	(3)	(4)
Labor supply	0.008	-0.004	0.012	-0.004
[0 / 1]	(0.01)	(0.01)	(0.01)	(0.01)
	3219	3219	3186	3186
Working	0.002	**-0.029***	0.006	**-0.031***
[0 / 1]	(0.01)	(0.01)	(0.01)	(0.01)
	3219	3219	3186	3186
Searching for	0.006	**0.026***	0.006	**0.027****
a job	(0.02)	(0.01)	(0.01)	(0.01)
[0 / 1]	3219	3219	3186	3186
Hours worked	-0.386	**-2.362****	-0.255	**-2.625****
last week	(0.58)	(0.76)	(0.66)	(0.75)
	3219	3219	3186	3186
Registered	0.00	0.024	0.004	0.025
unemployed	(0.01)	(0.01)	(0.01)	(0.01)
[0 / 1]	3219	3219	3186	3186
Controls	No	No	Yes	Yes

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

Notes: (1) Robust standard errors reported in parentheses. The number of observations included in the respective regression is stated below. The numbers in this table report the estimated coefficient of interest in separate regressions on the respective integration outcome. The explanatory variable of interest is "real treatment" (whether German citizenship is actually reported for a child) in the OLS columns and "assignment" (whether child is born after enactment) in the ITT columns. (3) Controls include: age, educational attainment, born in Germany, and years since arrival for both parents, whether parents are citizens of another EU country or Turkey, whether the parents are married, the age of the youngest child, dummies for the region of living in Germany, as well as the quarter and the year of the interview.

Data: *Microcensus 2001-2008*

Table 3 (continued)
The Effect of Citizenship of the Child on the Parental Labor Market Integration

B. Mothers

	Without controls		With controls	
	ENDOG	ITT	ENDOG	ITT
Integration outcome	(1)	(2)	(3)	(4)
Labor supply	**0.059****	**-0.047****	**0.028***	0.00
[0 / 1]	(0.02)	(0.01)	(0.01)	(0.01)
	3520	3520	3487	3487
Working	**0.032****	**-0.044*****	**0.024****	-0.008
[0 / 1]	(0.01)	(0.01)	(0.01)	(0.01)
	3520	3520	3487	3487
Searching for	**0.027****	-0.002	0.005	0.008
a job	(0.01)	(0.01)	(0.01)	(0.01)
[0 / 1]	3520	3520	3487	3487
Hours worked	0.297	**-0.997*****	0.076	-0.024
last week	(0.34)	(0.24)	(0.22)	(0.35)
	3520	3520	3487	3487
Registered	0.01	-0.009	0.008	0.011
unemployed	(0.01)	(0.01)	(0.01)	(0.01)
[0 / 1]	3520	3520	3487	3487
Controls	No	No	Yes	Yes

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

Notes: (1) Robust standard errors reported in parentheses. The number of observations included in the respective regression is stated below. The numbers in this table report the estimated coefficient of interest in separate regressions on the respective integration outcome. The explanatory variable of interest is "real treatment" (whether German citizenship is actually reported for a child) in the OLS columns and "assignment" (whether child is born after enactment) in the ITT columns. (3) Controls include: age, educational attainment, born in Germany, and years since arrival for both parents, whether parents are citizens of another EU country or Turkey, whether the parents are married, the age of the youngest child, dummies for the region of living in Germany, as well as the quarter and the year of the interview.

Data: *Microcensus 2001-2008*

the fathers' total labor supply, defined as either working or searching for a job, the results show a negative and significant effect on the fathers' current employment status and the number of hours worked last week. At the same time, the estimated coefficient for searching for a job is positive and significant. This pattern may be a sign for a negative effect on the composition of the assignment group, causing the average members of this group to have a lower probability to find suitable jobs. Alternatively, this result could stem from migrant families who originally wanted to stay only temporary but now decide to remain permanently in Germany to reap the full benefits of citizenship for their child. As permanent migrants, they may tend to increase their reservation wages and take moretime to search for a good job that fits with their qualifications as described in Dustmann (1997).

Finally, the results for the social and cultural integration variables from the GSOEP sample are presented in table 4. As the GSOEP does not provide information on the citizenship status of the children, specification 1 cannot be used for these variables. The results therefore only report the estimates of the ITT specification. Overall, the introduction of birthright citizenship seems to have a positive impact on this integration dimension. Basically every estimate is positive, although most of them are insignificant, which could be due to the small sample size. But even like this, the estimates show that having a child born after the enactment of the new law appears to improve the German language proficiency of both fathers and mothers significantly. If both parents are examined jointly, we additionally observe a positive and significant effect on the frequency of reading German newspapers. In terms of relative magnitudes, these coefficients indicate an impact of roughly 0.25 times the standard deviation for German language proficiency and 0.2 times the standard deviation for reading German newspapers.

The last results confirm and extend previous findings by Avitabile et al. (2010) who examine the effect of the *transition regulation* in the new law on a partly overlapping set of social integration variables from the GSOEP. Their results provide evidence that granting foreign-citizen parents the *option to apply* for the German citizenship for their children led to significant increases in their contact with German natives and using the German language at home compared to non-eligible parents. Since the present paper compares the integration behavior of parents treated by the transition component (the treatment group in Avitabilie et al, 2010, and control group in this paper) with those whose children *automatically* become German citizens at birth

Table 4

The Effect of Citizenship of the Child on Social and Cultural Integration Outcomes

	Fathers		**Mothers**		**Both parents**	
	Uncond.	Cond.	Uncond.	Cond.	Uncond.	Cond.
Integration outcome	(1)	(2)	(3)	(4)	(5)	(6)
Feel German	0.23	0.374	0.298	-0.085	0.265*	0.115
[1 - 5]	(0.18)	(0.23)	(0.21)	(0.27)	(0.14)	(0.18)
	129	128	141	140	270	268
German language	0.263	**0.24****	**0.701*****	**0.325***	**0.486*****	**0.275****
proficiency	(0.23)	(0.12)	(0.25)	(0.18)	(0.18)	(0.11)
[1 - 5]	172	171	189	188	361	359
Reading German	0.219	0.205	**0.526****	0.169	**0.379****	**0.233***
newspapers	(0.23)	(0.20)	(0.26)	(0.19)	(0.18)	(0.13)
[1 - 5]	175	174	179	178	354	352
Contact with German	0.134	-0.033	0.078	0.024	**0.104***	0.023
friends	(0.09)	(0.07)	(0.07)	(0.08)	(0.05)	(0.05)
[0 / 1]	162	161	180	179	342	340
Political interest	0.136	0.183	0.004	-0.139	0.062	0.068
[1 - 4]	(0.17)	(0.13)	(0.12)	(0.11)	(0.11)	(0.10)
	372	370	414	412	786	782
Controls	No	Yes	No	Yes	No	Yes

* = 10%, ** = 5%, *** = 1% significance le

Notes: (1) Clustered standard errors reported in parentheses. The number of observations included in the respective regression is stated below. (2) The numbers in this table report the estimated coefficient for the "assignment" variable (whether child is born after enactment) in separate regressions on the respective integration outcome. (3) Controls include: age, educational attainment, born in Germany, and years since arrival for both parents, whether parents are citizens of another EU country or Turkey, whether the parents are married, the age of the youngest child, dummies for the region of living in Germany, as well as the quarter and the year of the interview.

Data: *GSOEP* 2000-2005

(treatment group), we can interpret the results of this paper jointly with those in Avitabile et al. (2010) to provide a full account of the impact of the two children-related components on the social integration of the affected parents. They suggest that the effect increases with the treatment intensity, from (1) not being able to apply for the host-country citizenship for your children to (2) obtaining the opportunity to do so to (3) being treated automatically.

2.7.2 Robustness checks

I check the robustness of these results in several ways. The first is to vary the "bandwidth" of the two comparison groups around the enactment date, i.e., the range of birth dates to include on either side of the cutoff. In principle, the estimates should capture the causal effect better the closer we get to the enactment date. On the other hand, this reduces the number of observations and thus the precision of the estimates, so it is not clear a priori which bandwidth to use. Since the Microcensus does not provide the month of birth, I examined all births within a one-year period before and after in the main section of the paper (that is, from January, 1999, to December, 2000). In this section, I vary the range to a) *seven months* (May, 1999, to July, 2000) and b) *two years* (January, 1998, to December, 2001) around the enactment date.

To get the seven-months subsample, I combined the available information on the year of birth of the child, her age at the time of the interview, and the quarter of the interview. As an example, for an interview conducted in the second quarter of 2005, I coded a child as born in the first seven months of 2000 if the child was born in 2000 and has already turned 5 years old. On the other hand, a child born in 2000 who is still 4 years old at the time of the interview in the second quarter must have been born later in the year, so she is not included in this sample.

The results for the different bandwidths are reported in columns 1-3 in tables B.1 and B.2 in the appendix. In general, the findings for the one-year bandwidth seem to hold in the other two samples as well. The estimates show the same sign and the sizes are roughly in the same order of magnitude as in the main sample for most of the cases. A notable exception is the number of hours the fathers worked last week, where we see a change in sign from being negatively significant in the one-year sample to positively significant in the two-year sample. Apart from this variable, almost all significant results from the main sample are also confirmed in terms of statistical significance in the two-year sample. This is not the case for the seven-months sample, however, but it hardly reports any significant result at all, probably due to the much smaller sample size.

Next, I test whether the results could be caused by a general cohort effect. As the empirical strategy relies on comparing families which differ systematically in the birth year of their children, it could be that there is some other factor that affects the two cohorts differently and thus drives the results.To see whether this is the case, I use

another group of foreign-citizen families which is not affected by the introduction of automatic citizenship at birth to conduct a difference-in-difference analysis. In particular, I include foreign-citizen families with children born in 1999 and 2000 who were not eligible for birthright citizenship at the time of birth, because their parents did not fulfill the minimum residence requirement. If there are also differences in integration outcomes between the two birth-year cohorts for this group, the results obtained in the previous section could have simply picked up a cohort trend rather than the causal effect of automatic citizenship at birth.

The empirical specification for this test is as follows:

$$INT\_MEASURE_{it} = \alpha_0 + \alpha_1 AFTER_{it} + \alpha_2 ELIGIBLE_{it} + \\ + \alpha_3 (AFTER * ELIGIBLE)_{it} + \gamma CONTROLS_{it} + \varepsilon_{it} \qquad (3)$$

Column 4 of tables B.1 and B.2 in the appendix reports the results for the DID estimator, α_3, for the respective integration outcome. In general, the precision of the estimates seems to be much lower in this specification, leading to mostly insignificant coefficients. Nevertheless, the point estimates are very similar to those of the main specification in column 1 for most variables, suggesting that the main results are not driven by unobserved cohort effects. The main exception to this pattern is the effect on naturalization, where the positive (and for the mothers significant) DID coefficients contrast with the negative and significant estimates for both parents in the main specification.

This finding may be less puzzling than it seems on the first glance, however, since the group of ineligible families with children born in 1999 has been living in Germany for a longer time on average than those with children born in 2000 by construction.[32] Thus, the fraction of "ineligible" parents who eventually fulfill the requirements to become eligible for their own naturalization is larger in the group with

[32] There are two reasons for this: Immigrants who arrived in 1992 and got a child in Germany in 1999 were not eligible for birthright citizenship and are thus included in the ineligible sample, but they would not be considered if they got their child in 2000, as it is not clear whether they fulfilled the requirement at birth or not. Likewise, parents who immigrated in 2000 are included in the group of ineligible families if their child is born in 2000, but not in 1999, because the focus is on analyzing the impact of the citizenship reform on families with children born *in* Germany and this cannot be the case if reported arrival happened later than the year of birth of the child. Taken together, parents with a child born in 1999 may have arrived between 1992 and 1999, and those with a child born in 2000 between 1993 and 2000.

children born in 1999 in each of the Microcensus years included in this study, which leads to a higher probability of becoming naturalized for the parents in the 1999 cohort compared to those in the 2000 cohort. This effect may be large enough to cause a greater difference between the two "ineligible" groups than what we observe in the "eligible" sample, which would explain the positive DID estimator.

Finally, comparing two very specifically defined small groups carries the danger that the results simply reflect noise in the data. To test whether this may be the case, I conduct a series of "placebo" tests in which I move the enactment date to hypothetical points in time (here, to every 1st of January between 1995 and 2001) and apply the same analysis for these fictional enactment dates.[33] The estimated coefficients for the "assignment" indicator for each integration outcome and hypothetical enactment date are displayed in tables B.3 to B.5 in the appendix.

The results provide a mixed picture. On the one hand, there are no other significant estimates over the different hypothetical enactment dates for the effect on the naturalization of the fathers and the parents' German language proficiency, supporting the main results for these variables. On the other hand, however, there are several variables for which we can see significant estimates of the same magnitude or larger for several hypothetical enactment dates. This is the case, for instance, for home ownership and the mothers' labor supply, but especially for most of the measures of the fathers' labor market integration. The frequency and fluctuating pattern of the estimates for these variables indicates that there is a large level of noise in the data. Therefore, the results for the parents' labor market integration in particular should be considered with great caution.

2.8 Conclusions

In this paper, I study the effect of automatically granting citizenship to immigrant children at birth on the integration behavior of their parents in Germany. I use the introduction of birthright citizenship at the beginning of 2000 as an exogenous source of variation in newborn children's citizenship status in order to circumvent the self-

[33] For example: If the hypothetical enactment date is the 1st of January 1997, I compare the integration behavior of parents with children born in 1996 with those whose children are born in 1997 over the period following the enactment of the new law, i.e., between 2001 and 2008. The restrictions are again that the child needs to be born in Germany, the parents have to have arrived at least 8 years before the birth of the child, and they are still foreign-citizens at the beginning of 2000.

selection problem that commonly prevents causal inference. The unbiased intent-to-treat effect is identified by comparing two very similar groups of migrant families who only receive a different treatment because the children of one group were born directly before the enactment of the reform and those of the other afterwards.

Comparing various integration measures between these two groups indicates that granting citizenship to immigrant children may have diverse effects on their parents' integration behavior across several integration dimensions. In particular, it seems to reduce the parents' willingness to naturalize themselves (formal integration), on the one hand, and to improve their German language proficiency (social integration), on the other. These results shed light on some of the potential effects of the introduction of birthright citizenship in Germany, namely the short- and medium-run direct impact on those immigrant parents who have legally lived in Germany for a long time already and whose children were treated right at the beginning of the new policy.

Many other important questions still remain, however. As I have shown in this paper, it is still unclear whether and how birthright citizenship for immigrant children affects their parents' labor market integration. A crucial issue here will be to go beyond survey data towards using more reliable administrative data with larger sample sizes. Then, there is the possibility of effects on the composition of immigrants, i.e., that 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 and retain motivated and skilled migrants is necessary to devise efficient policies. Most importantly, we also need to know how the introduction of automatic birthright citizenship affects those who are the actual target, i.e., the children that now grow up as German citizens. Further research on these different areas is therefore strongly recommended.

For policymakers, the results of this paper imply that there is no "free-lunch" in granting citizenship to second-generation immigrants and hoping that it would solve existing integration problems of their parents at the same time. Improving the current problems will require other measures like ongoing efforts to improve the immigrants' qualifications and language abilities as early as possible, but also fighting discrimination in the labor market or changing immigration laws to make it easier for high skilled migrants to come.

Appendix to Chapter 2

Table B.1

Different bandwidth and comparison with ineligible parents - Naturalization, home ownership, and further education efforts

Integration outcome	1 Year (1)	7 Months (2)	2 Years (3)	DiD (4)
Naturalization of the father	**-0.064****	-0.036	**-0.026*****	0.031
	(0.018)	(0.020)	(0.013)	(0.029)
[0 / 1]	{1730}	{487}	{3394}	{2435}
Naturalization of the mother	**-0.027****	0.018	-0.006	**0.048***
	(0.008)	(0.024)	(0.011)	(0.016)
[0 / 1]	{1931}	{548}	{3943}	{2898}
Home ownership	**-0.069***	-0.061	**-0.024***	-0.033
[0 / 1]	(0.007)	(0.000)	(0.021)	(0.024)
	{920}	{134}	{1882}	{1343}
Further education efforts of the father	-0.004	-0.023	0.001	-0.01
	(0.008)	(0.021)	(0.007)	(0.018)
[0 / 1]	{3186}	{487}	{6228}	{4497}
Further education efforts of the mother	0.006	0.007	0.002	0.015
	(0.005)	(0.036)	(0.007)	(0.036)
[0 / 1]	{3487}	{548}	{7083}	{5192}
Controls	Yes	Yes	Yes	Yes

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

Notes: (1) Robust standard errors reported in parentheses, the number of observations included in the respective regression in curly brackets. (2) The numbers in this table report the estimated coefficient of interest in separate regressions on the respective integration outcome. The variable of interest is "assignment" (whether child is born after enactment) in columns 1-3 and "after*eligible" (the interaction between being born after enactment and being eligible for the treatment) in column 4. (3) Controls include: age, educational attainment, born in Germany, and years since arrival for both parents, whether parents are citizens of another EU country or Turkey, whether the parents are married, the age of the youngest child, dummies for the region of living in Germany, as well as the quarter and the year of the interview.

Data: *Microcensus 2001-2008*

Table B.2

Different bandwidth and comparison with ineligible parents - Labor market integration

A. Fathers

Integration outcome	1 Year (1)	7 Months (2)	2 Years (3)	DiD (4)
Labor supply	-0.004	-0.014	-0.004	0.004
[0 / 1]	(0.007)	(0.015)	(0.007)	(0.015)
	{3186}	{487}	{5847}	{4497}
Working	**-0.031***	-0.043	**-0.03****	-0.025
[0 / 1]	(0.014)	(0.041)	(0.011)	(0.036)
	{3186}	{487}	{5847}	{4497}
Searching for	**0.027****	0.029	**0.018***	0.029
a job	(0.011)	(0.051)	(0.010)	(0.031)
[0 / 1]	{3186}	{487}	{6228}	{4497}
Hours worked	**-2.625****	-2.755	**2.134*****	**-2.896***
last week	(0.753)	(1.507)	(0.591)	(1.513)
	{3186}	{487}	{6228}	{4497}
Registered	0.025	0.03	0.013	0.035
unemployed	(0.013)	(0.050)	(0.010)	(0.032)
[0 / 1]	{3186}	{487}	{5916}	{4377}
Controls	Yes	Yes	Yes	Yes

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

Notes: (1) Robust standard errors reported in parentheses, the number of observations included in the respective regression in curly brackets. (2) The numbers in this table report the estimated coefficient of interest in separate regressions on the respective integration outcome. The variable of interest is "assignment" (whether child is born after enactment) in columns 1-3 and "after*eligible" (the interaction between being born after enactment and being eligible for the treatment) in column 4. (3) Controls include: age, educational attainment, born in Germany, and years since arrival for both parents, whether parents are citizens of another EU country or Turkey, whether the parents are married, the age of the youngest child, dummies for the region of living in Germany, as well as the quarter and the year of the interview.

Data: *Microcensus 2001-2008*

Table B.2 (continued)
Different bandwidth and comparison with ineligible parents - Labor market integration

	B. Mothers			
Integration outcome	1 Year (1)	7 Months (2)	2 Years (3)	DiD (4)
Labor supply	0.00	-0.026	**-0.036***	-0.004
[0 / 1]	(0.013)	(0.043)	(0.012)	(0.014)
	{3487}	{548}	{6971}	{5192}
Working	-0.008	-0.056	**-0.032****	0.00
[0 / 1]	(0.008)	(0.058)	(0.011)	(0.015)
	{3487}	{548}	{6971}	{5192}
Searching for	0.008	0.03	-0.006	-0.005
a job	(0.009)	(0.039)	(0.007)	(0.013)
[0 / 1]	{3487}	{548}	{7083}	{5192}
Hours worked	-0.024	0.152	-0.347	0.519
last week	(0.350)	(1.229)	(0.346)	(0.465)
	{3487}	{548}	{7083}	{5192}
Registered	0.011	0.019	-0.006	0.008
unemployed	(0.010)	(0.034)	(0.009)	(0.012)
[0 / 1]	{3487}	{548}	{4876}	{3593}
Controls	Yes	Yes	Yes	Yes

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

Notes: (1) Robust standard errors reported in parentheses, the number of observations included in the respective regression in curly brackets. (2) The numbers in this table report the estimated coefficient of interest in separate regressions on the respective integration outcome. The variable of interest is "assignment" (whether child is born after enactment) in columns 1-3 and "after*eligible" (the interaction between being born after enactment and being eligible for the treatment) in column 4. (3) Controls include: age, educational attainment, born in Germany, and years since arrival for both parents, whether parents are citizens of another EU country or Turkey, whether the parents are married, the age of the youngest child, dummies for the region of living in Germany, as well as the quarter and the year of the interview.

Data: *Microcensus 2001-2008*

Table B.3

Falsification tests - Hypothetical enactment dates:
Naturalization, home ownership, and further education efforts

Integration outcome	1995	1996	1997	1998	1999	2000	2001
Naturalization	0.007	-0.018	0.003	-0.002	0.039	**-0.064****	0.016
of the father	(0.014)	(0.014)	(0.010)	(0.026)	(0.017)	(0.018)	(0.022)
[0 / 1]	{1775}	{1748}	{1819}	{1918}	{1810}	{1730}	{1662}
Naturalization	**0.023****	**-0.025****	0.012	0.007	0.002	**-0.027****	**0.026****
of the mother	(0.006)	(0.007)	(0.013)	(0.010)	(0.008)	(0.008)	(0.005)
[0 / 1]	{2076}	{2038}	{2063}	{2150}	{2045}	{1931}	{1827}
Home ownership	0.021	-0.025	0.00	-0.044	**0.074***	**-0.069***	**0.04***
[0 / 1]	(0.021)	(0.017)	(0.013)	(0.022)	(0.011)	(0.007)	(0.005)
	{959}	{932}	{943}	{978}	{953}	{920}	{857}
Further education	0.006	0.002	0.01	-0.017	-0.001	-0.004	0.014
efforts of the father	(0.006)	(0.011)	(0.007)	(0.009)	(0.008)	(0.008)	(0.008)
[0 / 1]	{3298}	{3299}	{3362}	{3502}	{3370}	{3186}	{2669}
Further education	-0.003	0.011	-0.017	-0.002	0.01	0.006	-0.002
efforts of the mother	(0.003)	(0.006)	(0.009)	(0.007)	(0.006)	(0.005)	(0.011)
[0 / 1]	{3781}	{3774}	{3752}	{3866}	{3729}	{3487}	{2917}
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Notes: (1) Robust standard errors reported in parentheses, the number of observations included in the respective regression in curly brackets. (2) The estimates displayed in this table are calculated with the same method as the main results, but the sample changes between each column to include only those immigrant families with children born around the respective hypothetical enactment date (the 1st of January of the year at the top of the column). (3) The explanatory variable of interest is "assignment" in each case (i.e., whether a child is born in the year after the hypothetical enactment date). (4) Controls include: age, educational attainment, born in Germany, and years since arrival for both parents, whether parents are citizens of another EU country or Turkey, whether the parents are married, the age of the youngest child, dummies for the region of living in Germany, as well as the quarter and the year of the

Data: *Microcensus 2001-2008*

Table B.4
Falsification tests - Hypothetical enactment dates:
Labor market integration

	A. Fathers						
Integration outcome	1995	1996	1997	1998	1999	2000	2001
Labor supply	**0.014****	-0.01	0.003	0.002	0.003	-0.004	-0.011
[0 / 1]	(0.003)	(0.008)	(0.012)	(0.008)	(0.006)	(0.007)	(0.013)
	{3276}	{3279}	{3348}	{3496}	{3370}	{3186}	{2669}
Working	0.009	-0.024	**0.043***	**-0.02***	**0.032****	**-0.031***	-0.016
[0 / 1]	(0.008)	(0.017)	(0.021)	(0.011)	(0.009)	(0.014)	(0.031)
	{3276}	{3279}	{3348}	{3496}	{3370}	{3186}	{2669}
Searching for	0.004	0.014	**-0.039****	**0.023***	**-0.029***	**0.027****	0.005
a job	(0.009)	(0.011)	(0.016)	(0.011)	(0.013)	(0.011)	(0.023)
[0 / 1]	{3298}	{3299}	{3362}	{3502}	{3370}	{3186}	{2669}
Hours worked	0.488	**-1.349***	**1.635***	**-1.289****	**2.65*****	**-2.625****	-0.514
last week	(0.836)	(0.587)	(0.839)	(0.408)	(0.470)	(0.753)	(1.322)
[hours]	{3298}	{3299}	{3362}	{3502}	{3370}	{3186}	{2669}
Registered	0.005	0.012	**-0.04****	**0.026****	**-0.028****	0.025	-0.001
unemployed	(0.008)	(0.013)	(0.017)	(0.009)	(0.011)	(0.013)	(0.025)
[0 / 1]	{3230}	{3233}	{3290}	{3441}	{3305}	{3121}	{2629}
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Notes: (1) Robust standard errors reported in parentheses, the number of observations included in the respective regression in curly brackets. (2) The estimates displayed in this table are calculated with the same method as the main results, but the sample changes between each column to include only those immigrant families with children born around the respective hypothetical enactment date (the 1st of January of the year at the top of the column). (3) The explanatory variable of interest is "assignment" in each case (i.e., whether a child is born in the year after the hypothetical enactment date). (4) Controls include: age, educational attainment, born in Germany, and years since arrival for both parents, whether parents are citizens of another EU country or Turkey, whether the parents are married, the age of the youngest child, dummies for the region of living in Germany, as well as the

Data: *Microcensus 2001-2008*

Table B.4 (continued)
Falsification tests - Hypothetical enactment dates:
Labor market integration

B. Mothers

Integration outcome	1995	1996	1997	1998	1999	2000	2001
Labor supply	**0.033****	**0.033****	-0.012	-0.009	0.039	0.00	**-0.023****
[0 / 1]	(0.010)	(0.013)	(0.010)	(0.008)	(0.022)	(0.013)	(0.007)
	{3780}	{3774}	{3749}	{3863}	{3728}	{3487}	{2917}
Working	**0.022***	0.014	-0.013	0.001	**0.023***	-0.008	-0.005
[0 / 1]	(0.010)	(0.016)	(0.012)	(0.009)	(0.011)	(0.008)	(0.009)
	{3780}	{3774}	{3749}	{3863}	{3728}	{3487}	{2917}
Searching for	0.011	**0.019****	0.001	-0.01	0.016	0.008	**-0.019***
a job	(0.007)	(0.006)	(0.008)	(0.007)	(0.014)	(0.009)	(0.008)
[0 / 1]	{3781}	{3774}	{3752}	{3866}	{3729}	{3487}	{2917}
Hours worked	0.464	0.208	-0.137	-0.24	0.53	-0.024	-0.258
last week	(0.292)	(0.513)	(0.379)	(0.286)	(0.385)	(0.350)	(0.284)
[hours]	{3781}	{3774}	{3752}	{3866}	{3729}	{3487}	{2917}
Registered	-0.008	**0.023****	0.006	-0.006	0.004	0.011	-0.01
unemployed	(0.011)	(0.008)	(0.011)	(0.012)	(0.015)	(0.010)	(0.012)
[0 / 1]	{2827}	{2830}	{2774}	{2784}	{2627}	{2410}	{2119}
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Notes: (1) Robust standard errors reported in parentheses, the number of observations included in the respective regression in curly brackets. (2) The estimates displayed in this table are calculated with the same method as the main results, but the sample changes between each column to include only those immigrant families with children born around the respective hypothetical enactment date (the 1st of January of the year at the top of the column). (3) The explanatory variable of interest is "assignment" in each case (i.e., whether a child is born in the year after the hypothetical enactment date). (4) Controls include: age, educational attainment, born in Germany, and years since arrival for both parents, whether parents are citizens of another EU country or Turkey, whether the parents are married, the age of the youngest child, dummies for the region of living in Germany, as well as the

Data: *Microcensus 2001-2008*

Table B.5

Falsification tests - Hypothetical enactment dates:
Social and cultural integration

Integration outcome	1995	1996	1997	1998	1999	2000	2001
Feel German	-0.159	-0.084	0.108	0.024	-0.263	0.136	**0.448***
[1 - 5]	(0.159)	(0.204)	(0.174)	(0.168)	(0.162)	(0.178)	(0.243)
	{254}	{216}	{255}	{274}	{249}	{265}	{240}
German language	0.025	-0.153	-0.094	0.164	-0.201	**0.307****	-0.157
proficiency	(0.150)	(0.184)	(0.182)	(0.173)	(0.180)	(0.151)	(0.167)
[1 - 5]	{348}	{300}	{347}	{371}	{341}	{355}	{325}
Reading German	-0.001	-0.117	0.095	0.044	**-0.364*****	**0.315****	0.151
newspapers	(0.172)	(0.206)	(0.180)	(0.158)	(0.137)	(0.151)	(0.195)
[1 - 5]	{342}	{304}	{349}	{363}	{346}	{352}	{297}
Contact with	**-0.097***	0.078	**-0.106***	0.076	-0.1	0.038	-0.07
Germans	(0.050)	(0.048)	(0.057)	(0.055)	(0.067)	(0.053)	(0.064)
[0 / 1]	{334}	{289}	{323}	{352}	{328}	{336}	{307}
Political	**-0.227****	0.131	-0.123	0.082	0.115	0.073	0.031
interest	(0.088)	(0.109)	(0.103)	(0.087)	(0.101)	(0.096)	(0.089)
[1 - 4]	{745}	{649}	{747}	{790}	{746}	{775}	{677}

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

Notes: (1) Robust standard errors reported in parentheses, the number of observations included in the respective regression in curly brackets. (2) The estimates displayed in this table are calculated with the same method as the main results, but the sample changes between each column to include only those immigrant families with children born around the respective hypothetical enactment date (the 1st of January of the year at the top of the column). (3) The explanatory variable of interest is "assignment" in each case (i.e., whether a child is born in the year after the hypothetical enactment date). (4) Controls include: age, educational attainment, born in Germany, and years since arrival for both parents, whether parents are citizens of another EU country or Turkey, whether the parents are married, the age of the youngest child, dummies for the region of living in Germany, as well as the quarter and the year of the interview. (5) Both parents are included in each regression.

Data: *GSOEP 2000-2005*

3. Is Bob the Banker more Electable than Bob the Banker? - The Effect of Candidate Profession on Voter Satisfaction And Behavior in Open-List Elections

3.1 Introduction

Open-list elections (also known as preferential voting) are becoming increasingly popular around the world. In 2011, they were used at different institutional levels in countries as diverse as Indonesia and Switzerland, Brazil and Finland, and Germany and Peru. The particular feature of open-list elections is that citizens do not only vote for a certain party or fixed party list, but can directly indicate their preference for individual candidates of that list. The number of seats for a party is then determined by the sum of votes obtained by all of its candidates together, whereas which candidate ends up elected depends on the intra-party ranking of individual votes obtained.

Proponents of this election system claim that it provides two often desired features simultaneously: (1) *proportional representation* of the public opinion in the parliament, and (2) *accountability of representatives*, as citizens are able to vote individual politicians out of office. As such, open lists are increasingly demanded by citizen initiatives, popular movements, and research committees on electoral reform to overcome deficiencies in many countries' political system and popular wariness about voting (e.g., DemocraciaRealYa, 2011; The POWER Inquiry, 2006), as exemplified by falling voter turnout in most Western countries (Lijphart, 1997; Gray and Caul, 2000; and Gallego, 2009).

Recent research has confirmed that voter satisfaction tends to be higher in open-list elections and attributes this finding to the greater influence and choice voters possess in comparison to closed-list elections (Farrell and McAllister, 2006). Having more choices, however, does not necessarily lead to greater happiness, if for instance, the number of available options is large but it is hardly possible to make out any difference between them (Schartz et al., 2002). In the case of open elections, this means that they are only successful in raising satisfaction if the voters know enough about the

single candidates to form an opinion. In short, it can be argued that it is not important to have a choice, but to have a *meaningful* choice.

For the common voter, however, the costs of informing herself about all the candidates and their positions is prohibitively high (Downs, 1957). Thus, Norris (2002) finds that about 45% of the voters in various open-list elections could not correctly recall *any* single candidate of the list they voted for shortly after the elections. Although this fraction is much lower than the 66% she reports for countries that use closed-lists, it still means that a large share of voters does not place particular importance on informing themselves about the candidates before election day.

The aim of this paper is to evaluate the importance of a specific source of information about the candidates which can be made available to each voter easily: statements about the candidates on the ballot itself. This part of ballot design is usually overlooked in comparative election studies, although it possesses great potential to influence voters in "low-information" elections. The standard ballot just states the lists of candidates per party with their rank and full name, but nothing else. In many occasions, however, the respective election law requires that additional information is added, e.g., on the candidates' place of living, birth year, education, or current profession. If a voter does not know any of the candidates and decides only in the voting booth, this information may have a significant influence on her voting decision and the way she feels about the election itself.

In the literature, this issue has been widely neglected so far. The relation between information on the ballot and voter satisfaction has not been investigated at all, and the two most recent studies that discuss the effect of profession information on voting behavior come to opposing conclusions. McDermott (2005) evaluates experimental survey data to show that voters in run-off elections for state-wide offices in California in 1994 used occupational information to judge candidate competence through inferred qualifications, i.e., they are more likely to elect someone with a business background for treasurer. By contrast, Mechtel (2011) examines the results of open-list elections for local councils in a German state in 2009, and finds that comparatively low-skill professions such as bakers, butchers, policemen, farmers, and gardeners attract the most votes, while on average higher-skilled occupations like salesmen, employees in the financial/insurance sector, secretaries, and management consultants fare worst.

The present paper contributes to this literature in two ways: On the one hand, it is the first to investigate empirically the relation between information about the candidates on the ballot and voter satisfaction. And on the other, it attempts to solve the dispute over the impact of profession information on voting behavior by conducting a randomized experiment in an open-list setup, i.e., using a similar method as in McDermott (2005) on an environment similar to Mechtel's (2011). Thus, the two main research questions are: (1) *Does including additional information about the candidates on the ballot affect voter satisfaction and support for open lists?* and (2) *Does knowing the candidates' profession change the voters' selection decision and if so, who gains and who loses?* I focus on profession, because it may provide a particularly meaningful signal, enabling voters to deduce a lot about the candidates' daily work and life, their education, and their potential position on relevant issues.

To answer these questions, I conducted a voting experiment built into an exit poll of voters in the Spanish local elections of 2011. Among other things, respondents stated their satisfaction with the real, closed-list election, and participated in a hypothetical election under an open-list system. Then they indicated their satisfaction with this hypothetical election and whether they preferred it to the real one. To identify the causal effect of stating profession information on the ballot, respondents were randomly assigned to different ballot versions in the hypothetical election, which varied the amount of information about the candidates while holding their position in the list constant. The information available was either (a) just the family names[34] of the candidates, (b) their full name (i.e., first name and family names), or (c) the full name plus a profession. This setup enables me to disentangle the impact of stating profession information from name or ballot order effects, as well as from the sex of the candidate.

The results obtained provide evidence for a significant positive effect of stating more information on the ballot on the satisfaction of voters with open-list elections. Furthermore, profession information is found to significantly affect voters' decisions in two ways. First, it enables the voters to select those candidates whom they believe to be more qualified for the job, leading to a significantly higher probability to get a vote for candidates working in high-skill professions. And second, it helps the voters to identify those candidates who appear to be either very similar to them or ideologically closer. The former is shown by the finding that a candidate is almost certain to get a vote from

[34] People usually have two family names in Spain, one from each parent.

a voter if they work in exactly the same profession, the latter is illustrated, for instance, by the favorite professions of voters of left-leaning parties (physicians, teachers, and sociologists) in contrast to those of voters of right-leaning parties (lawyers, engineers, and managers).

Looking additionally at the gender dimension, stating the profession on the ballot does not seem to greatly affect the chances of neither male nor female candidates to get elected. On the one hand, voters preferred female over male candidates on average when their occupation was not known, while they seem quite indifferent between the two sexes once they know their professions, suggesting that profession information blurs the power of the gender signal. This effect is compensated, on the other hand, by a small preference for candidates working in female-dominated sectors versus male-dominated ones.

Apart from the political implications for candidates and parties, these results also suggest that including candidate profession on the ballot may have *economic consequences* by changing the composition of the elected parliament or council. As individuals working in high-skill professions are supposed to have different attitudes and preferences over a range of political issues than workers in low-skill occupations, we can expect that an increase in elected representatives with high-skill professions may lead to different decisions on certain relevant policy issues. Mayda and Rodrik (2005), for instance, show that an individual's human capital and relative economic status are significant determinants of favorable attitudes towards further international economic integration. Likewise, high skilled individuals tend to be more supportive of immigration (Mayda, 2006; Hanson et al, 2007) and to prefer spending on public education over social transfers (Bursztyn, 2011).

The remainder of this paper is organized as follows: In section 2, I introduce a theoretical framework of voting in open-list elections with multiple votes, which is used to derive several testable hypotheses. Section 3 describes how the data were collected, followed by section 4 which provides descriptive statistics of the sample obtained and discusses its quality. The results of the empirical evaluation are then presented in section 5. Finally, section 6 concludes.

3.2 A Theoretical Framework of Voting Behavior in Open-List Elections

3.2.1 Voting with Perfect Information

To understand how voters cast their votes in circumstances in which they are uninformed about the candidates and their political positions, it is helpful to look first at voting behavior under perfect information, i.e., a situation in which a citizen knows her own political position on all relevant issues and possesses correct and complete information about all the candidates, their platforms and their chances to get elected. In general, it can be assumed that the typical voter aims at electing representatives who share the voter's opinions on relevant issues, are likeable, and effective in pursuing their political aims. More specifically, citizens with perfect information can therefore be expected to allocate their votes based on:

(1) the proximity of the candidates' position to their own,
(2) their sympathy for the candidate,
(3) the candidate's prospect to get elected, and
(4) her perceived ability / qualification to implement her program.

The first two points are straightforward and probably highly correlated, since we tend to sympathize with others who agree with us on important issues (Byrne et al., 1986; Eisinger, 2000), but 3 and 4 need some explanation. First, imagine that the (perceived) chances to get elected differ across candidates and there are several acceptable candidates on the one hand, and a number of totally unacceptable candidates on the other. In such a case, voters may strategically use their vote to push a second-best candidate in terms of political proximity if they expect her to have a better shot at getting elected than the first-best candidate. Second, if candidates possess different capacities to implement their proposals, candidates with more distant positions but greater ability to get things done may be preferred to ideologically closer candidates (Cox, 1997).

These considerations are illustrated in figure 1. It depicts a continuum of political positions on the policy dimension a particular voter cares about most (e.g., left-right on social and economic issues), the preferred position of the voter on that dimension, *P*, the ideological location of 30 different candidates as little strokes on that political dimension, and the votes cast by the voter, *X*, under two scenarios. In the upper

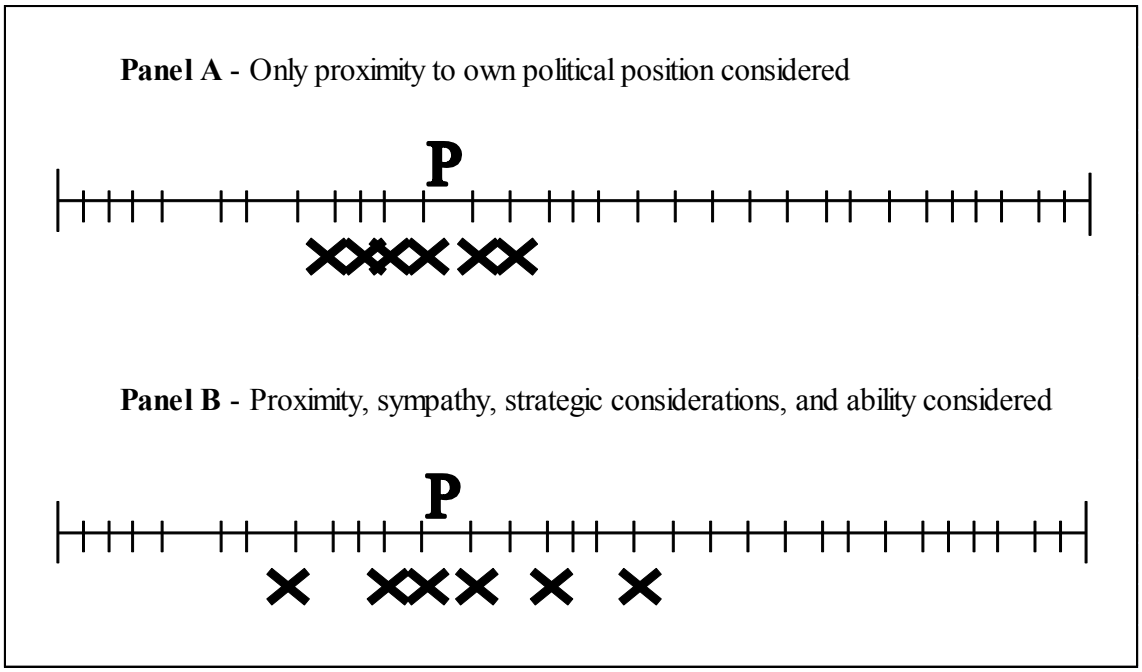

Fig. 1. *Allocation of votes under perfect information on the candidates' political position, likability, probability to win, and ability to enact their program*

Note. The lines represent the continuum of political positions possible on the policy dimension the voter cares about most. The candidates' positions on that dimension are depicted as little strokes and the final allocation of the, here 6, votes is marked by the X's below the lines.

panel of figure 1, the voter only takes the political stance of the candidate into account and thus only votes for those candidates closest to her own preferred position. The lower panel, on the other hand, gives an example of how the allocation of votes could change if the voter also considers sympathy towards the individual candidates, as well as their chances to win, and their ability to carry out their program. In this case, the distribution of votes could be much more dispersed.

Mathematically, the voting decision can be described as an expected utility maximization problem in which voter *i* wants to maximize her utility from voting for a candidate c_j, $U_i(c_j)$, weighted by that candidate's probability to get elected and successfully implement her program, π_j.

$$max\, EU_i(c_j) = \pi_j U_i(c_j) = \pi_j U_i(g_{i,j} - |p_j - P_i|) \quad (1)$$

The utility of voting for candidate *j* is composed of two parts. First, a general utility that voter *i* derives from voting per-se, $g_{i,j}$. This general utility of voting arises, for instance, from fulfilling the civic duty of voting, the feeling of having expressed one's opinion, and the small chance of having a decisive influence over the outcome of the election.

Additionally, it may vary with voter *i*'s degree of sympathy for the respective candidate *j*. Second, the candidate's policy position, p_j, enters the equation through a penalty term that decreases the utility of voting for candidate *j* with the distance between p_j and the voter's policy position, P_i.

In the case of *N* votes, this maximization problem changes to voting for the optimal set of *N* non-identical candidates:

$$max\, EU_i\left(c_j^1, \dots, c_j^N\right) = \sum_{n=1}^{N} \pi_j U_i\left(c_j^n\right) = \sum_{n=1}^{N} \pi_j U_i\left(g_{i,j} - \left|p_j - P_i\right|\right) \quad (2)$$

The optimality condition in this case is that any deviation from voting for the optimal set of candidates leads to a non-positive change in the voter's expected utility.

3.2.2 Voting under Imperfect Information

In most elections with multiple candidates and votes, however, it is prohibitively costly for a voter to inform herself about each and every candidate, their programs and chances to win. Therefore, the typical voter usually knows fewer candidates than she has votes to allocate, which means that she cannot cast her votes in accordance with equation 2. In these "low-information" elections, voters often rely on heuristics when making their decision in the voting booth, that is, they use the available informational cues provided on the ballot to form their opinion about the candidates, their positions, likeability, and ability to do a good job (McDermott, 2005). Usually, a ballot contains at least the full name of the candidates, their party affiliation, and the predetermined order within each party's list. If the voter does not know the candidates, *party membership* can be assumed to be the strongest information cue, as members of one party typically share a certain view of the world or ideology which is commonly known (Rahn, 1993; Lau & Redlawsk, 2001). If there is a large list of candidates from each party, however, this criterion is not sufficient and needs to be complemented. The *name of a candidate* may reveal some important information, especially the gender of the candidate, but sometimes also her regional or ethnic origin or social class (Guell et al., 2007). Likewise, a candidate's *rank* may allow to infer how the respective party views the candidate's importance and quality.

On top of this, many ballots provide additional and more direct information about the candidates to facilitate the selection problem. This often includes stating the candidates' *profession* in order to convey their education and what they are occupied

with in their daily life, allowing voters to form their own opinion on the candidates' policy position and ability to represent them well.[35]

Thus, the voting decision under imperfect information about the candidates is likely to be influenced by differences in the amount and content of information stated on the ballot. This can be incorporated in the expected utility maximization problem by letting the candidates' probability to get elected and enact their platform as well as the utility of voting for them depend on I_j, the information on the candidate provided on the ballot.

$$max\ EU_i\left(c_j^1, \dots, c_j^N\right) = \sum_{n=1}^{N} \pi_j(I_j) U_i\left(c_j^n(I_j)\right) = \\ = \sum_{n=1}^{N} \pi_j(I_j)\ U_i\left(g_{i,j}(I_j) - \left|p_j(I_j) - P_i\right|\right) \qquad (3)$$

We can imagine the impact of I_j as a sorting device which helps the voters to distinguish the different candidates. To clarify this point, let us consider a situation in which there is hardly any information about the candidates available on the ballot, for instance, only their family names. In this case, it is basically impossible to deduce anything about the candidates' likely policy position, their individual chances to get elected, or their ability to carry out their proposals. The consequence is that all candidates appear rather similar to the voter and selecting particular ones does not matter, so we would expect the voting to happen randomly and the resulting distribution of votes over the candidates to be rather flat.

By contrast, consider an alternative situation with considerable information on the candidates on the ballot, for instance, their full names, birth year, and current profession. Now voters are able to infer much more about who the candidates are, what they are probably interested in, the type of daily problems they encounter, their education, their qualifications and abilities, etc. On this basis, voters can form their own opinion about the candidates, select those they consider best for them, and thus come closer to their solution under perfect information.

On the other hand, looking at several different information cues and spending some time in the voting booth figuring out the best candidates also involves certain costs in terms of invested time and effort, C_i. These costs reduce voter i's utility of

[35] For instance, profession information is stated on the ballot in in 10 out of 11 states in Germany which use open-lists to elect their state parliament. In the US, states like California also include candidate occupation for all state-wide races (McDermott, 2005).

participating in the election by $C_i(I_i^u)$, which increases in the amount of information (and thus time) *used* to come to a decision, I_i^u. Therefore, the overall utility of participating in an election without perfect information on the candidates, OU_i, may be described as the expected utility of voting for *N* specific candidates minus the cost of investing time and effort into the decision process:

$$OU_i = EU_i\left(c_j^1, \ldots, c_j^N \mid I_i^u\right) - C_i(I_i^u) \tag{4}$$

In general, the importance of information cues for the selection of individual candidates and the overall utility of voting depends on how many candidates a voter knows or believes to know relative to the number of votes to be cast. The more candidates are known and the fewer votes have to be allocated, the less important are information cues for voting behavior. As consequence, the theoretical framework for voting under imperfect information described here will have more predictive power if the institutional level of the election and thus the efforts of the candidates to get known are low than in a highly publicized election with a lot of media attention and high-effort campaigns by the candidates.

3.2.3 Hypotheses

The theoretical framework laid out above incorporates several aspects of existing theories of voting behavior, in particular, that voters react to information cues (e.g., Bartels, 1996; Goodman and Murray, 2007; McDermott, 2005) and that (perceived) similarity between the candidate and the voter matters, as it is taken as signal of close political positions (Sigelman and Sigelman, 1982, Cutler, 2002). Additionally, it can be used to derive new testable hypotheses about how information cues in general and profession information in particular affect voter satisfaction (hypotheses 1 and 2) and voting patterns (hypotheses 3 and 4).

Hypothesis 1: Profession information enables voters to select candidates by whom they feel better represented.

We have seen in equations 3 and 4 that differences in the amount and content of information stated on the ballot may affect the utility citizens gain from casting their votes for certain candidates. In particular, providing more information should help voters to select "better" candidates, i.e., those they expect to be closer to their own position and/or more capable of enacting their respective program. In terms of the

theoretical model, this increases the expected utility from voting for these candidates by minimizing $\left|p_j(I_j) - P_i\right|$ and raising π_j, respectively.

Hypothesis 2: Profession information increases satisfaction with and demand for open-list election systems.

Apart from enabling the voters to select "better" candidates, stating profession information may also improve voter satisfaction with the election system overall. Imagine, for instance, a situation in which the voter does not know the candidates and no further information about them is provided on the ballot. In this case, the candidates are anonymous and interchangeable and selecting one over the other may seem irrelevant. Possessing information about the candidates, on the other hand, enables the voter to have a more concrete image of the candidates (however correct it may be) and increases her feeling of making a real impact on the outcome. In the model, this should raise the utility of voting per-se, $g_{i,j}$, and thus further increase the overall expected utility obtained from voting in this election system.

This result may not apply to every other information cue, however. As equation 4 illustrates, having more information at your disposal may also raise the cost of voting, C_i, by making the decision process increasingly more tedious. Therefore, if the information added is not really meaningful and does not contribute to a better selection of candidates, the total effect of including another information cue on the overall utility of participating in an election, OU_i, may even be negative. Stating the candidates' hair and eye color, for instance, could probably distract some voters from picking suitable candidates without providing any useful signal about their political position or qualification.

Next, the following two hypotheses describe the way adding profession information may influence the actual voting behavior and outcomes in the election.

Hypothesis 3: Given that voters do not know the candidates, profession is more important for their choice than the candidates' name, rank on the list, and gender.

In many elections for multi-person bodies (i.e., parliaments or councils), there are hundreds of candidates running on the lists of several competing parties. Facing the inherent trade-off in equation 4 between considering more information cues to more closely identify good candidates and trying to minimize the cost of a protracted decision-making process, voters can be expected to focus on those cues that help them

the most to differentiate between the candidates and quickly cut down their large number, while disregarding less meaningful ones.[36] The importance of a particular information cue in this respect is likely determined by two features, *applicability* and *predictive power*. "Applicability" of an information cue can be considered as its capacity to establish a preference ordering of the available alternatives, and "predictive power" as how well it may signal ideological proximity or similarity between candidate and voter, and/or the candidate's qualification to perform well.

Applying these two criteria, profession information should trump anything that could be inferred from the candidates' name and rank on the list (e.g., gender from the first name, ethnic origin from the family name, or party loyalty from the rank). It is more applicable than gender, as it has a larger range of different values and can thus create a more detailed preference ordering of the candidates of a list. Furthermore, it seems at least as good in signaling ideological proximity and certainly better in signaling individual qualification. Profession information should also possess greater predictive power than the candidates' family name and rank in the list, as the information that could be inferred from these two cues is more speculative. Following this reasoning, we may expect individuals to allocate their votes mainly based on profession information, once it is available.[37]

Hypothesis 4: Given the party affiliation of the candidates, voters care more about their qualification than about perceived political proximity.

The most important selection criterion to start with, however, is certainly the party identity of the candidates, since voters are usually much better informed about the general position of parties than about the specific proposals of the individual candidates (Rahn, 1993). Thus, the predictive power of the party cue should be very large, enabling the voter to get rid of the large majority of candidates on the ballot, thereby reducing the selection problem to the members of only one list. As candidates of one party tend to share at least a certain set of core values or a common ideology, the voter can assume they all have policy positions that should be acceptable to her. A voter who attempts to

[36] The method described in this paragraph leans on the "take-the-best" algorithm presented in Gigerenzer and Goldstein (1996).

[37] In different societies, however, the importance of the different cues may vary with the main cleavages and current political topics. If the main issue voters care about is ethnic origin or clan affiliation, for instance, then the name of the candidates may be a more accurate signal of their political position than their profession.

maximize her overall utility of voting according to equation 4 should therefore primarily care about who has the highest potential to work effectively. In terms of the theoretical framework, the net marginal benefit of selecting a seemingly high quality candidate over a low quality one should be larger than that of finding someone whose personal characteristics and work in a certain type of profession may indicate a closer ideological proximity than the average candidate on the preferred party's list.

3.3 Survey Design and Identification Strategy

To test these hypotheses, I conducted an exit poll of voters at the Spanish local elections on the 22nd of May, 2011, asking every third individual leaving one of three different polling stations in the city of Barcelona to participate in a survey on voting behavior. The survey consisted of three parts: First, respondents answered some questions on the real election they had just participated in, which used closed lists. These contained: how satisfied they were with the possibility to express their own political opinion in the election, which party they voted for, how many candidates of that party they knew, and how well they thought the candidates of that party would represent them if elected. Then the survey participants were asked to vote in a hypothetical election with open lists, in which they should assign a total of 6 votes on a list of 30 fictional candidates of their preferred list, i.e., they should assume this was the list of the party they usually vote for.

After that, respondents answered questions about the hypothetical election and reported some individual characteristics. These questions mirrored the ones about the real election. They included the voter's satisfaction with the hypothetical election, how well they felt the chosen hypothetical candidates would represent them, whether they would have liked to see more information on the candidates and, in that case, what specific piece of information, whether they used some kind of method to allocate their votes and if so which, and which of the two election systems they preferred. With respect to the individual characteristics, participants were asked to state their gender, age group (10-year intervals), marital status, education level, and current profession. At the end of the questionnaire, participants also had the possibility to note down some comments and suggestions. The response rate was relatively high, with about half of the

1. Initials + Family names		
2. Full name		
3. Full name + "male" HS prof.	**4.** Full name + "neutral" HS prof.	**5.** Full name + "female" HS prof.
6. Full name + "male" LS prof.	**7.** Full name + "neutral" LS prof.	**8.** Full name + "female" LS prof.

Fig. 2. *Information on each candidate included in the 8 different versions of the ballot*

contacted individuals filling out the questionnaire, which took them between five and ten minutes.[38]

The core part of the survey was the hypothetical election. In order to test the impact of differences in the amount and content of information cues on voting behavior and voter satisfaction, eight different versions of the list of hypothetical candidates were randomly assigned to the respondents. On each list, half of the 30 candidates were female and half male,[39] appearing in alternating order on the list starting with a male top candidate. Over all eight versions, every candidate kept the same family names[40] and the same rank in the list in order to prevent any confounding effects due to information contained in the name (Guell et al., 2007) or the position on the list (Ho and Imai, 2008). The versions differ, however, with respect to the first name and the candidates' profession. The first only includes the initials of the first names, making it impossible to distinguish female and male candidates. The second states the candidates' full name, but nothing else. Finally, versions three to eight additionally indicate the candidates'

[38] The complete questionnaire of one ballot version with professions and the exact wording of the questions (in Spanish) is available in the appendix.

[39] The equal representation of women and men on the ballot follows the Spanish Equality Law of 2007, which prescribes parties to include at least 40% of candidates from each sex on their election lists.

[40] The names of the candidates were taken from real ballots for the election to the European Parliament in Spain in 1994, in order to be as close to a "real" Spanish election list as possible. I only chose names from the very bottom of the respective parties' list (which contained more than 60 candidates each) to avoid any name recognition effect. Furthermore, given the limited authority the European Parliament possessed at that time, the candidates were not the high-profile party leaders, but rather unknown party functionaries. Finally, I removed concrete indications for the regional origin of the candidates, for instance, the Catalan "i" (meaning "and") between the two family names.

profession after their full name, but vary the type of profession for each candidate from version to version.

Figure 2 summarizes the information available on the eight different ballot versions for each individual candidate. As we can see, the allocation of professions to the candidates follows a strict pattern: For each candidate, three ballot versions report a profession that usually requires some college education, and three where this is not necessary (in the following, "high-skill" and "low-skill" professions, respectively). Additionally, two versions per candidate state female-dominated professions, two in which the gender fraction is around equal, and two which are predominantly male. In each of these three categories, one profession is of the high-skill and the other of the low-skill type. The sorting of professions into skill level and gender dominance is based on information from the Spanish Labor Force Survey of 2010. I define a profession as "high-skill" if more than 70% of its workers report to possess some college education and as "low-skill" if less than 30% do so. Further, a profession is characterized as "female-dominated" if the share of women working in it is greater than 70%, "neutral" if it lies between 40 and 60%, and "male-dominated" if it is less than 30%. A detailed list of the 30 professions used and their respective shares of women and academics in the Spanish Labor Force Survey of 2010 is provided in table 1.

Each of these 30 professions is assigned to only one candidate in any single ballot version. As a consequence, the skill level and the gender category of professions are exactly balanced in each individual ballot version that includes information on profession. That is, each ballot contains 15 candidates with high-skill and 15 with low-skill professions and within both skill groups, 5 candidates state a male-dominated, neutral, and female-dominated profession, respectively.[41]

Given the experimental setup of the survey, the strategy to identify the causal effect of stating the candidates' profession on voting behavior and voter satisfaction is to compare the outcomes of the different versions. As respondents are randomly assigned, the different groups form credible counterfactuals for each other. At the same time, the amount and content of information provided in the various versions is designed to change only incrementally. Thus, the causal effect of knowing the candidates' gender (first name) on voter satisfaction and the distribution of votes is

[41] The exact allocation of professions to each individual candidate can be seen in table A.1 in the appendix.

Table 1

The sorting of professions into the different categories

	Male-dominated (<30% women)			*Gender-neutral* (40 - 60% women)			*Female-dominated* (>70% women)		
	Profession	% females	% academics	Profession	% females	% academics	Profession	% females	% academics
High-skill profession (>70% academics)	Engineer	8.8	100.0	Physician	41.4	100.0	Psychologist	76.3	100.0
	Architect	22.6	100.0	Lawyer	56.7	100.0	Nurse	81.8	100.0
	Information Scientist	15.8	84.2	Tax inspector	56.7	100.0	Sociologist	76.3	100.0
	Chemist	28.9	77.8	Teacher	56.3	99.1	Historian	76.3	100.0
	Manager	25.0	72.2	Civil servant	50.0	87.5	Kindergarten teacher	82.4	98.9
Low-skill profession (<30% academics)	Firefighter	3.2	29.0	Self-employed	58.6	15.7	Caregiver	87.5	20.1
	Plumber	1.1	15.6	Porter	41.4	13.8	Medical secretary	93.1	19.9
	Bus driver	8.9	8.9	Salesperson	41.7	10.4	Hairdresser	88.5	17.2
	Painter	2.2	8.8	Waiter	50.0	8.7	Cleaner	89.5	4.4
	Carpenter	1.5	3.0	Baker	41.7	6.5	Launder	89.5	4.4

Source : Spanish Labor Force Survey, 2010.

obtained by comparing the ballot version that states the full names of the candidates with the one that only contains the family names and the initial of the first name. Likewise, the effect of displaying the candidates' professions on top of knowing their gender can be computed as the difference between the versions with information on profession and the version with the full name of the candidates.

Design and identification strategy of this study are thus similar in several ways to that of McDermott (2005), who uses data from an experimental survey conducted by the Los Angeles Times in 1994, in which registered voters were asked about their intended voting behavior in upcoming elections for six different state-wide offices in California. As in my survey, respondents were randomly assigned to one of two different information settings. Half of the respondents obtained only the candidates' name and party affiliation, while their profession was added for the other half. The impact of profession information was then evaluated by comparing the vote shares of the candidates in the different samples.

There are several differences between this paper and McDermott's, however. First, I analyze the impact of profession on how voters choose between candidates from one party, whereas McDermott studies the choice between candidates of different parties in a run-off. Second, I have complete control over the professions stated on the ballot, which gives me the possibility to vary professions for the same candidate. And third, there is a lot of additional information contained in my survey with respect to the respondents' satisfaction, their self-reported voting considerations, and their personal characteristics. This allows to examine heterogeneous effects of profession information across certain socio-demographic subgroups, on the one hand, and to evaluate its impact on different outcome variables like voter satisfaction, on the other.

3.4 Descriptive Statistics

In total, 372 individuals participated in the survey, with the sample size for each individual ballot version ranging between 42 and 49. Table 2 reports the descriptive characteristics for the whole sample and broken down by ballot version. About 45% of all survey respondents are female, roughly 80% have at least a high-school degree, and the median age lies in the range between 46 and 55. Furthermore, the participants are, on average, modestly content with the possibility to express their political opinion in the

Table 2

Descriptive statistics for the different ballot versions

Variable	**Full sample**	**Ballot version**							
		Family name	Full name	Prof 1	Prof 2	Prof 3	Prof 4	Prof 5	Prof 6
Frequency	372	45	47	42	46	48	48	49	47
Satisfaction [1]	6.50	5.98	6.64	6.24	7.11	6.69	6.65	6.29	6.36
[1-10]	(2.88)	(3.03)	(3.00)	(2.52)	(2.87)	(2.74)	(2.77)	(3.18)	(2.92)
Good representatives [2]	5.87	6.17	5.63	5.67	6.10	5.89	5.98	5.35	6.20
[1-10]	(2.54)	(2.38)	(2.73)	(2.68)	(2.76)	(2.50)	(2.28)	(2.80)	(2.18)
# of candidates known	2.17	2.12	1.98	2.15	2.26	2.34	2.39	1.91	2.20
[topcoded at 3]	(1.02)	(1.13)	(1.10)	(1.11)	(0.91)	(0.91)	(0.93)	(1.07)	(0.98)
Female	0.449	0.556	0.426	**0.619****	0.391	0.383	**0.292****	**0.583***	0.362
	(0.50)	(0.50)	(0.50)	(0.49)	(0.49)	(0.49)	(0.46)	(0.50)	(0.49)
High-school degree	0.799	0.756	0.745	0.810	**0.891***	0.826	0.750	0.792	0.830
	(0.40)	(0.43)	(0.44)	(0.40)	(0.31)	(0.38)	(0.44)	(0.41)	(0.38)
Age 18-25	0.117	0.114	0.128	0.071	0.130	0.085	0.128	0.106	0.170
	(0.32)	(0.32)	(0.34)	(0.26)	(0.34)	(0.28)	(0.34)	(0.31)	(0.38)
Age 26-35	0.150	0.182	**0.064****	0.167	0.087	0.191	0.128	0.213	0.170
	(0.36)	(0.39)	(0.25)	(0.38)	(0.28)	(0.40)	(0.34)	(0.41)	(0.38)
Age 36-45	0.202	0.182	0.213	0.167	0.217	0.255	0.234	0.191	0.149
	(0.40)	(0.39)	(0.41)	(0.38)	(0.42)	(0.44)	(0.43)	(0.40)	(0.36)
Age 46-55	0.234	0.227	0.277	0.286	0.283	0.191	0.234	**0.106****	0.277
	(0.42)	(0.42)	(0.45)	(0.46)	(0.46)	(0.40)	(0.43)	(0.31)	(0.45)

Age 56-65	0.188	0.205	0.170	0.286	0.152	0.213	0.170	0.191	0.128
	(0.39)	(0.41)	(0.38)	(0.46)	(0.36)	(0.41)	(0.38)	(0.40)	(0.34)
Age 66+	0.109	0.091	0.149	**0.024*****	0.130	0.064	0.106	0.191	0.106
	(0.31)	(0.29)	(0.36)	(0.15)	(0.34)	(0.25)	(0.31)	(0.40)	(0.31)
Single	0.341	0.386	0.298	0.286	0.304	0.340	0.375	0.396	0.340
	(0.47)	(0.49)	(0.46)	(0.46)	(0.47)	(0.48)	(0.49)	(0.49)	(0.48)
Married	0.515	0.364	0.511	0.643	0.565	0.553	0.479	0.521	0.489
	(0.50)	(0.49)	(0.51)	(0.48)	(0.50)	(0.50)	(0.50)	(0.50)	(0.51)
Separated / Divorced /	0.144	0.250	0.191	0.071	0.130	0.106	0.146	0.083	0.170
Widowed	(0.35)	(0.44)	(0.40)	(0.26)	(0.34)	(0.31)	(0.36)	(0.28)	(0.38)

*, **, *** = Significant difference from the all-sample average on 10%, 5%, and 1% level, respectively

[1] Satisfaction with the possibilities to express one's own oppinion in the real Spanish local elections.

[2] Belief about how well the chosen representatives in the real Spanish local elections will represent the participant.

Note. Standard deviations reported in parentheses.

real election (6.5 out of 10), but not very enthusiastic in their view about how well the candidates they voted for would actually represent them (5.9 out of 10).[42]

Looking at the numbers of table 2 with respect to the two important dimensions of random assignment and representativeness, we can see that the *randomization* of respondents over the different versions has worked well with respect to both the participants' answers about the real election and their individual characteristics. Only in 7 out of 112 cases (8 versions x 14 variables) do we observe a statistically significant deviation of the average of a ballot version from the mean of the overall sample in a certain characteristic. This concerns mostly the fraction of women among the participants, ranging from about 30% to more than 60%. On all the other variables, however, randomization of respondents has worked very well. This includes the variables related to the real close-list elections, in terms of their satisfaction with the possibilities to express their own political opinion, the belief that the chosen candidates would represent them well, and the number of candidates they actually knew, as well as the fraction of high-school degree holders, age group, and legal status.

The sample obtained also fares very well on *representativeness*. Table 3 compares the vote shares for the different parties in the real local election in Barcelona with the answers of survey participants to the question of which party they voted for. It shows that there are only three significant differences between the real election and the survey results. The first concerns the Partido Popular (PP), the Spanish conservative party, which is significantly underrepresented among those survey participants who answered this question. This could be caused, for instance, by a lower willingness of PP supporters to take part in the survey. An alternative reason could be that some Catalans may not want to reveal that they have voted for a party that supports the supremacy of the central government over the autonomous regions.[43] The second difference lies in the greater vote share in the survey for Iniciativa per Catalunya Verds - Esquerra Unida (ICV-EU), the joint list of the Catalan Green party and the United Left party. There are again several possible reasons for this deviation. Green party voters may be more inclined to take part in an election survey, for instance, or more willing to indicate their

[42] The possible answers for these two questions range from 1 to 10, with 1 the worst and 10 the best outcome. This scale is the same as used in Eurobarometer interviews, to guarantee the comparability of the results with similar questions there.

[43] The election poll website Electometro.es (2010) reported, for instance, that polls taken right before the state elections in Cataluña in the fall of 2010 consistently underestimated the performance of the PP.

Table 3

Votes for the different parties in the local elections in Barcelona, real results vs. statements in the survey

	Real elections			Survey			Difference
Party	Votes	% share	Std. dev.	Votes	% share	Std. dev.	in vote shares
CiU	174022	28.7	(0.45)	77	25.4	(0.44)	-3.3
PSC-PM	134084	22.1	(0.42)	57	18.8	(0.39)	-3.3
PP	104301	17.2	(0.38)	28	9.2	(0.29)	**-8.0*** **
ICV-EUIA	62939	10.4	(0.31)	48	15.8	(0.37)	**5.5** **
UpB-ERC	33593	5.6	(0.23)	22	7.3	(0.26)	1.7
CUP-AB	11805	2.0	(0.14)	6	2.0	(0.14)	0.0
C's	11750	1.9	(0.14)	7	2.3	(0.15)	0.4
Eb-CenB	10104	1.7	(0.13)	5	1.7	(0.13)	0.0
S.I.	6802	1.1	(0.11)	8	2.6	(0.16)	1.5
EV-GVE	6118	1.0	(0.10)	4	1.3	(0.11)	0.3
PIRATA.CAT	4659	0.8	(0.09)	3	1.0	(0.10)	0.2
PACMA	4298	0.7	(0.08)	3	1.0	(0.10)	0.3
Other parties	14003	2.3	(0.15)	5	1.7	(0.13)	-0.7
Blank votes	27093	4.5	(0.21)	30	9.9	(0.30)	**5.4*** **
	605571	100.0		303	100.0		

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

Note. "Survey" only includes those respondents who answered something to the question about which party they voted for, even if it was that they wouldn't reveal their vote.

Source : ElPais, http://resultados.elpais.com/elecciones/2011/municipales/09/08/19.html, visited on 28.7.2011.

choice. And finally, a greater share of survey respondents answered they had handed in a blank vote. It is hard to tell, however, whether this means that these respondents really decided not to vote for any party in the real election or simply did not want to reveal their choice in the survey.

3.5 Results

3.5.1 Impact on voter satisfaction

The first important aspect of stating more information on the ballot is whether this causes the voters to feel that they can make better choices and thus be happier with their representatives and the whole election system (*hypotheses 1 and 2*). To test this, I compare the answers to two questions about voter satisfaction between the respondents of the three different ballot versions containing "only family names", "full names", and "names and professions". The questions are: (1) "*Do you think the individual candidates of this hypothetical list would represent you well in case they were really elected?*", and (2) "*Are you satisfied with the way you could express your own political preferences in this hypothetical election?*" Additionally, I compare the answers to the question that directly asked participants to state their preference for one of the two election systems: "*In which election system do you prefer to vote, the one that you really used in these local elections or the one of this survey?*" Answers could range from 1 to 10 for the first two questions, whereas respondents were asked to make a choice between the real and the hypothetical election in the third one.

Figure 3 depicts the average response to these questions by ballot version.[44] It conveys a consistent pattern that more information about the candidates is correlated with higher satisfaction with the chosen representatives and the possibility to express one's own political opinion in this election system, as well as larger support for voting with open lists. This is true for both the change from only observing the family names to obtaining the full name, and the introduction of profession information on top of the full name. While the average satisfaction with the candidates and the election system is 4.8 and 5.5, respectively, in the "only family names" sample, it rises to 5.0 and 5.9 in the "full name" group, and even to 6.4 and 6.8 among the respondents with a "profession" ballot. The same pattern holds for the fraction of respondents who prefer to vote in

[44] I excluded 19 survey participants from the analysis who stated their satisfaction but did not vote in the hypothetical election.

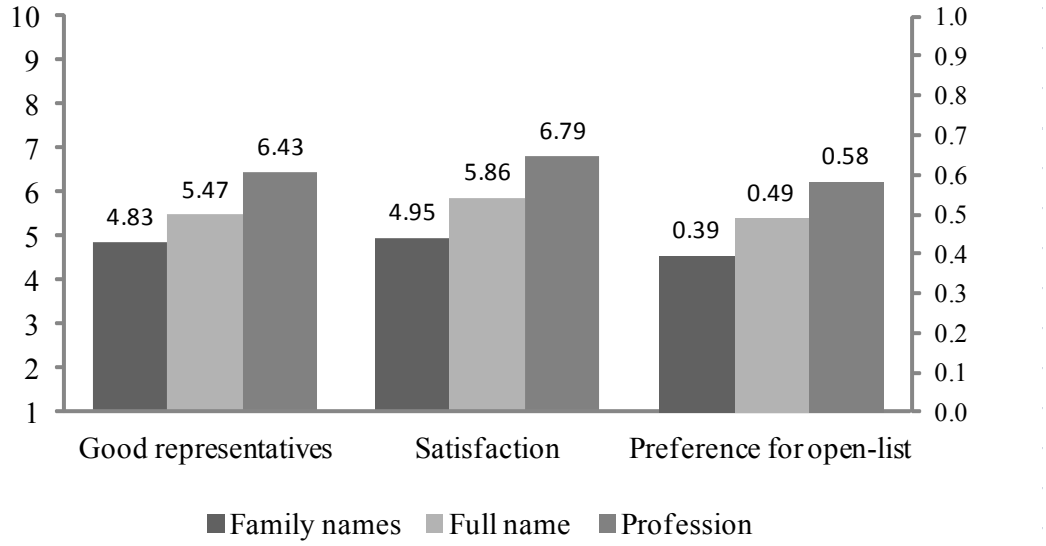

Fig. 3. *Average answers to questions about satisfaction and preference for open lists [by treatment]*

Notes. The bars represent the average answers of respondents of the different ballot versions to the following three questions: (1) "*Do you think the individual candidates of this hypothetical list would represent you well in case they were really elected*?" (= "Good representatives"), (2) "*Are you satisfied with the way you could express your own political preferences in this hypothetical election*?" (= "Satisfaction"), and (3) "*In which election system do you prefer to vote, the one that you really used in these local elections or the one of this survey*?" (= "Preference for open-list").
The relevant scale for "good representatives" and "satisfaction" is on the left-hand side (1 to 10), the one for "preference for open-lists" is on the right-hand side (the fraction of respondents preferring to vote in an open-list election system).

open-list rather than closed-list elections. Here, the increase is from 0.39 to 0.49, and eventually 0.58. These numbers suggest that providing more information about the candidates on the ballot, and in particular, stating the candidates' profession, may have a positive effect on voter satisfaction and support for open-list elections.

To test whether this finding holds when controlling for voter- and interview-specific characteristics, I run a series of OLS regressions of the following type:

$$depvar_i = \alpha_0 + \alpha_1 FAMNAMES_i + \alpha_2 PROFESSION_i + \gamma X_i + \varepsilon_i \qquad (5)$$

depvar is the answer of participant *i* to one of the three questions stated above. *FAMNAMES* and *PROFESSION* are dummy variables indicating whether respondent *i* answered an "only family names" ballot or one in which the full name and the profession were stated (all "profession" versions are bundled together for this analysis), leaving the "full name" sample as comparison group. *X* is a vector of control variables, including both voter-specific characteristics as age group, gender, legal status, and whether the respondent possesses a high-school degree, and interview-specific controls

Table 4

The impact of stating the candidates' profession on voter satisfaction

(1) Belief about how well the hypoth. candidates would represent the respondent if elected [1

	All respondents		By subgroup					
Dependent variable:			Men	Women	< 46	>= 46	Left	Right
Good representatives	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
"Only family names"	-0.64	-0.62	-0.32	-1.12	-0.86	-0.84	-1.23	0.86
	(0.70)	(0.65)	(1.03)	(0.79)	(0.95)	(1.02)	(1.10)	(1.04)
"Profession"	**0.97***	**1.08****	0.78	**1.19****	**1.42***	0.57	**1.55***	-0.41
	(0.51)	(0.48)	(0.73)	(0.58)	(0.74)	(0.68)	(0.84)	(0.85)
Voter-characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interview controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	347	345	189	156	167	178	130	103
R^2	0.04	0.18	0.11	0.36	0.27	0.16	0.31	0.24
Ad. R^2	0.04	0.13	0.01	0.27	0.19	0.08	0.20	0.08

(2) Satisfaction with possibilities to express the own opinion in the hypoth. election [1-10]

	All respondents		By subgroup					
Dependent variable:			Men	Women	< 46	>= 46	Left	Right
Satisfaction	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
"Only family names"	-0.91	-0.82	-0.85	-1.14	-1.47	-1.02	-0.94	0.93
	(0.77)	(0.71)	(1.13)	(0.89)	(1.03)	(1.09)	(1.29)	(1.20)
"Profession"	0.93	**1.09****	0.45	**1.46****	1.16	0.50	**1.56***	0.57
	(0.57)	(0.52)	(0.75)	(0.67)	(0.78)	(0.73)	(0.83)	(0.96)
Voter-characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interview controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	350	348	191	157	168	180	131	104
R^2	0.04	0.18	0.09	0.37	0.29	0.14	0.26	0.26
Ad. R^2	(0.04)	(0.12)	-(0.01)	(0.29)	(0.21)	(0.05)	(0.14)	(0.11)

(3) Preference for the hypothetical election with open list [fraction of respondents]

	All respondents		By subgroup					
Dependent variable:			Men	Women	< 46	>= 46	Left	Right
Preference for open li	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
"Only family names"	-0.09	-0.11	0.06	**-0.30***	-0.17	-0.13	**-0.41****	0.05
	(0.11)	(0.11)	(0.16)	(0.16)	(0.16)	(0.17)	(0.19)	(0.24)
"Profession"	0.09	0.10	0.14	0.02	0.09	0.12	-0.02	0.04
	(0.08)	(0.08)	(0.11)	(0.13)	(0.12)	(0.12)	(0.15)	(0.16)
Voter-characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interview controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	337	334	184	150	163	171	129	100
R^2	0.02	0.10	0.09	0.20	0.10	0.13	0.29	0.18
Ad. R^2	0.01	0.04	-0.01	0.08	0.00	0.04	0.17	0.00

*, **, *** = significantly different from 0, i.e., the "full names" ballot version, on the 10%, 5%, or 1% level, respectively.

Notes. (1) Robust standard errors are reported in parentheses. (2) Respondents who did not vote in the hypothetical election are excluded from the sample. (3) "Left" includes all respondents who reported to have voted for left-leaning parties, i.e., socialist, communist, green, republican, or animal-rights parties. (4) "Right" contains the voters of nationalist and conservative parties, independent of whether they are Catalan nationalists or Spanish ones.

for the location of the interview, the interviewer, and whether the interview took place in the morning or the afternoon.

Table 4 presents the results for each question. In column 1, only the means of the three different versions are compared, without adding any controls. The coefficients of *PROFESSION* show that the increase is only statistically significant for the satisfaction with the chosen representatives, but not for the satisfaction with the electoral system or the preference for voting with open lists. Controlling for voter- and interview-specific characteristics in column 2 increases the precision of the estimates, however, and results in significant coefficients for both satisfaction measures on the 5% level. Stating profession information on the ballot can therefore be expected to raise both the belief that the candidates would do a good job in representing the voter and the satisfaction with the election system by about 1.1 points on the 1 to 10 scale, an increase of almost 20% each compared to the respective level in the “full name” sample.

Furthermore, the increase of 9% points in the share of respondents who prefer to vote with open lists lends additional support to the general impression that providing

profession information leads to more satisfied voters and greater preference for open list elections. And although this change from 49% to 58% in favor of open-list elections is not statistically significant by itself, it does make a difference, as 58% are significantly different from 50% in a two-sided t-test (p-value of 0.008), whereas 49% is not. This means that there is a clear absolute majority pro open-list elections in the "profession" sample that is not present among the respondents of the "full names" version.

These findings supports the claim that the decisive aspect for voter satisfaction is not necessarily that they can choose between the candidates of their party, but that they have a *meaningful* and *informed* choice. It thus adds an important aspect to the literature which states that voting with open lists increases satisfaction with the way democracy works per-se (Farrell and McAllister, 2006). In elections for nationwide parliaments, this may be less important as there is usually a lot of media coverage and attention and only a small number of votes to allocate. For elections in a low-information environment, however, the amount and content of information provided on the ballot may be key to understanding voter satisfaction.

Columns 3 to 8 examine whether profession information is differentially important for various subgroups. Splitting the sample by gender, age (over and under 46 years old), and political position (voting for a left-leaning or right-leaning party in the real election[45]), we can see that there is a great consistency over the subgroups in reacting favorably to the inclusion of the profession information on the ballot, demonstrated by increases in the two satisfaction measures and the support for open list elections. The only exception to this finding are supporters of right-leaning parties, who seem to be rather indifferent on the whole to obtaining more information. At the same time, some examined subgroups react more strongly to getting to know the profession of the candidates than others. In particular, women and voters of left-leaning parties seem to value more profession information the most, as shown by statistically significant increases in their satisfaction with the chosen representatives and the way they can express their political opinion. Thus, it is not surprising to find the strongest supporters of open list elections in the group of women below 46 who voted for a left-leaning party (70% in the "profession" sample). The least supportive group, on the other hand, consists of men below 46 who voted for a right-leaning party. In this group, only 44.4%

[45] I define all socialist, communist, green, republican, or animal-rights parties as "left-leaning" in this case, and all nationalist and conservative parties as "right-leaning", independent of whether they are Catalan nationalists or Spanish ones.

of the respondents of the "profession" sample preferred the hypothetical open list election to the real one with closed lists.

3.5.2 Effect on Voting Behavior

The other important aspect of stating profession information is to know how such a move may affect the outcome of real elections (*hypotheses 3 and 4*). In particular, we want to know (1) whether voters take the profession cue into account when allocating their votes, (2) what they are looking for when using profession information for their decision, and (3) how this changes the composition of the elected body.

a) The importance of profession information for the voting decision

Let us start by looking at how survey respondents allocated their 6 votes across the 30 candidates in the hypothetical election. Table 5 displays for each candidate *j* the fraction of survey participants in ballot version *k* who gave her one of their votes. As it was not allowed to allocate more than one vote to a single candidate, this fraction is simply the total number of votes candidate *j* obtained in version *v*, X_{jv}, divided by the number of respondents with that ballot version, N_v:

$$\text{Fraction}(vote = 1)_{\text{jv}} = {}^{X_{jv}}\!/_{N_v} \tag{6}$$

If voters did not care at all about the information contained in the ballot and since they knew none of the candidates, we would expect them to either vote randomly or simply for the first six candidates in each of the ballot versions. In this case, differences between the eight ballot versions should be unsystematic and statistically insignificant. Testing the underlying distributions of votes against each other with a Chi2-test, however, shows that all of them are significantly different, with the exception of the first two. This indicates that, while the addition of the first name does not significantly change the overall distribution of votes, stating the candidates' profession largely influences the voters and their choice of candidates.

Table 5 additionally illustrates that the individual success varies significantly across candidates and from one version to the other. When the candidates' probability to get a vote in the various ballot versions is tested for equality with their respective success in the "full name" sample, we can see from the stars in the table that large

Table 5

Fraction of survey respondents giving a vote to candidate *j*, by ballot version [in %]

	Version								Difference
Candidate	Family names	Full name	Prof 1	Prof 2	Prof 3	Prof 4	Prof 5	Prof 6	Max-Min
1	25.0	34.9	45.0	27.3	17.4*	30.4	31.9	2.4***	42.6
2	37.5	27.9	17.5	18.2	54.3**	50.0**	8.5**	38.1	45.8
3	15.0*	30.2	10.0**	11.4**	34.8	50.0*	51.1**	7.1***	43.9
4	20.0**	41.9	35.0	6.8***	17.4**	19.6**	42.6	35.7	35.7
5	32.5	23.3	7.5**	6.8**	8.7*	28.3	31.9	26.2	25.7
6	25.0	16.3	55.0***	40.9**	10.9	43.5***	6.4	26.2	48.6
7	22.5	34.9	7.5**	27.3	39.1	17.4*	25.5	9.5	31.6
8	12.5	23.3	35.0	25.0	41.3*	8.7*	10.6	7.1***	34.2
9	25.0	14.0	42.5**	11.4	4.3	8.7	12.8	28.6**	38.2
10	15.0	16.3	7.5	36.36**	8.7	28.3	29.8	21.4	28.9
11	30.0	18.6	35.0*	22.7	10.9	10.9	8.5	21.4	26.5
12	17.5	18.6	5.0*	9.1	21.7	13.0	19.1	23.8	18.8
13	17.5	16.3	35.0*	2.3**	13.0	30.4	36.2**	0.0***	36.2
14	17.5	20.9	20.0	6.8*	4.3**	10.9	25.5	45.2**	40.9
15	12.5	7.0	2.5	2.3	10.9	15.2	36.2***	33.3***	33.9
16	17.5**	37.2	37.5	38.6	10.9***	6.5***	8.5**	11.9***	32.1
17	20.0	11.6	20.0	50.0***	45.7***	4.3	6.4	14.3	45.7
18	27.5	20.9	17.5	22.7	19.6	47.8***	6.4**	7.1*	41.4
19	30.0**	11.6	30.0**	11.4	10.9	0.0**	12.8	28.6*	30.0
20	17.5	9.3	2.5	4.5	6.5	17.4	31.9***	16.7	29.4
21	12.5	14.0	12.5	2.3**	30.4*	2.2**	27.7	26.2	28.3
22	25.0	30.2	2.5***	15.9	23.9	28.3	19.1	9.5**	27.7
23	10.0	11.6	2.5	25.0	19.6	19.6	6.4	7.1	22.5
24	15.0	11.6	32.5**	15.9	28.3**	2.2*	8.5	11.9	30.3
25	17.5	18.6	2.5**	11.4	23.9	47.8***	42.6**	7.1	45.3
26	5.0	14.0	32.5**	6.8	6.5	2.2**	10.6	45.2***	43.1
27	7.5	9.3	5.0	4.5	2.2	41.3***	12.8	35.7***	39.1
28	12.5	18.6	20.0	38.6**	8.7	0.0***	8.5	26.2	38.6
29	20.0*	7.0	15.0	38.6***	23.9**	0.0*	10.6	11.9	38.6
30	25.0	25.6	7.5**	38.6	41.3	15.2	10.6*	2.4***	38.9
Average	19.6	19.8	20.0	19.3	20.0	20.0	20.0	19.6	35.3
Variance	57.4	87.9	233.6	197.3	192.8	272.0	174.4	168.8	
N	40	43	40	44	46	46	47	42	
Sign. diff.[1]	5	0	14	11	11	16	9	14	

*, **, *** = significantly different from the result of the same candidate in the "full name" version.

[1] Number of candidates who probability to get a vote is significantly different to the "full name" version.

Notes. (1) The fraction of voters who give 1 of their 6 votes to candidate *j* is calculated as the absolute number of votes obtained by the candidate in a certain ballot version divided by the number of survey respondents with that version. (2) The highest and the lowest fractions for each candidate are highlighted in light grey. (3) Respondents who did not cast any vote or who cast invalid ballots are excluded.

differences in electoral chances exist. This holds in particular for the "profession" versions, for which the number of significant differences in electoral success with respect to the "full name" sample is 12.5 on average, compared to only 5 such cases in

the "only family names" sample. Thus, it seems evident that voters are very aware of the profession information provided on the ballot and that they take it into account when deciding for whom to vote.

As an example, consider the results for the highest-listed female candidate on rank 2. Just knowing her family names but nothing else (column 1), 37.5% of the respondents of the first ballot voted for her, whereas a random distribution of votes would lead to a probability of 20%. This could be due to her position within the top six on the ballot or because survey respondents liked her family names for some reason. Revealing her gender in the next ballot version reduces her success somewhat (but not significantly) to getting a vote of only 27.9% of the participants. Using this result as a benchmark to compare the impact of different professions on her electability, we see that this sends her on a roller-coaster ride. In version "Profession 3", in which she supposedly works as a lawyer, she hits a maximum of getting a vote from 54.3% of the respondents (26.4% points more than the benchmark), while she falls down significantly to only 8.5% as a hairdresser in "Profession 5" (-19.4% points). Similar results can be found for all other candidates in table 5 as well, as depicted by the numbers in the last column reporting the difference in probabilities between the version with the highest and the lowest for each candidate. Even the smallest number in this column still reports the large difference of 18.8% points, here between candidate 12 as a porter and an information scientist.

The finding that survey participants react to the differences in amount and content of information contained on the ballot can be tested more formally. For that purpose, I analyze the impact of gender first, and then, in a second step, the effect of providing profession information on top of that. So, let us start by considering the following linear probability model for the probability of candidate *j* to get one of the six votes of voter *i*, taking only the "full name" version into account:[46]

$$Prob(vote = 1)_{ij} = \alpha_1 * FEMALE_j + \alpha_2 * NAME/RANK_j + \varepsilon_{ij} \qquad (7)$$

In this model, the information cue for the gender of the candidate is represented by the dummy variable *FEMALE*, which is coded as 1 if the first name of the candidate signals a woman. I further include the continuous variable *NAME/RANK*, which states, for each candidate, the fraction of participants who gave her a vote in the "only family names"

[46] The results are qualitatively the same for logit / probit models, but the coefficients from a linear probability model are easier to interpret and thus provide a more intuitive understanding of the results.

sample. Including this benchmark probability allows us to properly isolate the impact of providing the first name and thus revealing the candidates' gender by controlling for the importance of the candidates' rank in the list and any information that respondents could potentially obtain from their family names. If the estimated coefficient of *NAME/RANK* is found to be close to 1 although other cues than rank and family names are available, this would indicate that voters do not care about these additional sources of information. In this case, the distribution of votes across candidates should be very similar to the one in the "only family names" sample. If, on the other hand, the coefficient turns out to be small or close to 0 when more information cues are added, we would know that voters take these other signals into account and that the resulting distribution of votes should look quite different compared to the "only family names" one. [47]

Given the setup of the experiment, voter- or interview-specific controls are redundant for this and all following specifications that use the probability to get a vote as dependent variable. The reason for this is that every voter distributed the same number of votes over the same set of candidates, which means that if a vote is not cast for one candidate, then it is given to one of the others. Hence, since all candidates are included in the estimation, there is zero correlation between possible control variables like gender, education level, marital status, interview location and time, and the particular interviewer, on one side, and the average probability to get a vote, on the other.[48] Including them would therefore be completely irrelevant.

The estimates of a series of linear probability models following specification 7 are reported in table 6. We can see in column 1 that voters still seem to be greatly influenced by the candidate's rank and family names in this version, as the coefficient of *NAME/RANK* is almost 0.8 and highly significant. Conditional on gender, a candidate who obtained a vote from 30% of the respondents in the "only family names" sample is

[47] As the value of *NAME/RANK* is constant for each candidate across ballot versions and voters, it acts as a candidate-specific constant. For this reason, I do not include a constant in any of these models to facilitate the interpretation.

[48] Table A.2 in the appendix demonstrates this feature by presenting the result of regressing the probability to get a vote on all possible voter- and interview-specific controls. As expected, the estimated coefficients are all insignificantly different from 0, both individually and jointly (as shown by the very low F-statistic).

Table 6

The impact of gender on a candidate's probability to get a vote, by subgroups of voters

		Sample divided by voters' ...					
	Whole sample "Full names"	Sex		Age		Ideology	
Variable	(1)	Men (2)	Women (3)	< 46 (4)	>= 46 (5)	Left (6)	Right (7)
Female	0.071 ***	0.073 ***	0.068	0.024	0.108 ***	0.091 **	0.004
	(0.022)	(0.025)	(0.041)	(0.040)	(0.022)	(0.040)	(0.032)
Name/Rank[1]	0.78 ***	0.768 ***	0.797 ***	0.912 ***	0.676 ***	0.727 ***	0.948 ***
	(0.061)	(0.066)	(0.116)	(0.113)	(0.056)	(0.113)	(0.086)
N	1290	750	540	570	720	450	480
R^2	0.201	0.2	0.204	0.205	0.203	0.2	0.206
Ad. R^2	0.2	0.197	0.201	0.202	0.201	0.197	0.202

*, **, *** = significantly different from 0 on the 10%, 5%, or 1% level

[1] Name/Rank is the probability of a candidate to get a vote in the "Only family names" version.

Notes. (1) Clustered standard errors (on the voter level) are reported in parentheses. (2) "Left" includes all respondents who reported to have voted for left-leaning parties, i.e., socialist, communist, green, republican, or animal-rights parties. (3) "Right" contains the voters of nationalist and conservative parties, independent of whether they are Catalan nationalists or Spanish ones.

thus predicted to obtain 24% in the "full name" ballot version due to her rank in the list and family names.

On the other hand, the *NAME/RANK* estimate is statistically different from 1 as well, indicating that survey respondents also reacted to the new information cue for the gender of the candidates. This is confirmed by the positive and significant coefficient for *FEMALE*, meaning that the probability of female candidates to get a vote is 7.1% points higher than for male candidates in this sample. This result corresponds with that of Esteve-Volart and Bagues (2009), who find that female candidates in Spanish senate elections get more votes than their male counterparts, and polling data from Spain stating that 87% of the citizens believe more women should participate in public institutions (CIS, 2005). Columns 2 to 7 report the results for the same model, but dividing the sample along the lines of gender, age (younger and older than 46), and political orientation[49]. The estimates for these subgroups suggest that the boost in voting for female candidates comes mainly from men, individuals older than 46, and people voting for a left-leaning party.

[49] Respondents are coded as left-leaning if they state to have voted for i.e., socialist, communist, green, republican, or animal-rights parties. "Right", on the other hand, contains the voters of nationalist and conservative parties, independent of whether they are Catalan or Spanish nationalists.

These results show that the "gender" cue seems to matter in circumstances in which only the name of the candidate and her rank in the party list is known. In a second step, we can now evaluate how the inclusion of the candidates' profession affects voters. Looking at the six ballot versions that additionally state the candidates' profession, I add indicators for each profession l to the model in equation 6. Thus we have:

$$Prob(vote = 1)_{ijl} = \sum_{l=1}^{30} \beta_l * PROFESSION_{lj} + \alpha_1 * FEMALE_j + \\ + \alpha_2 * NAME/RANK_j + \varepsilon_{ijl} \quad (8)$$

As there is no constant included in this model, the coefficients of the single *PROFESSION* dummies provide us with direct estimates for the probability of a *male* candidate to get one of the six votes if he states to work in a particular profession, only conditional on his family names and ranking in the list. Any systematic difference for *female* candidates is captured by the *FEMALE* dummy as before, and there are again no voter or interview controls included. The results are displayed in table 7, again for the total sample and the subgroups.

The first aspect to note is that the appearance of information on the candidates' profession drastically reduces the importance of both the gender cue and the control for name and rank effects. Whether a candidate is female completely loses importance for the respondents of the "profession" ballot versions. Likewise, the coefficient of only 0.157 for *NAME/RANK* means that being placed well on the list and having appealing family names does still exert some influence on voters, but very limited in comparison to the other ballot versions and to the estimated coefficients for most of the professions. The coefficients are ranked by their magnitude in column 1 and range from 0.441 for lawyers to basically 0 for bus drivers, meaning that, conditional on the name of the candidate, her sex, and the position on the list, lawyers have a 44% chance to get one of the six votes of a survey respondent, whereas bus drivers are apparently not considered as suitable members of a parliament or council. To complete the picture, the other top 5 positions are held by sociologists, engineers, physicians, and psychologists, whereas waiters, hairdressers, plumbers, and porters complete the bottom 5 (both in descending order).

It seems also interesting to look at the differences in the estimated probabilities of the different professions across subgroups. Between men and women,

Table 7

The effect of including the candidates' profession on the probability to get a vote

		Sample divided by voters' ...			
	All profession versions	Sex		Ideology	
		Men	Women	Left	Right
Variable	(1)	(2)	(3)	(4)	(5)
Female	0.01	0.009	0.013	0.018	-0.013
	(0.008)	(0.011)	(0.013)	(0.013)	(0.017)
Name/Rank [1]	0.157**	0.233**	0.066	0.17	0.135
	(0.064)	(0.091)	(0.092)	(0.114)	(0.106)
Lawyer	0.441***	0.432***	0.444***	0.374***	0.532***
	(0.035)	(0.048)	(0.052)	(0.057)	(0.062)
Sociologist	0.375***	0.332***	0.42***	0.379***	0.339***
	(0.033)	(0.045)	(0.049)	(0.055)	(0.058)
Engineer	0.349***	0.377***	0.314***	0.323***	0.389***
	(0.033)	(0.044)	(0.049)	(0.050)	(0.061)
Physician	0.338***	0.297***	0.393***	0.418***	0.324***
	(0.032)	(0.043)	(0.049)	(0.056)	(0.058)
Psychologist	0.337***	0.301***	0.383***	0.342***	0.31***
	(0.033)	(0.045)	(0.050)	(0.055)	(0.061)
Historian	0.322***	0.326***	0.308***	0.29***	0.334***
	(0.031)	(0.042)	(0.047)	(0.050)	(0.056)
Teacher	0.32***	0.286***	0.365***	0.394***	0.284***
	(0.033)	(0.045)	(0.049)	(0.054)	(0.062)
Self-employed	0.319***	0.262***	0.384***	0.322***	0.275***
	(0.032)	(0.043)	(0.047)	(0.049)	(0.056)
Tax inspector	0.246***	0.264***	0.225***	0.277***	0.314***
	(0.032)	(0.044)	(0.046)	(0.050)	(0.065)
Manager	0.241***	0.243***	0.227***	0.162***	0.381***
	(0.032)	(0.044)	(0.047)	(0.048)	(0.065)
Architect	0.234***	0.244***	0.216***	0.212***	0.31***
	(0.032)	(0.045)	(0.045)	(0.051)	(0.062)
Firefighter	0.166***	0.191***	0.129***	0.125***	0.199***
	(0.028)	(0.039)	(0.039)	(0.044)	(0.050)
Chemist	0.148***	0.132***	0.167***	0.09**	0.179***
	(0.027)	(0.036)	(0.041)	(0.039)	(0.046)
Kindergarten	0.143***	0.079**	0.224***	0.165***	0.089*
teacher	(0.028)	(0.033)	(0.048)	(0.049)	(0.046)
Civil servant	0.141***	0.131***	0.151***	0.135***	0.173***
	(0.025)	(0.034)	(0.037)	(0.040)	(0.049)
Nurse	0.124***	0.104**	0.147***	0.135***	0.163***
	(0.026)	(0.036)	(0.039)	(0.047)	(0.047)
Information	0.118***	0.097**	0.145***	0.144***	0.136***
Scientist	(0.027)	(0.037)	(0.042)	(0.046)	(0.050)

Table 7 (continued)

Caregiver	0.092***	0.072**	0.118***	0.093**	0.087**
	(0.026)	(0.035)	(0.038)	(0.046)	(0.041)
Cleaner	0.087***	0.069**	0.11***	0.057	0.091**
	(0.024)	(0.031)	(0.036)	(0.038)	(0.040)
Medical	0.053**	0.044	0.06*	0.076*	0.028
secretary	(0.022)	(0.031)	(0.031)	(0.041)	(0.031)
Salesperson	0.052**	0.051	0.041	0.018	0.129***
	(0.022)	(0.031)	(0.031)	(0.035)	(0.046)
Painter	0.048**	0.042	0.054*	0.026	0.023
	(0.020)	(0.026)	(0.029)	(0.029)	(0.029)
Baker	0.045**	0.017	0.078**	0.044	0.09**
	(0.022)	(0.029)	(0.036)	(0.041)	(0.040)
Carpenter	0.036*	0.037	0.032	-0.007	0.052
	(0.021)	(0.030)	(0.031)	(0.033)	(0.035)
Launder /	0.031	0.002	0.068**	0.01	0.055*
Laundress	(0.019)	(0.024)	(0.031)	(0.029)	(0.032)
Waiter /	0.028	0.000	0.062**	0.047	0.009
Waitress	(0.018)	(0.023)	(0.030)	(0.033)	(0.028)
Hairdresser	0.027	0.008	0.052*	0.025	0.048
	(0.018)	(0.023)	(0.028)	(0.030)	(0.034)
Plumber	0.025	0.038	0.006	0.027	0.044
	(0.018)	(0.027)	(0.020)	(0.029)	(0.030)
Porter	-0.003	-0.027	0.024	-0.015	0.016
	(0.018)	(0.025)	(0.027)	(0.030)	(0.034)
Bus driver	-0.004	-0.02	0.013	-0.014	0.004
	(0.017)	(0.024)	(0.025)	(0.029)	(0.028)
N	7950	4500	3420	3000	2430
R^2	0.287	0.289	0.292	0.297	0.3
Ad. R^2	0.284	0.284	0.285	0.289	0.29

*, **, *** = significantly different from 0 on the 10%, 5%, or 1% level

[1] Name/Rank is the probability of a candidate to get a vote in the "Only family names" version.

Notes: (1) Clustered standard errors (on the voter level) are reported in parentheses. (2) Coefficients report the probability that a candidate gets a vote if she states to work in the respective profession, conditional on gender and name/rank. (3) "Left" includes all respondents who reported to have voted for left-leaning parties, i.e., socialist, communist, green, republican, or animal-rights parties. "Right" contains the voters of nationalist and conservative parties, independent of whether they are Catalan nationalists or Spanish ones.

men are significantly more likely to vote for engineers, firefighters, and plumbers, while women prefer self-employed workers and kindergarten teachers. There are also statistically significant differences in the voting behavior of left-leaning and right-leaning survey respondents. Those who voted for a left-leaning party in the real elections are more likely to select candidates working as teachers, physicians, kindergarten teachers, medical secretaries, and waiters, while supporters of right-leaning parties voted for managers, lawyers, and salespersons in much larger numbers.

b) Ideological proximity or qualification?

The next question to answer is the following: What information does a profession reveal about a candidate that really matters for the voters (*hypothesis 4*)? The first possibility is that it conveys information about the candidates' *political attitudes*. The more similar a candidate seems to be to the voter and her political ideals, the more likely it is that she may share the same political preferences and priorities on concrete issues (Sigelman and Sigelman, 1982, Cutler, 2002). The main alternative is that knowing a candidate's profession may influence the voter's opinion on how *qualified* the candidate is in getting things done. A recent poll among Spanish adults, for instance, finds that efficiency and qualification rank second and third on the list of most important aspects for a politician, only topped by honesty (CIS, 2011).

A first approach to answer the question is to look again at the results for each profession reported in column 1 of table 7. Nine out of the 10 highest-ranking professions, i.e., those with the largest estimated probability to get a vote, belong to the group of high-skill occupations such as lawyer, sociologist, engineer, or physician. The only exception is self-employed candidates on the 8th rank. By contrast, the bottom 10 of the ranking is occupied exclusively by low-skill professions such as hairdresser, plumber, porter, and bus driver, whose estimated coefficients are not significantly different from zero.

This pattern seems to confirm the conclusion of McDermott (2005) that voters look at the candidates' profession as a cue for their qualification or ability to do the job properly. At the same time, there is a striking difference from the results obtained by Mechtel (2011), who finds that the most "electable" professions in local elections in south-west Germany in 2009 were bakers, butchers, policemen, and farmers, compared

to a detrimental effect of working as management consultant, secretary, banker or insurer.[50]

On the other hand, table 7 also provides reason to belief that voters select candidates working in professions that may be linked to certain ideological positions in common opinion. As mentioned above (at the end of section 3.5.2.a), respondents who voted for a left-leaning party in the real elections are significantly more likely than supporters of right-leaning parties to vote for candidates in the hypothetical election who presumably work in professions that could be perceived as more "socially oriented", like teacher, physician, and kindergarten. At the same time, voters of right-leaning parties seem to prefer more "business oriented" occupations such as manager, lawyer, and salesperson. This suggests that individuals may possess certain beliefs with respect to the political attitudes of people working in particular professions and select those who correspond best with their own political position.

To disentangle the respective importance of the two proposed main channels of influence, political proximity and qualification, I run the following econometric model:

$$\begin{aligned} Prob(vote = 1)_{i,j} = {} & \alpha_1 * SAMESEX_{i,j} + \alpha_2 * SAMESKILL_{i,j} + \\ & + \alpha_3 * SAMEPROF_{i,j} + \alpha_4 * SAMEVIEW_{i,j} + \\ & + \alpha_5 * HSPROF_j + \alpha_6 * NAME/RANK_j + \varepsilon_{i,j} \end{aligned} \quad (9)$$

In this model, qualification is represented by the simple indicator for whether a candidate works in a high-skill profession, *HSPROF*. The different aspects and degrees of proximity between voter *i* and her political ideals, on the one hand, and candidate *j*, on the other, are represented by four dummy variables: *SAMESEX* is coded as 1 if voter and candidate have the same sex, *SAMESKILL* if both voter and candidate either work in a high-skill or a low-skill profession (defined as in table 1 by whether they typically require a university education), *SAMEPROF* if voter and candidate work in exactly the same profession, and *SAMEVIEW* if the candidate works in an occupation that the voter may perceive as more sympathetic to her own political position.

[50] A possible reason for this difference may be that Mechtel (2011) analyzed the behavior of voters who went to the polls at the height of the world financial crisis and wanted to "punish" those candidates who appeared to be linked to the financial sector.

To be able to sort the professions into perceived left-leaning, neutral, or right-leaning, I conducted a small additional survey asking voting-age individuals in Barcelona to indicate the most likely political position of a person working in each of the 30 professions used in this study.[51] The only available options were "center-left" and "center-right". Based on the statements of the 57 respondents, I code the six professions with the highest share of "center-left" answers as left-leaning. These are: caregiver, kindergarten teacher, teacher, nurse, waiter, and painter. Equivalently, the six professions with the highest share of "center-right" answers compose the group of right-leaning professions. In this category, we can find tax collector, manager, lawyer, physician, architect, or engineer. *SAMEVIEW* is then coded as 1 if a left-leaning (right-leaning) voter coincides with a left-leaning (right-leaning) candidate.

The regression results are shown in table 8.[52] In columns 1 and 2, the similarity indicators and the qualification measure are examined separately from each other, only conditional on the effect of family names and the rank in the party list captured by *NAME/RANK*. Judging from the goodness of fit of both specifications (the adjusted R^2 is 0.212 and 0.244, respectively) and the fact that all coefficients are statistically significant, we can see that both channels seem to be reasonable explanations for the effect of stating profession on the ballot. Putting all variables together in column 3, however, shows that only two specific aspects of similarity matter: The first is whether the candidate works in exactly the same profession as the voter (*SAMEPROF*). If this is the case, the candidate's probability to get a vote from this participant increases by about 47% points from what is predicted by the candidate's name and rank. The second is that voters are on average 4.7% points more likely to vote for candidates who work in a profession that they perceive as a signal of a certain political attitude close to their own political ideals (*SAMEVIEW*). On the other hand, the remaining two measures of similarity, *SAMESEX* and *SAMESKILL*, turn completely insignificant once the indicator for high-skill professions is included. By contrast, the magnitude and significance of *HSPROF* is not significantly influenced by the inclusion

[51] This question was not included in the main survey on election day, as it could have influenced the respondents' voting choice.

[52] In all specifications of table 9, only those survey respondents are included who stated their profession and had not already retired in order to have a coherent sorting into the similarity variables. This restriction is not crucial for the results, however, as the results hardly change if retirees and respondents without profession information are included.

Table 8

Type of further information participants would have liked to know about the candidates and method reportedly used to allocate the votes [by ballot type]

	A. Fraction of voters who wanted information on:				**B. Fraction of voters who used a certain method:**			
	Ballot version				Ballot version			
	Family names	Full names	Profession		Family names	Full names	Profession	
Program	0.422	0.426	0.332	Name	0.136	0.064	0.007 *	
	(0.50)	(0.50)	(0.47)		(0.35)	(0.25)	(0.08)	
Profession	0.178	0.149	0.025 * †	Random	0.114	0.043	0.007 *	
	(0.39)	(0.36)	(0.16)		(0.32)	(0.20)	(0.08)	
Education	0.133	0.085	0.018 *	Gender	0.023	0.106 *	0.046	
	(0.34)	(0.28)	(0.13)		(0.15)	(0.31)	(0.21)	
Age	0.089	0.064	0.036	6 first	0.023	0.043	0.007	
	(0.29)	(0.25)	(0.19)		(0.15)	(0.20)	(0.08)	
Gender	0.067	0.000 *	0.000 *	Profession	0.000	0.000	0.407 * †	
	(0.25)	(0.00)	(0.00)		(0.00)	(0.00)	(0.49)	
CV	0.067	0.128	0.154 *	Qualification	0.000	0.000	0.114 * †	
	(0.25)	(0.34)	(0.36)		(0.00)	(0.00)	(0.32)	
Party	0.044	0.021	0.011					
	(0.21)	(0.15)	(0.10)					
Prev. political	0.044	0.085	0.036					
activity	(0.21)	(0.28)	(0.19)					
Experience	0.000	0.043	0.046 *					
	(0.00)	(0.20)	(0.21)					

* = significant difference at 10% level with respect to the "only family names" sample
† = significant difference at 10% level with respect to the "full names" sample

Notes. (1) Clustered standard errors (on the voter level) are reported in parentheses. (2) "Same sex" coded as 1 if both voter and candidate belong to the same sex. (3) "Same skill" is equal to 1 if both voter and candidate work in a high-skill or low-skill profession. (4) "Same profession" is 1 if voter and candidate work in exactly the same profession. (5) "Same view" is based on which party the respondent supported in the real election and the perceived political position of individuals working in the various professions, based on a separate survey of 57 individuals in Barcelona. For voters of left-leaning parties, "same view" is equal to 1 if a candidate states to work in the following professions: caregiver, kindergarten teacher, teacher, nurse, waiter, and painter. The relevant professions for supporters of right-leaning parties are: tax inspector, manager, lawyer, physician, architect, and engineer. (6) "Other view" is coded in the opposite way as "same view". (7) "High-skill profession" is 1 for all professions that typically require a university degree.

of the similarity measures. Working in a high-skill occupation seems to be a powerful predictor of qualification for the job and leads to a 20% points higher probability to get a vote.

These results suggest that voters use profession information as a signal both for the candidates' qualification *and* their political position. If we compare the importance of the two channels of influence, however, it seems that voters care more about the candidates' qualification and then about their exact political position. The exception to this pattern are the rare cases of very close similarity between voters and candidates in terms of working in the same profession (in only 1.1% of the analyzed

voter-candidate observations did their professions coincide). This behavior seems reasonable, since the party identity of the candidate may already sufficiently reveal her position on most policy issues.

To check whether the result for *SAMEVIEW* simply picks up a part of the effect of *HSPROF* (9 of the 12 professions in *SAMEVIEW* are high-skill ones), I replace *SAMEVIEW* by a variable called *OTHERVIEW* in column 4, which is defined exactly opposite to *SAMEVIEW*. That is, it is coded as 1 if a left-leaning (right-leaning) voter coincides with a candidate working in a right-leaning (left-leaning) profession. The estimated coefficient for *OTHERVIEW* in column 4 is close to 0 and completely insignificant, while there is hardly any change in the results for the other variables. This suggests that the effect of *SAMEVIEW* is independent of the effect of high-skill professions, but rather represents the voters' considerations on what the profession of a candidate may reveal about her political attitude.

Finally, columns 5 and 6 report the results for the two skill subgroups separately. Both show that the probability to get a vote is about 19% points higher when stating a high-skill rather than a low-skill profession, independent of whether the voter herself works in a low-skill or a high-skill profession. This confirms the main conclusion of this section that voters primarily care about the candidates' qualification.

c) Gender aspects

Having shown that the inclusion of profession information on the ballot changes the composition of elected bodies towards individuals working in high-skill professions, the next question is now whether it also affects the electoral chances of men and women differently. The results discussed so far suggest that profession information per-se does not benefit or discriminate either sex, but it causes the preference for women in the "full names" sample to disappear. These findings could be biased by two aspects, however, one favoring men and the other women.

On the one hand, the real impact could actually be worse for women, as the perceived neutrality with respect to gender could be due to the design of the ballots in which men and women are equally distributed over both high-skill and low-skill professions and female- and male-dominated areas. In reality, it is of course more likely to find men employed in male-dominated sectors of the economy like finance, engineering, and construction, whereas women tend to work in female-dominated areas like teaching, nursing, and cleaning. If, for instance, traditionally male-dominated

occupations command higher public esteem than traditionally female areas and attract more votes because of that, the overall result could be worse for female candidates than suggested by the insignificant coefficient of *FEMALE* in table 7. Looking at scales like the *International Socio-Economic Index of Occupational Status* (ISEI) (Ganzeboom et al., 1992), this seems to be the case for the professions examined in this study.[53] On a scale that ranges from 16 for launders and cleaners to 90 for judges, the average ISEI score of the female-dominated professions in this study is 44, while the male-dominated professions have a significantly higher average of 51.[54]

On the other hand, the real effect could be better for women if controlling for every single profession in the regression specification (8) disguises a still existing real preference for female candidates. This could be the case if respondents replaced voting for female candidates directly by favoring female-dominated over male-dominated professions. If this was true, we would observe an advantage of female candidates in real elections, as the fraction of women is disproportionately high in traditionally female sectors. As such, it seems important to examine whether there are systematic differences in electability between sectors before concluding about the overall impact of stating profession on the electoral chances of female versus male candidates.

Column 1 in table 9 reports the results of regressing the probability of a candidate to get a vote on indicators for professions in either male- or female-dominated areas, leaving professions in the gender-neutral sector as benchmark (the professions continue to be classified as in table 1). The estimates show that working in a gender-neutral profession yields the best chances to get a vote on average, followed by professions in the traditionally female sector and, as last, by male-dominated professions (all differences are significant). This finding is surprising, as there are more male voters in the sample, and it suggests that female candidates may experience an electability advantage even when their professions are stated on the ballot.

In column 2 of table 9, I include the dummy for a female candidate and interact it with the gender-sector indicators to check whether voters react to any particular combination of gender and male- or female-dominated professions. This seems not to be the case, however, as none of the interaction terms has a significant coefficient. Column 3 introduces the distinction between high-skill and low-skill

[53] The ISEI measures both the amount of education that a certain occupation requires and the occupation's ability to turn that education into income. As such, it is also often used as a "prestige" scale.

[54] The ISEI values for professions in Spain are taken from Carabaña and Gomez (1996).

Table 9

The importance of similarity and qualification

	All "profession" versions				Low-skill voters	High-skill voters
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Same sex	0.021***		0.008	0.009	-0.003	0.017
	(0.008)		(0.008)	(0.008)	(0.016)	(0.011)
Same skill level	0.066***		0.006	0.006		
	(0.013)		(0.010)	(0.010)		
Same profession	0.503***		0.472***	0.476***	0.646***	0.430***
	(0.053)		(0.054)	(0.054)	(0.131)	(0.058)
Same view	0.101***		0.047***		0.01	0.084***
	(0.017)		(0.017)		(0.031)	(0.024)
Other view				-0.007		
				(0.016)		
High-skill profession		0.205***	0.190***	0.197***	0.203***	0.189***
		(0.010)	(0.011)	(0.011)	(0.020)	(0.013)
Name/Rank	0.663***	0.455***	0.414***	0.427***	0.449***	0.374***
	(0.036)	(0.027)	(0.036)	(0.035)	(0.061)	(0.045)
N	7920	7950	7920	7920	2220	4260
R^2	0.213	0.244	0.256	0.255	0.252	0.267
Ad. R^2	0.212	0.244	0.255	0.254	0.25	0.267

*, **, *** = significantly different from 0 on the 10%, 5%, or 1% level

Notes: (1) Clustered standard errors (on the voter level) are reported in parentheses. (2) "Same sex" coded as 1 if both voter and candidate belong to the same sex. (3) "Same skill" is equal to 1 if both voter and candidate work in a high-skill or low-skill profession. (4) "Same profession" is 1 if voter and candidate work in exactly the same profession. (5) "Same view" is based on the the real vote of the respondent and the perceived political position of individuals working in the various professions, based on a separate survey of 57 individuals in Barcelona. For voters of left-leaning parties, "same view" is equal to 1 if a candidate states to work in the following professions: caregiver, kindergarten teacher, teacher, nurse, waiter, and painter. The relevant professions for supporters of right-leaning parties are: tax collector, manager, lawyer, physician, architect, and engineer. (6) "Other view" is coded in the opposite way as "same view". (7) "High-skill profession" is 1 for all professions that typically require a university degree.

professions and interacts this dummy with the gender-sector indicators. Again, we see that candidates working in high-skill professions have a much greater chance to get a vote. This effect is apparently smaller for the male-dominated high-skill sector, however, suggesting that the greater likelihood to get a vote for female-dominated professions found in column 1 is caused in particular by a greater preference for female professions in the high-skill sector (psychologists, nurses, sociologists, historians, and

kindergarten teachers). Finally, combining all sector dummies with the indicator for female candidates in column 4 shows that there is on average no distinction in the way voters treat female and male candidates within the different sectors.

Summarizing, it can be said that introducing information on the candidates' profession on the ballot does not seem to weaken the electoral chances of women and thus should not change the gender composition of the elected parliament or council. Although I cannot find a direct preference for female candidates in the "profession" sample anymore, this is made up by a greater probability to vote for candidates working in typically female professions. As these are usually women, there should also be some advantage for female candidates in real elections with profession information.

3.6 Conclusions

This paper makes two main contributions: (1) It is the first to examine the causal relation between the amount and content of information provided on the ballot and voter satisfaction in low-information, open-list elections. For the empirical analysis, I use data from an experimental survey of voters at a local election in Barcelona, Spain, in which respondents were randomly allocated to different "treatments", i.e., hypothetical ballots with different amounts and contents of information about the candidates. Comparing reported satisfaction measures of the respondents across treatments suggests that stating the profession of the candidates on the ballot causes greater satisfaction with the selected candidates and the election system used, and leads to a majority of voters being in favor of open-list elections versus the closed-list system they had used in the real election. (2) I also show that providing profession information on the ballot significantly affects the voters' choice between the candidates of their preferred party and thus changes the composition of the elected body. Most importantly, voters seem to use profession information to identify candidates which they perceive as more "qualified" for the job. This is demonstrated by a large and significant increase in the probability to get a vote for candidates working in high-skill professions. Additionally, voters seem to take advantage of profession information by selecting either those candidates with whom they share the same occupation or those whose work in certain professions could be perceived as a signal of ideological proximity to the voter. The electability of female candidates, on the other hand, seems not to be affected by stating their occupations.

Overall, the inclusion of profession information on the ballot can thus be expected to increase the fraction of elected representatives from high-skill professions. A consequence of this change in the composition of the parliament could be a change in its preferences with respect to relevant policy areas. Examples are more favorable attitudes towards international economic integration (Mayda and Rodrik, 2005), immigration (Mayda, 2006; Hanson et al, 2007) and spending on public education (Bursztyn, 2011), as well as more liberal positions on certain cultural issues like birth control or gender equality (Felling and Peters, 1986; Wilcox, 1991).

Two aspects could limit the external validity of the results found in this paper, in particular with respect to the effect of profession information on voting behavior. In real elections, it is the parties who decide which candidates appear on their list and the candidates themselves have to declare their profession. As the importance of information cues and signals is most likely known to these actors (Reynolds and Steenbergen, 2006; Tessin, 2007; Vavreck, 2001), the typical party list may not present the balanced mix of candidates with different high- and low-skill professions that participants faced in my survey. On the contrary, parties can be expected to have a strong tendency to nominate candidates with high-skill professions to signal competence and professionalism. At the same time, candidates surely use their leeway in describing their profession to their advantage. A "cashier" in the local supermarket could thus truthfully declare to be an "employee", whereas an "employee" who has studied sociology would probably state her profession as "sociologist". Thus, the actions of parties and candidates may lead to ballots in which hardly any candidate with a low-skill occupation appears. This may significantly reduce the importance of profession information as a signal for qualification. Even in this case, however, profession information would still be useful for voters to select those candidates they perceive to be closer to their own political position.

Further research could therefore focus on how the relevant political actors (party selection committees and potential candidates) respond to the inclusion of profession on the ballot in open-list elections. Besides, as the statement of profession is sometimes accompanied by other information about the candidate like birth year or education, it would be interesting to know how its impact on voting behavior varies with different levels of pre-existing information. And finally, the power of profession information could also depend on the main political cleavage in the respective society. Thus, further studies could repeat the analysis presented in this paper in contexts where,

for instance, ethnicity or religious denomination are of major importance for the voters. If profession was found to significantly influence voters in such settings as well, it may even be helpful for the designers of electoral systems to prevent individuals from voting purely along the lines of the main social conflict.

Appendix to Chapter 3

Table C.1

Assignment of professions to the individual candidates over different ballot versions

	Ballot version					
Candidate	Prof 1	Prof 2	Prof 3	Prof 4	Prof 5	Prof 6
1	Engineer	Teacher	Nurse	Firefighter	Self-employed	Hairdresser
2	Caregiver	Information scientist	Lawyer	Sociologist	Plumber	Self-employed
3	Baker	Hairdresser	Engineer	Teacher	Historian	Plumber
4	Historian	Bus driver	Salesperson	Caregiver	Engineer	Teacher
5	Painter	Waiter	Medical secretary	Architect	Tax collector	Psychologist
6	Lawyer	Sociologist	Plumber	Self-employed	Laundress	Chemist
7	Launder	Chemist	Physician	Kindergarten teacher	Firefighter	Salesperson
8	Architect	Tax collector	Psychologist	Painter	Waitress	Medical secretary
9	Physician	Kindergarten teacher	Bus driver	Salesperson	Caregiver	Manager
10	Firefighter	Self-employed	Laundress	Manager	Teacher	Nurse
11	Psychologist	Firefighter	Baker	Medical secretary	Information scientist	Civil servant
12	Porter	Cleaner	Information scientist	Civil servant	Nurse	Firefighter
13	Self-employed	Launder	Chemist	Tax collector	Psychologist	Bus driver
14	Kindergarten teacher	Carpenter	Porter	Cleaner	Manager	Lawyer
15	Carpenter	Porter	Cleaner	Information scientist	Lawyer	Sociologist
16	Tax collector	Psychologist	Firefighter	Baker	Hairdresser	Information scientist
17	Information scientist	Lawyer	Sociologist	Carpenter	Porter	Cleaner
18	Cleaner	Architect	Tax collector	Psychologist	Bus driver	Baker
19	Teacher	Nurse	Carpenter	Porter	Cleaner	Architect
20	Bus driver	Baker	Caregiver	Chemist	Physician	Kindergarten teacher
21	Nurse	Plumber	Self-employed	Launder	Architect	Tax collector
22	Salesperson	Medical secretary	Architect	Physician	Kindergarten teacher	Carpenter

Table A.1 (continued)

23	Medical secretary	Manager	Civil servant	Historian	Painter	Waiter
24	Manager	Civil servant	Historian	Plumber	Salesperson	Laundress
25	Waiter	Caregiver	Manager	Lawyer	Sociologist	Painter
26	Sociologist	Painter	Waitress	Hairdresser	Chemist	Physician
27	Plumber	Salesperson	Hairdresser	Engineer	Civil servant	Historian
28	Civil servant	Historian	Painter	Waitress	Medical secretary	Engineer
29	Chemist	Physician	Kindergarten teacher	Bus driver	Baker	Caregiver
30	Hairdresser	Engineer	Teacher	Nurse	Carpenter	Porter

Note. Professions highlighted in grey are defined as "high-skill", that is, more than 70% of the workers in this area possess an academic degree.

Table C.2

The insignificance of voter- and interview-specific controls

Variable	Coefficient	Robust Std. Err.	t-value	p-value
Age 26-35	0.001	(0.016)	0.06	0.951
Age 36-45	0.002	(0.017)	0.1	0.92
Age 46-55	-0.004	(0.018)	-0.23	0.821
Age 56-65	-0.001	(0.018)	-0.04	0.969
Age 66 +	0.002	(0.020)	0.11	0.912
Female	0.002	(0.008)	0.21	0.835
Afternoon	0.000	(0.008)	0.06	0.954
Married	-0.003	(0.012)	-0.28	0.778
Separated	0.001	(0.015)	0.04	0.966
Location 2	0.000	(0.049)	0.0	0.997
Location 3	0.002	(0.049)	0.03	0.975
Interviewer 2	-0.006	(0.044)	-0.14	0.886
Interviewer 3	-0.005	(0.044)	-0.11	0.914
Interviewer 4	0.000	(0.044)	0.01	0.992
Interviewer 5	0.001	(0.044)	0.01	0.989
Interviewer 6	0.000	(0.028)	0.0	0.998
Interviewer 7	-0.003	(0.026)	-0.1	0.922
Constant	0.201	(0.025)	8.02	0.0
R^2	0.0001			
F(17, 10302)	0.050			
Prob > F	1.000			
N	10320			

Note. Dependent variable is the indicator for whether a candidate has obtained a vote from a voter. The comparison group is a male voter between 18 and 25 years old, single, and was interviewed in the morning at interview location 1 by interviewer 1.

Instrucciones:

Muchas gracias por participar en esta encuesta hecha por la

Universitat Pompeu Fabra, Barcelona.

La encuesta se divide en tres partes. Por favor, es importante que la conteste **en el orden dado**.

Toda la información recabada será tratada de manera confidencial y solamente usada para fines de investigación en el marco de este estudio.

Muchas gracias!

Si tiene alguna duda, por favor póngase en contacto con nosotros.

Parte 1: Preguntas sobre las Elecciones Locales 2011 de Barcelona

1. ¿Está satisfecho con la forma en que ha podido expresar sus propias preferencias políticas en estas elecciones?

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

1 2 3 4 5 6 7 8 9 10

Para nada contento Muy contento

2. ¿Qué lista ha votado usted? ☐

3. ¿Cuántos candidatos de esa lista conoce?

☐ Ninguno ☐ Uno ☐ Dos ☐ Tres y más

4. ¿Cree que los candidatos individuales de esta lista le representarán bien?

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

1 2 3 4 5 6 7 8 9 10

No Si, muy bien

Parte 2: Elecciones hipotéticas

En la lista de la página siguiente, puede encontrar los nombres de **30 candidatos hipotéticos** para un consejo de distrito local. Todos ellos pertenecen a **su partido preferido** (es decir, sólo se ve la lista del partido que usted suele elegir), pero no conoce a ningún candidato mejor que a los otros.

En esas elecciones, tiene un total de **6 votos** que puede distribuir libremente entre los 30 candidatos que aparecen abajo. **Usted elige un candidato marcando la casilla detrás de su nombre**.

Sólo un voto por cada candidato se le permite.

Si distribuye más de 6 votos, la papeleta entera será nula!

(Nota: Todo el proceso no debería tomar más tiempo de lo que suele necesitar para votar.)

Por favor, indique sus **6 opciones** marcando una **X** en la casilla detrás del nombre respectivo.

Lista 1	Mi Partido Favorito en Cataluña	
1	**Antonio Fernandez Poyato,** Bombero	
2	**Francisca Samblas Llorens,** Socióloga	
3	**Victor Ruiz de Diego,** Profesor de secundaria	
4	**Sara Suarez Solis ,** Cuidadora	
5	**Juan Alberto Martin Martin,** Arquitecto	
6	**Maria Cruz Carrio Boquera,** Autónomo	
7	**Oscar Bergasa Perdomo,** Maestro de párvulos	
8	**Adela Sanchez Bustos,** Pintora	
9	**Martin Guillermo Ramirez,** Vendedor	
10	**Maria Dolores Martinez Risquez,** Directivo	
11	**Pedro Molina Garcia,** Auxiliar de medico	
12	**Isabel Rodriguez Gonzalez,** Funcionaria	
13	**Carlos Vales Vazquez ,** Fiscal	
14	**Maria Concepcion Tolosa Minguez**, Señora de la limpieza	
15	**Fernando Puerto Fernandez,** Informático	
16	**Esther Balestra Martinez,** Panadera	
17	**Jose Luis Cortes Gomez,** Carpintero	
18	**Maria Nieves Casas Iglesias,** Psicóloga	
19	**Pedro Margalef Masia,** Ordenanza	
20	**Maria Luisa Aguin Martinez,** Química	
21	**Manuel Cabaleiro Fabeiro,** Lavandero	
22	**Margarita Romero Lorenzo,** Medica	
23	**Ramon Bouzas Gonzalez,** Historiador	
24	**Maria Laura Dieguez Rojo,** Fontanera	
25	**Alberto Alonso Pereira,** Abogado	
26	**Andrea Garcia Suarez,** Peluquera	
27	**Francisco Xabier Pena Diaz**, Ingeniero	
28	**Maria Teresa Navaza Gonzalez**, Camarera	
29	**Rafael Iglesias Blanco,** Conductor de autobuses	
30	**Irene Molleda Silva,** Enfermera	

Parte 3: Preguntas sobre las Elecciones Hipotéticas

5. ¿Está satisfecho con la forma en que ha podido expresar sus propias preferencias políticas en estas elecciones hipotéticas?

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

1 2 3 4 5 6 7 8 9 10

Para nada contento Muy contento

6. ¿Cree que los candidatos individuales de esta lista hipotética le representarían bien, si fueran elegidos de verdad?

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

1 2 3 4 5 6 7 8 9 10

No Si, muy bien

7. ¿Le hubiera gustado tener más información sobre los candidatos en esta lista?

☐ Si ☐ No

→ En el caso que si, ¿qué información? ☐

8. ¿Ha usado algún método para asignar los votos de los candidatos?

☐ Si ☐ No

→ En el caso que si, ¿cuál?

☐

9. ¿En cuál sistema de elecciones prefiere votar, el que ha utilizado realmente en las elecciones municipales o el de esa encuesta?

☐ Real ☐ Hipotético

10. ¿Cuál es su edad? (Indique el rango de edad correspondiente)

☐ 18 - 25 ☐ 26 - 35 ☐ 36 - 45 ☐ 46 - 55 ☐ 56 - 65 ☐ 65+

11. ¿Es usted…?

☐ Mujer ☐ Hombre

12. ¿Es usted…?

☐ Soltero ☐ Casado ☐ Separado ☐ Divorciado ☐ Viudo

13. ¿Tiene un grado académico?

☐ Si ☐ No

→ En el caso que si, ¿cuál? ☐

14. ¿Cuál es su profesión? ☐

15. Por favor, indique que piensa sobre las siguientes 4 profesiones.
¿Son principalmente para hombres, mujeres o los dos igualmente?

	Hombres	Mujeres	Ambos
Panadero	☐	☐	☐
Enfermero	☐	☐	☐
Abogado	☐	☐	☐
Arquitecto	☐	☐	☐
	Hombres	Mujeres	Ambos

16. Por último, ¿tiene algún comentario sobre el procedimiento de votación en esta encuesta, la forma en que votó, o el cuestionario?

☐

Muchas gracias por su colaboración!

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