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.

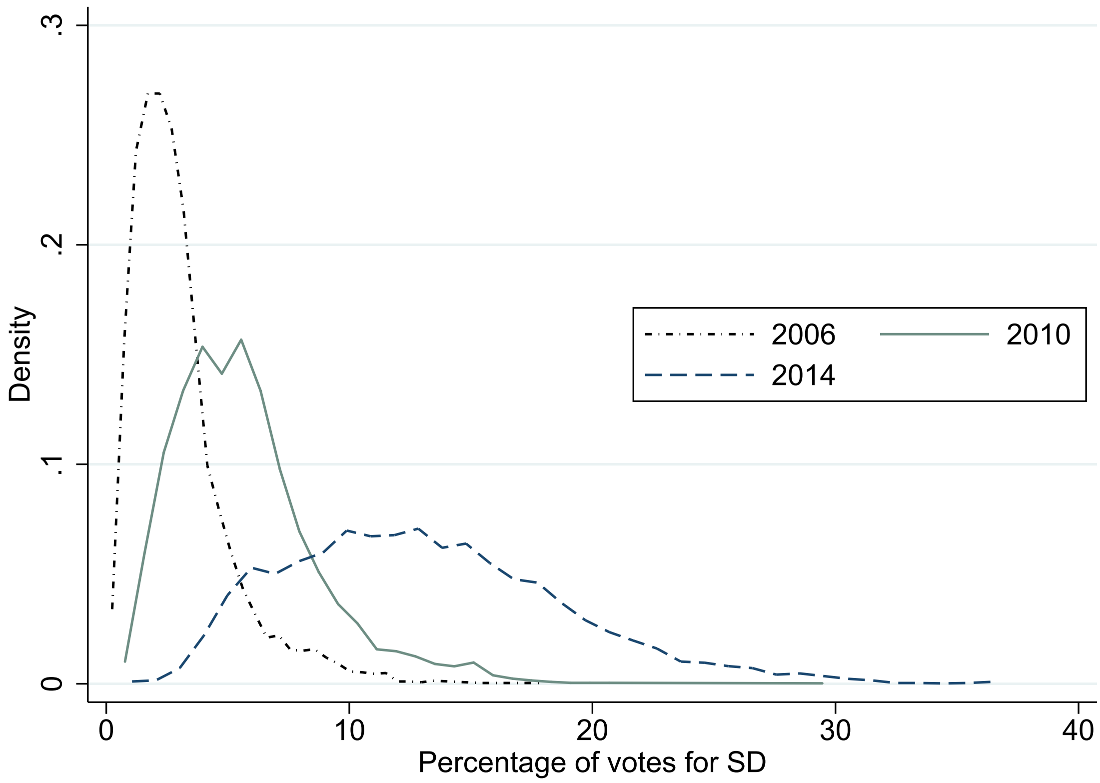
The precinct-level mean support for SD during our entire period (2006–2014) was 7.3%. The higher figures in the distribution are skewed toward the more recent elections since the party has increased its share of votes by an average of 5 percentage points per election (see Table B1). Figure B1 shows the 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

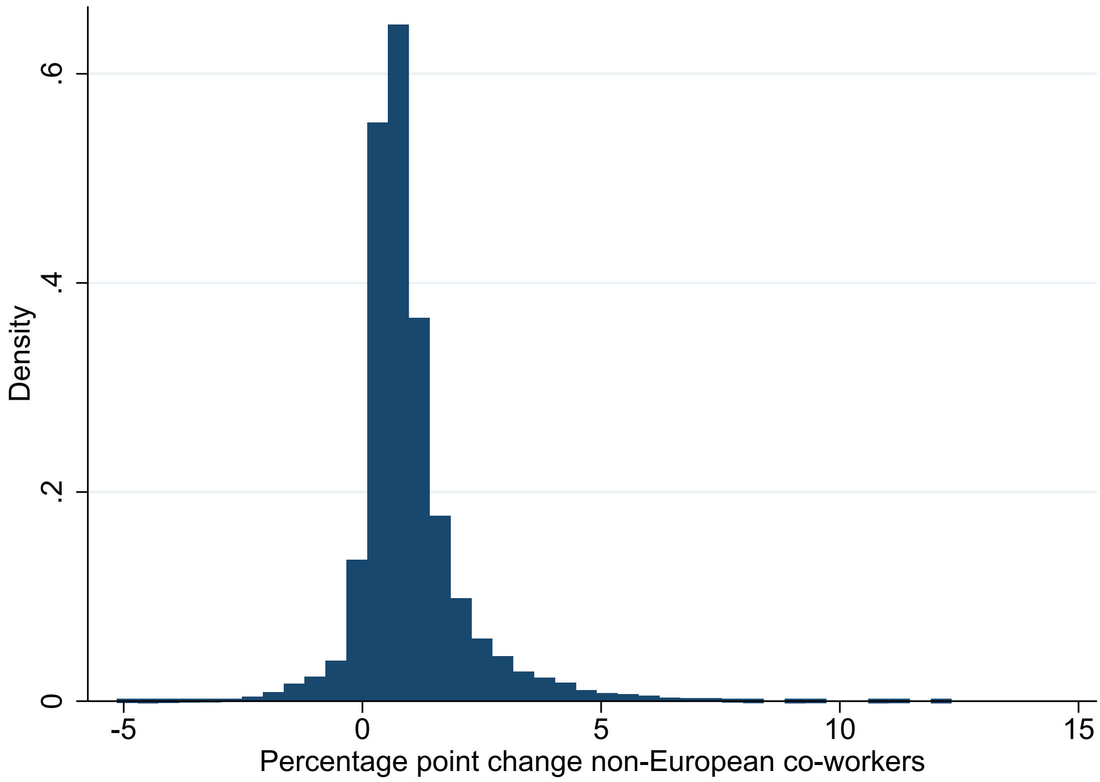


Notes: Distribution of votes in percentage for SD in 2006, 2010, and 2014.

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

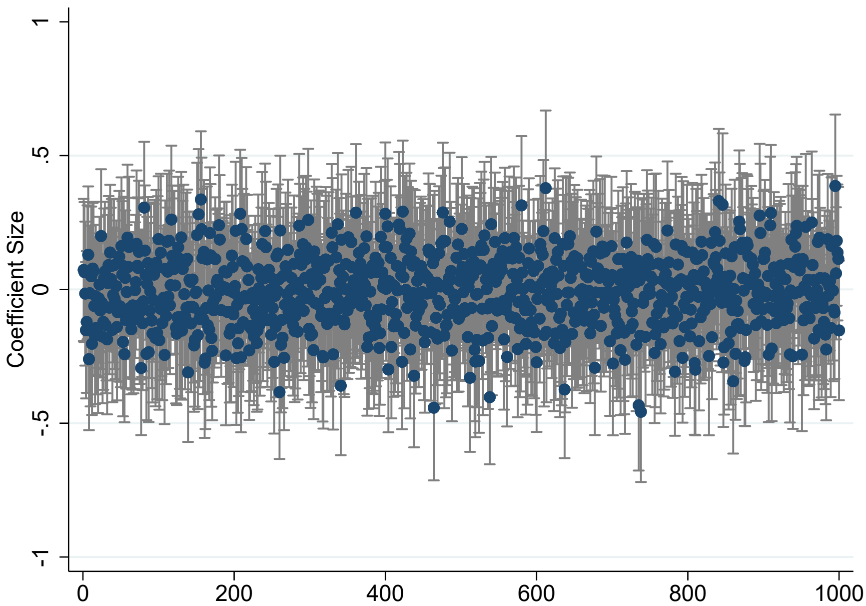
Notes: The effect of the precinct-level share of non-European co-workers among native-born workers on the share of votes for the Sweden Democrats, also considering matched and non-matched skill level among natives and immigrants. We exclude precincts with unusually large estimated residuals. We first 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

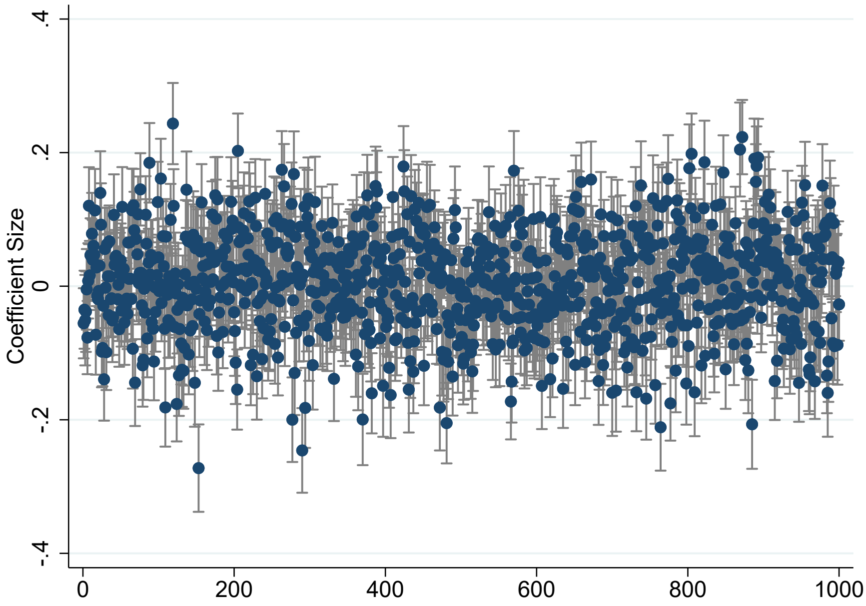
Notes: In column (1), we use the same set-up as in the baseline model in column (3) in Table 1, but we add a lagged dependent variable on the right-hand side of the regression equation. This means that we lose all outcomes in 2006. To better compare coefficients, column (2) uses the same set-up as in 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

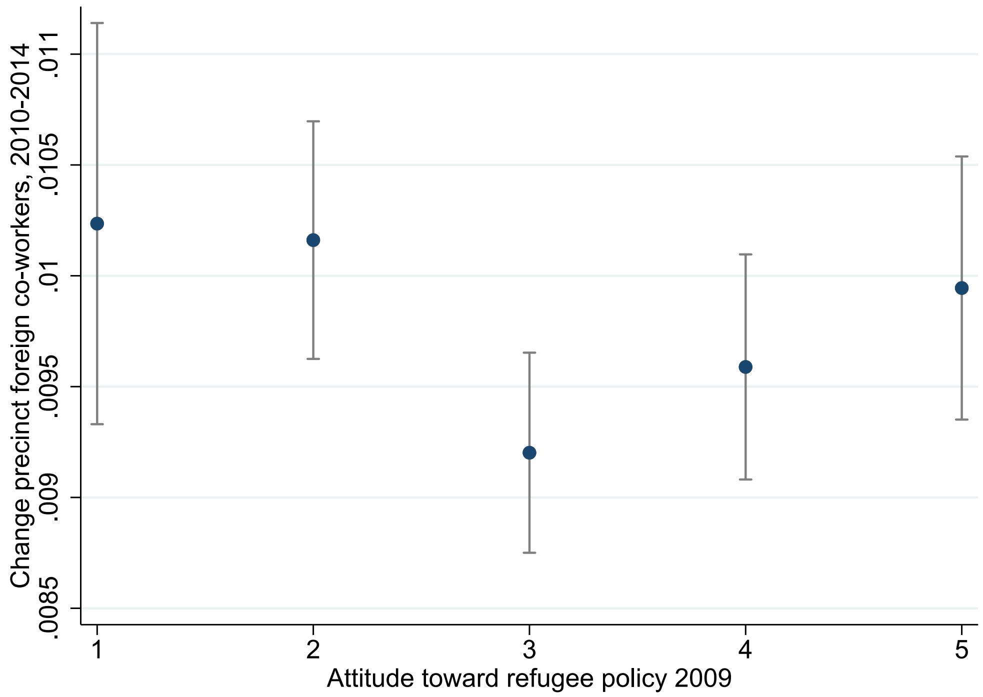
Notes: In column (1), we measure the effect of the precinct-level average number of workplace co-workers, rather than the share of *non-European* co-workers. In column (2), we use the same set-up as in the baseline effect in column (3), Table 1. However, the share of non-European co-workers for each 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

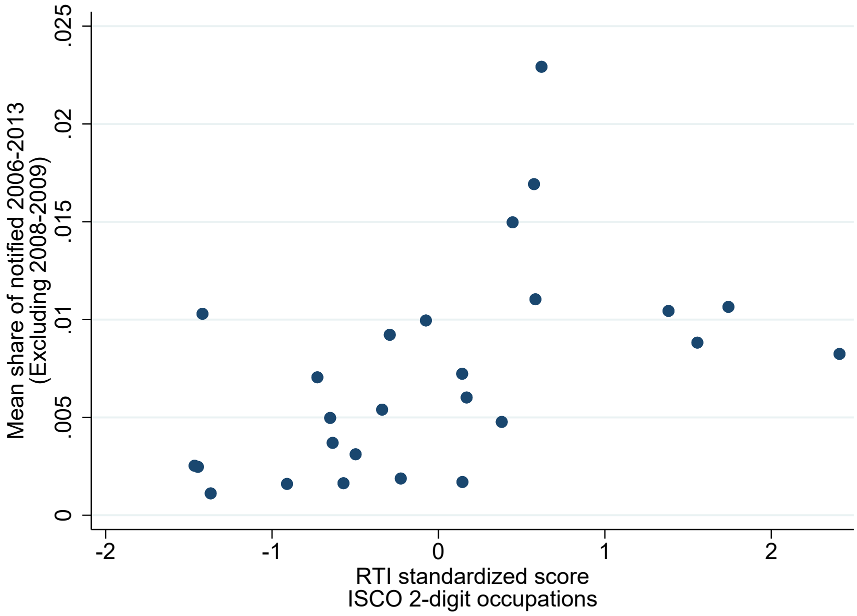
Notes: The effect of the precinct-level share of non-European co-workers among native-born workers on the share of votes for the Sweden Democrats. Sample includes self-employed individuals and all workers, regardless of number of co-workers. ***, **, and * indicate statistical significance at 0.1%, 1%,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