Tipping Points and the Dynamics of Ethnic Segregation Across Industries in Germany

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Abstract

Tipping points in the composition of groups have been argued to explain observed patterns of segregation in residential markets, labour markets, or schools. I use social security data for the period 1990-2010 to study whether such tipping dynamics can explain observed patterns of segregation across industries by ethnicity in Germany. I consider two tests for the existence of tipping points in the composition of local industries' workforces, one based on a regression discontinuity design (RDD) around a candidate tipping point, the other based on a threshold regression that includes an unknown breakpoint. I find only limited support for the existence of tipping dynamics in native employment flows using RDD methods and no evidence when estimating a threshold regression. The RDD evidence is strongest for the period 1990-1995, when immigrant inflows to Germany were largest. Furthermore, my findings suggest that inference methods previously used to test for the existence of tipping points in labour markets may have a tendency to over-reject the null of no tipping points. Taken together, my results may be cause for some scepticism about the existence of tipping points in labour markets.

1 Introduction

Recent research has established that immigrants and natives are highly segregated across industries and workplaces in many developed economies (Hellerstein and Neumark, 2008; Åslund and Skans, 2010; Andersson et al., 2014; Glitz, 2014). In the case of Germany, where the foreign-born made up 12.8 per cent of the population in 2008 (OECD, 2020), 40 per cent of immigrants would have needed to change firms to achieve a degree of segregation consistent with a random assignment of workers to firms. Even accounting for differences in location, education, and gender between immigrants and natives, 26 per cent of immigrants would have needed to change firms (Glitz, 2014).

Workplace segregation unexplained by observed characteristics suggests factors of production are misallocated, which can have large negative consequences for aggregate productivity and output (Hsieh et al., 2019). At the individual level, segregation across workplaces or industries could help explain the widely-studied persistence of employment and wage gaps between immigrants and natives (e.g. Lubotsky, 2007; Sarvimäki, 2011) and the fact that immigrants tend to work at lower-paying firms (Aydemir and Skuterud, 2008; Barth et al., 2012). Since coworker networks are an important source of information and referrals in the labour market (Cingano and Rosolia, 2012; Eliason et al., 2019; Glitz and Vejlin, 2020), segregation across workplaces or industries could restrict immigrants' access to better-paying jobs and firms if it means they lack the native coworkers necessary to land these jobs.

The causes of observed segregation, however, are not yet fully understood. Three broad theoretical explanations have been advanced. First, the oldest set of explanations show how discrimination on the part of employers towards certain types of workers, such as immigrants, leads to workplace segregation. Such discrimination could be for reasons of taste (Becker, 1957), or for statistical reasons (Aigner and Cain, 1977). Second, there is a long tradition arguing that spillovers in either consumption, in the tradition of social interaction models (Schelling, 1971)—immigrants might prefer to work with other immigrants and natives with other natives—or in productivity, say due to communication costs (Lazear, 1999), could optimally lead to the formation of homogeneous workplaces. Finally, the most recent group of explanations focuses on how segregated social networks—the tendency, for example, of immigrants to befriend other immigrants—can lead to segregation in labour market outcomes, including the place of work (see Jackson et al., 2017, for a review).

The objective of this paper is to empirically examine the second of these proposed explanations, that is, to search for evidence that spillovers, particularly spillovers in preferences, can lead to workplace segregation. I do so in the context of Germany in the period 1990-2010. Preference spillovers arise when the composition of the workplace or industry directly affects individuals' utility. This creates a strategic interaction between individuals as they choose which industry to supply their labour to. The preference spillovers I study here could be caused by a simple distaste for working with immigrants, or they could be caused by career concerns. Such concerns could arise if immigrants are a worse source of information about the labour market and referrals, leading to lower job mobility for their coworkers, or if an increasing immigrant share is taken as a signal that an industry has experienced a negative productivity shock (c.f. Goldin, 2014). My results do not depend on the specific cause of preference spillovers.

There are several reasons to focus on preference spillover-based explanations of segregation. First, preference spillovers are one of the leading explanations of observed patterns of residential segregation (Schelling, 1971; Cutler et al., 1999; Becker and Murphy, 2000; Card et al., 2008). Second, while systematic evidence over time is relatively scarce, in 2017 only 37 per cent of Germans stated they would be "totally comfortable" having an immigrant as a work colleague, similar to the proportion (36 per cent) stating that they would be totally comfortable having an immigrant as a neighbour (European Commission, 2018).[1] These two facts suggest that the preference spillovers that help explain residential segregation might also be a leading explanation for workplace segregation. Third, a common feature of many models of preference spillovers in residential choice is that they generate stark predictions about the dynamics of neighbourhood composition, which readily lend themselves to empirical testing. Specifically, neighbourhood composition is often predicted to follow a "tipping" dynamic; once the immigrant share exceeds a certain threshold it is predicted to rapidly increase towards one. This stark prediction makes it relatively straightforward to test whether preference spillovers might be a cause of workplace segregation.

I proceed by presenting a simple model of local industry workforce composition that adapts the model of neighbourhood composition of Card et al. (2008). In this model, the labour supply to an industry in a local labour market depends on both the wage offered in the industry and the composition of the industry's workforce, a form of social preference in the spirit of Schelling (1971). An equilibrium is characterised by the immigrant share in the local industry. The model has multiple equilibria for a given relative supply of immigrants, though typically only one stable integrated equilibrium, where the immigrant share is greater than zero and less than one. Importantly, there is a discontinuity in the response of the equilibrium immigrant share to changes in the relative supply of immigrants. Following Card et al. (2008), a tipping point is defined as the maximum of the set of stable integrated equilibria.

This definition of a tipping point implies a discontinuity in the evolution of the immigrant share over time. Local industries below the tipping point, in the interior of the set of stable integrated equilibria, should see their immigrant share change little in response to small changes in the relative supply of immigrants. However, once the immigrant share in a local industry is above the tipping point, the immigrant share should start increasing rapidly as the industry shifts towards the segregated equilibrium. This definition of a tipping point differs from the traditional definition of a tipping point as a single unstable equilibrium (Schelling, 1971; Becker and Murphy, 2000).

I carry out two distinct empirical tests for the presence of such tipping dynamics in workforce composition. In the first test, I implement the two-step procedure proposed by Card et al. (2008). In the first step, I identify candidate tipping points in the composition

[1] The other options were "somewhat comfortable", "somewhat uncomfortable", "totally uncomfortable", or "don't know". Across the EU, the share "totally comfortable" was 43 per cent for neighbours and 44 per cent for colleagues.

of local industries in West Germany for the periods 1990-1995, 1995-2000, 2000-2005, and 2005-2010, using an ad hoc search procedure proposed by Card et al. (2008). I allow the location of the candidate tipping point to vary across local labour markets. In the second step, I apply regression discontinuity design (RDD) techniques (Imbens and Lemieux, 2008; Lee and Lemieux, 2010) and look for evidence of discontinuities at the identified candidate tipping points. The evidence from this approach is mixed. There is some evidence of tipping, in the form of either native flight from or native avoidance of relatively high immigrant-share industries, during the first two periods, when net immigration to Germany was high, though no evidence in the later two periods, when net immigration was low and Germany experienced several recessions.

Since this evidence is not conclusive, I conduct a second test, applying formal techniques for identifying and testing for the existence of breakpoints in a conditional expectation function using a threshold regression (Hansen, 1996, 2000). Threshold regressions have not yet to my knowledge been formally used to test for the existence of tipping points. This test leads me to reject the hypothesis of a discontinuous change in the evolution of workforce composition in all periods. I also check for evidence of tipping points in the composition of individual establishments, rather than industries, using a threshold regression. Here too I fail to find evidence of tipping dynamics that could explain observed workplace segregation.

More careful comparison of RDD and threshold regression approaches suggests that the discrepancy between the two is due to a tendency of RDD-based tests for the existence of tipping points using bootstrap standard errors to over-reject the null of no discontinuity. Specifically, standard errors for the size of the discontinuity in the RDD approach are typically calculated using the bootstrap. However, the location of the tipping point is treated as fixed across bootstrap samples, reducing the variability of the discontinuity relative to a procedure where both the location of the tipping point and the size of the discontinuity at the tipping point can vary over bootstrap samples. Since the location of the true tipping point is unknown, the latter procedure is the correct one for conducting inference on the size of any discontinuity in the outcome at the tipping point.

The use of bootstrap standard errors was originally suggested by Card et al. (2008), although they adopt a different inference procedure in their main results, based on splitting the sample into separate subsamples for identifying the location of the tipping point and testing for a discontinuity. The sample splitting approach requires relatively abundant data, as a result of which more recent work on tipping points in labour markets (Pan, 2015) has relied on bootstrapped standard errors when testing for tipping points. My results suggest some scepticism may be appropriate when evaluating the results of these bootstrap-based tests for the existence of tipping points in labour markets.

The paper is structured as follows. In the following section I review the literature on segregation and tipping points in firms and neighbourhoods, where the question has been more extensively studied. In section 3 I outline a model of workplace segregation and show how discontinuities in industry workforce growth arise. In section 4 I outline the empirical implications of the model and the two tests I propose for the existence of tipping points. In section 5 I present the results of the two tests. Finally, section 6 concludes.

2 Literature

The best-known studies on segregation have tended to focus on residential segregation. Extensive residential segregation has been documented by ethnicity in the US (Cutler et al., 2008), and Europe (Semyonov and Glikman, 2009). Early papers on the consequences of residential segregation found that it was negatively associated with the outcomes of minorities (Cutler and Glaeser, 1997) and immigrants (Borjas, 1995), while nevertheless recognising that segregation arises endogenously and might still be optimal from the individuals' perspective (Borjas, 1998). More recent empirical evidence has emphasised the role of non-random selection in driving observed negative findings (Edin et al., 2003; Damm, 2009), noting that segregation can improve job-finding probabilities and consequently employment and wages in the short run, though residential segregation appears to lower immigrants' human capital acquisition in the the long run (Battisti et al., 2018).

The literature on workplace segregation developed in parallel to the literature on residential segregation, though it is smaller in comparison. Earlier papers suffered from limited access to disaggregated data and were constrained to show evidence of segregation by occupation or industry (Albelda, 1986) or for historical periods (Higgs, 1977). The proliferation of large-scale firm datasets drawn from administrative records in the last two decades has now allowed researchers to document significant workplace segregation in the US (Hellerstein and Neumark, 2008; Andersson et al., 2014), Sweden (Åslund and Skans, 2010), and Germany (Glitz, 2014). Glitz (2014) shows that workplace segregation also correlates with immigrants' economic outcomes: immigrant cohorts become less segregated from natives with time spent in Germany, just as their wages converge to those of natives.

There is a long tradition of models that seek to explain observed patterns of segregation by appealing to social interactions models, (Schelling, 1971, 1978; Becker and Murphy, 2000). In these models, a small preference for majority-dominant units (neighbourhoods, firms, schools, etc.) can lead to extensive segregation across units. These models are characterised by a multiplicity of equilibria, some of which may be unstable, potentially leading the observed composition of integrated units to shift rapidly to a stable, segregated equilibrium when subject to some shock. In contrast, Card et al. (2008, 2011) propose a model where integrated equilibria are stable, however only low immigrant shares can be supported in equilibrium; if the relative demand of immigrants for housing in a neighbourhood increases too much, no integrated equilibrium will exist.

While Easterly (2009) claimed to find no evidence of tipping in the composition of US neighbourhoods, there is growing evidence of the empirical relevance of tipping points. Card et al. (2008) develop a reduced-form method, applied in this paper, where a tipping point is understood as an immigrant share at which there is a discontinuity in the expected change in the share of natives in a neighbourhood, and find ample evidence of tipping in the composition of US neighbourhoods. Aldén et al. (2015) apply the same method and find similarly clear evidence of tipping points in the composition of Swedish neighbourhoods. Böhlmark and Willén (2020) use neighbourhood tipping points identified by a Card-style procedure to study the effect neighbourhood composition on children's educational and future labour market outcomes in Swedish metropolitan areas, arguing that the identified tipping points can be treated as an RDD-style cutoff in the immigrant share of the neighbourhood where a child grows up when studying these later outcomes. In the labour market, the method of Card et al. has been used to identify tipping points in occupational composition by gender in the US (Pan, 2015), which is the paper most similar to the current one. However, the method has not yet been used to study ethnic segregation in the labour market, the focus of this paper. Caetano and Maheshri (2017) have also developed a more structural method for identifying tipping points, and use it to identify tipping points in the composition of schools' student bodies in Los Angeles.

3 Theory

3.1 Unit of analysis

The model of tipping I propose here builds on the work of Schelling (1971) and Card et al. (2008, 2011), where members of the majority group experience increasing disutility as the share of minority individuals in their unit of analysis increases. In the papers just cited, the unit of analysis is the neighbourhood. Before showing how such a model of social preferences can lead to tipping dynamics and explain patterns of workplace segregation, I therefore need to establish the relevant unit of analysis in which natives might experience disutility from working with a larger share of immigrants.

In this paper I focus on tipping in the composition of 3-digit industries within local labour markets, a unit of observation I refer to as a *local industry*, or simply *industry*. There are theoretical, empirical, and practical reasons to focus on local industries.

Theoretically, the choice of unit of analysis will depend on the reasons for which natives experience disutility from working with immigrants. If the disutility were experienced in personal interactions with colleagues, the production unit (team, small plant) would be the natural unit of analysis. If however the disutility arises because increasing the immigrant share lowers the prestige of a sector, then the local industry will be a more natural choice. Goldin (2014) has argued that an increasing female share can be taken as a signal of a negative productivity shock in an occupation, lowering its prestige. The same is plausibly true of the immigrant share in an industry. Certainly there is historical evidence that the low prestige associated with jobs in certain industries led natives to avoid employment in those industries with the resulting labour shortage made up for through immigration (Noiriel, 1988). Finally, if tipping dynamics are driven by the decisions of new workers searching for jobs, the fact that the immigrant share is more readily observable to an outsider at the level of a local industry than at a firm also suggests that local industries are a more natural level at which to build a test for evidence of tipping dynamics.

Empirically, segregation is observed across teams, plants, firms, local industries, national industries, or occupations, and social preferences could potentially be present and cause tipping dynamics in the composition of any of these units. However, segregation across local industries does account for a large proportion of total workplace segregation. Table 1 reports the index of coworker segregation, defined by Hellerstein and Neumark (2008) as the excess probability that an immigrant has of working with other immigrants, relative to a native, for West Germany in 1990-2010. This index can be normalised to account for differences in the distribution of immigrants and natives across larger units of aggregation, such as regions, yielding what is known as an *effective* index of coworker segregation. Conditioning the index on the distribution of workers over local labour markets and 3-digit industries explains around 45 per cent of observed segregation. Indeed, segregation across 3-digit industries accounts for around 40 per cent of observed segregation *within* local labour markets. Explaining segregation across industries within local labour markets would therefore go a long way to explaining observed patterns of total workplace segregation.

Finally, as a practical consideration, establishments tend to be relatively small, with over 70 per cent of firms in my dataset employing fewer than 10 employees, implying that both the size and composition of workforces can vary widely over time. This introduces considerable noise to the data, potentially making it harder to empirically detect tipping dynamics. Furthermore there is selection out of the sample over time as establishments close, potentially introducing bias since tipping also takes place over time. Focusing on local industries will allow me to overcome these practical difficulties. In a robustness check I will nevertheless confirm that firm-level estimates are consistent with my local industry-level estimates.[2]

[2]Note that there is no straightforward logical relationship between tipping at the industry level and at the firm level. Industry-level tipping does not imply firm-level tipping, since it could occur through the entry of high immigrant-share or the exit of high native-share firms as the industry passes the tipping

3.2 A model of tipping

In this section I adapt the model of Card et al. (2008, 2011) of neighbourhood composition in the presence of social interactions to study the composition of local industries' workforces. The model is static and partial equilibrium. A representative, nondiscriminating firm hires two types of workers, immigrants and natives, denoted $j \in \{I, N\}$, which it treats as perfectly substitutable in production. The industry's size, and hence the representative firm's size, is taken as given, so the total workforce is normalised to equal one. The inverse supply of type j is given by $\omega^j(n_j, s)$, a primitive of the model. Crucially, the inverse supply depends not only on the quantity of workers of type j hired, n_j, but also on the share of immigrants in the firm, s.

The partial derivatives $\partial\omega^j(n_j, s)/\partial n_j$ are assumed to be weakly positive, that is, for a constant immigrant share, the firm needs to raise wages to hire more workers of a given type. The partial derivative $\partial\omega^j(n_j, s)/\partial s$ represents the social interaction effects. In particular, I assume that $\partial\omega^N(n_N, s)/\partial s > 0$ for s greater than some threshold; that is, as the immigrant share in the firm increases beyond some threshold, the firm needs to pay a higher wage to hire a given quantity of natives. Under the normalisation that the total workforce is one, we have $n_N = 1 - s$, and the derivative of the native inverse supply function with respect to the migrant share will be

$$\frac{d\omega^N}{ds} = -\frac{\partial\omega^N}{\partial n_N} + \frac{\partial\omega^N}{\partial s}. \tag{1}$$

Under the previous assumptions, the first term will be negative above some threshold, while the second term will be positive. I follow Card et al. (2008) in assuming that the social interaction effect is sufficiently strong such that $d\omega^N/ds > 0$ for high levels of s, i.e. supply of natives $n_N = 1 - s$ is downward sloping for low levels of n_N and only becomes upward sloping as n_N rises and the immigrant share s falls below a certain threshold. I also assume for simplicity that $d\omega^I/ds > 0$ for all $s \in (0, 1)$, that is that supply of immigrants is upward sloping for all values of n_I.[3]

There are multiple ways one could interpret the social interaction effects captured by the assumption that $\partial\omega^N(n_N, s)/\partial s > 0$. The simplest way, consistent with the original model of Card et al. (2008) and the tradition of social interactions models going back to Schelling (1971), is to interpret this as a consumption externality. Natives experi-

point. Similarly, tipping at the firm level need not imply tipping at the industry level if individual firm tipping only implies a reallocation of a fixed pool of workers within the industry.

[3]There is therefore an asymmetry in the strength of the social interaction effects between immigrants and natives that drives an asymmetry in the shape of the inverse supply curves of migrants and natives. This asymmetry is also present in the model of Card et al. (2008). The empirical predictions of the model can still be derived when social interactions cause immigrant inverse supply to be downward sloping for low values of s; what is strictly necessary however is that the inverse supply curve of immigrants be flatter than the inverse supply curve of natives, i.e. $d^2\omega^I/ds^2 < d^2\omega^N/ds^2$, for all $s \in (0, 1)$.

ence disutility from working with immigrants, so the marginal native worker will become unwilling to work at the firm if the immigrant share increases.

The source of this disutility could be a simple distaste or discomfort experienced by individual natives when working with immigrants. Alternatively, the disutility could arise from dynamic considerations, if natives believe that working with immigrants will harm their future job-finding prospects and earnings, say because immigrants are not a good source of referrals or information about job openings. The disutility might also arise indirectly, if an inflow of immigrants into an industry provides a negative signal about productivity in the industry, as in the pollution model of Goldin (2014), lowering the prestige of working in the industry.

Alternatively, one could also interpret the social interaction effect as a productivity externality, reinterpreting n_N as the effective supply of natives. Under this interpretation, an increase in the immigrant share lowers the productivity of natives; to keep a constant effective supply of native workers, the firm must raise the wage offered to hire more natives. This interpretation is consistent with recent evidence on negative productivity spillovers between immigrants and natives in certain firms (Glover et al., 2017), however it would also complicate the derivation of Equation (1), since now $n_I \neq s$, so I do not entertain it further here.

The inverse supply curves of immigrants and natives are plotted in Figure 2. As $s = n_I = 1 - n_N$, the supply of immigrants increases moving to the right on the x-axis, while the supply of natives increases moving to the left on the x-axis. At an integrated equilibrium, where both types of workers are employed in the industry, the wages paid to both types of workers must be equal, since the firm is assumed to be non-discriminating. Again under the normalisation that the total workforce is one, equilibrium therefore requires

$$\omega^N(1-s,s) = \omega^I(s,s). \tag{2}$$

As the inverse supply curves are drawn, there are three equilibria. Equilibrium A is stable in the sense that a small increase in the firm's minority share raises the wage that must be paid to immigrants above the wage paid to natives, so the firm hires natives until it returns to the equilibrium at A. The same remark holds *mutatis mutandis* for a decrease in the minority share at A or at C. Equilibrium B is, however, unstable. After a small increase in the immigrant share from B, the wage demanded by natives is greater than the wage demanded by immigrants, the firm will replace natives with immigrants until it reaches the equilibrium at C.

In Figure 3 I plot what happens as the supply of immigrant workers to the firm increases, say as a result of an inflow of immigrants to the local labour market where the firm is located. Suppose the firm is initially in equilibrium at E_1. As the supply of immigrants increases, their inverse supply curve shifts downward, and the equilibrium gradually moves to the right. However if the inflow of immigrants continues, the point of tangency E_2 will eventually be reached, which is stable with respect to decreases in the immigrant share, but unstable with respect to increases. If there are any further increases in the supply of immigrant workers, no integrated equilibrium will exist, the only equilibrium will involve the firm hiring only immigrants, as at point E_3. Traditional social interaction models such as Becker and Murphy (2000) identify the unstable equilibrium B in Figure 2 as a tipping point. Here, however, I follow Card et al. (2008) in defining the tipping point as the maximum possible immigrant share in an integrated equilibrium. In Figure 3, this is the immigrant share s^*, associated with the equilibrium E_2.

Two caveats are worth noting with this model. First, it does not account for the distribution of immigrants across industries, only the composition of a single industry. Implicitly I assume that the natives who leave the industry after the tipping point is exceeded would either prefer to be unemployed than keep working in a high-immigrant-share industry, or are able to find jobs in other industries that have not faced a similar supply shock. The latter scenario could arise if the size of other industries is not similarly constrained, or if the immigrant supply shock is specific to a single industry, perhaps because it is mostly located in the neighbourhood where newly arrived immigrants settle. Second, social interaction models are typically thought to lead to an inefficiently high degree of segregation across neighbourhoods, because agents cannot coordinate on where to locate. The model presented here, by only considering a single representative firm, is silent about the potential welfare consequences of such social preferences. It has traditionally been argued that firms, by internalising any spillovers across workers arising from their hiring decisions, choose a socially optimal degree of segregation (Becker and Murphy, 2000). However, these arguments do not account for the possibility that workplace segregation could be dynamically inefficient, if it keeps immigrants from developing the network necessary to move up the job ladder.

3.3 Dynamic implications

While the model presented in the previous section is static, it is still possible to use it to make dynamic predictions about the composition of the representative firm's workforce.

Consider a firm whose initial static equilibrium immigrant share is $\bar{s}_0 < s^*$, where s^* is the tipping point defined previously as the immigrant share associated with the maximum possible integrated equilibrium. The firm experiences a small increase in the supply of immigrants, i.e. a fall in the wage immigrants need to be paid, $\Delta\omega^I(n_I, s) < 0$, between period 0 and period 1.[4] There will be some $r \in (0, s^*)$ such that if $\bar{s}_0 \in [0, s^* - r)$, the

[4] The discussion here in fact holds for an increase in the relative supply of immigrant, $\omega^N(n_N, s) - \omega^I(n_I, s)$. However, to simplify the discussion I assume the supply of natives is fixed and only the supply

firm's new equilibrium will be at $\bar{s}_1 \in (0, s^*]$, whereas if $\bar{s}_0 \in [s^* - r, s^*]$, the increase in the immigrant supply takes the firm beyond the point of tangency at E_2 in Figure 3 and the new equilibrium will be $\bar{s}_1 = 1$. As the increase in the immigrant supply $\Delta\omega^I(n_I, s)$ becomes infinitesimally small, r also approaches zero. Note that no firm can initially be at an equilibrium at $\bar{s}_0 \in (s^*, 1]$ except for at $\bar{s}_0 = 1$, where a small increase in the supply of immigrants will have no effect on the equilibrium.

Assume that the firm myopically adjusts its immigrant share in response to changes in the supply of immigrants such that the immigrant share s_t remains close to its equilibrium value. To allow for the possibility that search or other labour market frictions prevent the immigrant share from fully adjusting within a single period to a new equilibrium value as the supply of immigrants changes, I use the notation s_t to refer to the observed immigrant share at a point in time, to distinguish it from the static equilibrium at that point in time, $\bar{s}_t$. For an observed $s_0 \in [0, s^* - r)$, the observed increase in the immigrant share Δs_1 in response to the increase in the immigrant supply $\Delta\omega^I(n_I, s)$ will be small. However, for $s_0 \in [s^* - r, s^*]$, $\Delta\omega^I(n_I, s)$ will cause a large observed Δs_1, as the firm converges to the new equilibrium at $\bar{s}_1 = 1$. For firms initially at $s_0 \in (s^*, 1)$, the tipping process is already underway, and one should expect to see $\Delta s_1 > 0$ and larger the closer the firm is to s^*. There will therefore be a discontinuity in Δs_1 around the tipping point s^*. We will observe Δs_1 to be small and positive for s_0 to the left of the tipping point and large and positive for s_0 close to or beyond the tipping point.

Whilst the foregoing discussion restricts attention to the case of an increase in the immigrant supply, where the discontinuity appears clearly, the discontinuity will also exist in the case where there is a decrease in the immigrant supply. This is because once a firm has started tipping and $s_0 \in (s^*, 1]$, a small decrease in the supply of immigrants will typically not reverse the tipping process, implying that for these firms too $\Delta s_1 > 0$. The condition for tipping to continue after a decrease in the immigrant supply is for the marginal immigrant to continue to accept a lower wage than the marginal native, which is more likely to be satisfied the smaller the decrease in the immigrant supply or the further to the right of s^* the firm initially finds itself. On the other hand, for a firm that is close to tipping, but where $s_0 < s^*$, a small decrease in the immigrant supply will lead to a small decrease in the immigrant share in the firm.

Combining these observations about the effect of increases and decreases in the immigrant supply on the firm's immigrant share, one can conclude that there will be a discontinuity in the expected change in the immigrant share as a function of the base-year immigrant share:

$$E[\Delta s_t | s_{t-1}] = \mathbf{1}(s_{t-1} < s^*) g(s_{t-1}) + \mathbf{1}(s_{t-1} \geq s^*) h(s_{t-1}) \tag{3}$$

of immigrants varies.

where $\lim_{\epsilon \to 0^+} h(s^* + \epsilon) - g(s^* - \epsilon) > 0$. $h(s_{t-1}) > 0$, while the sign of $g(s_{t-1})$ will depend on whether firms more commonly face increases or decreases in the immigrant supply. The existence of a discontinuity in $E[\Delta s_t | s_{t-1}]$ at the tipping point s^*, which does not depend on whether the immigrant supply is increasing or decreasing, is the key dynamic implication of the model I will test in the empirical analysis below.

The location of the tipping point s^* will depend on the value of the partial derivatives of the inverse supply functions and, in particular, the strength of native distaste for immigrants, measured by the partial derivative $\partial \omega^j(n_j, s)/\partial s$. If the value of the partial derivatives of the inverse supply functions is the same across labour markets, then the tipping point will also be the same for different labour markets. Both Card et al. (2008) and Aldén et al. (2015) assume different tipping points for different residential markets, while Pan (2015) assumes the location of tipping points in labour markets varies regionally. The strength of native distaste of immigrants, which determines the location of the tipping point, likely varies with the level of historical exposure to immigrants, which varies across locations. I will therefore follow the previous literature and assume the location of the tipping point varies across local labour markets in the two-step estimates presented below. However, I will also consider specifications where the tipping point is assumed to be common to all labour markets, which does not alter my conclusions.

While the model presented above assumes that the size of the industry is fixed, in reality industries vary in size, and grow and contract over time. Let the number of immigrants employed in an industry at time t be I_t, the number of natives be N_t, and the total number of employees be L_t. The immigrant share is then defined as $s_t = I_t/(N_t + I_t)$. Rather than focussing on changes in s_t, the main dependent variable in my empirical specifications is the five-year growth in the industry's native and immigrant workforces, normalised by the total workforce in the base year, $\Delta n_{t,t+5} = (N_{t+5} - N_t)/L_t$ and $\Delta i_{t,t+5} = (I_{t+5} - I_t)/L_t$. This has the advantage relative to using $\Delta s_{t,t+5} = I_{t+5}/L_{t+5} - I_t/L_t$ as a dependent variable of keeping the denominator fixed, focusing on changes in workforce composition not driven simply by changes in workforce size.

An analogous version of Equation (3) asserts that the growth of the native workforce, $\Delta n_{t,t+5}$ and the growth of the immigrant workforce, $\Delta i_{t,t+5}$ are smooth functions of the base-year migrant share, s_t, except at the tipping point s^*. Here, $\Delta n_{t,t+5}$ will fall discontinuously, and $\Delta i_{t,t+5}$ will increase discontinuously. A discontinuous fall in $\Delta n_{t,t+5}$ is consistent with either native flight from, or native avoidance of industries past the tipping point.

4 Empirical approach and data used

Any method testing for the existence of a tipping point in industry workforce composition needs to reckon with the fact that the theoretical tipping point s^* is unknown. I consider two different approaches to doing this: the original method of Card et al. (2008) and a method based on a threshold regression (Hansen, 1996, 2000).

4.1 Two-step empirical specification

Card et al. (2008) propose a two-step procedure to test for the existence of a tipping point. In the first step, they propose an ad hoc method, based on the literature on structural breaks, to identify a candidate tipping point from the data. The method works by approximating the change in the dependent variable, in this case normalised native workforce growth in industry j, $\Delta n_{j,t,t+5}$, as a constant function with a single discontinuity at some unknown break point,

$$\Delta n_{j,t,t+5} = a_{c(j),t} + \delta_{c(j),t}\mathbf{1}(s_{j,t} \geq s^*_{c(j)t}) + \epsilon_{jt} \tag{4}$$

The tipping point $s^*_{c(j)t}$, which is assumed to be specific to the local labour market $c(j)$ in which industry j operates and base year t, is chosen as the the value in $[0, 60]$ that maximises the R^2 of Equation (4). Card et al. (2008) note that the procedure is somewhat sensitive to outliers, sometimes choosing a candidate tipping point that clearly reflects the influence of a single observation. To avoid this problem, I modify their procedure and choose the candidate tipping point via five-fold cross-validation. That is, rather than choose the candidate tipping point that minimises the in-sample R^2 of Equation (4), I choose the candidate tipping point that minimises the average out-of-sample R^2 of Equation (4).

Figure 1 illustrates a candidate tipping point identified in this manner for the Düsseldorf local labour market for the period 1990-1995. The vertical line marks the candidate tipping point identified by the search procedure, while the horizontal line marks the unconditional normalised native workforce growth across industries in Düsseldorf over the same period. To the left of the candidate tipping point, the normalised native workforce growth is positive and clearly larger than the unconditional average. To the right of the tipping point, it is typically negative and less than the unconditional average native workforce growth.

In the second step, once the candidate tipping points have been identified, Card et al. propose treating the candidate tipping point as a discontinuity in the spirit of regression discontinuity (RD) designs, and estimating the jump in the conditional expectation of the outcome variable. Following this method, I will conclude that a tipping point exists if native workforce growth typically experiences a significant and negative jump at the candidate tipping point, and immigrant workforce growth experiences a positive, significant jump at the candidate tipping point. To test this hypothesis, I pool all local labour markets in a given base year and estimate the following empirical version of Equation (3):

$$y_{jt,t+5} = p(s_{jt} - s^*_{c(j)t}) + \delta \mathbf{1}(s_{jt} \geq s^*_{c(j)t}) + \beta X_{jt} + \alpha_{c(j)} + \epsilon_{jt}. \tag{5}$$

The dependent variable, the five-year normalised change in either immigrants or natives in industry j, is assumed to evolve according to some smooth function of the distance of the base-year immigrant share from the local labour market-specific tipping point, $p(\cdot)$, with a discontinuous jump at the candidate tipping point equal to δ. I also consider as control variables X_{jt} the log of average firm size in the local industry, the log of median wages, the share of low-qualified workers and the Herfindahl-Hirschman index of concentration of the local industry, all in the base year. I follow Card et al. (2008) in modelling $p(\cdot)$ as a fourth-order polynomial in $s_t - s^*_{c(j)t}$, and I restrict my estimations to firms within 30 percentage points of their industry-year-specific tipping point.

Given that Card et al. explicitly motivate this approach by referring to the candidate tipping point as a discontinuity in the sense of RD designs, I also estimate Equation (3) non-parametrically via local linear regression, which is the standard in RD designs (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). As in the parametric specification, I treat the distance between an industry's base-year immigrant share and the candidate tipping point, $s_t - s^*_{c(j)t}$, as the running variable and pool all labour markets in the same base year. Again, I conclude there is a tipping point if I find a negative, significant fall in native workforce growth and a positive, significant increase in immigrant workforce growth. It should be noted, however, that notwithstanding the language used by Card et al., the problem of testing for tipping points arguably does not correspond to an RD design. The defining feature of an RD design is that the probability of receiving some treatment jumps discontinuously when the running variable crosses a known threshold. It is not at all clear what, if anything, could correspond to the "treatment" in the present case, other than the tautologically defined treatment "having an immigrant share beyond the tipping point".

Card et al. note that using the same data to both identify the location of the candidate tipping points and test for a significant effect on workforce growth at these tipping points creates a specification search bias. This creates a risk that standard inference methods will lead us to over-reject the null hypothesis of no effect on the outcome at the tipping point. To address this bias, they randomly split their sample in two, using one subsample to search for the candidate tipping points and the other to test the effect on workforce growth. As an alternative, they also propose using the full sample to both identify the tipping point and estimate the effect on the outcome, bootstrapping their estimates to obtain standard errors, although they do not use the bootstrap approach in their main results. Subsequent work identifying tipping points in labour markets, in particular Pan (2015), has preferred bootstrap standard errors to the split-sample approach, perhaps because sample splitting requires relatively abundant data. Since my dataset is considerably smaller than that of Card et al., I follow Pan (2015) in calculating nonparametric bootstrapped standard errors for $\hat{\delta}$ to test the null hypothesis.

4.2 Threshold regression specification

While the approach developed by Card et al. is intuitively appealing, it is ad hoc. The distribution of $\hat{\delta}$ when following their procedure is not known, since it is a function of $s^*_{c(j)t}$, which is itself estimated. It is unclear whether either the split-sample inference conducted by Card et al. or the bootstrap inference conducted by Pan adequately deals with this problem.[5] This concern, and the observation that the tipping point is arguably not a discontinuity in the sense of an RD design, since there is no treatment variable, lead me to consider a threshold regression model as an alternative. Threshold models are conceptually appealing in this context since they are explicitly designed to deal with the situation where the conditional expectation of an outcome changes discontinuously at some unknown threshold. They do this by simultaneously estimating the location of the threshold and the effect on the outcome at the threshold as a non-linear least squares problem (Hansen, 1996, 2000). They are practically appealing since tests for the existence of a discontinuity have been developed for them and their distributional theory is well-understood (Andrews and Ploberger, 1994; Hansen, 1996, 2000).[6]

Consider the following threshold regression model, again derived from Equation (3):

$$y_{jt,t+5} = p(s_{jt}) + \delta \mathbf{1}(s_{jt} \geq \gamma) + \beta X_{jt} + \alpha_{c(j)} + \epsilon_{jt}. \tag{6}$$

$p(s_{jt})$ is again a fourth-order polynomial, though now in the base year immigrant share. The crucial difference between equations (6) and (5) is that the tipping point is now included as γ, a parameter to be identified at the same time as δ, β, and the coefficients

[5] Andrews et al. (2020) have recently considered the general problem of inference conditional on an estimated breakpoint, and propose an alternative, quantile-unbiased estimator and an alternative sample-splitting approach, both of which have yet to be adopted in the literature on tipping points.

[6] More recently, related nonparametric methods have also been developed for RD designs when the location of the discontinuity in the probability of receiving treatment is unknown. Porter and Yu (2015) propose a two-step procedure where one (i) tests for the existence of a breakpoint at some unknown value of the running variable; and, should the null of no break point be rejected, (ii) estimates the location of the breakpoint as the value of the running variable for which the treatment effect is maximised. While this and related methods are interesting, they are explicitly developed within a potential outcomes framework for an RD design, which, I have argued, is not appropriate here since there is no clearly defined treatment, so I do not consider this approach further.

of $p(\cdot)$. Equation (6) is therefore nonlinear in the parameters and can be estimated by nonlinear least squares.

To test for the existence of a discontinuity, one tests the restriction that $\delta = 0$. A standard statistic for this kind of test is $T_n = \sup_{\gamma \in \Gamma} T_n(\gamma)$, where $T_n(\gamma)$ is a test (Wald, Lagrange Multiplier, F, or other kind of test) of the restriction that $\delta = 0$ when γ is treated as known. Hansen (1996) points out that the distribution of T_n is a function of γ, which is not identified under the null hypothesis that $\delta = 0$, invalidating the usual distributional theory of the test. However, he shows that a bootstrap procedure will give the correct p-values for the test. The procedure works as follows: (i) estimate (6) via nonlinear least squares; (ii) at each bootstrap iteration, generate a new dependent variable $y_{jt} = \hat{\epsilon}_{jt} z_{jt}$ where $\hat{\epsilon}_{jt}$ is the estimated residual and z_{jt} is a draw from a standard normal distribution; and (iii) re-estimate the model and calculate the test statistic for the generated dependent variable at each bootstrap iteration.

It is important to note the key difference between the bootstrap procedure proposed by Hansen (1996) and the one mentioned by Card et al. (2008) and used by Pan (2015). In the former case, the location of the tipping point is re-estimated in each bootstrap iteration, while in the latter case it is fixed at the value identified by the search procedure. I will show below that this has large consequences for the test that $\delta = 0$. More generally, the two-step procedure of Card et al. is related to the threshold regression approach. Equation (4), used to identify the candidate tipping point, is a special case of the threshold model in Equation (6) with the imposed assumption that the polynomial $p(\cdot)$ is of order zero, i.e. a constant function, and $\beta = 0$, i.e. the covariates do not enter the regression. Furthermore, a consistent implementation of a threshold regression would involve testing the hypothesis that $\delta = 0$ at the first step, eliminating Card et al.'s second step.

4.3 Data

The data used in this paper come from the Institute for Employment Research of the German Federal Employment Agency. I use the Establishment History Panel (BHP), a fifty per cent sample of all establishments making social security contributions for at least one employee between 1975 and 2010. An establishment covers all production sites of a firm within the same municipality operating within the same three-digit sector. I follow standard practice when working with the BHP in indiscriminately referring to establishments as firms or establishments.

The sampling frame of the BHP includes all firms making social security contributions in West Germany since 1975, and all such firms in East Germany since 1993. I limit the sample to four five-year periods: 1990-1995, 1995-2000, 2000-2005, and 2005-2010. This allows me to investigate potential differences in tipping dynamics as immigrant flows and macroeconomic conditions change over time. I also limit myself to West Germany (excluding Berlin) since East Germany is not covered through the whole period and a large majority of Germany's immigrants live and work in the old West Germany.

I further impose the following restrictions on the observations included. I drop all local industries where fewer than 10 firms are operating, or fewer than 30 individuals are employed in the base year, to ensure that any large changes in workforce composition are unlikely to be the result of firm-specific idiosyncratic factors and to guarantee a degree of homogeneity across local industries. I also drop industries where the normalised growth in the native workforce in the period under consideration is greater than 400 per cent, since these are likely to be the result of large already-existing establishments entering the BHP after a change in establishment ID, through mergers. Finally, similar to the restriction Card et al. (2008) impose on the number of neighbourhoods per metropolitan statistical area, I restrict attention to local labour markets where at least 100 industries satisfying the above restrictions are operating, to be able to estimate local labour market-specific tipping points.[7] Focusing on large cities is less of a restriction than it may appear, since that is where most immigrants are located. After imposing these restrictions, my sample covers 60 per cent of total employment in West Germany in the BHP over the sample period, and 67 per cent of immigrant employment.

Summary statistics for the included industries are presented in Table 2. In the first two periods, 1990-1995 and 1995-2000, respectively 15 and 14 local labour markets covering 54 and 53 per cent of relevant BHP employment satisfy the sample definition, while in the latter two periods, 2000-2005 and 2005-2010, 24 and 25 local labour markets covering 64 and 65 per cent of relevant BHP employment satisfy the size restriction. The newly included local labour markets correspond to smaller cities and the average size of a local industry and the median real wage accordingly decline over time. One notes an increase in the average immigrant share from 1990 to 1995, consistent with the large net immigration experienced by Germany in that period, and declines thereafter.

Before evaluating dynamic patterns of workforce composition, I also report static measures of segregation in the whole of West Germany over the period under consideration, calculated from the BHP. I report two types of measures, originally proposed by Hellerstein and Neumark (2008), the index of coworker segregation and the index of effective coworker segregation. The index of coworker segregation, which measures the difference between (i) the probability that a randomly drawn coworker of an immigrant also be an immigrant; and (ii) the probability that a randomly drawn coworker of a native be an immigrant. To calculate the effective index of coworker segregation, I randomly redistribute the individuals (immigrants and natives) in my sample across firms, conditional on either firm location, industry of the firm, or both, and keeping firm size equal to actual

[7]The local labour markets are constructed by Kropp and Schwengler (2011) from municipality commuting flows for 1993-2008 and correspond roughly to an urban core and its adjacent counties (*Kreise*).

firm size, and recalculate the standard index of coworker segregation on the simulated sample. I then take the average index of coworker segregation from 30 such simulations and subtract the result from the true index of coworker segregation. The index of effective coworker segregation provides a measure of the extent to which differences between immigrants and natives in observable characteristics, in this case geographic location and industry affiliation, can explain observed patters of workplace segregation.

The obtained indexes are reported in Table 1. The unconditional index of coworker segregation rises somewhat over this period, from 0.15 to 0.18, as does the index of effective segregation, conditional on local labour market and three-digit industry, from 0.08 to 0.1. The figures reported here, while calculated from a different dataset, are similar to those reported in Glitz (2014), though unconditional segregation is slightly higher in my dataset, and conditional segregation is slightly lower.

5 Results

5.1 Two-step procedure

5.1.1 Candidate tipping points

Table 3 summarises the estimated tipping points by five-year period. The location of the tipping point is assumed to be specific to each local labour market. The distribution of estimated candidate tipping points is also presented graphically in Figure 4. The average location of the candidate tipping point does not appear to change over different periods, with the modal tipping point around 5 per cent in each period, and the mean around 7.5 per cent. The pairwise correlation between the candidate tipping points over periods is moderately high, ranging between 0.42 and 0.72.

The distribution across local labour markets of the break in expected native growth as calculated during the search procedure, defined as $\hat{\delta}_{c(j),t}$ in Equation (4), is plotted in Figure 5. Theoretically, $\hat{\delta}_{c(j),t}$ should be negative, since we expect a decrease in native workforce growth as we move beyond the tipping point. However, this assumption is not imposed on the search procedure, and Figure 5 shows that some candidate tipping points are identified where there is a *positive* break in native workforce growth, particularly in more recent periods. In Table 3 I report the share of local labour markets for which the estimated break in workforce growth is negative, as expected. In 1990-1995 all breaks are negative, and the modal decline is around 15 per cent. However in 1995-2000, 2000-2005, and 2005-2010, the share of negative breaks is respectively 71 per cent, 46 per cent, and 40 per cent. This preliminary finding can already be considered *prima facie* evidence that tipping dynamics are more likely to be present in the period 1990-2000, when immigration was higher, than in the period 2000-2010, where immigration was lower.

5.1.2 Discontinuities at identified candidate tipping points

I first present graphical evidence of the change in the normalised native growth rate at the tipping point in Figure 6. In particular, I plot fitted values calculated by estimating the pooled specification in Equation (5), omitting covariates and local labour market fixed effects. The graph shows clear evidence of a discontinuity in native workforce growth around the tipping point over the period 1990-1995. Normalised native workforce growth drops by around 10 percentage points around the tipping point to below zero. Furthermore, a binned scatter plot of the underlying data show that there is relatively little variance from this pattern. For the period 1995-2000, the drop is similar in magnitude, around 10 percentage points, though native workforce growth remains positive to the right of the tipping point, and the binned scatter plot points to greater variance in the underlying data. For the later periods, the change is zero in 2000-2005 and even slightly positive in 2005-2010. These findings reflect the fact that for many local labour markets in these periods the search procedure identified candidate tipping points where the break in native workforce growth was positive, as documented in Figure 5. However, the binned scatter plots show that there is some variation in the underlying data, and statistical tests will fail to reject the null of no break, positive or negative, in native workforce growth.

The observed pattern of differing effects over time is striking when considered in relation to Germany's migration history. The nineties was a period of high immigration, linked to the wars in ex-Yugoslavia and the immigration of so-called *Spätaussiedler*, typically Russian-speaking ethnic Germans from the former Soviet Union who emigrated to Germany in large numbers from the late eighties onward. In contrast, the first decade of the twentieth century was marked by several recessions and historically low rates of immigration. The fact that the relative supply of immigrants was rising strongly throughout the period 1990-2000 but much less so in 2000-2010 might explain why the observed tipping dynamics are much weaker in the latter period.

Column one of Table 4 presents the same information as Figure 6. The bootstrapped standard errors show that the positive discontinuities in the period 2000-2010 are indeed not statistically significant. However, the discontinuous drop in the period 1995-2000, while relatively large at seven percentage points, is also not significant. Column two adds covariates and local labour market fixed effects to the specifications, which do not materially alter the conclusions.

In columns three and four, I take seriously the suggestion that one think of the candidate tipping point as a regression discontinuity and estimate the change in expected native workforce growth at the discontinuity using local linear regressions, as is standard in the literature on RD designs (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). The choice of bandwidth is a key parameter in these specifications, as in all local smoothing methods (Cattaneo and Vazquez-Bare, 2016). I therefore report results using both an ad hoc bandwidth equal to 0.05 in column three as a baseline, and using a mean squared error-minimising bandwidth, calculated from the data, in column four (Imbens and Kalyanaraman, 2012; Calonico et al., 2014).

The pattern of results is broadly similar to that identified using parametric methods. Using the optimal bandwidth, the effect is slightly larger in 1990-1995 at -11 percentage points but much smaller and still insignificant in 1995-2000 at -2 percentage points. The effects estimated for the period 2000-2010 are more clearly positive, and even statistically significant in 2005-2010. These positive effects in 2000-2010 are perhaps unsurprising, given that a majority of candidate tipping points identified by the search procedure were break points where the discontinuity was positive (see Table 3).[8]

Previous work identifying tipping points in residential or labour markets has tended to focus on breaks in native growth. While such breaks alone are consistent with patterns of native flight or native avoidance of an industry once its immigrant share passes a certain point, theoretical work on tipping points (e.g. Becker and Murphy, 2000) has tended to focus on the composition of the group, and not only native behaviour. Column five therefore repeats the nonparametric specification, using the same candidate tipping points, only now treating immigrant workforce growth as the dependent variable. The theory outlined above predicts immigrant workforce growth should increase discontinuously at the candidate tipping point, or at least not decrease. The estimated change in immigrant growth at the candidate tipping points is typically small and insignificant, though the effect is negative and significant at the five per cent level in 1990-1995. Finally, column six combines the information on immigrant and native workforce growth by using the change in the immigrant share of the workforce over the period under study as the dependent variable in the same RD specification. In all periods, the estimated change at the candidate tipping point is less than half a percentage point, and it is never significant. This would tend to cast doubt on whether the effects on native workforce growth identified even in the period 1990-2000 can be interpreted as evidence of tipping dynamics, as opposed to a more modest form of native flight from or native avoidance of industries beyond the tipping point.

5.1.3 Test of identifying assumption

If one does treat the test of a discontinuity in the regression function as an RD design, the key identifying assumption is that potential outcomes must change continuously around the candidate tipping point. While this assumption is not directly testable, potential outcomes being unobservable, it is typically tested either by checking that predetermined

[8]In supplementary results (available on request) where I drop all labour markets where the search procedure identified candidate tipping points with a positive discontinuity, the estimated effect at the tipping point becomes negative in 2000-2010, but is small and statistically insignificant. This supplementary manual selection step is adopted by Aldén et al. (2015), however it is not justified econometrically.

characteristics change smoothly through the threshold or by testing formally for bunching in the running variable, in this case the distance of the immigrant share from the candidate tipping point in the base year, around the threshold. Even if one is sceptical about whether the candidate tipping point corresponds to a discontinuity in the sense of an RD design, tests of the RD identifying assumption are still useful in establishing that the observed discontinuities cannot be attributed to discontinuities in other relevant local industry characteristics or to selection into having a high- or low-immigrant share by local industries.

Table 5 reports the results of local linear regressions treating different local industry characteristics in the base year as the outcome. I consider four outcomes: the share of low-skilled workers in the local industry, the log of wages, the average firm size, and the Herfindahl-Hirschman index of industry concentration. I again use MSE-optimal bandwidths derived from the data in each specification. Across the four outcomes and four time periods, I find a a single significant discontinuity at the ten per cent level, in the low-skill share in 1995.

To test for bunching in the running variable, I apply the test of Cattaneo et al. (2019), which is conceptually similar to the original bunching test of McCrary (2008), but has greater power. Such tests have not been conducted in previous papers testing for the existence of tipping points using RDD-like methods, and are a rigorous complement to tests of discontinuities in other base-year characteristics. To conduct the test, I estimate the density of the running variable, the distance between the base-year immigrant share and the candidate tipping point, to the right and left of zero using local polynomials and test whether there is a significant discontinuity. Figure 7 plots the estimated densities and their 95 per cent confidence intervals. The confidence intervals are constructed using the bias corrected robust confidence intervals proposed by Calonico et al. (2014) and are therefore not centred on the point estimates. There does not appear to be strong visual evidence of bunching of the running variable on either side of the candidate tipping point in any period, though Table 6 shows that the formal test of discontinuities rejects the null of no discontinuity at the five per cent level in 1990.

Given that the period 1990-1995 is the one where the evidence of tipping is strongest, the existence of a discontinuity in the running variable in the base year is problematic. Given the absence of discontinuities at the five per cent level in any of the other base year outcomes considered, it is tempting to conclude that the observed bunching in the running variable is due to sampling variation. This conjecture receives some support from the fact that it is difficult to conceive of the establishments that make up a local industry, of which there are at least ten, managing to systematically collude to keep the local industry immigrant share to the right or left of an unknown cut-off point. Nevertheless, regardless of whether the bunching is due to random chance or systematic factors, its existence raises the question of whether the same factors might not also explain the observed discontinuity in native workforce growth over the period 1990-1995.

5.1.4 Robustness checks

One objection to the claim that there are tipping dynamics in the period 1990-1995 and not in later periods is that the set of labour markets considered is changing over time (as discussed in Section 4.3). In particular, the sample in the period 2000-2010 includes more small labour markets, where the choice of industry facing a worker may be more constrained, limiting the scope for tipping dynamics to arise. In Table A.1, I repeat the specifications presented in this section on only those 14 local labour markets that are in my sample in all years. The total number of observations (local industries) may still change slightly from year to year, as the set of industries within each labour market is not held constant. The pattern of results and indeed the point estimates in each specification are almost identical to the full sample, although I no longer conclude that any discontinuities are significantly positive during the 2000-2010 period.

5.2 Threshold regression

In addition to the conceptual issues with the two-step approach of Card et al. (2008), noted above, the procedure does not allow us to conclusively accept or reject the existence of tipping points in the composition of local industries during the decade 1990-2000. This is an additional reason for considering threshold regressions as an alternative approach, given that such methods are well-understood and explicitly intended for the problem at hand.

5.2.1 Industry results

Ideally, to compare the results of threshold models with the results obtained previously, one would fit a different threshold model for each local labour market separately, thereby allowing the location of the tipping point to vary by local labour market. Unfortunately, this is not feasible, as threshold models require a relatively large number of observations to identify the threshold, since the location of the threshold is identified by observations near the threshold. Hansen (2020) recommends a sample size of $n \geq 500$, however each labour market in my sample typically does not contain more than 150 local industries. In presenting the results of a threshold model, I therefore focus on a specification where I allow for a single threshold or tipping point for all labour markets, the location of which may vary by time period. I consider specifications where I estimate a different threshold for each labour market as a robustness check.

Table 7 summarises my main results. Columns 1-3 use the normalised native growth as the dependent variable, successively adding the same set of base-year controls and labour market fixed effects to the specification. The location of the threshold and the effect on the outcome at the threshold are robust to these inclusions. However, native workforce growth *increases* at the identified threshold in 1995-2000 and 2000-2005, so it does not appear possible to interpret the threshold as a tipping point, at least in these years. Turning to the formal test of the discontinuity, I report bootstrapped p-value for the test of no jump in the outcome at the threshold , $\delta = 0$, following the procedure of Hansen (1996). Focusing on column 3, including controls and fixed effects, the test fails to reject the null in all specifications.

For comparison with previous work, I also report an alternative bootstrapped p-value. This p-value differs from the parametric bootstrap p-value in two respects. First it is non-parametric, constructed by repeated sampling from the empirical distribution of the observations. Second, and more importantly, it treats the location of the threshold identified by nonlinear least squares as known, and only re-estimates the remaining linear parameters of the model, including the intercept shift at the threshold $\hat{\delta}$, at each bootstrap iteration.

Note that setting the threshold equal to the value estimated via NLS and re-estimating the linear parameters via OLS will give estimates of the linear parameters that are equal to their NLS estimates if the same data are used in both estimation procedures. To see this, observe that the NLS estimates can be obtained in two steps by (i) estimating OLS models for the range of possible threshold values in which the threshold is treated as known in each OLS estimation, and then (ii) choosing the threshold associated with the OLS model for which the sum of squared residuals is minimised. However, given that the bootstrap samples are different from the original dataset, one will obtain different estimates of the linear parameters depending on whether one estimates them via OLS with an imposed threshold, or via NLS with a simultaneously estimated threshold.

The p-values obtained from the OLS procedure are considerably smaller than those obtained by bootstrapping the full NLS procedure. In all periods the test rejects the null that $\delta = 0$ at at least the five per cent level. The reason for this large discrepancy is that the OLS bootstrap procedure does not account for uncertainty in the location of the threshold itself. If the estimated discontinuity varies for different possible values of the threshold, then uncertainty about the location of the threshold will feed into uncertainty about the size of the discontinuity. Shutting down the first source of variability (in the location of the threshold), as the OLS bootstrap procedure does, therefore has knock-on effects on the variability of the estimate of δ.

This finding is significant, since previous studies of tipping points in the composition of neighbourhoods (Card et al., 2008; Aldén et al., 2015) or occupations (Pan, 2015) do not report measures of the precision with which the location of their candidate tipping points is estimated, nor do they account for the effect that imprecision in the estimation of the location of the tipping point will have for the estimate of the effect on the outcome at the tipping point. Indeed the nonparametric bootstrap standard errors reported by Pan (2015) are constructed in the same manner as the nonparametric bootstrap standard errors reported here. The only difference is that she fixes the threshold at the candidate tipping points estimated from the simple constant, intercept shift model of Equation (4), whereas the OLS p-values reported in Table 7 fix the threshold at the threshold obtained by estimating the full polynomial model of Equation (6) via NLS. The results reported here suggest it is likely that inference procedures that treat the location of the breakpoint as fixed across bootstrap samples tend to over-reject the null of no tipping points.

Turning briefly to other outcomes, I show in column four of Table 7 that there is no greater evidence of a tipping point when using immigrant workforce growth as the dependent variable. The discontinuity in the outcome at the estimated threshold is frequently of the wrong sign and is never significant. In column five, I treat the change in the immigrant share as the dependent variable. Here there is some evidence of a tipping point for the period 2000-2005, when immigration was low, at a base-year immigrant share of around 10 per cent. While significant, the effect is relatively small, as the expected change in the immigrant share jumps by only 1.2 percentage points.

5.2.2 Robustness checks

One might object that the comparison between the two-step and threshold methods does not compare apples to apples, since I allow the location of the breakpoint to vary by labour market when using the two-step procedure and keep it fixed across labour markets in the threshold regression. I therefore re-estimate the two-step procedure, searching for a single candidate tipping point by year, and re-estimate the second-step discontinuities using the same specifications as previously. The results are reported in Table A.2. The results are somewhat closer to the results of the threshold specifications. The estimated discontinuities are a couple of percentage points smaller and less strongly significant in 1990-1995 than when the location of the tipping point is allowed to vary by labour market. Interestingly, the discontinuities in 2000-2010 are now negative, although still not significant. All in all, however, my conclusion remains unchanged. While point estimates in both the two-step and threshold procedures sometimes give support to the existence of tipping points, inference in the two-step procedure is over-optimistic in rejecting the null of no tipping.

As an alternative way of addressing this objection, I also estimate separate threshold models for each local labour market, using native workforce growth as the dependent variable. In only two out of the 78 labour markets and time periods considered is there a significant drop in native workforce growth at the identified threshold. However, given the sample size requirements for threshold regressions, noted above, it is difficult to conclude much from local labour market-level specifications, which use on average fewer than 150 observations per threshold regression.

Finally, I also consider the possibility that different point estimates in my threshold regressions are driven by a changing sample over time. In Table A.3 I report the results of threshold regressions using a constant set of labour markets. My conclusions are unchanged relative to the full set of local labour markets.

5.2.3 Firm results

Until now I have focused on tipping points in the composition of local labour markets. This was motivated by, among other reasons, an assumption that the preference spillovers that generate the tipping behaviour are primarily caused by concerns for the prestige of the industry one works in. However, one cannot rule out *a priori* that a distaste for directly interacting with immigrants in the workplace is the source of preference spillovers. In this case, the correct unit of observation would be the establishment. Furthermore, tipping in the composition of establishments does not translate mechanically to tipping in the composition of the local industry, since the composition of individual firms can tip while the immigrant share in the industry is far from the tipping point. Conversely, the immigrant share in the industry might pass the tipping point even as most firms remain far from the tipping point.

As a robustness check, to ensure that the lack of evidence of tipping points is not a result of focusing on the wrong unit of analysis, I re-estimate the threshold model treating firms as the unit of observation. This causes a practical problem, in that most firms are small (on average 70 per cent of firms in local labour markets have fewer than 10 employees), so their base-year immigrant share bunches around certain values such as 0.25, 0.33, or 0.5. Furthermore, there is selective attrition, and small firms that survive the five year period generally experience native and immigrant workforce growth that is much greater than the average. This artificially creates discontinuities in the regression of workforce growth on immigrant share around these mass points. I therefore restrict my estimates to firms employing at least 10 employees in the base year.

There is however a countervailing practical benefit to using firms; since there are many more firms than industries it is now possible to estimate a separate threshold model for each local labour market. The results are no different to the industry specifications. Of the 78 local labour markets and time periods considered, only five have a significant break in native workforce growth, and in only three is the break negative. There is therefore no greater evidence in favour of the existence of tipping points in the composition of firms than there is for the existence of tipping points in the composition of local industries.

6 Conclusion

Tipping-like dynamics have been identified in neighbourhood composition, school enrolments or occupational composition, and have been used to explain segregation in these different settings. This paper considered whether such tipping points could also contribute to explaining documented patterns of segregation between immigrants and natives across workplaces.

Applying the two-stage procedure of Card et al. (2008), which first identifies tipping points and then uses regression-discontinuity methods to test for discontinuities in the evolution of the firm's workforce, this paper has found some support for the existence of tipping points in the composition of German firms, particularly in the period 1990-2000, when immigration to Germany was high. However, the evidence is not entirely robust to the choice of estimation method and there is some evidence of bunching in the base-year immigrant share relative to the candidate tipping point.

These inconclusive results motivate me to locate candidate tipping points and test for the existence of significant discontinuities using a single unified procedure, the test for the existence of a breakpoint in a threshold regression proposed by Hansen (1996, 2000). This procedure more conclusively rejects the hypothesis of tipping points in all periods. Furthermore, comparing this method to the methods used in previous work on tipping points in labour markets (Pan, 2015) suggests that the two-step procedure where only the second step is repeated for each bootstrap sample has a tendency to under-reject the null of no tipping point. This finding is cause for some circumspection about whether there are tipping points labour markets.

Given the limited evidence of tipping points presented here, one can conclude that preference interactions are unlikely to be a leading explanation of observed workplace segregation. Future research could productively investigate what role alternative theoretical mechanisms, particularly the role of social networks in the job search process, play in explaining observed patterns of segregation.

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7 Figures

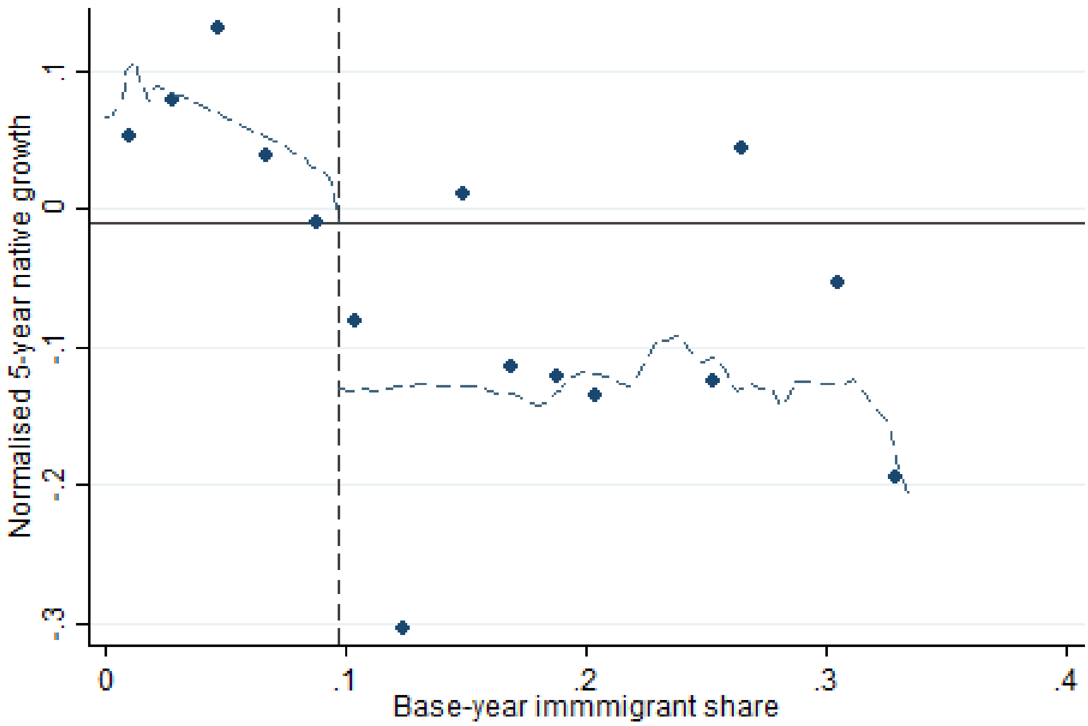

Figure 1: Growth in native workforce normalised by total workforce in base year, plotted against base year immigrant share for 3-digit industries in Düsseldorf, 1990-1995. Observations are grouped by base-year immigrant share in 2 percentage point bins, for data protection reasons. The dashed line represents fitted values from a local linear specification, estimated separately on either side of the candidate tipping point with a bandwidth of 7.5 percentage points.

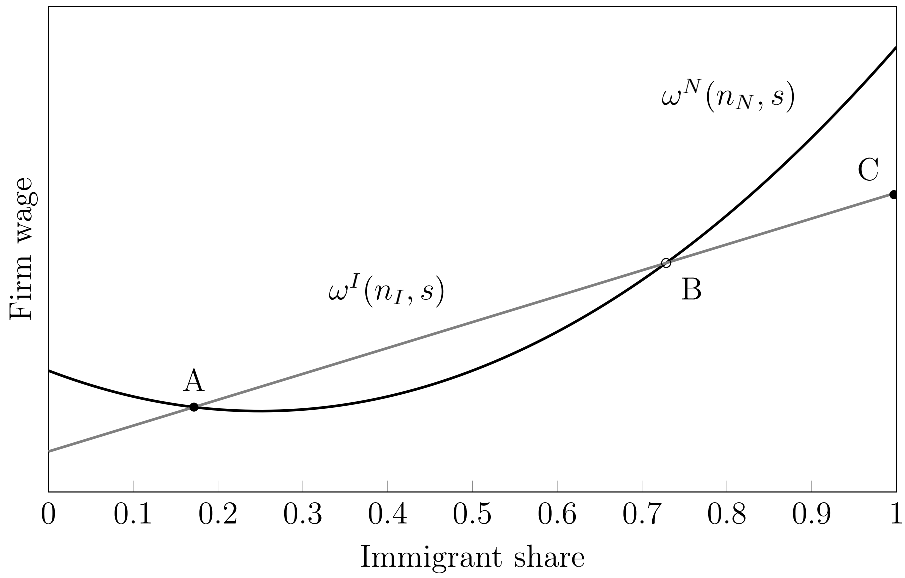

Figure 2: Immigrant and Native inverse labour supply to the firm with three equilibria. A and C are stable, B is unstable.

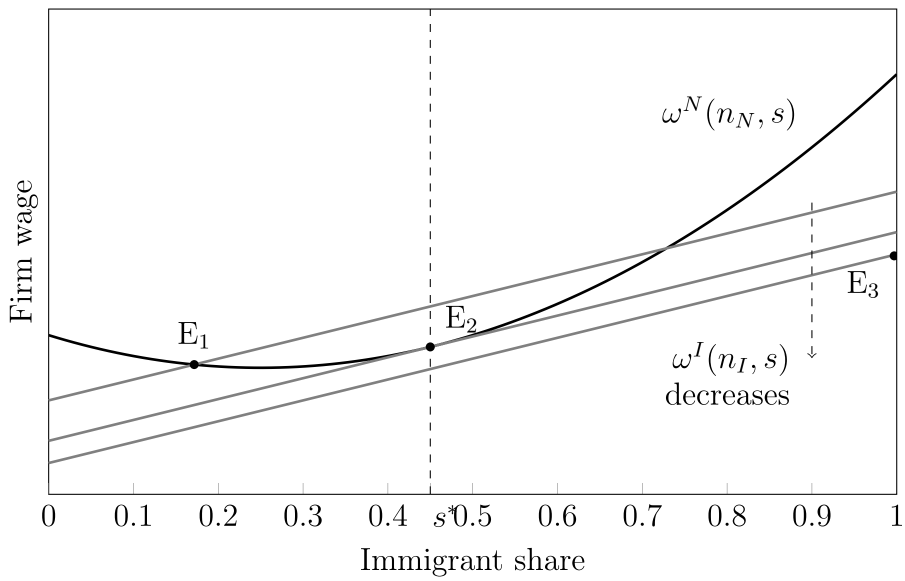

Figure 3: Increasing supply of immigrant workers shifts their relative supply outwards, decreasing the wage demanded for any value of s. The equilibrium immigrant share starts at E_1 and shifts right as the inverse supply of migrants increases. The equilibrium E_2 is the maximum integrated equilibrium, the associated migrant share is s^*. If the supply of immigrant workers increases further, the firm will jump to the segregated equilibrium E_3, hiring only immigrants.

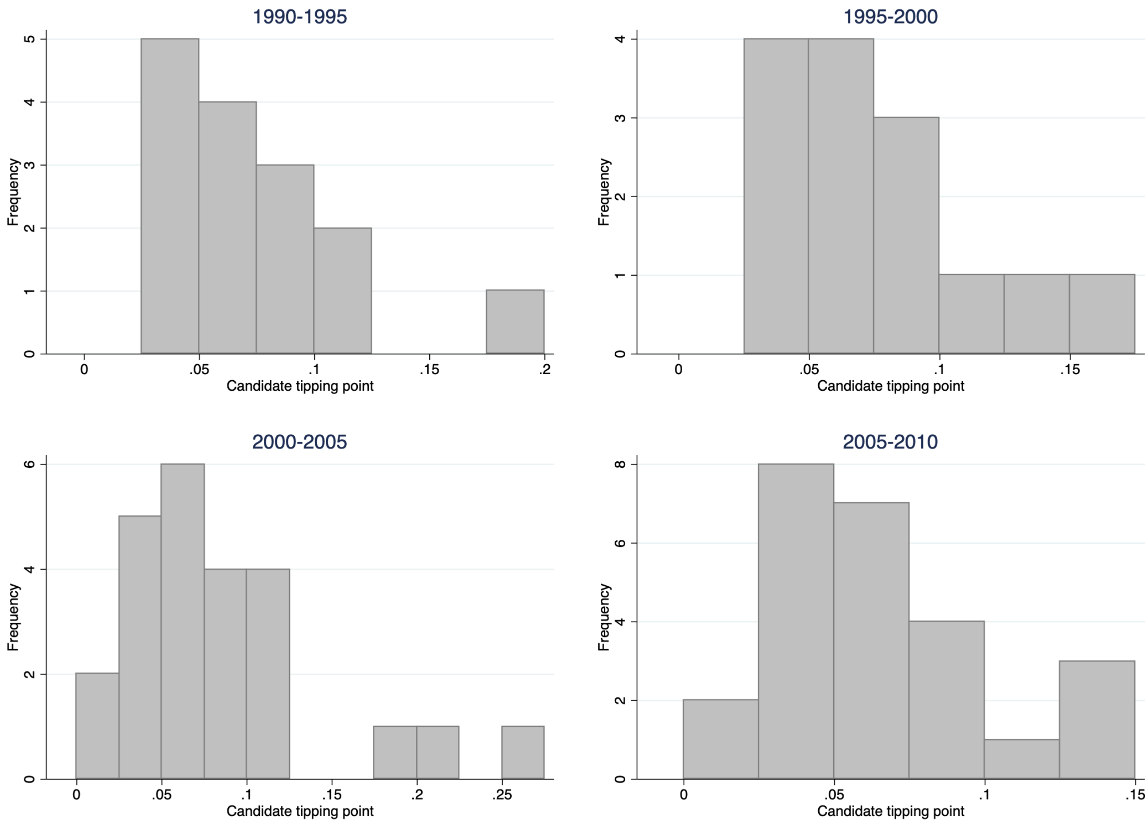

Figure 4: The figures plot the distribution of candidate tipping points identified by the search procedure described in section 4.1. The candidate tipping points are specific to each local labour market

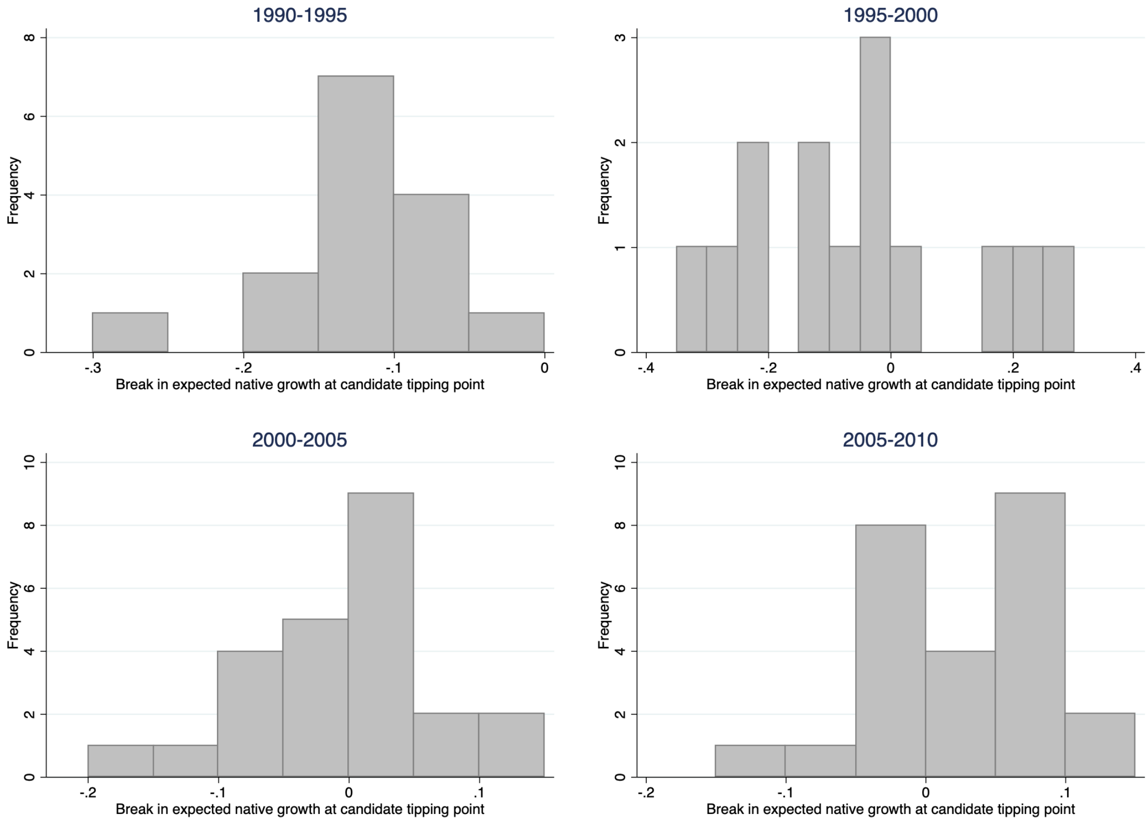

Figure 5: The figures plot the distribution of $\hat{\delta}_{c(j),t}$ the estimated change in native growth in the search procedure described in section 4.1, defined in Equation (4). The estimated breaks are specific to each local labour market.

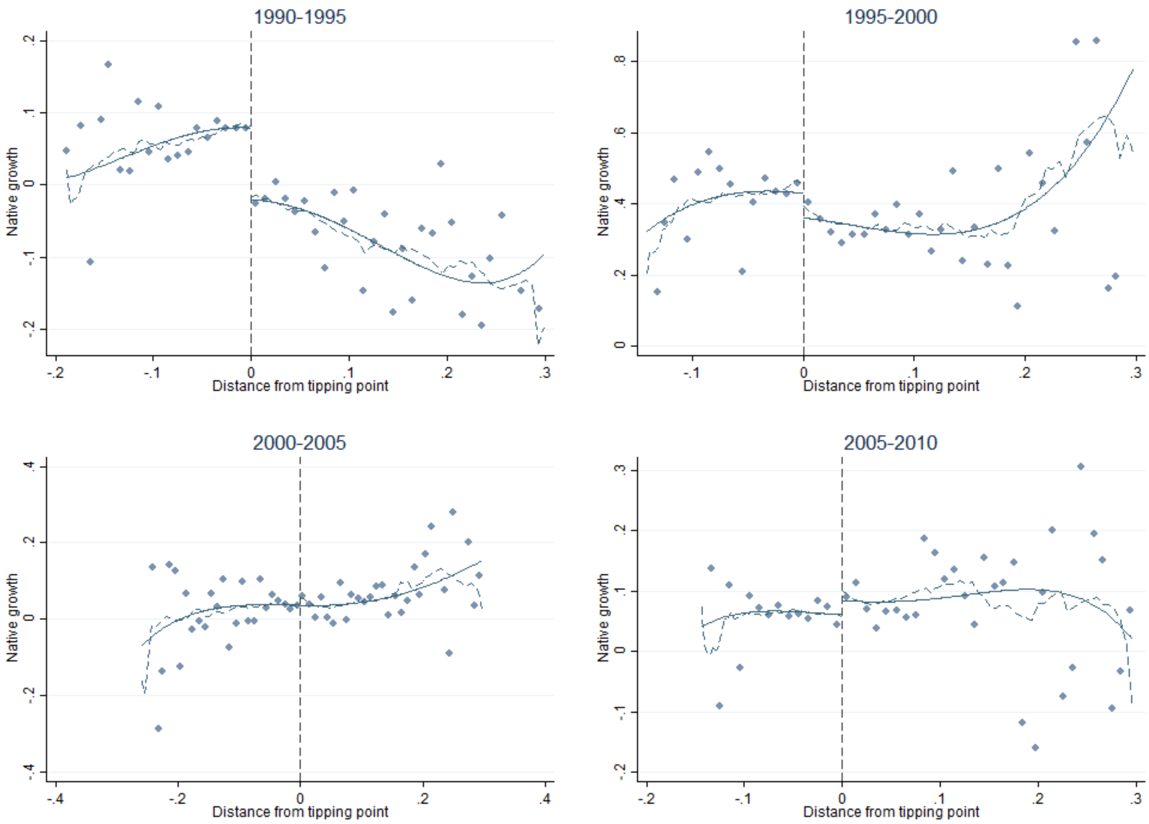

Figure 6: The figures plot the change in the five-year change in the native workforce, normalised by the total base-year workforce, against the distance between the base-year immigrant share and the local labour market-specific tipping point. Local industries are binned in one percentage point bins, for data protection reasons. The solid line represents fitted values from a global parametric specification with no controls and an intercept shift; the dashed line represents fitted values from a local linear specification estimated on either side of zero, with a bandwidth of five percentage points.

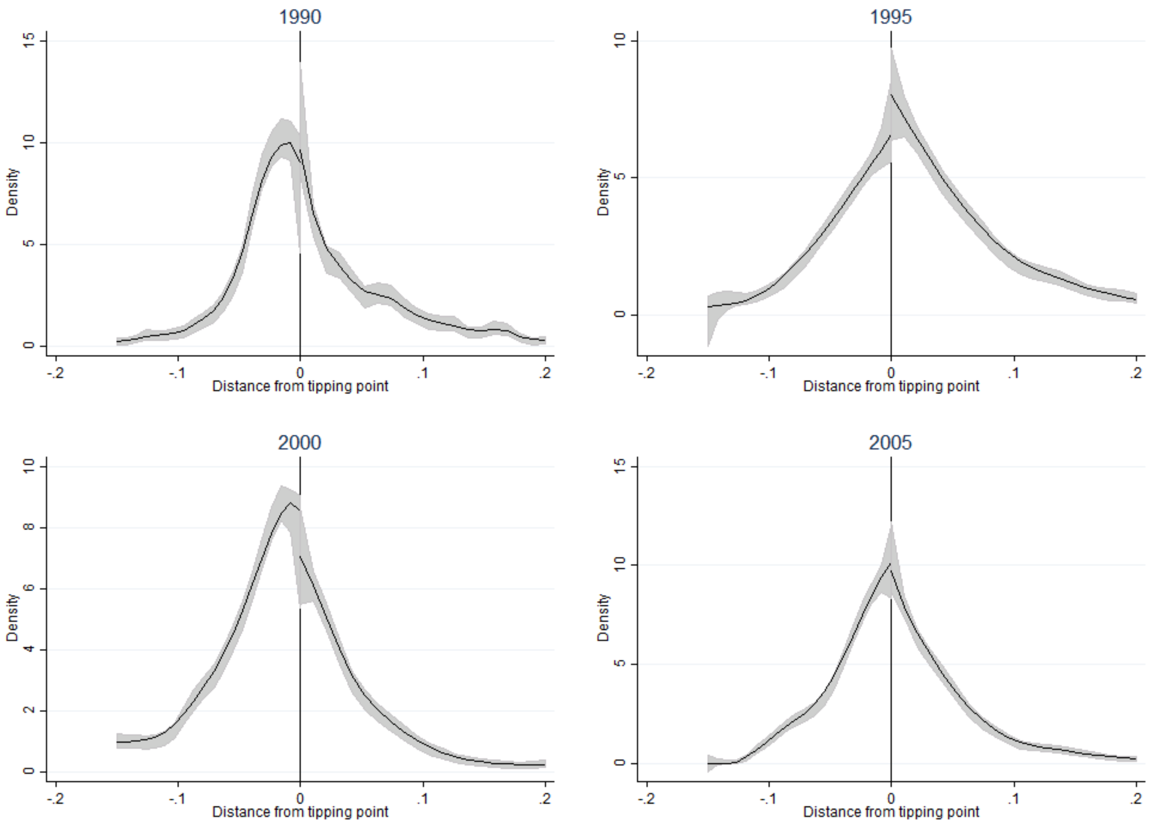

Figure 7: The solid line represents the estimated density of the running variable, the distance between the base-year immigrant share and the candidate tipping point in the base year, using a second-order local polynomial and MSE-optimal bandwidths, estimated separately on either side of zero. The shaded area represents a bias-corrected (asymmetric) 95 per cent confidence interval for the density, where the bias is estimated using a third-order polynomial.

8 Tables

Table 1: Index of coworker segregation

	1990	1995	2000	2005
	ICS	ICS	ICS	ICS
Unconditional	0.15	0.17	0.18	0.17
Conditional on industry	0.12	0.13	0.14	0.13
Conditional on location	0.14	0.16	0.17	0.16
Conditional on location and industry	0.08	0.09	0.10	0.10
Establishments	523246	548910	723572	747317

Note: Indexes of coworker segregation calculated from the *Betriebshistorikpanel*, including all establishments in West Germany hiring two or more employees. The conditional indexes condition on either three-digit industry (NACE Rev. 1), local labour market, or both.

Table 2: Summary statistics, local industries

	1990–1995		1995–2000		2000–2005		2005–2010	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Size	2836.54	4888.10	2897.99	4693.43	2684.20	4629.99	2649.98	4610.44
Median wage (2010 EUR)	86.24	20.35	92.10	21.86	84.88	25.90	83.00	28.48
Native growth	0.02	0.31	0.38	0.57	0.04	0.31	0.07	0.27
Immigrant growth	0.02	0.07	0.02	0.12	0.00	0.05	0.01	0.05
Workforce growth	0.04	0.35	0.41	0.65	0.04	0.35	0.09	0.30
Immigrant share	0.09	0.07	0.11	0.09	0.08	0.07	0.08	0.06
Low-education share	0.23	0.12	0.20	0.11	0.18	0.10	0.14	0.09
Share of small firms	0.69	0.21	0.70	0.19	0.70	0.19	0.71	0.18
Local industries	2001		1867		3073		3172	
Local labour markets	15		14		24		25	

Note: Reports summary statistics on either base-year characteristics or changes in workforce composition for included local industries. See main text for sample selection. The local labour markets of Bremen, Düsseldorf, Essen, Frankfurt am Main, Hamburg, Hanover, Karlsruhe, Köln, Mannheim, München, Münster, Nürnberg, Stuttgart, and Wiesbaden are included in all periods. The local labour markets of Augsburg, Freiburg im Breisgau, Heilbronn, Kassel, Kiel, Koblenz, Oldenburg, Regensburg, Saarbrücken, Siegen, and Ulm are included in at least one period.

Table 3: Summary statistics, candidate tipping points

	1990–1995	1995–2000	2000–2005	2005–2010
Mean	0.073	0.077	0.086	0.065
St. dev.	0.043	0.040	0.062	0.038
Labour markets	15	14	24	25
Share $\hat{\delta}_{c(j),t}$ negative	1	0.71	0.46	0.40
Correlations				
1990-1995	1.00			
1995-2000	0.56	1.00		
2000-2005	0.76	0.50	1.00	
2005-2010	0.42	0.64	0.48	1.00

Note: Tipping points for identified candidate tipping points. Share $\hat{\delta}_{c(j),t}$ negative refers to the share of local labour markets for which there is a negative change in native workforce growth at the identified candidate tipping point. See main text for details on the method used to calculate the tipping points. Correlations refer to pairwise correlations across base years, for local labour markets where a candidate tipping point is identified in both base years.

Table 4: Discontinuities in workforce growth at candidate tipping points

	Global spec.		Local spec.			
	natives	natives	natives	natives	migrants	share
Discontinuity, 1990	-0.10**	-0.08**	-0.10**	-0.11**	-0.01*	0.00
	(0.03)	(0.03)	(0.03)	(0.04)	(0.01)	(0.00)
Robust p-value	–	–	0.05	0.03	0.07	0.84
Bandwidth	–	–	0.050	0.031	0.032	0.041
Local industries	1995	1995	2001	2001	2001	2001
Discontinuity, 1995	-0.07	-0.08	-0.06	-0.02	-0.00	0.00
	(0.06)	(0.06)	(0.07)	(0.09)	(0.01)	(0.00)
Robust p-value	–	–	0.65	0.98	0.90	0.83
Bandwidth	–	–	0.050	0.026	0.037	0.026
Local industries	1840	1840	1867	1867	1867	1867
Discontinuity, 2000	-0.00	0.01	0.03	0.03	0.01	0.00
	(0.02)	(0.02)	(0.03)	(0.03)	(0.00)	(0.00)
Robust p-value	–	–	0.62	0.18	0.15	0.78
Bandwidth	–	–	0.050	0.049	0.041	0.039
Local industries	3059	3059	3072	3072	3072	3072
Discontinuity, 2005	0.02	0.02	0.04*	0.05*	0.00	0.00
	(0.02)	(0.02)	(0.02)	(0.03)	(0.00)	(0.00)
Robust p-value	–	–	0.01	0.04	0.61	0.93
Bandwidth	–	–	0.050	0.028	0.028	0.023
Local industries	3159	3159	3172	3172	3172	3172
Labour market FE	no	yes	no	no	no	no
Controls	no	yes	no	no	no	no

Note: Estimated discontinuity in the dependent variable at the candidate tipping point. In columns 1-4 the dependent variable is the change in the native workforce, normalised by the total workforce in the base year, in column 5 it is the normalised immigrant workforce change, in column 6 it is the change in the share of immigrants in the workforce. I report bootstrap standard errors from 1000 replications in parentheses. For the nonlinear local specifications, I also report bias-corrected robust p-values and the bandwidth used. Column 3 uses an imposed bandwidth, while columns 4-6 use data-generated MSE-optimal bandwidths. $^{+}$ $p<0.1$, * $p<0.05$, ** $p<0.01$.

Table 5: Discontinuities in covariates at candidate tipping points

	Low-skill share	Log wage	Average firm size	HHI
Discontinuity, 1990	0.01	-0.05	-7.39	0.00
	(0.01)	(0.03)	(7.73)	(0.01)
Robust p-value	0.43	0.06	0.33	0.74
Bandwidth	0.031	0.028	0.036	0.040
Local industries	2001	2001	2001	2001
Discontinuity, 1995	0.03^{+}	-0.04	12.06	-0.03
	(0.02)	(0.04)	(10.26)	(0.02)
Robust p-value	0.04	0.41	0.27	0.43
Bandwidth	0.023	0.026	0.028	0.027
Local industries	1867	1867	1867	1867
Discontinuity, 2000	0.02	-0.02	-0.09	-0.00
	(0.01)	(0.04)	(6.27)	(0.01)
Robust p-value	0.23	0.59	0.85	0.80
Bandwidth	0.033	0.034	0.029	0.048
Local industries	3073	3073	3073	3073
Discontinuity, 2005	0.00	0.01	-4.46	0.01
	(0.01)	(0.04)	(6.36)	(0.02)
Robust p-value	0.64	0.58	0.54	0.27
Bandwidth	0.022	0.022	0.031	0.019
Local industries	3172	3172	3172	3172

Note: Estimated discontinuity in the dependent variable at the candidate tipping point for various base year characteristics, estimated by local linear regression on either side of zero. I report bootstrap standard errors from 500 replications in parentheses. I also report bias-corrected robust p-values and the data-generated MSE-optimal bandwidth used. $^{+}$ $p<0.1$, * $p<0.05$, ** $p<0.01$.

Table 6: Test of discontinuities in the distribution of the running variable

	Test
1990–1995	
P-value	.038
Industry-cities	2001
1995–2000	
P-value	.374
Industry-cities	1867
2000–2005	
P-value	.772
Industry-cities	3073
2005–2010	
P-value	.791
Industry-cities	3172

Note: Running variable is distance between base-year immigrant share and candidate tipping point. The test is constructed by estimating the density of the running variable to the right and left of zero, using a second-order local polynomial. I report robust, bias corrected p-values for the test of no discontinuity, where the bias is estimated by fitting the density with a local third order polynomial.

Table 7: Threshold regressions with intercept shift

	natives	natives	natives	immigrants	share
1990-1995					
Tipping point	0.076	0.076	0.048	0.156	0.057
NLS p-value	0.114	0.146	0.248	0.756	0.296
OLS p-value	0.004	0.004	0.023	0.059	0.010
Discontinuous change	-0.078	-0.076	-0.076	-0.026	0.0075
Local industries	2001	2001	2001	2001	2001
1995-2000					
Tipping point	0.042	0.042	0.042	0.151	0.167
NLS p-value	0.576	0.368	0.434	0.890	0.694
OLS p-value	0.023	0.008	0.011	0.233	0.082
Discontinuous change	0.14	0.14	0.14	-0.020	-0.0077
Local industries	1867	1867	1867	1867	1867
2000-2005					
Tipping point	0.041	0.041	0.041	0.100	0.106
NLS p-value	0.376	0.526	0.680	0.068	0.022
OLS p-value	0.038	0.022	0.030	0.004	0.000
Discontinuous change	0.050	0.051	0.049	0.016	0.012
Local industries	3073	3073	3073	3073	3073
2005-2010					
Tipping point	0.059	0.062	0.062	0.101	0.101
NLS p-value	0.142	0.228	0.124	0.548	0.806
OLS p-value	0.006	0.006	0.003	0.110	0.119
Discontinuous change	-0.052	-0.048	-0.052	-0.0083	-0.0044
Local industries	3172	3172	3172	3172	3172
Controls	no	yes	yes	yes	yes
FE	no	no	yes	yes	yes

Note: In columns 1-3 the dependent variable is the normalised change in the native workforce, in column 4 it is the normalised change in the immigrant workforce, in column 5 it is the change in the immigrant share. The table reports the estimated location of the breakpoint, the tipping point, and the discontinuous change in the outcome at the tipping point. The NLS p-value is calculated from 500 parametric bootstrap iterations, allowing the breakpoint to vary, the OLS p-value is calculated from 500 nonparametric bootstrap iterations, keeping the location of the breakpoint at the value identified using the original sample. The p-values refer to the null of the test of no-discontinuities in the outcome variable.

A Tables

Table A.1: Discontinuities at candidate tipping points, constant set of labour markets

	Global spec.		Local spec.			
	natives	natives	natives	natives	migrants	share
Discontinuity, 1990	-0.11**	-0.09**	-0.11**	-0.11**	-0.01*	0.00
	(0.03)	(0.02)	(0.03)	(0.04)	(0.01)	(0.00)
Robust p-value	–	–	0.05	0.02	0.03	0.90
Bandwidth	–	–	0.050	0.034	0.034	0.041
Local industries	1895	1895	1901	1901	1901	1901
Discontinuity, 1995	-0.07	-0.08	-0.06	-0.02	-0.00	0.00
	(0.06)	(0.06)	(0.07)	(0.09)	(0.01)	(0.00)
Robust p-value	–	–	0.65	0.98	0.90	0.83
Bandwidth	–	–	0.050	0.026	0.037	0.026
Local industries	1840	1840	1867	1867	1867	1867
Discontinuity, 2000	0.01	0.02	0.04	0.05	0.01	0.00
	(0.02)	(0.02)	(0.04)	(0.04)	(0.01)	(0.00)
Robust p-value	–	–	0.58	0.14	0.17	0.38
Bandwidth	–	–	0.050	0.049	0.034	0.029
Local industries	2008	2008	2019	2019	2019	2019
Discontinuity, 2005	0.00	0.01	0.03	0.04	-0.00	-0.00
	(0.02)	(0.02)	(0.03)	(0.04)	(0.00)	(0.00)
Robust p-value	–	–	0.19	0.23	0.71	0.66
Bandwidth	–	–	0.050	0.029	0.019	0.019
Local industries	1992	1992	2002	2002	2002	2002
Labour market FE	no	yes	no	no	no	no
Controls	no	yes	no	no	no	no

Note: Estimated discontinuity in the dependent variable at the candidate tipping point. In columns 1-4 the dependent variable is the change in the native workforce, normalised by the total workforce in the base year, in column 5 it is the normalised immigrant workforce change, in column 6 it is the change in the share of immigrants in the workforce. I report bootstrap standard errors from 1000 replications in parentheses. For the nonlinear local specifications, I also report bias-corrected robust p-values and the bandwidth used. Column 3 uses an imposed bandwidth, while columns 4-6 use data-generated MSE-optimal bandwidths. $^{+}$ $p<0.1$, * $p<0.05$, ** $p<0.01$.

Table A.2: Discontinuities at candidate tipping points, common tipping point by year

	Global spec.		Local spec.			
	natives	natives	natives	natives	migrants	share
Discontinuity, 1990	-0.05^{+}	-0.04	-0.03	-0.06	-0.00	0.00
	(0.03)	(0.03)	(0.03)	(0.07)	(0.01)	(0.01)
Robust p-value	–	–	0.52	0.43	0.93	0.52
Bandwidth	–	–	0.050	0.015	0.015	0.016
Local industries	1989	1989	2001	2001	2001	2001
Discontinuity, 1995	-0.01	-0.00	-0.00	-0.02	-0.00	0.00
	(0.05)	(0.05)	(0.07)	(0.12)	(0.01)	(0.00)
Robust p-value	–	–	0.81	0.77	0.44	0.20
Bandwidth	–	–	0.050	0.021	0.019	0.022
Local industries	1842	1842	1867	1867	1867	1867
Discontinuity, 2000	-0.02	-0.01	-0.06	-0.03	-0.00	0.00
	(0.03)	(0.02)	(0.04)	(0.05)	(0.01)	(0.01)
Robust p-value	–	–	0.34	0.67	0.96	0.89
Bandwidth	–	–	0.050	0.032	0.031	0.025
Local industries	3060	3060	3073	3073	3073	3073
Discontinuity, 2005	-0.03	-0.03	-0.03	-0.01	-0.00	0.00
	(0.02)	(0.02)	(0.03)	(0.04)	(0.00)	(0.00)
Robust p-value	–	–	0.61	0.93	0.88	0.38
Bandwidth	–	–	0.050	0.011	0.013	0.013
Local industries	3147	3147	3172	3172	3172	3172
Labour market FE	no	yes	no	no	no	no
Controls	no	yes	no	no	no	no

Note: Estimated discontinuity in the dependent variable at the candidate tipping point. In columns 1-4 the dependent variable is the change in the native workforce, normalised by the total workforce in the base year, in column 5 it is the normalised immigrant workforce change, in column 6 it is the change in the share of immigrants in the workforce. I report bootstrap standard errors from 1000 replications in parentheses. For the nonlinear local specifications, I also report bias-corrected robust p-values and the bandwidth used. Column 3 uses an imposed bandwidth, while columns 4-6 use data-generated MSE-optimal bandwidths. $^{+}$ $p<0.1$, * $p<0.05$, ** $p<0.01$.

Table A.3: Threshold regressions with intercept shift, constant set of labour markets

	natives	natives	natives	immigrants	share
1990-1995					
Tipping point	0.075	0.049	0.049	0.156	0.160
NLS p-value	0.190	0.260	0.550	0.900	0.460
OLS p-value	0.008	0.046	0.042	0.158	0.173
Discontinuous change	-0.071	-0.073	-0.073	-0.022	-0.011
Local industries	1901	1901	1901	1901	1901
1995-2000					
Tipping point	0.042	0.042	0.042	0.151	0.167
NLS p-value	0.576	0.368	0.434	0.890	0.694
OLS p-value	0.023	0.008	0.011	0.233	0.082
Discontinuous change	0.14	0.14	0.14	-0.020	-0.0077
Local industries	1867	1867	1867	1867	1867
2000-2005					
Tipping point	0.042	0.042	0.042	0.151	0.167
NLS p-value	0.560	0.330	0.420	0.890	0.710
OLS p-value	0.019	0.012	0.013	0.240	0.099
Discontinuous change	0.144	0.141	0.136	-0.020	-0.008
Local industries	2019	2019	2019	2019	2019
2005-2010					
Tipping point	0.131	0.062	0.062	0.141	0.141
NLS p-value	0.440	0.640	0.770	0.620	0.850
OLS p-value	0.024	0.044	0.047	0.153	0.140
Discontinuous change	0.067	-0.046	-0.045	0.011	0.006
Local industries	2002	2002	2002	2002	2002
Controls	no	yes	yes	yes	yes
FE	no	no	yes	yes	yes

Note: In columns 1-3 the dependent variable is the normalised change in the native workforce, in column 4 it is the normalised change in the immigrant workforce, in column 5 it is the change in the immigrant share. The table reports the estimated location of the breakpoint, the tipping point, and the discontinuous change in the outcome at the tipping point. The NLS p-value is calculated from 100 parametric bootstrap iterations, allowing the breakpoint to vary, the OLS p-value is calculated from 100 nonparametric bootstrap iterations, keeping the location of the breakpoint at the value identified using the original sample. The p-values refer to the null of the test of no-discontinuities in the outcome variable.