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The Deterrent Effect of an Anti-Minaret Vote on Foreigners' Location Choices*

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Abstract

In a national ballot in 2009, Swiss citizens surprisingly approved an amendment to the Swiss constitution to ban the further construction of minarets. The ballot outcome manifested reservations and anti-immigrant attitudes in regions of Switzerland which had previously been hidden. We exploit this fact as a natural experiment to identify the causal effect of negative attitudes towards immigrants on foreigners' location choices and thus indirectly on their utility. Based on a regression discontinuity design with unknown discontinuity points and administrative data on the population of foreigners, we find that the probability of their moving to a municipality which unexpectedly expressed stronger reservations decreases initially by about 40 percent. The effect is accompanied by a drop of housing prices in these municipalities and levels off over a period of about 5 months. Moreover, foreigners in high-skill occupations react relatively more strongly highlighting a tension when countries try to attract well-educated professionals from abroad.

Keywords: attitudes, foreigners, location choice, popular initiative, regression discontinuity design

JEL classifications: D83, J61, R23, Z13

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1 Introduction

The question of whether people care about how they are perceived by others is important for understanding individual decision-making since many choices involve aspects of social interaction. People's attitudes towards one another are obviously central for bonding decisions, but they are also crucial in determining the choices individuals make when selecting an organization for employment or leisure. The issue might also arise when people decide about which community they wish to live in. In this paper, we focus on this relationship and ask to what extent the resident population's attitudes are relevant for such decisions. In particular, we analyze how foreigners react to unexpectedly revealed reservations towards their group studying their location choices.

Attitudes towards foreigners influence the interaction between immigrants and the native population in many ways. This matters for the successful realization of gains from trade between these two parties as well as for the immigrants' integration (see, e.g., Algan et al., 2012; Akay et al., 2017). Specifically, a host country's culture of welcome might affect where mobile foreigners are willing to locate, and is crucial for regions that need to attract experts from abroad. There is a large body of literature which discusses the sources and expression of negative attitudes towards foreigners as reflected, for example, in right-wing extremism (e.g., Hainmueller and Hopkins, 2014). However, foreigners' behavioral reactions to these attitudes and the consequences that they have for their welfare are less often addressed.[1] While economic models and analyses of foreigners' location choices offer an approach to learn about foreigners' valuations of the overall attractiveness of an environment, this field of research typically neglects the political attitudes of the resident population. Moreover, the attempt to analyze the relationship between the presence of foreigners and residents' attitudes towards them poses a severe methodological challenge, as it is necessary to overcome an inherent simultaneity (Dustmann and Preston, 2001). On the one hand, the presence of foreigners may affect natives' preferences and attitudes. Getting to know foreigners may reduce prejudices, as postulated by the so-called contact hypothesis, or the presence of foreigners may aggravate negative attitudes, as natives fear pressure on the labor market and welfare system, and expect increases in the crime rate or the alienation of their own culture. On the other hand, the attitudes of natives towards foreigners may affect the presence of immigrants. Foreigners might move less to areas where residents have reservations about them, where they fear discrimination or even physical abuse.

In this paper, we analyze the role of attitudes towards foreigners in their choice of residence based on a unique setting which allows us to address the identification problem

[1] A notable exemption is the study by Elsayed and De Grip (2018) who find that Islamist terrorist attacks worsen attitudes towards Muslims in the Netherlands and go hand in hand with an increase in Muslims' intention to re-migrate. Moreover, Muslims' attitudes towards integration are also negatively affected by the terrorist attacks. Both effects are relatively larger for the highly educated immigrants.

of simultaneity. As a moderator, we integrate identity utility, as introduced by Akerlof and Kranton (2000). It allows us to capture the idea that perceived negative attitudes towards foreigners affect their utility. In a national ballot 2009, Swiss citizens voted on whether the further construction of minarets should be prohibited (the so-called minaret initiative). Against the recommendation of the Swiss Federal Assembly and the predictions of leading opinion pollsters, the amendment to the Swiss constitution was accepted with a clear majority. The ballot outcome manifested reservations and anti-immigrant attitudes in regions of Switzerland which had previously been hidden. There were municipalities where voters unexpectedly deviated strongly from their past voting behavior on migration-related issues. We exploit this fact and study whether the inflow of foreigners to these particular municipalities declined in the aftermath of the vote, which would be consistent with lower expected utility and provide evidence that individuals react to perceived attitudes towards their social group.

The empirical analyses draw on administrative data for the universe of foreigners living in Switzerland which allow us to study their moving behavior before and after the national vote on the minaret initiative. We proceed in two steps. First, in a preliminary simple analysis, we estimate the change in the probability that some foreigner moves to a municipality that unexpectedly approved the minaret initiative in the months after the vote. The results suggest a reduction in this probability about three months after the vote has taken place. Second, in the main analysis, a more appropriate econometric approach is pursued flexibly taking canton-specific situations into account. Based on a simulation study, we expect that any effect materializes as a sharp decline in the likelihood that a foreigner chooses to move to a municipality that had unexpectedly revealed its reservations towards foreigners some time after the vote. Since the point in time at which the effect can be observed is not deterministically predictable, the change in residential relocation patterns is analyzed based on a regression discontinuity design (RDD) with unknown discontinuity points (Porter and Yu, 2015). We find such discontinuous jumps in the moving pattern of foreigners in 12 cantons. We observe that foreigners are deterred from locating in municipalities that unexpectedly revealed negative attitudes towards them ('switcher municipalities', for a definition see Section 2.2). The estimated effect is sizable. In cantons where reactions could be identified, the probability that a foreigner chooses one of these municipalities drops, on average, by approximately 4.9 percentage points within the first month relative to a pre-intervention level of 13 percent, and thus by about 40 percent. Our interpretation that switcher municipalities became less attractive due to the change in perceived political attitudes is consistent with reactions in the moving behavior to other types of municipalities. We observe that foreigners are more likely to move to municipalities that became relatively more open and are equally likely to move to municipalities whose relative position has not changed.

Several validation checks suggest that the assumptions underlying the RDD hold: Neither the number of foreigners moving nor the municipalities that unexpectedly reveal their reservations about immigrants are systematically different before and after the threshold dates. The effect holds to a similar extent for groups of foreigners other than Muslims. This suggests that foreigners identify with the affected minorities and that they perceived the support for the initiative as being an expression of negative attitudes towards immigrants in general. Moreover, we document that the overall effect is similar in size for individuals moving within a narrow radius, which is most probably not attributable to a job change. Still, the effect differs across specific groups of foreigners and over time. While it is substantial for first-generation foreigners, there is no discernible effect for second-generation foreigners. The latter are often well assimilated and seem to be less affected by the revealed attitudes. This finding emerges from exploring a potential alternative explanation, i.e. that landlords will dare to discriminate more against foreigners once they learn about the reservations of their fellow citizens. This supply-driven reaction, however, would to some extent affect foreigners in general. We further find that foreigners in high-skill occupations react more strongly than foreigners in low-skill occupations. While this finding is interesting in itself, it further suggests a primarily demand driven mechanism. Finally, the effect fades after about 5 months. This might be due to the declining salience of the issue as time elapses and/or due to adjustments in the housing market. In the latter case, lower rents compensate foreigners for the disutility of living in an environment with negative attitudes towards them. The results of a supplementary analysis indeed suggests a reduction in housing prices for switcher municipalities in the aftermath of the vote. Our data does not allow investigating potential reactions by the native population. Otherwise we could explore whether the identified reaction on the housing market is due to changes in citizens' location choices in response to the newly revealed attitudes. It is well conceivable that some Swiss also shun municipalities where they consider their fellow citizens to hold political preferences they could not align with. This suggests that voting outcomes could be considered an important source of information contributing to preference-based spatial sorting more broadly.

Our analysis contributes inter alia to the growing literature on the interdependent relationship between the presence of immigrants and attitudes towards them. Studies that investigate the effect that an inflow of foreigners has on the native population's attitudes hereby face a methodological challenge. There is the possibility of common unobserved drivers as well as potential reverse causality, which arises due to a possible effect from attitudes on foreigners location choices. In pioneering work, Dustmann and Preston (2001) analyze the effect of the local ethnic composition on natives' attitudes towards foreigners in the United Kingdom. They propose an IV approach by using the ethnic composition of large-scale areas as an instrument for the composition in local areas. In their study, simultaneity bias matters and correcting for it reveals that a higher share in the (ethnic)

minority population is associated with more hostile attitudes in the majority population. Several studies build on this idea to take potential endogeneity into account in other contexts (see, e.g., Dill, 2013; Kuhn and Brunner, 2018; Barone et al., 2016; Méndez Martínez and Cutillas, 2014; Halla et al., 2017; Gerdes and Wadensjö, 2008). While the potential relationship between attitudes and location choices is acknowledged, there is little direct evidence on whether foreign individuals' location choices indeed relate to perceived attitudes per se or whether the potential relationship is rather driven by resulting tangible (living) conditions.

To the best of our knowledge, there are five studies that are concerned with the question of whether minority groups' location choices react to attitudes or behavior towards them. An early analysis by Tolnay and Beck (1992) documents the relationship between racial violence in U.S. counties and the outmigration of the threatened group between 1910 and 1930. In related work, Henry (2009) finds that African Americans are less likely to move to places where they face a higher risk of becoming the victim of a hate crime. These interesting findings are primarily descriptive in nature and likely linked to tangible living conditions, i.e., the fear of physical violence. Waisman and Larsen (2016) investigate the quasi-random variation in the initial placement of asylum seekers in Sweden in conjunction with measures of attitudes based on survey data, which they use to approximate migrants' living conditions and potential discrimination. They find some evidence that refugees' subsequent location choices are related to natives' surveyed attitudes. Damm (2009) investigates regional factors that influence immigrants' location choices in Denmark. She also uses a quasi-random placement of asylum seekers and finds that the hazard to leave the municipality which he or she is assigned to increases with the share of votes for right-wing parties. The behavioral reaction is interpreted as welfare seeking behavior. The most recent study is by Gorinas and Pytliková (2017), who investigate whether surveyed attitudes towards immigrants have an impact on migration flows at an international level. They perform a cross-national study and find that the surveyed dismissive attitudes of natives are negatively correlated with migrant inflows.

While these five studies indicate that adverse conditions are related to minorities' location choices, perceived attitudes cannot be separated from, and likely capture, related tangible (living) conditions, they might possibly work indirectly, and/or their potential endogeneity is not accounted for. We pursue a novel strategy and measure attitudes towards immigrants directly based on voting behavior in ballots widely observed by the public. We thereby exploit a surprising revelation of negative attitudes to identify the causal effect of attitudes on location choices. This approach allows us to compare location choices within the same choice set of municipalities and the same population before and after the new information is available and thus enables us to capture the effect of perceived attitudes independently of tangible living conditions. Furthermore, our analysis is not confined to a specific group of foreigners, such as refugees, but applies to the large group of labor

migrants. Finally, our empirical study presents the first comprehensive application of a regression discontinuity design with unknown thresholds as recently proposed by Porter and Yu (2015).

The remainder of the paper is structured as follows. Section 2 provides background information on the institutional setting as well as on the informational conditions defining the municipalities that unexpectedly reveal a shift towards an anti-immigration position. Section 3 describes our reasoning on how perceived attitudes influence an individual's utility and how this affects his or her location choice. Based on these considerations, Section 4 outlines the empirical strategy. Our data sources are described in Section 5. The results are presented in Section 6, and validated and discussed in Section 7. Section 8 offers complementary evidence on the development of housing prices. Section 9 concludes.

2 Background and Informational Setting

Our study draws on information about voting behavior in national referendums that is used to infer residents' revealed attitudes towards foreigners and the changes in these publicly visible attitudes over time. In order to understand this specific informational setting, we first provide some background information about one specific proposition, the minaret initiative, and briefly introduce direct democratic decision-making in Switzerland. In a second step, we specify the circumstances which characterize voting behavior in the minaret initiative and explain why the vote outcome unexpectedly reveals reservations towards foreigners. Finally, we discuss how the ballot outcome entered foreigners' information base.

2.1 The vote on the minaret initiative

On November 29, 2009, a majority of Swiss voters unexpectedly approved an initiative that banned the further construction of minarets in Switzerland.[2]

[2]The Swiss direct democratic system, among other things, allows the electorate to propose amendments to the federal constitution by launching a so-called popular initiative. Initiators have to collect 100,000 valid signatures amounting to about two percent of the population of Swiss citizens. Once this hurdle is overcome, voters decide in a national referendum on whether the constitution should be changed. This is the case if the initiative is approved by the majority of voters overall as well as in a majority of cantons. If this holds, the federal government is obliged to implement the will of the majority. For an overview about the Swiss political system, see, for example, Linder (2010). Swiss citizens are accustomed to expressing their opinions at the poll (Stutzer et al., 2018). Direct democratic decision-making is very common in Switzerland, even at the national level. Citizens openly discuss the proposals and can rely on being provided with ample coverage of the issues in the media. Newspapers, for example, publish the statements and voting recommendations of political parties and opinion leaders. Frey (1994) analyses the role of public discussion in the process of direct democratic decision-making, with an emphasis on the Swiss experience.

In the aftermath of 9/11 with its increased fear of terrorism, a discussion arose about whether practicing Muslims in Switzerland would threaten the democratic order. In this anxious atmosphere, two Swiss center-right conservative parties, i.e. the Swiss People's Party and the Federal Democratic Union, started preparations for the minaret initiative against the further construction of minarets. At this point in time, there were only four minarets in the whole of Switzerland. For their construction a permit was necessary, requiring that the construction plans comply with the cantonal and communal rules. Due to these existing restrictions, it was not to be expected that many minarets would be built in Switzerland. It was also clear that the proposed amendment would not change anything in the religious practice of Swiss Muslims. Mosques were already present, and their right to exist was not affected by the initiative.

During the campaign and beyond, the initiative was widely discussed in the Swiss media and attracted global attention. The degree of public attention that this issue attracted can partly be ascribed to the campaign advertisement. One of the initiative's campaign posters showed a woman in a black niqab and the Swiss flag with looming minarets representing rockets (see Figure A.1 in Appendix A). The Federal Commission against Racism judged that this visual tactic would jeopardize public peace and therefore banned it from being displayed in some towns and cantons.[3] Overall, the campaign raised fundamental discussions about religious freedom, cultural diversity and tolerance in Switzerland and was very salient in the media. The discussion was not limited to Muslims in Switzerland, but also involved the issue of migration and the treatment of minorities in general (see, e.g., Ettinger and Imhof, 2014).

Most parties and the Swiss Federal Assembly recommended that the initiative be rejected. However, contrary to general expectations as well as the forecasts of leading opinion researchers that the initiative would not be passed, it was accepted with a clear majority of 57.5 percent of the votes. Many citizens were mobilized (53.3 percent) and expressed more than a signal against the spread of the Islam in Switzerland but a general discomfort in the face of societal change and immigration experienced as a threat to Swiss cultural identity (see, e.g., Freitag and Rapp 2013 or Wehrli 2009). From the perspective of many foreigners, the vote outcome was perceived as a disturbing sign that provoked a general sense of exclusion (see, e.g., the individual reactions reported in Thiriet 2009). The decision came into effect at the national level and no minarets have been built since. In the exercise of their religion, however, nothing has changed for Muslims in Switzerland.

In the aftermath of the unexpected approval, there was substantial post-election news coverage. On all the Swiss media channels, the unreckoned outcome was discussed and the municipal voting patterns made prominent news. Most Swiss newspapers presented maps indicating which municipalities of a canton supported or rejected the initiative. Some-

[3]The campaign poster was, for example, forbidden in Basel, Freiburg, Lausanne, Morges, Neuenburg, Nyon, and Yverdon.

times, even the vote district results were discussed and displayed.[4] For Swiss citizens and foreigners alike, it was easy to learn about attitudes across municipalities and to spread the word about the municipalities that were expressing more pronounced reservations towards foreigners.

2.2 Attitudes towards foreigners across municipalities

The acceptance of the initiative did not only come as a surprise, but the voting pattern across municipalities did not match the rather stable political spectrum of views on migration manifested in prior votes. There were municipalities that unexpectedly revealed anti-immigrant attitudes, which external observers had not previously perceived. We thus see the event as an exogenous shock in *perceived* attitudes towards foreigners in some municipalities.

The vote on the minaret initiative provides a particularly attractive setting to capture information about citizens' attitudes. It can be conceptualized as a low-cost decision involving virtually no instrumental considerations about economic consequences, but rather allowing fully expressive voting behavior. Previous votes, such as the initiative "for the regulation of immigration" held on September 24, 2000, provoked serious concerns about negative economic repercussions in the case of their approval. The voting behavior on the minaret initiative thus revealed rather unambiguous signals about attitudes towards foreigners and allows us to measure potential changes in the publicly visible positioning of municipalities, i.e., changes relative to the best approximation as derived from previous voting behavior. Importantly, access to representative information about regional attitudes is just as accessible to us as researchers as it is to the people observed in our study. We consider the changes in perceived attitudes as exogenous shock to individuals' information base. The outcome is not anticipated and reveals new information. Information, which foreigners can incorporate in their perception about the political orientation of people across municipalities.

In order to use the exogenous shock to measure the effect of perceived attitudes on foreigners' location choices, we identify the municipalities that revealed previously unknown preferences based on a comparison with relevant votes in the past. In particular, we rely on four votes that occurred within a time frame close to the minaret initiative and which are sufficiently contextually related to it, i.e., in that they also involve the expression of attitudes towards foreigners. We code the voting results such that a higher vote share reflects more restrictive attitudes towards migration and foreigners.[5]

[4] Figures A.2 and A.3 in Appendix A provide examples of maps displaying the outcome where approval is marked green and rejection is marked red.

[5] These votes include (i) the initiative "For the regulation of immigration" ("Initiative für eine Regelung der Zuwanderung") held on September 24, 2000, (ii) the initiative "Against the abuse of the asylum law" ("Initiative gegen Asylrechtsmissbrauch") held on November 24, 2002, (iii) the referendum on "The federal

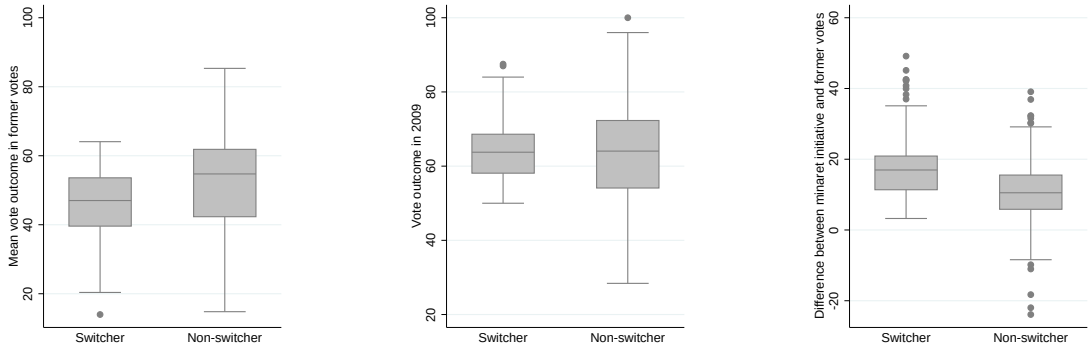


Figure 1 Voting outcomes in switcher and non-switcher municipalities

Specifically, a municipality is characterized as having unexpectedly revealed negative attitudes towards foreigners if the following criteria are fulfilled:

- The average support for restrictive migration policies in the past is below the mean for the canton they belong to.
- The swing to a more restrictive position, as expressed in the vote on the minaret initiative, is larger than the mean change across all the municipalities within the canton.
- The support for the minaret initiative exceeds 50 percent of the municipality's active electorate.

We label these municipalities with the generic term "switcher municipalities".

The first criterion is motivated by the idea that the respective municipalities were perceived as being relatively tolerant up to the vote on the minaret initiative. The second criterion ensures that only municipalities which experienced a large shift are considered to be switchers, given that nearly all the municipalities shifted to the right. The third criterion guarantees that the municipality voted in favor of the minaret initiative thus providing a signal that the majority agrees with the proposition. The latter point is important for the media coverage (see below). Based on these three criteria, we classify approximately 24 percent of all municipalities in Switzerland as switchers. The graphs in Figure 1 show how the voting results are distributed when comparing switcher and non-switcher municipalities. First, citizens in switcher municipalities voted less restrictive on migration issues in former votes (used to calculate the ex ante perceived level of critical

decree regarding the regular naturalization and the easier naturalization of young, and second-generation foreigners" ("Referendum zum Bundesbeschluss über die ordentliche Einbürgerung sowie über die erleichterte Einbürgerung junger Ausländerinnen und Ausländer der zweiten Generation") held on September 26, 2004, and (iv) the referendum on "The federal decree about the acquisition of citizenship rights by third-generation foreigners" ("Referendum zum Bundesbeschluss über die ordentliche Einbürgerung sowie über erleichterte Einbürgerung junger Ausländerinnen und Ausländer der dritten Generation") also held on September 26, 2004.

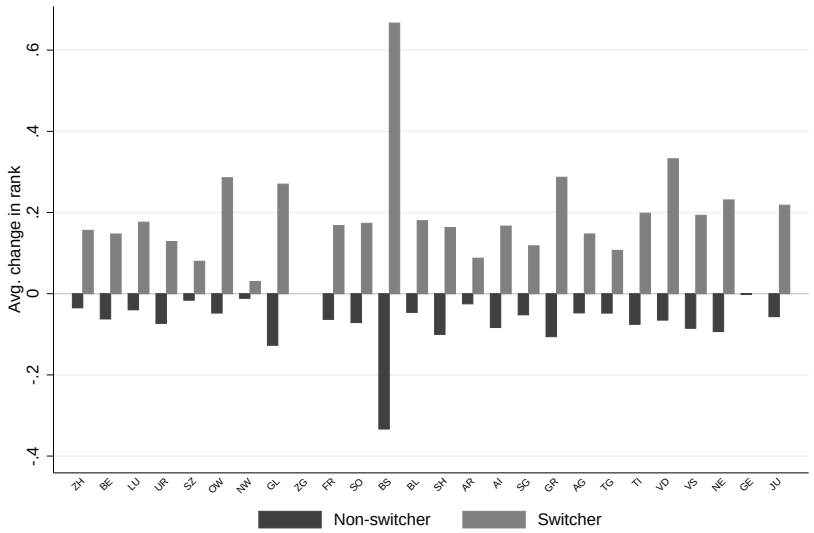


Figure 2 Rank change of switcher and non-switcher municipalities. A positive rank change indicates that the involved municipalities voted more restrictively on migration in the minaret initiative than in previous votes (compared to the other municipalities in a cantonal ranking). The rank change is measured as a percentage of the number of municipalities within the canton.

attitudes towards foreigners). Second, however, in the minaret initiative municipalities from the two groups cast similarly restrictive votes, on average. Third, when focusing on the change in the support of a restrictive position towards migration, a larger difference is observed for switcher municipalities than for non-switcher municipalities. Descriptive statistics on the criteria are reported in Table E.2 in Appendix E.1.

As a generally large fraction of the voting population approved a restrictive position in the minaret initiative, it could well be that the relative positioning of the municipalities in terms of expressed reservations has not changed at all. Accordingly, we rank municipalities within their cantons, once with regard to their average yes vote share in the four migration-related propositions that took place before the minaret initiative, and once on the basis of the vote outcomes in the minaret initiative. Figure 2 shows the average relative rank changes for switcher and non-switcher municipalities in all Swiss cantons. A positive change indicates that the municipality increased its rank by expressing a relatively more restrictive position towards immigration. Overall, a clear picture emerges that switcher municipalities gained in rank and non-switcher municipalities lost in rank. The relative strength of expressed reservations towards foreigners has thus changed between switcher and non-switcher municipalities. Two exceptions being, firstly the canton of Geneva, where there are no switching municipalities as no municipality accepted the initiative and the canton of Zug where the average rank changes are zero and no switcher municipality changed its relative position.

3 Theory

In this section, we derive our main hypothesis of how newly revealed perceived attitudes could affect individuals' location choices and under which conditions such reactions can be ascribed to perceived attitudes per se. We continue by our reasoning of how such reactions could materialize and be identified in observational data of individuals moving behavior.

3.1 Location choice including identity utility

We assume that the economy is in equilibrium and that individuals take municipality characteristics as given when choosing their location. We explicitly focus on the location choice and take the decision to move as given. Thus, we think about the choices of individuals once they have decided that they will move and face a continuum of municipalities and their characteristics in their choice set. We argue that their decision to move is much less affected by the new information about attitudes towards foreigners than their decision about where to move. First, the decision to move is much more costly than a change in the decision about where to move. Second, individuals residing in municipalities which were disclosed to hold negative attitudes towards foreigners might have already been aware of their neighbors' attitudes and would know that living conditions have not changed. As we argue that the surprising vote outcome is primarily a revelation of attitudes, rather than a change in attitudes.[6]

The utility U_{ijt} of an individual i $(i = 1, ..., n)$ living in municipality j $(j = 1, .., J)$ at time t depends on several municipality characteristics C_{jt} and individual characteristics Id_{it}. Municipality characteristics that influence individual utility are, e.g., taxes, housing prices, the provision of public goods, but also natural beauty or social ties. Individual characteristics involve an individual's income, the preference for housing, and other individual characteristics such as family status.

If people have a sense of self and care about how they perceive themselves as human beings and how they are perceived by others, this sense (or identity utility) should be an additional factor entering the expected utility of living in a particular municipality (see, e.g., the model on identity by Akerlof and Kranton, 2000). This proposition would be in line with research in social psychology. There, it is well documented, that perceiving even slightly repellent behavior of others can produce psychological and physical distress. Investigations on ostracism show that individuals strongly react to perceived exclusion, as it threatens the fundamental needs of belonging and increases negative affect.[7] In

[6] We test this supposition in a supplementary empirical analysis (see Section 7.3) and do not find systematic reactions in the decisions to move.

[7] For example, regions of the brain that are important for pain detection are activated. Moreover, individuals are highly sensitive in detecting situations of exclusion (see, e.g., Williams, 2009; Williams

a recent survey study, Rudert et al. (2017), for example, investigate whether foreigners feel ostracized after a popular vote on migration in Switzerland in 2014. They find that foreigners' perceptions of their environment reflect the vote outcome and that immigrants' need for belonging is less satisfied where citizens voted more restrictively. This holds even though the investigated group would not have been materially affected by the resulting legislation.

The behavior or perceived attitudes of others which, in our case, refer to the revealed attitudes of natives can differ across municipalities j and in time t. We assume that individuals update their information about the set of choices at the level of municipalities when they move. This includes their perceptions of attitudes towards their social class based on the past voting record of the municipalities.

3.2 Changes in perceived attitudes and moving decisions

Revealed attitudes towards foreigners in a municipality may change over time and thus also individuals' optimal location choices. Individuals choose their location such that their utility is maximized within their choice set. Thus, individual i will choose to locate in municipality j if, and only if,

$$U_{ijt} > U_{ikt} \text{ for all } k \neq j,$$

the utility to live there is higher than in all other municipalities in the choice set (see, e.g., McFadden, 1974).

The location choice of individuals is in equilibrium before t and new information about attitudes across municipalities is revealed between t and $t+1$, leading individuals to update their utility expectations. In our specific setting, the newly revealed information is the voting outcome on the minaret initiative in Switzerland. This new information can be seen as an exogenous change in perceived attitudes, as it was unexpected and does not have direct implications for the other municipality characteristics. Assuming that the utility is decreasing in reservations or negative attitudes, this revelation should lead to a lower probability to choose a municipality revealing more negative attitudes than has been expected. However, it has to be considered that this is only true if the relative positioning of municipalities with respect to this characteristic also changes. If all municipalities in an individuals' choice set were to express stronger reservations and the relative positioning stayed the same, we would not expect any change in observed behavior. Our definition of switcher municipalities above does take this reasoning into account. We would thus expect that individuals shun to move to municipalities which expressed stronger reservations than expected, and changed their positioning relative to the other municipalities in the choice set, after the vote has taken place.

and Nida, 2011). Related evidence based on panel data in Germany shows that higher vote shares of right-wing parties are associated with lower reported life satisfaction for immigrants (Knabe et al., 2013).

Whether somebody is affected by a change in attitudes towards foreigners depends on the extent to which he or she identifies with this social group or cares about political attitudes in this dimension. Residents with a foreign passport might identify strongest with the social group of foreigners due to their ancestry, ethnicity or prior experiences. As an individual to some extent chooses his or her identity, it is also possible that an individual's self-ascribed social category reacts to the new information. This idea is grounded in the *rejection-identification model* in social psychology (Tajfel, 1978; Branscombe et al., 1999). This theory asserts that when a minority group member perceives prejudice against his or her group, that member will develop a stronger identification with the group in order to reduce the psychological cost of perceived prejudice. Moreover, group identities may develop as a result of perceived prejudice, i.e., minority groups tend to identify with other minority groups under threat (Schmitt et al., 2003; Wesselmann et al., 2009).

Building on these considerations from economics and social psychology, we hypothesize that a negative change in perceived attitudes leads to a lower expected utility of residing in these municipalities, and thus to a lower probability that foreign individuals move to municipalities surprisingly revealing negative attitudes. In our context, the minaret initiative, we would expect not only Muslims but also other groups of foreign residents to experience ostracism, as their identity as foreigners in Switzerland becomes salient. Specifically, we would also expect EU citizens to be negatively affected by the initiative, if the identity as a foreigner is the moderating factor. This hypothesis would in principle also hold for Swiss citizens who care about political attitudes of fellow residents in this dimension.

If an individual who would have chosen j in t, chooses a different municipality k in $t+1$, in t the expected utility to live in j is higher than in k, and the reverse in $t+1$. This might be the case either because the utility to live in j has fallen from t to $t+1$, or the utility to live in k has risen; or both.

There are two crucial conditions that need to hold in order to ascribe a potential change in the location choice to the change in perceived local attitudes towards the social group.

I. Municipality characteristics have not changed systematically between t and $t+1$ ($C_{jt} = C_{jt+1} = C_j$).

II. Moving individuals are, on average, the same in t and $t+1$. Thus, individual-specific characteristics have not changed systematically between t and $t+1$ ($Id_{it} = Id_{it+1} = Id_i$).

Given these conditions hold, the component in the utility function that could have changed the relative attractiveness of municipality j are perceived attitudes towards individuals' social categories, potentially through identity utility.[8]

3.3 Identification of reactions to perceived attitudes based on location decisions

Given the conditions mentioned before hold, it should be possible to identify the effect of the signal on the location choice by comparing the location decisions of foreigners who choose their residency before and after the vote has taken place. Thus, before the information was revealed. Individuals decide to move at some point in time. Whether they find a new home on a specific day, say the day before or after the vote has taken place, can be considered as random with respect to a specific voting date. While an individual who finds a new apartment the day before the vote cannot incorporate the information on the outcome that will be revealed the next day in his or her choice, an individual who finds an apartment the day after can utilize the information on the outcome. Accordingly, individuals who decide to move and choose an apartment narrowly before the voting day can be used as a control group for those who make this decision shortly after the vote has taken place (Thistlethwaite and Campbell, 1960). If revealed preferences of the voting population about their attitudes towards foreigners play no role for the relative attractiveness of municipalities, foreigners' location decisions should, on average, be the same before and after the vote. However, if the mover is affected by perceived attitudes towards his or her group, we should observe a clear change in a mover's location decision.[9]

In order to apply this theoretical identification strategy, we would like to compare the location choices of people who decided where to move before the new information became public with those who decided afterwards. In particular, we would like to exploit the presence of switcher municipalities (described in Section 2), for which we argue that new information was unexpectedly revealed and which changed their relative positioning with respect to the openness towards foreigners. We thus aim to compare the probability that a foreigner chooses to move to one of the identified switcher municipalities before receiving the new information and after receiving it. If there was no lag between the time of the decision to move and the observed moving date, any effect should materialize right away,

[8]Please note that the two conditions are close to the identifying assumptions in a RDD context. Condition I is close to the the *conditional independence assumption*, and condition II is close to the *local randomization assumption*.

[9]Our identification strategy deviates from related strategies based on close election outcomes (see, e.g., Folke, 2014). In particular, we are not studying the consequences of the vote outcome on the people who voted as it is in some form usually the case in studies using election results. Instead, we study the consequences of the newly revealed information for a group of people, i.e. the foreigners outside of the municipality, who are not themselves involved in generating the vote outcome. These foreigners face the same (or a more) limited set of information as we the statisticians. Given the publicly available information, the deviations from the past voting records were news.

and the test could be approached using a regression discontinuity design, i.e., exploiting the local randomization in the time of the moving decision around the vote date. However, in observational data where only the moving and not the decision date is observed and where there is a lag, it is not evident which date separates the two groups. Shortly after the vote on the minaret initiative, many observed movers must have made their location choice before the vote took place and who were therefore unable to incorporate the new information. The longer the time is that elapses after the polling day, the larger will be the number of movers who have made their location choice based on the updated information set. The two groups might therefore overlap for some time until only movers who could incorporate the new information are observed. The corresponding time lag relative to the polling day might vary by region, as it might be affected by the length of notice periods and the situation in the housing market. In the next subsection, we provide a framework to analyze how the new information is incorporated into individuals' observed location choices. In Section 4, we translate the insights into our main empirical strategy.

3.4 Incorporation of new information in observed location choices

The location choice equilibrium for foreigners materializes in a stable aggregate probability of choosing a particular municipality type. This holds over time and refers in our context to the switcher and non-switcher municipalities. For the individual location decision, we as statisticians, however, typically only observe the moving date, but not the decision date. Accordingly, "observed" individual location choices might be based on different information sets. There is a timespan after the vote, during which we observe a mixture of those people who made their location choice before the information became public and those who decided subsequently. Any effect of the change in perceived attitudes materializes to the extent that the search generation that could incorporate the new information outweighs the prior search generations. This transition between the search cohorts could be smooth or sharp.

To gain an idea of how the effect could materialize, we perform a simulation analysis of individual location choices.[10] We simulate a simple location search with overlapping search generations. Two institutional scenarios are considered, and a negative treatment effect on the probability of moving to a switcher municipality is assumed. First, only search frictions exist, and people move once they have found an apartment. Second, a notice period is taken into account. It is revealed that in the second setting, the new information is incorporated later on in the aggregate outcome, i.e. in the inflow of individuals to the switcher municipalities, than in the first setting. Moreover, the adjustment pattern also reveals a much steeper drop in the probability of moving to a switcher municipality in

[10] Appendix B presents the details of the analysis.

the scenario with a notice period than in the scenario without one. With search frictions alone, in most cases, a rather gradual adjustment to the new equilibrium occurs.

Based on the assumption that it is random when an individual chooses to move and finds an apartment and that we know at which date the one group outweighs the other, a regression discontinuity framework seems suitable to analyze the incorporation of the new information. However, the threshold value to separate treated from control individuals is not as evident in the situation where the housing market and institutions affect the lag between the decision to move and the observed move. We apply the method of a regression discontinuity design with unknown discontinuity points (as introduced in the following Section 4) to identify the threshold and to measure the drop. The simulation clearly indicates that in an environment with legal restrictions that delay moving - such as a notice period - the method can separate the groups and find the simulated discontinuity. The discontinuity materializes at the point in time when the search cohort that could incorporate the new information starts to outweigh the cohort that could not incorporate the new information. This is not always the case in the scenario with search frictions alone. Given the rental agreements in Switzerland that stipulate mandatory as well as discretionary notice periods, the effect of a change in attitudes towards foreigners should be identifiable in observed moving dates.

4 Empirical strategy: RDD with unknown discontinuity points

Based on the theoretical considerations, we derived predictions on how individual location choices in response to new information translate into changes in aggregate migration flows between municipalities. In an environment with institutional restrictions, any displacement effect, is expected to materialize as a jump (or discontinuity) in the probability that a foreigner will choose to locate in a switcher municipality some time after the vote has taken place. In principle this estimation problem shares many features of a classical regression discontinuity design (RDD). However, in most RDD applications the discontinuity point is known, since it is, for instance, determined by the institution being analyzed. In our application though, this is not the case. The discontinuity point is the result of the interaction between the state of the housing market and institutional conditions such as the notice period. As a consequence, it is not deterministically predictable a priori when the discontinuity is expected to occur.

Porter and Yu (2015) have recently proposed an approach that is particularly well suited for the problem at hand. They develop a nonparametric procedure to perform regression

discontinuity analyses when discontinuity points are unknown, but predicted by theory.[11] The procedure starts with a specification test. It tests the null hypothesis of no treatment effect, or that of no discontinuity in the relationship between a dependent variable y and a running variable x. Subsequently, the discontinuity point or threshold is estimated, given that the specification test rejects the null hypothesis. Finally, a standard RDD analysis is performed. As the authors find that the estimation of the threshold value does not affect the efficiency of the estimator of the discontinuity, the threshold value can be treated as known when estimating the RDD. The proposed approach, in principle, allows us to separate the treated and the untreated individuals in settings where a RDD would be appropriate; however the threshold is unknown.

In classical RDD studies, the forcing variable, also referred to as the running variable, is used to determine the treatment status. When the value of this variable exceeds a certain threshold, observations are treated, but not so below this value. If individuals have no perfect control over the value of the forcing variable, the random variation narrowly around the threshold can be exploited to estimate the local average treatment effect (LATE) at this particular point (see, e.g., Hahn et al., 2001, Imbens and Lemieux, 2008, and Lee and Lemieux, 2010 for reviews of the RDD). The crucial assumptions for the validity of a sharp RDD are the *local randomization* and the *continuity of conditional expectations*, which provides that, without the treatment, both groups are alike at the threshold. The latter assumption makes sure that in a world without the treatment or in a world where everyone is treated, there would be no discontinuity in the conditional expectations at the threshold, i.e., that any effect can be attributed to the treatment.

In order to determine the point at which we can potentially separate the treatment and control group in our setting, we follow the approach proposed by Porter and Yu (2015) and apply the RDD with unknown discontinuity points. While this section concentrates on conveying the basic idea behind the procedure in a sharp RDD, we refer the interested reader to Section C in the Appendix for more details about the implementation of the test statistics and the proceeding. As we are, to our knowledge, the first to comprehensively apply the approach, we lay out how applied econometricians can implement the proposed procedure.[12]

The starting point of the procedure is to test whether there is a discontinuity in the relationship between the outcome variable and the forcing variable. In our application this is testing for a discontinuity in the relationship of the probability to move to a switcher

[11] Card et al. (2008) are - to our knowledge - the first who try to estimate an unknown threshold value in order to estimate effects at this point. In their application, they are interested in the presence of a minority population in a municipality and the share after which the majority starts to leave the municipality; i.e., a tipping point based on the Schelling model about dynamics in segregation (Schelling, 1971).

[12] A discussion of the fuzzy case and an in-depth discussion of the theoretical derivations and the method are presented in the original paper (Porter and Yu, 2015).

municipality and the moving date. We test

$$H_0^{(2)} : \text{no effects \& selection only}$$
$$H_1^{(2)} : \text{treatment effect only \& both selection and treatment effect.} \quad (1)$$

If $H_0^{(2)}$ cannot be rejected, we abstain from further analysis as we are interested exclusively in cases where there is a treatment effect. According to the approach by Porter and Yu (2015), it can be assumed that there is a discontinuity in the relationship if $H_0^{(2)}$ is rejected by the specification test. As the convergence rate of the derived test statistic (T_n) to a standard normal is slow, they propose approximating the finite-sample distribution by using the wild bootstrap of Wu (1986) and Liu (1988). The B bootstrap samples are generated by imposing the null hypothesis; i.e., such that they will mimic the null distribution of T_n. The null can be rejected if the value of the test statistic T_n exceeds the upper α-percentile of the resulting empirical distribution in the bootstrap sample $T^*_{n(\alpha B)}$ ($T_n > T^*_{n(\alpha B)}$).

If $H_0^{(2)}$, which implies that there is no discontinuity in the relationship, can be rejected by the specification test, the threshold value c and the discontinuity size τ_c (the LATE) can be estimated. The discontinuity point c is thereby determined by

$$\hat{c} = \arg\max_{c \in \Pi} \hat{\tau}^2(c),$$

and thus by checking whether $c = x_i$ ($x_i \in \Pi$) maximizes $\hat{\tau}^2(c)$, where Π is the search interval of the assignment variable.

The estimate of $\hat{\tau}(c)$, i.e., the non-parametric estimate of the discontinuity in the relationship between y and x at c, is estimated as in the classical RDD literature. Importantly, Porter and Yu (2015) show that $\hat{\tau}_c$ is a natural by-product of the estimation of the discontinuity point, and that c can be treated as if known when estimating treatment effects in RDDs. It is therefore not necessary to correct the inference for the fact that the threshold is an estimate itself, as the asymptotic distribution of $\hat{\tau}(\hat{c})$ is the same as if c was known. Thus, after the estimation of c, one can perform a standard RDD analysis.

For the point estimates in our analysis, we follow, e.g., Hahn et al. (2001), Porter (2003) and Imbens and Lemieux (2008) and estimate the limits by a local linear regression (LLR) of the form

$$y_i = \alpha + \beta(moving\_date_i - \hat{c}) + \epsilon_i.$$

using a uniform kernel, as in the specification testing.

A peculiarity of using moving dates as assignment variable is that it is necessary to control for periodical patterns before running the analysis (Davis, 2008; Paola et al., 2012). We take these potential periodical patterns in moving dates into account by controlling for

a battery of indicator variables, i.e. month, day of the month, and the weekday of the movement fixed effects. As often done in RDD studies, we first run a linear regression of the dependent variable on covariates. Subsequently, the residual of this regression is used as the dependent variable in our analysis (see, e.g., Lee and Lemieux, 2010). For notational convenience, we will nonetheless refer to our dependent variable as y_i or the probability that an individual moves to a switcher municipality.[13] For graphical representation, we plot a local linear regression of y over x using a rectangular kernel to gain an impression of the basic relationship and we present a series of validation checks for the identifying assumptions after presenting the results in Section 7.

5 Data

Our main empirical application draws on administrative data on the whole foreign population in Switzerland for the years between 2006 and 2011, which was kindly provided by the Swiss Federal Statistical Office (BFS) in cooperation with the State Secretariat for Migration (SEM). We thus rely on high quality data when it comes to individuals' moving dates.[14] We restrict our estimation sample to be the universe of all foreigners in Switzerland holding either the residence permit B, for temporary residency, or C, for permanent residency. We thus exclude foreigners with other types of permits such as asylum seekers, who are not allowed to choose their location freely. We exclusively use data on individuals moving within Switzerland, and thus exclude those just immigrating or leaving the country. Moving individuals are assigned to their destination canton. In total, this sample includes roughly 7,854,025 observations and 559,951 incidences of foreigners moving.

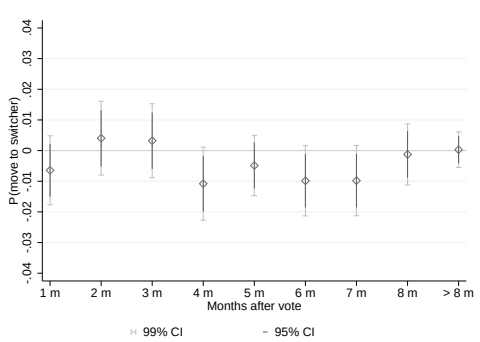
Further, we use data on municipality characteristics and voting outcomes. This data is freely available on the website of the Swiss Federal Statistical Office. Data on the municipality tax rates has been kindly provided by Parchet (2014).

6 Results

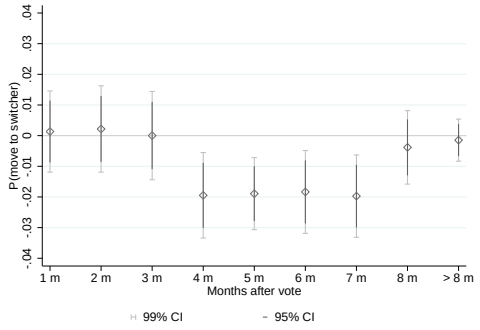
The results of our empirical analyses are presented in several steps. In a first step, we follow a simple approach using a linear probability model to test whether the location choices of individuals change systematically after the vote. We then proceed by presenting our

[13] Appendix C offers additional details on the discontinuity estimation in the context of our analysis.

[14] When moving within Switzerland foreigners have to register in the new municipality of residence, as well as to de-register in the former municipality of residence within 14 days. In addition, they have to provide a copy of the rental contract. This data is then systematically collected by the BFS. As this registration is an obligation, i.e., a deviation is an offense, and the period is rather close to the moving date, the registered moving dates should provide us with an accurate representation of the actual individual moving behavior.



(a) Effect on the probability of moving to a switcher municipality for all cantons



(b) Effect on the probability of moving to a switcher municipality for cantons with a discontinuity

Figure 3 These Figures visualize the estimates presented in Table 1. Row (1) is shown on the left hand side and row (2) on the right hand side.

main results of the proposed regression discontinuity approach, i.e., pooled estimates of those cantons for which a discontinuity is detected. In the following discussion section, we first validate that the change in the relative political position of switcher municipalities is relevant. Second, we test alternative supply-side explanations of the general phenomenon; i.e., landlords' discriminatory behavior and labor market discrimination. Next, we report the results of the validation of the identifying assumptions of the RDD and perform a placebo test. Finally, we present some complementary evidence on the development of housing prices in switcher municipalities after the minaret vote.

The empirical analysis draws initially on data for 22 Swiss cantons.[15] We abstain from testing for a treatment effect in four cantons, namely the cantons of Uri, Obwalden, Appenzell-Innerrhoden and Geneva. The former three cantons are rather small and, although we draw on register data, there are too few observations to perform the analysis. Furthermore, we exclude the canton of Geneva, since no municipality in this canton accepted the minaret initiative, and thus there is no switcher municipality.

6.1 Results of a simple approach

In a preliminary analysis, we abstract from the reasoning about the time when the new information about attitudes is incorporated and reflected in moving dates and location choices, and simply study the probability of moving to a switcher municipality after the vote on the minaret initiative and test whether this probability declined in the months after the vote. We fit a linear probability model (OLS) to the observation that someone

[15] Our empirical strategy draws on the following cantons: Zürich (ZH), Bern (BE), Luzern (LU), Schwyz (SZ), Nidwalden (NW), Glarus (GL), Zug (ZG), Fribourg (FR), Solothurn (SO), Basel-Stadt (BS), Basel-Landschaft (BL), Schaffhausen (SH), Appenzell-Ausserrhoden (AR), St. Gallen (SG), Graubünden (GR), Aargau (AG), Thurgau (TG), Ticino (TI), Vaud (VD), Valais (VS), Neuchâtel (NE), and Jura (JU).

Table 1 Probability of moving to switcher municipalities after the vote

Dependent variable: Moving to a switcher municipality

Sample	$post_{30}$	$post_{60}$	$post_{90}$	$post_{120}$	$post_{150}$	$post_{180}$	$post_{210}$	$post_{240}$	$post_{>240}$
(1) Overall	–0.006	0.004	0.003	–0.011**	–0.005	–0.010**	–0.010**	–0.001	0.000
	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.002)
No. of obs.	156,958								
R^2	0.0001								
F	2.13								
(2) Cantons with	0.001	0.002	0.000	–0.019***	–0.019***	–0.018***	–0.020***	–0.004	–0.001
discontinuity	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)
No. of obs.	103,146								
R^2	0.0005								
F	5.62								
(3) Cantons without	–0.022***	0.008	0.009	0.007	0.021***	0.007	0.010	0.004	0.004
discontinuity	(0.008)	(0.009)	(0.008)	(0.009)	(0.007)	(0.008)	(0.008)	(0.007)	(0.004)
No. of obs.	53,812								
R^2	0.0004								
F	2.44								

Notes: Ordinary least squares estimates of the temporal development of the probability of moving to a switcher municipality for the sample of all foreigners in the pooled sample of cantons. The $post_{\cdot}$ capture the coefficients for deviations from the level before the vote for intervals of 30 days (and 120 days for the last indicator). Standard errors in parentheses. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

chooses to locate in a switcher municipality for individuals moving in the two years around the vote. As any effect might be observable for a limited amount of time only, for example, due to reactions on the housing market, we choose a flexible specification. Specifically, we estimate a specification including eight monthly indicators $post_{\cdot}$ for up to 240 days after the vote. They show how the moving pattern evolves during the eight months after the vote. We further include one indicator for the period between 240 days up to one year after the vote. The coefficient of the latter variable $post_{>240}$ shows whether the probability to move to a switcher municipality is lower after the vote in the medium-term, i.e., after eight months. As mentioned before, we correct for possible periodical patterns in the moving behavior, by using the residual of a regression on month, day of month, and weekday indicators by canton as the dependent variable.

The results are presented in Table 1 and visualized in Figure 3. Row (1), indicated by 'Overall', shows the model estimate for the 22 cantons in the sample. We observe systematic drops in the probability that foreigners move to a switcher municipality in the time frame between four and seven months after the vote. This is consistent with our main hypothesis and the time lag after the vote fits the institutional feature of a three months notice period in many rental contracts. The drop of around one percentage point amounts to a 7.7 percent decline vis-à-vis the fraction of about 13 percent of foreigners

moving to switcher municipalities before the vote. Given the short-comings of the simple approach to identify behavioral reactions, a larger effect size is expected in the RDD.

6.2 Main estimates for observed location choices applying the regression discontinuity approach

In the main analysis, separate analyses are performed for each destination canton of movers as the threshold date might vary depending on the situation on the housing market and the institutional variation. The specification tests[16] detect a discontinuity in 12 cantons.[17] The corresponding results of the specification test are presented in Table E.3 in Appendix E.2. In Appendix D, we perform an exemplary analysis for one canton, i.e., the canton of Thurgau, to show how the empirical strategy is applied. The null hypothesis of no effect is not rejected for the remaining ten cantons.[18] This does not necessarily exclude the possibility that foreigners in these cantons also reacted. However, the situation on the housing market or the incorporation of the new information might not have followed a pattern which would allow the applied method to capture the effect.

As we learned from the simulation (see Appendix B), our identification strategy requires some form of rigidity in the rental market, for example a binding notice period, so that the control and the treatment group can be distinguished. While there is a legal default for a notice period of three months in rental agreements, actual moving dates (and related lease payments) are negotiable. So whether the notice period is binding or not depends to a large extent on whether the old tenant can find a new tenant. This might hold in a tight market, as well as in one where numerous people simultaneously want to change their domicile. If tenants intending to move have to wait until the end of their lease, a possible sign of market rigidity, this should be reflected in cumulated movements at the end of the month. The moving patterns within cantons could thus provide some indication about the situation on the rental market (see Figure E.12 and Figure E.13 in Appendix

[16] Regarding the necessary bandwidth choice in the applied nonparametric procedure, we abstain from using a technique that either depends on the variance of the assignment variable or cross-validation. First, our running variable is a date and the variation of such a variable makes no sense. Cross-validation would be theoretically appealing. However, it is computationally intensive, given the large number of observations and the simulation-based approach. We rather rely on eye-balling and group cantons according to size when choosing the bandwidth that is as small as possible but which makes the graphs reasonably smooth. We start with a bandwidth size of 45 days for the largest cantons ZH, BE, AG, and VD. This bandwidth size ensures that each estimation includes one beginning and one end of the month, when relocations are frequent. The next group contains the cantons LU, FR, SO, BL, SG, TG, TI, and VS for which a bandwidth of 60 days is applied. We use 75 days for the cantons SZ, ZG, BS, SH,GR, and NE. For the smallest ones, i.e., NW, GL, AR, and JU, a bandwidth of 90 days is chosen. These bandwidths are used for the RDD graphs, the RDD estimates and the threshold search. As the bandwidth for the testing has to be considerably smaller than for these estimates, we choose to use half of it in the testing procedure.

[17] These cantons are: ZH, BE, SZ, NW, GL, SO, AR, GR, TG, TI, VD, and VS. They cover about 66% of the moves within our sample period.

[18] The canton of Zug is a special case where switching municipalities have not changed their relative position, and thus we are not surprised by not detecting a reaction.

E.3). While there are clear peaks in the share of moving individuals at the end of each month in many cantons, there are some cantons (LU, FR, BS, SH, and NE) for which no distinct periodical pattern is observed. For these latter cantons, also no discontinuity is detected. The situation on the housing market could thus be one consistent explanation for why we find discontinuities in some but not all the cantons.

For the 12 cantons with a discontinuity, we potentially are able to separate treated from untreated individuals and thus estimate the local average treatment effect.[19] While this provides us with a local estimate for the sample at hand, there is no obstacle for its causal interpretation.

If we again apply the former simple approach to a sample restricted to those cantons for which a discontinuity is detected, we find stronger drops in the four to seven months after the vote (see Table 1, row (2)). The estimated coefficients indicate that during this period the probability of moving to a switcher municipality went down by around 2 percentage points. The estimates are visualized in Figure 3. No clear pattern is observed in the cantons where no discontinuity has been detected (see row (3)). The probability of moving to a switcher municipality seems to drop initially during the first month after the vote. However, it promptly returns to the old level. While these results provide further evidence that is consistent with our main hypothesis the procedure is still too rough to capture the full effect, primarily because the time indicators do not separate between treated and untreated individuals. Moreover, the effects likely do not materialize in all cantons at the same time, such that an averaging by month leads to an underestimation of the impact of the revelation of reservations towards foreigners. In the next sections, we therefore proceed with the proposed RDD approach at the detected threshold dates.

Overall effect at the threshold values

The main results build on a pooled analysis of the data of those cantons for which it is possible to estimate a discontinuity. The threshold dates are estimated to lie between about 1 month and 5.5 months after the vote. For observations from each canton, we center the moving dates at the detected thresholds, such that the resulting threshold for the pooled results is at zero. Combining data for all moving foreigners, we represent the main finding in a RDD graph presented in Figure 4.[20] A pronounced negative jump at the threshold is observed. Moreover, the probability of moving to a switcher municipality seems to return to its former level after some time.

[19] In our specific setting, the division between treated and untreated individuals is probably not perfect at the threshold value, as there is most likely no perfect temporal separation between those people who decided where to move after the new information became available (treated group) and those people who decided where to move beforehand (control group). Consequently, our estimates should rather be interpreted as a local intent-to-treat effect.

[20] The discontinuity estimates and RDD graphs for single cantons are reported in Table E.4 and Figure E.9 in Appendix E.2. As addressed before, we use the residual of a regression on time indicators to control for periodical patterns, thus in "normal" times it varies around zero.

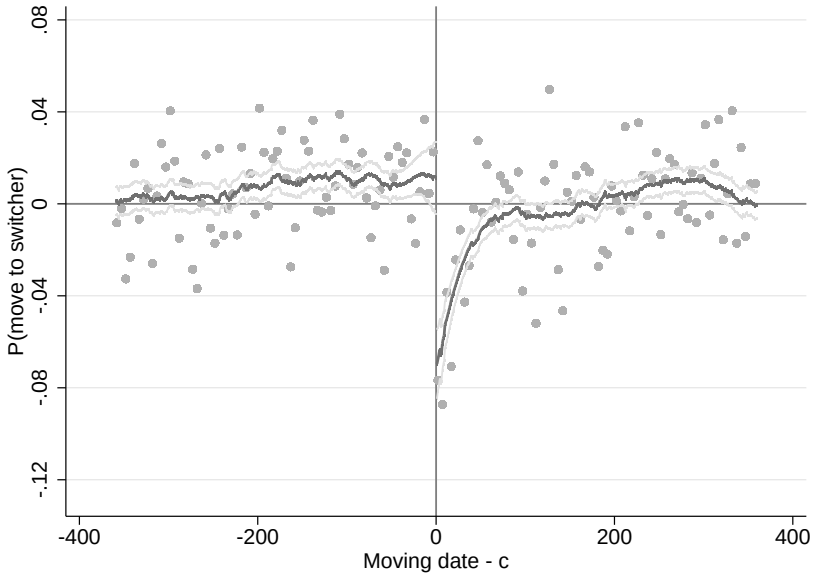


Figure 4 Probability of moving to municipalities revealing stronger reservations against foreigners in the pooled sample of cantons. Local linear smooth of the probability that a foreigner moves to a switcher municipality separately from both sides of the threshold, and using a bandwidth of 45 days. The light gray dots represent raw means within bins of 5 days.

Estimating the treatment effect at the threshold as described in Section 4, the estimated drop in the probability that a foreigner moves to a switcher municipality is about 4 to 8 percentage points, depending on the applied bandwidth.[21] The corresponding estimation results are presented in Table 2. The estimated effect is sizable. Before the threshold dates, the average probability that foreigners move to a switcher municipality was about 13 percent. The jump at the threshold of about 4 to 8 percentage points, thus amounts to a drop of about 30 to 60 percent.

Temporal development of the effect

As is evident from Figure 4, the effect fades after some time. To gain an impression of how long the effect persists, we run a linear regression model including indicators for time periods after the break on the pooled data for a time span of half a year around the threshold dates. We estimate a model of the form

$$y_i = \beta_0 + \sum_{k=1}^{6} \gamma_{(k\cdot 30)} \mathbb{1}(x_{c,i} \geq (k-1)\cdot 30 \quad \& \quad x_{c,i} < k \cdot 30) + \epsilon_i,$$

where y is our dependent variable, i.e., the residual of a regression of time fixed effects on an indicator set to one if an individual moves to a switcher municipality, x_c is the moving date centered at the estimated threshold for the corresponding canton, and $k \cdot 30$ stands for a number series rising in intervals of 30 days: {0, 30,...,180}. The $\gamma_{\cdot}$ coefficients

[21] Note that a decreasing estimate of the discontinuity with increasing bandwidths is consistent with the fading of the effect over time.

Table 2 RDD estimates of the probability that foreigners move to municipalities revealing stronger reservations towards foreigners

Dependent variable: Moving to a switcher municipality						
	(1)	(2)	(3)	(4)	(5)	(6)
Effect ΔP	-0.0824***	-0.0819***	-0.0601***	-0.0389***	-0.0358***	-0.0385***
	(0.0112)	(0.00997)	(0.00804)	(0.00698)	(0.00628)	(0.00566)
$P(switch)_{before}$	0.13	0.13	0.13	0.13	0.13	0.13
Bandwidth	45	60	90	120	150	180
N left	5,971	7,415	11,636	16,904	21,278	25,968
N right	5,876	8,156	12,729	17,020	21,232	26,000

Notes: Local linear sharp RDD estimates for different bandwidths in the pooled sample of cantons. Standard errors in parentheses. $P(switch)_{before}$ refers to the average probability of moving to a switcher municipality within a range of 60 days before the threshold date for the respective sample. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

consequently indicate whether the probability of moving to a switcher municipality is different from the level before the break within the specified time span. The estimation results are presented in Table 3. Given that the average probability of moving to a switcher municipality before the break is about 12.5 percent, the initial decline within the first month amounts to about 40 percent. It turns out that the effect becomes weaker over time, as suggested by the graphical evidence. There is no systematic difference in the level anymore about 150 days, or 5 months, after the break. This leveling off could occur, because the issue is no longer salient, i.e., expressed attitudes are no longer being discussed in the media. Alternatively, or in some combination, the housing market could have adjusted to a new equilibrium in which foreigners are compensated by, for example, lower rents for their disutility of residing in a municipality in which attitudes towards them are more negative.

Table 3 Temporal development of the probability that foreigners move to switcher municipalities

Dependent variable: Moving to a switcher municipality						
Coef.	γ_{30}	γ_{60}	γ_{90}	γ_{120}	γ_{150}	γ_{180}
	–0.049***	–0.015***	–0.012**	–0.025***	–0.011**	–0.005
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
No. of obs.	51,817					
R^2	0.0019					
F	16.18					

Notes: Ordinary least squares estimates of the temporal persistence of the effect in the pooled sample of cantons. The $\gamma_{\cdot}$ are coefficients capturing the deviations from the level before the break for intervals of 30 days. Standard errors in parentheses. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Table 4 RDD estimates of reactions in location choices for different groups of foreigners

Dependent variable: Moving to a switcher municipality

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Baseline	Temp. residency	Perm. residency	Non-European	European	Muslim
Effect ΔP	-0.0819***	-0.0841***	-0.0761***	-0.0934***	-0.0795***	-0.0563**
	(0.00997)	(0.0122)	(0.0168)	(0.0231)	(0.0110)	(0.0216)
$P(switch)_{before}$	0.13	0.11	0.15	0.09	0.14	0.1
Bandwidth	60	60	60	60	60	60
N left	7,415	4,441	2,974	1,072	6,340	757
N right	8,156	4,930	3,226	1,170	6,984	899

Notes: Local linear sharp RDD estimates for different subsamples in the pooled sample of cantons. Standard errors in parentheses. $P(switch)_{before}$ refers to the average probability of moving to a switcher municipality within a range of 60 days before the threshold date for the respective sample. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

We find at least suggestive evidence that the latter adjustment channel is relevant in an additional analysis described in Section 8, where we try to assess whether housing prices in switcher municipalities decrease in the aftermath of the vote.

Effects for different groups of foreigners

The vote on the minaret initiative prohibited Muslim communities from constructing further minarets. The change in the substantive law was thus narrow and affected solely one particular group within the foreign population of Switzerland. However, the public discourse was much broader in that it addressed general concerns about migration, as well as the role of (religious) tolerance and cultural identity in Switzerland. Based on the content of the initiative, we would expect that foreigners, in particular those with a Muslim background, experience lower expected identity utility from moving to a municipality where the initiative had unexpectedly received wide support. However, due to the general discourse, the outcome of the vote might well have been perceived as a revelation of attitudes towards foreigners in general. According to our understanding of the event, we share such an interpretation. Moreover, identity theories predict that other (foreign) minority groups identify with the beleaguered minority group. Accordingly, similar effects for different groups of foreigners are expected. This reasoning is in line with the finding in Rudert et al. (2017), documenting that even foreigners, and in particular highly-skilled foreigners, who are not directly affected by an anti-immigration vote in Switzerland in 2014 experienced considerable distress in regions where the support for this initiative was high.

In order to learn about any heterogeneity across groups of foreigners, we re-estimate the effect for various subsamples of foreigners. As the results in Table 4 show, we find sys-

tematic effects for foreigners holding either temporary or a permanent residence permit, for European and non-European foreigners, as well as for foreigners from Muslim countries.[22] This is evidence that the change in perceived attitudes affected the attractiveness of particular municipalities for the group of foreigners across nationalities and cultural backgrounds. While the relative magnitude of the effect seems to be strongest for non-European foreigners, it is still considerable for European foreigners. Overall, according to our interpretation, the reaction was in response to the unexpectedly revealed negative perceptions of the native population. The comparatively large reaction of non-Muslims might thereby be rationalized along two lines of reasoning: First, it has not been equally easy (or it has been differentially costly) for all groups in the foreign population to react. Recently immigrated Muslims are often less well educated and economically less well off than other immigrants, such as work migrants from the rest of Europe. This latter group of people has relatively more resources (including information) to react in their location choice. Second, highly-skilled individuals are more sensitive to reservations towards immigrants. However, there are fewer highly skilled individuals among the Muslim population in Switzerland. Both lines of reasoning would be consistent with the observation that all groups react to a rather similar extent.

Effects for foreigners with different occupational skill levels

So far, the evidence suggests that, in general, residents of Switzerland with a foreign nationality expected to experience a loss in utility if they were to move to a municipality that unexpectedly revealed increased reservations towards foreigners. However, the effects might differ depending on individuals' skill level. Regarding foreigners with a low educational background, studies investigating the determinants of attitudes towards foreigners frequently argue that concerns about the welfare system play an important role (see, e.g., Hainmueller and Hiscox, 2010; Dustmann and Preston, 2007). Thus, immigrants with a low level of education, who are more likely to rely on the welfare system, might be the main targets of negative attitudes towards foreigners. It is, however, open whether they also experience the highest loss in identity utility when residing in an environment with strong reservations against foreigners. Instead, foreigners with a high education might be particularly sensitive towards such reservations. They might be more interested in politics and more likely to follow the discussion in the media than their less well educated compatriots (Rudert et al., 2017; Elsayed and De Grip, 2018).

In our data, we have only limited information about the formal education of the individuals to test for differential effects across skill levels. Instead, we rely on information about individuals' occupations. As the immigration authorities do not collect this information

[22]We have no information about an individuals' religious affiliation. Instead, we infer that individuals originating from countries where Islam is the main religion are more likely to be Muslim. The classification on the main religion of countries is based on the *Cross-National Socio-Economic and Religion Data, 2011* and was downloaded from the Association of Religion Data Archives, www.TheARDA.com.

systematically, it is only available for about 15% of individuals in our data. We infer people's (occupational) skill level by matching occupations to standards of required formal education. It is important to note that immigrants work in occupations that do not match their skill level more often than nationals. Their formal educational degrees might not be accepted. This might be especially the case for, for example, refugees from developing countries. Miscategorizations are therefore likely, in particular for people in occupations with formally low skill requirements. To mitigate this miscategorization problem we use only individuals originating from countries from which formal education is most likely accepted in Switzerland. These are the countries of the European-Union and countries on the American continent, as the United States, Canada, and Brazil. We further include Australia, and New Zealand. Concentrating on these individuals, we expect that occupation is a good approximation of the skill level.

We use the ISCO-08 classification of the International Labour Office (2012) to classify the skill level needed to perform specific occupations. The four point classification considers not only required formal education, but also the task complexity of the respective occupation. We differentiate between high, upper-medium, medium and low skilled individuals.[23]

Table 5 RDD estimates of reactions in location choices for foreigners with different occupational skill levels

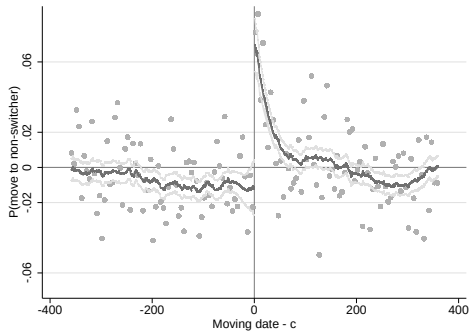
Dependent variable: Moving to a switcher municipality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sample	High skill	High skill	High skill	Up-med. skill	Up-med. skill	Up-med. skill	Med. skill	Med. skill	Med. skill	Low skill	Low skill	Low skill
Effect ΔP	-0.203**	-0.151**	-0.155***	-0.104	-0.0643	-0.0324	-0.109	-0.00349	0.0353	0.0600	0.00904	0.0233
	(0.0803)	(0.0658)	(0.0595)	(0.0716)	(0.0940)	(0.0955)	(0.0931)	(0.0789)	(0.0662)	(0.116)	(0.0968)	(0.0744)
$P(switch)_{before}$	0.09	0.09	0.09	0.12	0.12	0.12	0.10	0.10	0.10	0.08	0.08	0.08
Bandwidth	60	90	120	60	90	120	60	90	120	60	90	120
N left	77	112	156	33	55	69	70	124	181	50	83	125
N right	65	108	140	31	45	62	82	124	164	51	85	120

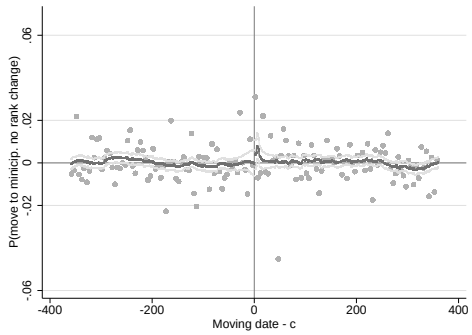
Notes: Local linear sharp RDD estimates for different subsamples in the pooled sample of cantons. Standard errors in parentheses. $P(switch)_{before}$ refers to the average probability of moving to a switcher municipality within a range of 60 days before the threshold date for the respective sample. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

The number of observations in this sample get scarce. Nonetheless we can interpret the results, presented in Table 5, qualitatively. We find that within this sample of nationalities

[23]We code somebody as high-skilled if his or her occupation corresponds to an ISCO skill level of 4, i.e., the second stage of tertiary education or first stage of tertiary education (medium duration). Upper-medium skilled, i.e., ISCO skill level of 3 and thus the first stage of tertiary education (short or medium duration). Medium skilled people refer to an ISCO skill level of 2, i.e., lower to post-secondary education. Foreigners considered low-skilled are in an occupation of ISCO skill level 1 which requires primary education.



(a) Probability of moving to a non-switcher municipality



(b) Probability of moving to a municipality with no rank change

Figure 5 Probability of moving to alternative groups of municipalities. Local linear smooth of the probability that a foreigner moves to a specific group of municipalities separately from both sides of the threshold, and using a bandwidth of 45 days. The light gray dots represent raw means within bins of 5 days.

the high-skilled group is most sensitive to the revelation of new information about citizens' attitudes towards foreigners. The estimate is significantly different from zero despite the few observations. For the other groups, the discontinuity estimates seem to fall the lower the skill level. However, the estimates for these other groups are not significantly different from zero at conventional levels. These results suggest that the group of high-skilled immigrants seems to react the most.

7 Discussion

This section contains a discussion of potential challenges to the causal interpretation of our results and two alternative explanations behind the documented effect, as well as the validation for the assumptions behind our empirical approach.

7.1 Validation check for the relevance of the change in the relative political positioning of the switcher municipalities

The definition of the switcher municipalities results in a dichotomous categorization of municipalities. Accordingly, we see per construction an equivalent positive jump in the probability of moving to a non-switcher municipality at the threshold, as we saw a negative one in the probability to move to a switcher municipality. The corresponding pattern is presented in Figure 5a. RDD estimation results are reported in Table E.5 in columns 1, 2, and 3 in Appendix E.3. Moreover, the classification itself rests on a central argument emphasizing that the voters in a switcher municipality not only expressed a more restrictive position towards migration than in the past, but that the swing in expressed

reservations is also more pronounced than for the average municipality of a canton (second condition of the definition for switcher municipalities outlined in Subsection 2.2). In order to validate the importance of this rank condition, i.e. the fact that it is the relative position within the choice set that matters, we re-estimate the basic specifications with the pooled data for an alternative classification of municipalities. We construct the dependent variable such that it captures the probability of moving to a municipality that has not changed its rank, i.e., its position regarding the support of critical attitudes towards foreigners compared to the other municipalities in the canton. Specifically, we focus on municipalities that are not switcher municipalities and have not experienced a rank change. If our findings were driven by a change in the positioning, we would not expect to see a pronounced jump, either positive or negative, in the probability of moving to these municipalities at the estimated thresholds. The resulting pattern is presented graphically in Figure 5b. The results of the RDD estimates are reported in Table E.5 in columns 4, 5, and 6 in Appendix E.3. We do not find a systematic reaction for this category of municipalities.

In an additional test, we analyze whether the positive jump within the group of non-switcher municipalities is consistent with our reasoning. We define two further alternative groups of municipalities, one including non-switcher municipalities with a positive rank change, and thus a relative right shift, after the vote on the minaret initiative and one including non-switcher municipalities with a negative rank change, and thus a relative left shift. Re-estimating the jump in the probability of moving to one of these municipality types at the threshold, we find that the positive jump in the non-switcher municipalities seems to be driven by municipalities with a negative rank change. The estimation results can be found in Table E.5, columns 7 to 12 in Appendix E.3. This finding is consistent with our reasoning that foreigners are deterred from moving to municipalities revealing reservations, and rather choose to move to municipalities that got relatively more attractive in this dimension.

We interpret this as further support that the observed negative jump in the probability of choosing a switcher municipality is driven by a mover's perception of an unexpected negative shift in the social attitudes of these municipalities towards foreigners.

7.2 Two alternative supply side explanations

So far, the empirical regularity is interpreted within the theoretical framework based on demand-side reactions to perceived negative attitudes. However, the observed patterns in aggregate moving behavior across municipalities would also be consistent with supply-side reactions. First, native landlords in switcher municipalities might feel free to discriminate against foreigners, once they are informed about their co-residents' attitudes towards immigrants. If this supply-driven explanation holds, native landlords are expected to dis-

Table 6 RDD estimates of the reaction in location choices for second-generation immigrants

Dependent variable: Moving to a switcher municipality					
	(1)	(2)	(3)	(4)	(5)
Effect ΔP	-0.0174	-0.00497	0.00188	0.00144	-0.00911
	(0.0359)	(0.0280)	(0.0240)	(0.0214)	(0.0195)
$P(switch)_{before}$	0.16	0.16	0.16	0.16	0.16
Bandwidth	60	90	120	150	180
N left	882	1,342	1,925	2,387	2,921
N right	927	1,436	1,919	2,377	2,845

Notes: Local linear sharp RDD estimates in the pooled sample of cantons. $P(switch)_{before}$ refers to the average probability of moving to a switcher municipality within a range of 60 days before the threshold date for the respective sample. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

criminate against foreigners in general. When considering second-generation immigrants who hold the same foreign names, the same potentially foreign appearance, and the same residence permit types as their parents, one would thus expect landlords to also - at least to some extent - discriminate against this group of foreigners. However, if the phenomenon is primarily demand-driven, a smaller reaction is expected for the second-generation immigrants, as they are in general better assimilated. They grew up in the Swiss institutional environment, for example, being familiar with referendums on migration issues. We are aware though that this is not a clear-cut test as discrimination might be based on characteristics along which first- and second-generation immigrants differ. The test thus loses bite if, for example, due to language skills first-generation and second-generation migrants are differentially discriminated against. To explore differential reactions, we repeat our estimates for the group of second-generation foreigners, i.e., individuals who were born in Switzerland but have not been naturalized. Table 6 reports the estimates of the treatment effect. We find a much smaller reaction for second-generation foreigners, whereby no statistically significant treatment effect is estimated. Together with the preceding finding that highly skilled foreigners react the most, this suggests to us that the main effect is primarily demand-driven. The highly skilled (well-payed) foreigners are likely the more attractive tenants for landlords than the less skilled foreigners, so in case of discrimination we would expect to see the largest reactions for this latter group.

A second potential supply-side explanation focuses on the labor market. After the ballot, employers in switcher municipalities might feel free to discriminate against foreign labor, discouraging immigrants from moving into these municipalities. In order to test this explanation, we analyze relocations in narrow neighborhoods, as they are less likely to be due to a job change. Thus, if the effect were driven by labor market discrimination, we would expect relocations within a narrow radius to be less affected. If, however, the effect

Table 7 RDD estimates of the reaction in location choices for immigrants moving within a distance of less than 15 minutes of travel distance

Dependent variable: Moving to a switcher municipality

	(1)	(2)	(3)	(4)	(5)	(6)
Effect ΔP	-0.0947***	-0.103***	-0.0835***	-0.0604***	-0.0506***	-0.0460***
	(0.0183)	(0.0163)	(0.0132)	(0.0114)	(0.0102)	(0.00919)
$P(switch)_{before}$	0.14	0.14	0.14	0.14	0.14	0.14
Bandwidth	45	60	90	120	150	180
N left	2,492	2,992	4,690	6,672	8,286	10,148
N right	2,456	3,354	5,142	6,836	8,533	10,394

Notes: Local linear sharp RDD estimates in the pooled sample of cantons and the sample of individuals moving within a distance of less than 15 minutes. $P(switch)_{before}$ refers to the average probability of moving to a switcher municipality within a range of 60 days before the threshold date for the respective sample. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

is, as we argue, due to the perceived change in attitudes, we would expect the effect to also be present in movement patterns within a narrow radius. To implement this test, we repeat our estimates for a subsample of foreigners who move within a narrow radius. The distance between the old and the new municipality of residence is measured in minutes of travel time, and we use 15 minutes as a measure of relatively short distance.[24] The results reported in Table 7 show that the effect for this subsample of individuals who move within a 15-minute radius is very similar to the baseline effect. If anything, it is slightly stronger. A stronger effect within a narrow radius seems sensible given that it is less costly to get the information about vote outcomes in the direct surrounding. The observation that individuals who move within a narrow radius, probably not attributable to a job change, are just as likely to react as those who move within a wider radius leads us to the conclusion that labor discrimination most likely cannot explain our finding.

7.3 Validation of the identification assumptions

There are some crucial assumptions that need to hold for a valid RDD setting. In our setting, they translate to the assumptions that individuals deciding where to move before or after the vote are on average the same and that the characteristics of switcher municipalities, except for their voting behavior, have not changed systematically. There is no formal test for this assumption and we do not observe the date of the moving decision. However, most RDD studies check whether individual characteristics are balanced around the threshold to validate it. We, in a first step, follow this strategy and estimate a sharp RDD, taking some individual characteristics, which are available in our data (i.e., age,

[24]Travel time is collected using Stata's Traveltime3 command (Bernhard, 2013), which retrieves travel time between two locations from Google Maps.

civil status, duration of stay in CH, citizenship of parents, and individuals' skill level), as dependent variables and using the estimated threshold date as threshold value. We report the results of this exercise in Table E.6 in Appendix E.2. We find only one significant estimate for age and a bandwidth of 45 days. As it is not robust to the bandwidth choice and seems to be rather small we are not alarmed that our results might be driven by this difference. We do not find systematic differences in the other individual characteristics around the threshold.

We secondly test, whether the characteristics of the switcher municipalities have not changed systematically after the vote in 2009. Table E.7 in Appendix E.2 reports the results of the corresponding validation estimates. While most characteristics have not systematically changed, we find evidence for two systematic changes: on the one hand, there is a decrease in the resident population, and on the other hand, there is a slight increase in the rate of vacant apartments in switcher municipalities. Both findings are rather complementary evidence in support of our main hypothesis, as they are consistent with the observed individual reactions of foreigners. It can be expected that there will be, at least temporarily, more vacant apartments if a sizable group of the population shuns switcher municipalities. When we study the number of Swiss and foreign residents separately, we find that the point estimate is larger for foreigners. The reduction in the number of Swiss citizens in switcher municipalities further suggests that there might be a group of Swiss citizens that reacts to the newly revealed information as well. This effect might be driven by naturalized immigrants, who we do not observe in our data, or by native citizens, who care about their neighbors' attitudes in this dimension. Overall, the observation suggests some form of general preference-based spatial sorting. While it would be interesting to investigate the individual reactions for Swiss citizens in a separate analysis, corresponding individual level data for Swiss citizens is not available.

A third implication of our design is that there is no change in the probability of moving due to the event. It is important that this condition holds, for example, because a large increase in the prevalence of moving within the group of non-switcher municipalities would be reflected in a relative decline in the probability of moving to a switcher municipality. In order to investigate how many foreigners move per day around the threshold, we generate counts of movers within each canton and moving date. This approach again requires that we control for periodical patterns before running the analysis. There are dates at which it is much more likely to observe individuals moving due to the municipalities' mandatory moving dates and contractual arrangements on tenancy changeover.[25] We first estimate

[25] We again follow, for example, Davis (2008) in controlling for these periodical patterns by using a battery of indicator variables. We first run the specification

$$y_t = \beta_0 + \beta x_t + u_t,$$

where the dependent variable is y_t, the number of moving individuals at a certain date t, and x_t includes indicator variables for months, the day of the month and weekdays. We estimate over the period between

a sharp RDD on the number of foreigners moving in general. The corresponding RDD graph is presented in Figure E.10a in Appendix E.3. The results for two bandwidths are reported in columns 1 and 2 of Table E.8 in Appendix E.2. We find no significant change in the number of movers. Second, we repeat the exercise using counts of individuals leaving switcher municipalities. The graphical evidence is presented in Figure E.10b in Appendix E.3, and the corresponding estimates are reported in columns 3 and 4 of Table E.8 in Appendix E.2. As before, we do not find a systematic change in the probability that a foreigner moves from this group of municipalities. The latter finding could also be interpreted as evidence that the interaction between foreigners and citizens in switcher municipalities has not radically changed after the vote. If it had, we would expect to see foreigners leaving these municipalities. Third, we check whether the aggregate data still allows us to detect the pattern we found in the individual data. We repeat the estimation using the residual counts of individuals moving to a switcher municipality. As is evident from the estimates in columns 5 and 6 of Table E.8 in Appendix E.2 (see also Figure E.11 in Appendix E.3), a drop in the probability of moving to a switcher municipality is still observed. Foreigners shun switcher municipalities after the vote.

Finally, as in many RDD studies, we run a placebo test by repeating the test procedure for the same time periods in the previous year (-365 days). We cannot reject the null hypothesis of no treatment effect in either of the cantons, and thus find no indication for a discontinuity the year before. Results for this placebo test are reported in Table E.3 in Appendix E.2. This test also indicates that our results are most likely not driven by periodical moving patterns.

8 Complementary evidence on the development of housing prices

The empirical findings indicate a fading out of the effect of the unexpectedly revealed reservations towards foreigners in switcher municipalities. While this fading might be due to reduced salience over time, theoretical models of the housing market would rather predict a market reaction. In particular, the reduced inflow is expected to lead to a drop in relative housing prices in these municipalities. Given the regulations of the Swiss rental market, any market reactions, however, are expected to be primarily confined to new rental contracts and property transfers. In order to test this complementary hypothesis, we draw on data of rental prices and selling prices for private apartments and houses from online advertisements collected on all major Swiss online platforms. We use 332,343 price quotes for the period between January 2006 and November 2010, i.e., up to one year after

2006 and 2008, thus before the event, in order to control for moving behavior in normal times. We then predict the residual u_t for the entire time period and use it as our dependent variable. For convenience, we still refer to this variable as the number of moving individuals.

the vote. The data on rental as well as selling prices are per square meter (and per month for rental objects) for apartments and houses with one to seven rooms. They are linked to time according to the online announcement date of the respective add. In our estimation sample, the average rental price per square meter is 21.61 Swiss Francs and the average selling price about 5,174.85 Swiss Francs. We use a difference-in-differences (DID) type approach to evaluate whether prices change systematically in switcher municipalities. We estimate a linear regression model including time indicators for eight monthly periods after the vote to capture the development of any effect on prices. A separate indicator for the remaining four months in the first year after the vote captures the change in the "equilibrium" price. We separately analyze rental and selling announcements, as market reactions might differ in time. Given the integration of the housing market, the magnitude of any detected change in (relative) prices depends on the chosen comparison groups. In order to capture a possible *drop* in housing prices in switcher municipalities, we compare them to prices in municipalities that did barely change their position in the municipality ranking with regard to reservations towards foreigners.[26] It is important to note that, there is no control group in a DID sense in our setting. In principle all non-switcher municipalities are also treated and market adjustments could be expected. However, as we want to capture whether prices start diverging, we consider this group of municipalities to be the most valid comparison group, as we observe little change in the inflow of foreigners after the minaret vote in these municipalities. In contrast, we would expect relative price changes to be largest vis-à-vis municipalities that became relatively more open and experienced an increase in foreigners moving in. However, in this latter case, (relative) prices might change primarily due to price movements in the municipalities that experienced a higher inflow. We therefore concentrate on the former comparison group in our DID analysis and investigate how prices diverge.

The estimation results are reported in Table E.9 and the temporal evolvement of the raw means of prices is presented in Figure E.14 in the Appendix. In order to control for general level differences and time patterns in the price announcements, we perform the estimates correcting for municipality, month, day of month, weekday, and year fixed effects. We perform one overall estimation and two separate ones, one for cantons in which we found a discontinuity in location choices and one for those for which our approach did not detect a sharp reaction. Further, we report separate estimations for rental and selling prices. The '$Switch * post_{.}$' interactions capture whether the price evolvement of switcher municipalities differs from that of the comparison municipalities. We find that newly announced rental prices start systematically diverging between switcher municipalities and the relative stable comparison municipalities only a few months after the minaret vote. Towards the end of the first year after the vote, the difference, on average, amounts to roughly 1.0 Swiss Francs (or to 4.6% when compared to the average price of 21.61 Swiss

[26] More specifically, we choose those municipalities that changed their position in the ranking by only up to two steps.

Francs) for rental prices and to about 258.2 Swiss Francs (or to 4.9% when compared to the average price of 5,174.85 Swiss Francs) for selling prices respectively. While the estimated effects are larger for switcher municipalities in cantons where a discontinuity has been detected, we also observe a drop in prices, which is smaller, in the switcher municipalities of the remaining cantons. While we do not want to overstate the results from this simple analysis, they suggest that there was some systematic reaction in housing prices. The price reaction is consistent with our main result that foreigners shun switcher municipalities after the surprising revelation of negative attitudes towards foreigners. Moreover, it points to an interpretation of the overall phenomenon as a demand side reaction.

9 Concluding remarks

This paper uses the vote on the Swiss minaret initiative in 2009 as a natural experiment to identify the effect of newly revealed reservations towards immigrants on their location choices. A simple before after comparison suggests that the probability of foreigners moving to a municipality that unexpectedly revealed stronger negative attitudes towards them is reduced in the time after the vote. In a refined analysis, we exploit the quasi-random variation in the exact date of individuals' decision to move applying a RDD with unknown discontinuity points. We find discontinuities in the probability of foreigners moving to a municipality which had unexpectedly revealed strong reservations against immigrants for 12 out of 22 cantons. The estimated reaction is sizable. The probability of an immigrant relocating to a switcher municipality drops initially, on average, by about 40 percent. This initial effect levels off over a period of 5 months before it vanishes.

The reaction is not confined to Muslims or non-European immigrants, but also holds for European immigrants. The latter finding is in line with research on ostracism and the rejection-identification model in social psychology which asserts that minority groups tend to identify with other minority groups when the latter are under threat and that even a vague feeling of being ostracized can produce psychological distress. Moreover, foreigners in high-skill occupations react more strongly than foreigners in medium- and low-skill occupations. Well-educated immigrants therefore seem to be particularly sensitive to reservations towards foreigners. This observation highlights a tension when countries try to attract highly-skilled professionals from abroad.

Complementary evidence suggests that this leveling off is due to a market reaction, as rental and selling prices for apartments and houses in switcher municipalities fall by roughly 5% within a year vis-à-vis municipalities that neither became relatively more or less tolerant towards foreigners. Our data does not allow us to test whether this latter effect is partly due to a systematic reaction in the location choices of Swiss citizens. A

reaction of Swiss citizens is well conceivalbe and refers to a more general preference-based spatial sorting with respect to expressed attitudes at the ballot box.

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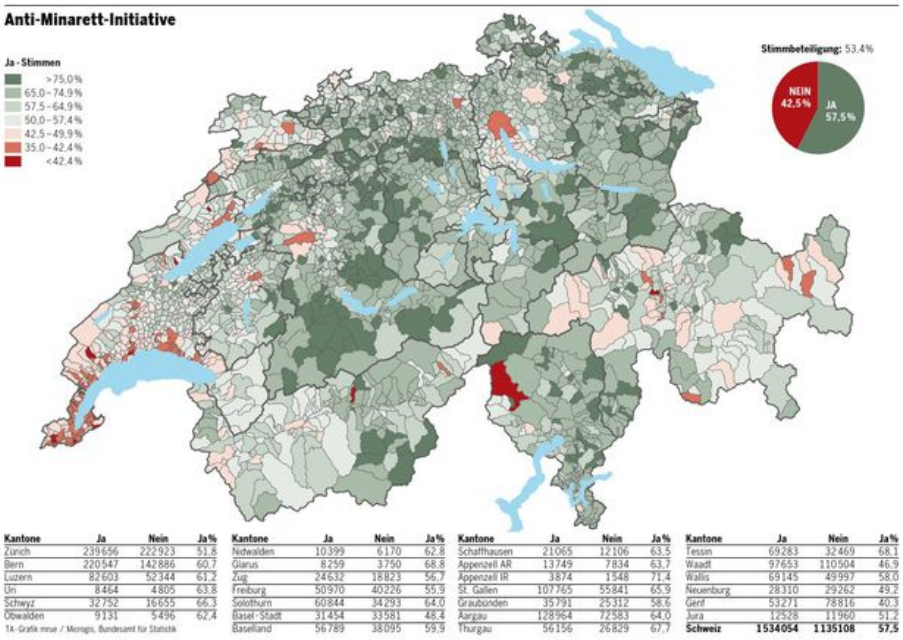
Appendix

A Campaign poster and media reporting of the vote outcome

Figure A.1 Campaign poster in support of the minaret initiative.



Kantone	Ja	Nein	Ja%	Kantone	Ja	Nein	Ja%	Kantone	Ja	Nein	Ja%	Kantone	Ja	Nein	Ja%
Zürich	239656	222923	51.8	Nidwalden	10399	6170	62.8	Schaffhausen	21065	12106	63.5	Tessin	69283	32469	68.1
Bern	220547	142886	60.7	Glarus	8259	3750	68.8	Appenzell AR	13749	7834	63.7	Waadt	97653	110504	46.9
Luzern	82603	52344	61.2	Zug	24632	18823	56.7	Appenzell IR	3874	1548	71.4	Wallis	69145	49997	58.0
Uri	8464	4805	63.8	Freiburg	50970	40226	55.9	St. Gallen	107765	55841	65.9	Neuenburg	28310	29262	49.2
Schwyz	32752	16655	66.3	Solothurn	60844	34293	64.0	Graubünden	35791	25312	58.6	Genf	53271	78816	40.3
Obwalden	9131	5496	62.4	Basel-Stadt	31454	33581	48.4	Aargau	128964	72583	64.0	Jura	12528	11960	51.2
TA-Grafik mrue / Microgis, Bundesamt für Statistik				Baselland	56789	38095	59.9	Thurgau	56156	26829	67.7	**Schweiz**	**1534054**	**1135108**	**57.5**

Figure A.2 Local media reporting about the vote on the minaret initiative. The excerpt of an article from the Swiss newspaper *Tages-Anzeiger* entitled "Land sagte Ja, Städte Nein" (November 30, 2009) shows how the outcome of the vote on the minaret initiative was visualized in the press.

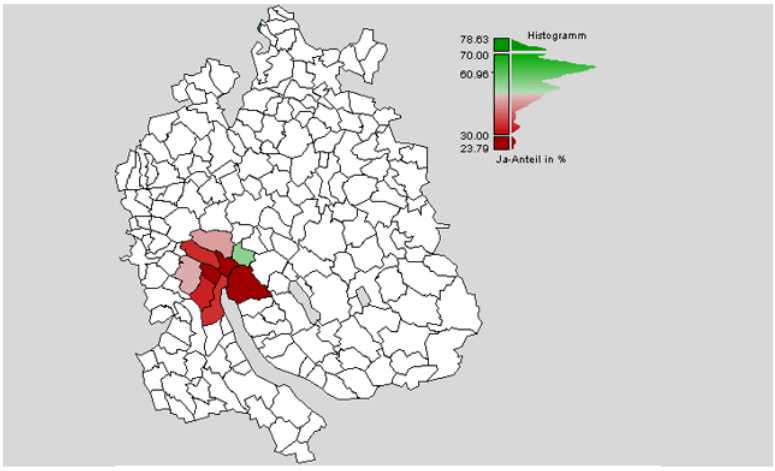


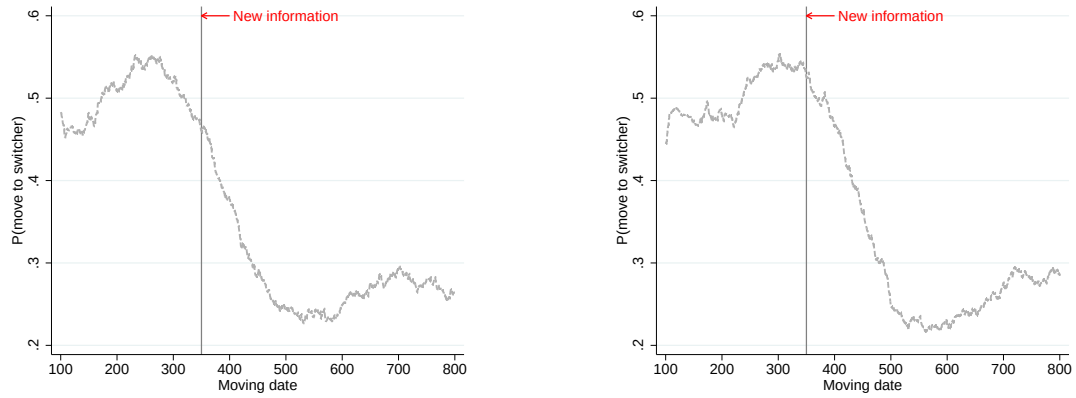
Figure A.3 Local media reporting about the vote outcomes at the voting district level. The graph was presented in the Swiss newspaper *Tages-Anzeiger* in the article entitled "Warum die Schwamendinger anders ticken als der Durchschnittszürcher" (November 30, 2009) , it depicts the outcome of the vote on the minaret initiative at the level of voting districts within Zurich.

B Simulation of a simple location search with overlapping search generations

How does the inflow of people with particular preferences into a municipality change when the municipality becomes less attractive for them? An ex ante understanding is necessary in order to choose appropriate econometric techniques when analyzing data about moving behavior to test hypotheses about location choices. This also holds in the context of our analysis concerning the effect of the revelation of negative attitudes towards foreigners on immigrants' location choices.

Whereas our theoretical model is about location decisions, our empirical data is restricted to information about moving dates. It is thus ex ante unclear how the incorporation of the new information about attitudes towards foreigners is reflected in moving patterns after the vote on the minaret initiative. As it can be considered random whether an individual makes his or her location choice immediately before or after the polling day, a regression discontinuity design could be applied regarding the decision to locate in a switcher municipality with the polling day as the threshold date. However, actual moves materialize only after a decision has been made, and we do not know when people made their location decisions. We therefore have to decide whether we would predict that an unexpected negative change in a municipality's attitudes towards foreigners (switcher municipality) would elicit a sharp drop in the probability of foreigners choosing to move to that municipalities or whether we, instead, predict that a gradual transition to a new equilibrium takes place.

To asses how the effect might materialize, we perform a simulation analysis of individual location choices. We simulate a simple location search with overlapping search generations to evaluate which incorporation pattern we would expect. We assume as given that only those individuals who search a location after the vote can incorporate the information about revealed attitudes into their choice. We generate a population of 3,500 observations and randomly assign a point in time at which they decide to relocate. We assume that individuals terminate the contract with their landlord as soon as they have decided to move and start to search for an alternative apartment. The probability of a searcher finding an apartment on each successive day after starting their search is set to $0,015$, such that the average search length is $66,66$ days. In the baseline setting, the probability that the apartment lies in a switcher municipality is 0.5. The new information enters the system at time $t_0 = 350$ and reduces the probability of choosing a switcher municipality by $\tau = -0.25$. We simulate the location search, once with search frictions alone, i.e., individuals move at the point in time in which they find an apartment, and once considering a notice period of 3 months (or 90 days) assuming that individuals cannot move before 90 days after starting their search even if they find housing before this limit. The two scenarios mark the extreme cases in terms of the transaction costs related to moving. While these



(a) without a binding notice period

(b) with a binding notice period

Figure B.4 Simulated probability of moving to a switcher municipality. Local linear smooth of the probability that an individual chooses to move to a switcher municipality over the moving date using a bandwidth of 60 days.

costs are assumed minimal in the first scenario, they are assumed to be maximal in the second. Movers cannot afford to rent two apartments at the same time and do not find follow-up tenants. Based on the two scenarios, we compare how the new information is incorporated and reflected in the pattern of the probability of moving to a switcher municipality over time.

Figure B.4a visualizes the resulting pattern without a notice period, estimated by a local linear smooth (LLS). The straight line indicates the point in time in which the new information enters the system. The effect seems to be incorporated gradually, and it takes some time until the probability of choosing a switcher municipality stabilizes at its new equilibrium. Figure B.4b shows the resulting picture if a notice period is taken into account. It again takes some time until the new equilibrium is reached. However, the new information is incorporated with a time lag and the pattern reveals a much steeper decline in the probability of moving to a switcher municipality. The effect of the information seems to cause a sharp drop in the probability of moving to a switcher municipality, rather than a gentle decline as in the case when the information is gradually incorporated. As notice periods are very common in Switzerland (and the default option in rental agreements), we think that the second pattern of information incorporation is realistic with regard to our application, and in the choice of econometric techniques for the empirical analysis.

Assuming that it is random when individuals choose to relocate and find an apartment, a regression discontinuity framework is suitable to analyze the incorporation of the new information. The probability of moving to a switcher municipality seems to drop sharply once the new information takes effect on individuals' relocation behavior. This sharp drop, if it really exists, can be measured by estimating a jump or a discontinuity. However, the

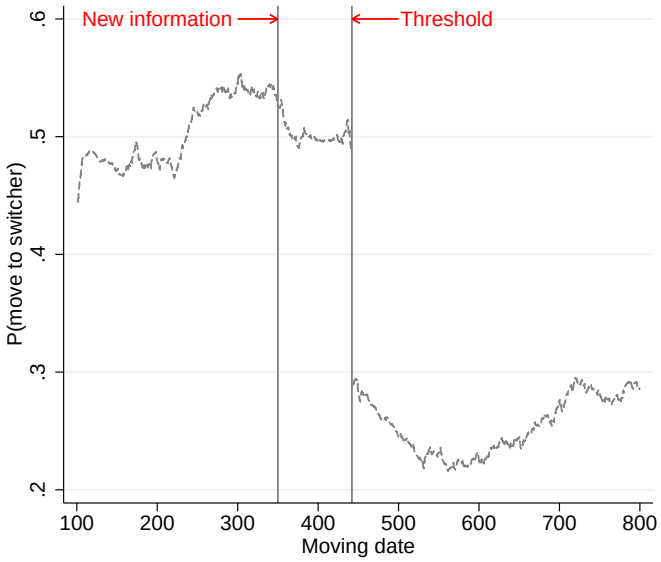
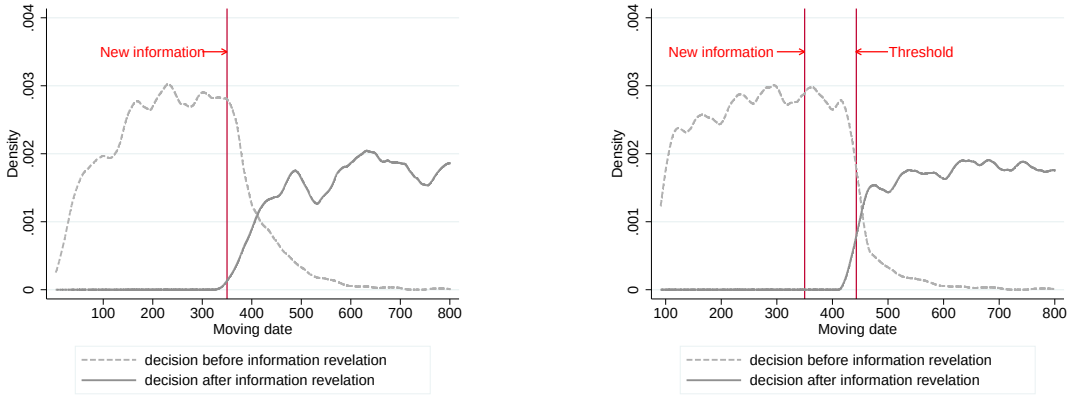


Figure B.5 Simulated probability of moving to a switcher municipality (with a binding notice period). Local linear smooth of the probability that an individual chooses to move to a switcher municipality over the moving date separately from both sides of the estimated threshold date, and using a bandwidth of 60 days.

exact date when the jump materializes is not known. We, therefore, apply a regression discontinuity design with unknown discontinuity points to test whether the relationship features a discontinuity and to determine its position. This method is discussed in detail in Appendix C on the empirical strategy. For the particular run of the simulation, there is a discontinuity at $t = 442$ for the notice period scenario.

Re-estimating the relationship using LLS separately from both sides of the estimated threshold, we find in Figure B.5 that the probability of choosing a switcher municipality features a clear negative jump at the threshold value. To gain an idea why the jump mostly appears when a notice period is considered, we decompose the distributions of the moving dates. We separate between individuals who decided to relocate before the new information entered the system and those who decided to relocate afterwards. Figure B.6b shows the resulting densities for the scenario considering a notice period. We see that the estimated threshold date is temporally close to the point in time at which the movers who decided on the basis of the new information outweigh those who decided without it. Furthermore, the densities show steep slopes just before the threshold date. Thus, the change in the composition of the population regarding individuals with and without the new information occurs quite abruptly. The transition period in the baseline scenario, see Figure B.6a, is wider. The densities have a less steep slope in the period after the new information is released. This explains why the effect materializes much more smoothly than in the scenario with the notice period in most simulation runs.



(a) without a binding notice period

(b) with a binding notice period

Figure B.6 Composition of the pool of individuals moving regarding their information level. Kernel density estimates.

In a last step, we repeat the procedure of drawing a population and testing for the presence of a jump in the resulting moving pattern 500 times. Table B.1 summarizes the results of this simulation. We find a discontinuity in 82 percent of the draws in the notice period setting. In contrast, it is only found in about 26 percent of cases in the setting with search frictions alone.

Table B.1 Test results for discontinuities detected in simulated data

	Mean	SD	Min.	Max.	N
Without notice period					
Discontinuity detected	0.26	0.44	0	1	500
Threshold	399.04	31.01	360	499	128
τ at threshold	–0.16	0.11	–0.29	0.26	128
With notice period					
Discontinuity detected	0.82	0.38	0	1	500
Threshold	440.84	13.72	360	495	412
τ at threshold	–0.23	0.08	–0.42	0.36	412

Note: Summary of simulation results, once without notice period and once considering a notice period of 90 days.

The regression discontinuity method with unknown discontinuity points thus seems an appropriate econometric technique for analyzing information effects on moving patterns in a context with notice periods.

C Regression discontinuity designs with unknown discontinuity points

This section describes how we implement the regression discontinuity design with unknown discontinuity points proposed in Porter and Yu (2015) in applied empirical work.

In our application and this description, we concentrate on the sharp design. We further only describe our proceedings for the case with one discontinuity point, even though their approach allows for multiple discontinuity points.[27]

The relationship of interest between the dependent variable y and the running variable x when estimating a RDD can be expressed by the following reduced-form expression:

$$y = m(x) + \epsilon = m_c(x) + \tau_c d_c + \epsilon.$$

Where $m(x)$ is some function of x, d_c is a treatment indicator ($d = \mathbb{1}(x > c)$), $E[\epsilon|x, d_c] = 0$ and τ_c is the potential treatment effect at the threshold c. $m_c(x) = m(x) - \tau_c d_c$ and $c \in \Pi = [\underline{c}, \overline{c}]$.

The starting point of the procedure is to test whether there is a discontinuity in the relationship. Therefore the proposed approach by Porter and Yu (2015) first tests two hypotheses in order to disentangle whether there is a selection effect, a treatment effect, both, or neither of the two:

$$\begin{aligned} &H_0^{(1)} : \text{no effects} \\ &H_1^{(1)} : \text{selection only} \end{aligned} \tag{2}$$

$$\begin{aligned} &H_0^{(2)} : \text{no effects \& selection only} \\ &H_1^{(2)} : \text{treatment effect only \& both selection and treatment effect.} \end{aligned} \tag{3}$$

They further propose conducting these tests sequentially and starting with the test of (3). If $H_0^{(2)}$ cannot be rejected, a test of (2) could be performed. They develop a test statistic for which the only difference in testing (2) or (3) is the choice of a different smoothing parameter.[28] The bandwidth in testing (2) should be considerably larger than the bandwidth in testing (3). As we are interested exclusively in cases where there is a treatment effect, we only test (3). If $H_0^{(2)}$ cannot be rejected, we abstain from further analysis. According to the approach by Porter and Yu (2015), it can be assumed that there is a discontinuity in $m(x)$ if $H_0^{(2)}$ is rejected by the specification test. It can be proceeded with the estimation of the threshold value c. Once c is known, the size of the

[27] A discussion of the fuzzy case and an in-depth discussion of the theoretical derivations and the method are presented in the original paper (Porter and Yu, 2015).

[28] When testing (2), $m(x)$ is estimated non-parametrically, thereby overersmoothing the estimate so that the resulting bias generates power. When testing (3), however, $m(x)$ is undersmoothed, and the bias in the selection case will disappear asymptotically, while a jump in $m(x)$ would still generate power.

discontinuity τ_c can be estimated. Before discussing these latter aspects, we describe how the specification test is performed by simulating the empirical distribution of the test statistic under the null to find critical values.

Consider the relationship between y and x as

$$y = \overline{m}(x) + e. \tag{4}$$

Under $H_0^{(1)}$ $E[e|x] = E[\epsilon|x] = 0$ for $x \in \Pi$ and under $H_1^{(1)}$ $E[e|x] \neq 0$. By observing that $E[eE[e|x]\mathbb{1}_x^\Pi] = E[E[e|x]^2\mathbb{1}_x^\Pi] = E[(m(x) - \overline{m}(x))^2\mathbb{1}_x^\Pi] \geq 0$ and that the equality only holds if $H_0^{(1)}$ is true, they construct a consistent test for (2) based on $E[eE[e|x]]\mathbb{1}_x^\Pi$. e_i is estimated by the nonparametric residual from (4) ($\hat{e}_i = y_i - \hat{y}_i$) and $E[e_i|x_i]f(x_i)$ by a kernel estimator. $\hat{y}_i$ is a kernel estimator of $\overline{m}(x_i)$ defined as

$$\hat{y}_i = \frac{1}{n-1}\sum_{j\neq i} y_j L_{b,ij}/\hat{f}_i,$$

and $\hat{f}_i$ is estimated by

$$\hat{f}_i = \frac{1}{n-1}\sum_{j\neq i} L_{b,ij},$$

where $L_{b,ij} = \frac{1}{b}l(\frac{x_i - x_j}{b})$ is a second-order kernel[29] and b is the bandwidth. Note that (x_i, y_i) is excluded in estimating $\overline{m}(x_i)$. $E[e_i|x_i]f_i$ is estimated by $\frac{1}{n-1}\sum_{j\neq i}\hat{e}_j L_{h,ij}\mathbb{1}_j^\Pi$. Finally, the test statistic is based on

$$I_n = \frac{nh^{1/2}}{n(n-1)}\sum_i\sum_{j\neq i}\mathbb{1}_i^\Pi\mathbb{1}_j^\Pi L_{h,ij}\hat{e}_i\hat{e}_j.$$

Under $H_0^{(1)}$, $\hat{e}_i$ is a good estimator for ϵ_i, but under $H_1^{(1)}$ it includes a bias in the neighborhood of c. This generates power.

The asymptotic variance of the test statistic can be consistently estimated by

$$v_n^2 = \frac{2h}{n(n-1)}\sum_i\sum_{j\neq i}\mathbb{1}_i^\Pi\mathbb{1}_j^\Pi L_{h,ij}^2\hat{e}_i^2\hat{e}_j^2$$

for the testing of both hypotheses. The resulting studentized test statistic is $T_n = I_n/v_n$. As the convergence rate of T_n to a standard normal is very slow, they propose approximating the finite-sample distribution by using the wild bootstrap of Wu (1986) and Liu (1988). The bootstrap samples are generated by imposing the null hypothesis; i.e., such that they will mimic the null distribution of T_n. The algorithm is specified as follows:

Step 1: Generate the two-point wild bootstrap residual for $i = 1, ..., n$. $\epsilon_i^* = \hat{e}_i(1-\sqrt{5})/2$ with probability $(1+\sqrt{5})/(2\sqrt{5})$, and $\epsilon_i^* = (1+\sqrt{5})/2$ with probability $(\sqrt{5}-1)/(2\sqrt{5})$. Then $E^*[\epsilon_i^*] = 0$ and $E^*[\epsilon_i^*] = \hat{e}_i^*$.[30]

[29] Please note that we use a uniform or rectangular kernel for the specification testing and the estimation of $\hat{c}$ in our application. We do not want to give too much weight to single days because of the possible periodical patterns in individuals' moving behavior.

[30] Note that ϵ_i^* is, in effect, simulated based on the centered $\hat{e}_i$, $\overline{\hat{e}_i} = \hat{e}_i - \overline{\hat{e}}$. For more details, see Porter and Yu (2015).

Step 2: Generate the bootstrap resample $\{y_i^*, x_i\}_{i=1}^n$ by $y_i^* = \hat{y}_i + \epsilon_i^*$. Only data with $x_i \in [\underline{c} - b, \overline{c} + b]$ are needed to construct I_n^*. Then calculate the bootstrap residuals $\hat{e}_i^* = y_i^* - \hat{y}_i^*$.[31]

Step 3: Use the resulting samples to calculate the test statistic $T_n^* = I_n^*/v_n^*$ using the same bandwidths as in the estimation of T_n. Where

$$I_n^* = \frac{nh^{1/2}}{n(n-1)} \sum_i \sum_{j \neq i} \mathbb{1}_i^{\Pi} \mathbb{1}_j^{\Pi} L_{h,ij} \hat{e}_i^* \hat{e}_j^*$$

and

$$v_n^{*2} = \frac{2h}{n(n-1)} \sum_i \sum_{j \neq i} \mathbb{1}_i^{\Pi} \mathbb{1}_j^{\Pi} L_{h,ij}^2 \hat{e}_i^{*2} \hat{e}_j^{*2}.$$

Step 4: Repeat the three former steps B[32] times and use the resulting empirical distribution to approximate the null distribution of T_n. H_0 can consequently be rejected if $T_n > T_{n(\alpha B)}^*$. $T_{n(\alpha B)}^*$ is the upper α-percentile of the empirical distribution of T_n^*.

If $H_0^{(2)}$, which implies that there is no discontinuity in the relationship, can be rejected by the specification test, the threshold value c and the discontinuity size τ_c can be estimated. Porter and Yu (2015) determine the discontinuity point by

$$\hat{c} = \arg\max_{c \in \Pi} \hat{\tau}^2(c),$$

and thus by checking whether $c = x_i$ ($x_i \in \Pi$) maximizes $\hat{\tau}^2(c)$.

The estimate of $\hat{\tau}(c)$, i.e., the nonparametric estimate of the discontinuity in the relationship between y and x at c, is thereby estimated as in the classical RDD literature. Importantly, Porter and Yu (2015) show that $\hat{\tau}_c$ is a natural by-product of the estimation of the discontinuity point, and that c can be treated as if known when estimating treatment effects in RDDs. The asymptotic distribution of $\hat{\tau}(\hat{c})$ is the same as if c were known. After the estimation of c, one can perform a standard RDD analysis.

C.1 Estimation in the regression discontinuity design

We briefly describe how we estimate the local average treatment effect (LATE) in our RDD[33] setting with time as the assignment variable, given that the null hypothesis of no treatment effect could be rejected and the threshold value was determined.[34]

[31] $\hat{y}_i^*$ is defined as $\hat{y}_i$, except that y_i in (4) is replaced by y_j^*.

[32] In our application, B is set to $1,500$.

[33] A practical guide for the estimation of a RDD is provided by Imbens and Lemieux (2008).

[34] There are few studies that use time as the assignment variable so far. Davis (2008) is, to our knowledge, the first. He uses the introduction date of a program aimed at reducing pollution in Mexico City as the threshold. Another example is the study by Paola et al. (2012). They use a RDD approach over time to investigate the effect of the introduction of a system with penalty points for driving offenses in Italy. They identify the causal effect of the new system on road safety.

A peculiarity of using moving dates as assignment variable is that it is necessary to control for periodical patterns before running the analysis. We control for these potential periodical patterns in moving dates by controlling for a battery of indicator variables, i.e. month, day of the month, and the weekday of the movement fixed effects. As often done in RDD studies, we first run a linear regression of the dependent variable on covariates. Subsequently, the residual of this regression is used as the dependent variable in our analysis (see, e.g., Lee and Lemieux, 2010).[35] For notational convenience, we will nonetheless refer to our dependent variable as y_i or the probability that an individual moves to a switcher municipality.

Given that the necessary assumptions for a sharp RDD hold, the treatment effect τ can be estimated by the following limits[36]

$$\widehat{E(\tau_i|x_i = c)} = \lim_{x \to c^+} \widehat{E(y_i|x_i = x)} - \lim_{x \to c^-} \widehat{E(y_i|x_i = x)}. \tag{5}$$

Where y stands for the dependent variable, x represents the assignment variable (in our application the moving date), $\lim_{x \to c^-}$ stands for the limit approaching the threshold from the left, and $\lim_{x \to c^+}$, the limit coming from the right.

For the point estimates in our analysis, we follow Hahn et al. (2001) and Porter (2003) and estimate the limits in (5) by a local linear regression (LLR) using a uniform kernel, as in the specification testing.[37] To obtain, for example, an estimator for $\lim_{x \to c^-} E(y_i|x_i = x)$, i.e., to the left of the threshold, a local polynomial regression with a constant term with coefficient α_{Yl} (l for left) and a linear term in x_c with coefficient β_{Yl}, to correct for the influence of x_c on the outcome, is specified ($y_i = \alpha_{yl} + \beta_{yl} x_{ci} + \epsilon_i$). Thereby, x_c corresponds to the forcing variable x centered at the threshold c ($x_c = x - c$). This estimation includes all data points within the range $x - h \leq x < c$ (or $x_c - h \leq x_c < 0$). The estimate $\hat{\alpha}_{yl}$

[35] We first run the specification

$$y_i = \beta_0 + \beta_1 x_i + u_i,$$

where y_i is our dependent variable and thus an indicator of whether an individual moved to a switcher municipality. The variable x_i includes indicator variables for the month, the day of the month, and the weekday of the movement. We perform this procedure separately for each canton and use data between 2006 and 2008 for the estimation. We then predict the residual u_i for the whole time span and use it as our dependent variable.

[36] The identification of a treatment effect in a RDD setting is only possible at $x = c$, i.e., a LATE is identified at $x = c$ (Hahn et al., 2001).

[37] Since the LLR framework is well understood, our analysis, like many others, abstains from applying a more complicated model (such as the GAM) and uses a linear probability model, which directly models $P(Y = 1) = X'\beta$. This model can be directly estimated using LS, or LLR in the present case. The drawbacks of this approach are clear. Using this linear model, it is possible to obtain predicted probabilities below zero or above one, which is at odds with the definition of a probability. However, in many applications this approach seems to be a useful simplification (see, e.g., Cameron and Trivedi, 2005, p. 466ff.).

then emerges from the solution to the following minimization problem

$$\min_{\alpha_{yl};\beta_{yl}} \sum_{i=1}^{N} (y_i - \alpha_{yl} - \beta_{yl} x_{ci})^2 K_h \left(x_{ci} - 0\right) I(x_{ci} < 0)$$

(Hahn et al., 2001). The analogous procedure is applied for the estimation of the other limit in equation (5). h represents the suitable bandwidth $h > 0$ and $K_h \left(x_i - c\right)$, the kernel weighting function, defined as $K_h \left(x_i - c\right) = \frac{1}{h} K \left(\frac{x_i - c}{h}\right)$. The treatment effect estimator in the sharp RDD is given by $\hat{\tau} = \hat{\tau}_c = \hat{\alpha}_{yr} - \hat{\alpha}_{yl}$.

The specification for the estimation on both sides of the determined threshold can be formulated as

$$y_i = \alpha + \beta(moving\_date_i - \hat{c}) + \epsilon_i.$$

D Exemplary analysis for a single canton

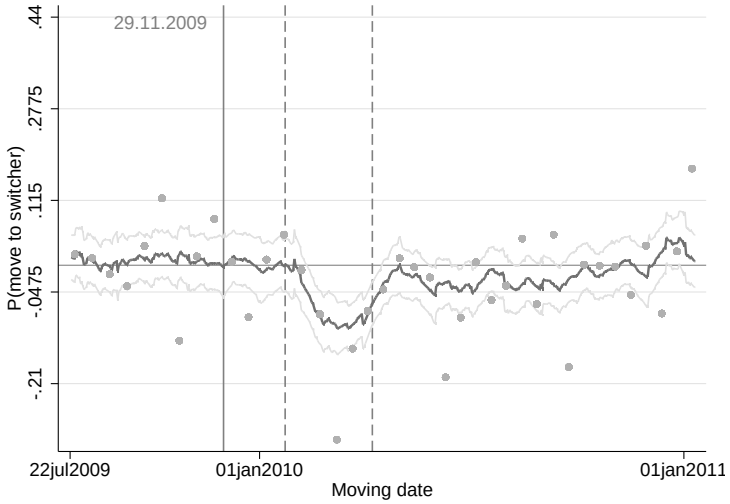


Figure D.7 Probability of moving to a switcher municipality in the canton of Thurgau. Local linear smooth of the probability that a foreigner moves to a switcher municipality, using a bandwidth of 30 days. The light gray dots represent raw means within bins of 15 days.

In order to provide an intuition for the application of the methodology used, we exemplify our econometric proceeding for the canton of Thurgau (TG). The dependent variable of interest is an indicator of whether a moving individual chooses to locate in a switcher municipality.[38]

We first visually inspect how the probability that a foreigner chooses to move to one of the switcher municipalities evolves over time, whereby we undersmooth the estimate using the same bandwidth as in the specification testing. As can be seen in Figure D.7, the probability is rather stable until the vote, which is marked by the solid vertical line. Some time after the vote has taken place, the probability seems to drop temporarily. For the detection of a possible threshold, we define a time span within which we expect the jump to occur, if there is one. This time span is marked by the two dashed vertical lines.

We perform the specification test as described in Section 4 (or in Appendix C in more detail); i.e., testing the null hypothesis of no treatment effect, within the defined time span. We find that the null hypothesis is rejected for this canton. The test statistic is 2.62 and exceeds the critical value of 0.79 at the 5% significance level. These results are also listed in Table E.3 in Appendix E.2. The estimated threshold date for the canton of Thurgau is March 2, 2010 and, thus about 3 months after the vote has taken place. We proceed with a classical RDD analysis and plot a local linear smooth separately from both sides of the estimated threshold. In Figure D.8, a negative jump in the probability

[38] As addressed before, we use the residual of a regression on time indicators to control for periodical patterns, thus in "normal" times it varies around zero.

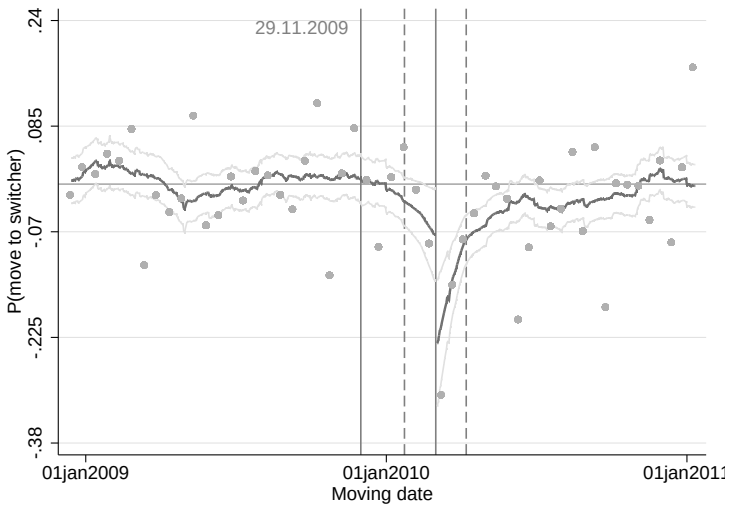


Figure D.8 Probability of moving to a switcher municipality in the canton of Thurgau - RDD graph around the threshold date March 2, 2010. Local linear smooth of the probability that a foreigner moves to a switcher municipality separately from both sides of the threshold, and using a bandwidth of 60 days. The light gray dots represent raw means within bins of 15 days.

that a foreigner chooses to locate in a switcher municipality is observed at the estimated threshold date. Moreover, it seems that the effect fades and that after some time the probability that a foreigner moves to a switcher municipality returns to its former level.

When we estimate the treatment effect at the detected threshold, we find a statistically significant negative discontinuity at the particular date. The probability of moving to a switcher municipality drops by 16 percentage points at the threshold. The details of the estimation result are listed in Table E.4 in Appendix E.2.

E Additional results

E.1 Descriptive Statistics

Table E.2 Descriptive statistics

Variable	Mean	SD	Median	Min	Max	N
Switcher and Non-switcher municipalities in 2009						
Switcher	0.24	-	-	0	1	2,515
Mean vote shares $_{former}$						
All	50.82	12.6	52.48	13.98	85.33	2,515
Switcher	46.05	9.93	47.01	13.98	64.08	612
Non-switcher	52.35	12.98	54.82	14.8	85.33	1,903
Mean vote shares 2009						
All	63.26	10.71	64.1	25	100	2,515
Switcher	63.34	7.18	63.55	50	87.5	612
Non-switcher	63.24	11.62	64.3	25	100	1,903
Δ =(vote share 2009 - mean vote share$_{former}$)						
All	12.44	7.57	12	-23.9	49.15	2,515
Switcher	17.29	7.25	17.14	3.25	49.15	612
Non-switcher	10.88	6.98	10.63	-23.9	39.08	1,903
rank change						
All	-0.3	48.39	-1	-294	299	2,515
Switcher	39.91	50.60	21.5	-8	299	612
Non-switcher	-13.23	39.81	-6	-294	131	1,903

Notes: Summary statistics for the main variables for the municipality classification.

E.2 Results for the individual cantons

Table E.3 Results for the specification test and the estimated threshold dates

		Original					Placebo		
Canton	h_{test}	T_n	$T^*_{n(90B)}$	$T^*_{n(95B)}$	h	Date $\hat{c}$	T_n	$T^*_{n(90B)}$	$T^*_{n(95B)}$
ZH	22.5	1.07	0.45	1.089	45	Jan 06, 2010	-0.695	0.294	0.655
BE	22.5	2.245	0.559	1.702	45	Feb 06, 2010	-2.227	0.248	0.672
SZ	37.5	4.768	0.796	1.751	75	May 13, 2010	-1.863	0.567	1.308
NW	45	2.405	0.583	0.952	90	Dec 21, 2009	-0.556	0.717	1.155
GL	45	0.777	0.38	0.764	90	Jan 08, 2010	-2.619	1.007	1.398
SO	30	2.191	0.864	1.441	60	Mar 18, 2010	1.229	2.265	2.752
AR	45	3.327	0.837	1.398	90	May 16, 2010	-0.249	0.674	1.228
GR	37.5	1.358	0.626	0.913	75	Mar 11, 2010	0.164	0.564	0.926
TG	30	2.624	0.272	0.796	60	Mar 02, 2010	-1.211	0.39	0.658
TI	30	2.15	0.434	1.01	60	Mar 18, 2010	-3.043	0.083	0.518
VD	22.5	1.866	1.46	2.442	45	Feb 18,2010	-3.248	0.332	0.631
VS	30	2.471	0.348	0.625	60	Jan 08, 2010	0.369	0.437	0.76
LU	30	-2.442	0.677	1.023	60	-			
ZG	37.5	-3.919	0.768	0.986	75	-			
FR	30	-2.017	0.28	0.634	60	-			
BS	37.5	-2.844	0.229	0.895	75	-			
BL	30	-3.848	-0.342	0.071	60	-			
SH	30	-.285	0.451	1.06	60	-			
SG	30	-1.661	0.54	1.124	60	-			
AG	22.5	-12.331	-1.95	-0.463	45	-			
NE	30	.193	0.331	0.842	60	-			
JU	45	-2.005	1.492	2.284	90	-			

Notes: Summary of the results of the RDD with unknown discontinuity points. The left hand side (first 6 columns) reports the results for the actual testing period, the last three columns report the testing results for a placebo testing period (testing period -365 days). We report the resulting test value (T_n), the bandwidth used in the specification test (h_{test}), the critical values on the 10 and 5 percent level, the bandwidth used in the threshold search (h), and the estimated threshold date (Date $\hat{c}$) for each canton in which the null was rejected. The bootstrap results are obtained using 1,500 resamples.

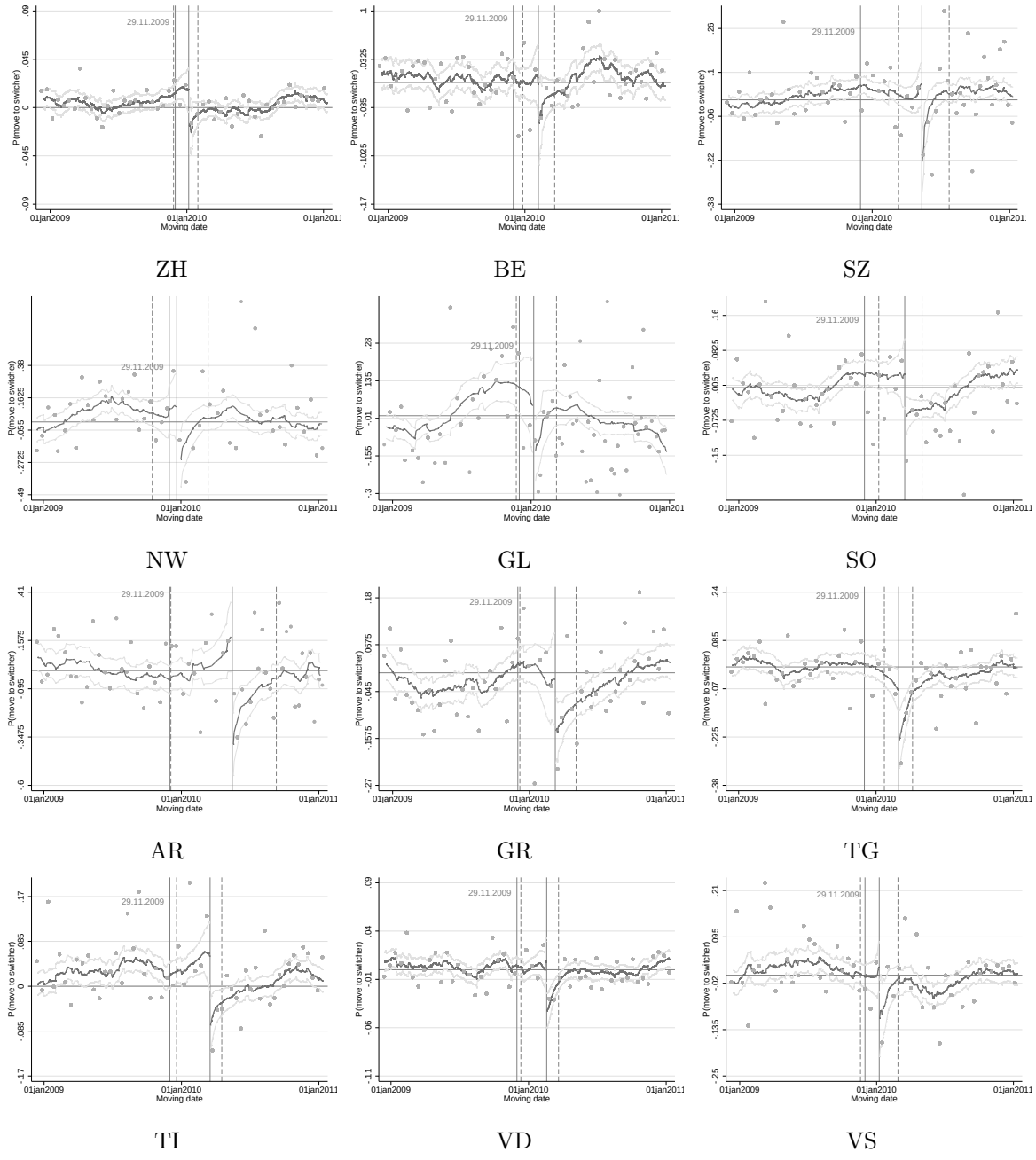


Figure E.9 Probability of moving to a switcher municipality in 12 cantons - RDD plots around the estimated threshold dates. Local linear smooth, using the same bandwidth (h) as in the estimations in Table E.4, of the probability that a foreigner chooses to move to a switcher municipality over the moving date, separately from both sides of the threshold for all cantons in which a discontinuity was found. The dashed vertical lines indicate the testing region, the first solid line indicates the vote date, and the second the estimated threshold date. The light gray dots represent raw means within bins of 15 days.

Table E.4 RDD estimates of the probability that foreigners move to a switcher municipality at the estimated thresholds for 12 cantons

Dependent variable: Moving to a switcher municipality									
Canton	ZH	BE	SZ	NW	GL	SO	AR	GR	TG
Effect ΔP	-0.0345**	-0.0674*	-0.296***	-0.437***	-0.187*	-0.0953**	-0.611***	-0.121*	-0.168***
	(0.0156)	(0.0405)	(0.0689)	(0.153)	(0.106)	(0.0483)	(0.121)	(0.0683)	(0.0541)
$P(switch)_{before}$	0.07	0.17	0.20	0.36	0.27	0.14	0.39	0.22	0.27
Bandwidth	45	45	75	90	90	60	90	75	60
N left	2,106	687	340	109	73	308	72	204	453
N right	1,543	664	350	68	115	460	88	395	500

Dependent variable: Moving to a switcher municipality			
Canton	TI	VD	VS
Effect ΔP	-0.132***	-0.0536***	-0.131**
	(0.0435)	(0.0158)	(0.0583)
$P(switch)_{before}$	0.21	0.05	0.27
Bandwidth	60	45	60
N left	541	1,090	576
N right	621	1,562	376

Notes: Local linear sharp RDD estimates of the probability of moving to a switcher municipality for single cantons. Standard errors in parentheses. $P(switch)_{before}$ refers to the average probability of moving to a switcher municipality within a range of 60 days before the threshold date for the respective sample. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

E.3 Additional tables and figures

Table E.5 RDD estimates of the probability that foreigners move to different definitions of non-switcher municipalities

Dependent variable: Moving to a particular type of non-switcher municipality

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.	all	all	all	$\Delta rank = 0$	$\Delta rank = 0$	$\Delta rank = 0$
Effect ΔP	0.0824***	0.0819***	0.0601***	0.00431	-4.22e-05	-0.00159
	(0.0112)	(0.00997)	(0.00804)	(0.00504)	(0.00443)	(0.00354)
$P(type)_{before}$	0.87	0.87	0.87	0.02	0.02	0.02
Bandwidth	45	60	90	45	60	90
N left	5,971	7,415	11,636	5,971	7,415	11,636
N right	5,876	8,156	12,729	5,876	8,156	12729
	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Var.	$\Delta rank > 0$	$\Delta rank > 0$	$\Delta rank > 0$	$\Delta rank < 0$	$\Delta rank < 0$	$\Delta rank < 0$
Effect ΔP	-0.00190	0.0191*	0.0128	0.0800***	0.0628***	0.0488***
	(0.0123)	(0.0107)	(0.00842)	(0.0158)	(0.0138)	(0.0109)
$P(type)_{before}$	0.11	0.11	0.11	0.74	0.74	0.74
Bandwidth	45	60	90	45	60	90
N left	5,971	7,415	11,636	5,971	7,415	11,636
N right	5,876	8,156	12,729	5,876	8,156	12,729

Notes: Local polynomial sharp RDD estimates for the probability of moving to the respective municipality type in the pooled sample of cantons. Standard errors in parentheses. $P(type)_{before}$ refers to the average probability of moving to a municipality of the respective type within a range of 60 days before the threshold date. Dependent variables: *all* refers to an indicator set to one if the municipality is a non-switcher municipality, $\Delta rank = 0$ refers to an indicator set to one if the municipality has not changed the relative position, $\Delta rank > 0$ refers to an indicator set to one if the municipality experienced a positive rank change, and $\Delta rank < 0$ refers to an indicator set to one if the municipality experienced a negative rank change. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Table E.6 Individual characteristics of movers around the threshold dates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var.	Age	Age	P(married)	P(married)	Duration CH	Duration CH	P(par. CH)	P(par. CH)	P(high skill)	P(high skill)
Effect Δ	-1.238***	-0.549	-0.0173	-0.0178	0.000222	0.464	0.000141	-3.18e-05	0.00359	-0.0256
	(0.443)	(0.382)	(0.0190)	(0.0165)	(0.381)	(0.328)	(0.00226)	(0.00205)	(0.0445)	(0.0391)
Bandwidth	45	60	45	60	45	60	45	60	45	60
N left	5,971	7,415	5,971	7,415	5,971	7,415	5,971	7,415	661	815
N right	5,876	8,156	5,876	8,156	5,876	8,156	5,876	8,156	624	884

Notes: Local linear sharp RDD estimates for different dependent variables in the pooled sample of cantons. Standard errors in parentheses. Dependent variables: *Age* of the individual, *P(married)* an indicator of whether a person is married, *Duration CH* the duration of stay in Switzerland, *P(par. CH)* an indicator of whether a parent of an individual is Swiss, and *P(high skill)* is an indicator set to one if an individual has an occupation in skill level 1 or 2. The sample size with regard to the skill level of individuals is considerably smaller, as the information is only available for a small subsample in our data. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Table E.7 Municipality characteristics before and after the vote on the minaret initiative

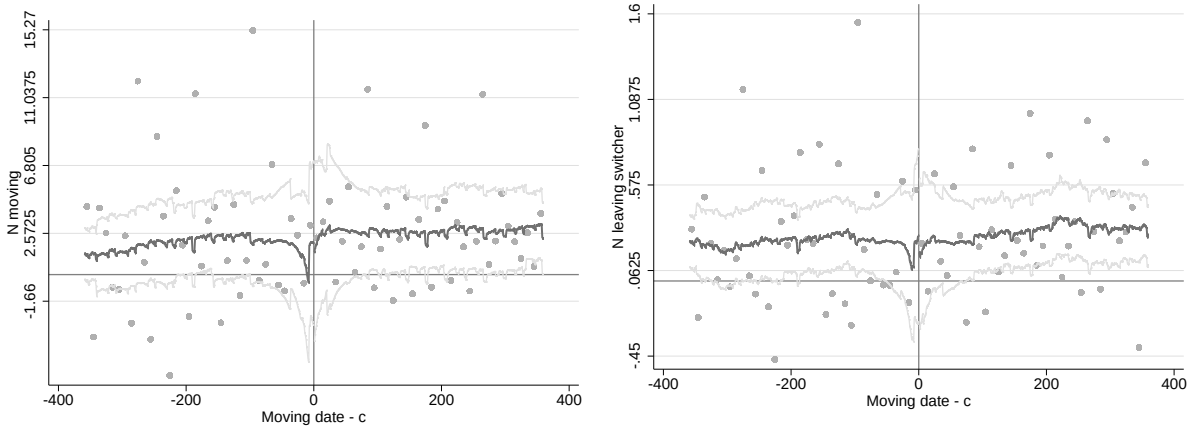
Dep. Var.	(1) $Tax\ rate$	(2) Pop	(3) Pop_{Swiss}	(4) $Pop_{foreign}$	(5) Pop_wel	(6) Pop_{poor}	(7) Pop_{rich}	(8) $Vacant$	(9) N_{comp}	(10) $Natur$
$\mathbb{1}(year \geq 2009)$	–0.219	83.597***	34.454***	49.143***	–0.035	–0.000*	0.017***	–0.308**	–0.402*	–0.088
	(0.908)	(20.452)	(9.794)	(11.013)	(0.022)	(0.000)	(0.001)	(0.136)	(0.213)	(0.068)
Switcher municipality	–0.963	–1424.448***	–1034.673***	–389.775***	–0.279**	–0.001	0.026***	–0.269	–4.356***	–0.018
	(1.014)	(326.205)	(234.113)	(104.269)	(0.104)	(0.000)	(0.007)	(0.293)	(1.425)	(0.017)
$Switcher * \mathbb{1}(year \geq 2009)$	0.447	–38.086**	–13.744**	–24.342**	0.008	0.000	–0.001	0.499*	0.311	0.035
	(0.630)	(15.127)	(6.208)	(9.502)	(0.078)	(0.000)	(0.001)	(0.243)	(0.255)	(0.031)
No. of obs.	9,392	9,404	9,404	9,404	6,810	10,028	10,028	9,403	8,376	9,355
No. of clusters	23	24	24	24	25	25	25	24	24	24
R^2	0.93	0.07	0.08	0.07	0.13	0.14	0.37	0.07	0.05	0.00

Notes: OLS estimations for annual municipality level data between 2007 and 2010, including cantonal fixed effects. Standard errors are adjusted for clustering at a cantonal level. Dependent variables: $Tax\ rate$ is the municipality tax multiplier, Pop is the municipality's population measured as the sum of the number of Swiss Pop_{Swiss} and foreign residents $Pop_{foreign}$, Pop_{wel} is the share of the population that depends on welfare, Pop_{poor} is the proportion of the population classified as poor, Pop_{rich} is the fraction of the population that is classified as rich, $Vacant$ is the number of vacant apartments per 1,000 inhabitants, N_{comp} measures how many companies have been incorporated, and $Natur$ measures the naturalization rate of foreigners in the municipality. Differences in sample sizes occur, because low number of cases in a municipality are not reported in the official statistics for reasons of privacy. Significance levels: * .05<p<.1, ** .01<p<.05, *** p<.01.

Table E.8 Number of moving individuals around the threshold dates

Dep. Var.	(1) N_{move}	(2) N_{move}	(3) N_{from_switch}	(4) N_{from_switch}	(5) N_{to_switch}	(6) N_{to_switch}
Effect ΔN	-0.0476	-0.609	-0.250	-0.103	-1.348***	-0.989**
	(4.393)	(4.310)	(0.423)	(0.380)	(0.461)	(0.389)
Bandwidth	60	90	60	90	60	90
N left	501	753	501	753	501	753
N right	492	738	492	738	492	738

Notes: Local linear sharp RDD estimates in the pooled sample of cantons. Standard errors in parentheses. Dependent variables: N_{move} corresponds to the number of foreigners moving. N_{from_switch} refers to the number of foreigners leaving switcher municipalities, and N_{to_switch} refers to the number of foreigners relocating to switcher municipalities. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.



(a) Number of moving individuals in general

(b) Number of moving individuals leaving switcher municipalities

Figure E.10 Number of individuals moving around the threshold in the pooled sample of cantons. Local linear smooth of the number of individuals who move over the moving date, separately from both sides of the threshold, using a bandwidth of 90 days. The light gray dots represent raw means within bins of 10 days.

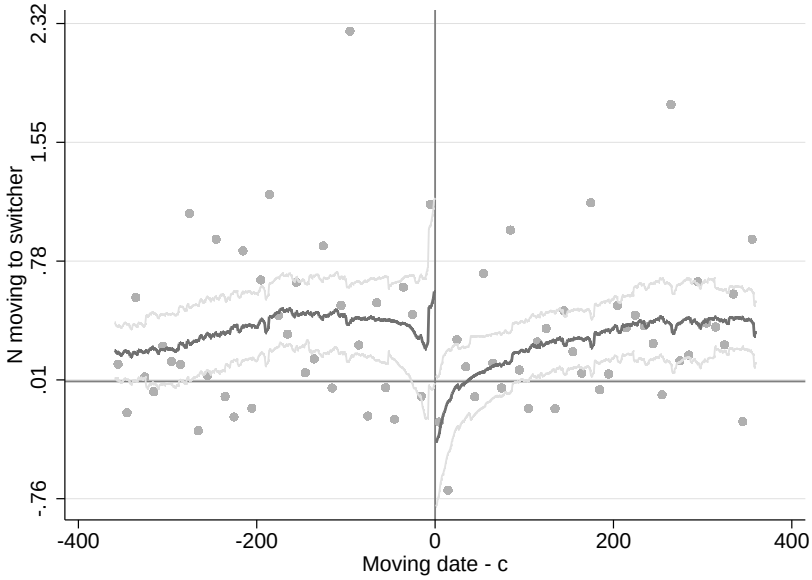


Figure E.11 Number of individuals moving to a switcher municipality in the pooled sample of cantons. Local linear smooth of the number of individuals who move to a switcher municipality over the moving date, separately from both sides of the threshold, using a bandwidth of 90 days. The light gray dots represent raw means within bins of 10 days.

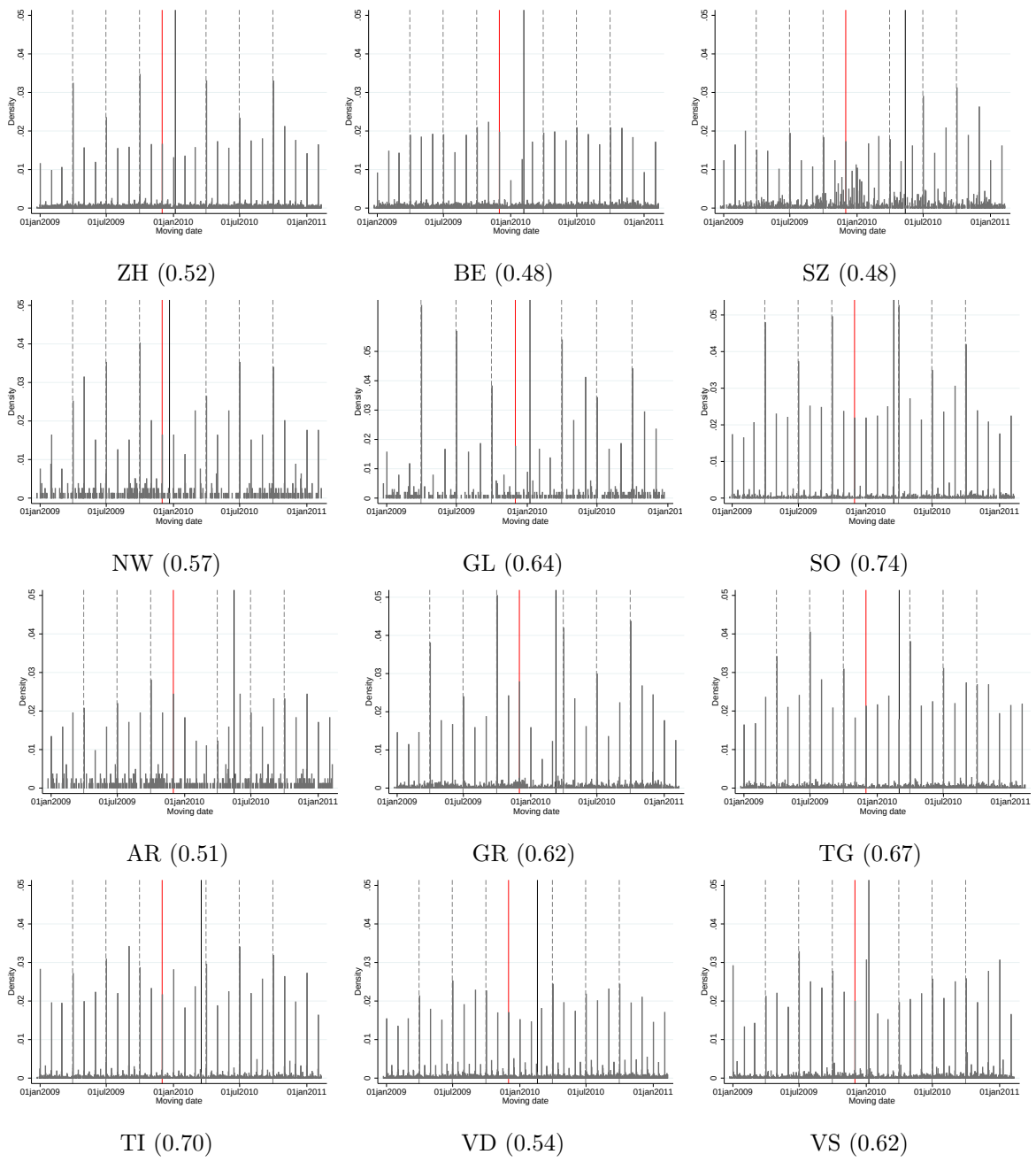


Figure E.12 Moving-pattern in 12 cantons for which a discontinuity was detected. This graph shows the histograms of moving dates in our estimation sample and for cantons in which the method detects a systematic discontinuity. The share in the parentheses after the canton abbreviation indicates the share of individuals moving at the last or the first day of the month. The date of the minaret initiative is marked by the red solid vertical line, the estimated threshold date by the black solid vertical line, and the typical moving dates (31 March, 30 June, and 30 September) by the gray dashed vertical lines.

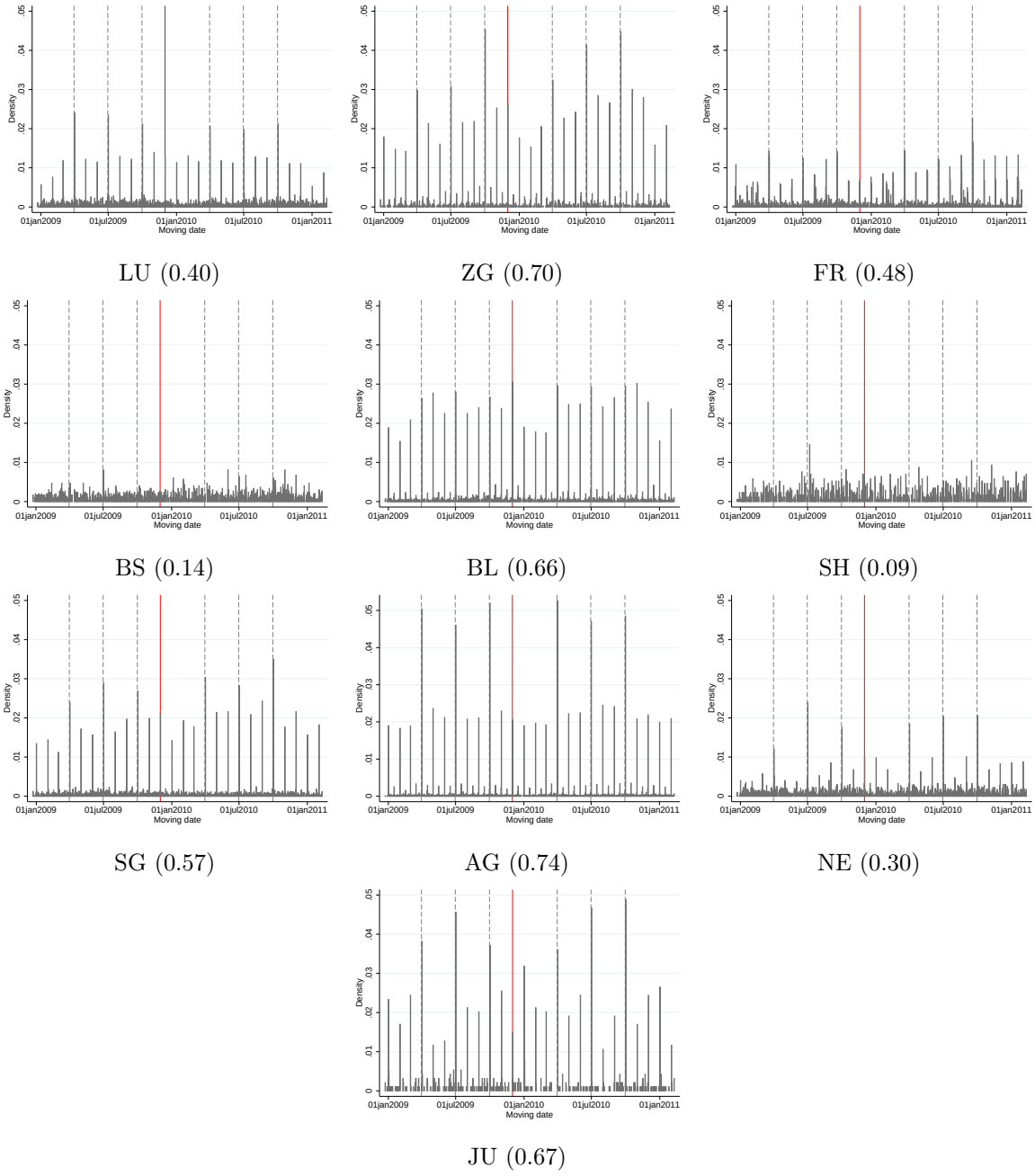


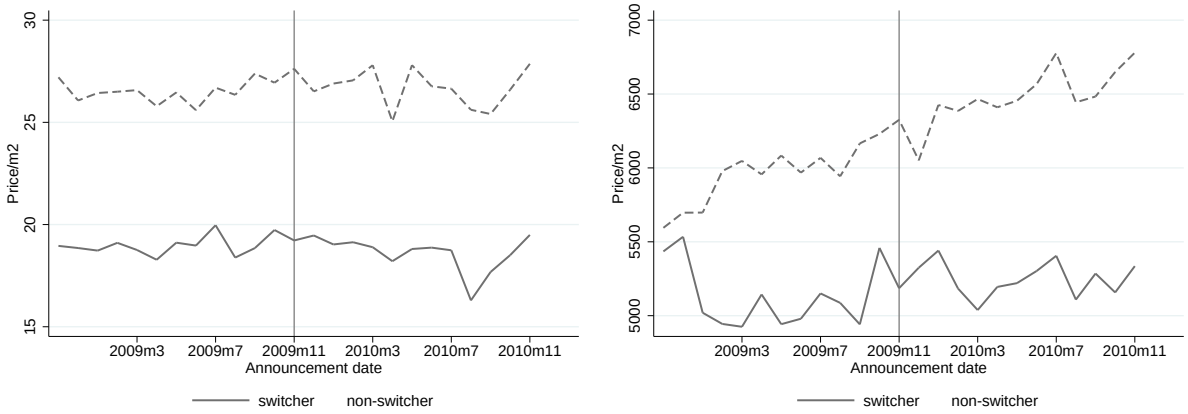
Figure E.13 Moving-pattern in 10 cantons for which no discontinuity was detected. This graph shows the histograms of moving dates in our estimation sample for cantons for which no systematic discontinuity was detected. The share in the parentheses after the canton abbreviation indicates the share of individuals moving at the last or the first day of the month. The date of the minaret initiative is marked by the red solid vertical line and the typical moving dates (31 March, 30 June, and 30 September) by the gray dashed vertical lines.

Table E.9 Development of announced housing prices in switcher municipalities after the vote

Dependent variable: Prices in Swiss Francs per m^2

	Rental Overall	Rental Cantons with discontinuity	Rental Cantons without discontinuity	Selling Overall	Selling Cantons with discontinuity	Selling Cantons without discontinuity
$Post_{30}$	–0.256	–0.326	–0.127	164.165	292.630*	58.036
	(0.181)	(0.275)	(0.164)	(113.750)	(151.217)	(153.638)
$Post_{60}$	0.670	0.312	1.028	168.799	573.462**	–193.682
	(0.478)	(0.635)	(0.598)	(190.575)	(244.266)	(234.200)
$Post_{90}$	0.373	–0.416	1.144*	76.054	526.348**	–327.995
	(0.667)	(0.779)	(0.526)	(193.501)	(202.643)	(281.329)
$Post_{120}$	0.702*	0.671	0.837	53.623	435.576	–287.920
	(0.394)	(0.624)	(0.494)	(248.235)	(254.310)	(382.660)
$Post_{150}$	–0.557	–1.360	0.245	77.123	400.031	–222.698
	(0.728)	(0.870)	(0.540)	(253.740)	(250.727)	(411.115)
$Post_{180}$	0.589	0.316	0.958	147.632	484.474	–170.629
	(0.540)	(0.751)	(0.599)	(256.009)	(295.717)	(407.314)
$Post_{210}$	0.559	0.125	1.053**	210.680	544.005*	–93.838
	(0.550)	(0.766)	(0.456)	(236.134)	(261.888)	(358.334)
$Post_{240}$	0.437	–0.056	0.952	224.783	678.366**	–275.280
	(0.556)	(0.700)	(0.586)	(285.593)	(283.655)	(430.179)
$Post_{>240}$	0.079	–0.500	0.697	85.801	443.324*	–229.747
	(0.611)	(0.762)	(0.545)	(226.690)	(234.343)	(362.431)
$Switch * post_{30}$	–0.007	–0.230	0.177	–66.802	–197.549	11.642
	(0.257)	(0.300)	(0.251)	(152.091)	(211.957)	(202.288)
$Switch * post_{60}$	–0.818**	–1.183***	–0.445	–226.937	–327.660	–223.532
	(0.340)	(0.336)	(0.355)	(164.973)	(247.702)	(151.280)
$Switch * post_{90}$	–0.226	–0.392	0.010	–260.725*	–476.821***	–75.101
	(0.209)	(0.255)	(0.246)	(134.579)	(122.383)	(184.265)
$Switch * post_{120}$	–0.624	–1.562***	0.268	–207.293**	–351.431**	–70.410
	(0.593)	(0.500)	(0.310)	(97.257)	(147.794)	(96.809)
$Switch * post_{150}$	0.275	0.584	–0.049	–193.704*	–285.968**	–79.669
	(0.262)	(0.359)	(0.410)	(94.483)	(106.670)	(158.219)
$Switch * post_{180}$	–0.470	–1.127***	0.154	–194.859**	–228.517	–228.255
	(0.412)	(0.343)	(0.176)	(78.974)	(127.500)	(142.476)
$Switch * post_{210}$	–0.789***	–1.118***	–0.523**	–284.153**	–395.400**	–184.505
	(0.262)	(0.243)	(0.221)	(123.615)	(178.271)	(143.849)
$Switch * post_{240}$	–0.406*	–0.611***	–0.262	–304.131**	–445.619**	–153.113
	(0.216)	(0.186)	(0.315)	(134.040)	(175.722)	(144.426)
$Switch * post_{>240}$	–1.009***	–1.408***	–0.577**	–258.177***	–364.187***	–189.018***
	(0.300)	(0.401)	(0.219)	(59.834)	(88.756)	(36.129)
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Day of month FE	Yes	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep.	21.61	23.64	19.76	5,174.85	5,325.35	4,970.69
No. of obs.	234,439	111,570	122,869	97,904	56,359	41,545
No. of clusters	22	12	10	22	12	10
R^2	0.37	0.35	0.27	0.46	0.49	0.40

Notes: Ordinary least squares estimates. The data is a collection of all housing advertisements on all major Swiss online housing platforms. Our sample consists of 332,343 rental and selling price announcements for apartments and houses with up to seven rooms between 2006 and 2010. The *Post* coefficients capture the time pattern of prices for the period after the vote on the initiative in the comparison municipalities. The coefficients of the $Switch * post_{\cdot}$ indicators measure the deviation in the time pattern of prices between treated and comparison municipalities. Standard errors are clustered at the cantonal level and reported in parentheses. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.



(a) Rental prices per month

(b) Selling prices

Figure E.14 Raw monthly means of rental and selling prices over time for cantons in which a discontinuity was detected. The dashed line represents the means for non-switcher municipalities in the comparison group and the solid line the treatment group of switcher municipalities. The solid vertical black line indicates the month of the vote.