Workplace Contact and Support for Anti-Immigration Parties

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Workplace Contact and Support for Anti-Immigration Parties*

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Abstract

How does an increased presence of immigrants in the workplace affect anti-immigration voting behavior? While cooperative interactions between natives and immigrants can reduce intergroup prejudice, immigrant co-workers might be regarded as a threat to native-born workers' labor market position. We combine detailed Swedish workplace data with precinct-level election outcomes for a large anti-immigration party (the Sweden Democrats) to study how the share of non-Europeans in the workplace affects opposition to immigration. We show that the share of non-Europeans in the workplace has a negative effect on support for the Sweden Democrats and that this effect is solely driven by same-skill contact in small workplaces. We interpret these results as supporting the so-called contact hypothesis: that increased interactions with minorities can reduce opposition to immigration among native-born voters, which, in turn, leads to lower support for anti-immigration parties.

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1 Introduction

A majority of Western European countries have seen a large increase in the number of immigrants in recent decades. In many EU countries, the number of foreign-born individuals as a share of the population has come to exceed 10% and is in some cases approaching 15% or higher (Eurostat 2020). At the same time, the rise of anti-immigration parties constitutes one of the largest changes to the European political landscape, as an increasing share of voters now support anti-immigration and protectionist policies. A causal relationship between increased support for these types of parties and a higher influx of immigrants, especially from non-European countries, is often suggested in academic debates: in other words, natives are believed to oppose immigration when they are exposed to immigrants and refugees.

Yet, it is likely that certain conditions influence the effects of increased exposure to immigrants with regard to voting for anti-immigration parties, such as the skill level of immigrants (Hainmueller and Hiscox 2010; Valentino et al. 2019) or the socio-economic status among natives (Strömblad and Malmberg 2016). An additional important aspect is the *type* of intergroup contact associated with an influx of immigrants. According to the classic *contact hypothesis* (Allport 1954), extensive and cooperative interactions between majority and minority groups can undermine prejudices and/or negative sentiments about members of minority groups. At the same time, *superficial* contact might instead have an adverse effect on prejudices toward minority groups. Indeed, studies finding a positive association between exposure to immigrants and opposition to immigration often capture superficial contact (Knigge 1998; Lubbers et al. 2002; Becker and Fetzer 2016; Hangartner et al. 2019), while *meaningful* contact, typically cooperation between majority and minority group members, has been shown to reduce prejudice (McLaren 2003; Steinmayr 2020; Simonovits et al. 2018; Finseraas and Kotsadam 2017; Finseraas et al. 2019; Mousa 2019).[1]

This study focuses on the *workplace*, which facilitates cooperative contact between majority and minority group members. Due to this feature, it serves as a social arena in which interactions are

[1] Throughout the paper, we use the term *contact* to refer to the presence of non-European co-workers in general, and we specify prejudice-reducing contact as *meaningful* while the opposite type is referred to as *superficial* or *casual*. The latter type is also referred to as *exposure* (cf. Allport 1954; Amir 1969).

expected to help reduce opposition to immigration, in line with the contact hypothesis. However, immigrants in the workplace may also reinforce natives' fear of increased competition for higher wages and employment, as predicted by the *ethnic competition hypothesis*. For these reasons, an increased presence of immigrants in the workplace potentially has both positive and negative effects on voting for anti-immigration parties. The cooperative and repetitive nature of meaningful workplace contact is expected to reduce negative sentiments toward immigrants, whereas a high share of foreign-born co-workers might signal to native workers that their employment status is threatened. Thus, the overall effect of contact in the workplace could yield mixed results. Despite being one of our most important arenas for social interactions, to the best of our knowledge, there are no large-N empirical studies addressing this particular question.

We examine the consequences of workplace contact between native-born and non-European workers in terms of support for anti-immigration parties. Our focus is Sweden, which offers an interesting setting due to *i*) high-quality administrative data and *ii*) a high and, for a long time, growing share of foreign-born individuals in the workforce. We use a full population Swedish data set that includes demographic and socioeconomic information on all legal residents, while also linking all employed residents to a unique workplace. This allows us to compute the share of co-workers with a non-European background for each native-born worker. These individual-level shares are aggregated at the election precinct level and matched with election outcomes for the largest Swedish anti-immigration party, the Sweden Democrats (SD) in the national elections in 2006, 2010, and 2014. Our research design allows us to estimate the effect of precinct-level workplace contact on support for SD.

We find large and statistically significant negative effects of the workplace share of non-Europeans on support for SD. In our preferred specification, a one standard deviation increase in the average share of non-European co-workers among resident natives decreases the share of votes for SD in the election precinct by more than 0.4 percentage points, or roughly 8% of the standard deviation in the outcome. An extensive set of sensitivity checks corroborate the estimated negative effect.

We further extend our baseline result with a number of auxiliary analyses and reach the following conclusions. First, increased workplace contact between natives and non-Europeans results in a

decreased support for SD, which is in line with the contact hypothesis (Allport 1954). This suggests that overall intergroup workplace contact is characterized by meaningful contact. Several features of the workplace allow contact to lower the level of opposition to immigration. Workers usually interact repeatedly over longer time periods and work together toward a common goal.

Second, we find that the negative relationship between workplace contact and opposition to immigration is solely driven by same-skill contact. This means that any potential positive effect of same-skill labor market competition on voting for anti-immigration parties is offset by the effect of meaningful workplace contact between co-workers. At the same time, the presence of different-skill non-European co-workers has no statistically significant effect on opposition to immigration. Since many workplaces exhibit low levels of vertical integration, we expect contact to primarily occur within the same skill level.

Third, we categorize workplaces based on their size and find that same-skill contact in small workplaces has a negative effect on opposition to immigration, while the opposite effect is estimated for same-skill contact in large workplaces. Exposure to non-European co-workers in the latter setting does not offset the positive impact of fear of same-skill labor market competition on opposition to immigration. Hence, contact in this setting lends support to the ethnic competition hypothesis. When considering contact with different-skill non-European co-workers, the estimates are statistically indistinguishable from zero, regardless of workplace size. These results suggest that superficial contact only leads to opposition to immigration when there is a threat of increased labor market competition. In other words, both superficial contact and the threat of same-skill labor market competition are necessary but not sufficient conditions for workplace contact with foreign-born co-workers to result in higher support for anti-immigration parties.

Finally, we provide suggestive evidence of a negative association between workplace contact and votes for SD *only* among workers employed in occupations characterized by a low risk of experiencing job loss. A possible interpretation of these results is that labor market competition from immigrants becomes more salient for natives when the natives' occupation is vulnerable and there is a greater risk of job loss.

Our results add to the extensive scholarship on the emergence and success of anti-immigration and radical right parties. This literature mainly focuses on two explanatory variables: economic conditions and immigration. In both cases, however, the evidence is inconclusive as some studies find economic distress (e.g., job separations, unemployment rates) and exposure to immigrants to be positively correlated with voting for anti-immigration parties, while other studies find the opposite or no correlation. Specifically, we add to the literature concerning exposure and contact with immigrants and how these affect support and demand for anti-immigration policies. Most studies in this field of scholarship rely on measures of local, regional, or national shares of immigrants in addition to either aggregated vote shares or survey data on anti-immigration attitudes. In most cases, the types of interactions that exist between majority and minority group members are unknown. We add to the literature by specifically considering interactions between co-workers in the workplace.

We also relate to the literature on the perceived increase in labor market competition facing natives as a result of same-skill immigration. Native workers with a particular skill are expected to lose from the immigration of people with the same skill, as this raises the level of competition for jobs and lowers the relative wages for the same skill level (Borjas et al. 1996, 1997). We show that the positive impact of fear of labor market competition on opposition to immigration can be offset by meaningful workplace contact, thus in support of the contact hypothesis.

2 Theoretical background and related research

The Sweden Democrats were founded in 1988 by former members of the neo-Nazi party the Sweden Party. The party had limited electoral success during its first decade, which led to a re-branding process in the late 1990s, and the party received 1.4% of the votes in the 2002 national election. Eight years later, SD entered the Swedish parliament for the first time after receiving 5.7% of the cast votes, resulting in 20 seats. In 2014, the party became the third-largest party in Sweden by receiving close to 13% of the cast votes.

SD presents itself as a social conservative, anti-immigration party, and its supporters have been shown to view immigration as one of the most salient political issues (Rydgren and Van der Meiden

2019). It thus seems natural to assume that the consequence of increased immigration is higher support for SD, and there are several hypotheses predicting this. Indeed, a large number of studies provide evidence of a positive association between immigration and support for anti-immigration parties in many European countries (for instance, Knigge 1998; Lubbers et al. 2002; Becker and Fetzer 2016; Valdez 2014; Hangartner et al. 2019). A popular explanation for this relationship is that natives fear that their social status and identity will be challenged as they are exposed to ethnic minorities (*group positioning theory*). This means that natives in neighborhoods with a high share or large influx of immigrants are more likely to oppose immigration, which subsequently leads to higher support for anti-immigration parties.

Similarly, the *ethnic competition hypothesis* argues that opposition to immigration among natives is based on their fear of competition for employment, housing, and general social welfare between ingroup and outgroup members. One can imagine that this fear intensifies following a large influx of immigrants, which leads to increased opposition to immigration. The fear of competition for economic resources can be split into two parts: *i)* fear of reduced access to welfare services and benefits and *ii)* fear of competition for employment and high wages.[2] In terms of the latter factor, it seems reasonable that the presence of immigrants in one's workplace highlights the threat of competition to a greater extent than by observing them in your neighborhood. Hence, we identify the following prediction in line with the ethnic competition hypothesis:

Prediction 1. *An increased presence of foreign-born workers in the workplace highlights the threat of competition for employment and high wages, thereby increasing opposition to immigration.*

However, the expected competition between natives and immigrants depends on the skill level of the members of the two groups. According to the *factor-proportion analysis model* (see, for instance, Borjas et al. 1996, 1997), natives with a particular skill level are expected to be worse off from the immigration of people with the same skill level, as this increases the relative supply of workers with

[2]Natives may also oppose low-skill immigrants whom they expect to be a net burden on public finances.

this particular skill level and lowers their relative wages.[3] This suggests that the existence of *same-skill* foreign-born co-workers has a stronger impact on opposition to immigration. We thus identify an augmented prediction:

Prediction 1.1. *An increased presence of* ***same-skill*** *foreign-born workers in the workplace is more likely to highlight the threat of competition for employment and high wages, thereby increasing opposition to immigration.*

A different relationship between the presence of immigrants and opposition to immigration is predicted by the *contact hypothesis* (Allport 1954), which argues that intergroup contact may reduce prejudice. Instead, an increased presence of immigrants predicts a lower support for anti-immigration parties. However, the *type* of contact is crucial in terms of how it influences prejudice. Allport (1954) notes that cooperation between members of ingroups and outgroups reduces negative sentiments, while prejudice may increase from superficial or casual contact. It is further argued that contact is more likely to reduce prejudice if it occurs at an equal level, is repetitive, and aims toward a common goal. In accordance with the contact hypothesis, we identify the second prediction:

Prediction 2. *An increased presence of foreign-born workers in the workplace leads to meaningful intergroup contact, which, in turn, reduces prejudice toward minorities and opposition to immigration.*

Since teamwork dominates the modern labor market (Hamilton et al. 2003), the workplace is likely to satisfy the conditions under which intergroup contact reduces opposition to immigration. This is particularly true for interpersonal contact between co-workers with the same skill. Imagine a firm employing both low-skill and high-skill workers. At one of this firm's workplaces, all employees potentially interact at, for instance, the workplace cafeteria. However, since workers with the same skill perform similar tasks, they are more likely to encounter each other frequently in the workplace.

[3] The empirical evidence of the consequences of immigration on natives' wages is inconclusive. See Longhi et al. (2005) for an overview.

This means that there is a larger opportunity for meaningful and cooperative contact. Same-skill workers are also more likely to share similar backgrounds and experiences and to consider each other as being equals. Same-skill contact with foreign-born co-workers is thus more likely to reduce opposition to immigration, contrary to the consequences of an increased presence of same-skill immigrants formulated in Prediction 1.1.

This study examines the effect of workplace contact between natives and their non-European-born co-workers on support for SD. The ethnic competition hypothesis and the contact hypothesis predict effects going in opposite directions, and the net effect is ambiguous. Our main analysis considers the *overall* workplace contact between natives and workers born outside Europe, and we extend the analysis to same-skill and different-skill contact. It is important to note that our analyses do not allow us to reject any of these hypotheses with certainty. A positive net effect of intergroup contact in the workplace on opposition to immigration lends support to the ethnic competition hypothesis, while the opposite effect supports the contact hypothesis. Still, the consequences of workplace contact with regard to opposition to immigration could differ depending on the workplace environment. While some native workers might engage in meaningful workplace contact with their immigrant co-workers, others might view immigrants as a threat to their labor market position. Although the net effect of overall workplace contact might be negative, both hypotheses could still be valid.

In addition, workplaces with a certain set of characteristics might facilitate more of one specific type of contact. One example is same-skill contact where a closer and more meaningful form of contact is to be expected. The net effect in a workplace environment characterized by same-skill contact might differ compared to other environments.

Another important aspect potentially influencing the type of workplace contact is the social and economic status of the members of the majority group. According to Allport (1954), one needs to consider whether an individual engaging in contact with, for instance, an ethnic other has "basic security in his own life, or is [...] fearful and suspicious" (p. 263). An important basic security is natives' labor market position: the fear of being separated from their jobs (e.g., due to globalization or

technological change) can affect how natives react to foreign-born co-workers.[4] In occupations where the expected job loss is at a very low level, an increased share of immigrants in the workplace in the same occupation is arguably less of a threat to natives compared to occupations where separation or turnover is high. This leads to a final prediction:

Prediction 3. *The threat of increased competition for employment and high wages with foreign-born workers is only highlighted among native workers employed in* ***vulnerable*** *occupations.*

Empirically, the aforementioned studies on the association between exposure to minorities in one's neighborhood and voting for anti-immigration parties point in the same direction, namely to increased support for such parties.[5] However, a recent wave of research provides evidence of reduced prejudice due to intergroup contact in a large variety of settings.[6] For instance, a 3-week camp for Israeli and Palestinian teenagers was found to improve the participants' outgroup attitudes (Schroeder and Risen 2016). Mousa (2019) shows that Iraqi Christians randomly assigned to soccer teams consisting of both Christians and Muslims were more likely to interact with Muslims in the future. Finseraas and Kotsadam (2017) and Finseraas et al. (2019) use a field experiment where Norwegian soldiers were randomly assigned to share rooms with ethnic minorities. The former study finds that contact with ethnic minorities reduced negative stereotypes regarding immigrants' work ethics, while the latter study finds that intergroup contact increased trust toward immigrants. The setting in these two studies resembles the workplace, as the recruits were working closely together and had to cooperate in order to solve tasks.

The social settings in these studies encompass a much closer and more meaningful contact than the superficial contact one would expect from minorities simply being visible in the neighborhood, and they do not highlight intergroup competition for economic resources. In these types of settings,

[4] Dehdari (forthcoming) shows that layoff notices received by low-skill native workers lead to stronger support for SD in areas populated by low-skill immigrants.

[5] There are a few exceptions. Steinmayr (2020) finds that support for the Austrian right-wing party the Freedom Party of Austria increased in areas where refugees were merely passing through in 2015, while it was lower in communities actually hosting refugees.

[6] Pettigrew and Tropp (2006) conduct a meta-analytic test based on 515 studies and conclude that intergroup contact typically reduces intergroup prejudice.

the effect of contact is less ambiguous.

So far, we have described the conditions under which meaningful contact can occur without discussing the empirical evidence of the existence of these conditions. Is there any corroborating evidence of workplace settings facilitating the type of contact required for reducing prejudice? If so, what characterizes these settings? Several studies have noted that interactions in the workplace differ from those in other contexts. For instance, Mutz and Mondak (2006) emphasize how a larger portion of the day is spent in the workplace compared to other organizations or the neighborhood. Furthermore, almost all jobs "require contact with coworkers or customers" (Mutz and Mondak 2006, p. 141). Kokkonen et al. (2014) add that cooperation between colleagues is essential for achieving work-related tasks and that individuals are "typically assigned to interact with colleagues who they have not chosen themselves" (p. 269). Opting out from such tasks is costly as this might lead to displacement.

In other words, the workplace facilitates frequent and cooperative contact between co-workers who would not necessarily interact in other contexts. This contact fulfills several of the conditions set up by the contact hypothesis and is thus positioned to reduce prejudice.[7] Kokkonen et al. (2014) note that one of the conditions might not be fulfilled, namely equal status between members of the ingroups and outgroups.[8] This highlights the importance of also considering same-skill workplace contact, where co-workers are arguably equal in status.

The relationship between intergroup workplace contact and prejudice is examined in a small number of studies using survey data on self-reported interactions with minority members or the presence of immigrant co-workers. While the outcome of interest is typically ethnic prejudice (Quillian 1995; Wagner et al. 2006; Strabac 2011), Escandell and Ceobanu (2009) measure foreign exclusionism and Thomsen (2012) examines attitudes toward ethnic minority rights. The latter study uses Danish survey data to show that perceived group threat is weakened by intergroup workplace contact, which, in turn, leads to higher ethnic tolerance. Thus, the study specifically examines whether workplace contact reduces the perception of a threat imposed by members of other ethnic groups. This is di-

[7] Another important condition is that the contact is sanctioned by a higher authority. The workplace also fulfills this condition as contact between co-workers is supported by owners and managers.

[8] It is worth noting that Pettigrew and Tropp (2006) argue that contact can still reduce prejudice even when one or several of the key conditions are not fulfilled.

rectly related to the predictions formulated in this study, with one exception. Thomsen (2012) does not specify in which contexts the threat emerges nor which type of threat perception is weakened as a result of contact. For instance, workplace contact might reduce perceptions of a threat to social status caused by exposure to immigrants in the neighborhood.

We argue that the presence of immigrants in the workplace first and foremost highlights the threat of increased labor market competition, which, in turn, can affect the possibility of meaningful intergroup workplace contact. In particular, competition is likely to influence natives' feelings towards cooperation with their immigrant coworkers. For instance, natives who fear increased labor market competition might be predisposed to avoid collaborative contact with their immigration coworkers simply due to feelings of mistrust towards those they perceive as being their competitors (cf. Finseraas et al. 2019). Another possibility is that natives refrain from cooperation that benefits both sides to avoid improving the labor market competitiveness of their immigrant coworkers.[9] In both cases, prejudice-reducing intergroup contact will be affected. In workplace contexts with a strong fear of labor market competition with immigrants, the perceived group threat is likely to impede cooperative contact, resulting in increased opposition to immigration.

A substantial body of literature examines perceived group threat as resulting from interactions between majority and minority group members in neighborhoods. However, less is known when it comes to intergroup conflict in the workplace. Bonacich (1972) argues that antagonism between ethnic groups can be caused by large differentials in the price of labor, a so-called *split labor market*. In these situations, high-price labor—typically natives—will try to prevent low-price labor—typically ethnic minorities—from entering the labor market. This occurs as high-price labor fears being displaced in favor of cheaper labor. It seems plausible that the presence of ethnic minorities in the workplace highlights group-level competition for employment and high wages. Dixon and Rosenbaum (2004) note that workplaces "dually encourage collaboration and competition" (p. 261). While collaboration is in line with our second prediction, competition speaks to the first.

Similarly, Laurence et al. (2018) argue that the presence of immigrant co-workers in the work-

[9] Lowe (2021) studies how collaborative intergroup contact between Indian men of different castes affects cross-cast friendship. He finds that meaningful contact increases cross-cast trade that benefits both parties engaging in transactions.

place can lead to both positive and negative contact. Greater outgroup exposure indirectly increases (reduces) prejudice through greater positive (negative) intergroup contact. The authors note that contact in the workplace can be superficial, involuntary, or competitive, thus leading to more negative intergroup attitudes (see also Amir 1969). According to Escandell and Ceobanu (2009), austere economic environments are likely to increase individual and group competition. Labor market competition could offset the potential benefits of meaningful workplace contact. Indeed, their analysis based on Spanish survey data finds no statistically significant association between workplace contact and foreigner exclusionism.

Summing up, according to Predictions 1 to 3, the expected net effect of increased contact between natives and non-Europeans in terms of support for anti-immigration parties is ambiguous. If workplace contact is mainly superficial, natives might fear that their economic (and social) status is threatened, which would boost anti-immigration voting behavior. If, on the other hand, the workplace facilitates contact between co-workers that is meaningful and cooperative, we expect it to reduce natives' opposition to immigration. In such workplaces, it is important that the natives' fear of labor market competition does not lower the tendency to collaborate or in other ways diminishes prejudice-reducing intergroup contact. To the best of our knowledge, this paper is the first to study the relationship between intergroup contact in the workplace and support for anti-immigration parties.

3 Data and empirical design

3.1 Sample construction and data

The choice of situating our empirical analysis in the Swedish context is not only driven by the availability of high-quality administrative data. Several other factors make Sweden particularly suitable for this type of analysis. First, it presents a setting with a relatively high and growing share of foreign-born individuals in the workforce. At the same time, the share of votes for the largest Swedish anti-immigration party has increased substantially over the last two decades. Following the 2010 elections, which saw SD enter the national parliament for the first time, the Swedish political landscape

resembles that of other Western countries, thus making the phenomenon studied in this particular case more generalizable to the contexts of other countries.

Second, compared to other Western countries, Swedish voters are among the least prejudiced (Messing and Ságvári 2019). Any reduction in prejudice stemming from workplace contact would thus occur at relatively low initial levels. In addition, teleworking is more common in Sweden than in most other countries in the European Union, with a share of employed individuals working from home "regularly or at least sometimes" close to 20% in 2009 (Milasi et al. 2020). So, if we find that workplace contact affects support for SD in Sweden, we would expect to find similar effects in other countries with *i)* a higher initial level of ethnic prejudice and *ii)* workplace contact entailing more in-person contact.

Election data: Vote shares for SD

National and local elections in Sweden take place once every fourth years, of which we use the outcomes in three consecutive (national) elections between 2006 and 2014. The reason for choosing these years is fairly straightforward: we do not yet have access to any data from the latest election (2018), thus making 2014 the most recent election available in the data. Before the 2006 national election, SD remained very small, receiving close to 1.5% of the votes in the 2002 national election.

Our outcome of interest is the election results for SD. Due to secret ballots, we cannot track voting for each individual voter. Instead, we use the lowest level of aggregation, namely the election precincts. There are more than 5,500 precincts in each election, and each voter is linked to a unique election precinct based on their residence. On average, each precinct comprises around 1,000 eligible voters. A number of precincts are either eliminated, altered, or merged with other precincts between two consecutive elections. To get comparable geographical units over time, we match precincts between elections using data on population density. This matching procedure leaves us with a total of 5,836 comparable precincts and is described in more detail in the online Appendix, Section B.1.[10]

[10]Tables, figures, and section names beginning with a Latin letter (e.g., A1, C3) refer to the online Appendix.

Using workplace data to measure workplace contact

To construct our treatment and control variables, our analyses use detailed individual-level full population administrative data. These data are provided by Statistics Sweden and include both matched workplace and individual-level data for a rich set of socioeconomic and demographic variables. The individual-level information is used to construct aggregated measures at the election precinct level.

To create our treatment variable, we utilize the fact that the vast majority of working individuals are not only linked to a specific firm but also to a unique workplace. A workplace is defined as an address, property, or group of connected premises where a company operates. All firms have at least one workplace, while firms with operations in more than one address or property are divided into several workplaces. Since we wish to capture contact, a workplace is arguably more suitable than a firm, which can entail large enterprises with many workers who never encounter each other.

We connect each individual to the share of workers born *outside Europe* in his or her workplace.[11] We choose to focus on non-Europeans rather than immigrants in general, since we expect negative views on or prejudice against immigrants from, for example, Nordic or Western European countries to be less prevalent. Since the outcome is measured at the precinct level, we aggregate the workplace shares to this level. The treatment variable is thus created in two steps. We first define the share of non-European immigrant co-workers at workplace w for individual i living in precinct p in election year t if individual i is born in Sweden:

$$Im\_Share_{iwpt} = \frac{Numb\_Im_{iwt}}{Numb\_Cowork_{iwt}}, \tag{1}$$

where $Numb\_Im_{iwt}$ is the number of non-European co-workers while $Numb\_Cowork_{iwt}$ is the total number of co-workers of worker i at workplace w. We use the shares for each native to create the precinct average share of non-European co-workers among natives *residing* in precinct p ($i \in P$, where P represents the set containing all natives in precinct p):

[11] We exclude North American and Japanese immigrants when constructing the workplace shares of non-European workers.

$$Mean\_Im\_Share_{pt} = \frac{1}{|P|} \sum_{i \in P} Im\_Share_{iwpt} \tag{2}$$

$Mean\_Im\_Share_{pt}$, is our main treatment variable and measures the mean share of non-European co-workers for native workers residing in precinct p and election year t. It is important to note that we use information from the administrative data on where workers reside, which means that the workplace contact for each native is linked to where they vote, rather than where they work. When constructing our treatment according to Equation (2), we restrict the pool of workers by removing *i)* all self-employed individuals and *ii)* all workers with no co-workers at their workplace. These restrictions exclude very small workplaces in which the share of non-European co-workers is not an informative indicator.

Constructing the precinct-level treatment comes with two important caveats. First, the individual-level share according to Equation (1) assumes a (weakly) monotonically increasing relationship between workplace contact and the share of non-European co-workers. In other words, a larger share of non-European co-workers is assumed to be indicative of more workplace contact with immigrant co-workers. As the individual-level measures of contact are aggregated at the precinct level, we are unable to capture potential non-linearities in this relationship.

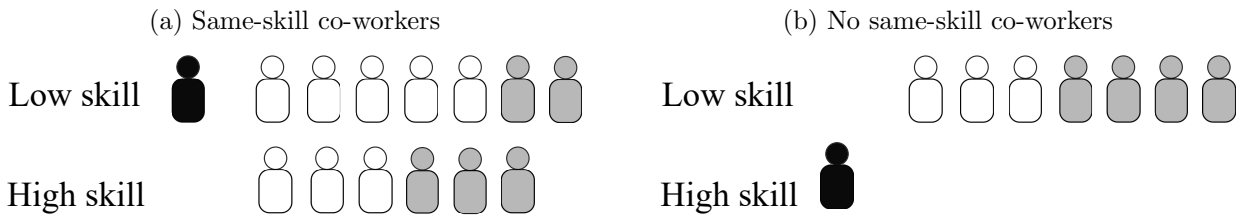
Second, the precinct-level measure of workplace contact for the average precinct resident (Equation (2)) does not use the share of non-European workplace co-workers for *all* natives. Self-employed individuals and those with no workplace co-workers are excluded, which means that the degree of contact with non-Europeans at work is not accounted for with regard to a large portion of the precinct (native) population. On the other hand, the election outcomes are the result of (almost) all natives' individual-level voting actions.[12] This measurement error will induce a downward bias in our estimated treatment effect.

Crucially, this bias is larger when we further restrict the pool of workers to only include, for instance, same-skill co-workers. This is illustrated by Figure 1, which demonstrates how the individual-

[12] Although Sweden has a relatively large share of foreign-born citizens, the turnout rate is much higher among natives (Aggeborn et al. 2020).

level shares of co-workers are computed. In 1a, native worker i (represented by the black figure) has a total of thirteen co-workers, eight of whom are natives (white figures) and five of whom are non-European (light gray figures). Thus, native worker i's share of non-European co-workers is 5/13, or approximately 0.38. If we instead compute the share of same-skill non-European co-workers, this share is 2/7, or close to 0.29. In the second part, Figure 1b, the high-skill native worker (black figure) has a total of seven co-workers, four of whom have a non-European background, but no same-skill co-workers. This worker is included when computing the precinct-level average for the share of non-European co-workers and excluded when we only consider the share of same-skill co-workers. Consequently, the estimates using the same-skill shares are even more likely to suffer from attenuation bias.[13] This is important to keep in mind as we interpret the magnitude of our estimates. In the online Appendix, Section B.2, we discuss these caveats further.

Figure 1: Computing the individual-level share of non-European co-workers



Notes: Example of workplaces with both same-skill and different-skill co-workers (Figure 1a) and with only same-skill co-workers (Figure 1b).

3.2 Empirical design

We examine the relationship between the workplace presence of non-Europeans among natives and support for SD by estimating the parameters of the following linear regression model:

$$SD_{pt} = \beta Mean\_Im\_Share_{pt} + \mathbf{\Gamma}'\boldsymbol{x}_{pt} + \Phi_p + \tau_t + \varepsilon_{pt}, \tag{3}$$

where SD_{pt} gives the share of votes for SD in precinct p and election t, while $Mean\_Im\_Share_{pt}$ measures the precinct-level mean of non-European immigrant co-workers among native-born precinct

[13] We analyze this potential attenuation bias due to measurement errors by restricting the size of the pool of workers used to compute the precinct-level measures of workplace contact. This restricted size corresponds to the size used when only computing same-skill contact. As expected, the induced measurement errors yield estimates closer to zero. We discuss this method in more detail in the online Appendix, and the results are presented in Table B5.

residents. We add the vector $\boldsymbol{x}_{pt}$ of precinct-level time-varying controls, which is based on the entire adult population in the precinct. This vector includes precinct population and population squared, mean number of days unemployed, percentage of non-working individuals among natives, percentage of non-European immigrants living in the precinct, percentage of non-European immigrants who are citizens, and percentage of individuals with a low level of education (separately for natives and non-Europeans).[14] The same vector also includes a number of controls aggregated from the workplace level for all working residents in a precinct: average wage among co-workers, share of young co-workers, and share of male co-workers. These variables are constructed in a manner similar to our contact variable according to Equations (1) and (2).[15] We include time and precinct fixed effects, represented by τ_t and Φ_p, respectively. These account for national trends as well as time-invariant precinct characteristics. Our preferred specification interacts the time trends with commuting zone fixed effects to capture any specific time trend for a specific commuting zone (60 regions in Sweden). The arguably most important component is Φ_p, representing precinct fixed effects. The analysis thus compares outcomes within precincts over time.[16]

3.3 Identification

For identification of β, we require that there is no unobserved variation related to votes for SD in precinct p that also confounds the share of non-European co-workers among native residents in precinct p. Our preferred specification addresses most potential threats with regard to identification. The precinct fixed effects (Φ_p) capture all static differences between neighborhoods. Using only variation over time, our research setting thus asks: are the precinct-level votes for SD significantly higher or lower than the average in an election year with unusually many non-European co-workers among the precinct natives? The precinct fixed effects likely account for a large part of the omitted factors that are simultaneous with both the share of non-European co-workers and support for SD.

[14] Low level of education is defined as having at most a high-school education. Note that the socioeconomic control variables (concerning employment and education) are based on data the year before the election to account for potential problems with bad controls.

[15] Ideally, one would like to cluster the standard errors at the workplace level. However, since all workplace-specific characteristics are aggregated at the precinct level, we have to rely on precinct-level clustered standard errors.

[16] For more information on the correlation between the control variables, including the estimated precinct fixed effects, see Table B3.

A second concern is that developments in local labor markets are related to both the political outcome of precincts located in the labor market as well as the labor market integration of migrants. Hence, we include year fixed effects interacted with Sweden's 60 local labor markets (commuting zones). The inclusion of these interactions captures labor-market-specific trends in terms of both workforce composition and support for SD. Both SD as a political force and immigrants in the workforce have evolved positively over the relevant period (see Figures 2 and B1).

There are several confounding factors that could influence changes in terms of both support for SD and the share of non-European co-workers. For example, due to networking and simple geographical proximity, the share of non-Europeans residing in a precinct is likely to be positively correlated with the share of non-European co-workers among the neighboring natives. That is why we control for the percentage of non-European immigrants *residing* in each precinct. Since the effect potentially differs between subgroups of migrants, we also control for the share of low-skill non-Europeans as well as the share of non-Europeans with citizenship (with voting rights). Most individuals do not work in the same precinct as they live, which enables us to separate neighborhood exposure from workplace exposure.

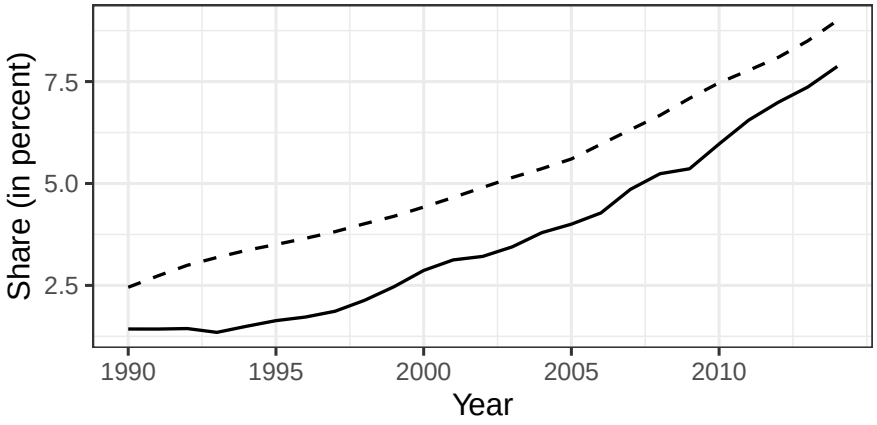
The preferred specification given by Equation (3), together with local labor market time trends, addresses all of the above concerns. To be clear, the source of the variation in the intensity of workplace contact is given by changes in the composition of employees in the large number of workplaces across Sweden. So, how exactly should one think about the precinct-level variation?

The share of the Swedish population born in a foreign country has increased steadily over the past couple of decades. This has naturally also increased the share of foreign-born workers. Figure 2 depicts the share of the Swedish population born in a non-European country between 1990 and 2014 and the yearly average of the same share among workplace co-workers. Both these shares have grown over the last two decades. However, the yearly change does not remain constant across all workplaces, which means that native-born workers will to a varying degree see their workplace contact with non-European co-workers change over time.[17] Most importantly, each native's share of non-European

[17]Figure B2 shows the between-elections precinct-level change in the share of non-European co-workers among native residents. Although most precincts experience a change between 0 and 3 percentage points, there is great variation

co-workers will vary in-between elections if their workplace share changes or if they start working in another workplace.

Figure 2: Share of non-European individuals



Notes: Dashed line shows non-European-born adults as share of total adult population, while the solid line shows averages of the share of non-Europeans in the workplace.

The precinct-level treatment intensity takes advantage of changes in each native's workplace contact, which rely on changes in workplace composition. These changes are not random: for numerous reasons, some firms, or even sectors, are more likely to employ non-natives. In addition, changes to treatment intensity also depend on where native workers reside. Nor is this decision made by the worker random, and it will affect the precinct-level variation in the treatment. Thus, what drives the variation in the treatment are non-random actions of both firms and native workers. Our identification strategy relies on the assumption that these actions, conditioning on our time-varying controls and year and precinct fixed effects, are orthogonal to the potential outcomes in the support for SD.

4 Results

4.1 Workplace contact and support for SD

Our baseline estimates are presented in Table 1. The coefficients represent the effect of a standard deviation increase in the average precinct-level share of non-European co-workers among working natives on the share of votes for SD. Column (1) includes the main treatment variable, time and

across precincts. Table B2 presents similar figures for the workplace-level change between elections.

precinct fixed effects as well as time-varying precinct-level controls to account for precinct-specific time trends in a set of key factors, most importantly the share of resident non-Europeans. In column (2), we add an interaction term between the time and labor market region dummies, which accounts for labor market-specific time trends, while column (3) adds a set of workplace-specific controls.

Table 1: Share of non-European co-workers and support for SD

	(1)	(2)	(3)	(4)
WP contact with non-Europeans	-0.811***	-0.297**	-0.428***	
	(0.0840)	(0.0934)	(0.0996)	
WP contact with same-skill non-Europeans				-0.377***
				(0.107)
WP contact with different-skill non-Europeans				0.0770
				(0.0496)
Observations	17,465	17,465	17,465	17,465
Model	FE	FE	FE	FE
Year FE	Yes	Yes	Yes	Yes
Precinct FE	Yes	Yes	Yes	Yes
Precinct Controls	Yes	Yes	Yes	Yes
Labor market time trends	No	Yes	Yes	Yes
Workplace controls	No	No	Yes	Yes

Notes: ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

The estimated effect of the share of non-European co-workers among natives on the share of votes for SD drops significantly as we control for local labor market trends (compare columns (1) and (2)). As workplace controls are added, the estimated slope coefficient instead turns more negative (column (3)). The third column represents our most conservative and most preferred specification. The estimated slope coefficient suggests that a one standard deviation increase in the share of co-workers (approximately a 3-percentage point increase) is associated with a decrease of 0.428 percentage points in votes for SD. This decrease represents around 8% of a standard deviation in the dependent variable, or around 12% of a standard deviation in the *change* in votes for SD. In our sample, a standard deviation in the precinct votes for SD is 5.8%, and a standard deviation in the change in votes for SD is equivalent to 3.4% points.[18]

[18] All models in Table 1 include precinct-level time-varying controls. Some of these could be partial functions of the treatment variable and thus *bad controls*. In Table C8, we show that the estimated negative effects do not hinge on including the precinct-level time-varying controls.

The negative effects in Table 1 (columns (1)–(3)) are in line with the contact hypothesis: meaningful contact with members of ethnic minority groups reduces opposition to immigration and, subsequently, voting for anti-immigration parties. At the very least, any increase in anti-immigration voting stemming from more contact with non-Europeans seems to be offset by the decrease resulting from meaningful contact. However, Prediction 1.1 suggests that the presence of *same-skill* foreign-born co-workers captures the threat of increased labor market competition between natives and foreign-born workers more than the overall presence. For example, *high-skill* natives would not expect to compete for employment and high wages with *low-skill* immigrants, since these two groups essentially compete in different labor markets. At the same time, we expect co-workers with the same skill level to have even more meaningful workplace contact, as they are most likely involved in similar tasks. To shed light on the importance of same-skill non-European co-workers in the context of workplace contact, we divide the pool of workers into two groups: one for high-skill workers and one for low-skill workers based on occupation categories.[19]

In the following case, an arbitrary native's share of non-European co-workers is computed by only using co-workers of the *same* skill level as that native. These individual-level shares are then aggregated to the precinct level. For completeness, we also create a measure for the share of non-European co-workers in the *different* skill cell. As we can see in the last column in Table 1, a negative coefficient is estimated only for same-skill workplace contact. Compared to the estimated slope coefficient of our preferred specification (column (3)), the estimate for same-skill contact is slightly less negative but qualitatively the same.[20] In terms of contact with different-skill co-workers, the estimated slope coefficient is not distinguishable from zero.

This illustrates that the negative effect of workplace contact on voting for SD is solely driven by same-skill contact, which does not fit well with the competition hypothesis. Instead, it offers support for the predictions in line with the contact hypothesis. Social networks tend to be stratified depending

[19]These skill levels are constructed using 1-digit occupation categories from the Swedish Standard Classification of Occupations (SSYK). These classifications are similar to the International Standard Classification of Occupations (ISCO). High-skill workers include, for example, legislators and senior officials, while armed forces are excluded from the classification. Table D1 describes the classification in detail.

[20]The estimates presented in column (4) plausibly suffer from attenuation bias caused by classic measurement errors. We discuss this in the online Appendix, Section B.2.

on social status and class, and a stylized fact in many workplaces is the lack of vertical integration. We thus expect colleagues to engage more frequently and in similar tasks at the same skill level, which explains the negative effects in the same-skill estimation. The lack of vertical integration could also explain the absence of any effect of different-skill contact on the share of votes for SD. Co-workers with different skills are less likely to engage repeatedly in cooperative interactions and do not tend to share the same or similar backgrounds and social status.

4.2 Robustness checks and placebo tests

We validate our main effects by conducting a number of robustness checks. The results from these robustness checks can be found in the online Appendix, Section C. First, we test the sensitivity of our analysis by excluding potential outliers. We identify outliers by considering observations with *i)* unusually large or small precinct populations (or population growth), *ii)* unusually large or small regression residuals, and *iii)* high leverage points. Our main findings are robust to all these alterations (see Tables C1-C4).

Second, in a series of placebo tests, we regress different, ex-ante unrelated outcomes on the precinct-level treatment variable. Most importantly, we regress lagged election outcomes for SD on future levels of workplace contact. This yields no statistically significant association. Nor does including the same lagged dependent variable on the right-hand side in the main specifications alter the results (Tables C5 and C6). In addition to the lagged dependent variable, we use vote shares for other non-mainstream parties, the share of blank ballots, and the share of newly married and newly divorced precinct residents. These placebo tests yield statistically insignificant estimates.

Third, we make alterations to the treatment variable. We replace the treatment with the average number of workplace co-workers among native residents, regardless of co-worker origin. We also assign a randomly drawn workplace share—from the actual workplace data—to each native worker 1,000 times and re-estimate the model using aggregated data based on each randomization. These tests yield statistically insignificant estimates. In addition, we show that the results are robust to using a treatment computed from non-European co-workers as deviations from industry-level averages

(Table C7 and Figure C2).

Fourth, we address selection into treatment. We use a large Swedish survey from 2009 to show that natives with preferences for restrictive immigration policies were not more likely to reside in or move to precincts experiencing a smaller future increase in the average share of non-European co-workers (Figure C3). We also show that our main results are robust to only focusing on natives who did not change precinct residence over two consecutive elections (Table C7). These results lend further support to our identification strategy, as it suggests that voters did not select into precincts based on their opposition to immigration.

Finally, the treatment variable in Table 1 only includes workplaces with at least one workplace co-worker and excludes all self-employed individuals. In Table C9, we drop these restrictions and show that the results are virtually the same. Having shown that the estimated negative effect of increased workplace diversity on support for anti-immigration parties is robust to various specifications and also that these results are likely not driven by selection or unobservable trends, we next explore whether the estimated effect can be attributed to workplace contact.

5 Mechanism

While our data contain many strong features, a clear drawback is that we do not know to what extent individuals actually engage with each other in the workplace. Our interpretation of the results presented in the previous section hinges on the assumption that cooperative and meaningful contact actually occurs between natives and their co-workers of non-European origin. The mechanism we propose is that of contact—either meaningful or superficial—between natives and immigrants and that this explains how the share of non-European co-workers affects support for SD. This section provides suggestive evidence in line with the proposed mechanism.

To better identify actual contact between work peers, we can stratify the pool of workers in at least two ways. First, we can divide the *workers* into categories we deem more likely to engage with each other, which we did when only considering same-skill co-workers (Table 1, column (4)). Second, we can perform the analysis for different sets of *workplaces*. One obvious division in terms of expected contact

Table 2: Small vs. large workplaces

	(1)	(2)
WP contact with non-Europeans, small WP	-0.546***	
	(0.0771)	
WP contact with non-Europeans, large WP	0.393***	
	(0.109)	
WP contact with same-skill non-Europeans, large WP		0.345**
		(0.108)
WP contact with different-skill non-Europeans, large WP		-0.0101
		(0.0355)
WP contact with same-skill non-Europeans, small WP		-0.476***
		(0.0776)
WP contact with different-skill non-Europeans, small WP		0.0582
		(0.0395)
Observations	17,465	17,465
Model	FE	FE
Year FE	Yes	Yes
Precinct FE	Yes	Yes
Controls	Yes	Yes
Labor market time trends	Yes	Yes

Notes: ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

between co-workers is the size of the workplace. A plausible scenario is that meaningful contact is more likely to occur at small workplaces, as the probability of repeated interactions with the same co-workers is lower in large settings with more co-workers. In large workplaces, contact between co-workers is more likely superficial or casual in nature. As a result, we categorize all workplaces as either *large* or *small* by using the number of employees.[21] Small (large) workplaces are defined as having less (more) than the median number of employees, which was 57 in 2006. The average share of non-European co-workers at the precinct level is then computed exactly as before, with the key difference that we construct separate precinct-level measures for the average native workers employed at small or large workplaces, respectively.

The results in the first column of Table 2 use information from both large and small workplaces. As

[21] Unfortunately, we do not have any information on the actual physical size of the workplaces, which could also affect the likelihood of co-workers interacting. However, the physical size of a workplace is likely positively correlated with the number of employees.

we can see, the negative effect is exclusively driven by contact at small workplaces, which corresponds well with our notion of contact being the key mechanism. On the other end, the effect for large workplaces is positive, which we return to below.[22]

Note that even in smaller workplaces, it is not certain to what degree individuals with different work-related tasks actually engage with each other. In what follows, we further decompose the subgroups and estimate the effect of four different levels of contact, namely same-skill or different-skill level at small or large workplaces. Since our expectation is that meaningful contact is the key mechanism, we would expect a larger coefficient (i.e., more negative) for same-skill level workplace contact with non-Europeans in small workplaces due to the higher likelihood of repeated interactions. At the same time, we expect that the fear of labor market competition can impede prejudice-reducing intergroup contact by, for instance, decreasing natives' will to collaborate. This offsetting mechanism is plausibly less likely for the context of same-skill workers at smaller workplaces, where the limited number of same-skill workers makes it more difficult to "opt out" of collaborations.

The estimates are presented in Table 2, column (2), and offer several take-aways. First, note that both slope coefficients based on different-skill variation, regardless of workplace size, render small and statistically insignificant point estimates. This is identical to the baseline results in Table 1, column (4). Second, as hypothesized, the point estimate using the share of same-skill level workplace contact with non-Europeans at small workplaces is negative and qualitatively similar to the point estimate for small workplaces in column (1).[23] This is again in line with the contact hypothesis: meaningful contact with same-skill non-European co-workers at small workplaces reduces support for anti-immigration parties.

Furthermore, the positive effect for large workplaces in column (1) is almost exclusively driven by same-skill contact. Any potential reduction in opposition to immigration from workplace contact with non-Europeans at these workplaces seems to be offset by mechanisms going in the opposite

[22] It is possible that smaller workplaces have other characteristics compared to larger workplaces that might be correlated with our variables of interest. Note, however, that we already control for the share of men, share of young individuals, and the average wage in the workplace, all of which likely take care of a large share of the different key characteristics across workplaces.

[23] Just as the case with the division of workers into low and high skill (Table 1, column (4)), the estimated effect of same-skill workplace contact most likely suffers from attenuation bias due to classic measurement errors.

Table 3: Meaningful intergroup contact and labor market threat

	Small workplaces	Large workplaces
Same skill	Meaningful contact, labor market threat	Superficial contact, labor market threat
Different skill	Limited contact, no labor market threat	Limited contact, no labor market threat

Notes: Types of contact and degrees of labor market threat for different combinations of workplace size and skill level.

direction; for example, fear of increased (same-skill) labor market competition. We believe that the most plausible explanation for the diverging results between small and large workplaces can be attributed to the actual size of these workplaces. The much larger pool of same-skill co-workers at large workplaces means that it is less likely that a native shares tasks and has repetitive and cooperative contact with any given same-skill non-European co-worker. The large number of workers also increases the possibility to select out of collaborations with certain segments of the workforce. These co-workers will, however, still be visible in the workplace and potentially be regarded as a threat to natives' high wages and employment opportunities. In the absence of meaningful contact, the presence of same-skill non-European co-workers will thus strengthen opposition to immigration in accordance with the ethnic competition hypothesis.

The conclusions from the point estimates in Table 2 are summarized in Table 3. The lack of vertical integration in many workplaces, both small and large, limits the amount of contact taking place between workers with different skill levels. At the same time, workers with a particular skill are not expected to compete with immigrant workers with a different skill. This explains the precisely estimated zero effects of workplace contact with different-skill non-Europeans at both small and large workplaces. On the contrary, the divergent results for same-skill contact can be explained by differences in the type of contact most likely to occur in small and large workplaces, respectively. Only meaningful contact will offset the positive effect of the presence of a labor market threat on opposition to immigration.

Extension: Is meaningful contact less common in vulnerable occupations?

We have so far assumed that the threat of competition is merely "highlighted" when natives are exposed to immigrants in the workplace. A natural question is whether *actual* labor market vulnerability

dampens the overall negative effect of meaningful workplace contact on opposition to immigration as specified in Prediction 3.[24] To examine this possibility, we consider the effect of workplace contact among natives employed in vulnerable occupations.

The labor economics literature notes that technological developments have led to an increasing job loss in occupations with a higher degree of routine-based tasks (Autor et al. 2003; Goos et al. 2014). The logic behind this development, somewhat simplified, is that jobs with a high degree of repetitive tasks are easily codifiable and computerized, implying that tasks previously performed by humans can now be performed by computers or robots. The opposite is true for occupations involving more abstract, problem-solving tasks.

To measure workplace vulnerability, we use 2-digit International Standard Classification of Occupations (ISCO) codes, effectively categorizing individuals into 27 different occupations.[25] We then separate the precinct-level averages of the share of non-European co-workers into *vulnerable* and *non-vulnerable* occupations using the Routine Task Intensity (RTI) index in Goos et al. (2014), The RTI index classifies the 27 two-digit ISCO occupation codes with a mean of 0 and a standard deviation of 1, which gives a scale from office clerks at 2.26 to the, supposedly, safe role of manager at -1.52. To deconstruct even further for our purposes, we consider the top quartile of the index to be vulnerable occupations and the bottom quartile of the index to be non-vulnerable occupations. In the interest of not losing too much information when computing the precinct-level average shares, we use our baseline measure as treatment (i.e., the overall share of non-European co-workers for natives).

The effect of non-European co-workers is estimated separately for the two categories of vulnerability, and the results are presented in Table 4. As shown in column (1), there is a small positive effect of workplace contact on support for SD for natives in the most vulnerable occupations, statistically significant at the 5% level. At the other end, the effect of workplace contact remains negative and statistically significant for natives employed in non-vulnerable occupations (column (2)). The estimate is smaller than the baseline case and can most likely be attributed to attenuation bias due to

[24] The perception of a threat to, for instance, natives' economic status can lead to opposition to immigration. Haaland and Roth (2020) show that information on whether immigration actually affects natives' wages influences their opposition to immigration.

[25] ISCO codes are used to categorizes occupations into groups defined by the tasks and duties typically undertaken by workers. These classifications are under the responsibility of the International Labor Organization.

Table 4: Vulnerable vs. non-vulnerable occupations

	Vulnerable (1)	Non-vulnerable (2)
% Non-European co-workers vulnerable occupations	0.0911*	
	(0.0447)	
% Non-European co-workers non-vulnerable occupations		-0.155*
		(0.0678)
Observations	17,461	17,465
Model	FE	FE
Year FE	Yes	Yes
Precinct FE	Yes	Yes
Controls	Yes	Yes
Labor market time trends	Yes	Yes

Notes: ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

the restricted number of natives included when computing the precinct-level averages.

An important limitation is that the pool of native workers used for computing the share of non-European co-workers in vulnerable occupations consists of natives who, despite the relatively higher risk of job loss, were still employed. The characteristics among those who did not lose their jobs might differ from those who were laid off. Thus, we have a potential selection problem that would bias our estimates. The direction of the bias is not obvious, as we do not know how those who stayed differ from those who left. Hence, we should interpret these results with caution.

With this caveat in mind, how should we approach the results in Table 4? One possible story is that contact between colleagues is more prevalent among non-vulnerable occupations. While this would fit very well with our suggested mechanism, we remain skeptical of this interpretation. It is not certain that physical professionals or drivers and mobile plant operators (low vulnerability) have more day-to-day contact with colleagues than office clerks (high vulnerability). Instead, our preferred interpretation is in line with Allport's condition regarding "basic security" (Prediction 3): an increased presence of immigrants is less likely to result in meaningful contact if natives feel that they are at risk of losing their jobs.[26] In other words, natives might care more about labor market competition from immigrants if they indeed face a high risk of losing their jobs. In such a setting,

[26]Note that the fact that higher RTI-scored occupations in general are threatened to a higher degree than low RTI-scored occupations can be validated using data on layoff notices. In Figure D1, we show how the probability of receiving a layoff notice increases with the RTI score (2006–2013).

workplace contact could instead increase opposition to immigration, which is in line with the ethnic competition hypothesis.

Note that this does not require natives *blaming* immigrants for their vulnerable labor market position. Instead, a higher share of immigrant co-workers can increase opposition to immigration among natives in vulnerable occupations if they expect to have to compete with immigrants for the remainder of the available jobs in their sector or occupation. The mere presence of immigrants in the workplace could highlight the threat of increased competition for employment.

6 Conclusion

In this paper, we use Swedish full population data to present evidence of an overall increased share of non-European co-workers in natives' workplaces having a negative impact on support for anti-immigration party the Sweden Democrats. We examine the consequences of intergroup contact in different workplace environments and arrive at the following conclusions. First, we interpret the negative effect of an overall increased share of non-European co-workers as support for the contact hypothesis, namely that the increased presence of immigrants (in this case at work) makes natives less inclined to vote for an anti-immigration party. To support this interpretation, we stratify results along skill levels. Workplaces often have low vertical integration, and social interactions—both positive and negative—primarily occur within the same skill level. We thus only expect the presence of non-Europeans within the same skill cell to influence opposition to immigration. Our results show that negative effects on votes for SD arise solely from an increase of non-European co-workers with the same skill level.

Second, when categorizing workplaces as either small or large based on the number of employees, we find that only same-skill workplace contact with non-Europeans at small workplaces has a negative impact on SD votes. While native workers with a particular skill are expected to lose from the immigration of people with the same skill, we do not find support for same-skill contact causing a rise in support for SD in small workplaces. On the other hand, same-skill contact in large workplaces leads to greater support for SD. These results suggest that in settings facilitating intimate and repetitive

contact (small workplaces), the potential increase in opposition to immigration from exposure to immigrant co-workers is, on average, offset by meaningful contact. Conversely, when contact is less repetitive, same-skill immigrant co-workers are likely to exhibit increased opposition to immigration.

Third, we present suggestive evidence of different estimated effects of workplace contact for vulnerable and non-vulnerable occupations, where a negative impact on support for SD only appears for non-vulnerable occupations. The opposite is true for vulnerable occupations. A possible interpretation of these results is that labor market competition from immigrants becomes more pressing for natives when there is a greater risk of job loss, thus offsetting the potential benefits of meaningful contact. However, one should be careful when interpreting these estimates as they potentially suffer from selection bias.

The results add new evidence to the expanding literature on the rise of anti-immigration and radical right parties. The main part of this literature, which considers neighborhood contact, finds a positive association between the share of immigrants and voting for anti-immigration parties. Our focus on the workplace suggests that the opposite relationship exists. Our empirical design, which relies on precinct-level election results for three consecutive Swedish national elections as well as a measure of the average share of non-European workplace co-workers among native residents, allows us to study the effect of workplace contact on support for SD. There are limitations to this approach. First, the precinct-level average shares do not allow us to capture any non-linear effects of workplace contact on SD votes. The consequences of additional workplace exposure to immigrants might depend on the initial level, and the intensity of contact is not necessarily linear in the share of immigrant co-workers. Second, the aggregate-level data give us limited possibilities to study the channels through which workplace contact translates into party preferences. These issues could instead be addressed using detailed survey data. Students of intergroup contact should direct more attention to designing surveys enabling us to learn more about channels and potential non-linearities.

The choice to situate our empirical analysis in the Swedish context involves both advantages and disadvantages. Besides the advantage of having access to high-quality administrative and election data, Sweden constitutes a *least-likely case* with its high share of employees working from home and

low overall level of prejudice. While the former suggests a stronger negative association between the share of immigrant co-workers and opposition to immigration in countries with a higher degree of on-site working, the latter condition could potentially limit generalizability: individuals in countries with a higher overall level of prejudice are potentially more likely to be predisposed to negative or superficial contact with immigrant co-workers (see Laurence et al. 2018). In these contexts, workplace contact could have different consequences in terms of support for anti-immigration parties.

It is also important to note that another distinct aspect of the Swedish context is the low level of sociability between neighbors. As noted by Daun (1991), Swedes are in general reluctant to engage with their neighbors, and contact between neighbors is not a socially sanctioned norm (Goldschmidt et al. 2017). This has potential consequences for the likelihood of friendship between co-workers. In countries with a stronger norm of sociability between neighbors, workplace-based friendships are perhaps less likely to arise. Given these limitations, we propose that future research takes into account both the initial level of prejudice and the likelihood of contact in other contexts.

Another important avenue for future research relates to the increasing tendency to work from home. In the wake of the COVID-19 pandemic, interactions between co-workers have changed at many workplaces: while virtual meetings allow workers to connect with each other from home, physical meetings and interactions at the water cooler arguably entail more intimate contact. Is the decline of on-site working related to the rise of the radical right?

The results in this paper on job security and workplace contact should be interpreted with caution as the data employed are likely less suited for this type of analysis. Another way of examining the relevance of job security—both actual and perceived—is by using detailed survey data on respondents' share of immigrant co-workers, as well as their perceived unemployment risk and stated policy preferences. By matching the survey respondents to administrative data on socio-economic characteristics and workplace information, one could analyze the impact of the actual presence of immigrant co-workers for different levels of job security. This could greatly enhance our understanding of how economic distress influences the relationship between workplace contact and opposition to immigration.

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Online Appendix for "Workplace Contact and Support for Anti-Immigration Parties"

A Replication and data availability

In this paper, we apply data from Swedish administrative registers. There are several rules and regulations on how to process and store such data, which is why we run our empirical analyses through a secured remote desktop system where the data are stored in a server. As a consequence, we cannot make these data available online.

Should a reader wish to gain access to these data in order to replicate our analysis, there are two ways to do so. All the data we have used are available at Statistics Sweden (SCB) and can be requested from them (please follow this link: https://www.scb.se/vara-tjanster/bestalla-mikrodata/). Before initiating such a process of requesting data, however, one must seek approval from the Ethical Review Board. Other researchers will then be able to request these data directly from SCB.

Another possibility would be for a person to temporarily become part of our research team. He or she would then be able to replicate our analysis by using the same remote desktop system we used in our work. The feasibility of this option depends on where the researcher in question is based, since there are geographical restrictions related to data access. If the reader is interested in this option, he or she should contact us beforehand so that we may add him or her to our research group on a temporary basis, after which we inform the Swedish Ethical Review Board to this effect.

B Descriptives

Table B1 presents descriptive statistics of the data used in the precinct-level analysis, and a few things are worth mentioning. First, the population is fairly concentrated around the average of 1,200 (adult) inhabitants (including residents both with and without voting rights). Second, on average almost 30% of the precinct population does not work. This number is important to keep in mind since the variation in our main treatment will only come from individuals linked to a workplace,

while the outcome—voting—is an aggregate outcome of all precinct voters. Third, around 6% of the precinct population consists of non-European immigrants, but the variation here is noticeable. In general, a clear majority of the non-Europeans are citizens (% citizens of non-Europeans) and have some education longer than nine years (% low educ. non-Europeans in precinct). Fourth, while our mapping procedure creates more comparable units over time, a few peculiarities in the maximum and minimum values follow. For example, the least populated precinct has 3.46 inhabitants, and the precinct with residents who have the most male co-workers has more than 200% male co-workers. These are outliers and an issue in very few observations. In the robustness section, we use several different methods to show that outliers do not seem to be a key driver for the results.

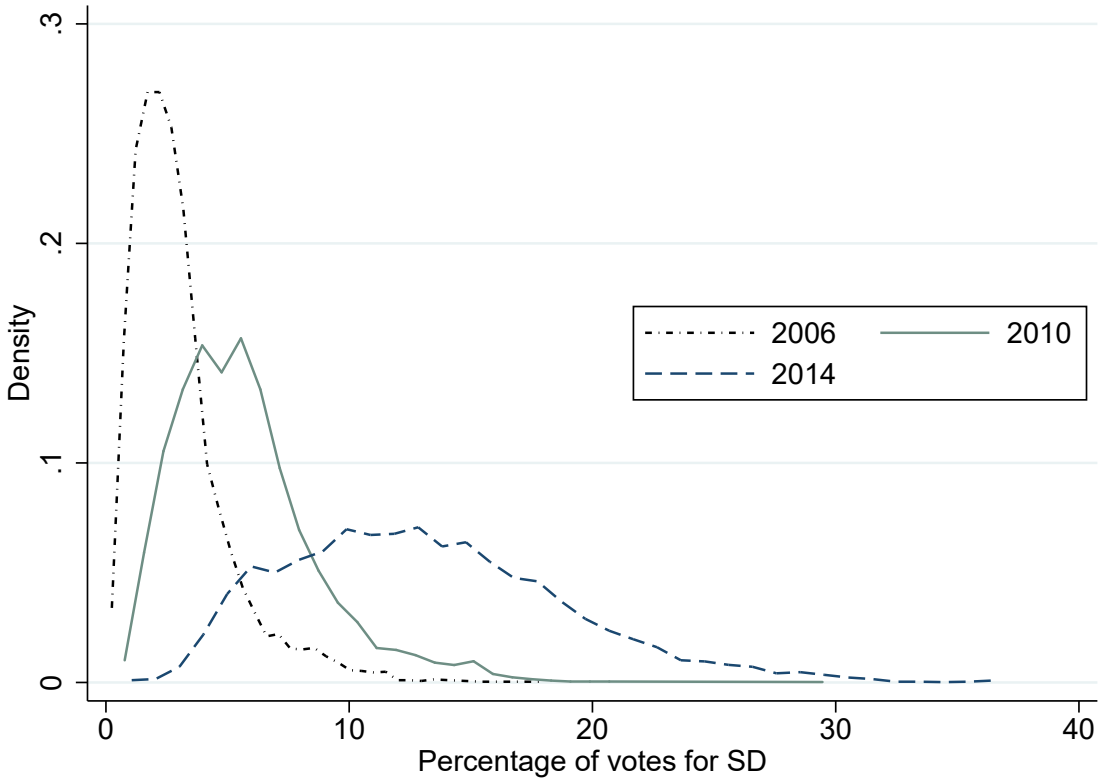
The precinct-level mean support for SD during our entire period (2006–2014) was 7.3%. The higher figures in the distribution are skewed toward the more recent elections since the party has increased its share of votes by an average of 5 percentage points per election (see Table B1). Figure B1 shows the distribution of votes (in percent) for SD in all precincts for 2006, 2010, and 2014. In 2014, the average precinct registered around 13% of the votes going to SD. In 2006, very few precincts (6 out of around 5,500 precincts) had a level of support higher or equal to 13%. The increase has thus been substantial and nationwide.

In our sample, the average of $Mean\_Im\_Share_{pt}$ is 4%. This share ranges from practically no non-European co-workers up to almost 30%, and the mean is equivalent to around 23 non-European co-workers in a workplace (see Table B1).[27] In Figure B2, we depict the *change* in the precinct-level mean share of non-European immigrant co-workers. Between each election in 2006, 2010, and 2014, this share increased in most precincts. Only 5% of the precincts experienced a decrease in the share of non-European co-workers, either between 2006 and 2010 or 2010 and 2014. This is hardly surprising given the general increase of foreign-born individuals as a share of the Swedish population during the same time period (see Figure 2 in the main paper).

In Table B2, we also present the mean change in percentage points between elections years 2006 and 2010, and 2010 and 2014, respectively. The mean change is around 0.7 percentage points for both

[27] For more detailed information on, for example, the skill level and birth region of immigrant co-workers, see Table B4.

Figure B1: Distribution over precincts, percentage voting for SD, 2006–2014

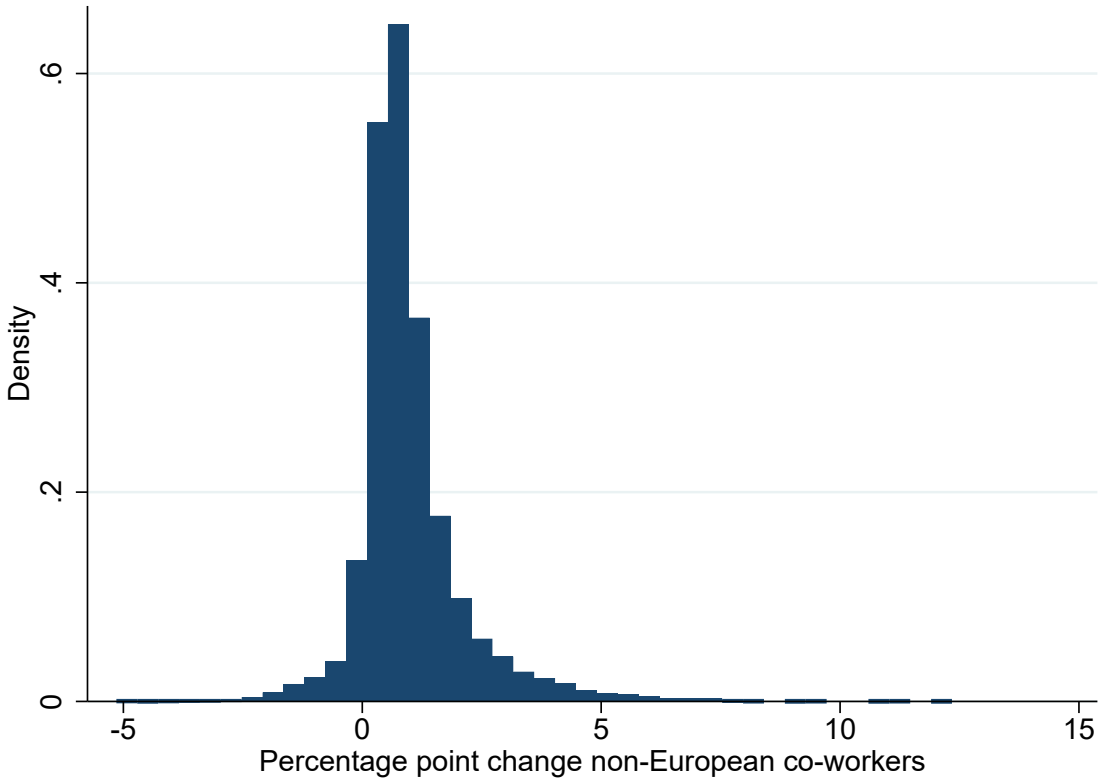


Notes: Distribution of votes in percentage for SD in 2006, 2010, and 2014.

time periods and the standard deviations are close to 7.5. In other words, there is great variation in terms of changes in this share between two elections.

Finally, Table B3 shows a correlation matrix between precinct, time-varying variables, and estimated fixed effects from the baseline regression (Table 1, column (3)). Based on the table, we note at least two things: 1) that correlations between the estimated fixed effects and the time-varying precinct controls are not particularly high. With the exception of the share of people with a low level of education (p=0.44), no correlation coefficient (in absolute value) is larger than 0.25. 2) The signs of the coefficients are generally as expected. For example, precincts with larger estimated fixed effects (a high tendency to vote for SD) have precinct residents with lower wages, lower education, and more days of being unemployed. These precincts also contain fewer non-Europeans and more non-working natives.

Figure B2: Percentage point change in precinct-level share of non-European co-workers



Notes: Histogram of precinct-level change in the mean share of non-European co-workers between the 2006 and 2010 elections and between the 2010 and 2014 elections.

Table B1: Summary statistics full sample, 2006–2014

	mean	sd	min	max	count
Outcome					
% votes for SD	7.26	5.76	0.00	36.89	17,508
Votes for SD, Δ %-units	5.05	3.58	-1.37	24.10	11,670
Treatment					
% non-European co-workers among native precinct residents	4.36	3.06	0.02	30.00	17,508
Controls					
Population	1,205	323	3.46	2,489	17,508
% low education	13.12	6.74	0.44	39.18	17,508
% non-working natives	27.63	8.53	2.28	66.88	17,508
Log(Wage)	7.34	0.37	2.09	8.52	17,508
# of unemployment benefit days	10.55	6.00	0.03	71.37	17,508
% non-Europeans in precinct	6.28	9.36	0.00	77.26	17,508
% citizens of non-Europeans	78.45	14.08	0.00	100	17,465
% low educ. non-Europeans in precinct	15.46	11.11	0.00	100	17,465
Wage of co-workers among precinct residents	2,947	670	13.24	9,572	17,508
% males of co-workers among precinct residents	49.78	11.16	0.19	221	17,508
% young of co-workers among precinct residents	19.91	4.70	0.08	66.65	17,508

Notes: Descriptive statistics. Figures aggregated at election precinct and election year level. Treatment represents the percentage of non-European-born co-workers among native workers residing in a given precinct and election year. Wages represent yearly gross income in hundreds of SEK. The education variable is taken from the Swedish education registers, which is in this case divided into seven steps, with 5–7 representing any education above 12 years (high school). We label this as high education. Low education includes those with only 9 years or lower. Mean number of days as unemployed is calculated using the number of days a given individual is registered at the Swedish Public Employment Service as unemployed (job searching). Young refers to anyone under the age of 30.

Table B2: Change in share of non-European co-workers

	mean	sd	min	max	count
Change in share, 2006–2010	0.60	7.69	-100	100	141,131
Change in share, 2010–2014	0.77	8.64	-100	100	149,539

Notes: Change in share of non-European co-workers between 2006 and 2010, and 2010 and 2014. Includes workplaces with at least 2 workers.

Table B3: Correlation matrix

	Fixed effect	Population	Pop^2	Log(wage)	% low education	# of unemployment benefit days	% citizens of non-European origin	% non-working natives	% non-Europeans in precinct	% low educ. non-Europeans in precinct
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Fixed effect	1.00									
Population	0.02	1.00								
Pop^2	0.04	0.98	1.00							
Log(wage)	-0.16	0.21	0.17	1.00						
% low education	0.44	0.02	0.05	-0.55	1.00					
# of unemployment benefit days	0.16	0.18	0.18	-0.32	0.46	1.00				
% citizens of non-European origin	-0.11	-0.01	-0.03	0.14	-0.18	-0.13	1.00			
% non-working natives	0.22	0.14	0.14	-0.52	0.65	0.42	-0.15	1.00		
% non-Europeans in precinct	-0.20	0.09	0.07	-0.32	0.14	0.55	-0.01	0.30	1.00	
% low educ. non-Europeans in precinct	0.21	0.08	0.09	-0.27	0.41	0.32	-0.36	0.35	0.22	1.00

Notes: Correlation matrix between precinct level controls varying over time and the estimated precinct fixed effect ($\hat{\Phi}$).

Who are the co-workers?

As an additional description, we provide more detailed characteristics of non-European co-workers in Table B4. This table shows the average percentage of co-workers with certain characteristics of all residents in a given precinct.

Table B4: Non-European co-workers and their characteristics

	mean	sd	min	max	count
% non-Eur. co-workers on natives' workplaces	4.17	2.90	0.02	27.55	17,508
Separated by years in country					
≤5 years in country	0.51	0.39	0.00	6.28	17,508
≤10 years, ≥5 years in country	0.55	0.42	0.00	4.94	17,508
≤15 years, ≥10 years in country	0.56	0.44	0.00	5.51	17,508
≥15 years in country	2.55	1.79	0.01	15.40	17,508
Separated by skill level					
% high-skilled	0.34	0.31	0.00	5.73	17,508
% low-skilled	3.54	2.36	0.02	22.96	17,508
Separated by origin					
% Latin America	0.74	0.53	0.00	3.97	17,508
% MENA	1.76	1.53	0.00	16.88	17,508
% Other Asia	1.09	0.54	0.01	6.53	17,508
% Other Africa	0.53	0.53	0.00	6.74	17,508
% Oceania and Stateless	0.04	0.04	0.00	1.00	17,508

Notes: Figures aggregated at election precinct and election year level.

As we can see, an overwhelming majority are low-skill migrants. On average, the working native population in a precinct has 3.5% low-skill non-European co-workers, compared to the corresponding number for the share of non-European co-workers with any skill level at 4.2%. Furthermore, separating by years since receiving a residence permit, the most common group consists of migrants with an extended period in the country. The figures are suggestive of a long labor market integration period for many migrants, especially those from outside Europe.

Finally, we also separate by five regions of origin: Latin America, Middle East and North Africa (MENA), Other Asia, Other Africa and for completeness, we group migrants from Oceania with stateless individuals. This separation shows that all groups are present except for the last category. Many originate from the MENA countries or other Asian countries, which is expected given that some of the largest immigrant groups in Sweden (e.g., Iraq, Iran, Afghanistan, Syria) belong to this category.

B.1 Matching procedure

The number of precincts in 2006 and 2010 was 5,783 and 5,668, respectively, while this number had increased to 5,837 in the 2014 election. We create a time-invariant unit by first matching the 2006 precincts and the 2014 precincts with detailed population data coming in the form of 100×100 meter squares. The population of each overlapping part of a precinct in 2006 with precincts from 2014 is divided by that precinct's total population to create *population weights*. The number of votes in 2006 for each party and the total number of eligible voters are then multiplied by the population weights before being aggregated at the 2014 precinct level. Thus, the total number of votes for each party in 2006 is separated into overlapping parts with the 2014 precincts, and the number of votes distributed into each part depends on the population weights. We use the same method to match the 2010 and the 2014 election precincts. A similar method is used in Dehdari (forthcoming).

B.2 Measurement considerations

Using the share of non-European co-workers to proxy for intergroup workplace contact hinges on two important assumptions. First, we assume that workplace contact is a monotonically (weakly) increasing function of the share of non-European co-workers. For any initial level of contact, an additional non-European co-worker at any given workplace will weakly increase the workplace contact of all natives at said workplace with non-Europeans. Second, this increase is constant and does not depend on the initial level of contact. This means that the increase in workplace contact experienced by natives when going from 10% to 11% non-European co-workers is the same as going from, say, 50% to 51%.

Both of these assumptions are arguably strong and one can imagine scenarios where one of these—or both—are violated. The relationship between the share of non-European co-workers and workplace contact might indeed be represented by a non-linear function and not monotonically increase in all parts of its support.[28] In addition, an increase in the share of non-European co-workers could lead to either meaningful contact, superficial contact, or both, depending on the workplace environment.

[28] Wagner et al. (2006) analyze survey data on contact and the share of foreigners in the workplace and find a strong positive correlation.

An increase in this share could lead to more superficial contact for some initial levels of workplace contact, while it leads to meaningful contact for other levels. After all, we estimate a reduced-form relationship between the share of non-European co-workers and opposition to immigration, where our estimates should be interpreted as average treatment effects. This is a clear drawback of the methods employed in this study.

Another caveat relates to measurement errors. As mentioned in the main paper, our outcome of interest is precinct-level support for SD, which, in turn, is the aggregated result of a large number of individual actions by, mainly, native voters. We hypothesize that workplace contact between natives and immigrants, meaningful or superficial, can impact individual voting decisions and thereby the aggregate precinct-level election results for SD. Ideally, we would like to obtain actual information on the degree of workplace contact of each native with his or her immigrant co-workers, which we would then aggregate to the precinct level. In absence of this measure, we approximate the degree of contact by aggregating the share of non-European co-workers in the workplace for each native-born individual. However, as the pool of native individuals linked to a specific workplace is smaller than the total number of native voters, we do not accurately capture the precinct-level average workplace contact, and we induce further noise as we also exclude self-employed individuals and natives with no workplace co-workers. These natives still potentially interact with immigrants in their respective workplaces, which possibly affects their voting decision. For instance, self-employed accountants or lawyers might have meaningful interpersonal contact with clients of immigrant backgrounds. Our estimates thus potentially suffer from attenuation bias due to these measurement errors.

In addition, in some specifications, we restrict the measure of contact to only include same-skill co-workers. In these specifications, the number of native workers used when approximating the precinct-level average workplace contact is further reduced, as natives with no co-workers of the same skill are removed. This is illustrated by Figure 1 in the main paper. In Figure 1a, native worker i (represented by the black figure) has a total of thirteen co-workers, eight of whom are natives (white figures) and five of whom are non-European (light gray figures). Thus, native worker i's share of non-European co-workers is 5/13, or approximately 0.38. If we instead compute the share of same-skill

non-European co-workers, this share is 2/7, or close to 0.29. In the second part, Figure 1b, the high-skill native worker (black figure) has a total of seven co-workers, four of whom are of non-European background, but no same-skill co-workers. This worker is included when computing the precinct-level average for the share of non-European co-workers and excluded when we only consider the share of same-skill co-workers. Consequently, the estimates using the same-skill shares are even more likely to suffer from attenuation bias.

As a more concrete illustration, we may consider the actual figures: in 2010, there were 6.1 million native adults eligible to vote in national elections. Of these, close to 30% (1.8 million) were registered as non-working, most of them either unemployed, students, or retired. Slightly fewer than 8% (around 470,000 individuals) were running businesses, which leaves around 3.8 million, or 63% of the eligible voting population, as working. Of these, we use 3.1 million natives who were linked to a unique workplace and had at least one co-worker.

We analyze this potential attenuation bias due to measurement errors by restricting the size of the pool of workers used to compute the precinct-level measures of workplace contact. We show that the estimate in Table 1, column (3), drops in magnitude if we insert a measurement error by randomly replacing values in $Mean\_Im\_Share_{pt}$ with missing information. We redo this random procedure 1,000 times and show the mean of the coefficients with inserted missing information. This estimate is found in Table B5.

Table B5: Analyzing the role of measurement errors

	Baseline (1)	With Measurement-error (2)
WP contact with non-Europeans	-0.428***	-0.382***
	[-0.623, -0.233]	[-0.569, -0.195]
Observations	17,465	17,465
Model	FE	FE
Year FE	Yes	Yes
Controls	Yes	Yes
Precinct FE	Yes	Yes
Labor market time trends	Yes	Yes

Notes: Column (1) replicates the baseline results in Table 1, column (3). In the second column, we insert a "classic" measurement error. We do this by constructing 1,000 new treatment variables, all of which are based on the same information as our main treatment ($Mean\_Im\_Share_{pt}$). To insert a measurement error, we then randomly replace the values in our 1,000 new replicas of $Mean\_Im\_Share_{pt}$ with missing information. The number/degree of missing values is the same across the 1,000 new variables and corresponds to the degree of missing values when only analyzing same-skill contact (see Table 1, column (4)). We re-run the baseline regression (Table 1, column (3)) for all 1,000 randomly created treatment variables. The value of the coefficient in column (2) in this table indicates the arithmetic mean of all those coefficients. ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level). Mean 95% confidence intervals in brackets.

C Robustness checks

First, we use a number of different methods to analyze whether the sizes of our main coefficients are sensitive to the exclusion of potential outliers. In Table C1 and C2, we re-estimate the preferred specification presented in Table 1 (column 3) as well as the skill-sorted results in column (4), using samples where we drop precincts with the largest, smallest, most increasing, or decreasing populations. Moreover, in Table C3, we exclude observations with unusually large or small residuals (with an absolute value larger or smaller than 2 or 1.5 standard deviations). Finally, in Table C4, we drop high leverage observations. We believe that the most appropriate approach is to not only drop high leverage observations, but also high leverage *precincts*. We thus drop all precincts with *at least one high leverage point*. In column (1) and (2) in Table C4, we follow the convention in the literature and define a high leverage observation as one with a leverage point ("hat value" (h_i)) equal to or larger than 2 times the mean of h.[29] To be even more sure that high leverage points are not an issue, in columns (3) and (4) we also try dropping all precincts with at least one observation within the top 2.5 percentile of leverage points. Studying the coefficients in Tables C1 to C4, our main findings seem to be robust to all these alterations of our data. Hence, we deem it unlikely that outliers are the key

[29] "Hat values" (h_i) are a measure of leverage in a regression setting implying that a regressor is much higher or lower than the mean. They are measured by estimating a regression and then using the projection matrix: $\mathbf{X}(\mathbf{X^T X})^{-1}\mathbf{X^T}$.

drivers of what we observe.

Second, we add a number of placebo estimations by altering the dependent variable. These are found in Table C5 and Figure C1. In the latter, we plot coefficients from 1,000 regressions. The right-hand side of the specifications used in the regressions are always the same and equivalent to the baseline case (Table 1, column (3)). However, the dependent variable is created in three steps. We first sort the sample by municipality and year. We continue by randomly shuffling the precinct order within each municipality. Finally, we re-assign votes for the Sweden Democrats based on the new random order of precincts within municipalities and year. This procedure is repeated 1,000 times to generate 1,000 randomly created dependent variables. The percentage of votes for the Sweden Democrats thus always represents an actual percentage number in another precinct within the same municipality and election year. Figure C1 plots the 1,000 coefficients and 95 % confidence intervals. The mean over all coefficients is -0.077, and the distribution is fairly well-centered around 0. In other words, the effect is small and insignificant.

In Table C5, we alter the dependent variable to seven different outcomes that should all be unrelated to the treatment of interest. In the first (column (1)), we use a lagged dependent variable as the new left-hand side variable. The precinct percentage of votes for SD is thus matched to the future precinct-level average share of non-European co-workers. This test is crucial as the existence of a relationship between the lagged dependent variable and future levels of the treatment intensity would suggest that there are underlying trends in both support for SD and workplace contact not absorbed by our large number of controls and fixed effects. Thus, this test is similar to a parallel trends test for a difference-in-differences design.[30] We continue in columns (2) and (3) by considering the percentage of votes for two other non-mainstream parties. In column (2), we use the percentage of votes for the Pirate Party, a small party with an agenda largely focused on individual privacy

[30] An alternative test to using a lagged dependent variable as an outcome is to add the lagged dependent variable as a control variable on the right-hand side and check whether this significantly changes the coefficient of interest. However, if the added control variable is a poor measure of the underlying confounder, a more powerful test is to put the control variable on the left-hand side (see Pei et al. 2019). This might be relevant for our case, as the lagged dependent variable is not necessarily a good measure of the underlying trend in SD support and workplace composition. For completeness, we estimate the main regression model by adding the lagged variable, and we compare the coefficients to a model where we only include the 2010 and 2014 data. The results are presented in Table C6 and show no statistically significant difference in the coefficient of interest.

and copyright laws. In column (3), we use votes for the Feminist Initiative, also a smaller party, with a focus on feminist issues. Moreover, columns (4–6) consider more demographic features: the percentage of i) newly married, ii) newly divorced, and iii) men. We do not find any significant effects from our treatment in any of the cases in columns (1) to (6).[31]

The only placebo outcome in Table C5 for which there is any detectable effect is in column (7), where we measure the effect on the percentage of precincts votes cast in the form of blank ballots. Given the number of robustness checks and placebos we estimate, it is not unreasonable to expect that at least one of the placebos may come out statistically significant, if nothing else than due to random variation in the data. Nevertheless, to be sure that the effect in column (7) is not problematic, we re-estimate the regression but eliminate the top 1 percent of observations in terms of blank ballots. These are precincts where more than 3 percent of the voters cast a blank ballot and precincts we would consider special cases or outliers. As we can see in column (8), simply dropping these fairly extreme observations renders a statistically insignificant and small coefficient.

Third, rather than changing the outcome, we further test the robustness of our results by re-specifying the *treatment* in a number of ways. In column (1) in Table C7, we re-estimate our preferred specification, but with the number of *any* co-workers as the explanatory variable. These results are not statistically different from zero. We may also worry that workplaces belonging to certain industry sectors, which are prone (or less prone) to hire both immigrants *and* natives with more liberal (or less liberal) attitudes, concentrate locally. Accordingly, we create a new treatment variable, in which we subtract each native's share of non-European co-workers from the share of non-European workers employed in that native's industry.[32] These shares are then aggregated to the precinct level and used as our treatment variable. Studying the results in column (2) in Table C7, we can observe that the coefficient is even more negative than the baseline case. We thus conclude that the results are robust to industry-specific trends.

In Figure C2, we replace the actual individual-level workplace shares with randomly assigned

[31] The significance levels reported for the placebo test with the lagged dependent variable are based on the null hypothesis that the true parameter is equal to zero. Another way to formulate the null hypothesis is that the true parameter is equal to the main effect presented in Table 1, column (3). In such a hypothesis test, we reject the null hypotheses.

[32] We use two-digit industry sector codes that divide all Swedish firms into close to 70 sectors.

shares. This test is used to address concerns that our treatment captures individual-level unobservable workplace characteristics rather than the employee composition at each native's workplace. We randomly draw, without replacement, workplace shares from the actual vector of individual-level workplace shares and assign these to new individuals. Thus, each native will receive the workplace share of another native. This is performed 1,000 times and the resulting vectors of randomly assigned workplace shares are used to construct precinct-level measures of workplace contact. These are then used to re-estimate the coefficients of our baseline specification in column (3) in Table 1. Figure C2 presents 1,000 estimated slope coefficients from each random assignment of workplace shares and their corresponding 95% confidence intervals. None of these estimates, nor their confidence intervals, are close to our baseline estimate, using actual individual-level shares, presented in column (3) in Table 1. Instead, a vast majority of the 1,000 estimates are centered around zero. This suggests that the treatment does not simply pick up characteristics that are specific to each native's workplace.

Fourth, we examine potential selection into treatment. Given the variation in our treatment, this selection could be either due to potential SD voters moving between precincts or staying in the precinct but moving between workplaces in-between elections. If an individual with preferences for restrictive immigration policies chooses workplaces based on the workplace composition of foreign-born individuals, our results may be biased. Clearly, we do not have access to a natural experiment placing immigrant workers as good as randomly in workplaces. However, we can provide suggestive evidence against the selection story by only focusing on natives who stay in a precinct in-between elections. By showing that our results are robust to focusing on *stayers*, we at least eliminate the possibility of the results solely being driven by individuals moving from one precinct to another in-between elections. The results for stayers are found in column (3) in Table C7 and suggest that the effect remains negative and statistically significant. Given our large number of precinct-level controls and the fact that the results are robust to focusing on stayers, we deem it unlikely that this type of selection is the key driver of the results.

That said, staying is also a choice, and we have not considered selection into workplaces. A somewhat separate way of arguing against this selection story is to consider preferences for immigration.

While the administrative data do not include party preferences, we do have access to a fairly sizable survey conducted in 2009. This survey contains answers from around 11,000 natives in Sweden about their health, personality, and attitudes.[33] Questions spanned from current medical treatments to moral statements, but some questions on political preferences were also included. In one of the survey questions, respondents were asked to read a political statement that is part of the Swedish political discourse. Examples include "reduce income inequalities," "leave the European Union," or "admit fewer refugees into Sweden," which is a salient issue for radical right parties. The respondents were then asked to assess their position from 1 (very poor suggestion) to 5 (excellent suggestion) on a Likert scale.[34] The survey was conducted before the 2010 election, which means that attitudes were not influenced by anything taking place between 2010 and 2014.

We use the answer to the question on refugees as an indication of preference for restrictive immigration policies and relate this answer to changes in workplace context between the two subsequent elections, 2010 and 2014. Since we can observe the election precinct of each respondent, we match the survey answers of each individual to the share of non-European immigrant co-workers among precinct natives (in other words, we match it onto our treatment variable). Our goal is to make sure that respondents with preferences for more restrictive immigration policies in 2009 did not move to or stay in precincts with, on average, a smaller share of non-European immigrant co-workers. Most likely, should the selection story pose a problem for our identification, we would expect individuals with a more restrictive view on immigration to have a more negative change in precinct immigrant co-workers, compared to individuals with a more liberal view on immigration policy.

As we can see in Figure C3, we do not find support for the selection story. Individuals strongly agreeing with the suggestion of accepting fewer refugees (i.e., reporting a 5) experience an increase in the share of precinct-level non-European co-workers on average larger than both those reporting 4 or 3. Survey respondents reporting the suggestion as either poor (2) or very poor (1), thus favoring more liberal policies, exhibit somewhat larger increases in the share of migrant co-workers compared

[33] This survey is called Screening Across the Lifespan Twin Young (SALTY) and is part of the Swedish Twin Registry. The survey was sent out in 2009 to 24,914 Swedish twins born between 1943 and 1958. Of these, 11,261 responded and agreed to have their answers stored and analyzed.

[34] 3 represents "neither good nor bad."

to those who prefer a restrictive policy; however, the estimates are not statistically distinguishable from those reporting a 5. While not a definitive proof, the result from the attitude survey gives some support to our identification strategy.[35]

Fifth, we evaluate the importance of our time-varying controls on our main estimates. Due to the lack of a clear natural experiment, our identification relies on the inclusion of both fixed effects and several time-varying controls by precinct. Unlike the precinct fixed effects, time-varying controls by precinct may be bad controls, meaning that they could be potential outcomes of the main independent variable. Hence, it would be potentially concerning if our negative coefficients in Table 1 were primarily or solely driven by the time-varying controls. However, dropping the time-varying controls and focusing solely on the precinct and time fixed effects makes the negative effects of workplace contact on anti-immigration voting even more negative. We show this in Table C8 and conclude that our estimated negative effects do not hinge on the inclusion of potentially non-independent controls.

Sixth and finally, we include a robustness check altering the sample. In Table C9, we drop sample restrictions using only non-self-employed individuals and workers with more than 1 co-worker. The Table shows that the conclusions in the paper are not contingent on using the restricted sample.

Table C1: Removing precincts with unusually large, small, increasing, or decreasing populations

Dropping:	top 10 % in pop. (1)	bottom 10 % in pop. (2)	top 10 % in Δpop. (3)	bottom 10 % in Δpop. (4)
WP contact with non-Europeans	-0.392***	-0.400***	-0.448***	-0.510***
	(0.104)	(0.102)	(0.116)	(0.106)
Observations	15,714	15,759	15,710	15,725
Model	FE	FE	FE	FE
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Precinct FE	Yes	Yes	Yes	Yes
Labor market time trends	Yes	Yes	Yes	Yes

Notes: The effect of the precinct-level share of non-European co-workers among native-born workers on the share of votes for the Sweden Democrats. We exclude precincts with unusually large or small populations (columns (1) and (2)) and precincts with unusually large increases or decreases in population (column (3) and (4)). ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

[35] Ideally, we would like to test this more directly by showing how the attitudes of the respondents relate to the change in their *own* workplace composition. Unfortunately, this is currently not possible. The workplace ID used in the full population data, which is the basis of our treatment, is not possible to merge with the workplace ID in the survey, which is based on a separate key.

Table C2: Removing precincts with unusually large, small, increasing or decreasing populations, same-skill and different-skill contact

Dropping:	top 10 % in pop. (1)	bottom 10 % in pop. (2)	top 10 % in Δpop. (3)	bottom 10 % in Δpop. (4)
WP contact with same-skill non-Europeans	-0.380***	-0.313**	-0.375**	-0.468***
	(0.112)	(0.109)	(0.123)	(0.115)
WP contact with different-skill non-Europeans	0.124*	0.0409	0.0785	0.0959
	(0.0518)	(0.0499)	(0.0518)	(0.0579)
Observations	15,714	15,759	15,710	15,725
Model	FE	FE	FE	FE
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Precinct FE	Yes	Yes	Yes	Yes
Labor market time trends	Yes	Yes	Yes	Yes

Notes: The effect of the precinct-level share of non-European co-workers among native-born workers on the share of votes for the Sweden Democrats, considering matched and non-matched skill level among natives and immigrants. We exclude precincts with unusually large or small populations (columns (1) and (2)) and precincts with unusually large increases or decreases in population (column (3) and (4)). ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

Table C3: Removing precincts based on the absolute value of residuals.

Dropping:	$\lvert\widehat{\epsilon_i}\rvert >= 2\sigma$ (1)	$\lvert\widehat{\epsilon_i}\rvert >= 1.5\sigma$ (2)	$\lvert\widehat{\epsilon_i}\rvert >= 2\sigma$ (3)	$\lvert\widehat{\epsilon_i}\rvert >= 1.5\sigma$ (4)
WP contact with non-Europeans	-0.426***	-0.426***		
	(0.0911)	(0.0893)		
WP contact with same-skill non-Europeans			-0.316**	-0.371***
			(0.0975)	(0.0964)
WP contact with different-skill non-Europeans			0.0172	0.0401
			(0.0433)	(0.0447)
Observations	16,656	16,101	16,656	16,101
Model	FE	FE	FE	FE
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Precinct FE	Yes	Yes	Yes	Yes
Labor market time trends	Yes	Yes	Yes	Yes

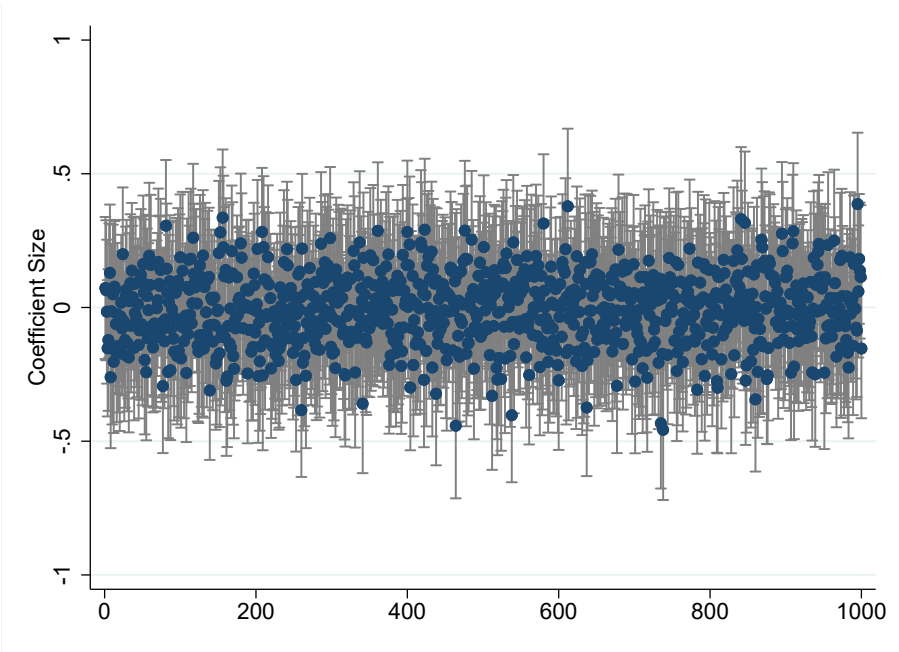
Notes: The effect of the precinct-level share of non-European co-workers among native-born workers on the share of votes for the Sweden Democrats, also considering matched and non-matched skill level among natives and immigrants. We exclude precincts with unusually large estimated residuals. We first estimate the baseline regression (Table 1, column (3)). We then use the estimated residuals from this regression and standardize the residuals with mean=0 and standard deviation (σ)=1. In columns (1) and (3), we drop all observations with a standardized residual with an absolute value >= 2, and in columns (2) and (4), we drop those with values >= 1.5. ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

Table C4: Removing precincts based on high leverage points.

Dropping:	$h_i >= 2\bar{h}_i$ (1)	$h_i >= 2\bar{h}_i$ (2)	$h_i >=$ p97.5(h) (3)	$h_i >=$ p97.5(h) (4)
WP contact with non-Europeans	-0.425***		-0.408***	
	(0.0996)		(0.101)	
WP contact with same-skill non-Europeans		-0.370***		-0.356**
		(0.107)		(0.109)
WP contact with different-skill non-Europeans		0.0755		0.0781
		(0.0496)		(0.0512)
Observations	17,424	17,424	17,001	17,001
Model	FE	FE	FE	FE
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Precinct FE	Yes	Yes	Yes	Yes
Labor market time trends	Yes	Yes	Yes	Yes

Notes: The effect of the precinct-level share of non-European co-workers among native-born workers on the share of votes for the Sweden Democrats, also considering matched and non-matched skill level among natives and immigrants. We exclude precincts with high leverage observations. We first estimate the baseline regression (Table 1, column (3)). We then use the projection matrix (*hatvalues*, (h_i)) from this regression. In columns (1) and (2), we drop all precincts with at least one observation with h_i larger than or equal to two times the mean of h. Column (3) and (4) drops all precincts with at least one observation in the top 2.5 percent of the distribution of h. ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

Figure C1: Randomizing the dependent variable



Notes: In this figure, we plot the estimates and 95% confidence intervals from regressions with randomly created dependent variables. The right-hand side of the specifications used are the same as in Table 1, column (3). The dependent variable is created by first sorting the sample by municipality and year. We then randomly shuffle the precinct order within each municipality and re-assign votes for the Sweden Democrats based on the new random order of precincts. We redo this 1,000 times to generate 1,000 randomly created dependent variables where the votes for the Sweden Democrats always represent a percentage number in another precinct within the same municipality and election year. In this figure, we plot the 1,000 coefficients. The mean over all coefficients is -0.08, with mean 95 % confidence intervals given by [-0.27, 0.25].

Table C5: Placebo regressions, changing the dependent variable to a placebo outcome

	% L.SD	% PP	% FI	% Men	% Married	% Divorced	% Blank votes	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WP contact with non-Europeans	0.110	-0.0101	0.00960	-0.0950	-0.0120	-0.0190	0.0487*	0.0277
	(0.0760)	(0.0146)	(0.0713)	(0.0498)	(0.0201)	(0.0163)	(0.0191)	(0.0184)
Observations	11,645	17,465	17,465	17,465	17,465	17,465	17,465	17,200
Model	FE	FE	FE	FE	FE	FE	FE	FE
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Precinct FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Labor market time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All columns measure the effect of the precinct-level share of non-European co-workers among native-born workers in election year t on different outcomes. Column (1) uses the share of votes for the Sweden Democrats in election year $t-1$. Column (2) uses the percentage of votes for the Pirate Party, and column (3) uses the percentage of votes for the Feminist Initiative. Column (4) uses the percentage of men living in the precinct, column (5) the percentage of precinct residents who got married in t, while column (6) shows the percentage who got divorced in t. Column (7) uses the percentage of votes that where blank ballots. In column (8), we drop the top 1 percent of observations in terms of percentage of blank ballots. ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

Table C6: Lagged dependent variable as covariate

	Lagged Dependent (1)	Baseline, but only 2010 and 2014 (2)
WP contact with non-Europeans	-0.811***	-0.758***
	(0.136)	(0.144)
Observations	11,645	11,648
Model	FE	FE
Year FE	Yes	Yes
Controls	Yes	Yes
Precinct FE	Yes	Yes
Labor market time trends	Yes	Yes

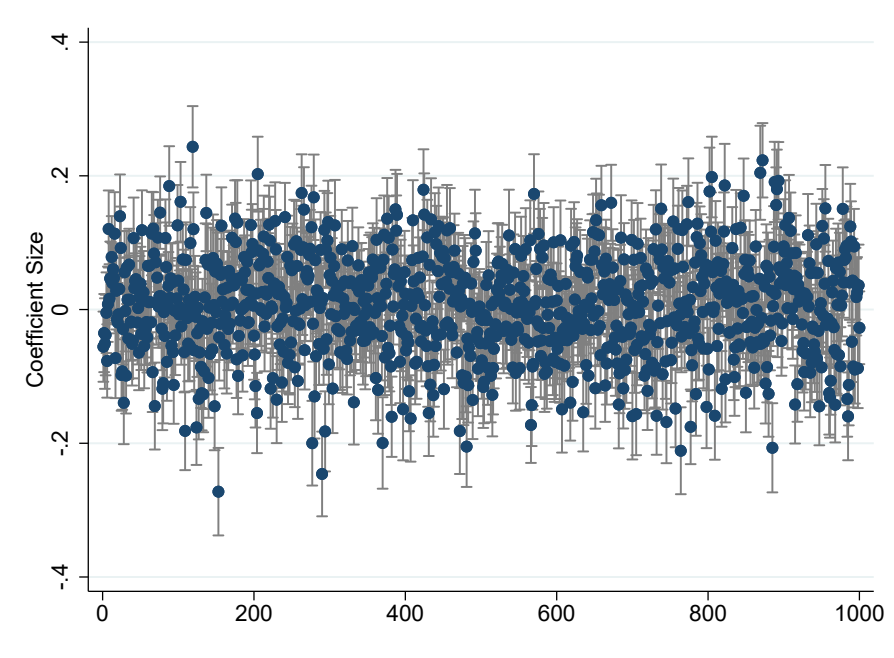
Notes: In column (1), we use the same set-up as in the baseline model in column (3) in Table 1, but we add a lagged dependent variable on the right-hand side of the regression equation. This means that we lose all outcomes in 2006. To better compare coefficients, column (2) uses the same set-up as in the baseline effect in column (3) but drops all observations from 2006. ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

Table C7: Altering the treatment variable

	# Co-workers (1)	Industry FE (2)	Stayers (3)
WP contact with any co-workers	0.147		
	(0.189)		
Deviation from national sector		-0.654***	
		(0.0992)	
WP contact with non-Europeans, stayers			-0.335***
			(0.0382)
Observations	17,465	17,465	16,760
Model	FE	FE	FE
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Precinct FE	Yes	Yes	Yes
Labor market time trends	Yes	Yes	Yes

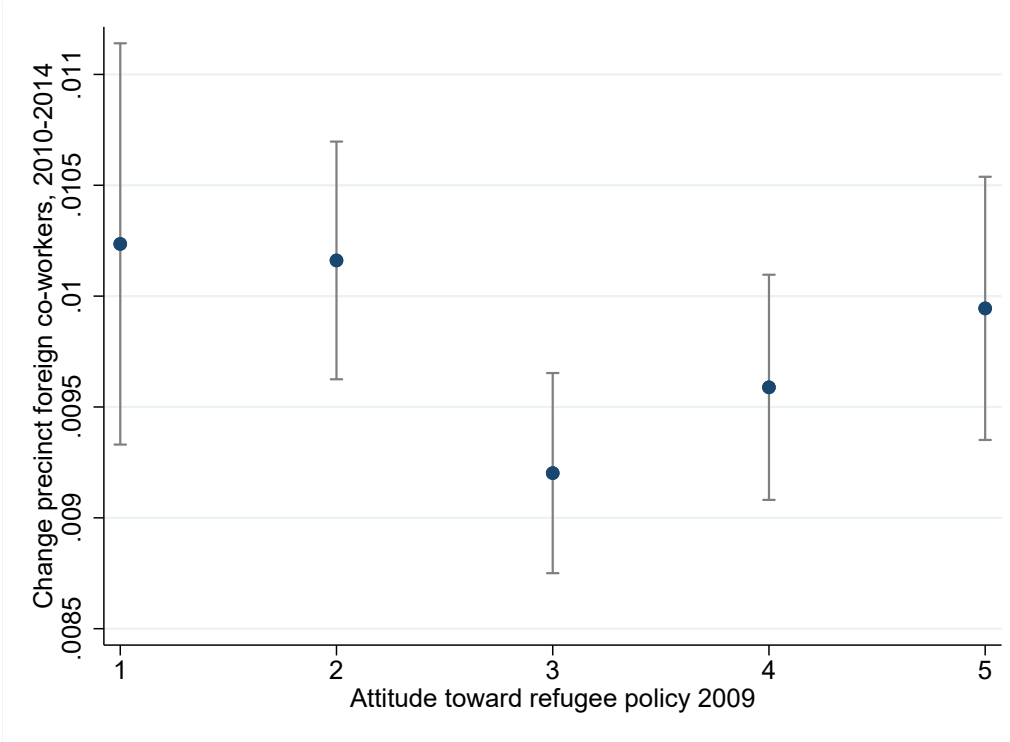
Notes: In column (1), we measure the effect of the precinct-level average number of workplace co-workers, rather than the share of *non-European* co-workers. In column (2), we use the same set-up as in the baseline effect in column (3), Table 1. However, the share of non-European co-workers for each native-born worker is computed as the deviation from the industry-specific national average. In column (3), the treatment is calculated using only natives who remained in the same precinct over election years t and $t+1$. ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

Figure C2: Randomizing the treatment variable



Notes: In this figure, we plot the estimates and 95% confidence intervals from regressions with randomly created treatments. The specifications used are the same as in Table 1, column (3). Instead of the actual share of non-European co-workers in workplaces within a precinct ($Mean\_Im\_Share_{pt}$), we randomly allocate shares of non-Europeans to each workplace (1,000 times) and then aggregate 1,000 different versions of $Mean\_Im\_Share_{pt}$, all based on different shares of randomly allocated non-European co-workers. In this figure, we plot the 1,000 coefficients. The mean over all coefficients is 0.005.

Figure C3: Comparing change in treatment variable depending on attitudes to refugee policy



Notes: Scale on x-axis represents to what extent survey respondents in 2009 believed that Sweden should take in fewer refugees. 5 = excellent suggestion, 1= very poor suggestion. Data from Statistics Sweden.

Table C8: Effect of non-European co-workers on votes for SD, including matched and non-matched skill level, changing fixed effects and controls.

	(1)	(2)	(3)	(4)
WP contact with non-Europeans	-1.256***		-0.360***	
	(0.0782)		(0.0717)	
WP contact with same-skill non-Europeans		-1.150***		-0.609***
		(0.0911)		(0.0815)
WP contact with different-skill non-Europeans		0.0560		0.411***
		(0.0529)		(0.0520)
Observations	17,508	17,508	17,465	17,465
Model	FE	FE	RE	RE
Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Precinct FE	Yes	Yes	No	No
Labor market time trends	Yes	Yes	No	No

Notes: The effect of the precinct-level share of non-European co-workers among native-born workers on the share of votes for the Sweden Democrats, considering matched and non-matched skill level among natives and immigrants. Columns (1) and (2) estimate the precinct fixed effects model with fixed effects for year and labor market time trends, while excluding the time-varying precinct and workplace controls. Columns (3) and (4), however, drop the precinct fixed effects as well as the labor market time trends but keep the time-varying controls. However, we keep the time fixed effects due to the strong national trend of increasing votes for SD as well as an increasing labor market presence of foreign-born individuals during the relevant time period. ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

Table C9: Share of non-European co-workers and support for SD, including self-employed individuals and workers with few or no co-workers

	(1)	(2)	(3)
WP contact with non-Europeans	-0.857***	-0.339***	-0.475***
	(0.0857)	(0.0950)	(0.101)
Observations	17,465	17,465	17,465
Model	FE	FE	FE
Year FE	Yes	Yes	Yes
Precinct FE	Yes	Yes	Yes
Precinct Controls	Yes	Yes	Yes
Labor market time trends	No	Yes	Yes
Workplace controls	No	No	Yes

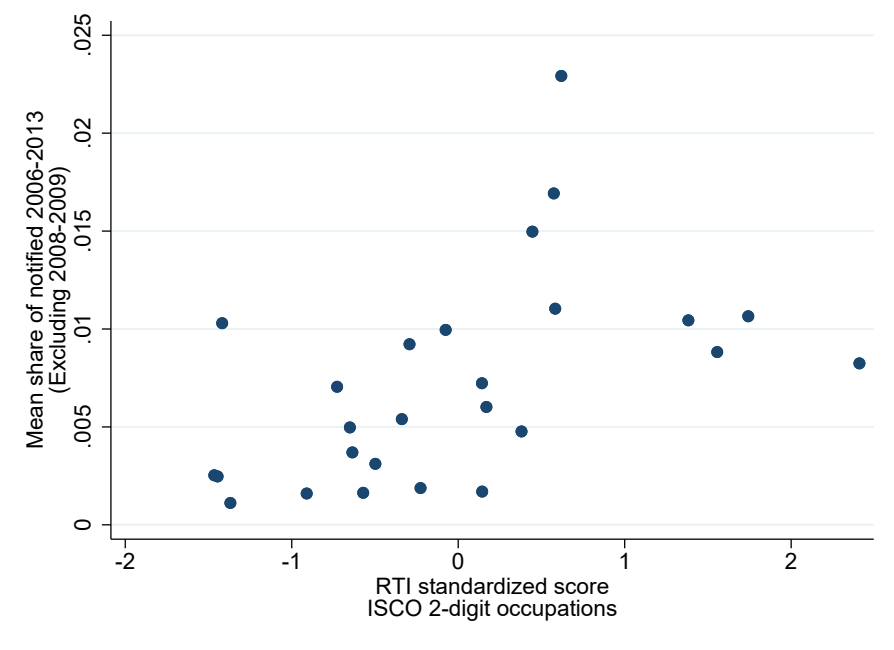
Notes: The effect of the precinct-level share of non-European co-workers among native-born workers on the share of votes for the Sweden Democrats. Sample includes self-employed individuals and all workers, regardless of number of co-workers. ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on clustered standard errors (at precinct level).

D Miscellaneous

Figure D1 shows the correlation between the Routine Task Intensity index scores and the percentage of employed individuals receiving a notice that they were being laid off.

Table D1 shows the classification of occupations according to the 1-digit SSYK-codes.

Figure D1: Correlation between notifications and RTI score



Notes: Scale on x-axis represents standardized RTI-scores Goos et al. (2014), while the y-axis shows percentage of employed individuals within occupation who received a notice that they were being laid off. Data pooled from 2006 to 2013. 2008 and 2009 excluded due to financial crisis.

Table D1: Skill level based on occupational classification

1-digit SSYK code (2012)	Name of occupation category	Skill level
0	Armed forces	–
1	Legislators, senior officials, and managers	High
2	Professionals	High
3	Technicians and associate professionals	High
4	Clerks	Low
5	Service workers and shop sales workers	Low
6	Skilled agricultural and fishery workers	Low
7	Craft and related trades workers	Low
8	Plant and machine operators and assemblers	Low
9	Elementary occupations	Low

Notes: Description of 1-digit Swedish Standard Classification of Occupations (SSYK) occupation categories. Source: Statistics Sweden (SCB).