

# **Close Neighbors, Separate Lives.**

**An Investigation of Residential Barriers and Bridges to  
the Social Integration of Young Immigrants in Germany**

**Inaugural dissertation**

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# Chapter 1

## Introduction

### 1.1 Advice from an unusual visitor

It is the morning of May 12, 2015. The schoolyard of Röntgen-Schule—a comprehensive secondary school at the border of Berlin-Neukölln<sup>1</sup>—is crowded with people. Students and teachers alike have lined up, impatiently awaiting the upcoming event. Many of them hold their cellphone cameras ready, hoping to get a snapshot of the unusual visitor who is about to arrive.

Excitement rises when the long-awaited motorcade arrives in front of the school building. As part of the country-wide *EU-Project Day at Schools*, German Chancellor Angela Merkel has come to promote and spark an interest in the European idea among the students.

While Merkel enters the stage of the school auditorium to discuss with a panel of selected students, most attending realize that today's discussion will not revolve much around the European idea, not to mention the EU. Instead, the discussion quickly shifts toward the issue of integration. The young panelists on stage—representing a cross-section of students at Röntgen-Schule—share their personal experiences of what it is like to have an immigrant background in Germany.

One of the students wonders why Germans have such a strong opinion about his district; after all, he would simply like to have German friends. Merkel replies, she has a suggestion: why not for once go and see a movie in Marzahn instead of Neukölln or maybe spend your spare time in Charlottenburg!?! Clearly, this is rather practical advice from the unusual visitor: if you want to make native friends just spend your time in neighborhoods where they live.<sup>2</sup>

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<sup>1</sup>Over the last two decades the northern part of Neukölln has gained country-wide infamy as being one of Germany's prime examples of a decaying neighborhood. Its rather large immigrant population in combination with high poverty rates is argued to provide leeway for so-called *Parallelgesellschaften* (parallel societies) to emerge, tight-knit immigrant communities whose norms and rules differ from those of the majority population. One prominent advocate of this view is the former mayor of Neukölln, Heinz Buschkowsky. Besides repeated appearances on German television, he gained popularity by speaking his mind in his bestseller titled *Neukölln ist überall* (Neukölln is everywhere).

<sup>2</sup>I took all details on Merkel's visit from <http://www.tagesspiegel.de/berlin/angela-merkel-in-berlin-neukoelln-geht-doch-einfach-mal-ins-kino-nach-marzahn/11768834.html>, from <http://www.roentgen-sekundarschule.de/archiv/besuch-der-bundeskanzlerin/>, and from <http://www.spiegel.de/politik/deutschland/angela-merkel-besucht-berliner-schule-und-geraet-in-integrationsdebatte-a-1033464.html>, all accessed on October 4, 2016.



About one year later, on May 25, 2016, the German Federal Government—with Chancellor Merkel still holding office—comes together in a special Cabinet Meeting in Meseberg, about 70 km south of Berlin-Neukölln and its Röntgen-Schule. It is here where the German Government adopts the *Integration Act*, introducing a set of laws aimed at the improvement of recent immigrants' integration into German society. Meanwhile, Germany has seen an unprecedented net inflow of nearly one million immigrants<sup>3</sup>—to a large part due to an increase in young asylum seekers from war-driven Syria, and from Afghanistan and Iraq. The new arrivals have made the integration of immigrants a prominent topic in German media and on political agendas, now culminating in the adoption of the *Integration Act*. One of the new laws addresses refugees' choice of residence. In a nutshell, it enables federal states to assign a specific place of residence to refugees for up to three years after their arrival. Some weeks later, the Federal Government will justify this *Residence Rule* in a public statement:

What makes for successful integration? One key aspect is the question of where someone lives. That is why asylum seekers will in future be assigned a place of residence. Because if, for example, too many refugees move to urban centres integration becomes very difficult.<sup>4</sup>

Whether aimed at one single student from a Neukölln school or at hundreds of thousands of new arrivals all over the country, Merkel's advice clearly carries the same message: For the social integration of young immigrants in Germany to succeed, neighborhoods matter greatly. These can serve either as *residential barriers* that block their paths towards successful social integration or as *residential bridges* that support them.

This book is not about Angela Merkel. Instead, it examines whether her advice really takes root.

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<sup>3</sup>According to a press release of German Federal Statistics the net migration of foreigners coming to Germany in the year 2015 reached an all-time high of 1.1 million (Pressemitteilung Nr. 105, 21.03.2016).

<sup>4</sup>see [https://www.bundesregierung.de/Content/EN/Artikel/2016/07\\_en/2016-05-25-integrationsgesetz-beschlossen\\_en.html](https://www.bundesregierung.de/Content/EN/Artikel/2016/07_en/2016-05-25-integrationsgesetz-beschlossen_en.html), accessed on October 4, 2016.

### Why the advice may not always help

It may seem odd to question the existence of residential barriers and bridges in the course of (young) immigrants' social integration. Who would doubt that it takes local contact possibilities for lasting social exchange to emerge? And yet, previous scholarly attempts at identification in various Western European countries are surprisingly inconclusive. On the one hand, there is clear evidence that residential barriers and bridges are at work, especially when it comes to lasting relations between immigrants and natives (Martinovic et al., 2009; Semyonov and Glikman, 2009; Vervoort, 2012; Vervoort et al., 2011). However, there are also a number of studies yielding no association between the ethnic compositions of immigrants' neighborhoods and of their social relations, especially so among those conducted in Germany (Drever, 2004; Esser, 1986).

And these negative findings may not be accidental. On closer examination, several objections arise suggesting that residential barriers and bridges play a smaller role in the social integration of young immigrants in Germany than usually assumed. First, spatial closeness may not necessarily breed social closeness. Generally, physical proximity plays an important role in friendship formation (Hipp and Perrin, 2009; Preciado et al., 2012). More and more studies, however, suggest that this tendency may apply more strongly to some groups than for others. A study on adolescents from two German cities, for example, finds that girls' friends live significantly more often in other neighborhoods than boys' friends do. The same applies for children of higher educational background as compared to children with less educated parents (Oberwittler, 2004). Environments other than the local neighborhood—with potentially different ethnic compositions—may thus be important meeting contexts, at least so for some adolescents. Consequentially, being surrounded by native neighbors does not necessarily imply that one's actual meeting contexts will be native-dominated, as well.

As a second objection, ethnic residential segregation in Germany may be too low for residential barriers to emerge. Most immigrants in Germany concentrate in urban areas in the Western part of the country (Alba and Foner, 2015). Previous studies have shown, however, that the level of ethnic residential segregation within these German cities is moderate in size (Alba and Foner, 2015; Musterd,

2005). There are several reasons why, ranging from successful desegregation policies to a heterogeneous housing stock within German neighborhoods (Drever and Clark, 2006). Whatever the reasons may be, young immigrants are likely to live in areas where they encounter a substantial number of natives. So, even if the first objection did not hold and friendships really formed in local neighborhoods, local ethnic concentrations may not be elevated enough to turn into actual residential barriers impeding contact with natives.

Finally, a third objection suggests that immigrants and natives may refuse to establish a lasting exchange even though they could. Many studies have shown over and over again that friendship formation follows systematic patterns. Individual tastes and structural constraints guide peoples' friendship choices. The homophily principle clearly ranks among the most dominant tie formation tendencies. It states that people tend to prefer contact with similar others (McPherson et al., 2001). Ethnic homophily in friendships (i.e., a taste for ethnically similar friends) is an especially well-documented phenomenon, also among young immigrants and natives in Germany (Kalter and Kruse, 2015; Smith et al., 2014; Windzio and Bicer, 2013). From this perspective, young immigrants may not transform the presence of native neighbors into residential bridges, because one or even both of the two sides (i.e., natives or immigrants) prefer their own kind as friends.

To summarize, residential barriers and bridges to young immigrants' social integration are not as self-evident as initially thought. Instead, both the outlined theoretical arguments and inconclusive findings suggest a *conditional effect of the neighborhood*: some young immigrants transform the presence/absence of native neighbors into residential bridges/barriers, whereas others do not.

### **This book's aim**

This being said, the ultimate aim of this book becomes obvious: to explain why neighborhoods affect the social integration of young immigrants differently. While there is first empirical support that residential barriers and bridges are stronger for some immigrants than for others (Schlueter, 2012; van der Laan Bouma-Doff, 2007), we do not know *why*. This book therefore asks:

What are the conditions under which residential barriers and bridges to the social integration of young immigrants in Germany emerge?

Needless to say that answers to this question are of strong public and political interest. This book, however, is not primarily intended as practical guidance for policy makers. Instead, it addresses a more general, theoretical debate concerning persisting integration differences among (young) immigrants in Western societies. Proponents of a *straight-line assimilationist* perspective (e.g. Gordon, 1964) argue that residential relocations are a central mechanism by which immigrants assimilate into the mainstream over generations.<sup>5</sup> In other words, residential barriers and bridges should function universally: a high share of native neighbors should generally translate into a high share of native friends. Any deviation from this association should be temporary and rather unsystematic. In line with this assumption, the extent to which immigrants are spatially separated has often been seen as a measure of social integration (e.g. Massey and Denton, 1985). In contrast, proponents of a *segmented assimilationist* perspective posit that there is not one single mainstream but several societal segments for immigrants to assimilate into. An interplay of individual and contextual characteristics thereby determines immigrants' integration paths or whether they remain more bounded within their own ethnic community (Wilson, 1987; Zhou, 1997). In other words, residential barriers and bridges should function conditionally: a high share of native neighbors does not necessarily translate into a high share of native friends (and vice versa).<sup>6</sup> This book determines and explains the conditionality of residential barriers and bridges to the social integration of young immigrants in

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<sup>5</sup>Both *Classic* and *New Assimilationists* share the view that two other mechanisms being important are intermarriage and occupational mobility. In contrast to their predecessors *New Assimilationists* (e.g. Alba and Nee, 2003) thereby acknowledge that assimilation into the mainstream may not solely come via boundary crossing (i.e., immigrants moving into the mainstream) but also via boundary shifting (i.e., mainstream boundaries move such that immigrants are incorporated).

<sup>6</sup>Segmented assimilation theorists make the idea of conditional effects very explicit, arguing that integration paths depend on “factors external to a particular immigrant group, such as [...] spatial segregation, and factors intrinsic to the group, such as financial and human capital upon arrival, family structure, community organization, and cultural patterns of social relations. These two sets of factors affect the life chances of immigrant children not only additively but also interactively.” (Zhou, 1997, p.999)

Germany, showing whether differential integration paths are driven by individual decisions and tastes and are as such potentially temporary in nature or whether they are structurally determined and thus more persisting over time.

To find out, the book develops a comprehensive theoretical perspective, relying on a spatially informed framework of friendship formation. This framework lays out how adolescents' place of residence affects the two central decisions they face when making friends: the choice of a meeting context—with a focus on their most important one, the school—and the subsequent choice of friendships. Taking this perspective, the book makes two further contributions: on the one hand it is the first to comprehensively determine the mechanisms through which the place of residence affects friendship choices, thus adding to the debate on adolescent friendship formation. On the other hand, it is the first work to systematically investigate the role that the institutional rule of ability tracking plays in the emergence of ethnic segregation across schools, thus adding to the debate on secondary school choices.

Most importantly, however, this book aims to provide an integrated view on residential barriers and bridges by combining the two fields of study—school and friendship choices. Taking a combined perspective allows me to test where exactly in the process of friendship formation residential barriers and bridges come into existence, whether in the course of context or of friendship choices. Only with this knowledge is it possible to learn whether and when to focus on residential patterns in order to let young immigrants establish lasting contact with the native population.

## 1.2 The empirical puzzle

### Young immigrants in Germany

Now that we have learned about the central aims of this book, it is time to make matters concrete. The best way to do so is by turning to the problem at hand, the situation of young immigrants in Germany. At this point I refrain from providing lengthy details about the data the book makes use of. This will be done in due time at later stages of the book. For now, it suffices to note the

following: the analyses throughout this book rely on different sources. The most frequently applied one is thereby the first wave of the *Children of Immigrants Longitudinal Survey in Four European Countries* (CILS4EU, Kalter et al., 2016). The data consist of a nationally representative sample of all adolescents who attended a ninth grade in the school year 2009/10 in Germany, amounting to a net sample size of  $N = 5,013$ . In the following, I will rely on these CILS4EU data; the analyses thus inform about the population of all 14-15 year old students throughout Germany.<sup>7</sup>

Given that we are interested in how adolescents with an immigrant background fare in terms of contact to those without one, the first task is to clearly distinguish the two groups. When should an adolescent be referred to as having an immigrant background and when is he or she considered to have none? The categorization pursued in this book is as follows: All adolescents having at least one parent born outside of Germany (i.e., second generation) or who were born abroad themselves (i.e., first generation) are thought of as *immigrants* or *minority members*. All other adolescents, including those whose grandparents were born abroad (i.e., third generation) are referred to as *natives* or *majority members*.<sup>8</sup>

Table 1.1 provides an overview of the group sizes. The outlined categorization splits the sample into two more or less equally sized groups: about half of the respondents are defined as immigrants ( $N = 2,393$ ), the vast majority among them being of the second generation ( $N = 1,858$ ). However, this high immigrant proportion does not reflect the actual proportion of immigrants in the target population. The reason is twofold: First, in order to include a sufficiently high number of immigrants in the data, students attending schools with high immigrant proportions have been oversampled (i.e., stratified sampling approach). Second, not all sampled cases could actually be realized (i.e., unit non-response). To account for both sources of bias and to be able to infer the actual immigrant share among 14-15 year old students in Germany, the data have to be weighted.

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<sup>7</sup>There is one exception: students living in the federal state of Bavaria are not included in the CILS4EU data (cf. CILS4EU, 2016).

<sup>8</sup>I fully acknowledge that this binary categorization does not do justice to the manifold ways in which adolescents identify with one or more ethnic groups. From an analytical perspective, however, it seems necessary to take this simplified perspective when identifying ethnic disparities in social relations. The analyses in this book will account for more specific categorizations whenever I deem it to be necessary and possible.

Table 1.1: Adolescents' immigrant background in the German CILS4EU sample

	N	rel freq. (unweighted)	rel freq. (weighted)
Immigrants	2,393	.48	.28
<i>1st generation</i>	535	.11	.06
<i>2nd generation</i>	1,858	.37	.22
Natives	2,620	.52	.72
<i>3rd generation</i>	494	.10	.12
<i>w/o immigrant background</i>	2,126	.42	.59
Total	5,013	1.00	1.00

Source: CILS4EU, w1, v1.2.0

Doing so shows that still 28% of all 14-15 year old students in Germany are immigrants according to the chosen categorization (see last column of Table 1.1). In the following I concentrate on exactly this group of young immigrants. Moreover, given the descriptive purpose of the analyses in this first chapter, I will rely on weighted data—unless noted otherwise—thus referring to the target population of young immigrants in Germany.

According to the CILS4EU sample young immigrants in Germany are a diverse group in several regards, for example concerning their ethnic origin. They represent more than 100 different groups, the larger ones being Turkish ( $\sim 36\%$  of all immigrants), followed by groups from the Former Soviet Union ( $\sim 12\%$ ) and Former Yugoslavia ( $\sim 9\%$ ) and Polish immigrants ( $\sim 7\%$ ). But heterogeneity not only exists in ethnic terms, there is also a wide variation concerning their socioeconomic status (*SES* from here on). One prominent way to quantify a person's SES is the *International Socio-Economic Index of Occupational Status (ISEI)*. The index assigns values to different occupational categories, whereby higher values indicate a higher SES (within a range of 18-88, Ganzeboom et al., 1992). Given that respondents' parents reported their occupations it was possible to construct ISEI scores for all respondents.<sup>9</sup> On average, young immigrants

<sup>9</sup>Not all parents agreed to participate in the survey. If no parental information was available, I relied on students' reports of their parents' occupations. If these were unavailable as well, due to item nonresponse, I imputed respondents' ISEI scores applying chained imputation techniques (White et al., 2011).

in Germany have an ISEI score of  $\sim 42$  (results not shown here); a parental SES associated with, for example, many occupations in the service sector.<sup>10</sup> Of course, this does not mean that most immigrant parents hold such an occupation. Instead, their ISEI scores vary substantially, as the standard deviation of  $\sim 19$  suggests. As we will learn in a moment, this variation in SES will play an important role for our question of interest.

### Separate lives?

Having learnt about the diverse ethnic and social backgrounds of young immigrants in Germany, we are now ready to inspect their levels of social integration, more specifically the extent to which they form friendships with native peers.<sup>11</sup> Being asked to share information about their current five best friends, respondents reported the ethnic background of each one of them. Based on this information I constructed a measure that informs about the number of natives among these five best friends.

Figure 1.1 shows that on average, 2.4 out of an immigrant's five best friends are native. At first sight, this value does not seem worrisome; on average, half of young immigrants' friends are native. However, at closer inspection several questions arise.

First, is having 50% native friends really an indication of a successful social integration? To see this, we should compare the observed friendship compositions to those that would result if having an immigrant background simply did not matter in friendship formation (i.e. friendship choices happening at ran-

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<sup>10</sup>As a point of reference, young natives in Germany have, on average, an ISEI score of  $\sim 52$ ; a value associated with, for example, higher occupations in the sales sector.

<sup>11</sup>There are numerous ways to think of and to measure the social dimension of immigrants' integration in their host country. One stricter indicator—often regarded as the final step in immigrants' social integration—would be intermarriage rates (Kalter, 2008). In contrast, a weaker indicator would be, for example, rates of club membership which provide recurring contact with natives. By focusing on friendship patterns with natives I choose a middle ground between these two extremes: Friendships are neither at risk of reflecting only artificial contact (as would be the case with measures relying on club memberships etc.) nor do they occur only among the most integrated groups (as would be the case with measures relying on intermarriage and romantic relations). At the same time friendships constitute a central part of adolescents' social lives, as the vast literature on processes of peer influence demonstrates (for an overview, see DiMaggio and Garip, 2012).



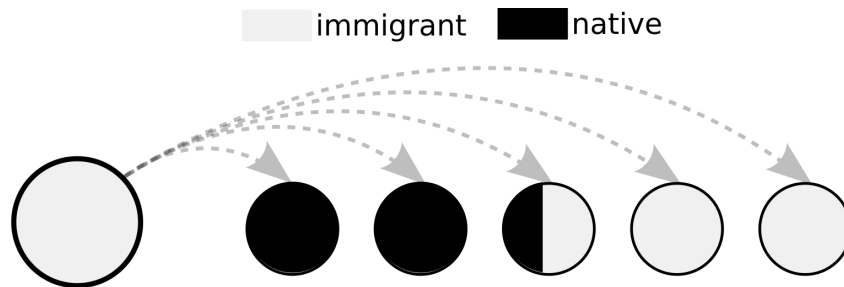


Figure 1.1: Native proportion among young immigrants' five best friends (overall mean, weighted)

dom). Assuming that adolescents tend to befriend peers of a similar age, the relative group sizes from our data—being a representative sample of adolescents in Germany—can provide an answer: As reported in Table 1.1, 72% of all adolescents around the age of 14 in Germany are native. Consequentially, if friendship choices happened at random this would result in an average native proportion of 72% among immigrants' friends. Friendship compositions below this threshold are therefore indicative of deficient social integration. From this perspective, Figure 1.1 suggests that, on average, young immigrants' friendships do not reach this integration threshold.

A second question arising is whether the average may only mask that there are many young immigrants with more extreme friendship compositions. And indeed, the distribution of friendship compositions (not shown here) reveals that on the one hand one third of all young immigrants in Germany reaches the integration threshold. On the other hand, however, 20% of all immigrants have no native friends at all. In other words, one out of five young immigrants in Germany lives a *separate life*, in a friendship network without any majority member.

Why are one third of the immigrants socially well integrated but the rest is not? Why does one out of five immigrants have no native friends whatsoever? Do the latter deliberately choose to stay among themselves? Or do they simply not come into contact with any native in their everyday lives? In order to find out we need to inspect young immigrants' social environments. A very natural and promising factor to start with is their place of residence.

## Residential barriers and bridges?

Asking young immigrants directly about the presence of natives in their neighborhoods, their answers vary widely (see distribution across Likert-scale in grey-lined bars in Figure 1.2). Some respondents report that “none/very few” natives are present (i.e., 5%), whereas others state that “(almost) all” of their neighbors are native (i.e., 23%). Most young immigrants, however, report to live in moderately mixed neighborhoods entailing “a lot” of native neighbors. But do these reports really align with the actual neighborhood compositions? Are adolescents really capable of correctly evaluating their local environment? For example, individual perceptions of neighborhoods may easily be confounded by everyday interaction patterns; those with native friends perceive more natives in their environment than those without native friends, even though both may live in similar neighborhoods. To examine residential barriers and bridges we therefore need a more objective measure, indicating the *actual* instead of the *perceived* proportion of natives in young immigrants’ neighborhoods.

The CILS4EU data do not provide such objective information on adolescents’ neighborhoods. I therefore rely on an additional, external data source, namely the private geomarketing company *Microm*.<sup>12</sup> Among other indicators, Microm offers information on the native proportion of local neighborhoods on small spatial scales. Note, however, that this neighborhood measure is based on the ethnic origin of residents’ *names* instead of their country of birth, as usually would be the case.<sup>13</sup> Nevertheless, I add this information to each respondent in the CILS4EU sample, providing a more objective measure on the native proportion of their local neighborhoods. On average, the Microm neighborhoods merged to the CILS4EU data contain  $\sim 700$  households.

And indeed, the more objective measure provides a different image: neighborhoods with less than 50% natives are virtually absent in Germany (see dark-

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<sup>12</sup>Administrative neighborhood data are no alternative, either: country-wide context information only exists on larger spatial scales such as municipalities (see, for example, Chapter 4). More fine-grained information is instead always regionally specific (see, for example, Chapter 2).

<sup>13</sup>In Chapter 2, I will investigate whether this may lead to systematic measurement bias.

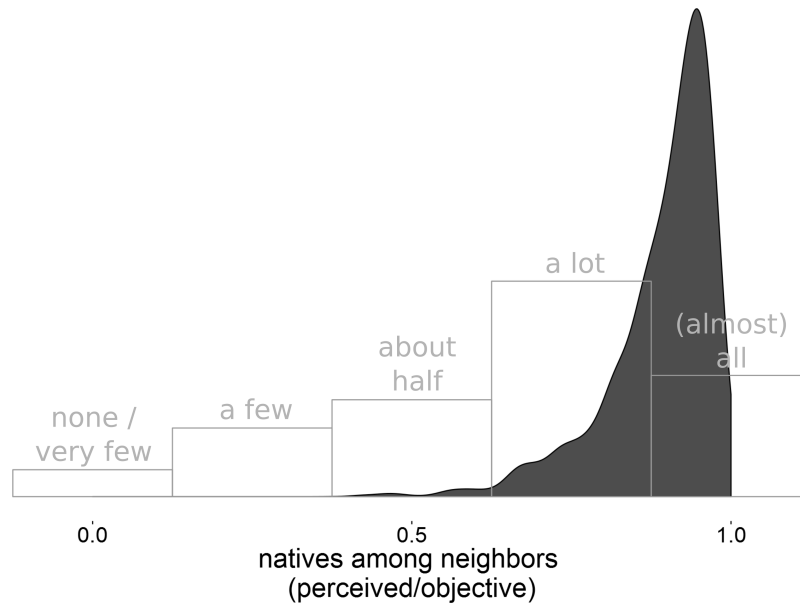


Figure 1.2: Native proportion among young immigrants' neighbors (weighted distributions). Grey-lined bars: subjective self-reports; dark-shaded area: objective measurement from Microm data

shaded area in Figure 1.2).<sup>14</sup> Instead, most young immigrants live in neighborhoods where at least three out of four neighbors are native. These numbers suggest that mutual, initial contact with natives should be generally possible for all young immigrants in Germany, in other words, that strong residential barriers may not even exist.

And yet, the data tell a different story. To see this, I divide the sample of young immigrants into four equally-sized groups conditional on the (objective) native proportion among their neighbors (i.e., into neighborhood quartiles). Figure 1.3 displays how respondents in each neighborhood quartile fare in terms of their friendships with natives. In the lowest quartile—containing all young immigrants living in neighborhoods with up to 79% natives—only a very small share reaches the integration threshold ( $\sim 8\%$  of all young immigrants, see light grey area of

<sup>14</sup>Admittedly, the actual percentage of native interaction partners may be somewhat lower than what the neighborhood compositions suggest, as the latter includes residents of all ages. Lasting social exchange, however, usually unfolds among peers around the same age and there are relatively lower native proportions within the younger cohorts.

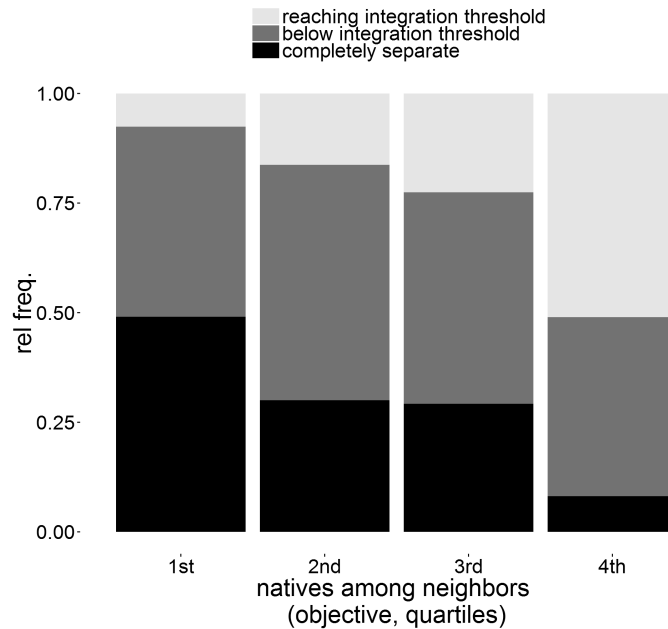


Figure 1.3: Young immigrants’ social integration across neighborhood compositions (objective, weighted quartiles)

first quartile). Moreover, half of all young immigrants living in neighborhoods with the lowest native proportions have no native friends, at all (see black area of first quartile). In the upper quartile—containing all young immigrants living in neighborhoods with native shares beyond 92%—the opposite holds: whereas only about 8% have no native friends, whatsoever, half of all young immigrants in the upper quartile reach the integration threshold.

Taken together, the findings from Figures 1.2 and 1.3 thus carry two important messages. First, immigrant-dominated neighborhoods (i.e., areas well below 50% natives) may not be a widespread issue in Germany. Nevertheless, already modestly mixed neighborhoods serve as strong residential barriers; living there implies a substantial lack of native friends. Second, whereas it is rather the rule for young immigrants in Germany to live among many native neighbors, native-dominated neighborhoods do not necessarily serve as residential bridges. Every second young immigrant living among mostly native neighbors still remains below the integration threshold concerning native friends. In a nutshell, *residential barriers operate universally, whereas residential bridges operate conditionally*.

### **The puzzle: a curious link between neighborhood and friendship compositions**

It seems as if young immigrants differed in how much they depend on their neighborhoods when making friends. Why do some immigrants remain separate despite being surrounded by native neighbors, while others use this environment as a residential bridge?

One explanation suggesting itself is varying degrees of local mobility, leading to friendship choices that depend more or less strongly on a person's local environment. Greater resources in terms of economic or social capital usually promise greater independence—also in spatial terms. This brings us back to the beginning of our empirical analysis and to immigrants' SES. Previous work argued that immigrants of higher SES should have the resources to be more mobile and thus to maintain more friendships outside the neighborhood than low-SES immigrants can (van der Laan Bouma-Doff 2007, Schlueter 2012). The idea is also known under the term *residual neighborhood*, implying that “neighboring is an alternative form of socializing for people who do not have access to broader networks” (Logan and Spitze, 1994, p.457).

Let's see if immigrants' SES really defines whether or not their friendship compositions align with the ethnic compositions of their neighborhoods. Figure 1.4 shows the relation between the actual native proportions among young immigrants' neighbors and their friends. The distribution of cases across the value space (each indicated by light-grey circles) confirms our previous impression: residential barriers seem to exist for all young immigrants in Germany alike, no matter who they are. In contrast, residential bridges exist as well, though they can be much more supportive for some than for others.

Moreover, the linear trend lines now clearly suggest who *some* and who the *others* are: high-SES immigrants (i.e., those with an ISEI score in the upper decile) profit greatly from residential bridges, low-SES immigrants (i.e., ISEI score in the lowest decile) do so far less. This leads to the interesting—somewhat counterintuitive—finding that for high-SES immigrants their place of residence plays a more important role in their social integration than it does for low-SES immigrants; a stark contrast to what the residual neighborhood argument sug-

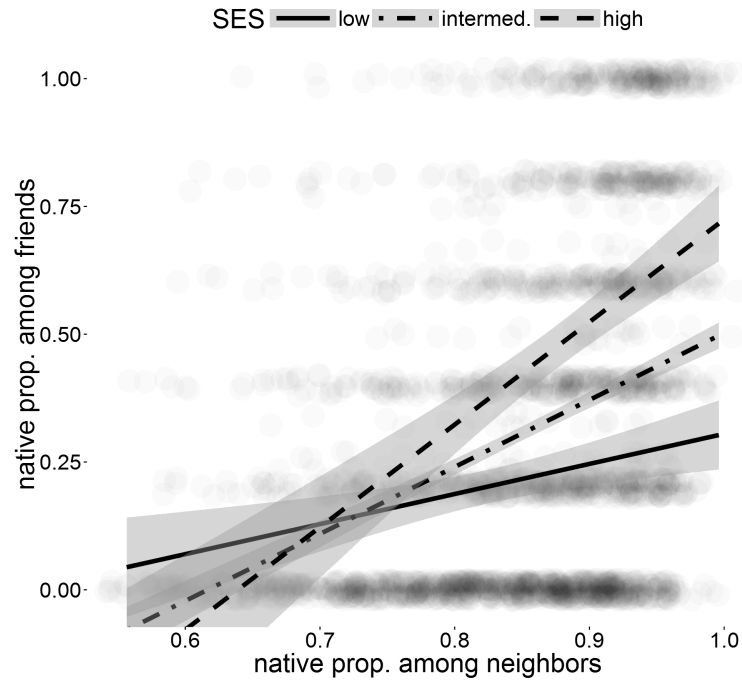


Figure 1.4: The relation between the native proportions among young immigrants' neighbors and their friends. SES-specific linear trends (unweighted).

gested. After all, residential barriers and bridges may not be SES-specific because of differences in local mobility. The obvious question arising then is: why? What exactly impedes young immigrants of low-SES from using their native neighbors as residential bridges? I therefore refine the overarching research question as follows:

Why are residential barriers to the social integration of young immigrants in Germany universal, whereas residential bridges primarily emerge for high-SES immigrants only?

## 1.3 Plan of attack

### A spatially informed framework of friendship formation

This book wants to provide answers. The following chapters will—in a stepwise manner—ultimately lead to an explanation for the curious, SES-specific link between neighborhood and friendship compositions for young immigrants in Germany.

My starting point is a preliminary best guess about the functioning of residential barriers and bridges—a spatially informed framework of friendship formation. The framework provides a condensed image of the current knowledge about residential barriers and bridges to immigrants’ social integration (cf. Mouw and Entwisle, 2006; Vermeij et al., 2009; Wimmer and Lewis, 2010). As such, it not only provides preliminary working assumptions to get started. The framework will serve as a point of reference throughout the book: First, I will test it empirically and refine it accordingly in a stepwise manner. Subsequently, its revised version will provide the foundation to solve the central empirical puzzle of this book; SES-specific residential bridges.

Figure 1.5 outlines the preliminary framework graphically. It shows how immigrants living in ethnically segregated neighborhoods are thought to end up with ethnically homogenous friendships. The framework’s main properties are simple: It models the process of friendship formation as the aggregated result of individual actor choices. Two subsequent actor decisions are thereby important: the choice of a meeting context and the choice of their friends (see large arrow on the left). Both decisions are determined by two factors: the choice *restrictions and opportunities* actors face (dark grey shaded area) and their *preferences* for specific choice alternatives (light grey shaded area).

In its current, preliminary form the framework suggests three ways how residential barriers or bridges to immigrants’ social integration emerge, each indicated by a thin arrow. The most obvious way is clearly the indirect, opportunity-based path via meeting context compositions (center arrow): In a nutshell, neighborhoods (partly) determine meeting context compositions. Ethnically segregated neighborhoods thus leave little opportunity to meet and befriend outgroup mem-

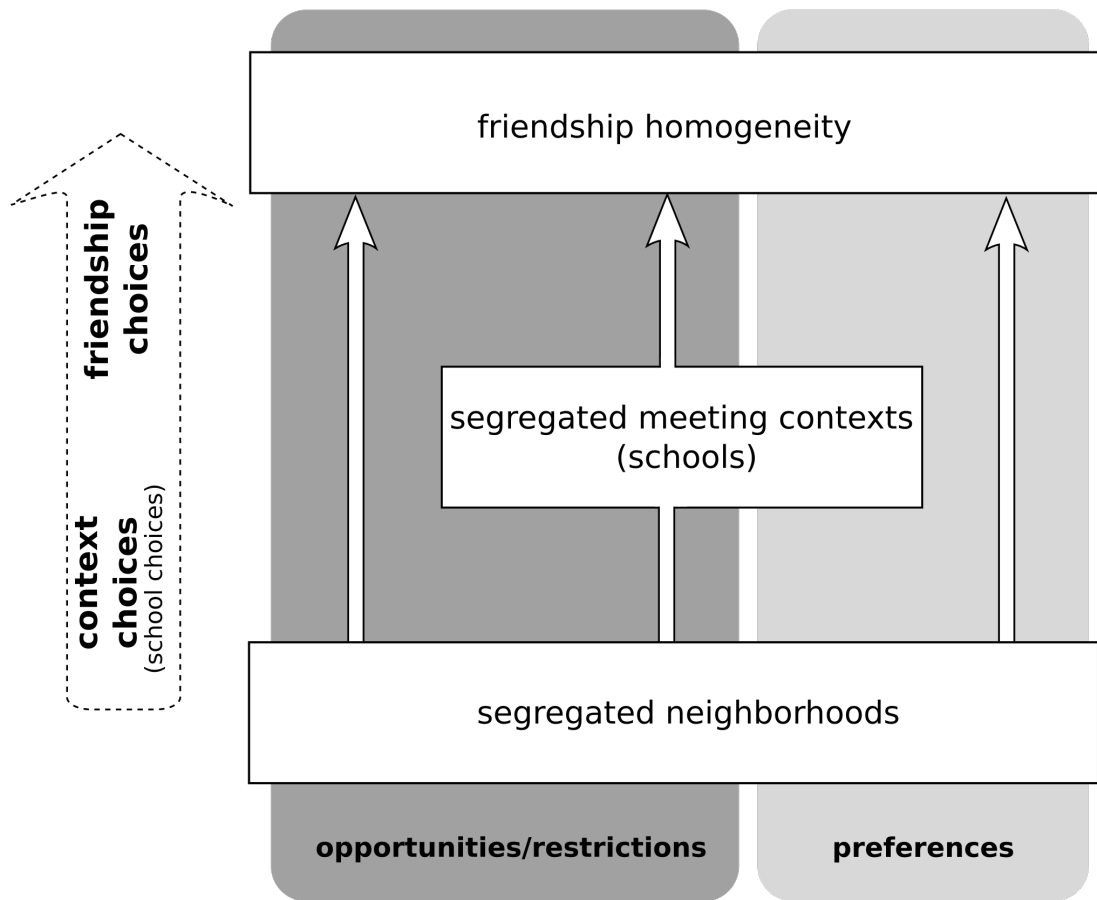


Figure 1.5: The spatially informed framework of friendship formation (preliminary)



bers (i.e., residential barriers). Living among natives implies the opposite (i.e., residential bridges). In his *Primitive Theory of Social Structure* Blau emphasized this fact, arguing that social interaction patterns are usually a direct result of structural compositions (Blau, 1977).

Beside this rather obvious social-structural path, however, previous research suggests further ways how neighborhood compositions shape friendship compositions more directly. These more direct ways can be both opportunity- (left arrow) and preference-driven (right arrow). Later in this book, I will lay out these additional mechanisms in greater detail (see Chapter 3). For now it suffices to know the framework's general structure.

### Short outline of the book

Based on these first insights, we are now prepared to make a quick tour through the book.

As a first step, *Chapter 2* investigates whether the data that the outlined explanandum relies on are adequate to do what they are supposed to: to provide an unbiased image of the relation between young immigrants' neighborhoods and their friendships. In other words, it tests whether the framework's starting and end points—segregated neighborhoods and friendship homogeneity (cf. Figure 1.5)—are really measured in a comparable way. To do so, the chapter tests a central methodological assumption that the Microm neighborhood data rely on; that ethnic compositions of neighborhoods can be accurately inferred from the names of their residents. As such, the chapter raises awareness about the potential and the restrictions of the data used throughout the book. The chapter's analyses rely on CILS4EU data as well as on neighborhood compositional data from local statistics of two German cities.

*Chapter 3* puts the preliminary, spatially informed framework of friendship formation to a first empirical test. Its rationale is as follows: neighborhoods affect friendship choices via the composition of meeting contexts (cf. Figure 1.5, center arrow). But is that all? Or does the neighborhood determine friendships also in other ways? In other words, the chapter tests the existence of the two direct causal pathways of residential barriers and bridges (cf. Figure 1.5, left and right arrows)

while controlling for the indirect pathway via meeting context compositions (i.e. center arrow). As such, the chapter revises the spatially informed framework of friendship formation, indicating which mechanisms will be important later on when trying to explain the central puzzle of the book. All analyses in Chapter 3 rely on CILS4EU and Microm data.<sup>15</sup>

*Chapter 4* turns to the mechanism that the previous chapter implicitly took for granted. Focusing on adolescents' most important meeting context—their schools—the chapter investigates why the link between neighborhood and meeting context/school compositions may not be so straightforward, after all. The chapter's focus is therefore on the first decision of the framework; actors' context/school choices. It investigates the causes why ethnic segregation usually exceeds residential patterns. As such, it helps to once more revise the spatially informed framework of friendship formation and provides clear hints at an explanation for the central puzzle of the book.

*Chapter 5* then tackles this puzzle, SES-specific effects of the neighborhood on young immigrants' friendships with natives. Based on the by then re-revised theoretical framework, this chapter finally solves the question why in the social integration of low-SES immigrants it seems not to matter much where they live, whereas for high-SES immigrants it does. As we will see, it will thereby be important to take into account the complete, revised framework. Both decisions within the process of friendship formation—immigrants' context and friendship choices—play a decisive role.

*Chapter 6* closes with a summary of the central findings of the book. It thereby summarizes each chapter separately and lays out how the findings relate to the overarching question of this book. Finally, the chapter proposes—in light of the book's shortcomings—the most pressing avenues that future research should take.

Depending on the reader's interest and the time available, there are different ways to read this book. One shortcut through the book allowing the reader to catch its most fundamental message would be to continue with the conclusions of Chapters 3 and 4, followed by reading Chapters 5 and 6 entirely. Alternatively,

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<sup>15</sup>Realizing that we shared an interest in the same question three colleagues from the Dutch CILS4EU team and I decided to join forces. As a consequence, I did not only profit from an inspiring exchange: the empirical analyses in this chapter also turned out to be based on adolescents both in Germany and in the Netherlands.

those interested in specific pieces of the puzzle can, without greater problems, also read each chapter separately. For example, readers interested in determinants of adolescent friendship formation may refer to Chapter 3, whereas those interested in the causes of ethnic segregation in secondary schooling can focus on Chapter 4. Chapter 2 instead provides methodological food for thought. However, for a comprehensive perspective on the functioning of residential barriers and bridges to young immigrants' social integration in Germany, there is, unfortunately, only one recommendation: to read this book from beginning to end.



## Chapter 2

Testing the data.

Are neighborhood and friendship  
measures comparable?\*

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\*A different version of this chapter, co-authored by Jörg Dollmann, is currently under review by a peer-reviewed journal. To guarantee consistency across chapters, I have rewritten the chapter from a first-person perspective and reformulated various sections.

### **Abstract**

This chapter examines whether the available neighborhood data for Germany is appropriate to investigate residential barriers and bridges. More specifically, given that the applied Microm data derives neighborhood compositions from the ethnic origin of residents' names, I test if such name-based ethnicity classification is subject to systematic bias. Drawing upon previous research, I assert that ethnic groups differ as to how well they are identifiable via name-based classification. This implies that a name-based classification bias exists and that its size differs between neighborhoods. Results concerning the German case indicate a tendency to overestimate the proportions of natives in immigrant-dominated neighborhoods and slightly underestimate them in native-dominated neighborhoods. The chapter closes with a discussion of potential strategies to cope with the name-based classification bias. One such solution is applied to the neighborhood data used in this book.

## 2.1 Introduction

A central prerequisite for any empirical research on migration and integration issues is a clear-cut distinction between people with an immigrant background and those without one. Name-based classification—an approach to identify the immigrant status of persons via the ethnicity of their personal names—is in this regard becoming increasingly important and applicable for various purposes (c.f. for an overview: Mateos, 2007). Among the more frequent applications are measures of context composition: a growing interest in contextual characteristics as determinants of social action (for a recent overview on neighborhood effects, see Sharkey and Faber, 2014) led to a rise in demand for compositional information on very fine-grained spatial scales (i.e., small-level neighborhood data). Given that measures of ethnic composition on these lower spatial scales might often not be readily available—for example, in the case of Germany—proxies derived from the ethnic origin of inhabitants’ personal names have become popular alternatives (Drever, 2004; Sager, 2012). As laid out in the previous chapter, this book relies on such name-based proxies when identifying residential barriers and bridges (see, for example, Figure 1.4).

Despite the development of different name-based classification techniques in recent years (Humpert and Schneiderheinze, 2000; Mateos, 2007; Schnell et al., 2013*a,b*), they remain estimations and thus are always at risk of being subject to systematic bias. When comparing name-based classifications to those resulting from persons’ reported countries of birth—as a more objective measure, misspecifications become apparent: false negative (i.e., immigrants wrongly classified as natives) and false positive (i.e., natives wrongly classified as immigrants) classifications are thereby both a matter of concern. In this chapter, I investigate the exact nature of the potential bias that name-based approaches can exert in the construction of measures of context composition. Doing so informs about whether the observed link between young immigrants’ neighborhood and friendship compositions is potentially due to measurement bias.<sup>1</sup>

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<sup>1</sup>It seems reasonable to assume that adolescents know about whether or not a friend was born abroad. Similarly, they may know about their friends’ parents. Less clear, however, is whether adolescents know if, for example, *Kowalski* is a name of German origin. Hence, there is ample reason to believe that respondents’ reports about their friends’ ethnicity rather align

The point of departure is an argument already established by previous research (Schnell et al., 2014). Whereas the names originating from some ethnic groups are clearly distinct from those of the native population (e.g., Turkish versus German in Germany), this dividing line can be harder to trace for other ethnic groups (e.g., Polish versus German), potentially even more so in subsequent immigrant generations. I demonstrate that indeed the probability of true or false classifications of immigrants in Germany depends on the specific ethnic origin of a person as well as on his/her generational status. The analyses rely again on the CILS4EU data (Kalter et al., 2016). As a reminder, it is a representative sample of ninth grade adolescents for whom I have both information on their actual immigrant background (i.e., their own/parents' country of birth) and their full names. This allows me to apply name-based classification and directly assess its validity.

Subsequently, and as the main contribution of this chapter, I demonstrate the potential consequences of such ethnic differences in classification accuracy for the construction of measures of context composition. The argument accounts for the fact that some ethnic groups are more likely than others to live in immigrant-dominated neighborhoods. Areas with very low proportions of natives attract different ethnic immigrant groups than areas with a higher presence of natives, thus yielding locally specific classification accuracies. To substantiate the argument, I simulate the process of name-based classification in two German cities with sizable immigrant populations and compare the resulting proportions of natives in the neighborhoods to the *actual* neighborhood compositions as reported by local statistics. Doing so allows me to infer that name-based approaches tend to overestimate the proportions of natives in immigrant-dominated areas, while underestimating them in native-dominated German neighborhoods. Proceeding in this manner, I will add to the present state of research by providing an encompassing view not only of the causes, but also especially of potentially problematic consequences of misspecification in name-based classifications for the construction of measures of context composition.

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with more objective measures like their friends' (parents') country of birth than with their names' ethnic origin.



The structure of the remainder of the chapter is as follows: Section 2.2 provides a general overview of name-based classification approaches. It discusses the causes of possible misclassification as well as its consequences for the construction of measures of composition. Section 2.3 lays out the analytical approach taken, before introduction of the data and variables used in the analyses in section 2.4. The results are presented in section 2.5 and are summarized in section 2.6, together with a discussion of limitations and provision of practical guidance on how to cope with potential bias in name-based measures of composition in general and, more specifically, in this book.

## 2.2 Theory

### **A brief review of name-based classification approaches**

Several recently developed name-based classification techniques seek to determine a person's ethnicity based on information about the ethnic origin of his or her name (cf. for an overview: Mateos, 2007). Whereas these techniques may differ in the number of targeted ethnic groups listed, in the size of the respective target groups, and in the number of unique fore-/surnames used in the reference lists, Mateos' overview of 13 studies reveals a general communality of all approaches. They classify persons in a target population as having a specific ethnic origin according to the ethnic origin of their names as reported in more or less exhaustive name reference lists. This so-called name-based classification is then validated by using a more objective measure of ethnicity, like self-reported ethnicity, country of birth, or nationality (Mateos, 2007, p. 249).

In contrast to such name-based classification procedures, Schnell et al. (2013*a,b*) recently proposed another technique, which does not rely on complete names, but rather on substrings of consecutive characters in a name, so called n-grams. These n-grams are extracted from the target names, which themselves are then Bayes-classified according to the relative frequency of the n-grams within predefined lists of names from specific ethnic origins (Schnell et al., 2014). Compared to previous approaches, the advantage of this method is that it is less prone to misspellings

and variations of names in both sources given that it does not rely on complete names in either the target names or the reference list.

Despite these recent developments in the realm of approaches to ethnic classification, the prevailing and most popular method applied in the German context remains that based on complete names, the classification approach developed by Humpert and Schneiderheinze (from here on *HS approach*). While the studies reviewed by Mateos (2007) aimed at separating one or just a few ethnic groups from the rest of the underlying population, the HS approach uses much more comprehensive dictionaries comprising a large number of combinations of forenames and surnames and the respective probability that each empirically observed combination will have a specific ethnic origin. Using in total over 2,000,000 surnames and 600,000 forenames results in over 21,000,000 existing combinations of forenames and surnames together with their regional classification (for the general procedure cf. Humpert and Schneiderheinze (2000); for recent developments cf. Humpert and Schneiderheinze (2015)). Due to this extensive database with name-group relationships, the HS approach has become the standard approach for name-based classifications in Germany (c.f. Ersanilli and Koopmans, 2013; Kogan, 2012; Mammey and Sattig, 2002; Rother, 2005; Schenk et al., 2006). In the following, I will therefore concentrate exclusively on this approach.

### Causes for misclassification

The aim of all of the above-mentioned approaches is to identify correctly actual members of ethnic minorities as such (*true positives*) and actual members of the majority population as natives (*true negatives*). However, like any estimation-based procedure these approaches do not yield results that align perfectly with empirical reality, which is why some natives will be wrongly coded as immigrants (*false positives*), while some persons who actually have an ethnic minority background will be wrongly identified as natives (*false negatives*).<sup>2</sup>

Turning first to the reasons for misclassifying actual immigrants as natives (false negatives), things are rather clear. Whereas the causes may be manifold,

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<sup>2</sup>Approaches that classify specific immigrant groups and differentiate between not only natives and ethnic minorities but between different ethnic minorities additionally face the problems of coding minorities from one ethnic origin to another ethnic origin.

almost all of them relate to common, assimilation-driven mechanisms (cf. for an elaborated overview of the following arguments, see Schnell et al., 2014, p.234). For example, intermarriage between (usually better-assimilated) members of the minority and the majority population may lead to the subsequent adoption of the majority spouse's surname by the minority member. This may be one reason for the misclassification of ethnic minority members as natives. Given that females are still more likely than males to adopt their spouse's name, intermarriage will lead to misclassifications especially among female minority members and their binational children (Waters, 1989). Secondly, minority members' names may also be adapted in the course of their naturalization process. For example, given that an immigrant's original forename is *Piotr*, he might adjust it to its German equivalent *Peter* upon naturalization. A third assimilation-driven reason for misspecifications can be immigrant parents' naming of their children, influenced perhaps by their degree of assimilation. Better-assimilated ethnic minorities are usually more likely to provide their children with first names that are more similar to the first names common among the majority population (Becker, 2009; Gerhards and Hans, 2008, 2009), thus leading to greater ambiguity about the child's actual immigrant status. Finally, beside reasons related to immigrants' degree of assimilation, their ethnicity may equally play a role in determining how successfully an approach can identify them as such. If minority members stem from regions with languages similar to that of the receiving country, misclassifications will more likely occur. This also holds true for minority groups from regions where names are common that are similar to those among the majority population, for example due to historical idiosyncrasies linking the sending and the receiving country. In the case of immigrants in Germany, one example would be former German emigrants to South America whose descendants return to Germany, but also Ethnic Germans from Eastern Europe who migrate to Germany.

Turning to the erroneous classification of actual natives as having an immigrant background (i.e., false positives), less is known about the causes. This is perhaps because name-based sampling approaches regard false positives as less problematic, given that they do not lead to an omission of immigrant subsamples but only to increased survey costs due to inflated sample sizes (Schnell et al., 2014). However, both false negatives and false positives may equally lead to sub-

stantial bias in the construction of context measures. From this perspective, it seems necessary to inspect also potential causes of the emergence of false positives.

One reason for the misclassification of members of the majority population as members of an ethnic minority is that their families may look back on an ancient, long-forgotten immigration history. This ancient immigrant background may often still manifest itself in the family names, but can no longer be assessed based on more objective measures such as nationality, (self-reported) ethnic identity and/or (grand-)parents' country of birth. Furthermore, the practice among the majority population of providing their children with unusual forenames with a foreign connotation may also lead to false positive classifications. Finally, intermarriage as outlined above may lead not only to false negative classifications, but also to false positives, especially if a domestic spouse adopts the name of the minority partner.

Given the arguments above, it becomes obvious that misclassifications will not occur at random, but are to be expected, especially among specific demographic groups. Persons categorized as immigrants according to name-based classifications will—besides some wrongly coded natives—mainly comprise actual immigrants, with immigrants who are less integrated (i.e., migrated more recently) showing lower error rates. In contrast, persons classified as natives according to name-based approaches will largely comprise actual natives, but will also include immigrants from subsequent generations (i.e., 2<sup>nd</sup> generation and later). Furthermore, immigrants' ethnic background plays a decisive role when it comes to probabilities of correct specification. In the case of Germany, Ethnic Germans from the Former Soviet Union and Eastern Europe will show higher error rates than will culturally more distant ethnic groups, such as Turks.

### **Consequences of misclassification for measures of context composition**

Next, I want to explore the consequences of misspecification for the construction of measures of context composition derived from name-based classifications. I therefore turn to the following hypothetical example: imagine a city inhabited by natives and immigrants, both groups being ethnically homogeneous (later I will

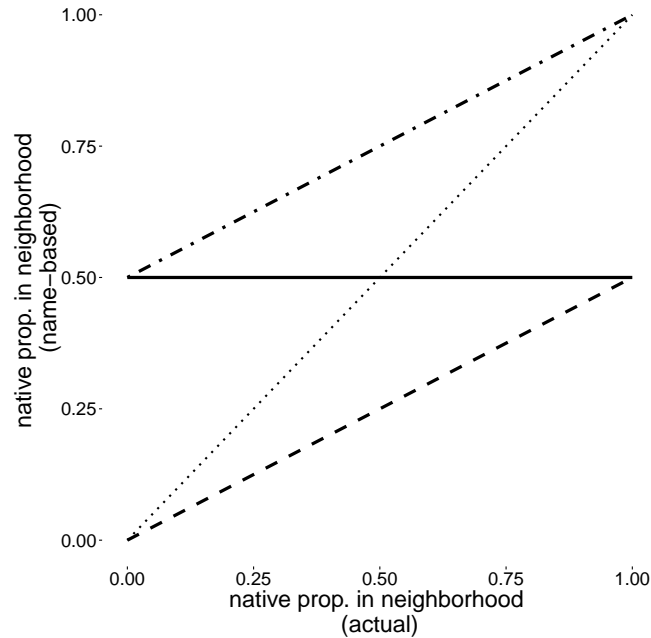


Figure 2.1: Hypothetical relations of actual proportions of natives and name-based proportions of natives in neighborhoods (with different error rates)

relax this assumption and introduce variation in ethnic backgrounds). The city consists of an arbitrary number of neighborhoods whose actual proportions of natives vary between 0 and 100%. Further, assume that I want to estimate each neighborhood's composition by means of name-based classification. Depending on the error rates with which I falsely identify actual natives as immigrants (i.e., the probability of false positives) and actual immigrants as natives (i.e., the probability of false negatives), the estimation might either closely match the actual composition of the neighborhoods or differ from it substantially. Figure 2.1 visualizes the relation between actual (x-axis) and name-based (y-axis) neighborhood compositions based on four scenarios with different error rates (as indicated by the four different lines).

Starting with the two extreme cases, first consider a scenario where both the probability of false positives as well as that of false negatives is zero. Of course, this means that there is no error in the classification whatsoever, such that all immigrants and all natives in all neighborhoods are correctly classified. Actual and name-based neighborhood compositions therefore align perfectly along the

bisecting line (dotted line). The other extreme would be a scenario where name-based classifications are completely uninformed, categorization thus being purely random. In this scenario, I would therefore assume that the probability of classifying both natives and immigrants correctly is 50%. What consequences would this have for the name-based neighborhood compositions? Again, the answer is rather simple. Looking at a neighborhood exclusively inhabited by immigrants, 50% of them would be classified correctly, whereas the other half would be misspecified as natives. Similarly, in an all-native neighborhood, 50% of all residents would be correctly identified as natives, while the other half would be misspecified as immigrants. As this random classification also holds true for all mixed neighborhoods, I consequently observe a null relation between actual and name-based neighborhood compositions, with all possible actual neighborhood compositions having an estimated immigrant proportion of 50% (solid line). Needless to say, a context compositional proxy based on name-based classification with these error rates would be worthless.

Of course, natives and immigrants are not necessarily equally well identifiable. In a third scenario, I therefore relax this assumption by defining that natives are always correctly classified (i.e., the probability of false positives is zero), whereas immigrants are classified purely at random (i.e., the probability of false negatives is 50%). An all-native neighborhood therefore would be correctly identified as such. The composition of an all-immigrant neighborhood, however, would be clearly misspecified, given that 50% of its inhabitants would be falsely identified as being native. The same holds true for mixed neighborhoods: considering again an evenly mixed neighborhood (i.e., 50% actual native proportion) all natives would be correctly specified as natives, while half of the immigrants would be mistakenly identified as natives, thus leading to an overestimation of the native proportion by 25 percentage points. To summarize, in this third scenario the higher the actual proportion of natives in a neighborhood, the better the estimation of the native proportion based on name-based classification (dashed-dotted line). Respectively, the exact opposite would be true in a scenario where immigrants are always correctly classified (i.e., the probability of false positives is zero) and natives are classified purely at random (i.e., the probability of false negatives is 50%). The relation would then be as follows: the higher the actual

proportion of natives in a neighborhood, the stronger the overestimation of the immigrant proportion based on name-based classification (dashed line). In contrast to the second hypothetical scenario both scenarios 3 and 4 would create estimations of context composition that clearly correlate with the actual context compositions. However, analyses based on such rather crude estimations could still lead to serious bias. Returning to the general aim of this book, imagine we investigate to what extent the native proportion among actors' close friendships reflects the native composition in their local environments. Let's assume that in reality the share of natives among actors' friends is equal to that in their actual neighborhood compositions. An analysis based on an estimated context composition as outlined in the fourth scenario (dashed-dotted line) would come to different conclusions. Based on the biased measure, I would infer that actors located in all-native contexts have friendships whose compositions closely match those of their local surroundings, whereas actors residing in immigrant-dominated areas maintain more friendships with natives than natives are relatively present in their environment. This could lead, for example, to erroneous conclusions about ethnically specific friendship preferences of the latter. From this perspective, it seems important to know if and how exactly name-based context compositions deviate from actual compositions.

Given that the four scenarios are based on extreme error rates (either zero or completely at random), they not only inform about the relation between actual and estimated compositions of context in general, but at the same time also define the outer boundaries of what relation to expect in any empirical situation. Empirically, I can expect neither that error rates be zero, nor that a name-based classification will yield purely random results. The probabilities of false positives and false negatives will therefore range somewhere in between 0% and 50%. Concerning the relation depicted in Figure 2.1 I would thus expect any empirical estimation of compositions of context that relies on name-based classification to result in a relation that is located somewhere in the area between the dashed and the dashed-dotted line.

What remains unclear and case-dependent is the specific functional form emerging between actual and name-based compositions of context. So far, I have assumed that both natives and immigrants are ethnically homogeneous groups,

all group members thus having the same error rates. Empirically, however, this is almost never the case. Turning to the example of German cities, not one homogeneous immigrant group, but rather different ethnic groups, the largest of them being of Turkish, Polish, Russian, and Italian backgrounds, inhabit neighborhoods. As argued in section 2.2, however, error rates are expected to be ethnically specific. It would therefore be oversimplifying matters to assume that the probability of false negatives is the same across all immigrants in German cities. Relaxing this assumption leads to new open questions that need to be specified in order to learn about how actual and estimated compositions of context relate: first, I need to specify the ethnic composition of immigrants in the hypothetical city. Then, I need to make explicit where the different ethnic groups live, that is, whether an all-immigrant neighborhood entails the same ethnic mix of immigrants as a neighborhood with only a modest share of immigrants. Assuming equal distribution, things would be rather simple: I could derive an overall probability of false negatives for all immigrants from an (ethnic-group-size) weighted average of the ethnically specific error rates and proceed as outlined above. For example, assuming that half of the actual immigrants in the hypothetical city are Turks, identifiable at an error rate of 5%, and the other half are Russian, at an error rate of 30%, the overall probability of false negatives would be 17.5% (i.e.,  $\frac{1}{2} \cdot .05 + \frac{1}{2} \cdot .3 = .175$ ). The immigrant proportion of an all-immigrant neighborhood in this city would therefore be underestimated at 82.5%.

However, in real-world situations it seems rather unlikely that an all-immigrant neighborhood would entail the same ethnic mix as a neighborhood with only a modest share of immigrants. It may well be that some ethnic groups—especially those of lower average social background—are more likely than other ethnic groups to live in immigrant-dominated neighborhoods. From this perspective, I would need to specify not only the overall ethnic composition of the hypothetical city, but also the ethnic mix within each of the city’s neighborhoods. When adding this further complexity to the hypothetical example, it becomes much less straightforward and more case-specific to derive the resulting relation between actual and name-based proportions of natives in the neighborhoods. Take the following example: Assume that local statistics provide information about the respective composition of two neighborhoods A and B. Each neighborhood



comprises 100 residents, among them 60 natives. The actual native proportion in neighborhoods A and B—as reported by local statistics—would thus be 60%. Neighborhood A further comprises 30 Turkish and 10 Polish immigrants, whereas neighborhood B accommodates 10 Turkish and 30 Polish immigrants. Finally, assume that the respective error rates to classify a native via a name-based approach turned out to be 20%, for a Turkish immigrant 10%, and for a Polish immigrant 40%. The simulated, name-based native proportion in neighborhood A would then be calculated as follows:  $(60 \cdot .8 + 30 \cdot .1 + 10 \cdot .4)/100 = 55\%$ . The name-based measure thus underestimates the actual native proportion in neighborhood A by five percentage points. For neighborhood B, the name-based native proportion would be  $(60 \cdot .8 + 10 \cdot .1 + 30 \cdot .4)/100 = 61\%$ , thus slightly overestimating the actual native neighborhood proportion by one percentage point. As the example clearly demonstrates, the extent and the direction of bias in measures of context due to name-based classification is not simply a question of the error rates but also of the actual ethnic mix present in the contexts to be measured. In order to learn what bias to expect in the case of contextual compositions in Germany, I make use of a small simulation setup that I will specify in section 2.5.

### Implications for the case of Germany

Due to the discussed causes of misclassification in name-based approaches I expect error rates vary to systematically across specific demographic groups: first-generation immigrants should show lower error rates than those from the second or subsequent generations. Further, error rates should be lower for immigrants of Turkish and Former Yugoslavian background than those of Polish or Russian background should.

These generational and ethnic differences in error rates have an important impact on the formation of measures of context composition that rely on name-based approaches, resulting in possible biases of measures of composition in Germany. Given that culturally more distant ethnic groups (i.e., Turkish) will be more likely than culturally closer groups (i.e., Polish and FSU immigrants) to reside in immigrant-dominated neighborhoods, the error rates in immigrant-dominated neighborhoods should be somewhat lower than in native-dominated neighbor-

hoods. The resulting type of bias, however, depends to a large degree on the exact neighborhood compositions and is thus mainly an empirical question.

### 2.3 Analytical approach

I proceed in two consecutive steps. First, I test whether the accuracy of a name-based classification varies across ethnic groups and immigrant generations, thereby relying on the example of adolescents in Germany. More specifically, I use the CILS4EU data introduced in Chapter 1; a representative sample of 14 year-old adolescents in Germany for whom I have detailed knowledge about their actual countries of origin (for more information, see section 2.4). I apply a name-based classification of their immigrant status according to the HS approach (see section 2.1), thereby identifying whether a respondent is an immigrant or not. The resulting binary name-based measure is then compared to a classification according to respondents' countries of origin (from here on 'actual immigrant status'). In order to disentangle ethnic- from generational-specific classification error, I apply multivariate logistic models, regressing whether a respondent is misidentified or not (with two separate models: one containing all respondents and one including actual immigrants only).

Next, I investigate the extent of bias that measures of context composition face when being constructed via name-based classification. Optimally, one would test the extent of bias by applying a name-based classification to all residents of an exemplary larger region or city and compare the resulting neighborhood compositions to the actual ones (i.e., those reported by local statistics). However, given that complete lists of the names of residents of an entire city or region are not available to me, I proceed differently and simulate the name-based classification process for two German cities with sizable immigrant populations for which information on their actual ethnic neighborhood composition is available from local statistics. I derive name-based neighborhood compositions for the two cities based on the ethnic- and generation-specific classification error rates attained in the first analytical step. Subsequently, I compare the simulated, name-based neighborhood measure to the actual native proportion in the neighborhood from local statistics. The extent of bias induced by name-based classification will

thereby depend both on the classification error rates as well as on the ethnic mix in the empirically observed neighborhoods.

## 2.4 Data and variables

### Data

Like Chapter 1, the analyses rely on the representative sample of 14-year-old adolescents in Germany, more specifically on the CILS4EU data (Kalter et al., 2016). Now it is time to get more familiar with the data: CILS4EU is a representative, school-based panel survey carried out in England, Germany, the Netherlands, and Sweden in 2010/11, with subsequent yearly follow-up waves. The survey applied a three-stage sampling approach (cf. CILS4EU, 2016). In the first stage, schools were chosen at random from nation-wide lists of all secondary schools in a country. Given that the main aim of CILS4EU is the investigation of the integration paths of adolescents with an immigrant background, schools with high immigrant proportions were thereby oversampled. In the second stage, two ninth grade classrooms within the selected schools were chosen at random. Finally, in the third stage, all students within the chosen classrooms became part of the gross sample. The first wave of CILS4EU yields a net sample size of 18,716 adolescents attending 958 classrooms in 480 schools. The data comprise information on various dimensions of young immigrants' integration; be it social (e.g. friendships, club membership), structural (e.g. grades, educational aspirations), emotional (e.g. national and ethnic identification), or cognitive-cultural (e.g. religiosity, attitudes and values). Beside these measures the data entails detailed information on adolescents' ethnic origin; in terms of their own, their parents', and their grandparents' birth countries. In addition, I was able to append information on adolescents' first and last names, which rendered the data optimal for my purposes. The following analyses rely on data from the first wave of the German part of CILS4EU, encompassing 5,013 students in 144 schools and 271 classrooms.

Beside the CILS4EU sample, I further make use of neighborhood compositional data from two German cities, Nuremberg and Berlin, when investigating the consequences of name-based approaches in the realm of measures of context

composition. Nuremberg has one of the largest immigrant proportions among all German cities and Berlin accommodates the largest number of immigrants in absolute terms, thus yielding sufficient variation in terms of native proportions in their neighborhoods. The data stem from local statistics and provide information on the respective ethnic composition of the two cities in the year 2015 (Amt für Stadtforschung und Statistik für Nürnberg und Fürth, 2015; Amt für Statistik Berlin-Brandenburg, 2015). The spatial scale is rather fine-grained, with an average neighborhood size in Berlin of  $\sim 8,000$  residents, and in Nuremberg of  $\sim 6,400$  residents (see Table A.3 in Appendix II). In both cities, the information on residents' ethnic background is based on a combination of their (parents') nationality and country of birth (Böckler and Schmitz-Veltin, 2013); in other words on their actual immigrant status. Several ethnic groups reported in the local statistics were combined into aggregate categories such that the final ethnic grouping closely matches that chosen in the CILS4EU data (see next subsection and Table A.3 in Appendix II).

### Variables

Respondents in the CILS4EU-survey are said to have an 'actual' immigrant status if they themselves or at least one of their parents was born outside of Germany. Otherwise I define them as being natives. In doing so, I classify immigrants from the third generation as natives. Further, given that the names of all respondents were readily available, the immigrant status was additionally defined according to the name-based HS approach. Taken together, the information on respondents' actual immigrant status and their name-based immigrant status identifies those respondents misclassified by the name-based approach (i.e., false negatives and false positives). The dummy variable *error* contains this information and serves as the dependent variable in the logistic regressions.

Two variables enter the logistic regressions as independent variables: respondents' *ethnic background* and their *immigrant generation*. The former variable, ethnic background, is derived based on respondents' and their parents' reported countries of origin (for more information, see Dollmann et al., 2014). The ethnic background variable was conflated to seven categories, among them the five

Table 2.1: Name-based classification of German CILS4EU sample (wave 1)

	Rel.freq. (in %)	Incorrect (in %)	N (students)
Native	58.1	9.2	2,904
Immigrant	41.9	16.8	2,092
Total	100.0	13.6	4,996

*Source:* CILS4EU, w1, v1.2.0, unweighted

largest ethnic groups—Turkish, Former Soviet Union (FSU), Polish, Former Yugoslavian (FYR)—as well as two residual categories combining the remaining smaller groups (i.e., other Western and other Non-Western). The second independent variable, immigrant generation, distinguishes finally whether a respondent is a native, a second-generation immigrant (i.e., born in Germany, at least one parent born abroad), or a first-generation immigrant (i.e., born abroad him-/herself) (c.f. Dollmann et al., 2014). All analyses were carried out in R (v.3.2.3).

## 2.5 Results

### Differential accuracies across ethnic groups and generations

First, I take a descriptive look at how the HS approach classified the German CILS4EU sample into respondents with or without an immigrant background. Table 2.1 shows that  $\sim 42\%$  of the students were classified as immigrants. Based on their reported countries of origin, I can ascertain whether the name-based approach classified them correctly. Almost 17% of the respondents classified as immigrants are actually natives. Among those classified as natives, the percentage of incorrectly classified respondents ranges lower at  $\sim 9\%$ . This yields an overall error rate of  $\sim 14\%$ . In the following, I will test whether such error is especially prevalent among specific demographic groups, as the laid-out causes for misclassifications suggest.

Table 2.2 provides a first indication in this regard, showing the actual ethnic composition of the German CILS4EU sample. The sample’s actual share of im-

## 2. Testing the data

Table 2.2: Actual composition of German CILS4EU sample (wave 1)

	Rel.freq. (in %)	2nd-generation immigrants (in %)	Name-based error rates (in %)	N (students)
Native	52.2	–	7.4	2,609
Immigrant	47.8	77.7	20.4	2,387
<i>Turkish</i>	17.4	88.5	2.3	867
<i>FSU</i>	5.8	45.0	54.6	291
<i>Polish</i>	3.3	74.3	70.7	167
<i>FYR</i>	4.4	80.6	7.2	222
<i>Other Western</i>	7.3	86.5	30.3	363
<i>Other Non-Western</i>	9.5	71.3	13.4	477
Total	100.0	–	13.6	4,996

*Source:* CILS4EU, w1, v1.2.0, unweighted

migrants is, at  $\sim 48\%$ , six percentage points higher than the same share identified according to name-based classification (cf. Table 1.1). The name-based approach thus underestimates the immigrant share actually present. Moreover,  $\sim 20\%$  of all actual immigrants are identified incorrectly via name-based classification whereas among actual natives it is only  $\sim 7\%$ .

A closer look at the ethnic subgroups reveals substantial variation in error rates among immigrants. In line with expectations, culturally more distant ethnic groups (Turkish, FYR, Other Non-Western) show very low error rates, partly even lower than those of natives. In contrast, among respondents with a Polish immigrant background more than 70% were identified incorrectly as natives. Polish respondents are thus more likely to be classified incorrectly as natives than correctly as immigrants. Similarly high error rates are also present among respondents from FSU countries.

However, not all of these observed group differences in misclassification may be ethnically specific. More recent immigrant generations are probably harder to identify correctly than earlier generations. From this perspective, the observed ethnic differences may be due partly to the fact that some ethnic groups are dominated by immigrants recently arrived, whereas other groups are composed mainly of second-generation immigrants. To dissect ethnic from generational

differences in misclassification I subsequently present results from multivariate analyses accounting for both attributes at the same time.

Results of the multivariate analyses are in line with expectations: substantial ethnic differences exist with regard to the likelihood of incorrect identification of an immigrant (see Figure 2.2, for full model results see Tables A.1 and A.2 in Appendix I). Natives and Turkish and FYR immigrants have the lowest probabilities of incorrect specification, while Polish and FSU immigrants have clearly the highest. Moreover, first-generation immigrants are better identifiable than second-generation immigrants, which holds true for all ethnic groups, however differently pronounced. A comparison of the patterns in Figure 2.2 to the gross ethnic differences reported in Table 2.2 reveals that some of the ethnic differences depicted in Table 2.2 are indeed due to compositional differences across ethnic groups in terms of immigrants' generational status. For example, the differences between Turkish and FYR immigrants seem to be largely attributable to the fact that second-generation immigrants are slightly more prevalent in the Turkish group. To summarize, the results corroborate the expectations concerning differential accuracies across ethnic groups and generations and are in line with findings of Schnell et al. (2014). Next, I will investigate what consequences these differences may have for measures of composition that rely on name-based classification.

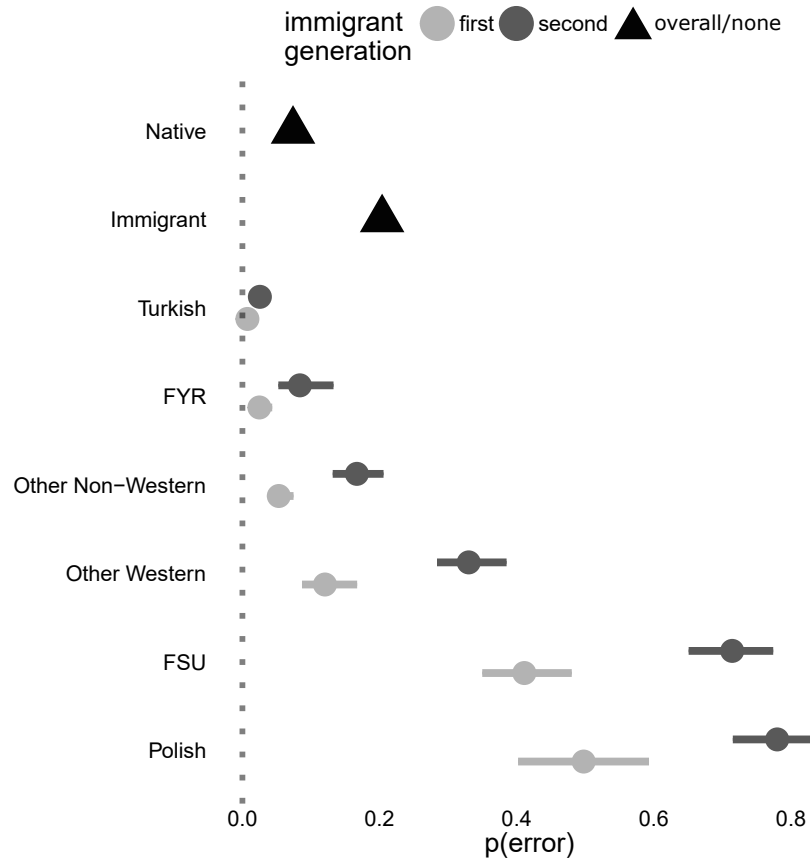


Figure 2.2: Predicted error rates in name-based classification (ethnic- and generation-specific, unweighted)

## Resulting context compositional bias

Based on the overall error rates of natives and immigrants (see upper two point estimates in Figure 2.2), it is rather straightforward to derive a first approximation of the bias induced for measures of context composition. At an error rate of immigrants of  $\sim 20\%$ , an all-immigrant neighborhood would be misspecified as having  $20\%$  natives. Vice versa, an all native neighborhood would be identified as having  $93\%$  natives, given that natives' error rate ranges around  $\sim 7\%$ . I can therefore already assert that the native proportion in mixed neighborhoods tends to be overestimated, while it is underestimated in native neighborhoods.



But what does it look like if I account for ethnically specific error rates and for differences in the ethnic mix in the neighborhoods?

First I have to explore the actual ethnic mix present in neighborhoods in Germany. Figure 2.3 provides information on the neighborhoods in the two exemplary German cities. The ethnic mix in immigrant-dominated neighborhoods (1<sup>st</sup> quintile) differs substantially from that in native-dominated neighborhoods (5<sup>th</sup> quintile). For example, Turkish immigrants make up a much larger share of all immigrants in the former type of neighborhood than in the latter.<sup>3</sup> As discussed, this may have important consequences for the bias induced by name-based classifications, given that the different ethnic groups show different error rates in terms of name-based classification. Note that the coloring of ethnic groups in Figure 2.3 was chosen such that darker colors indicate lower error rates. Hence, it becomes clear that the overall error rate that occurs when identifying immigrants who live in immigrant-dominated neighborhoods will be lower than the rate that occurs when they are identified in native-dominated neighborhoods. In line with expectations, the latter type of neighborhood has the lowest overall error rates.

Given the knowledge of about the actual ethnic mix in German neighborhoods, we are now prepared to investigate how biased name-based measures of context composition may be. I take the actual neighborhood compositions in the two cities<sup>4</sup> as exogenously given and simulate—based on the derived predicted error rates—the neighborhood compositions that would be yielded if I constructed the contextual measure via name-based classification. Figure 2.4 shows how the actual and the simulated name-based compositions of context relate. In the absence of any bias every neighborhood should be located on the bisecting line, as this would imply name-based proportions of natives to mirror those actually present in the neighborhoods (all grey lines correspond to those in Figure 2.1, serving as reference points). However, this is not the case, as the position of the grey circles suggests (each circle representing one simulated neighborhood). The cor-

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<sup>3</sup>Further analyses also based on the German first wave of CILS4EU reveal very similar patterns in school compositions, with culturally distant ethnic groups being overrepresented in immigrant-dominated schools (analyses not shown here, available upon request).

<sup>4</sup>In both cities, information is available on the actual ethnic compositions of the neighborhoods, but not on immigrants' generational status. I therefore constructed two simulated neighborhoods for every empirical neighborhood, one assuming all immigrant residents to be of the first generation, the other assuming all immigrant residents to be of the second generation.

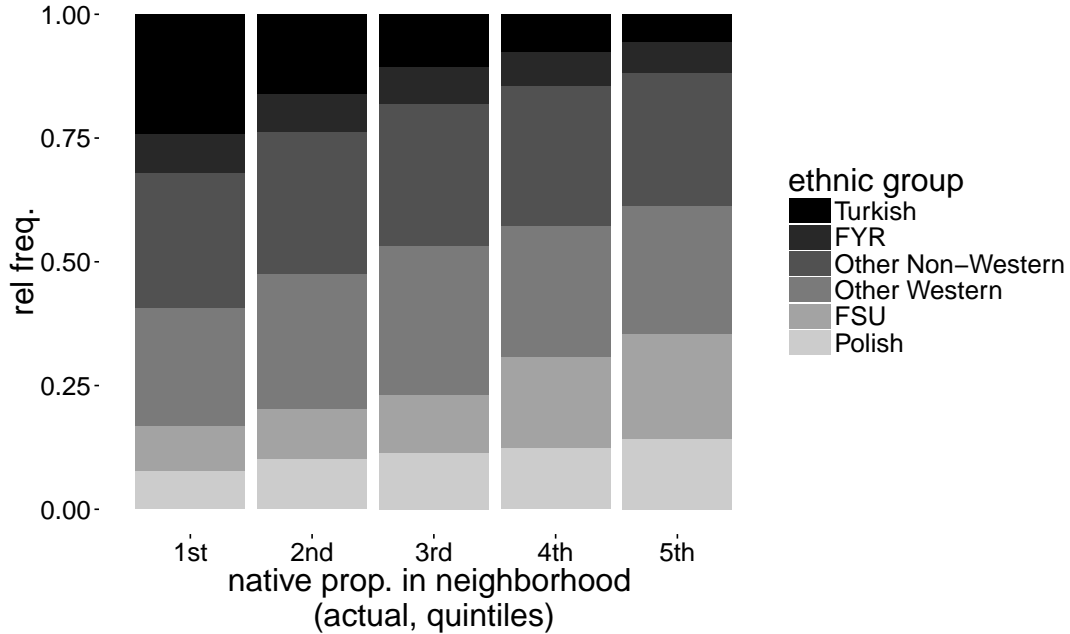


Figure 2.3: Ethnic mix in neighborhoods with different proportions of natives in two German cities.

responding LOWESS curve deviates from the bisecting line (see solid, black line). This deviation is strongest in neighborhoods with lower proportions of natives. This means neighborhood measures relying on name-based classification generally overestimate the native proportion in immigrant-dominated neighborhoods. For example, an actual native proportion of 40% in the neighborhood would be overestimated as that neighborhood's having  $\sim 50\%$  natives. In native-dominated neighborhoods the native proportion is slightly underestimated.

If I did not account for ethnically specific error rates and neighborhood mixes but only for overall error rates among natives and immigrants, the bias would be very similar for most neighborhoods (see black, dashed line). Accounting for the ethnic mix in the neighborhoods seems to matter only in neighborhoods with very low proportions of natives. Here, the two lines differ the most. However, such neighborhoods rarely exist.

To summarize, in line with expectations, measures of context composition that rely on name-based classification are subject to bias. More specifically, the proportions of natives in immigrant-dominated neighborhoods tend to be over-

estimated, whereas they are slightly underestimated in native-dominated neighborhoods. Moreover, accounting for differences in the ethnic mix in the neighborhoods does not substantially change this bias.

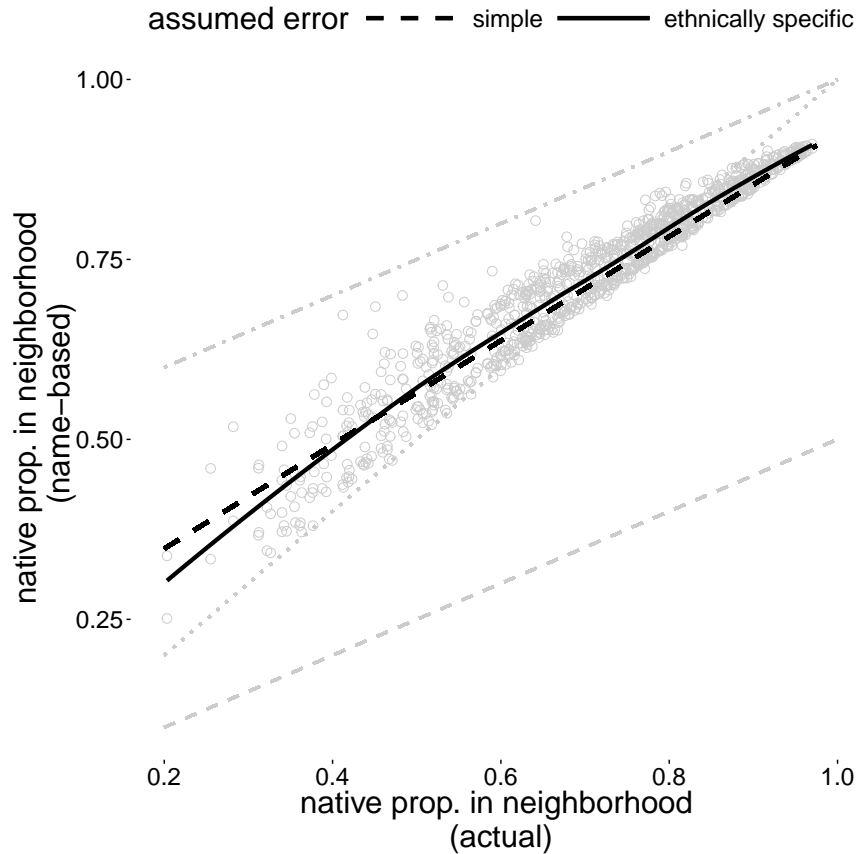


Figure 2.4: Actual and name-based proportions of natives in neighborhoods in two German cities. LOWESS trends across neighborhood compositions (black lines).

## 2.6 Conclusion

In this chapter, I investigated the accuracy of name-based approaches to identify the immigrant status among adolescents in Germany. The main contribution of the chapter is its test of how systematic misspecification may lead to bias in measures of context composition.

The analyses suggested the following: Specification error rates varied substantially across ethnic groups and across immigrant generations. Natives and immigrants of Turkish or of former Yugoslavian background were usually classified correctly (i.e., they showed very low error rates). Polish and FSU immigrants, however, were severely at risk of misclassification as natives. Given that the two latter groups are rather sizable in Germany, the observed error rates proved to have important consequences for measures of context composition that rely on name-based classification. Neighborhoods with extremely high or low proportions of natives were subject to bias, with both types tending toward values that were more moderate. In contrast, moderate proportions of natives were captured correctly. In other words, name-based measures of composition underestimated the variation present in proportions of natives across neighborhoods. The fact that neighborhoods usually differ in their ethnic mix did not substantially affect this bias.

Please, note that I focused on one specific name-based approach only, the HS approach. It is the most frequently applied technique in the German context, including the Microm neighborhood data used in the analyses of this book. However, as laid out, various other approaches exist and it is unclear whether they would perform similarly to the HS approach. Also, the findings are restricted to the example of a representative sample of adolescents in Germany in the year 2010. It may well be that name-based approaches perform differently in other targeted contexts.

Nevertheless, these findings hold a number of practical implications. Most importantly, name-based approximations of measures of context composition seem to work rather well in areas with moderate proportions of natives. Only extreme values are biased. Accounting for this bias they are a useful option for researchers interested in context effects, in the absence of more precise information.

To account for the bias they carry one of the following three things should be done when applying name-based measures of composition: 1) If there is information available about the target groups' error rates, one would optimally derive a correction factor from these error rates and apply it to the name-based measures of composition. Given that the ethnic mix in the neighborhoods did not substantially affect the bias, it may suffice here to account only for the overall error rates

of immigrants and natives in order to arrive at a satisfying correction factor.<sup>5</sup> 2) If no information on the target groups' error rates is available, but instead information about additional characteristics (beside their names) that correlate with the target group's ethnicity (e.g., age structure of households), use the latter to correct the name-based compositional data ex-post. Of course, this second alternative calls for additional theorizing, sophisticated modeling, and rich data, thus being rather cumbersome. 3) Finally, if no additional information about the target population whatsoever is available, the only option left is to be aware of the name-based bias when interpreting one's results.

The neighborhood data applied throughout this book has been corrected via the second option. The German geomarketing company Microm conducts ex-post corrections on their measures of ethnic neighborhood compositions, thereby relying on rich information on further contextual characteristics. Whereas there is no information about how successful these ex-post corrections may be, it seems reasonable to assume that the remaining bias in the applied measure will be at least not larger than what I could observe in this chapter. From this perspective, it may not seem too far fetched to rely on the name-based neighborhood measures in the next step of this explanatory endeavor.

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<sup>5</sup>A simple correction factor taking account of the overall error rates of immigrants and natives looks as follows:

$$p(nat)_{actual} = \frac{p(nat)_{name-based} - p(e|mig)}{1 - p(e|nat) - p(e|mig)}, \quad (2.1)$$

$p(nat)_{actual}$  being the desired, actual proportion of natives in a neighborhood (name-based proportion respectively),  $p(e|mig)$  being the error rate among actual immigrants and  $p(e|nat)$  among actual natives. Deriving this correction factor is very straightforward, as is demonstrated in Appendix III.



## Chapter 3

### Testing the framework pt.I. How do neighborhoods affect friendship choices?\*

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\*A different version of this chapter, co-authored by Sanne Smith, Frank van Tubergen, and Ineke Maas, was published in *Social Networks* (Kruse et al., 2016). To guarantee consistency across chapters, I have rewritten the chapter from a first-person perspective and reformulated various sections.

## Abstract

This chapter puts the spatially informed framework of friendship formation to a first empirical test. It examines whether neighborhoods determine more than the availability of outgroup peers in meeting contexts. To test this, the chapter focuses on friendships in the classroom—a well defined meeting context—allowing me to control for outgroup availability. Analyzing 3,345 students within 158 German and Dutch school classes, I find that sharing a neighborhood provides additional meeting opportunities to become friends in class as adolescents are likely to befriend classmates who live nearby them or who live nearby a friend of them (propinquity mechanism). However, this hardly explains why adolescent friendship networks in school classes tend to be ethnically homogeneous. Also, I find no convincing evidence that an adolescent's preference for same-ethnic friends in class would be affected by his/her neighborhood composition (exposure mechanism). This suggests that residential barriers and bridges for young immigrants' social integration are mainly caused by the availability mechanism.



## 3.1 Introduction

A consistent observation over time and space is that friendship networks among adolescents are ethnically homogeneous: From weak to strong types of friendship; and from the Netherlands to Belgium and Germany, Israel and the United States, scholars find that adolescents befriend members of their own ethnicity more often than those of other ethnicities (Baerveldt et al., 2007; Eshel and Kurman, 1990; Hallinan, 1982; Wimmer and Lewis, 2010; Windzio and Bicer, 2013).

Adolescents' place of residence can be a crucial determinant for ethnic homogeneity in their friendships, as Chapter 1 demonstrated. To learn more about the reasons why, this third chapter puts the book's preliminary theoretical framework to a first empirical test. It examines the different mechanisms how adolescents' place of residence affects their (same-ethnic) friendship formation. The chapter's focus is thereby on friendship formation in one of the most important meeting contexts for adolescents; their schools.

Previous studies unanimously argue and show that adolescents' place of residence restricts the number of outgroup peers in school (see Figure 1.5, arrow in center): Because neighborhoods are often ethnically homogeneous and because adolescents often attend schools nearby their homes, the neighborhood's ethnic composition can be held accountable for a lack of outgroup school peers that would be available as potential friends (Huckfeldt, 1983; Karsten et al., 2006; Mouw and Entwisle, 2006; Noreisch, 2007).

Less clear, however, is whether adolescents' place of residence affects same-ethnic friendship choices above and beyond constraining the set of outgroup school peers. A first argument posits that a neighborhood's ethnic composition affects its residents' same-ethnic friendship preferences (see Figure 1.5, right arrow). Relying on data of 1,589 adolescents in 84 classes in the Netherlands, Vermeij et al. (2009) show that adolescents have a stronger tendency for having same-ethnic social relations in class when they are exposed to fewer ethnic outgroup members in their neighborhood, irrespective of the opportunities they have for same-ethnic friendships within class. In line with intergroup contact theory (Allport, 1954), they argue that getting to know outgroup members in the neighborhood reduces ethnic prejudice, and as such, stimulates adolescents to befriend beyond

the boundaries of their own ethnic group in school. I term this effect the *neighborhood exposure effect* on same-ethnic school friendship.

A second argument describes an effect that I term the *neighborhood propinquity effect* on same-ethnic school friendship. The propinquity effect is based on the idea that living in the same neighborhood leads to recurrent meeting opportunities between school peers. In line with Feld's theory of focused organization of social relations (1981), this recurrent meeting in the neighborhood is likely to increase chances of friendship between peers in the school context. When same-ethnic school peers are more often neighbors than interethnic school peers (due to residential segregation), it may consequently explain why adolescents have so many same-ethnic friends in school (Mouw and Entwisle, 2006). In this case, adolescents would not necessarily prefer so many same-ethnic friends, but happened to have befriended these same-ethnic peers due to their neighborhood propinquity (see Figure 1.5, left arrow).

This chapter aims to test the existence of the neighborhood exposure and neighborhood propinquity effect, thus examining whether neighborhoods determine more than the mere availability of outgroup peers in meeting contexts. Doing so helps to get a better understanding of the importance of adolescents' place of residence for same-ethnic friendship formation in the school class context and beyond. Therefore, the research question reads: *How is adolescents' place of residence related to the tendency of having same-ethnic friends in school classes?*

The study's starting point is to replicate the exposure effect as well as the propinquity effect on same-ethnic school friendship as there is hardly any research devoted to these relations. Replication of the exposure effect is especially important given the conclusions drawn from a closely related field of study: Studies generally find no evidence that mere interethnic exposure leads to less ethnic prejudice or more positive interethnic attitudes because superficial exposure lacks meaningful contact necessary to build positive interethnic experiences (for a review, see Pettigrew and Tropp, 2006). Neighborhood interethnic exposure does not automatically include actual interethnic contact, and as such, the finding that neighborhood interethnic exposure relates to strong positive interethnic contact such as friendship contrasts a large body of research. Therefore, corroboration of Vermeij and colleagues' study is necessary.

Furthermore, I want to test whether the two outlined neighborhood effects work independently of each other: Living close to school peers of a different ethnicity is closely correlated with the ethnic composition of a neighborhood. The exposure effect may therefore not hold when the propinquity effect is taken into account and vice versa. For example, any decrease in the tendency of same-ethnic school friendships with decreasing neighborhood segregation may be due to increased propinquity to outgroup school peers, and not necessarily because general interethnic exposure in the neighborhood reduces ethnic prejudice. In other words: When I observe lower tendencies for same-ethnic friendship in schools among students who live in less ethnically segregated neighborhoods, it is unclear if both propinquity and exposure mechanisms contribute to this observation. Alternatively, one effect may be a spurious effect of the other. The current study therefore provides valuable information on the relation between the ethnic composition of neighborhoods and same-ethnic school friendship by studying the exposure and propinquity effect of the neighborhood simultaneously.

I test the hypotheses using not only the German but also the Dutch first wave of the CILS4EU data (Kalter et al., 2014).<sup>1</sup> The CILS4EU dataset contains rich and representative sociometric and attribute data on 9,376 students in 493 classes in 244 Dutch and German secondary schools. Not only can I replicate previous work on the subject and extend it to two countries, these data also provide improved measures of the neighborhood and same-ethnic friendship. Also, they allow me to exhaustively account for interdependencies in tie formation commonly found in (adolescent) friendship networks.

Previous research has used a varied terminology for the tendency for same-ethnic ties in friendship networks. Some scholars use the term *ethnic homophily* to refer to the observed overrepresentation of same-ethnic friendships without distinguishing how they have developed (McPherson et al., 2001). Other scholars reserve it for the social-psychological preference for same-ethnic friends only (Wimmer and Lewis, 2010). Also, there are notions of *baseline* versus *inbreeding* homophily (McPherson et al., 2001), and *gross* versus *net* homophily (Moody, 2001) to tell apart the tendency for having same-ethnic friendships uncontrolled

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<sup>1</sup>As mentioned, the inclusion of a second country was mainly due to this chapter being a joint work.

and controlled for a particular confounding concept of interest, respectively. In line with its introduction in Chapter 1 I will use the term ethnic homophily to refer to the theoretical concept of same-ethnic preferences. The term *ethnic homogeneity* is used to denote the overrepresentation of same-ethnic friendships in social networks that I observe.

## 3.2 Theory

Friendship formation in general has been studied extensively and several theoretical mechanisms have been proposed to explain how friendship choice comes about (Wimmer and Lewis, 2010). In line with this book's theoretical framework I follow an established research tradition that argues same-ethnic friendship to be the outcome of the preferences for same-ethnic friends over interethnic friends and the opportunities to meet same-ethnic peers in comparison to interethnic peers.

### Friendship preferences and the neighborhood exposure mechanism

Previous work on homophily argues that adolescents generally strive to befriend similar peers instead of dissimilar peers as they provide social resources, such as moral support and social affirmation (McPherson et al., 2001). Assuming that ethnicity signals or entails specific attitudes, beliefs or interests, it is usually argued that adolescents prefer same-ethnic friendships over interethnic friendships because they expect or find a better match between themselves and members of their group in comparison to members of other groups (Baerveldt et al., 2007; Moody, 2001; Wimmer and Lewis, 2010).

The strength of ethnic homophily, however, is likely to vary among individuals. Whereas ethnic similarity may be an essential friendship requisite for some, ethnicity may not be the characteristic that signals similarity and good friendship to others. The social surrounding, that is the neighborhood, may shape an adolescent's interethnic attitudes in such a way that he/she is more or less willing to choose an interethnic friend.

More interethnic contact in neighborhoods diminishes ethnic prejudice due to increasing opportunities for adolescents to positively experience ethnic outgroup members according to intergroup contact theory (Allport, 1954). As a consequence of reduced ethnic prejudice, peers from another ethnic group may be considered to be not too different after all, or at least not different from a negative perspective. For example, consider two students A and B in the same ethnically diverse school class. Student A lives in an ethnically diverse neighborhood and has interethnic contact when she plays outside, works in the local supermarket, or babysits for the neighbors who are from a different ethnic group. Going to an ethnically diverse school provides thus familiar interethnic interaction with the result that student A would have little reservation to make interethnic friends in class. Student B, however, lives in a neighborhood with mostly members of his own ethnic group. As such, he may solely interact with same-ethnic classmates because student B is hesitant to engage in non-familiar interaction with ethnic outgroup classmates.

Vermeij et al. (2009) showed evidence for the neighborhood exposure mechanism. They find that adolescents living in neighborhoods with more ethnic outgroup members have a weaker tendency to have same-ethnic friends in school, which results in less ethnic homogeneity in friendship networks observed in school. Although interethnic exposure in neighborhoods can be considered superficial contact, neighborhoods with more outgroup members provide at least more possibilities for interethnic contact than neighborhoods with more ingroup members (Semyonov and Glikman, 2009). Going back to the example, I do not necessarily know if student A really has positive interethnic neighborhood contact, but student A has at least a higher likelihood for it than student B because student B has no opportunity to engage in positive interethnic neighborhood contact in the first place. As such, I aim to replicate Vermeij and colleagues' study by testing that *the larger the share of ethnic outgroup members in the neighborhood is, the weaker the tendency to have same-ethnic friends in class (H1)*.

## Friendship opportunities and the neighborhood propinquity mechanism

A second set of mechanisms responsible for the prevalence of same-ethnic friendships can be referred to as opportunities for same-ethnic friendship. The chances of meeting same-ethnic peers within schools is to a large degree determined by the size of the ethnic ingroup within schools; in other words by the availability of ingroup and outgroup peers. In addition, however, it is determined by the propinquity of adolescents to same-ethnic peers (Wimmer and Lewis, 2010). As laid out, there is consensus about the existence of the availability mechanism. As I focus on same-ethnic friendship within school classes while taking the class ethnic composition into account (i.e., the relative size of ethnic groups), I will only elaborate on the propinquity aspect and take the availability mechanism as given.<sup>2</sup>

Propinquity refers to the possibilities adolescents have to interact within a given context and these are generally facilitated by any entity through which social behavior is structured, also known as foci (Feld, 1981). Examples of foci within schools that facilitate recurrent meeting of individuals are sharing a class or extracurricular activities like sports and arts clubs (Moody, 2001). These foci lead to more contact between peers above and beyond the opportunity structure for same-ethnic friendship in school. The more frequently school peers meet, the more likely a friendship between them becomes because recurrent encounters let adolescents spend more time together or may even signify a shared interest that adolescents hold.

Neighborhoods can also function as foci around which friendships in school develop. I refer to this as the *neighborhood propinquity mechanism*. For example, peers from the same neighborhood may share the same way to school, or participate in the same activities in a sports club or youth center close to their place of residence. Therefore, friendship between adolescents from the same neighborhood is more likely than friendship between adolescents who only share the same school. Mouw and Entwisle (2006) showed that a propinquity effect of the

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<sup>2</sup>The relation of neighborhood and school/class compositions is a question by itself addressing adolescents' school choices which I will turn to in Chapter 4.

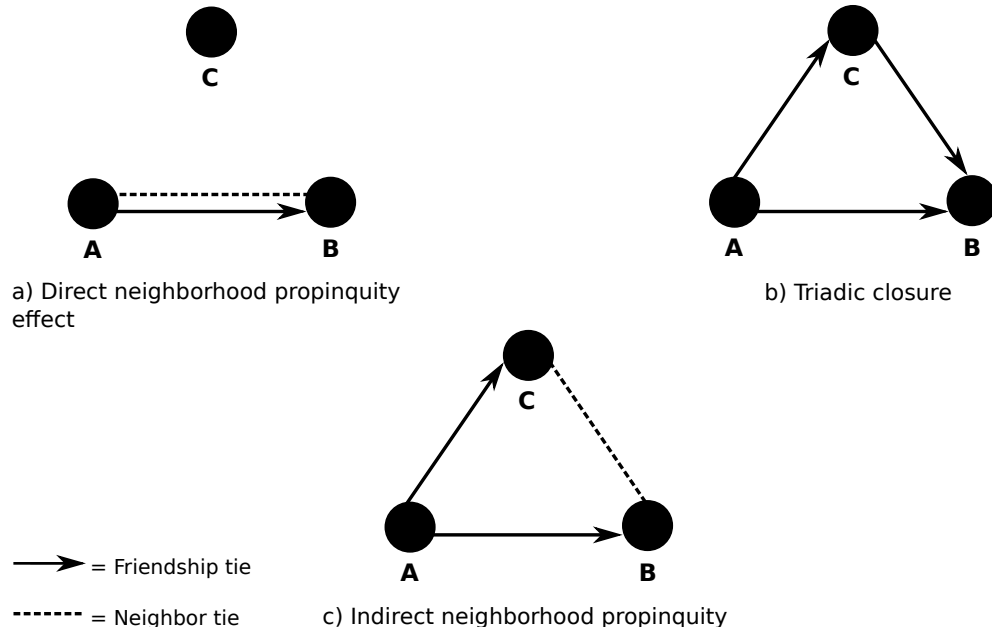


Figure 3.1: (a) Direct propinquity effect, (b) triadic closure, and (c) indirect propinquity effect.

neighborhood is very local: Only school peers that live very nearby are likely to become friends in school. Therefore, I consider a classmate that lives less than five minutes away to be a neighbor. For example, section a in Fig. 3.1 shows an adolescent (A), that has a classmate living nearby (B) and a classmate not living nearby (C). Because living nearby stimulates friendship, A is more likely to befriend B than C. I test whether *classmates who are neighbors are more likely to be friends than classmates who are not neighbors (H2)*. I refer to this effect as the direct neighborhood propinquity effect.

In Europe, many studies have shown evidence for substantial and even increasing ethnic residential segregation (Logan, 2006; Musterd and De Vos, 2007). As a consequence, the neighborhood propinquity effect may amplify the ethnic homogeneity of friendship networks in school classes. After all, in ethnically segregated neighborhoods, it is to be expected that classmates who live close by are more likely from the same ethnic group than those who do not live close by. Therefore, I examine if the direct neighborhood propinquity effect partly explains the tendency of adolescents to have same-ethnic friends in class. I test

the hypothesis that *befriending neighbors explains the tendency of adolescents to have same-ethnic friends in class (H3)*, assuming that neighbors are more often same-ethnic than interethnic due to residential segregation.

Adolescents, and people in general, get introduced to a subset of potential friends through the friends they made on an earlier occasion: Friendship formation is not an independent process as friendships form conditional on the already existing network structure (Goodreau et al., 2009; Moody, 2001; Mouw and Entwisle, 2006). As such, initial friends can be considered as foci as well (Feld, 1981). Due to initial friendship choice and that of their friends, particular peers are met more often than others, which results in those peers being more likely to become friends than others with whom an adolescent does not share friends. Consequently, friends of friends are often friends as well. This is known as triadic closure and is shown in section b of Fig. 3.1: A is likely to mention C as a friend, because both are friends with B.<sup>3</sup>

Mouw and Entwisle (2006) argued and showed that endogeneity in networks is not restricted to the school class setting. If it is the case that friends of friends are often friends, it should apply to geographical closeness as well. As friends are likely to spend time at each other's house, they may also become more likely to meet their friend's neighbors more often. The same mechanism of shared foci and increasing opportunities to meet may then also hold for classmates who are neighbors of friends. For example, section c of Fig. 3.1 shows that A is friends with B. Being friends with B may increase the time A spends in B's neighborhood. As such, A is likely to become friends with C as C lives close to B. I test, therefore, whether *adolescents are more likely to befriend a school peer who is a neighbor of a friend than a school peer who is not a neighbor of a friend (H4)*. This effect will be referred to as the indirect neighborhood propinquity effect. Note that this effect is different from the direct propinquity effect (section a, Fig. 3.1) because friendship between A and B is not necessarily caused by being neighbors. In

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<sup>3</sup>An alternative explanation for triadic closure in friendships is preference-driven and equally popular. It rests on the assumption that people generally strive to balance their social relations: Open triadic structures—friends of a friend are not one's own friends—induce strain and are hence avoided (Heider, 1946). Similar arguments also apply for actors' tendency to reciprocate friendship (Hallinan, 1978). Triadic closure and reciprocity are thus often referred to as *balancing* mechanisms (cf. Wimmer and Lewis, 2010).



addition, this effect is also different from a common triadic closure effect (section b of Fig. 3.1) because friendship between B and C is not necessary for A and C to become friends.

The indirect neighborhood propinquity effect could partly explain same-ethnic friendship within class if a neighborhood effect functions like a ‘snowball effect’. Consider a girl making an initial same-ethnic friend (who may or may not be a neighbor). This initial same-ethnic friend introduces her intentionally or unintentionally to his or her neighbors, who are likely to be same-ethnic too if neighborhoods are ethnically homogeneous. Transitive closure through the neighborhood may as such lead to increasingly ethnically homogeneous friendship networks. Therefore, I test the hypothesis that *befriending neighbors of friends explains the tendency of adolescents to have same-ethnic friends in class (H5)*.

### **A simultaneous examination of the neighborhood exposure and propinquity mechanism**

The question arises if superficial contact in neighborhoods may actually be so influential as to influence ethnic homophily. When reexamining the arguments of the independent neighborhood exposure effect and neighborhood propinquity effect, it is plausible to posit that the neighborhood exposure effect may be at least partly driven by the neighborhood propinquity effect. Previous research has shown evidence for residential ethnic segregation (Logan, 2006; Musterd and De Vos, 2007; Semyonov and Glikman, 2009) and adolescents often attend nearby schools to minimize traveling time, to join neighborhood acquaintances in the same school or, in the case of some countries, to comply to legal obligations (e.g., fixed school placement areas in the U.S. or England). As such, it is likely that general interethnic exposure in the neighborhood is related to having interethnic neighbors that go to the same school and are in the same class. Seemingly weaker ethnic homophily may in that case be actually due to more frequent outgroup contact because of neighborhood propinquity effects, and not necessarily because of a change in preferences due to the neighborhood exposure effect. Conversely, the propinquity effect may be driven by the exposure effect. In order to examine if one of the neighborhood effects on same-ethnic friendship preferences is not a

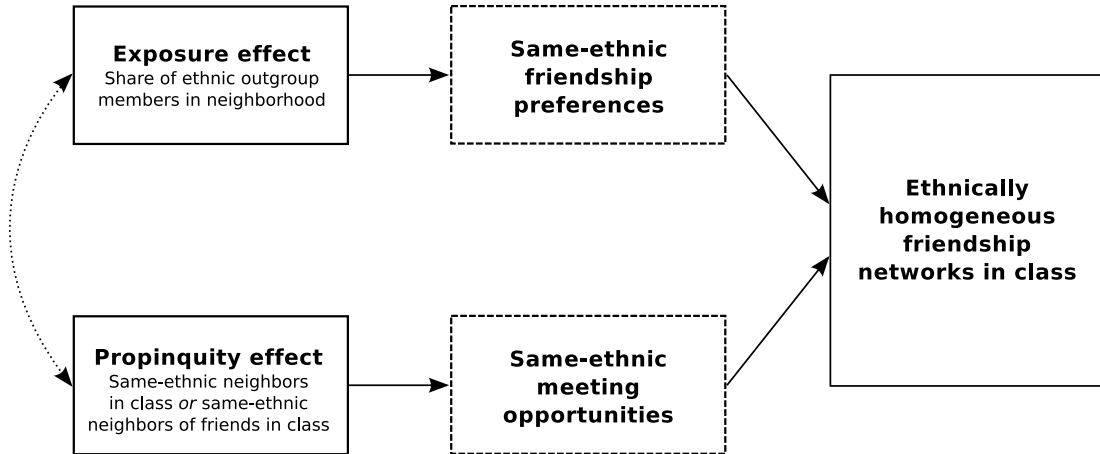


Figure 3.2: Theoretical arguments.

spurious effect of the other, I test the hypothesis that *the exposure and propinquity effect on the tendency of adolescents to have same-ethnic friends in class exist independently from each other (H6)*.

Fig. 3.2 summarizes the theoretical arguments. Note that the solid squares are what I observe and can measure, whereas the dashed squares are the theorized mechanisms. First, I examine previously researched relations of exposure and propinquity with same-ethnic friendship in school classes independently. Second, I test both mechanisms simultaneously.

### 3.3 Data

In this chapter I use the school class network data entailed in the first wave of the CILS4EU data (Kalter et al., 2014). Beside using data from the German part of the survey I also examine students in the Netherlands. The first wave data were collected in 2010/2011 and comprise a total of ( $N_{students} = 9,376$ ) interviews in ( $N_{classes} = 493$ ) classes and ( $N_{schools} = 244$ ) schools for these two countries. All students were asked to report their best friends within the school class with a maximum of five nominations. This information constructs the friendship networks that are to be modeled.

As laid out, objective neighborhood data have not been collected within the CILS4EU project. Therefore, I use external data sources, and, in lack of a single

internationally comparable data source, I rely on country-specific information on the ethnic composition of neighborhoods in which adolescents reside. Information on the ethnic composition in Dutch neighborhoods is based on official statistics published by the Dutch Bureau of Statistics (StatLine, 2013). The neighborhood is the smallest geographical unit available in the Netherlands and is defined by municipalities. On average, a neighborhood contains  $\sim 650$  households. Previous Dutch research has often relied on a larger geographical unit, that is, the four digit postal code (among which Vermeij et al., 2009). Neighborhoods defined by the municipality are argued to be more meaningful contexts to people than postal code areas are (Vervoort et al., 2011). Buildings within these local neighborhoods are often similar in style and age, and hence, inhabitants have often a similar socioeconomic status. Furthermore, neighborhoods are surrounded by natural borders such as water ways, main roads and train tracks.

For the German case, I follow Chapter 1 as well as other recent studies (Lersch, 2013; Sager, 2012) and use the Microm neighborhood data. Their data on immigrant proportions in neighborhoods is based on name-based classification (see Chapter 2): The ethnic origins of residents' first and family names were thereby used as a proxy for their own ethnic background (Humpert and Schneiderheinze, 2000). Microm offers information on a so-called *eight-digit postal code level* with an average size of  $\sim 700$  households.

## 3.4 Methods and measures

I analyze friendship by applying exponential random graph models (ERGMs from here on) to the school class friendship network data.<sup>4</sup> The estimation process of ERGMs operates on the network level, that is, it counts a specific tie constellation in an empirical network (e.g. the number of mutual ties present), and compares these counts to those obtained from simulated networks to examine how likely a hypothesized tie-generating mechanism is (e.g., there are more or fewer mutual ties than expected at random). Applying this method allows me to examine same-ethnic friendship formation while taking into account other network-structural

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<sup>4</sup>All analyses were carried out in R (v.3.0.2) and made foremost use of the *statnet* (v.2014.2.0) library (Handcock et al., 2008).

characteristics such as the availability of same- versus interethnic dyadic pairings or higher order structural effects such as triadic closure (for more general information about ERGMs and their functioning, see Robins et al. (2007) or Lusher et al. (2013)). Instead of analyzing single classes, I opted for school-wise models.<sup>5</sup> Estimating school-wise instead of class-wise models proves to be helpful in finding informative estimates due to more variation in ethnic background and neighborhood composition on the student level.

The data structure calls for a two-step procedure in the analysis, as proposed by Snijders and Baerveldt (2003): I first apply the same ERGM to each empirical school network separately. Secondly, I summarize school-specific results by using a meta-analysis to investigate the proposed hypotheses above and beyond the single-school case.

#### Within-school ERGMs

I apply an identical model setup to each of the empirical school networks. As class networks within the same school are disconnected from each other by study design (see Kruse and Jacob, 2014), I rule out between-class ties, assuming the tie-generating mechanisms to be similar across classes and schools (cf. de la Haye et al., 2011; Dijkstra et al., 2011; Svensson et al., 2012; Van Zalk et al., 2013).<sup>6</sup> The outdegree was constrained to 5, as adolescents could nominate no more than 5 friends in class.

The theoretical concept of ethnic homophily is captured by a statistic that sums all friendship nominations in which the sender of a nomination (ego) and the receiver of a nomination (alter) are both from the majority group (*both majority*) and one that counts those ties in which ego and alter are both from the same immigrant minority group (*same minority*). The reference group consists of friendships between majority and immigrant minority adolescents, and friendships between immigrant adolescents with a different immigrant background. Note, however, that the assignment of an immigrant status is wider than in Chapter 1 in order

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<sup>5</sup>In more than 80% of all sampled schools data of two or more classrooms per school are available.

<sup>6</sup>I included a network statistic in the model that identifies all between-class ties and fixed its coefficient value at negative infinity.

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to guarantee sufficiently high immigrant numbers in all classrooms. If at least one of the students' grandparents or parents was born in a foreign country, I categorize the student as having an immigrant background.

There are around 100 countries from which children of immigrants in the data originate. Most of these immigrant groups are so small, that the adolescents from these groups hardly ever meet a same-ethnic peer in class. Therefore, I collapse small immigrant groups in the categories Non-Western and Western immigrants. Western immigrant countries are European countries and countries where the dominant language is English (e.g., the US, Australia and New Zealand). The largest immigrant groups, however, are accounted for separately. These are Turks, immigrants from the Former Soviet Union (FSU), Poles, immigrants from the Former Yugoslavian Republic (FYR), other Western and other Non-Western immigrants in Germany; and Turks, Moroccans, Surinamese, Antilleans/Arubans, other Western and other Non-Western immigrants in the Netherlands. A friendship in which ego has a Turkish background and alter a Moroccan background, for example, would therefore not count as a same-ethnic friendship. The same minority variable is as such best interpreted as the averaged ethnic homophily of immigrants. Collapsing the other Western and other Non-Western immigrants implies an underestimation of immigrant ethnic homophily, because same-ethnic pairs in these groups can be interethnic pairs as well.<sup>7</sup>

Propinquity is also measured with two variables, that is, *direct* and *indirect propinquity*. The direct propinquity mechanism is captured by a network statistic that counts all ties in which at least one of the two students reported to live within a 5-min walking distance to the other. Students' reports of classmates living close by might account better for spatial boundaries such as railway tracks, lakes or bigger highways, than an objective measure of spatial distance between students' homes (cf. Mouw and Entwisle, 2006). I thus assume that ego and alter live close to each other (i.e., direct propinquity) if at least one of them reported to live within a 5-min walking distance to the other. Indirect propinquity is operationalized as the sum of all ties in which alter lives within a 5-min walking

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<sup>7</sup>I repeated the analysis by separating Western and non-Western immigrant homophily from immigrant homophily to examine if the results are robust. The conclusions on the hypotheses are the same as for the analyses shown.

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distance to a friend of ego. It is possible that adolescents only report their class peers to be neighbors if they are friends. Given the fact that 79% of all neighbor nominations are directed at non-friends, I assume that such a possible bias is not problematic in this study.

The ethnic composition of a neighborhood is measured using *the proportion of immigrants in the neighborhood*.<sup>8</sup> Both the German and the Dutch neighborhood data refer to individuals, not households, and thus include children in their counts. The proportions of immigrants enter the model as an ego-effect interacting with the same-ethnic statistic (including its main effect), thus measuring whether adolescents living in neighborhoods with high immigrant proportions send more (for immigrants) or fewer (for natives) ingroup nominations than adolescents living in neighborhoods with lower proportions.

Besides these main covariates of interest, several further network statistics enter the models as controls. The general tendency for adolescents to nominate peers as friends is represented by the variable *edges*, counting all friendship nominations present in a network. Even though I am not specifically interested in the degree to which adolescents have friends, it is necessary to include this measure as it functions as a model intercept.

I also control for lower and higher order balancing mechanisms commonly found in adolescent friendship networks (Goodreau et al., 2009; Moody, 2001; Mouw and Entwisle, 2006; Wimmer and Lewis, 2010). First, *reciprocity* is measured by a statistic counting all mutual friendship nominations. Transitivity, that is, students' tendency to befriend friends of their friends, is measured by capturing shared friends. Empirically, I see that friendship nominations in which ego and alter share many friends are less common than structures in which ego and alter share few friends. The underlying theoretical idea here is that friendships generate a positive but decreasing marginal utility. The *geometrically weighted edge-wise shared partner (GWESP)* measure captures the tendency that shared

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<sup>8</sup>I take the assumption that the measure reflects outgroup members to natives, and ingroup members to immigrants. Even though not every immigrant is an ingroup member to immigrants (e.g., a Chinese neighbor is not an ingroup member to a Turkish adolescent), I will use the share of immigrants in the neighborhood instead of a measure like the share of outgroup members for the following reason: Natives have very low values on a share of outgroup members in the neighborhood, whereas immigrants have very high values. These skewed data resulted into very high coefficients and unreliable results.

friends increase the likelihood of friendship and thus offers a better model fit and minimizes problems with model convergence (Hunter, 2007; Hunter et al., 2008). Similarly, I also include geometrically weighted indegree and outdegree parameters (*GWIDegree* and *GWODegree*) to capture the tendency to send friendship nominations and receive friendship nominations.

I additionally control for sex homophily by including a network statistic into the model that counts all ties in which ego and alter have the *same sex*, as having the same sex has repeatedly been shown to be one of the strongest predictors for friendships between adolescents (McPherson et al., 2001; Poulin and Pedersen, 2007; Shrum et al., 1988). For the same reason, I include a variable accounting for the *difference in socioeconomic status*. I measure the socioeconomic status by using the 2008 4-digit International Standard Classification of Occupations code (ISCO-08) in combination with the International Socio-Economic Index of occupational status ranking (ISEI-08) (Ganzeboom et al., 1992). The ISEI measure relies on parental job information provided by the parents if available, and otherwise on information provided by the adolescents. I take the highest ISEI score in the household for each student and include a statistic counting all ties present in the network weighted by the absolute difference between ego and alter's parental ISEI score into the model. For all dyadic variables (same ethnicity, same sex, and difference in socioeconomic status), I additionally include sender and receiver effects to control for sociality and popularity effects.

114 missing values on individual neighborhood data in Germany were imputed using the sociometry items (5.4% of country total, no missings in the Netherlands). Adolescents with missing neighborhood data were assigned the neighborhood data of the peers that were nominated living within a 5-min distance. If no peers lived nearby, the average neighborhood values of the school were imputed. Missing values on other attribute data was so low (< 5%) that I did not impute them.

## Meta-analysis

I summarize the school-network specific ERGM results in a meta-analysis following Snijders and Baerveldt (2003). I calculate weighted least squares estimates for

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all model coefficients based on the school-specific coefficient estimates and their respective standard errors. As such, schools with more precise coefficients contribute more to the averaged coefficient over schools than schools with coefficients that are characterized by more uncertainty.

Some schools had to be excluded from the analysis a priori, due to unit non response or other data problems.<sup>9</sup> Further, only those school-specific ERGM coefficients entered the meta-analysis where estimation of all model setups turned out to be successful. One requirement is therefore, that in- and outgroup nominations had been possible in at least one class in a school. This means that there should be at least 2 majority and 2 minority students in one of the classes. These data requirements are similar to those of previous studies (Lubbers, 2003; Smith et al., 2014).

Further, I exclude the ERGM results of schools where the universally applied model set up did not fit the data well. I examined  $t$ -ratio's for convergence and checked if the absolute values corresponding to the estimates were close to zero. Estimates that did not satisfy this condition (at least one  $t$ -ratio  $> .2$ ) were excluded from the analysis. Goodness of fit (GOF) was examined by simulating networks based on the modeled coefficients and by comparing the simulated values for the edgewise-shared partner, outdegree, and geodesic distance statistics with the respective observed values using statnet's built-in GOF command for ERGMs (Goodreau et al., 2008). GOF-ratio's larger than 2 indicate an unsatisfying goodness of fit (Robins et al., 2009) and also these school networks were excluded from the analyses. Table 3.1 indicates that most school networks met this requirement, that the mean GOF-ratio is relatively low, and that the GOF-ratios are maximally 0.6 points larger than 2. Lastly, when standard errors in one of the model setups exceed 5 or coefficient sizes exceed  $\pm 10$ , it is also highly likely that the model setup did not fit the observed network or that the network is an outlier. I exclude these schools from the meta-analysis. After these exclusions, I analyze 89 schools and refer to this sample as the *balanced model population*.

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<sup>9</sup>The school class networks had to match the following conditions to be considered: (1) at least 75% of the students participated in the network survey; (2) class size of at least 10 students; (3) no more than 10% of all nominations are invalid; and (4) no more than 4 students in class have never (been) nominated in any of the network-related items



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Table 3.1: Goodness of fit.

	% schools with GOF-ratio < 2	Mean GOF-ratio over schools	Max. GOF-ratio
M1	94.52		0.53
M2	94.62		0.54
M3	94.46		0.55
M4	94.53		0.55
N(students)		3,345	
N(classes)		158	
N(schools)		89	

*Source:* CILS4EU, w1, v1.1.0 / Microm, unweighted

Table 3.2 shows the descriptive statistics of the individual attributes of adolescents in the balanced model population that I use in the analyses. Values are shown in total, separately for Germany and the Netherlands, and separately for the majority and minorities. Note that Table 3.2 does not show representative data, but is merely a description of the data I work with. The descriptive statistics between countries and groups show mostly similar values, but some differences are notable. For example, minorities have higher proportions of immigrants in their neighborhood and school class, which is already an indication of neighborhood and school segregation. Also, note that the average share of immigrants in the neighborhood is 11% even though minorities make up 50% of the balanced sample. Higher immigrant shares in school than in the neighborhood are to be expected because schools with a higher share of immigrant students are oversampled in the CILS4EU data and school classes are small units that receive pupils from multiple larger unit neighborhoods. In addition, the percentage of immigrants is generally higher among adolescents than among older people and so schools have higher proportions of immigrants than neighborhoods. Finally, self-selection of Muslim immigrant children into Islamic or Christian schools instead of secular schools (Van Kessel, 2000) and overrepresentation of immigrant children in lower educational tracks (Dijkstra et al., 1997) may account for the discrepancy between the ethnic composition of schools and neighborhoods (more on this in Chapter 4).

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Table 3.2: Descriptive statistics of balanced model population.

	Germany		the Netherlands		Total	
	Mean	s.d.	Mean	s.d.	Mean	s.d.
Immigrant background	.564		.396		.502	
<i>Turkish</i>	.157		.035		.112	
<i>FSU</i>	.073		.000		.046	
<i>Polish</i>	.057		.000		.036	
<i>FYR</i>	.046		.000		.029	
<i>Moroccan</i>	.000		.039		.015	
<i>Surinamese</i>	.000		.046		.017	
<i>Antillean</i>	.000		.027		.010	
<i>Other Western</i>	.116		.094		.108	
<i>Other Non-Western</i>	.116		.155		.130	
Friendship nominations	3.758	1.343	3.485	1.327	3.657	1.343
<i>Majority</i>	3.746	1.317	3.515	1.302	3.642	1.315
<i>Minorities</i>	3.767	1.362	3.439	1.364	3.671	1.370
Classmates living close	2.028	2.013	1.033	1.289	1.659	1.843
<i>Majority</i>	1.929	1.938	1.036	1.279	1.528	1.732
<i>Minorities</i>	2.104	2.067	1.028	1.305	1.789	1.939
Immigrants in neighborhood	.102	.079	.110	.123	.105	.098
<i>Majority</i>	.076	.062	.086	.098	.080	.080
<i>Minorities</i>	.123	.085	.146	.147	.129	.107
Male	.528		.500		.518	
<i>Majority</i>	.535		.514		.526	
<i>Minorities</i>	.522		.480		.510	
Socioeconomic status (ISEI)	43.791	19.428	54.128	19.944	47.646	20.246
<i>Majority</i>	47.993	18.439	55.742	19.091	51.500	19.124
<i>Minorities</i>	40.461	19.554	51.554	21.000	43.691	20.606
Immigrants at school	.563	.155	.397	.134	.501	.168
<i>Majority</i>	.509	.148	.366	.122	.445	.154
<i>Minorities</i>	.604	.147	.444	.138	.557	.162
N(students)	2,104		1,241		3,345	
N(classes)	98		60		158	
N(schools)	58		31		89	

Source: CILS4EU, w1, v1.1.0 / Microm, unweighted

## Interpretation of mediating effects: Simulations

I hypothesize that propinquity in the neighborhood (partly) explains why adolescents tend to choose same-ethnic friends in class. A standard method to answer mediation questions like this would be to compare coefficients between models with and without the hypothesized mediator. Comparisons of coefficient sizes across different ERGM setups are a rather unreliable indication for mediating effects, however. As ERGMs are in the family of logistic models, the size of coefficients between models may be dependent on the explained variance within these models (Mood, 2010).

A more promising approach, instead, is to make use of network simulations that are based on the coefficients derived in the between-school meta-analyses.

Here, I suggest to compare the formation of same-group friends that would result from the empirically observed scenario (i.e., number of schools, school sizes, and actor attributes are as empirically observed in the balanced school sample) to those that would result from a counterfactual scenario where all class peers live apart from each other such that a propinquity effect would be completely absent.<sup>10</sup> I quantify the tendency for same-ethnic friendship in terms of each simulated network's measure  $\alpha$ . The measure  $\alpha$  is defined as the logged ratio of the odds of ingroup friends versus ingroup non-friends divided by the odds of outgroup friends versus outgroup non-friends. Whereas the lack of a short and clear-cut interpretation of  $\alpha$  is clearly a shortcoming of the measure, it has one major advantage:  $\alpha$  conveniently controls for relative sizes of the different groups in the school classes, thus allowing me to compare the tendency for same-ethnic friends across different networks and scenarios (see also Moody, 2001; Mouw and Entwisle, 2006).<sup>11</sup>

To arrive at a reliable comparison I conduct 250 simulation runs per scenario.<sup>12</sup> Within each simulation run I first generate school-specific networks based on the laid out setups of each scenario. All simulations thereby rely on a model configuration that includes propinquity effects (for the exact configuration refer to Section 5) with the coefficients derived in the between-school meta-analyses. To guarantee comparability to the empirical networks I constrain students' outdegree to a maximum of 5 in all simulations. Once the school-specific networks are simulated I then determine each network's  $\alpha$  and take its mean value over all

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<sup>10</sup>The setup of this counterfactual scenario is as follows: almost all actor attributes follow the empirically observed distributions, namely actors' sex, and their social and ethnic background. The only difference is that I set the dyadic covariate of propinquity to zero for all dyads. Due to this latter setup adjustment the contribution of a propinquity effect to actors' tie formation will be zero. Any difference in same-ethnic estimates between the empirical and counterfactual scenario would therefore be indication for an actual contribution of the propinquity effect to the overall level of same-ethnic estimates.

<sup>11</sup>Note, however, that due to the assumption of interdependence between tie formation mechanisms it is impossible to provide the strictly isolated contribution of one single tie formation mechanism, as, for example, for other model types predicted probabilities would do. A comparison of the same-ethnic effects in the presence and complete absence of a propinquity effect therefore actually yields the contribution of the propinquity mechanism while simultaneously accounting for the interdependency between all tie formation mechanisms included in the model.

<sup>12</sup>All simulations are carried out based on the built-in simulation function for ERGM results provided in the statnet package (Handcock et al., 2008).

schools, thus ending up with one (mean)  $\alpha$  value per simulation run. Proceeding as such, I finally end up with 250 (mean)  $\alpha$  values per scenario. By comparing the distributions of  $\alpha$  across different scenarios I can infer whether propinquity effects partly explain why adolescents tend to choose same-ethnic friends in school classes.

## 3.5 Results

### Descriptive results

Linear estimations of the empirical distribution of  $\alpha$  across different neighborhood compositions are depicted in Figure 3.3. In both countries (Germany left, the Netherlands right), majority and minority adolescents show mainly positive levels of  $\alpha$  implying that the odds of forming a tie in the ingroup are higher than those of forming one in the outgroup. The regression slopes indicate that there is variation in  $\alpha$  across neighborhood compositions. Majority  $\alpha$  rises with the immigrant percentage in adolescents' neighborhoods, both in Germany and the Netherlands. For immigrant  $\alpha$ , there is no clear-cut trend across neighborhoods with varying ethnic compositions. These bivariate effects seem to contradict contact theory, the finding of Vermeij et al. (2009), and the hypotheses (H1, H3, and H5). Note however, that  $\alpha$  is not a direct measure of ethnic homophily, as it solely captures observed ethnic homogeneity net of relative group size effects (i.e., it captures a tendency for same-group friends). It is not controlled for other important variables that may also affect same-ethnic friendship in school classes. To arrive at a more informative proxy for ethnic homophily I will therefore have to turn to the explanatory analyses where I additionally control for propinquity mechanisms, structural network mechanisms, and other important control factors.

Table 3.3 provides a first indication that propinquity mechanisms could explain why adolescents tend to befriend same-ethnic peers (H3 and H5). Both in Germany and in the Netherlands students have a higher ingroup share among those classmates who live close by than among those who do not live close by. Of the classmates that live within a 5-min distance, 59% is on average same-ethnic for majority members, whereas 54% is so of the peers who live further away. For

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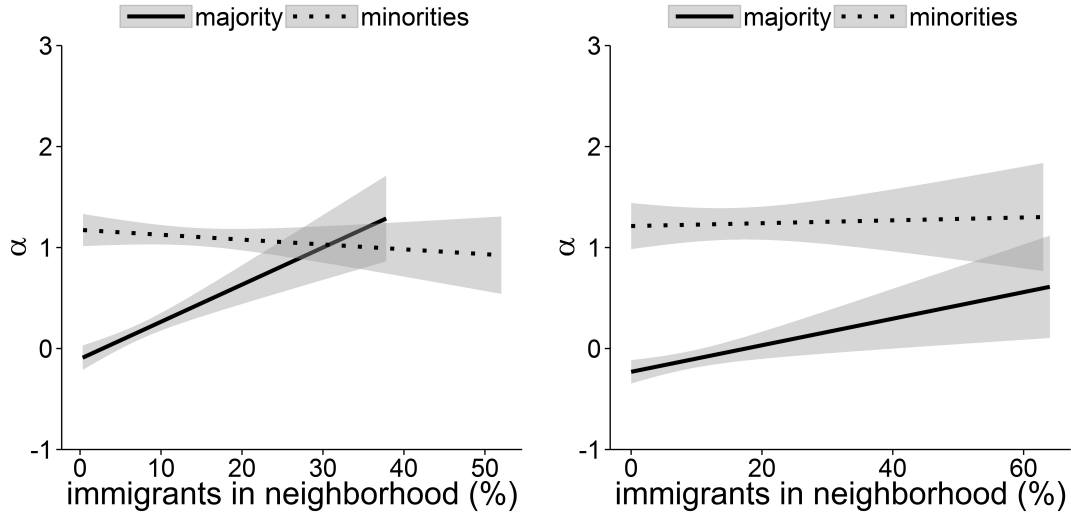


Figure 3.3: Tendency for same-ethnic-group friends in school classes ( $\alpha$ ) across different neighborhood compositions. Linear trends with 5%-confidence intervals in gray; left: Germany, right: the Netherlands.

Table 3.3: Proportion of ingroup members among class peers who (do not) live within a 5-min walking distance for majority and minority students.

	Germany			the Netherlands			Total		
	Living close	Not living close	p-value	Living close	Not living close	p-value	Living close	Not living close	p-value
Majority	.529	.486	.008	.681	.628	.010	.587	.540	.000
Minorities	.216	.175	.000	.151	.143	.666	.201	.168	.001

Source: CILS4EU, w1, v1.1.0, unweighted

immigrants, 20% of the peers that live nearby are same-ethnic, compared to 17% of the peers that live further away. Both differences are significantly different from zero.

Note, the differences between same-ethnic and interethnic neighbors shown in Table 3.3 are relatively small despite their significance. Also, they are more pronounced in Germany than they are in the Netherlands. It is therefore questionable if a general tendency to form friendships due to propinquity could explain the tendency of adolescents to befriend same-ethnic peers in school classes.

## Explanatory results

In order to test the hypotheses I turn to the results of the multivariate ERGMs. Tables 3.4 and 3.5 show the results of the between-school meta-analysis for Germany and the Netherlands, respectively. I report unstandardized mean coefficient estimates that provide an uncertainty-weighted average of the school-specific coefficients of four different ERGM setups. The Fisher test shows if there is at least one school with a significant positive (as indicated by ‘+’) or negative effect (as indicated by ‘-’). Each setup reveals different information about how adolescents’ neighborhood affects the ethnic composition of their friendships in school classes.

Table 3.4: Pooled meta-analysis results of schoolwise ERGMs in Germany.

	M1		M2		M3		M4	
	coef <sup>a</sup>	s.e.	coef <sup>a</sup>	s.e.	coef <sup>a</sup>	s.e.	coef <sup>a</sup>	s.e.
Density	-4.228***	0.109	-4.381***	0.116	-4.192***	0.150	-4.312***	0.135
Reciprocity	2.175***	0.063	2.148***	0.063	2.179***	0.065	2.149***	0.063
GWIndegree	0.805***	0.122	0.907***	0.127	0.785***	0.023	0.879***	0.127
GWOutdegree	0.860***	0.224	0.850***	0.221	1.011***	0.228	0.978***	0.228
GWESP	0.883***	0.029	0.872***	0.029	0.881***	0.117	0.869***	0.030
Boy ego	0.127	0.079	0.129**	0.078	0.143*	0.102	0.149*	0.083
Boy alter	-0.014	0.052	0.003	0.053	-0.015	0.102	-0.001	0.054
Same sex	0.759***	0.033	0.763***	0.034	0.781***	0.115	0.788***	0.036
SES ego	0.003*	0.002	0.003*	0.001	0.003	0.002	0.002	0.002
SES alter	-0.002	0.001	-0.002	0.001	-0.001	0.001	-0.002	0.001
Difference SES	-0.002**	0.001	-0.002*	0.001	-0.002**	0.001	-0.002*	0.001
Majority ego	0.019	0.069	0.031	0.068	0.014	0.072	0.015	0.075
Majority alter	-0.184***	0.069	-0.175**	0.070	-0.181**	0.063	-0.172**	0.073
Both majority	0.295***	0.072	0.280***	0.077	0.224*	0.131	0.138	0.130
Same minority	0.324***	0.048	0.318***	0.049	0.143	0.113	0.174	0.137
Propinquity			0.498***	0.040			0.504***	0.040
Indirect propinquity			0.303***	0.042			0.293***	0.044
Prop. immig. neighb. ego							-0.009*	0.006
Both majority *							0.005	0.014
prop. immig. neighb. ego								
Same minority *								
prop. immig. neighb. ego								
N(schools)		58		58		58		58

Source: CILS4EU, w1, v1.1.0 / Microm, unweighted

Note: a Unstandardized mean coefficient estimate according to Snijders and Baerveldt (2003).

\*  $p < .1$ ; \*\*  $p < .05$ ; \*\*\*  $p < .01$ ; + right-sided Fisher test score  $< .025$ ; - left-sided Fisher test score  $< .025$ .

Table 3.5: Pooled meta-analysis results of schoolwise ERGMs in the Netherlands.

	M1		M2		M3		M4	
	coef <sup>a</sup>	s.e.	coef <sup>a</sup>	s.e.	coef <sup>a</sup>	s.e.	coef <sup>a</sup>	s.e.
Density	-4.905***	0.166	-5.068***	0.187	-4.889***	0.196	-5.027***	0.204
Reciprocity	2.164***	0.086	2.144***	0.086	2.161***	0.087	2.146***	0.087
GWIndegree	1.452***	0.141	1.599***	0.148	1.451***	0.144	1.585***	0.149
GWOutdegree	1.969***	0.267	1.980***	0.268	2.096***	0.276	2.108***	0.278
GWESP	1.076***	0.031	1.063***	0.031	1.077***	0.032	1.064***	0.031
Boy ego	0.194*	0.114	0.194*	0.109	0.226**	0.115	0.219**	0.109
Boy alter	-0.041	0.067	-0.049	0.074	-0.050	0.069	-0.056	0.075
Same sex	0.595***	0.033	0.628***	0.039	0.609***	0.034	0. * **	0.041
SES ego	-0.004***	0.002	-0.004***	0.002	-0.004***	0.002	-0.004***	0.002
SES alter	0.003**	0.001	0.003**	0.001	0.003**	0.001	0.003**	0.001
Difference SES	-0.002*	0.001	-0.001	0.001	-0.002*	0.001	-0.001	0.001
Majority ego	-0.069	0.096	-0.069	0.095	-0.062	0.100	-0.067	0.101
Majority alter	-0.133*	0.077	-0.144*	0.076	-0.128	0.078	-0.150**	0.076
Both majority	0.326***	0.067	0.316***	0.065	0.322***	0.085	0.298***	0.091
Same minority	0.183*	0.106	0.163*	0.099	0.165	0.180	0.162	0.182
Propinquity			0.605***	0.049			0.614***	0.050
Indirect propinquity			0.477***	0.096			0.471***	0.098
Prop. immig. neighb. ego					-0.001	0.004	-0.002	0.004
Both majority *					-0.005	0.010	-0.002	0.010
prop. immig. neighb. ego								
Same minority *								
prop. immig. neighb. ego					0.008	0.008	0.005	0.007
N(schools)	31		31		31		31	

Source: CILS4EU, w1, v1.1.0 / Microm, unweighted

Note: a Unstandardized mean coefficient estimate according to Snijders and Baerveldt (2003).

\*  $p < .1$ ; \*\*  $p < .05$ ; \*\*\*  $p < .01$ ; + right-sided Fisher test score  $< .025$ ; - left-sided Fisher test score  $< .025$ .



### 3. Testing the framework pt.I

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Model 1 serves as the baseline model to find out about the general level of same-ethnic friendship throughout all schools of the balanced sample when controlling for other friendship formation mechanisms. The same-ethnic effects are positive, both for the majority ( $b_{Germany} = 0.295, p \leq 0.01$  ;  $b_{theNetherlands} = 0.326, p \leq 0.01$ ), as well as for minority adolescents ( $b_{Germany} = 0.324, p \leq 0.01$  ;  $b_{theNetherlands} = 0.183, p \leq 1$ ). This means that, compared to an interethnic tie, a same-ethnic friendship is  $e^{0.295} \approx 1.34$  times and  $e^{0.326} \approx 1.39$  times more likely for a majority group student in Germany and the Netherlands, respectively. Same-ethnic friendship is  $e^{0.324} \approx 1.38$  times and  $e^{0.183} \approx 1.20$  higher for an adolescent with minority group background in Germany and the Netherlands, respectively. Results with respect to the control variables are in line with previous findings about friendship formation in school classes: Friendship nominations are rather sparse as the negative edges effect suggests. Also, the effects of reciprocity and transitivity (GWESP) show that friendships tend to be reciprocated and triadic structures tend to be closed. Besides significant same-ethnic effects, there is also a positive effect of having the same sex and somewhat weaker—but marginally significant ( $p < .1$ )—evidence for friendships occurring more often within the same socioeconomic status group than across.

In Model 2 I introduce the measures of direct and indirect propinquity. Tables 3.4 and 3.5 reveal, in line with hypotheses 2 and 4, that adolescents are more likely to nominate classmates living close by as friends than classmates living further away. Furthermore, they are more likely to nominate someone as a friend if he/she lives close to another friend. Note that the triadic indirect propinquity effect is positively significant net of the other triadic control variable (*GWESP*). The observed indirect propinquity effect should therefore not be thought of as an artefact that would result from the general tendency to close triadic structures.

Comparing the same-ethnic coefficients from model setups 1 and 2, we get a first impression of whether or not propinquity can (partly) explain the tendency of adolescents to have same-ethnic friends. The same-ethnic effects among majority and minority adolescents between Models 1 and 2 decrease only slightly. The reduction of same-ethnic effects may be underestimated due to rescaling effects, however. Significant effects of direct and indirect neighborhood propinquity friendship imply that their inclusion to the model adds to the explained variance

of friendship. The more variance is explained, the larger the coefficients are, which in turn may mask the reduction in the same-ethnic coefficients. Therefore, I turn to simulations of same-ethnic friendship to make further inference. Throughout all simulated scenarios, I use the parameter estimates obtained from the meta-analyses of Model 2 (Tables 3.4 and 3.5) to simulate scenario-specific sets of synthetic networks.

Fig. 3.4 reports the distribution of the 250 (mean) values of  $\alpha$  for each of the scenarios (combined for the two countries). Neighborhood propinquity seems to contribute a little to ethnic homogeneity (net of group size effects) in friendships in class: The distribution of  $\alpha$  in the empirically observed scenario is slightly above the distribution following the counterfactual scenario where propinquity effects are completely absent (scenario a). This finding holds both for majority group students and for minority group students. There is a slight decrease noticeable in  $\alpha$ , but it is very small. As such, I do not find strong evidence for hypotheses 3 and 5.

It might be rather puzzling to find no effect of propinquity on same-ethnic friendship given that propinquity was found to be conducive to friendship formation in general. There are two possible explanations for this: Either same-ethnic adolescents simply do not live as close to each other as common wisdom might suggest (i.e., low levels of residential segregation) or propinquity is not such a strong driver of friendship formation when compared to the other tie generating mechanisms. Given the former applies, that is low levels of residential segregation being responsible, we should observe a rise in same-ethnic friendship if residential segregation was higher. In order to find out whether this is actually the case, I conduct yet another set of simulations based on a second counterfactual scenario that assumes extreme residential segregation. Like before, all simulations rely on the coefficient estimates from Model 2. The scenario is set up as follows. The number of schools, school sizes and actor attributes follow the empirically observed setup except for the dyadic covariate of propinquity: Here, all same-ethnic class peers are now assumed to be living close by and all outgroup class peers are not. Fig. 3.4 corroborates that propinquity has little explanatory power in same-ethnic friendship within school classes because there are simply few same-ethnic peers who live nearby. The outlined counterfactual scenario (scenario b) shows

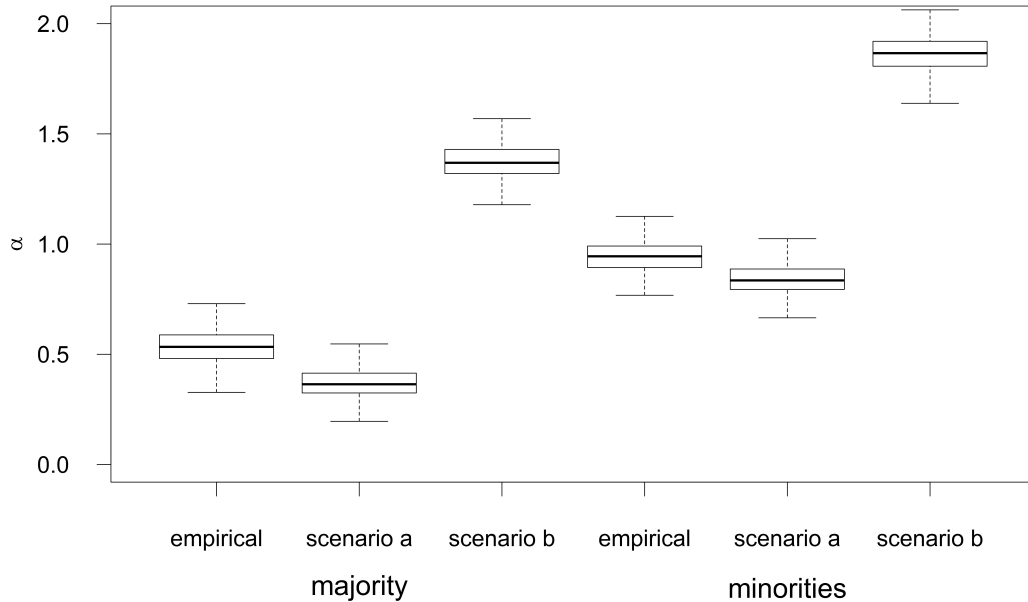


Figure 3.4: Tendency for same-ethnic-group friends in school classes ( $\alpha$ ). Simulated networks based on the empirical school population in Germany and the Netherlands; where propinquity effects are completely absent (scenario a); where all same-ethnic peers live close by (scenario b).

clearly higher levels of  $\alpha$  than the scenario that is empirically observed. This suggests that it is not the relative importance of the tie formation mechanism of propinquity as such but the rather low empirical level of ethnic segregation that is responsible for the negligible impact of propinquity on the tendency for same-ethnic friends. Note, however, that this is merely an indication, as scenarios a and b are counterfactual, thus not empirically observed. Even though the share of immigrants in a neighborhood range between 0 and 52% in Germany and 76% in the Netherlands, I do not observe as many same-ethnic peers living nearby as I simulate.

With the third model setup (Model 3) I test the neighborhood exposure mechanism (H1). I add the proportion of immigrants in ego's neighborhood to the baseline model (*prop. immig. neighb. ego*), as well as its interactions with

### 3. Testing the framework pt.I

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both being of the majority group and both being of the same minority group (*both majority\*prop. immig. neighb. ego, same minority\*prop. immig. neighb. ego*). In terms of effect directions the multivariate analyses are in line with an exposure effect following the contact hypothesis: whereas the immigrant proportion in the neighborhood affects majority homophily negatively in both countries (GE:  $-0.007 - 0.003 = -0.01$ ; NL:  $-0.001 - 0.005 = -0.006$ ), it has a positive effect on minority homophily (GE:  $-0.007 + 0.015 = 0.08$ ; NL:  $-0.001 + 0.008 = 0.007$ ). This finding contrasts the bivariate findings shown earlier in Fig. 3.3, which underlines the importance to control for alternative mechanisms of tie formation. However, the evidence that majority or minority members with varying exposure to immigrants in the neighborhood vary in the strength of ethnic homophily is very weak and marginal, as the interaction effects are not consistently significant and very small.<sup>13</sup> Hence, the results show no convincing evidence for a neighborhood exposure effect on the tendency of adolescents to befriend same-ethnic peers.

In the fourth model setup (Model 4) I conduct a combined test of both propinquity-related and preference-related mechanisms to test whether they each exert an independent effect on friendship formation. Results are in line with the models of separate tests: Direct and indirect propinquity are robust predictors of friendship within school classes but hardly explain same-ethnic friendship, and the proportion of immigrants in the neighborhood does not have a relevant effect on same-ethnic friendship. I do not find evidence for hypothesis 6 that the exposure and propinquity effects affect same-ethnic friendship independently because I find little evidence for these effects in the first place.

Lastly, I examine the between-school variance of the propinquity and exposure coefficients. It is especially important to further examine the small effect of neighborhood exposure as it may be due to exposure effects being significantly positive in some, but significantly negative in other classes so that the effects counterbalance each other. Table 3.6 shows the number of schools with significant

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<sup>13</sup>The same *minority* effect is noticeably larger in Model 1 compared to Model 3 in Germany. This reflects that the slope of the same *minority* effect is steeper in neighborhoods without immigrants than the slope of the overall *same minority* effect in Germany. Because the interaction effect is insignificant, however, I do not conclude that minority homophily depends on the share of immigrants in the neighborhood.

Table 3.6: Between-school-network variability of the exposure and propinquity effects.

	N(schools) with significant effect		N(schools)
	beta < 0	beta > 0	total
Propinquity	1	42	89
Indirect propinquity	3	34	89
Both majority*prop. immig. neighb. ego	6	1	89
Same minority*prop. immig. neighb. ego	4	5	89

*Source:* CILS4EU, w1, v1.1.0 / Microm, unweighted

positive and negative propinquity and exposure effects for all schools. It reveals that an exposure effect is rarely significant in any school and it can be either negative or positive. The propinquity effect, in contrast, is significantly positive in about half of the schools and the indirect propinquity effect in every third school.

### 3.6 Conclusion

The aim of this study was to examine whether and how neighborhoods can influence friendship choices apart from determining the mere availability of friendship possibilities. More specifically, I investigated how the neighborhood's ethnic composition is related to adolescent same-ethnic friendships in German and in Dutch school classes.

The results corroborate the previous U.S. finding that adolescents are more likely to be friends in school if they live close to each other (Mouw and Entwisle, 2006). Further, I established that this effect also applies to classmates who live close to another class friend. The results are based on data of adolescents nominating classmates who live within a 5-min distance.

It is plausible that this measure is biased toward friends being nominated as neighbors (i.e., if adolescents are not friends, they do not know if they are neighbors). However, such bias seems relatively limited as 79% of the neighbor nominations go to non-friends. Because a 5-min distance refers to a small local area and because the German and Dutch school classes in the sample are relatively small ( $\sim 20$  students), adolescents seem to know who lives close by regardless of

being friends or not. This is in line with Banerjee and colleagues' study (2014) that reported people being surprisingly accurate in identifying network characteristics (i.e., central persons in networks) above and beyond friendship ties.

The effects of direct and indirect neighborhood propinquity imply that ethnic segregation in the neighborhood has the potential to explain the tendency for same-ethnic friends: Adolescents may have so many same-ethnic friends in class, because same-ethnic peers are more likely to meet in the neighborhood than peers with a different ethnic background. With the use of simulations, however, I could show that the neighborhood propinquity effects do not lead to a much higher same-group tendency, most likely because there are too few same-ethnic adolescents within a class that live close to each other (in this case, a 5-min distance). This is in line with Mouw and Entwisle's study (2006), who also did not find that propinquity explains individual variation in same-ethnic friendship *within* schools. The propinquity effect seems to be very local, and school classes include students from larger areas.

The lack of evidence for a mediation effect of neighborhood propinquity does not mean, however, that ethnic residential segregation can be neglected: simulations suggested that under extreme residential segregation ethnic friendship homogeneity would be amplified by a factor of almost 3. Concerning the empirical cases of Germany and the Netherlands, however, the propinquity mechanism seems to be rather negligible.

Besides creating meeting opportunities with ethnic outgroup peers, outgroup exposure in the neighborhood may also dampen preferences for same-ethnic friendship. At first sight, the descriptive analyses show the opposite. Turning to multivariate analyses (that adequately account for alternative tie formation mechanisms such as triadic closure) results are actually in line with the exposure effect. However, the evidence in favor of a neighborhood exposure effect is very small and marginal. This suggests that exposure to immigrants in the neighborhood does not reduce prejudice to such an extent that native adolescents make interethnic friends like intergroup contact theory would predict (Allport, 1954). In contradiction to Vermeij et al. (2009), I find as such no convincing evidence that the exposure to outgroup members in the neighborhood weakens ethnic homophily in friendships in school classes.

One possible explanation for the lack of evidence in favor of an exposure effect may be found in two opposite mechanisms working simultaneously. It could be that some of the outgroup exposure in neighborhoods coincides with actual positive outgroup contact, whereas it leads to feelings of interethnic threat in other cases. These effects may cancel each other out, resulting in a small and irrelevant effect. Another explanation for not finding evidence for the neighborhood exposure mechanism may be that exposure has no effect at all as it is a superficial form of interethnic contact. On the school level, I found that there are few schools where neighborhood exposure has a significant effect, so I recommend future research to explore conditions that trigger contact and competition theory mechanisms on the student level. Preferably, such research can further dive into the causality of this relation using longitudinal data as well.

Returning to the book's puzzle, these findings provide important insights. They suggest that the spatially informed framework of friendship formation introduced in Chapter 1 needs substantial refinement. In its preliminary form, the framework suggested three possible pathways how neighborhood compositions translate into friendship compositions (see Figure 1.5). Now we know better. Neighborhoods do not affect actors' friendship preferences in any significant way (i.e., exposure mechanism). Neither does living close make same-group friendships more likely (i.e. propinquity mechanism), given that neighborhood segregation in Germany is too low for the mechanism to contribute to ethnic friendship homogeneity. From this perspective, the findings discard two of three potential reasons for the emergence of residential barriers and bridges—at least so for the social integration of young immigrants in Germany and the Netherlands. Figure 3.5 shows a revised version of the framework.<sup>14</sup>

Obviously, this makes life simpler for us. Instead of having to account for three different pathways at the same time, the refinement of the framework shifts all attention to the availability of outgroup friends. Focusing on this remaining mechanism will suffice to arrive at a better understanding of residential barriers and bridges.

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<sup>14</sup>I subsumed the tie formation mechanisms reciprocity and transitivity/triadic closure under *balancing* to be in line with seminal work on friendship formation (cf. Wimmer and Lewis, 2010). Note, that this classification as preference- and not an opportunity-driven mechanism is arbitrary.

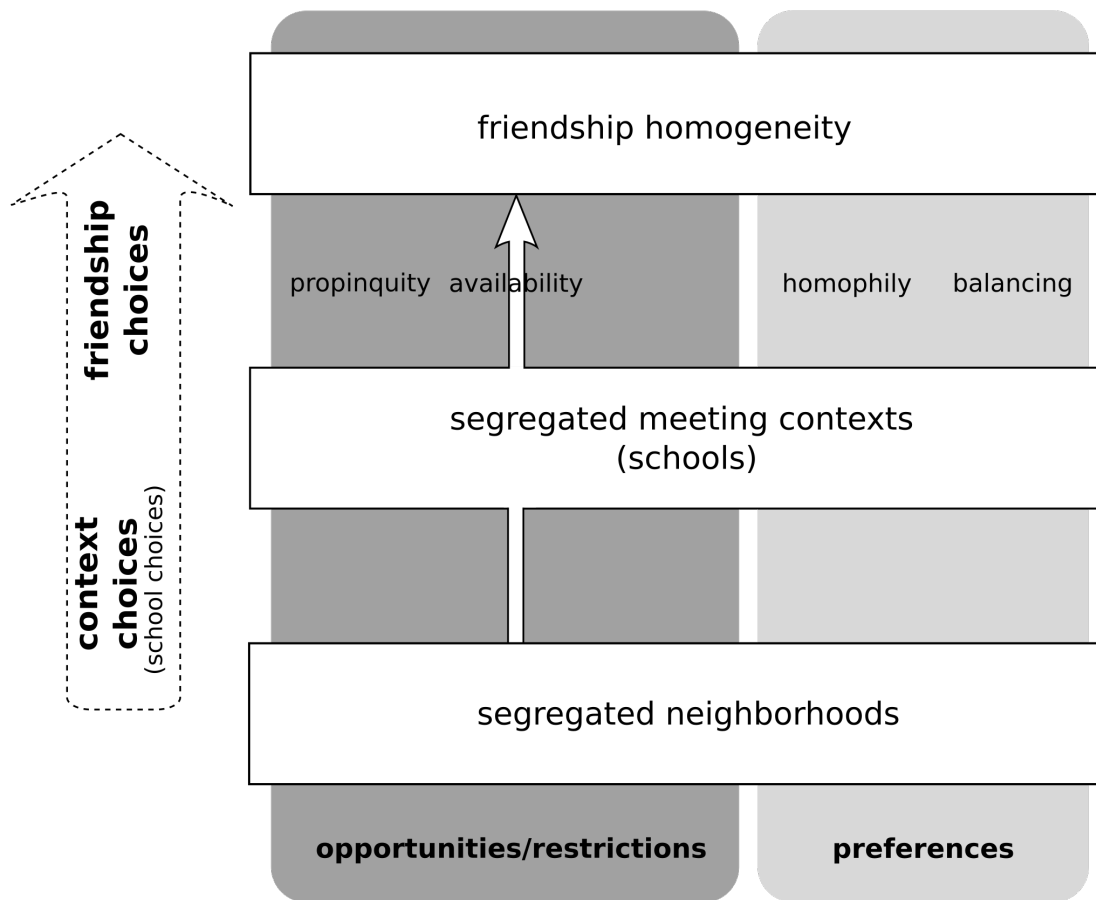


Figure 3.5: The spatially informed framework of friendship formation (revised)



However, the refined framework also raises a new question, now addressing the lower half of the remaining white arrow in Figure 3.5—the link between actors' neighborhoods and their meeting contexts: If ethnic residential segregation in Germany is too weak to affect adolescents' interethnic friendship formation via the propinquity mechanism, why is this different via the availability mechanism? Do the context choices of young immigrants and of their native peers amplify segregation patterns? And if so, how? The next chapter will tackle these questions, thereby again focusing on adolescents' most important meeting context: their schools.



## Chapter 4

Testing the framework pt.II.

Why are schools more segregated  
than neighborhoods?\*

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\*A different version of this chapter is currently under review by a peer-reviewed journal. To guarantee consistency across chapters, I have reformulated various sections.

### Abstract

Ability tracking—the assignment of students to different school types based on their prior achievement—is usually associated with increased ethnic sorting into schools. Being applied in the German secondary school system, ability tracking may hence explain why adolescents in Germany end up in meeting contexts (i.e. schools) that are ethnically more segregated than their neighborhoods. This chapter examines the role that ability tracking plays for the emergence of ethnic segregation across schools. It demonstrates that the effect of tracking is actually twofold: besides an ethnic sorting over school types, it hampers parental tendencies towards white flight. To identify the twofold effect of tracking I introduce a method based on counterfactual reasoning to decompose observed school segregation. Moreover, I exploit a unique feature of the German secondary school system: regional variation in tracking strength. Analyses rely on administrative data entailing geocoded information on all secondary schools in Germany in 2008/09. Results corroborate expectations of a twofold effect: ability tracking increases segregation via ethnically specific track sorting while at the same time decreasing it via school sorting within each track.

## 4.1 Introduction

Ethnic segregation is often stronger in schools than in the respective residential areas, a finding that holds at different educational stages in the U.S. (Bifulco et al., 2009; Saporito and Sohoni, 2006; Sohoni and Saporito, 2009) and European societies (Burgess et al., 2005; Horr, 2016; Karsten et al., 2003; Noreisch, 2007; Rangvid, 2007). Such school segregation beyond residential patterns (termed *net segregation* from here on) threatens the integration of young immigrants by steering minority and majority adolescents into separate school lives.

Net segregation can be an unintended consequence of the institutional setting of a school system: segregating school choices emerge where not all racial and ethnic groups equally meet school admission criteria (e.g. ability to pay tuition fees, fulfillment of formal qualifications). One example is the institutional rule of ability tracking—applied in school systems across the world, such as in France, Israel, Japan, Korea, Mexico, and also in Germany (OECD, 2013). Ability tracking implies a segmentation of the secondary school market into different school types, whereby track sorting is conditional on students’ prior achievement. As such, it has been argued to bear a straightforward unintended consequence for ethnic mixing in secondary school systems (Oakes, 1985; Shavit, 1984): ethnic and racial disparities in achievement lead to an ethnically specific sorting into the different tracks, with minority groups usually being overrepresented in the lower tracks. The result is increased racial and ethnic school segregation.<sup>1</sup>

However, parental school choices can also be deliberately segregating. A long line of research—mainly on the U.S. case (but see Betts and Fairlie 2003; Rangvid 2010)—established that majority parents actively avoid local schools with high minority shares (Billingham and Hunt, 2016; Goyette et al., 2012; Saporito, 2003; Saporito and Lareau, 1999; Schneider and Buckley, 2002). This avoidance tendency is often referred to as *white flight* (Fairlie and Resch, 2002; Renzulli and Evans, 2005). Recently, scholars’ interest has shifted towards the structural conditions under which white flight emerges (Fiel, 2015; Rich and Jennings, 2015).

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<sup>1</sup>In 1967, the district court decision in *Hobson vs. Hansen* was based partly on this argument, when it ordered an abolishment of ability tracking in the Washington, D.C. public school system. The decision marked a turning point for secondary schooling in the U.S., with a shift from ability tracking toward course-specific ability grouping (Lucas, 1999).

The institutional setting of a school system thereby affects parental tendencies towards white flight decisively, as revealed in numerous investigations of market-based reforms (i.e., increased choice possibilities due to the introduction of magnet and charter schools) in the U.S. school system (Bifulco et al., 2009; Logan et al., 2008; Renzulli and Evans, 2005; Saporito and Sohoni, 2006; Sohoni and Saporito, 2009).

Taking these insights as a point of departure this chapter traces the extent and origins of net segregation in German secondary schooling. As such, it puts the book's theoretical framework to a second empirical test, now focusing on young immigrants' first decision; the choice of a meeting/school context. Given Germany's institutional setting the chapter therefore revisits the role of ability tracking in the emergence of net segregation.

The chapter's central claim is the following: the effect of ability tracking on net segregation is twofold. It increases net segregation via ethnically specific track sorting caused by ethnic disparities in achievement. At the same time, however, ability tracking curbs school—avoidance tendencies within the tracks, given the less frequent exposure of parents with white-flight preferences to schools with high immigrant shares. The result is an additional, decreasing effect on net segregation.

Two characteristics of the German secondary school system make the existence of a twofold tracking effect likely. First, the German secondary school system applies a strict form of ability tracking. Second, parental school choices in Germany are independent of their place of residence. The absence of legal attendance zones in the German secondary school system leaves room for avoidance tendencies to emerge, rendering possible a moderating effect of ability tracking.

To identify the effect of ability tracking on net segregation I exploit the fact that the strength of ability tracking varies substantially across German districts, partly because education falls within the sovereignty of the sixteen German federal states rather than that of the national authority. Relying on within-country variation instead of on a cross-country comparison allows me to hold other aspects of the data constant (e.g., ethnic background of majority and main minority groups, process of data collection). All analyses rely on unique administrative

data containing geocoded information on all secondary schools in Germany in the school year 2008/09.

Beside substantiating the book's theoretical framework this chapter makes three contributions. First, it is the first to investigate explicitly the role of ability tracking in ethnic segregation in secondary schooling (but see Gramberg 1998), thereby showing that the effect is twofold. Second, it introduces a straightforward method based on counterfactual reasoning in order to decompose observed school segregation into segregation due to track sorting and due to sorting within the tracks. Finally, it participates in the more general discussion on parental school choices, more specifically, it explores the question of how the institutional setup of a school system may shape ethnically specific school sorting.

The remainder of the chapter consists of five sections. Section 4.2 discusses the causes of ethnic school segregation usually brought forward. Further, it theorizes about the role of ability tracking and applies these ideas to the case of the German secondary school system. Section 4.3 lays out the analytical approach to putting these claims to an empirical test. Section 4.4 introduces the data on which the analyses rely. Section 4.5 presents the results of the analysis. The paper closes with a final summary and discussion of the results in section 4.6.

## 4.2 Theory

### Ethnic segregation in schools

Ethnic segregation in schools—in terms of an uneven distribution of ethnic groups across schools in a region—is a common phenomenon throughout most Western societies (Karsten, 2010). Students' school enrollment results from parental school choices. Different theoretical accounts exist that aim to explain parental school choices (Berends and Zottola, 2009). This chapter follows the general action-theoretical approach introduced in the book's theoretical framework, assuming the existence of two main determinants: the *restrictions* parents face in choosing a school for their children and their *preferences* for specific schools.

The most direct reason for the emergence of ethnic segregation in schools is an uneven distribution of ethnic groups across local neighborhoods. *Neighborhood*

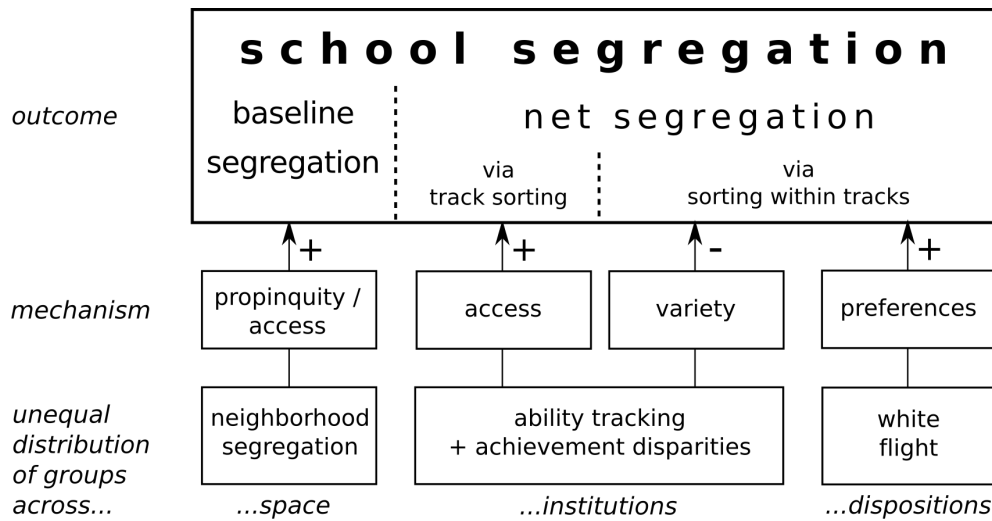


Figure 4.1: Assumed mechanisms leading to ethnic segregation in secondary schooling

*segregation* is a globally observable phenomenon (Iceland et al., 2002; Musterd, 2005). It affects the extent of school segregation via two different restriction-mediated mechanisms. The first, and universal, one is the *propinquity* mechanism. Parents' choice-alternatives are usually restricted to schools located a short distance from their homes, even if they are free to choose more distant ones (Burgess et al., 2004; Karsten et al., 2003). This is for practical reasons, for example, home-to-school distances that become unbearable on a daily basis. In addition, a less universal, restriction-mediated mechanism may be at work, the *access* mechanism. Access to schools is determined by the institutional setting of the school system and as such is rather country-specific. In countries like the U.S., Canada, or Finland, school catchment areas restrict parental school choices locally (OECD, 2013). In other words, school choice-alternatives are contingent upon parents' place of residence. Both mechanisms suggest that ethnic segregation in neighborhoods translates automatically into segregation across schools. I refer to the extent of school segregation due solely to residential patterns as *baseline segregation* from here on. The left-hand side of Figure 4.1 summarizes this outlined pathway.<sup>2</sup>

<sup>2</sup>Figure 4.1 is based loosely on the theoretical visualization in Wimmer and Lewis (2010).



### Net segregation in schools

The observed levels of school segregation, however, usually exceed the baseline, following residential patterns. In other words, net segregation is the rule rather than an exception (U.S.: Bifulco et al. 2009; Saporito and Sohoni 2006; Sohoni and Saporito 2009; Europe: Burgess et al. 2005; Horr 2016; Karsten et al. 2003; Noreisch 2007; Rangvid 2007). The most prominent reason for net segregation is parental white flight from local schools, a preference-mediated mechanism (Fairlie and Resch, 2002; Renzulli and Evans, 2005): Various school characteristics can be decisive in the course of parental school choices. Depending on the explicit form of the school system parents may face a choice between public versus private schools, state versus denominational schools, etc. In addition, schools that might be formally identical, like two public schools, are likely to differ in further informal characteristics, such as learning-related resources (i.e., teacher-student ratios or extra-curricular offers), reputation as being a good or a bad school, or composition of the student body. Concerning the latter, parents often face a choice between schools of different ethnic and social composition. A long line of research argues that majority parents tend to avoid schools with high minority shares, either because they associate them with unresourceful learning environments (Wells and Crain, 1992) or due to explicit ethnic bias (Billingham and Hunt, 2016). Regardless of whether or not the avoidance is explicit, it increases net segregation in schools (see Figure 4.1, right-hand side).<sup>3</sup>

### The twofold effect of ability tracking

Depending on the institutional setup of an educational system, there can be further access-related causes of net segregation. One such institutional cause is the existence of *ability tracking in combination with ethnic disparities in achievement*.<sup>4</sup> Ability tracking can come in various forms and depending on the national

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<sup>3</sup>In addition, a preference for ethnically similar schoolmates—on the part of both immigrants and natives—may amplify this avoidance tendency further (Bifulco and Ladd, 2007; Denessen et al., 2005; McPherson et al., 2001).

<sup>4</sup>Another institutional rule affecting the access to schools and thus net segregation pertains to tuition fees for private schools. Given that the focus of this article is on German secondary schooling—where tuition fees do not play any important role—I will not elaborate this point further.

context, students may encounter this institutional rule at different stages in their school careers. Tracking causes a segmentation of the school market, introducing a differentiation into school types specifically tailored to students of varying ability levels. Students' ability thereby is usually evaluated based on test results or teacher evaluations of their prior school performance (Lucas, 1999). Since the track types usually offer different levels of formal qualification (i.e., vocational versus academic tracks), the assignment to a track can mark a decisive point in an adolescent's life.

To date, the effect of ability tracking on overall net segregation has been rather uncontroversial: as immigrant students tend to show lower achievement levels than natives (Heath and Brinbaum, 2014), track sorting is not only ability- but also ethnically specific. Despite potentially even higher educational aspirations (Salikutluk, 2016), immigrants are therefore more likely to attend low-track schools than are natives. Ability tracking thus leads to increased net segregation via ethnically specific track sorting (see Figure 4.1, center left).

However, this may not be the only effect that ability tracking exerts on net segregation. Previous research investigating the consequences of school policy reforms has repeatedly shown that a change in institutional restrictions may affect parental avoidance tendencies decisively. For example, recent increases in local school choice options led to changes in both ethnic and social segregation across schools in the U.S. (Bifulco et al., 2009; Logan et al., 2008; Renzulli and Evans, 2005; Rich and Jennings, 2015). I argue that similar arguments apply to the extent of ability tracking deployed in a school system.

The reason is the restriction-driven *variety* mechanism (see Figure 4.1, center right): due to ethnic disparities in achievement, schools with high immigrant proportions become less prevalent in the higher tracks and more prevalent in the lower tracks. In other words, the within-track variety in schools' ethnic composition decreases. Thus, parental avoidance of local (immigrant-dominated) schools is less often evoked in the higher tracks. At the same time, however, white flight from low-track schools does not necessarily become more prevalent, given that low-SES parents—being less at risk of avoiding schools (Phillips et al., 2012; Sikkink and Emerson, 2008)—are clearly overrepresented here. From this perspective, ability tracking leads to a situation where those parents most at risk

of avoiding immigrant-dominated schools (i.e., high-SES parents) are less often exposed to schools evoking such avoidance behavior (i.e., schools with high immigrant shares). Consequentially, ability tracking hampers parental avoidance tendencies, thus additionally exerting a decreasing effect on net segregation via sorting within the tracks.

### The case of German secondary schooling

Germany deploys a strict form of ability tracking (Lucas, 1999; Müller and Kogan, 2010). Generally, its tracking system consists of three track types: two vocational tracks—low- and intermediate-track schools (i.e., *Hauptschule* and *Realschule*)—as well as an academic high track directly qualifying students for higher tertiary education (i.e., *Gymnasium*). Usually students enter the tracking system after grade four upon completion of primary school (around the age of 11), with their track choice being conditional upon teacher recommendations at primary school.

The exact institutional setup of the tracking system varies to some degree across the sixteen German federal states. One important difference is that many federal states offer a comprehensive school type as an alternative to the classical three-tier tracking system.<sup>5</sup> Such comprehensive schools usually entail two or more track types under the same roof and as such are more similar to secondary schools in the U.S. or the U.K., where ability grouping takes place within schools only (Lucas, 1999). In other words, the strength of ability tracking varies within the German secondary school system. Moreover, this variation is present not only across federal states but even within them, providing leverage to identify tracking's (twofold) effect on net segregation.

Several reasons make the existence of a twofold effect of tracking in German secondary schooling likely. First, achievement disparities between native and immigrant students in Germany are substantial, with the latter group being in a disadvantaged position. Immigrants therefore are more likely to attend lower-track-schools than natives are (Kristen and Granato, 2007). Second, secondary school choices are not geographically bounded by school catchment areas (Hofman

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<sup>5</sup>Moreover, the age at which students enter the tracking system and the degree to which the teacher recommendation for a specific track is binding differ from state to state.

et al., 2004). Parents are thus free to choose a school within the track segment appropriate for their child. This leaves room for avoidance tendencies and for a moderating effect of ability tracking to emerge. Taken together, the German secondary school system provides fertile ground for the emergence of a twofold effect of tracking on net segregation. Finding no indication of a twofold tracking effect in Germany would thus be a strong indication of its nonexistence.

### 4.3 Analytical approach

To identify the twofold effect of ability tracking, the conducted analyses proceed in four steps. The first determines the extent of net segregation in Germany. The second step identifies the overall effect of tracking on net segregation in German secondary schooling, thereby relying on regional differences in tracking strength. Part three of the analyses then tests whether the tracking effect is twofold. The last part of the analyses provides a set of robustness checks to rule out a number of alternative explanations for the observed patterns. This section describes the first three analytical steps in greater detail. The robustness checks will be laid out on the go.

#### Deriving net segregation

Segregation is a multidimensional concept (Massey and Denton, 1988). Investigating how tracking leads to an uneven distribution of ethnic groups across schools, this paper relies on a measure of unevenness, the widely applied dissimilarity index  $D$  (ibid.).<sup>6</sup> In this chapter, I derive  $D$  on the geographical scale of German administrative districts, given that distances between schools in a district are still close enough to render them potential alternative choices. I determine  $D$  cohort-specifically within each district, thus ruling out any unevenness that is due to compositional differences over time (in the ethnic composition of student cohorts and grade sizes). Cohort-specific districts are therefore the unit of analysis (see example below).

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<sup>6</sup>Additional analyses relying on Theil's Information Theory Index yield substantially identical results (results not shown here).

I define net segregation in a cohort-specific district  $i$  as the difference between observed unevenness in  $i$  and a counterfactual level of unevenness emerging if all students in  $i$  attended the school closest to their home; in other words, under a scenario where unevenness strictly follows residential patterns (i.e., the baseline scenario), formally

$$D_{net_i} = D_{actual_i} - D_{baseline_i}. \quad (4.1)$$

To take a hypothetical example, assume that local cohort  $i$  attends seventh grade in a district as depicted in Figure 4.2, with three low-track and two high-track schools of equal sizes. The numbers below each school indicate the number of native (left) and immigrant (right) students in each seventh-grade. Overall, the district entails 500 seventh-graders, 200 of whom are immigrants (i.e., 40%). An even distribution of natives and immigrants across schools would imply that all seventh-grades in the district show the same immigrant proportion of 40%. In the example, however, schools deviate from this even distribution. In other words, schools in  $i$  are ethnically segregated, which is reflected in a dissimilarity index that is larger than zero. More specifically,  $D_{actual_i} = .42$ , which indicates that in order to arrive at an even distribution of immigrant and native seventh-graders across schools 42% of all immigrant seventh-graders in the district would have to attend a different school (cf. Massey and Denton, 1988).

How much of this school segregation in cohort-specific district  $i$  is due to residential segregation and should therefore be subtracted in order to arrive at the level of net segregation among seventh-graders in the district? To see this, consider the counterfactual baseline scenario: taking the empirical ethnic compositions of the 11 neighborhoods in  $i$  as given (values not shown in Figure 4.2), as well as the observed school locations and the grade sizes at each school, I construct counterfactual compositions for all five seventh-grades in  $i$ , assuming that students always attend their locally closest school (regardless of whether it is a low- or a high-track school). Consequentially, the five resulting grade compositions in the baseline scenario directly mirror those of the areas surrounding them. Given that Germany has no fixed catchment areas, I define two parsimonious, yet reasonable, rules to assign all 11 neighborhoods in  $i$  to an appropriate school: First, assign each neighborhood to that school whose location is closest

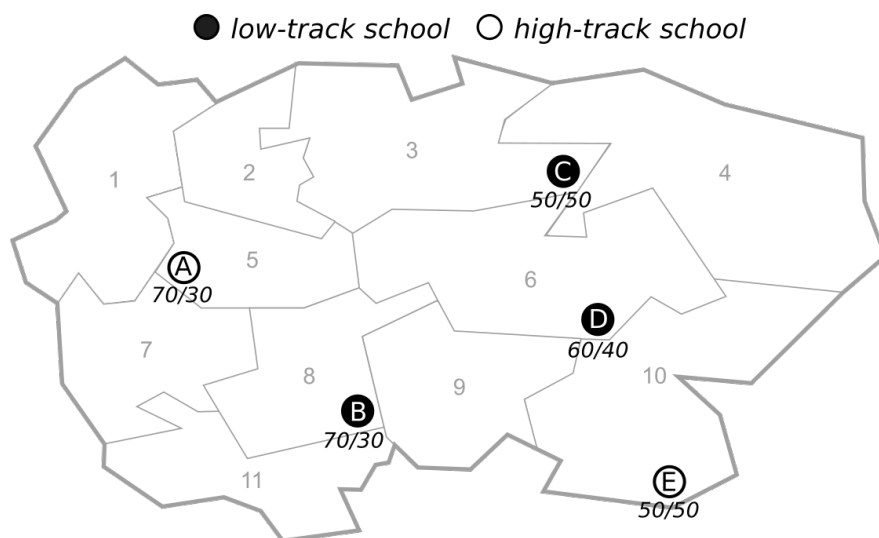


Figure 4.2: The distribution of seventh-graders across schools in a hypothetical district

to its geometric center (the latter being indicated by the position of the neighborhood’s number in Figure 4.2): neighborhoods 1, 2, 5, and 7 are thus assigned to school A, neighborhoods 8, 9, and 11 to school B, et cetera. Doing so may leave a number of schools without any assigned neighborhoods, as is the case for school E in the example.<sup>7</sup> As a second rule, I therefore additionally assign to each school that neighborhood whose geometric center is the closest: thus, neighborhood 10 is additionally assigned to school E, neighborhood 6 is assigned to schools C and D, et cetera. Applying both these rules to all schools and neighborhoods in  $i$  leads to clearly defined *as-if catchment* areas for all five schools (A: 1, 2, 5, 7; B: 8, 9, 11; C: 3, 4, 6; D: 6, 10; E: 10).<sup>8</sup> Their ethnic compositions (i.e., population-weighted averages of the pertinent neighborhoods) yield each school’s seventh-grade counterfactual composition, with which we can derive the extent of baseline segregation in  $i$ ,  $D_{baseline_i}$ . Subtracting the latter from the observed

<sup>7</sup>Note that whereas segregation is measured on the district level, the assignment of as-if catchment areas does not stop at district boundaries, as the example might suggest. Instead, the as-if catchment area of school E would most likely cross district borders, with an additional neighborhood located outside of the district. The analyses account for such cases. For reasons of simplicity, I did not address this further in the outlined example.

<sup>8</sup>Note that these as-if catchment areas may overlap—as is the case for schools C and D, as well as for schools D and E. Given that there are no fixed legal catchment areas, this seems well likely.

segregation in  $i$  ( $D_{actual_i}$ ) finally yields the extent of net segregation present in the cohort of seventh-graders in the district ( $D_{net_i}$ ).

### Identification of the overall tracking effect on net segregation

To identify the effect of tracking on net segregation the analyses rely on cohort-specific regional variation in tracking strength throughout Germany. Local student cohorts differ in the strength to which ability tracking is enforced upon them. One central difference is the number of places available at comprehensive schools. Comprehensive schools serve as an alternative to the tracking system, as they combine the different track levels within one school type, and thus accept students regardless of their prior achievement. Whereas there are some cohort-specific districts where comprehensive schools are completely absent (i.e., strong tracking), there are others where most students attend a comprehensive school (i.e., weak tracking). In other words, the percentage of students enrolled in the actual tracking system in cohort-specific district  $i$  serves as a measure of tracking strength,  $t_i$ .

To derive the overall tracking effect I regress the levels of net segregation ( $D_{net_i}$ ) within cohort-specific district  $i$  on the tracking strength  $t_i$ . In contrast to a comparative analysis of different national educational systems, the analysis of variation within the same country allows me to hold a number of decisive attributes constant across cases (e.g., ethnicities of majority and main minority groups, spatial units, process of data collection). Nevertheless, there are also a number of potential confounders. Some of them are directly observable: the proportion of native students in  $i$ , the proportion of students at private schools in  $i$ , as well as the number of schools in  $i$  from which to choose have all been argued and shown to increase the extent of ethnic school segregation in a region (Fiel, 2015; Logan et al., 2008). Even though there are no direct arguments why these attributes would also correlate with the tracking strength present in a cohort-specific district, they are included as controls in the model. The most notable unobserved confounders are policy differences (beyond tracking strength) present across federal states and cohorts within states, given that education in Germany

falls within the sovereignty of the sixteen German federal states. To account for this unobserved heterogeneity I apply cohort-specific state fixed effect regression, yielding

$$D_{net_i} - \bar{D}_{net_{cs}} = \beta_{net}(t_i - \bar{t}_{cs}) + \gamma_{net}(X_i - \bar{X}_{cs}) + \varepsilon_i, \quad (4.2)$$

where  $\bar{D}_{net_{cs}} = E[D_{net_i}|c(i) = c, s(i) = s]$  is the mean net segregation of cohort  $c$  in federal state  $s$ ,  $\bar{t}_{cs} = E[t_i|c(i) = c, s(i) = s]$  is the mean percentage of students enrolled in the tracking system of cohort  $c$  in federal state  $s$ , and  $\bar{X}_{cs} = E[X_i|c(i) = c, s(i) = s]$  is a vector entailing the means of the observed confounding attributes of cohort  $c$  in federal state  $s$  mentioned before. Parameter  $\beta_{net}$  is the overall tracking effect to be estimated,  $\gamma_{net}$  is a vector of confounder parameters to be estimated, and  $\varepsilon_i$  is the fixed effect residual error component of  $i$ . Due to the fixed-effects approach all estimates rely solely on variation present within cohort-specific states. In combination with the set of additional controls this yields an appropriate estimate of  $\beta_{net}$ .

### Identification of the twofold tracking effect on net segregation

To test for the twofold role of tracking I introduce an approach to decompose net segregation in  $i$  into two parts: one that is due to ethnically specific track sorting ( $D_{tracksorting_i}$ ) and a second, residual, part that can be attributed to school sorting *within* each track ( $D_{schoolsorting_i}$ ), formally

$$D_{net_i} = D_{tracksorting_i} + D_{schoolsorting_i}. \quad (4.3)$$

Applying the fixed effect regression from eq. 4.2 to both parts of net segregation separately provides a direct test of whether the role of ability tracking is twofold—whether we see an increasing effect via ethnically specific track sorting and a decreasing effect via school sorting within tracks. Based on the two regression models

$$D_{tracksorting_i} - \bar{D}_{tracksorting_{cs}} = \beta_{tracksorting}(t_i - \bar{t}_{cs}) + \gamma_{tracksorting}(X_i - \bar{X}_{cs}) + \varepsilon_i, \quad (4.4)$$



$$D_{schoolsorting_i} - \bar{D}_{schoolsorting_{cs}} = \beta_{schoolsorting}(t_i - \bar{t}_{cs}) + \gamma_{schoolsorting}(X_i - \bar{X}_{cs}) + \varepsilon_i, \quad (4.5)$$

the test is whether  $\hat{\beta}_{tracksorting} > 0$  and  $\hat{\beta}_{schoolsorting} < 0$  really holds.

To arrive at the decomposed values of unevenness, I construct for all cohort-specific districts a second counterfactual scenario which is similar to the baseline scenario outlined before, except that all students are now assumed to attend their locally closest school whose track type is in line with their prior achievement (i.e., *tracking scenario*)<sup>9</sup>. In other words, segregation now emerges due to residential patterns and due to ethnically specific attendance in the different tracks. Returning to the example in Figure 4.2, let us first inspect how to construct the counterfactual grade compositions in the two high-track schools A and E. The assumption now is that high-track seventh-graders always attend the high-track school closest to them. Applying the two assignment rules splits the 11 neighborhoods into two as-if catchment areas (A: 1, 2, 3, 5, 7, 8, 11; E: 4, 6, 9, 10). However, the ethnic compositions of these two areas do not directly yield the two counterfactual grade compositions. Instead, the local high-track attendance rates of immigrants and natives have to be accounted for (see Appendix IV). I approximate these rates specifically for each school as the proportion of high-track students among all immigrants/natives who attend this school or one maximally 2 km away. The same logic can be applied to the three low-track schools in  $i$ , such that we end up with seventh-grade counterfactual compositions of all five schools. Based on these I derive the extent of unevenness according to the tracking scenario, formally  $D_{tracking_i}$ .

Based on the tracking scenario a decomposition of net segregation becomes possible: Whereas under the baseline scenario all observed unevenness results from residential patterns, the tracking scenario additionally takes into account ethnically specific track choices. The difference between the two counterfactual levels of unevenness thus yields that part of net segregation in  $i$  that is due to ethnically specific track choices via the access mechanism, formally

$$D_{tracksorting_i} = D_{tracking_i} - D_{baseline_i}. \quad (4.6)$$

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<sup>9</sup>If a comprehensive school is closer than a school of the appropriate school type, it is assumed that students attend that comprehensive school.

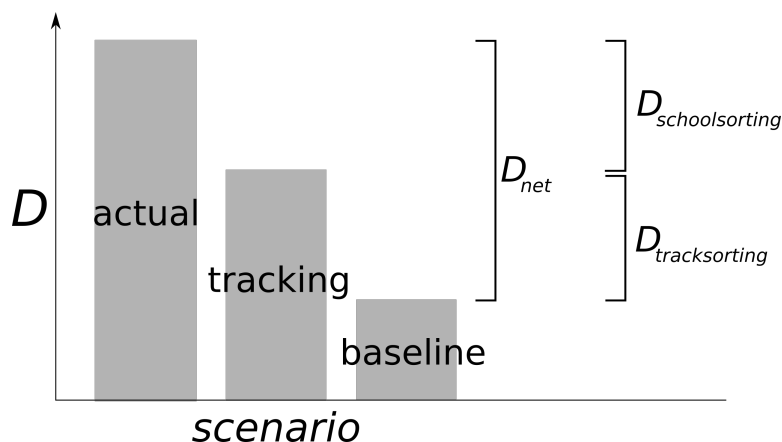


Figure 4.3: Overview of the observed, counterfactual and decomposed forms of school segregation

Similarly, the difference between the actual, observed unevenness ( $D_{actual_i}$ ) and that in the tracking scenario ( $D_{tracking_i}$ ) constitutes that part of net segregation in  $i$  that can neither be attributed to ethnically specific track choices nor to residential patterns. Consequentially, it can be attributed to parental school choices *within* the tracks, formally

$$D_{schoolsorting_i} = D_{actual_i} - D_{tracking_i}. \quad (4.7)$$

As an overview, Figure 4.3 summarizes the different forms of segregation and their relation to each other. All analyses are executed in R (v.3.2.3).

## 4.4 Data and Variables

### Data

Measuring ethnic segregation in grade-specific districts requires information about the distribution of a complete student cohort across all schools within these districts. To gather this information for all districts in Germany, I combined restricted-access administrative data provided by courtesy of all sixteen state-specific statistical offices (*Landesämter für Statistik*). The data contain information on the number of natives and non-natives in three student cohorts, attending

grades 7-9 in all German secondary schools in the school year 2008/09, amounting to  $\sim 33,000$  grade compositions in  $\sim 12,000$  schools across more than 400 districts. The sample of analysis had to be restricted in a number of ways. First, I dropped all *schools for special needs*, given that this school type is neither part of the tracking system nor a feasible alternative choice. Second, I excluded all schools/grades located in districts where no appropriate neighborhood information was available to infer the as-if catchment areas. Finally, I excluded all schools/grades in districts where the hypothetical minimum value of  $D_{actual}$  was greater than zero.<sup>10</sup> Table 4.1 provides an overview of the resulting sample of analysis and compares it to the complete school sample. The number of administrative districts entering the analyses is 182. Given that rural districts often contain very low numbers of immigrant students in Germany, more rural than urban districts were excluded (see third and fourth row of Table 4.1). Moreover, the exclusion of districts with a hypothetical minimum of  $D_{actual} > 0$  leads to a lower immigrant percentage in the sample of analysis (see last row of Table 4.1).

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<sup>10</sup>This could be the case in districts with very few schools and/or only few immigrant students, such that an even distribution of ethnic groups across schools is logically not possible (cf. Taeuber and Taeuber, 1976).

Table 4.1: Comparison between the complete German sample and the sample of analysis

	Complete German sample				Sample of analysis			
	N	Rel. freq.	Student population mean	s.d.	N	Rel. freq.	Student population mean	s.d.
Federal states	16		145,800	161,239	14		85,430	136,021
Administrative districts								
<i>Urban (pop.&gt;100k)</i>	413	1.00	5,649	5,633	182	1.00	6,571	5,565
<i>Rural (pop.&lt;100k)</i>	76	.18	8,534	10,008	66	.36	7,641	7,118
	346	.84	4,868	4,868	122	.67	5,669	4,045
Schools (7th-9th grade)								
<i>Low-track</i>	11,504	1.00	203	128	6,026	1.00	199	127
<i>Intermediate-track</i>	4,077	.35	115	65	2,203	.37	111	66
<i>High-track</i>	2,505	.22	249	111	1,474	.24	223	109
<i>Comprehensive</i>	2,981	.26	285	112	1,688	.28	260	118
	1,941	.17	203	152	661	.11	277	277
Grades								
<i>7th</i>	33,449	1.00	70	43	15,143	1.00	79	43
<i>8th</i>	11,082	.33	70	44	4,810	.32	79	45
<i>9th</i>	11,101	.33	69	42	5,087	.34	77	42
	11,266	.34	70	42	5,246	.35	81	42
Students								
<i>Native</i>	2,331,910	1.00			1,195,972	1.00		
<i>Immigrants</i>	2,112,885	.91			1,040,820	.87		
	220,025	.09			155,152	.13		

Source: Statistische Ämter des Bundes und der Länder

To construct the as-if catchment areas for each school in the sample of analysis—as outlined in the previous section—I rely on information about the composition of all neighborhoods throughout Germany. Similar to the schools, this information stems from administrative data from both federal and local authorities. In rural districts, neighborhoods are defined on the municipality level. The resulting average population of a rural neighborhood is 2,977. All information on the municipality level stems from census data from federal statistics in 2011, the closest time point available (Statistisches Bundesamt, 2014). In more-urban districts, however, municipalities take on sizes too large to still be used as neighborhoods. For districts with more than 100,000 residents ( $N=76$ ) I therefore opted for neighborhood information on a finer spatial scale, gathered from local statistics of all these districts from 2008/09. The resulting average population in an urban neighborhood is 6,063.

Table 4.2 summarizes the sizes of the resulting as-if catchment areas school-type-specifically. Note, the four school types—low-, intermediate-, and high-track, and comprehensive—are neither equally prevalent nor of equal size. Whereas low-track schools are most frequent in the analysis sample, the largest number of students is present in high-track schools. However, the sizes of the as-if catchment areas according to the baseline scenario do not differ across school types (except for comprehensive schools). Each area consists on average of about 1.5 neighborhoods, covering a population between 20,000-24,000 residents. Not surprisingly, things are different in the tracking scenario, as here the construction of as-if catchment areas is school-type-specific. The more prevalent a school type is, the smaller is its as-if catchment area. The average population size covered within an as-if catchment area ranges between about 24,000 (low-track schools) and 86,000 (comprehensive schools).

Table 4.2: As-if catchment areas of the two counterfactual scenarios across school types

	Low-track schools	Interm.-track schools	High-track schools	Comprehensive schools	All schools
N(schools)	2,203	1,474	1,688	661	6,026
N(grades)	5,495	3,725	4,311	1,612	15,143
N(students)	245,401	329,220	438,399	182,952	1,195,972
As-if catchment areas in Baseline scenario					
<i>Neighborhoods per area</i>	1.5 (1.4)	1.5 (1.3)	1.6 (1.6)	2.5 (2.8)	1.6 (1.7)
<i>Population per area</i>	20,420 (19,679)	24,020 (22,506)	25,080 (22,100)	23,920 (21,339)	23,000 (21,367)
As-if catchment areas in Tracking scenario					
<i>Neighborhoods per area</i>	2.3 (2.5)	3.3 (3.5)	3.1 (3.3)	11.9 (63.3)	3.8 (21.)
<i>Population per area</i>	23,810 (19,986)	32,850 (22,316)	32,460 (22,157)	85,660 (350,091)	35,080 (117,340)

Source: Statistische Ämter des Bundes, der Länder und der Kommunen

## Variables

Table 4.3 provides a cohort-specific overview of all variables included in the regression models. Cohort-specific districts are the observational unit. All three cohorts are represented equally in the sample of analysis, as the numbers of cohort-specific districts suggest. Further, no systematic differences exist between the cohorts regarding any of the variables' mean values. The first rows of the table inform about the three different dependent variables of the regression models (i.e., eqs. 4.2, 4.4, and 4.5), their mean values suggesting that eq. 4.3 holds: on average, the extent of net segregation equals the sum of net segregation via track sorting and via school sorting within tracks. The central independent variable, *trackingstrength*, shows that comprehensive schools make up a small part of the German school market (as already suggested in Table 4.2). An average German district—regardless of the cohort inspected—entails about 84 % students who are enrolled in the tracking system (i.e., attending a low-, intermediate-, or high-track school). However, this percentage varies substantially across cohort-specific districts, as the variable's standard deviation suggests. Additional control variables are the proportion of students attending a private school, the proportion of natives among students, and the number of schools in the cohort-specific district. The total number of cohort-specific districts to be analyzed amounts to  $N=458$ .

## 4.5 Results

### Net segregation

Figure 4.4 describes the relation between observed ( $D_{actual}$ , see y-axis) and baseline segregation across schools ( $D_{baseline}$ , see x-axis). The observed segregation varies between .25 and .79 across cohort-specific districts (each one represented by a grey circle), with a mean value of .43. In order to arrive at an even distribution across schools, on average, more than 40 % of all immigrant students in a cohort in a district would have to change their school. All cases are located above the dotted bisecting line, indicating that observed school segregation exceeds the level of segregation that would emerge from residential patterns only. In other words, we observe positive levels of net segregation in all cohort-specific districts

## 4. Testing the framework pt.II

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Table 4.3: Mean values of variables used in the regression analyses (cohort-specific, s.d. in parentheses)

	Grade			Overall
	7th	8th	9th	
Net segregation ( $D_{net}$ )	.240 (.082)	.240 (.073)	.241 (.089)	.240 (.081)
School sorting segregation ( $D_{schoolsorting}$ )	.063 (.06)	.057 (.056)	.060 (.071)	.060 (.063)
Track sorting segregation ( $D_{tracksorting}$ )	.177 (.074)	.183 (.077)	.181 (.079)	.180 (.077)
Prop. in tracking system ( $trackingstrength$ )	.848 (.165)	.845 (.171)	.832 (.182)	.842 (.173)
Prop. attending private school	.080 (.072)	.078 (.073)	.072 (.068)	.077 (.071)
Prop. natives among students	.880 (.059)	.883 (.061)	.887 (.06)	.883 (.06)
Number of schools (in 10)	3.460 (2.842)	3.220 (1.931)	3.258 (2.61)	3.306 (2.473)
N(cohort-specific districts)	139	158	161	458

*Source:* Statistische Ämter des Bundes, der Länder und der Kommunen



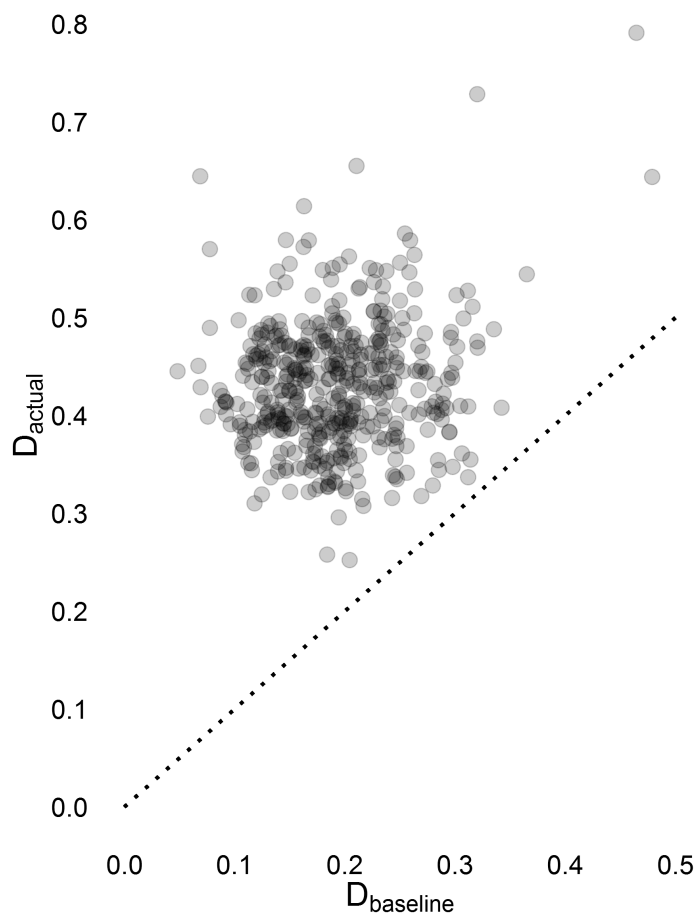


Figure 4.4: The relation between observed ( $D_{actual}$ ) and baseline school segregation ( $D_{baseline}$ )

in the sample of analysis. The values vary between .03 and .57, the average level of net segregation being .24 (see also Table 4.3).

### The overall effect of ability tracking

To learn about the overall effect of ability tracking on net segregation, turn to the model estimates provided in Table 4.4. The first model setup, M1, informs about the plain bivariate relation between *trackingstrength* and  $D_{net}$ . It shows that a higher percentage of students in the tracking system implies higher levels of net segregation. On average, net segregation is about .19 points higher in a

Table 4.4: Estimated overall effect of ability tracking (dep. var.:  $D_{net}$ )

	M1		M2		M3	
	coef	s.e.	coef	s.e.	coef	s.e.
Effect of <i>trackingstrength</i>	0.186	0.034 ***	0.172	0.034 ***	0.126	0.056 **
Controls	No		Yes		Yes	
Cohort-specific state fixed effects	No		No		Yes	
Adj. R-squared	.15		.23		.15	
N(cohort-specific districts)	458		458		458	

Source: Statistische Ämter des Bundes, der Länder und der Kommunen

Note: \*  $p < .1$  \*\*  $p < .05$  \*\*\*  $p < .01$ . All standard errors are cluster-corrected. See Table A.4 in Appendix V.

cohort-specific district deploying ability tracking (i.e.,  $trackingstrength=1$ ) than in one where all students attend comprehensive schools (i.e.,  $trackingstrength=0$ ). Controlling for the observable potential confounders leads only to a small decrease in the association (see M2). However, the inclusion of cohort-specific state fixed effects leads to a somewhat larger drop in the size of the estimate: model setup 3 suggests that a cohort-specific district deploying ability tracking shows levels of net segregation that are, on average, .126 points higher than for a cohort-specific track entailing only comprehensive schools. This estimate is subject to stronger uncertainty (see standard errors), as it is based solely on within-cohort variation within federal states. These results suggest that ability tracking accounts for about 50 % of net segregation in an average cohort-specific German district (i.e.,  $.126/.24=.525$ ).

## The twofold effect of ability tracking

Next, we turn to the question whether the effect of ability tracking is twofold. The analyses now distinguish between net segregation via track sorting as a first dependent variable and net segregation via school sorting within the tracks as a second one (i.e., models according to eqs. 4.4 and 4.5). Turning first to the plain relation between *trackingstrength* and  $D_{tracksorting}$ , we see a strong, positive association (see Table 4.5): the stronger the tracking in a cohort-specific district is, the stronger the extent of net segregation via track sorting is. Intriguingly, however, the association between *trackingstrength* and  $D_{schoolsorting}$  is negative,

implying that stronger tracking implies less net segregation via school sorting within tracks. Accounting for potential confounders does not alter this image (see M2), except that the association of tracking strength and  $D_{schoolsorting}$  is slightly more negative. The association with  $D_{tracksorting}$  remains unchanged, which is in line with expectations, as it is argued that the confounders affect school choices within tracks but do not affect tracking choices. Finally, M3 accounts for cohort-specific state fixed effects, yielding the final estimates of the twofold tracking effect: ethnically specific sorting into tracks implies a rise in net segregation by  $\sim .23$  points. School sorting within the tracks, however, buffers this increase to some extent, as it implies a decrease in net segregation by  $\sim .11$  points.

Table 4.5: Estimated twofold effect of ability tracking (dep. vars.:  $D_{tracksorting}$ / $D_{schoolsorting}$ )

	M1		M2		M3													
	$D_{tracksorting}$	$D_{schoolsorting}$	$D_{tracksorting}$	$D_{schoolsorting}$	$D_{tracksorting}$	$D_{schoolsorting}$												
	coef	s.e.	coef	s.e.	coef	s.e.												
Effect of <i>trackingsstrength</i>	0.235	0.034	***	-0.049	0.027	*	0.238	0.034	***	-0.065	0.028	***	0.231	0.045	***	-0.105	0.040	***
Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-specific state fixed effects	No	No	No	No	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.28	0.28	0.02	0.02	0.29	0.12	0.12	0.09	0.11	0.09	0.11	0.11	0.09	0.09	0.11	0.11	0.11	0.11
N(cohort-specific districts)	458	458	458	458	458	458	458	458	458	458	458	458	458	458	458	458	458	458

Source: Statistische Ämter des Bundes, der Länder und der Kommunen  
 Note: \* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected. See Tables A.5 and A.6 in Appendix V.

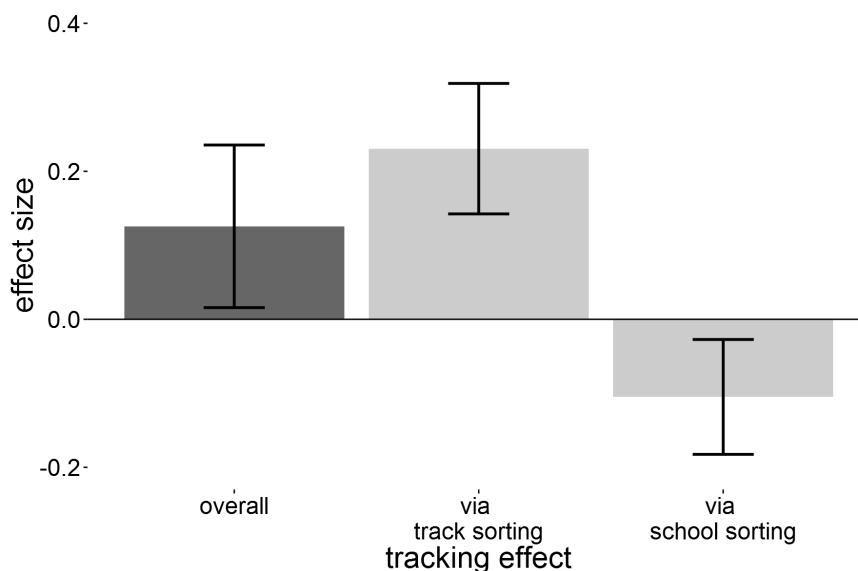


Figure 4.5: The (twofold) tracking effect on net segregation

Figure 4.5 summarizes the central finding of the paper: The observed overall effect of tracking on ethnic net segregation is positive (dark grey bar with 95 % confidence intervals), stricter tracking implies net segregation across schools. Closer inspection of the effect corroborates that it is twofold. More specifically, it is a combination of a positive effect due to track sorting and a negative effect due to school sorting within tracks (see two light-grey bars). Taken together, the two partial effects add up to the overall effect.

### Robustness checks

The outlined results may be flawed for a number of reasons. The applied rules assigning neighborhoods to schools are one possible source of bias in the analysis. Do these really yield reasonable counterfactual grade compositions, even though they were chosen rather arbitrarily? A comparison between actual and counterfactual grade compositions, at least, does not suggest otherwise, as they correlate as expected: whereas the baseline scenario produces compositions somewhat further off ( $r = .55$ ), the tracking scenario yields much higher correlations ( $r = .85$ ). However, a decent model fit is no definite confirmation of the taken

assumptions. To see how sensitive the results are when choosing alternative, yet still plausible assignment rules I derived the as-if catchment areas anew using the following assignment rules: first, assign all neighborhoods whose geometric centers fall within a predefined radius around a school to that school's catchment area. Next, to avoid mismatches, assign the two rules that have been applied before (i.e., assign each neighborhood to its closest school and each school to its closest neighborhood). Depending on the chosen size of the radius around the schools, this approach yields counterfactual grade compositions that differ more or less strongly from those of the more parsimonious approach (i.e., the smaller the radius, the more similar to the parsimonious approach). Reanalyses of model setup 3 all yield very similar results, as the upper part of Table 4.6 shows. The effect of tracking on net segregation is twofold, with an increasing impact via track sorting and a negative impact via school sorting within tracks. The combined overall effect of tracking is positive.

There is another possible source of bias. Even under fully correct assignment rules, the counterfactual school compositions may still be flawed due to two weaknesses of the neighborhood data. First, information on rural neighborhoods is not from 2008/09, but from 2011. Compositional changes within this time period that systematically correlate with *trackingstrength* could thus be driving the observed patterns. Second, the available data only provide neighborhood compositions concerning all ages, but no cohort-specific information. Compositional differences between cohorts that correlate systematically with *trackingstrength* could therefore be another source of bias.

To rule out these potential flaws, I propose an alternative analytical approach that does not rely on the neighborhood data at all. Instead of analyzing school segregation on the level of cohort-specific districts, I repeat the analyses on the level of cohort-specific local school clusters. I define local school clusters according to two important characteristics: first, they consist of schools located in close proximity to each other (i.e., maximally 500 meters away). Second, the location of the clusters themselves must be remote (i.e., the next neighboring school of the local cluster must be a certain distance away). These two characteristics provide the central advantage that all schools in a local cluster have the same as-if catchment area. Baseline segregation is then zero by definition, such that  $D_{net} =$

$D_{actual}$  for all cohort-specific local school clusters. Moreover, the composition of the as-if catchment area can be directly derived from the cohort-specific student body of all schools in the local cluster (i.e.,  $D_{tracking}$  solely based on school data). The more remote a local school cluster is, the more exact this approximation is (given that the school choices alternative to those in the cluster become less and less likely). In other words, the neighborhood compositional measures become obsolete.

For a meaningful reanalysis, I again restrict the sample of analysis to those cohort-specific local school clusters whose hypothetical minimum value of  $D_{actual}$  is zero. As a further restriction, both track sorting and school sorting have to be hypothetically possible within the cohort-specific local school cluster (i.e., alternative choices exist both between and within school types). Depending on the chosen remoteness that a cluster ought to be located in, this yields a varying number of cohort-specific local school clusters, each entailing between two to four different schools.<sup>11</sup>

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<sup>11</sup>Observed unevenness in grade-specific local school clusters differs from that in grade-specific districts. It is larger, on average, due to a small number of schools and a thus stronger impact of random perturbations. Nevertheless, the hypothesized mechanisms sorting students into tracks and schools should be similar.

Table 4.6: Alternative estimations of the overall/twofold effect of ability tracking

		M3									
		$D_{net}$		$D_{tracking}$		$D_{schoolsorting}$		N			
		coef	s.e.	coef	s.e.	coef	s.e.				
Alternative area assignment rules											
<i>1 km school radius</i>		0.118	0.050	***	0.212	0.036	***	-0.095	0.040	***	458
<i>2 km school radius</i>		0.082	0.047	*	0.183	0.037	***	-0.100	0.037	***	458
<i>3 km school radius</i>		0.065	0.056		0.163	0.044	***	-0.098	0.038	***	458
Alternative unit of analysis											
<i>Cohort-specific local clusters (2 km remote)</i>		0.295	0.070	***	0.280	0.050	***	0.015	0.055		319
<i>Cohort-specific local clusters (4 km remote)</i>		0.221	0.049	***	0.281	0.041	***	-0.060	0.053		108
<i>Cohort-specific local clusters (6 km remote)</i>		0.116	0.073		0.222	0.046	***	-0.106	0.080		60

Source: Statistische Ämter des Bundes und der Länder

Note: \* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected. See Tables A.7-A.12 in Appendix VI.



The lower part of Table 4.6 reports estimates from reanalyzing the fixed effects model based on local school clusters with varying levels of remoteness. The stricter the requirements concerning the remoteness of the clusters are (i.e., implying improved compositional proxies of the de-facto catchment areas, however, at the cost of smaller sample sizes and thus greater uncertainty in the estimates), the more similar the patterns become to those based on cohort-specific districts as the unit of analysis. Again, there is a positive effect via track sorting and a negative effect via school sorting. To summarize, the finding of the twofold effect of tracking on net segregation remains robust.

## 4.6 Conclusion

This chapter conveys three central findings, all in line with theoretical expectations. First, there is a clear indication of net segregation in German secondary schooling: the extent of ethnic segregation across schools exceeds residential patterns throughout all investigated cohort-specific districts. Second, half of this net segregation is traceable back to the institutional rule of ability tracking. Third, the effect of ability tracking on net segregation is twofold: whereas stronger tracking implies an increase in net segregation via track sorting, it leads to a decrease therein via school sorting within tracks. The provided evidence turns out to be strong. All analyses rely on regional variation in tracking strength within Germany, thus avoiding potentially confounding cross-country variation. Moreover, the application of state-specific fixed effect regression allows me to rule out any potential confounders related to policy differences across federal states within Germany. Finally, different robustness checks accounting for potential data problems all provide substantially identical results.

Beside its substantive contribution, the chapter introduces a straightforward method to decompose observed ethnic segregation via counterfactual reasoning. The approach provides reasonable results that seem rather robust to the choice of assumptions taken when constructing the counterfactuals. Moreover, the chapter gives ample indication for parental school choices to depend on the institutional setting they are faced with. What the findings suggest is intriguing: ability tracking steers minority and majority adolescents into separate school lives. At the

same time, however, it seems to hamper white flight in parental school choices. As such, the results align with the general notion that institutional factors shape the opportunities to segregate (Fiel, 2015). They carry a direct implication: school reforms shifting schools from ability tracking to more comprehensive school systems may have a weaker desegregating effect than usually expected, as part of the de-tracking effect would be counterbalanced by greater avoidance of immigrant-dominated schools.

The analyses also show a number of limitations, opening up avenues for future research. First, it is important to note that the current state of knowledge about parental secondary school preferences in Germany is sparse. As such, a desire for high-quality schools on the part of high-SES parents might drive parents' avoidance of immigrant-dominated schools (Wells and Crain, 1992). This would render possible a second explanation for the twofold effect, following the idea of statistical discrimination (Phelps, 1972): with the introduction of ability tracking, the track type of a school provides a very direct and overt indicator of school quality, rendering correlated proxies (i.e., the ethnic composition of schools) less important signals in the course of parental school choices. Consequentially, parents avoid immigrant-dominated schools to a lesser degree. According to this explanation, ability tracking would not only expose parents less often to immigrant-dominated schools, it also would affect parents' avoidance preferences directly. This alternative explanation bears the interesting implication that better information about the quality of schools has the potential to decrease ethnic school segregation. Whether this is really the case remains an open question—at least in the context of German secondary schooling.

Second, a simplifying assumption throughout this chapter is that track sorting and school choices within tracks unfold independently. In general, it seems reasonable to take this assumption as a natural first step. However, there may be situations where parents actually face a choice between schools of different track types. One example would be parents living far away from the next closest intermediate-track school but much closer to a low-track school. Even though their child may be eligible to attend an intermediate-track school, they may see the local low-track school as a feasible alternative. As a closely related limitation, residential choices are assumed to be independent of parental school choices.

Again, this assumption seems reasonable to take for the German case—given the absence of school catchment areas—but there may be exceptions. The robustness checks based on local school clusters accounted indirectly for this problem, yielding similar conclusions. More direct accounting for such interdependencies, however, would call for more complex modeling. This being the first work to investigate the segregating effect of ability tracking explicitly, such complex modeling would clearly go beyond its scope.

Another limitation of this chapter is that students' immigrant status might be based solely on foreign citizenship. Given that this approach fails to identify naturalized immigrants as such, it yields an underestimation of immigrant proportions in schools. Concerning the existence of the twofold effect of tracking on net segregation, however, this limitation ought not to bias the results in any way.

Also, the focus on one specific country in the analyses provides advantages but also limits the analyses' generalizability. As laid out, specific features of the German secondary school system make the existence of a twofold effect of tracking likely (e.g., strict ability tracking, no school catchment areas, and ethnic disparities in achievement). Any attempt to apply the findings to other contexts and school systems should be well aware of these specific side constraints.

Finally, this chapter focused on between-school segregation only. Low levels of between-school segregation do not necessarily imply increased interethnic contact possibilities in schools, as everyday school lives may still be segregated, for example due to ability grouping within schools. From this perspective, the article examined rather a necessary than a sufficient condition to impede the emergence of separate school lives.

Nevertheless, this chapter's findings carry an important message concerning the effectiveness of residential barriers and bridges for young immigrants' social integration: neighborhood compositions seldom represent the actual outgroup meeting opportunities they face in their most important meeting contexts, their schools. Instead, residential barriers (i.e., lack of outgroup school peers) and bridges (i.e., abundance of outgroup school peers) are amplified by an interplay between parents' school tastes and the institutional choice restrictions they face. From this perspective, it is not surprising to find moderate levels of neighborhood segregation in Germany producing tremendous residential barriers, as

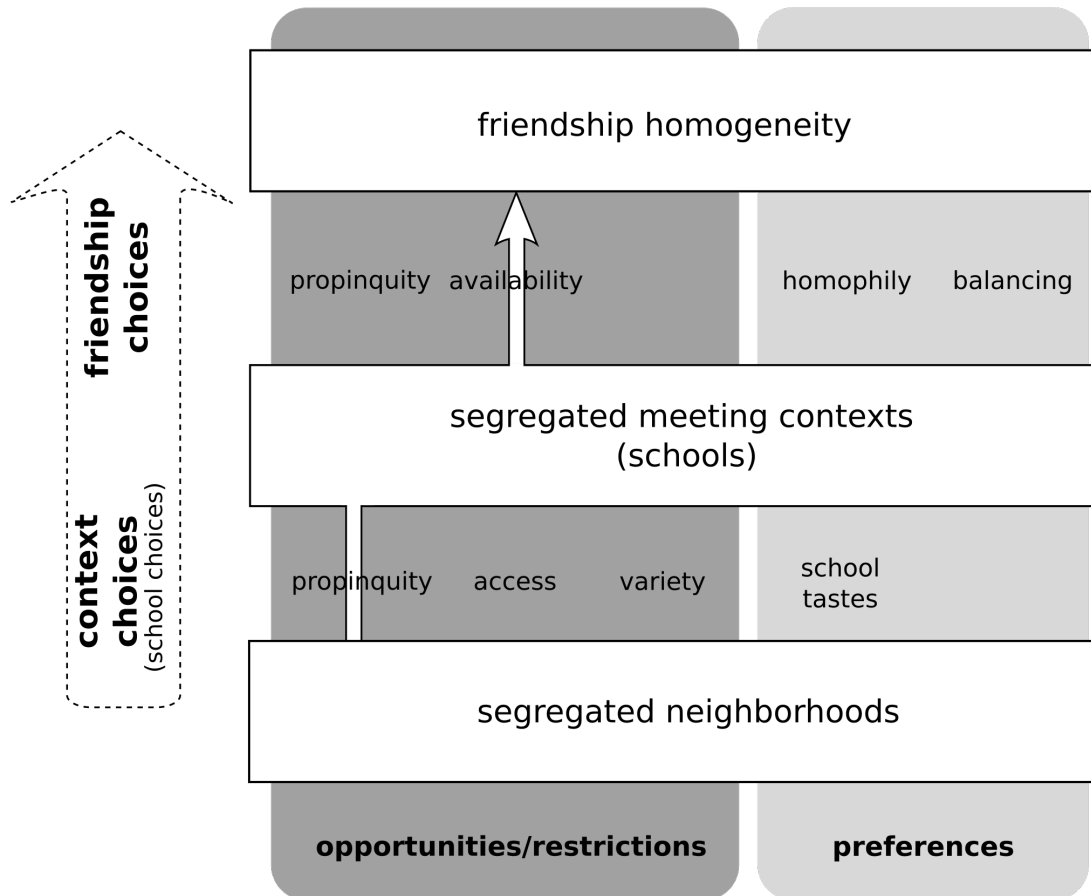


Figure 4.6: The spatially informed framework of friendship formation (re-revised)

demonstrated in Chapter 1. Hence, Figure 4.6 summarizes a *re-revised* version of the spatially informed framework of friendship formation.

Moreover, the chapter's findings provide hints about one potential explanation for this book's central explanandum. The ultimate reason for SES-specific residential bridges may lie in the institutional rule of ability tracking: Low-SES students are overrepresented in lower-track schools, thus facing different school compositions than high-SES students. Consequentially, the way neighborhood compositions translate into meeting context compositions may be SES-specific. The next chapter will—based on the findings of all previous chapters—test this explanation explicitly, thereby solving the puzzle of SES-specific residential bridges.

#### 4. Testing the framework pt.II

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## Chapter 5

Solving the puzzle.

Why are residential bridges  
SES-specific?\*

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\*A different version of this chapter was published in *European Sociological Review* (Kruse, 2017). To guarantee consistency across chapters, I have reformulated various sections.

### Abstract

Finally, this chapter shows why the neighborhood affects young immigrants' interethnic friendships SES-specifically; why residential barriers apply universally, whereas residential bridges primarily emerge for high-SES immigrants. Based on a formalized account in line with the re-revised framework the chapter proposes four potential explanations, three being empirically corroborated: First, SES differences are partly an artefact due to model misspecification. Second, correct specifications still show that low-SES immigrants attend more concentrated meeting contexts (i.e. schools) than high-SES immigrants, yielding different opportunities for native friends even when neighborhood compositions are identical. Third, SES-specific friendship preferences may be responsible, as well. There is no indication that SES groups differ in how much they rely on their neighborhoods when making friends.



## 5.1 Introduction

It is time to return to the central puzzle of this book: residential barriers are universal; residential bridges primarily emerge for high-SES immigrants. Chapter 1 established this curious finding, corroborating recent research (Schlueter, 2012; van der Laan Bouma-Doff, 2007). Based on the knowledge gained in the previous chapters I am now ready to examine *why* we observe such SES-specific neighborhood effects.

In this chapter I introduce a formalized version of the re-revised theoretical framework. Doing so helps to show that there are several explanations for SES-specific neighborhood effects: Previous findings may have been partly due to model misspecification. There are also reasons, however, why SES-specific neighborhood effects may be substantive. Results provide indication for three causes: First, they confirm that SES differences are partly an artefact due to model misspecification. Second, correct model specifications still show that low-SES immigrants attend more concentrated meeting contexts (i.e. schools) than do high-SES immigrants, yielding different opportunities for native friends even when neighborhood compositions are identical. Third, SES-specific friendship preferences may be responsible, as well. There is no indication that SES groups differ in how much they rely on their neighborhoods when making friends. For the analyses I return to using the CILS4EU data (Kalter et al., 2014) in combination with the Microm neighborhood data.

The remainder of the chapter starts with a theoretical discussion of neighborhood effects on immigrants' friendships with natives, thereby introducing the formalized account of the theoretical framework (section 5.2). Based on the model, section 5.3 discusses different mechanisms that suggest SES-specific neighborhood effects. Section 5.4 lays out the analytical strategy to identify these effects as well as their causes. Section 5.5 discusses the data and variables used. Results are presented in Section 5.6. The final section summarizes the chapter's main findings and discusses its limitations and potential further steps.

## 5.2 Theory

### A formal model of interethnic friendship formation

To make explicit how the ethnic composition of neighborhoods affects ethnic friendship compositions SES-specifically I will rely on a formalized account that is in line with the re-revised framework in Figure 4.6. As I will show, understanding the interplay of the main determinants of friendship choices is crucial to trace SES-specific neighborhood effects. The model follows these general notions: Actors attend different meeting contexts, one of them being their local neighborhood. Throughout these contexts they make encounters with two types of peers, natives and immigrants. In each encounter they decide whether or not to befriend the peer they are facing. All actors realize a finite number of friendships, thus ending up with some proportion of natives among their friends.

Take one single actor  $i$ . The native proportion among his/her friends is determined by two factors. The first are his/her opportunities for native contact, represented by the probability that a peer that  $i$  encounters is a native, formally  $p_i(nat)$ . Of course,  $p_i(nat)$  would not be fully determined by the share of natives in actor  $i$ 's neighborhood, formally  $n_i$ . Instead, it is shaped by the ethnic compositions of all contexts in which he/she encounters others, as well as his/her relative presence in these contexts. Beside local neighborhoods actor  $i$  may encounter peers in attended schools, workplaces, sport clubs, churches, or other associations (Edling and Rydgren, 2012). As we have seen in the previous chapter the compositions of these other meeting contexts may deviate from those of the neighborhood. For the sake of simplicity, I combine these contexts into one 'other' category ( $o_i$ ), yielding

$$p_i(nat) = \beta_i n_i + (1 - \beta_i) o_i, \quad (5.1)$$

where  $\beta_i$  may lie in  $[0,1]$ , representing the share of encounters actor  $i$  makes in his/her local neighborhood; in other words, actor  $i$ 's neighborhood importance.

The second important factor driving actor  $i$ 's friendship choices are his/her preferences for native friends (as compared to immigrant friends). A preference for natives or immigrants affects the ratio of the ethnically-specific conditional

probabilities of friendship formation, formally:

$$\frac{p_i(f|nat)}{p_i(f|mig)} = \alpha_i, \quad (5.2)$$

with  $\alpha_i$  being in the interval  $[0, \infty]$ . The term  $p_i(f|nat)$  represents the probability that an encounter between  $i$  and a native peer turns into a friendship and  $p_i(f|mig)$  indicates the respective probability for an immigrant peer. The higher  $\alpha_i$  the more likely actor  $i$  forms a friendship if the encountered peer is a native and not an immigrant. Note, however, that  $\alpha_i$  is not only driven by the preferences for native friends of actor  $i$  but also by his/her peers' preferences. Further, actor  $i$  may favor certain traits in friends that correlate with their ethnicity (e.g., music tastes, hobbies) thus also affecting  $\alpha_i$ . Finally, friendships are more likely between two actors if they already share a friend (i.e., transitive closure and other balancing mechanisms, cf. Chapter 3). Assuming that actor  $i$  is friends with a native peer  $j$  he/she is more likely to befriend  $j$ 's friends. This may also have an effect on  $\alpha_i$ . From this perspective,  $\alpha_i$  should be seen as the result of a combination of tie formation tendencies, among them actor  $i$ 's preference for native friends.<sup>1</sup>

Having laid out two important determinants of actor  $i$ 's friendship formation with natives—opportunities and preferences—I determine their interplay. Following Bayes' theorem, the native proportion among actor  $i$ 's friends (i.e., the probability that a peer is native given that he/she is a friend of  $i$  is determined by

$$p_i(nat|f) = \frac{p_i(f|nat)p_i(nat)}{p_i(f|nat)p_i(nat) + p_i(f|mig)[1 - p_i(nat)]}. \quad (5.3)$$

Substituting equations 5.1 and 5.2 into 5.3 and rearranging yields

$$p_i(nat|f) = \frac{\alpha_i[\beta_i n_i + (1 - \beta_i)o_i]}{(\alpha_i - 1)[\beta_i n_i + (1 - \beta_i)o_i] + 1} \quad (5.4)$$

which describes how  $n_i$  asserts a direct effect on the native proportion among  $i$ 's friends.

---

<sup>1</sup>Some scholars refer to  $\alpha$  as *inbreeding homophily* (McPherson et al., 2001).

However, beside this direct effect there is also an indirect effect of  $n_i$ , given that meeting contexts do not emerge independently of each other. Actor  $i$ 's neighborhood composition ( $n_i$ ) partly determines the ethnic compositions he/she faces in other meeting contexts ( $o_i$ ): for example, students' school choices are contingent on their place of residence, be it due to legal restrictions (i.e., pre-defined catchment areas) or in order to minimize home-to-school distances (cf. Chapter 4). For the sake of simplicity, I define this dependence between  $n_i$  and  $o_i$  by

$$o_i = \gamma_{0i} + \gamma_{1i}n_i, \quad (5.5)$$

thus assuming  $o_i$  to be a linear function of  $n_i$ . Parameter  $\gamma_{0i}$  represents the average native proportion in  $i$ 's other contexts, independent of where he/she lives;  $\gamma_{1i}$  quantifies the extent to which  $i$ 's neighborhood composition correlates with that of his/her other meeting contexts. In order to account both for the direct and the indirect neighborhood effect on friendship formation substitute eq. 5.5 into 5.4, yielding

$$p_i(nat|f) = \frac{\alpha_i[\beta_i n_i + (1 - \beta_i)(\gamma_{0i} + \gamma_{1i}n_i)]}{(\alpha_i - 1)[\beta_i n_i + (1 - \beta_i)(\gamma_{0i} + \gamma_{1i}n_i)] + 1}, \quad (5.6)$$

which completes the formal model of interethnic friendship formation. Based on this model we are now prepared to derive why SES-specific neighborhood effects emerge.

### SES-specific effects as an artefact

Existing explanations for SES-specific neighborhood effects abstract away from actors' friendship preferences (Schlueter, 2012; van der Laan Bouma-Doff, 2007). Disregarding preferences (and other factors affecting  $\alpha$ ) corresponds to implicitly assuming indifference between immigrant and native friends. Formally stated as  $\alpha_i = 1$ , eq. 5.6 would thus simplify to

$$p_i(nat|f) = \beta_i n_i + (1 - \beta_i)(\gamma_{0i} + \gamma_{1i}n_i). \quad (5.7)$$

The partial derivative of eq. 5.7 with respect to  $n_i$  identifies the neighborhood effect under this scenario:

$$\frac{dp_i(nat|f)}{dn_i} = \beta_i + (1 - \beta_i)\gamma_{1i}. \quad (5.8)$$

This neighborhood effect is linear: a one-unit change in  $n_i$  leads to a change of  $\beta_i + (1 - \beta_i)\gamma_{1i}$  units in the native proportion among his/her friends. Two parameters determine the effect, the correlation between  $i$ 's neighborhood and other meeting context compositions ( $\gamma_{1i}$ ) and the extent of neighborhood importance ( $\beta_i$ ). Previous accounts of SES-specific neighborhood effects argue that the latter are responsible: High-SES immigrants have the resources to be more mobile and thus to maintain friendships outside the neighborhood more easily than low-SES immigrants (Schlueter, 2012). Consequentially, their  $\beta$  values are lower, implying a weaker neighborhood effect. Figure 5.1A depicts the relation between neighborhood and friendship compositions that would result for actors from two SES groups (circles and crosses) with different levels of  $\beta$  or  $\gamma_1$ , assuming that  $\alpha = 1$ . Under this assumption, linear regression models would capture SES-specific neighborhood effects correctly.

However, friendship choices are usually ethnically homophilous which implies, *ceteris paribus*, that  $\alpha > 1$  for natives and  $\alpha < 1$  for immigrants.<sup>2</sup> Actors show a strong tendency to prefer friends with attributes similar to their own, for example boys befriending boys rather than girls, or natives and immigrants preferring co-ethnics as friends (McPherson et al., 2001). This also holds for young immigrants in Germany (Smith et al., 2014; Windzio and Bicer, 2013). The neighborhood effect depicted in eq. 5.8 is therefore an oversimplification given that it only holds when  $\alpha = 1$ .

Relaxing this assumption has important implications for the neighborhood effect on immigrants' contact to natives. The partial derivative of eq. 5.6 with respect to  $n_i$  shows how a change in actor  $i$ 's neighborhood composition would affect his/her friendship composition in the presence of homophily. In other

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<sup>2</sup>Other factors affecting  $\alpha$  (e.g. transitive closure) usually amplify homophilous preferences, making it even more likely that  $\alpha > 1$  for natives and  $\alpha < 1$  for immigrants (Goodreau et al., 2009; Wimmer and Lewis, 2010).

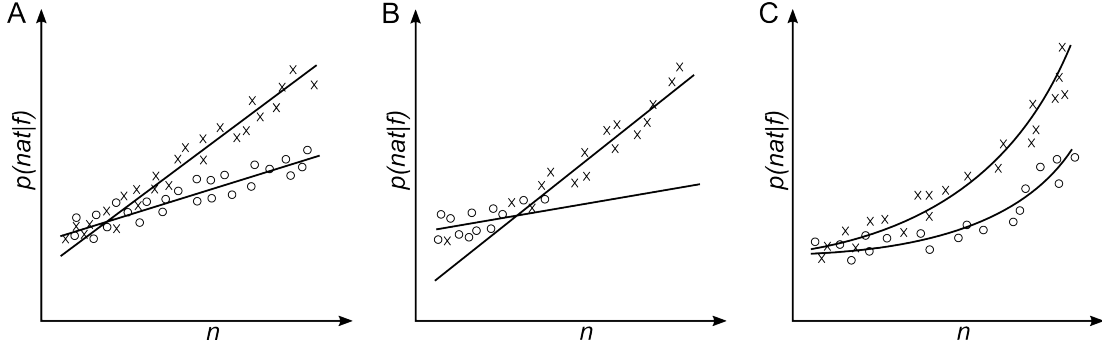


Figure 5.1: Hypothetical scenarios leading to the observance of SES-specific patterns in the relation between neighborhood and friendship compositions

words, it identifies the neighborhood effect for  $i$  when  $\alpha \neq 1$ :

$$\frac{dp_i(\text{nat}|f)}{dn_i} = \frac{\alpha_i(\beta_i + (1 - \beta_i)\gamma_{1i})}{\left[ (\alpha_i - 1)[(\beta_i + (1 - \beta_i)\gamma_{1i})n_i + (1 - \beta_i)\gamma_{0i}] + 1 \right]^2}. \quad (5.9)$$

As eq. 5.9 suggests, this neighborhood effect is far from trivial, as its size depends on the interplay of several factors. Most importantly the neighborhood effect is no longer linear, as it now depends on the size of  $n_i$ . The effect strength of neighborhoods with low native proportions thus differs from that of neighborhoods with higher proportions. Any observed SES-difference in the neighborhood effect may therefore actually result from the fact that the SES groups live in neighborhoods with different average native proportions, high-SES immigrants thereby residing in neighborhoods with higher native shares. Figure 5.1B illustrates this situation. As the graph shows, a linear model testing for the existence of SES-specific effects would falsely indicate SES differences, even though both SES groups are subject to the same (non-linear) effect. SES-specific patterns may thus be an artefact due to false assumptions about the functional form of the effect.

### Actual SES-specific effects

Following eq. 5.9, the assumption of homophilous preferences has further consequences for the nature of the neighborhood effect on immigrants' contact to

natives. Its size does not only depend on the native proportion among actor  $i$ 's neighbors, but also on  $\alpha_i$ ,  $\beta_i$ ,  $\gamma_{0i}$ , and  $\gamma_{1i}$ , with higher values indicating a stronger effect. Any systematic SES-group difference in one of these parameters would imply the existence of actual SES-specific neighborhood effects, as depicted in Figure 5.1C.

Concerning young immigrants in Germany, SES-specific levels of  $\alpha$  seem likely. Since natives have a higher SES on average than immigrants in Germany—where most former migration is (blue-collar) labor-related—immigrants with a higher SES might simply be both more attracted and more attractive to natives due to their greater similarity in SES. Balancing mechanisms like transitive closure might further amplify this tendency. In other words, SES homophily helps high-SES immigrants to bridge the ethnic friendship gap but not low-SES immigrants (Smith et al., 2014). From this perspective, high-SES immigrants should show higher values of  $\alpha$ , thus being subject to stronger neighborhood effects than low-SES immigrants.

SES-specific levels of  $\beta$  seem rather unrealistic concerning children and adolescents, as different SES groups are likely to be similarly bounded to their local neighborhoods. The balance of their everyday lives takes place in their neighborhood and school settings. Once adolescents have finished secondary schooling, they may be more likely to have entered meeting contexts outside their direct local surroundings. Hence, SES-specific values of  $\beta$  should not be present among children and adolescents, but rather later in life.

More likely, however, are SES-specific levels of  $\gamma_0$  and  $\gamma_1$ . One of adolescents' most important meeting contexts are their schools;  $\gamma_{0i}$  and  $\gamma_{1i}$  are therefore largely determined by the relation between  $i$ 's neighborhood and school composition. Germany deploys a strict version of ability tracking in secondary education, where low-SES and thus also immigrant students are overrepresented in lower-track schools (cf. Chapter 4, also Pfeffer, 2008). High-SES students therefore face on average higher native proportions in their schools than low-SES students do (i.e., SES differences in  $\gamma_0$ ). High-SES immigrants should thus be subject to stronger neighborhood effects than low-SES immigrants. Whether neighborhood and school compositions also correlate SES-specifically (i.e., SES differences in  $\gamma_1$ ) is rather an empirical question.

## Short summary

Concerning young immigrants in Germany, the formal model suggests the following: First, given that friendship choices are homophilous, the neighborhood effect on immigrants' contact to natives should be non-linear. Secondly, it should be SES-specific, even when accounting for non-linearity in the relation of interest. This is due to two causes; SES-specific context compositions other than the neighborhood ( $\gamma_0$ ) and/or SES-specific friendship preferences ( $\alpha_i$ ).

## 5.3 Analytical approach

### Identifying SES-specific neighborhood effects

To identify the existence of (SES-specific) neighborhood effects the chapter follows previous work and applies cross-sectional OLS models. It regresses the native proportion among immigrants' friends on the interaction between their SES and the native proportion in their neighborhood. Of course, not every association found between the ethnic composition of immigrants' neighborhoods and their friendships proves the existence of a neighborhood effect. First, an observed relation between neighborhood and friendship compositions may result from reversed causality: social contacts determine, at least to some degree, residential choices, for example, via information about vacant housing spreading through personal networks (Röper et al., 2009). However, given that we investigate friendship choices of adolescents, relocations triggered by friends are of limited concern. More challenging is the problem of potential confounders, which is why the models account for the most important ones explicitly, namely immigrants' ethnic background and their age of arrival (for a detailed discussion, see Appendix VII). Given that respondents cluster in schools, all standard errors are cluster-corrected.<sup>3</sup> Further, to account for non-linearity in the relation of

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<sup>3</sup>There is no clustering on the neighborhood level due to its fine-grained scale:  $\sim 90\%$  of the respondents live in neighborhoods with less than three other respondents. Accounting for clustering on the neighborhood level had no impact on the results (analyses not shown here, available upon request).



interest a quadratic term of the native proportion in immigrants' neighborhoods is included into the OLS model.<sup>4</sup>

### Identifying the causes of SES-specific neighborhood effects

Testing the causes of SES-specific neighborhood effects is challenging given that parameters  $\alpha$ ,  $\beta$ ,  $\gamma_0$ , and  $\gamma_1$  are unobserved. What can be observed, however, are the native proportions in respondents' schools, serving as an adequate proxy for their value of  $o$ .<sup>5</sup> Regressing the native proportion in immigrants' neighborhoods on the native proportion in their schools via OLS and in line with eq. 5.5 therefore yields useful estimates of  $\gamma_0$  and  $\gamma_1$ . To test for SES-specific values of  $\gamma_0$  and  $\gamma_1$  I add an interaction term between respondents' SES and the native proportion in their schools.

Also observable are measures of reported attitudes towards natives serving as a proxy for  $\alpha$  and the proportion of friends met in the neighborhood as a proxy for  $\beta$ . However, both proxies have central weaknesses: The first is prone to desirability bias (Hewstone et al., 2002). The second does not fully capture the concept of neighborhood importance. Instead of quantifying the share of encounters made in the neighborhood it measures the share of realized friendships, thus being partly affected by  $\alpha$ .

In order to avoid these problems,  $\alpha$  and  $\beta$  are also estimated. As applied in Chapter 3, one way to estimate friendship preferences is the analysis of complete network data based on exponential random graph models (e.g. Robins et al. 2007). These approaches conveniently control for actors' tie opportunity structure and allow researchers to derive preference estimates net of endogenous tie formation mechanisms such as reciprocity or transitive closure (see, for example, Chapter 3; also Mouw and Entwisle, 2006). However, their application comes at

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<sup>4</sup>Eq. 5.4 suggests a monotonic relationship with one inflection point between immigrants' friendship and neighborhood compositions. To approximate a monotonic relationship with one inflection point in OLS regression, it suffices to include a quadratic term. Moreover, additional analyses (not presented here) suggest that an additional interaction term between SES and the quadratic term would not contribute to the model fit in any way.

<sup>5</sup>In an alternative specification I restricted the analyses to friendships being formed in school or in the neighborhood, thus guaranteeing that the proxy for  $o$  is even more appropriate. Doing so provides substantially identical results (cf. Kruse, 2017).

the cost of being restricted to one predefined context only. Given that the present analyses aim to identify not only preferences (as a part of  $\alpha$ ) but also the relative importance of the neighborhood as a meeting context ( $\beta$ ) they necessarily rely on friendship data from several contexts (i.e., the neighborhood and school). The presented analyses make therefore use of ego-network data that combine different contexts and a different estimation approach. Doing so allows me to test the potential causes for SES-specific neighborhood effects simultaneously, while accounting for the non-additive relations between parameters as suggested by the formal model. In other words, the statistical model estimating  $\alpha$  and  $\beta$  directly corresponds to the formal model of friendship formation (i.e., eq. 5.4) yielding

$$p_i(\text{nat}|f) = \frac{\alpha[\beta n_i + (1 - \beta)o_i]}{(\alpha - 1)[\beta n_i + (1 - \beta)o_i] + 1} + \epsilon_i \quad \forall i \in \{1, N\} \quad (5.10)$$

with  $p_i(\text{nat}|f)$ ,  $n_i$  and  $o_i$  being observed variables,  $\alpha$  and  $\beta$  being the model parameters to be estimated, and  $\epsilon_i$  the residual error component. To test for SES-specific  $\alpha$  and/or  $\beta$  values they are successively replaced in eq. 5.10 by

$$\alpha = \alpha_0 + \alpha_{ses}ses_i + \sum_c \alpha_c c_i \quad (5.11)$$

$$\beta = \beta_0 + \beta_{ses}ses_i + \sum_c \beta_c c_i \quad (5.12)$$

with  $ses_i$  being  $i$ 's observed SES, and  $c_i$  being  $i$ 's observed confounding attributes (i.e., ethnicity and age of arrival).<sup>6</sup> Statistically significant estimates of  $\hat{\alpha}_{ses}$  and/or  $\hat{\beta}_{ses}$  are indication for SES-differences in preferences and/or neighborhood importance. Note that any SES-specific estimate of  $\alpha$  or  $\beta$  would be net of all SES differences in  $\gamma_0$ , and  $\gamma_1$ , since  $i$ 's observed school composition is used as  $o_i$ .

Due to non-additivity of the parameters, the estimation process is based on non-linear least squares estimation, whereby the functions' maxima are approximated iteratively, equivalent to maximum likelihood estimation. Standard errors are cluster-corrected. All analyses are applied in R (v.3.2.3).

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<sup>6</sup>Due to the model's complexity, several categories of the controls had been combined: ethnicity was controlled as a dummy indicating whether the respondent is part of the least integrated ethnic groups (Turks and FYR) and age of arrival was controlled by a dummy indicating whether the respondent was born in Germany.

## 5.4 Data and variables

### Data

The analyses rely on the by now well known first wave of the CILS4EU data (Kalter et al., 2014). More specifically, I use exactly the same subsample of young immigrants in Germany as in Chapter 1 when deriving the book’s explanandum. As a short reminder: the research question addresses young immigrants only. I therefore exclude all native respondents from the analysis; that is, all respondents who themselves were born and whose parents were both born in Germany, leaving  $N=2,393$  cases. Missing values (less than 11% in all variables) were multiply imputed applying chained imputation techniques (White et al., 2011), resulting in ten different data sets to be analyzed. All reported model results are based on all ten data sets, accounting for the variation across them (Rubin, 1987).

### Variables

The *dependent variable*, the native proportion among friends ( $p(nat|f)$ ), is—in line with Chapter 1—based on the reported ethnic background of respondents’ five best friends, capturing the percentage that they identified as having a native background.<sup>7</sup>

The *main independent variable*, the native proportion in the neighborhood ( $n$ ), is—also in line with previous chapters—taken from the Microm neighborhood data. The average neighborhood unit size in the sample is  $\sim 700$  households. The ethnic composition of these neighborhoods mainly relies on name-based identification, where a household’s ethnic background is derived from the ethnic origin of the household members’ names (see Mateos 2007).

The highest ISEI score among a respondent’s parents is used as a proxy for his/her SES. If available, the measure is based on information from the parental

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<sup>7</sup>More than 90% of all respondents reported the maximum of five friends. The number of friends reported is uncorrelated with respondents’ SES such that bias due to systematic differences in the amount of friends can be ruled out.

interview. If no parental interview could be realized ( $\sim 26\%$  of the investigated cases), information from the student interview was used.<sup>8</sup>

Further, despite the problems mentioned, I make use of a number of *descriptive proxies for  $\alpha$ ,  $\beta$ , and  $o$* . The first is based on respondents' reported attitudes towards different ethnic groups. Being asked to rate how they felt about different ethnic groups, students reported values between 0 (negative) and 100 (positive) with 50 indicating neutrality towards a group. The difference between respondents' reported scores for natives and the mean scores for all immigrant groups serves as a measure for *native favoritism*, and thus as a first proxy for  $\alpha$ . The second measure stems from information on the reported regular meeting contexts of respondents' five best friends. Being asked the question "Where do you see or meet each other?" respondents reported for each friend their usual meeting context: in school, the neighborhood, at a club, at work, at home, online, or elsewhere. Multiple answers were possible. The *proportion of friends met in the neighborhood* is used as a descriptive proxy for neighborhood importance  $\beta$ . The *native proportion at school ( $o$ )* is represented by the native share among all students sampled from the respondent's school, thus capturing the ninth grade of a school only. This fact makes the measure even more applicable, given that adolescents mainly befriend within their own age group.

Finally, the analyses use a number of *control variables*: The ethnic background of a respondent is based on his/her (parents') country of birth, distinguishing between the five largest immigrant groups (Turkish, Former Soviet Union, Polish, Italian, Former Yugoslavia) and two residual groups combining all smaller groups (other Western and other Non-western). The age of arrival of an immigrant stems from information about his/her generational background (see Dollmann et al. 2014), with the categories age 11 or older, ages 6-10, age 5 or younger, and being second generation immigrant (i.e., born in survey country). The social and age composition of respondents' neighborhood are measured by the neighborhood proportion of unemployed and aged 10-18, respectively. Both measures stem from the Microm neighborhood data, as well, and are located on the same spatial scale

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<sup>8</sup>The correlation between student and parental reports of parents' ISEI is of modest size ( $r \approx .69$ ). A reanalysis based only on parental reports provided substantially identical results, unless noted otherwise (cf. Kruse, 2017).

as the measure of the native proportion in the neighborhood. Table A.13 in Appendix XIII reports summary statistics of all measures used.

## 5.5 Results

### The existence of SES-specific neighborhood effects

To examine whether SES-specific neighborhood effects exist for young immigrants in Germany turn to the OLS model results in Table 5.1. Models 1-3 assume a linear relation between neighborhood and friendship compositions, implicitly assuming that  $\alpha = 1$ . Model 1 shows that the overall relation between immigrants' neighborhood and friendship compositions is significantly positive. Further, the relation is SES-specific, as the positive estimate of the interaction term in model 2 clearly indicates. Immigrants of higher SES show a stronger relation between their neighborhood and friendship compositions. As such, model 2 replicates this book's expandum established in Figure 1.4 in Chapter 1. This finding also holds when controlling for potential confounders, as demonstrated in model 3. It seems as if there is a strong neighborhood effect and it is really SES-specific, at least under linearity assumptions. But does this also hold if the model accounts for non-linearity in the relation (i.e., the possibility that  $\alpha \neq 1$ )?

Model 4 introduces a squared term of the neighborhood composition. The squared term is strongly statistically significant, suggesting a non-linear relation between immigrants' neighborhood and friendship compositions. At the same time, the interaction term decreases in effect size but remains significantly positive. This suggests that SES-specific effects persist among young immigrants in Germany when accounting for non-linearity in the relation.<sup>9</sup>

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<sup>9</sup>When SES is based only on parental reports, the interaction effect size even decreases such that it is not significantly different from zero (cf. Kruse, 2017).

Table 5.1: OLS regression results (dep. var.: native prop. among friends)

	M1: overall linear relation		M2: SES-specific linear relation		M3: SES-specific linear relation net of confounders		M4: SES-specific non-linear relation net of confounders	
	coef	se	coef	se	coef	se	coef	se
Native prop. in neighb. (centered)	1.194	0.101 ***	1.143	0.089 ***	0.707	0.105 ***	1.084	0.136 ***
SES (centered)			0.003	0.000 ***	0.002	0.000 ***	0.002	0.000 ***
Native prop. in neighb.*SES (both centered)			0.017	0.004 ***	0.015	0.004 ***	0.012	0.003 ***
Native prop. in neighb. <sup>2</sup> (centered)							2.352	0.514 ***
Confounders controlled		no		no		yes		yes
N(schools)		144		144		144		144
N(students)		2,393		2,393		2,393		2,393

Source: CILS4EU, w1, v1.1.0 / Microm, unweighted

Note: \* p<.05 \*\* p<.01 \*\*\* p<.001. Results from 10 multiply-imputed datasets combined via Rubin's rules (Rubin, 1987).

All standard errors are cluster-corrected. For complete model results, see Table A.14 in Appendix XIII.

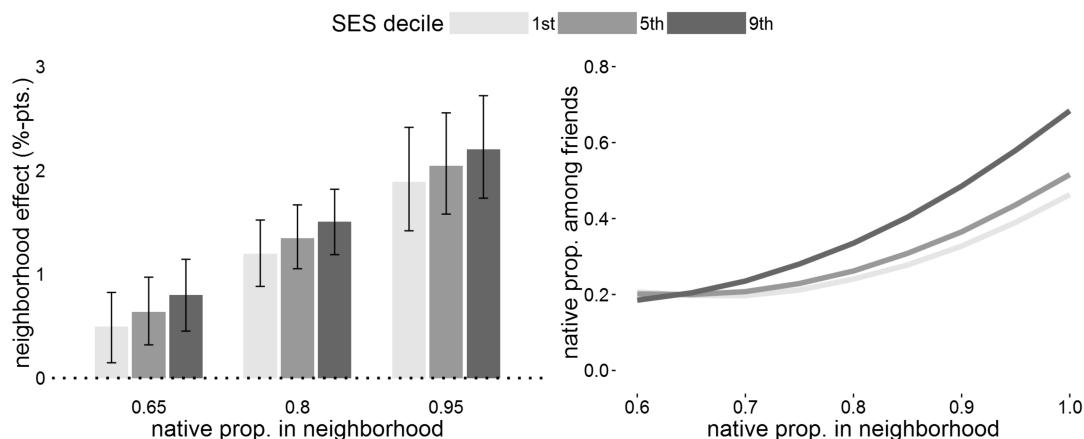


Figure 5.2: SES-specific neighborhood effect on native proportion among young immigrants' friends (left), Predicted native proportions for Polish adolescents arrived at age 11 or older with varying SES (right), both graphs based on model 4 from Table A.14, unweighted

The left panel of Figure 5.2 informs about the size of the estimated neighborhood effect. Due to its non-linearity, effect sizes vary across neighborhood compositions, the strongest effects being present in neighborhoods dominated by natives. Moreover, the effect varies across immigrants' SES (exemplified here by differences between the 1<sup>st</sup>, 5<sup>th</sup>, and 9<sup>th</sup> SES decile). The SES differences are rather small. Nevertheless, they lead to substantial differences in predicted native proportions among immigrants' friends. Consider the exemplary predicted friendship compositions of Polish adolescents across different neighborhoods in the right panel of Figure 5.2: In neighborhoods with low native proportions about 20% of their friends are native (i.e., one out of their five best friends), regardless of their SES. In native-dominated neighborhoods, however, substantial SES differences in predicted friendship compositions exist. Immigrants in the first SES-decile have only 40% native friends (i.e., two out of their five best friends), whereas those in the ninth decile have more than 60% native friends (i.e., 3 out of their 5 best friends). In short, actual (non-linear) SES-specific neighborhood effects exist among young immigrants in Germany.



Figure 5.3: Distribution of immigrants' best friends over meeting contexts. SES-group-specific averages, unweighted (*elsewhere* combining all meeting contexts other than school or neighborhood)

### The causes of SES-specific neighborhood effects

Before turning to the causes of the SES-specific neighborhood effects (i.e., descriptive proxies and estimations of  $\alpha$ ,  $\beta$ ,  $\gamma_0$  and  $\gamma_1$ ) consider where young immigrants meet their friends. Figure 5.3 reveals that neighborhoods are seldom used as meeting contexts, given that respondents meet only about one out of their five best friends there, regardless of their SES. In contrast, more than half of their friendships are maintained at school, making it the most important meeting context. These findings suggest that neighborhood friendships play a minor role for the explanation of (SES-specific) neighborhood effects and that much of the observed neighborhood effect is due to friendship formation in other meeting contexts.

I therefore first turn to the tests concerning SES-specific values of  $\gamma_0$  and  $\gamma_1$ . Table 5.2 provides SES-specific estimates of the two parameters derived from the respective OLS model results (see Table A.15). They show that an immigrant in the first SES-decile living in a neighborhood with an average native proportion attends a school with about 36% natives. In contrast, an immigrant from the ninth SES-decile living in the same type of neighborhood has about 45% native schoolmates (i.e., SES-specific  $\hat{\gamma}_0$ ). Moreover, for an immigrant in the first SES-decile a one unit change in his/her neighborhood composition is associated with a 0.8 unit change in his/her school composition. For an immigrant at the ninth



Table 5.2: SES-specific estimates of  $\gamma_0$  and  $\gamma_1$ 

	SES decile			SES differences significant?
	1st	5th	9th	
$\hat{\gamma}_0$	0.361	0.382	0.453	yes
$\hat{\gamma}_1$	0.793	0.897	1.231	yes

*Source:* CILS4EU, w1, v1.1.0 / Microm, unweighted

*Note:* Results from 10 multiply-imputed datasets combined via Rubin's rules (Rubin, 1987). All standard errors are cluster-corrected. For respective model results, see Table A.15 in Appendix XIII.

SES-decile this change is stronger with about 1.2 units (i.e., SES-specific  $\hat{\gamma}_1$ ). To summarize, neighborhood compositions translate differently into school compositions for low-SES immigrants than for high-SES immigrants. This is clearly a first explanation for actual SES-specific neighborhood effects. But what about the other potential explanations?

To see this, consider the descriptive proxies for  $\alpha$  and  $\beta$  first. Figure 5.4 shows how each proxy varies across immigrants' SES. The measure of immigrants' reported native favoritism suggests a positive relation with immigrants' SES, according to the measure's LOWESS trend (dashed line). High-SES immigrants are more in favor of natives in general than low-SES immigrants which is in line with expectations about SES-specific values of  $\alpha$ . The proxy for  $\beta$  corroborates the impression from Figure 5.3: the proportion among immigrants' five best friends who are met in their neighborhood is the same regardless of respondents' SES (solid line). There is no indication for SES-specific values of  $\beta$ .

To overcome the proxies' weaknesses and to test all potential causes simultaneously I finally turn to results of the non-linear least squares models following eqs. 5.8-5.10 (see Table 5.3). In the baseline model (M1) neither  $\alpha$  nor  $\beta$  is estimated SES-group-specifically; the only factor varying across SES-groups is  $\rho$ , given that the regression relies on the SES-specific school compositions observed empirically. In line with expectations,  $\alpha$  is smaller than 1 ( $\hat{\alpha}_0 \approx 0.395$ ,  $se \approx 0.062$ ). According to the model, immigrants make  $\sim 22\%$  of their social encounters in their neighborhoods ( $\hat{\beta}_0 \approx 0.220$ ,  $se \approx 0.087$ ). Model 2 allows for SES-specific values of  $\alpha$ . In line with expectations, high-SES immigrants show higher levels of  $\alpha$  than do low-SES immigrants, given that  $\hat{\alpha}_{ses}$  is positive and significantly different from

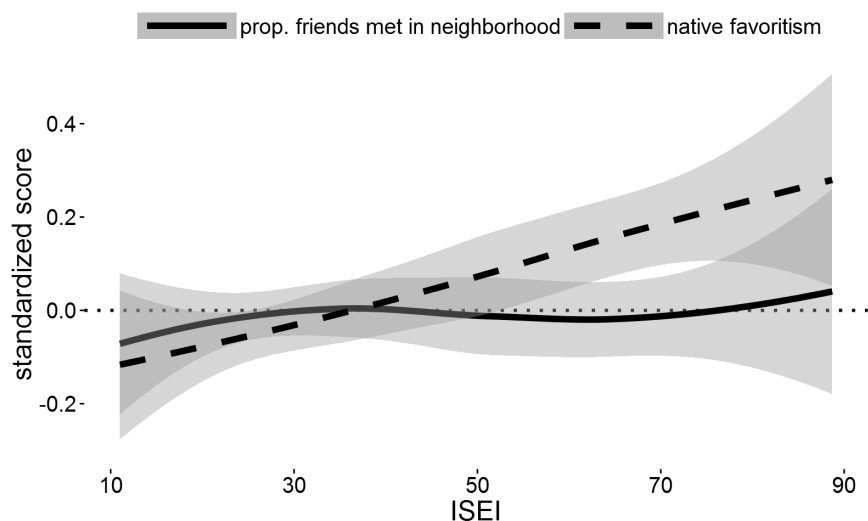


Figure 5.4: Proxies for  $\alpha$  and  $\beta$  across immigrants' SES. LOWESS trends of standardized scores, unweighted (grey-shaded areas indicating 95% c. i.).

zero ( $\hat{\alpha}_{ses} \approx 0.003$ ,  $se \approx 0.001$ ). In a next step, model 3 allows for SES-specific levels of  $\beta$ . Results suggest that high-SES immigrants make a higher share of their encounters in the neighborhood than do low-SES immigrants ( $\hat{\beta}_{ses} \approx 0.005$ ,  $se \approx 0.001$ ). Finally, model 4 tests all causes for SES-specific neighborhood effects simultaneously. Doing so shows that the SES differences in  $\beta$  from M3 do not stand their ground; effects change direction and are no longer statistically different from zero ( $\hat{\beta}_{ses} \approx 0.002$ ,  $se \approx 0.002$ ). The indication for SES-specific  $\alpha$  values, however, remains strong and statistically significant ( $\hat{\alpha}_{ses} \approx 0.005$ ,  $se \approx 0.002$ ). Summarizing, the analyses suggest SES-specific values of  $\alpha$ ,  $\gamma_0$  and of  $\gamma_1$ , but not of  $\beta$ .

Table 5.3: Non-linear least squares regression results (dep. var.: native proportion among friends)

	M1: baseline model			M2: + SES-specific $\alpha$			M3: + SES-specific $\beta$			M4: + SES-specific $\alpha$ + SES-specific $\beta$		
	coef	se		coef	se		coef	se		coef	se	
$\hat{\alpha}_0$	0.395	0.062	***	0.330	0.054	***	0.351	0.060	***	0.320	0.049	***
$\hat{\alpha}_{ses}$				0.003	0.001	***				0.005	0.002	**
$\hat{\beta}_0$	0.220	0.087	*	0.298	0.091	**	0.266	0.092	**	0.321	0.085	***
$\hat{\beta}_{ses}$							0.005	0.001	***	-0.002	0.002	
		yes			yes			yes			yes	
N(schools)		144			144			144			144	
N(students)		2,393			2,393			2,393			2,393	

Source: CILS4EU, w1, v1.1.0 / Microm, unweighted

Note: \*  $p < .05$  \*\*  $p < .01$  \*\*\*  $p < .001$ . Results from 10 multiply-imputed datasets combined via Rubin's rules (Rubin, 1987).

All standard errors are cluster-corrected. For complete model results, see Table A.16 in Appendix XIII.

## 5.6 Conclusion

This chapter investigated the causes of SES-specific neighborhood effects on young immigrants' friendships with natives in Germany. Results suggest that the neighborhood effect on their friendships with natives is non-linear. Accounting for this non-linearity SES differences persist (though weaker than under linearity assumptions), with the high-SES group being subject to stronger effects than the low-SES group. In consequence, living in ethnically concentrated neighborhoods almost always implies living a separate life (i.e. having a very small share of native friends), regardless of immigrants' SES. In contrast, residing in native neighborhoods does not automatically imply having similarly higher native proportions among one's friends. High-SES immigrants benefit more from their native residential environment than do low-SES immigrants.

Both descriptive evidence and non-linear least squares estimates suggest that these SES-specific neighborhood effects do not result from SES differences in neighborhood importance: young immigrants in Germany all rely more or less

equally on their neighborhood when making friends, regardless of their SES. Instead, there is indication for two other reasons. First, high-SES immigrants attend schools with on average higher native proportions, yielding different opportunities for native friends even when neighborhood compositions are identical. The segregating impact of ability tracking—as demonstrated in the previous chapter—seems to be a central cause. Second, the analyses show indication for SES-specific  $\alpha$  values, suggesting that SES-differences could exist in terms of friendship preferences, with low-SES immigrants tending more towards the ethnic ingroup and/or natives tending more toward high-SES immigrants. These differences may contribute to the emergence of SES-specific neighborhood effects.

Additional analyses provided substantially identical results for young immigrants in the Netherlands (cf. Kruse, 2017). The findings may thus be generalizable for young immigrants attending school systems that deploy a strict form of ability tracking (like Germany and the Netherlands). Moreover, they corroborate previous findings concerning immigrants of all ages in the Netherlands (van der Laan Bouma-Doff, 2007). Results differ, however, from what previous work showed for adult immigrants in Germany (Schlueter, 2012). Here, low-SES immigrants were the ones whose friendship compositions were more closely related to their neighborhood compositions. The framework of friendship formation may provide an answer why: Regarding different groups of immigrants—like adults versus adolescents—each of the proposed mechanisms might be more or less dominant. SES differences in neighborhood importance may evolve at later points in time, given that the low-SES group remains locally more stable over the life course and the high-SES group becomes more mobile. Finding different results for adolescents than for adults is therefore not surprising.

Some issues could still not be fully addressed. Most importantly, uncertainty remains whether SES-specific  $\alpha$  values are really caused by SES differences in friendship preferences, be it of immigrants or of natives. Previous research concerning interethnic friendships showed that general tie formation mechanisms amplify ethnic bonding (Goodreau et al., 2009; Wimmer and Lewis, 2010). Whereas I accounted for these tendencies in Chapter 3, this chapter’s approach did not allow me to control for such tie formation mechanisms in the presented analyses, suggesting that  $\alpha$  captured more than only friendship preferences. Future

## 5. Solving the puzzle

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research should therefore test whether SES differences in friendship preferences persist in a dyadic, network-analytical approach.



## **Chapter 6**

### **Conclusion**

## 6.1 Barriers for all, bridges for some

This book examined how residential patterns determine the social integration of young immigrants in Germany; whether a lack of native neighbors necessarily comes with a lack of native friends (i.e., residential barriers) and whether living among native neighbors always implies having more native friends (i.e., residential bridges).

Chapter 1 demonstrated that residential barriers apply universally among young immigrants in Germany—residential bridges, however, do not. Whereas high-SES immigrants profit greatly from native neighbors, low-SES immigrants do so only to a lesser extent. This finding came as a surprise given that low-SES immigrants are usually thought to depend on their neighborhoods when making friends, while high-SES immigrants are seen as more mobile (Logan and Spitze, 1994; Schlueter, 2012). As such, it led to this book’s central research question:

Why are residential barriers to the social integration of young immigrants in Germany universal, whereas residential bridges emerge primarily for high-SES immigrants?

As I will recap in this final chapter, the previous pages provide a clear answer why. First, I will summarize each chapter separately. I will state the central research question of the chapter, provide both a short and a longer answer, and discuss implications concerning the book’s central research question. Subsequently, I will combine all insights in a thought experiment that helps to illustrate the book’s take-home message. Finally, the chapter closes with a short discussion of shortcomings of this book that open avenues for future research.

## 6.2 Toward an answer

### Chapter 2

In Chapter 2 I tested whether the available measures of neighborhood composition are even appropriate to indicate the presence of residential barriers and



bridges. This seemed necessary, as I had to rely on a second-best option when measuring the ethnic compositions of German neighborhoods: name-based measures of context composition from the Microm neighborhood data. As such, I wanted to know:

Are name-based measures of neighborhood composition appropriate to indicate residential barriers and bridges?

*The answer in a nutshell:* Yes, they are—given that the Microm data correct for a name-based classification bias.

*A slightly longer answer:* Instead of relying on conventional indicators, like residents' (parents') country of birth, the Microm data derive the ethnic composition of a neighborhood from the ethnic origin of residents' names (cf. Mateos, 2007). Some ethnic origins, however, are harder to trace than others, resulting in ethnically specific error rates (Schnell et al., 2014). Given that neighborhoods vary in their ethnic mix, name-based measures of neighborhood composition may thus be subject to neighborhood-specific measurement bias. In this second chapter I tested the form and extent of such bias.

Conducting a name-based classification of the German CILS4EU sample I first derived ethnically specific error rates. The resulting rates corroborated previous findings: Culturally distant ethnic groups were almost always correctly identified as non-native. Error rates among Poles and groups from the Former Soviet Union, however, were much higher. Based on these error rates I simulated a name-based classification of the population of two exemplary German cities, Nuremberg and Berlin. Comparing the simulated name-based measures of neighborhood compositions to those from administrative data of the two cities gave an impression of the form and the extent of the induced bias.

In line with expectations, the name-based classification bias varied across neighborhoods: Native proportions were underestimated in native neighborhoods and overestimated in ethnically mixed neighborhoods. In other words, name-based classification led to an underestimation of variation in context measures.

The chapter closed with a discussion of different ways to account for this name-based classification bias. One such approach has been applied to the Microm

neighborhood data; using ex-post corrections based on additional context data that correlates with residents' ethnicity. As such, I could be confident about applying the Microm neighborhood measure as an indicator of residential barriers and bridges.

### Chapter 3

Since measurement error could not explain why residential barriers are universal and residential bridges are SES-specific, I wanted to learn about the mechanisms leading to residential barriers and bridges in general. Chapter 3 therefore asked how neighborhoods affect adolescents' interethnic friendship choices. Putting the spatially informed framework of friendship formation—introduced in Chapter 1—to a first empirical test I wanted to know:

Do neighborhoods determine more than the availability of outgroup peers in meeting contexts?

*The answer in a nutshell:* No, they do not—at least not among adolescents in Germany.

*A slightly longer answer:* Previous research provided both theoretical arguments and empirical evidence that neighborhoods may affect more than the size of the outgroup available as potential friends. A first argument—based on intergroup contact theory (Allport, 1954)—suggests that increased contact to the outgroup would improve actors' attitudes toward them (Vermeij et al., 2009). As such, adolescents exposed to many outgroup members in their neighborhoods would be less homophilous in their friendship choices (i.e., exposure effect). Outgroup neighbors therefore do not only affect the availability of potential outgroup friends, they also make adolescents more likely to accept outgroup peers as friends. A second argument—based on the idea of foci structuring social interaction (Feld, 1981)—suggests that spatial propinquity provides additional opportunities for contact within a given meeting context (Mouw and Entwisle, 2006). For example, neighbors attending the same school are more likely to establish contact at school than non-neighbors, given that they share their way to

school. Outgroup neighbors therefore do not only affect the availability of potential outgroup friends, they also increase the frequency of meeting outgroup peers (i.e., propinquity effect).

This third chapter was the first empirical test that investigated these different mechanisms simultaneously. To test whether propinquity or exposure effects existed net of the availability mechanism, the latter had to be controlled for. I therefore examined friendship formation in a context with clearly defined boundaries—the school—allowing me to control explicitly for outgroup availability. More specifically, I made use of the classroom network data from the first wave of the CILS4EU data.

Results suggested that adolescents' place of residence affects little more than the availability of the outgroup. I found no indication that outgroup exposure in the neighborhood made adolescents either more or less open towards outgroup friends. Spatial propinquity made friendships more likely (both directly and transitively). However, its contribution to ethnic homogeneity in friendships was negligible, simply because ethnic segregation is only moderate in German neighborhoods.

Concerning the puzzle of this book, these findings provided two insights. First, residential barriers and bridges primarily emerge via the availability mechanism (see final version of spatially informed framework in Figure 6.1). Second, the availability mechanism must amplify segregation patterns in some way, given that neighborhood segregation was too low to affect friendship homogeneity via propinquity but not via availability.

### Chapter 4

The next step was therefore to examine the availability mechanism more closely. This meant turning the focus from adolescents' friendship choices to their context choices. Acknowledging that adolescents' most important meeting context is the school, Chapter 4 asked:

(Why) is ethnic segregation in German secondary schools stronger than in respective neighborhoods?

*The answer in a nutshell:* Ethnic segregation in secondary schools exceeds residential patterns throughout Germany because of an interplay between the institutional rule of ability tracking and school avoidance behavior.

*A slightly longer answer:* Previous research provided little information about the extent and the causes of ethnic segregation in the German secondary school system. What it did provide, however, were two assertions: First, due to ethnic disparities in achievement the institutional rule of ability tracking increases ethnic segregation in schools (Shavit, 1984). Second, by avoiding local schools with high immigrant shares native parents' school choices increase school segregation, as well (i.e., white flight). Taking these assertions as a starting point, I wanted to test the causes, learn about the actual extent of ethnic segregation in German secondary schools and, most importantly, examine whether there would be an interplay between the institutional setting (i.e., ability tracking) parents face and the school choices they take (i.e., white flight).

To do so, I introduced a method based on counterfactual reasoning to decompose observed school segregation into a part that is due to sorting across tracks and another part due to sorting within each track. Moreover, I took advantage of a unique feature of the German secondary school system: regional variation in tracking strength. This allowed me to test whether tracking strength relates to segregation due to track sorting and/or to sorting within each track. All analyses relied on administrative data entailing geocoded information on all secondary schools in Germany in 2008/09.

Results corroborated that ethnic segregation in secondary schools exceeds residential patterns throughout Germany. Half of this net segregation could be accounted for by the institutional rule of ability tracking. Intriguingly, I saw a clear indication of a twofold effect of ability tracking, as theoretically expected: it increased school segregation via ethnically specific track sorting while at the same time decreasing it via school sorting within each track. In sum, this suggested that net segregation in German secondary schools results from an interplay between the institutional setting of the school system and parental white flight tendencies.

Concerning the puzzle of this book, the chapter's findings suggested that segregated neighborhoods indeed do not simply translate into segregated meeting contexts. Instead, segregation patterns are amplified, at least concerning adolescents' most important meeting context, their schools. I therefore adjusted the revised theoretical framework accordingly (see Figure 6.1). This insight helped to understand the assertion made in Chapter 3 that the availability mechanism affected friendship homogeneity whereas propinquity did not. Moreover, the chapter's findings already suggested where the SES-differences in residential bridges may really come from: ability tracking leads to an SES-specific sorting across school tracks and as such to SES-specific availability of outgroup peers.

### Chapter 5

Finally, in Chapter 5, I turned to solving the puzzle why residential barriers apply universally, whereas residential bridges emerge primarily for high-SES immigrants. Phrased differently, I wanted to know:

Why are neighborhood effects on young immigrants' friendships with natives SES-specific?

*The answer in a nutshell:* SES-specific neighborhood effects are in part due to misspecified models. Additionally, outgroup availability at school, and SES-specific friendship preferences (of immigrants, natives, or both at the same time) are responsible.

*A slightly longer answer:* As outlined, finding SES-specific neighborhood effects on young immigrants' friendships with natives was in line with previous findings (Schlueter, 2012; van der Laan Bouma-Doff, 2007). Finding stronger effects among high-SES immigrants, however, came as a surprise. Relying on the findings from previous Chapters 3 and 4, I aimed in Chapter 5 to lay out and test different reasons for these observed patterns.

To do so, I derived a formalized account of the re-revised theoretical framework. Returning to the first wave of the German CILS4EU data I tested the formal model explicitly.

Results suggested that SES-specific neighborhood effects detected in a linear model (as done in Chapter 1, see Figure 1.4) are partly an artefact due to model misspecification. Corrected model specifications, however, yielded weaker but still significant SES differences in the neighborhood effect. I could attribute these remaining SES differences to two reasons: First, there were SES-differences in availability; meaning that low-SES immigrants had fewer native peers available at school than high-SES immigrants did, even if they lived in the same neighborhood. Second, low- and high-SES immigrants differed in how likely they turn an encounter with a native into a friendship. This may have been due either to SES-specific friendship preferences or to a greater willingness among natives to accept high-SES immigrants as friends.

Figure 6.1 summarizes all findings in the finalized version of the spatially informed framework of friendship formation.

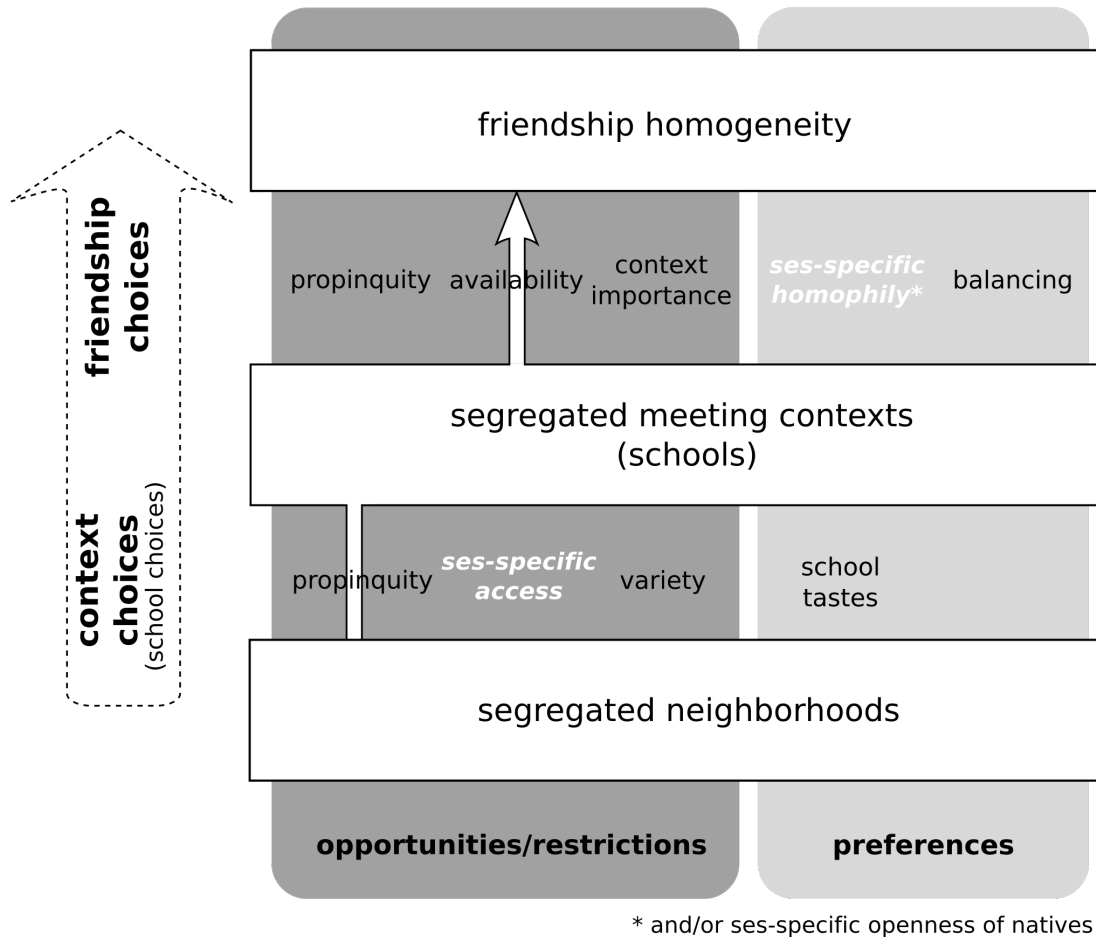


Figure 6.1: The spatially informed framework of friendship formation (final).

### 6.3 The take-home message

What do these results tell us? To see this, let us shortly return to our CILS4EU sample of young immigrants. The grey LOWESS trends in Figure 6.2 restate the book's explanandum (this time accounting for non-linearity in the relation and only for low- and high-SES groups, cf. Figure 1.4). We see universally applicable residential barriers and residential bridges that are SES-specific.

Chapter 5 taught us that these SES differences result in part from differences in availability (due to SES-specific access to schools, cf. 6.1) and in part from SES-specific friendship preferences. Of course, this information is already informative.

It becomes even more telling, however, if we think of a situation where only one of the reasons applied.

To conduct a short thought experiment, assume that we could desegregate young immigrants' meeting contexts such that net segregation is zero across schools, clubs, and all other meeting contexts. In other words, young immigrants face a situation where all their meeting context compositions perfectly align with their local neighborhood compositions; SES differences in availability (beyond neighborhood differences) are therefore absent. What residential barriers and bridges would young immigrants face then?

The black LOWESS trends in Figure 6.2 tell us.<sup>1</sup> The first thing to note, all black lines range above their grey counterparts: the absence of net segregation would boost native friendship rates for all young immigrants, regardless of their SES and of where they live. Second, residential bridges are now indeed universal: independent of immigrants' SES, living among natives would now imply being friends with them. Third, and maybe as a surprise, however, residential barriers would become more SES-specific: high-SES immigrants seem to profit more from the absence of net segregation than low-SES immigrants do.

This small thought experiment clearly conveys the book's take-home message: Neighborhood compositions alone tell us little about the existence and form of residential barriers and bridges to young immigrants' social integration. Not even compositions of meeting contexts or friendship preferences alone tell us much more; *it's the interplay between the latter two that does the trick.*

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<sup>1</sup>The black LOWESS trends in Figure 6.2 rely on predicted values derived from model 2 in Table 5.3, thereby setting respondents' school compositions equal to those of their neighborhoods.



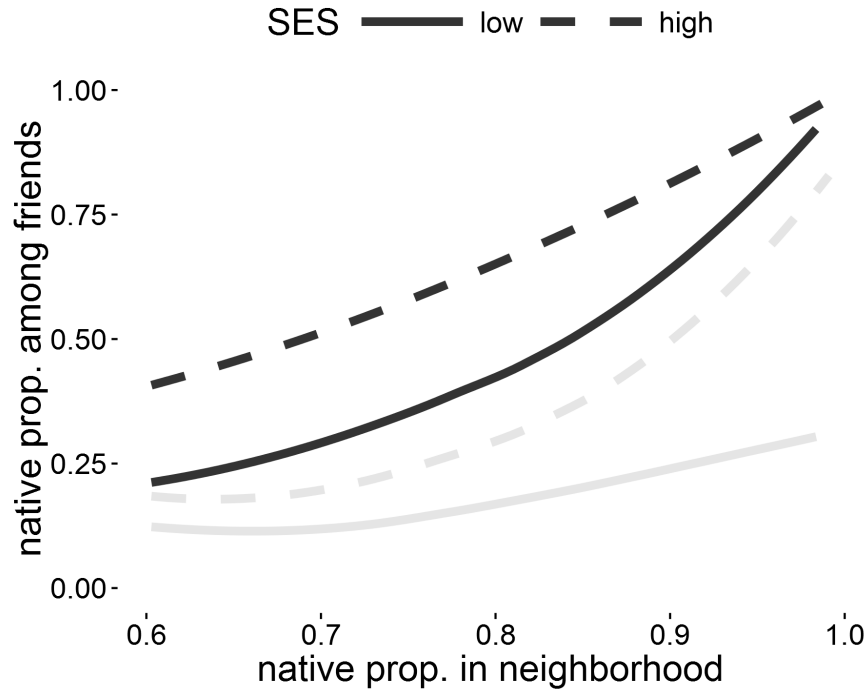


Figure 6.2: The relation between the native proportion among young immigrants' neighbors and their friends (SES-specific LOWESS trends; grey: actual, black: in absence of net segregation).

Focusing on only one of the two factors would imply that we either overlook that some young immigrants are unable to realize the friendships they wish for or that they may refuse to seize the meeting opportunities they face. Only a combined perspective on the (non-additive) interplay between availability and preferences—usually only possible in formalized terms—can reveal when preferences are the dominant mechanism driving friendship compositions or when it is the available opportunity structure.

This brings us back to the beginning of this book and to the question whether Chancellor Merkel's message really holds that for the social integration of young immigrants in Germany to succeed, neighborhoods matter greatly. As mentioned in the beginning, I did not write this book as practical guidance for policy makers. And yet, the outlined insights may hold a number of practical implications.

We saw that living in mixed neighborhoods indeed comes with a lack of social integration. From this perspective, efforts targeted at avoiding ethnically concentrated neighborhoods seem useful.<sup>2</sup>

However, my analyses suggest that such efforts alone will not automatically integrate young immigrants socially, as meeting contexts are segregated beyond residential patterns. Focusing on adolescents' most important meeting context, it would take additional efforts to not only desegregate German neighborhoods but to minimize net segregation in German schools, as well. One way to do so—potentially at the cost of further, unforeseen consequences—would be to rethink the institutional setting of ability tracking. As this book's analyses suggest, however, the desegregating impact of more comprehensive schooling would thereby be weaker than expected, due to increases in parental white flight.

But even if desegregating efforts concerning neighborhood and net segregation fully succeeded, immigrants' social integration would still not be a done deal. As the take-home message suggests, it is the interplay between availability and preferences that matters. Rising immigrant shares among the younger cohorts steadily reduce the maximum proportions of natives young immigrants can encounter in a German meeting context. For example, our cohort of ninth graders in the CILS4EU sample would—in the absence of any segregation—still encounter, on average, only 72% natives in a meeting context. In most cases, this provides more than enough opportunity to remain separate, if desired by either one of the two groups. This is why friendship preferences play an important role, as well. Efforts to ameliorate immigrants' openness toward native friends and natives' openness toward immigrant friends should therefore be a third concern if we want to arrive at a better social integration of young immigrants.

In a nutshell, Merkel's message thus describes a necessary condition for a successful social integration of young immigrants, but not a sufficient one. A native environment is what it all starts with—unfortunately, it is not the panacea to make integration work.

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<sup>2</sup>Time will tell whether the *Integration Act* with its *Residence Rule* is in this regard a success story or a failure.

## 6.4 What's next?

Examining residential barriers and bridges among young immigrants in Germany, this book focused on a specific type of social exchange among a specific age group of immigrants in a specific country at a specific point in time. Broadening the scope in one or more of these dimensions is thus certainly a logical next step. However, it may not be the most pressing and fruitful one.

With the take-home message in mind, other avenues for future research become obvious; they address the better identification of preferences, of availability, and of the interplay between the two. I will close this book by shortly laying out three more specific questions that I deem most essential.

### **What is really behind ethnic homophily?**

Even though it ranks among the most consistent findings in sociological research (cf. McPherson et al., 2001), (ethnic) homophily remains an unsettled phenomenon calling for further attention. Empirically, the taste for ethnically similar friends is usually measured in terms of the odds of observing an ingroup versus an outgroup friendship, net of other tie formation mechanisms controlled for (e.g., Goodreau et al., 2009; Kalter and Kruse, 2015; Smith et al., 2014). In this book, I proceeded identically: in Chapter 5, I derived a crude estimate of ethnic homophily among immigrants versus natives by controlling for availability and context importance only. The analytical setup in Chapter 3 allowed me to be somewhat more specific, as I additionally controlled for balancing mechanisms and propinquity when estimating the extent of homophily among specific ethnic groups of adolescents.

Such a residual approach, however, has a central disadvantage: regardless of the additional controls, we can never be sure that the resulting residual tendency toward the ingroup really reflects an explicit ethnic taste. Instead, it may result from unobserved additional meeting opportunities (cf. Mouw and Entwisle, 2006), from a preference among specific subgroups only (Wimmer and Lewis, 2010), or from an actual preference for attributes that only correlate with ethnicity (ibid.). All these explanations, however, would have important consequences concerning a better understanding of immigrants' process of social integration.

We therefore need to know better. Current research turns more and more toward the role of religiosity in friendship formation, thereby indicating its importance (see, for example, Leszczensky and Pink, forthcoming). As such, future research should examine whether differences in religious affiliation and practice between natives and immigrants may be one central ingredient of the emergent phenomenon of ethnic homophily. Methodologically, (quasi-)experimental approaches could help to overcome the shortcomings of the outlined residual approach, while still guaranteeing control over the availability of outgroup peers. Alternatively, qualitative evidence on how adolescents make their friendship choices may be helpful as well.

### **Is ethnic homophily context-dependent?**

Closely related—but not yet resolved—is the question to what extent ethnic homophily may be context-dependent. In my analyses I found no support for an exposure effect on ethnic homophily; adolescents' neighborhood compositions did not affect their friendship preferences, even though intergroup contact theory suggested otherwise. There may have been different reasons why I found no such effect: contact among neighbors is too artificial to have an independent effect on outgroup attitudes; variation in neighborhood exposure may have been too small due to only moderate levels of neighborhood segregation in Germany. Irrespective of the reasons, throughout this book I assumed that friendship preferences are context-independent (see, for example Chapter 3 and Figure 6.2).

Admittedly, however, the null finding of a neighborhood exposure effect does not imply that ethnic homophily would be generally independent of the contexts that adolescents interact in. Other contexts than the neighborhood—especially those being more segregated and where contact is more direct—may still affect adolescents' friendship preferences. For example, a number of scholars argue and provide evidence for an effect on ethnic homophily by the ethnic composition of schools (Moody, 2001; Smith et al., 2016). However, recent work challenged these findings by showing that they may be due to model misspecification (Flache, 2016).

One promising course toward better knowledge about the context-dependence of homophily would therefore be methodological advances that allow for re-analyses of previous findings concerning the school context based on more appropriate model specifications. Does ethnic homophily then still vary across schools with different ethnic compositions?

Beside such technical questions, I additionally suggest further theoretical development. To this date, most arguments concerning context-dependent homophily relied on theories of intergroup contact, of ethnic threat or on both perspectives (Kruse et al., 2016; Moody, 2001; Vermeij et al., 2009). Whereas these accounts are helpful, they ignore the multidimensionality of homophily. As mentioned, ethnicity is only one individual attribute among many others that may define a person's group belonging. Whether ethnicity becomes a salient characteristic in tie formation—or gender, religiosity, or SES instead—may thereby vary across contexts. Theoretical approaches that explicitly aim to explain group belonging, for example a *social boundary making* perspective (Wimmer, 2013), may help to identify such contextual differences.

### **What is really behind white flight?**

Finally, a central open question remained concerning the emergence of segregated meeting opportunities. Providing a more encompassing perspective on residential barriers and bridges this book did not only examine friendship choices but also adolescents' school choices. I demonstrated that net segregation in German secondary schools exists due to a non-trivial interplay of ability tracking and school avoidance behavior. Whereas this book provided a clearer image about how ability tracking affects ethnic segregation in schools—namely in a twofold way—it remains an open question whether the observed school avoidance behavior within tracks is really due to explicit white flight.

Recent evidence suggests that much of this parental school avoidance is actually due to explicit ethnic/racial bias, at least so concerning school choices in the U.S. (Billingham and Hunt, 2016). Whether the same applies for the case of German secondary schooling, however, is unclear. It may just as well be that the ethnic compositions of schools in Germany primarily serve as proxies for otherwise

unobserved school quality (Wells and Crain, 1992). Given the latter, supposed white flight may actually be an avoidance of low-quality schools, a finding that would carry important implications with regard to desegregation efforts.

Future research should therefore try to tackle the question what it really is that lets parents and their children avoid ethnically mixed schools in Germany. Given that parents are often unwilling to admit an ethnic/racial bias in surveys, the most promising approach may thereby be to assess parents' *revealed* preferences. One example are analyses of parents' online search patterns of potential schools (cf. Schneider and Buckley, 2002). Another option is to examine adolescents' home-to-school distances; providing a more direct test of who is willing to walk the extra mile to avoid local immigrant-dominated schools.







# Appendices

## I. Results concerning the name-based classification bias (Chapter 2)

Table A.1: Logistic model results (dep.var.: false classification, complete sample)

	coef	s.e.
Intercept	-2.533	0.075 **
Immigrant (ref.: <i>Native</i> )	1.171	0.091 **
AIC	3,790.8	
N(students)	4,996	

*Source:* CILS4EU, w1, v1.2.0, unweighted  
*Note:* \* p-value<.01 \*\* p-value<.001.

Table A.2: Logistic model results (dep.var.: false classification, immigrants only)

	coef	s.e.
Intercept	-4.931	0.278 **
Ethnic group (ref.: <i>Turkish</i> )		
<i>FSU</i>	4.567	0.274 **
<i>Polish</i>	4.919	0.291 **
<i>FYR</i>	1.261	0.345 **
<i>Other Western</i>	2.956	0.254 **
<i>Other Non-Western</i>	2.050	0.264 **
1st generation (ref.: <i>2nd</i> )	1.273	0.168 **
AIC	1,678.2	
N(students)	2,387	

*Source:* CILS4EU, w1, v1.2.0, unweighted  
*Note:* \* p-value<.01 \*\* p-value<.001.

## II. Neighborhood compositional data from two German cities (Chapter 2)

Table A.3: Neighborhood compositional data from local statistics (Nuremberg, Berlin)

	Nuremberg	Berlin	Total
N (neighborhoods)	81	447	528
Neighborhood population			
<i>mean</i>	6,365.1	7,969.1	7,723.0
<i>s.d.</i>	3,838.5	5,319.5	5,149.7
Ethnic grouping			
<i>Native</i>	Total net of immigrants	Total net of immigrants	
<i>Turkish</i>	Turkey	Turkey	
<i>FSU</i>	Russia + Ukraine	FSU + Kasachstan	
<i>FYR</i>	FYR	FYR	
<i>Other Western</i>	Europe (net of Turkey, Russia, FYR, Poland) + Australia/America	EU (net of Poland, Croatia) + USA	
<i>Other Non-Western</i>	Immigrants net of above cate- gories	Immigrants net of above categories	

Source: Kommunalstatistik Nürnberg and Berlin

### III. Deriving a correction factor for the name-based classification bias (Chapter 2)

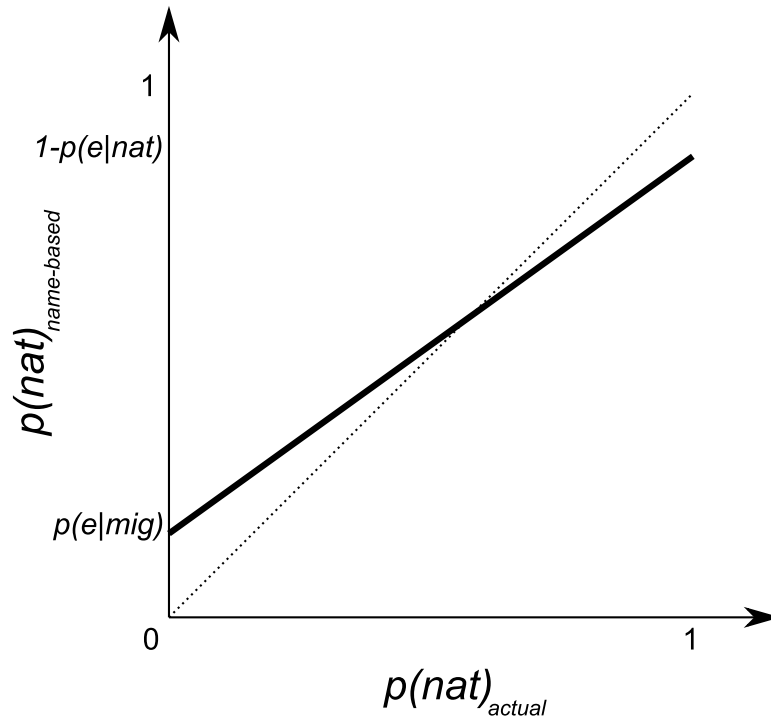


Figure A.1: Relation between actual and name-based native proportions in neighborhoods given overall error rates of immigrants and natives

Deriving a correction factor based on the overall error rates of immigrants  $p(e|mig)$  and of natives  $p(e|nat)$  is rather intuitive. Assuming both error rates to be positive, we know that an all immigrant neighborhood (i.e.,  $p(nat)_{actual} = 0$ ) would be falsely identified as having a native proportion of  $p(nat)_{name-based} = p(e|mig)$ . Vice versa, an all native neighborhood (i.e.,  $p(nat)_{actual} = 1$ ) would not be identified as such but as having a native proportion of  $p(nat)_{name-based} = 1 - p(e|nat)$ . The resulting relation between name-based and actual native proportions in the neighborhoods would thus look as depicted by the solid black line in Figure A.1. It is easy to see that the function's intercept is  $p(e|mig)$  and its slope is  $[1 -$

$p(e|nat) - p(e|mig)$ ], yielding

$$p(nat)_{name-based} = p(e|mig) + [1 - p(e|nat) - p(e|mig)]p(nat)_{actual} \quad (A.1)$$

Simple rearranging leads to the correction factor with

$$p(nat)_{actual} = \frac{p(nat)_{name-based} - p(e|mig)}{1 - p(e|nat) - p(e|mig)} \quad (A.2)$$

## IV. Deriving counterfactual school compositions in the tracking scenario (Chapter 4)

The tracking scenario restricts students' school choices to a specific track type. Consequentially, the ethnic compositions of the as-if catchment areas do not directly yield the counterfactual grade compositions. Instead, the ethnic composition of a high-track school  $j$ , formally  $p_j(nat|H)$ , is a function of the ethnic composition of the as-if catchment area,  $p_j(nat)$ , as well as of the local high-track attendance rate of natives,  $p_j(H|nat)$ , and of immigrants,  $p_j(H|mig)$ . Based on these three quantities, the school composition of a high-track school  $j$  (same logic applying to low-track schools, respectively) can be determined by applying Bayes' theorem with

$$p_j(nat|H) = \frac{p_j(H|nat)p_j(nat)}{(p_j(H|nat)p_j(nat) + p_j(H|mig)(1 - p_j(nat))}. \quad (\text{A.3})$$

## V. Results concerning the twofold tracking effect (Chapter 4)

Table A.4: OLS/FE model results (dep.var.:  $D_{net}$ )

	M1		M2		M3	
	coef	s.e.	coef	s.e.	coef	s.e.
Constant	0.084	0.029 ***	-0.150	0.068 **		
Trackingstrength	0.186	0.034 ***	0.172	0.034 ***	0.028	0.028 **
Prop. native students			0.254	0.076 ***	0.303	0.062 ***
Prop. in private school			0.226	0.068 ***	0.245	0.053 ***
Number of schools (in 10)			0.001	0.002	0.004	0.001 ***
Adj. R-squared		0.15		0.23		0.15
N(cohort-specific districts)		458		458		458

Source: Statistische Ämter des Bundes und der Länder

Note: \* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected

Table A.5: OLS/FE model results (dep.var.:  $D_{tracksorting}$ )

	M1		M2		M3	
	coef	s.e.	coef	s.e.	coef	s.e.
Constant	-0.017	0.029	-0.046	0.064		
Trackingstrength	0.235	0.034 ***	0.238	0.034 ***	0.231	0.045 ***
Prop. native students			0.035	0.070	0.063	0.051
Prop. in private school			0.026	0.080	0.005	0.052
Number of schools (in 10)			-0.002	0.002	-0.001	0.001
Adj. R-squared		0.28		0.29		0.09
N(cohort-specific districts)		458		458		458

Source: Statistische Ämter des Bundes und der Länder

Note: \* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected

Table A.6: OLS/FE model results (dep.var.:  $D_{schoolsorting}$ )

	M1			M2			M3		
	coef	s.e.		coef	s.e.		coef	s.e.	
Constant	0.101	0.024	***	-0.104	0.047	**			
Trackingstrength	-0.049	0.027	*	-0.065	0.028	***	-0.105	0.040	***
Prop. native students				0.219	0.055	***	0.240	0.036	***
Prop. in private school				0.200	0.064	***	0.240	0.041	***
Number of schools (in 10)				0.003	0.001	***	0.005	0.001	***
Adj. R-squared		0.02			0.12			0.11	
N(cohort-specific districts)		458			458			458	

Source: Statistische Ämter des Bundes und der Länder

Note: \*  $p < .1$  \*\*  $p < .05$  \*\*\*  $p < .01$ . All standard errors are cluster-corrected



## VI. Robustness checks concerning the twofold tracking effect (Chapter 4)

Table A.7: FE model results (alternative assignment rules, dep.var.:  $D_{net}$ )

	M3 (1 km school radius)			M3 (2 km school radius)			M3 (3 km school radius)		
	coef	s.e.		coef	s.e.		coef	s.e.	
Trackingstrength	0.118	0.050	***	0.082	0.047	***	0.065	0.056	
Prop. native students	0.279	0.058	***	0.014	0.033		-0.218	0.038	***
Prop. in private school	0.204	0.045	***	0.284	0.051	***	0.318	0.062	***
Number of schools (in 10)	0.004	0.001	***	-0.001	0.001		-0.004	0.001	***
Adj. R-squared		0.14			0.10			0.12	
N(cohort-specific districts)		458			458			458	

Source: Statistische Ämter des Bundes und der Länder

Note: \* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected

Table A.8: FE model results (alternative assignment rules, dep.var.:  $D_{tracksorting}$ )

	M3 (1 km school radius)			M3 (2 km school radius)			M3 (3 km school radius)		
	coef	s.e.		coef	s.e.		coef	s.e.	
Trackingstrength	0.212	0.036	***	0.183	0.037	***	0.163	0.044	
Prop. native students	0.053	0.046		-0.146	0.028	***	-0.302	0.033	***
Prop. in private school	-0.030	0.040		0.029	0.043		0.063	0.061	***
Number of schools (in 10)	0.000	0.001		-0.004	0.001	***	-0.006	0.001	***
Adj. R-squared		0.07			0.05			0.07	
N(cohort-specific districts)		458			458			458	

Source: Statistische Ämter des Bundes und der Länder

Note: \* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected

Table A.9: FE model results (alternative assignment rules, dep.var.:  $D_{schoolsorting}$ )

	M3 (1 km school radius)			M3 (2 km school radius)			M3 (3 km school radius)		
	coef	s.e.		coef	s.e.		coef	s.e.	
Trackingstrength	-0.095	0.040	***	-0.100	0.037	***	-0.098	0.038	***
Prop. native students	0.227	0.036	***	0.160	0.033	***	0.084	0.036	***
Prop. in private school	0.234	0.042	***	0.254	0.041	***	0.255	0.044	***
Number of schools (in 10)	0.004	0.001	***	0.003	0.001	***	0.002	0.001	
Adj. R-squared		0.10			0.10			0.08	
N(cohort-specific districts)		458			458			458	

Source: Statistische Ämter des Bundes und der Länder

Note: \* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected

Table A.10: FE model results (alternative unit of analysis, dep.var.:  $D_{net}$ )

	M3 (2 km remote clusters)			M3 (4 km remote clusters)			M3 (6 km remote clusters)		
	coef	s.e.		coef	s.e.		coef	s.e.	
Trackingstrength	0.295	0.070	***	0.221	0.049	***	0.116	0.073	
Prop. native students	-0.103	0.225		0.829	0.328	***	1.171	0.310	***
Prop. in private school	0.036	0.098		0.314	0.050	***	0.340	0.048	***
Number of schools (in 10)	0.008	0.014		0.031	0.013	***	0.067	0.041	
Adj. R-squared		0.15			0.18			0.19	
N(cohort-specific districts)		319			108			60	

Source: Statistische Ämter des Bundes und der Länder

Note: \* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected

Table A.11: FE model results (alternative unit of analysis, dep.var.:  $D_{tracksorting}$ )

	M3 (2 km remote clusters)			M3 (4 km remote clusters)			M3 (6 km remote clusters)		
	coef	s.e.		coef	s.e.		coef	s.e.	
Trackingstrength	0.212	0.036	***	0.183	0.037	***	0.163	0.044	
Prop. native students	0.053	0.046		-0.146	0.028	***	-0.302	0.033	***
Prop. in private school	-0.030	0.040		0.029	0.043		0.063	0.061	***
Number of schools (in 10)	0.000	0.001		-0.004	0.001	***	-0.006	0.001	***
Adj. R-squared		0.07			0.05			0.07	
N(cohort-specific districts)		458			458			458	

Source: Statistische Ämter des Bundes und der Länder

Note: \* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected

Table A.12: FE model results (alternative unit of analysis, dep.var.:  $D_{schoolsorting}$ )

	M3 (2 km remote clusters)		M3 (4 km remote clusters)		M3 (6 km remote clusters)	
	coef	s.e.	coef	s.e.	coef	s.e.
Trackingstrength	0.015	0.055	-0.060	0.053	-0.106	0.080
Prop. native students	0.300	0.205	0.805	0.589	0.959	0.742
Prop. in private school	0.015	0.063	-0.076	0.044	-0.050	0.040
Number of schools (in 10)	-0.018	0.014	0.016	0.013	0.027	0.037
Adj. R-squared		0.10		0.10		0.08
N(cohort-specific districts)		458		458		458

Source: Statistische Ämter des Bundes und der Länder

Note: \* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected

## VII. Assumed causal relations concerning SES-specific neighborhood effects (Chapter 5)

Observed correlations between neighborhood and friendship compositions may be spurious due to determinants affecting both immigrants' neighborhood and friendship choices at the same time. More recently arrived immigrants are more likely than later immigrant generations to sort into contexts and areas with low native proportions, given that they promise special (short-term) benefits for new immigrants (Wilson and Portes, 1980). At the same time, recently arrived immigrants are still less acculturated and have less social contact to natives—regardless of their place of residence—making immigrants' age of arrival a potential confounder. Similar arguments also hold for immigrants' ethnicity or their SES.

Figure A.2 summarizes the assumed causal relations graphically in a so-called directed acyclic graph (from here on DAG, see Morgan and Winship, 2007). At the center of interest is the effect of  $n$  on  $p(nat|f)$ —both directly and via  $o$ —and how  $ses$  affects this relation. Note that DAGs do not make any assumptions about the concrete functional form of the assumed causal relations. From this perspective, Figure A.1 is compatible with the idea of an interaction  $ses*n$  affecting  $p(nat|f)$ . Furthermore, Figure A.2 is also in line with the idea that  $\alpha$ ,  $\beta$ ,  $o$ , and  $n$  affect  $p(nat|f)$  interdependently, as postulated in the formal model of interethnic friendship formation in eq. 5.4.

To meet the challenge of potential confounders the article applies cross-sectional OLS models regressing the native proportion among immigrants' friends on the interaction between their SES and the native proportion in the neighborhood, including controls for immigrants' SES, their ethnic background, and their age of arrival. In addition, two further neighborhood characteristics are controlled: the age composition and the social composition of the neighborhood.

To avoid an underestimation of the SES differences, the presented OLS models do not account for respondents' school compositions. In line with Figure A.1, it suffices to condition on the outlined confounders as well as  $ses$  to block all potential 'backdoor paths' (ibid.) of the actual relation of interest, assuming that no further unobserved confounders exist. The fact that  $ses$  is both a confounder and part of the interaction of interest is thereby unproblematic.

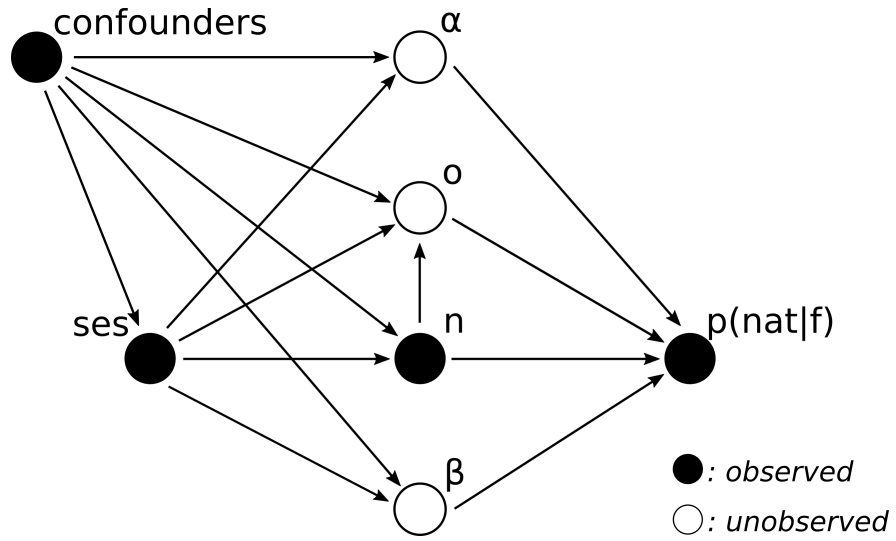


Figure A.2: Assumed causal relations

## VIII. Results concerning SES-specific neighborhood effects (Chapter 5)

Table A.13: Summary statistics

	Original data					Imputed datasets*			
	mean	s.d.	min	max	missing (in %)	mean	s.d.	min	max
Native prop. among friends	.307	.334	.000	1.000	.2	.307	.334	.000	1.000
Native prop. in neighborhood	.845	.103	.377	.996	10.9	.842	.104	.377	.998
SES	37.9	18.7	11.0	88.7	10.0	37.7	18.6	11.0	88.7
Native prop. at school	.395	.223	.025	.974	.0	.395	.223	.025	.974
Native favoritism	11.7	32.1	-100.0	100.0	8.9				
Prop. friends met in neighborhood	0.186	.279	.000	1.000	.2				
Ethnic background (in %)					0.0				
<i>Turkish</i>	36.3					36.3			
<i>FSU</i>	12.2					12.2			
<i>Polish</i>	7.0					7.0			
<i>FYR</i>	9.3					9.3			
<i>Other Western</i>	15.3					15.3			
<i>Other Non-Western</i>	19.9					19.9			
Age of arrival (in %)					2.6				
<i>11+</i>	4.3					4.4			
<i>6-10</i>	6.0					6.2			
<i>0-5</i>	12.0					12.3			
<i>born in Germany</i>	75.0					77.1			
Prop. aged 10-18 in neighborhood	.076	.013	.042	.112	10.9	.076	.013	.032	.116
Prop. unemployed in neighborhood	.104	.065	.000	.262	10.9	.105	.066	.000	.262

Source: CILS4EU, w1, v1.1.0 / Microm, unweighted

Note: \* 10 multiply-imputed datasets for (non-)linear least squares regression, Descriptives combined via Rubin's rules (Rubin, 1987)

Table A.14: OLS model results (dep.var.: native prop. among friends)

	M1 overall linear relation			M2 SES-specific linear relation			M3 SES-specific linear relation net of confounders			M4 SES-specific non-linear relation net of confounders		
	coef	s.e.		coef	s.e.		coef	s.e.		coef	s.e.	
Intercept	0.310	0.013	***	0.306	0.012	***	0.050	0.073		0.051	0.071	
Native prop. in neighborhood (centered)	1.194	0.101	***	1.143	0.089	***	0.707	0.105	***	1.084	0.139	***
SES (centered)				0.003	0.000	***	0.002	0.000	***	0.002	0.000	***
Native prop. in neighb. * SES (both centered)				0.017	0.004	***	0.015	0.004	***	0.012	0.003	***
Native prop. in neighb. squared (centered)										2.352	0.514	***
Ethnic background (ref.: <i>Turkish</i> )												
<i>FSU</i>							0.184	0.026	***	0.179	0.026	***
<i>Polish</i>							0.296	0.028	***	0.285	0.027	***
<i>FYR</i>							0.082	0.022	***	0.086	0.022	***
<i>Other Western</i>							0.212	0.024	***	0.207	0.025	***
<i>Other Non-Western</i>							0.118	0.020	***	0.117	0.020	***
Age of arrival (ref.: <i>11+</i> )												
<i>6-10</i>							0.063	0.037		0.061	0.061	
<i>0-5</i>							0.095	0.037	**	0.091	0.091	*
<i>born in Germany</i>							0.164	0.034	***	0.159	0.159	***
Prop. aged 10-18 in neighb.							0.655	0.741		0.220	0.723	
Prop. unemployed in neighb.							-0.418	0.185	*	-0.266	0.178	
Adj. R-squared (1st imp.)		0.14			0.19			0.27			0.28	
Adj. R-squared (2nd imp.)		0.14			0.19			0.28			0.29	
Adj. R-squared (3rd imp.)		0.14			0.18			0.27			0.28	
Adj. R-squared (4th imp.)		0.14			0.18			0.27			0.28	
Adj. R-squared (5th imp.)		0.14			0.19			0.27			0.28	
Adj. R-squared (6th imp.)		0.14			0.19			0.27			0.28	
Adj. R-squared (7th imp.)		0.14			0.19			0.27			0.28	
Adj. R-squared (8th imp.)		0.14			0.19			0.27			0.29	
Adj. R-squared (9th imp.)		0.14			0.19			0.27			0.28	
Adj. R-squared (10th imp.)		0.14			0.19			0.27			0.28	
N(schools)		144			144			144			144	
N(students)		2,393			2,393			2,393			2,393	

Source: CILS4EU, w1, v1.1.0 / Microm, unweighted

Note: \* 10 multiply-imputed datasets for (non-)linear least squares regression, Descriptives combined via Rubin's rules (Rubin, 1987).

\* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected.

Table A.15: OLS model results (dep.var.: native prop. in school)

	M1			M2		
	coef	s.e.		coef	s.e.	
Intercept	0.398	0.017	***	0.396	0.017	***
Native prop. in neighborhood (centered)	0.988	0.101	***	0.959	0.096	***
SES (centered)				0.002	0.000	***
Native prop. in neighb. * SES (both centered)				0.008	0.003	**
Adj. R-squared (1st imp.)		0.22			0.24	
Adj. R-squared (2nd imp.)		0.21			0.24	
Adj. R-squared (3rd imp.)		0.21			0.24	
Adj. R-squared (4th imp.)		0.22			0.24	
Adj. R-squared (5th imp.)		0.21			0.24	
Adj. R-squared (6th imp.)		0.22			0.25	
Adj. R-squared (7th imp.)		0.21			0.23	
Adj. R-squared (8th imp.)		0.22			0.24	
Adj. R-squared (9th imp.)		0.21			0.24	
Adj. R-squared (10th imp.)		0.21			0.24	
N(schools)		144			144	
N(students)		2,393			2,393	

Source: CILS4EU, w1, v1.1.0 / Microm, unweighted

Note: \* 10 multiply-imputed datasets for (non-)linear least squares regression, Descriptives combined via Rubin's rules (Rubin, 1987).

\* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected.



Table A.16: NLLS model results (dep.var.: native prop. among friends)

	M1 baseline model		M2 +SES-specific $\alpha$			M3 +SES-specific $\beta$			M4 +SES-specific $\alpha$ +SES-specific $\beta$			
	coef	s.e.		coef	s.e.		coef	s.e.		coef	s.e.	
$\alpha_0$	0.395	0.062	***	0.330	0.054	***	0.351	0.060	***	0.320	0.049	***
$\alpha_{ses}$				0.003	0.001	***				0.005	0.002	**
$\beta_0$	0.220	0.087	*	0.298	0.091	**	0.266	0.092	**	0.321	0.085	***
$\beta_{ses}$							0.005	0.001	***	-0.002	0.002	
$\alpha_{TurFYR}$	0.055	0.049		0.040	0.046		0.056	0.049		0.034	0.045	
$\alpha_{2ndgen}$	-0.240	0.065	***	-0.142	0.054	**	-0.201	0.062	***	-0.112	0.052	*
$\beta_{TurFYR}$	0.180	0.086	*	0.192	0.090	*	0.168	0.090		0.197	0.089	*
$\beta_{2ndgen}$	-0.042	0.107		-0.146	0.093		-0.056	0.106		-0.193	0.092	*
Adj. R-squared (1st imp.)	168.4			165.2			165.9			165.2		
Adj. R-squared (2nd imp.)	168.5			165.0			165.7			165.0		
Adj. R-squared (3rd imp.)	168.2			165.4			166.2			165.3		
Adj. R-squared (4th imp.)	168.4			165.4			166.2			165.2		
Adj. R-squared (5th imp.)	168.5			165.7			166.2			165.7		
Adj. R-squared (6th imp.)	168.7			165.5			166.0			165.5		
Adj. R-squared (7th imp.)	168.6			165.1			166.1			165.0		
Adj. R-squared (8th imp.)	168.6			165.6			166.4			165.5		
Adj. R-squared (9th imp.)	168.5			165.4			166.1			165.4		
Adj. R-squared (10th imp.)	169.0			166.0			166.9			165.8		
N(schools)	144			144			144			144		
N(students)	2,393			2,393			2,393			2,393		

Source: CILS4EU, w1, v1.1.0 / Microm, unweighted

Note: \* 10 multiply-imputed datasets for (non-)linear least squares regression, Descriptives combined via Rubin's rules (Rubin, 1987).

\* p<.1 \*\* p<.05 \*\*\* p<.01. All standard errors are cluster-corrected.



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