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Group threat and voter turnout: Evidence from a refugee placement program

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Abstract

We study the impact of refugee inflows on voter turnout in Sweden in a period when shifting immigration patterns made the previously homogeneous country increasingly heterogeneous. Analyzing individual-level panel data and exploiting a national refugee placement program to obtain plausibly exogenous variation in immigration, we find that refugee inflows significantly raise the probability of voter turnout. Balancing tests on initial turnout as well as placebo tests regressing changes in turnout on future refugee inflows support the causal interpretation of our findings. The results are consistent with group-threat theory, which predicts that increased out-group presence spurs political mobilization among in-group members.

KEYWORDS
group threat, refugee immigration, voter turnout

1 | INTRODUCTION

In the past decades, immigration to European countries has increased considerably. Between the periods 1960–1969 and 1990–1999, net-migration flows increased from 1.1 million to about 10 million, generating large demographic shifts in many societies throughout the continent. The increased flows also reflect a new pattern of immigration since the 1980s. Following World War II, migration in Europe consisted mostly of intracontinental labor flows, especially from southern to northern countries, which were frequently reversed after employment contracts ended. However, since the end of the 1970s, such flows have been replaced by immigration from the developing world, which led to more permanent settlement in the host countries (Dustmann & Frattini, 2012; Wanner, 2002). In the latter part of the 20th century, previously ethnically homogeneous countries thus became increasingly heterogeneous—and the consequences of this change have been the subject of intense debate.

One important possible consequence of immigration could be altered political engagement among natives. In America, over half a century ago, Key (1949) noted a positive correlation between the presence of African Americans and voter turnout among whites; the perception of "group threat" allegedly stimulated higher political mobilization. However, other scholars have suggested that sustained positive interethnic contact may decrease existing perceptions of threat (e.g., Allport, 1954), implying that immigration, if it generates such contact, could potentially reduce existing in-group bias as a source of political engagement and thus, by extension, lower turnout (see Zingher & Moore, 2019; Zingher & Thomas, 2014). Others instead argue that increased ethnic diversity, at least in the short run, decreases societies' stock of social capital and leads to a general withdrawal from public life, one consequence of which may be depressed voter turnout (see Putnam, 2007). These different theoretical insights have generated a large empirical literature with mixed results. Yet identification problems involved in analyzing the effects of ethnic diversity are severe because of potentially endogenous settlement and mobility patterns as well as other sources of unobserved heterogeneity.

This study provides new evidence on the impact of immigration on individual-level voter turnout in Sweden. In 1985, increasing refugee inflows led the government to enact a placement program, through which most new refugees were contracted to the country's municipalities each year until the program was dismantled in 1994. It is possible to exploit the municipal contracts to obtain variation in refugee inflows that is free from bias due to endogenous settlement patterns and measurement error, and, once we adjust for municipal-fixed effects and a few control variables, also plausibly exogenous to changes in individual-level turnout more generally.

Combining the municipal-level contracts with data from the Swedish National Election Studies Program, carried out in conjunction with every Swedish election since 1956, the paper analyzes how refugee inflows due to the placement program affected changes in individual-level voter turnout between the national, municipal, and county elections in 1985 and 1988, 1988 and 1991, as well as 1991 and 1994. Two features of this survey are especially useful: (1) each individual is observed in two elections in a row, which allows us to adjust for individual-fixed effects and ensure that native mobility does not bias our findings, while (2) data on voter turnout are obtained directly from official records, which gets rid of potential response bias and minimizes panel attrition. To increase the likelihood that the sample analyzed is in fact representative of the Swedish voting-age population, we weight each individual with the inverse probability of selection in the population of eligible voters. Still, since the survey was designed to be representative at the national rather than municipal level, the estimated effects should primarily be seen as valid for the randomly sampled population in each municipality.

Our study thus analyzes whether refugee immigration in one period, spurred by the placement contracts, altered individuals' propensity to vote over the same period. The results display that larger refugee inflows raise individuals' propensity to vote in national and local elections: a rise in the refugee inflow by 1 percentage point increases the likelihood of voter turnout by about 5 percentage points. Balancing tests on initial turnout and placebo tests regressing changes in turnout on future refugee inflows support the causal interpretation of our findings, as do several robustness checks. In addition, supporting our expectations, we find that OLS estimates are biased toward zero, indicating that it is important to take into account endogenous settlement and mobility patterns and/or measurement error in refugee inflows.

Overall, our findings thus provide support for group-threat theory: existing residents appear to mobilize politically because of refugee immigration. While silent on the effects of long-term positive contact of the type thought to decrease perceptions of threat, the study provides new evidence of how real-world demographic changes—which do not necessarily generate positive contact between in- and out-group members—affect political engagement in the short-run perspective. Given the relative scarcity of convincing research on the relevance of group threat for understanding individual-level voter turnout in general, the study thus provides an important contribution to the literature.

The study proceeds as follows. Section 2 discusses the theoretical mechanisms linking refugee immigration to voter turnout; Section 3 reviews the empirical literature; Section 4 describes the Swedish setting and the refugee placement program; Section 5 discusses the data and methodology utilized; Section 6 outlines the estimation strategy; Section 7 presents the results; and Section 8 concludes.

2 | THEORY

Why would refugee immigration affect voter turnout? One theoretical mechanism rests on the social psychological concept of social identity, defined as "that part of an individual's self-concept which derives from his knowledge of his membership in a social group" (Tajfel, 1978; p. 63). People's social identity may in turn be connected to the formation of their preferences, generating "in-group bias." Such group identification could be based on, for example, ethnicity, religion, nation, or class. And antipathy among in-group members toward out-group members may increase with diversity, as the latter's salience rises with proximity without necessarily leading to much contact between in- and out-group members. Out-group salience may in turn boost the perceptions of differences between groups, which in turn may raise the perceived value of political engagement among in-group members (Enos, 2014, 2017). As a result, turnout among in-group members may increase because of out-group influxes. Increased political mobilization following immigration would in turn be in line with group-threat theory, as originally stipulated by Key (1949), who noted a strong correlation between the presence of African Americans and voter turnout among whites in the American South.

Certainly, in the original formulation of group-threat theory, the salience of out-group presence was not seen as key to political mobilization, which instead was hypothesized to stem from interested elites' demagoguery (Key, 1949). Yet such demagoguery may also be more effective in an environment of increasing perceptions of differences between groups. While others have proposed instrumental mechanisms for why diversity may increase turnout—such as competing interests among in- and out-group members—such mechanisms are rarely plausible. This is because there are few instrumental reasons for individuals to vote because of differential group interests, leaving psychological factors the most plausible mechanisms behind group threat (Enos, 2016). Indeed, at the time of the original formulation of group-threat theory in the American South, out-group members were extremely restricted in their ability to vote and thus could not realistically pose a threat to any perceived interests of whites via the ballot box. Rather, according to Key (1949), it was the "symbolic potency" (p. 656) of the presence of African Americans that generated “intense political consciousness” among whites (p. 517).

While group-threat theory predicts increased turnout because of immigration, there are other reasons to believe the impact may be the opposite. The “contact hypothesis” holds that increased, sustained interaction between in- and out-group members, under specific circumstances, has the potential to reduce prejudices and conflicts (Allport, 1954). If this holds true, existing perceptions of group threat and, in the end, political mobilization among in-group members may in fact decrease as the share of out-group members increases (see Zingher & Moore, 2019; Zingher & Thomas, 2014). In this story, immigration may thus lower turnout: increased diversity stimulates more interethnic contact, which reduces perceptions of threat and in-group bias as sources of political engagement among natives.

Still, it is important to note that group-threat theory and the contact hypothesis are not necessarily in tension for the purposes of understanding the impact of immigration on voter turnout. This is because immigration may not, in the short run at least, generate the type of positive, sustained interactions between in- and out-group members that are thought to have the potential to reduce conflict. Indeed, in Allport's (1954) original formulation, contact decreases threat if it occurs between in- and out-group members of equal status, in situations where they work together toward common goals in a noncompetitive environment, and it is supported by institutional authorities. One may not expect these conditions to come into play following short-term influxes of refugees. And if they do not, there is little reason to expect existing perceptions of threat to decrease because of increased immigration, according to the contact hypothesis. On the contrary, increased out-group presence combined with a lack of direct contact may provide the ideal conditions for increased perceptions of threat, as salience of out-group members is maximized without any possibility of contact to ameliorate its psychological effects (see Enos, 2016, 2017). The contact hypothesis could, in such circumstances, not be used to predict decreased turnout from increased immigration via decreased perceptions of threat.

However, a very different proposed mechanism for why increased diversity may decrease turnout would be relevant also in such circumstances. This mechanism comes from theories emphasizing the importance of social capital for various societal outcomes. In this story, increasing diversity, in the short run, decreases social capital among both in- and out-group members and thus produces social isolation and a general withdrawal from public life, one manifestation of which would be depressed voter turnout (see Putnam, 2007). If this theoretical suggestion holds true, one would expect refugee immigration to have negative effects on turnout among residents, even if no actual contact occurs between in- and out-group members because of such immigration. Indeed, the posited mechanism explaining the supposed negative effect of diversity on turnout is its depressing impact on contact more generally.

3 | PREVIOUS LITERATURE

The findings from existing empirical research on the political effects of diversity is rather mixed. Indeed, studies analyzing political engagement and turnout display varied results (Bhatti et al., 2017; Fieldhouse & Cutts, 2008; Hill & Leighley, 1999; Leighley & Vedlitz, 1999; Matthews & Prothro, 1963; Schlichting et al., 1998; Zingher & Moore, 2019; Zingher & Thomas, 2014). A similarly heterogeneous picture emerges from research exploring the effects of diversity and immigration on vote outcomes (Arzheimer, 2009; Della Posta, 2013; Gerdes &

Wadensjö, 2010; Giles & Buckner, 1993; Roch & Rushton, 2008; Rydgren & Ruth, 2011, 2013; Voss, 1996; Voss & Miller, 2001), and, more generally, on interethnic attitudes (e.g., Avery & Fine, 2012; Bobo & Hutchings, 1996; Dustmann & Preston, 2001; Fox, 2004; Hopkins, 2010; Markaki & Longhi, 2012; L. McLaren & Johnson, 2007; L. M. McLaren, 2003; Newman, 2013; Oliver & Mendelberg, 2000; Oliver & Wong, 2003; Schlueter & Scheepers, 2010). Yet this research does not generally exploit plausibly exogenous variation in diversity and thus likely fails to isolate causal relationships.[1] For example, settlement patterns of ethnic minorities and immigrants are not random, but often depend on community characteristics (e.g., Bracco et al., 2018; Damm, 2009)—which may also affect the outcomes under investigation.

Focusing on methodologically advanced research, studies analyzing American college students who are randomly allocated to roommates of different ethnicities indicate that contact does indeed often breed more positive intergroup attitudes (see Boisjoly et al., 2006; Carrell et al., 2015; Shook & Fazio, 2008; Van Laar et al., 2005), suggesting that diversity in such conditions does decrease perceptions of threat. Randomized studies subjecting young adults to interethnic interactions find similar effects in other settings (see Paluck et al., 2019). However, the extent to which these findings are relevant for understanding the impact of real-world demographic changes is questionable. This is because in-group members are usually not forced to interact frequently with out-group members under specific conditions determined by authorities, and the choice to engage with out-group members is endogenous to intergroup attitudes.[2] As noted in Section 2, it appears unlikely that refugee immigration would generate the type of contact that has been found to produce more positive intergroup attitudes.

Indeed, available evidence does not necessarily suggest that real-world demographic changes by themselves help spur positive contact that improves interethnic attitudes. In one study, Enos (2014) analyzes the impact of randomly increasing the daily presence of Hispanics at Boston train stations in homogeneously white neighborhoods, finding that treatment induced stronger exclusionary attitudes among residents. Similarly, exploiting quasi-random variation, Hangartner et al. (2019) find that exposure to refugees made natives on Greek islands close to Turkey more hostile to immigration. Such studies are more relevant for understanding the effects of real-world demographic changes on exclusionary attitudes than studies analyzing various forms of involuntary interethnic interactions.

In general, group-threat theory also often receives support in studies analyzing aggregate vote outcomes, which tend to find that diversity boosts aggregate vote shares for right-wing and anti-immigration parties (Barone et al., 2016; Dustmann et al., 2016; Edo et al., 2019; Halla et al., 2017; Harmon, 2017; Otto & Steinhardt, 2014), although some research reaches different conclusions (Mendez & Cutillas, 2014; Steinmayer, 2016). At the same time, studies analyzing aggregate turnout find mixed effects (Barone et al., 2016; Bratti et al., 2017; Dustmann et al., 2016; Edo et al., 2019; Mendez & Cutillas, 2014). Regardless, research studying aggregate outcomes cannot normally separate voter responses from mobility effects that occur due to immigration.[3]

While most studies focus on the effects of increasing diversity on vote outcomes and turnout, some research instead investigates the effects of decreasing diversity. In an interesting contribution, Enos (2016) analyzes a natural experiment in Chicago when the reconstruction of public housing displaced African Americans living close to neighborhoods predominantly inhabited by whites. As a result of the African American outflow from nearby communities, turnout decreased by over 10 percentage points among whites, who also became less likely to vote Republican. These effects decrease the farther away voters lived from the housing projects. At the same time, turnout and party choices among African Americans nearby were unaffected.

Overall, the empirical literature on the effects of diversity on intergroup attitudes and political behavior is thus mixed. Stronger studies analyzing political behavior tend to support the notion of group threat, but effects on turnout specifically are not consistent across these studies either. Furthermore, prior methodologically advanced research on the attitudinal and political effects of diversity generally analyzes aggregate outcomes. This makes it difficult to separate voter responses from the effects of native mobility, which may also change as a result of diversity. Finally, the preponderance of research has focused on immigration or ethnic diversity in a broader sense rather than on refugee immigration specifically. In this study, we exploit a refugee placement program and individual-level panel data in an attempt to further contribute to our understanding of the political consequences of increased diversity in general.

4 | SWEDISH IMMIGRATION AND THE REFUGEE PLACEMENT PROGRAM

To study the effects of refugee immigration on voter turnout, we exploit data from Sweden, which has seen significant changes in its immigration patterns since World War II. After a brief period of refugee influx from Eastern Europe in the late 1940s, intra-European labor migrants dominated inflows between the 1950s and the 1970s. These migrants came primarily from Nordic countries (especially Finland). However, because of more restrictive rules and a less favorable economic climate, economic immigration gradually decreased in the 1970s, while refugee immigration from the developing world increased, first gradually and then rapidly from the mid-1980s onwards (Lundh & Ohlsson, 1999; Nilsson, 2004). Indeed, Sweden accepted the highest number of refugees per capita in Europe each year between 1983 and 2003 (Ruist, 2015). Unsurprisingly, this development has generated an increasingly heterogeneous population. In 1960, 4% of the population were born abroad; by 2014, this figure had increased to 17% (Statistics Sweden, 2015).

As a consequence of the shifting migration patterns, the immigrant population has also become increasingly ethnically different from native Swedes. As late as 1970, only 5% of the foreign-born population came from outside Europe, a figure that had increased to 12% in 1980, 28% in 1990, 39% in 2000, and 48% in 2012 (Aldén & Hammarstedt, 2014). In other words, in the last 30 years of the 20th century, Sweden was transformed from an ethnically homogeneous country to an ethnically diverse country.

The refugee situation changed especially from 1986 onwards, which is the starting point for the period under investigation in this study. Whereas on average fewer than 5000 refugees arrived annually in the period 1982–1985, this figure increased significantly to 19,000 refugees annually in the period 1986–1991, and further to 35,000 refugees annually in the period 1992–1994 (Dahlberg et al., 2012). To cope with the unprecedented situation, the Swedish government implemented a refugee placement program, which lasted between 1985 and July 1994, with the intention to distribute refugees more evenly across the country's then 284 municipalities and especially break their concentration to large cities. The idea was first to contract about 60 municipalities, but more came to participate as the number of refugees increased radically (Edin et al., 2003). According to the data utilized in this study—which were extracted from the official contracts by Nekby and Pettersson-Lidbom (2012, 2017)—196 municipalities agreed to accept at least some refugees in 1986, a figure that increased to 243 only a year later and to 279 in 1990. In other words, the placement program affected essentially the entire country, although to different degrees.

It also appears clear that the goal to distribute incoming refugees more evenly was achieved: there is a sharp trend break in 1985 in terms of the share of refugees settling in Stockholm, Gothenburg, and Malmö. Between 1982 and 1984, the share of new refugees who moved into these three municipalities increased from about 50% to 60%. In 1985, however, the figure decreased to about 35%, and it continued to decrease to just over 10% in 1990, while it increased slightly again from 1991 to just below 20% in 1994. Meanwhile, the share of incoming refugees who ended up in municipalities with fewer than 50,000 inhabitants increased from below 20% in 1984 to over 50% in 1989 (Dahlberg et al., 2012). Thus, the placement program both altered settlement patterns and, in a context of rapidly increasing refugee inflows, induced considerable within-municipality variation over time.

Importantly, refugees who were allocated via the program were assigned to municipalities based on contracts rather than being able to choose where to settle initially. The contracts were essentially decided through bargaining between the Swedish Immigration Board and the municipalities, which committed to accept a certain number of refugees for a specified period of time, often between 1 and 3 years. The contracts became effective following ratification by the municipal government in question (see Nekby & Pettersson-Lidbom, 2012, 2017). Along with the fact that the Swedish Immigration Board could not legally force municipalities to accept refugees (Soininen, 1992), this makes it possible that local political forces came into play in the contracting process. Nevertheless, almost all municipalities appear to have accepted the idea of a placement policy based on national solidarity and placements proportional to municipal population size, and the main obstacle to placements was not ideological hostility but rather housing availability (see Bengtsson, 2002; SOU, 1996; p. 55). Indeed, the fact that the goal to distribute incoming refugees more evenly across the country was reached suggests that the municipalities, although there may have been specific exceptions, in practice generally accepted that they would have to agree to contract refugee settlements in line with the Swedish Immigration Board's wishes.

While refugees were allowed to move after the initial assignment, by exploiting the number of contracted refugees as an instrument for refugee inflows in the municipalities, it is possible to entirely circumvent the problem of endogenous settlement and mobility patterns during this period. More generally, as noted above, while the program did not place (or contract) refugees randomly across municipalities (see Dahlberg et al., 2017; Nekby & Pettersson-Lidbom, 2012, 2017), for this study's purposes it is only necessary that predicted refugee inflows based on the contracts are exogenous to changes in individual-level turnout once other relevant variables are held constant. In the next section, we discuss the data and methodology used to exploit the placement program to obtain conditionally exogenous variation in refugee immigration across Sweden.

It may further be noted that the refugee placement program coincided with societal and political changes in Sweden that would be consistent with increasing perceptions of threat. For example, in the 1980s and the early 1990s, there was an increase in right-wing extremism and violence, which received considerable media interest at the time (Boréus, 2006; Falkheimer & Mithander, 1999). While these groups never received popular support, mainstream political rhetoric against refugee immigration increased in the last years of the 1980s. In late 1989, facing ever increasing refugee inflows, the Social Democratic government significantly restricted overall arrivals, a decision supported by the largest center-right party. Furthermore, in the 1991 election, the right-wing populist party New Democracy, the policies of which included decreasing refugee immigration, won almost 7% of the vote, 25 seats in parliament, and 335 seats in municipal councils across the country, despite having been founded only seven months earlier. In the first years of the 1990s, rhetoric and media coverage of issues related to refugee immigration also appear to have been especially negative (Boréus, 2006). Overall, societal and political changes in Sweden during the period analyzed are thus at least consistent with increased perceptions of threat.

5 | DATA AND METHODOLOGY

During the period analyzed, elections occurred every three years, always on the third Sunday in September. All elections—national, municipal, and county—are held on the same day. Non-citizens were eligible to vote in municipal and county elections if they have been officially registered as living in Sweden for at least three years before the election. Immigrants (refugees) are required to be officially registered as living in Sweden for five (four) years before being eligible to apply for Swedish citizenship, which gives them the right to vote also in national elections. The study's estimation strategy, described formally in Section 6, hinges on being able to link data on individual-level turnout to municipal-level variables, including the measure we exploit to obtain plausibly exogenous variation in refugee inflows. This section describes the data and methodology utilized in the analysis.[4] The descriptive statistics are displayed in Table 1.[5]

TABLE 1 Descriptive statistics

	Mean	SD	Min.	Max.
ΔTurnout (national elections)	−0.006	0.317	−1.000	1.000
ΔTurnout (municipal elections)	−0.006	0.324	−1.000	1.000
ΔTurnout (county elections)	−0.006	0.328	−1.000	1.000
Δ% Refugees	0.823	0.489	−1.386	7.465
Δ% Contracted refugees	0.794	0.417	0.000	11.801
Average welfare spending per capita	317.288	183.995	34.813	938.155
Average population	121,883	185,723	2,958	693,719
% Average unemployment rate	3.263	2.202	0.207	10.080
% Average vacant public housing	1.805	2.632	0.000	21.128
% Foreign citizens	5.457	3.407	0.412	25.568
% Left-wing seats	46.850	9.505	13.333	75.556
% Right-wing seats	46.938	8.860	17.143	80.000
% New democracy party seats	0.930	1.968	0.000	15.385
Left-wing majority	0.332	0.471	0.000	1.000
Right-wing majority	0.324	0.468	0.000	1.000

Note: $n = 4777$ for all variables apart from ΔTurnout (county elections), where $n = 4{,}366$. The variable denoting welfare spending is given in Swedish Krona. The descriptive statistics for the independent variables are calculated based on the sample for the national elections. Δ% Refugees is calculated using data from the municipality of residence at the time of each election, while Δ% Contracted refugees is calculated using data from the home municipality only.

5.1 | Voter turnout

Data on voter turnout are obtained from the Swedish National Election Studies Program (SNES, 2015), a survey that has been carried out in conjunction with every election since 1956.[6] Since 1973, the surveys are carried out as rotating panels in which individuals are interviewed at two elections in a row, with a new panel sample being drawn on each occasion. The panel sample is randomly selected and is representative of the Swedish population of eligible voters in national elections at the time of selection.[7] The survey contains information on political and voting preferences as well as background characteristics of the respondents.

The study analyzes data from the elections in 1985, 1988, 1991, and 1994 in the main analysis, and exploits the rotating panel structure to create three different survey panels for pairwise samples: 1985/1988, 1988/1991, and 1991/1994. Each panel thus includes respondents who were surveyed in both years only. This is necessary since we focus on the individual-level voter responses to immigration rather than effects on aggregate turnout, which risk mixing the impact on turnout with demographic changes across communities. If people who feel more threatened by refugee immigration "vote with their feet" and move to municipalities with less immigration, or avoid moving to municipalities with higher immigration, the comparison groups become contaminated. This is not a trivial concern in this context since prior Swedish research suggests that non-European immigration induces both native flight and avoidance (Aldén et al., 2015), which is also confirmed in our data as shown in Table A1.[8] Focusing on respondents in the panel sample allows us to take into account mobility bias directly by assigning all movers—corresponding to 393 individuals or 8% of the final sample—to the municipality in which they lived at the time of the first election. This ensures that the comparison groups are defined based on individuals' municipality of residence at the first election, which gets rid of any potential mobility bias due to differential treatment intensity (see Angrist & Pischke, 2009).[9]

However, such an intention-to-treat analysis assumes that movers were exposed to the full refugee inflow in the municipality of origin, even though they relocated between the elections. Thus, as outlined in Section 6, we calculate the variable capturing refugee inflows using data from the municipality of residence at the time of each election, while calculating the instrument predicting that change using data from the municipality of origin only. In robustness tests, we also provide estimates from intention-to-treat analyses as well as reduced-form analyses, where the instrument is used as the main predictor.

While all panel surveys normally contain significant nonresponse rates and attrition, we circumvent this problem by exploiting a useful feature of this particular survey: certain information is collected directly from administrative records, and this includes voter turnout in national, municipal, and county elections. As a consequence, data are also available for individuals who were sampled, but who did not end up participating in the survey. However, the probability of selection in the second survey of the rotating panel sample for respondents who did not respond in the first survey was 50%, which means that half of them were unselected for participation in the survey carried out in conjunction with the second election. In addition, a few observations, totaling less than 1% of the sample, have missing voting records. Overall, therefore, the attrition rate is just 14% out of a total panel sample of 5,571 individuals, which is comparatively low and means that endogenous sampling is unlikely to be a problem. This is especially true since the principal reason for attrition is that half of the sampled individuals who did not end up participating in the first survey were randomly unselected as potential participants in the second survey.[10]

Yet to decrease the likelihood that the remaining attrition threatens the validity of our findings, we also employ inverse probability weighting (Horvitz & Thompson, 1952; Solon et al., 2015). This means that all individuals are weighted according to the inverse probability that they were randomly drawn from the entire population of eligible Swedish voters at the time of the second election in the panel.[11] If attrition or changes in the underlying population between two consecutive elections have made the sample unrepresentative, inverse probability weighting increases the likelihood that its representativeness is restored. To increase the probability of avoiding sample-selection bias, we thus always include such weights in the regressions. To be sure, these weights do not guarantee that the problem of attrition is entirely eliminated, but they are likely to decrease the likelihood of sample-selection bias. This is especially true since attrition is essentially entirely due to the fact that some people were randomly unselected for participation in the survey at the time of the second survey.

As there are three observations with missing values on our primary independent variable described in Section 5.2, the total sample in the analysis of the national and municipal elections includes 4,777 individuals from 284 municipalities, each observed in two elections in a row. For the analysis of county elections, the sample is reduced to 4,366 individuals because three municipalities—Gothenburg, Gotland, and Malmö—carried out the responsibilities of the counties in the period analyzed.

5.2 | Refugee inflows and the placement program as instrument

To capture refugee inflows, we utilize the number of refugees for whom municipalities received a grant from the government for the cost of initial integration. While this variable has previously been used as an exogenous measure for the placement program itself (e.g., Dahlberg & Edmark, 2008; Dahlberg et al., 2012), it in fact covers all newly arrived refugees rather than just those who arrived in the municipalities via the placement program.[12] This means that the measure is likely to be partly endogenous since it suffers from some self-selection of refugees into the municipalities. In addition, since grants were sometimes paid out with a time lag, there is some measurement error in the variable in terms of precisely when the refugees arrived in the municipality (see Nekby & Pettersson-Lidbom, 2012, 2017).[13] For these reasons, we prefer to treat the variable as an endogenous measure of refugee inflows rather than as an exogenous measure of such inflows.

To solve the problems of endogenous settlement patterns and measurement error, and hopefully other sources of omitted variable bias, we exploit the placement program as an instrument, captured by the number of refugees that municipalities were contracted to receive over each period (Nekby & Pettersson-Lidbom, 2012, 2017).[14] This variable by definition gets rid of any endogenous settlement patterns and measurement error in refugee inflows, as it is solely based on predetermined contracts covered by the placement program. The study thus exploits the number of refugees contracted to arrive in the municipalities during the panel periods as instrument for the number of refugees for which municipalities received grant payments during the same periods, both normalized by the municipality population.

Since we utilize two ratios with (almost) the same divisor as main independent variable and instrument, it is important to ensure that the correlation between them does not merely arise because of the common divisor (see Bazzi & Clemens, 2013; Hunt & Clemens, 2017).[15] We test whether or not this is the case by creating a placebo instrument, where we replace the number of refugees contracted to arrive in the municipalities during the panel periods as numerator with Poisson-distributed white noise with the same mean. If the findings are solely due to variation in the divisor, we expect them to be similar when using the placebo instrument. In addition, we test whether the results are robust to splitting the two ratios into separate variables, instrumenting the absolute inflow of refugees with changes in the absolute number of contracted refugees, while simultaneously adjusting for changes in the municipal populations (see Kronmal, 1993). If the findings depend entirely on variation in the divisor, one would expect them to change considerably when doing so.

5.3 | Control variables

As noted in Section 4, the placement program did not contract refugees randomly across different municipalities, but it is still possible to extract variation that is potentially exogenous to individual-level turnout, at least if we adjust for a few municipal-level controls. Apart from breaking the concentration of new refugees in the larger cities, one goal was to place refugees in municipalities with good housing and local labor-market conditions. In interviews with Swedish Migration Board officials, available housing has been upheld as a key factor (see Bengtsson, 2002; Dahlberg et al., 2012).[16] As noted in Section 4, housing was also generally the main obstacle to accept more refugees through the contracting process (SOU, 1996; p. 55). Both local labor-market conditions and housing availability may in turn impact turnout and are thus included as controls in our main models. The former is captured by the local unemployment rate, while the latter is (at least partly) captured by the rate of vacancies in public housing or rental flats, both measured as period averages.[17] Similarly, to account for the placement program's aim to distribute refugees more evenly around the country, we include the period average of the municipal-population size as control.

In addition, it is conceivable that preferences for redistribution in the different municipalities, possibly linked to the functioning of the welfare system, affected the number of refugees they were willing to contract. Indeed, refugees had to be supported by welfare benefits in the short run and were also generally more welfare prone in a longer-term perspective (Dahlberg & Edmark, 2008). Differential preferences for, and ability to engage in, welfare spending may also be linked to turnout—and we therefore include the period average of per-capita welfare spending as a control.[18]

Furthermore, as noted in Section 4, it is possible that the local political situation affected the contracting process and thus the number of contracted refugees. Indeed, Folke (2014) finds that the local political makeup generally affected the influx of refugees into municipalities in the period 1985–2006. Thus, we include the (predetermined) shares of municipal government seats held by left-wing parties (the Social Democratic Party and the Left Party), right-wing parties (the Moderate Party, the Liberal Party, the Center Party, and the Christian Democrats), and the New Democracy Party, a populist anti-immigration party, during each election period in the analysis.[19] We also include dummies indicating left-wing and right-wing majorities in the municipal government. Since municipal governments had to ratify the contracts, and the local political makeup has been found to be related to general refugee inflows, these variables may be important to adjust for differential preferences for refugee immigration among local politicians. As our estimator, discussed in Section 6, effectively adjusts for unobserved differences between municipalities that do not vary over time, and since local ideology in fact did not seem to have been a large obstacle to the contracting process itself (SOU, 1996; p. 55), we believe these controls are sufficient to adjust for the local political context's impact on the contracting process.

Finally, we include the initial total share of foreign citizens to account for the possibility that municipalities may have been less likely to contract a higher number of refugees in the subsequent period if they already had seen high overall immigration in the recent past, which may also affect changes in voter turnout at the individual level.

To study whether or not the variables above are in fact relevant for understanding the placement program, we regressed changes in the share of contracted refugees over the panel periods on the initial levels of the variables. The results are reported in Table A2. As predicted, when excluding municipal-fixed effects, higher rates of vacancies in public housing and per-capita welfare spending predict larger increases in contracted refugee shares, whereas larger populations and initial shares of foreign citizens predict smaller increases in the contracted refugee shares. There is also evidence that municipalities with larger shares of municipal government seats held by the New Democracy Party experienced smaller increases in the shares of contracted refugees, while the share of municipal government seats held by left-wing parties positively predicts changes in contracted refugee shares. The coefficient for unemployment is negative, as expected, but does not reach statistical significance. Nevertheless, the joint F test for all variables combined strongly rejects the null hypothesis of no relationship.

However, when we include municipality dummies in the regression, the picture changes substantially. The only variables that remain statistically significant are the share of municipal government seats held by the New Democracy Party and population size. Consequently, the joint F test now fails to reject the null hypothesis ($p = 0.14$). Thus, while the initial levels of our control variables appear to matter for changes in the contracted refugee shares in the expected ways, these effects mostly disappear when including municipal-fixed effects.

Overall, this exercise suggests that the municipal-level variables discussed above are indeed relevant for understanding the distribution of changes in contracted refugee shares across the country. However, it also suggests that our instrument is much more likely to be exogenous once we condition on time-invariant unobserved differences across municipalities. Of course, despite doing so, it is possible that the municipal-level controls still have some impact on the evolution of the placement program. In our main empirical strategy, formally outlined in Section 6, we thus adjust for these controls as well as municipal-fixed effects.

Certainly, the municipal-level controls, especially those that are averaged over the panel period, may to some extent be affected by immigration—and could thus potentially act as "bad controls" in the regressions (Angrist & Pischke, 2009). Yet since we are interested in turnout responses to refugee immigration itself—rather than indirect responses via changes in the economic and demographic environment—we believe the controls are relevant for retrieving the parameter of interest. Our main results thus most likely capture psychological effects on turnout of increasing diversity, once the impact on any potentially endogenous variables is held constant. As noted, in Section 2, such psychological effects are the most theoretically relevant for the implications of group-threat theory for voter turnout.

Nevertheless, in robustness checks, we also report estimates from models in which we (1) only adjust for the predetermined local political makeup and (2) exclude all municipal-level controls entirely. This serves as a test of the extent to which potentially endogenous controls affect the findings, and more generally the likelihood that our instrument is exogenous. If potentially endogenous municipal-level controls are not crucial for our methodology, we expect the results to be robust to these exercises.

Finally, we include a few individual-level variables measured at the first election in each panel as noise controls. More specifically, we adjust for respondents' year of birth, gender, marital status, labor-market status, blue-collar status, whether they live in villa areas or in high-rise apartments, and an indicator for high income.[20] The latter represents roughly the top 20% in the income distribution at each election. We also include interactions between these variables and the year dummies.[21] The variables constitute key background variables and may all be related to future changes in turnout, which should increase precision in the estimates, but should not affect or be affected by our instrument, at least once we condition on the municipal-level controls.[22] In addition, as noted in Section 6, we include turnout at $t-1$ in some models to test for potential mean reversion and ensure that initial turnout at the individual level does not affect future treatment intensity.

6 | ESTIMATION STRATEGY

The key problem in research studying the relationship between ethnic diversity and voter turnout consists of endogenous settlement patterns and other sources of omitted variable bias. To address these issues, as discussed in Sections 5.3 and 5.4, we exploit the placement-program contracts as instrument for refugee inflows, while taking advantage of the panel structure in the data to analyze the dynamics of individual-level voter turnout. The following are thus our formal baseline equations, estimated in a regular 2SLS set-up:

$$\Delta r_{mt} = \beta_1 \Delta c_{mt} + \beta_2 \bar{x}_{mt} + \beta_3 p_{mt} + \beta_4 b_{im} + \beta_5 i_{mt-1} + \mu_t + \varepsilon_{imt}, \tag{1}$$

$$\Delta v_{imt} = \beta_1 \widehat{\Delta r_{mt}} + \beta_2 \bar{x}_{mt} + \beta_3 p_{mt} + \beta_4 b_{im} + \beta_5 i_{mt-1} + \mu_t + \varepsilon_{imt}. \tag{2}$$

where Δv_{imt} is the change in individual turnout between $t-1$ and t; Δc_{mt} denotes the change in the accumulated number of refugees contracted to arrive in the respondent's municipality of origin, normalized by the municipal population, which serves as the excluded instrument; Δr_{im} represents the change in the accumulated share of refugees for which municipalities received a grant, calculated using data from the respondent's municipality of residence at the time of each election; $\widehat{\Delta r_{mt}}$ denotes the predicted values from the first stage; $\bar{x}_{mt}$ denotes a vector including the period averages of population levels, public housing vacancies, the unemployment rate, and per-capita welfare spending in the municipality of origin; p_{mt} is a vector of variables denoting the shares of municipal government seats held by left-wing parties (the Social Democratic Party and the Left Party), right-wing parties (the Moderate Party, the Liberal Party, the Center Party, and the Christian Democrats), and the New Democracy Party, as well as dummies indicating left-wing and right-wing majorities, in the municipality of origin following the first election; b_{im} is a vector including the individual-level noise controls; i_{mt-1} denotes the initial share of foreign citizens in the municipality of origin in each period; and μ_t represents time-fixed effects, capturing nation-wide trends in the dependent variable. Standard errors are clustered at the municipality level to allow for correlation at the level at which the independent variable of interest is measured.

By differencing the dependent and main independent variables, the model effectively becomes a difference-in-difference instrumental-variable estimator with individual- and municipal-fixed effects.[23] The identification depends on the assumption that the predicted influx of refugees is exogenous to changes in individual turnout, when conditioning on the control variables outlined above. We test whether or not our assumptions are likely to hold by adding turnout at $t-1$. Lagged turnout is likely to be a strong predictor of changes in turnout and should thus increase the precision of our coefficient of interest. Yet, if the results are causal, we should not expect lagged turnout to affect the coefficient as such significantly (Angrist & Piscke, 2009).[24] Including lagged turnout also means that we can test whether mean reversion is likely to bias our findings and is thus an important robustness check of our results.

Based on the above intuition, we would not expect that changes in diversity over the periods analyzed should affect turnout at the first election in the rotating panel. In other words, the sample should be balanced on the dependent variable at the outset. We thus estimate Equations (1) and (2) but swap Δv_{imt} for v_{imt-1} as dependent variable. On the other hand, we should not expect the same sample to be balanced at the second election following treatment, which we test by swapping Δv_{imt} for v_{imt}. If the samples are balanced at the first election but not at the second, this further supports the idea that our main estimates capture the causal effect of refugee inflows on voter turnout.

In addition, we estimate placebo tests in which we regress changes in turnout on future refugee inflows. The idea behind this exercise is simple: if our research design picks up the causal impact of refugee immigration on turnout, changes in refugee shares should not affect past changes in turnout. In the placebo test, we thus study whether changes in individual-level turnout between $t-1$ and t are affected by refugee inflows between t and $t+1$. We then use changes in the accumulated number of contracted refugees in the municipality of origin, normalized by the municipal population, between t and $t+1$ as an instrument for changes in the accumulated refugee share over the same period, calculated using data from the respondents' municipality of residence at the time of each election. Since we have access to data from the election study carried out in 1982, we can carry out also the placebo analysis using three rotating panels—1982/1985, 1985/1988, and 1988/1991—with samples of similar sizes as in the main analyses.

Overall, we believe our research design is likely to capture a causal impact of refugee immigration on individual-level turnout. Particularly, exploiting the contracts as instruments entirely eliminates the problem of self-selection of refugees into certain municipalities, whereas the panel structure of our data allows us to ensure that native mobility does not bias the findings. Importantly, we are able to test whether our assumptions hold using the balancing and placebo tests described. However, we note that if there is any remaining bias, it is likely to drive coefficients in a direction that would make it easier to reject group-threat theory. This is because municipalities with politicians and populations who feel less threatened by, and are more tolerant toward, refugees should be more likely to accept larger contracted refugee inflows, thus biasing any mobilization effects downwards.

7 | RESULTS

In this section, we report our main findings as well as results from the balancing, placebo, and robustness tests. We begin by noting that the results from OLS models in Table 2, which assume that exposure to refugee inflows is exogenous, display a positive, but not always statistically significant, correlation between such inflows and changes in voter turnout: a 1 percentage point increase in the refugee share predicts a 3 percentage point higher probability of voter turnout. This holds true whether or not we include lagged turnout in the models,

TABLE 2 The relationship between refugee inflows and changes in turnout (OLS)

Panel 1: Municipal-level controls and individual-level noise controls			
Election type	National	Municipal	County
	(1)	(2)	(3)
Δ% Refugees	0.030*	0.031**	0.025
	(0.015)	(0.015)	(0.017)
Panel 2: Add initial turnout			
	(7)	(8)	(9)
Δ% Refugees	0.031**	0.032**	0.027*
	(0.012)	(0.013)	(0.014)
n	4,777	4,777	4,366
Municipalities	284	284	281

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects. Respondents are weighted by the inverse of the probability of being selected for the survey in the relevant election.
*$p < .10$; **$p < .05$.

which makes the estimates slightly more precise, and regardless of the election type analyzed. However, since our main independent variable to some extent suffers from endogenous settlement patterns and measurement error, and since the variable also picks up the impact of changes in refugee exposure among movers, these results may well be biased toward zero.

Turning to the 2SLS models in Table 3, this is indeed what the results suggest. The estimates in the first panel display a positive impact of refugee inflows on voter turnout that is about twice as large as the OLS estimates: a 1 percentage point increase in the refugee inflow raises the probability of voter turnout by 5–6 percentage points. In the second panel, which adds lagged turnout, the effects are again very similar but more precise. This is expected if our strategy captures causal effects, as lagged turnout is a strong predictor of changes in turnout.

Meanwhile, the Hausman tests generally display statistically significant values, indicating that the OLS estimates in Table 2 are biased downwards, while the *F*-tests display values considerably higher than 23.1, which is the valid threshold when utilizing cluster-robust standard errors (Olea & Pflueger, 2013). This indicates that refugees self-select into municipalities where natives are less likely to respond by mobilizing politically, that natives leave/avoid municipalities with larger refugee shares (as our findings in Table A1 show), and/or that measurement error drives the OLS results toward zero. It also shows that our instrument based on contracted refugees is strong enough to be utilized. Indeed, the estimates indicate that a 1 percentage point increase in the share of contracted refugees in the home municipality raises the exposure to refugee inflows by 0.65 percentage points. Our results thus suggest that refugee immigration has causal positive effects on voter turnout.

7.1 | Balancing tests

If our research design isolates exogenous variation in refugee inflows during the periods analyzed, we do not expect future treatment to be related to initial turnout. That is, the sample

TABLE 3 The causal effect of refugee inflows on changes in turnout (IV)

Panel 1: Municipal-level controls and individual-level noise controls			
Election type	National	Municipal	County
	(1)	(2)	(3)
Second stage			
Δ% Refugees	0.049***	0.046**	0.051**
	(0.019)	(0.018)	(0.021)
First stage			
Δ% Contracted refugees	0.654***	0.654***	0.653***
	(0.043)	(0.043)	(0.043)
Hausman test	0.142	0.284	0.079
F-statistic	235.81	235.75	228.53
Panel 2: Add initial turnout			
	(4)	(5)	(6)
Second stage			
Δ% Refugees	0.055***	0.053***	0.057***
	(0.016)	(0.016)	(0.018)
First stage			
Δ% Contracted refugees	0.654***	0.654***	0.653***
	(0.043)	(0.043)	(0.043)
Hausman test	0.020	0.050	0.012
F-statistic	235.71	235.55	228.44
n	4,777	4,777	4,366
Municipalities	284	284	281

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects. Respondents are weighted by the inverse of the probability of being selected for the survey in the relevant election.
$^{**}p < .05$; $^{***}p < .01$.

should be balanced on the dependent variable at the first election in the panel, once we adjust for the other variables in the model. However, the same sample should not be balanced following treatment at the second election in the panel. Table 4 displays the findings from this exercise. There is no evidence that future treatment is related to initial turnout, suggesting that the sample is indeed balanced. However, treatment is related to turnout at the second election in the same sample. In fact, the results are essentially identical to our main findings, even though these models analyze the level of, rather than change in, turnout without initial turnout as a control. Yet this is to be expected if there is no correlation between initial turnout and future refugee inflows, as the change in turnout is the difference between turnout at the second and first elections, and thus supports our methodology. Overall, the results thus corroborate the idea that our main results do indeed reflect the causal effects of refugee immigration on voter turnout at the individual level.

TABLE 4 Balancing tests

Panel 1: Turnout at the first election (level)			
Election type	National	Municipal	County
	(1)	(2)	(3)
Second stage			
Δ% Refugees	0.012	0.016	0.010
	(0.018)	(0.017)	(0.018)
First stage			
Δ% Contracted refugees	0.656***	0.656***	0.655***
	(0.043)	(0.043)	(0.043)
Hausman test	0.501	0.410	0.687
F-statistic	238.05	237.99	230.42
Panel 2: Turnout at the second election (level)			
	(4)	(5)	(6)
Second stage			
Δ% Refugees	0.061***	0.060***	0.061***
	(0.017)	(0.018)	(0.018)
First stage			
Δ% Contracted refugees	0.654***	0.654***	0.653***
	(0.043)	(0.043)	(0.043)
Hausman test	0.018	0.030	0.016
F-statistic	235.81	235.75	228.53
n	4,777	4,777	4,366
Municipalities	284	284	281

Note: Standard errors clustered at the municipality level in parentheses. All regressions include municipal-level controls, individual-level noise controls, and period-fixed effects. Respondents are weighted by the inverse of the probability of being selected for the survey in the relevant election.

***$p < .01$.

7.2 | Placebo tests

To provide further evidence on the causality of our findings, we carry out the formal placebo test in treatment, as described in Section 6. We do so by lagging the panel of voters by one period and pretend that refugee inflows occurred in the previous panel period. We add data from the election study in 1982 to carry out the placebo tests over three panels to make it as similar as possible to the main analysis. This means that changes in voter turnout in the periods 1982–1985, 1985–1988, and 1988–1991 are regressed on refugee inflows due to the placement program in the periods 1985–1988, 1988–1991, and 1991–1994 respectively. If our assumptions hold true, we should not find any significant estimates in this analysis since treatment occurs in the period after the change in individual turnout is observed. Reassuringly, the results in

TABLE 5 Placebo tests regressing changes in turnout on future refugee inflows

Panel 1: Municipal-level controls and individual-level noise controls			
Election type	National	Municipal	County
	(1)	(2)	(3)
Second stage			
Δ% Refugees$_{t+1}$	−0.001	−0.002	−0.005
	(0.010)	(0.010)	(0.010)
First stage			
Δ% Contracted refugees$_{t+1}$	0.692***	0.691***	0.693***
	(0.076)	(0.076)	(0.077)
Hausman test	0.947	0.863	0.833
F-statistic	82.54	82.64	80.69
Panel 2: Add initial turnout			
	(4)	(5)	(6)
Second stage			
Δ% Refugees$_{t+1}$	0.001	0.001	-0.001
	(0.012)	(0.013)	(0.013)
First stage			
Δ% Contracted refugees$_{t+1}$	0.692***	0.691***	0.693***
	(0.076)	(0.076)	(0.077)
Hausman test	0.272	0.273	0.325
F-statistic	83.86	83.76	81.80
n	4,930	4,881	4,450
Municipalities	284	284	281

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects and weight respondents by the inverse of the probability of being selected for the survey in the relevant election.

***$p < .01$.

Table 5 display no relationship between changes in individual turnout and future immigration spurred by the placement program. The point estimates are very close to zero and far from statistically significant. Thus, the results from the placebo tests support the causal interpretation of our main findings.

7.3 | Robustness tests

In this section, we provide evidence on the robustness of our findings. First, we test to what extent the results change when we utilize the placebo instrument described in Section 5.2. This is to ensure that the results are not merely a consequence of changes in the size of the municipal population, which serves as the divisor in both the main independent variable and the instrument. We also test whether the results are robust to splitting each of the two ratios into separate variables, instrumenting the absolute inflow of refugees with changes in the absolute number of contracted refugees, while simultaneously adjusting for changes in the municipal population. The results in Table A3 show that the second-stage results turn insignificantly negative when using the placebo instrument, with the *F*-tests in the first stage showing no correlation whatsoever between the placebo instrument and the main independent variable. Furthermore, when splitting the ratios into separate variables, the results are very similar to our main findings, while the Hausman tests indicate that the OLS estimates are even more biased downwards than in our main models. Indeed, in sharp contrast to the IV models, the equivalent OLS estimates in Table A4 generally display no statistically significant relationship between the absolute inflow of refugees and voter turnout.[25] Overall, these robustness tests thus confirm that our main results are due to the variation in refugee inflows spurred by the placement program, rather than a spurious correlation arising from the common divisor, and further highlight the importance of utilizing variation in refugee immigration that is free from bias due to endogenous settlement patterns and measurement error.

In further robustness tests, as noted in Section 5.3, we analyzed the extent to which the results were affected by only including the local political makeup as control at the municipal level, and by excluding all municipal-level controls entirely. We also tested omitting all individual-level noise controls apart from respondents' year of birth. Furthermore, we excluded municipalities with more than 50,000 inhabitants from the analysis, thus halving the sample size to 2,382–2,389 respondents in 244 municipalities. As one of the aims behind the placement program was to redistribute refugee inflows to smaller municipalities, we expect results to be similarly pronounced in this analysis. Finally, as noted in Section 5.1, we carried out the intention-to-treat analysis, in which both changes in refugee shares and the instrument are calculated from the municipality of origin, as well as a reduced-form analysis. As Tables A5–A10 show, the results are entirely robust to these exercises.[26]

Overall, all results thus point in one direction: (1) refugee inflows spurred by the placement program had positive effects on voter turnout, and (2) strategies that do not properly deal with endogenous settlement and mobility patterns as well as measurement error are likely to bias estimates toward zero. More generally, the findings provide relatively strong evidence in favor of group-threat theory, in the context of changing immigration patterns in Sweden in the last decades of the 20th century.

8 | CONCLUSION

This study has investigated the impact of refugee immigration on voter turnout in Sweden in the period 1985–1994. Exploiting a placement program through which refugees were contracted to the country's municipalities, and individual-level panel data almost entirely free from attrition, we have sought to obtain conditionally exogenous variation in refugee inflows, to be able to draw conclusions regarding causal inference. This is also our principal contribution to a literature that thus far mostly has provided associational evidence and/or analyzed aggregate outcomes.

Our results showed that refugee inflows spurred by the placement program increased the probability of voter turnout. Balancing tests on initial turnout, and placebo tests analyzing whether refugee inflows predict prior changes in individual-level turnout, corroborate the causal interpretation of the results, as do several robustness checks. The main findings also differ from OLS estimates, which are smaller as well as less precise and robust. Endogenous settlement and mobility patterns and/or measurement error thus appear to bias the results toward zero, highlighting the importance of obtaining variation in refugee inflows free from these problems to draw credible causal inferences.

Certainly, since the survey we exploit was designed to be representative at the country level rather than the municipal level, the external validity of the findings may be questionable; the results should primarily be seen as valid for the randomly sampled population in each municipality. It is also important to note that the effects are obtained in the specific context of increasing refugee immigration in a previously homogeneous country; whether other types of immigration have similar effects is unclear. Furthermore, it is unclear whether the increased probability in individual-level turnout translates into higher aggregate turnout, which partly depends on how immigration affects overall demographic changes across communities. The extent to which these issues affect our conclusions is an important venue for future research. Future research may further analyze potential heterogeneity in the turnout responses between native, naturalized, and foreign citizens from refugee immigration. It should also further examine how such immigration impacts voter preferences for different political parties, although such analyses generally require aggregated data that make it difficult to disentangle the effects of interest from native mobility as a response to immigration.

Since group-threat theory was formulated over half a century ago, a large body of research has sought to evaluate its relevance for understanding various social and political outcomes. Perceptions of threat may potentially be addressed through increased meaningful intergroup interactions, which have been found to improve intergroup attitudes in some contexts, but such interactions tend to be endogenous to existing attitudes. While silent on the effects of positive contact, our study provides evidence supporting group-threat theory in the context of real-world demographic changes that do not necessarily generate positive contact between in- and out-group members in the short-term perspective. In light of intense debate regarding potential consequences of current European immigration patterns, this is an important finding to which policymakers should pay attention.

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CONFLICT OF INTEREST

The author declares no conflict of interest.

DATA AVAILABILITY STATEMENT

Some of the data that support the findings of this study are available from Swedish National Data Service (SND). Restrictions apply to the availability of these data. Data are available from the author with the permission of SND.

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ENDNOTES

[1] This appears to include Dahlberg et al.'s (2012) study, which utilizes the Swedish placement program analyzed here and finds negative effects of diversity on redistributive preferences. Yet Nekby and Pettersson-Lidbom (2012, 2017) show that the results are unreliable because of a partially endogenous instrument and sample selection bias due to considerable attrition (see Dahlberg et al. [2013, 2017] for rejoinders). As discussed in Section 5.2, we circumvent these problems by (1) using the endogenous instrument as our primary independent variable capturing refugee inflows, while (2) exploiting an alternative instrument developed by Nekby and Pettersson-Lidbom (2012, 2017) that is more likely to be exogenous, (3) analyzing a dependent variable with very little attrition, and (4) weighting respondents by the inverse of their probability of being selected for the survey in the relevant election.

[2] While the effects of residential segregation may make studies analyzing larger geographical areas less likely to pick up significant intergroup interaction (see Enos, 2016; Zingher & Thomas, 2014), voluntary contact is always endogenous regardless of geographical area analyzed.

[3] It is also not always clear whether the identification strategies work as intended. For example, some research exploits historical levels of immigration and housing stock as instruments (e.g., Barone et al., 2016; Harmon, 2017), which may generate bias because of potential serial correlation in unobserved regional characteristics that both attract immigrants, or change the housing stock, and affect outcomes.

[4] We thank Karin Edmark and Per Pettersson-Lidbom who kindly supplied data on most municipal-level variables used in the study, which are obtained from Statistics Sweden, the Swedish Migration Agency, and the Swedish Public Employment Service. We obtained the remaining municipal-level variables, and updated some variables supplied by the authors, using Statistics Sweden's (2016) database.

[5] The negative minimum value for Δ% Refugees is due to (1) two observations where the municipal population increased faster during the period than the refugee inflow over that period and (2) 28 observations covering respondents who moved to a municipality with a smaller accumulated refugee inflow in the year of the second election than the initial accumulated refugee inflow in the home municipality. If these observations are excluded, the minimum is zero.

[6] The election-survey data were obtained from the Swedish National Data Service (SND) and were originally collected within a research project at the Department of Political Science, University of Gothenburg. For the main years analyzed in this study, the principal investigators were Sören Holmberg and Mikael Gilljam. Neither SND nor the principal investigators bear any responsibility for the analyses and findings in this study.

[7] This means that we can only study the impact of refugee immigration on voter turnout among Swedish citizens. An interesting question is whether native and naturalized (and foreign) citizens respond differently to refugee immigration. However, due to sample type and size, this issue cannot be convincingly analyzed in this study.

[8] For example, our results indicate that a 1 percentage point larger accumulated share of contracted refugees in the municipality of origin at $t - 1$ is associated with a 10 percentage point higher likelihood of moving. Movers are also exposed to a 0.13–0.19 percentage point smaller increase in the accumulated contracted refugee share relative to non-movers. As the findings in Table A1 display, the results are very similar when studying the share of refugees for which municipalities received a grant. Also, unreported findings show that movers display more positive changes in turnout on average, are younger, and have lower income than non-movers. This shows the importance of accounting for mobility bias, which is only possible when analyzing individual-level panel data.

[9] Of course, our choice to focus on individual-level turnout also means that the estimated effects cannot necessarily be extrapolated to the average municipal population. This is because the survey we exploit was designed to be representative of the national population rather than the municipal populations. Thus, the results should primarily be seen as valid for the randomly sampled population in each municipality.

[10] The available panel sample with voting records totaled 1,599 individuals in 1988 (compared with 1,901 individuals in the original 1985 panel sample), 1,700 individuals in 1991 (compared with 1,956 individuals in

the original 1988 panel sample), and 1,481 in 1994 (compared with 1,714 individuals in the original 1991 panel sample), giving a total available panel sample of 4,780 individuals out of the original panel sample of 5,571.

[11] The inverse probability of selection is defined as: 1/(eligible voters in the municipality of origin/eligible voters in the country). We obtain data on the number of eligible voters, nationally and in the different municipalities, from Statistics Sweden (2016).

[12] This is supported by a comparison of the total number of refugees received in the municipalities and the total number of refugees for which municipalities received grant payments in the period 1991–1994: in 1991, 1992, 1993, and 1994 respectively, a total of 18,961, 18,472, 25,300, and 61,500 refugees were received (Swedish Migration Agency, 2018) and there were 18,842, 18,546, 25,218, and 62,853 refugees for which municipalities received grants, according to our data. The slight discrepancies between the two measures are likely explained by the fact that there was sometimes a time lag between refugee arrival and the grant payment, as noted below.

[13] As indicated by the previous footnote, measurement error due to the time lag between refugee arrival and the grant payment appears to be relevant for a relatively small proportion of refugees in total, at least in the period 1991–1994, but we cannot rule out that there are differences between the municipalities in this respect.

[14] Further showing the differences between the contracts and grant payments, yearly changes in the absolute number of contracted refugees are not always strongly correlated with yearly changes in the absolute number of refugees. However, they are strongly correlated over the panel periods, which is what is relevant for this study (see Dahlberg et al., 2013; Nekby & Pettersson-Lidbom, 2012, 2017). Apart from the micro-level regressions reported here, we also found support for this relationship in unreported municipal-level panel regressions over the periods analyzed.

[15] The ratios do not have exactly the same divisor because of how we calculate the endogenous variable for movers, as explained in Section 5.1.

[16] The empirical evidence on whether available housing in fact was important for refugee placement is inconclusive (Dahlberg et al., 2013; Nekby & Pettersson-Lidbom, 2012, 2017). However, we present evidence indicating that initial levels of available housing predict changes in the contracted refugee shares, suggesting that available housing at least played a role in determining the contracts.

[17] We use the rate of vacancies in public housing or rental flats in September each year, but results are essentially identical if we use the rate of vacancies in March each year.

[18] Data on welfare spending are missing for 38 respondents in five municipalities in 1991 and 1994 in our sample, which we deal with by inter- and extrapolating welfare spending based on the municipal-specific trend in such spending. However, results are unsurprisingly essentially identical if we instead drop these respondents from the sample. In unreported regressions, we also adjusted for the period average of the per-capita tax base, which theoretically may affect, and be affected by, the share of contracted refugees as well as voter turnout for similar reasons. However, the results were almost identical when doing so and the per-capita tax base was not related to refugee inflows in the first stage.

[19] The excluded category is thus the shares of municipal government seats held by the Green Party and small political parties that were never represented at the national level in the period analyzed. Results are very similar if we also include the Green Party seat share, with or without exclusion of some of the other parties' shares, or if we include the vote shares of all parties separately.

[20] There are some missing values on the marriage, labor-market, blue-collar, and housing indicators. We replace these with zero and include separate indicators for missing values in the regressions.

[21] This means that we allow the effects of all noise controls to differ depending on the year of the election. Since the income information provided is not entirely consistent across survey years, these indicators vary slightly across years. Also, the income information in the 1988 survey was calculated from 1986 rather than from the

year of the election, as in the other surveys. Including interactions between the indicators and the year dummies picks up the effects of these slight differences across years.

[22] As noted in Section 7.3, the results are very similar when excluding all noise controls apart from year of birth, although they do become slightly less precise.

[23] A possible alternative would be to aggregate the data at the municipal level and weight the observations by the number of respondents in the panel sample, or the total voting-age population, in each municipality for each election (see Angrist & Pischke, 2009; Nekby & Pettersson-Lidbom 2017). However, this is not possible in models where we calculate Δr_{mt} for movers using the municipality of origin in $t-1$ and municipality of residence in t, since the primary independent variable then differs also between respondents originating in the same municipality. Furthermore, to increase precision, we include individual-level noise controls, which makes it more complicated to replicate the regressions in aggregate form (Angrist & Piscke, 2009). Thus, since there is no inherent advantage in studying aggregated data for the sake of it, we stick with individual-level data and cluster the standard errors at the municipal level.

[24] Note that the inclusion of voter turnout at $t-1$ means that the model no longer includes individual- and municipal-fixed effects. If the results are very similar without such effects included, while adjusting for individuals' initial turnout, it further supports the causal interpretation of the findings.

[25] The exception is Model 6, which is the OLS equivalent of Model 12 in Table A3, where the coefficient is statistically significant at the 5% level. However, also in this case does the Hausman test suggest that the OLS coefficient is strongly biased downwards. Indeed, the OLS coefficient is only 13% of the IV coefficient.

[26] However, the results are not robust to using the reduced samples in Dahlberg et al.'s (2013) study, which analyzes a survey response as dependent variable and consequently suffers from attrition. When analyzing their samples, giving us up to 2,439 respondents in 276 municipalities, with the exact number depending on the sample restrictions, our estimates are closer to zero and not statistically significant. The bias toward zero is also stronger in proportion to the degree of attrition. These findings support Nekby and Pettersson-Lidbom's (2012, 2017) argument: significant nonrandom attrition is likely to pose a problem for studies linking the placement program to survey data more generally. A key strength of this study is that we are able to circumvent this problem almost entirely.

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APPENDIX

Tables A1-A10

TABLE A1 Refugee inflows and native mobility

The probability of moving			
	No controls	Municipal controls	All controls
	(1)	(2)	(3)
% Refugees (destination)$_t$	−0.050***	−0.050**	−0.044**
	(0.018)	(0.020)	(0.019)
% Refugees (origin)$_{t-1}$	0.073**	0.072**	0.068**
	(0.037)	(0.035)	(0.034)
	(4)	(5)	(6)
% Contracted refugees (destination)$_t$	−0.057**	−0.054**	−0.044*
	(0.023)	(0.023)	(0.022)
% Contracted refugees (origin)$_{t-1}$	0.099**	0.102**	0.097**
	(0.046)	(0.044)	(0.044)
The refugee/contracted refugee exposure among movers relative to nonmovers			
	Δ% Refugees (exposure)		
	(7)	(8)	(9)
Mover	−0.180***	−0.164**	−0.148**
	(0.060)	(0.065)	(0.064)
	Δ% Contracted refugee (exposure)		
	(10)	(11)	(12)
Mover	−0.190***	−0.168***	−0.133**
	(0.054)	(0.057)	(0.056)
n	4,777	4,777	4,777
Municipalities	284	284	284

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects and weight respondents by the inverse of the probability of being selected for the survey in the relevant election. The municipal controls are the same as in Table A2. All controls include the municipal controls as well as the individual controls outlined in Section 5.3.
*$p < .10$; **$p < .05$; ***$p < .01$.

TABLE A2 Predictors of changes in contracted refugee shares (% of municipal population)

	(1)	(2)
Welfare spending per capita/1000$_{t-1}$	0.948***	0.892
	(0.353)	(1.111)
% Vacant public housing$_{t-1}$	0.049**	0.016
	(0.022)	(0.024)
% Unemployment$_{t-1}$	−0.029	−0.093
	(0.024)	(0.112)
Population/1000$_{t-1}$	−0.001*	−0.046**
	(0.001)	(0.022)
% Foreign citizens$_{t-1}$	−0.018*	0.054
	(0.009)	(0.060)
% Left-wing seats	0.014**	0.006
	(0.007)	(0.011)
% Right-wing seats	0.006	−0.004
	(0.007)	(0.015)
% New Democracy Party seats	−0.048***	−0.036**
	(0.017)	(0.016)
Left-wing majority	−0.065	−0.120
	(0.064)	(0.202)
Right-wing majority	0.061	0.228
	(0.073)	(0.216)
Adjusted R^2	0.339	0.490
Joint F-test of all variables (p-value)	0.001	0.137
Municipal-fixed effects	No	Yes
n	852	852
Municipalities	284	284

Note: Standard errors clustered at the municipality level in parentheses. All models include period-fixed effects.
*$p < .10$; **$p < .05$; ***$p < .01$.

TABLE A3 Using the placebo instrument and analyzing levels instead of ratios

Placebo instrument			
Panel 1: Municipal-level controls and individual-level noise controls			
Election type	National	Municipal	County
	(1)	(2)	(3)
Second stage			
Δ% Refugees	−0.110	−0.119	−0.111
	(0.120)	(0.130)	(0.131)
First stage			
Δ% White noise	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)
Hausman test	0.049	0.039	0.070
F-statistic	2.58	2.58	2.61
Panel 2: Add initial turnout			
	(4)	(5)	(6)
Second stage			
Δ% Refugees	−0.082	−0.088	−0.079
	(0.111)	(0.119)	(0.119)
First stage			
Δ% White noise	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)
Hausman test	0.071	0.056	0.115
F-statistic	2.58	2.58	2.60
n	4,777	4,777	4,366
Municipalities	284	284	281
Analyzing levels instead of ratios			
Panel 1: Municipal-level controls and individual-level noise controls			
Election type	National	Municipal	County
	(7)	(8)	(9)
Second stage			
ΔRefugees/1000	0.091**	0.082**	0.107**
	(0.041)	(0.041)	(0.046)
First stage			
ΔContracted refugees/1000	0.714***	0.714***	0.745***
	(0.137)	(0.137)	(0.114)
Hausman test	0.056	0.100	0.038

(Continues)

F-statistic	27.11	27.11	42.37
Panel 2: Add initial turnout			
	(10)	(11)	(12)
Second stage			
ΔRefugees/1000	0.107***	0.101***	0.122***
	(0.036)	(0.036)	(0.041)
First stage			
ΔContracted refugees/1000	0.714***	0.714***	0.745***
	(0.137)	(0.137)	(0.115)
Hausman test	0.008	0.016	0.005
F-statistic	27.04	27.04	42.19
n	4,777	4,777	4,366
Municipalities	284	284	281

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects and weight respondents by the inverse of the probability of being selected for the survey in the relevant election.

**$p < .05$; ***$p < .01$.

TABLE A4 OLS estimates when analyzing levels instead of ratios

Panel 1: Municipal-level controls and individual-level noise controls			
Election type	National	Municipal	County
	(1)	(2)	(3)
ΔRefugees/1000	0.010	0.011	0.015
	(0.009)	(0.009)	(0.013)
Panel 2: Add initial turnout			
	(4)	(5)	(6)
ΔRefugees/1000	0.009	0.010	0.016**
	(0.007)	(0.007)	(0.008)
n	4,777	4,777	4,366
Municipalities	284	284	281

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects and weight respondents by the inverse of the probability of being selected for the survey in the relevant election.

**$p < .05$.

TABLE A5 Excluding all municipal-level controls apart from local political makeup

Panel 1: Local political makeup and individual-level noise controls			
Election type	National	Municipal	County
	(1)	(2)	(3)
Second stage			
Δ%Refugees	0.041**	0.038**	0.039**
	(0.018)	(0.017)	(0.019)
Hausman test	0.165	0.300	0.090
F-statistic	195.60	195.60	189.41
Panel 2: Add initial turnout			
	(4)	(5)	(6)
Second stage			
Δ% Refugees	0.042***	0.041***	0.040**
	(0.015)	(0.015)	(0.016)
Hausman test	0.023	0.050	0.015
F-statistic	195.96	195.78	189.71
n	4,777	4,777	4,366
Municipalities	284	284	281

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects and weight respondents by the inverse of the probability of being selected for the survey in the relevant election.

**$p < .05$; ***$p < .01$.

TABLE A6 Excluding all municipal-level controls

Panel 1: Individual-level noise controls			
Election type	National	Municipal	County
	(1)	(2)	(3)
Second stage			
Δ% Refugees	0.036**	0.032**	0.035**
	(0.016)	(0.015)	(0.017)
Hausman test	0.239	0.411	0.146
F-statistic	185.60	185.63	179.38
Panel 2: Add initial turnout			
	(4)	(5)	(6)
Second stage			
Δ% Refugees	0.035***	0.033**	0.034**
	(0.013)	(0.014)	(0.014)
Hausman test	0.047	0.095	0.029
F-statistic	186.10	185.90	179.79
n	4,777	4,777	4,366
Municipalities	284	284	281

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects and weight respondents by the inverse of the probability of being selected for the survey in the relevant election.

**$p < .05$; ***$p < .01$.

TABLE A7 Excluding all individual-level noise controls apart from year of birth

Panel 1: Municipal-level controls and respondents' year of birth			
Election type	National	Municipal	County
	(1)	(2)	(3)
Second stage			
Δ% Refugees	0.042**	0.037**	0.043**
	(0.019)	(0.019)	(0.022)
Hausman test	0.226	0.389	0.117
F-statistic	232.05	231.98	224.22
Panel 2: Add initial turnout			
	(4)	(5)	(6)
Second stage			
Δ% Refugees	0.053***	0.051***	0.054***
	(0.017)	(0.018)	(0.019)
Hausman test	0.025	0.055	0.012
F-statistic	231.69	231.49	223.98
n	4,777	4,777	4,366
Municipalities	284	284	281

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects and weight respondents by the inverse of the probability of being selected for the survey in the relevant election.

**$p < .05$; ***$p < .01$.

TABLE A8 Excluding municipalities with more than 50,000 inhabitants

Panel 1: Municipal-level controls and individual-level noise controls			
Election type	National	Municipal	County
	(1)	(2)	(3)
Second stage			
Δ% Refugees	0.047**	0.046**	0.047**
	(0.020)	(0.020)	(0.023)
Hausman test	0.145	0.252	0.110
F-statistic	223.16	223.07	220.24
Panel 2: Add initial turnout			
	(4)	(5)	(6)
Second stage			
Δ% Refugees	0.058***	0.058***	0.056***
	(0.018)	(0.018)	(0.020)
Hausman test	0.019	0.039	0.021
F-statistic	222.84	222.70	219.97
n	2,388	2,389	2,382
Municipalities	244	244	244

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects and weight respondents by the inverse of the probability of being selected for the survey in the relevant election.

**$p < .05$; ***$p < .01$.

TABLE A9 Intention-to-treat estimates

Panel 1: Municipal-level controls and individual-level noise controls			
Election type	National	Municipal	County
	(1)	(2)	(3)
Second stage			
Δ%Refugees	0.046***	0.042**	0.047**
	(0.017)	(0.016)	(0.019)
Hausman test	0.341	0.646	0.242
F-statistic	145.58	145.55	144.60
Panel 2: Add initial turnout			
	(4)	(5)	(6)
Second stage			
Δ% Refugees	0.051***	0.049***	0.052***
	(0.014)	(0.014)	(0.016)
Hausman test	0.094	0.202	0.073
F-statistic	145.67	145.58	144.68
n	4,777	4,777	4,366
Municipalities	284	284	281

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects and weight respondents by the inverse of the probability of being selected for the survey in the relevant election.

**$p < .05$; ***$p < .01$.

TABLE A10 Reduced-form estimates

Panel 1: Municipal-level controls and individual-level noise controls			
Election type	National	Municipal	County
	(1)	(2)	(3)
Δ% Contracted refugees	0.032**	0.030**	0.033**
	(0.013)	(0.013)	(0.015)
Panel 2: Add initial turnout			
	(4)	(5)	(6)
Δ% Contracted refugees	0.036***	0.035***	0.037***
	(0.014)	(0.013)	(0.012)
n	4,777	4,777	4,366
Municipalities	284	284	281

Note: Standard errors clustered at the municipality level in parentheses. All regressions include period-fixed effects and weight respondents by the inverse of the probability of being selected for the survey in the relevant election.

**$p < .05$; ***$p < .01$.