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Are They Hunkering Down? Revisiting the Relationship between Exposure to Ethnic Diversity, Intergroup Contact, and Group Trust

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Are They Hunkering Down?

Revisiting the Relationship between Exposure to Ethnic Diversity, Intergroup Contact, and Group Trust

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Abstract

Past research suggests that the migration-induced diversification of everyday living spaces creates uncertainty about shared norms and rules of engagement, leading individuals to "hunker down" and become distrustful. Theory distinguishes between mere exposure and actual contact effects. For mere exposure, the assumption is that ethnic diversity matters even in the absence of one-on-one interactions, as observing the unknown from afar will serve to activate negative prejudice which lowers trust. But diverse environments may also provide opportunities for positive contact, leading individuals to revise their distrust. Improving upon existing studies, we investigate simultaneously the association between group trust and diversity via static and cumulative mere exposure in the neighborhood setting and actual intergroup contact at the workplace, relying on administrative register data rather than self-reporting for our main predictors. We find that trust in neighbors is significantly negatively associated with cumulative exposure to ethnic diversity, while the widely-used measure of current exposure shows no effect. Workplace contact neither has a statistically significant association with trust in neighbors, nor does it mediate the negative association between neighborhood exposure and trust. We thus find some support for the hunkering down hypothesis, but also find that it takes much more precise measures of expo-sure than studies commonly use to reliably establish this effect.

Key Words: trust, ethnic diversity, exposure, contact, Sweden

Introduction

A growing body of research investigates the relationship between migration-induced ethnic diversity and social cohesion within and across Western democracies. Much of this research conceptualizes social cohesion in terms of identity-based or group-based trust and situates itself in direct response to Putnam's (2007) prediction that increased ethnic diversification of everyday living spaces will cause individuals across ethnic groups to "hunker down," that is, to withdraw from social life and become generally distrustful. The reasoning is that people who belong to the same ethno-cultural group tend to share common behaviors, traditions, and values which, among other things, facilitate trust (Stolle, 2002). By extension, diversity is believed to induce uncertainty in everyday transactions, making it more difficult for people to base their actions on trust rather than case-by-case assessments.

Studies have indeed revealed negative associations between ethnic diversity at various levels of aggregation (such as neighborhoods, municipalities, and countries) and trust at the individual level (Alesina and Ferrara, 2002; Gustavsson and Jordahl, 2008; Sturgis et al., 2011). However, Meer and Tolsma's (2014) detailed literature review has shown that it is ethnic diversity in residential settings that is most consistently negatively associated with trust, and with trust in neighbors only.

Sociological and social psychological theories on the relationship between ethnic diversity and trust distinguish between mere exposure and actual contact. First, for mere exposure the assumption is that diversity matters even in the absence of one-on-one interactions, as observing the unknown from afar will activate negative outgroup prejudice and hence lower trust (Hamilton and Bishop, 1976). Second, however, diverse environments may provide opportunities for positive intergroup contact that will lead individuals to revise their prejudicial distrust (Allport, 1954). However, due to data limitations, prior research has largely been unable to model both mere exposure and actual contacts simultaneously or

struggled with issues of endogeneity, due to the use of self-reported contact measures (see, e.g., Dinesen and Sønderskov, 2015; Laurence et al., 2017). Improving upon existing studies, we investigate the association between different forms of group trust and diversity via cumulative exposure to various ethnic groups in the neighborhood setting and then proxy for actual intergroup contact by workplace diversity, relying on administrative register data rather than self-reporting in both cases.

This empirical contribution is facilitated by the Swedish Social Networks and Xenophobia Survey of 2013, which we linked to official population statistics on neighborhoods and workplaces from the Swedish administrative registers. We know from Allport's definition of "meaningful contact" (1954, Ch. 16), and from prior research on workplace networks, that work sites are very likely to enforce direct contact among coworkers of different ethnic backgrounds, in particular in small and medium sized workplaces (Feld, 1982; Marsden, 1990; Mutz and Mondak, 2006). This is contrary to neighborhoods, which are less structurally constraining and hence less likely to lead to cross-ethnic contacts (Feld 1982), at least in countries in which sociability with neighbors is not a socially sanctioned norm (Edling and Rydgren, 2012). In this way, neighborhoods are likely sites of mere exposure to ethnic diversity, rather than places of actual contact.

We also improve upon the static exposure indicators used in earlier studies and utilize the longitudinal structure of the Swedish household registration system to measure exposure to ethnically diverse neighborhoods as a cumulative variable. Crisp et al. (2009) were among the first to suggest that the effect of exposure to ethnic diversity on trust is likely to develop incrementally, with the effect becoming more negative as the duration of exposure lengthens, but up until now data limitations have made an extensive exploration of this hypothesis impossible.

We find that cumulative exposure to the presence of Eastern European and non-European, non-Western immigrants is significantly negatively associated with trust in neighbors, regardless of their region of origin or ethnicity, while current exposure shows no significant effect. It is worth noting that neighborhood exposure to diversity does not appear to be relevant for outgroup trust, but that the negative association seems to be limited to generalized trust in neighbors of any ethnic background. Contrary to our expectations, workplace diversity – our proxy for actual contact – has neither an independently statistically significant association with trust in neighbors nor does it mediate the negative association between neighborhood exposure and trust. We thus find some support for Putnam's hunkering down hypothesis, but also find that it takes much more precise measures of exposure than commonly used to establish this effect. Most importantly, the mistrust-inducing effect of diversity seems to accumulate in individuals over time and does not occur as a short-term, direct neighborhood effect.

Theoretical Background

Despite varying wording, studies on the relationship between ethnic diversity and trust mainly investigate forms of identity-based or group trust, which is based on expectations about the trustworthiness of people belonging to a social group other than one's own, that is, an "outgroup" (Freitag and Bauer, 2013). While ingroup and outgroup membership can be defined along a virtually endless number of arbitrary dividing lines (such as team colors in experimental settings), national origin and ethnicity are arguably among the most potent group identifiers (Eriksen, 2010). Predominant theories on the relationship between ethnic diversity and ethnic group trust differ in terms of whether they attribute their hypothesized effects to *mere exposure* or to *actual contact* with ethnic outgroups. In this context, exposure

is usually defined as "being around and casually observing people of different ethnic backgrounds," whereas contact denotes more "intimate forms of social interaction, such as talking to people of a different ethnic background" (Dinesen and Sønderskov 2015: 553; also see Zajonc 1968). It should be noted that such interactions refer to casual, intermittent weak tie relations, rather than intimate friendships, which are often used to proxy the kind of contact believed to foster trust (see, e.g., Phan, [2008] and Rydgren et al. [2013], but note Stolle et al. [2013]) – a point to which we shall return shortly.

A related theoretical question that has received little empirical attention is whether the effect of mere exposure, in particular, is instantaneous or rather incremental, developing over a longer period of time. In the following, we will first introduce two dominant theories on the role of mere exposure to and actual contact with ethnic outgroups, also discussing how Putnam's hunkering down hypothesis fits in with these two approaches, before we turn to discussing the issue of cumulative effects.

Mere Exposure vs. Actual Contact

What theories proposing either trust enhancing or trust hampering effects of both exposure and contact have in common is that they assume some degree of latent prejudice, defined as an affective disliking of ethnically defined immigrant outgroups, usually on the part of the native-born in-group.[1] The question in dispute is then whether mere exposure or contact evokes or reduces latent prejudice as a basis of group trust. We shall discuss each hypothetical pathway in turn.

As a representative of the mere-exposure mechanism, the well-known *conflict hypothesis* states that ethnicity emerges as an important fault line in the competition over scarce resources, as the relative size of and hence exposure to ethnic outgroups increases (Blumer, 1958; Bobo and Hutchings, 1996; Tajfel and Turner, 1979). In diverse high-

migration settings, where ethnicity is activated as a salient group feature, majorities have been shown to be more willing to share resources with members of their own ingroup, trusting them to behave fairly, while ethnic outgroups are not met with the same good faith – a regularity that has been observed in small group contexts (Brewer, 1979) as well as at the societal level (Gilens, 2000; Gorodzeisky, 2013; Senik et al., 2008). The conflict hypothesis is commonly understood to view ethnic majority and minority interests to be diametrically opposed and hence assumes that *ingroup trust*, also called *ethnocentric trust*, can only exist at the expense of *outgroup distrust* (Putnam, 2007).[2] However, whether this is indeed the case largely remains an empirical question, as most prior research has focused on the relationship between ethnic diversity and attitudes toward outgroups, simply "assuming that ingroup attitudes must vary inversely" (Putnam 2007: 144). We shall address this question by investigating mere exposure with respect to both interethnic outgroup trust and ethnocentric trust:

H1 – conflict hypothesis: the higher the cumulative *mere exposure* to residents of other world-regional origins across a respondent's neighborhoods of residence, the weaker is her trust in people of other world-regional origins and the stronger is her relative trust in people from her own world region

We focus our analysis of the conflict hypothesis as a mere exposure effect on the neighborhood context, because sociability is not a socially sanctioned norm in Swedish residential settings, making it very likely that neighborhood encounters largely remain at the level of merely observing of 'the other' (Edling and Rydgren 2012). Indeed, it is important to bear in mind that the conflict hypothesis of mere exposure assumes that "personal experience with the target of trust is [...] not a prerequisite" as "membership in a given category bypasses

the need for personal knowledge" (Freitag and Bauer 2013: 26). This goes back to the notion of the mere exposure effect being rooted in affective prejudice, which overstates both the difference of ethnic outgroups and the notion that belonging to a given group implies common behaviors, such as traditions, and values, which facilitate trust (Stolle, 2002).

While the conflict hypothesis posits that mere exposure to ethnic outgroups will serve to strengthen the prejudicial assumption of difference and hence lower outgroup trust, the well-known *contact hypothesis* opposes this idea and stresses the importance of actual, meaningful encounters. Originally specified by Allport (1954), contact is considered meaningful when it recurs, especially among individuals of about equal status "within the given situation" (i.e., not necessarily in society at large), where individuals ideally pursue a common goal (as is the case for members of the same housing associations, workplaces, sports teams, etc.). Given these requirements, Allport points out that acquaintances rather than loose encounters make neighborhood contacts matter (1954, Ch. 16). A number of studies have investigated whether the negative association of group trust and neighborhood diversity is conditional on having contacts with neighbors or, more often, interethnic friendships more broadly. Stolle, Soroka, and Johnston (2008) find that ethnic diversity at the census tract level is negatively associated with trust only in white Americans who do not regularly interact with their neighbors. Studying both members of the majority and "visible minorities" in Canada, Phan (2008) shows that those who have ethnically diverse friendship ties display significantly higher levels of generalized trust in "most people" than those with mostly ingroup friends. She also finds that diverse friendships moderate the otherwise negative relationship between very diverse city contexts and generalized trust, though the interaction effect is small. Although finding the baseline association between ethnic heterogeneity in neighborhoods and group trust as well as generalized trust to be positive in Northern Iraq, Rydgren, Dana, and Hällsten (2013) show that the relationship there is largely

mediated by interethnic friendships, too, suggesting that interethnic neighborhoods provide a ground for friendship formation, which fosters trust.

A central problem with the use of friendship ties as predictors of trust is, of course, the issue of endogeneity. People trust their friends and become friends with those they trust. Because the studies cited above use cross-sectional data, it is impossible to estimate whether trust was increased by self-reported friendships or actually served to facilitate the latter. Theoretically, since making friends already presuppose some degree of trust in the first place, friendship ties may be expected to strengthen trust, at best, but not to turn distrust or no-trust into trust. It thus seems likely that weaker, less intimate forms of contact should be most pivotal for the formation of trust. Here, we are thinking of casual intermittent interactions that nonetheless move beyond mere observation and passive exposure. This is consistent with Allport's (1954) notion of prejudice-reducing neighborhood contacts as mere acquaintances, that is, as loose connections to people with whom one talks and whom one greets on the street, representing what Granovetter (1973) called "weak ties." In one of the few studies aiming to disentangle the moderating role of intimate or "strong" as opposed to weak ties, Stolle et al. (2013) showed that it was the presence of weak ties (captured as occasional conversations with immigrants in the neighborhood of residence) but not friendships that increased trust in diverse neighborhoods across German cities. Despite its strengths, the study by Stolle et al. (2013) still faces the issue of endogeneity.

To address this problem and to stay true to Allport's theory, we sought to find a way of capturing actual contact that was sufficiently close without being intimate, and at the same time less fraught by self-selection than commonplace self-reported friendship or neighborhood contact measures. We believe that workplace encounters match our criteria well. Except for the publication by Rydgren et al. (2013), we know of no other study that has investigated the relevance of contact for group trust in the context of workplaces.[3] Yet, as

highly confined interaction spaces, workplaces encourage close and repeated contact among often status-equal individuals (Feld, 1981). To whom people develop ties has been shown to depend heavily on the opportunity structure, that is, the sociodemographic composition, of the workplace at hand (Feld, 1982). Rather than following a preference for connections to similar others observed in many interaction spaces (McPherson et al., 2001), people tend to develop ties to those with whom they work (also see Blau [1977] on the importance of opportunity structures in contact formation). This is why networks in workplaces are marked by greater racial, ethnic, religious, and social class-based diversity than those developed in other, more voluntary social settings, like the neighborhood (Briggs, 2007; Marsden, 1990; Mutz and Mondak, 2006; Wright and Cho, 1992). At the same time, work-related ties tend to be notably more homogenous in terms of sex and education, given that workplaces are usually segregated along these lines. All in all, workplaces encourage "tangible forms of interdependence and collective identity" among individuals of similar status, pursuing common goals, within the confines of cooperation-facilitating structures (Briggs 2007: 285). According to Allport's above-specified definition, these are exactly the kinds of contact likely to foster the revision of outgroup prejudice and, potentially, increase interpersonal trust among members of different ethnic groups. We thus test the following hypothesis:

H2 – contact hypothesis: the higher the proportion of and hence *actual contact* with coworkers of non-Western origin at a Swedish-born respondent's workplace, the stronger is their trust in people of non-Western origin.

Lastly, in his critically debated work on the consequences of ethnic diversity for social cohesion, Putnam (2007) challenges the notion that either contact with or exposure to ethnic outgroups will affect group trust only. Based on analyses of the 2000 US Social

Capital Community Benchmark Survey (SCCBS), Putnam finds that interethnic trust as well as generalized trust in neighbors, regardless of their race, are significantly positively correlated with neighborhood homogeneity (as measured by the Herfindahl index for census tracts; Putnam 2007: 147-149). Based on these findings, Putnam argues that ethnic diversity triggers a negative chain reaction of deteriorating trust across types, as all forms of trust appear to be correlated and thus interdependent. In affecting one type of trust (for instance, toward ethnic outgroups), ethnic diversity will ultimately reduce trust in others more broadly, the argument goes. If generalized trust is low, so is people's likelihood to connect to others or participate in civic activities. Life in ethnically diverse settings would thus neither lessen ethnic dividing lines (as suggested by the contact hypotheses) nor reinforce them (as implied by the conflict hypothesis) but instead lead to anomie and cause people to *hunker down* in personal and societal isolation, regardless of their own ethnicity.

Many of the theoretical and empirical weaknesses of Putnam's study have already been discussed in great detail by Portes and Vickstrom (2011), Sturgis et al. (2011), and others. The issue we would like to emphasize here is that Putnam dismisses both the conflict and the contact hypothesis without considering what his primary independent variable (census tract homogeneity) actually measures. To investigate whether Putnam's hunkering down hypothesis truly holds, we need to analytically differentiate among a hierarchy of social relations: mere exposure, casual contact, and intimate friendships. As we have argued earlier, only the former two should be pivotal for trust formation since strong-tie friendships already require some degree of prior trust. As we argue, both mere exposure and casual contact will be subject to the opportunity structure in the foci rather than already established personal preferences. Based on this conceptual clarification, we will test whether generalized trust in neighbors is also lowered when we distinguish between mere exposure (in the neighborhood context) and our proxy for actual, casual contact (at the workplace). Indeed, combining

Putnam's insight about the interrelated nature of different types of trust with the theoretical mechanisms implied by the conflict and contact hypotheses, we suggest that

H3a – hunkering down hypothesis via mere exposure: the higher the cumulative *mere exposure* to residents of other world-regional origins across a respondent's neighborhoods of residence, the *lower* her generalized trust in neighbors.

H3b – hunkering down hypothesis via contact: the higher the proportion of and hence *actual contact* with coworkers of non-Western origin at a Swedish-born respondent's workplace, the *higher* her generalized trust in neighbors.

Immediate vs. Cumulative Effects

Part of the debate amongst researchers of attitude formation more broadly is whether the effects of exposure and contact can be expected to occur immediately or rather cumulatively, over time. Referring to the conflict hypothesis in particular, Crisp et al. (2009) were amongst the first to suggest that the expected negative group threat effect is likely to develop with increasing exposure to a group-threat inducing stimulus. However, to date, the literature on the attitudinal relevance of cumulative exposure to or contact with ethnic outgroups remains very small. Since, to the best of our knowledge, attitudinal panel surveys that repeatedly record both respondents' levels of group trust and long-term exposure to ethnic heterogeneity remain unavailable, the cumulative effect of exposure to or contact with ethnic outgroups on group trust has not been the subject of large-scale empirical investigations. Crisp et al.'s (2009) evidence comes from an experiment in which British university students were asked to rate their liking of French names. Ratings were lower for names that were shown more often, however, only in British students who had been told that other, fictive French

participants had given persistently low ratings to British names in the experiment. Crisp et al. argue that telling British participants about the French ratings activated group identification, which then became increasingly salient as exposure accumulated. It seems likely that a similar process would occur in real-world settings as well. To use the example employed in our study: in many cases, native-born Swedish residents identify neighbors of Eastern European or broadly "non-Western" immigrant origin, in particular, due to differences in language, looks, etc. These differences may then become increasingly salient as time of joint residence increases. Depending on the nature of this exposure – for instance, depending on the socioeconomic characteristics of immigrant residents – group salience may translate into group threat and, ultimately, low trust. However, a well-known field experiment by Hamilton and Bishop (1976) suggests that this may not be the case when intergroup exposure increases in socioeconomically privileged and homogenous, low-threat environments, such as American suburbs of the 1970s. In their experiment, Hamilton and Bishop (1976) measured symbolic racism in white American, suburban home owners before and at various intervals after a black family moved into the immediate vicinity. They found that after one year, white residents with black neighbors had significantly lower racism scores, regardless of whether or not they actually had had any direct interactions with their neighbors, suggesting a long-term positive effect of mere exposure, probably via expectancy or prejudice disconfirmation[4] over time (Bornstein, 1993).

Unfortunately, our study provides only static measures of various forms of group trust. However, thanks to our ability to link the survey to Swedish register data, we are able to calculate a cumulative measure of exposure to ethnic diversity within respondents' neighborhoods of residence, adding to our knowledge from prior research. To take full advantage of our unique register data resources and other existing knowledge, our hypotheses focus on cumulative, rather than static measures of exposure to immigration-induced, ethnic

heterogeneity at the Swedish neighborhood level (see Hypotheses 1-3a). For comparative purposes, we nevertheless present and discuss results for static exposure as well. Before we expand on our analytical strategy, the following section provides some necessary information on our empirical test case, Sweden.

The Case of Sweden

Since the Second World War, when a mere one percent of the Swedish population had been born abroad, large-scale migration has transformed Sweden into an ethnically diverse society. Currently, Sweden's five largest foreign-born population groups hail from Finland, Iraq, Poland, the former Yugoslavia, and Iran (Statistics Sweden, 2014). Like many other European countries, Sweden experienced a first wave of large-scale immigration in the wake of officially mandated labor migration schemes in the 1950s and 1960s. After the official labor migration policy was suspended in the 1970s, family reunions, work-related immigration, and, importantly, refugee inflows from countries plagued by humanitarian crises and wars, continued to diversify Sweden's population. Looking at Figure 1, we see that between 1990 and 2012 the number of immigrants granted residency nearly quintupled, from about 35,000 to 170,000 persons per year.

[Insert Figure 1]

Segregation is present in all spheres of society. Immigrants to a larger extent reside with other immigrants, but not necessary with fellow nationals. Ethnic enclaves are very uncommon in Sweden, and most immigrant-dense neighborhoods are heterogeneous with regard to national origins. Figure 2 shows the extent to which immigrants and native-born Swedes are exposed to residential segregation. Only about 20 percent of native and foreign-born individuals live in Swedish neighborhoods where the share of foreign-born residents is

greater than 10 per cent. Due to relative group sizes, Swedes are more prone to living in neighborhoods with large foreign-born shares than the foreign-born themselves. For instance, about 10 percent of the native-born live in areas where around 38 percent of all inhabitants were born abroad. This is not true for any foreign-born residents, further underscoring the assertion that ethnic enclaves are not present in the Swedish context. Even if there are large differences across the groups, many native-born are actually highly exposed to at least some proportion of immigration within their neighborhoods.

[Insert Figure 2]

Similar to the neighborhood setting, immigrants tend to be segregated from natives in workplaces (Åslund and Nordström Skans, 2010). Immigrants are overexposed both to workers from their own birth region and to immigrants from other regions. This is reflected in Åslund and Nordström Skans' estimate that "even when accounting for age, gender, education, region and industry, the average immigrant has 40 percent more immigrants in his or her workplace" than expected from a completely random distribution, while "natives are on average underexposed" to immigrant colleagues in Sweden (2010: 489). In addition, foreign-born groups with low employment rates are the most segregated from natives (Åslund and Nordström Skans, 2010).

Needless to say, immigrants face harsher economic conditions than native-born citizens do, and non-Western immigrants in particular suffer persistently worse labor market outcomes than native-born Swedes. Even seven years after immigration, non-Western immigrants' levels of employment are well below those of native Swedes or Western immigrants (Nekby, 2002). Non-Western immigrants face substantially higher unemployment risks (Arai and Vilhelmsson, 2004), earn lower wages (le Grand and Szulkin, 2002), and tend

to be segregated into lower ranked jobs (Åslund and Skans 2010) than natives. There is also some evidence of direct discrimination in the job hiring process against non-Western immigrants (Bursell, 2014; Carlsson and Rooth, 2007). Consequently, contact among the native and the non-Western foreign-born in particular is most likely to occur in lower-status, lower-income jobs. Interethnic contact more broadly is less likely to occur at workplaces than in neighborhoods, as natives and various immigrant groups are persistently channeled into different types of employment. Labor market segregation should only affect the likelihood with which potentially beneficial, work-related ties occur, but we see no reason that it should influence the nature or quality of intergroup relationships. As Figure 3 shows, most of our sample finds itself in small to medium-sized workplaces, where the likelihood that all colleagues are in more or less regular contact is quite high. This gives us some confidence in our decision to use the workplace share of two major immigrant groups as a proxy for actual majority-minority contact.

[Insert Figure 3]

Before turning to the empirical part of our study, we want to address the issue of external validity. As Schierup and Ålund state, Sweden is internationally regarded as "the model of a tolerant, egalitarian, multicultural welfare society" (2011: 47), affording immigrants and natives alike extensive social protections. Scholars have argued that it is thanks to Sweden's universal welfare state and comparatively low levels of political corruption that Swedes are happier and more trusting than other, non-Nordic Europeans (Figure 4; also see Kumlin and Rothstein 2010; Rothstein 2010). However, long-term trends in trust and happiness should not cause us to overlook more recent developments that put the notion of "Swedish exceptionalism" into question: waves of youth unrest swept the

socioeconomically disadvantaged, high-immigration neighborhoods of the Swedish cities of Malmö, Gothenburg and Uppsala in 2009 and Stockholm in 2013 (Malmberg, Andersson, and Östh 2013). In Sweden, as in many other wealthy democracies, processes of rising immigration, urban segregation, and professional precaritization coincide (Schierup and Ålund, 2011), along with radical right gains in political support and visibility (Rydgren and van der Meiden, 2016; Rydgren 2017). Social unrest and dissatisfaction with social conditions thus manifest both in often marginalized, non-Western immigrant groups and in native-born Swedes, in supporters of the exclusivist, ethnonationalist radical right. Given these developments, we argue that Sweden should be regarded as a case in point, representative of the current situation in Europe more broadly, rather than an exception from it.

[Insert Figure 4]

Data and Measures

We use the *Social Networks and Xenophobia Survey,* which was fielded in 2013 by Statistics Sweden and was designed with our study and related research projects in mind. The sample is targeted at the general population aged 18 to 79. The net sample comprises 2,282 individuals with a response rate of 46 per cent. The survey was primarily designed to capture attitudes toward immigrants and immigration, and our questionnaire includes a range of items on political attitudes and individual attributes. Most importantly for our study, the survey contains rich information on neighborhood and workplace characteristics for each respondent, established via register data links.

Our focus in this paper is on immigrants who are born either in Eastern Europe or in non-Western countries outside of Europe. These two immigrant groups are broad, but are the

most marginalized in the Swedish context. The non-Western group in particular experiences the highest levels of exclusion. In our analyses, we regard the foreign-born as immigrants only when calculating their proportions in neighborhoods, workplaces, and (self-reported) friendship networks. But we do consider whether respondents are either first- or second-generation immigrants themselves by introducing a detailed immigrant status as a control. Much of the disadvantage experienced by immigrants is overcome in the second generation, and remaining differences tend to reflect socioeconomic inequality (Böhlmark, 2008; Hällsten and Szulkin, 2009).

Dependent Variables: Three Types of Trust

Table 1 describes our key variables. Based on prior research, we consider three types of trust (cf. Putnam 2007), which are adapted to the Swedish context and our study design: generalized trust in neighbors, as well as two forms of ethnic group trust, namely Swedes' interethnic trust and Swedes' ethnocentric trust. The latter two measures are applicable only to Swedish-born individuals, simply because defining relevant ethnic outgroups is very difficult in multiethnic settings, and would require a very complex survey instrument that, unfortunately, is not at our disposal. Interethnic trust is defined as trust in Eastern European and non-Western, non-European immigrants, and ethnocentric trust is the contrast (difference) between interethnic trust and trust in Nordic-born persons. Using the Nordic rather than the strictly Swedish category is preferable in this case, since the measure will be more general than just narrow ingroup trust, which could be confounded with nationalism. However, sensitivity checks show that such an alternative measure produces essentially the same results.

[Insert Table 1]

Independent variables: Contextual heterogeneity and actual contact

The key contextual variables are neighborhood characteristics, but also characteristics of networks, workplaces, and municipalities (see Table A1 for summary statistics). The neighborhood is defined by SAMS (small area market statistics) codes obtained from national registers. These areas are designed to describe homogeneous neighborhoods in terms of geographic boundaries and type of dwellings, and consist of on average 1,000 inhabitants, with some variations in size (SD = 1,247). There are 9,200 SAMS areas in Sweden, nested within 290 municipalities. In comparison to US census tracts, the SAMS areas are (in most cases) smaller. Due to their small size and the typical structure of Swedish cities and towns, in which housing areas are built around their own local shopping, GP, and community centers, SAMS have been argued to capture actual experienced neighborhood settings (Edling and Rydgren 2012). Even though not all SAMS units achieve this perfectly (Amcoff, 2012), they nevertheless represent an improvement over prior measures of neighborhood contexts, such as census tracts or postal code areas.

For each neighborhood, we calculate the proportion of immigrants, using a range of alternative definitions, but focus on Eastern European and non-Western, non-European origins, and foreign-born in general.[5] Rather than capturing ethnic homogeneity or heterogeneity per se, this is a measure of outgroup size or ethnic concentration, which has been argued to be of particular relevance in making group boundaries salient (Blumer, 1958; Bobo and Hutchings, 1996). Table A2 shows correlations for the contextual variables.[6]

Importantly, we have access to complete histories of neighborhood residence since 1990, and also population data for the same time period, which enables us to calculate yearly neighborhood characteristics. We use this information to compute both the proportion of immigrants in the last observed year (2012), and the cumulative exposure during the last 22

years (or shorter, if the respondent is a more recent immigrant). In the latter measure, we average the measures over all observed years; this reduces classical measurement error but also has more sociological relevance since it tends to capture more individual experiences than does the 2012 snapshot.

To account for economic heterogeneity or inequality, which has been linked to levels of trust in its own right (Rothstein and Uslaner, 2005), we calculate the interquartile range ($p_{75} - p_{25}$) in equivalent disposable incomes, and do so both for 2012 and for a cumulative measure. The proportion of non-Western and Eastern European immigrants correlates positively with economic heterogeneity (.18). Population density and proportion living in economic deprivation correlate strongly positively with the proportion of non-Western and Eastern European immigrants and more moderately with economic heterogeneity, whereas the proportion of tertiary educated residents correlates positively with economic heterogeneity and negatively with the proportion of immigrants. This means that heterogeneity in the ethnic and socioeconomic dimensions have much in common, but cannot be collapsed into one dimension.

We also calculate a number of neighborhood controls: the proportion in relative poverty (using the OECD measure of ½ median of equivalent disposable incomes as threshold); the average years of schooling (based on the highest attained level of education), and population density; all coded in two versions as described above.

We replicate the above-mentioned context measures for municipalities. There are 290 municipalities in Sweden with large variations in population size. The median size is around 11,000 and the 75th percentile (Q_3) is around 23,000. As opposed to SAMS areas, municipalities are politically and economically functional, with responsibilities for schooling (except higher education), infrastructure, and care of children and the elderly. In this sense, municipalities are potentially relevant for intergroup relations, as they are the administrative

level at which political debates are held and important decisions about the allocation of government resources are made. However, to remain with our proposed hypotheses' focus on neighborhood and workplace contexts, we include the municipality proportion employed and the proportion foreign-born as additional sensitivity checks only.

For workplaces, we have data on the proportion of female workers, which proxies for sector/industry, and a cruder division of the proportion of immigrants, which merely identifies foreign-born workers. In the analyses that include workplaces we thus focus on the proportion of immigrants or foreign-born overall.

We control for a number of mediators, i.e., variables that could explain the association between immigrant exposure and trust. Social network composition is one such factor.

The questionnaire includes a name generator battery, in which respondents describe characteristics of up to five close friends (Burt, 1984). We characterize the close friendship network based on this list. The survey explicitly asks for the birth countries of both friends and parents of friends so we can identify the proportions of Eastern European and non-Western non-European friends (by first- and second-generation immigrant background). But since few individuals within our sample have any immigrant friends, we use these more detailed measures only in dedicated analyses and generally use the broader category of foreign-born friends instead. We also code the proportion of females and the proportion of unemployed individuals among our respondents' friends. We regard the latter as an indicator of social marginalization within the network, which can be important for trust. The questionnaire also includes a position generator to measure individual-level social capital (Lin and Dumin, 1986). Specifically, the position generator asks survey participants whether they have contacts within a list of 36 occupations. Based on these occupations, we create a latent measure of social capital based on diversity and extensity among occupation contacts (Lin and Dumin, 1986; van der Gaag et al., 2008). We also account for the experience of

victimization. This is captured by detailed questions on the experience of violent crimes or property crimes. In addition, we account for fear of victimization and for xenophobia, which we measure as a latent variable based on several items.

Lastly, in order to account for selection into neighborhoods, workplaces and municipalities, we control for demographics such as gender and age; the presence of children in the household and respondents' civil status; and socioeconomics, such as education, disposable income, and a combination of employment status and social class (i.e., when not stably employed, the class position is unemployed, studying, on sick-leave retired etc.).

Selection Issues

Research efforts attempting to study so-called neighborhood effects are usually plagued by the issue of self-selection. On the face of it, our study is no exception in this regard. Individuals can be expected to prefer certain neighborhoods, based on a host of observable and non-observable characteristics. Among them are preferences regarding the ethnic composition of residential areas as well as socioeconomic status markers, characterizing both self-selecting individuals and neighborhoods. Consequently, the proportion of non-Swedish residents and, by extension, the degree of ethnic heterogeneity is unlikely to vary randomly across neighborhoods. How might self-selection into neighborhoods and, though to a much lesser extent, workplaces *theoretically* affect the relationship between ethnic diversity and trust? On the one hand, people with higher trust and more positive attitudes toward ethnic outgroups may be more likely to select into ethnically heterogeneous spaces. However, if this were the case, self-selection would certainly not be driving the widely asserted negative relationship between contextual diversity and trust (Hypotheses 1 and 3a). It would rather lead us to underestimate or fully miss the presence of any such negative association which, arguably, is the relationship most detrimental to society at large and hence of greatest concern

to policy makers. Putnam has argued that positive selection (on higher trust and favoring diversity) seems much more likely than negative selection (of distrusting, diversity-averse individuals into ethnically heterogeneous contexts), rendering estimates underlying any negative relationship between diversity and trust rather conservative (2007: 153-154). If people were to operate under perfect mobility with respect to their preferences for ethnic neighborhood compositions, moving into or staying in diversifying settings does indeed not seem a likely strategy on the part of distrusting or change-averse individuals, as living among people of various national origins likely implies exposure to the unknown – be it in direct interactions or via mere observation from a distance. However, at least in Sweden, residential mobility should not be overestimated, as it is limited by the interplay of two factors: the population being most densely concentrated around three urban hubs and these hubs being subject to severe housing shortages.

As of November 2015, about 40 percent of Sweden's population lived in the country's three largest metropolitan areas (Swedish: *Storstadsområden*), greater Stockholm, Gothenburg, and Malmo (Statistics Sweden, 2016). Following 25 years of increasing deregulation and market liberalization, coupled with an increased conglomeration of jobs in these metropolitan hubs, these regions have been facing a severe housing crisis, with demand vastly exceeding supply (Christophers, 2013). The resultant rise in both rental and selling prices can be expected to hamper mobility for most people who already own their home or rent at pre-crisis rates, but it goes without saying that poorer individuals are even more affected than wealthier ones (Hedin et al., 2012). In this way, poorer Swedes in particular may happen to live in more ethnically diverse neighborhoods, either because they already lived there before immigrants arrived and cannot afford to move out, or, in case they have only recently moved into one of the metropolitan hubs, because housing prices continue to be lower in the more diverse areas and they simply cannot afford to move elsewhere. This is

important, because socioeconomic deprivation at the individual level and living in deprived neighborhoods have been linked to lowered levels of generalized and group trust, probably because poverty leads to a heightened sense of both threat and powerlessness, which in turn hampers faith in others (Lancee and Dronkers, 2011; Ross et al., 2001). Drawing on further register-based indicators, our analyses thus consider the competing impact of material deprivation (of neighborhoods and individuals) on trust to address the possibility that ethnic diversity may simply be an epiphenomenon of neighborhood or individual deprivation. If poorer neighborhoods are homes to predominantly non-Swedish, non-Western residents, such segregation is also likely to have a bearing on interethnic contact opportunities that are in turn hypothesized to be associated with trust (Uslaner, 2011). In addition, as explained previously, socioeconomic neighborhood characteristics may set the scene for the extent to which intergroup threat in particular develops (Crisp et al., 2009; Hamilton and Bishop, 1976).

Even if we assumed that neighborhood choice was unrestrained, we argue that the selection effects operating at the residential level are much less likely to affect the distribution of individuals across workplaces (Kokkonen et al., 2015; Mutz and Mondak, 2006). By comparing estimates for the relationship between trust and neighborhood versus workplace heterogeneity, we can thus get a sense of the extent to which self-selection may play a role in driving our findings.

Method

Since the *Social Networks and Xenophobia Survey* is based on a random sample of the Swedish population and the number of neighborhoods is large, we mostly observe only one case (for very few areas, up to three cases) per neighborhood unit. The same holds true for workplaces. Consequently, modeling the contextual associations within a multilevel framework is not possible, but the independence of observations is most likely given, and the

traditional single-level approach thus seems appropriate. Most of our outcome variables are ordinal (trust in neighbors, interethnic trust, ethnocentric trust, ethnicity-specific trust), but other outcomes are continuous in character. We use ordered logit regression for the former and present results in the form of average marginal effect in terms of the probability of the highest response categories.[7] We have examined the parallel slopes assumption of the ordered logit, and while it is not strictly met in all models, we found no deviations that would change our conclusions. For continuous outcomes, we treat the outcomes as interval scales and use standard OLS regressions. We use standard errors robust to heteroscedasticity.

Results

Trust in neighbors: The hunkering down hypothesis

We start by addressing the most general type of trust included in our analyses, namely trust in neighbors regardless of their ethnicity or world-regional origin. The hunkering down hypothesis (Hypothesis 3a) predicts that this kind of trust should be lowered by exposure to ethnic heterogeneity in everyday living spaces, which we operationalize as the proportion of non-Western and Eastern European immigrants in the neighborhood of residence. The first central question we seek to answer here is whether cumulative exposure to ethnic outgroups over a longer time period is a better predictor of trust in neighbors than the much more widely used snapshot measure (taken from the year preceding our survey). This question is addressed by the four ordinal logit models presented in Table 2, where the coefficients refer to average marginal effects (AME) for the probability of observing the highest level of trust in neighbors (= 4, "trust completely"). Based on the snapshot measure, we fail to establish a statistically significant relationship between exposure to Eastern European and non-Western immigrants in the neighborhood and trust in neighbors, even before we added our full set of

controls (Models 1 and 2). With the cumulative measure, however, we find a statistically significant effect that persists after our neighborhood and individual controls are added to the model (Model 3). The estimated marginal effect of -.339 implies that our probability of observing the highest level of trust in neighbors is lowered by 34 percentage points if we compare respondents with an average or cumulative exposure to neighborhoods with 0 vs. 100 per cent Eastern European and non-Western immigrants. This is, however, an unrealistic comparison. Instead, comparing cumulative exposure to neighborhoods with 0 vs. 25 percent immigrants seems more relevant in the Swedish context and still produces a notable difference of almost 9 percentage points. When we add additional sociodemographic and municipality controls to the model (Model 4), the exposure effect is strengthened further. The estimated AME for the cumulative exposure variable is now -.400, implying a 10 percentage-point difference in the probability of being most trusting of neighbors for a quarter difference in exposure to Eastern European and non-Western immigrants. We also want to note that, perhaps counterintuitively, economic heterogeneity (income range) appears to have some weakly positive association with trust. At the same time, the proportion of neighbors living in economic deprivation or having a tertiary degree seems to have no statistically significant association with trust in neighbors. Neighborhood tenure, however, is weakly positively associated with trust, which seems reasonable in the light of theory on expectancy disconfirmation over time (Bornstein, 1993).

[Insert Table 2]

To further assess the hunkering down hypothesis, we examine the raw empirical association between cumulative exposure to ethnically diverse neighborhoods and trust in neighbors in Figure 5. The graph describes average levels of trust for bins based on exposure.

It also documents a negative association, but, importantly, shows that despite the sampling noise, the underlying pattern is highly linear, and that there is little evidence of discontinuities or threshold effects. We thus conclude that the linear specification used in the above is adequate.

[Insert Figure 5]

In sum, we find support for the hunkering down hypothesis (as specified in Hypothesis 3a), but note that it requires a more precise measure of exposure than commonly used to establish this effect. Our findings also suggest that the mistrust-inducing effect of mere exposure to ethnic diversity has an individual cumulative component, rather than the characteristics of a short-term direct neighborhood effect. After all, it is our long-term measure of exposure that turns out to be important, which we believe to be an important modification to Putnam's theory.

In Table 3, we further scrutinize the factors that confound but also mediate the association between cumulative exposure to ethnic diversity and trust in neighbors. Models 1 through 3 add the confounders in sequence to arrive at the estimates shown above. In Model 3, we see that being born in a non-Western or Eastern European country is associated with lower trust in neighbors. Yet, this seems to be largely driven by selection, as adding more comprehensive individual, sociodemographic (Model 4) and, ultimately, neighborhood and municipality composition controls (Models 5 – 7) explains away most of the differences.

[Insert Table 3]

In Model 4 – 7, we examine to what extent the significantly negative association between exposure and trust is mediated by intermediary factors. When we control for social network composition in Model 4, a quarter of the association is removed. The remaining coefficient is however not significant, but due to our small sample, statistical power is limited. Hence, the finding that the mediator variables make the exposure coefficient insignificant may be due to a Type II error problem (wrongly accepting the null hypothesis that states that there is no effect).

In Table A3, we have estimated the model for Swedish born individuals only. Here we tend to find somewhat stronger associations between cumulative exposure and trust in neighbors, and the association is more robust to mediators. In this limited sample, the cumulative exposure coefficient remains significant when we add mediating variables. We thus believe that the coefficient turns non-significant because of limited statistical power and not because networks explain away the exposure effect entirely. Since we condition on strong tie relations, which are in all likelihood highly conditional on trust (see our previous discussion of endogeneity), the quarter removed is, on the contrary, surprisingly small.

Controlling for social capital has small, statistically non-significant effects and, if anything, strengthens the association between trust in neighbors and exposure to ethnic diversity somewhat. This suggests that social and economic embeddedness in Swedish society play no or a merely minor role in how exposure and trust are related. However, personal victimization is statistically significantly negatively associated with trust in neighbors and appears to reduce the association between trust and exposure to Eastern European and non-Western non-European immigrants in the neighborhood. Distrust in neighbors may thus be based on real negative experiences in the residential setting, which may be more common in immigrant-dense neighborhoods. Perhaps unsurprisingly, we also find that xenophobia explains part of the exposure effect, i.e., that xenophobic respondents

distrust neighbors more, but that they tend to live in more immigrant-dense neighborhoods as well. In comparison, however, the key mediator is clearly social network composition. Lastly, one striking finding is that having non-Western and Eastern European friends is associated with lowered trust in neighbors. We also find that having unemployed friends produces a similar negative association.

Outgroup trust and ethnocentric trust: Conflict and contact hypotheses

We now consider two alternative dimensions of group trust – outgroup trust and ethnocentric trust – as outcomes and turn to our investigation of the conflict and the contact hypotheses in turn. The conflict hypothesis (Hypothesis 1) states that lower trust in ethnic outgroups should be offset by higher trust in ethnic ingroups. To investigate this proposition, we limit our sample to Swedish-born individuals. In Table 4, Model 1 introduces Swedes' interethnic trust as the outcome to test the first proposition, namely that exposure to ethnic diversity should lower trust in ethnic outgroups, here immigrants from Eastern Europe or Non-Western, non-European countries of origin. However, we do not find this association to be statistically significant, neither in the bivariate setting nor after controls are added to the model. We conclude that being exposed to immigrants in the neighborhood has no relevance for outgroup trust in native-born Swedes. In Model 3, we study Swede's ethnocentric trust to test the second proposition, namely that exposure to ethnic diversity should heighten respondents' trust in people of their own ethnic ingroup. Here, too, we find no statistically association with neighborhood exposure, and can thus reject the conflict hypothesis in full. It appears that exposure to immigrants has no relevance for either in- or outgroup trust, at least in Sweden.

[Insert Table 4]

Finally, in Table 5, we address the contact hypothesis. We also investigate our alternative specification of the hunkering down hypothesis (Hypothesis 3b), which states that the negative mere exposure effect of ethnic diversity on generalized trust in neighbors regardless of their ethnicity should be offset by actual contact. As explained earlier, we focus on the proportion of foreign-born individuals at the workplace to proxy actual contact. Due to data limitations, the analyses regard foreign-born individuals at large, rather than Eastern-European and non-Western, non-European immigrants in particular.[8] The analyses are, again, limited to Swedish-born respondents. Since just a subset of the sample is employed, the analyses are also based on fewer observations.

[Insert Table 5]

In Models 1 and 2, we address the hunkering down hypothesis in its contact version and thus re-analyze trust in neighbors with and without mediator variables but also with indicators for interethnic contact in the workplace. In this more limited sample of employees, we find a stronger association between neighborhood exposure and trust in neighbors. Controlling for confounders only in Model 1, we estimate an AME of .68, which suggests that a quarter difference in neighborhood exposure to ethnic diversity will lead to a 17-percentage-point lower probability of being in the most trusting category. This is a substantial effect. However, ethnic diversity at the workplace has no statistically significant association with trust in neighbors, which goes against the alternative hunkering hypothesis (Hypothesis 3b).

Models 3 and 4 analyze trust in coworkers and show no effect of either neighborhood exposure or workplace exposure to immigrants. While these findings are important in their own right, the contact hypothesis (Hypothesis 2) focuses on Swedes' interethnic trust, which

is analyzed in Models 5 and 6, and ethnocentric trust, which is the outcome for Models 7 and 8. For both types of group trust, we again find no significant effect of either neighborhood exposure or ethnic diversity at the workplace. Assuming that the kind of small to mid-sized workplaces represented in our sample provide ample opportunities for the kind of meaningful, prejudice-reducing and trust-enhancing contacts Allport and others envisioned, we have to reject the contact hypothesis as well. It should be mentioned that our measure of workplace diversity is not one that captures the cumulative experience of our respondents. Instead, it is a snapshot measure based on the last year of available data, and if the contact mechanism operates via repeated, enduring contact rather than instantaneous encounters, we may actually underestimate the effect here. One should also note that the workplace coefficient is positive, in line with the contact hypothesis, but that it is of marginal relevance in assessing a coefficient that has not attained statistical significance.

Discussion

Our study revisits the long-standing debate over the putatively negative consequences of ethnic diversity for group trust. We do so using a theoretically motivated, high-quality dataset that addresses some of the main issues faced by prior research, such as the cumulative nature of exposure effects and the question of endogeneity in (self-reported) contact measures. Looking at our Swedish test case, we find that the negative effect of mere exposure in the neighborhood setting becomes apparent only once we consider cumulative exposure over time, but not when we investigate the kind of static exposure measure commonly used in the research field. The effect appears to be highly linear, without any apparent thresholds; that is, the longer and stronger the exposure, the larger the negative effect. Contrary to theoretical expectations derived from Allport's contact hypothesis, we do not find that presumed workplace contacts either independently affect trust or affect the relationship between

neighborhood exposure and trust. Finally, in line with prior research (cf. Meer and Tolsma, 2014), we find trust in neighbors to be the only type of trust affected by exposure to ethnic diversity.

Reviewing these findings, we argue that our study raises two central questions with important implications for future research. First, can data of a quality adequate to truly disentangle the causal relationship of interest in the debate at hand be realistically obtained? And second, what is the real-world relevance of our findings and of the current research consensus more broadly?

When it comes to the first question, our theoretical discussion and empirical analyses demonstrated that exposure to ethnic diversity should be measured both in a context where the *mere exposure condition* can be assumed (as is arguably the case in Swedish neighborhoods; see Edling and Rydgren, 2012) and in a cumulative rather than static manner. Moreover, we sought to improve upon standard self-reported measures of intergroup contact, by instead using observations of respondent placement in ethnically diverse workplaces, as rather confined, contact-enforcing environments. While we believe that our use of mixed survey and register data constitutes a significant improvement in terms of matching theory and empirical analyses, the limitations of our analyses also reveal that we still have some way to go to achieving ideal data quality, especially when it comes to capturing actual contact as a theoretically crucial intervening variable.

Even though we can argue that a) actual contact is very likely within the kinds of small and mid-sized workplaces we observe in our data and b) self-selection is likely less of an issue for workplace contacts compared to widely measured friendships, we still know nothing about the exact contents or valence of the contacts we observe. This is problematic, as prior research suggests that the negative effect of negatively connotated contact is larger than the positive effect of positive contact on trust (Barlow et al., 2012; also see Paolini et al.,

2010). We have argued that our small sample size and lack of statistical power may have led us to miss a positive effect of workplace contacts on trust, or a mediation of mere exposure in the neighborhood setting by contact at work. However, an alternative explanation could be that we may observe about equal amounts of respondents experiencing positive, neutral, and negative workplace contacts. If positive encounters reverse negative neighborhood effects, neutral encounters do not have any effect, and negative ones strengthen the negative neighborhood effect, then overall, the mediated effect is "netted out." A slowly growing body of research is now looking into the importance of contact valence for trust, though existing studies continue to rely on self-reports to capture both contacts and their characteristics, which poses the usual problems of endogeneity with the outcome of interest (see, e.g., Koopmans and Veit, 2014; Laurence et al., 2017). This is where future research should seek to make improvements, perhaps by sticking to the observation of tight-knit, low-selection interaction spaces (such as workplaces) and aiming to gauge contact valence therein. Stating this, however, also raises the question of whether and how data of sufficient quality and detail can actually be obtained within the limits of what is both realistically achievable and ethically permissible. After all, the possibility of covertly or experimentally observing human behavior (e.g., individual contact valence at workplaces) is – for good reason – limited in the social sciences. Given these restrictions, we believe that our study provides a dataset that already comes quite close to fulfilling the requirements and still serves the purpose of outlining the considerable way left toward an ideal type of investigation of the diversity-trust nexus.

A second question that we, like other studies before us, have to ask is what our findings teach us about society. Taken at face value, the fact that cumulative rather than static exposure to immigrant-dense residential settings appears to decrease trust in neighbors seems to imply that diversity will have rather daunting consequences in the long run. Indeed, this is contrary to what Putnam (2007) argued, about diversity being harmful to trust in the short-

term, but turning into an important asset to society as time goes on and people grow accustomed to it. Especially given low residential mobility, a substantial group of people may be unable to follow their preference of moving out of ethnically diverse neighborhoods and see their dislike of the neighborhood and distrust in their neighbors grow over time. But does this mean that allowing people to follow their potential preference for ethnically segregated living arrangements would make (Swedish) society a better, more trusting place? Despite our insights into the cumulative effect of exposure to diverse neighborhoods, we highly doubt this interpretation. The reason is simple: We only find significant results for trust in neighbors, so even if segregation were to increase this type of trust it would likely leave many other important forms of intergroup trust unaffected. One explanation for this finding may be rooted in issues of measurement error and/or the validity of our underlying survey questions. A neighbor is something most individuals can envision and relate to. A specific type of immigrant may, by comparison, fall into a more abstract category. In this sense, it may require more cognitive work to assess whether one has ever met an immigrant from the specified world region, which makes the response more likely to be imagined rather than factual. In addition, "neighbor" is a rather neutral term whereas "immigrant" has a more (negative) connotation in Sweden's public discourse, which makes answers pertaining to views on immigrants susceptible to social desirability bias.

In substantive terms, the finding may, however, also speak to the rather limited consequences of diversity for trust: Perhaps negative effects occurring in individuals' immediate neighborhood environments do not scale up, that is, do not transcend into the wider societal context. If this is the case, we should be less worried about social cohesion in diversifying societies. Indeed, if, as it seems, studies repeatedly show that any negative effects of diversity appear to be limited to the neighborhood setting alone, we also need to ask whether our persistence in studying the issue does not implicitly make a dangerous

normative assumption: that we should, in principle, be able to tease out those long-predicted adverse effects of diversity, if we only looked at "the right case," unable to accept that the conflict, contact, and hunkering down hypotheses in their current, standard reading may provide all too simplified conceptualizations of attitude formation processes.

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Notes

1 According to Judd, Blair, and Chapleau (2004: 75), the identification of automatic, latent prejudice against ethnic or racial outgroups has been "one of the most active research areas in social psychology," with the resulting studies (for the most part, experiments which use racial/ethnic priming as a treatment, to thereafter observe participants' reaction to various cues) providing generally confirmatory evidence. A review and critical discussion of this literature is beyond the scope of our paper. We would just like to emphasize that the assumption of latent prejudice underlying both conflict and contact theory is rooted in a long tradition of social psychological research.

2 While this appears to be a standard interpretation, some scholars have proposed a different reading of conflict theory, for instance, adding insights from the literature on social disorganization to Blumer's original proposal (cf. van der Meer and Tolsma 2014). Admitting to using "the label 'conflict theory' freely," van der Meer and Tolsma state that the interethnic distrust resulting from competing groups' perceived threat to their rightful resources may lead people to withdraw from social life and thus encourage distrust more broadly (2014: 463). Given this interpretation, conflict theory is not only in line with the hunkering down argument, but also describes a potential mechanism underlying Putnam's hypothesis. However, to replicate and extend Putnam's 2007 study as closely as possible within the Swedish context, we stick to the 'diametrical opposition' take on trust resulting from Putnam's more orthodox reading of conflict theory.

3 Another exception is Öberg et al.'s (2011) study, which uses Swedish register data on parishes to proxy neighborhoods (where we use the more fine-grained measure of SAMS units; cf. section "Independent variables") and workplaces to predict trust in colleagues and generalized trust in others. However, the theoretical premise and actual variables

employed in their paper are very different from ours. Öberg et al. contrast the effects of *individuals' similarity* with their living and work environments in terms of income (measured as the gap between the respondent and the neighborhood/workplace average) and ethnicity (measured as belonging to the given context's ethnic majority) with the effects of *context heterogeneity* (captured by measures of income spread and the Herfindahl index). By contrast, we seek to distinguish effects of mere exposure (in the neighborhood setting) from those of actual contact (at the workplace) and investigate how workplaces and neighborhoods may jointly act to shape trust. Due to their solely separate analyses of workplaces and neighborhoods, the latter issue of workplace/neighborhood mediation or interaction effects is not part of in Öberg et al.'s discussion.

4 The expectancy disconfirmation hypothesis posits that mere exposure will have a positive effect on evaluative attitudes (e.g., group trust) when exposure is positive (Oliver, 1977). In order to formally test this we would need, first, to define what positive exposure means in the context of our study (e.g., based on employment and income levels of local immigrant populations), and, second, interpret the interaction between mere exposure (the proportion of immigrants in the neighborhood) and exposure quality (e.g., average immigrant household income in the neighborhood). Unfortunately, given our rather low number of cases and the resulting limitations in statistical power, this kind of stratified analysis would likely produce unreliable estimates (type II error). It is for the same reason that we refrain from estimating and interpreting interaction terms throughout the study.

5 The survey contains only an identifier for each respondent's SAMS code. However, Sweden has detailed population-level administrative data that can be used for research, and these national registers also contain SAMS codes. All of the neighborhood characteristics we describe are thus based on population aggregates, i.e., they are retrieved

from the national registers and linked to the survey by us, using the SAMS codes. Because the first four digits of each SAMS code identify the municipality within which it is located, we were able to retrieve municipality characteristics in the same way.

6 We have also computed the Herfindahl index, which captures the homogeneity of specific birth countries within neighborhoods, but the proportion of non-Western and Eastern European immigrants correlate very highly (-.95) and thus capture the same underlying dimension (Dinesen and Sønderskov 2015). This is expected, given that immigrant-dense neighborhoods tend to be heterogeneous by birth country, with very few examples of ethnic enclaves (cf. Hällsten 2011). We estimate results for both measures, but base our main results on the proportion measure since it produces the strongest results.

7 We calculate the marginal effect for each and every observation, where we set its values on the independent variables to what is observed for that observation, and the take the average of these effects in the sample.

8 Workplace characteristics are based on register data as well. However, other than the neighborhood characteristics, this data was not retrieved by us but made available to us by Statistics Sweden to ensure ethical standards of respondent anonymity. This is why our information on workplaces is much more limited than that on neighborhoods.

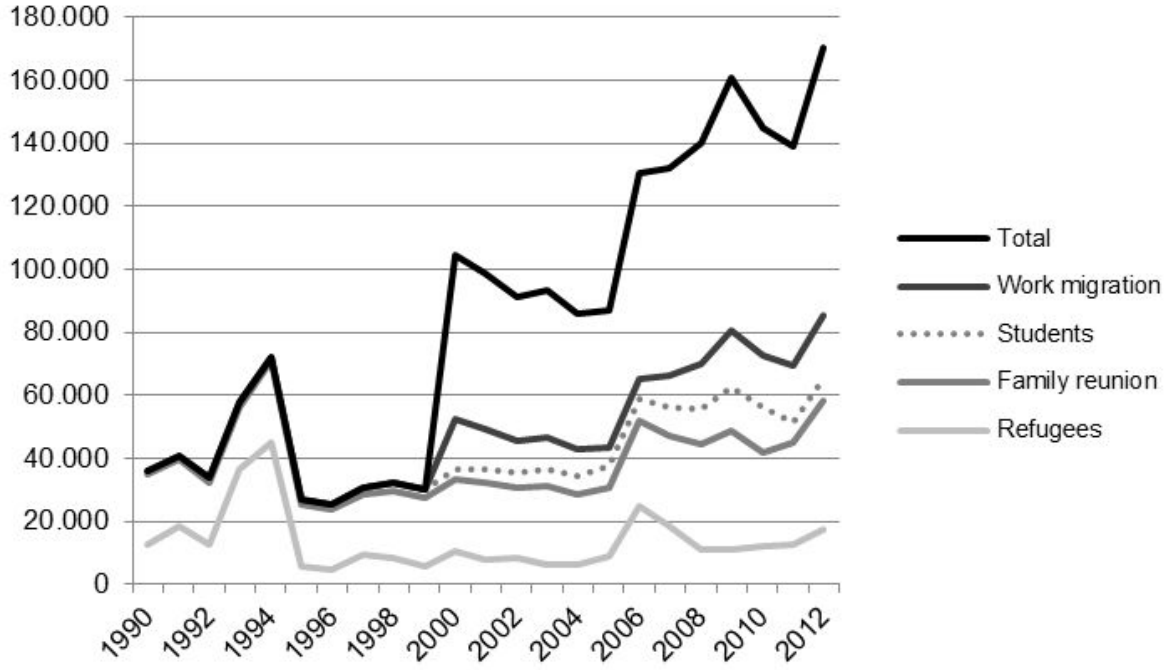

Figure 1. Number of residence permits granted to foreign-born persons, by type of migration
Source: Swedish Migration Board (2016).

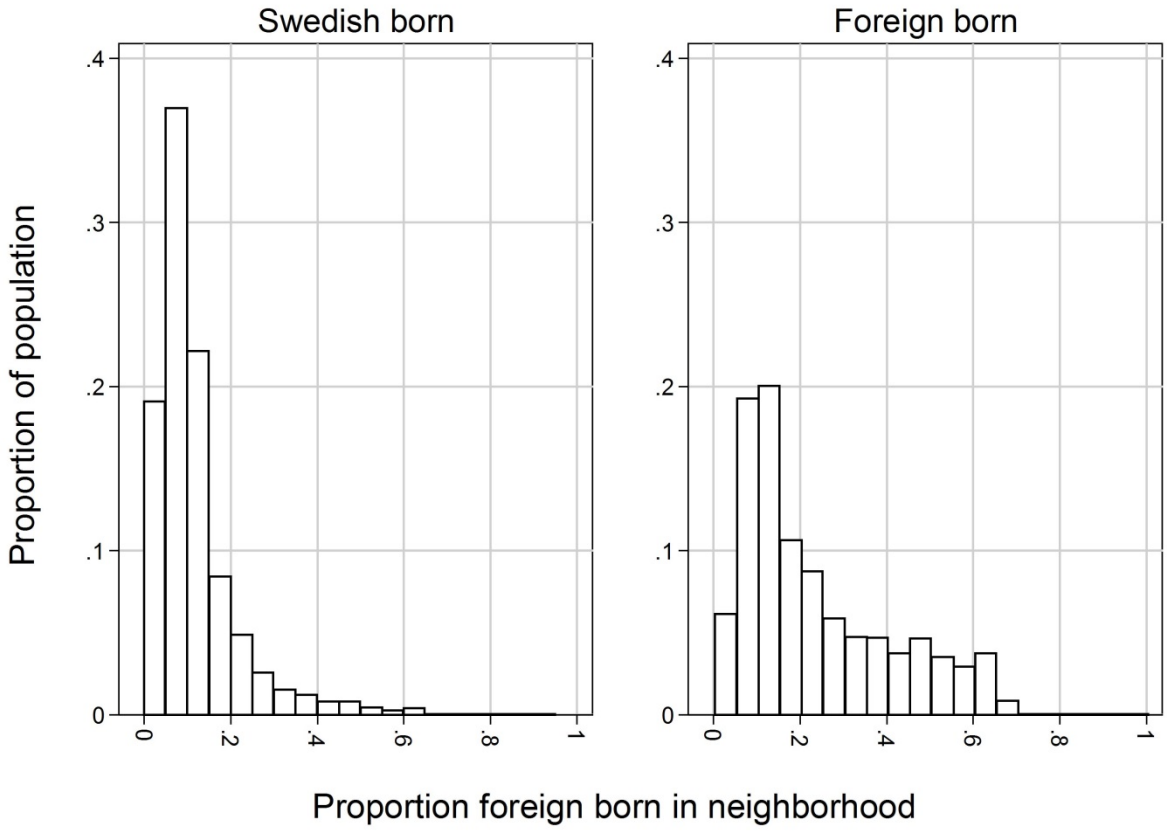

Figure 2. Exposure to foreign-born residents in the neighborhood across native-born Swedes and immigrants
Note: Own calculations based on register data for SAMS neighborhoods (2012).

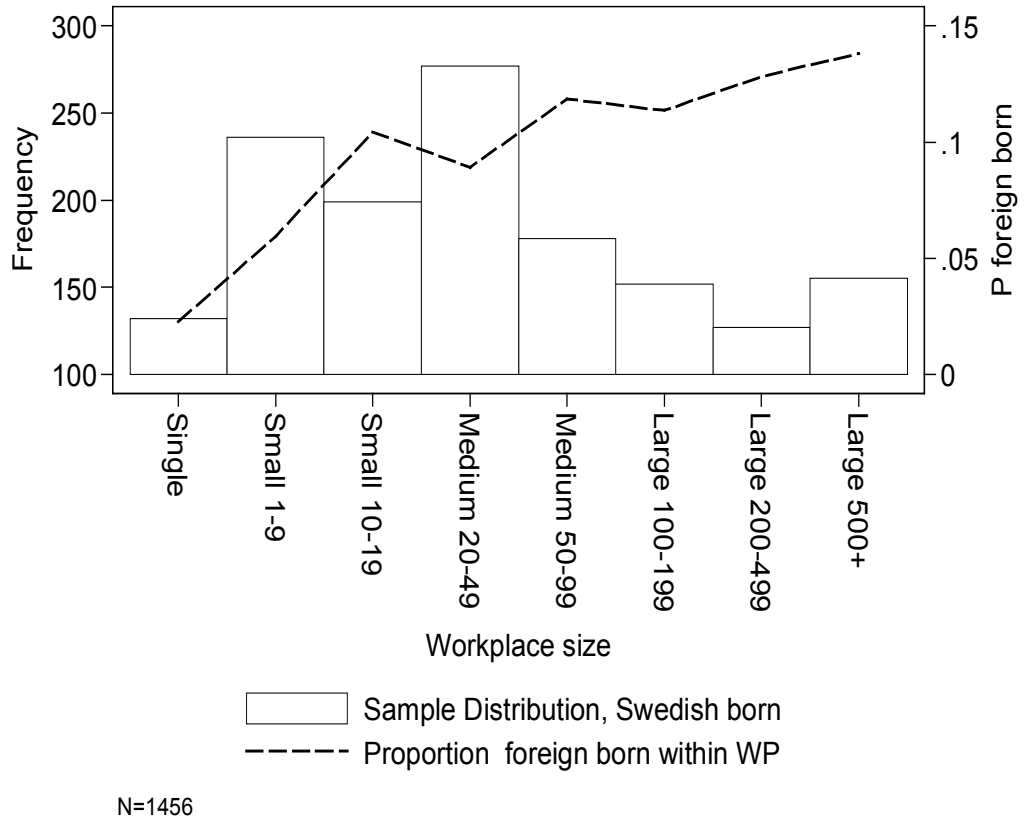

Figure 3. Distribution of workplace sizes among Swedish-born workers and their exposure to foreign-born colleagues within workplaces
Source: Social Networks and Xenophobia register-linked dataset

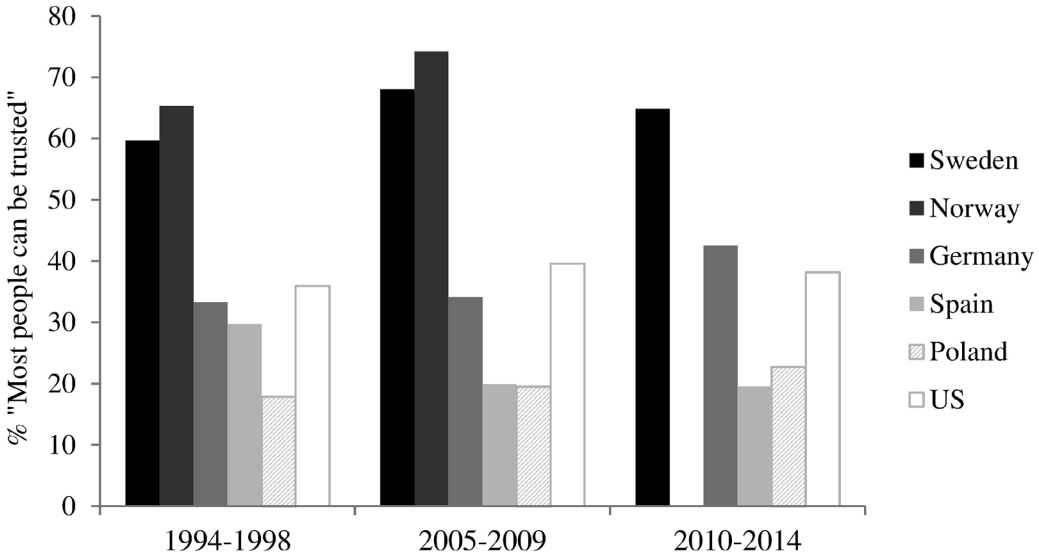

Figure 4. Social trust over time for selected countries
Note: Data for Norway is missing in 2010-2014.
Source: World Values Survey 1981-2014, Longitudinal Aggregate, V.20150418. World Values Survey Association. Aggregate File Producer: Jdsystems, Madrid.

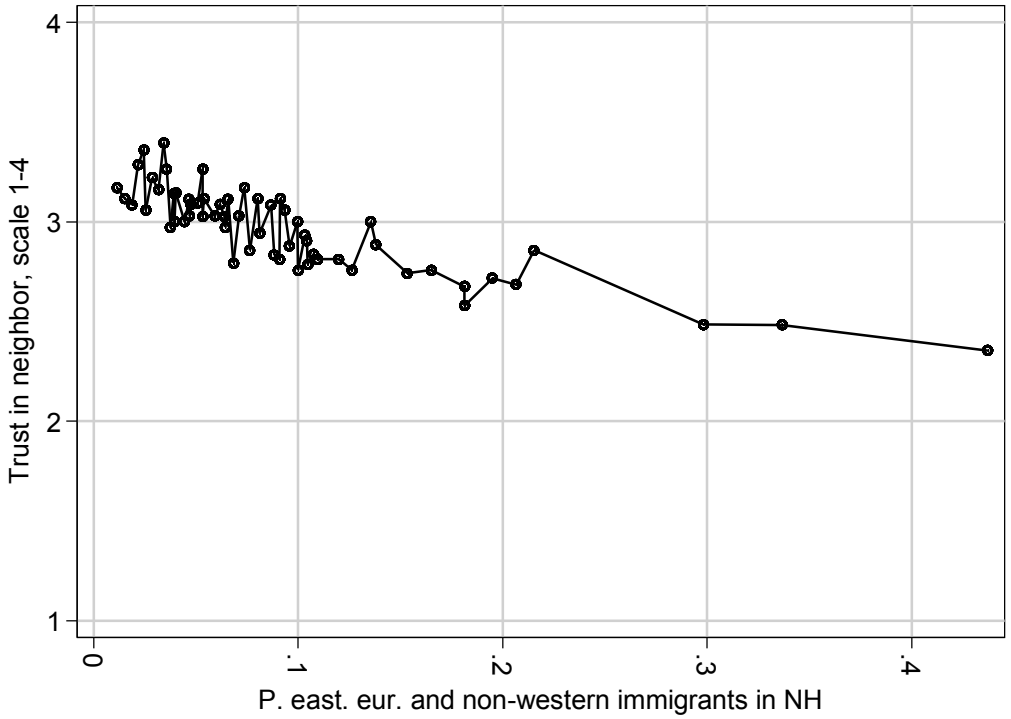

Figure 5. Unadjusted association between trust in neighbors and neighborhood exposure to Eastern Europeans and Non-Western foreign-born residents

Note: The graphs are computed by binning proportion foreign-born by percentiles and aggregating trust for each bin. Trust is scaled 1 (little trust) to 4 (most trust).

Table 1. Description of key variables

Variable	Description
Outcomes	
Trust in neighbors	"In general, how much do you trust your neighbors"? With responses (1) "Not at all", (2) "Rather little", (3) "Rather much", (4) "Completely".
Trust in coworkers	"In general, how much do you trust your coworkers"?
Swedes' interethnic trust[a]	Trust in Eastern Europeans and individuals from the Middle East. Additive scale (α=.93) based on items "In general, how much do you trust [ethnic group]"?
Swedes' ethnocentric trust[a]	Trust in Nordic born minus trust in Eastern Europeans and individuals from the Middle East. Additive scale based on items "In general, how much do you trust [ethnic group]"?
Key context variables	
NH exposure: P East-NW[b]	Proportion individuals born in Eastern Europe and in Non-Western countries (i.e., outside of Western Europe and the US, Canada and Australia) in neighborhood (SAMS area).
NH exposure: P foreign-born[b]	Proportion individuals born outside Sweden in neighborhood (SAMS area).
WP, P foreign-born	Proportion individuals born outside Sweden in workplace
Network, P East-NW	Proportion born in Eastern Europe and in Non-Western countries among 5 nominated friends

[a] Only defined for subsample of Swedish-born w/ Swedish-born parents. [b] Average over residential history for 1990-2012 (with year specific measures).

Table 2. Ordered logit regression of trust in neighbors on static vs. cumulative exposure to foreign-born residents in neighborhoods.

	Last neighborhood		Cumulative exposure 1990-2012	
	(1)	(2)	(3)	(4)
NH (2012), P East-NW	-0.225	-0.239		
	(-1.925)	(-1.818)		
NH (2012), IQR disp. income	0.066*	0.060*		
	(2.190)	(2.103)		
NH (2012), P in rel. deprivation	-0.351*	-0.229		
	(-2.068)	(-1.258)		
NH (2012), P tertiary degree	0.099	0.066		
	(1.039)	(0.483)		
NH exposure, P East-NW			-0.339*	-0.400*
			(-2.175)	(-2.355)
NH exposure, IQR disp. income			0.09	0.097*
			(1.893)	(2.115)
NH exposure, P rel. deprivation			-0.522	-0.359
			(-1.528)	(-0.968)
NH exposure, P tertiary degree			0.083	-0.04
			(0.710)	(-0.285)
NH (2012), ln population density	-0.009*	-0.006	-0.009**	-0.007
	(-2.231)	(-1.521)	(-2.594)	(-1.794)
NH (2012), tenure	0.004**	0.003*	0.003**	0.003*
	(2.802)	(2.320)	(2.706)	(2.153)
Number of moves	-0.003	-0.008	-0.002	-0.008
	(-0.660)	(-1.726)	(-0.513)	(-1.763)
Acquiescence	Yes	Yes	Yes	Yes
Age, gender	Yes	Yes	Yes	Yes
Immigrant dummies	Yes	Yes	Yes	Yes
SES		Yes		Yes
Demographics		Yes		Yes
Municipality controls		Yes		Yes
N	1,998	1,998	1,998	1,998
Pseudo R-squared	0.068	0.081	0.066	0.081

Note: Coefficients describe AME for the probability of observing the highest level of trust in neighbors (= 4, "trust completely"). t-values in parentheses. The static and cumulative measures correlate as follows: P East-NW (0.77), IQR disp. Income (.55), P in rel. deprivation (.64), P tertiary graduation (.84).

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table 3. Ordered logit regression of trust in neighbors by exposure to foreign-born in the neighborhood and in personal networks.

		Confounders			Mediators		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NH exposure, P East-NW	-1.079***	-0.551***	-0.400*	-0.311	-0.324	-0.272	-0.258
	(-9.960)	(-3.546)	(-2.355)	(-1.785)	(-1.860)	(-1.578)	(-1.496)
NH exposure, IQR disp. income		0.103	0.097*	0.095*	0.093*	0.106*	0.104*
		(1.771)	(2.115)	(2.028)	(1.981)	(2.271)	(2.260)
NH exposure, P rel. deprivation		-0.827*	-0.359	-0.353	-0.334	-0.421	-0.438
		(-2.340)	(-0.968)	(-0.966)	(-0.912)	(-1.145)	(-1.194)
NH exposure, P tertiary degree		0.14	-0.04	-0.015	-0.021	-0.022	-0.025
		(1.190)	(-0.285)	(-0.109)	(-0.145)	(-0.156)	(-0.173)
NH (2012), ln population density		-0.009*	-0.007	-0.008	-0.007	-0.008	-0.008
		(-2.493)	(-1.794)	(-1.893)	(-1.822)	(-1.856)	(-1.878)
NH (2012), tenure		0.006***	0.003*	0.003*	0.003*	0.003*	0.003*
		(4.900)	(2.153)	(2.122)	(2.030)	(2.256)	(2.241)
Number of moves		-0.007	-0.008	-0.009	-0.009	-0.008	-0.008
		(-1.690)	(-1.763)	(-1.857)	(-1.909)	(-1.693)	(-1.742)
Female			0.000	0.017	0.015	0.023	0.022
			(0.009)	(0.689)	(0.613)	(0.932)	(0.914)
Age			0.003***	0.002***	0.002***	0.002***	0.002***
			(3.866)	(3.444)	(3.495)	(3.337)	(3.425)
Foreign-born, non-Western			-0.196***	-0.110*	-0.109*	-0.092	-0.092
			(-4.815)	(-2.124)	(-2.101)	(-1.769)	(-1.786)
Foreign-born, East. European			-0.104*	-0.048	-0.052	-0.04	-0.037
			(-2.209)	(-1.001)	(-1.066)	(-0.803)	(-0.747)
Foreign-born, Western			-0.049	-0.042	-0.043	-0.039	-0.037
			(-1.217)	(-1.063)	(-1.071)	(-0.961)	(-0.915)
Second-generation immigrant			-0.089*	-0.082*	-0.083*	-0.073	-0.075
			(-2.226)	(-2.046)	(-2.066)	(-1.828)	(-1.880)
Network, P unemployed friends				-0.154*	-0.151*	-0.146*	-0.145*
				(-2.187)	(-2.127)	(-2.046)	(-2.046)
Network, P East-NW friends				-0.169**	-0.162**	-0.170**	-0.170**
				(-2.861)	(-2.730)	(-2.883)	(-2.893)
Network, P female friends				-0.028	-0.025	-0.021	-0.025
				(-0.885)	(-0.786)	(-0.669)	(-0.791)
Social capital, z-score					0.005	0.005	0.003
					(0.579)	(0.543)	(0.402)
Number of personal victimizations						-0.056*	-0.055*
						(-2.106)	(-2.072)
Number of property victimizations						-0.004	-0.003
						(-0.172)	(-0.147)
Worry about crime (0-2)						-0.058***	-0.054***
						(-4.072)	(-3.778)
Xenophobia							-0.022*
							(-2.109)
Acquiescence	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SES			Yes	Yes	Yes	Yes	Yes
Demographics			Yes	Yes	Yes	Yes	Yes
Municipality			Yes	Yes	Yes	Yes	Yes
N	1,998	1,998	1,998	1,998	1,996	1,992	1,992
Pseudo R-squared	0.025	0.048	0.081	0.084	0.084	0.088	0.090

Note: Coefficients describe AME for the probability of observing the highest level of trust in neighbors (= 4, "trust completely"). t-values in parentheses.
* p<0.05, ** p<0.01, *** p<0.001

Table 4. OLS regression of four types of trust by proportion foreign-born in neighborhoods and personal networks

	Swedes' interethnic trust		Swedes' ethnocentric trust	
	(1)	(2)	(3)	(4)
NH exposure, P East-NW	-0.319	0.189	-0.562	-0.962
	(-0.528)	(0.324)	(-0.811)	(-1.442)
Network, Social capital		Yes		Yes
Victimization		Yes		Yes
Xenophobia		Yes		Yes
N	1,767	1,762	1,622	1,618
Adjusted R-squared	0.101	0.175	0.057	0.156

Note: All models contain controls for all confounding factors presented in Table 3. Coefficients from linear OLS models, where the outcome is a standardized index. t-values in parentheses.
* p<0.05, ** p<0.01, *** p<0.001

Table 5. Ordered logit and OLS regressions of three types of trust on workplace characteristics in the Swedish-born sample

	Trust in neighbors[a]		Trust in coworkers[a]		Swedes' interethnic trust[b]		Swedes' ethnocentric trust[b]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WP, P female	-0.012	-0.016	0.058	0.052	0.193*	0.152	-0.332**	-0.260*
	(-0.331)	(-0.434)	(1.230)	(1.119)	(1.967)	(1.612)	(-2.976)	(-2.450)
WP, ln establishment size	-0.003	-0.002	-0.027***	-0.027***	0.01	0.014	-0.036*	-0.037**
	(-0.675)	(-0.514)	(-4.284)	(-4.328)	(0.782)	(1.132)	(-2.444)	(-2.650)
NH exposure, P foreign-born	-0.683**	-0.555*	-0.53	-0.45	-0.424	-0.109	-0.264	-0.488
	(-2.972)	(-2.426)	(-1.803)	(-1.527)	(-0.696)	(-0.185)	(-0.381)	(-0.736)
WP, P foreign-born	-0.054	-0.027	-0.181	-0.158	0.106	0.125	0.132	0.05
	(-0.640)	(-0.325)	(-1.651)	(-1.455)	(0.479)	(0.589)	(0.513)	(0.202)
Network, Social capital		Yes		Yes		Yes		Yes
Victimization		Yes		Yes		Yes		Yes
Xenophobia		Yes		Yes		Yes		Yes
N	1,312	1,309	1,184	1,180	1,368	1,364	1,271	1,268
Pseudo R-squared	0.066	0.077	0.051	0.062				
Adjusted R-squared					0.08	0.156	0.053	0.149

Note: All models contain controls for all confounding factors presented in Table 3. [a] Coefficients describe AME for the probability that the respective trust measure takes on its highest value. [b] Linear OLS coefficients; outcome is a standardized index. t-values in parentheses.
* p<0.05, ** p<0.01, *** p<0.001

Table A1. Descriptive Statistics

	Mean	(SD)	Min	Max	N
Outcomes					
Trust in neighbors	2.97	(0.751)	1	4	2,036
Trust in coworkers	3.135	(0.604)	1	4	1,591
Swedes' interethnic trust, α= 0.93[a]	0.024	(0.981)	-2	1.93	1,816
Swedes' ethnocentric trust[a]	-0.003	(0.998)	-3	5.89	1,640
Context					
NH exposure, P East-NW	0.072	(0.077)	0	0.65	2,171
NH exposure, P foreign-born	0.116	(0.087)	0	0.67	2,171
NH exposure, IQR disp. income	0.517	(0.123)	0	4.03	2,171
NH exposure, P rel. deprivation	0.054	(0.035)	0	0.35	2,171
NH exposure, P tertiary degree	0.126	(0.075)	0	0.52	2,171
NH (2012), ln population density	6.221	(2.544)	-2	10.37	2,171
NH (2012), tenure	10.734	(8.364)	0	22	2,171
Number of moves	2.39	(2.344)	0	17	2,171
Municipality, P foreign-born	0.174	(0.084)	0	0.47	2,171
Municipality, IQR disp. income	0.636	(0.090)	0	0.82	2,171
Municipality, P in rel. deprivation	0.112	(0.037)	0	0.24	2,171
Municipality, P tertiary degree	0.164	(0.065)	0	0.4	2,171
WP, P female	0.481	(0.321)	0	1	1,663
WP, ln est. size	3.62	(2.098)	0	9.32	1,663
WP, P foreign-born	0.185	(0.277)	0	1	1,789
Network, P unemployed friends	0.032	(0.108)	0	1	2,171
Network, P East-NW friends	0.061	(0.194)	0	1	2,171
Network, P female friends	0.49	(0.386)	0	1	2,171
Social capital, z-score	0.017	(0.978)	-5	1.81	0.017
Individual characteristics					
Female	0.493	(0.500)	0	1	2,171
Age	48.971	(16.711)	18	79	2,171
Foreign-born, non-Western	0.049	(0.216)	0	1	2,171
Foreign-born, East. European	0.027	(0.163)	0	1	2,171
Foreign-born, Western	0.053	(0.223)	0	1	2,171
Second-generation immigrant	0.032	(0.175)	0	1	2,171
>0 child in household	0.302	(0.459)	0	1	2,171
Married	0.468	(0.499)	0	1	2,171
Divorced	0.12	(0.325)	0	1	2,171
Years of education	12.403	(2.669)	6	19	2,171
Ln disposable income	5.2	(0.651)	0	8.89	2,171
Economic status					
Working, no occupation info	0.041	(0.198)	0	1	2,171
Working/studying, no occupation info	0.011	(0.102)	0	1	2,171
Studying	0.037	(0.190)	0	1	2,171
Unemployed	0.03	(0.170)	0	1	2,171
Household/other	0.009	(0.096)	0	1	2,171
Sick-leave	0.016	(0.124)	0	1	2,171
Retired, no occupation info	0.047	(0.211)	0	1	2,171
Unskilled manual	0.226	(0.418)	0	1	2,171
Skilled manual	0.099	(0.298)	0	1	2,171
Routine non-manual	0.102	(0.302)	0	1	2,171
Lower service	0.183	(0.387)	0	1	2,171
Upper service	0.177	(0.382)	0	1	2,171
Entrepreneurs	0.017	(0.128)	0	1	2,171
Farmers	0.006	(0.080)	0	1	2,171
Number of personal victimizations	0.047	(0.223)	0	2	2,171
Number of property victimizations	0.116	(0.339)	0	2	2,171
Worry about crime (0-2)	0.326	(0.522)	0	2	2,133
Xenophobia	-0.011	(1.002)	-2	3.34	2,171

Note: [a] Only defined for the subsample of Swedish-born w/ Swedish-born parents. See Table 1 for definitions of central variables.

Table A2. Correlations among contextual variables

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
(1)	NH exposure, P East-NW	1														
(2)	NH exposure, P foreign-born	0.97	1													
(3)	NH exposure, IQR disp. income	0.18	0.23	1												
(4)	NH exposure, P relative deprivation	0.72	0.71	0.46	1											
(5)	NH exposure, P tertiary degree	-0.07	-0.04	0.37	-0.02	1										
(6)	NH (2012), ln population density	0.41	0.42	0.18	0.22	0.36	1									
(7)	NH (2012), tenure	-0.18	-0.18	-0.11	-0.2	-0.1	-0.25	1								
(8)	Number of moves	0.14	0.14	0.05	0.13	0.09	0.2	-0.68	1							
(9)	Network, P unemployed friends	0.11	0.11	0.01	0.11	-0.04	0.03	-0.09	0.08	1						
(10)	Network, P East-NW friends	0.48	0.46	0.1	0.37	-0.05	0.17	-0.13	0.09	0.07	1					
(11)	Network, P female friends	0.01	0.01	-0.01	0.02	0.01	0.02	0.02	-0.04	0	-0.01	1				
(12)	Municipality, P foreign-born	0.46	0.53	0.21	0.3	0.3	0.56	-0.15	0.13	0.04	0.18	0.02	1			
(13)	Municipality, IQR equ. disposable income	0.38	0.42	0.32	0.36	0.49	0.56	-0.19	0.14	0.01	0.14	0.03	0.81	1		
(14)	Municipality, P in relative deprivation	0.33	0.34	0.18	0.39	0.16	0.33	-0.15	0.1	0.02	0.12	0.02	0.71	0.78	1	
(15)	Municipality, P tertiary graduation	0.23	0.24	0.31	0.19	0.66	0.54	-0.18	0.16	0	0.08	0.01	0.43	0.71	0.25	1

Table A3. Ordered logit regression of trust in neighbors by exposure to foreign-born in the neighborhood and in personal networks among Swedish-born only

		Confounders		Mediators			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NH exposure, P East-NW	-0.910***	-0.38	-0.569*	-0.525*	-0.509*	-0.433*	-0.414
	(-4.924)	(-1.705)	(-2.556)	(-2.338)	(-2.259)	(-1.963)	(-1.867)
NH exposure, IQR disp. income		0.194	0.232	0.252	0.248	0.249	0.244
		(1.947)	(1.691)	(1.638)	(1.680)	(1.873)	(1.868)
NH exposure, P rel. deprivation		-0.571	-0.253	-0.303	-0.314	-0.386	-0.418
		(-1.209)	(-0.497)	(-0.583)	(-0.607)	(-0.770)	(-0.837)
NH exposure, P tertiary degree		0.027	-0.101	-0.098	-0.108	-0.108	-0.11
		(0.184)	(-0.598)	(-0.576)	(-0.633)	(-0.638)	(-0.653)
NH (2012), ln population density		-0.010*	-0.007	-0.007	-0.007	-0.007	-0.007
		(-2.406)	(-1.402)	(-1.410)	(-1.375)	(-1.493)	(-1.521)
NH (2012), tenure		0.006***	0.003	0.003	0.002	0.003	0.003
		(4.000)	(1.758)	(1.715)	(1.611)	(1.859)	(1.870)
Number of moves		-0.010*	-0.012*	-0.013*	-0.013*	-0.012*	-0.012*
		(-2.031)	(-2.303)	(-2.406)	(-2.473)	(-2.250)	(-2.291)
Female			0.002	0.022	0.021	0.03	0.03
			(0.088)	(0.784)	(0.781)	(1.096)	(1.087)
Age			0.003***	0.002**	0.002**	0.002**	0.002**
			(3.414)	(2.926)	(2.926)	(2.828)	(2.924)
Foreign-born, non-Western			N/A	N/A	N/A	N/A	N/A
Foreign-born, East. European			N/A	N/A	N/A	N/A	N/A
Foreign-born, Western			N/A	N/A	N/A	N/A	N/A
Second-generation immigrant			N/A	N/A	N/A	N/A	N/A
Network, P unemployed friends				-0.234**	-0.226**	-0.215**	-0.210*
				(-2.878)	(-2.765)	(-2.585)	(-2.538)
Network, P East-NW friends				-0.145	-0.143	-0.148	-0.149
				(-1.620)	(-1.607)	(-1.674)	(-1.656)
Network, P female friends				-0.035	-0.033	-0.027	-0.032
				(-0.974)	(-0.921)	(-0.740)	(-0.892)
Social capital, z-score					0.012	0.012	0.01
					(1.289)	(1.203)	(1.075)
Number of personal victimizations						-0.046	-0.047
						(-1.373)	(-1.382)
Number of property victimizations						-0.001	0
						(-0.022)	(0.005)
Worry about crime (0-2)						-0.068***	-0.063***
						(-4.090)	(-3.755)
Xenophobia							-0.027*
							(-2.335)
Acquiescence	No	No	Yes	Yes	Yes	Yes	Yes
SES	No	No	Yes	Yes	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,696	1,696	1,696	1,696	1,696	1,692	1,692
Pseudo R-squared	0.008	0.032	0.064	0.067	0.068	0.073	0.075

Note: Coefficients describe AME for the probability of observing the highest level of trust in neighbors (= 4, "trust completely"). t-values in parentheses .

* p<0.05, ** p<0.01, *** p<0.001

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