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Anders Böhlmark
Alexander Willén

WORKING PAPER 2017:14

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ISSN 1651-1166

Tipping and the Effects of Segregation[1]

by

Anders Böhlmark[2] and Alexander Willén[3]

August 21, 2017

Abstract

We examine the effect of ethnic residential segregation on short- and long-term education and labor market outcomes of immigrants and natives. Our identification strategy builds on the one-sided tipping point model, which predicts that neighborhood native population growth drops discontinuously once the immigrant share exceeds a certain threshold. After having identified a statistically and economically significant discontinuity in native population growth at candidate tipping points in the three metropolitan areas of Sweden between 1990 and 2000, we show that these thresholds also are associated with a discontinuous jump in ethnic residential segregation. We exploit these thresholds to estimate the intent-to-treat effect of tipping. We find modest adverse education effects among both immigrants and natives. These effects do not carry over to the labor market.

Keywords: residential segregation, education, labor market, regression discontinuity
JEL-codes: J15, J16, R23

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[1] We are grateful to Matz Dahlberg, Maria Fitzpatrick, Helena Holmlund, Michael Lovenheim, Jordan Matsudaira, Zhuan Pei, Jesse Rothstein and Alex Solis as well as seminar participants at IFAU in Uppsala, Cornell University, SOFI, Stockholm University and the 2016 ZEW Workshop on Assimilation and Integration of Immigrants in Mannheim for valuable comments and suggestions on earlier drafts of this paper. We further thank David Card, Alexandre Mas and Jesse Rothstein for sharing their program codes. Alexander Willén gratefully acknowledges financial support from the Mario Einaudi Center at Cornell University and Dr. Tech. Marcus Wallenberg Foundation [2015-040]. Anders Böhlmark is grateful to the Swedish Research Council for Health, Working Life and Welfare (FORTE) [2013-064] for financial support.

[2] SOFI, Stockholm University, IFAU and CReAM, anders.bohlmark@sofi.su.se

[3] Department of Policy Analysis and Management, Cornell University, Ithaca, NY 14853, +1 (607) 229-7507, alw285@cornell.edu (corresponding author)

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1 Introduction

Ethnic and racial residential segregation are persistent features of society that generate considerable policy concern. These concerns stem from the potential for segregation to fuel an unequal allocation of resources and opportunities across space that leads to the development of parallel societies, poses a threat to social cohesion and may impede education and labor market performance. Despite a large theoretical literature discussing how segregation may affect individual outcomes, very little empirical work credibly addresses this question.

In this paper, we use detailed administrative data from Sweden to examine how ethnic residential segregation affects short- and long-term education and labor market outcomes of non-Western immigrants and natives. Over the past 60 years, Sweden has transformed from one of the world's most ethnically homogeneous countries to one where 22 percent of the population is either born abroad or has a foreign-born parent, making it an interesting case for the study of residential segregation (Statistics Sweden 2015).

The central challenge associated with empirical analysis on this topic is selection: Individuals are likely to sort across neighborhoods for reasons that are unobserved by the researcher but relevant as determinants of individual outcomes. Such nonrandom selection will lead to invalid inference in correlational studies since individuals in neighborhoods with different levels of segregation are not comparable even after adjusting for differences in observable characteristics. To overcome this problem, we borrow theoretical insight from the one-sided tipping point model formalized by Card et al. (2008). This model predicts that residential segregation can arise due to social interactions in native preferences: once the immigrant share in a neighborhood exceeds a critical tipping point, the neighborhood will be subject to both native flight and avoidance, causing a discontinuity in native population growth.[4, 5] This may occur due to, for example, natives seeking to minimize interaction with other-race residents (Massey and Denton 1998) or because they associate such areas with lower quality services, worse schools and higher crime rates (Krysan et al. 2008; Bayer et al 2007).

[4] Card et al. (2008) derive the one-sided tipping-point model from a theory of neighborhood choice by Becker and Murphy (2000). However, several alternative models of neighborhood choice suggest similar types of behavior (see Card et al. 2011). The first formal model on the tipping phenomenon is Schelling (1971).

[5] Here and throughout the paper, we define the growth rate of the native population in the same way as Card et al. (2008): the change in native population between 1990 and 2000 expressed as a fraction of total neighborhood population in 1990.

In a first step, we use administrative data from 1990 to 2000 to replicate the work of Card et al. (2008) in Sweden's metropolitan areas - Stockholm, Gothenburg and Malmo. This exercise uses a regression discontinuity design to examine if neighborhoods on opposite sides of the tipping point in 1990 experience significant differences in native population growth between 1990 and 2000. The candidate tipping point we use is the immigrant share at which neighborhood native population growth equals the average city-specific native growth. We use this point because the one-sided tipping point model predicts neighborhoods with immigrant shares below the threshold to experience a faster-than-average native population growth and neighborhoods above the threshold to experience a relative decline.

We find robust evidence that the dynamics of segregation in Sweden's metropolitan areas is characterized by tipping behavior. Specifically, we find that native population growth between 1990 and 2000 drops discontinuously by more than 16 percentage points among neighborhoods with immigrant shares just above 18 percent in 1990, with neighborhoods below the threshold experiencing faster-than-average native growth and neighborhoods above the threshold experiencing a relative decline. We extend this analysis and demonstrate that the tipping point is also associated with a large positive discontinuity in segregation, with an effect close to 30 percentage points. The tipping behavior we identify is driven exclusively by native aversion toward non-Western immigrants: the effects disappear when the model is re-estimated using Western immigrants.[6]

After having found support for the tipping phenomenon, we disaggregate the data to the individual-level and employ regression discontinuity models that compare later-in-life outcomes of individuals who resided in neighborhoods just above the threshold in 1990 to later-in-life outcomes of individuals who lived in neighborhoods right below the threshold in 1990. The intuition behind this approach is that individuals who resided in neighborhoods just above the threshold in 1990 should be very similar to individuals who resided in neighborhoods right below the threshold in 1990 on both observable and unobservable dimensions. However, individuals who lived in neighborhoods just above the threshold in 1990 will be exposed to tipping and to a very different change in population composition between 1990 and 2000 compared to individuals who lived in

[6] This is consistent with prior literature, which suggests that segregation in Sweden is isolated to that between non-Western immigrants and the rest (Le Grand and Szulkin 2003).

neighborhoods just below the threshold in 1990. Thus, we compare individuals who resided in comparable neighborhoods in 1990 but experienced vastly different changes in the ethnic population composition of their neighborhoods in the following decade due to very small initial differences in neighborhood immigrant shares.

It is important to emphasize that the change in ethnic population composition caused by the tipping phenomenon may generate changes on other neighborhood dimensions that also affect the outcomes; such as reducing the quality of services, or worsening the socioeconomic composition, of the affected neighborhoods. The reduced-form results produced by our estimation strategy does therefore not represent the effect of segregation holding all other factors constant, but the combined effect of segregation and everything else that may occur as a consequence of tipping. This is an interesting parameter from a policy perspective that captures the total effect of tipping, including neighborhood composition changes and changes in local services that typically accompany changes in segregation.

The source of variation we exploit comes from within city across neighborhood deviations in immigrant share from the tipping point in 1990. The main assumption we invoke is that treatment assignment is as good as random around the identified threshold, so that individuals in neighborhoods just below the threshold are comparable to individuals in neighborhoods just above the threshold in 1990. Though this assumption cannot be tested directly, the Swedish registry data allow us to provide extensive evidence consistent with the idea that there are no statistically significant differences in the characteristics of individuals in neighborhoods on either side of the tipping point in 1990, and that there are no discontinuities in other potential confounders at the threshold.

Our reduced-form estimates identify adverse effects of tipping on the educational attainment of natives. As a percentage of the control mean, we find that tipping causes a 4.2% reduction in national GPA percentile ranking at age 16 and a 5.3% reduction in the probability of pursuing university education.[7] These effects are mainly driven by males and individuals with low parental education. We find less consistent evidence with respect to immigrants, though similarly sized effects can be observed for immigrants of low socioeconomic status. Based on Fredriksson et al. (2013), we

[7] The control mean is defined as the average value of the outcome variable among individuals in neighborhoods just to the left of the threshold.

calculate that a class size reduction of 2-3 pupils in tipped neighborhoods is required to offset the effect of tipping on educational attainment. However, we find no evidence that the education effects carry over to the labor market: as a percentage of the control mean, we can rule out adverse employment earnings effects greater than 0.29% for natives and 0.60% for non-Western immigrants.

Our reduced-form estimates identify the average effect of residing in a neighborhood just above the tipping point in the base year on later-in-life outcomes. These intent-to-treat estimates capture the effect of tipping both on individuals who stay in tipped neighborhoods and on individuals who move out of these neighborhoods at some point during our analysis period. One concern with these estimates is that individuals who leave tipped neighborhoods before the outcomes are measured will be exposed to a lower treatment dose, and including these individuals in the treatment group may lead us to underestimate the average effect of tipping on individual outcomes. We examine this possibility through auxiliary analyses that restrict the sample to individuals who did not move during the analysis period. Stayers in tipped neighborhoods are on average more disadvantaged than movers and are exposed to a larger treatment dose, and even if the average effects of tipping are larger than our baseline estimates due to post-tipping migration from treated neighborhoods, they will be smaller than the effects identified for this subsample. With the exception of our baseline estimates for immigrants' educational outcomes, which increase in absolute magnitude, these results are similar to those using the full sample.

It is important to highlight that the discontinuity in segregation is identified at a margin where neighborhoods are just beginning to become segregated, and the results should not be used as evidence of the effects associated with residing in all-minority neighborhoods.[8] In Section 5 we show that very few areas can be categorized as fully segregated, and in Section 6 we demonstrate that the tipping points are very close to the mean immigrant share across the metropolitan areas. This is thus a margin that is relevant to many communities, and it is important to understand the consequences of segregation at this margin.

This is the first paper to estimate the effect of tipping on individual outcomes. It contributes to the literature in several important ways. First, we provide a novel solution

[8] Although the model anticipates neighborhoods above the tipping point to transform into all-minority neighborhoods, this does not occur during the ten-year period that we focus on.

to the identification issue caused by sorting across neighborhoods. The application of this approach is not limited to residential segregation and provides an interesting direction for future workplace and school segregation research. Second, this paper investigates segregation effects at a margin where neighborhoods are just beginning to become segregated, which has not been examined before. Given the scarcity of fully segregated neighborhoods this is a margin of great policy interest, and if individuals are negatively affected by segregation at this margin it may have far-reaching policy implications. Third, while previous literature has focused on segregation of African-Americans, non-white Hispanics and refugees, this paper looks at a more heterogeneous group – non-Western immigrants (O'Flaherty 2015).[9] Given the current migration crisis in Europe, this is a group of great policy interest. Fourth, our identification strategy permits an investigation of segregation effects among natives, something we know very little about. Finally, while most segregation research has been constrained to analyzing short- and medium term outcomes, the rich Swedish registry data enables us to follow individuals over time and investigate long-run effects.

The rest of this paper is organized as follows: Section 2 provides a brief background on residential segregation in Sweden and relates it to that in the US, Section 3 discusses previous research on the topic, Section 4 presents our empirical strategy, and Section 5 introduces the data. All results are shown in Sections 6 and 7, and Section 8 concludes.

2 Background

2.1 Ethnic Residential Segregation in Sweden

During the past 60 years, Sweden has transitioned from a homogeneous to a heterogeneous society with a substantial immigrant base. Foreign-born individuals as a share of the total population have increased from 2.8% in 1950 to 17% in 2015, and the number of non-Western foreign-born residents has increased more than twenty-fold over the same time period (Appendix Table A1). Currently, immigrants as a share of the total population in Sweden marginally exceeds that of the US, and many similarities can be drawn between the two countries. First, immigrants are spatially concentrated, and the probability of residing in an ethnic neighborhood in Sweden (0.42) is similar to that in

[9] Western immigrants are defined as individuals born in, or with at least one parent born in: Norway, Denmark, Finland, Iceland, Belgium, France, Ireland, Luxemburg, the Netherlands, Great Britain and Northern Ireland, Germany, Austria, Switzerland, Israel, the United States, Canada or Oceania.

the US (0.48) (Edin et al. 2003).[10] Second, both countries have experienced changing immigration patterns, from in-migration of Europeans to in-migration of individuals from less developed countries. An important implication of this pattern is that immigrants have become distinctly different from natives (Chiswick and Miller 2005). Third, both countries experience disparities across ethnic groups with respect to education and labor market outcomes. In Sweden, OECD estimates suggest that the immigrant-native labor market differential is one of the largest across all member states, and recent PISA results show a 0.8 standard deviation gap in the test score distribution between natives and immigrants in math, science and reading (Åslund et al. 2011).

There are also important differences between the US and Sweden: while there are several layers of ethnic and racial residential segregation in the US, both across nativity status and minority groups, segregation is restricted to that between non-Western immigrants and the rest in Sweden (Le Grand and Szulkin 2003). Further, there are major differences in source countries. While Sweden has a large inflow of immigrants from the Middle East and Europe, the US has large inflows from Central America, the Caribbean and Asia.[11] Finally, the share of refugees is much larger in Sweden.[12] Sweden is therefore often characterized as subject to push-migration rather than by the pull-migration present in the US.[13]

2.2 Costs and Benefits of Residential Segregation

A common finding in the literature is the existence of a correlation between a group's spatial position and socioeconomic well-being. This has motivated researchers to investigate the costs and benefits associated with residential segregation (Stark 1991; Cutler and Glaeser 1997; Borjas 1999; Edin et al. 2003; Cutler et al. 2008). The large theoretical literature within this field point to the existence of both negative and positive mechanisms, and the resulting predictions of the effects associated with segregation are therefore ambiguous.

[10] An ethnic neighborhood is a neighborhood in which the share of the neighborhood population with a specific ethnicity is at least twice as large as the share of the national population with that ethnicity. Note that the US probability is based on information from 1979, while the Swedish probability is based on data from 1997.
[11] In 2010, Sweden and the US did not share a single country on their top-10 source country lists. The US top-10 list consists of Mexico, Korea, India, Guatemala, El Salvador, Dominican Republic, Cuba, Vietnam, China and the Philippines (MPI, 2010). None of these countries was on the Swedish top-10 list (Table A1).
[12] In 2014, 0.15 percent (491,730) of the US population was made up of individuals who entered the country as refugees and asylum-seekers. In Sweden, this figure was 2.04 percent (198,342). See UNHCR (2015).
[13] See Zimmermann (1996) for a discussion of pull- and push-migration.

In terms of costs, existing literature suggests that ethnic residential segregation may negatively affect the desire to acquire host country specific human capital, such as language skills (Chiswick 1991; Lazear 1999). This may restrict immigrant job opportunities, in particular if the lack of such skills leads to a hesitation to explore jobs outside the neighborhood (Borjas 2000). Further, it could inhibit immigrant youth from advancing through the educational system at the same pace as natives due to inadequate proficiency in the language of instruction. Concurrently, native youth who live in neighborhoods with a high concentration of immigrants might be adversely affected if the resources at their local schools are directed toward aiding immigrants in acquiring language skills (Gould et al. 2009).

Residential segregation may also reduce the quality of public and private services, especially if such segregation is accompanied by an outflow of high-quality workers (Farley et al. 1994; Andersson 1998; Charles 2000). Given that the tipping phenomenon is driven mainly by native flight and avoidance, there could be sizable effects flowing through this channel, particularly if this behavior is isolated to natives of high socioeconomic status.[14]

Finally, evidence from the US suggests that neighborhoods with high ethnic concentration tend to be far removed from the suburban areas that experience job growth (Ihlanfeldt and Sjoquist 1998). According to the spatial mismatch hypothesis, the difficulty of expanding beyond neighborhood networks can cause adverse labor market effects by raising both job search and commuting costs (Kain 1968; Ihlanfeldt and Sjoquist 1998). Even though high quality transportation systems coupled with less rapid shifts in job opportunities to the suburbs make this theory less applicable to Western Europe, we are aware of no Swedish research on this hypothesis and can therefore not rule it out (Muster and Andersson 2006).

Although the majority of theories concerned with residential segregation predict adverse effects on immigrants, conventional social interaction models suggest that an expansion of ethnic networks may generate beneficial effects through two channels: information and norms (Bertrand et al. 2000). With respect to the former, the expansion of ethnic networks may facilitate the acquisition of important information pertaining to education, job opportunities and social welfare programs (Patacchini and Zenou 2012;

[14] However, the direction and magnitude of the effect flowing through this channel is subject to some uncertainty, since increased segregation may also benefit and attract businesses that target immigrants.

Munshi 2003). With regard to the latter, norms may improve immigrant outcomes through the transmission and sharing of work ethics and attitudes towards welfare (Borjas 1995; Glaeser, Sacerdote, and Scheinkman 1996; Bertrand et al. 2000; Åslund and Fredriksson 2009).[15]

In addition to the mechanisms discussed above, existing research suggests that increased segregation may prolong the assimilation process, and that there thus may be treatment heterogeneity by group characteristics (Cutler et al. 2008). Specifically, if immigrants separated from majority neighborhoods revert to the native mean more slowly, then immigrants with worse labor market and educational attainment characteristics than natives may suffer while immigrants with better characteristics may benefit. Several papers have examined this hypothesis with respect to education- and skill-level, and the results are consistent with this hypothesis (Borjas 1999; Edin et al. 2003; Cutler et al. 2008).

The above discussion demonstrates that the net effect of residential segregation is difficult to predict. This ambiguity is augmented by the fact that the benefits are immediate in nature while the costs have both short- and long-term elements. Further, the theories above assume much greater segregation than that present at the tipping margin, and the extent to which they apply to tipping phenomenon is unknown. In addition, tipping may fuel changes in the quality of services and in the socioeconomic composition of the neighborhood that we cannot observe but that also impact the outcomes that we examine. These ambiguities underscore the importance of an empirical investigation on how tipping affects outcomes.

3 Prior Empirical Research

Research on residential segregation falls within the literature on neighborhood effects, and the central challenge associated with analyzing such effects concerns selective sorting across neighborhoods. Researchers have tried to overcome this problem using several identification strategies, ranging from randomized control trials (Katz et al. 2001; Kling et al. 2007; Chetty et al. 2015) and quasi-experiments (Jacob 2004) to propensity score matching (Harding 2003) and the use of instrumental variables (Cutler

[15] It is not clear that the effects flowing through these channels must be positive. Specifically, beneficial effects would exist only if the information (norm) benefit of expanded ethnic networks outweighs the information (norm) loss associated with a reduction in exposure to the native population.

and Glaeser 1997).[16] The non-monolithic nature of neighborhood effects has led to substantial heterogeneity in results across these studies, and no consensus has been reached on how neighborhoods affect individual outcomes (Cutler et al. 2008). Within this field of research, residential segregation has been one of the most popular subjects to examine, and this literature follows four distinct lines.

The first strand attempts to solve the endogeneity issue through aggregation to the city level (Cutler and Glaeser 1997; Collins and Margo 2000; Card and Rothstein 2007; Cutler et al. 2008; Quillian 2014). This approach is based on the assumption that neighborhood choice is endogenous to individual outcomes, but city choice is not. If correct, one can overcome the endogeneity bias by using cross-city differences in segregation as identifying variation. However, this assumption does not align with empirical evidence on migration patterns (Chiswick and Miller 2004), and several researchers have complemented this approach with additional empirical methods. For example, Cutler et al. (2008) constrain their analysis to the effect of location early in life on adult outcomes, exploit instrumental variable strategies and use fixed effects models.[17] Results from this strand are mostly negative, though some papers find mixed results (Collins and Margo 2000; Cutler et al. 2008).[18]

A second strategy limits the analysis to the effect of residential segregation early in life on adult outcomes (e.g. Cutler and Glaeser 1997; Borjas 1995; Cutler et al. 2008).[19] The assumption underlying this method is that parents choose place of residency, and if that choice is uncorrelated with unobserved characteristics that affect the children's adult outcomes, parental neighborhood choice can be used to estimate the effect of segregation among children. Although estimates using this approach suggest that immigrants are adversely affected by segregation, it is likely that parental residential

[16] Some of the most credible neighborhood effect estimates are derived from the Moving to Opportunity Experiment, in which families in public housing were assigned housing vouchers through a lottery, encouraging moves to areas with lower poverty rates (Sanbonmatsu et al. 2007). Unfortunately, the MTO design makes it impossible to isolate racial segregation effects from economic segregation effects.

[17] The fixed-effects analysis uses country-of-origin and MSA fixed effects to compare outcomes between groups that are more or less segregated within a city relative to their own group-level averages. Their IV analysis uses mean years since migration for group members within a MSA as an instrument for segregation. Though informative, it is important to note that the authors do not look at the effect of segregation on natives, and they only focus on individuals between the ages of 20 and 30. Our paper addresses both of these limitations.

[18] Cutler et al. (2008) find heterogeneous effects on the skill dimension, with individuals at the bottom of the skill distribution suffering negative effects and those in the right-tail of the distribution benefitting.

[19] Borjas (1995) estimates the effect of ethnic externalities and neighborhood effects in the intergenerational transmission process and thus focuses on questions distinct from the ones that we investigate in this paper.

choice is driven in part by unobserved family characteristics that also affect the offspring's adult outcomes.

The third attempt to overcome the endogeneity problem has been to exploit spatial dispersal policies on refugees and asylum-seekers that generate plausibly exogenous variation in initial residential location. These policies allocate newly arrived refugees to districts based on certain observable characteristics, and if this allocation is random with respect to unobserved characteristics that also affect the outcomes, these policies can be used to estimate causal segregation effects. However, existing spatial dispersal studies have mainly focused on examining the effects of residing in an area with individuals from the same source country, as an analysis on the broader policy issue of residential segregation would require a stronger set of assumptions (Edin et al. 2003; Damm 2009; Åslund et al. 2011; Beaman 2012).[20] With the exception of Beaman (2012), these studies suggest that ethnic enclave size has a positive effect on educational and labor market outcomes.[21] Grönqvist et al. (2016) is the only paper to use these policies to examine the effect of growing up in a neighborhood with a high concentration of immigrants and finds that increased exposure leads to an increase in crime. Unfortunately, this method is restricted to looking at refugees and asylum-seekers, and the results cannot be generalized to the wider non-Western immigrant population. Further, this approach does not permit an investigation of the effect of residential segregation on natives.

In addition to these three strands of literature, Ananat (2011) attempts to overcome selection through a novel identification strategy that instruments African-American residential segregation in the 20th century using 19th century railroad configurations.[22] The results suggest that black residential segregation reduces human capital accumulation among blacks and reduces human capital inequality among whites. Unfortunately, this method is necessarily restricted to looking at black-white segregation, and the results cannot be used to infer the likely effects associated with ethnic residential segregation of non-Western immigrants.

[20] See Åslund et al. (2011) for a discussion.

[21] Åslund et al. (2011) and Beaman (2012) further find substantial heterogeneity in treatment effects: Åslund et al. (2011) find the positive effects to increase in the number of highly educated adults of the same ethnicity, and Beaman (2012) find that tenured co-nationals improve employment prospects and increase wages.

[22] Cities that were subdivided by railroads into a greater number of neighborhoods in the 19th century became more segregated during the great migration of the 20th century.

Our study is the first to estimate the effect of tipping on individual outcomes. However, a number of studies have performed the first part of our estimation procedure, investigating discontinuities in neighborhood population composition around candidate tipping points (Card et al. 2008; Card et al. 2011; Easterly 2009; Aldén et al. 2015; Ong 2015).[23] With the exception of Easterly (2009) that relies on a method distinct from that used by Card et al. (2008), these studies have found evidence in favor of the tipping phenomenon both in Sweden and the United States.[24, 25] While there is value in examining the validity of the one-sided tipping point model, the importance of these studies is ultimately contingent on the consequences of this phenomenon on individual outcomes, which is the focus of our paper.

4 Empirical Methodology

The first part of our analysis extends the work of Card et al. (2008) to Sweden's three metropolitan areas. This analysis builds on the one-sided tipping point model, and a formal derivation of the empirically testable implications of this model is available in Card et al. (2008).[26] To understand our empirical method it suffices to know that the model predicts segregation to arise due to social interactions in native preference: once the immigrant share in a neighborhood exceeds a critical point, the neighborhood will experience both native flight and avoidance, causing a discontinuity in native population growth in the neighborhood. The implication of this prediction is that native population growth can be modeled as a smooth function of the immigrant share, except at the tipping point.

[23] The tipping point literature is not isolated to looking at residential segregation. For example, Pan (2015) uses the same model to look at the dynamics of gender discrimination in the workplace.

[24] Looking at Malmo, Gothenburg and Stockholm, as well as 9 smaller cities, Aldén et al. (2015) find support for the tipping phenomenon in Sweden. However, their results cannot be compared to ours: they do not include children younger than 16 years old, do not account for second-generation immigrants and use a different definition of immigrants (individuals born outside Europe). Finally, they estimate tipping points using a method that has a tendency to identify tipping points off of outliers (Card et al. 2008), particularly in smaller cities.

[25] Using census-tract data for US metropolitan areas from 1970 to 2000, Easterly (2009) finds that white flight is more pronounced in neighborhoods with a high initial share of whites. To the best of our knowledge, Ong (2015) is the only paper that has examined this question outside of Sweden and the US, and the author fails to find support for the tipping phenomenon in the Netherlands.

[26] The one-sided tipping point model is an alternative to the original model outlined by Schelling (1971). Schelling argues that integrated neighborhoods are inherently unstable and that social interactions in preferences will generate a completely segregated equilibrium. This can be seen as a two-sided tipping point model in which small changes in neighborhood ethnic composition will generate either white flight or minority flight. Card et al. (2011) compares the two models and finds that the one-sided tipping point model fits the data better. Specifically, their results show that neighborhoods with immigrant shares below the tipping point are relatively stable while neighborhoods above the identified tipping points are subject to significant white flight.

4.1 Identifying the Location of the Tipping Points

We follow Card et al. (2008) and assume that the tipping point is city- and decade-specific, and focus on decadal change in ethnic composition between 1990 and 2000.[27] To identify the location of the tipping point, we note that neighborhoods with immigrant shares below the tipping point should experience a faster-than-average native growth while neighborhoods above the threshold should experience a relative decline. One possible tipping point value is therefore the immigrant share at which neighborhood native population growth equals the average city-specific growth rate (Card et al. 2008).

To identify this point, we fit the difference between the neighborhood's decadal native growth rate and the city's mean growth rate of natives to a quartic polynomial in neighborhood base year immigrant share, measured as the fraction of non-Western first and second generation immigrants in the neighborhood.[28] As global polynomial models are sensitive to outliers, we restrict the analysis to neighborhoods with less than a 60% immigrant share:[29]

$$Dn_{sm,2000} - Dn_{m,2000} = f\left(i_{sm,1990}\right) + \varepsilon_{sm,2000} \quad (1)$$

where $Dn_{sm,2000} = \frac{N_{sm,2000} - N_{sm,1990}}{P_{sm,1990}}$ and denotes the change in native population N in neighborhood *s* and metropolitan area *m* between *1990* and *2000,* measured as a fraction of total population P. *f()* is a quartic polynomial in base year neighborhood immigrant share *(i)* and $\varepsilon_{sm,2000}$ is the error term. The root of this polynomial satisfies the tipping condition: that $Dn_{sm,2000} - Dn_{m,2000} = 0$. This root is our candidate tipping point.[30] Appendix Figure A1 illustrates how the location of the tipping point is derived based on equation (1) for a hypothetical city.

[27] 1990 is the first year for which we have all the data necessary for our analysis.

[28] We focus on non-Western immigrants as Western immigrants are not visible minorities and do well on the Swedish labor market (Le Grand and Szulkin 2003). Thus, it is unlikely that increases in Western immigrant shares cause native flight. We provide empirical support for this assertion in section 6.

[29] The 60% immigrant share restriction is identical to that in Card et al. (2008) and is chosen based on visual inspection of the data to prevent outliers from affecting the identification of the tipping points. However, our results are not significantly affected by changing this restriction to 50% or 70%.

[30] In the event of several roots, we follow Card et al. (2008) and pick the one with the most negative slope. To ensure consistency with Card et al. (2008), we treat this as a two-step procedure. After we identify a candidate tipping point (CTP), we repeat the procedure using only neighborhoods with $abs\left(i_{sm,1990} - CTP\right) < 10$ to zero-in on the true tipping point.

4.2 Estimating the Magnitude of the Discontinuity

To determine if there is a sufficient discontinuity in the decadal growth of neighborhood native population at the threshold to consider it a genuine tipping point, a replication of Card et al. (2008) requires that we estimate the following model:

$$Dn_{sm,2000} = f\left(i_{sm,1990} - i^{*}_{m,1990}\right) + d_m \mathbf{1}\left[i_{sm,1990} > i^{*}_{m,1990}\right] + \tau_m + X_{sm,1990}\beta + \varepsilon_{sm,2000} \quad (2)$$

where *f()* is a quartic polynomial, $i_{sm,1990} - i^{*}_{m,1990}$ is the relative distance between a neighborhood's immigrant share and the identified metropolitan-common tipping point in the base year, $d_m \mathbf{1}[i_{sm,1990} > i^{*}_{m,1990}]$ is an indicator equal to one if the neighborhood had an immigrant share greater than the tipping point in the base year, X is a vector of neighborhood covariates and τ_m are metropolitan fixed-effects.[31] $d_m \mathbf{1}[i_{sm,1990} > i^{*}_{m,1990}]$ is the variable of interest, and d_m captures the change in native population growth between 1990 and 2000 caused by having an immigrant share greater than the tipping point in 1990.

Our preferred model specification deviates from this econometric framework in two important ways. First, the dichotomous treatment variable used by Card et al. (2008) generates attenuation bias due to the presence of crossovers. Specifically, they analyze decadal change in neighborhood native population based on the neighborhood's distance to the tipping point in the base year.[32] However, a regular inflow of immigrants to control neighborhoods will cause control neighborhoods close to the tipping-point to move beyond the threshold later in the decade. To limit the extent of dilution caused by these crossovers, we convert the treatment dummy into a partial exposure index $Q_{sm} = \frac{2000 - Year\ of\ Tipping_{sm}}{10}$, where $Year\ of\ Tipping_{sm}$ represents the year in which the immigrant share in neighborhood *s* and municipality *m* exceeds the tipping point estimated for the base year ($i^{*}_{m,1990}$). Thus, Q_{sm} ranges from 0 to 1 in 0.1 intervals and represents the fraction of the decade since tipping. Neighborhoods that did not tip during the decade are assigned 0. As a consequence of this model adjustment, 62 neighborhoods coded as untreated in equation (2) are now partially treated (Table A2). It is worth noting that we have performed this analysis using three alternative models

[31] Covariates are not necessary in a regression discontinuity framework. However, they can reduce the sampling variability and improve precision (Lee and Lemieux 2010).

[32] Card et al. (2008) are unable to account for crossovers due to their reliance on the decennial census.

that account for crossovers in different ways.[33] The difference in coefficient estimates is not statistically significant across these models.

Second, we directly investigate if the tipping points are associated with discontinuities in ethnic segregation by using a segregation index as our dependent variable. The index we use is based on the overexposure index (OE) of Åslund and Nordström Skans (2010):

$$\mathrm{D}OE_{sm,2000} = \left[\frac{(i_{sm,2000} - i_{m,2000})}{i_{m,2000}}\right] - \left[\frac{(i_{sm,1990} - i_{m,1990})}{i_{m,1990}}\right] \quad (3)$$

The first term on the right-hand side calculates the deviation in neighborhood immigrant share from city immigrant share as a fraction of city immigrant share in 2000, and the second term performs the same calculation for 1990. Each term represent the extent of neighborhood overexposure to immigrants, and the overall index measures the change in overexposure between 1990 and 2000.[34] The equation we use to examine if the identified tipping points are associated with a discontinuity in residential segregation is

$$\Delta OE_{sm,2000} = f\left(i_{sm,1990} - i^{*}_{m,1990}\right) + \varphi Q_{sm} + \tau_m + X_{sm,1990}\beta + \varepsilon_{sm,2000} \quad (4)$$

We follow Card and Lee (2008) and cluster the standard errors on distinct values of the running variable. It is important to note that equation (4) represents our baseline model, and we estimate several modified versions of this equation to examine the robustness of our results. First, equation (4) restricts the running variable coefficients to be the same on both sides of the threshold, and we also report results from more flexible models that allow the control function to differ on each side of the tipping point. Second, although the global polynomial approach offers greater precision than the nonparametric approach, it is difficult to identify the correct functional form. In addition to examining the sensitivity of our results to alternative polynomial specifications, we also report

[33] First, we omit all crossovers from the sample. Second, we assign all neighborhoods that moved beyond the tipping point pre-1995 to the treatment group. Third, we omit pre-1995 crossovers from the analysis. However, we prefer the fractional treatment model as it is the most comprehensive one and does not force us to throw out any of the observations. As can be seen in Table A3, the difference in coefficient estimates is not statistically significantly different across these models. Note that the latter two modifications are based on the belief that neighborhoods subject to tipping post-1995 will only cause a small bias as there is not sufficient time for these neighborhoods to experience a substantial change in the growth rate of the natives. This hypothesis is confirmed in Table A3, as the difference in the coefficient estimates between these two models and the one that omits all crossovers is not economically or statistically different.

[34] More conventional indices (e.g. the isolation index and the dissimilarity index) measure the degree of segregation across neighborhoods in a given city and do not generate within-city variation in segregation. These can therefore not be used for the purpose of our study.

results using local linear regressions. Third, we acknowledge that the location of the tipping point may be subject to measurement error which makes it harder to detect an effect, and we therefore complement our baseline analysis with donut-style regression discontinuity models that allow tipping to occur within a small range around the threshold rather than exactly at that point. Finally, we estimate equation (2) to shed light on the potential attenuation bias present in Card et al. (2008).

A random 2/3 of neighborhoods within each metropolitan area is used for the data-intensive process of identifying the location of the tipping points via equation (1). To estimate the magnitude of the discontinuities and determine if the identified thresholds represent genuine tipping points, we rely on the 1/3 of neighborhoods within each metropolitan area not used to identify the location of the threshold.[35] This split-sample procedure is used due to specification search bias – the magnitude of the discontinuity will have a non-standard distribution under the null hypothesis of no structural break if the same sample is used to identify the tipping point and estimate the discontinuity (Card et al. 2008; Leamer 1978). As a consequence, conventional test statistics will reject the null hypothesis of no discontinuity too often. Using two random subsamples means that the samples are independent and will have a standard distribution even under the stated null hypothesis (Card et al. 2008).

4.3 The Effect of Tipping on Individual Outcomes

After having found support for the tipping phenomenon in the metropolitan areas of Sweden between 1990 and 2000, we disaggregate the data to the individual level and exploit the identified tipping points to estimate the intent-to-treat effect of tipping on key education and labor market outcomes. We perform this analysis separately for non-Western immigrants and natives from three different age groups: those born 1980-1990, those starting school between 1980 and 1990, and those who have completed their education between 1980 and 1990 (born 1948-1958). Our decision to perform cohort-specific analyses is guided by Chetty et al. (2015), who show that there may be substantial birth cohort heterogeneity with respect to neighborhood effects. Using the same approach as in equation (4), we estimate the following reduced-form model:

[35] We restrict attention to the three largest cities in part because they are the only metropolitan areas in Sweden, and in part due to power concerns. The ten largest cities excluded from our sample have an average of less than 70 neighborhoods. We would therefore have less than 50 neighborhoods to identify thresholds from, and less than 24 neighborhoods to use for identifying the magnitude of the discontinuity in these areas.

$$Y_{rsm,t} = f\left(i_{sm,1990} - i^{*}_{m,1990}\right) + \varphi Q_{sm} + \tau_m + \partial_r + X_{rsm,1990}\beta + \varepsilon_{rsm,t} \tag{5}$$

where $Y_{rsm,t}$ is an outcome at time t for resident r that lived in neighborhood s in metropolitan area m in 1990, X is a vector of individual-level covariates and ∂ are birth year fixed effects. The variable of interest is Q_{sm}, and φ captures the intent-to-treat effect of tipping.

The intuition behind this approach is that individuals in neighborhoods just above the threshold should be very similar to individuals right below the threshold in 1990. However, individuals in neighborhoods above the threshold will be subject to a tipped neighborhood and therefor to a very different change in population composition between 1990 and 2000 compared to individuals who lived in neighborhoods just below the threshold in 1990. Thus, we compare individuals who resided in comparable neighborhoods in 1990 but experienced vastly different changes in the population composition of their neighborhoods in the following decade due to very small initial differences in neighborhood immigrant shares.

The source of variation we exploit comes from within city across neighborhood deviations in immigrant share from the tipping point in 1990. The main assumption we invoke is that treatment assignment is as good as random around the identified threshold, so that individuals in neighborhoods just below the threshold are comparable to individuals in neighborhoods just above the threshold in 1990. Though this assumption cannot be tested directly, the Swedish registry data enable us to provide extensive evidence consistent with the idea that there are no statistically significant differences in the characteristics of individuals in neighborhoods on either side of the tipping point in 1990, and that there are no discontinuities in other potential confounders at the threshold.[36]

In addition to our main assumption, the validity of our estimation strategy also requires that the tipping points are correctly estimated, that there are no coincidental shocks affecting neighborhoods once they hit the tipping point that also affect the outcomes of interest and that the functional form used to model the relationship between the conditional mean of the outcome and running variable is correctly specified. In

[36] The results in Altonji and Mansfield (2014) suggest that controlling for group averages of observed individual characteristics can absorb all across-group variation in unobserved individual characteristics. That neighborhoods on either side of the threshold are not statistically significantly different from each other is therefore sufficient for showing that individuals are not systematically different on either side of the tipping point.

Section 4, we report results from several robustness and diagnostic tests that show that our data are consistent with these assumptions.

5 Data

We rely on detailed administrative data drawn from four registries of the IFAU database, originally collected by Statistics Sweden. The first registry is *LOUISE,* which contains annual socioeconomic and demographic information on all residents between the ages of 16 and 65. The data also contain information on the number of children below the age of 16 in every household, allowing us to incorporate children into the estimation of tipping points. To examine if the tipping points are associated with increased segregation we use data from 1990 to 2000, focusing on decadal change in ethnic composition. To evaluate if tipping impacts individual outcomes we append LOUISE data from 2001 to 2011, allowing us to investigate long-run effects of the tipping phenomenon.

The ability to follow individuals over time is crucial to our analysis, as our estimation strategy requires knowledge of each individual's residential history as well as his labor market and education progression. The demanding data requirement of this empirical strategy is one of the reasons why this analysis has not been performed in the past. Another crucial data component is the neighborhood classification system (*SAMS).* *SAMS* is the most detailed geographic division in Sweden and divides Sweden into municipality-confined blocks, with a mean size of 1,000 individuals, and represents a finer level of geographic division than that used by Card et al. (2008).

The socioeconomic information in LOUISE includes education, labor market, income and welfare program participation. We use all of these as outcome measures to estimate the effect of tipping. We supplement these measures with data from the *Grade 9 Registry* and the *High School Registry*, which provide information on the academic performance of all individuals at the compulsory and high school levels.

The fourth registry we use is *The 2009 Multigenerational Registry (FLERGEN),* which links all individuals born after 1931 that resided in Sweden at some point after 1961 to their family members. We use these data for two purposes. First, by linking Swedish-born individuals in LOUISE to their parents via FLERGEN, we are able to account for second-generation immigrants. Second, by linking individuals from the

1975-1990 cohorts to their parents via FLERGEN, we can use the parental characteristics of the children to identify where they lived in the years before they turned 16.

Consistent with prior literature, we impose three sample restrictions. First, we exclude neighborhoods with growth rates five standard deviations above that of the city, as there may be coincidental secular trends that bias the estimates in these areas (Card et al. 2008). Second, we exclude neighborhoods with less than 200 residents, as a small change in the number of immigrants can cause tipping in these areas (Aldén et al. 2015). Finally, we drop neighborhoods that only exist for part of the decade. These restrictions reduce our sample by 886 neighborhoods, 762 of which have less than 200 residents.[37] Our final data set consists of 1,560 neighborhoods in the 3 metropolitan areas of Sweden. About 85% of the populations in these cities are included in our sample.[38]

Table 1 provides a breakdown of total and native growth rates stratified by baseline immigrant share. Most neighborhoods have between 5 and 40 percent immigrants in 1990. This stands in contrast to the US, which had larger shares of neighborhoods in the tails of the immigrant share distribution (Card et al. 2008). This suggests that there is a greater degree of residential segregation in the US, or that the recent influx of immigrants to Sweden had not yet led to the type of segregation predicted by the tipping model. That the percent of neighborhoods with more than 40% immigrants doubles between 1990 and 2000 provides suggestive evidence in favor of the latter explanation.

Table 2 provides summary statistics of the individuals used for the second part of our analysis, stratified by birth cohort. Columns 1-3 provide descriptive statistics of all individuals in Sweden, columns 4-6 provide the same statistics for all individuals in the three metropolitan areas, and columns 7-9 show the characteristics of the individuals in the three cities after our sample restrictions have been imposed. The characteristics of the individuals included in our sample closely mirror the average characteristics of the three cities, and there are no statistically significant differences between the two groups.

[37] Card et al. (2008) also drop neighborhoods with ten-year native growth rates in excess of 500 percent of the total base-year population. However, this restriction does not lead us to drop any additional neighborhoods.

[38] With the exception of the growth rate variables, the excluded neighborhoods are not statistically significantly different from those included in the sample (Appendix Table A15). The difference in growth rates between the included and excluded neighborhoods is likely driven by the relatively small size of the excluded neighborhoods, as the addition of one more individual can have a substantial effect on those measures. Including these neighborhoods would greatly increase the risk of identifying tipping points based on outliers.

Thus, our final sample is strongly representative of the population in the three metropolitan areas of Sweden.[39]

Table 1: Summary statistics of neighborhoods in sample

	1990	2000
Panel A: 0-5% Immigrants		
Percent Neighborhoods	2.10	1.98
Native Growth, t-10 to t as % of t-10 population	5.53 (19.12)	2.25 (19.76)
Total Growth, t-10 to t	7.43 (19.86)	5.69 (21.72)
Panel B: 5-20% Immigrants		
Percent Neighborhoods	73.03	65.06
Native Growth, t-10 to t as % of t-10 population	12.54 (28.10)	7.70 (26.81)
Total Growth, t-10 to t	16.42 (31.17)	12.64 (32.40)
Panel C: 20-40% Immigrants		
Percent Neighborhoods	19.15	21.42
Native Growth, t-10 to t as % of t-10 population	0.27 (34.37)	-2.18 (21.99)
Total Growth, t-10 to t	14.07 (41.73)	11.35 (25.98)
Panel D: 40-100% Immigrants		
Percent Neighborhoods	5.72	11.54
Native Growth, t-10 to t as % of t-10 population	-12.95 (15.92)	-6.21 (15.36)
Total Growth, t-10 to t	7.51 (19.08)	12.26 (23.59)

Notes: Authors' own calculations. Values based on unweighted means across cities.

[39] A concern with the data is that it fails to account for undocumented immigrants. However, recent estimates suggest the upper bound of undocumented immigrants in Sweden to be 35,000 during the time period that we analyze (SOU 2011). This represents 0.37% of the total population (or 1.02% of the immigrant population).

Table 2: Descriptive statistics of individuals in sample

	Whole Country			Analysis Municipalities			Analysis Sample		
	Mean	S.D.	Observations	Mean	SD	Observations	Mean	S.D.	Observations
Panel A: Young Cohort									
Female	0.488	0.500	1,408,511	0.486	0.500	299,094	0.480	0.500	233,091
Age	4.921	3.205	1,408,511	4.623	3.224	299,094	4.661	3.225	233,091
Mother's Education	11.530	2.142	1,126,038	11.802	2.299	287,365	11.769	2.280	225,416
Father's Education	11.623	2.333	1,103,778	12.030	2.530	278,958	11.977	2.503	218,015
Parental Income (000s SEK)	145.857	83.427	1,118,008	157.862	102.227	284,737	158.129	100.188	223,517
Panel B: Middle Cohort									
Female	0.489	0.500	1,443,459	0.486	0.500	271,548	0.486	0.500	220,671
Age	12.150	3.176	1,443,459	12.122	3.200	271,548	12.251	3.247	220,671
Mother's Education	11.447	2.242	1,074,293	11.786	2.388	255,682	11.749	2.374	210,925
Father's Education	11.573	2.450	1,039,277	12.034	2.639	242,993	11.983	2.619	199,122
Parental Income (000s SEK)	174.926	96.468	1,049,611	194.683	120.396	246,345	194.539	118.912	204,264
Panel C: Old Cohort									
Female	0.490	0.500	1,318,727	0.491	0.500	374,807	0.497	0.500	348,397
Age	37.117	3.191	1,318,727	36.983	3.205	374,807	36.989	3.203	348,397
Mother's Education	9.984	1.652	545,278	10.441	1.959	135,327	10.438	1.956	128,803
Father's Education	10.480	2.106	331,966	11.069	2.408	86,018	11.065	2.402	81,965
Parental Income (000s SEK)	109.049	93.030	297,356	131.592	109.491	76,644	131.897	109.180	73,098
Employed	0.894	0.308	1,318,727	0.873	0.333	374,807	0.889	0.314	348,397
Employment Income (000s SEK)	165.555	114.868	1,318,727	177.743	132.507	374,807	181.568	130.369	348,397
Social Insurance Benefits (000s SEK)	16.512	31.134	1,318,727	18.604	33.471	374,807	18.686	33.259	348,397
Compensation From the SIA (000s SEK)	23.608	37.976	1,318,727	24.971	39.279	374,807	24.888	38.934	348,397
Social Welfare Participation	0.069	0.254	1,318,727	0.086	0.281	374,807	0.082	0.275	348,397

Notes: The unit of observation is an individual. The first three columns display descriptive statistics using all individuals in Sweden, the second three columns display statistics using only those individuals that resided in Stockholm, Malmo and Gothenburg in 1990, and the last three columns display descriptive statics using only those individuals included in our analytical sample. Young Cohort refers to individuals born between 1980 and 1990, Middle Cohort refers to individuals that started school between 1980 and 1990, and Old Cohort refers to individuals born between 1948 and 1958.

6 Tipping Point Results

6.1 Baseline Estimates

Using a random 2/3 of neighborhoods within each metropolitan area, estimation of equation (1) identifies the unweighted mean tipping point across the three metropolitan areas to be located at an immigrant share of 17.94, with a standard deviation of 0.92. The tipping point is located at a slightly higher immigrant share in Gothenburg (18.99) than in Malmo (17.28) and Stockholm (17.55).[40] It is important to note that the mean tipping point is close to the mean immigrant share across the meteropolitan areas in 1990 (19.03). This phenomenon thus occurs at an immigrant share relevant to a large number of neighborhoods.[41]

By pooling the neighborhoods and normalizing the city-specific tipping points to zero, Figure 1 corresponds to Figure V of Card et al. (2008) and provides preliminary evidence of a discontinuity in neighborhood native population growth at the threshold.[42] Only the 1/3 of neighborhoods within each metropolitan area not used for estimating the location of the tipping points are used for this depiction. The dots show mean change in native population between 1990 and 2000, grouping neighborhoods into 2% bins by the deviation in immigrant share from the tipping point in 1990. The horizontal line is the unconditional mean, and the vertical line represents the normalized tipping point. The solid line is a local linear regression fit separately on either side of the threshold weighted by the size of the neighborhoods, using an Epanechnikov kernel and a bandwidth of 4.[43]

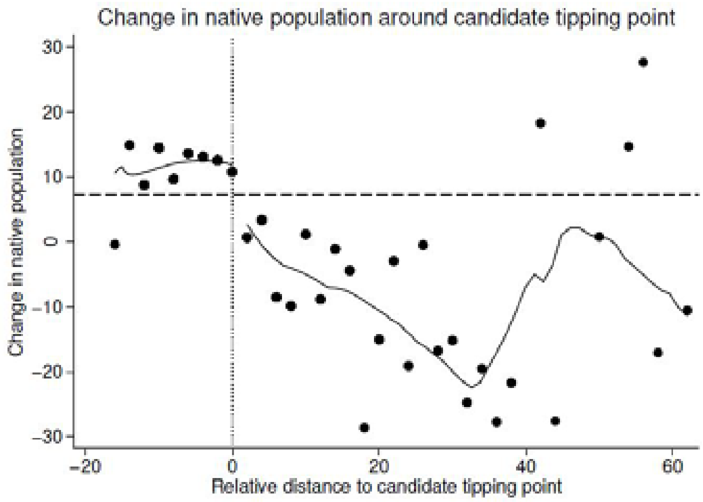
[40] We have also estimated tipping points using the structural break method described in Card et al. (2008). See Appendix A for a description of this method. Although this method is very susceptible to outliers in smaller cities, it produces results consistent with our main findings: the mean tipping point is 19.90, with a standard deviation of 1.96.

[41] This conclusion is contingent on the shape of the density of the fraction non-Western immigrants in the base year. For example, if this density is bimodal, this assertion would not follow. However, Figure 4 shows that this is not the case.

[42] Figure V of Card et al. (2008) restricts the sample to [-30, 30] of the running variable. We do not impose this restriction, and in this regard our Figure 1 corresponds to Figures I and IV in Card et al. (2008).

[43] The bandwidth has been chosen based on visual inspection of the data, and the result is robust to changes in both bandwidth and polynomial order (Figure A19).

Figure 1: Discontinuity in native population growth around candidate tipping point



Notes: Dots show mean change in neighborhood native population between 1990 and 2000 as a percentage of total neighborhood population in 1990, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point in 1990. The dashed horizontal line represents the unconditional mean, and the dotted vertical line depicts the estimated tipping point (normalized to zero). The solid line is a local linear regressions fit separately on either side of the tipping point weighted by the size of the neighborhoods, using an Epanechnikov kernel and a bandwidth of 4. Only the 1/3 of the sample not used for identifying the location of the tipping points is used for this visual depiction.

Figure 1 provides strong evidence of a negative discontinuity in native population growth at the tipping point, with neighborhoods below the threshold experiencing above-average native growth and neighborhoods above the cutoff experiencing below-average native growth. The discontinuity at the tipping point is approximately 10 percentage points. The positive and flat slope of the local linear regression fit to the left of the tipping point coupled with the u-shaped fit to the right of the threshold is consistent with Card et al. (2008).

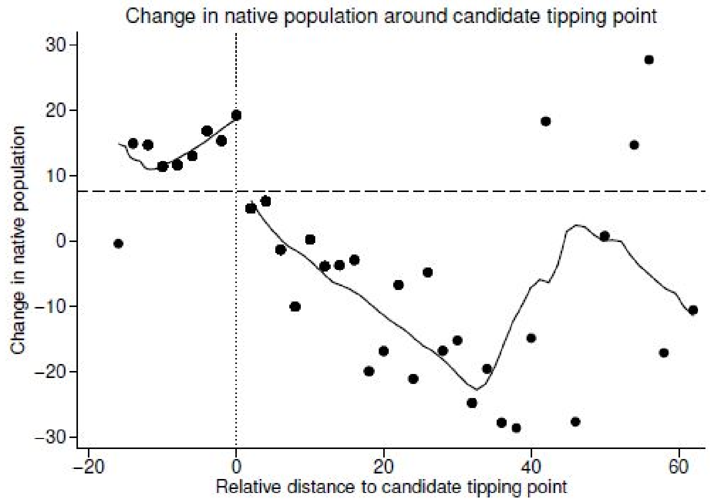
As elaborated on in the empirical methodology section, the presence of crossovers may lead to an attenuation bias. Specifically, Figure 1 shows decadal change in neighborhood native population based on the neighborhood’s distance to the tipping point in the base year.[44] However, a regular inflow of immigrants to control neighborhoods will cause control neighborhoods close to the tipping-point to move beyond the threshold later in the decade, and this will attenuate our point estimates.

[44] Card et al. (2008) are unable to account for crossovers due to their reliance on the decennial census.

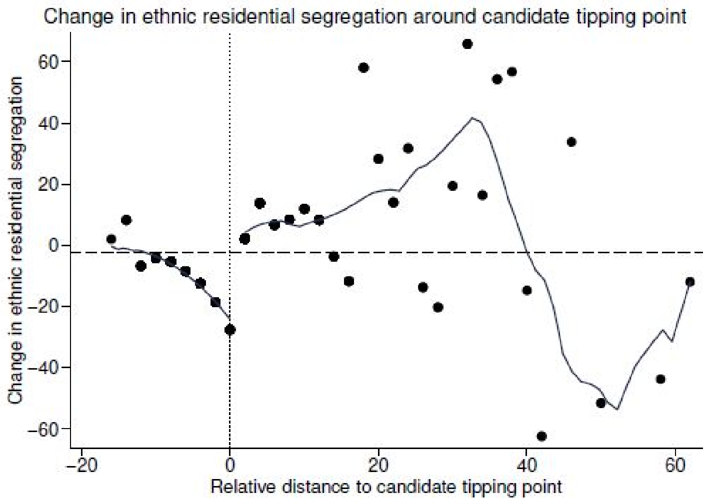
To limit the extent of dilution caused by these crossovers, we make two adjustments to Figure 1. First, rather than grouping neighborhoods into 2% bins by the deviation in immigrant share from the tipping point in 1990, we group neighborhoods into 2% bins by the deviation in the immigrant share from the tipping point in the year of tipping (the year in which the neighborhood's immigrant share exceeds the meteropolitan-specific tipping point calculated for 1990). For neighborhoods that did not move above the threshold at any point during the decade, the running variable is still based on base year values. Second, we weight the local linear regression by both the size of the neighborhoods and the fraction of the decade since tipping. It should be noted that this is not a fully representative depiction of our preferred model specification (equation (4)), but it is as close as we can get, and it does achieve the purpose of illustrating the potential attenuation bias induced by crossovers.

Figure 2 (a) depicts the result from this exercise. The discontinuity in Figure 2 (a) is approximately 7 percentage points larger than that in Figure 1 and demonstrates the importance of accounting for crossovers in an analysis of the tipping phenomenon. It is worth noting that the local linear regression fit to the left of the tipping point has a slight positive slope in Figure 2 (a), a slope that is not present in Figure 1. This is expected, as crossovers have very low decadal native growth rates and are located to the left of the threshold in Figure 1 (thus bringing down the average growth rate in the bins just to the left of the threshold) but to the right of the threshold in Figure 2 (thus bringing up the average growth rates in bins just to the left of the threshold). Figure 2 (b) further shows that there is a positive discontinuity in overexposure to immigrants at the threshold, with an effect size close to 30 percentage points. To explore the outliers located close to the threshold in Figure 2 (a) and (b), we replicate these figures but weight the size of each marker by the total base year size of the neighborhoods used to obtain that point. Figure 3 (a) and (b) show that the noisiest dots are generated by the smallest neighborhoods in the cities, increasing our confidence in the empirical strategy.

Figure 2: Discontinuities in native population growth and ethnic residential segregation around candidate tipping point



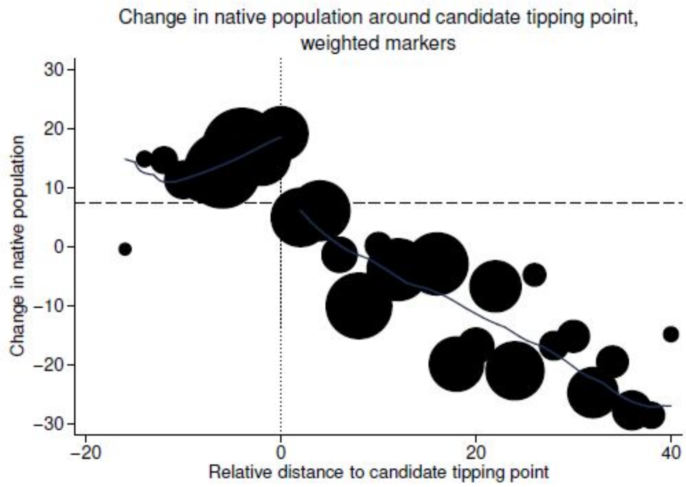
(a)



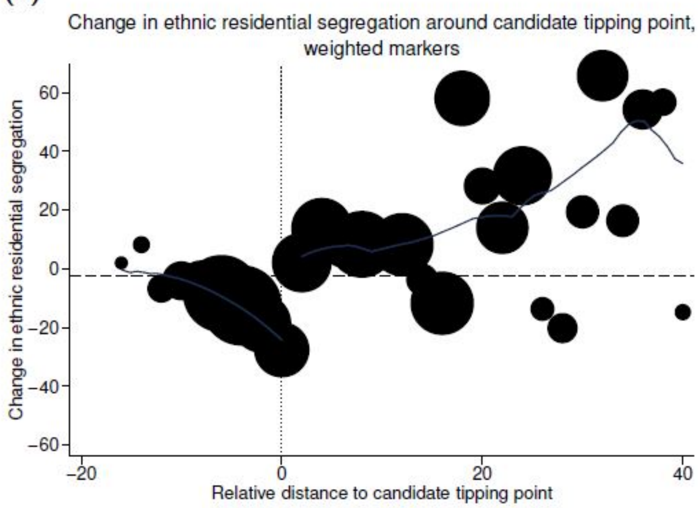
(b)

Notes: Dots show mean change in neighborhood native population between 1990 and 2000 as a percentage of total neighborhood population in 1990 (a) and change in overexposure to immigrants between 1990 and 2000 (b), grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The dashed horizontal lines represent the unconditional means, and the dotted vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fit separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhoods spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4. Only the 1/3 of the sample not used for identifying the location of the tipping points is used for these visual depictions.

Figure 3: Discontinuities in native population growth and ethnic residential segregation around candidate tipping point



(a)



(b)

Notes: Dots show mean change in neighborhood native population between 1990 and 2000 as a percentage of total neighborhood population in 1990 (a) and change in overexposure to immigrants between 1990 and 2000 (b), grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The size of each dot is weighted by the total size of the neighborhoods used to obtain that dot. The horizontal lines represent the unconditional means, and the vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fit separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhoods spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4. Only the 1/3 of the sample not used for identifying the location of the tipping points is used for these visual depictions.

The above figures are encouraging, but do not allow for formal hypothesis tests. To this end, Table 3 shows decade-specific estimates of equation (4) for overexposure to immigrants as well as for the growth rate of natives and immigrants and total population growth.[45] The coefficient on *Beyond TP* captures the effect of being above the tipping point on the change in the respective outcome between 1990 and 2000.

The effect of tipping on neighborhood native population growth is shown in columns 1 (without neighborhood-level control variables) and 2 (with neighborhood-level control variables), and the point estimates provide clear evidence of the existence of a large negative discontinuity, with an effect size of more than 16 percentage points. Consistent with the regression discontinuity framework, including covariates does not affect the magnitude of the discontinuity. It is worth reiterating that we have performed this analysis using three alternative models that account for crossovers in different ways, and the difference in coefficient estimates is not statistically significant across these models (Table A3).

The identified discontinuity in native population growth is larger than that in Card et al. (2008). To examine if this is due to Swedish natives having stronger preferences for residing with native neighbors or if it is due to our ability to account for crossovers, Table A4 displays results obtained from estimating equation (2). These estimates are smaller and in line with the results obtained by Card et al. (2008). This suggests that the results in Card et al. (2008) may be diluted due to the presence of crossovers.

The large positive coefficient on overexposure (0.29) in the last column of Table 3 mirrors the visual depiction in Figure 2 (b) and provides clear evidence of a discontinuous jump in ethnic residential segregation at the threshold. The magnitude of this jump exceeds the negative discontinuity in native growth due to the positive and significant coefficient on immigrant growth.[46] That immigrants display own-type preferences that augment the segregation effect associated with tipping is interesting. While this result does not show up in the analysis of Card et al. (2008), it is consistent with the idea that residential segregation is, in part, driven by self-segregation of minorities (e.g. Ihlanfeldt and Scafidi 2002).

[45] If total population size has an independent effect on individual outcomes, the negative discontinuity in total population growth shown in Table 3 may drive some of our results in Section 7. Though we are aware of no studies that examine this question, if present, we consider this part of the treatment.

[46] We have also performed this analysis for the 2000-2010 decade. The point estimate is smaller for this decade (0.19) but remains statistically significant at the one percent level. It is worth noting that SAMS codes are not available for 2004, and this analysis thus suffers from measurement error that complicates inference.

Table 3: Regression discontinuity models for changes in population composition and segregation around candidate tipping points

	Native Growth		Immigrant Growth		Population Growth		Overexposure	
Treatment Measure	**(i)**	**(ii)**	**(iii)**	**(iv)**	**(v)**	**(vi)**	**(vii)**	**(viii)**
Beyond TP	-0.176***	-0.162***	0.062***	-0.114***	-0.108***	0.317***	0.317***	0.287***
	(0.045)	(0.039)	(0.017)	(0.036)	(0.033)	(0.086)	(0.086)	(0.079)
Baseline Controls		x		x		x		x
Observations	520	520	520	520	520	520	520	520
R-Squared	0.233	0.304	0.336	0.362	0.035	0.081	0.256	0.331

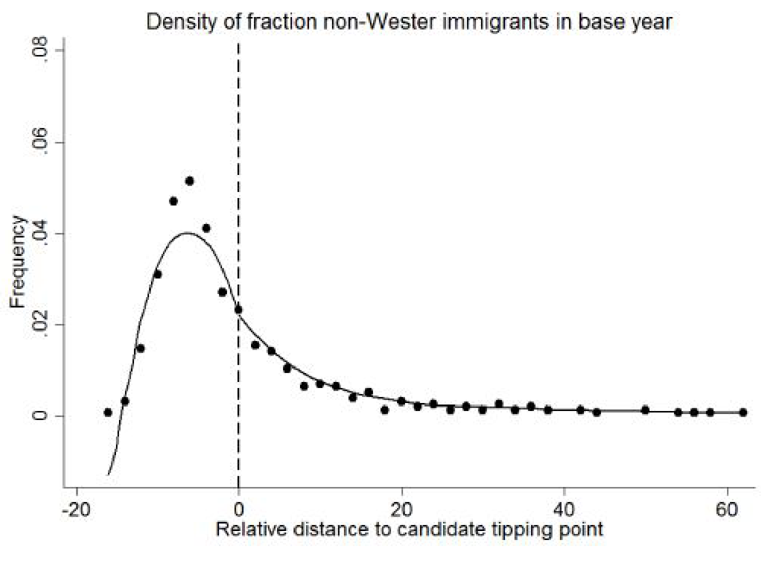
Notes: These results are obtained from estimating equation (4). Unit of observation is a neighborhood. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Sample is the 1/3 sample not used for identifying the tipping points. Demographic controls are years of schooling, income and gender, all measured in the base year. Regressions are weighted by the size of the neighborhoods. All specifications include metropolitan fixed effects. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

6.2 Robustness Tests and Sensitivity Analyses

In order to exploit the identified discontinuities to estimate the reduced-form effect of tipping on education and labor market outcomes, we need to invoke a number of assumptions. The main assumption is that neighborhood immigrant shares move smoothly through the tipping points in the base year. If this is not the case, there may be manipulation of the running variable that could threaten the internal validity of our estimation strategy. In this context, such manipulation is highly unlikely as it would require coordinated action of multiple individuals or explicit government policies that keep the immigrant share right below (or above) the threshold that we have identified.

Figure 4 plots the frequency of observations by 2 percent bins in the deviation in immigrant share from the estimated tipping point in the base year. The vertical line depicts the normalized tipping points. The solid line is a local linear regression fit separately on either side of the tipping point, using an Epanechnikov kernel and a bandwidth of 4. The figure shows that there is no discontinuity in the density at the cutoff. This result is robust to changes in bandwidth and polynomial order. In results not shown, we have also performed the McCrary (2008) density test, which fails to rejects the null that the discontinuity is zero.

Figure 4: Density plot of fraction non-Western immigrants in base year



Notes: The x-axis represents the deviation in non-Western immigrant share from the estimated tipping point, grouping neighborhoods into 2% bins. The y-axis measures the frequency of observations for each of the 2% neighborhood bins. The vertical line depicts the estimated tipping point (normalized to zero). The solid line is a local linear regression fitted separately on either side of the tipping point, using an Epanechnikov kernel and a bandwidth of 4. The full sample has been used for this depiction.

The absence of a discontinuity in the density of immigrant shares around the threshold in 1990 is encouraging but does not exclude the possibility that there is systematic sorting of individuals across neighborhoods around the threshold that affects the outcomes. We therefore estimate equation (4) for a set of covariates measured in the base year. These covariates are determined prior to treatment and should not be subject to discontinuities at the threshold. Table 4 shows that only one coefficient (social welfare participation) is statistically significant at the 10% level, and the size of this coefficient is small. We interpret these results as evidence against treatment manipulation around the threshold.

Table 4: Testing for jumps in baseline covariates

Variable	Coefficient
Age	0.513 (0.554)
Gender	0.003 (0.004)
Employment Income	-0.028 (0.029)
Years of Schooling	-0.154 (0.206)
Social Welfare	0.016* (0.009)
Social Insurance Benefits	-0.047 (0.056)
Income	-0.051 (0.051)
Employment	-0.010 (0.008)
Compulsory School Drop-Out	0.012 (0.015)
High School Drop-Out	-0.019 (0.024)
Years Since Migration	-0.709 (0.623)
University Enrollment	-0.013 (0.014)
Number of Children	0.007 (0.030)

Notes: Unit of observation is a neighborhood. Dependent variables are measured in the base year. Each row is a separate estimation of equation (4). All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Sample is the 1/3 sample not used for identifying the tipping points. Regressions are weighted by the size of the neighborhoods. All models include metropolitan fixed effects. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

A concern specific to this analysis relates to variation in the tipping points over time. Individuals are assigned to treatment based on tipping points in 1990, and substantial fluctuation in tipping point values over time may dilute our estimates. To investigate this concern, we re-estimate equation (1) for the decade succeeding our analytical period.[47] The mean tipping point for this decade is 20.77, slightly higher than that identified for the 90-00 period.[48] This increase is driven by Gothenburg: in Stockholm and Malmo the tipping points increase by less than 0.8. We therefore re-estimate equation (4) using only neighborhoods from Stockholm and Malmo (Table A5). The coefficient is not statistically significantly different from our baseline result, and we continue to use the full sample throughout.[49]

Another worry pertains to measurement error in the location of the tipping points, as this can smooth away true discontinuities and attenuate our results. One way to investigate if this constitutes a problem is by allowing tipping to occur within a certain range of the identified tipping point, and estimate donut-style regression discontinuity models in which neighborhoods with baseline immigrant shares within this range are excluded. Table A6 show the results from this exercise for four different donut-hole sizes: 0.10, 0.30, 0.50 and 1.00. None of these specifications produce estimates that are statistically significantly different from our baseline results, illustrating that measurement error in the location of the tipping point is unlikely to bias our results.

An additional concern relates to our decision to define Western immigrants as natives. If natives are averse to having Western immigrant neighbors, this grouping will cause attenuation bias. To examine this possibility, we calculate new tipping points and re-estimate equation (4) with immigrants defined as first-or second-generation Western immigrants (Table A7). There is no evidence in favor of discontinuities at these alternative tipping points, consistent with the literature which has found residential segregation in Sweden to be isolated to that between non-Western immigrants and the rest (Le Grand and Szulkin 2003).

A final worry relates to the functional form used to model the relationship between the conditional mean of the outcome and running variable. An incorrect functional form will cause the resulting estimator to be biased, and it thus is appropriate to explore the robustness of the results with respect to alternative specifications. Table 5 shows the results from this exercise. The point estimate is largely insensitive to polynomial order,

[47] Unfortunately, we lack data to perform a similar calculation for the decade preceding our analytical period.
[48] The slight increase is consistent with Card et al. (2008) and Aldén et al. (2015), as well as with Oliver and Wong (2003) who argue that exposure to immigrants in integrated neighborhoods may counter stereotypes.
[49] When Gothenburg is excluded the discontinuity becomes 0.300, when Malmo is excluded the discontinuity is 0.270 and when Stockholm is excluded the point estimate is 0.250. All estimates are significant at the one percent level, and none is statistically significantly different from the baseline estimate of 0.287.

and the discontinuity is statistically significant across all specifications.[50] Using a more flexible model that allows the control function to differ on either side of the threshold does not affect the result, and neither does the exclusion of outliers.[51]

Table 5: Sensitivity analysis on overexposure results

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Beyond TP	0.271*** (0.072)	0.215** (0.095)	0.302*** (0.073)	0.287*** (0.079)	0.335*** (0.093)	0.278*** (0.079)
Polynomial	Linear	Quadratic	Cubic	Quartic	Quintic	Quartic
Baseline Controls	x	x	x	x	x	x
Additional Controls						x
Control for Population Density						
Excluding Outliers						
AIC Value	4845.778	4844.097	4811.798	4811.044	4809.296	4813.008
Observations	520	520	520	520	520	520
R-squared	0.277	0.282	0.328	0.331	0.336	0.342

	(vii)	(viii)	(ix)	(x)	(xi)
Beyond TP	0.277*** (0.075)	0.281*** (0.10)	0.281*** (0.075)	0.260*** (0.076)	0.296*** (0.076)
Polynomial	Quartic	Quartic	Quartic	Quartic	Quartic
Baseline Controls	x	x	x	x	x
Additional Controls				x	x
Control for Population Density	x			x	x
Excluding Outliers		x			x
Fully Interacted			x	x	x
AIC Value	4809.488	4661.985	4810.567	4812.295	4636.889
Observations	520	520	520	520	520
R-squared	0.336	0.339	0.331	0.340	0.380

Notes: The results are obtained by estimating equation (4), with modifications as indicated in the table. The unit of observation is a neighborhood as identified by the SAMS code. Dependent variable is change in overexposure to non-Western immigrants between 1990 and 2000. Standard errors are clustered on one percent bins of the running variable. The sample used for estimation is the 1/3 sample not used for identifying the tipping points. Baseline controls are years of schooling, income and gender, all measured in the base year. Additional controls are years since migration, number of children in household and social welfare recipient status. The regressions are weighted by the size of the neighborhoods. All specifications include metropolitan area fixed effects. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

An alternative to the global polynomial approach is to estimate a local linear regression in a given window around the threshold. The advantage of this approach is that it does not rely on observations far from the threshold for identification, but if the underlying function is not exactly linear in the area being examined, there may be

[50] Table 5 also provides the Akaike Information Criteria values, which measures the bias-precision trade-off of utilizing a more complex model (Jacob and Zhu 2012). While all models produce similar AIC values, the fully interacted quartic polynomial specification with additional controls, that exclude outliers, fits the data best. However, the difference in AIC value from our baseline model is very small.

[51] Figure 1 (b) shows the change in overexposure is monotonically increasing after the threshold, until a certain point at which it begins to fall. Although consistent with Card et al. (2008), it is useful to exclude neighborhoods above this turning point to examine the sensitivity of the results.

substantial bias. Nevertheless, both approaches estimate the same statistic, and to further investigate the sensitivity of our results Table A8 displays results from the nonparametric approach. The results show that our estimates are robust to the use of local linear regressions.[52]

7 Effect of Tipping on Individual Outcomes

In this section we identify the intent-to-treat effect of residing in a neighborhood with an immigrant share greater than the estimated tipping point. This effect represents the local average effect of treatment assignment and is based on individuals' neighborhood of residence in the base year. It is worth reiterating that tipping may fuel discontinuities in the quality of services and the socioeconomic composition of neighborhoods that affect the outcomes of interest. However, any such discontinuities represent indirect effects of tipping and should be considered part of the treatment.[53]

7.1 Education Effects

We investigate three different sets of educational outcomes. First, academic performance in the 9th and final year of compulsory school (GPA and grades in the core subjects Swedish, English and mathematics). We look at subject-specific grades as the discussion in Section 2 suggests a clear link between residential segregation and the motivation and ability to acquire host country language skills, and it is therefore possible that student performance in Swedish is more affected than that in other languages and subjects. Second, high school performance (GPA, probability of attending a science track and the probability of enrolling in an academic (university-preparatory) program).[54] Third, post-secondary educational attainment measured in 2011 (years of schooling and the probability of having attended university).[55] We have converted the grades into yearly national percentile rankings. These outcomes span

[52] To perform this analysis, we rely on the cross-validation method proposed by Ludwig and Miller (2005) to obtain an optimal bandwidth. We have also used a bandwidth twice as large and half that recommended by the cross-validation method. The point estimates are not statistically significantly different.

[53] To explore if tipping contributes to socioeconomic segregation, we stratify the native sample by income (top and bottom quartile), education (more or less than a high school diploma) and gender, and estimate equation (5) for two outcomes: (a) the probability of moving from a treatment to a control neighborhood and (b) the probability of moving from a control to a treatment neighborhood. Table A9 shows that all estimates hold the expected sign and are statistically significant but that there are no statistically significant differences in these coefficients on any of the dimensions explored. The effect of tipping on socioeconomic segregation therefore appears minimal.

[54] We have also examined these outcomes by restricting the sample to individuals who graduate on time, as segregation could affect one's decisions of when to enroll. However, this has no effect on our estimates.

[55] It is important to note that all grades are set by the teachers, and if tipping affects the grade setting behavior of teachers this will also be captured by these outcome measures.

almost the entirety of our cohorts' educational experiences, allowing us to estimate the short-, medium- and long-term education effects of tipping.

Baseline estimates of the tipping effect on educational attainment, stratified by nativity status and cohort, are shown in Table 6.[56] Each cell comes from a separate estimation of equation (5) and represents the intent-to-treat effect of tipping on the outcome listed at the top of the column.[57] The table further shows the effect as a percentage of the control mean (the average value of the outcome variable among individuals in neighborhoods just to the left of the threshold).

The top panel displays the reduced-form effect on the young immigrants' educational outcomes. Only the cohort's probability to enroll in a high school science program is statistically and economically significant: as a percentage of the control mean, the tipping phenomenon is associated with a 22 percent reduction in the probability of pursuing a high school science program. With the exception of the high school science variable, the estimates are larger for the middle cohort, and several of them are statistically significant at the 10 percent level. As a percentage of the control mean, the results show that tipping leads to a 3.8 percent reduction in the national GPA percentile ranking and reduces the probability to enroll at university by 12.1 percent.

With respect to natives, our estimates point to adverse effects on educational attainment.[58] In contrast to the results for non-Western immigrants, the native estimates are statistically significant even for the young cohort. For example, as a percentage of the control mean, the tipping phenomenon leads to a 4.2 percent reduction in 9th grade national GPA percentile ranking among the young cohort. These adverse compulsory schooling effects persist as the individuals move up the education ladder. As a percentage of the control mean, the tipping phenomenon leads to a 3.8 percent reduction in the probability to enroll in an academic high school program, a 7.6 percent reduction in the probability to pursue university education and a 0.9 percent reduction in years of schooling among the middle cohort.

[56] Figures A2-A5 plot each of these outcomes by the forcing variable.

[57] In results not shown we have estimated each of the education and labor market regressions without adjusting the model to account for crossovers. The relationship between the results obtained through that model and the results obtained through our preferred model that does account for crossovers (when looking at individual outcomes) is identical to that when looking at discontinuities in neighborhood population composition: the crossover-adjustment leads to an increase in the absolute magnitude of the estimates and reduces the standard errors.

[58] The results are robust to estimation by birth cohort. There is larger volatility with respect to the immigrant sample due to power issues, but the interpretation remains the same.

Table 6: Effect of tipping on educational attainment

	Compulsory school					High School		Higher Education	
	GPA	**Math**	**English**	**Swedish**	**Science**	**GPA**	**Academic Track**	**University Enrollment**	**Years of Schooling**
Panel A: Immigrants									
I. Young Cohort									
Beyond TP	-1.173	-1.221	0.474	-0.720	-0.065***	-1.502	-0.000	-0.012	-0.042
	(1.046)	(1.207)	(0.982)	(1.139)	(0.017)	(1.423)	(0.017)	(0.016)	(0.084)
Percentage Change	-2.496	-2.709	0.967	-1.593	-22.414	-3.450	-0.000	-4.800	-0.341
Observations	27,092	22,534	26,414	26,509	18,364	12,000	18,364	27,696	27,696
II. Middle Cohort									
Beyond TP	-1.759*	-2.702*	-2.174*	-1.270	-0.017	-0.658	-0.030*	-0.029*	-0.164
	(0.996)	(1.385)	(1.290)	(0.787)	(0.023)	(1.526)	(0.018)	(0.017)	(0.099)
Percentage Change	-3.832	-6.183	-4.387	-2.837	-8.947	-1.636	-5.455	-12.083	-1.298
Observations	25,765	24,891	23,599	25,064	18,332	9,472	18,332	26,558	26,558
Panel B: Natives									
I. Young Cohort									
Beyond TP	-2.266***	-	-1.981***	-1.965***	-0.000	-0.342	-0.03**	-0.015*	-0.091***
	(0.639)	1.409**	(0.658)	(0.521)	(0.003)	(0.503)	(0.010)	(0.008)	(0.027)
Percentage Change	-4.208	-2.669	-3.644	-3.696	-0.000	-0.670	-4.854	-5.300	-0.728
Observations	194,465	190,22	190,731	190,784	149,234	92,788	149,234	193,661	193,661
II. Middle Cohort									
Beyond TP	-0.510	-0.468	-1.318***	-1.107***	0.000	-0.458	-0.02**	-0.019***	-0.119***
	(0.536)	(0.473)	(0.436)	(0.398)	(0.006)	(0.397)	(0.011)	(0.007)	(0.031)
Percentage Change	-1.03	-0.958	-2.518	-2.164	0.000	-0.900	-3.846	-7.600	-0.921
Observations	184,594	181,95	181,602	181,936	147,979	82,141	147,979	182,565	182,565

Notes: The results are obtained by estimating equation (5). The unit of observation is an individual residing in one of the 1560 neighborhoods included in our analysis. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Demographic controls are gender, mother's education, father's education, parental income and binaries for whether this information was not available for the individual, all measured in the base year. For the Old Cohort, parental education and income have been replaced with own education and income. All models include birth year and metropolitan area fixed effects. Immigrants refer to individuals born in, or have at least one parent born in, a non-Western country. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

To further examine the effect on cognitive and non-cognitive skill, we supplement our data on the middle cohort with information from the military tests that men took when conscription was mandatory.[59] The test scores range from 1 to 9 and were used to place individuals into branches. Results from estimating equation (5) using these test scores as our dependent variables are shown in Table A10. Tipping does not affect the cognitive and non-cognitive ability of these individuals, and the adverse effects identified above are thus driven by skills or behaviors not captured by these tests.

The estimates in Table 6 capture the average effect of treatment assignment, driven both by individuals who stay in tipped neighborhoods and individuals who move out of these neighborhoods at some point during our analysis period before the outcomes are measured.[60] One concern with these results is that individuals who leave tipped neighborhoods are exposed to a smaller treatment dose, potentially attenuating the point estimates. We examine this possibility by re-estimating equation (5) using only individuals in treated neighborhoods that did not move during the decade (Table A16). Stayers in tipped neighborhoods are on average more disadvantaged than movers and are exposed to a larger treatment dose, and even if the average effects of tipping are larger than our baseline estimates in Table 6, they will be smaller than the effects identified for this subsample.

With respect to natives, restricting the sample to stayers does not yield statistically significantly different results, suggesting that post-tipping migration from tipped neighborhoods does not impact our estimates. Concerning the immigrant sample, this exercise leads to larger coefficient estimates on the educational outcomes, with several coefficient estimates doubling in size both among the young and the middle cohort. The baseline adverse education results for the immigrant subpopulation (Table 6) may thus be attenuated by post-tipping migration from tipped neighborhoods. Nevertheless, there is a relatively large overlap between the 95 percent confidence intervals produced by this exercise and those produced by our baseline model, and the general interpretation of the results is unaffected.

[59] This is similar to the AFQT in the United States. See Mårdberg and Carlstedt (1993) for a description.

[60] Appendix Table A11 provides statistics on the fraction of individuals who remain in the same treatment group (tipped versus non-tipped neighborhood) over time. Treatment compliance is relatively high, but not perfect, with compliance rates of 89% and 69% among individuals in the control and treatment groups respectively.[60] The lower compliance rate of individuals in treated neighborhoods is driven by natives (64% versus 80% for immigrants), and this is expected given the results in Section 6.

To understand the size of these coefficients and their policy implications, it is useful to place them in relation to the effects of traditional education interventions, such as class-size reductions. In a recent study, Fredriksson et al. (2013) find that a one-pupil reduction in class-size in grades 4-6 in Sweden improves 9th grade academic achievement by 0.023 of a standard deviation and increases the probability of having a college degree by 0.8 percentage points. Our reduced form estimates for the young native cohort and the middle immigrant cohort suggest that the effect of tipping on 9th grade GPA is about -0.05 of a standard deviation, and the effect on university enrollment is between -1.5 and -2.9 percentage points (Table 6).[61] This suggests that neighborhoods just above the tipping point would need to reduce the average class size by 2-3 pupils to offset the effect of tipping on educational attainment.

7.2 Labor Market Effects

We focus on four labor market outcomes and estimate both intensive and extensive margin effects: *Employment income* (annual earnings from employment, excluding self-employment income but including work-related compensation from the Social Insurance Agency), *Self-employment income*, *Government-funded benefits* (compensation from 32 social security programs, including educational grants, grants to immigrants for learning Swedish, unemployment benefits, early-retirement supplemental compensation, compensation for start-ups and compensation for voluntary military service), and *Social insurance benefits* (income from a set of social security programs for which participation is conditioned on employment).[62] The outcomes are measured in 2011. We transform the income variables to their natural logarithms when analyzing the intensive margin effects and we convert the variables to dichotomous variables when analyzing the extensive margin effects.

Table 7 shows estimates of equation (5) for each outcome.[63] Concerning immigrants, all intensive margin results are small and insignificant. As a percentage of the control

[61] To obtain the value of -0.05, we first note that the coefficient on GPA is around -2 (both for the young native cohort and the middle immigrant cohort). We convert this estimate to standard deviation units by dividing it by the standard deviation of the GPA variable. As the GPA estimate is in percentile ranks, we first apply the inverse of the standard normal distribution to convert it to a point on the standard normal distribution.

[62] In results not shown, we have also examined the effect on *Socialbidrag* – government assistance to individuals who earn less than the amount considered necessary for supporting oneself financially. We find no effects.

[63] Figures A6-A11 plot each of these outcomes by the forcing variable on the intensive margin, stratified by immigrant status and cohort. Figures A12-A17 display the same plots on the extensive margin.

mean, we can rule out adverse employment earnings effects greater than 0.60%.[64] With respect to natives, the results tell a similar story. Although a couple of estimates are marginally statistically significant, they are very small. As a percentage of the control mean, we can rule out adverse employment earnings effects greater than 0.29%.[65] Concerning the extensive margin, none of the estimates for the immigrant population are significant. Among natives, there is a slight decrease in being self-employed and a slight increase in being in the social insurance benefits sample for the old cohort. Constraining the sample to individuals in treated neighborhoods that did not move during the decade does not yield statistically significantly different results (Table A16). This suggests that post-1990 migration from tipped neighborhoods does not affect our labor market results.

[64] To obtain this value, we first identify the cohort for which the coefficient estimate on employment earnings is the most negative. With a coefficient estimate of -0.001, this is the middle cohort. We then subtract 1.96 times the standard error to obtain the lower bound of the coefficient estimate at the 95% confidence level: -0.0735.
This suggests that the lower bound of the intent-to-treat effect on employment income is -7.35%. Dividing this number by the control mean (12.19) yields the largest adverse effect at the 95% confidence level: 0.603%.

[65] Excluding work-related compensation from employment income does not alter the interpretation of the results.

Table 7: The effect of tipping on labor market outcomes

	Intensive Margin				Extensive Margin			
	Social Insurance Benefits	**Self-Employment**	**Employment Income**	**Government-Funded Benefits**	**Social Insurance Benefits**	**Self-Employment Income**	**Employment Income**	**Government-Funded Benefits**
Panel A: Immigrants								
I. Young Cohort								
Beyond TP	0.143 (0.089)	-0.147 (0.278)	0.059 (0.045)	0.004 (0.049)	0.005 (0.011)	-0.003 (0.005)	0.015 (0.014)	0.005 (0.013)
Percentage Change	1.396	-1.404	0.507	0.042	1.087	-8.571	2.027	3.330
Observations	4,945	1,049	22,855	14,407	29,354	29,354	29,354	29,354
II. Middle Cohort								
Beyond TP	0.052 (0.078)	-0.108 (0.249)	-0.001 (0.037)	-0.034 (0.052)	0.025 (0.013	0.002 (0.009)	0.025 (0.012)	0.007 (0.016)
Percentage Change	0.515	-0.949	-0.008	-0.356	5.682	2.778	3.330	2.258
Observations	10,626	1,714	22,257	14,828	29,017	29,017	29,017	29,017
III. Old Cohort								
Beyond TP	-0.134 (0.097)	0.049 (0.088)	0.023 (0.021)	-0.049 (0.040)	0.001 (0.004)	-0.001 (0.005)	0.018 (0.015)	-0.002 (0.010)
Percentage Change	-1.213	0.436	0.188	-0.502	0.270	-1.408	2.687	-1.818
Observations	7,473	4,157	41,112	28,917	68,925	68,925	68,925	68,925
Panel B: Natives								
I. Young Cohort								
Beyond TP	-0.024 (0.031)	0.013 (0.098)	0.024 (0.016)	-0.021 (0.016)	0.011 (0.006)	-0.003 (0.002)	0.001 (0.005)	-0.004 (0.004)
Percentage Change	-0.231	0.128	0.204	-0.212	2.245	-10.345	0.118	-0.235
Observations	35,032	6,373	173,261	99,455	200,695	200,695	200,695	200,695

	Intensive Margin				Extensive Margin			
	Social Insurance Benefits	**Self-Employment**	**Employment Income**	**Government-Funded Benefits**	**Social Insurance Benefits**	**Self-Employment Income**	**Employment Income**	**Government-Funded Benefits**
II. Middle Cohort								
Beyond TP	-0.026 (0.019)	0.019** (0.090)	-0.014 (0.011)	-0.019 (0.023)	0.011* (0.006)	-0.003 (0.003)	0.009** (0.004)	0.008 (0.006)
Percentage Change	-0.258	0.182	-0.113	-0.194	1.964	-5.263	1.034	1.778
Observations	87,245	10,613	167,346	107,007	191,654	191,654	191,654	191,654
III. Old Cohort								
Beyond TP	0.058 (0.044)	0.080 (0.056)	0.024* (0.014)	0.035 (0.024)	0.005*** (0.002)	-0.009** (0.005)	0.010 (0.007)	0.007 (0.005)
Percentage Change	0.541	0.746	0.192	0.363	1.724	-12.676	1.250	5.833
Observations	32,104	19,219	222,079	78,100	279,472	279,472	279,472	279,472

Note: These results are obtained by estimating equation (5). The unit of observation is an individual residing in one of the 1560 neighborhoods included in our analysis. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Demographic controls are gender, mother's education, father's education, parental income and binaries for whether this information was not available for the individual, all measured in the base year. For the Old Cohort, parental education and income have been replaced with own education and income. All models include birth year and metropolitan area fixed effects. All dependent variables are measured in 2011. Immigrants refer to individuals born in, or have at least one parent born in, a non-Western country. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

The young and middle cohorts are between 21 and 38 years old when the labor market outcomes are measured, and the majority of these individuals are not on a part of their earnings profiles where yearly earnings are informative about lifetime earnings (Haider and Solon 2006; Böhlmark and Lindquist 2006). In results not shown, we have estimated equation (5) only using individuals who are between 33 and 38 when the outcomes are measured. The results are not statistically or economically significantly different from those in Table 7.

Table 7 shows that the adverse education effects identified above do not carry over to the labor market, and although the transitory nature of the education effects is an interesting finding, it is perhaps not surprising. First, the return to education in Sweden is low compared to other OECD countries. Based on data from 1997 to 2000, OECD found the internal private rate of return for males at the upper secondary level in Sweden to be only 39% of that in the US, and at the tertiary level only 75% (OECD 2002). Second, despite substantial wage decentralization efforts both within the public and the private sectors, wage compression remains very high in Sweden. In 2011, the 5th to 1st decile male wage ratio was 1.4 in Sweden while it exceeded 2.2 in the US (Kahn 2015). These two factors combined with the relatively modest education effects identified in Table 6 may explain the results.[66]

7.3 Robustness and Sensitivity Analyses

We perform a series of sensitivity checks to investigate the robustness of our results to minor alterations of the empirical model. Each of the alterations deals with a specific concern associated with our empirical strategy. The first concern relates to variation in neighborhood population density both within and across the metropolitan areas. This variation in population density will affect the level of individual exposure to immigrants and natives, and may therefore impact the effect of tipping.

Second, the effects may differ for individuals in neighborhoods that are surrounded by other neighborhoods that have tipped, as individuals who live in tipped neighborhoods may work and study in neighboring areas. We use Statistics Sweden's geographic neighborhood atlas to identify neighborhoods that surround areas that have tipped and create a measure of exposure based on the fraction of neighboring

[66] Further, many studies have found that contemporaneous effects on student test scores can be very different from effects on long-run outcomes (e.g. Chetty et al. 2011; Deming et al. 2013; Lovenheim and Willén 2016).

neighborhoods that have tipped (Table A13).[67] We look at whether individuals in neighborhoods with more or less than 50% tipped neighbors are differentially affected by the identified tipping phenomenon.

The third concern relates to outliers. As can be seen in Figure 2 (b), the change in overexposure is monotonically increasing after the identified threshold until a certain point at which it begins to decline. Although consistent with previous literature (e.g. Card et al. 2008), individuals in neighborhoods above this turning point may be different from individuals in neighborhoods at the margin of tipping on dimensions that equation (5) cannot control for.

Table 8 (immigrants) and Table 9 (natives) display results obtained from running each of the modified regressions for each of the cohorts. The first row of each panel controls for neighborhood population density, the second and third rows show the results obtained when stratifying the sample based on the tipping behavior of neighboring areas, and the fourth row displays the results when outliers are omitted.[68] The results show that our baseline estimates are robust to these alternative model specifications. Although some of the intensive margin labor market outcomes for the old immigrant cohort become statistically significant, the effects are very small and remain within the 95% confidence intervals of the baseline results.

[67] www.scb.se/sv/Vara-tjanster/Regionala-statistikprodukter/Marknadsprofiler/Postnummer-och-SAMS-atlasen/
[68] Using the alternative models that account for crossovers, discussed in Section 4, does not affect the results.

Table 8: The effect of tipping on immigrants, sensitivity table

	Compulsory School				High School			Higher Education		Labor Market Outcomes			
	GPA	**English**	**Swedish**	**Math**	**Science**	**GPA**	**Academic Track**	**University Enrollment**	**Years of School**	**Soc. Ins. Benefits**	**Self Empl. Income**	**Empl. Income**	**Gov. Fun. Benefits**
Panel A: Young Cohort													
Population Density	-1.536	0.326	-1.054	-1.467	-0.070***	-1.554	-0.003	0.012	-0.054	0.125	-0.115	0.035	-0.005
	(1.106)	(1.032)	(1.242)	(1.293)	(0.017)	(1.459)	(0.018)	(0.016)	(0.088)	(0.089)	(0.258)	(0.043)	(0.052)
More Than 50% Tipped Neighbors	0.175	0.757	-0.778	-1.114	-0.068***	-0.695	0.013	0.007	0.013	0.183*	-0.253	0.094	0.016
	(1.002)	(1.032)	(1.205)	(1.280)	(0.023)	(1.588)	(0.020)	(0.022)	(0.092)	(0.095)	(0.393)	(0.060)	(0.052)
Less Than 50% Tipped Neighbors	-1.700*	1.461	0.079	-1.549	-0.071***	-0.960	-0.018	0.035*	0.058	0.170*	0.003	0.024	0.053
	(0.849)	(0.980)	(0.993)	(1.218)	(0.019)	(1.774)	(0.022)	(0.019)	(0.094)	(0.096)	(0.413)	(0.055)	(0.061)
Excluding Outliers	-2.242*	0.347	-0.578	-1.460	-0.068***	-0.816	-0.011	0.020	-0.032	0.114	-0.153	0.020	0.009
	(1.136)	(0.967)	(1.077)	(1.222)	(0.017)	(1.387)	(0.019)	(0.016)	(0.082)	(0.087)	(0.294)	(0.047)	(0.052)
Panel B: Middle Cohort													
Population Density	-1.975*	-2.353*	-1.442*	-2.677*	-0.019	-0.899	-0.027	-0.028	-0.151	0.057	-0.143	-0.008	-0.035
	(1.006)	(2.696)	(0.833)	(1.392)	(0.023)	(1.599)	(0.019)	(0.018)	(0.010)	(0.080)	(0.252)	(0.035)	(0.055)
More Than 50% Tipped Neighbors	-0.710	-1.838	-1.083	-2.450	-0.016	-0.792	-0.031	-0.033*	-0.145	-0.010	0.070	0.001	-0.117
	(1.148)	(1.682)	(1.000)	(1.603)	(0.026)	(1.996)	(0.024)	(0.018)	(0.107)	(0.098)	(0.268)	(0.040)	(0.075)
Less Than 50% Tipped Neighbors	-2.087*	-0.608	0.057	-1.979	0.005	-0.108	-0.027	-0.020	-0.114	0.145	-0.202	-0.049	0.064
	(1.232)	(1.318)	(1.105)	(1.715)	(0.023)	(2.088)	(0.025)	(0.015)	(0.132)	(0.089)	(0.339)	(0.045)	(0.060)
Excluding Outliers	-2.44**	-2.175	-0.958	-2.812*	-0.012	0.072	-0.033*	-0.021	-0.121	0.061	-0.127	-0.012	-0.035
	(1.061)	(1.403)	(0.787)	(1.409)	(0.021)	(1.505)	(0.019)	(0.016)	(0.103)	(0.079)	(0.270)	(0.036)	(0.052)

	Compulsory School				High School		Higher Education			Labor Market Outcomes			
	GPA	English	Swedish	Math	Science	GPA	Academic Track	University Enrollment	Years of School	Soc. Ins. Benefits	Self Empl. Income	Empl. Income	Gov. Fun. Benefits
Panel C: Old Cohort													
Population Density	–	–	–	–	–	–	–	–	–	-0.110	0.042	0.006	-0.025
										(0.094)	(0.091)	(0.023)	(0.036)
More Than 50% Tipped Neighbors	–	–	–	–	–	–	–	–	–	-0.199**	0.235**	0.033	-0.096**
										(0.081)	(0.115)	(0.027)	(0.047)
Less Than 50% Tipped Neighbors	–	–	–	–	–	–	–	–	–	-0.152	-0.036	0.058***	-0.011
										(0.101)	(0.115)	(0.021)	(0.043)
Excluding Outliers	–	–	–	–	–	–	–	–	–	-0.146	-0.041	0.049**	-0.035
										(0.099)	(0.096)	(0.020)	(0.038)

Notes: These results are obtained by estimating equation (5). The unit of observation is an individual residing in one of the 1560 neighborhoods included in our analysis. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Demographic controls are gender, mother's education, father's education, parental income and binaries for whether this information was not available for the individual, all measured in the base year. For the Old Cohort, parental education and income have been replaced with own education and income. All models include birth year and metropolitan area fixed effects. Immigrants refer to individuals born in, or have at least one parent born in, a non-Western country. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 9: The effect of tipping on natives, sensitivity table

	Compulsory School				High School		Higher Education			Labor Market Outcomes			
	GPA	**English**	**Swedish**	**Math**	**Science**	**GPA**	**Academic Track**	**University Enrollment**	**Years of School**	**Soc. Ins. Benefits**	**Self Empl. Income**	**Empl. Income**	**Gov. Fun. Benefits**
Panel A: Young Cohort													
Population Density	-2.149***	-1.576***	-1.697***	-1.359***	-0.002	-0.115	-0.023**	-0.012*	-0.106***	-0.035	0.007	0.000	-0.011
	(0.639)	(0.521)	(0.521)	(0.481)	(0.004)	(0.495)	(0.010)	(0.007)	(0.027)	(0.033)	(0.100)	(0.010)	(0.015)
More Than 50% Tipped Neighbors	-2.042**	-2.226***	-1.873**	-1.266**	0.007	-0.342	-0.036***	-0.017*	-0.100***	-0.050	-0.079	0.049**	-0.019
	(0.848)	(0.725)	(0.736)	(0.606)	(0.005)	(0.535)	(0.012)	(0.010)	(0.031)	(0.032)	(0.138)	(0.019)	(0.021)
Less Than 50% Tipped Neighbors	-3.247***	-2.753***	-2.594***	-2.498***	-0.001	0.029	-0.038***	-0.018*	-0.123***	-0.011	-0.094	0.019	-0.0.003
	(0.868)	(0.830)	(0.842)	(0.594)	(0.004)	(0.584)	(0.014)	(0.011)	(0.038)	(0.035)	(0.159)	(0.018)	(0.022)
Excluding Outliers	-2.295***	-2.047***	-2.131***	-1.496**	-0.001	-0.148	-0.027**	-0.013	-0.088***	-0.009	-0.094	0.028	-0.012
	(0.792)	(0.733)	(0.701)	(0.590)	(0.004)	(0.530)	(0.013)	(0.009)	(0.030)	(0.034)	(0.124)	(0.017)	(0.018)
Panel B: Middle Cohort													
Population Density	-0.834	-1.318***	-1.107***	-0.748	-0.004	-0.523	-0.017*	-0.17***	-0.139***	-0.027	0.194	-0.027**	-0.009
	(0.536)	(0.436)	(0.417)	(0.532)	(0.006)	(0.405)	(0.009)	(0.006)	(0.034)	(0.019)	(0.090)	(0.011)	(0.024)
More Than 50% Tipped Neighbors	0.267	-1.253***	-0.673	0.067	0.010	-0.335	-0.021*	-0.022***	-0.092***	-0.016	0.113	0.011	-0.010
	(0.593)	(0.372)	(0.422)	(0.503)	(0.005)	(0.469)	(0.012)	(0.008)	(0.033)	(0.019)	(0.093)	(0.012)	(0.024)
Less Than 50% Tipped Neighbors	-0.635	-1.023**	-1.089**	-0.280	0.001	0.193	-0.012	-0.010	-0.075*	0.016	0.124	0.012	0.022
	(0.689)	(0.497)	(0.488)	(0.633)	(0.006)	(0.576)	(0.015)	(0.010)	(0.043)	(0.019)	(0.118)	(0.012)	(0.028)
Excluding Outliers	0.042	-0.755	-0.735	0.209	0.004	-0.074	-0.006	-0.008	-0.074**	0.009	0.163*	-0.001	0.012
	(0.585)	(0.505)	(0.441)	(0.529)	(0.007)	(0.471)	(0.012)	(0.007)	(0.035)	(0.018)	(0.096)	(0.013)	(0.026)

	Compulsory School				High School			Higher Education			Labor Market Outcomes		
	GPA	English	Swedish	Math	Science	GPA	Academic Track	University Enrollment	Years of School	Soc. Ins. Benefits	Self Empl. Income	Empl. Income	Gov. Fun. Benefits
Panel C: Old Cohort													
Population Density	–	–	–	–	–	–	–	–	–	0.055	0.077	0.002	0.064**
										(0.044)	(0.058)	(0.011)	(0.025)
More Than 50% Tipped Neighbors	–	–	–	–	–	–	–	–	–	0.079	0.089	0.029*	0.063**
										(0.052)	(0.069)	(0.015)	(0.027)
Less Than 50% Tipped Neighbors	–	–	–	–	–	–	–	–	–	0.056	0.137	0.021	0.061
										(0.059)	(0.076)	(0.016)	(0.030)
Excluding Outliers	–	–	–	–	–	–	–	–	–	0.066	0.098	0.028	0.048
										(0.051)	(0.063)	(0.015)	(0.026)

Notes: These results are obtained by estimating equation (5). The unit of observation is an individual residing in one of the 1560 neighborhoods included in our analysis. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Demographic controls are gender, mother's education, father's education, parental income and binaries for whether this information was not available for the individual, all measured in the base year. For the Old Cohort, parental education and income have been replaced with own education and income. All models include birth year and metropolitan area fixed effects. All models include birth year and municipality fixed effects. Natives refer to individuals not born in, and do not have a parent born in, a non-Western country. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

7.4 Heterogeneous Treatment Effects

Section 2 suggests that there may be treatment heterogeneity by group characteristics. To this end, we stratify the sample along several socioeconomic dimensions and re-estimate equation (5) for each of these subgroups. For the old cohort the stratifications are based on the individuals' baseline (1990) characteristics, while they are based on the parental baseline characteristics of individuals in the young and middle cohort. First, we look at differential effects for individuals with and without parental post-secondary education. Second, we examine if individuals with parental income in the bottom quartile of the earnings distribution are differently affected than those in the top quartile. Third, we stratify the sample by gender. The results are shown in Table 10 for non-Western immigrants and Table 11 for natives.

With respect to natives, there are statistically significant differences in education effects by gender and parental education. Specifically, boys and children to low-educated parents are more affected by tipping. Concerning immigrants, a similar pattern can be observed, though this heterogeneity exists with respect to parental income as well. This exercise further demonstrates that our inability to document statistically significant education effects among young immigrants in our baseline results is due to treatment heterogeneity across socioeconomic dimensions: We find statistically significant negative effects on short-term educational outcomes among young immigrant males, immigrants from low-income households and immigrants with low parental education. These effects are either not statistically significant or are only marginally statistically significant among immigrants from high-income households and with high parental education. With respect to labor market outcomes, neither immigrants nor natives display heterogeneous treatment effects; all estimates are within the 95% confidence intervals of our baseline results.

Table 10: Heterogeneous treatment effects, immigrant sample

	Educational Level		Income Level		Gender	
	Low	**High**	**Low**	**High**	**Male**	**Female**
Panel A: Young Cohort						
GPA Ranking	-2.024*	0.914	-3.763**	-1.241	-2.193*	-0.061
	(1.167)	(2.463)	(1.680)	(1.701)	(1.153)	(1.700)
Years of Schooling	-0.083	0.116	-0.242	-0.091	-0.018	-0.056
	(0.088)	(0.153)	(0.148)	(0.110)	(0.143)	(0.071)
Employment Income	0.031	0.179**	0.066	0.078	0.081	0.035
	(0.052)	(0.084)	(0.116)	(0.082)	(0.091)	(0.045)
Social Insurance Benefits	0.119	0.242	0.309	0.179	0.028	0.195
	(0.096)	(0.226)	(0.313)	(0.136)	(0.158)	(0.172)
Panel B: Middle Cohort						
GPA Ranking	-2.344*	-1.363	-5.517**	-1.758	-3.172**	-0.224
	(1.363)	(2.669)	(2.531)	(1.363)	(1.307)	(1.240)
Years of Schooling	-0.60	-0.156	-0.472***	-0.241*	-0.260	-0.059
	(0.111)	(0.164)	(0.178)	(0.132)	(0.159)	(0.129)
Employment Income	-0.004	0.029	-0.067	0.018	0.034	-0.037
	(0.039)	(0.097)	(0.112)	(0.054)	(0.059)	(0.050)
Social Insurance Benefits	0.031	0.012	-0.019	-0.013	0.063	0.035
	(0.086)	(0.150)	(0.209)	(0.093)	(0.123)	(0.130)
Panel C: Old Cohort						
Employment Income	0.028	0.026	0.139**	-0.001	0.071**	-0.016
	(0.029)	(0.039)	(0.059)	(0.028)	(0.027)	(0.037)
Social Insurance Benefits	-0.027	-0.132**	-0.047	-0.490	-0.099*	-0.006
	(0.044)	(0.064)	(0.053)	(0.054)	(0.053)	(0.052)

Notes: These results are obtained by estimating equation (5). The unit of observation is an individual residing in one of the 1560 neighborhoods included in our analysis. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Demographic controls are gender, mother's education, father's education, parental income and binaries for whether this information was not available for the individual, all measured in the base year. For the Old Cohort, parental education and income have been replaced with own education and income. All models include birth year and metropolitan area fixed effects. Immigrants refer to individuals born in, or have at least one parent born in, a non-Western country. Columns (1) and (2) stratify the sample based on whether the individual has at least one parent with post-secondary education for the young and middle cohorts, and based on whether the individual has or does not have post-secondary education for the old cohort. Columns (3) and (4) stratify the sample based on whether the individual's parental income is in the bottom or top quartile of the income distribution for the young and middle cohorts, and based on whether the individual is in the bottom or the top quartile of the income distribution for the old cohort. Columns (5) and (6) statify the sample based on gender. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 11: Heterogeneous treatment effects, native sample

	Educational Level		Income Level		Gender	
	Low	High	Low	High	Male	Female
Panel A: Young Cohort						
GPA Ranking	-2.689***	-1.590**	-2.115**	-2.621***	-2.589***	-1.928***
	(0.824)	(0.712)	(1.053)	(0.709)	(0.865)	(0.678)
Years of Schooling	-0.136***	-0.032	-0.080*	-0.137***	-0.120***	-0.059*
	(0.031)	(0.026)	(0.045)	(0.040)	(0.034)	(0.031)
Employment Income	0.008	0.043**	0.045	-0.017	0.034	0.015
	(0.014)	(0.020)	(0.036)	(0.023)	(0.020)	(0.014)
Social Insurance Benefits	-0.026	-0.015	-0.078***	0.018	-0.010	-0.029
	(0.020)	(0.018)	(0.025)	(0.031)	(0.027)	(0.021)
Panel B: Middle Cohort						
GPA Ranking	-1.517***	-0.146	-0.022	-1.446	-0.879	-0.146
	(0.491)	(0.801)	(0.695)	(1.064)	(0.550)	(0.718)
Years of Schooling	-0.164***	-0.037	-0.145***	-0.142***	-0.130***	-0.108***
	(0.033)	(0.036)	(0.039)	(0.049)	(0.036)	(0.036)
Employment Income	-0.018	-0.008	-0.007	-0.021	-0.005	-0.023
	(0.012)	(0.022)	(0.017)	(0.022)	(0.017)	(0.016)
Social Insurance Benefits	0.003	-0.059**	0.025	-0.015	-0.010	-0.026
	(0.030)	(0.023)	(0.023)	(0.049)	(0.030)	(0.024)
Panel C: Old Cohort						
Employment Income	0.0131	0.041**	0.037	0.019	0.029*	0.020
	(0.015)	(0.016)	(0.029)	(0.026)	(0.016)	(0.015)
Social Insurance Benefits	-0.011	-0.035	-0.014	-0.026	-0.005	-0.026
	(0.027)	(0.048)	(0.036)	(0.043)	(0.032)	(0.030)

Notes: These results are obtained by estimating equation (5). The unit of observation is an individual residing in one of the 1560 neighborhoods included in our analysis. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Demographic controls are gender, mother's education, father's education, parental income and binaries for whether this information was not available for the individual, all measured in the base year. For the Old Cohort, parental education and income have been replaced with own education and income. All models include birth year and metropolitan area fixed effects. Natives refer to individuals not born in, and do not have a parent born in, a non-Western country. Columns (1) and (2) stratify the sample based on whether the individual has at least one parent with post-secondary education for the young and middle cohorts, and based on whether the individual has or does not have post-secondary education for the old cohort. Columns (3) and (4) stratify the sample based on whether the individual's parental income is in the bottom or top quartile of the income distribution for the young and middle cohorts, and based on whether the individual is in the bottom or the top quartile of the income distribution for the old cohort. Columns (5) and (6) stratify the sample based on gender. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

In addition to treatment heterogeneity by group characteristics, Section 2 suggests that there may be treatment heterogeneity on the time dimension. To explore this possibility, we extend our analysis in two ways. First, we complement our analysis of the oldest cohort by looking at their labor market outcomes in 2000. Second, we look at the effect of tipping on employment income for the oldest cohort for each year between 1990 and 2011.[69]

Results from the first exercise are shown in Table A14. The estimates for the intensive margin closely resemble our baseline results. Although the coefficient on Social Insurance Benefits changes sign among immigrants, the estimate remains within the 95% confidence interval obtained from the 2011 estimates and is not statistically significant. Concerning the extensive margin, the point estimates are virtually unchanged, though a decrease in the standard errors makes the extensive margin coefficients on Employment Income and Compensation from SIA among natives statistically significant.

Results from the second exercise are shown in Figure A18. The dots are coefficient estimates obtained by estimating equation (5) for three-year averages of Employment Income between 1990 and 2010, and the bars identify the 95% confidence intervals.[70] The baseline 2011 results are included for ease of comparison. As can be seen, all the coefficient estimates are precisely estimated zeros for both the native and the immigrant samples.

8 Discussion and Conclusion

Identifying the effects of ethnic residential segregation through empirical analysis is difficult due to selective sorting across neighborhoods, and prior research in this area has been hampered by a lack of exogenous variation in neighborhood choice. We overcome this problem by utilizing a novel identification strategy that borrows theoretical insight from the one-sided tipping point model of Card et al. (2008).

Our results show large and robust discontinuities in native population growth and ethnic residential segregation at the tipping points, demonstrating that the phenomenon documented by Card et al. (2008) in the US extends to Sweden. An interesting finding is that immigrants display own-type preferences that augment the segregation effect associated with the tipping phenomenon, which supports the idea that residential segregation is, in part, driven by self-segregation of minorities.

[69] This is the only labor market outcome that exists for every year between 1990 and 2011.
[70] We use three-year averages to prevent potential year shocks from confounding the results. However, this decision does not impact the conclusion drawn.

In the second part of our analysis we find that tipping has adverse education effects on natives and that these effects are driven mainly by males and individuals with low parental education. We find less consistent evidence with respect to immigrants, though similarly sized effects can be observed for immigrants of low socioeconomic status. We find no evidence that the education effects carry over to the labor market: as a percentage of the control mean, we can rule out adverse employment earnings effects greater than 0.29% for natives and 0.60% for non-Western immigrants.

Given that we focus on a country with a very generous social policy systems, it is noteworthy that we identify adverse education effects of tipping. It is also important to highlight that Sweden's social policy system may mute some of the effects associated with tipping. For example, Sweden's financial equalization schemes and generous welfare policies may hedge against the anticipated quality reductions in services and institutions discussed in Section 2. Therefore, one should be careful to extrapolate these results to other countries and settings, as variation in social policies and public institutions likely affect the results. For example, market-driven housing, property tax funded schools and a large share of unauthorized immigrants that cannot access welfare services make it possible that the effects would be larger in a country such as the US.

In terms of policy implications, our results demonstrate that social interactions in native preferences represent a clear obstacle to neighborhood integration. Conventional place- and people-based policy solutions to residential segregation would only have a minimal impact on reducing the prevalence of this phenomenon, and policymakers may need to look at alternative approaches that target the root cause of the problem.

Although we do not find that the education effects carry over to the labor market, recent findings on the relationship between education and outcomes such as health and crime suggest that the education effects could impact individuals on dimension we are unable to observe (Cutler and Lleras-Muney 2006; Machin et al. 2010; Hjalmarsson et al. 2014). Policy interventions aimed at counteracting the adverse education effects might therefore still be warranted, and one such intervention would be to inject additional resources into schools in affected areas. Based on Fredriksson et al. (2013), we calculate that a class size reduction of 2-3 pupils in affected areas would be sufficient to offset the total education effect of tipping. Depending on whether the education effects are non-permanent, or affect individuals on dimensions that we cannot examine, this may or may not pass a cost-benefit test.

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APPENDIX FOR ONLINE PUBLICATION ONLY

Appendix A – The Structural Break Method

This method is similar to that of identifying breaks in time series data, and consists of estimating the following regression

$$Dn_{s,m,t} = \alpha_m + d_m \mathbf{1}[i_{s,m,t-10} > i^*_{m,t-10}] + \varepsilon_{s,m,t}, \qquad \text{for } 0 \leq i_{s,m,t-10} \leq I$$

where $Dn_{s,m,t} = \frac{N_{s,m,t} - N_{s,m,t-10}}{P_{s,m,t-10}}$ and represents the change in the native population in neighborhood *s* in metropolitan area *m* between *t-10* and *t*, and $d_m \mathbf{1}[i_{s,m,t-10} > i^*_{m,t-10}]$ is an indicator variable that takes the value of one if the immigrant share in the neighborhood exceeds the tipping point of the metropolitan area.

To obtain estimates of the tipping points in the metropolitan areas, $i^*_{m,t-10}$, we restrict the tipping points to be in the interval *[0, 50%]* and choose the values that maximizes R^2 of the above equation, separately for each metropolitan area. According to Card et al. (2008), this method works well for identifying tipping points in large cities, but performs less well in small cities due to a tendency to identify tipping points that reflects clear outliers. Given the average size of the metropolitan areas in Sweden it is therefore inappropriate to rely on this strategy for the purpose of identifying the tipping points.

Table A1: Foreign-born by country of birth

Country	1950	1960	1970	1980	1990	2000	2010	2010
Panel A: Largest source countries 2015								
Finland	44,821	101,307	235,453	251,342	217,636	195,447	169,521	156,045
Iraq	5	16	108	631	9,818	49,372	121,761	131,888
Syria	0	6	100	1,606	5,874	4,162	20,758	98,216
Poland	7,832	6,347	10,851	19,967	35,631	40,123	70,253	85,517
Iran	110	115	411	3,348	40,048	51,101	62,120	69,067
Yugoslavia	171	1,532	33,779	37,982	43,346	71,972	70,819	67,190
Somalia	0	0	16	100	1,441	13,082	37,846	60,623
Bosnia and Herzegovina	0	0	0	0	0	51,526	56,183	57,705
Germany	21,652	37,580	41,793	38,974	37,558	38,155	48,158	49,586
Turkey	87	202	3,768	14,357	25,528	31,894	42,527	46,373
Panel B: Largest source countries 1950								
Finland	44,821	101,307	235,453	251,342	217,636	195,447	169,521	156,045
Norway	31,312	37,253	44,681	42,863	52,744	42,464	43,430	42,047
Estonia	25,062	*	18,513	15,331	11,971	10,253	10,010	10,303
Denmark	22,801	35,112	39,152	43,501	43,931	38,190	45,584	41,870
Germany	21,652	37,580	41,793	38,974	37,558	38,155	48,158	49,586
United States	10,713	10,874	12,646	11,980	13,001	14,413	17,179	19,515
Poland	7,832	6,347	10,851	19,967	35,631	40,123	70,253	85,517
Latvia	4,423	*	3,244	2,664	2,025	2,305	4,686	7,026
Czechoslovakia	3,548	3,562	7,392	7,529	8,432	7,304	5,970	5,293
Austria	2,665	5,809	7,927	6,995	6,530	6,021	5,829	5,772
Panel C: Source countries by continents								
The nordic countries	99,080	174,043	320,913	341,253	319,082	279,631	263,227	245,633
EU25 (excluding the nordic countries)	75,631	75,138"	137,251	148,459	164,961	172,599	274,247"'	331,026
Europe (excluding EU25 and the nordic countries)	1,766	4,048	43,104	57,292	81,885	189,766	215,975"'	238,565
Africa	355	596	4,149	10,025	27,343	55,138	114,853	178,624
North America	11,334	11,665	15,629	14,484	19,087	24,312	31,263	35,780
South America	412	679	2,300	17,206	44,230	50,853	63,725	68,571
Asia	905	1,476	5,949	30,351	124,447	220,677	410,083	565,050
Oceania	93	211	558	962	1,866	2,981	4,529	5,245
Unknown	137	162	488	97	73	257	818	1,148
Panel D: Non-Western foreign-born								
Non-Western	48,904	30,070	130,804	201,373	380,945	623,042	991,482	1,285,961
Panel E: Total immigration								
Total Foreign-born	197,810	229,879	537,585	626,953	790,445	1,003,798	1,384,929	1,676,264
Percent Foreign-born	2.8	3.1	6.7	7.5	9.2	11.3	14.7	17.0
Total Population	7,041,829	7,495,129	8,076,903	8,317,235	8,590,630	8,882,792	9,415,570	9,851,017

Notes: * Included in the calculation of Soviet Union immigrants; ' Including Estonia, Latvia and Lithuania; " Excluding Estonia, Latvia and Lithuania; "'Calculation based on EU28. Source: Authors' calculations based on data from Statistics Sweden (2015)

Table A2: Neighborhood crossovers

Year of tipping	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999
Neighborhoods	156	16	9	8	9	5	6	4	4	1

Notes: The values represent the number of neighborhoods that moved above the identified tipping points in each year of the decade. The sample used is the 1/3 sample not used for identifying the tipping points.

Table A3: Regression discontinuity models for changes in residential population composition and ethnic segregation around candidate tipping points, alternative specifications

Treatment Measure	Native Growth (i)	Immigrant Growth (ii)	Population Growth (iii)	Overexposure (iv)
Panel A: Excluding Crossovers				
Beyond TP	-0.144***	0.041**	-0.104***	0.223***
	(0.035)	(0.016)	(0.038)	(0.079)
Observations	458	458	458	458
Panel B: Pre-1995 Tipping				
Beyond TP	-0.134***	0.046***	-0.089***	0.242***
	(0.032)	(0.015)	(0.028)	(0.068)
Observations	520	520	520	520
Panel C: Excluding Pre-1995 Crossovers				
Beyond TP	-0.146***	0.032*	-0.114***	0.199***
	(0.042)	(0.016)	(0.048)	(0.057)
Observations	478	478	478	478

Notes: Results are obtained by estimating modified version of equation (4). The unit of observation is a neighborhood as identified by the SAMS code. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. The sample used for estimation is the 1/3 sample not used for identifying the tipping points. Demographic controls are years of schooling, income and gender, all measured in the base year. The regressions are weighted by the size of the neighborhoods. All specifications include metropolitan area fixed effects. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A4: Replication of Card et al. (2008)

	Native Growth	Immigrant Growth	Population Growth
Beyond TP	-0.090**	0.017	-0.074*
	(0.039)	(0.021)	(0.043)
R-Squared	0.304	0.362	0.081
Observations	520	520	520

Notes: Results are obtained by estimating equation (2). The unit of observation is a neighborhood as identified by the SAMS code. Dependent variables are changes in the relevant populations between 1990 and 2000 as a percentage of the total baseline population in 1990. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. The sample used for estimation is the 1/3 sample not used for identifying the tipping points. Demographic controls are years of schooling, income and gender, all measured in the base year. The regressions are weighted by the size of the neighborhoods. All specifications include metropolitan area fixed effects. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A5: Regression discontinuity models for changes in ethnic segregation around candidate tipping points, excluding one municipality at a time

	Excluding Gothenburg	Excluding Malmo	Excluding Stockholm
Beyond TP	0.300***	0.270***	0.250***
	(0.088)	(0.089)	(0.092)
R-squared	0.355	0.360	0.309
Observations	320	444	276

Notes: Results are obtained by estimating equation (4). The unit of observation is a neighborhood as identified by the SAMS code. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. The sample used for estimation is the 1/3 sample not used for identifying the tipping points. Demographic controls are years of schooling, income and gender, all measured in the base year. The regressions are weighted by the size of the neighborhoods. All specifications include metropolitan area fixed effects. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A6: Donut-style regression discontinuity models for changes in residential population composition and ethnic segregation around candidate tipping points

	Native Growth	Immigrant Growth	Population Growth	Overexposure
Panel A: 0.10 Donut Hole				
Beyond TP	-0.167***	0.056***	-0.111***	-0.297***
	(0.038)	(0.017)	(0.033)	(0.079)
Panel B: 0.30 Donut Hole				
Beyond TP	-0.166***	0.056***	-0.110***	-0.293***
	(0.040)	(0.019)	(0.033)	(0.085)
Panel C: 0.50 Donut Hole				
Beyond TP	-0.172***	0.054***	-0.118***	0.290***
	(0.038)	(0.020)	(0.033)	(0.088)
Panel D: 1.00 Donut Hole				
Beyond TP	-0.181***	0.060***	-0.121***	0.316***
	(0.039)	(0.021)	(0.034)	(0.093)

Notes: Results are obtained by estimating a modified version of equation (4), where neighborhoods with base year immigrant shares +/- 0.05 (Panel A), 0.15 (Panel B), 0.25 (Panel C) and 0.50 (Panel D) of the identified tipping point are excluded from the estimation. The unit of observation is a neighborhood as identified by the SAMS code. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. The sample used for estimation is the 1/3 sample not used for identifying the tipping points. Demographic controls are years of schooling, income and gender, all measured in the base year. The regressions are weighted by the size of the neighborhoods. All specifications include metropolitan area fixed effects. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A7: Regression discontinuity models for population changes around candidate tipping points, Western immigrants

	Native Growth	Immigrant Growth	Total Growth	Overexposure
Beyond TP	0.006	-0.060	-0.053	-0.107
	(0.053)	(0.043)	(0.064)	(0.071)
Observations	520	520	520	520
R-Squared	0.0876	0.1932	0.3367	0.4007

Notes: The results are obtained by re-estimating the tipping points using equation (1), and then using these results to re-estimate equation (4). The unit of observation is a neighborhood as identified by the SAMS code. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. The sample used for estimation is the 1/3 sample not used for identifying the tipping points. Demographic controls are years of schooling, income and gender, all measured in the base year. The regressions are weighted by the size of the neighborhoods. All specifications include metropolitan area fixed effects. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A8: Regression discontinuity models for changes in residential population composition and ethnic segregation around candidate tipping points, local linear regression

Treatment Measure	Native Growth (i)	Immigrant Growth (ii)	Population Growth (iii)	Overexposure (iv)
Beyond TP	-0.169***	0.046**	-0.123***	0.256**
	(0.050)	(0.020)	(0.041)	(0.096)
Baseline Controls	x	x	x	x
R-squared	0.208	0.315	0.084	0.352
Observations	433	433	433	433

Notes: Bandwidth chosen using the cross-validation method proposed by Ludwig and Miller (2005). $h = 11.58483$. The sample used for estimation is the 1/3 sample not used for identifying the tipping points. Demographic controls are years of schooling, income and gender, all measured in the base year. The regressions are weighted by the size of the neighborhoods. All specifications include metropolitan area fixed effects. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A9: Selective migration

	Baseline	Educational Level High	Educational Level Low	Income Level High	Income Level Low	Gender Men	Gender Women	Excluding Outliers
Panel A: Young Cohort								
Control to Treatment	-0.094***	-0.068***	-0.123***	-0.068***	-0.122***	-0.090***	-0.070***	-0.099***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.004)	(0.003)	(0.005)
Treatment to Control	0.190***	0.181***	0.190***	0.168***	0.205***	0.189***	0.226***	0.164***
	(0.032)	(0.036)	(0.030)	(0.034)	(0.031)	(0.033)	(0.015)	(0.034)
Observations	200,695	91,959	108,736	49,170	48,677	103,008	97,687	197,789
Panel B: Middle Cohort								
Control to Treatment	-0.182***	-0.146***	-0.216***	-0.165***	-0.193***	-0.176***	-0.149***	-0.189***
	(0.009)	(0.008)	(0.011)	(0.008)	(0.011)	(0.009)	(0.005)	(0.011)
Treatment to Control	0.187***	0.194***	0.178***	0.192***	0.189***	0.181***	0.238***	0.161***
	(0.033)	(0.041)	(0.029)	(0.044)	(0.031)	(0.033)	(0.015)	(0.036)
Observations	191,654	85,877	105,777	46,715	46,731	98,369	93,285	187,008
Panel C: Old Cohort								
Control to Treatment	-0.100***	-0.074***	-0.120***	-0.071***	-0.106***	-0.107***	-0.067***	-0.103***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.003)	(0.004)
Treatment to Control	0.189***	0.214***	0.172***	0.235***	0.173***	0.198***	0.212***	0.164***
	(0.037)	(0.046)	(0.032)	(0.051)	(0.033)	(0.039)	(0.017)	(0.038)
Observations	279,472	106,610	172,862	64,443	64,508	141,502	137,970	275,254

Notes: The results are obtained by estimating equation (5). The unit of observation is an individual residing in one of the 1560 neighborhoods included in our analysis. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Demographic controls are gender, mother's education, father's education, parental income and binaries for whether this information was not available for the individual, all measured in the base year. For the Old Cohort, parental education and income have been replaced with own education and income. All models include birth year and metropolitan area fixed effects. Immigrants refer to individuals born in, or have at least one parent born in, a non-Western country. Column (2) and (3) stratify the sample based on whether the individual has at least one parent with post-secondary education for the young and middle cohorts, and based on whether the individual has or does not have post-secondary education for the old cohort. Columns (4) and (5) stratify the sample based on whether the individual's parental income is in the bottom or top quartile of the income distribution for the youth and middle cohorts, and based on whether the individual is in the bottom or the top quartile of the income distribution for the old cohort. Columns (6) and (7) statify the sample based on gender.Column (8) exclude individuals from neighborhoods in the right-tail of the immigrant share distribution. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A10: Effect of tipping on cognitive and non-cognitive military test scores

	Baseline	Population Density	>50% Tipped Neighbors	<50% Tipped Neighbors	Excluding Outliers	Low Parental Education	High Parental Education	Low Income	High Income
Panel A: Immigrants									
i. 1973-1983									
Cognitive	-0.052	-0.060	-0.055	-0.042	0.030	0.001	-0.104	0.079	-0.319
	(0.081)	(0.080)	(0.111)	(0.116)	(0.076)	(0.155)	(0.121)	(0.153)	(0.208)
Non-Cognitive	-0.145	-0.143	-0.052	-0.162	-0.100	-0.159	-0.132	-0.067	-0.261
	0.133)	(0.131)	(0.110)	(0.149)	(0.140)	(0.226)	(0.128)	(0.183)	(0.187)
i. 1973-1980									
Cognitive	-0.092	-0.092	-0.061	-0.016	-0.011	-0.049	-0.120	-0.025	-0.349
	(0.100)	(0.096)	(0.130)	(0.142)	(0.097)	(0.177)	(0.170)	(0.170)	(0.274)
Non-Cognitive	-0.149	-0.138	0.023	-0.168	-0.113	-0.261	-0.080	-0.135	-0.121
	(0.154)	(0.150)	(0.144)	(0.202)	(0.162)	(0.199)	(0.215)	(0.174)	(0.216)
Panel B: Natives									
i. 1973-1983									
Cognitive	-0.051	-0.051	-0.050	0.004	0.020	-0.074*	-0.007	-0.115*	-0.130*
	(0.043)	(0.043)	(0.042)	(0.055)	(0.047)	(0.042)	(0.049)	(0.061)	(0.076)
Non-Cognitive	-0.014	-0.042	0.037	0.034	0.007	-0.042	0.026	-0.073	0.058
	(0.034)	(0.029)	(0.034)	(0.042)	(0.035)	(0.035)	(0.050)	(0.066)	(0.100)
i. 1973-1980									
Cognitive	-0.047	-0.049	-0.040	-0.007	0.016	-0.048	-0.016	-0.103	-0.106
	(0.049)	(0.049)	(0.049)	(0.062)	(0.054)	(0.051)	(0.054)	(0.062)	(0.081)
Non-Cognitive	-0.002	-0.031	0.036	0.045	0.022	-0.046	0.055	-0.072	0.148
	(0.036)	(0.031)	(0.033)	(0.050)	(0.036)	(0.039)	(0.054)	(0.074)	(0.097)

Notes: Results are obtained by estimating equation (5). The unit of observation is an individual starting school between 1980 and 1990 that resides in one of the 1560 neighborhoods included in our analysis. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Demographic controls are gender, mother's education, father's education, parental income and indicators for whether this information was not available for the individual. All models include birth year and metropolitan area fixed effects. Immigrants refer to individuals born in, or have at least one parent born in, a non-Western country. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A11: Fraction of individuals that maintain treatment status over time

	All		Natives		Immigrants	
Year	Control	Treatment	Control	Treatment	Control	Treatment
1991	0.97	0.92	0.98	0.91	0.93	0.96
1992	0.96	0.89	0.97	0.87	0.89	0.94
1993	0.95	0.85	0.96	0.82	0.87	0.91
1994	0.94	0.81	0.95	0.78	0.85	0.88
1995	0.93	0.79	0.94	0.75	0.83	0.87
1996	0.92	0.77	0.93	0.72	0.81	0.85
1997	0.91	0.74	0.92	0.70	0.80	0.83
1998	0.90	0.72	0.91	0.67	0.79	0.82
1999	0.89	0.71	0.90	0.65	0.79	0.81
2000	0.89	0.69	0.90	0.64	0.79	0.80

Notes: The unit of observation is an individual residing in one of the 1560 neighborhoods included in our analysis. The Treatment columns depict the fraction of individuals that reside in a neighborhood subject to tipping in the base year and remain in a neighborhood subject to tipping in year t. The Control columns depict the fraction of individuals that reside in a neighborhood not subject to tipping in the base year and remain in a neighborhood not subject to tipping in year t.

Table A12: Neighborhood population density

	All	Stockholm	Gothenburg	Malmo
Mean	4074.34	2437.36	5595.21	5326.58
S.D.	4535.89	2992.11	5390.15	4414.88

Notes: Authors' own calculations based on information on land size from Jan Amcoff and data from IFAU. See Amcoff (2012) for the methods he employed to obtain land size values.

Table A13: Tipping behavior of neighboring neighborhoods

	Mean	Standard Deviation	No Tipped Neighbors	All Neighbors Tipped	Number of Tipped Neighborhoods
All	0.62	0.35	0.12	0.25	459
Stockholm	0.43	0.29	0.17	0.08	166
Gothenburg	0.75	0.34	0.09	0.53	208
Malmo	0.65	0.31	0.09	0.24	85

Notes: Authors' own calculations using Statistic Sweden's SAMS Atlas. First, we identify neighborhoods that have tipped. Second, we use the Atlas to manually obtain the names of the neighborhoods surrounding the tipped neighborhoods. Finally, we use our data to identify the fraction of these neighborhoods that have tipped.

Table A14: The effect of tipping on short-term labor market outcomes

	Social Insurance Benefits	Self-Employment Income	Employment Income	Government-Funded Benefits
Panel A: Immigrants				
i. Intensive Margin				
Beyond TP	0.052	0.080	-0.009	-0.002
	(0.052)	(0.101)	(0.029)	(0.054)
Observations	19,679	5,150	50,918	35,396
ii. Extensive Margin				
Beyond TP	0.019*	-0.005	0.018	0.010
	(0.009)	(0.004)	(0.014)	(0.013)
Observations	68,925	68,925	68,925	68,925
Panel B: Natives				
i. Intensive Margin				
Beyond TP	-0.036	-0.066	0.015	-0.049
	(0.031)	(0.056)	(0.012)	(0.043)
Observations	75,899	18,634	251,987	107,316
i. Extensive Margin				
Beyond TP	-0.006	-0.008	0.011*	-0.013*
	(0.004)	(0.005)	(0.006)	(0.008)
Observations	279,472	279,472	279,472	279,472

Notes: These results are obtained by estimating equation (5). The unit of observation is an individual born between 1948 and 1958, residing in one of the 1560 neighborhoods included in our analysis. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Demographic controls are gender, years of schooling, income and indicators for whether this information was not available for the individual. All models include birth year and metropolitan area fixed effects. All dependent variables are measured in 2000. All controls are measured in 1990. Immigrants refer to individuals born in, or have at least one parent born in, a non-Western country. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A15: Descriptive statistics of neighborhoods included/excluded from analysis

	Included	Excluded
Fraction Natives	0.81(0.14)	0.82(0.21)
Fraction Females	0.49 (0.03)	0.43 (0.17)
Age	39.33 (2.97)	41.02 (6.19)
Years Since Migration	17.26 (3.92)	17.90 (8.12)
Fraction With University Education	0.10 (0.08)	0.08 (0.12)
Employment Income (000s SEK)	165.52 (46.81)	138.78 (70.34)
Fraction on Social Welfare	0.07 (0.09)	0.07 (0.16)
Native Growth Rate	0.09 (0.30)	2.48 (17.12)
Immigrant Growth Rate	0.07 (0.12)	0.91 (7.02)
Total Growth Rate	0.15 (0.33)	3.39 (23.80)

Notes: Authors' own calculations. Values represent unweighted means, and standard deviations are provided in brackets. Salary refers to income from primary occupation, and includes zeros.

Table A16: The effect of tipping for individuals that remained in their initial neighborhood throughout the decade

	9th Grade GPA	High School GPA	Academic Track	University Enrollment	Years of Schooling	Employment Income
Panel A: Immigrants						
I. Young Cohort						
Beyond TP	-1.715	-3.103*	-0.007	0.007	-0.010	0.046
	(2.421)	(1.831)	(0.050)	(0.025)	(0.147)	(0.075)
II. Middle Cohort						
Beyond TP	-4.942*	-2.626	-0.070*	-0.064*	-0.396*	-0.052
	(2.567)	(3.460)	(0.040)	(0.035)	(0.210)	(0.071)
III. Old Cohort						
Beyond TP	-	-	-	-	-	0.020
						(0.033)
Panel B: Natives						
I. Young Cohort						
Beyond TP	-2.547***	-0.517	-0.035**	-0.014	-0.085**	0.026
	(0.777)	(0.676)	(0.016)	(0.010)	(0.037)	(0.023)
II. Middle Cohort						
Beyond TP	-1.209	-0.423	-0.034*	-0.024**	-0.141***	-0.040
	(0.854)	(0.647)	(0.017)	(0.009)	(0.052)	(0.029)
III. Old Cohort						
Beyond TP	-	-	-	-	-	-0.040
						(0.023)

Note: These results are obtained by estimating equation (5). The unit of observation is an individual residing in one of the 1560 neighborhoods included in our analysis. Only individuals that remained in the base year (1990) neighborhood throughout the entire decade have been used for this analysis. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Demographic controls are gender, mother's education, father's education, parental income and binaries for whether this information was not available for the individual, all measured in the base year. For the old cohort, parental education and income have been replaced with own education and income. All models include birth year and metropolitan area fixed effects. University enrollment, years of schooling and employment income are measured as of 2011. Immigrants refer to individuals born in, or have at least one parent born in, a non-Western country. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

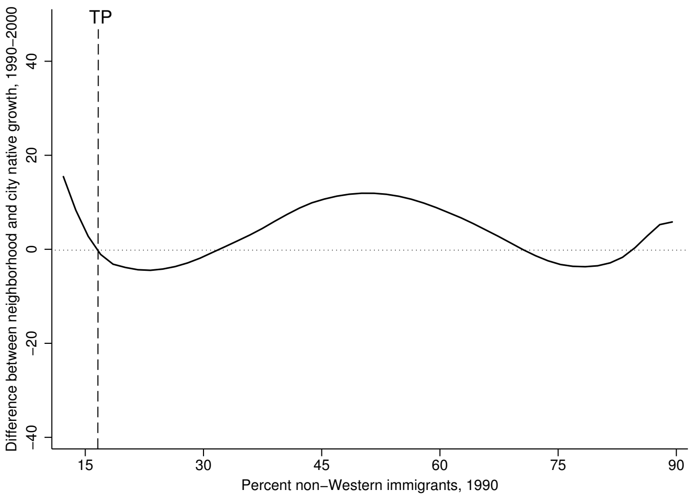


Figure A1: Illustration of the search method for identifying the tipping point
Notes: The figure demonstrates how the location of the tipping point is derived from equation (1) for a hypothetical city. The solid line depicts the growth function of neighborhood native population modelled as a fourth-order polynominal. The horizontal line shows where the dependent variable of equation (1) is equal to zero. The proposed tipping point is located at the intersection of this line and the growth function, denoted by the dashed vertical line. As illustrated in the Figure, and discussed in the text, there can be more than one root, and in such cases we follow Card et al. (2008) and pick the root associated with the most negative slope.

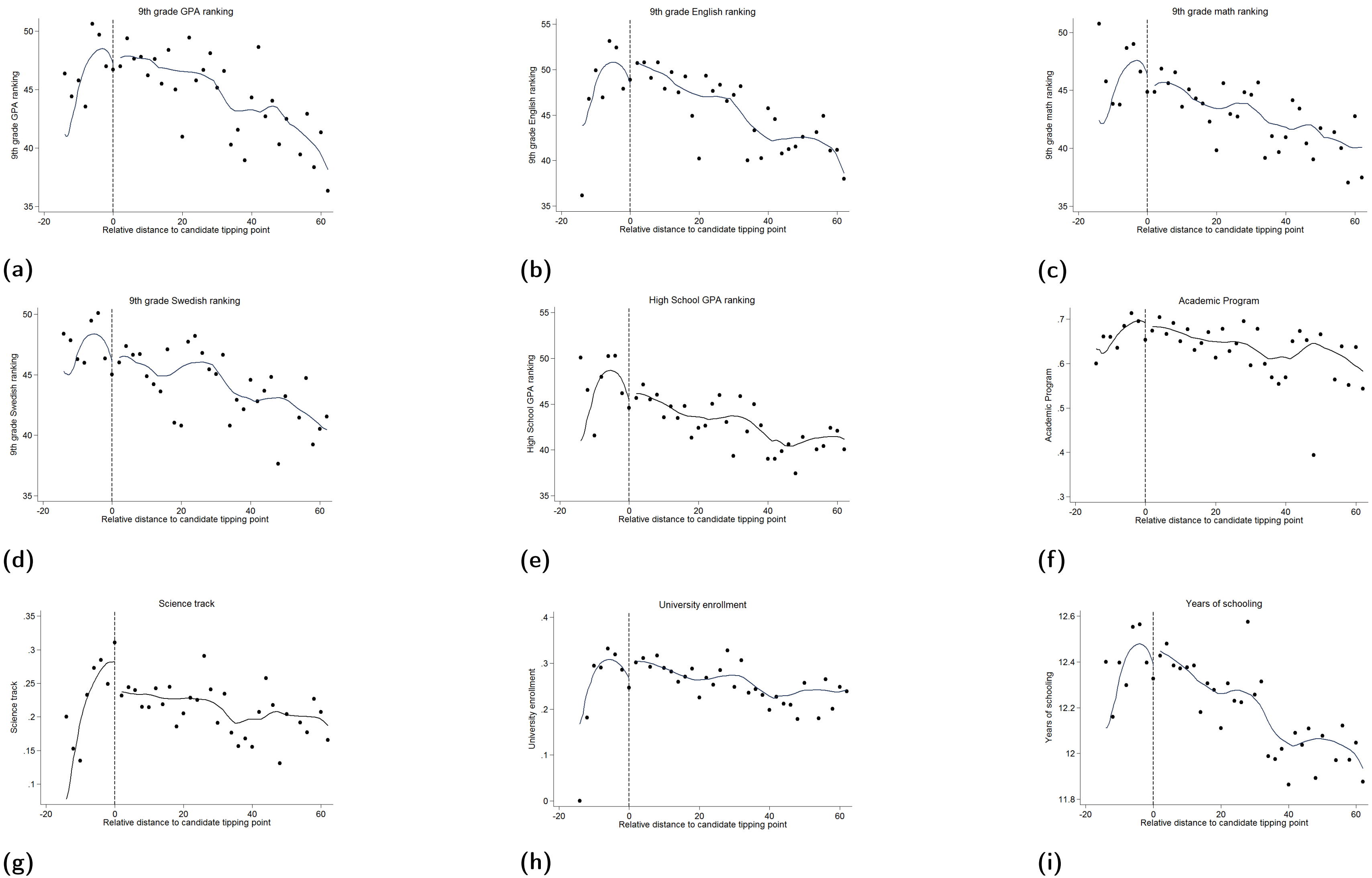


Figure A2: Discontinuities in educational attainment, young immigrants

Notes: Dots show mean values of educational attainment outcomes,grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.

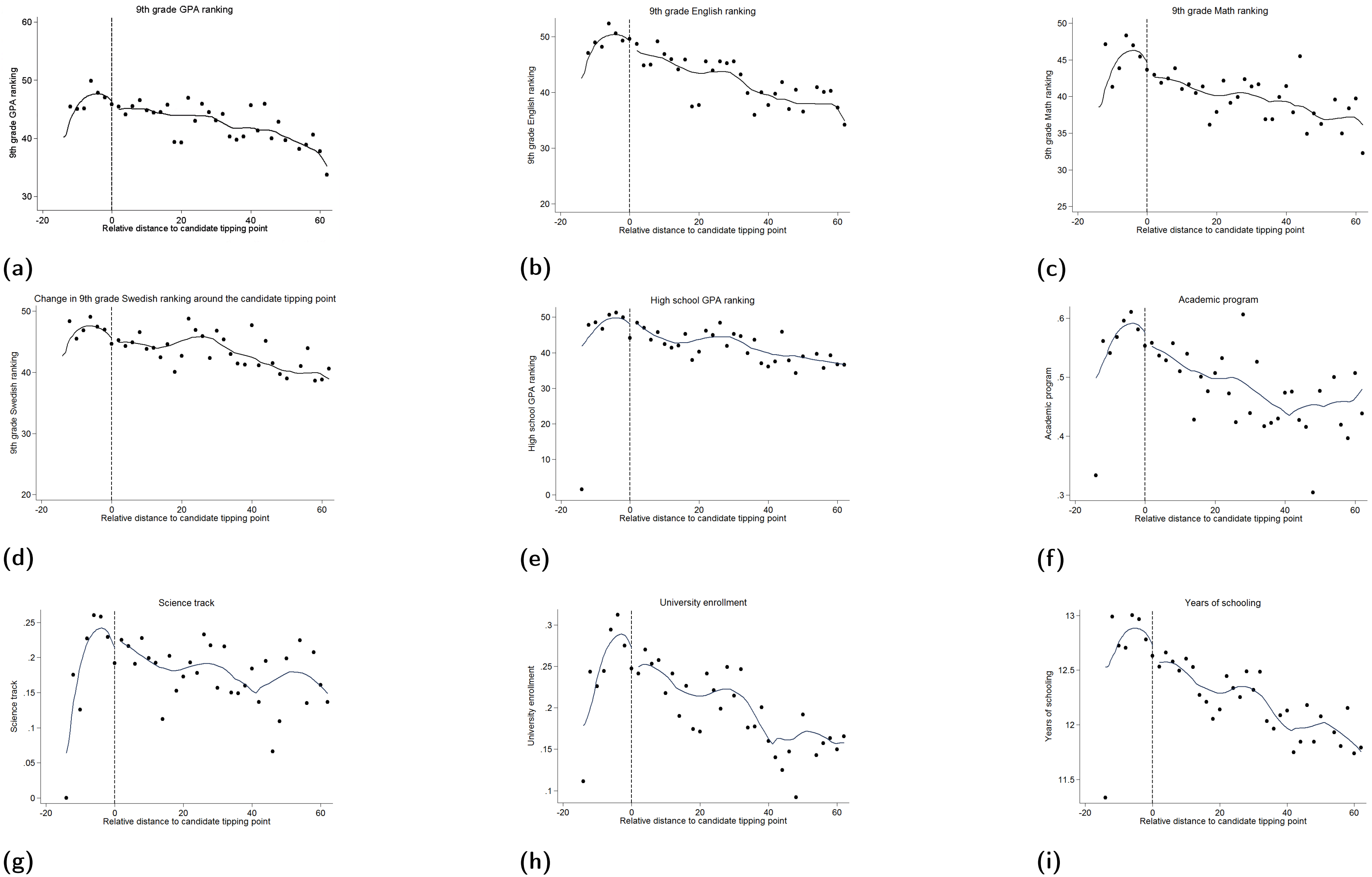


Figure A3: Discontinuities in educational attainment, middle immigrants

Notes: Dots show mean values of educational attainment outcomes,grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.

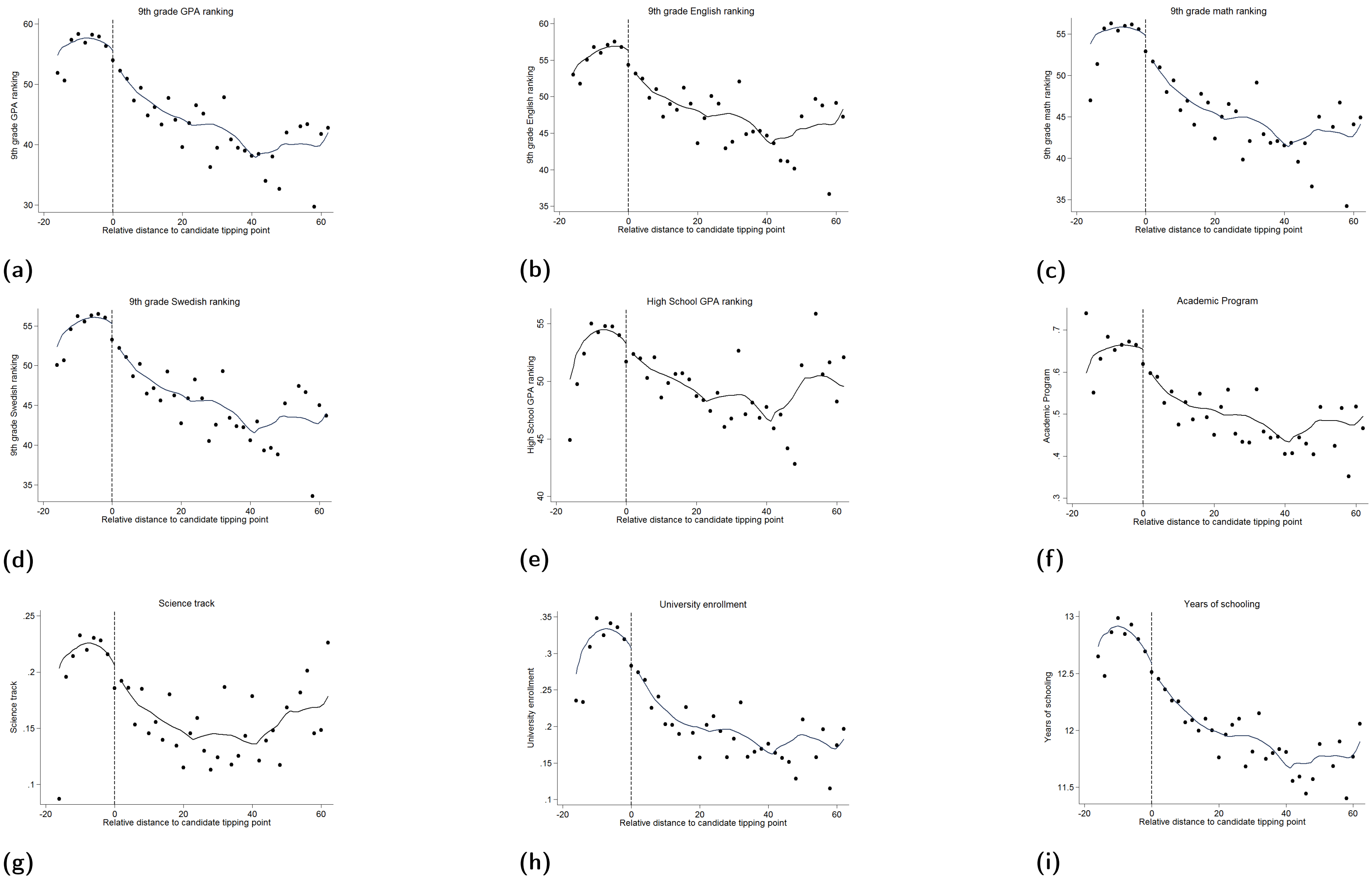
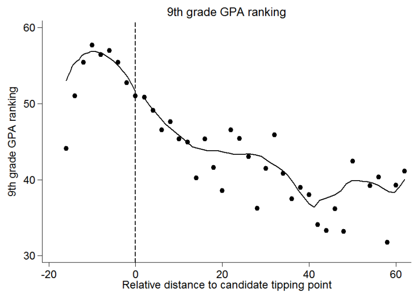


Figure A4: Discontinuities in educational attainment, young natives

Notes: Dots show mean values of educational attainment outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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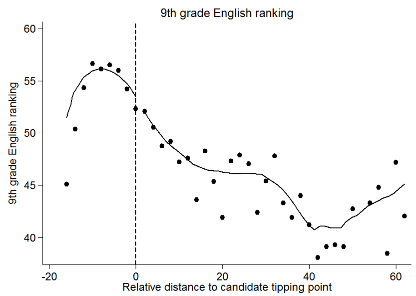

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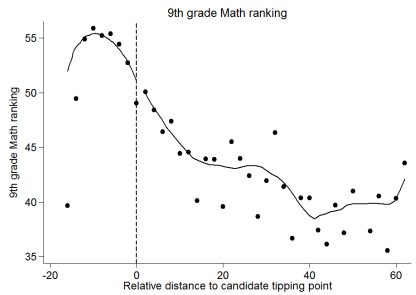

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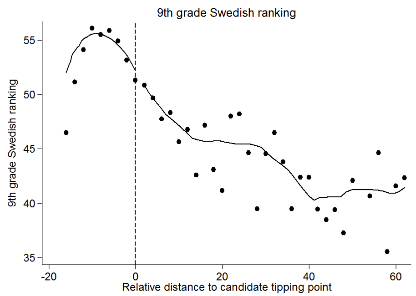

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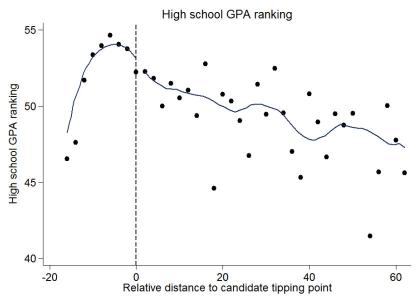

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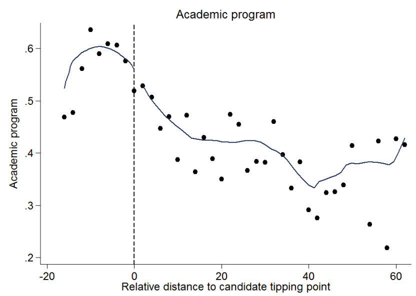

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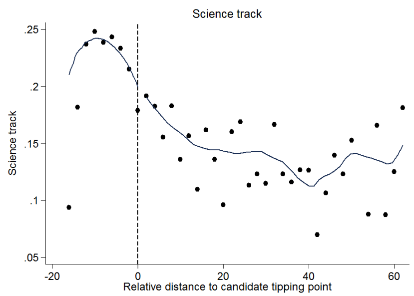

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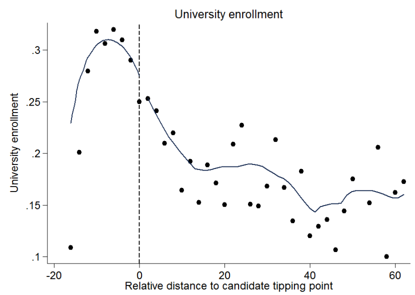

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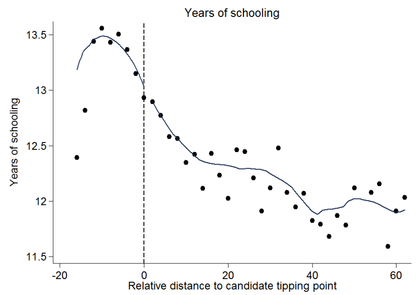
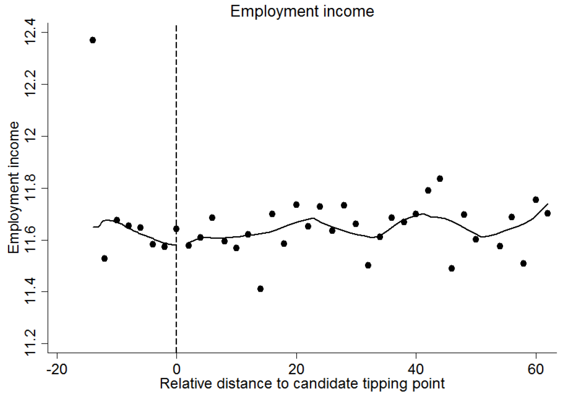


(i)

Figure A5: Discontinuities in educational attainment, middle natives

Notes: Dots show mean values of educational attainment outcomes,grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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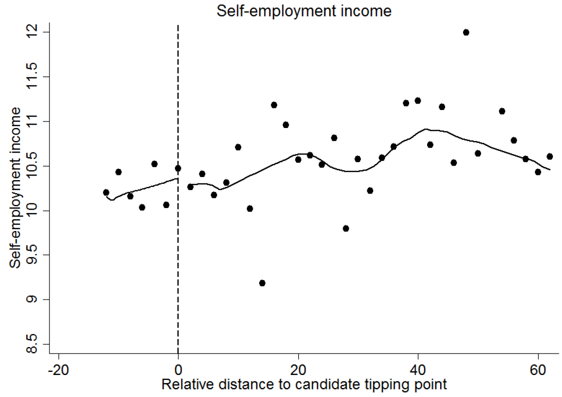

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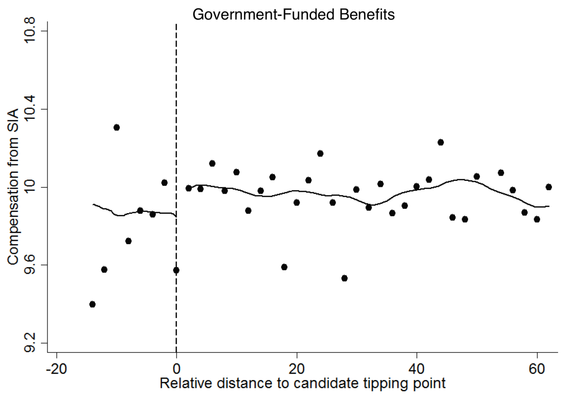

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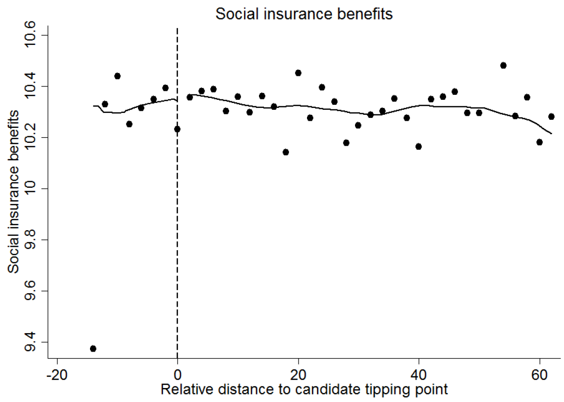
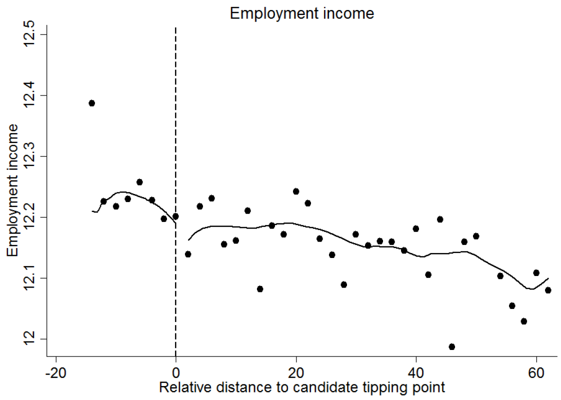


(d)

Figure A6: Discontinuities in labor market outcomes, young immigrants
Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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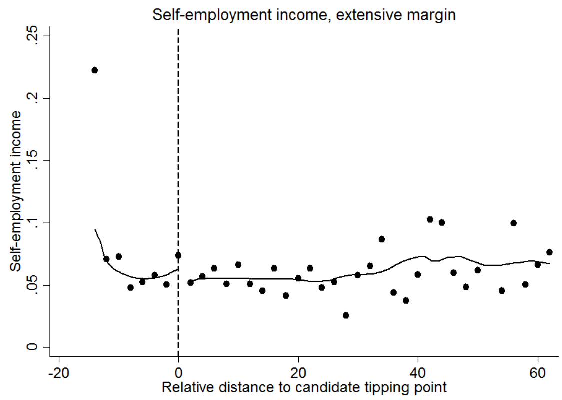

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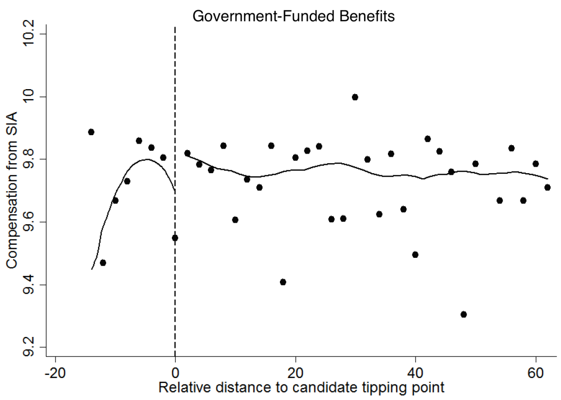

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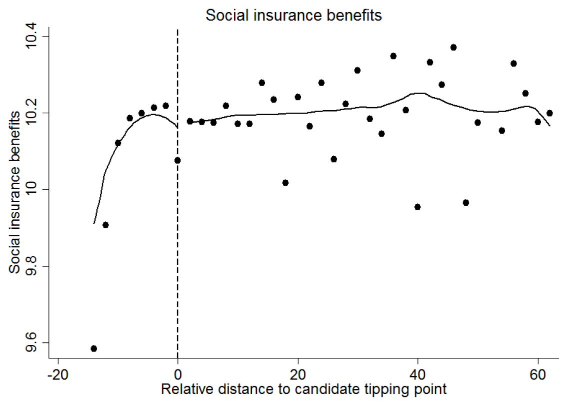

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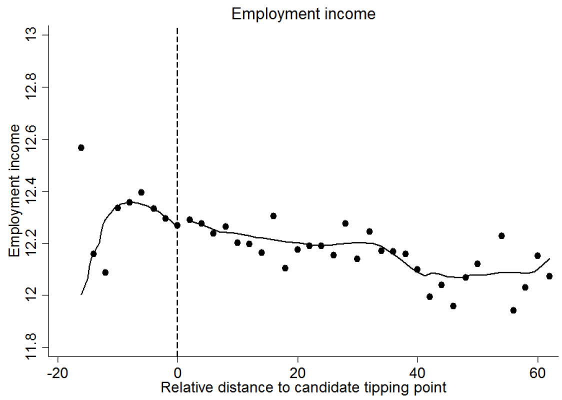
Figure A7: Discontinuities in labor market outcomes, middle immigrants
Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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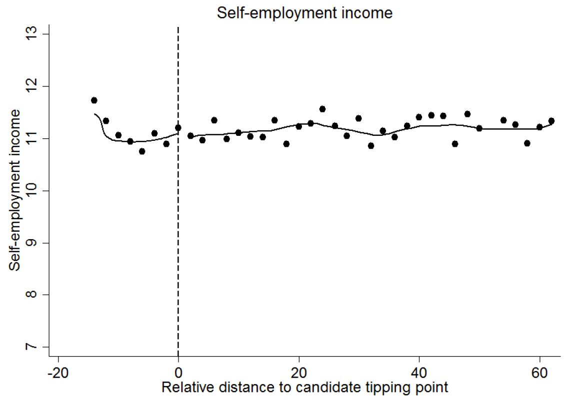

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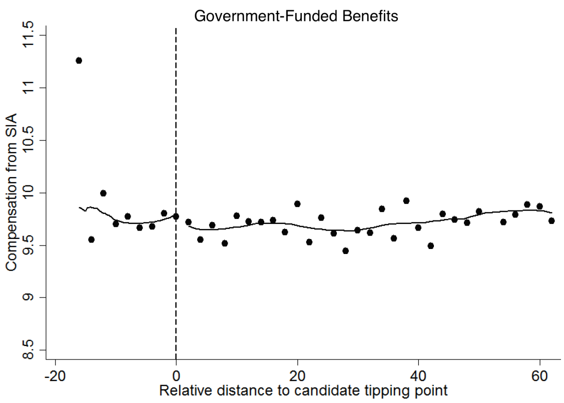

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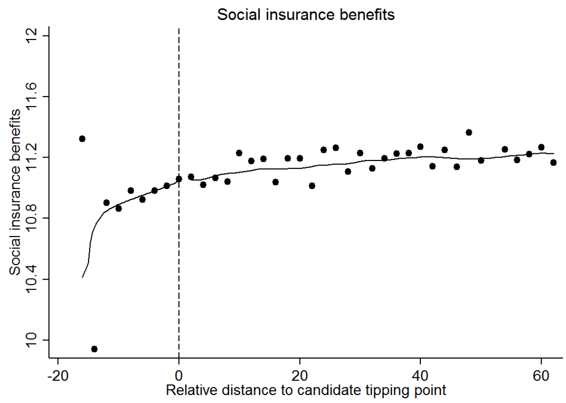

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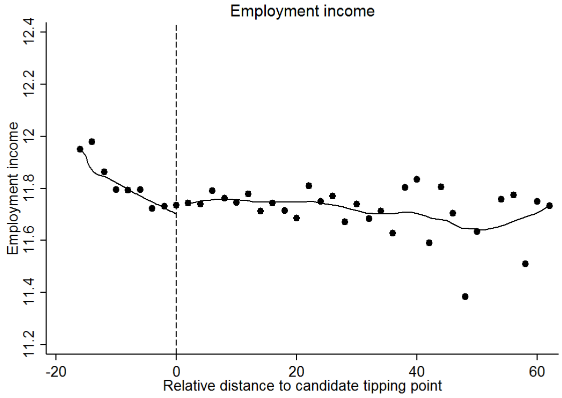
Figure A8: Discontinuities in labor market outcomes, old immigrants

Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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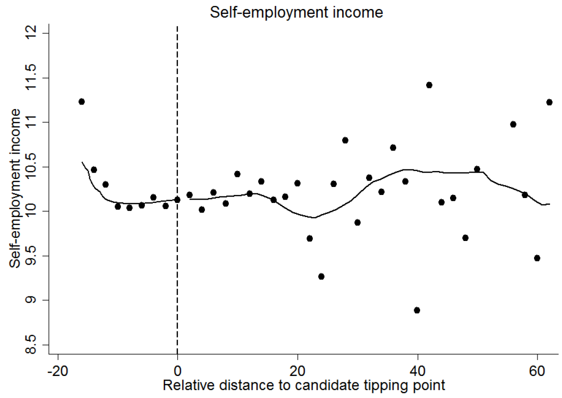

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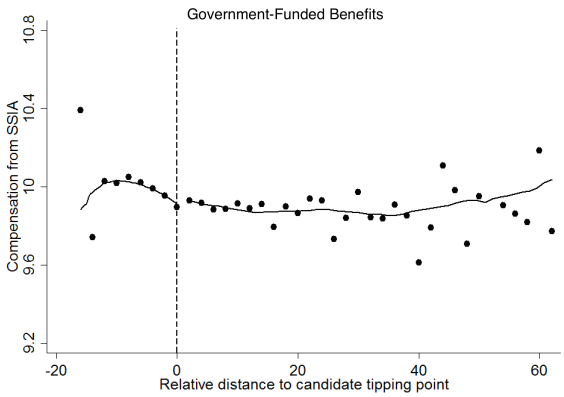

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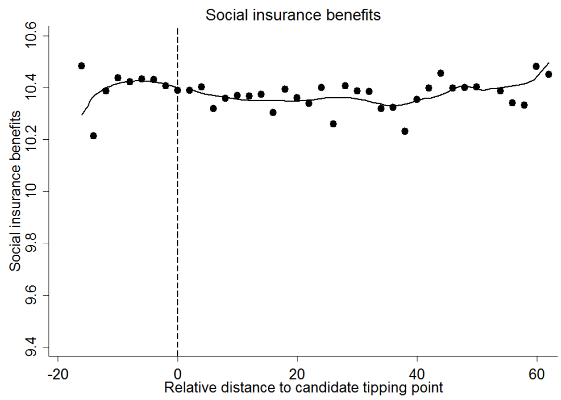
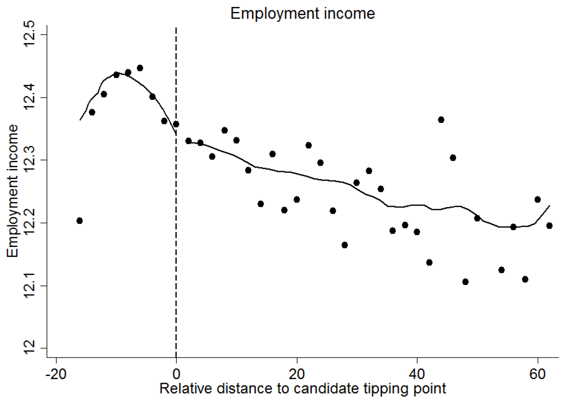


(d)

Figure A9: Discontinuities in labor market outcomes, young natives
Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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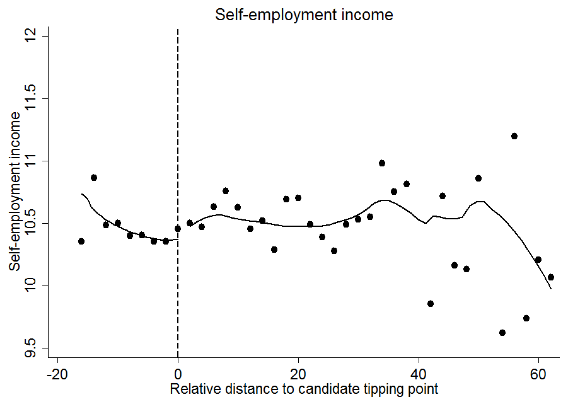

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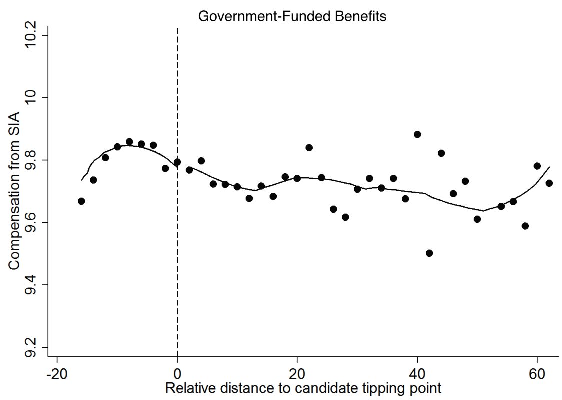

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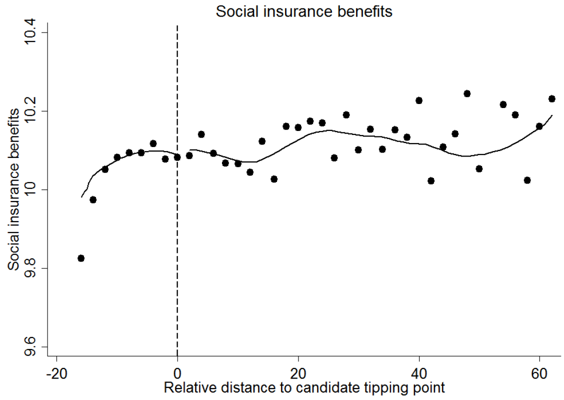
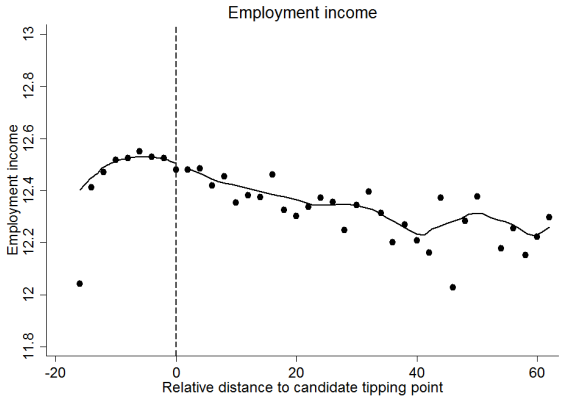


(d)

Figure A10: Discontinuities in labor market outcomes, middle natives
Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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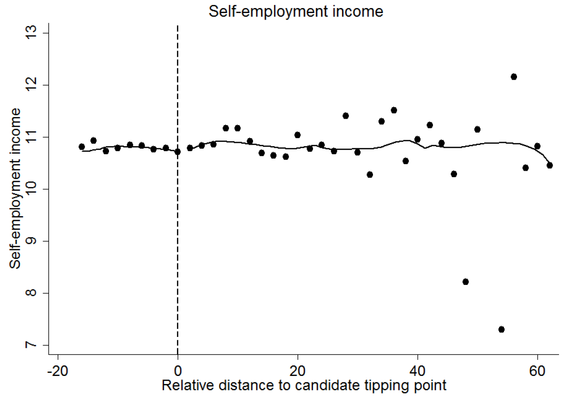

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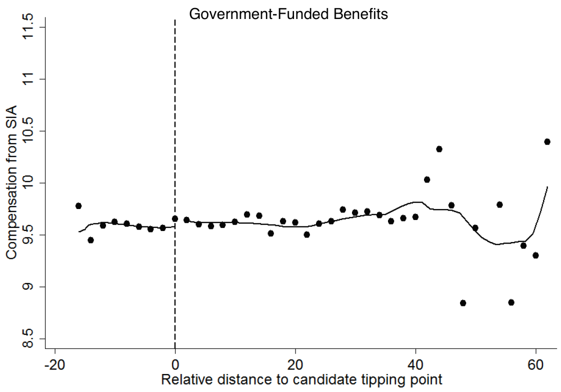

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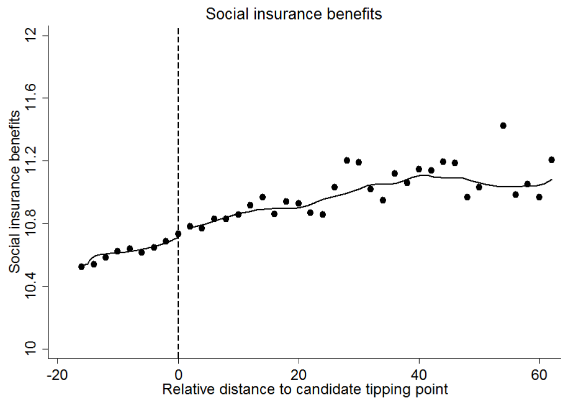
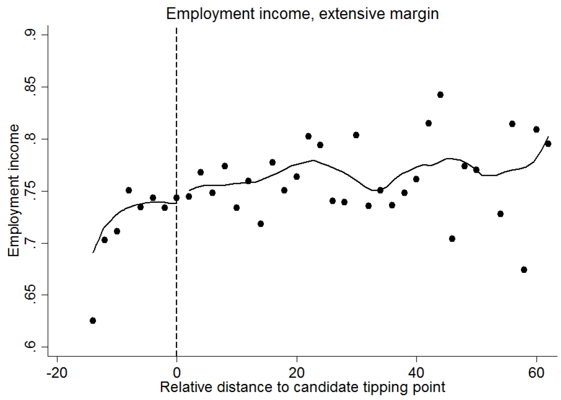


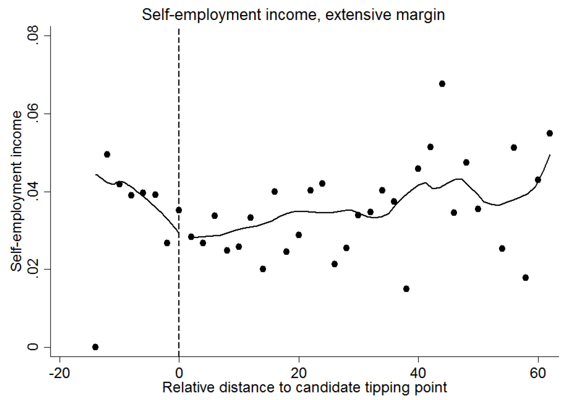
(d)

Figure A11: Discontinuities in labor market outcomes, old natives

Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



(a)



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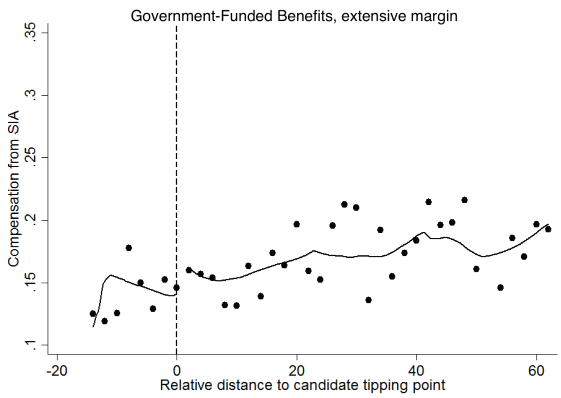

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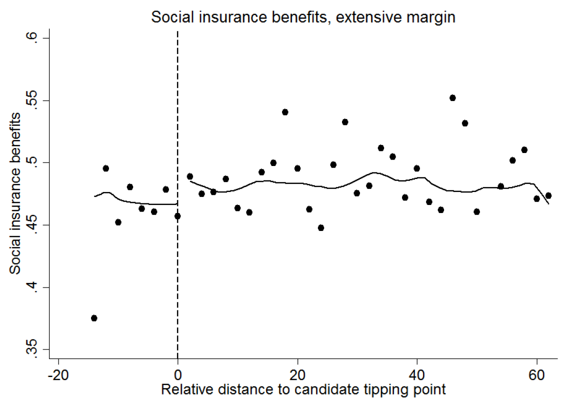
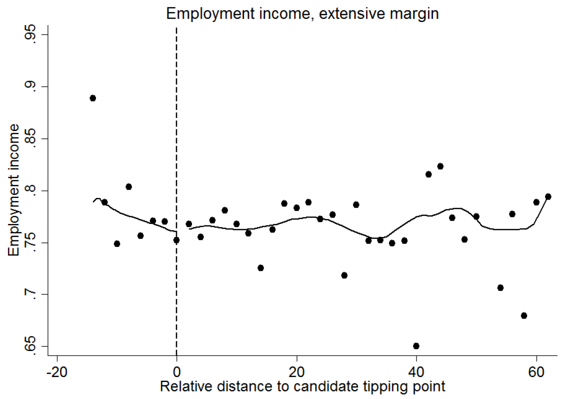


(d)

Figure A12: Discontinuities in extensive margin labor market outcomes, young immigrants
Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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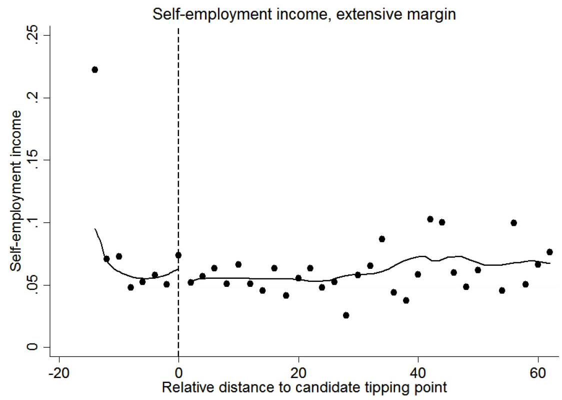

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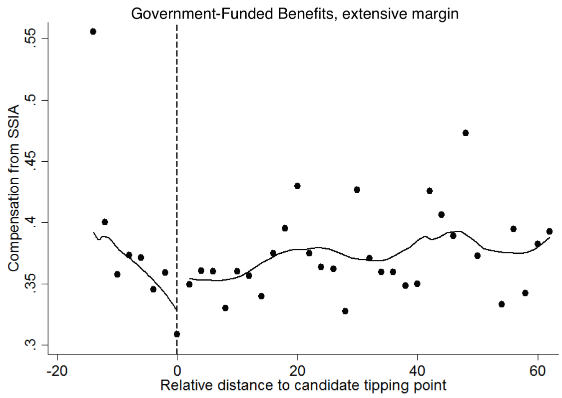

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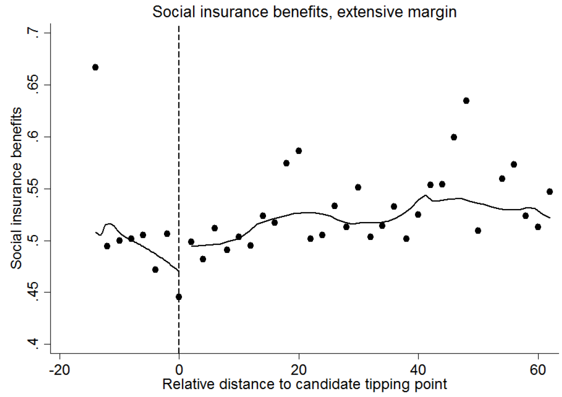
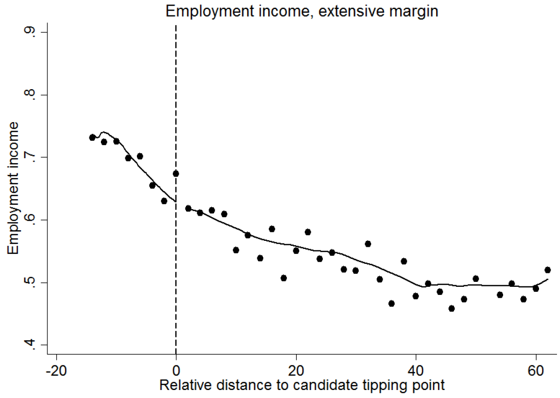
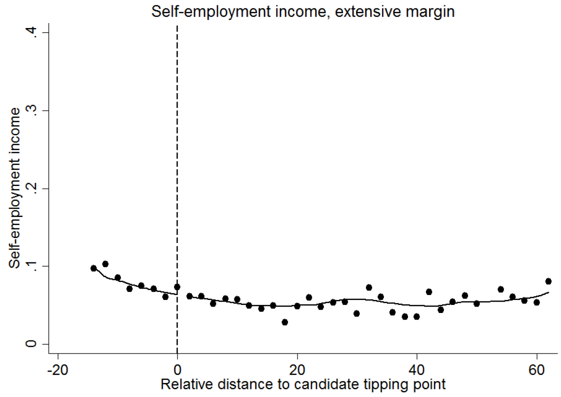


(d)

Figure A13: Discontinuities in extensive margin labor market outcomes, middle immigrants
Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



(a)



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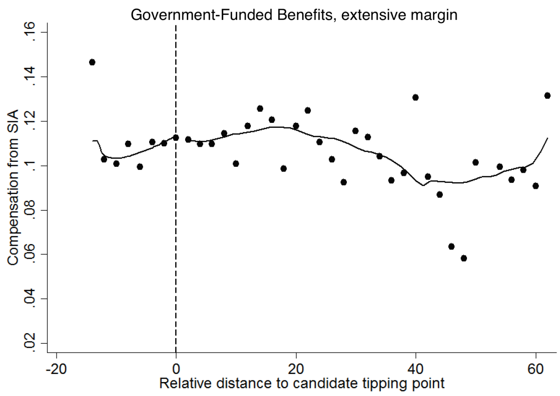

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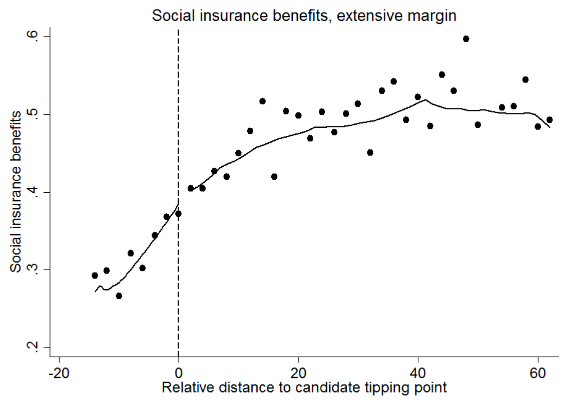
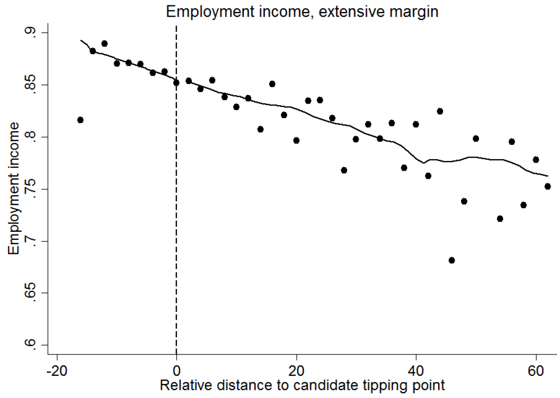


(d)

Figure A14: Discontinuities in extensive margin labor market outcomes, old immigrants

Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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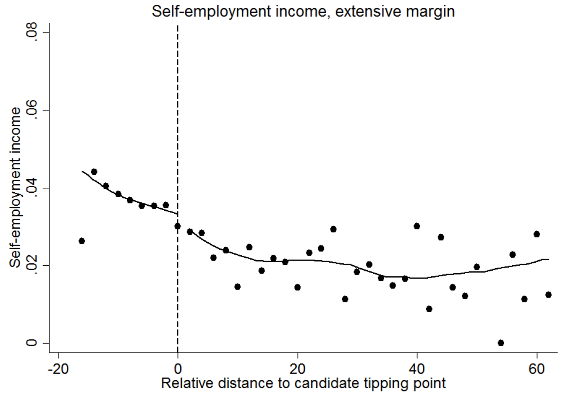

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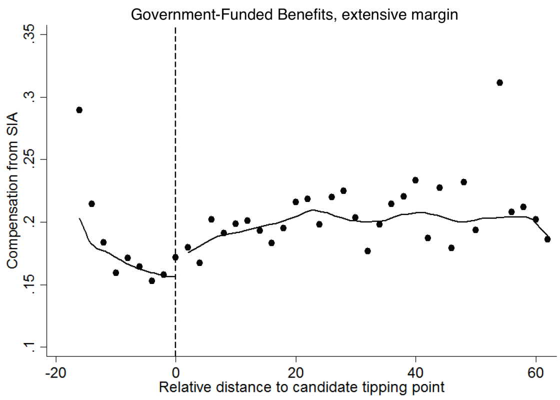

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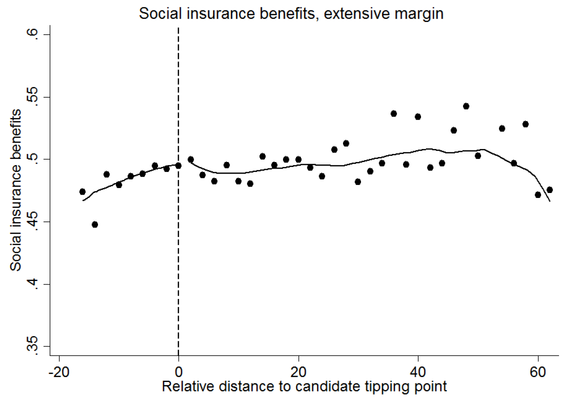
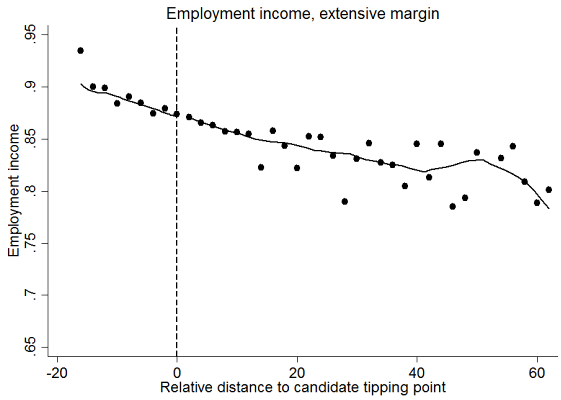


(d)

Figure A15: Discontinuities in extensive margin labor market outcomes, young natives
Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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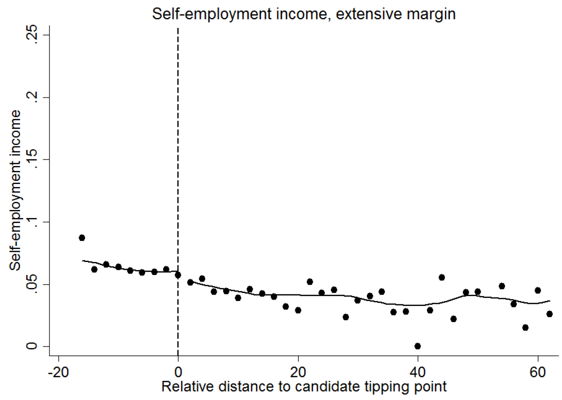

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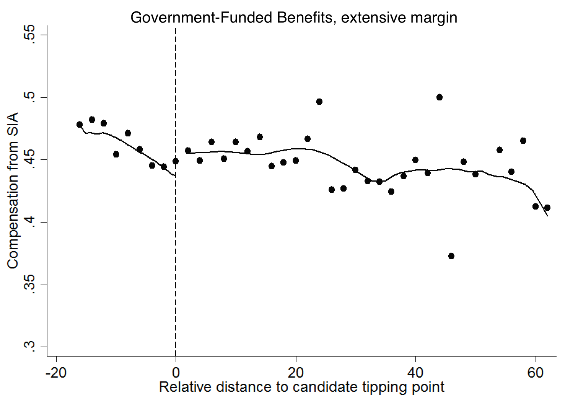

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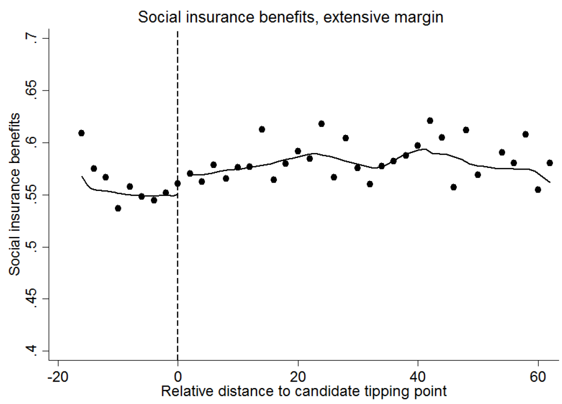
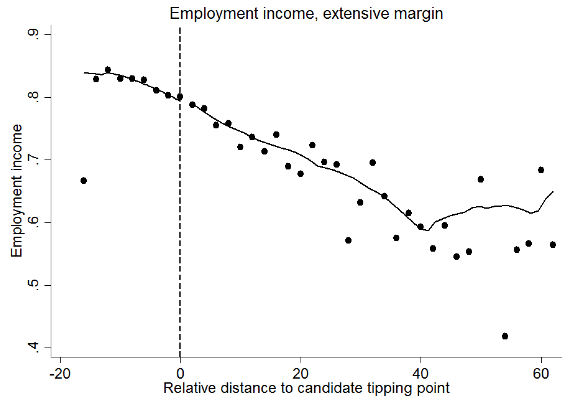


(d)

Figure A16: Discontinuities in extensive margin labor market outcomes, middle natives
Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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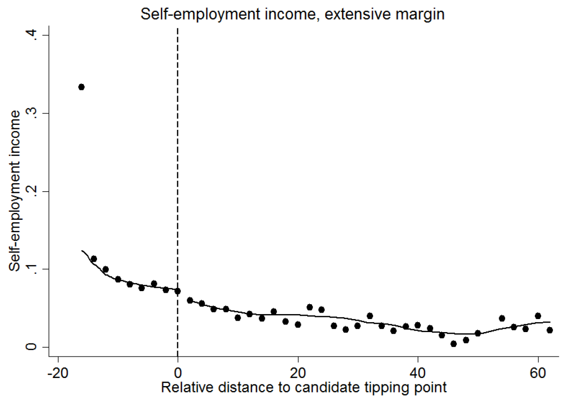

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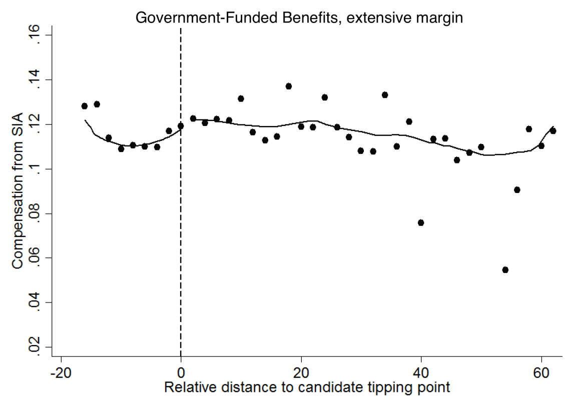

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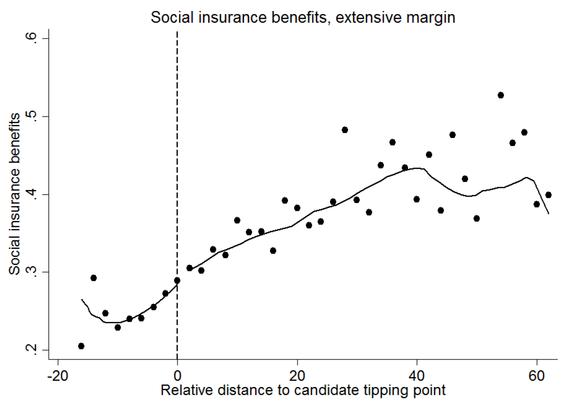
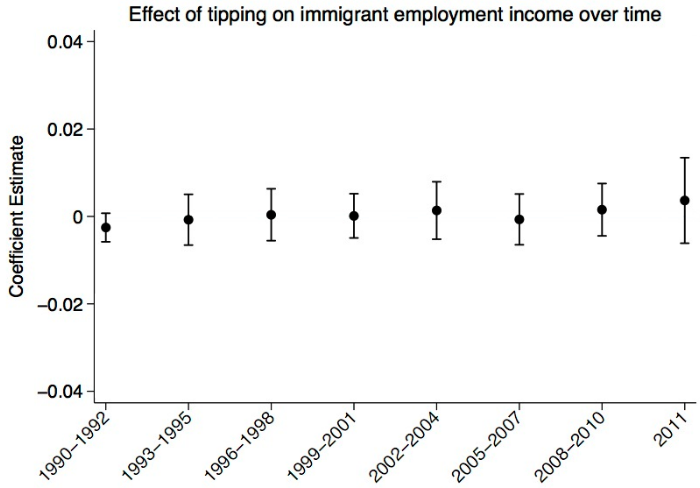


(d)

Figure A17: Discontinuities in extensive margin labor market outcomes, old natives

Notes: Dots show mean values of labor market outcomes, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines are local linear regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel and a bandwidth of 4.



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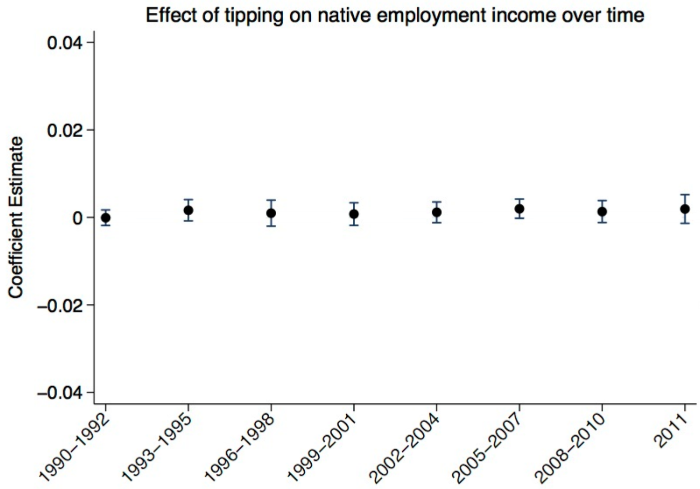

(b)

Figure A18: Time heterogeneity in treatment effects

Notes: The figure depicts the point estimates obtained from estimating equation (5) seperateley on three year averages of employment income, stratified by nativity status. The unit of observation is an individual born between 1948 and 1958 (Old Cohort) residing in one of the 1560 neighborhoods included in our analysis. All specifications include a quartic polynomial in the difference between the neighborhood's minority share and the estimated tipping point. Standard errors are clustered on one percent bins of the running variable. Demographic controls are gender, educational attainment, income and binaries for whether this information was not available for the individual, all measured in the base year. All models include birth year and municipality fixed effects. Natives refer to individuals not born in, and do not have a parent born in, a non-Western country. The bars depict the 95% confidence intervals associated with each point estimate.

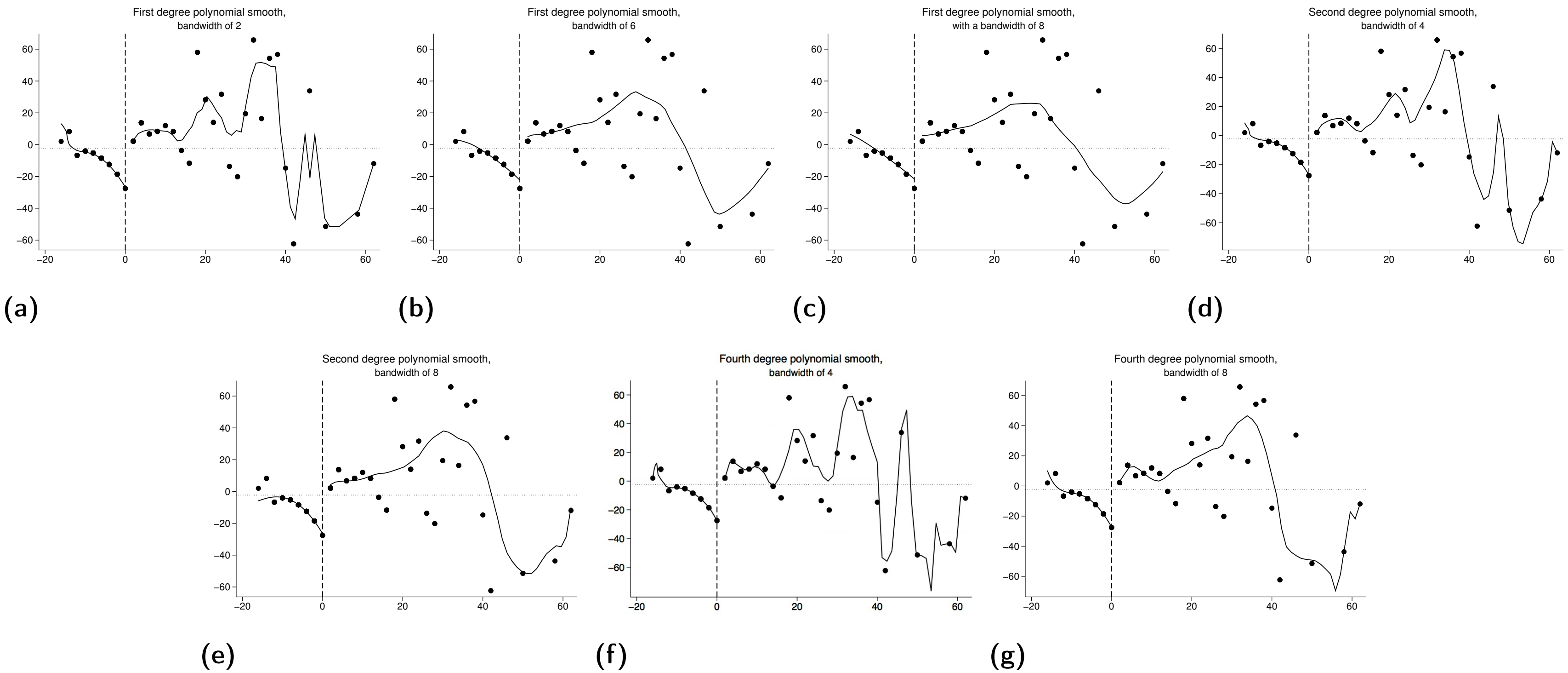

Figure A19: Discontinuity in ethnic residential segregation around candidate tipping point, alternative bandwidths and degrees of smoothing
Notes: Dots show mean change in neighborhood native population growth between 1990 and 2000, grouping neighborhoods into 2% bins by the deviation in immigrant share from the estimated tipping point at the year of tipping. For neighborhoods that did not tip, the running variable is based on base year values. The vertical lines depict the estimated tipping points (normalized to zero). The solid lines represent regressions fitted separately on either side of the tipping point weighted by the size of the neighborhoods and the fraction of the decade that the neighborhood spent above the tipping point, using an Epanechnikov kernel. Only the 1/3 of the sample not used for identifying the location of the tipping points is used for these visual depictions.